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HISTORIES OF OLD AGES

Essays in honour of Rhys Jones

S JONES concluded a distinguished academic career when he retired from The Australian National University in June 2001. A Professor in the Research School of Pacific and Asian Studies, he is a graduate of Cambridge and Sydney (PhD) universities, a Fellow of the Australian Academy of the Humanities and the Society of Antiquaries, London, and an Honorary Fellow and Professor of the University of Wales, Lampeter and Newport respectively. He was the Australian Fellow in the Institute of Advanced Studies in the Humanities, Edinburgh, in 1991 and held the chair of Australian Studies at Harvard University from 1995–96. For many years he served on the Council of the Australian Institute of Aboriginal and Torres Strait Islander Studies.

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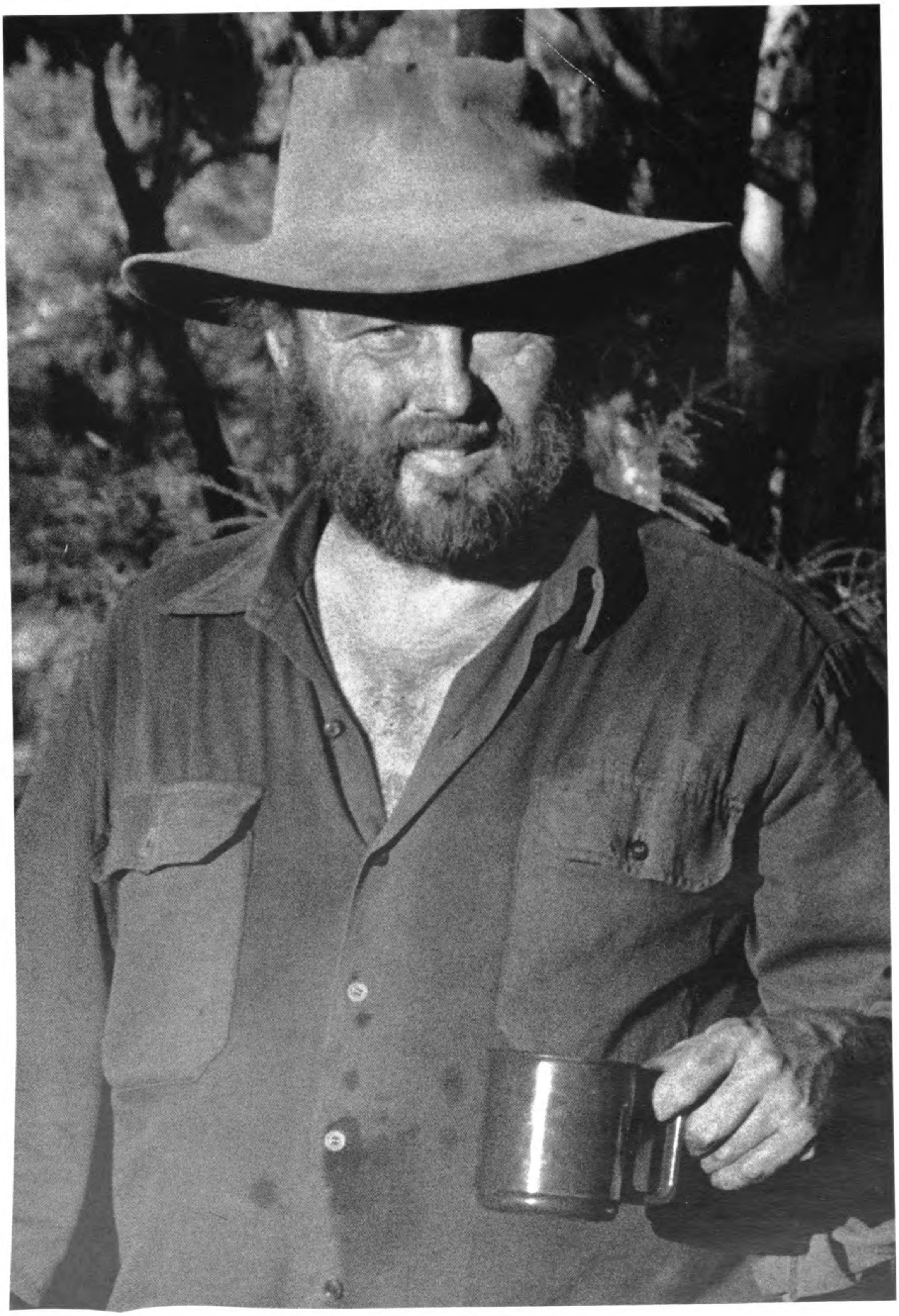

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PANDANUS BOOKS
Research School of Pacific and Asian Studies
THE AUSTRALIAN NATIONAL UNIVERSITY

Cover picture: Rhys Jones exploring the Walls of China at Lake Mungo in 1974.
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Contents

Foreword

Rhys Jones concludes distinguished academic service by retirement from the Australian National University in June 2001. Several coincident proposals in 1999 to publish a volume of papers in his honour resulted in the formation of an Editorial Committee which, in addition to the editors, included Jim Allen, Wal Ambrose and Betty Meehan. Their substantial efforts in all aspects of the organisation, editing and publication of this volume are gratefully acknowledged. We wished them to be listed as editors but they declined.

Rhys has been an adornment to the academic life of his adopted country and we take this opportunity to record our deep appreciation of his fine qualities of intellectuality, collegiality and friendship – and courage too, in the face of serious illness.

The idea for the title comes from an englyn by John Morgan (1688–1733) in memory of Edward Lhuyd (1660–1709) the pioneer antiquarian, linguist, palaeontologist and second Keeper of the Ashmolean Museum Oxford. It is one of Rhys' favourite poems.

Er Côf Am Edward Lhuyd

Meini nadd a mynyddoedd — a gwaliau
Ac olion dinasoedd
A dail, dy fyfyrdod oedd,
A hanesion hen oesoedd.

In Memory of Edward Lhuyd

Worked stones and mountains - and walls
And remains of cities
And leaves, your interests were,
And histories of old ages.

John Morgan, 1688–1733

(From Thomas Parry (ed.) 1962 The Oxford Book of Welsh Verse.
Oxford: Clarendon Press). As translated by Rhys Jones

We thank all the contributors to this volume for their patience and cooperation, and Mr Ian Templeman and staff of Pandanus Books for their assistance in publication. Contributions towards the cost of publication were made by the Aboriginal and Torres Strait Islander Studies Unit, University of Queensland, The Department of Archaeology and Natural History, RSPAS, The Centre for Archaeological Research, ANU, The Division of Society and Environment, RSPAS and the Research School of Pacific and Asian Studies, ANU. We thank them all.

The Editors

The Early Life of a New Chum, 1941–1969

Betty Meehan
'Ty'r Paith'
Hoskinstown NSW Australia

FIRST SIGHTING

I remember what Rhys was wearing when I first met him in late August 1963 — grey flannel trousers with cuffs, a blue and white striped shirt with the sleeves rolled up and the collar unbuttoned, and sturdy brown leather, Lotus Veldschoen shoes which had a traditional pattern punched into the leather uppers. He was fit, slim and beardless (Fig. 1)[1]. His boyish face ended with a delicately pointed chin and his body radiated energy, strength, enthusiasm and combativeness. Altogether, he was a most attractive man, but a little daunting for a working class country girl who had enrolled at Sydney University as a mature age student the year before, after teaching in infant schools for several years. Both Rhys and I were attending a conference on Australian archaeology organised by the Department of Adult Education in the University of Sydney and had accompanied a group of fellow conferencees to have lunch in a Chinese restaurant in Glebe Road just opposite the main gates of the University. I cannot remember all the people at that lunch but apart from Rhys and me, Jim Allen was certainly present. Rhys was seated at one end of the table, I was at the other. We argued about archaeology. It was immensely exciting, the beginning of my intellectual — and later, personal-love affair with this vibrant and charming Welshman from Cambridge.

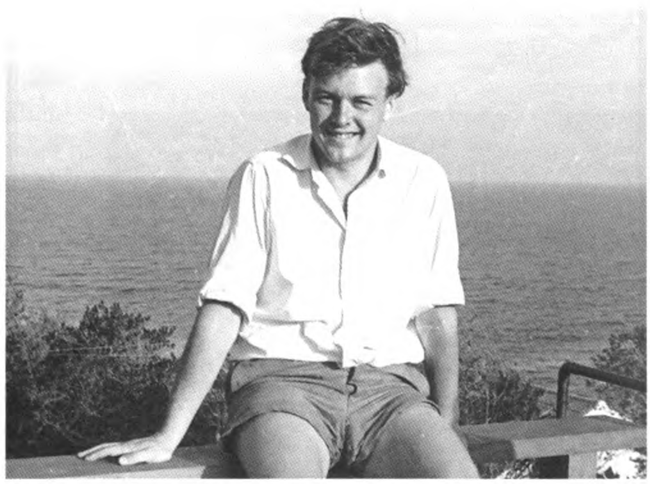

Figure 1. September 1963. Rhys at Ruth Finke's house, northern beaches, Sydney.

FAMILY BACKGROUND

Rhys Maengwyn Jones was born during the Second World War, on 26 February 1941, in Bangor, a town on the Menai Straits in Gwynedd, north Wales. He weighed 'six and a half pounds' and was delivered by Dr O. V. Jones – affectionately and amusingly nicknamed by local Welsh people, 'Ovary Jones'. Rhys' family was at that time living in a house called 'Y Gilfach' (Little Sanctuary) in Llangefni, on the island of Sir Fôn, across the famous Menai suspension bridge built ca. 1820 by the Bangor engineer, Thomas Telford (Fig. 2).

His mother, Enid Watkin Jones, was born in 1906 in Maesteg, south Wales. Her father, William Rhys Watkin, was a Baptist minister in the Tabernacle chapel. Later he moved to Moriah chapel in Llanelli, also in south Wales, where he died in 1947. Rhys bears his maternal grandfather's name and shares many of his intellectual

1. Unless stated otherwise all the of the images used in this article come from the Meehan-Jones collections

Figure 2. 1942. Rhys aged c. eighteen months old at 'Y Gilfach', Llangefni, Sir Fôn, north Wales.

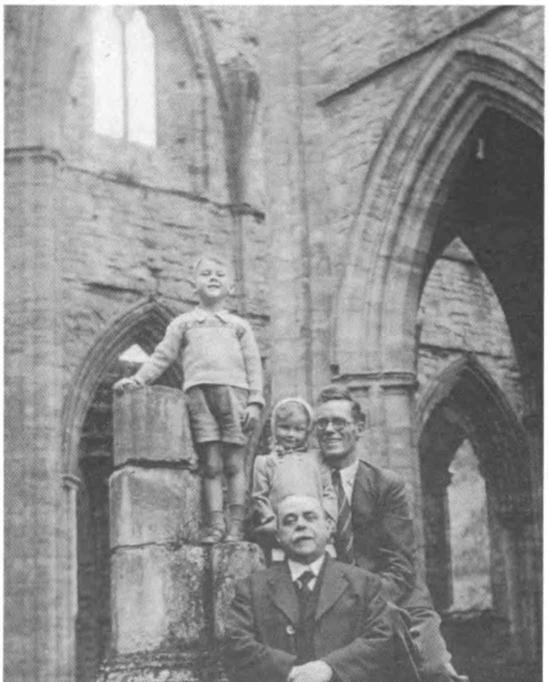

Figure 3. 1946. Rhys (top left) with his sister Non, his father Griffith and his grandfather William Rhys Watkin (his mother's father) at Tintern Abbey, Wales.

and physical characteristics: William Rhys was a prolific reader and collector of books and other items of historical significance (Fig. 3). He was short and stout in stature. The oldest child in his family, he was orphaned at the age of 12 and soon after began to work in the mines. He educated himself at night school before becoming a Baptist minister. As well, he was a Celtic scholar, producing the first MA thesis about a Welsh poet for Professor John Morris at the University of Bangor. At another stage in his life he became President of the Baptist Union and a Liberal Party stalwart assisting Lloyd George to organise his political campaigns. A gifted orator, he was invited to preach many times in Baptist chapels in southern USA.

Enid's mother, Rhys' maternal grandmother, Jane Williams, was headmistress of Maesteg's Plas Newydd Girls School before her marriage.

Rhys' father, Griffith Maengwyn Jones (or 'GM' as he was known) was a physicist by training, who carried out research into x-ray diffraction while lecturing in physics at the University of Wales, Aberystwyth, in the 1920s. Later he became Inspector of Schools in the same area. He was born in the famous slate quarry town of Blaenau Ffestiniog in north Wales. During the Second World War he continued physics research as an officer in the Royal Air Force. He died in 1954 with a brain tumour when Rhys was 12 years old. Rhys carries his father's middle name, Maengwyn which in English means 'white stone'. This name is derived from Ynys Maengwyn — an island of hard ground in coastal wetlands near the town of Tywyn , south Merionethshire where several standing stone arrangements are extant.

GM's mother, Elizabeth Jones, eventually moved from Tywyn, where she was born, to Blaenau Ffestiniog where she ran her own tailoring business. His father, David Jones, was a shot-man, the person who drilled the holes for explosive charges in the Maen Offeren slate quarry. He learnt to read and write as a deacon in the Independent chapel of Jerusalem. Seven of GM's brothers and sisters survived into adulthood. One of them, Cadfan, eventually graduated from Cambridge.

Rhys' only sibling, his sister Non Watkin Evans, was born in 1943, also in Bangor. While Non has visited Rhys in Australia several times, she has continued to live in Wales in a magnificent medieval stone manor house near Cardiff, called 'Trewallter Fawr', which she has painstakingly restored over the last 30 years. After rearing two children, Dylan and Lowri, she returned to teaching in a school where lessons are in Welsh. She now has two grandchildren, Gruffydd and Twm.

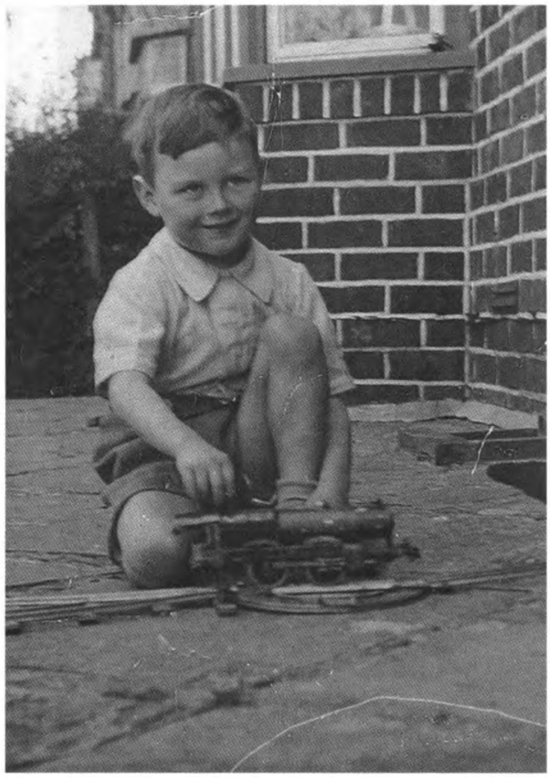

Figure 4. c.1945. Rhys at his home 'Laburnam Cottage', Usk, Monmouthshire with his 'Flying Scotsman' engine.

Early Childhood (Fig. 4)

Welsh was the first language for both of Rhys' parents, though of course they also spoke English. Rhys' mother (who died in 1996 at the age of 90) was especially gifted linguistically, being truly trilingual; in addition to Welsh and English she spoke excellent Parisian French. Welsh was also the language of the home and thus Rhys and his sister Non first spoke Welsh and acquired English after starting school. When Rhys was 10 years old his family began to visit Brittany, where they stayed with the Omnes family at Plougrescant near Tregier, on the north coast; this family were then the owners of an hotel. Rhys' mother had been visiting them, with her mother and father, since 1924. This family was also bilingual, speaking both Breton (a Celtic language familiar to some Welsh speakers) and French. Unlike the majority of French people, they were Protestant, not Roman Catholic. Thus the two families had both language and religion in common, and have now maintained their friendship through many decades. It was in this domestic context that Rhys began to learn French. He has certainly inherited his mother's linguistic gift. He now speaks Welsh, English and French and has considerable knowledge of several other European languages. Observing him in different cultural contexts, I have always been struck by his confidence when attempting to communicate in languages other than those he speaks fluently. Local speakers usually respond to this confidence enthusiastically and behave as if he is speaking their language well! He is unafraid to try, does not fear making mistakes, listens to corrections with eagerness — and appears always to derive great pleasure and satisfaction from these linguistic challenges.

Because of the nature of his father's work, Rhys' family moved many times before his father died. Afterwards they settled permanently in Cardiff. One childhood move was in 1945 from Llangefni to Usk, where Rhys attended infants' school (Fig. 5). It was here that he learned English from an age-mate called Roger Donovan who lived next door and from Ivy who helped in the house. The family moved again in 1947 to the seaside town of Barmouth in Merionethshire. Both Rhys and his sister Non attended school there.

Figure 5. 1946. Rhys (front, third from left) at Usk Council School, Wales.

In 1949, the family moved once again, this time to Blaenau Ffestiniog in north Wales. This was Rhys' father's ancestral town, where he had many relatives. Some of GM's older brothers still worked in the local slate quarries, which towered over the town like great grey fortresses. By now, Rhys' father had become an Inspector of Schools. In Blaenau Ffestiniog the family lived in a large four-storeyed Victorian house called 'Bryn Offeren' or Hill of Mass. Both Rhys and Non attended Ysgol Sir Ffestiniog (Ffestiniog County School). Eventually Rhys was enrolled at the Maen Offeren State Quarry School for Boys (Fig. 6). Being a modern woman, his mother wished him to wear sensible 'shoes' to school, but Rhys would not agree to this, no matter how well-intentioned her desire was. He insisted on having '*sgidja* – heavy boots with metal protectors on toes and

Figure 6. 1951. Rhys in Blaenau Ffestiniog, north Wales wearing his Festival of Britain badge.

heels. All the other boys in the school wore these. Apparently, you got beaten up if you wore 'shoes'. At that time, Rhys' father's sister, Essie, owned the Cambrian Boot Store in the main street of Blaenau Ffestiniog. While Essie is now deceased, her shop has survived.

Rhys loved Blaenau Ffestiniog and his home, 'Bryn Offeren'. Most of the childhood stories I have heard from him are located in one of the many private places inside this large house, or in well-hidden spots in the extensive and overgrown garden, which was full of exotic plants such as chestnuts, Californian sequoia, New Zealand kauri and rhododendrons. Rhys' memories of this time in his life are happy ones — a wonderful house full of books and interesting nooks and crannies where he and his sister played games of great fantasy about which they continue to reminisce. Rhys also played war games in the garden, building earthworks and deploying his collection of toy soldiers both ancient and modern. At the same time, with his parents, he began to visit megalithic and historic sites (Fig. 7). As a result of these visits he constructed full-scale models of a Neolithic turf house and a pit house in the garden. The book which stimulated Rhys' initial interest in palaeolithic archaeology was C.W. Airne's *The Story of Prehistoric and Roman Britain Told in Pictures* (1949). The antiquities illustrated in this book came from the British Museum (Fig. 8). Whilst at 'Bryn Offeren' Rhys bartered 'conkers' with his school mates. He harvested these fruits from the horse chestnut trees growing in the garden. As a consequence of this trade, his school mates called his house 'Plas Conkers' (Place of Conkers). Of all the places Rhys has lived in or visited in Wales, Blaenau Ffestiniog is the one to which he is most attached. It is his 'home'. He was not born there, but it is this great, grey, cold and wet place that is imprinted deeply in his memory. Both his mother and his father, and many of his father's family are buried there in a hillside cemetery full of austere grey slate headstones which look out over a wall of rugged peaks clothed, almost always, in mist.

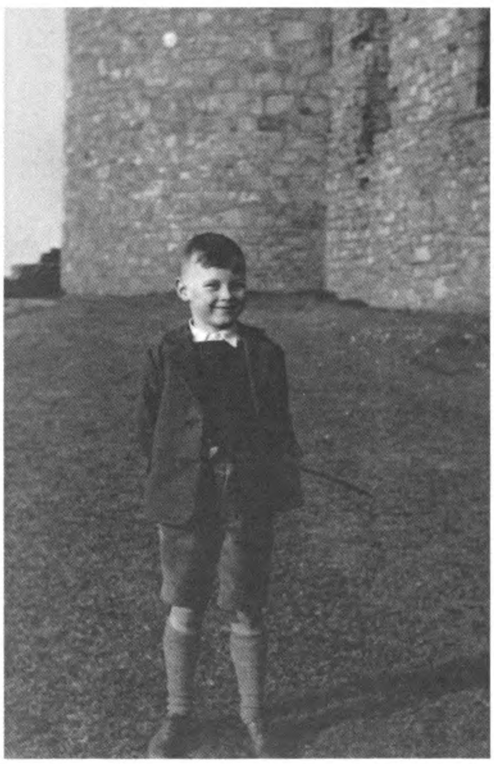

Figure 7. 1949. Rhys during his first visit to Harlech Castle, Merionethshire.

Teenage years

Rhys' idyllic childhood came to a tragic end when his father died in 1954. Grief-stricken and with limited financial backing, his mother decided to leave Blaenau Ffestiniog and move to Cardiff in south Wales where she could be near her own family. She bought a house in Radyr, a suburb of Cardiff - 'Y Gilfach', 78 Heol Isaf (Lower Road). Rhys was enrolled at Whitchurch Grammar School and his mother began teaching French again. When she had married Griffith Maengywn Civil Service regulations required that she resign from her teaching position, but after his death she was permitted to resume her profession again. Indeed, she needed the income to support her children, both of whom were still at school. At school, Rhys and his two cousins, Rhodri (now the Labour leader in the newly-formed Welsh Assembly) and Prys Morgan, 'spoke Welsh in a sea of English' (Fig. 9). Rhys studied physics, mathematics and chemistry for his A Levels. Here too, his love of rugby union began. He played prop forward in the school's first fifteen. They played many teams in competition — tough teams from the Catholic school of St Illtyds, the 'valley' teams from Pontypridd and Caerffili as well as teams from 'more toney' schools such as Cardiff High. While he was throwing himself into the game of rugby, he was also captain of the school's chess team (Fig. 10)!

He began also to roam the countryside on his bicycle. His strong and shapely legs are a legacy of this activity. He had already developed an interest in history but archaeology was his passion. To this day, he maintains a special interest in castles and possesses an encyclopaedic knowledge of them. Another book which he remembers reading with relish at this time was *Great Explorers* (1902) which contained a chapter on early European explorations in Australia as well as in other parts of the world.

CROMAGNARD CAVE DWELLERS AND HUNTERS. The Cromagnards, who are so named from Cromagnon, France, an important centre of their culture, were one of the most advanced of the Palaeolithic races. Their remains are abundant throughout Western Europe and, in Britain, in the Paviland Cave, Wales.

Figure 8. Illustration of 'Cromagnard cave dwellers and hunters' from C.W. Aire's *The Story of Prehistoric and Roman Britain Told in Pictures* (1949:7). This book and particularly this image stimulated Rhys interest in Palaeolithic archaeology.

In 1958 Rhys met Dr H. Savory, Keeper of Archaeology at the National Museum of Wales in Cardiff, who was an expert in the Spanish Bronze Age. Under Dr Savory's supervision, he excavated a Bronze Age barrow called St Y Nyll, which is located on a farm a few miles from Cardiff. This has been dated to 1500 BC. His excellent report (Fig. 11), beautifully handwritten (his colleagues would say that this is a skill he now appears to have lost), gained him a Trevelyan Scholarship to Emmanuel College, Cambridge between 1959 and 1963, named for the great English social historian. The qualifications for this scholarship read:

> Candidates will be selected for both personal and intellectual quality. There will be no conventional written examination but the selectors will look for a high level of intellectual attainment and promise in the candidates, as disclosed by their school record and performance, by interview and by the standard of their written report on the project they will be required to undertake. In addition, the selectors will have regard to the candidate's personal qualities and will look for evidence, not only of powers of leadership, but also for independence, enterprise and originality.... Projects may be a purely intellectual study either in a different field from the candidate's principal subjects of specialisation or of an unusual aspect of these subjects or they may take the form of a venture involving travel..., contact with others and observation. (Trevelyan Scholarships 1958)

In hindsight, these criteria could have been designed specifically for Rhys!

In the late 1950s, all students planning to go to Cambridge had to have a good working knowledge of Latin. Rhys' mother's father's sister, 'Auntie' Mary Ann Powell, a Latin scholar of repute, tutored him well, and he passed Latin in his O Levels.

Figure 9. c.1950. Rhys (front right) with his sister (Non) and his two cousins - Rhodri (back left) and Prys Morgan.

In 1959, at the end of his school career, Rhys, then 18 years of age, was a member of the British Schools Exploration Society Expedition to Arctic Swedish Lapland where he carried out studies of katabatic winds off glacier snouts and made meteorological observations on Aurora Borealis phenomena (Fig. 12). The latter research was an official part of the International Geophysical Year and his records went to Cambridge to be used in research programs. This was but the first of a long line of expeditions Rhys has undertaken in his lifetime. They also show no sign of ending and as I write we are preparing to leave for Lake Eyre in South Australia, and planning another expedition to the Kimberley and Lake Eyre towards the end of 2000.

CAMBRIDGE AND EUROPE

For his Tripos Part 1 at Cambridge, Rhys completed Natural Sciences - Physics, Geology and the History and Philosophy of Science. For Tripos Part 2 he read Archaeology (Palaeolithic Archaeology and Quaternary Studies) and Anthropology, or 'Arch. and Anth.' as it was affectionately known by students! Rhys revelled in his time at Cambridge. He was taught by many distinguished scholars such as Graham Clarke (archaeology), Harry Godwin (botany), Richard Hay (geology), Eric Higgs (archaeology), and Charles McBurney (archaeology).

Rhys shared his time at Cambridge with many gifted students who themselves have now become well-known scholars — for example, Barry Cunliffe, Paul Mellars, Nicolas Peterson, Colin Renfrew, Nic Shackleton, Carmel Schrire, Jim Specht and Peter White (Fig. 13). He moved in a lively circle of archaeology students who discussed and criticised the latest ideas and publications with vigour. I experienced a sample of this aggressive and somewhat acerbic mode of 'discussion' when Rhys was re-united with Carmel Schire and Peter White in Sydney in 1963 — and was somewhat intimidated by its apparent ferocity. [A Cambridge legacy inherited by the Prehistory Department, RSPacS, where Schrire, White and Specht all became graduate students in advance of Rhys' appointment there; the Friday afternoon seminars led to the department being widely known as the 'Piranha Pool' - eds.]

Rhys threw himself into the intellectual and social life of Cambridge with all the energy for which he has now become famous. He was a member of the Emmanuel College rugby union team which at the time contained

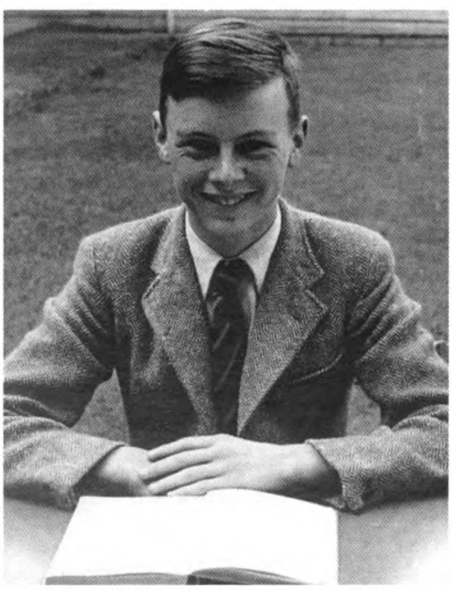

Figure 10. c.1955. School photograph of Rhys whilst at Whitchurch Grammar School, Cardiff, southeast Wales.

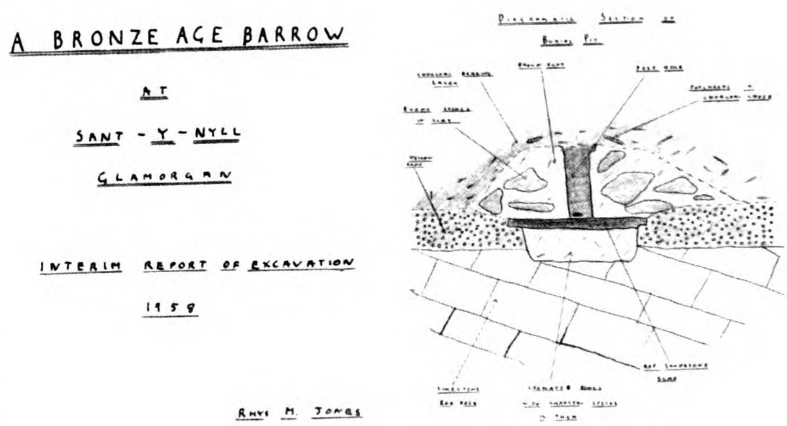

Figure 11. 1958. Front page and diagram of burial pit from *A Bronze age Barrow at Sant-Y-Nyll, Glamorgan. Interim Report of Excavation* prepared by Rhys as part of his application for a Trevelyan Scholarship.

two members, one of whom became the Supreme Commander of NATO; the other the Governor of the Bank of England! He was a member of Heretics, a group whose patron was the famous pacifist philosopher and Nobel Laureate, Bertrand Russell; a member of the Labour Club and the Campaign for Nuclear Disarmament; he also belonged to a private essay club called Mildmay, named after the founder of Emmanuel College who was the Lord Mayor of London in the time of Elizabeth 1 (ca. 1573). This club met in John Harvard's room where members read essays, dined well and were waited upon by College Servants (Fig. 14).

Whilst at Cambridge, Rhys participated in several important excavations at the Mousterian sites of La Cotte de St Brelade, Jersey, in 1962 and Ogof Coygan, south Wales in 1963, both under the direction of Charles McBurney. In 1962 he was the geologist on the Cambridge University Palaeolithic Expedition to northwest Greece under the direction of Eric Higgs. During this first expedition to that area Rhys co-discovered Kokkinopolis, an important Mousterian/early Upper Palaeolithic site. In 1963 he joined the Cambridge University Palaeolithic Expedition to the Caspian Sea and northeast Iran under the direction, once again, of Charles McBurney. It was soon after the latter expedition that he arrived in Australia. From that expedition, he brought with him a beautiful embroidered lambs wool coat which he still owns. This coat came in handy when Frank Gurrmanamana, an old friend and colleague from the Blyth River in Arnhem Land, was visiting Canberra during winter. He became so cold at a workshop being held at Lake Jindabyne in the Snowy Mountains that he thought he was dying. Rhys' coat and some hot water bottles relieved his fear and he went on the enjoy his first alpine experience (Fig. 15).

During university vacations, when he was not participating in excavations, Rhys walked and hitch-hiked his way through much of Europe — usually alone and with a minimum of equipment and money. He had begun mountain climbing, hitch-hiking and cycling, in the Brecon Beacons of central Wales, during high school when he was about 15 years old. In 1960 he travelled to northern Italy as far as Naples and then through Yugoslavia. He walked in the Dinvic Alps (Croatia), on to Bosnia, Austria and then across the Austrian Alps parallel to the high pass near Gross Glockener, through German-speaking south Tyrol (under Italian military occupation at the time) and on to Milan to where he caught a train back to Britain. He also made several trips to France and western Germany. Rhys still owns the khaki-coloured woollen cap he wore on those expeditions and the small spirit stove he carried with him.

In 1962 when Eric Higgs concluded his expedition at Kalcycles in Greece and returned to England, Rhys hitch-hiked and walked through Thrace to Turkey and then to Gallipoli where his father's brother, Ivor, had fought in the First World War. Stimulated by his

Figure 12. 1959. Rhys at his home in Cardiff showing off the gear he wore on the British Schools Exploration Society Expedition to Artic Swedish Lapland to his mother and sister.

Figure 13. Easter 1962 at the Cotte de St Brelade excavation, Jersey, under the direction of Charles McBurney. Rhys Jones and fellow Cambridge student Carmel Schrire, climbing up the scree slope at the entrance to the cave. (Photograph courtesy of Jim Specht)

Auntie Mary Ann Powell's Latin teaching from Virgil's *Aenied* he visited Troy, ending up in Istanbul. From there, he travelled as steerage on a coastal tramp steamer along the Pontic west coast of Turkey to Trabzon near the Soviet border where Xenophon and his soldiers had once again seen the sea. He hitch-hiked across central Turkey on the Anatolian Plateau to see the Hittite capital of Boghazkoy, to Konya, St Paul's 'Iconrum' and south through the mountains to the Cilesian Gates and Tarsus where St Paul was born. On this journey Rhys followed some of the journeys of St Paul in southern Turkey, wild country dotted with Crusader castles, to Alanya. He then took a boat to Rhodes and on to Athens where he measured the width of chariot tracks from classical times on the Athens to Corinth road which, at the time, was being demolished. He collected this information for a colleague at Cambridge who was doing research on historic tracks. He recalls that the width of the track was the same as the current British rail track width (4 feet 8 inches, c.1.42 metres)! From Athens he travelled north to Thessalonika and hitch-hiked on the Orient Express (that is, he had no ticket) to Salzburg and finally north to Britain where he arrived in time to begin his final year at Cambridge.

ARRIVAL IN SYDNEY, AUSTRALIA

After Rhys had successfully completed his degree at Cambridge, he began to look for a job. None were on offer at Cambridge and, even though he probably would have liked to prolong his idyllic undergraduate lifestyle, he applied for jobs in three different continents (north America, Africa and Australia) and decided to take the first one he was offered. Added pressure to leave Cambridge came from his Professor, Graham Clarke, who was encouraging graduates to move to far flung parts of the British Empire to teach archaeology and carry out research on local problems. Already some Cambridge archaeology graduates had taken up positions in the antipodes — for example, Wilfred Shawcross and Peter Gathercole in New Zealand, and Richard Wright and Jack Golson in Australia (Jack arriving via New Zealand) — but Clarke was convinced that the colonies had room for more, especially Australia. Australian scholars, John Mulvaney, Isobel McBryde and Peter White, who had been to Cambridge as postgraduates, also returned to teach and research archaeology in the region.

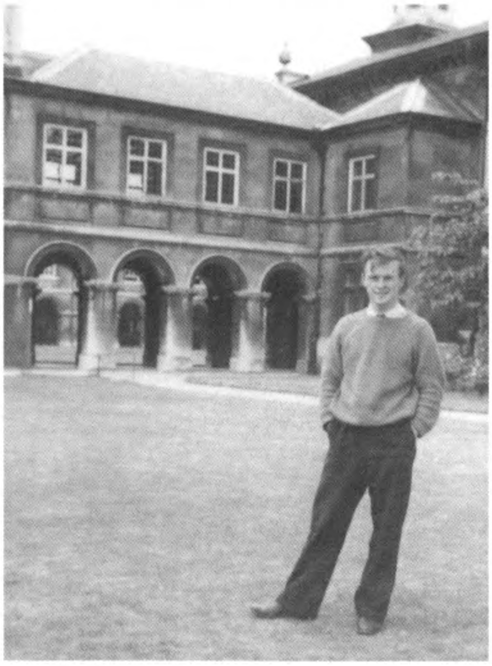

Figure 14. Spring/summer 1960. Rhys in Emmanuel College courtyard, Cambridge.

Figure 15. Full winter late 1970s. Rhys with Frank Gurrmanamana during a workshop-cum-fishing trip at Lake Jindabyne, NSW. Frank is wearing the coat that Rhys bought in Persia after he left Cambridge in 1963 and just before he came to Australia.

In Canada Rhys had applied for the position of Assistant Archaeologist at St Louis castle, designed by Vautin, on the St Lawrence River. This job was to investigate the outer fortifications of the structure which were built by the French in the 17th and early 18th centuries.

In Ghana the upper Volta/Niger River was to be dammed. The job there was to carry out an archaeological survey of the area before this area was flooded.

In Australia, a Teaching Fellowship for one year in the Department of Anthropology, University of Sydney had been advertised. This offer arrived first and Rhys accepted it.

At the age of 22, Rhys flew to Australia as a 'ten pound migrant' (Fig 16). This meant that he had to stay for at least two years. To ensure that he did indeed remain in the country for that time, the Department of Immigration kept his passport.

Figure 16. 1963. Rhys with his sister (Non) at his home in Cardiff just before he left for Australia. He is wearing the coat he purchased in Persia, the same one being worn by Frank in Figure 15.

He was met at Mascot Airport in Sydney in late August 1963 by Richard Wright — an earlier Cambridge graduate who had worked at Olorgesailie in Kenya before taking up a lectureship in the Department of Anthropology at the University of Sydney. There, with encouragement from the Professor, Bill Geddes, Richard had begun teaching prehistory.

According to Rhys, when he got off the plane in Sydney, the day was warm and clear, the light 'bright and brazen' and the aroma of eucalyptus oil was in the air. Apparently, his host, Richard Wright, offered to take him home to sleep off the jet-lag, but he opted instead to visit Vincent Megaw's excavation in the Royal National Park, just south of Sydney.

Within days of his arrival on Australian shores, he made a trip to the Genolan Caves in the Blue Mountains west of Sydney. He had spotted these as limestone on the geological map he had brought with him and thus they had good possibilities for preserving archaeological remains. He did not find any archaeological sites on that trip but thoroughly enjoyed his excursion to the mountains and the spectacular caves.

Rhys' position at the University required him to teach and at the same time enroll for a PhD which involved fieldwork. He shared the teaching with Richard Wright and this very soon extended to all undergraduate as well as to postgraduate years in anthropology. His students included undergraduates from both the social anthropology and prehistory streams — for example, Harry Allen, Annie Bickford, Sandra Bowdler, Emily Coleman, Ian Glover, Annette Hamilton, Ian Hughes, Stewart Hume, Jo Kamminga, Harry Lourandos, Leslie Maynard, Don Miller, Ron Wild and myself. All these people eventually became professionals in archaeology, social anthropology, history, geography, heritage or foreign affairs. I was part of the first intake of four anthropology students who majored in prehistory. The other three who completed their BA Honours theses in 1965 were Ian Glover, Stewart Hume and Leslie Maynard.

At one stage, first year classes in Anthropology contained about 700 students. Lectures usually took place in Wallace Lecture Theatre. Rhys was, from the beginning, a gifted lecturer (Fig. 17) and enjoyed the challenge he faced at each lecture — engaging and keeping the attention of 700 students, quite a few of whom could be high-spirited if not unruly. This was an exciting period for anthropology at the University of Sydney. Students were required to do some prehistory units in first year as well as others in social anthropology, linguistics and physical anthropology (the latter, taught by Richard Wright, was part of prehistory). Many famous scholars were on the teaching staff — for example, Arthur Capel, Les Hiatt, Ian Hogbin, Chandra Jayawardena, Peter Lawrence, Margaret McArthur, Mervyn Meggitt and Michael Swift.

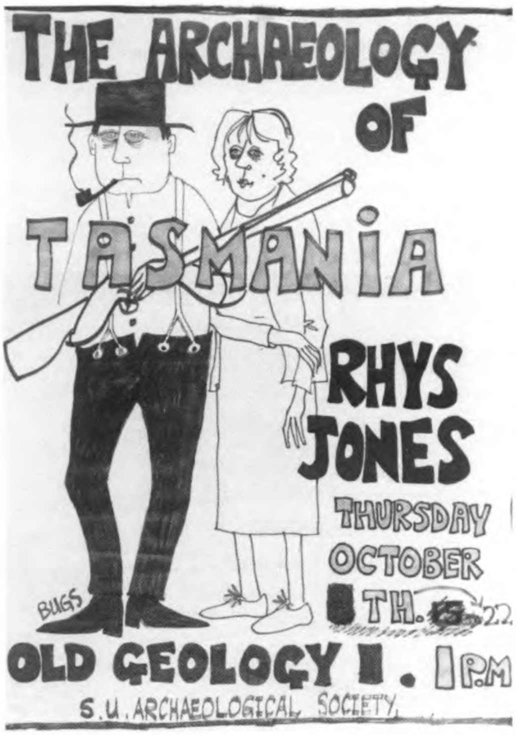

Figure 17. 1960. Poster prepared by Dorothy Bingham for a lecture given by Rhys in the Sir Edgeworth David Lecture Theatre at the University of Sydney.

Also at Sydney University, Judy Birmingham and Vincent Megaw both taught in the then Classical Archaeology Department, where Jim Allen was an undergraduate.

The intellectual life for students of anthropology was stimulating especially for those majoring in prehistory. They were given the opportunity to grapple with interesting new methodologies and ideas from Europe (mainly Britain) and America. Rhys brought with him from Cambridge ideas that were driving research there, especially a focus on economic aspects of archaeological research. Here Eric Higgs was a crucial figure and as Rhys knew Eric well and had been on the Greek expedition with him, he had first hand knowledge of his latest theories. (Richard Wright, being married to Eric Higgs' daughter Sonia, was also very familiar with these ideas.) Rhys taught and stimulated a cohort of students who were later to contribute to a distinctive Australian archaeological tradition — for example, Harry Allen, Jim Allen, Annie Bickford, Sandra Bowdler, Emily Coleman, Ian Glover, Harry Lourandos, Leslie Maynard and Alan Thorne.

During the 1960s, prehistory students had to come to terms with the relationship between anthropology and prehistory. In Australia, this was particularly pertinent since the ancestors of modern Aboriginal people were the first to settle in this continent and this occupation had continued up to the present. It is salutary to recall that in the 1960s, the oldest ^{14}C date for the occupation of Australia was ca. 7000 BP. Nowadays, some scholars (including Rhys) believe that the first people to arrive in Australia did so somewhere between 50,000 BP and 60,000 BP. Despite this extended antiquity, the anthropology-prehistory nexus has remained an important intellectual issue since the 1960s.

Rhys began fieldwork for his PhD in Tasmania during the antipodean summer of 1963–64 (Fig. 18). He took a seedy-looking bunch of males on his first expedition and received quite a bit of flack for not including any women! His team consisted of Jim Allen, Campbell Macknight, Ian Glover, Bob Reece and Ron Wild. With a grant from the Australian Institute of Aboriginal Studies (see Lambert, this volume) they carried out a reconnaissance of Tasmanian sites, excavating Sisters Creek, the Bay of Fires, Roaring Beach and discovering and carrying out preliminary excavation at the great West Point midden. On one occasion on that trip the team was directed to a so-called large and important midden by a local museum director. This 'midden' turned out to be the dump from an adjacent oyster processing factory!

Jim Allen tells a wonderful story about Christmas Day in 1963. Having completed the Sisters Creek and Bay of Fires excavations the team were staying with a University of Tasmania academic and his wife in a farmhouse at Glenorchy outside Hobart, once the home of famous Tasmanian bushranger, Martin Cash. Christmas morning was a bustle of activity in the kitchen, the wife preparing chooks and ducks, the husband bringing to light treasures from the cellar. Rhys got up late and abruptly

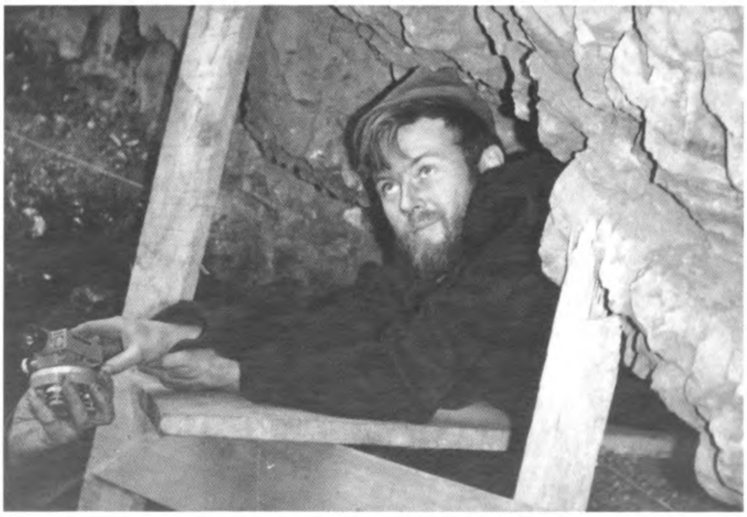

Figure 18. 1967. Rhys being handed a theodolite in the cave at Rocky Cape.

announced that the expedition was 'losing momentum' and that they were leaving immediately, to the despair of the hosts and the dark rebellion of the crew. The day-long drive to Queenstown was very quiet, Rhys' attempts at conversation falling on unresponsive ears. At Queenstown Rhys decided to retrieve the situation by taking the crew into a hotel for dinner and accommodation for the night. It was drizzling rain, getting dark, the main street cold and deserted, but the bar was buzzing. As they walked in, the place went quiet and they were told in no uncertain terms that the party was private and there was no food or accommodation to be had there or anywhere else in Queenstown. Just on dark they broke into a tin hut on the Queenstown-Zeehan road. The roof leaked, it took ages to find firewood in the surrounding ti-tree scrub in the dark, and longer to get it to burn. Christmas dinner was tinned camp pie and a Christmas cake made by Jim Allen's mother.

The corollary to this story came in 1982 when Rhys and Jim were working in southwest Tasmania along the Franklin River. One evening Rhys recalled that journey to Queenstown and how on one high pass they had glimpsed the western valleys of Tasmania wending their way south. Rhys had asked Bob Reece to stop the vehicle so that he could look for limestone outcrops with the binoculars, but Bob had just kept driving. 'You know,' Rhys reminisced, 'if you bastards had stopped that day we might have discovered the Pleistocene here 20 years ago and changed the face of Tasmanian archaeology.' Jim Allen observes that had Rhys got out of the vehicle that day he would have had a long walk to Queenstown.

Vincent Street, Balmain

Having arrived in Australia, Rhys at first lived with Richard Wright and his wife Sonia in Five Dock, an inner suburb of Sydney. A short time later he began to share a house with Ian Glover and me. This pretty white, double-storeyed terrace house whose verandahs were decorated with delicate wrought iron, was located at 25 Vincent Street, Balmain, then a working class harbourside suburb which overlooked Pyrmont docks and the city of Sydney. Old, gnarled Moreton Bay fig trees framed the northeastern side of our view. Much to our chagrin these were cut down a few years after we took up residence to make way for a development. Rhys and I still own a sculpture made for us by John Clegg (artist and archaeologist and one of Rhys' Sydney University colleagues) from one of the logs.

'Vincent Street', as it came to be known, was a meeting-place for friends and colleagues. Many luncheons and dinner parties were held there where current archaeological and anthropological issues were discussed vigourously. Large parties of up to 200 people — not always all invited guests — took place occasionally. At these events the house reverberated with music of the day — the Stones, the Beatles, Joan Baez and Bob Dylan.

One of the highlights of social life during the late 1960s in Balmain, was what came to be known as 'Happenings'. These usually took place in December after term at the University had finished but before people left for Christmas holidays. Each of these was organised around a theme — for example, 'La Belle Epoque'. The Happenings began in the late afternoon and usually continued into the early hours of the next morning. Food, costume, house decorations and entertainment (poetry, music, drama, prose reading) were, as far as possible, consistent with the theme. Everyone who attended - usually about 50 people - were encouraged to come in appropriate costume and were required to contribute in some way - preparing or serving food and/or participating in the entertainment. Rhys was always an enthusiastic participant in these events. While he did not possess any spectacular culinary skills, he had an excellent tenor voice and was a talented actor. He enjoyed wearing fancy dress and always managed to stay in character throughout the event (Fig. 19). The Happenings all took place in the home of Richard and Sonia Wright who by then had moved from Five Dock to Balmain, where they lived in a large and elegant home on the waterfront in Campbell Street — a perfect venue for these events.

Figure 19. c.1967. Rhys with Betty Meehan at the 'La Belle Epoch' Happening held at the home of Richard and Sonia Wright in Balmain. Both costumes were based on a Beardsley's drawing – Betty's on 'The Peacock Skirt' (Plate 3); Rhys' on 'The New Star' (Plate 57) depicted in R.A. Walker (ed.) 1948 *The Best of Beardsley*. London: Spring Books.

Whilst in Sydney, Rhys met the Andersonians, a group of intellectuals named after the Professor of Philosophy from Edinburgh who had joined the University of Sydney in the 1930s, and the Libertarians (Coombs 1996) who frequented the University but concentrated their social life in pubs in the inner city — for example, the Tudor, the United States, the Gresham, the Newcastle and the Royal George. He became especially friendly with the social anthropologist, Les Hiatt, who was not only one of the Libertarian intellectuals but also taught in the same department as Rhys at the University of Sydney. At one stage during his time in Sydney, Rhys shared a house with Les 'Bon Sejour' in Bellevue Hill. Their friendship has endured to the present day. They share, among many things, a love of good red wine, a healthy scepticism and a dislike of political correctness.

TASMANIA

Whilst studying at the University of Sydney I became increasingly interested in anthropology and the way in which it could be used to enrich the study of recent archaeology. My BA Honours thesis, done in 1965, examined literature about Tasmanian Aborigines at the time of European contact and compared that with the conclusions drawn from archaeology that had been carried out there — which, at that time, was not much. This meant that Rhys and I had intellectual interests in common because we were both investigating the history of Tasmanian Aborigines. While Rhys was interested in the deep past, I used published and archival accounts from the time of first European contact to build up a picture of Aboriginal life at this time (Hiatt 1967/8).

After I completed my BA honours degree, Rhys and I shared various houses in Balmain. It was possible then to obtain charming Victorian houses for very moderate rents. Rhys continued to teach at the University, I began research for an MA Honours thesis on Aboriginal mortuary practices, which I completed in 1971.

In 1965 Rhys made a highly significant appearance at the ANZAAS (Figs 20 and 21) in Hobart where he addressed the Plenary session on Tasmanian Aboriginal prehistory. During that session he was engaged by Norman Tindale, whose work had stimulated him to carry out research in Tasmania in the first place. While Rhys was challenging some of Tindale's findings, they were able to have productive discussions and consequently became firm friends and colleagues until the latter's death a few years ago. Subsequently, Tindale asked Rhys to contribute a large Appendix, 'Tasmanian Tribes' for his monumental work *Aboriginal Tribes of Australia* (Jones 1974).

Rhys' work in Tasmania continued until 1990 but because the Tasmanian Aborigines in that State have increasingly placed stringent restrictions on archaeological work there he has turned his attention to other areas on the mainland, where archaeologists are more welcome. He has not willingly let go of his passionate intellectual interests in Tasmania — after all he carried out research on that island for nearly 30 years. He has a lot of unfinished work to do there. However, he believes that the current political context is far too difficult and negative to allow research to be carried out successfully.

OLD SOUTH WALES IN NEW SOUTH WALES

In 1966 Rhys' mother, Enid, joined the cruise ship *Marconi*, calling at Hong Kong on her way to Sydney. She stayed with us in Vincent Street until we all left to join Jim Allen's excavation at Port Essington in the

Northern Territory. We drove to Darwin via Port Augusta and Alice Springs with Alan Thorne. At Port Augusta the car was loaded onto a train, the Ghan, to travel to Alice Springs. During our drive to Darwin we encountered heavy rain with extensive flooding and extremely cold conditions. We were held up at Sterling Creek with many other travellers for several days. Enid was amazed that the so-called dry, hot heart of Australia could be so cold and wet. From Darwin we flew in a small plane to Smith Point and thence by motor boat to Jim's campsite. Once there Enid worked energetically on the excavations and she and I together also did most of the cooking (Fig. 22). For someone who had never been in the bush anywhere very much during her lifetime — certainly not in the northern Australian bush with its flies, mosquitoes, crocodiles, ants and snakes — she coped extremely well and Rhys was extremely proud of her. One hot, steamy day, Enid inadvertently disturbed a nest of the sweet-tasting but vicious green ants which inhabit the trees and shrubs of the north. Confronted with dozens of biting ants inside her shirt, she shed her clothing rapidly, Welsh Baptist modesty the last thing on her mind. On another occasion on a deserted beach, she was confronted by a long-haired, bearded man wearing only skimpy nylon underpants and elastic sided boots, unusual even by Territory standards. He had jumped ship in Darwin some years earlier and when he spoke Enid detected a Welsh accent. A 20 minute conversation in Welsh ensued. Later she remarked that he was 'a lovely man'.

ESTABLISHED 1890

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WESTERN & WESTERN TASMANIA

ONDAY, MAY 22, 1967 5c

CAVE INHABITED ABOUT 8000 YEARS AGO

Expert claims

A cave which was discovered early this year at Rocky Cape has been found to have been occupied by aborigines about 8000 years ago.

The cave was examined by Mr. Rhys Jones, senior tutor in anthropology at the University of Sydney. He said the cave was dwelt in more than 8000 years ago.

Mr. Jones said that from what had already been assessed, the cave was perhaps the best find yet in the discovery of the aborigines' way of life before the white man came to Tasmania.

"The most important fact of the discovery is that the cave has been undisturbed and we can reconstruct, through plans and photographs, the cave as it was," Mr. Jones said.

Undisturbed

rab states ont Israel

euter) — Twelve Arab states have nnounced that they will stand to-

Figure 20. 1967. Article from *The Advocate* reporting Rhys' Rocky Cape excavation and the discovery of occupation dating to 8,000 BP.

Figure 21. End January 1965. Rhys' team at West Point, northwestern Tasmania. From left: Rhys, Harry Lourandos, Annie Bickford, Bill Rodman, Alan Thorne, Dorothy Bingham.

Once Rhys took up his position at the University of Sydney, he began his PhD fieldwork in Tasmania. Delivering papers at seminars and conferences, he also felt the need to discuss his work in the Welsh language. Up until then he had spoken Welsh for everyday aspects of his life but had used English for philosophical and intellectual subjects. His written Welsh was fine. He published his first archaeological article in Welsh (1965) with some help from Alice Powell (a Welsh-speaking Latin scholar married to Rhys' uncle Dewi) who had been an archaeological officer at the National Museum of Wales. But when he visited Wales in 1968, he had some difficulty carrying on professional discussions in Welsh. He was disturbed by this and henceforth made a great effort to upgrade his academic vocabulary and verbal expression. He was successful in this effort and is now fluent in academic Welsh. Rhys' improved fluency has parallelled an increase in the number of Welsh speakers in Wales, the result of the establishment of Welsh schools and the development of the Welsh language itself to facilitate intellectual communication.

Figure 22. July-August 1966. Rhys' mother, Enid Watkin Jones (left), Jim Allen and Rhys on Jim Allen's archaeological expedition to Port Essington, Northern Territory.

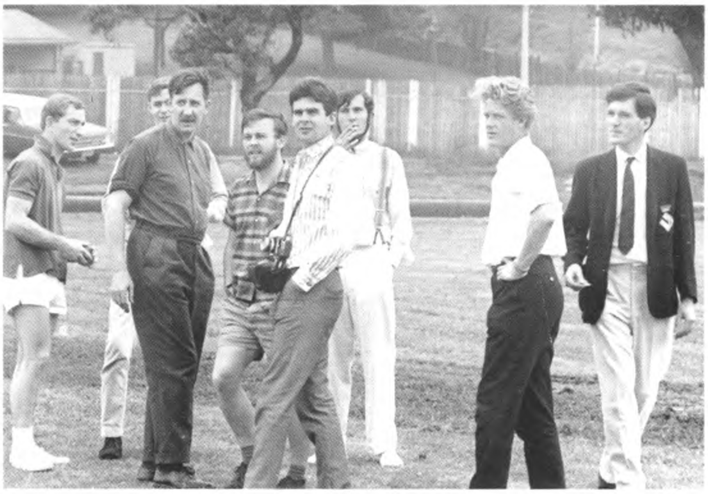

Figure 23. 1967. Cricket match, Sydney Anthropology versus ANU Anthropology held at Glebe Park in Sydney. Left to right: John Byrne, John ?, Harry Oxley, Rhys Jones, Nicolas Peterson, Harry Allen, Ken Maddock and Richard Wright.

Figure 24. 1997. Rhys at his home near Hoskinstown, NSW with his much-loved 1974 Mazda utility known affectionately as 'Hal'. Rhys' study (two large farm sheds) is in the background.

MOVE TO THE AUSTRALIAN NATIONAL UNIVERSITY

Disappointed at being overlooked for a lectureship at the University of Sydney, after having put so much energy into teaching, Rhys applied for and got a Research Fellowship in the Department of Anthropology and Sociology (later Prehistory) at the Australian National University (Fig. 23). He became a Fellow in 1972, a Senior Fellow in 1975 and was awarded a Personal Chair in 1993.

Soon after we arrived in Canberra, Rhys had his first television interview. It was on the occasion of the investiture of Prince Charles as the Prince of Wales in mid-July 1969. He was interviewed on the ABC program, 'This Day Tonight', as a Welshman, about this event. We had no television at the time, but hired one for the night so that we could assess his performance. At the time, he was inexperienced in media performance and did not realise that his interview might be heavily edited. In addition, with his reddish beard, navy blue shirt and his passionately expressed views about the investiture, he appeared a little like the caricature of a Celtic revolutionary. However, he learnt a great deal that night about the techniques used by the media and very soon developed a sophisticated, though always enthusiastic, style in dealing with interviews. Since then he has become extremely relaxed and skilled in all forms of media - radio, newsprint, television and film.

When we moved to Canberra in 1969 for Rhys to take up his position at ANU, we were both completing postgraduate theses — Rhys his PhD, me my MA. We lived in a University semi-detached house in Hughes until 1972. In this place Rhys finished his PhD about 2.00 am one morning and I completed my MA Honours about four hours later! We had the necessary copies made, jumped into our green Volkswagon and drove to Sydney. At Sydney University we took our volumes, by now quite a load, to the office where they were to be deposited. As can be imagined, we were feeling pretty pleased with ourselves having finally completed years of work (seven for both of us). The receptionist obviously did not share our pleasure. She barely looked up when we arrived with our volumes and told us to take them into 'that room'. When we walked into 'that room' we understood why. We were confronted by hundreds of other peoples' theses — lined up on floor to ceiling shelves waiting to be marked! Our elation was a little dampened by this deflating experience, but as we were leaving the following day to spend three months in Wales, this did not last long.

In 1973, we bought a small house on 40 acres of land ('Ty'r Paith' or House of the Plain) in the Molonglo Valley near Hoskinstown in New South Wales, where we have lived and worked ever since (Fig. 24).

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Memories of Rhys at Lalarr Gu-jirrapa, 1973

Betty Ngurraba Ngurraba
An-barra Community
C/- Bawinanga Aboriginal Corporation
Maningrida NT 0822

What follows is a set of reminiscences concerning the period in the first half of 1973 when Betty Meehan and Rhys Jones were living with the An-barra and other Gu-jingarliya (Gidjingali, Burarra) communities at the coastal home base of Lalarr Gu-jirrapa which is located at the mouth of the Blyth River in central Arnhem Land. Betty Ngurraba Ngurraba was then about ten years old (Meehan 1982:106).

Alay Rhys. Gun-guna janguny gun-nginyipa. (To Kim: Nipa marn.gi).
Hello, Rhys. Here is your story. (To Kim: He'll remember).

Wola yorr gu-be-na barra a-garlma-na.
Many years ago the rains came and the northwest monsoon arrived.

Nginyipa gochilawa gun-gapa Lalarr Gu-jirrapa rrapa ngayburrpa gochilawa.
You were camped on the beach there at Lalarr-Gujirrapa, and so were we.

Gun-guna ngguluwa, gun-guna nggula janguny nginyipa marn.gi.
This is for you, a story about things you remember.

An-jarranga jichicha rrapa barrwa minypa diyama an-jiwirrgiya gun-gata gochilawa.
We got lots of fish off that beach, and cockles and mussels too.

Rrapa gu-gata lika shop gu-na-ga-nja tucker-run a-ni plane gu-guyinda sea-plane.
And then they brought in supplies by sea-plane from the store.

Nginyipa marn.gi.
You remember.

Rrapa nginyipa marn.gi minypa jichicha m-bo-na gapala anagotipa minypa Frank nginyipa marn.gi old lady Nancy nyiburr-bo-na.
And remember when you and Frank and Nancy went on that fishing boat?

Jichicha nguburr-bama-na rrapa ngaypa minypa ngudelipa minypa eleven or ten years old.
I was a child, about ten or eleven years old, and we all went fishing together.

Nguburr-bo-na fishing nguburr-ni jichicha nguburr-bama-na.

We all went off to get fish.

Nginyipa marn.gi gochilawa Lalarr Gu-jirrapa jaranga an-gata an-baykarda rrapa minypa nginyipa marn.gi nguburr-jaranga nguburr-ni minypa from-other-side people rrapa ngayburrpa gata nguburr-ninya.

You remember the time when we were camped on that long beach at Lalarr-Gujirrapa and people from the other side of the river came over and camped with us?

Nginyipa marn.gi gun-gata rrawa Lalarr Gu-jirrapa rrapa minypa jin-gata Betty Meehan nipa marn.gi rrapa ngayburrpa nyiburr-ni nyiburr-jaranga nguburr-bama-na an-gata a-ni burrguta jarrka rrapa barrwa minypa ana-niya an-bambula rrapa barrwa minypa barnda burrguta an-jaranga nguburr-bama-na.

And how you and Betty Meehan and all of us went hunting for goannas, mussels, tortoises, and so on, and got lots of them?

Nyinyipa marn.gi gun-gata cyclone-time gu-be-na wana yorr nginyipa mu-jalanggaka-ja ngguluwa tent and n-jonyja-nga rrapa help bubu-nega-rra?

And you remember the time when that cyclone and heavy rain knocked down your tent, and you called out and we came and helped you?

Put up nyibu-nega-rra nggula tent rrapa barrwa minypa steel a-jalanggaka-ja.

We put up the tent for you, then the frame blew down.

Gun-narda nggula janguny. Lika minypa nginyipa barrwa barra nyi-ni wola gun-guyinda Lalarr-Gujirrapa nguburr-nyi.

So there's the story for you. Afterwards you and all of us stayed for a long time at Lalarr-Gujirrapa.

Nginyipa marn.gi. Ganapiya.

You remember. That's all.

Acknowledgements

These reminiscences were recorded by Kim McKenzie at Maningrida during 2000 and translated from Gu-jingarliya into English by Lester Hiatt.

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Peopled Landscapes: From Prehistoric Tasmania to Contemporary Arnhem Land

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UNLIKE PRESENT stressful and time-consuming requirements for research grant application, when Rhys Jones, B.A. arrived in Sydney in 1963, an assured Australian Institute of Aboriginal Studies grant awaited his brief formal application; the award exceeded his request; an entire State was his given research oyster. His enthusiastic team was in the field by year's end, including three students — Jim Allen, Ian Glover and Campbell Macknight — destined to be his Australian National University departmental colleagues. Previous unsanctioned, destructive activities directed Jones to sites in northwestern Tasmania, with the bonus of the location of a promising new site.

While Rhys Jones' objectives reflected traditional Old World Palaeolithic anticipation of stratigraphic excavation and artefact typology, his expectations already were broader (Jones 1965:191):

> My particular interest was to try and isolate total industries for the purpose of setting up definitions which might be compared with mainland sites, and also to investigate any ecological or geographical variations and adaptions [sic] within the island'.

During a remarkably productive season his team made systematic excavations at Sister's Creek shelter and West Point midden, surveyed further northwestern sites and probed others on the east coast, including a stone arrangement (Jones 1964). During his second field season Jones concentrated upon the northwest, adding the two Rocky Cape caves to his excavation tally.

Across a decade, with ramifications still persisting, his discoveries and imaginative explanations were debated hotly by Australia's burgeoning archaeological community. These included his biological insights, such as reasons why Tasmanians ceased consuming fish and why elephant seals became extinct; his stratigraphic, typological and chronological evidence together with sea-level inferences; and the cultural implications of the adoption of dogs into terminal indigenous society (Jones 1970). To apply his own favoured term, Rhys Jones was at'the cutting edge' of research.

The Australian education of young Rhys led him naturally from Tasmania's past to Arnhem Land's present, in an intellectual adventure embracing prehistory and anthropology, history and ethnology, ecology and several other sciences, a seamless interdisciplinary web unequalled by any other researcher in application and exposition.

Context matters, and fortunately for Jones, he arrived in the right place at the appropriate time, and he enthusiastically grasped his opportunities. While his Cambridge environmental archaeological training equipped him admirably, his standing was facilitated by a congenial Welsh fervour as he secured mates across a wide spectrum from scientists to 'bushies'. Tasmania was in intellectual ferment around the time of his intervention, climaxing with the 1965 Hobart ANZAAS Congress, where Jones first presented his ideas. (For his own version of the significance of this event, see Jones and Meehan 2000).

The Congress handout of the *Atlas of Tasmania* (Davies 1965) played a key role. Its excellent maps and cogent discussions included W.D. Jackson on vegetation. He highlighted the ecological consequences of Aboriginal landscape firing (elaborated in a lecture). At the same time he fired Jones' fertile imagination. Norman Tindale, who attended the Congress, was an early exponent of the consequences of Aboriginal burning (Tindale 1959). Although Jones and Tindale were generations apart, literally and metaphorically, Tindale was impressed by Jones and invited him to contribute an analysis of Tasmanian 'tribes' and demography to his important *Aboriginal Tribes of Australia* (Tindale 1974).

N.J.B. Plomley's magisterial edition of G.A. Robinson's journals was soon to appear (Plomley 1966) and the unpublished text was made available to Betty Meehan and Jones while at the Congress. Meehan used it extensively for her Sydney University honours thesis upon Tasmanian diet and material culture (published as Hiatt 1967–68). Her research also involved exhaustive combing through published journals of maritime exploration. Consequently she directed Jones to the varied sources and he immersed himself in mastering them.

The breadth of his historical reading may be gauged by comparing the few historical references in his otherwise masterly archaeological survey (Jones 1966) with his study of Tasmanian demography delivered late in 1968 (Jones 1971), or his chapter in Tindale's *Tribes* (Jones 1974). Jones was now better informed than anyone on the realities of Tasmania hunter-gatherer existence.

The French maritime voyagers supplied graphic verbal and pictorial vignettes to supplement Robinson's data. French influences took a more philosophical direction for Jones following the republication of J.M. Degérando's *The Observation of Savage People* (Moore 1969), written as a social theorist's advice to the proposed Baudin expedition (1800–1804). Jones' reading of French Enlightenment literature eventually led to his pilgrimage to Le Havre's Muséum d'Histoire Naturelle, repository of much Baudin expedition material. One of his most thoughtful studies resulted, an historical and philosophical Australian Bicentenary contribution to *Baudin in Australian Waters* (Jones 1988; see also Jones 1991; Jones 1992). (Given these interests it is understandable that Jones was one of only four Australian subscribers to the 1980 publication of John Aubrey's *Monumenta Britannica.*)

Knowledge of Enlightenment versions of conditions in the State of Nature, compared with the reaction of explorers actually visiting such a presumed State, provided Jones with intellectual pabulum before he entered Arnhem Land.

In his first synthesis of Tasmanian research, Jones (1966:1) referred to Jackson and other scientists on the ecological consequences of Aboriginal firing regimes. Here is the clue which directed him to innovative research. His classic paper, 'The geographical background to the arrival of man in Australia', soon resulted (published in 1968), his thoughts bridging Bass Strait to tackle continental issues. From interest in fire to 'fire-stick farming' (Jones 1969) he soon initiated an internationally accepted term, surely a remarkable feat for a popular piece.

It surely was time for Jones to experience Aboriginal firing in practice. He came near in 1966, when he visited Port Essington as a member of Jim Allen's archaeological team. However, he only 'saw from the air, smoke from fires extending … scores of miles' (Jones 1968:206).

As Jones synthesised disciplines during the 1960s, a further intellectual influence impinged. This was the celebrated Chicago *Man the Hunter* conference (Lee and De Vore 1968), which focussed attention upon contemporary hunter-fisher-collector societies. Neither the technological nor economic life of Aboriginal Australians figured prominently (neither did female activities), but its influence was pervading. Besides, Jones and Meehan already were swayed by a Sydney colleague, Mervyn Meggitt (e.g. 1964), who emphasised the economic and dietary data awaiting field documentation, especially the food gathering role of women.

Archaeologists became persuaded that Australia offered opportunities for 'ethnoarchaeological' studies. In late 1966, Richard A Gould, an American, commenced his 'living archaeology' research in the Western Desert (Gould 1978, 1980), followed by Brian Hayden (1977) during 1971–72. Nicolas Peterson, an anthropologist and colleague of Jones, who worked in Arnhem Land during the late 1960s, grasped the potential for research on economy, ecology and material culture for providing archaeological models and analogies (Peterson 1968, 1973; White and Peterson 1969).

In retrospect, the 1960s were the critical decade in Australian archaeology, in which Rhys Jones played a central role, both in Tasmania and in research and speculation into 'the deep past' from 1969, when he was a participant in the Lake Mungo discoveries. His involvement in 'living archaeology' and firing regimes followed in 1972–73, when Jones and Meehan lived in coastal Arnhem Land with Anbarra and Gidjingali people. Jones revelled in the experience and found illuminating insights, parallels and analogies with which to interpret Tasmanian prehistory and beyond. It is evident that his Tasmanian experience pre-adapted him to comprehend and interpret this functioning hunter-gatherer society, together with its use of fire as a tool.

The first indication of his mature comprehension of such societies, past and present, came in his 1973 paper in which he compared Australian and New Zealand indigenous societies and their related ecologies. Its global implications for his perspective were evident (Jones 1975). Consider the extent to which this fusing of archaeology and history with contemporary societies has stamped his later work as unique within Australia. One insightful example is 'Landscapes of the mind: Aboriginal perspectives of the natural world' (Jones 1991); 'Aboriginal conceptions of the workings of nature' is another (Hiatt and Jones 1988).

Jones first witnessed the superb rock art of Kakadu in 1972, around the time when Harry Allen and Jo Kamminga extended the range of Pleistocene occupation sites and the antiquity of ochre use there. Then, in 1974, the ochre-impregnated Pleistocene Mungo burial raised in dramatic manner the antiquity of aesthetic and cultural sensitivity. Since that time, in Kakadu and elsewhere, Jones has combined with dating specialists to apply the latest techniques to establish the chronology of both the first colonisation and of rock art (Jones 1985, 1999).

These interests have Tasmanian application. Unfortunately Jones' desire to return to Tasmanian fieldwork was curtailed by local Aboriginal opinion and politics. Sadly, their invective was directed against the person who did more than any other non-indigenous person to demonstrate the antiquity, cultural significance and humanity of their ancestors.

He was there to fight the cause against the Gordon-Franklin dam, however, even to the High Court (Jones 1982). Indeed, his association with excavating and dating Kutikina Cave meant that his research had turned full circle. From his youth in Tasmania to maturity in the contemporary Top End, he used his experience of both shared camp life and mainland Pleistocene archaeology to infuse spirit into the culture and ecology of Tasmania's remote past. The encompassing vision of this Welsh-Australian nationalist enabled Jones to proclaim the global importance of the peopling of Tasmania (Jones 1992:755). It is a philosophy which regrettably runs counter to much current indigenous belief but correctly he remains steadfast: 'Here people established themselves with a regional economic system … . When I first entered Kutikina Cave under its dense rainforest cover … I was strongly reminded of caves of a similar ambience and antiquity … in … the Gower Peninsula … Viewed from the perspective of the Pleistocene archaeologist, there is a fundamental unity in the history of humankind'. Jones is not afraid to express opinions which fail the test of political correctness. Future generations of Australians, both indigenous and immigrant, are likely to commend his fortitude.

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Rhys Jones and the Australian Institute of Aboriginal and Torres Strait Islander Studies

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THE MINUTES of the Interim Council meeting of 18–19 October 1963 of the then Australian Institute of Aboriginal Studies, record that Mr Rhys Jones, a 22 year old PhD student and Teaching Fellow in Prehistory from the University of Sydney, was seeking funding of £400 to carry out a survey of archaeological sites in Tasmania. The application was late, necessitating Council delegation to the Acting Chairman of Council and one other to make a decision based on its subsequent assessment by the Advisory Panel on Prehistory and Material Culture. Mr Jones got his grant, increased on the Panel's recommendation to £500, with the unanimous support of some key figures in the discipline at that time, including N.W.G. Macintosh, Jack Golson, Isabel McBryde and Norman B. Tindale. It was the start of Rhys' long and continuing relationship with the Institute.

In the years subsequent to 1963, Rhys not only continued his Institute-funded reseach in Tasmania but extended it to western New South Wales and into the Northern Territory where he worked extensively with the Anbarra people of central Arnhem Land. In May 1966 he was elected as an Institute Member and he served as an elected member of Council of the Institute from 1978 until 1990. As well, he was a member of numerous committees and specialist panels, including the Strategic Planning Committee, the Research and Membership Committee, the Prehistory Advisory Committee and the Research Advisory Committee, where he distinguished himself by being elected as subject specialist in the area of Prehistory, Sites and Material Culture on each occasion he stood. In the heady 1970s, in particular, it was not uncommon for debate to range well beyond the day or so set aside for a meeting to extend, often over a bottle of red wine, into the early hours of the following morning. Rhys was at the centre of such debate, often taking discussion into unexpected directions and leading the Institute to explore new intellectual territory.

Rhys Jones (left), Betty Meehan, Frank Gurrmanamana and Les Hiatt during a trip to Perisher, Mount Kosciusko National Park NSW. Frank Gurrmanamana had been elected to the Membership of the Australian Institute of Aboriginal Studies and was in Canberra for a meeting (May 1976). From the Meehan Jones Collection.

Rhys' contribution to the intellectual life of the Institute extended well beyond his involvement with committees and panels. In 1976 he was invited to present the Institute's inaugural Wentworth

Lecture, *Calories and Bytes: Towards a History of the Australian Islands*, and he played a key role, together with Les Hiatt, Margaret Clunies Ross and Betty Meehan, in facilitating the Rom ceremony, an Aboriginal ritual of diplomacy, performed by the Anbarra at the Institute in November 1982 and December 1992. The ceremony is a testament to Rhys' commitment to Aboriginal people and to the friendships forged over many years with the Anbarra. It remains a highlight of the Institute's involvement with Aboriginal people.

Betrayal as a Universal Element in the Sundering of Bass Strait

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IN RECENT YEARS, the consequences of repatriation and cultural restitution have given the impression that only the dispossessed can lay claim to pain, and only the indigene to a soul. The ensuing polarisation of archaeologists and native peoples sometimes makes one forget it was not always so: archaeologists have not always been cast as strangers, and their research has often been motivated by a deep affinity and empathy for the people they uncover. In this context, I should like to explore the power of Rhys Jones' work on the Tasmanian holocaust of the last century. Over the past 35 years its reach has extended beyond archaeology into popular fiction (Boyle 1979), film (Annaud and Brach 1982, from the novel by Rosny 1982), Pulitzer Prize-winning science (Wilson 1979: 180–2) and equally prize-winning world history (Diamond 1998:312–3). I suggest that Jones' remarkable ability to propel the tragedy of a small island into the conscience of the wider world emanates not only from the intrinsic appeal of the subject, but also from his particular empathy for the plight of people drowned in an ocean of betrayal.

The former Van Diemeners, who became the Tasmanians, were birthed when the sea rose to create their island fastness. The water barrier that cut them off from Australia became a highway, not for 'Iron Age Europeans' (Diamond 1998:317), but for European mercantile capitalists, who sutured them back again into the wider ambit. Thus the Tasmanians entered Western discourse in the Age of Enlightenment as primitives, whose technological achievements were possibly the simplest ever encountered. With a cultural repetoire of simple wooden spears, clubs, spatulae and digging sticks, pebbles, stone tools, a basket or two, a couple of rudimentary hut forms, a shell necklace and an eminently sinkable canoe, to say nothing of an inability to make fire, and a refusal to eat fish - it was a situation to startle the staunchest Luddite (Jones 1977a:196–99).

In 1963 Jones arrived in Australia fresh from Cambridge. John Mulvaney had recently set the stage for work in Tasmania with his masterly summary of the Australian archaeological state-of-play (Mulvaney 1961). Prehistory was envisaged largely in terms of distinctive tool types, though where Tasmania was concerned, the record was, of necessity, brief. The Tasmanians — and I am speaking here of the island residents themselves up to 1876, not of today's people with links to former residents of the island — were an archaeological unknown, devastated by genocide when invaders became settlers. Preliminary research revealed archaeological sequences at Rocky Cape, but observers had invested the finds with such a stultifying confusion of patination, angles, and striking platforms, that Mulvaney insisted that any comparisons between Tasmania and the mainland were premature, pending systematic excavations (1961:99).

Jones' first response to Mulvaney's cry to arms, was a model of crisp, objective reportage that integrated his test excavations at several key Tasmanian sites with a small series of radiocarbon dates from earlier excavations (Reber 1965; Jones 1966). He highlighted two major events, the coincidence between the earliest occupation of the northwest coast and the arrival of the post-glacial sea at its present shore, and the abrupt cessation of fish consumption and bone tool manufacture ca. 3500–3800 BP. Jones followed this up with a series of papers about the arrival of people in Australia, asking when people arrived, who they were, how they adapted and what impact they made (Jones 1979:447). He theorised about the general impact of people on an uninhabited continent (1968), the implications of the Pleistocene cremation at Mungo (Bowler et al. 1970), and the general state of Australian prehistory (1973). The sea was a crucial element throughout, and Jones integrated geology, eustasy, and climatology with ethnography, travel accounts, and actualistic studies, to speculate about the pace and impact of marine fluctuations on human existence (Jones 1975:1977b).

It is instructive to contextualise his early work with that being done in another southern hemisphere country. In much the same way as in Australia, a population of Cambridge-trained archaeologists converged upon South Africa in the mid-1960s to try and take its prehistory beyond the pioneering sequences into new fields of prehistoric economy and ecology. Jones' early work on prehistoric man-land relations compared well with Later Stone Age studies in South Africa (Deacon and Deacon 1963; Parkington 1972, 1976), and likewise, his later work on world climatic events was well in line with findings at sites like the Klasies River Mouth (Klein 1972, 1976; Singer and Wymer, 1982). All this was comparable to work being done world-wide, the main difference being that, unlike Europe, Australia and South Africa shared a rich prehistoric tradition coupled with a very recent active forager past, encoded in history, travel and modern ethnography. But whereas South Africans used ethnography as a secondary adjunct, Jones and his Australian colleagues incorporated ethnography deeply into their prehistoric research.

The minor role of ethnography in pre-1980s South African prehistory is all the more surprising because the most famous foragers of all lived there. The San, or Bushmen, of the Southern African Kalahari were rendered immensely popular in the early 1960s by a Berkeley graduate student named Richard Lee, who saw them as free spirits, generous sharers of their bounty and much-envied avatars of what we might all have been before capitalism and its ills got us by the throat (Lee 1968). This impression continued in the work of the Harvard-Kalahari Research Group, (Lee and DeVore 1976) who held that the San still managed to retain active aspects of the forager existence despite being relegated to the ranks of the impoverished, dispossessed, and disenfranchised (Guenther 1976). This position stemmed in large part from a peculiarly ahistorical perspective, untouched by critical history or fine-tuned archaeological sequences, and it enabled San ethnographers of that time to ignore the varied adventures of hunters and herders over the past 350 years — as clients, miners, traders, and hot-shot ivory hunters — as well as the murderous, bloody past of the exterminated San tribes, so dramatically rendered by later authors (Gordon 1992; Marks 1972; Schrire 1980a, 1984; Skotnes 1996; Wilmsen 1989).

With this take in mind, it is not surprising that South African archaeologists of the 1970s were more concerned with major sequences from sites like Klasies River Mouth, Nelson Bay Cave, and Boomplaas, than with the Bushmen themselves (Deacon and Deacon 1999). They did not ignore ethnography, but they used it in a strictly apolitical way, as a supplement to the archaeological data (Deacon and Deacon 1963; Parkington 1972). Prehistoric humans were posited as responsive to climatic change rather than as agents of their own habitat modification through predation and the use of fire. When I first noted this difference 20 years ago, with regard to the way the two countries viewed the causes of Pleistocene extinctions, I concluded that the South African prehistoric record was so rich that it needed no amplification from modern ethnography (Schrire 1980b). In point of fact, I spoke too soon: the interpretation of South African prehistory was about to dive deeply into ethnographic waters with Lewis Binford's famously mistaken parallels between modern Kalahari water hole foragers and prehistoric Klasies River Mouth foraging-beachcombers (Klein 1976; Binford 1981), and more important, Lewis-Williams' (1981) persuasive interpretation of rock art in the light of San shamanism.

Australian Aborigines were never seen as being quite as free and happy as the Harvard-Kalahari-rendered Bushmen. Although the most revered anthropologist of his time may have doubted that they had much to say about their own past, being mere sad remnants of the colonial onslaught (see Levi Strauss vs Hiatt in Lee and DeVore 1968:210-12), in general, they were regarded as 'aristocrats of the savage world' (Deiley 1979) and heirs to a rich spiritual legacy. They had an active presence in many parts of Australia, and those who lived in the very same regions where sites were being excavated often joined the expeditions as guides, informants and workers. Archaeologists went in hot pursuit of ethno-archaeological data for direct application to prehistoric situations, using the seasonal round of 20th century Arnhem Landers to interpret the artefacts of their Holocene ancestors (White and Peterson 1969), and the foraging patterns of 19th century Aborigines of the Murray-Darling river system to amplify the Pleistocene remains at Lake Mungo (Bowler et al. 1970:55).

Set in this context, Jones' earlier work is all the more admirable. Like his South African colleagues, he was excavating sites in regions where the last active foragers had vanished more than a century earlier. But unlike them, he saw the present landscape as the legacy of a genocidal process, being, as he put it, in a close evocation of the infamous *judenrein*, 'empty of Aborigines'(Haydon and Jones 1978). The absence of recognisable, self-identifying Tasmanian Aborigines meant that Jones could never equivocate about the effect of European contact there, as did ethnographers of the living Kalahari San. Likewise, he never embraced their ahistorical perspective on foragers, but rather, he made extensive use of the Tasmanian archival records and compiled comprehensive ethnographic records that he used in concert with actualistic studies of such things as spear making and canoe use, to specify and comprehend things that could no longer be actively observed. He then directed his findings to bear on two problems that arose entirely from the prehistoric record: firstly, the nature and consequences of Tasmanian technology, and secondly, the impact of the rise of the post-glacial sea.

Starting with material culture, Jones articulated a fundamental paradox. He contrasted the simplicity of Tasmanian material culture with the complexity of its mainland Australian equivalent, and noted that despite the differences, both ethnographically recorded societies supported similar population densities. In other words, Aboriginal population densities were proportional to resources and not to technology. Moving into the implications, he concluded that mainlanders did not apparently use their more complex tool kits to get more food and grow more people, but instead, they invested their surplus into ceremonial matters of 'the ego, the mind and the soul' (1977a:202).

Turning next to sea levels, Jones was perfectly sanguine where the post-glacial histories of Arnhem Land and New Guinea were concerned, treating them briskly as phenomena that involved climatic and environmental changes and a certain loss of former coastal land, but hardly catastrophic either for the prehistoric inhabitants or their later observers (Jones and Bowler 1980). Tasmania, however was different. Here, not a single Aborigine seemed to have prevailed in any respect, let alone to join an archaeological expedition. In a land where rain and cold permeated each page of every 19th century account, no-one since the ideologues of the French Revolution had regarded the former foragers as happy, 'aristocratic', or laid back — let alone successful. Here isolation and contact had engendered genocide and extinction. In Tasmania, Jones posited the rising sea as a metaphorical betrayal, which precipitated an inevitably tragic outcome, limiting the Tasmanians' immediate field of operation as well as their long term options, and eventually costing them their lives. He concluded that the enforced isolation of 10,000 years engendered so few ideas, that even without European invasion, the Van Diemeners were probably doomed to extinction (Jones 1975, 1977a, 1977b, 1977c, 1978).

Jones arguments about the consequences of the rising sea came into public view in *The Last Tasmanian* (Haydon and Jones 1978). Everyone who saw the film has his own most memorable moment; surely no-one has forgotten the purring cadences of Leo McKern's narration in counter-point to Rhys' breathless rendition of what the holocaust implied? Arms out wide, he looked small and lost as he declaimed: 'Suddenly ... wiped out ... a terrible thing.' For me, the singular image was Jones crouched in a miserable hole, reading from an old archival source. It came after a mercifully brisk trot through the archaeologically accurate, but inexpressibly boring monuments of the Macquarie Island settlement. Jones ended up in a ruined building, reading the words of Aborigines penned up here over a century ago. Reading, mind you, — engaging, in that quintessentially enlightened activity of the very Age that started all the trouble in Tasmanian in the first place — reading, as the very gales that once conjured up devil-devils for the Tasmanians, whipped the words from his mouth.

For all the acclaim and prizes that the public showings garnered, and for all the publicity that the movie afforded the Tasmanian genocide, its particular view of this tragedy evinced howls of protest. Accusations of racism littered the reviews, especially in the light of a famous analogy that Jones (1977a:202–3) used to speculate about the inevitability, not of the Tasmanian holocaust, but of extinction:

> the severance of the Bassian (was) (l)ike a blow above the heart, it took a long time to take effect, but slowly but surely there was a simplification in the tool kit, a diminution in the range of food eaten, perhaps a squeezing of intellectuality… Even if Abel Tasman had not sailed the winds of the Roaring Forties in 1642, were they in fact doomed — doomed to a slow strangulation of the mind?

The pairing of 'intellectuality' and 'squeezing' raised a red flag to all purveyors of what would later be known as political correctness. Remember, this was the era when the Kalahari Research Group were debating whether to tithe for the San, and learned academics, burdened as ever with the collective guilts of mankind, lined up to take full blame for the past. Some Australian archaeologists advocated an immediate cessation of all excavations in Tasmania, and urged that we go out of business there forthwith. In the far dour reaches of environmental adaptation where diet is simply a function of availability and where everything is hunky-dory as long as you get enough calories, Jones' suggestion that abnegation of cultural practices might be symptomatic of a more serious situation, struck at the core of accepted wisdom. Ecologically-minded scholars rejected the suggestion that devastating consequences came in the wake of cultural losses, by asking, who needs fish when you can half drown getting abalone, who needs an awl to sew two skins together when you can survive naked in the 'roaring forties', and who needs to make fire when you can drag a smoldering ember through the perpetual drizzle? (Allen 1979; Horton 1979). True, Jones' Tasmanian paradox masked a far bigger one, in that while recognising that the Tasmanians employed the simplest tool kits ever recorded, he held that they were nevertheless sufficiently masterful of the very fire they could not make, to transform their universe with fire-stick farming (Jones 1975). But all these were quibbles compared with the outrage felt at the film's message about extinction. In particular, descendants of the original island population, whether living on Tasmania or the mainland declared themselves as living proof of Aboriginal survival. Some demanded that the film's proceeds should be given to Aboriginal people to make their own version, and all later renewed their political identity in demanding that their land-rights be recognised (Jones 1992:56–59).

Jones rejected the critique of what some saw as his 'dying race' thesis and insisted instead that these issues emanated more from the style than from the substance of his work. He reiterated his view of the Tasmanian genocide as a tragedy in the full-blown, classical, sense of the word (1992a:60), and certainly, this universal sentiment lent weight to his words, drawing as much acclaim as fire. Who better to convey the drama of the matter than this witty, elegant, even elegiac Welshman? He was dispatched all over Australia as Convocation Lecturer for the Australian National University, appearing to tumultuous applause in no fewer than 24 venues from Perth to Wollongong in under three months!

But beyond theatricality, what exactly engendered this huge public interest, to say nothing of private empathy with a century-old genocidal tale in a distant land? It was not as though the arguments were invincible; even his imagery was faulty when he compared the effect of the drowning of the Bassian bridge to a blow to the heart, because a blow to the heart does not take a long time to take effect: it kills immediately. A blow to the head is another matter.

But all this is of no real consequence, because the key to the universal appeal of his work is that Jones was never really talking about heads: he was talking about hearts.

'Consistency' said the pragmatic American philosopher, Ralph Waldo Emerson, 'is the hobgoblin of little minds.' He actually said 'Foolish consistency', meaning a blinkered obstinacy, but if you leave the 'foolish' out, he could be construed to have meant that any idiot can carp about contradictions, when in fact, life, and even science, are a mass of contradictory information that eventually makes sense. Emerson also said: 'With consistency, a great soul has simply nothing to do', and this is really what resonates in the whole Jones-Tasmanian discourse: the implicit analog of the waters of Bass Strait as a saline medium where life was birthed, withdrawing, cutting off the child from the Motherland, leaving Tasmania high, dry, and defenceless.

The point is that Jones tapped a mother lode of universal sorrow by compressing the minutiae of archaeological discourse into one a big issue. One of the great thinkers of our time classified the scholarly enterprise as being populated by foxes and hedgehogs. Using the Greek proverb: 'The fox knows many things: the hedgehog knows one big thing', Isaiah Berlin (1953) distinguished those who see the world in terms of detail, and those who see one big determining principle governing the way things work. Jones, the archaeologist, was generally a fox; a master of detail, counting the fish bones, timing the buoyancy of a bark canoe (Colley and Jones 1987; Jones 1977b). But where his long-term, overarching interpretation of the Tasmanian past was concerned, Jones was the quintessential hedgehog who knows one big thing: the island Tasmanians were wiped out.

On a gray, shore, in the sour light of British imperialism, Jones spoke of rising seas, and drowned hopes, of universal elements, - betrayal, isolation, broken hearts, contracted intellects — issues so close to home, to which he felt such empathy that he clothed them in legends of his own (Jones 1977c: 339):

Seithenyn's response according to legend, was a flask of mead, when in customary alcoholic stupor he forgot to close the sea dykes, and so Cantref Gwaelod (The Bottom Hundred), its bells now tolled by the flow of the tide, was lost beneath Celtic waves.

Dinas dawel môr o heli,	Silent city of the salty sea,
Hafan y dwfn ydyw hi.	Haven of the deep is she.

He reiterated the theme of abandonment again and again. It squeezes more than the intellect, it literally drains the life blood. It is inconsequential whether abandonment is intentional or not, metaphorical or not, Tasmanian or Welsh, it is all the same universal pain, and as such it speaks to a 'fundamental unity' peoples and places over times and space. Jones again (1992b:755):

> When I first entered Kutikina Cave under its dense rainforest cover in March 1981, I was strongly reminded of caves of a similar ambience and antiquity on the other end of the globe in inland valleys of the Gower Peninsula in Wales. Viewed from the perspective of a Pleistocene archaeologist, there is a fundamental unity to the history of humankind. At 14,000 years ago the way of life of these palaeo-Tasmanians, in terms of subsistence, technology and social scale, must have been similar to those of their contemporaries in western Europe.

The parallels are not perfect but they are there: Wales, with its distinctive language and culture stretching back 2000 years, was not sundered by a rising sea from England and abandoned to its fate, but rather, as *Plaid Genedlaethol Cymru*, the Welsh Nationalist Party would argue, it was yoked to the rising tide of a greater nation and culturally submerged within it. After the Napoleonic Wars, the Welsh suffered the contempt of their English masters, in a moral indictment of their religion, language, and even their women, an indictment that echoes the Victorian appraisal of Tasmanian humanity by Sollas, Tylor, and innumerable natural philosophers of that time. Welsh attempts to achieve a separate identity of custom and homeland were still advocated quite explicitly a century later by one of the founders of *Plaid Cymru*, J. Saunders Lewis, when he said: 'Civilisation must be more than an abstraction … it must have a local habitation and a name. Here, its name is Wales' (in Davies 1999:966). It is a view that still echoes there today as it does, where Tasmania is concerned, in the corridors of Canberra.

Jones continued working in Tasmania long after those heady times, but the emotional and evocative tone of research there dissipated. Accounts of the impact of the Holocene in Southwestern Tasmania , as envisaged by the Southern Forests Archaeological Project carry no overtones of betrayal or abandonment. They are objectively brisk, as they discuss the return of the impenetrable forest (Cosgrove et al. 1990:63):

> 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. ... the dates of 12,000 years b.p. may only represent the abandonment of one type of Pleistocene economic strategy focused on cave sites.

This is very similar to the way South Africans currently present their Holocene, (Deacon and Deacon 1999:149-50), and utterly different to the way Jones rendered the impact of the rising sea. But make no mistake, I am not simply setting Jones' universalist poetics up against other people's science. Nor am I seeking to contrast the highly professional work of Southern Forest Tasmanian researchers with the creativity of Jones' early research. Indeed, the fact that Jones participated in the Southern Forests work, renders those particular comparisons moot (Jones et al. 1988). What I am saying is that a soul invigorates Jones' vision of Tasmania - from its Pleistocene beginnings in a glaciated land, through the knell of the returning sea, to the final shores of Flinders Island - and invests it with an humanity seldom glimpsed in archaeological renditions of the human past. It was not the sad tale of Tasmania in itself, nor Jones' archaeological work there that so fired the wider imagination, but rather, Jones' rendition of Tasmania into common experience of love and betrayal. Jones empathised with what it is like to be on the receiving end of cultural drowning, and cultural loss. He understood what it felt like to look out across a grey sea, whether from Bangor or Burnie, and feel your back pressing against the wall of what you thought was your own land. He had a very good idea what it might mean to feel ringed in by an implacable ocean and colonised by a hostile intelligensia. It was a measure of his genius that he converted this personal sense of loss into a universal sense of suffering, that transformed not only the way people thought about Tasmania, but the way they understand their own deep past.

I will sign off this small contribution on a personal note, with a childhood memory that links Rhys and me to Van Diemens Land and the Cape of Good Hope, through the networks of the old colonial world.

I was born and raised in Cape Town, at the southern tip of Africa, at a cross-road of the old colonial world (Schrire 1996). Here, around 350 years ago, the ships of the Dutch East India Company (VOC) plied the routes of their Portuguese predecessors. They anchored in the roads of the Cape settlement, disgorging their dead and dying sailors before taking on fresh supplies and heading out to the riches of the eastern markets. There, Batavia, the jewel in their crown, stood on the ruins of the ancient city of Yogyakarta. Mired as it was in the swamps of the last marine transgression, the Dutch replicated the solution they used to handle the same problem back home, by digging canals. But unlike the bracing climate of Holland, which often froze the wastes in its canals, the hot tropics stewed VOC detritus into a toxic broth, so that the Company men on the banks of the Tiger canal fell victim to malaria and dysentery, faster than to the sharpest kris.

For all this, the VOC managed to keep its proverbial head above water, speading a huge network of trade. In 1642, the Governor-General at Batavia dispatched Abel Jansz Tasman in search of the Great Southern Terra Incognita. Tasman set a course for Mauritius where the denizens of the VOC Fort Frederik Hendrik, oscillated between misery and violence in the path of the cyclones, as they waited to welcome their VOC mates. Their hopes were dashed when Tasman refused to come ashore, preferring to drink and debauch aboard ship as he refurbished his fleet in Vieux Grand Port (Moree 1998:34). Whatever his shortcomings there, Tasman later managed to navigate the southernmost tip of the great southern continent. It was too far south for spices, but close enough to warrant a claim. He planted the VOC marker and the flag of the Prince of Orange, and named it for his boss, Antonio van Diemen, a undischarged bankrupt, who had risen through the ranks, from common soldier to become the brilliant, belligerent Governor of Batavia from 1636–45 (Boxer 1977: 52).

Three centuries after Antonio Van Diemen, my father built his home in the Table Valley of Cape Town. Much as he would have liked to live in Georgian splendour, he could not afford one of the crumbling VOC mansions that stood on the steep slopes of Table Mountain. Instead, he built a new house on a tract called *Verlatenbosch* – Forsaken Bush – where a leprous son of an old VOC governor was supposed to have been hidden away.

Father excavated deep into the hillside to protect the back of the house from the fierce gales that swept down the mountain wall. He envisaged a series of terraces, made up of stoeps, lawns and rockeries, all converging on the ponds in the lowest lawn. He found a foreman to transform the wilderness into a garden and together they walked the land, splashing through streams and kicking at the stones that littered the slope. The foreman and his team were 'Coloured' descendants of colonists, indigenes, and slaves at the Cape. They cleared the slopes and raised terraces. They paved the stoeps with dark gray slates set in cement, and the foreman carved their outlines to serve as runnels for the rain. Father scoured old demolition sites for heavy slabs of banded Robben Island shale, and they set these as steps leading down to the ponds.

The children watched as the garden took shape. They demanded rides in the wheelbarrows, and stuck their fingers in the metal buckets that held the cement. One particularly skinny child, who usually refused to eat regular food, watched avidly as the foreman unpacked his lunch. He delved into the a creased paper bag, satined with the grease of previous meals and laid out a feast of Cape creole cuisine, drawn from centuries of Eastern slave cooks: pickled fish, chutney, fried meat balls, and kumquats in cinnamon syrup (Jordan and Schrire in press). The men laughed as the child gobbled the spicy food from his hand: the builder smiled at the exasperation on her mother's face.

When the work was nearly done, the foreman smoothed a last layer of cement over the tops of the walls. He called all four children to his side. One by one, from oldest to youngest, they pressed their right palms into the cement. Under each impression, the foreman carved the initial of their first name. Then wiping the tip of his trowel, he went from wall to wall, signing his own full initials in his work.

His mark, slightly worn by wind and rain, remains perfectly legible today: 'A v D.' Anthony van Diemen.

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The Last Typologist: Rhys Jones and the Problem of the Archaeologists

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ROCKY CAPE and the Problem of the Tasmanians was the title of Rhys Jones' PhD thesis, submitted to the University of Sydney in 1971. It is, I would suggest, his magnum opus, and by any standards an outstanding contribution to Australian archaeology and our understanding of the Tasmanian past. As the title suggests, the thesis revolves around the excavation of the cave sites at Rocky Cape in northwest Tasmania, but it covers considerably more ground than that. At its core is a detailed typological study of the stone artefacts recovered from the Rocky Cape excavations. This seems to represent almost the last such study in Australian archaeology, and in this paper I want to discuss the original work with an emphasis on the typological study, and the implications of its apparent lack of issue.

Jones came to Australia in September 1963 with an appointment as Teaching Fellow in the Department of Anthropology at the University of Sydney (Jones 1971b:56). For reasons which he discusses in his thesis, he set off for Tasmania as leader of an archaeological expeditionary team in the summer of 1963–4 (see Meehan, this volume). This was the beginning of a long-term involvement in Tasmanian archaeology, documented in a series of published papers leading up to the thesis (Jones 1964–5, 1965a, 1965b, 1966, 1967, 1968, 1969, 1970, 1971a, 1971b), and, of course, continued since (e.g. Jones 1974, 1976, 1977a, 1977b, 1978, 1979, 1982, 1987a, 1987b, 1990, 1995, Jones and Allen 1978, 1984, Jones and Lampert 1978, Jones et al. 1983, 1988).

The papers which Jones published between his first field trip to Tasmania and the preparation of the thesis did not necessarily anticipate the content of the latter. While some primarily described the results of the fieldwork (Jones 1964–5, 1965a, 1965b, 1967), others set forth more interpretative matters (Jones 1966, 1968) and others again concentrated on specific, and not entirely archaeological topics (Jones 1969, 1970, 1971a). Very little attention was paid in any of these to stone artefacts; the main areas of interest were broader and often interdisciplinary topics. Jones addressed the importance of an understanding of past environments in interpreting the human occupation of Tasmania (and Australia) (Jones 1968, 1969), the significance of analysing the faunal aspect of the archaeological record (1966, 1968), the use of the ethnohistoric record in developing models of past Tasmanian society (1970, 1971a) and a range of related issues.

THE THESIS

When he came to collate the information gathered into a formal thesis, Jones did not abandon these issues, but neither were they as central as might have been expected. He felt it was incumbent on him, as the first professional archaeologist carrying out substantial fieldwork in Tasmania, to address those questions about the Tasmanian Aborigines which seemed to him the most timely and historically important. These questions were embodied in the sub-title of the thesis, 'the problem of the Tasmanians'. This, he argued, consisted in fact of two problems. 'The first concerned the origin of the Tasmanians, of who they were, and how they had got to their island. The second concerned their cultural status, their place within the evolutionary sequence of human societies' (Jones 1971b:17). Elsewhere, he expands the first of these questions, as follows, and incidentally describes himself as a 'student of Tasmanian culture history':

> I regard this thesis as part of a long term investigation into the culture and history of the Tasmanian Aborigines. In it I have chosen to tackle certain problems which I consider to be crucial to solve, before other more general discussions can be pursued. ... For the Tasmanian Aborigines these [questions] are still the old ones, of who were they, how long had they been on their island, what happened to them there, and what was their relationship to the mainland Aborigines? (Jones 1971b:55).

The thesis took the form of a stone artefact study embedded in a site report within a wide ranging context of ethnohistory, geography, previous research in Tasmania and the overall context of Australian archaeology of the time. Particularly illuminating is Jones' discussion of how he came to excavate the caves at Rocky Cape ('Personal research 1963–1970', Jones 1971b:55–66).

Jones' description of his excavations of the two caves, Rocky Cape South and Rocky Cape North, and their stratigraphy and chronology must rank as one of the outstanding pieces of archaeological writing in Australia (Jones 1971b:111-97). Rarely have archaeological sites and their excavation been described in such glowing and painstaking detail, with illustrations (particularly the section drawings by Winifred Mumford) to match. The 'master sequence', derived from both caves and comprising seven analytical units, was convincingly shown to span a period of 8,000 years, up until a few hundred years ago. The delineation of the master sequence was followed by three sections which comprised nine chapters on the stone assemblage, to which I shall return. A shorter section of two chapters was devoted to organic remains including artefacts and food debris. The concluding chapter discussed the results of the analyses for the Rocky Cape sites overall, and these were then extrapolated to generate a sequence of occupation in northwest Tasmania generally. The northwest was then contrasted with what was known of eastern Tasmania, and an overall Tasmanian view generated which was then considered in the context of the history of occupation of Aboriginal Australia.

The Rocky Cape sequence was presented in the conclusion in almost the form of a narrative, but the fundamental argument, derived from the typological analysis, was that it 'documents the internal evolution of a single, historically related technological tradition, over a period of 8,000 years' (Jones 1971b:607). With respect to northwest Tasmania, the Rocky Cape sites were compared particularly with the West Point midden site. Jones concluded that:

> the cultural remains at this site [West Point], particularly the stone tools were similar to those in the contemporary Units 1 and 2 at Rocky Cape. I suggest that the two sites were occupied by people who shared the same economy and culture, most probably by bands of the same tribe. ... in terms of diet, pattern of seasonal movement, regional or tribal affiliation, burial customs and art, we can take this culture and economy back to about 2,000 years ago at least (Jones 1971b:610-11).

The comparison with eastern Tasmanian sites (and I will return to this later) led to the view that the 'eastern assemblages quite obviously belong to the same tradition as those from the northwest' (Jones 1971b:615). Jones was thus encouraged to conclude for the island overall that '[t]hroughout Tasmania's known prehistory, there has been practiced [sic] a single tradition of stone tool manufacture. In its latest phases this can be related to several other aspects of economy and culture which characterised the ethnographically recorded Tasmanian Aborigines. ... this can be called the *Tasmanian Culture*' (Jones 1971b:617).

The conclusion placed Tasmania in the wider context of Aboriginal Australia, particularly with respect to archaeological sites dating to the Pleistocene. These were at the time considered to have tool assemblages:

> which show strong morphological similarities to each other ... [and which] belong to a single pan-continental tradition for which [Harry] Allen and I ... have suggested the term *Australian core tool and scraper tradition* ... . Tasmania formed one of the regional provinces of the core tool and scraper

> tradition. ... There was no greater difference between Tasmanian and mainland assemblages than there was internally between some mainland provinces (Jones 1971b:626–7).

From this, he concluded as follows:

> I think that one aspect of the problem of the Tasmanians has been solved, namely the place of immediate origin and the cultural context of the direct ancestors of the ethnographically recorded inhabitants of the island. The Tasmanians came from the Australian mainland across a land bridge during late glacial times and were isolated by the post glacial rising sea. On the island, adaptations had to be made to new and changing environmental situations, but there was also a fundamental continuity of this cultural tradition right through to the time of the arrival of Europeans (Jones 1971b:627).

The centrality of the stone tool typology to these conclusions is I think obvious. I would now like to consider the typological exercise which Jones carried out, both in terms of how it was done and the assumptions which underlie it.

THE TYPOLOGY

At the time that Jones' analysis was carried out, computers were just coming into use by archaeologists; these were main frame computers which necessitated quite complex input procedures, unlike today's desk top units which are far smaller yet immeasurably more powerful. In this sense, Jones' analysis was a pioneering step in Australian archaeology, putting the new technology in the service of major research issues (Jones 1971b:315).

The term 'typology' is used in several different ways by archaeologists. Generally, it is understood to mean the classifying of artefacts (of any kind) into 'types', that is, recognisable categories. Sometimes it is used, rather inaccurately, to indicate a particular approach to stone tool analysis, which is the classification of only flaked stone artefacts which have secondary working, and which for that and/or other reasons are considered to be tools rather than waste (usually now referred to as 'debitage'). As with all typologies, there has been discussion as to the emic reality of such stone tool types.

Jones did carry out a typology in this restricted sense, but he also analysed his stone artefact materials in other ways. Debitage, consisting of primary flakes and cores, was analysed in terms of its 'manufacture' (what would now be called 'technology'), and a lithological analysis was also carried out. The technological analysis did not include all the characteristics which are now more or less standard, but did look at issues of core preparation and curation (Jones 1971b:215–48). Utilised pebbles and 'slightly retouched pieces' were considered on their own (Jones 1971b:292–308).

'Implements' were defined as 'pieces which had one or more systematically worked edges' and 'included both "core tools" and "flake tools"' (Jones 1971b:215). It was within this category that 'types' were sought. Following a review of Tasmanian stone tool typologies which had been carried out prior to his own, Jones (1971b:313–15) described what he felt distinguished them from his own analysis:

> These typologies were all 'traditional', in the sense that they were dependent entirely on experience and intuition. Implements were grouped together on the basis of morphological similarity and characterised by reference specimens, where were often extreme examples of the general class. ... their validity rested on one's assessment of the professional skill of the author. These procedures are the essential first step in any archaeological investigation, and in some circumstances where tool types are specialised and well differentiated from each other such as in the European upper Palaeolithic, or Australian small tool assemblages, they have proved adequate for the creation of refined culture historical sequences. However such methods are quite inadequate where types are

difficult to define, or where there is an overlap between one and the other, as is often the case with old Australian assemblages (Jones 1971b:315).

Having set out clearly what he felt were the deficiencies of the traditional typologies, Jones described his own approach as differing in that he considered artefacts as populations rather than *belles piéces*, and he used population statistics to 'describe them in terms of the dimensions and inter-relationships of their attributes' (Jones 1971b:315). 'The solution seemed to lie in plotting the frequency distributions of measurable attributes, and comparing assemblages in terms of these' (Jones 1971b:315). As a model, he referred to Lampert's research at Burrill Lake, NSW:

> Lampert's analysis of the Burrill Lake assemblage (1971:16-28) constitutes the most rigorous analysis of an old Australian industry so far published. Attribute distributions were transformed into means and standard deviations, most quantitative statements tested in a formal statistical manner and a pilot study of correlation coefficients carried out. It marks the most that can reasonably be achieved without resorting to computer calculation (Jones 1971b:319).

Jones took what he clearly believed to be the next logical step, using the same approach and resorting to the use of a computer. The other major departure of his analysis was the consideration of worked *edges*, rather than (or, really, as well as) whole artefacts, a precedent adopted from Mellars, J.P. White and others (Jones 1971b:318, 320). He classified the edges into intuitive types, then carried out measurements and recording of discrete attributes to see whether the types could be statistically validated. This he refers to as an R-style analysis, rather than a Q-style one. The difference is that for the latter, all attributes are fed into a computing program to see if clustering occurs without predetermining what such clusters (i.e. types) might be.

He summarised the typing process as follows:

> Where the separate identity of groups could be justified [by statistical significance tests], they were formally defined as types. Where this was not the case, they were combined with each other and the tests repeated, until a discrete group was found. In practice, I found that I could justify most of my intuitive types and the analysis enabled me to give a precise description of the discriminating characteristics of each one. ... While I am confident of the identify of each of my types presented here, other equally good, or perhaps better sub-divisions may exist (Jones 1971b:320).

I will not repeat the details of the analysis here, the variables used and the statistical tests carried out; the interested reader can consult the thesis. I will hasten to the substantive conclusion, which was that five types were represented at Rocky Cape North and South, as follows: type 1 comprised *round edge scrapers*; type 2 comprised *steep edge scrapers* (possibly with younger and older sub-groups); type 3 comprised *flat, straight edge scrapers*; type 4 comprised *notched scrapers*; and type 5 comprised *concave and nosed scrapers* (within which further sub-division might eventually be possible) (Jones 1971b:437). Jones asserted that:

> ...considering them as populations I am convinced of the separate identity of the five artefact groups described above. These were not only significantly different to each other in terms of the absolute dimensions of attributes, but each had a distinctive pattern of inter-relationship of attributes ... . The differences between types are so strikingly marked and strong, that although refinements will be made, and individual artefacts re-sorted, the types themselves have a robust quality which I am sure will survive a more sophisticated analysis (Jones 1971b:437).

What is not entirely clear is what Jones considered these types *represent*. While it is not stated, the implication would seem to be that they are emic constructions which would have been recognised (in different terms) by the makers, and a further implication is that they are *functional types*. In this case, it is believed that each type would have been deliberately designed and produced to serve a particular use. Jones (1971b:437) observes that '[t]hese clusterings are intelligible both in a technological and in a functional sense.'

This was demonstrated in the section on stone use. After discussing the ethnographically observed uses of stone artefacts in Tasmania, and observed wear patterns on the artefacts themselves, there is a section of the thesis entitled 'an attempt to identify the functions of the Rocky Cape tool types' (Jones 1971b:483 and ff.), which indicates clearly to me the idea that each type was a dedicated functional type. I am not at this point concerned with whether this is in any sense true or not. The Australian ethnoarchaeological literature indicates arguments both for (Cane 1988) and against (Hayden 1977) such an idea. In any case, it does not, as some believe, invalidate the use of typology if it is *not* the case, rather perhaps the reverse (see Bowdler and Smith 1999).

Jones' interpretation of his stone tool assemblage is that '... the Rocky Cape sequence documents the internal evolution of a single, historically related technological tradition, over a period of 8,000 years' (Jones 1971b:607). Jones' final conclusions addressed his initial question, 'who were [the Tasmanian Aborigines], how long had they been on their island, what happened to them there, and what was their relationship to the mainland Aborigines?' (Jones 1971b:55). He wrote:

> Tasmania formed one of the regional provinces of the core tool and scraper tradition. ...There was no greater difference between Tasmanian and mainland assemblages than there was internally between some mainland provinces. ... I think that one aspect of the problem of the Tasmanians has been solved, namely the place of immediate origin and the cultural context of the direct ancestors of the ethnographically recorded inhabitants of the island. ... On the island, adaptations had to be made to new and changing environmental situations, but there was also a fundamental continuity of this cultural tradition right through to the time of the arrival of Europeans (Jones 1971b:627).

In other words, the Tasmanians had a common ancestry with the mainland Australian Aborigines, and differences between them perceived in the ethnographic present were due to subsequent adaptations to the respective changing environments. I do not believe anyone subsequently has seriously challenged this conclusion.

Nor has anyone challenged the typological model thus developed; but then, no-one has really tested it on any other assemblage. Jones himself alludes to a more rigorous statistical testing of his typology. Using his measurements for the Rocky Cape assemblages, a 'full multivariate, principal components analysis' was carried out. 'Gratifyingly they confirm my original definitions of artefact types and also trends within the chronological sequence' (Jones 1987a:32, which contains reference to J. Fethney et al. 'A Multi-variate analysis of stone tools from Rocky Cape, Tasmania', in preparation 1987).

Subsequent archaeological research in Tasmania has sheered away from typological analysis. This is not to say that no typological classification has taken place, as most researchers have at least described their excavated assemblages, and in general they have used Jones' type categories to do so. There has however been little comparative work involving detailed observation and measurement of implements to see how closely they conform to Jones' types, let alone to test the argument that these type categories are objectively replicable, that is, whether in his own words 'they will survive a more sophisticated analysis' (Jones 1971b:437).

Why should this matter? There is at least one issue which to my mind remains unresolved, which is the extent to which the stone artefact tradition which Jones documented at Rocky Cape is representative of the island as a whole. His own assertions on the matter were somewhat ambivalent. On the basis of what can only be called a cursory description of assemblages from eastern Tasmania, he argued that '[t]hroughout Tasmania's known prehistory, there had been practiced [sic] a single tradition of stone tool manufacture' (Jones 1971b:617). He argued that 'Tasmania formed one of the regional provinces of the core tool and scraper tradition. ... [t]here was no greater difference between Tasmanian and mainland assemblages than there was internally between some mainland provinces' (Jones 1971b:627). Yet, he follows this by saying:

> within Tasmania itself, the north-western and southeastern assemblage sequences described above, were as distinct from each other as either were with some of the mainland provinces. ... A study of

Tasmanian ethnography gives an insight into the sociological and ecological parameters which dictated artefactual and historical diversity and tradition of this order of magnitude (Jones 1971b:627).

It might be argued that there are differences of degree indicated here, but it does suggest an issue needing resolution. In the 30 years since Jones' thesis was submitted, however, nobody has addressed it.

SUBSEQUENT RESEARCH IN TASMANIA

Following Jones' pioneering research, the next professional archaeologist to work in Tasmania was Harry Lourandos. His fieldwork was carried out in the southeast, and was intended to provide a comparison with the northwest (Lourandos 1970, 1977). In particular, he wished to test the hypothesis that settlement patterns were significantly different, with a more dispersed pattern of exploitation across the landscape in the east contrasting with a more limited exploitation of a narrow coastal hinterland in the west. To this end, he concentrated on two sites, a shell midden at Little Swanport on the coast and the inland lakeside site of Crown Lagoon. In analysing the stone artefact assemblages from these sites, Lourandos aimed to deal with them 'as indicators of independent site function, and to describe assemblage variability between sites in functional terms' (Lourandos 1977:219). His aim was 'to construct behavioural models at contrasting sites' in order to examine hypotheses 'demonstrating local and regional environmental adaptations' (Lourandos 1977:219). To this end therefore he considered only technological and functional parameters of his assemblages, and did not in any way classify them into types as Jones had done. Jones' observations of similarity were based apparently on Lourandos's illustrations (Jones 1971b:615).

My research work in Tasmania began two years after the submission of Jones' thesis, and was naturally considerably influenced by it (e.g. Bowdler 1974). In the final account of my fieldwork (Bowdler 1979, 1984), the emphasis was on the analysis of the faunal and environmental data, but a description of the stone tools from Cave Bay Cave on Hunter Island was included. I had hoped to 'identify functional types, which would be informative of economic activities, and [to use the stone tool analysis] to indicate cultural connections' (Bowdler 1984:105). I found however that on the one hand the Cave Bay Cave assemblage was 'somewhat intractable in this respect', and observed also that 'more work is required on more Tasmanian assemblages to outline the variables' (Bowdler 1984:105).

I carried out as many measurements and observations as seemed warranted by a relatively small assemblage composed mostly of pieces of quartz (Bowdler 1984:107–122). With respect to artefacts with secondary modification, I saw little point in an attempt to establish independent, statistical types, and categorised pieces intuitively, guided mainly by observable secondary working and checking them against Jones' categories 'to see whether they fall within his numerical ranges' (Bowdler 1984:116). I could only assert that there were 'hints of wider relationships', and that a 'more typologically tractable Tasmanian assemblage of Pleistocene age' was needed (Bowdler 1984:122).

Subsequent to my PhD research, I carried out a larger scale excavation ($31m^2$) of one of the Hunter Island middens into which I had previously sunk a small sounding ($2m^2$) (Bowdler 1979, 1981, 1988). The Stockyard Site produced a relatively large stone artefact assemblage dating within the last 1000 years. My initial perception of this assemblage was that it contained fewer types than those recognised by Jones from his excavations at Rocky Cape. In an argument which started from Jones' identification of his types as functional types, I hypothesised that either the range of such types represented on Hunter Island was more restricted due to a more restricted range of activities being carried out in the island situation, or that in fact the tools were multi-functional and perhaps dictated by style as much as function (Bowdler 1981). Subsequently this hypothesis was investigated by honours student Jeannette Neden (1984) in a detailed analysis of the Stockyard Site artefact assemblage.

Neden's project was an explicit attempt to 'explore the nature of stone artefact assemblage variation in a regional context' and to 'isolate and identify the variables that contribute to such variation' (Neden 1984:2). To this end, she carried out an analysis of the large (>18,000 pieces) assemblage, following Jones' methodology, with more detailed attention to debitage. One of her main results was a refutation of the basis of my original hypothesis. Three of Jones' types (steep-edge scrapers, round edge scrapers, notched and nosed scrapers) were in fact found in the Stockyard Site assemblage, and furthermore these were the only types present in the uppermost part of the Rocky Cape sequence (analytical unit 1), contemporaneous with the Stockyard Site. For the purposes of this paper, we may note that Neden put Jones' typology to the test, and concluded as follows.

> All the evidence suggests that the Stockyard Site assemblage conforms closely with the latter part of the continuum of morphological and manufacturing trends outlined by Jones. The internal attributes of tool types and the characteristics of the implement population in no way suggest a relaxation of the manufacturing impetus or traditions evident at Rocky Cape. Rather than reflecting a makeshift typology, the formal tool assemblage at the Stockyard Site mirror[s] the traditions and the trends of the Rocky Cape sequence faithfully (Neden 1984:83).

This was the last occasion on which the Jones typology was put to the test, nor has any more recent effort been made to produce another. The next major field project carried out in Tasmania was Vanderwal's research on the south coast of Tasmania (Vanderwal and Horton 1984). From his sites at Louisa Bay and on Maatsuyker Island, Vanderwal found an industry which could be described as 'simple and relatively uniform' (Vanderwal and Horton 1984:129). Much of it was of quartz. He identified four categories of secondarily worked artefacts (utilised flakes, scrapers, notched artefacts and nosed artefacts), of which there were 71 in all, 36 of which were utilised flakes (Vanderwal and Horton 1984:124). These types were apparently identified intuitively. Vanderwal carried out simple measurements of the artefacts, and some statistical testing was carried out to examine intra-regional variability. No attempt was made to compare the assemblages to the artefacts from Rocky Cape or anywhere else.

The discovery of Kutikina Cave (originally called Fraser Cave) in the course of a controversy concerning the building of dams in southwest Tasmania led to a flurry of activity in the southwest and southern forests area (e.g. Jones 1995:429–32). A considerable amount of excavation has been carried out, but much of it concerning stone artefact analysis is yet to be reported in detail. A range of mostly cave sites have provided evidence of occupation from ca. 35,000 BP until terminal Pleistocene times and later in some cases. Most of these sites thus antedate the oldest occupation at Rocky Cape, and show more continuity, and contain considerably larger amounts of cultural material, than Cave Bay Cave.

In Kutikina Cave, Jones described an interesting sequence. In the older levels, dating to ca. 20,000 BP, he saw stone tools resembling those from the lower levels of Rocky Cape South, consisting mostly of 'steep-edge scrapers and domed core-scrapers with steep edges that are often at right angles and show extensive stepped flaking', and also nosed and notched scrapers (Kiernan et al. 1983:3; Jones 1984:54–55, 1987a:36, 1990:279). Then in levels dating to between 18,000 BP and 16,000 BP, came what Jones describes as a 'great surprise', a change from mostly quartzite to quartz, and from the kinds of tools described to a single recognisable type only, the thumbnail scraper (Jones 1984:55, 1987a:36, 1990:279).

There has been discussion as to the function of these thumbnail scrapers, the timing of their appearance, whether the same sequence can be identified at the other sites, and whether they might represent a cultural/stylistic phenomenon (McNiven 1994; Cosgrove 1995:9, 51; Jones 1995:279-81). No-one has yet provided any rigorous metrical or technological parameters for defining a thumbnail scraper. No-one has inquired as to whether its dimensions might, for instance, overlap with that of Jones' round edge scraper type. Cosgrove in particular claims that 'intuitive typological groupings of stone 'tools' were avoided', yet goes on to note the presence of thumbnail scrapers and the absence of '"formal" artefacts like those identified at Rocky Cape' (Cosgrove 1995:34, 62). No better example of 'intuitive typological grouping' could be imagined.

There is also the curious case of McKay (1992), who discusses the work of Jones (1971b) in attempting to establish a rigorous typology and the lack of similar subsequent research, then jettisons all that to revert to a completely intuitive set of types based entirely on visual attributes. For example, 'side scrapers' are defined as having 'a retouched edge which ran parallel or near-parallel to the long axis of the artefact' (McKay 1992:19), which somehow means they are 'the least likely artefacts to be selected according to subjective criteria' (McKay 1992:35). Most of the other types are not even defined: 'Thumbnail scrapers. A common type with universal distribution' (McKay 1992:39). Measurements are provided only for side-scrapers, a ratio of width to length, and edge length; we are not told whether orientation is considered in the former (McKay 1992:35).

IMPLICATIONS

So Jones' monumental study of the stone artefacts from Rocky Cape stands as an isolated eminence. Many issues, as I have tried to indicate, remain unaddressed. Some will argue that they are not of interest, and indeed that would seem to be the case, not just in Tasmania, but in Australia generally. Nearly all stone tool analysis of recent years has consisted of technological or residue studies (see for example Fullagar 1998). McBryde commented some years ago now that 'as archaeologists we have swept aside the classificatory challenges of the corpus of stone artefacts from Australia' (McBryde 1986:23; see also Holdaway 1995). This goes hand in hand with a diminution of interest in culture history (as I have pointed out before, e.g. Bowdler 1997:17) and a trend, indeed a demand, for any study of stone tools to be 'tempered [?] with technological analyses based on appropriate behavioural models' (Veth 1997:75). Indeed, the word 'behavioural' and its variants are never far from the modern stone analyst's lips.

Pigeons playing ping-pong is my most enduring memory of first year psychology (in a film, not live, alas). They had been trained to do so by B. F. Skinner or one of his acolytes, adherents of Behaviourist psychology, begun by Pavlov and his celebrated dogs and greatly popular in the US in the 1930s and 1940s. Behaviourism based its methodology and theory on the unit 'stimulus-response' (S-R), and concentrated on the processes of learning (e.g. O'Neil 1982:101). Teach a pigeon to hit the ball with its bill (R) by giving it food (S) when it does so successfully. It has been described as a form of environmental determinism. 'Behaviour is selected depending on its consequences. A stimulus from the environment or other people carries a punishment or a reward and thus encourages 'strategic' behaviour as a response' (Bulbeck 1998:252).

While accepting that most modern archaeologists have probably never even heard of Skinner (but bearing in mind Maynard Keynes's dictum about defunct economists), this description could well apply to the apparent thinking underlying much of the work of Australian stone analysts, and its surface language. All variation in stone artefact assemblages is to be attributed to raw material availability, occasionally function, and technological factors, that is, how the things are made. The greater determinants for the latter are usually thought to be patterns of exploitation and ultimately, in all cases, environmental factors. This view is not limited to archaeologists in Australia (e.g. Andrefsky 1994; Neeley and Barton 1994), although some overseas scholars are willing to concede a role for cultural variation (e.g. Dibble 1987:116).

Environmental determinism in its many guises is a constant temptation and hazard for archaeologists, particularly of the deep past. In many cases, it is generally accepted that much behaviour can best be interpreted as a response to environmental stimuli. I would argue however that our best approaches to understanding the history of people in the past should be based on a fundamental humanism which allows human action some free play. Whatever else might be said about Rhys Jones, he has always shown himself to be *au fond* a humanist.

Acknowledgements

I would like to thank Rhys Jones who was my PhD supervisor, who has provided much fuel for many fires and whose fate it would seem to be to be contradicted by his many students; I think that is a sign of pedagogical success. Thanks also to Jane Balme for reading a draft of this essay, and referees who did not wish to be identified.

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The Last Tasmanian – A Personal View

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IN THE mid 1970s Tom Haydon was a prominent and innovative Australian documentary film maker. He grew up in Manly NSW and was educated at a state high school more renowned for its production of rugby internationals than intellectuals, which nonetheless offered students ancient Greek, Latin and advanced courses in English literature. Haydon got from such eclectic schooling both a love of learning — he took an honours degree in history from Sydney University — and a practical view of life which incorporated humour and larrikinism hand-in-hand with artistic achievement and hard work.

After formative years working in film production at the ABC, followed by a stint at the BBC in London, where he gained something of a reputation as an *enfant terrible* for his irreverent treatment of British social institutions — you could take the boy out of Manly but not Manly out of the boy — Haydon returned to Australia to continue an earlier flirtation with Australian prehistory. This had begun while filming Professor N. G. W. Macintosh at an ANZAAS conference in Melbourne in 1967. Taken by the dichotomy between Macintosh as showman and Macintosh as scientific authority, Haydon saw a way to overcome the stilted structure of scientific documentaries which normally used an interviewer to dish up 'Dorothy Dixers' for the expert effortlessly to dispatch. In *The Talgai Skull* (1969) Macintosh was more investigative reporter than eminent anatomist, and Haydon won a Logie. Haydon then attempted to repeat the formula in *The Long Long Walkabout* (1975), but in places science descended into slapstick. Academics, tempted from their ivory towers by audience numbers only dreamed of by academic journals, saw themselves demystified and ran for cover.

Rhys Jones was one who didn't. Jones and Haydon each found in the other an *alter ego*. Each fed off the other's vision and intellect, Jones the passionate prehistorian, Haydon the visual communicator, both working within an overtly intellectual frame. When working on the Tasmanian scenes for *The Long Long Walkabout* Jones had exposed Haydon to the dramatic counterpoint of ten thousand years of isolated Tasmanian hunter-gatherer society obliterated in a few decades by the inevitable juggernaut of nineteenth century European expansion. Earlier, Haydon, in coming to terms with his Australian-ness, had thought about a documentary on the Tasmanians. Here was the solution. Jones was not only the pre-eminent Tasmanian archaeologist, his impromptu lectures were articulate and compelling. In Jones, Haydon found both a visual focus for his camera and a co-author able to curb the excesses of farce which had diminished his earlier films on Australian prehistory.

Jones and Haydon planned *The Last Tasmanian* in Wales during 1976, the year that Truganini was given an official State funeral in Tasmania, exactly one hundred years after her death. Haydon had filmed this event, which was to become a central focus of the film. The remainder of the Tasmanian filming took place in March and April 1977. I had a bit part in the Tasmanian scenes, playing the fool to Jones' Lear, as one commentator put it. It was a time of contrasts, on the one hand intellectually intense and physically tiring, constantly in a car, boat or plane getting to the next location, lugging gear, putting up tents in the dark. On the other hand it was endlessly waiting; waiting for Haydon and Geoff Burton, the camera man, to line up the shot, waiting for the light to brighten, waiting for Haydon to get off the phone. In the waiting time I finished *Bleak House* and *Dombey and Son*, big reads, both.

There was no script. At Cave Bay Cave we sat and discussed the implications of extended isolation and the camera rolled as we improvised; elsewhere Haydon encouraged us to role-play. We were not actors, nor were meant to be, and we did it as much for our own self-awareness of the histories we were exploring as for the camera. But Haydon also had a consistent view of needing light relief as a change of pace when he came to cut the film. I was persuaded to learn to play the *Marseillaise* on a penny-whistle and eventually the scene was shot. It was note perfect and the crew applauded; but it was also musically bereft and ended on the cutting-room floor, usurped by an execrable French village brass band. Rhys, meanwhile, was filmed in a wetsuit sinking into the ocean off Hunter Island trying to manoeuvre a waterlogged bark canoe with a pole - literally up Bass Strait without a paddle. But constantly, beyond all of this, the melancholy of the locations we used and the story we were trying to convey was palpable.

I first saw the finished film at *Ty'r Paith*, the Meehan/Jones country house at Hoskinstown, NSW. I was overwhelmed by the beauty and the grief of the visual effects, the compulsion of Bach's music, the relentlessness of Leo McKern's monotonic narration. I felt shattered - as if my stomach had been stapled. We all of us sat still, silenced by the force of what we had seen, a reaction which would repeat itself again and again with audiences in and beyond Australia. Multiple subsequent viewings did little to change those initial emotional responses but allowed me more clearly to see the complexity of the film, shot through as it is with levels of meaning, paradox and ambiguity.

The film was certainly Haydon's *magnum opus* and its reception as a documentary film was remarkable. Shot with French and Welsh versions as well as English, it won Haydon another Logie, it had both commercial film release and prime time television screening, it toured overseas universities with Haydon and Jones as commentators, it was the subject of feature articles, reviews and even editorials in Australian national dailies and a wide range of magazines and periodicals. It was debated on a nationwide ABC TV prime time current events program, *Monday Conference*, televised live in Hobart. It was the cover article in an issue of the London *Sunday Times* colour supplement, and eventually it was screened in more than 20 countries around the world.

The attention received by *The Last Tasmanian* reflected two particularly different aspects of the film. In my view by far the more important of these is that it brought home to the wider Australian and British public for the first time the reality and violence associated with the British colonisation of Australia, particularly underlined by the immediacy of the visual medium employed. In some quarters today it might merely be dismissed as 'black armband history', but when the film was released, this was an unresearched, unknown area of the Australian past. Neither the film nor its viewers and reviewers could escape the concept and implications of genocide, a component of Australian history not entertained up to that time by Australians at large, and also one totally foreign to their perceptions of self.

The second aspect is that even before it was shot, let alone released, the film became a football used to focus attention on the emerging political aspirations of contemporary Tasmanian Aborigines. The debates and arguments used by Aborigines, academics and white apologists at that time require no review here (see Jones 1992 and references) but I note a few consequences. Most regrettably, the film was reduced in this process to a cipher of itself. Its great strength as an historical document was deflected as its central purpose and message were marginalised and converted for political expediency. The view that the film denied Tasmanian Aborigines their existence derived mostly from the film's title. Truganini, through the latter part of the nineteenth century and most of the twentieth century had been referred to by many historians as the last Tasmanian, and in this title Haydon saw 'several degrees of literary allusion, referring to the late 19th century "projections of melancholy" from novels such as Fennimore Cooper's *The Last of the Mohicans*, to Charles Woolley's searingly accusatory photograph [of Truganini] of 1866' (Jones 1992). The point is that a decade earlier there would have been no outcry about the title; a decade later the film would perhaps have been differently named. (But given Haydon's usual refusal to be diverted from his artistic purpose, perhaps not.) It is however a measure of the time that a well-known history of the Tasmanian Aborigines when first written in 1975 as a doctoral thesis used a range of alternate terms to 'Aborigine', but used only this term when published in 1981.

The events of that time were also a particular moral and political learning curve for me. I recall being very startled when two academic colleagues, one verbally, the other later in print, argued that if researchers uncovered history detrimental to the political aspirations of contemporary minority groups, it was their responsibility to suppress it. This was a far cry from the tenets of truth and learning which I had been taught in Western academia and to which I still cling, albeit among a diminishing minority in this postmodern world.

The ultimate irony of *The Last Tasmanian* is that far from denying Tasmanian Aborigines their existence, it played an integral role, both in the messages it contained and the debate it engendered, in the emergence of a strong contemporary Aboriginal community in Tasmania and a white Tasmanian recognition of its legitimacy.

* * *

In the early 1980s in Hobart, Rhys Jones and I were accosted by a young Aboriginal woman who abused us for making the film that said she didn't exist. When asked whether she had seen the film she said she had no need to, she knew what it said. I was startled by her youth and zeal and certainty, and my mind cast back to the night of that *Monday Conference*. It was 2am and Rhys and I were having a beer with one of the leading Aboriginal activists of that time in the bar of Hadley's Hotel in Hobart. At the other end of the bar the last of the reporters who had earlier interviewed us was chatting up the barmaid. 'Well, I wouldn't say it in front of him', said our companion, gesturing down the bar, 'but I thought it was a bloody good film.'

This note has drawn freely on Rhys Jones' 1992 obituary of Tom Haydon: Tom Haydon 1938–1991: film interpreter of Australian archaeology. *Australian Archaeology* 35:51–64. It is an article for which Rhys clearly sweated blood, both personally and professionally — a significant analysis of Haydon's work and the events which surrounded *The Last Tasmanian*.

Imagined Continents

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THE ISLAND of Kerguelen, uninhabited in the southern Indian ocean, is a real but imaginary place. It is real because it can be visited, although Rhys Jones is the only person I know who has done so. It is imaginary because like many remote places it produces dreams. During the 18th century when the search was on for the great southern continent Kerguelen was truly the stuff of dreams. It was first sighted in February 1772 by the French explorer who named it after himself. His reckoning was almost 5° out but because there is no other land in that part of ocean between latitude 48°S and 49°S there is no doubt that this was the island he found (Dunmore 1965:208).

But it was not the discovery he wanted. The crew suffered from frostbite and Kerguelen, experienced in Icelandic waters wrote in his log 'never was felt a cold so bitter' Dunmore (1965:213). Yet later in his verbal reports to the Governor of Mauritius, Kerguelen favourably compared the island's latitude to Normandy in the northern hemisphere. His false report led to dispatches being sent to Paris with claims that 'one cannot fail to attribute to it [Kerguelen] the mildest and most felicitous climate... . All that the eyes have been able to see is intersected by woods and greenery, which seems to indicate a country that is inhabited and carefully cultivated (Dunmore 1965:212).' The fabled southern continent which Gonneville was supposed to have discovered in 1504, and which had fuelled much speculation and exploration ever since, could now be claimed as 'Southern France'. Kerguelen's bubble was burst by Captain Cook. He visited the desolate isle at Christmas 1776. He found nothing but penguins and an insular rather than continental status.

My first encounter with Kerguelen Island was with Rhys Jones in 1987. We were sheltering from the rain in the British Museum and found ourselves by chance in a small exhibition *Views of the past: drawing as a record of place*. The picture that held both our attention was by John Webber, the artist on Cook's final voyage. It was a small watercolour of *Resolution* and *Discovery* in Christmas Harbour, Kerguelen (Catalogue number Additional MS 15513, f3 British Museum). Penguins rather than people greeted the boat's crew as they pulled up onto the beach. Desolation was all around them.

We were drawn to this picture because at the time the great interest in Australian prehistory was in the uncovering of the rich prehistory of Southwest Tasmania. The proposal to drown this uninhabited region with lakes for hydro-electricity had been overturned by an effective lobby of archaeologists, environmentalists and Tasmanians re-discovering a heritage they had forgotten about for over 10,000 years. Comparisons, also based on latitude, were being made to establish to UNESCO's satisfaction the world heritage significance of the discoveries. Attention was drawn to the richness of the Tasmanian sites and their contemporaries in the late glacial Pyrenees; southern France (Jones 1992:755 my emphasis):

> When I first encountered Kutikina cave under its dense rainforest cover in March 1981, I was strongly reminded of caves of a similar ambience and antiquity on the other end of the globe in inland valleys of the Gower Peninsula in Wales. *Viewed from the perspective of the Pleistocene archaeologist, there is a fundamental unity to the history of humankind*. At 14,000 years ago, the way of life of these palaeo-Tasmanians, in terms of subsistence, technology and social scale, must have been similar to those of their contemporaries in western Europe

Happily in this instance the reports which went back to Paris were both accurate and justified.

THINKING IN PALAEOCONTINENTS

Archaeologists have been great contributors to the creation of continents. We may not have discovered or named Pangaea or Gondwanaland but we have peopled Beringia, Sunda and Sahul. We have even named our own continents, or bits of them, as Bryony Coles (1998) has done to great effect with Doggerland, now covered by the North Sea. Archaeologists have realised their dreams, unlike explorers from an earlier era fired-up to find Gonneville-land, by keeping their heads in caves rather than in the clouds.

No better example can be put forward than the exploration of the archaeological potential of Sahul. This has been one of the great archaeological achievements of the second half of the 20th century and for which Rhys Jones, among others, must take the credit. The contributions to common knowledge are well known. They include the breaking of the Pleistocene barrier, the discovery of occupation across the full range of environments, the human creation of distinctive landscapes with selected flora and fauna, the isolation of Tasmania and the intensification of subsistence, but not society, throughout Papua New Guinea. Prehistorians elsewhere, who bothered to raise their eyes from the concerns of their own regions, were presented with much to explain because it challenged their parochial concerns. The legacy of the 19th century, the progressive technological evolution of Lubbock and later Sollas, was not so much overturned by the Australian evidence as steam-rollered. Hence in Rhys Jones' claim about unity, which I emphasised in the quote above, we should perhaps substitute Sahulian for Pleistocene archaeologist.

A very large part of this unified perspective on humankind, which has been as much a trademark of Rhys' career as his flat cap and Welsh quotations, stems from the imagined continents of Sunda and Sahul which geographically frame the immediate study of Australian prehistory. They have provided ample scope for broad thinking and placed archaeological evidence in the widest and most relevant of contexts, notably human evolution. Although Sahul only covers the last 60,000 years of hominid prehistory, and for some even less (O'Connell and Allen 1998), it has an importance far beyond this small slice of time. For me that importance is a readiness to imagine a different geography for human evolution, summed up in the name Sahul, and which is a way of thinking that Pleistocene archaeologists working in Europe, Africa and Asia generally lack. As a result they could learn more from Sahulian prehistory than new facts. One aspect could be the call for 'imaginative interpretation' which Rhys Jones made at the end of his edited report on the archaeology of Kakadu National Park (Jones 1985a). He used Coleridge's entrancing fantasy poem Xanadu, the stuff of dreams, to make his point. Following this lead I want to take the rest of this paper to extend Sahulian thinking to the rest of the world.

LOST CONTINENTS AND MISSED OPPORTUNITIES

A global perspective is the place to start. The question I wish to examine is the link between hominid dispersals, social evolution and speciation. If, as we all now believe, a global scale of analysis is the only appropriate frame for investigating such questions then what sort of continental scale geography does this suggest?

There have of course been earlier attempts to position human evolution in imagined continents. The influential German biologist Haeckel (1876) identified the cradle of humankind as the tropical continent of Lemuria beneath the Indian Ocean. His map of migrations showed 12 modern races originating in this now sunken spot. Although the lost continent of Mu still provides a happy home for the lunatic fringe, Haeckel, to his credit, revised his ideas in the light of fossil evidence, principally the finds of *Pithecanthropus* in Java by Dubois in 1891. Asia then became the preferred cradle with the sparsely populated roof of the world in Tibet a hot, sub-continental contender (Matthew 1915), until toppled by archaeological and fossil evidence from sub-Saharan Africa.

This tradition of imagined continents hosting the cradle for humankind has rather fallen out of fashion. They served their purpose, like Kerguelen Island for the fabled southern continent, when there was little or no data. Geography too has come to use other metaphors than those which link distance to deep time through an appeal to remoteness. Today we are all watchers of the wiggle curves from the deep sea record where pulses between ice and ocean bring Beringia and Sahul into being.

This is all very understandable. Science, of which archaeology is a part, has replaced supposition with classifications based on evidence. The price which has been paid however is the rather clumsy way in which the modern map is used to describe processes which took place up to 6,000,000 years ago. Hence such terms as sub-Saharan Africa describe a big piece of a very diverse continent and one, moreover, where the boundaries to the northeast are far from clear. As a result the common depiction of hominids leaving Africa at various times has them habitually passing through the bottleneck of the Near East before arrows take them to the limits of the *ergaster/ erectus/ heidelbergensis* and finally the *neanderthalensis* world.

These geographical pinch-points focus the discussion and often define in a rather parochial and deterministic way what needs to be explained. For example, did populations prior to *H. sapiens* cross the Straits of Gibraltar? Moreover, was there an early sea crossing from Sunda in the direction of Sahul? This is a possibility which for some has now become fact with the discovery of Lower Pleistocene age artefacts on the island of Flores between Sunda and Sahul (O'Sullivan et al. 1998).

While this is a highly significant discovery it might currently be compared at a global scale of reckoning to Leonardo da Vinci's helicopter. An interesting foretaste of things to come but at the time not something which changed the pattern of either world prehistory or history. If it had then the prehistory of Sahul should also be Lower Pleistocene in age and Firenze would have bombed Roma. But this will not stop the discoveries on Flores from being hailed by some as momentous in the history of technology and human evolution.

My point is that we are letting the circumstances of local geography set the agenda of hominid evolution. While the emphasis is on crossing water, passing through gateways and overcoming barriers such as deserts we turn hominid evolution into a series of events and lose sight of the process which it involved.

MODES AND AUSTRALIA

The interest in the Flores evidence is part of a wider debate, in which Australian evidence is important, concerning the appearance of modern humans. A good deal of interest has revolved around using Grahame Clark's (1969:31) stone tool Modes to identify the archaeological signature of *H. sapiens* and earlier hominids. The two earliest Modes, 1 (chopper tools and flakes) and 2 (bifacially flaked hand-axes), have been used to describe at least two separate dispersals out of sub-Saharan Africa (Carbonell et al. 1999) linked to different hominid species. Neither Mode exists in Sahul, despite the fact that Clark (1969:31) did class the earliest tools in Australia as Mode 1, as Lubbock and Sollas would probably have done before him. However, Mode 3 (flake tools from prepared cores) does, according to Foley and Lahr (1997:fig. 4.G), whereas Modes 4 and 5 (blades and microliths respectively) initially do not. These last two modes describe the European and African Upper Palaeolithic/Late Stone Age/ Mesolithic.

The attraction of the Modes is that they can be used to tackle variation on a global scale over large time spans (Foley and Lahr 1997:11). Foley and Lahr recognise however that they are not suited to investigate many other archaeological projects because they are too coarse grained either to examine local level adaptation or, most importantly, variation within relatively homogenous technologies.

I think the objections are even more fundamental. Clark's Modes, while seductive for global thinking, leave out too much. They privilege stone tools above aspects of landscape and resource use, social organisation, non-stone technology, living space arrangements; in fact almost everything that archaeologists, and particularly those from Sahul, have been striving to document as significant areas of variation between hominids in the last 30 years. This is too high a price to pay for bringing the evidence from anatomy, genetics and archaeology into much closer chronological agreement for the speciation of *H. sapiens* at ca. 250,000 BP, the date when Mode 3 is thought to first appear.

Furthermore, concentrating on only five modes clearly changes what should be an examination of the process of dispersal into a catalogue of discrete episodes. As shown by Carbonnell (et. al. 1999) five modes can become five major dispersal events of which they discuss the first two. Lahr and Foley (1994) are surely correct in their argument that the process by which hominids eventually became a global species was instead one of multiple dispersals. However, linking technological Modes to fossil species, even the creation of one (Lahr and Foley 1998:157) for which no widely agreed fossil record exists (*H. helmei*), is not the way to demonstrate their case. The result is that hominid speciation becomes analogous to geographical pinch-points such as the Straits of Gibraltar and the barrier of the Sahara.

What I would suggest is that the study of hominid dispersals is currently poorly served by the geographical framework we use. It is simply too literal for the time frames and processes involved. This is shown graphically in the numerous maps we produce of pinch-points, barriers and arrows of migration. We also argue the relative merits of short and long chronologies for initial occupation of all the major political and natural divisions which interest us. This is an exercise which invariably boils down to a modern continent by modern continent comparison even though these may not be appropriate units because they have never been adequately specified. Furthermore, when it comes to unpicking the issue of how many dispersals, then these same geographical divisions are equally unhelpful. Everyone it seems had to pass across the Bab-el-Mandeb Straits and through the narrow Levantine corridor. Instead of asking 'through where?' as though this was the only specific question we can answer about hominid dispersal, I would rather investigate instead, dispersals 'within which?'.

ARCHAEO-CONTINENTS FOR ARCHAEOLOGISTS

My solution is to revive the tradition of imagined continents. However, this time around they will be based on evidence rather than guesswork and designed to assist the investigation of the process of hominid dispersals and the globalisation of H. sapiens. This can be done with a minimum of three imagined archaeo-continents: 6,000,000 years to 2,500,000 years – Oldwania, 2,500,000 years to 100,000 years – Neowania, 100,000 years to the present – Globowania.The boundaries between the archaeo-continents are both chronological and distributional. For present purposes their limits are drawn with a very broad brush. Here I will concentrate on Neowania since its boundaries are instrumental in defining the other two. The lower limit of 2,500,000 years is pegged to the oldest current age for stone tools (Semaw et al. 1997). This date might be revised up or down and should not be taken to imply that hominids in Oldowania did not have technology. Currently 2,500,000 years also includes the speciation of all early *Homo*, while fixing the upper limit of Neowania at 100,000 BP also includes all later *Homo* as well.

Oldowania includes sub-Saharan Africa, the Arabian peninsula, the northern extension of the Rift Valley and the Plio-Pleistocene lakes of the Sahara. The speciation of *Ardipithecus*, the Australopithecines and whatever *H.habilis* should now be called took place in Oldowania.

Neowania encompasses all of Oldowania and adds the temperate grasslands of the old world which surround the Tibetan massif and the tropical regions of India and Southeast Asia. All four widely recognised speciation events of *Homo, H. erectus/ergaster, H. heidelbergensis, H. neanderthalensis* and *H. sapiens* took place in Neowania. The Lower Pleistocene occupation of Flores represents a clarification of the boundaries of Neowania.

Globowania excludes only Antarctica and the Polar icecap. No hominid speciation took place but for the first time in hominid prehistory a single, phenotypically diverse, global species emerged. Sahul is a part of Globowania.

The purpose of such archaeo-continents is to provide the appropriate scale of analysis for speciation and dispersal history (see Gamble 2001 for further discussion). Hominids did not colonise Neowania for the simple reason that they were already within it. At a Neowanian scale dispersals were occurring all the time. This was the world 'within which' the process of multiple dispersal took place. Furthermore, it was at a Neowanian scale that speciation took place. Let me be provocative. The differentiation of *Homo* that we see in Neowania would not have occurred at the geographical scale of Oldowania. The single hominid of Globowania was a product of further scale changes.

Obviously within Neowania there were speciation centres, regions where allopatry and bottlenecking occurred and away from which hominids radiated. But these centres are not the point of issue in the global view of the evolution of humankind and that unity to our prehistory which Rhys Jones has argued for. Clearly, if hominids were dispersing they were no longer either local or regional creatures. Instead we have to understand them as adapted to a much larger scale and range of environments and processes. These adaptations were appropriate to the archaeo-continent they inhabited.

AN ACHEULEAN EXAMPLE

Let us take the Acheulean, distributed from South Africa to Southampton and from Dartford to Delhi, as an example. Throughout this geographical range similarity in bifaces (handaxes and cleavers) as well as patterns of localised land use, control of fire, a mixture of hunting, foraging and scavenging and an apparent lack of campsites are remarkable (Gamble 1997). Moreover, the Acheulean comes and goes during the lifetime of Neowania. It is one of those archaeological projects which Foley and Lahr (1997) acknowledge will not be understood by using Modes. What then explains it?

At a functional level the Acheulean is a good example of transferable, generic, skills. It clearly does the job and many favour meat butchery as the reason behind bifaces and cleavers (White 1995) and for which there is experimental support. Claims that the Acheulean made dispersal possible by providing a technological advantage seem less likely. Neowania is peopled at localities such as Dmanisi in Georgia (Gabunia et al. 1999) by 1,700,000 BP with a pebble tool and flake technology. Moreover, it has been known ever since Movius' (1948) famous line was drawn, that the Acheulean pulls up short within the boundaries of Neowania. Indeed we must conclude that the transferable skills of the first hominids to disperse into Neowania were every bit as good as those associated with the later Acheulean Diaspora. Neither do new items, or rather items which survive for the first time, such as the 400,000 year old Schöningen spears recovered from a non-Acheulean context (Thieme 1999) change the pattern of global colonisation. Technology in a simple functional sense is not the answer. Neither is speciation in a coarse tools and species correlation.

My answer is to return to that unity of humankind which a Sahulian Pleistocene archaeologist possesses. The context within which dispersal, irrespective of how many, takes places is both geographical *and* social. Hominid dispersals whether they were within Neowania (such as the Flores evidence) or within Globowania (such as the Kakadu and Mungo evidence) were parts of social processes. Technology, whether it is bifaces or boats, is only part of the answer, either why? or when? Instead, what we see between the occupational histories of the archaeo-continents is the different social engineering of distance (Gamble 1993).

In the same way, what is different about the Acheulean lies in the social technology it quite literally embodies. Therefore I can see good reasons for regarding the Acheulean as a long-term landscape signature of more frequent dispersals over a long period within Neowania. Multiple dispersals at variable frequencies also took place in Oldowania and Globowania but I would not expect the signatures to resemble those of Neowania anymore than I would expect the fossil hominids to look the same.

CONCLUSION

Behind the unity of humankind as seen by a Pleistocene archaeologist working in the Sahulian segment of Globowania lies a social context for explanation.

What better example to make my point than Rhys Jones' (1985b) elegant comparison of the planned 'order' of Canberra, its regimented boulevards and artificial Lake Burley Griffin, with the untouched 'wilderness' of the Gidjingali people who hosted his research on the Blyth river in northern Arnhemland. As viewed by Rhys' friend, Frank Gurrmanamana, Canberra's neat lawns and fenced properties are nothing more than 'a wilderness of primordial chaos' (Jones 1985b:207). Technology counts for very little in such perceptions. Rather it is the social relationships and the principles of stewardship which the land requires which determines the different perspectives. Both landscapes possess order to those who constructed them. Both are wildernesses if you stray outside.

The three imagined continents presented here are intended to encourage us to remove the fences and let the lawn grow a little in the study of why hominids became global. I believe we need a phase of exploring our hard won data rather than merely feed it to traditional mills. We are, uniquely, a global species and we became one because somehow we transcended the million year long constraints on conducting social life at a remote distance (Gamble 1999). That ability alone unites us as much as the caves of Southwest Tasmania and a watercolour of desolation painted over two hundred years ago.

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People, Land, Fire and Food: Comments on Two Jonesian Themes

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IN AUGUST 1972, ensconced in the splendid isolation of the Wenner-Gren Foundation's castle at Burg Wartenstein in Austria, my conversation with John Pfeiffer, who had by then started work on his book *The Emergence of Society* (Pfeiffer 1977), turned to the role of fire in prehistory. As a former student of Carl Sauer, I was familiar with the contentious issue of how far aboriginal burning had fashioned the grasslands and woodlands of North America and other continents. But, John asked, had I seen Rhys Jones' 1969 article on 'Fire-stick farming'? I confessed I had not. Nor, on first hearing of it, did I much like the teasing title. Why, I thought, distort the accepted meaning of farming so cavalierly by applying it to the continent of hunter-gatherers (see also Anderson 1990:64)? But that was before I had met Rhys and learned to appreciate both his constructive iconoclasm and the power of his prose!

Three years later, during the 13th Pacific Science Congress in Vancouver in August 1975, for which Jim Allen, Jack Golson and Rhys organised the 'Sunda and Sahul' symposium (and to which I brought the sad news of Carl Sauer's death four weeks previously), I came to appreciate another dimension of Rhys' intellectual and practical world, his ethnographic work, with Betty Meehan, among the Anbarra people of the Blyth River on the northern coast of Arnhem Land. This was an especially fruitful encounter for me, because what Betty and Rhys recorded about the Anbarra community's use of plants and animals parallelled closely much of what I had been able to learn about traditional subsistence activities during fieldwork in 1974 among the Aboriginal peoples of the eastern Cape York Peninsula and the western Torres Strait Islands (Harris 1977).

In 1975, too, I was nurturing plans for an international symposium that would bring together natural and social scientists to examine how human populations had adapted to, and exploited, tropical savanna environments worldwide. I was determined that the human experience in Australian savanna environments should not be overlooked, and who better to turn to, to ensure that it wasn't, than Rhys? Happily, the Wenner-Gren Foundation agreed to sponsor the symposium, and in August 1978 we gathered at Burg Wartenstein for a week of intensive discussion. Rhys accepted the invitation to come 'north of Wallace's Line' and he contributed a key paper on hunters in the Australian coastal savanna to the resulting book (Jones 1980). Nor was this his only contribution. One evening at the castle was enlivened by his ritual execution at the hands of Desmond Clarke (Fig. 1).

Figure 1. The ritual execution of Rhys Jones by Desmond Clarke (centre), assisted by Norman Hammond (left) and Neville Dyson-Hudson (right) who holds a receptacle to receive the head; Burg Wartenstein, August 1978 (photo, the author).

I have begun with recollections of Rhys outside Australia because I wish especially to emphasise his international contribution to hunter-gatherer studies. But his work has been so wide-ranging, from megafaunal extinctions to hunter-gatherer ideology, that in this short appreciation I can only comment on a couple of themes. I have chosen two that are closely linked and have particularly influenced colleagues beyond as well as in Australia: fire and the food quest.

THE BURNING QUESTION

The significance and ubiquity of the deliberate burning of vegetation by hunter-gatherers was largely overlooked by anthropologists and archaeologists until the second half of the 20th century. But from the 1950s onwards the burning question, smouldering until then, burst into flame. Foremost among early contributors to the debates about the role of fire were Carl Sauer (1952:54–6, 1956:10–18) and Omer Stewart (1956). Sauer's emphatic views on the ecological impact of burning by hunter-gatherers and early farmers stemmed from his many years of fieldwork among rural communities in North and Middle America, whereas Stewart's contribution was the result of a comprehensive survey of ethnographic and historical evidence for the early use of fire, which led him to conclude 'that fire has been used by man to influence his geographic environment during his entire career as a human' (1956:129).

Twelve years after the appearance of Stewart's paper Rhys published his first thoughts on the impact on the Australian landscape of burning by Aboriginal people (Jones 1968:205–11). He devoted the second part of this landmark paper to a discussion of the impact of humans on their environments, in which he focussed on two topics to which he would return later in his career: the extinction of the Pleistocene megafauna, and fire, which he described as 'Aboriginal man's most effective tool for changing his environment' (1968:205), thus echoing Stewart's (1956) title: 'Fire as the first great force employed by man'. Rhys referred to Stewart's paper in his own, went on to cite historical and contemporary references to Aboriginal use of fire-sticks to carry and set fire, and in the following year encapsulated his view of Aboriginal burning of vegetation in the now-famous phrase 'fire-stick farming' (Jones 1969; see also Bowman et al. this volume).

It might be supposed that Rhys' early awareness of the capacity of hunter-gatherers to modifiy their environments by fire derived from his undergraduate days at Cambridge under the tutelage of Grahame Clark and Eric Higgs; but although both are thanked at the end of his 1968 paper 'for influential conversations … in the past', and two of Clark's early works are mentioned in the paper, this seems not to have been the case. In fact, neither Clark nor Higgs paid much attention to landscape burning by hunter-gatherers, and it was apparently when Rhys began working in Tasmania in the mid-1960s that his interest in the role of fire was ignited. When we met in London in October 2000, he recalled how, in particular, the work of the botanist W. D. Jackson on Tasmanian vegetation (1965, 1968), and also that of the geographer K. B. Cumberland on the Maori's use of fire in pre-European New Zealand (1962:162–4), had helped him to appreciate the role of seasonal burning by the Tasmanian Aborigines in maintaining grassland and open-canopy woodland. This realisation was also prompted by his reading of early explorers' accounts (e.g. Péron 1809:186) and, especially, of George Augustus Robinson's early 19th century journals (Plomley 1966), with their frequent observations of the deliberate burning of vegetation and the Tasmanian's use of fire sticks. Rhys' views on the role of Aboriginal burning in modifying the landscapes of Tasmania were further reinforced by his familiarity with Betty Meehan's 1965 BA thesis (Hiatt 1967–68) which reviewed the historical and ethnographic sources dealing with the food quest and economy of the Tasmanian Aborigines (see Meehan this volume).

When in July 1972 Rhys embarked with Betty Meehan on their year-long residence with the Anbarra, he seized the unique opportunity thus offered to learn how they traditionally regarded and used fire. The experience of sharing their perceptions of how their country should be managed ('cleaned up') by fire reinforced his conviction that Aboriginal burning had greatly increased fire frequency and had shaped and maintained many Australian plant communities over much of the continent, perhaps throughout the millennia since initial human colonisation. He later incorporated this interpretation of the role of fire into his famous 1975 paper with its provocatively paradoxical title 'The neolithic palaeolithic and the hunting gardeners: man and land in the Antipodes' (Jones 1975:25–7); and in his contribution to the 1978 savanna symposium at Burg Wartenstein he described the fire regime of the Anbarra and commented on how the imposition of a policy of fire prevention in the late 1960s had disrupted their 'pyro-management' and resulted in the sporadic eruption of gigantic wild fires (Jones 1980:124–5).

Rhys' interpretation of the relation between Aboriginal fire regimes and Australian vegetation has not, however, gone unchallenged. His principal adversary in debates on 'the burning question' has been David Horton, who in 1982 published a rebuttal in which he argued that Aborigines observed and made use of, but did not fundamentally alter, natural Australian fire regimes (Horton 1982). Horton has recently (2000: 70–101) reiterated his attack on the concept of fire-stick farming, questioning in particular whether early European observations of vegetation burning by Aborigines justify generalisations about the extent and antiquity of its impact on Australian environments.

The main reasons why the burning question remains unresolved is the difficulty of obtaining direct evidence for the incidence and extent of past burning, and the problem of distinguishing decisively between the results of naturally and culturally induced fires. Charcoal particles preserved in sediments, and fire-scarred annual rings of long-lived trees, can provide data on past fire frequency, but they cannot tell us incontrovertibly whether the fires from which they derive occurred naturally or were set by people. This problem has recently been addressed by Lesley Head in her assessments of palaeoecological, biogeographic and ethnographic evidence for fire and vegetation change in northern Australia (Head 1994, 1996). She focusses on the Holocene, for which there is more substantial evidence than for the Pleistocene, and argues that Aboriginal burning had greater impact on vegetation in the late than in the early-mid Holocene when climate was less variable. She suggests that the ethic of 'cleaning up the country' with fire, which is widely shared by Aboriginal people in northern Australia today, is likely to be 'at least three thousand years old', and she does not reject the possibility that it 'emerged in the disequilibrium of the Pleistocene' (Head 1994:179; see also Bowman et al. this volume).

The vigour with which the burning question continues to be investigated and debated in Australia, and the extent to which the Australian case has influenced studies of environmental change and of hunter-gatherers elsewhere in the world (e.g. Bell and Walker 1992:154–5; Lewis 1982; Mellars 1976:1–16), stems from the spark Rhys ignited in the late 1960s. That spark has long since been fanned into a flame that is still spreading, generating as it does both heat (controversy) and light (new knowledge).

PEOPLE, LAND AND FOOD

When Rhys first arrived in Australia in August 1963, he was already familiar with both Grahame Clark's concept of 'economic prehistory' and Eric Higgs' of 'palaeoeconomy'. In prehistoric archaeology the ecological paradigm was in the ascendant, especially in the study of hunter-gatherers and the origins of agriculture (e.g. Binford 1968; Flannery 1968; Harris 1969; Higgs and Jarman 1969). In his early research in Tasmania Rhys combined his archaeological survey and excavation data with historical evidence and ecological parameters to reconstruct pre-European subsistence patterns and population distributions (Jones 1968:207–10, 1971). The conclusions he reached about tribal territories and band estates in relation to group size, population density and seasonal movement made a major contribution to the understanding of tribes and boundaries in mainland Australia (Jones 1974; Peterson 1976:62–3), and their international relevance is evident in his comparison (Jones 1977:367) of the Australian tribal territory with Higgs' concept (1975 and earlier works) of the 'annual territory'. I can testify also to the pedagogic value of Rhys' Tasmanian data, having used them for many years when discussing hunter-gatherers in my undergraduate courses in London on 'resources and subsistence'.

Despite the originality and elegance of Rhys' reconstruction of territoriality, demography and the food quest in Aboriginal Tasmania, he must have remained acutely aware of the interpretative limitations of the island's archaeological and historical record. So when he and Betty Meehan had the opportunity to work among the Anbarra in Arnhem Land, and to observe at first hand their hunting, fishing and foraging activities through an entire year, it must have seemed like the chance of a lifetime. The uniquely detailed record of these activities that they were able to compile through the dry and wet seasons of 1972–73 (Meehan 1977, 1982; Jones 1980) revealed just how complex and subtle were the links between the Anbarra and the land that traditionally sustained them. Rhys' and Betty's analysis of Anbarra livelihood has few parallels in the literature on hunter-gatherer economies and it offers a cautionary lesson for any prehistorians who base their interpretations too narrowly and confidently on the material remains that happen to have survived archaeologically.

Since his year with the Anbarra - which must surely have been a transforming experience for Rhys the prehistorian - he has moved still further towards an integrated understanding of Australian Aboriginal life. In his 'Hunters of the Dreaming' (1990) he succeeded brilliantly in relating his knowledge of Aboriginal systems of production, first fostered by his work in temperate Tasmania and later matured by his experiences in tropical Arnhem Land, to the underlying ideology of Aboriginal life. Acknowledging that 'reductionist economic analysis is not sufficient' (1990:28), he returned to the themes of territoriality, land use, population and seasonality, but placed them in the enfolding context of an ideology that perceived the landscape and its plants, animals and people as being ordered, humanised and sustained through the medium of complex ritual. At the end of the paper, as in several of his earlier publications (e.g. 1977:6, 1985:1), Rhys drew a parallel with Palaeolithic western Europe, specifically here with the Aurignacians and Magdalenians. I especially welcome his conclusion that hunting and gathering societies 'were not of necessity on some track to a "higher" social or economic life ... in particular agriculture' (1990:49), an assumption that regrettably still colours much thinking about the origins of agriculture.

In this brief appreciation of Rhys' place in the twentieth-century discovery of Australian prehistory, and of the influence of his work outside the continent, I have done no more than touch on two themes in the Jones canon. Beyond these there is much more, but above all the man himself: full of energy, imagination and intellectual independence, one who (to steal and mangle one of his own quotations) is fascinated by Diderot's (1796) question 'Who knows the early history of our Globe?', and who has 'amused' himself (and we his readers) 'in this research' because it 'suits' him (Jones 1977:1).

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Kunj-ken Makka Man-wurrk
Fire is for Kangaroos: Interpreting Aboriginal Accounts of Landscape Burning in Central Arnhem Land

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IN 1969 Rhys Jones published a classic paper simply entitled 'Fire-stick Farming' where he argued that Aboriginal people had intentionally modified the Australian landscape with fire. An indication of its great impact is that despite being 30 years old, the paper continues to be controversial and challenging. Indeed, not only has Jones' idea influenced debates about appropriate land management but also it has shifted into the legal and political arenas (Hughes 1995; Langton 1998).

Scientifically speaking the concept of 'fire-stick farming' remains hypothetical because it is based upon a diverse collection of information, much of which has been gleaned from historical sources such as the writing of colonial explorers. The rapid transformation of Aboriginal hunter-gatherer economies throughout the 20th century drastically restricted the opportunities for anthropologists and other researchers to directly observe 'fire-stick farmers' at work. Researchers in the second half of the 20th century could only observe fire usage by Aborigines whose lifestyles had, to varying degrees, been influenced by Europeans. Significantly these 20th century descriptions show concordance with the 19th century accounts of Aboriginal landscape burning (Bowman 1998).

There is a general absence of ecological data to test the 'fire-stick farming' hypothesis (Gould 1971; Bowman 1998; Andersen 1999). Indeed, ecologists have been slow to take up the challenge set by Jones' beguilingly simple paper. However Bowman (2000) is endeavouring to remedy this situation by studying Aboriginal landscape burning in central Arnhem Land. The primary approach is to undertake a 'natural experiment' contrasting unoccupied and occupied areas.

His research project employs a range of different ecological methodologies including:
1. analysis of fire scar patterns registered on satellite imagery over a 10-year period;
2. comparing vegetation at sites surrounding Aboriginal outstations with ecologically similar but unoccupied areas and;

3. changes in the extent of fire sensitive rainforest pockets determined from sequences of aerial photographs.

In addition, Bowman's landscape ecology studies are bolstered by some traditional Aboriginal owners' oral testimonies about Aboriginal fire management. Recent publications (e.g. Head 1994; Lewis 1994; Rose 1996; Russell-Smith et al. 1997; Hill et al. 1999; Yibarbuk 1998) have recorded a range of Aboriginal knowledge about fire usage garnered from practical experience, retained in memories from their childhood or passed down from their parents and grandparents. The documented voice of Aboriginal people is extremely valuable because, as Haynes (1991:63) has argued, 'it illustrates discernment, discrimination and order in the culture of Aboriginal burning: it provides us with clues for investigating further the process of Rhys Jones' now well-known phrase 'fire-stick farming''.

The purpose of this article is to report interviews with Aboriginal men in central Arnhem Land recorded during Bowman's field research and to compare this information to similar research conducted in the Australian monsoon tropics, where most enquiries into Aboriginal landscape burning have been carried out. In the light of our results we then undertake a more general discussion of 'fire-stick farming'.

ARNHEM LANDERS TALK ABOUT FIRE

Below we summarise conversations we tape-recorded with four Aboriginal men in central Arnhem Land who were interviewed about landscape fires during the dry season of 1999. All live on outstations in central Arnhem Land: Mick Kubarkku and Djungkidj Ngindjalakku live at Yikarrakkal outstation, Big Bill Birriyabirriya at Marrkolidjban outstation and Jacky Bun.ganiyal at Korlohbidahdah outstation. They have maintained strong links to their traditional lands despite the regional upheavals triggered by Europeans through the second half of last century (Yibarbuk et al. 2001). These men represent a cross-section of ages: Jacky Bun.ganiyal is the oldest and Djungkidj Ngindjalakku the youngest, with Mick Kubarkku and Big Bill Birriyabirriya from the intervening generation. The men agreed to share their knowledge of fire as a contribution to a current project on fire management in western Arnhem Land. Jacky Bun.ganiyal spoke in Rembarrnga while the other men chose to speak in Bininj Gun-wok (Eastern Kunwinjku dialect). The Bininj Gun-wok and Rembarrnga interviews were conducted on separate occasions and were informal and unstructured with Bowman as ecologist and Garde and Saulwick as linguists translating Bowman's questions into Bininj Gun-wok or Rembarrnga. The responses were in Bininj Gun-wok or Rembarrnga and were then translated into English for Bowman's benefit. The interviews were conducted at the outstations where the men live and were carried out in the presence of their relatives.

The primary focus of the questions concerned the effect of landscape fires on vegetation and food plants. Garde transcribed the Bininj Gun-wok recordings into English, while Saulwick transcribed the Rembarrnga ones. The complete transcripts will be lodged with Institute of Aboriginal and Torres Strait Islander Studies.

Abridged transcripts and interpretation

It is recognised by all the men that the absence of Aboriginal people in the landscape, and hence the cessation of traditional burning, results in degradation of the bush because now intense fires periodically burn such unoccupied country. The men recognise that fires lit late in the dry season are also difficult to control because they are intense and can burn through the night. Such intense fires are considered dangerous and uncontrollable.

Big Bill Birriyabirriya: *Mm. Bu ngarri-wurlhke man-wurrk maitbi* half-way stop *ka-yime.* All day *ngarr-dalkbawon maitbi man-djewk, mani man-djewk, nother' man-djewk ka-wurlhke manekke kun-rak Maitbi ka-rung kurrambalk ka-djale ka-bebme kure djarre.*
'Mm, when we burn here the fires burn out quickly. If we don't burn the grass for a long time, year after year well when another year we burn it, the fires are very big. Maybe houses will get burnt and it'll burn for a long distance.'

Such intense fires are known to be able to kill trees.

Garde: *Yika bu kabirri-wurlhke man-wurrkkimuk maitbi birri-bawong mandjewk man-wern wanjh ka-wurlhme?*
'What happens sometimes if they burn and then don't burn for many years and then light a fire again?'

Djungkidj Ngindjalakku: *ka-birlikimukmen.*
'The flames will be enormous.'

Garde: and *ka-bun kun-dulk?*
'and it kills trees?'

Djungkidj Ngindjalakku: *Yoh kun-dulk ka-bun.*
'Yes, it kills trees.'

Garde: *Yika yi-nang? Konda larrk?*
'Have you seen this? Maybe not here?'

Djungkidj Ngindjalakku: *Laik ka-bun, manu kabirri-bawon laik, ngarri-bawon,* and *dalkraworren, ngarri-wurlhke wardi ka-bun kun-dulk nungkah.*
'It kills the trees, when they no longer burn, and the grass builds up and when we burn it can kill the trees.'

It was understood that without regular fires fuel would build-up along creek lines. If burnt in the late dry season, these build-ups would act as corridors for the spread of fires.

Big Bill Birriyabirriya: *djarredjarre kurralk, kun-dalk ka-wurlhme manih* grass. *Bu man-ngurlu karri-wurlhme wanjh ka-re kun-dalk ka-wurlhme o njalengeybun, karri-ngeybun kun-kod, kun-kod ka-rung wanjh ka-rrolkan maneke and ka-wurlhme kun-dalk.*
'[Fires spread] ... a really long way, the grass is what burns. When the stream line deposits [of debris] burn then it spreads to the paperbark (*Melaleuca* forests) there also and then it leaps up and onto other areas of grass.'

In areas where Aboriginal people were still burning their country the men expressed a relaxed attitude about the effect of fire on vegetable resources.

Jacky Bun.ganiyal: *Nœndama barrwarnalerrhminy mun'gu barrlerrhminy barrppuh matjjih. Ya mahgun ørdorlhmœ.*
'They were lighting fire and yes, it [plants] used to emerge.'

Jacky Bun.ganiyal: *Walang borloh ørdirditjmœ jubul.*
'Then many plants would be returning.'

Jacky Bun.ganiyal: *Jalma gukku matjjih naganh dawh naganh yirrppœgan bandawœ ganura.*
'Brown yam, long yam and 'dawh' from the rock country right around here they are found.'

Under traditional fire management practices, yam beds that had been dug up in the early dry season were usually burnt after the harvest. Aborigines planted the top of each yam they harvested and these were known to resprout following the onset of the wet season.

Garde: *Minj kabirri-wurlhke bu karrbarda-ken?*
'Fire is not used for yams?'

Mick Kubarkku: *Minj kabirri-wurlhke lamed [>la+ngamed] kabirrih-karung, ... man-dalk-kuk.*
'They don't burn, they dig them up .... the vegetation is wet [where yams grow].'

Garde: *Aa?*
Mick Kubarkku: *Man-dalk-kuk dalkno.*
'The grass there is wet [fire won't burn there].'

Garde: *Dalkno?*
'The grass?'

Mick Kubarkku: *Minj ka-wurlhme, ya kabirri-karung man-dalk-kuk.*
'It won't burn, yeah they dig them in moist places where there is green vegetation.'

Garde: *Ka-kirh.*
'It's wet.'

Mick Kubarkku: *Ya. Kabirri-karung wanjh kabirri-barlkei, ... kabirri-wurlhke.*
'They dig them and fill up containers with them, ... they [might] burn off [later].'

Garde: *Nawu karrbarda-ken minj birri-wurlhkemeninj bu?*
But they didn't burn to make yams grow?

Mick Kubarkku: *Ya. Birri-karuyi birri-wurlhkeng. Wanjh madorrhbebmeng.*
'They dug them up [and some time later] they burn off. Then the vine sprouts again.'

Mick Kubarkku: *Yo birri-karuyi nawu birri-djallurlurlhmeninj yimayi. La birri-dudjeyi. Birri-dudjeyi.*
'Yes they would dig them up, uprooting them all. Then they bury them. They would bury [a bit of them].'

Garde: *Njale birri-dudjeyi?*
'What would they bury?'

Mick Kubarkku: *Dedjno.*
'A part of the base of the yam [where the vine meets the tuber].'

Garde: *Birri-bakkeng?*
'They break it off?'

Mick Kubarkku: *Karrbarda yoh birri-bakkemeninj, birri-dudjeyi.*
'Yes, they break it off and plant it.'

Mick Kubarkku: *Birri-dudjeyi kolhdebebmeninj wanjh. Ngarrih-yime ka-kolhdebebme.*
'They plant it and it shoots again. We say it shoots up again.'

Garde: *Kaddum kabirri-dadje?*
'They cut it at the top?'

Mick Kubarkku: *Yoh, kabirri-kurrme kaluk ngalengman kah-kolung kah-kuk-kolung ka-djordmen.*
'Yes, they put it back and it grows down again by itself.'

Garde: *Wanjh birri-yakwong, birri-karuyi and birri-wurlhkeng?*
'So when they finished digging them all, they burned some time later?'

Mick Kubarkku: *Yoh,* [?] *birri-wurlhkemeninj.*
'Yes, they used to burn.'

There was consensus among the men that early dry season fires have no effect on fruit trees. However, fires lit later in the dry season would destroy flowers and hence inhibit the production of fruit.

Big Bill Birriyabirriya: *Kamak*, early, early *bu dry ka-yime, and yi-wurlhke manekke*, but *makka bonj, ka-bebme manu njamed man-nguy, man-nguy wanjh ka-nguydangen yiman* mango. But if late *yi-wurlhke, bu kah-nguydi wanjh kunukka larrk, ka-rung manu man-nguy minj ka-kukdi manme larrk. Yiman djarduk, man-dudjmi en man-dak ey. Ka-rung, minj ka-wukdi larrk minj ka-karrme man-me. Maitbi man-djewk na-buyika ka-kukdangen.*
'It's good to burn the country early in the dry season before these fruit trees flower so that the flowers will be OK just like on a mango. But if you burn when the flowers are already on these trees then the flowers get burnt and no fruit will appear. Like bush apple (*Syzygium suborbiculare*), green plum (*Buchanania obovata*) and *Persoonia falcata*. If they get burnt they won't produce fruit. Maybe a year later they'll fruit again.'

Big Bill Birriyabirriya: *Kurrung kunukka larrk. Kunukka* time *bu njamed yi-bengkan karri-ngun. Kunu wanjh late karri-wurlhke wanjh kunukka ka-rung minj karri-ngun man-me.*
'No, not in the late hot dry season. That's the time for us to eat the fruit and if there's burning at that time, the fruit gets burnt.'

During the dry season it was recognised that in order to maintain supplies of bush-honey it was important to leave areas where flowering plants were unburnt.

Big Bill Birriyabirriya: *Yoh, birri-wurlhkemeninj bu man-kung birri-yawayi birri-wurlhkemeninj but man-wurrk-ken yi-bengkan manih, kun-dulk. Bu ka-bun ka-karrme* flower *mani njamed man-barlanjdjarr and man-burlurddak, wanjh kunukka start bu man-kung.*
'Yes, they'd burn when out getting honey, searching around and they'd burn the place but they wouldn't burn the trees which were flowering, like woolybutt (*Eucalyptus miniata*) and stringybark (*Eucalyptus tetradonta*) trees because that's when the honey season starts [and the flowers of these trees provide the bees with the nectar].'

Various motives for burning were reported. These included clearing areas for access, such as fishing spots, locating small game like goannas and turtles on floodplains, felling trees with 'bush-honey' and clearing areas beneath fruit trees to provide bare ground upon which the fruit could subsequently fall, making it easy to collect. However, the primary motive concerned game. This was succinctly put by Mick Kubarkku.

Mick Kubarkku: *Kunj-ken makka man-wurrk.*
'Fire is for kangaroos.'

Djungkidj Ngindjalakku put a similar point of view in response to the question posed by Garde.

Garde: *Nawu man-wurrk kunj-ken but yirrok kabirri-wurlhke bu man-me ken kun-dulk karri-moyhmang manme o larrk nakka man-buyika?*
'Burning is for kangaroos but do people also burn for the fruit trees so they can collect the fruit or whatever or not?'

Djungkidj Ngindjalakku: *Nakka kunj-ken. Man-wurrk. Manekke.*
'Just for kangaroo hunting. Fire. That's what it's for.'

There are two aspects of the use of fire for game management: to attract kangaroos to areas of grass regrowth and as a hunting tool.

Garde: *Mak nga-bekkang birri-yimeng birri-wurlhkeng wanjh kun-dalk like kun-bule wanjh yimerranj*, and *ka-rralkbebme and kunj ka-kadjung.*
'I've heard that they also burned the country so that kangaroos would be attracted to the new grass shoots which resulted?'

Mick Kubarkku: *Yoh, man-nelk. Manbu man-dalknelk, ka-kolhdebebme. Yoh wanjh kolhdeno ka-kadjung ka-ngun wanjh.*
'Yes, the soft new green shoots. The new grass shoots. Yes, they follow after this to eat it [the kangaroos].'

Garde: *Ka-kolhdengun. Kolhdeno njamed kun-dalk?*
'It eats the new shoots? The new grass shoots?'

Mick Kubarkku: *Yoh ka-rralkbebme, yiman kadni kayime ka-rralkbebme.*
'Yes, when the grass appears, just like when we plant a vegetable garden, the new grass comes up.'

Garde: *Yoh ka-durndurndulubun?*
'It sprouts up everywhere?'

Mick Kubarkku: *Yoh, wanjh ka-re kunj ka-wake ka-ngun, ka-rralk-ngun.*
'Yes, and the kangaroos crawl across, eating the grass.'

Garde: *Kunekke mak, bininj birri-wurlhkeng ba ka-kolhdebebmeninj?*
'So people also burnt the country to make the new shoots grow up?'

Mick Kubarkku: *Yoh, wanjh djurrkmayi kunj kadjuyi bu nguyi.*
'Yes, it would attract kangaroos, they follow it to eat it.'

Mick Kubarkku: *Yoh nguyi wanjh balemdiwirrinj. Balemno nungkahke balemdiwirrinj.*
'They eat it and get fat. The fat of the kangaroo, it would get fat.'

Garde: *Kunukka-ken mak birri-wurlhkeng?*
'So this is also why they made fires?'

Mick Kubarkku: *Ng ng kunekke-ken.*
'Yes, for this reason.'

Similarly Jacky Bun.ganiyal understood that burning produced nutritious feed for kangaroos.

Jacky Bun.ganiyal: *Barrppuh matjjih gurhguwarl øborlhmœ.*
'Until the young fresh shoots come out.'

Jacky Bun.ganiyal: *Ya guwenygan walang øngunœ.*
'Yes for the kangaroo then they eat it.'

Fire drives were a very important hunting technique and on a large scale they could produce large quantities of meat.

Mick Kubarkku: *Kunj. Ngarri-djangkayi, bu ngarri-wurlhkemeninj ba ngarri-barurrimeninj. Ngarri-barurrimeni:nj, bonj* [disfluent] *man-wurrk diwihdiwirrinj. Manekke man-dume rayehrayi. Rayehrayi wanjh bid-karremerrimeninj bonj la ngarri-burldjdjarnmayi bonj. Ngarrih-djal-yameninj kunj. Ngarri-djal-yameni:nj ngarri-yakwoyi kunj kornobolo karndayh, wolerrk la yingalengale. Nakka ngarri-yameninj ngarri-yakwoyi. Wanjh ngarri-yikoluyi dungyibmeninj la ngarrihyi-koluyi ngarrih-kinjemeninj kanjdji. Ngarrih-kinjemeninj wanjh ngarrih-kayi kured, ngarrih-ngorrkayi.*
'We used fire for hunting kangaroos and when we did this we would cover our bodies in ochre. We would cover ourselves in ochre and the fires would burn. They would follow the line of the fire and all co-operate until meeting up together in the middle. We would all spear kangaroos. We would spear them all until they were finished, agile wallabies, female antilopine kangaroos, female euros and whatever. We'd spear them and finish them all. And then we'd go back down carrying them at sunset, carrying the kangaroos down to cook them below. We'd cook them and carry them down back to camp.'

The fire drives varied from large ceremonial gatherings, that rarely occur nowadays, to smaller scale hunts that still form an important part of the part of everyday life on outstations. Fire drives occurred at particular places where very specific tactics were employed to drive macropods and emus to waiting hunters. These tactics factored in the local topography and the prevailing winds. For example, a circle of fire might be placed around a hill where the hunters would lie in wait. Mick Kubarkku at Yikarrakkal talked about the use of fire in narrow sandstone gullies called *man-bambarr* where kangaroos would be driven up a gully towards a group of hunters lying in wait in a cleared area that had already been burnt. Sometimes the hunters would burn an area immediately in front of a large fire set by other members of their party. This was dangerous work with the very real risk of death or serious injury.

Mick Kubarkku: Korroko *manbu ruyi, … bulebuhbuhmeninj la manih manbu kamre manbang nakk' karr-rung, yoh. Ngarr-rung yi-mankan. Kaluk ka-worroworrowkme kuni ka-bolkmelme.*
'The area already burnt, … the ashes had cooled enough to stand on …, but sometimes we would get burnt by a really intense fire, yeah. You'd get burnt and fall down. We'd jump all over the place, stamping the ground there.'

The kangaroos would often stop in these burnt clearings to lick their singed skin making them easy targets for the hunters.

Mick Kubarkku*: Ya, ka-belenghmerren. Wanjh…ngarri-yame nudno.*
'Yes, they lick themselves. Then … we spear them whilst they're wounded with burns.'

Mick Kubarkku: *Yoh. Nawu korroko man-kole nakka nudno nganeh-yameninj. Ngale kari-kurlhwemeninj* ['we dropped heaps of them' and the spear is sticking out']. *Ka-kari-di. Ngale kurlhwemeninj kaberrkyuwurrinj kunj. Ngal' wanjh ngane-ngorrmayi ngaleh kayi. Ya.*
'Yes. Before we used to spear them whilst they were injured. We'd drop heaps of them. There'd be heaps of kangaroos lying all over the place. Then we'd go and carry them away. Yeah.'

Large fire drives allowed groups of people to come together from surrounding clan-estates and formed a large part of ritual life. Smoke signals and the delivery of message sticks and small torches made from the bark of stringbark trees (*Eucalyptus tetrodonta*) would be used to invite people to particular places. The timing of the drives would be determined by the drying out of the fuels during the course of the dry season. Special ceremonies and associated dances and songs would be performed during the fire drives to ensure the success of the hunt.

Mick Kubarkku: *Yoh. Birri-morrdjdjanjkarrmeninj man-wurrk-ken. Morrdjdjanjno kandakidj en nadjinem nungan morrdjdjanjno.*
'Yes, they had those special songs for fire. Each species of kangaroo had one, such as antilopine kangaroo and the black wallaroo.'

Mick Kubarkku: *Yoh. Morrdjdjanjkadjuyi bonj malakakdoyi wurlhkemeninj bonj. Nayi kunj rehrerri.*
'Yes, they would sing *morrdjdjanjno* songs early in the morning and then light the fires. And then you would see the kangaroos.'

It was critical that the areas remained unburned until the hunt was organised. Disputes could arise should somebody burn a hunting ground without appropriate customary law rights.

Mick Kubarkku: *Birri-yimeng 'Na-ngale wurlhkeng?' Wanjh namekke birri-duyi ngarri-durrimeninj wanjh. Ngadberre makkan man-wurrk ngurri-wurlhkeng. Ngarri-yameninj kunj ngarri-djareni. ôYo ngad ngarri-warrewongö birri-yimeninj. 'Ngarri-wurlhkeng'. Wanjh mak birri-durrimeninj yoh birri-yimeninj 'bonj'. Man-wurrk manekke birri-nganenghmeninj ya.*

'They'd say something like 'Who burned off here?' They'd get angry and argue then. 'That fire belonged to us and you burnt our fire. We wanted to spear the kangaroos there. 'Yes, we ruined it for you' they might say. 'We burnt the country there.' Then they would argue. 'That's it' They used to be strong in defending their control over their fire drives.'

Mick Kubarkku: *Bu birri-wurlhkemeninj birri-yamerrinj nakka bonj, bindi-woyi kanjno.*
'If they burnt off someone else's place they'd spear each other and have to give meat to the people whose land they burned.'

Amongst the three older men there was an awareness that the complex system that regulated fire drives was breaking down.

Mick Kubarkku: *Yoh korroko wam. Yo bu wale buyika. Ka-karrme rowk dabbarrabbolk. La nani kabirri-wakwan yawurrinj nawu nanih Mumeka, la Mankorlod makka bu kabirri-wakwan.*
'Yes, already gone. Yes, this was a special technique. The older generations had every kind of skill. The young people today don't know it such as those here, and Mumeka or Mankorlod, they don't know.'

Big Bill Birriyabirriya: *Kunj-ken manekke kunj makka kun-dalk, Mabara* all the place *birri-yimeng minj ngurri-wurlhke la ngarri-djare kunj korroko dabbarrabbolk. Bindi-duyi kobokohbanj bindi-marneyimeninj minj ngurri-wurlhke. Daluk la wurdurd ngurri-marneyime yun mak kabirri-wurlhke.* But no *djurlu* only *kun-djakkorl. Korroko laik Balanda-yakni. Bolkki* anyway *karri-wurlhke. Yoh.*
'Kangaroo fire drives at all the places with grass. Mabara [on the Liverpool River] and all the places where we wanted kangaroos, the old people [earlier generations]. Those older people would get angry and say 'Don't you burn there! Tell the women and children too, don't burn those places.' This was before there were white people here. Today they burn any old way. Yeah.'

Nonetheless, it is clear from the interviews that Aboriginal people still retain an enormous store of knowledge about landscape fires. In general terms the interviews are remarkably consistent with each other. Although most of the information on the everyday uses of fire is consistent with previous published studies an unexpected finding was the importance of large-scale fire drives. The men, particularly the older three, possibly turned the focus of the interviews to this subject because they saw fire drives as the quintessence of landscape burning. The centrality of fire drives in traditional life is well illustrated by the following unsolicited narrative of Jackie Bun.ganiyal about hunting kangaroos before European contact.

Nganyawkka Rembarrngawala am.
'I'm going to speak in Rembarrnga [language].'

Yo mitjjindah wel dumukkaniny.
'Long ago, he used to light hunting fires.'

Ngurah wel dumukkaniny.
'He used to light hunting fires.'

Barl dumukkaniny guweny.
'He used to light hunting fires to get kangaroo.'

Wurrk barl dumukkaniny bi wangginy.
'Grass and wood, one man used to light for hunting.'

Rdurhgaba nihgarnæh walkganæyimany.
'He used to make it go into (the hunting area).'

Rdurlhgabæyironginy.
'He would be lighting fires.'

Nihgarnœh matjjih bi wangginy.
'That one man.'

Wurlga bi rdurlhgabœyuwah.
'The "mantis" man is lighting fire.'

Barryappahremangœttiny bœnda.
'They were getting the animals for themselves.'

Bi jubul barrariya yap.
'Lots of people went.'

Nihgarnœh bi wangginy burltjjarn.
'There was one man in the middle.'

Bi wangginy gagarriny bebe.
'One man to the west, one to the north'

Nihgarnœh bi wangginy walhwalam barraritjyimany.
'One man to the south, they would be looking for kangaroo.'

Guweny barrna jubul bœnda ni.
'They saw many kangaroo there.'

Warrkyi barrguwahjubulwa.
'They go for all (the kangaroo) under the cover of smoke.'

Nattœnda bi malak birrinanœ waba warrkyi.
'They (the kangaroo) don't see those men for the smoke.'

Walang jarljarlyimany jangopjangopyimany barttabordiyinda bima.
'Then the men would be hunting and spearing them with a spear.'

Yawminy yawminy guweny garerdunghrdunghyimany.
'They speared and speared them, the kangaroos' bodies would be dropping and dropping.'

Borlohmiya buwa buwa.
'They took a stick from the river.'

Guwambartiya.
'They hit them on the neck.'

Nyarrminy guhmiya.
'They picked up the carcasses.'

Baganh berdehgah guhbordohminy.
'They carried the carcasses and put them here.'

Garerditjminy jœnœgah.
'They returned to camp with the carcasses.'

Rerditjminy dinghgappul jubul.
'They returned with the animals to all the women.'

Barrani yuganda.
'Then they would sit and relax.'

Guweny ngagorrœ yawminy.
'They speared the kangaroo for us.'

Naganh gaberdeh barrayininy nayukkahgan.
'This is how they carried them, long ago.'

Bi nanh nginda matjjih.
'This man was me.'

Ngaminyji.
'I knew it.'

Nganbajirrahmuttimuttiya dakkuyindah mitjjindah.
'Long ago when I was a child they were showing me how to do this.'

Walang ngaminyji jubul guwa.
'And so I knew everything.'

Nganyawkmœ barrlerrhminy ngurah dinghyih dinghnawœyih wurru jurranynama gayani.
'I was telling how women lit fire and their husbands relaxed.'

Barrppuh walang len guwenyma walang barrnguny.
'After this they would eat the kangaroo.'

Len burlttiya galitjma barrrdarlhminy nœndahna.
'They covered some with honey from the ground and others with bark from trees.'

Malak yiburlttungœ jolkkogah waba.
'We didn't cover it for roasting with earth.'

Barrrdarlminy gadawœh jambarl.
'They cut bark off trees and put them around (hot) oven stones.'

Barrppuh nyarrminy.
'Until it was cooked.'

Barrgomohminy gajahyih garrawala.
'They then covered it with paper bark.'

Malak yemet wangginy yawmœ nattœnda guweny bat jubul.
'They didn't just spear one kangaroo, but many.'

Jubulyi barrarerditjminy.
'They returned with many animals.'

Barttœh barttœh barrrneny guweny barrnguny.
'They roasted and ate the kangaroo.'

Bœnda barrawalangyininy 'gardah!'
'Then they said 'oh!'

Ngarrmanganonga gunborrk guwa ngarralukka ngarralukka.
'They sang a song so they could dance.'

Ngarrmunyilh ngarrhngarkkanyilla guwa.
'So we would freshen our legs (ready for hunting).'

'Ma barrayininy woh,' barrayininy.
''Okay, yes,' they said.'

Ngarrdappamanganong.
'We sang in the afternoon.'

Bih dingh nagunda matjjih narraluk warna yandagappul matjjih biyinda.
'Men, women and you all dance, all of us, and with the men.'

'*Narralukka yanda matjjih marrœyarralukka,' barrayininy.*
"You'll all dance and we'll all dance like us,' they said.'

Bi wangginy manganongminy.
'One man sang.'

Bœrlmœrr motminy wangginyyih gayamanganongminy bœnda.
'One man held the clap sticks, he sang. '

Barrmanganongmiiny barrawalangrderdehminy.
'They sang and sang and danced.'

Barralukminy bi garrhgarrh dingh garrhgarrh burrhgarnœ.
'They dance differently, men and those women, differently.'

Buhma wurrhwurrungungong barrlangaburrhminy langa barrbuwa.
'And all those other old people they clapped their hands.'

'*Woyh,' barrayininy.*
"Come here.' they said.'

Wurrhwurrungugappul.
'All the old people.'

Dinghngong warna bi barralangaburlhminy barrawalangjamgayhminy nœnda.
'All the women and men, they clapped hands.'

Bœndawala bayamungbuwaji.
'Then when we would stop.'

Barrawalangyuwern nah.
'They were lying down.'

Babarra Ngarrarumhminy ya.
'We slept.'

Jordohminy.
'Day broke.'

Malak matjjih rdarda berrengunœ.
'And they hadn't eaten any (sweet foods).'

Berrengunœ yenebukkarra munanganagan me.
'They didn't eat that white person's food.'

Rdardangerreh barrnguniny guwenybarrœ rdarda jalma warna gukku.
'They only ate honey kangaroo, honey, round yams and long yams.'

Nœnda barrngunœyima.
'That's what, they used to eat it.'

Munangaganma me nœndama waba.
'(But) not that European food.'

Malak birrimotmœ dambakkungong waba.
'They didn't have any tobacco.'

Barrnguny nœnda barrritjminyma jilayih.
'They would eat those [bush honey] and they would look for them with an axe.'

Bat barrwalangrdetjmœ yenehbukkarra gajahgah.
'They would then cut it that, what'sit, that paperbark.'

Baganhja barrardirditjmih.
'They would come back here.'

Walang gajahgah barrbordobordohminy nattœnda ngurrgunama.
'Then they would put it into the paperbark.'

Bœnda barrawalangdugaba.
'They would then immerse it (in water).'

Barrajohminy buluhyih.
'They would cover it with "buluh" bark.'

Malak matjjih berredommœ marrœjula.
'And they wouldn't drink it like water.'

Marrœyih naganh munangagan.
'Like the white people.'

Ngerredom naganhyinda.
'They would drink with this.'

Gajahngerreh jula rdarda barrdugabœ.
'Only water in paperbark, bush honey they immersed [in water].'

Walanggah barrdommœngerreh.
'Then they would only drink.'

Nœnda barrbuniyimany bony bony ngamungbuwa.
'That's the way they used to do it. Ok, the end. I've finished.'

Key findings

These interviews reflect various pieces of traditional knowledge about the past Aboriginal use of fire:

- Traditional burning reduced the occurrence of damaging fires, particularly those that occurred in the late dry season.
- Fires lit later in the dry season could destroy flowers on fruit trees and hence inhibit the production of fruit and bush honey.
- Yam beds could be burnt after the yams had been harvested in the early dry season.
- Landscape burning was undertaken to achieve a variety of immediate goals including clearing areas for access, locating small game like goannas and turtles on floodplains and felling trees for bush honey.
- The most important use of landscape burning concerned the management and harvesting of kangaroos.
- Burning was known to produced nutritious feed for kangaroos.
- Fire was used as a hunting tool, with use varying according to local geographic situations.
- Areas were intentionally kept unburned until they could be used for fire drives.
- Anybody who burnt a fire drive area without permission would be punished under strict traditional laws.
- Large scale fire drives enabled Aboriginal hunters to harvest quantities of meat sufficient to provide for ceremonial gatherings involving many people.

DISCUSSION

The fire stick – the hunters tool

The oral record recounted here is largely consistent with previous accounts of Aboriginal fire management in Arnhem Land, a region where more is recorded about Aboriginal landscape burning than anywhere else in Australia (Jones 1975, 1980a, 1980b; Haynes 1991; Russell-Smith et al. 1997). These authors report that Aborigines burn frequently to reduce the risk of uncontrollable late dry season fires that were understood to damage yam and fruit tree resources. Similarly, the relaxed attitude of the men we interviewed concerning the capacity of vegetation to recover from burning is in accordance with Bill Neidjie's comment that 'No-matter grass im burn but roots there..e'll grow. Even tree. E burn leaf alright but e come back' (Taylor 1989:28). The men also echoed the findings of Stocker (1966) and Russell-Smith et al. (1997) that hunters used fire skilfully to entrap, confuse or drive macropods species towards waiting hunters. Russell-Smith et al. (1997) and Yibarbuk (1998) also reported that great care had to be taken with hunting fires to avoid physical injury or death.

As previously mentioned, an unanticipated finding was the great emphasis placed upon fire for kangaroo management, and particularly the cultural importance of large scale fire drives. This contrasts with Jones (1980b) and Haynes (1991) who found that the primary motive for burning was 'cleaning the country'. Haynes noted that this motive had not been previously recorded by workers in the first half of the 20th century. He and other workers have suggested that such 'cleaning' of country is in response to a situation where traditional fire management has broken down and where Aboriginal people are attempting to regain control (Haynes 1985, 1991; Head 1994; Russell-Smith et al. 1997). This process has been described as 'corrective' burning by Lewis (1989:950, 1994:954–956). An explanation for the great emphasis placed on hunting fires by the men we interviewed may be that the landscapes where they live do not require 'corrective burning' because of the nearly continuous traditional fire management (Yibarbuk et al. 2001). It is also significant that the interviews were conducted in an Aboriginal language. This, no doubt, facilitated a more complex and accurate discussion of traditional landscape burning (Gould 1971). Finally, it is logical that the men we interviewed should place great emphasis on game in the context of landscape burning because 'Aborigines of the tropical savanna were substantially meat eaters' (Jones 1980a:136). Today, hunting accounts for 80% of the protein and over 40% of the calories consumed by Aborigines living on outstations (Altman 1984).

It would be interesting to discover if Aboriginal women would also emphasise the importance of hunting in landscape burning over vegetation management. We were unable to pursue this question because it is culturally inappropriate for men to interview women. Nonetheless, women were present when we interviewed Mick Kubarkku and Big Bill Birriyabirriya and made the occasional interjection expressing support for what was being said. Although women exploit different resources to men, they use fire for hunting small game and clearing areas (Russell-Smith et al. 1997). Further, for the north Kimberely region at least, women participated in kangaroo fire drive hunting. Kaberry (1939:18) reported that men 'burn off grass and spear game, while the women come behind and collect the reptiles and marsupials.'

Both Jacky Bun.ganiyal and Mick Kubarkku describe how country would be burnt to induce kangaroo and other fauna to feed on newly sprouted shoots. This would both fatten them and provide hunters with a known location for spearing animals. The use of fire to create 'green-pick' for kangaroos was quickly appreciated by some nineteenth century explorers (Jones 1969; Hallam 1975). Despite scientists repeatedly quoting Mitchell's perceptive comment that 'fire, grass, kangaroos and human inhabitants seem all dependent on each other for existence in Australia' (Jones 1969:225) there have been remarkably few studies investigating the effects of landscape burning on macropod populations. Indeed, the aerial survey of a traditionally managed Aboriginal estate on the Cadell River is the first scientific corroboration that there is a highly significant statistical relationship between macropod density and burnt areas in northern Australia (Yibarbuk et al. 2001)!

For Australia as a whole there are few records of how Aborigines used fire as a hunting tool (e.g. Basedow 1925; Love 1936; Finlayson 1943; Lommel 1952; Gould 1967; Stocker 1966; Jones 1969; Hallam 1975; Anell 1960; Tindale 1981; Russell-Smith et al. 1997; Yibarbuk 1998; Hill et al. 1999). Both Basedow (1925) and Tindale (1981) noted that fire was used by north Australian Aborigines to hunt small game but did not mention its application to larger animals such as kangaroos. However, Lommel (1952:2) described the use of fire in hunting kangaroos. He wrote:

> Various fires are lit on a plain that quickly spread and then enclose a certain area. The men take up position inside the circle of fire, which they can do without danger, as the burning grass is not so dense that it would be impossible to break through the fire. The kangaroos try to flee in fright out of range of the fire and are killed by the hunters with a fair degree of certainty. I have never seen a group using this method returning without any kill.

Finlayson (1943:64) provided one of the most vivid first hand accounts of a kangaroo fire drive undertaken by Aboriginal men who were helping him secure zoological specimens of the mala (*Lagorchestes hirsutus*) in the Musgrave Ranges in Central Australia. Finlayson noted that 'event followed event to final success with the precision of a ritual. The whole procedure adopted *appears* [our emphasis] to have been standardised and perfected by age-long repetitions.'

Contrasting Finlayson's lack of awareness of the cultural complexity of fire drives is Thomson's (1949:17) insightful account of their cultural context in northeast Arnhem Land. He wrote that fire drives for 'kangaroos, wallabies or other game is not a random business: it is well organised, and is carried out by the men as a communal enterprise, although in a restricted and controlled manner.'

Thomson also noted that the burning was 'directed by the old men of the clan, or by others who have an hereditary right, especially by men from other clans', who under the kinship system are of the opposite moiety. He pointed out that without the appropriate background knowledge these latter men 'may appear to usurp the authority, to dominate the affairs, of clans to which they do not belong' (Thomson 1949:17). Although regulation of burning was 'rigidly enforced' so that any violation would result in punitive measures or inter-clan conflict 'differences or disputes rarely, if ever, arise, and control or authority is not apparent' (Thomson 1949:18). Indeed, 'only those who know the people and understand their social organisation would be aware of the identity of the man in authority' (Thomson 1949:18). Thomson's findings are in concordance with Hernandez (1941) who observed that use of fires in hunting kangaroos was a strictly enforced territorial right amongst Aboriginal groups in the north Kimberley and large fire drives enabled neighbouring groups to participate in the hunt and associated ceremonial activities. Altman (1984) also noted the nexus between ceremonies and large scale fire drives in central Arnhem Land. He wrote that 'during the late dry ceremonies, males often partake in large scale fire drives that are managed by elders' (Altman 1984:188). He observed that 'massive meat surpluses' are produced from such fire drives, but added that guns rather than spears were used (Altman 1984:188). Yibarbuk (1998) has written an important account of traditional Aboriginal fire usage in Arnhem Land; it is also one of the few Aboriginal perspectives of landscape burning. The men we interviewed reiterated the various motives for burning by Aboriginal people and the consequences following the breakdown of traditional fire management put forward by Yibarbuk, who emphasised the great cultural significance of fire drives. He noted that these drives were restricted to specific places and particular times and that they involved considerable planning and discussion with strict protocols governed by the kinship system. He writes that fire drives formed an important part of traditional Aboriginal life because 'the fire drive is itself regarded as a sacred and very serious act, often first enacted by the major creative beings for that area' (Yibarbuk 1998:5). For instance meat derived from the fire drives requires ritual purification so woman and children can consume it (Yibarbuk 1998).

The intensity of Aboriginal fire management must have varied in the landscape in response to resource availability and areas suitable for fire drives. For example, Haynes (1991) recorded that Aborigines he was working with were unconcerned when a late dry season hunting fire escaped control because it burnt into an area that had 'no important food resources.' However, he noted that an adjacent vegetation type richer in resources was unaffected by the fire because of burning carried out earlier in the dry season by Aborigines (Haynes 1991:66). Similarly Jones (1980a) and Russell-Smith et al. (1997) reported that Aborigines burnt firebreaks around rainforest margins in the early dry season to protect these habitats from fires later in the dry season. Rainforests are important foci for both fruit trees and yams. Mapping locations of specific places used for fire drives, or that are known by Aborigines to have supported high densities of vegetable resources, may provide valuable insights into the spatial scale of fire management, and thus the intensity of land use, in any given landscape.

Aboriginal landscape burning - is it farming?

Jones' use of the word 'farming' was extremely provocative at the time of writing his paper. This was so because agriculture was perceived as the great divide separating hunter-gatherers from more 'advanced' societies. For example, Gould (1971) suggested that caution was required before embracing the notion of 'fire-stick farming' because of the difficulty in teasing out the longer-term ecological consequences from the pragmatic and immediate use of fire by Aborigines. He stated (Gould 1971:23) that in the case of western desert Aborigines:

> their burning tends to be directed towards limited goals and is carried out in a casual manner which results in many cases in extensive burned-over areas. These burned-over areas can, under certain local conditions of rainfall and processes of natural plant propagation which are not yet well understood, improve the availability of certain staple plant foods. Thus I hesitate to apply the term "farming" to this situation but prefer instead to try to understand it on its own terms with regard to traditional Aborigines subsistence practices and conditions of local ecology.

The term still causes discomfort to some authors. Anderson (1990:64) suggested that the use of the word 'farming' was a tactic to 'raise respect for the methods of intelligent hunters', a point also made by Horton (1982:248, 2000:98). Anderson (1990:64) argued that 'possession of domesticates is a minimal definition of farming which prehistoric Aborigines did not meet, the dingo aside' and concluded that 'the phrase fire-stick farming should be discarded'. Horton (2000:99) took a more extreme view by arguing that the fundamental distinction between hunter-gatherers and farmers is that only farmers modify their environment to produce food while hunter-gatherers are environmentally non-interventionist, or as he puts it 'observers' rather than 'modifiers'. He based this view on the erroneous and outdated idea that Aborigines did not intentionally modify environments with fire.

It is apparent from our interviews that there is great difficulty in deconstructing the use of fire by Aboriginal people into neat causes and effects, thus an analogy with a narrow interpretation of 'farming' is problematical. For instance, although the men we interviewed stated that landscape burning was primarily for hunting and managing game they also understood that use of fire had important effects for other components of the subsistence economy including yams and fruit trees. Further, the men alluded to the great cultural significance of large scale fire drives. 'Fire-stick farming' is open to misinterpretation if it is understood as being strictly equivalent to conventional forms of agriculture. The widespread use of the phrase demonstrates the need for a specific term to describe the skilful modification of landscapes with fire by Aborigines. 'Igniculture' may be more accurate but far less euphonious than Jones' evocative turn of phrase.

Implications for land management

The continuing unravelling of hunter-gatherer economies following European colonisation stymies the complete comprehension of 'traditional' subsistence and the role of landscape burning. Nonetheless, the interviews we conducted showed that there remains a great deal of information about the traditional use of fire in the minds of Aboriginal people. Priority should be given to support interpreters to help anthropologists and ecologists record knowledge of Aboriginal people. Although the traditional system of large fire drives has become very irregular, Aboriginal people in Arnhem Land still confidently use fire of a wide variety of purposes. For instance, young men like Djungkidj Ngindjalakku routinely use fire while hunting kangaroos. It remains unclear if such localised use of fire has any equivalence in pre-contact times. Undoubtedly the contemporary use of fire is not only driven by immediate needs but also demonstrates a humanising presence in the landscape that has significance for other spheres of life (Head 1994). A similar point has been made concerning the cultural significance of hunting and gathering when Aboriginal people have become enmeshed in the cash economy (Sackett 1979; Povinelli 1992).

Rose (1988) has described the commitment of some Aborigines to managing their land as a 'land ethic'. Such a land ethic provides an important, but sadly neglected, opportunity for ecosystem management that steps outside the classical model of national parks as isolated environmental 'Madonnas' surrounded by debased and exploited lands. For example, the recent study of Yibarbuk et al. (2001) demonstrated that a traditionally managed clan estate on the Cadell River in central Arnhem Land was in excellent condition compared to unoccupied areas of similar habitat in Kakadu National Park. These authors suggest that the cause of this difference is skilled fire management by Aborigines that has 'imposed a regime of high frequency, low intensity disturbance at a fine scale, to which the contemporary biota appears resilient.' They argue that such skilful management needs to be formally recognised and given long-term support by conservation agencies. Indeed, Whitehead (1999) has extended this argument by suggesting that facilitating a return by Aboriginal people to their tribal lands should be seen as an important strategy in the ecologically sustainable management of north Australia.

Andersen (1999) unfairly characterises Aboriginal fire management as being experiential and mythological, based on rigid traditions and beliefs, devoid of any profound appreciation of the workings of nature or clearly articulated objectives. He claims that contemporary land managers should not bother attempting to build upon Aboriginal landscape burning practices, as advocated by Lewis (1989); rather they should start anew, basing management upon rigorous experimental ecology to develop a system of 'adaptive management' (Andersen 1999). We have demonstrated that traditional Aboriginal landscape burning had the specific objective of managing game that was situated within a complex cultural milieu. This style of management had well understood ecological spin-offs such as controlling the intensity and spatial scale and location of landscape fires and the conservation of vegetable resources. The dogmatic views of Andersen (1999) are wrongheaded because he stubbornly ignores the body of evidence that Aboriginal burning had developed negative feedback which promoted ever smaller and lower intensity fires, in contrast to European burning that has produced a positive feedback cycle of increasing size and intensity of landscape fires. The reason why Aborigines achieved a high degree of ecological sustainability was that the ecological impacts of fire were closely coupled with a hunter-gatherer economy. Indeed, such close coupling can be aptly described as 'adaptive management' despite not being based upon the western scientific paradigm (cf. Andersen 1999).

The future of 'fire-stick farming'

Jones (1969) came to the profound realisation that European land managers must choose what sort of 'natural' landscapes they want, given the backdrop of an extraordinarily long period of Aboriginal landscape burning. Jones (1969:228) asked:

what do we want to conserve? We have a choice. Do we want to conserve the environment as it was in 1788, or do we yearn for an environment without man, as it might have been 30,000 or more years ago? If the former, then we must do what the Aborigines did and burn at regular intervals under controlled conditions.

However options are more limited for landscapes that have become fragmented by land clearance, infested with flammable weeds that support a variety of contrasting land-uses, or which are no longer occupied by Aboriginal people. Given the constraints of the real world, and particularly limited recurrent funding for fire management, the question is not so much 'what do we want' but 'what can we do'. The culturally elaborate use of fire by Aborigines pursuing a subsistence lifestyle is not readily mimicked by a western culture, and in any case the aims and objects of the two economic systems are different, as are the technologies. Detailed local knowledge accumulated over lifetimes and continually updated by day-to-day observation is being replaced by digital data collected by periodic satellite observations that are stored on geographic information systems (Bowman 1997). These data are intermittently interpreted by people who must move on to the next project or the next employment contract. Landscapes are ignited by incendiaries dropped from aircraft rather than by fire-sticks in the hands of hunter-gatherers (Haynes 1991).

Jones ends his famous paper by observing that 'the days of 'fire-stick farming' may not be over yet' (Jones 1969:228). This can be read as advocating a re-imposition of traditional Aboriginal fire management. A less ambitious reading is that there can be little doubt that the human-fire nexus will continue to be a potent ecological force in Australia (Bowman 1998). Given the enormous ecological changes wrought by European colonisation it is doubtful that fire regimes in the late 21st century will bear much resemblance to traditional Aboriginal landscape burning of the early 20th century. Nonetheless, and despite these differences, future fire management regimes will be, consciously or unconsciously, derived from traditional Aboriginal burning practices. 'Fire-stick farming' has not ended. Rather it has had a new beginning. Hopefully, the majority of Australians will eventually come to appreciate and respect the great importance of traditional Aboriginal use of fire in managing their landscapes. Rhys Jones' writings made the first steps in this important process of reconciliation.

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Of Cowboys and Core-Tools: Revisionist Reflections on Rhys Jones and 'The Great Intensification Debate'

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> ... archaeology seems to have been developed entirely by the efforts of Americans, Marxists, Frenchmen, Scandinavians or eccentric Welshmen ... (Clarke 1979:154)

I NEVER worked with Rhys Jones, I am sorry to say. We worked in the same department for a few years, but on very different questions in widely-separated field locations. This meant I only saw him socially or when we bumped into each other in a lab, at a seminar, or in the famous Prehistory tea-room (named in honour of Brian Egloff). He had a great impact on my life and career nonetheless. On one level, this is for much the same reason that he has influenced anyone else doing Australian archaeology at any time during the last four decades. On another, more personal level, it is because by way of a unanticipated visit to the said tea room in 1978, he introduced me to the RSPacS department at ANU, where I later undertook my PhD. I haven't been the same since, as this contribution may well attest!

How has Jones affected my professional practice? Profoundly, in relation to my sense of the history of archaeological thought in this country. This is especially the case in the way in which Jones' researches, and especially his views on what he (1977) called 'the Tasmanian paradox', provided a major impetus for Harry Lourandos' evolving views on intensification and other aspects of change in Australian prehistory (e.g. Lourandos 1980:245; Lourandos and Ross 1994:56; see also Bowdler 1993:129). Lourandos' work in turn prompted the studies of researchers such as Ross (e.g. 1985), Williams (e.g. 1987), Barker (1991) and David (e.g. 1991), as well as critical reactions from a wide variety of scholars, including Allen (e.g. 1993), Beaton (e.g. 1983), Cosgrove (e.g. Cosgrove et al. 1990), Davidson (e.g. 1997), Frankel (e.g. 1995) and Yoffee (1985), creating what Lourandos and Ross (1994) described as 'the great intensification debate'. In the terms of the opening epigram, therefore, I reflect below upon aspects of a scholarly relationship between a (neo)Marxist and a mildly eccentric Welshman that I consider critical to the development of Australian archaeology. If I have one difficulty in doing so, it is that Jones himself has largely refrained from engaging explicitly in the *fracas* (though see Jones 1984), which means I cannot simply line up Jones' and Lournados' views on the matter and compare them. Rather, I have to rely on interpreting what some may think are rather enigmatic comments from Jones about the issues involved. This task is made somewhat easier by the fact that Jones continued to publish on matters of direct relevance to the intensification debate well after he first aired his views on 'the Tasmanian paradox', even if he rarely weighed into the debate itself. Because it is central to the whole question of Jones' role in important arguments about intensification in Australian prehistory, I also consider the work of the American, Joe Birdsell, whom Lourandos often links with Jones in his characterisations of what was amiss in Australian archaeology before the intensification debate set the discipline on a new course in this country. In short, in this paper I examine elements of Jones's *oeuvre* in relation to Lourandos' perspectives on change in

Australian archaeology, against the background of Birdsell's writings. My particular concern is to take issue with the way in which Lourandos has interpreted Jones' position on change and the relationship between people and nature in Australian prehistory. I argue Lourandos has presented only a partial reading which obscures important areas of common ground between Jones' work and his own conceptions. This, I believe, has seen Lourandos inappropriately categorise Jones' perspective as highly deterministic in contrast to his own and at the same time has prevented Lourandos from realising the full potential of his own valuable work on change in Australian prehistory.

THE TASMANIAN PARADOX

Lourandos and Ross (1994:55–6; see also Lourandos 1997:2, 308–9) identified Birdsell as a principal author of 'traditional anthropological and archaeological paradigms which viewed Australian Aborigines and their prehistory as essentially static, with socioeconomic and demographic changes largely insignificant and under environmental control.' They went on to claim that:

> Jones ... too, argued against significant demographic and socioeconomic change in the mid-late Holocene prehistory of Australia, preferring instead long-term equilibrium between the number of people and the availability of resources.

In the second paragraph of 'The Tasmanian paradox', Jones (1977:189) wrote that in the mid-late Holocene, 'there was great spacial and temporal diversity, giving an impression of dynamism and flux compared with the previous millennia.' He also noted (Jones 1977:192) that in comparison 'with the unity of the previous 25,000 years [bearing in mind the antiquity of colonisation and pattern of Pleistocene occupation known in 1977], the next 5 or 6,000 years of Australian prehistory was one of bewildering diversity ... . It was a period says John Mulvaney, when "the tempo of technological innovation had quickened and transformed Australian Society"'. Regionalisation, too, gets a mention in a rudimentary form: a 'perplexing feature of the new developments is that there appears to have been several complexes or traditions...and that these tended to be employed differentially across the continent' (Jones 1977:192). Cultural developments are also separated from environmental control. Although 'tula adzes and associated tools' seem to be largely restricted to desert areas, Jones (1977:193) noted that:

> similar correspondences cannot be made for most of the other major tool kits. Indeed, the distribution of some, positively cut across major ecological zones, and bear no relation to known major 'culture areas'...It is not so much the presence of a tool type that needs explaining...but rather its absence in an ecologically equivalent and adjacent zone, especially when we have evidence of contact between the inhabitants of both regions.

I cannot see how comments such as these could be overlooked by Lourandos, much less construed by him as an argument against significant change in mid- to late Holocene Australia, or against the importance of sociocultural factors in determining past patterns of cultural variability. It is a paradox indeed, and one made all the more baffling by Lourandos' (1997:314) more recent comment that 'Jones ... closely associated the introduction of new technology ... in the mid-late Holocene...with widespread changes to economy, demography and social organization.'

Either Jones argued against substantial mid- to late Holocene change or he did not, and the evidence of his own words and at least some of Lourandos' shows very clearly that he did not. I might add in this context that Jones not only repeatedly emphasised the dynamism of the mid- to late Holocene but saw that dynamism in a light which also illuminated Lourandos' views. Thus in another, earlier paper, Jones (1975:25, 28) explicitly discussed the overlap between foraging and farming upon which Lourandos places so much weight, noting that:

> man-plant relationships in Aboriginal communities are far more complex, and have far more features in common with so-called 'agricultural' processes than has been generally realised…[O]ne can…suggest that it only required intensification of the sort of relationship between man and his edible plants seen within Tropical Australian hunting and gathering economies, to produce the horticultural or aboricultural economies that seem to have such high antiquity in the Malayo-Melanesian region.

This does not mean, however, that Jones thought Aborigines were on the path to sedentary agriculture. As he and Meehan (1989:133) noted 14 years later (cf. Spriggs 1996) for:

> a model for the type of social or economic systems that historically led to the earliest practices of systematic horticulture in New Guinea or Southeast Asia, we may need to think of societies quite different from [those in hunter-gatherer Australia] — perhaps systems of inherent instability, of less bounded resources, and of conceptual categories of what could and what could not be done with the natural world.

Lourandos (e.g. 1997: 2–4) has also frequently disassociated himself from unilinear models of sociocultural progress, arguing that rather than being evolutionarily linked in any inevitable way, foragers and farmers occupied different places along a cline of variation. Given these similarities, one is left wondering what it is about Jones's work, and especially his 1977 'Tasmanian paradox' paper, that Lourandos has objected to so regularly and so vehemently.

To think about it more clearly, Jones' paper needs to stripped of its rhetorical flourishes and its distracting speculation about cognitive devolution. We also need to leave aside disputes about relative population densities. I suspect these last cannot easily be resolved (e.g. Jones 1984:41–4, 1990:36-41; cf. Pardoe 1991), and while they are undoubtedly important when the details of the particular argument are in focus, they are not relevant in the present context. When this paring-back is done, we can see that Jones makes two general points. The first is that a simple toolkit did not doom Tasmanians to a hand-to-mouth existence at the bottom of an evolutionary ladder, unable to exploit their environments effectively. Rather, they did quite well, despite Jones' (1978) concerns about them not eating fish, and certainly as well as many mainland groups. Jones (1977:193) reinforced this point with an comment about Arnhem Land, where people had a simple toolkit and high population densities (see also Jones 1984). I cannot imagine Lourandos would disagree with this general proposition, in view of his (1997:xvi) claim to be a revisionist seeking to change unilinear evolutionary perspectives.

Jones' second general point is that evident behind the mid- to late Holocene changes on the mainland to which he, Jones, drew attention, there was a long-term *Australian* pattern of cultural variation, of which mid-Recent change was an integral part, rather than something imposed or introduced from outside the continent. This proposition neither implies that nothing really changed a great deal after initial colonisation of the continent, nor that there were no outside inputs. Rather, it is clearly an attempt to undermine or counter the idea that Aborigines were passive and uninventive, and could not initiate change themselves, but instead simply responded to change sparked from outside. In other words, Jones was arguing that the mid- to late Holocene change he described was generated primarily by the internal dynamics of Aboriginal society. Again, in my assessment, Jones was concerned to change lingering unilinear evolutionary stereotypes, much in the way that Lourandos argues he himself is. Moreover, Jones' views about the deep and primarily Australian roots of recent change are not fundamentally different from Lourandos' (1997:326–7, 334–5) recent disclaimers that he is 'not asserting that a dichotomy existed divorcing the late Holocene from prior periods, rather than in general they are clinally linked', and that he is 'not suggesting … that the cultural changes of the late Holocene…were due solely to external influences, but rather to interplay of internal and external forces.'

Like a number of his other works, Jones' paradox paper reveals a Malthusian approach to population and resources. Although Lourandos (e.g. 1983:92) has long been equivocal about the role of population increase as a cause of cultural variation, Jones' approach is not dissimilar at root from Lourandos' (1983:92, 1997:11) denials of Boserup's assertion that population growth is an independent variable which drives socioeconomic change (cf. Beaton 1983). I think Jones has been misunderstood on this issue in much the same way Malthus was (Bullock and Trombley 1988:496). Malthus argued population increase always has the potential to outstrip increases in food production, owing to 'the continuing passion between the sexes', unless there are strong checks on demographic growth. Such checks, he thought, took two forms: 'positive' and 'preventative'. The former include famine, war and disease, and the latter, 'prudential or moral restraint'. Since his own time, Malthus has usually been associated almost exclusively with the threat of positive checks, though he actually also saw preventative checks as important (Bullock and Trombley 1988:496). Similarly, I think Jones' Malthusian perspective has been interpreted by Lourandos as evidence that Jones sees Aboriginal society as enslaved to nature, responding passively to environmental exigencies which controlled the size of Aboriginal population by periodically visiting famine or pestilence upon them. As I read him, however, Jones has continually emphasised human agency in the relationship between people and nature. How else could people, rather than nature, preserve the homeostasy he argues that they, not nature, maintained (1977:201–2)?

Jones unquestionably believes that foragers 'contend with the vagaries of abundance and of disaster' (1977:202) differently from agriculturalists or pastoralists, but that is not the same as arguing that foragers live(d) hand-to-mouth, at nature's mercy. Rather, it is proposing much the same thing as Ingold (1991, 1996), for example, whom Lourandos often cites with approval (e.g. 1997:11). Ingold (1991:264) wrote that ethnography:

> shows quite convincingly that many of the peoples classically labelled "hunter-gatherers" have a particular understanding of their relations with their environments, an understanding which is implicate (sic - implicit?) in their own productive practices. It also shows that many of the peoples classically labelled "pastoralists" and "cultivators" have a radically different understanding of these relations and approach their environments with a different logic.

That such differences hinge, in Jones' view, on people actively maintaining homeostasy, or a socially-defined and regulated relationship between people and their environment, does not mean he sees those people as incapable of managing that relationship to their own ends. Surely, in fact, it implies precisely the opposite?

BIRDSELL

In seeking to amplify Jones' views in this connection, it is useful to turn to Lourandos' *bête noire*, Birdsell, with whom he often and quite appropriately associates Jones in connection with matters of Aboriginal demography (e.g. Lourandos 1997:308; cf. Jones 1977:200). Lourandos (1997:2) argued that the:

> traditional model viewed Australian prehistory with an emphasis on long-term equilibrium between numbers of Aboriginal people and natural resources. That is, Aboriginal demography was seen as largely under the control of long-term environmental forces, and sociocultural changes (where they could be identified) were largely negligible.

He went on to cite Birdsell (1977:149) as writing that it:

> is now realised that these economically simple people, and all of the Pleistocene occupants of Greater Australia, live in fact in a skilfully regulated state of homeostasis. Such people were in equilibrium with their environment and this balanced condition was maintained, despite some fluctuations, by a rather complex series of actions, beliefs and traditions.

It is hard to understand why Lourandos should have suggested that terms such as 'skilfully regulated' and phrases such as 'this ... condition was maintained ... by a rather complex series of actions, beliefs and traditions' mean that Birdsell, or those such as Jones who have agreed with Birdsell, see or saw 'Aboriginal demography ... largely under the control of long-term environmental forces', or that sociocultural changes were either hard to identify or 'largely negligible'. Had Birdsell meant this, would he not have said so, rather than explicitly describe 'skilful' sociocultural control of the process? Indeed, his words seem remarkably similar to some penned much more recently by Ingold (1996:149), who could never be mistaken for an environmental determinist, and with whom Lourandos (1988) has published on the matter of complex hunter-gatherers: 'the activities we conventionally call hunting and gathering are forms of skilled, attentive coping with the world.'

Birdsell's separation of people from the direct control of nature is even more clear when his (1977:149) entire passage is quoted. It begins, 'Early European contacts with the hunting and gathering peoples of all continents were so biassed that the early observers' literature treats them as really little better than animals [i.e. controlled by nature]. Anthropology has long passed through any such views, and it is now realised ... [that their relations with nature were 'skilfully regulated', and so on]'. Immediately following the section quoted by Lourandos, the passage concludes with the comment that there 'is evidence that this type of population control was self-consciously practised and involved, amongst other things, preferential female infanticide where that was deemed appropriate to the end.' This comment unquestionably highlights a conviction that people consciously made and operationalised assessments about what should be done to maintain themselves and their societies when they found themselves in changing circumstances. This is not to suggest that ancient Aboriginal people were able to appraise or predict long-term or large-scale trends of environmental variation, that they objectified themselves, their societies or their environments in the way people objectify such things in the modern industrial West, or that their attempts to manage change were always appropriate or successful. It is only to acknowledge that like other modern humans, Aboriginal people were capable of consciously devising and implementing change-management strategies based upon innovative patterns of behaviour. This general position is in keeping with current anthropological understandings of 'traditional' Aboriginal views on such issues, which although they never question the primacy of the Dreaming and the Law as guides for human action, also unequivocally accord people the capacity to initiate change when required to manage shifting circumstances (e.g. Tonkinson 1999:138-140, esp. fns 7 and 9). Moreover, far from denying change, Birdsell's (and Jones') position on homeostasy implies quite unambiguously that change was a constant process. To suggest otherwise is to imply either that no environmental change occurred in Australian prehistory, which is not the case, or that the natural environment has no influence on human action, which is also simply not true, particularly in societies with simple technology such as those in question here (cf. Lourandos 1997:78).

What is true is that Birdsell's much earlier formulations were couched in language which made it easy to mark him as an arch environmental determinist. In his well-known paper on Aboriginal demography (published in *American Naturalist*, the name of which heightens the impression of rampant determinism), he closely correlated population density and rainfall. He wrote (1953:177, 184, see also 201, 206) such things as:

> In mammalian ecology it is generally recognized that the density of a given type of population will be some function of the critical environmental variables ... [and it] seems likely that for hunting and gathering peoples similar forms of environmental determinism might obtain

and that the

> density of the aboriginal population in Australia was determined to a large measure by rainfall operating indirectly through the biota.

It should be borne in mind, though, that Birdsell excluded from his numerical analysis 286 groups from the total of 409 for which he had data, some 70% of the total. He did this because either ecological factors other than rainfall, such as rich coastal or riverine resources, or various cultural factors would have so biassed his analysis as to render it meaningless. Proclaiming that population density was a function of rainfall on the basis of a sample excluding the 70% of groups which could be presumed a *priori* not to fit is a highly-suspect methodology to begin with, even if in gross terms his findings stand up; people in very dry areas did and do live at much lower densities than elsewhere, and people along the Murray-Darling did live at higher densities than the regional rainfall regime would predict - owing, ultimately, to access to the 'unearned surface water' flowing through major river systems, which, then as now, enlivens areas of low biological productivity. Birdsell was well aware of his sampling bias. He noted (1953:181) that the sample of groups:

> while ranging through nearly the full variation in rainfall ... is predominantly representative of ... non-coastal Queensland. In so far as the ecological variables for this region may not be representative for the continent as a whole, a systematic error may have been introduced into the analysis.

More to the point in the present context, though, is that while he excluded sociocultural determinants from his analysis, he did not disregard or diminish their importance to Aboriginal demography. Rather, he drew attention to them, and deemed them important enough to affect the nature and direction of his numerical analysis so fundamentally that he left them out of it. Thus it is that the title of his paper (my emphasis) is 'Some environmental and *cultural* factors influencing the structuring of Australian Aboriginal populations'. He (1953:180) wrote that from 'the cultural point of view, several factors operate so as to cause systematic deviations ... .' Interestingly, in relation to Lourandos' work on the emergence of sociocultural complexity and 'closure' amongst Australian hunter-gatherers, Birdsell found that the two main cultural factors affecting his analysis were 'a more advanced type of political organization' found in parts of the continent and the way in which the spread of ceremonies, in this case associated with circumcision and sub-incision, led to 'tribal fragmentation.'

Birdsell was not sure why the latter relationship should obtain, but he was sure it would 'certainly be found in the realm of social forces and interactions.' Thus while he might have indicated that environment determined behaviour, he clearly meant, and indeed stated plainly (1953:204) that it was only in the broadest sense, and that cultural factors mediated the relationship between people and nature. In other words, Birdsell explicitly recognised that cultural factors, especially examples of more complex behavioural patterns, produced situations which vary substantially from the broad picture, much in the way Lourandos does (e.g. 1997:78, 329). Lourandos (1997:15, 308) acknowledges in passing Birdsell's 1953 references to cultural factors. However, he argues that by 1977, Birdsell had 'abandoned these ideas in preference for more environmental explanations.' My interpretation differs. Birdsell's earlier paper forcefully asserted the importance of the environment, despite a consciousness of social factors that is obvious. It did so, he wrote (1953:201-2), largely as a corrective to social anthropologists who 'tended to deny' the obvious importance of environment. While he certainly argued that under particular circumstances, rainfall was a good predictor of population size, he did not imply that environmental patterns always and inevitably overshadowed the equally obvious importance of culture in determining patterns of behavioural variability (cf. Lilley 1999). I think my earlier quotes from Birdsell show that his conceptualisation of the issues and, more particularly, the language he used to describe them, in fact came a long way between 1953 and 1977. This was presumably in part because he thought the need for correctives had diminished by 1977, when his language concerning environmental matters was very much more muted than it was in 1953. Indeed, in 1977 Birdsell only wrote two short paragraphs about the matter in a 54-page essay. Those paragraphs indicated that statistical refinements had improved his coefficients of correlation between rainfall and population, but nonetheless concluded (1977:150) with the appraisal that Aboriginal people managed their relationship with their environment 'through the working of a complex system of behaviour within a simple economy.' This can hardly be described strident environmental determinism! The previous section of this paper showed that the trends I have argued are evident in the evolution of Birdsell's formulations are also reflected in the development of Jones's body of work from the late 1970s (cf. Chappell this volume).

CONCLUDING DISCUSSION

The foregoing two sections of this paper have demonstrated three important things:

- Jones has not denied, but in fact has consistently drawn attention to mid- to late Holocene change in broadly the same way as Lourandos.
- Jones has argued explicitly for the primacy of human agency in managing such change within broad environmental constraints, also in much the same way as Lourandos.
- Like Lourandos, Jones has long sought to overturn offensive stereotypes of hunter-gatherers.

In the interests of achieving a clear view of the conceptual development of Australian archaeology, we must ask in the light of these observations why it is that Lourandos consistently portrays Jones' position as so divergent from his own. In the context of this festal volume, we ought to ask in particular whether Jones' perspective really deserves to be damned as not much more than a relic of racist 19th century unilinear evolutionism (e.g. Lourandos 1997:xvi), when it is abundantly clear from writings including his 1977 paradox paper (also 1984, 1990) that Jones has spent his professional life countering such ideas.

In view of the parallels I have described, I can only think that Lourandos has tried to distance himself from Jones because he takes exception to Jones' position that Aboriginal people worked to maintain what Western scholarship describes as their hunter-gatherer culture(s) rather than adjust to internally- or externally-generated sociopolitical pressures by developing significantly greater levels of sociocultural complexity. If this is the case, it presumably stems from Lourandos's investment in arguments proposing precisely that Aboriginal foragers were responding to inexorable sociopolitical pressure to change and in some respects shifting in the direction of sedentary agriculture and greater levels of sociocultural complexity, even if they were not actually on the pathway to farming and complex hierarchical social organisation (e.g. 1983:81, 1997:74–79). Such arguments flow inevitably from Lourandos' neo-Marxist position. This is because despite conceptual advances by the likes of Wolf (1982) and Lourandos' related disclaimers about his own work, at root neo-Marxism retains the flavour of Marx's and Engel's original unilinear-evolutionary tendencies (Layton 1997:155–6; also Dwyer 1996:181; Ingold 1996; Jones 1990:46–9; Spriggs 1997). Thus it seems to me that Lourandos' argument with Jones is not actually about whether or not change occurred, or even about the primacy of human agency over environmental determinism. It is, rather, a political argument about the fundamental motivation underlying the management of the change which both of them believe characterised Australian prehistory.

Lourandos has long argued that significant change was happening in Aboriginal society before being 'nipped in the bud by the coming of the Europeans' (1983:92). Such change was propelling Aboriginal society somewhere, socioculturally-speaking. It does not seem necessarily to have been heading anywhere in particular, other than in the direction of what on a global or even just a wider regional scale was only very slightly increased sociocultural complexity (e.g. Lourandos 1983:81, 92, 1997:321–3; cf. Yoffee 1985). We cannot know what may have happened in Australia had Europeans not colonised the continent. Rather than speculate about the unknowable, we can argue on the basis of global patterns of sociocultural change that if Aboriginal society was not being propelled towards sedentary agriculture and the significant levels of sociocultural complexity associated with it, it had to be heading along a path peculiar to hunter-gatherers who remain hunter-gatherers in the face of internal or external incentives or pressures to develop or adopt other ways of life. Jones has long argued that while their behaviour certainly varied through time, in the final analysis people in prehistoric Australia seem to have managed such change in such a way that they remained hunter-gatherers. To my mind, this position does not differ much in the end from Lourandos' acknowledgement that Australia remained a 'continent of hunter gatherers' even though some Australian foragers exhibited some sociocultural characteristics similar to those of some horticulturalists.

Lourandos is unwilling to recognise the parallels between his and Jones' views on this point, even though some of the issues he (e.g. 1997:32–5) has discussed bear directly upon the matter. I surmise this is because for (perfectly honourable) political reasons he is reluctant to countenance the possibility that despite certain parallel or overlapping characteristics, ***hunter-gatherers perceive or apprehend the world differently from agriculturalists***, and on that basis will endeavour to maintain what they see as the appropriate way to live. This is neither a new idea, nor an outdated one, nor one that necessarily applies only in Australia, as the longevity of the Mesolithic cultures of northwestern Europe, for example, may attest (cf. papers in Zvelebil 1986). Rather, it is an idea which has retained a currency for a long time because it accurately reflects lived reality, as documented by ethnographic research of the sort undertaken by Jones with Meehan, White and others in Arnhem Land (e.g. Jones and Meehan 1989; cf. Kus 1997). Thus in addition to its regular airings in the papers of Jones' mentioned above, the idea was discussed in the Australasian context by White (1971) and Mulvaney (1971), for instance, three decades ago, and by Chase (1989), Dwyer (1996), Frankel (1995), Spriggs (1996) and Yen (1995) much more recently. As Head and Fullagar (1997:426) observe, Chase's support for this notion contrasts with the fact that his earlier papers (e.g. Chase and Sutton 1981, Hynes and Chase 1982) have been widely used to support the idea that there were only minimal differences between hunter-gatherer and horticultural approaches to the land and its use by humans. Beyond our shores and at higher levels of abstraction, the proposal that there were profound differences between the world views of hunters and horticulturalists is espoused by scholars such as Ingold (1991, 1996), whose influence is evident in other aspects of Lourandos' work. It is also implicit in theoretical works such as Yoffee's (e.g. 1993) concerning sociocultural evolution, and is very much to the fore in recent appraisals of the decolonisation of anthropology (and by extension, archaeology) by the likes of Sahlins (1999).

The proposition that Australian hunter-gatherers, like certain other hunter-gatherers in other places and times, were conservative because they valued and sought to preserve fundamental aspects of their way of life does not deny - and is not denied by - the sort of revisionist anthropology concerning the nature of Aboriginal society that Lourandos (1997:32-35) has so usefully brought to the attention of Australian archaeologists. The proposition that Aboriginal society was conservative in this sense is thus ***not*** an assertion of a racist, nineteenth century unilinear evolutionism which places Aboriginal society on the bottom rung of some odious ladder of moral and intellectual standing. Neither are scholars who advance such notions of conservatism necessarily politically reactionary neocolonial oppressors of indigenous people, as some of Lourandos' (1997:xvi) work has plainly implied they are. Although not as egalitarian and communal as once thought, Aboriginal society nonetheless unquestionably placed very much less emphasis on the accumulation of physical capital and the development and maintenance of formal sociopolitical hierarchies than more socioculturally-complex agricultural or urbanised societies, as Lourandos would, I think, agree. Observing that it did so owing to an ideological commitment to a particular way of life is not, however, 'to explain away' (Lourandos 1983:89) as opposed to 'try to explain' Australia's unique position as a continent of hunter-gathers at the time of post-Columbian European expansion. To dismiss it as such on the basis of 'some Marxist-like notion of ... [human] gullibility in the face of blinding ideology' (Kus 1997:200) trivialises an issue which first-hand ethnographic experience continues to indicate is of absolutely pivotal importance in Aboriginal society. As Jones (1990:45) has reminded us, 'the carrying out of ceremonial was not regarded as some sort of entertainment or casual social activity, but was driven by an overriding sense of obligation to fulfil the human part of the contract with the supernatural powers [that kept the world working]' (cf. Tonkinson 1999:138, footnote 7). On that basis, I think 'the resistance of culture' (Sahlins 1999:xvi; see also Head and Fullagar 1997) to sociocultural pressure to change is an issue Lourandos and scholars pursuing similar lines of interpretation could profitably investigate rather than avoid as they have done to date. As Jones (1990:49) has pointed out, if they do not do so, they will never be free of 'the progressionist legacy of the 18th century Enlightenment' they intend their work to overturn.

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The Celtic Chronologist: Rhys Jones and the Dating of the Human Colonisation of Australia

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> SENATOR DUNN (New South Wales) — I give notice that, on the next day of sitting, I shall move that the Senate:
>
> (a) notes with interest the discovery of Dr Rhys Jones of the Australian National University, and his team, of art and artefacts in the Malakunanja II rock shelter in Kakadu that have been dated as at least 60 000 years old, a discovery that has been described as 'the most sensational archaeological discovery in 3 decades' which may prompt major revisions to site dating on this continent and enlarge our understanding of the prehistoric spread of humans across our planet;
>
> [*Hansard*, Notices of Motion to the Senate, 16 May 1990:425]

It began with a phone call. In early 1987, I was an earnest student of geomorphology newly arrived from the United Kingdom, working towards my PhD in the Alligator Rivers region east of Darwin. I was innocent in the ways of archaeology and knew nothing of the phenomenon that over the next decade I grew to appreciate as Rhys Jones, one of Australia's most respected and celebrated archaeologists. My purpose in phoning Dr Jones was to ask if he was interested in comparing the reliability of thermoluminescence (TL) dating against radiocarbon (^{14}C) dating at a site which he had excavated a few years previously — the famous Lindner Site (Nauwalabila I) — and, if such comparisons should prove favourable, in obtaining TL ages for the lowest, hitherto undated artefacts in the sequence? To quote from Lenore Nicklin's (1990:93) entertaining account in *The Bulletin:* 'Can the Welsh sing? You bet he was.'

Trying to discern the chronology of human occupation of Australia had, of course, been part and parcel of Rhys' researches over the previous 20 years. His prescient paper 'The geographical background to the arrival of man in Australia and Tasmania' (Jones 1968) raised two themes of global significance that Rhys was to pursue tenaciously for the next three decades — the timing of initial human colonisation of Australia, and the nature and extent of human impact on the indigenous flora and fauna. While he has been intimately involved in so many of the critical turning points in Australian archaeology, including the initial discovery of human remains at Lake Mungo (Bowler et al. 1970) and the recognition of an ice age human presence in Tasmania (reviewed by Jones 1995), I restrict this brief testimonial to my personal recollections of our first and, in my view, most significant collaboration since that fateful phone call in 1987 — the dating of the occupation deposits in Malakunanja II rock shelter — and invite readers seeking a scholarly and comprehensive treatment of the human colonisation debate to enjoy Rhys' own reviews of the subject (e.g. Jones 1973, 1975, 1979, 1989, 1993, 1998).

At around the time I contacted Rhys, the problem of determining the time of human arrival in Australia had become something of a Gordian knot. The age for initial landfall had been pushed back in time to almost 40,000 BP, close to the limits of ^{14}C dating as practised in the late 1980s. Consequently, many of the key questions concerning the life and times of the first Australians could not be answered with any confidence because of substantial dating uncertainties. The time of arrival of people could not be pinpointed reliably and nor could their postulated impacts on the landscape and biota, such as the extinction of the giant marsupials, reptiles and birds (the 'megafauna') that once inhabited Australia. Rhys' discomfort with accepting a strict ^{14}C chronology for human colonisation was clearly stated in his prophetic 'Ions and eons' paper (Jones 1982:30):

> Reviews of the antiquity of man's occupancy of the Greater Australian continent now suggest that this happened at least some 40,000 years ago. While such a figure is a proper conservative estimate from the existing data, it is suspiciously close to the theoretical limits of radiocarbon dating using conventional methods. If the true radioactivity of a sample was in fact indistinguishable from background, it would take only the smallest amount of contamination of some sort to result in a finite though smallest detectable value which, translated through the statistics of the date calculation, could result in a value of some 17–40,000 years old. Thus if more sensitive methods could be developed, then the reality or otherwise of the antiquity of sites in the 35,000 to say 55,000 year bracket could be investigated.

A further 17 years were to elapse before reliable ages of up to 55,000 years could be obtained using ^{14}C dating (Bird et al. 1999; Turney et al. 2001), but a new dating method capable of reaching beyond the 'radiocarbon barrier' (Roberts, Jones and Smith 1994) was poised to enter the scene. In the same year that the 'Ions and eons' paper (Jones 1982) appeared, the results of two separate studies were published that showed the widespread potential of dating late Quaternary deposits by TL to determine the time that sediments were last exposed to sunlight (Singhvi et al. 1982; Wintle and Huntley 1982). These studies were quickly followed by others (e.g. Wintle et al. 1984), and the potential for archaeological application to the Lindner Site was quickly seized upon by Rhys (Jones and Johnson 1985:182-3):

> The Lindner Site, Nauwalabila I, is one of several sites now found in Greater Australia (Jones 1979) with evidence of human occupation stratified well beneath the lowest surviving charcoal. It is one of the sites which must be tested to see whether or not we can obtain direct evidence for the occupation of Australia greater than the ca. 35–40 kyr limits on conventional radiocarbon dating methods. We have speck-sized samples of charcoal down to Unit 62 at a depth of 2.20 m which might be dated by accelerator methods and, beyond that, one potential area of enquiry might be via new developments in the thermoluminescence method (cf. Wintle et al. 1984).

It was this final sentence that prompted me to phone Rhys in 1987. But our initial attempts to obtain permission to re-excavate the Lindner Site were delayed by the sad death of Nipper Kapirrigi, who had been a strong Aboriginal supporter of Rhys' previous work in Deaf Adder Gorge and a participant in the excavations at the Lindner Site. A two-year ban was placed on entry into Deaf Adder Gorge. So Rhys proposed instead that we head into the field in September 1988 and test the TL method at Malakunanja II and Malangangerr, two rock shelters situated 70 km further north. The existing ^{14}C chronologies for these sites indicated that both were of Pleistocene antiquity. We collected our TL samples by hand-auger, with the assistance of Christopher Chippindale, then editor of the British archaeological journal ***Antiquity***, who happened to be in the Northern Territory at the time.

The initial set of TL ages for Malakunanja II indicated that the lowest artefacts encountered by Kamminga and Allen (1973), and by ourselves during augering, were between 50,000 and 65,000 years old. A hush descended on Rhys when I told him these results — a reaction that I now recognise as Rhys in contemplative rapture — and for a while we enjoyed the simple pleasure of being the only two people on the planet to know the age of a momentous event in human prehistory: the time of arrival of people in Australia. But Rhys cautioned that we now urgently needed to carry out a controlled excavation of the deposit to check that the lowest artefacts were in primary depositional context and to obtain tighter stratigraphic control on the TL ages. In her article 'The prehistory cowboy strikes again', Nicklin (1990:93) quotes Rhys as saying:

> We went very quiet about what we had found, not wanting to shoot our mouths off ahead of time. We decided to go back in July the following year [1989], taking with us Mike Smith who had formerly worked at the Northern Territory Museum… .
>
> Mike has very good credibility — and a very good reputation for finishing things, which I have not. He has a long interest in the area and had worked in the central desert. He is conservative.

And so the three of us — the 'cowboy', the conservative, and the chronologist — duly excavated the Malakunanja II deposits in July 1989, under the watchful eye of Big Bill Neiiji, the traditional Aboriginal custodian of the site. The TL dating results were published in the 10 May 1990 issue of *Nature* (Roberts et al. 1990), and a photo of the excavation team, with Rhys in his customary field attire, was printed in the December 1991 issue of *Antiquity* with the caption 'Rugged in appearance, and no less rugged in character and in their manners' (Chippindale 1991:762). Our most important, and controversial, claim was that humans had reached Australia as early as 60,000 BP — and thus that the 'radiocarbon barrier' had finally been breached by using this different dating technique. Predictably, there were many sceptics, and with a mixture of excitement and trepidation, at least on my part, we gave a public presentation of our results in the Department of Prehistory at the Australian National University on 25 May 1990. Nicklin was in the audience to capture the mood (1990:92):

> The ANU seminar room had standing room only — late-comers climbed under tables, sat on the floor, stood against the walls. 'They've all come out of the woodwork today', said Australia's grand old man of archaeology, Professor John Mulvaney. Each member of the team had half-an-hour to speak: Rhys Jones — not an easy man to limit to half-an-hour — was first… .
>
> Three hours after the start of the seminar, the speakers and members of the audience retired to the cellar bar to continue the discussion. Argument will come later.
>
> Will the date stick? Is the date real? 'Our work will be corroborated or rejected within the next five years,' said Jones, downing a beer. 'The test of the pudding will be replication.'

Rhys already had the perfect pudding tester in mind — the Lindner Site. Two years had now elapsed since Nipper's death, and immediately prior to excavating Malakunanja II we had obtained permission to enter Deaf Adder Gorge and collect fresh sediment samples for TL dating from the cleaned faces of Rhys' earlier excavation at the Lindner Site (Jones and Johnson 1985).

But a new calamity almost scuppered Rhys' plans. I was ready to date the Lindner Site samples in 1991 at the Australian National University, and had post-doctoral funding in hand from the Australian Institute of Aboriginal and Torres Strait Islander Studies. But on the same day that I submitted my PhD thesis for examination, I was told by the Department of Immigration that I must leave the country within 48 hours and could not return to Australia for a minimum of two years! It transpired that I had inadvertently overstayed my student resident visa in the throes of completing my PhD thesis. Rhys moved swiftly and decisively. In less than 48 hours, using only his legendary powers of persuasion and the commendation passed in the Senate for our work at Malakunanja II, Rhys managed to overturn the original ruling of the Department of Immigration and prevent my imminent deportation.

My blushes having being spared, it is perhaps ironic that I took the Lindner Site samples to the United Kingdom for dating in the Research Laboratory of Archaeology and the History of Art at Oxford University. By the early 1990s, a luminescence dating technique known as 'optical dating' (Huntley et al. 1985; Aitken 1998) had become the 'cutting edge' technology for sediment dating, and was set to supersede TL dating. Only two luminescence dating laboratories in the world had the laser-light capability then required for optical dating of quartz sediments, and one of those was at Oxford. Optical dating of the Lindner Site samples appeared to offer a stringent test of the TL-based chronology for Malakunanja II. Would we obtain similar ages for initial human occupation at a different site using an improved luminescence dating technique?

The answer was yes (Roberts, Jones et al. 1994; Roberts and Jones 1994). The lowest artefacts at the Lindner Site were bracketed by optical ages of 53,000 and 60,000 years — almost identical to the TL ages for initial occupation of Malakunanja II. The scientific requirement for replication of results had been met, though not to everyone's satisfaction. To quote George Chaloupka, the veteran Kakadu rock-art researcher from the Northern Territory Museum (Schultz 1995:37):

> Everybody's sceptical about Rhys because he tries always to be at the forefront of technology in archaeological excavation...The others just sit back, nod and criticise. But when I came to Australia in 1950, they said Aborigines were here only four or five thousand years. Now look.

After more than three decades, Rhys continues to be at the forefront of dating in Australian archaeology. He has been involved in developments in single-grain optical dating of mud-wasp nests and radiocarbon dating of mineral crusts, and their application to rock paintings in the Kimberley region of northwestern Australia (Roberts et al. 1997; Watchman and Jones 1998; Watchman this volume). Rhys was also a key participant in single-grain optical dating of the Jinmium and Malakunanja II rock shelter deposits. The results for Jinmium indicated a Holocene occupation history (Roberts, Bird et al. 1998), whereas the single-grain ages for Malakunanja II confirmed the original TL age estimates of 50,000–60,000 years for human arrival and showed that post-depositional disturbance of the artefact-bearing sediments has been insignificant (Roberts, Yoshida et al. 1998; Roberts and Jones 2000). We have recently completed a study on the timing of megafaunal extinction across Australia, and are in the process of systematically dating the Kimberley and Kakadu rock-art sequences. Each of these dating projects, and those to follow, will paint a more complete picture of human prehistory, in adherence to Rhys' long-held creed (Jones 1982:33) that: 'If archaeometry is not archaeology, it is nothing.'

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'Your Obedient and Humble Servant': Notes for an Antipodean Antiquary

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'AMONG [JOSEPH] BANKS'S many activities still awaiting professional assessment, his archaeological undertakings are not the least important' (Lysaght 1974a:221). In fact, it is not just the archaeological undertakings of Sir Joseph Banks (1740–1820) which await full assessment despite some major studies (Cameron 1952; Carter 1988). In a remarkable life, Joseph Banks, Squire of Revesby Abbey in the County of Lincolnshire, was admitted Fellow of the Society of Antiquaries of London in 1766 and in 1774 as member of that tavern-based club for the wealthier gentleman, the Society of Dilettanti. In the very same year that he was elected to the Antiquaries, Banks became Fellow of the Royal Society of London and President of the same Society in 1778, a position which he held until just one month before his death. *Ex officio* Banks was also a Trustee of the British Museum while, apart from playing an influential rôle in the politics of the day, he had a life-long interest in natural history. This began with Banks' largely undistinguished school days at Eton and culminated with his being granted by his admirer and champion King George III the position of Special Director of the Royal Botanic Gardens; Kew's standing as a pre-eminent scientific centre was largely due to Banks' own support. Lesser skills, though hinted at in his letters, are less well known; that Banks played the flute — and taught himself the guitar (Lysaght 1971:76) — comes as no surprise since musical accomplishments were then simply part of the education of many persons of even comparatively slender means. In parenthesis, one may recall that another Lincolnshire man and circumnavigator much supported by Banks, Matthew Flinders, found some consolation during his seven-year incarceration on the Île de France (Mauritius) playing flute sonatas with the

1. Following Flinders' removal to the cooler hinterland of the island where he was billetted on the estate of a Madame D'Arifat, in the journal he kept between 1803 and 1814 (given to the Mitchell Library, Sydney by Flinders Petrie in 1922: ML ref.Z Safe 1/58; for a fascimile see Ingleton 1986b), he describes on 25 October 1805 his evening routine: 'After tea which is usually served at half past six, we retire to the parlour for the evening, which is passed in reading French or English, in conversation, or sometimes in singing and flute playing ... .' That Flinders counted the nearby de Chazals amongst his closest friends on Mauritius can be ascertained by looking at the journal where there are no fewer than 50 references to the family. Early on in their acquaintance Flinders notes on Wednesday 25 November 1805: 'This morning I went by appointment to spend the day with Mr. Chazal ... . The afternoon was occupied with music, in accompanying Mad. Chazals harpsichord with the flute. This lady is indeed an excellent performer, and is besides one of the most agreeable women I have ever met with'. On a subsequent visit, Flinders, having caught de Chazal 'sketching his portrait on the back of a sonata [score]', the latter exclaimed: 'But seriously, you will let me paint you? The subject, if not the execution will then reserve for me a place on Olympus.' On 26 December 1806 Flinders writes: 'Went this morning by invitation to spend the day, with Mr. Chazal, to let him take a copy of my face, of the natural size, of which he finished the bouche. Spent the afternoon and evening at tric-track, in walking, and music very agreeably.' After five sittings, on 9 January 1807 we read 'After breakfast went again to give Mr. Chaz/al another sitting, and the portrait not being quite finished, I remained the night, and the next day Saturday 10, it was finished by noon'. Prior to its purchase in September 2000 for the Art Gallery of South Australia, this, the only surviving full-size portrait of Flinders painted from life, was last on public view in the Library of Flinders University during the bicentennial celebrations of white settlement of Australia in 1988. Prior to its purchase by Alan Bond in 1987 for $A451,000, the portrait had remained in the de Chazal family through five generations; in September 2000 it was once more on public display, this time in the Art Gallery of South Australia who had purchased the portrait through the liquidator of Bond's financial empire (Eiser and Smith 1988:cat. No.388; Mian 1988; Pineo 1988; Ingleton 1986a: pl.II; see also *Art Gallery of South Australia News* 9(6), 2000 with colour illustration).

wife of a local painter, the Paris-trained Toussaint Antoine de Chazal de Chamarel (1770–1822)[1]. Aspects of Banks' association with major expeditions such as that to the South Seas carried out in the company of Lieutenant James Cook aboard H. M. Bark *Endeavour* between 1768–71 have been well documented (Beaglehole 1963, I:1–126 especially on 'the young Banks'; Lysaght 1980). Equally documented has been Banks' own earlier expedition to Newfoundland and Labrador in 1766 (Lysaght 1971) and his later visit to Iceland in 1772 (Rauschenberg 1973), after Banks had withdrawn from Cook's second voyage following the Admiralty's refusal to refit the *Resolution* to his scientfic specifications (Beaglehole 1963:appendix V) — but these are exceptions.

One impediment to a total understanding of this extraordinary man is the breadth of Banks' many life-long interests. His many journals and other writings, notably his enormous surviving correspondence, have only comparatively recently been subjected to the kind of detailed scholarship that they demand (Dawson 1958, 1962, 1965; Lysaght 1971:chapter 13; Chambers 2000). Thus, the standard life of Banks is that of Edward Smith (1911), some 115,000 words cut down from a manuscript originally twice that length and, as such, far from adequate. There are other important sources as for example the detailed description by the Reverend William Sheffield, then Keeper of the Ashmolean Museum in Oxford, of Banks' own extraordinary ethnographic, natural history and geological collections, as they had been ordered at Banks' London house in New Burlington Street in 1771 by a young Edward Jenner, later the pioneer of smallpox innoculation — and yet another flautist as well as player on the violin (Lysaght 1971:253–6).

The following notes, then, are just that — a selection of 18th century samplers offered to a fellow Fellow whose own election to the Society of Antiquaries occurred in 1987, though probably not on as remarkable an occasion as that depicted by Thomas Rowlandson some 200 years earlier (Bruce-Mitford 1951) (Figure 1). On the other hand this Bard of the Gorsedd has not been averse to a bit of botanising and fossicking amongst the great names of the Age of Discovery either on the mainland of Australia or on what for many has become 'Rhys Jones's Other Island' — Tasmania (Jones 1980, 1988, 1999; for a South Australian footnote on the French in New Holland see also Bonnemains 1986; for a first autobiographical sketch of Rhys Jones himself see Jones with Megaw 2000).

Exactly 200 years after Banks' return from the Antipodes, Rhys Jones and Betty Meehan were beginning their ethnoarchaeological research amongst the Anbarra people of central Arnhem Land (Jones and Meehan 1997; Meehan 1982). Back in Sydney, Rhys' mother, the remarkable Enid Watkin Jones, was examining the Banks papers in the Mitchell Library for references to the correspondence between Joseph Banks and the Welsh antiquarian and naturalist, Thomas Pennant (1726–98), correspondence which was to bring the two together in Snowdonia. Pennant's own election to the Society of Antiquaries in 1754 had preceded that of Banks by a few years, and early in their acquaintance Banks tended to defer to the older man — whom he had first met in 1766

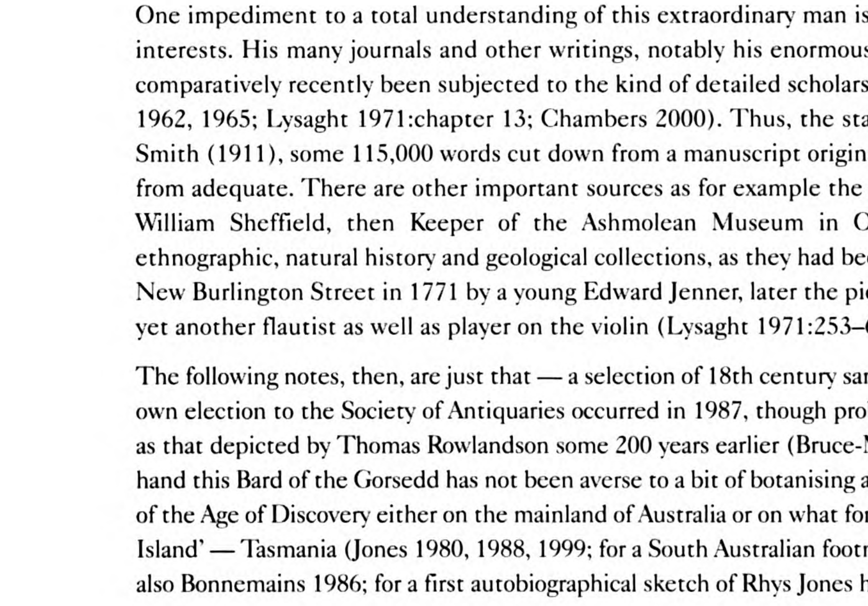

Figure 1. Thomas Rowlandson (1750–1827). 'The Reception of a New Member in the Society of Antiquarians'. Watercolour over pencil. Signed lower left and dated on mount 1782. Centre is the President, Jeremiah Milles DD. Photo: courtesy Society of Antiquaries.

— in antiquarian matters though later there was a falling out when Banks took exception to what can only be called unauthorised publication if not plagiarism. While our honorand was in warmer climes, the present writer, mindful of a decade Down Under (Megaw 1973) was in the East Midlands indulging in a minor study of one of our great intellectual heroes, Joseph Banks himself, a individual he had first become aware of in the 1960s through the researches of Averil Lysaght and Bernard Smith (for Banks and the development of empirical observation and the use of scientific illustration, see particularly Smith 1985:1, 11; and also Smith 1992). Having in a minor way been concerned to track down such artefacts as Banks had obtained in his brief sojourn on the east coast of Australia, it was perhaps natural that in the 1970s I should still be searching for first-hand evidence which might also shed light on Banks the antiquary (for the fullest account see Megaw 1993; see also Megaw 1994). But let us look first for other clues of Banks the antiquarian.

CAIRNS, CHEMISTRY AND COINS

We may begin, arbitrarily enough, in 1767, the year after Banks' election to the Society of Antiquaries, with an extract from a holograph 'Journal / From London to Edwinsford / Began August y^{e} 13th 1767.' [The original manuscript, which concludes on 29 April 1768, is in the University Library, Cambridge; we rely on a transcript held in the Dawson Turner copies in the Natural History Museum, London. A second version, transcribed by Banks' sister Sarah Sophia Banks (1744–1818) is in the National Library of Wales (NLW Ms 147C). A published version appeared in Evans (1922). (Note that throughout this paper, on questions of orthography, when citing correspondence to and from Banks, I have tried to follow as closely as possible the spelling and setting out of the holograph originals.) The 'Journey' which took Banks and his party from the East Midlands to Wales and back notes such chance discoveries as 'a small coin of Gallienus given to me which was found in Ploughing a feild at Gwinow on the Black Mountain' and the section quoted here concludes with the recording of the further gift of 'Some Roman Coins'. As will be noted below, Banks was much involved in numismatics, both ancient and modern. From 1797 as a Privy Councillor he was active in proposing reform of the Royal Mint and monetary systems in general. Both he and his sister, Sarah Sophia, amassed considerable collections, much of which passed to the British Museum, whose inventory of Celtic coins particularly benefitted. Another part formed the nucleus of a collection at the Royal Mint.

Banks' account continues:

> This day Employd in opening a Barrow which I had Long desird to Examine
> Cheifly on account of its situation on very high part of Llansaddern
> Mountain visible from a great many places which inclind me to beleive it
> the Burial place of some person of Distinction whose tomb should stand in
> so Conspicuous a place on the other hand the Size was not at [all]
> remarkable its largest circumference being not above thirty paces & its
> height three yards & a half
>
> We began to open [it] by a trench began at both Endes Level with the
> Soil intending it to meet in the Center as by this method the Earth being
> Constantly made to fall from over our heads gave us a good opportunity of
> Examining the different Substances of which it was made & we were in much
> less danger of Breaking any Brittle Vessell which might occur
>
> ... The first Covering for the thickness of three feet & in some places
> four was of a yellowish Gravelly Mould in which was intermixd a fe/w/
> scatterd peices of charcoal among this Exactly in the Center we found a
> very large rough Stone as large as three men could with some difficulty
> roll down a little Deeper was Lodgd a very Small Peice of Earthen ware but
> so small that it was impossible to guess what it might have been the grain
> exceedingly Coarse much more so even than what we use for garden pots

unglazd & appearing as if never thoroughly Burnt.

... Under this Earth we came to a Bed of stones which Seemd to have been /pild/heap'd upon one another dry as we found no Earth Between them tho there were many interstices into which I could Easily introduce my fist among which we found some few Peices of Charcoal these Stones reachd down to the surface of the soil but we could not Perceive that any thing had been dug away or any hollow place made under them among them was one which I have preservd tho I cannot Guess for what use it can have Servd it is squarish Rough its size 6 inches & 1/2 by 5 & 1/2 in the middle is an oblong hole peircd through it three quarters of an inch in Leng[t]h

... Immediately in the Center of these resting upon the Surface of the Soil appeard a most Singular curiosity it was a Gist Vaen or stone Chest Composd of five Stones viz 4 sides & a Cover the sides Curiously Luted together with Clay its dimensions taken on the inside Leng[t]h 3 feet 8 inches Breadth 2:5 Depth the same as leng[t]h & proportionably larger [?than] that it needed to have been all round the chest itself Bore S b W = on the Northernmost Corner was a hole into which a man might Easily introduce his hand but in Every other part it was perfectly tight

... On Lifting away the covering stone which was done with some dificulty it appeard filld with fine River Sand mixd with Small flattish Stones or peices of Slate which are common among such Sand this reachd within 4 inches of the top on the N = End but not nearer than 8 on the S = all this was perfectly dry owing to its being so well coverd tho Every thing on the outside had been remarkably wet owing to the raininess of the Season

... On Lifting it out which was done with Great Care Every Shovel full being lightly Shaken down we found that in it also was plenty of small peices of Charcoal but nothing Else till we came near the Bottom when to our Great Satisfaction we discovered Bones at the S = End resting upon a Clayey substance mixd with very many small sparks of Charcoal they were hollow Bones either of Arms or Legs but so miserably decayd that it is not Possible to Say which as the Largest peice we were able to get is barely 3 inches in Leng[t]h, they Lay diagonaly across the Chest in two rows with some space between as if the two members they belongd to (Probably from that Circumstance) Legs had been Lay'd in that Situation on Every part of the Bottom of the Chest was plenty of the Clay & Charcoal mentiond before but no where Else any bones possibly that very substance was the Consequence of their decay but in the S = E Corner very near those bones that remaind [was] a most valuable Curiosity as it may give some light into the Date of these monuments it is an arrow head of flint barbd in this form (Fig.2) Several of which I recollect to have seen in the Cabinets of the Curious but do not recollect any before this which were dug out of a Barrow in most of those that have been opend in England that I have heard have been Instruments Either of Brass or Iron whither we may not Conjecture this to be much more Ancient ? whither this will this will [sic] give us leave to Imagine that this Barrow was Erected before the Britains had the Use of

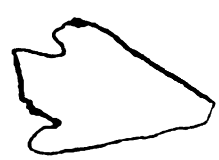

Figure 2.Sketch by Joseph Banks of barbed-and-tanged arrowhead.

Iron or at Least before it was well known to them ? I Leave to more Experienced antiquarians to determine

> ... That this Tumulus was of British origin I think no one can deny & it seems to be a proof that the Britons upon some occasions at Least Burnt their dead before interment a point which several antiquarians & doctor Plot among the rest will not subscribe to but make all Barrows in which Charcoal is found to be Roman
>
> ... If the Charcoal found here should not be thought a sufficient proof of the Bodys having been Burnt the size of the Coffin or Kist affords a still Stronger whose greatest Lengh 3 = 8 is little more than half Long Enough for a tall man to Lay at Lengh upon the same mountain are also several British Remains a Karnea or heap of Stones so Calld within 40 or 50 paces or the Barrow & a small Circular monument within 2 or 300 which is Calld Croos Vair or Marys Cross & is much used by the People in Riding their Rounds as it Lies immediately on the Bounds of the Parishes of Tally & Llansaddern on Entering upon this mountain also on the Edwinsford Side a circle may Plainly be observed in the Grass which seems to have been made of Stone but what it has been I do not at all Guess + Towards the Latter End of this Month some things have happened worth recording tho my Carelessness has omitted the Precise Times

The journal also has archaeological interest for its early description of the Dolaucothi, Dyfed gold mines, often considered to be Roman in origin (e.g. Woods 1987); it is interesting to note that though Banks records the Roman fort a mile away, he does not connect it with the mines. Returning to the Black Mountain and the above extract from Banks' journal, the general location of the sites described — and one should note Banks' interest in the topographical siting of the barrows — appears clear despite his free English translations as well as somewhat phonetic transliterations of the Welsh place names. The main clue is the mention of the Black Mountain itself, an area to the south of the River Tywi which lies on the border of Dyfed and Powys while, situated to the north of the Black Mountain are 'the Parishes of Tally & Llansaddern', obviously the villages of Talley (Talyllychau) and Llansadwrn which today lie just to the northwest of the A40. Banks' ancillary observations concerning a possible 'Circular monument' at 'Croos Vair' (*recte* Croes Fair) and the second circle which 'may Plainly be observed in the Grass' need checking on the ground but have the ring of true field archaeology.

Of interest, between the two parishes mentioned by Banks, lies Cwm Cile Farm which in 1767 was owned by Morgan Rhys, a direct ancestor of our honorand; such are the quirks of history (R.M. Jones pers. comm.). I am also grateful to our honorand's cousin, Dr Prys Morgan, Department of History, University of Wales at Swansea for locating 'Edwinsford' as Rhydodyn ('The ford of Odin'), while Dr C.S. Briggs, Royal Commission on Ancient and Historical Monuments who has described Banks' investigations as the 'earliest recorded cairn excavation in South Wales' (RCAHW 1997:67), locates the site as Pigyn Sion Niclas (National Grid SN66699538). The site was identified by the Ordnance Survey in 1976 as 'robbed to ground level in places, but elsewhere within the perimeter some cairn material is in situ with no traces of a central cist'.

Banks' activities in Wales were known to later travellers; in 1809 Richard Fenton, the Pembrokeshire antiquary, noted 'that Sir Joseph Banks, who, when on a visit some 25 years ago at Edwinsfor ... had in digging in boggy ground on the mountain, found a Hat of felt not decayed; and that about 3 ft lower, a Hilt of a sword was found; and about 2 1/2 ft lower, shoes and something like Buckles, supposed to have belonged to some person, who, crossing the mountains, had been sunk in the Bog' — a nice example of a latter day Oëtzi inexplicably missing from Banks' 'Journal' (Fisher 1917:56 quoted by Briggs 1995:179). Nor was this 'Journal' the first time that Banks had expressed an interest in Bronze Age barrows. In 1767 he had recorded tumuli in Somerset (Perceval 1899) but here Banks' actual excavation technique must be regarded as ingenious rather than effective — it reminds one of the often reproduced illustration from the *Gentleman's Magazine* for December 1852 illustrating the usefulness in inclement weather of digging a jagged slice through the centre of a Romano-British round barrow near Maidstone in Kent (see Wheeler 1954:7 and pl. I). On the other hand, his observations as to stratigraphy and the nature of the burial — not least his totally correct assumption that cremation was not confined to the Roman period — are pertinent indeed and should have earned Banks a place in the history of barrow digging (Marsden 1974:1–24; Ashbee 1960:chap.I). His comments on the flint barbed-and-tanged arrowhead (Figure 2) are equally in advance of his time, though it seems unlikely that here was the first recorded find of this Early Bronze Age type-fossil *in situ* (see for example those found in peat at Llyn Bugeilyn, Llanbrynmair, Powys: Wheeler 1925:132 and fig. 39). If this should seem an unremarkable observation today one should recall that in the eighteenth century there were still many who associated such prehistoric artefacts with elf-bolts shot by fairies or, more prosaically but no less wrong-headedly, with the Romans. It was after all another East Anglian Fellow of the Royal Society, John Frere, who in a famous letter communicated to the Society of Antiquaries in 1797 (published in *Archaeologia* 12 (1800):204) on the basis of stratigraphical observations in a gravel pit at Hoxne in Suffolk made the first tentative claims of extreme antiquity for what today would be classified as Acheulean hand-axes in a memorable phrase stating that they had been 'fabricated and used by a people who had not the use of metals' (see Daniel 1967:57–9). And it was the self-same Dr Robert Plot (1641–1696) with whom Banks takes issue over the use of cremation who, as Keeper of the Ashmolean Museum, had as his assistant Edward Lhuyd, father of Celtic linguistics (Daniel 1967:38–41; Emery 1971; Piggott 1989:92–129). Both countered popular myths by comparing stone artefacts found in Britain with ethnographic specimens from the Americas (Piggott 1989:86).

In contrast, our second example of Banks as archaeologist is as ghastly an example of misplaced scientific enquiry as Banks the barrow digger might be regarded as a praiseworthy pioneer. All the more ironical, then, that the incident to which we refer is in fact the one which has received most attention in modern times (Piggott 1959; Hunter in press). In 1768 an incomplete bronze object was dredged from the River Witham at Tattershall Ferry not far from Revesby Abbey. First published by a Dr George Pearson (1796:esp. 395, plate xi) and subsequently noted by John Kemble and published posthumously with a re-drawing of part of the original illustration by Orlando Jewitt (Kemble et al. 1863:171, plate xiii.2), it was recognised for what it most certainly is — or was — 'a Celtic war trumpet' or *carnyx*. Banks seems to have been the instigator of a detailed pen-and-wash drawing used as the basis for the later illustrations by, in turn, Pearson and Kemble and which, though undated, formed part of Banks' papers sold at auction after his death. Ultimately these papers returned to Lincolnshire and the ownership of W.F.R. King-Fane of Fulbeck Hall, Grantham, by whom they were given to what is now the Lincoln Central Library of the Lincolnshire County Council. The artist of the Tattershall Ferry *carnyx* was probably J.C.Nattes (?1765-1822) (Figure 3; ref. Banks 4 f.317). The additional annotations, omitted in all previous illustrations, are interesting and suggest that the drawing was intended for publication. The sheet is captioned below the double ruled border 'Witham' while top right is written in ink 'Tab.8.'. Along the left margin 'An Ancient Musical Instrument the Lituus of the Barbarian Nations represented on the Base of Trajans Column at Rome / Found in clearing out the bed of the River Witham near Tattershall Ferry about the year [omitted]'. Above the shorter section of the *carnyx* is pencilled 'Feb xii Fig 1.' while at the bottom of the sheet a drawing of the obverse and reverse of a Roman Republican coin has been pasted on, labelled 'Fig 2', with the additional pencilled note 'To be / engrved'. The coin is captioned: 'an ancient medal / on which the Lituus is represented'. It is typical of Banks that he should have made the link with Roman coinage, in this case thanks to the clarity of the drawing, a denarius of D.Iunius Brutus Albinus struck in Rome in 48 BC (Crawford 1974:no. 450.1). So far so good, but the question must obviously be asked, what of the actual instrument? Here we must return to Pearson's account which is in fact an early example of destructive — very destructive — metallurgical analyses applied not only to the *carnyx* but to a number other artefacts dredged from the Witham and in Banks' possession. Piggott (1959:20) extracts the major horror from the account of Pearson (1796: 405), who observes that Banks' 'zeal for science induced him to sacrifice them to chemical analysis' and goes on to comment on the necessity of comparing freshly fractured surfaces of metal'. Pearson continues: 'I therefore melted the old implements, and cast them into the same ingot mould … . … each of these ingots was fractured by a pretty smart stroke with a hammer'. Pearson ascertained that the metal was a tin-rich bronze, 88% copper and 12% tin. Alas, though, exit one of the very few surviving Iron Age war trumpets ever recorded in Europe.

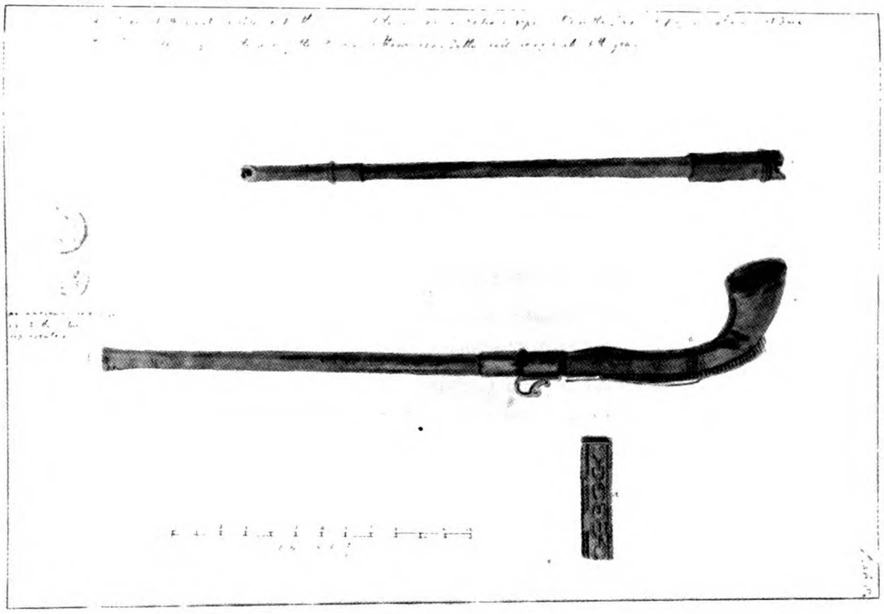

Figure 3. ?J.C. Nattes. *Carnyx* from Tattershall Ferry, R.Witham, Lincs. Watercolour over pencil. Unsigned. Local Studies Collection, Lincoln Central Library. Photo: Courtesy Lincolnshire County Council, Education and Cultural Services Directorate.

Banks' interest in the Fenland was not restricted to the occasional collection of antiquities but extended to devising means for controlling its drainage (see Lysaght 1971:plate 43 for the formal portrait of Banks now in the Guildhall, Boston, Lincs., painted towards the end of his life c.1814 by Thomas Phillips. This shows Banks holding a map of the fenland drainage with, behind him, other maps of the fens) and we shall return to the fens shortly. Firstly, however, we must briefly recall Banks' one sortie into the ethnography of the Great South Land. Banks was largely responsible for the scientific organisation of Cook's voyage in the *Endeavour*, another major event greatly assisted by his friend and First Lord of the Admiralty, the Earl of Sandwich. John Montagu, 4th Earl of Sandwich (1718–92), and at various times First Lord of the Admiralty, was one of Banks' Chelsea neighbours and another musician as well as a passionate fisherman. Sandwich had estates in Lincolnshire and Banks had fished with him when he was a boy, a passion they continued at night on the Thames. Out of this friendship developed the close association which was to assist Banks' planning not only of his expedition to Newfoundland but also the voyage to the Pacific on the *Endeavour* (Lysaght 1971:46). The four fishing spears now in the University Museum of Archaeology and Anthropology, Cambridge, all that survive from 'the 40 or 50 spears' collected by Banks at Botany Bay in April 1770, artefacts described in his journal some four months later (Beaglehole 1963, II:132–3), were given by Cook to Sandwich who in October 1771 donated them to his old College, Trinity (Megaw 1993:26-9, figs 1–4).

Less well known as having been collected by Banks from Botany Bay is a bark shield in the Department of Ethnography at the British Museum recorded simply as 'Captain Cook Acc.'. However, both the 'Shield from New Holland' in a pen-and-wash drawing signed and dated '1771' by John Frederick Miller (*fl.* 1768–80) (Figure 4) — apparently originally from a series being prepared for engraving — and Banks' own description of a shield abandoned by the one man who attempted to oppose the landing, strongly support the identification of the shield as one collected by Banks and brought back with him to London (Beaglehole 1963, II:133; Megaw 1993:28, figs. 5a–c, 6, 1994).

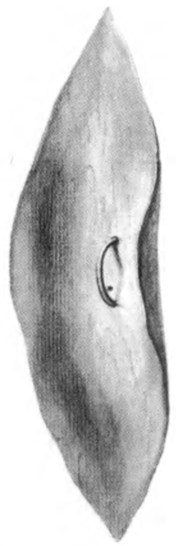

Figure 4. John Frederick Miller. '2. Shield from New Holland' Detail of pen and wash drawing. Signed and dated 1771 lower right, British Library Add. MS 23920 f.35. Photo: British Library.

John Frederick Miller was one of 27 children of the botanical artist, Johann Sebastian Müller. Both father and son were employed by Banks to complete the scientific illustrations from the Endeavour expedition left unfinished by the death at sea of Sydney Parkinson (for notes on Banks' scientific illustrators see Lysaght 1959:259–65). In 1772 Miller, together with his brother James and two other draughtsmen formed part of the small team that Banks, no longer involved with Cook's second voyage, took with him on board the brig *Sir Lawrence* on a somewhat shorter voyage of scientific enquiry — to Iceland by way of the Hebrides, returning via the Orkney Islands (Rauschenberg 1973; Lysaght 1974a, 1974b). While on Orkney, Banks noted in somewhat telegraphic manner '16 October See old lead works copper walk to Circles of Stone temples of Sun and Moon Druidical Places of Judgement bridges whether or no coeval with circles Tumuli.'

One of Banks' two servants, James Roberts, is rather fuller; his journal for 18 October reads: 'Little wind and pleasant weather, went this morning with Mr. Miller, Mr. Cleveley [both artists] and Mr. Walden [a marine surveyor] to a place called Stenhouse to assist them in measuring some Stones which stand a little distance from each other in the shape of a half moon, some of them were twenty feet high, six feet wide and about a foot and a half thick.'

The Stones of Stenness (Figure 5) and the nearby Ring of Brodgar on Mainland (Ritchie 1985) are amongst the most northerly of the 900 circular banked henge monuments of which Stonehenge and Avebury are simply the best known (Burl 1976:esp. 99–102). Banks' party certainly were fascinated by the Orkney monuments and spent some time in planning and drawing them. Miller's own impression of Stenness completed in 1775 (Figure 6) includes the standard conceit of depicting the artist himself in the foreground; one fancies it was more to his liking than the delineation of a 'Shield from New Holland'. Excavations in 1973–74 established that Stenness has a ^{14}C age of ca. 4000 BP. For at least two centuries stone circles have sparked considerable discussion as to their makers' utilisation of standard measures and astronomical observations (Ruggles 1996, 1999). Lysaght (1974a:231–3) has suggested that Banks himself may have been the catalyst for such theorising in his own day. Be that as it may, Banks the excavator was soon at it again.

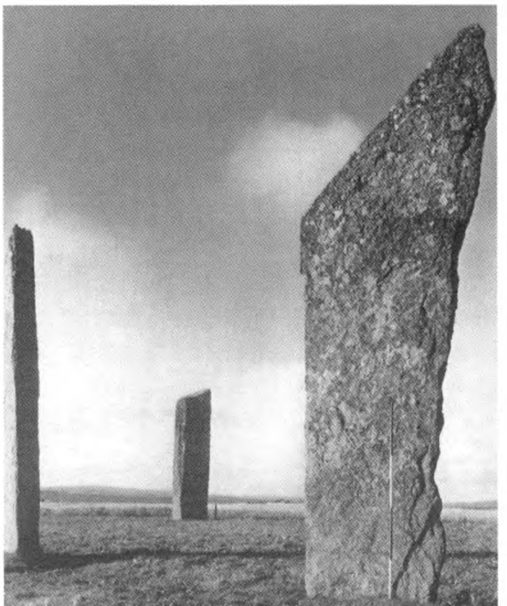

Figure 5. The Stones of Stenness, Orkney. Photo: Crown Copyright — Royal Commission on the Ancient and Historical Monuments of Scotland.

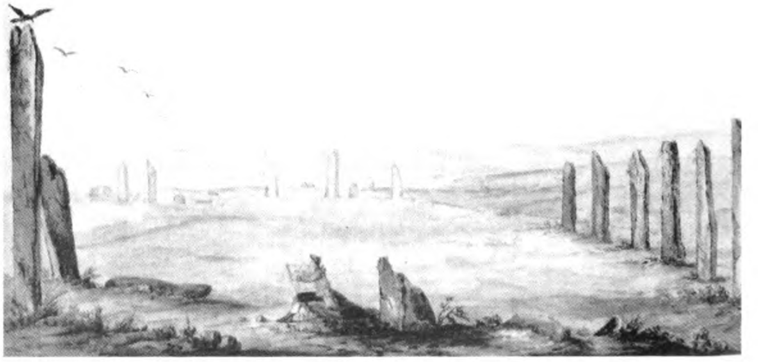

Figure 6. John Frederick Miller. The Stones of Stenness. Watercolour over pencil. Signed and dated 1775 lower right (the picture bears *recto* a note 'based on his pen and ink sketch of the stones of Stennis, 1772, known then as the Circle of Loda'). British Library Add. MS 15511 ff.7–8. Photo: British Library.

There is a considerable folklore in the Highlands and Islands concerning what might be termed the archaeology of the dead; see for example the present writer's first foray into print (Megaw 1957). Banks on this subject is brief almost to the degree of incomprehensibility but key phrases occur in his diary entry for 18 October:

> ... Burying places in the Links people of Barra superstitious in relation to the Standing Stones Story of Girl 8 ... Vitrified burnt stuff near the Parsons with bones burying places coffins of Slate 4-3 2.4 sides & top no bottom immense abundance of Tumuli commonly covered with stones ... in one coffin beads 400 locket with Dr Ramsay found among ashes bones in the same Coffin hair

Fortunately once more Roberts is much fuller than his master and his record for 21 October reads in part:

> Moderate Breezes and Hazy weather, at four this mornng Mr Banks, Dr Solander [the Swedish botanist who had made so important a contribution to the *Endeavour* expedition], with the rest of the Gentleman, and Servants, and many of the Natives wth Spades went to a place call'd Sandwick, where we open'd two Ancient Tombs, or Tumuli, in each of them was found the Bones of a man, and woman, the form of their Interment was somewhat Singular, they were laid in a very coarse mat which was entirely rotten. ... the man was laid with his feet nearly up to his Chin, which perhaps was the Custom of the times. ... . The draftsmen made drawings of both the Tombs. In the Evening our party (which consisted of about thirty hands) return'd to Stromness we had with us our two French horns and Saunders with his Violin, so that we spent the day very agreeably.

Leaving aside yet another musical note, one may readily identify the location, the Links of Skaill some 10 km northwest of the Stones of Stenness and close by that other famous Orcadian prehistoric site, the Neolithic 'village' of Skara Brae, excavated between 1927–31 by that most famous of Australian archaeologists, Vere Gordon Childe (1931; see also Clarke and Sharples 1985).

The Society of Antiquaries has the earliest printed record in the form of a letter communicated to the Society and published in *Archaeologia* 3, 1786, the original writer being the Reverend George Low (1746–95), an enthusiastic naturalist earning his living as a tutor to a family living at Stromness. Low also describes the 'bag of some very Coarse Vegetable stuff ... containing the Bones of a Younger person'. While Walden's survey of the Links of Skaill has survived, so too has a sketch of the crouched burial with the bag (Figure 7) and while it is no longer possible to pin-point the burials, there seems little doubt but that they were cist burials typical of the Early Bronze Age.

Figure 7. Artist unknown. Links of Skaill, Orkney. Pencil sketch of skeleton and bag of bones; *verso* detail showing bag. British Library Add. MS 15509 f.56. Photo: British Library.

What other glimpses of Joseph Banks the antiquarian can one find? 'Roman antiquities' and even Roman excavations are frequently alluded to. Amongst the letters in the Natural History Museum there is John Lloyd writing to Banks from Ruthin on 18 October 1787 noting the excavation on the Snowdonia estate by the Duke of Ancaster — Brownlow Bertie (1729–1809) 5th Duke of Ancaster and Kesteven and Lord Lieutenant of the County of Lincolnshire — of 'a Roman Villa, the hypocaust was almost pulled to pieces before it was known what it was; but what remains is perfect. ... I wish you would ask the Duke permission for me to dig about the Place to make what discoveries I can — I need not tell you that the greatest care shall be taken, and the whole of the digging performed under my own Eye ... .' Clearly Banks' standing as a field archaeologist as well as a great collector was common knowledge amongst his contemporaries — whatever we may think of him today. On 23 August of the following year Banks writes to Lloyd and his letter is worth quoting in full since it gives more than a little impression of its author and his insatiable curiosity in all things (Chambers 2000:no. 32):

To John Lloyd F.R.S.

Revesby Abbey
23 August 1788

My dear Lloyd,

I thank you heartily for 2 brace of Grouse, which indeed My Friends in London have Eat, but that is the same as if they had fallen into my own Share.

I am glad you had so much amusement in your Tour [of] Artoun in the Isle of Mull. I never Saw, nor did I ever hear of, Basaltic Pillars resting on Coal, which, if certain, appears to me a very Curious discovery, & very interesting to those Who search to discover the mode of their Chrystallisation. I shall be particularly glad to see an account of them presented to the R.S. [Royal Society], who Know the Abilities of your Companion so well as to wish to hear from him whenever he has leisure to Communicate his Observations to them.

I Observed the Whyn Dikes [Whin Sill], and have some good Drawings of them, which shall be at your Service if you have any thing to say on the subject. Your magnetic mountain is not a very unusual thing. In my Travels I have met with many.

The Scouring out of the Witham here has produced a most Extraordinary lot of Danish & Roman Antiquities. If I had you here to ride about, & assist to Collect them from the various hands into which they have fallen, I think they would make a most Curious Collection. I got last year above 20 articles, many of them Quite unique, and this year a larger Quantity of River has been Cleard out, but my harvest has not begun.

How do you go on with Your Digging on the Duke of Ancaster's Estate? He always asks me when I see him.

The Ladies have all desired their best Compts to you, as does Mr. Wheeler, who is with me. I do beleive you would like to hunt in our Antiquities, among which are some I have no doubt of first rate Curiosity.

Yours Faithfully,

Jos: Banks

Reading such letters it is hardly surprising that Banks should have had the Tattershall Ferry *carnyx* in his possession — if only briefly. On the other hand, as Lysaght has observed (1974b:289), despite the mass of material in his New Burlington Street house, he does not seem to have retained a large collection of archaeological discoveries and there is very little archaeological, let alone ethnographical, that can be ascribed to him in the British Museum. He frequently gave such material away. For example, the Society of Antiquaries has a fragment of a life-size Roman equestrian statue in bronze, and part of the foreleg of a horse found in Lincoln (Roman *Lindum*) some time before 1800, which Banks submitted to the Society on May Day 1800. The fragmentary bronze which Banks subsequently donated and which remains in the collections of the Society (Richmond 1944:5-9), shows signs both of burning and having been deliberately broken away from the rest of the statue, perhaps to be associated with the Saxon incursions of the 5th century AD.

Clearly Banks in his dual rôle as influential Fellow of both the Royal Society and the Society of Antiquaries was in a good position to glean information on antiquities from many sources. For example, amongst the Natural History Museum letters is one to Banks from yet another Fellow of the Royal Society, Thomas Johnes (1746–1816). Writing from Hafod in mid-Wales on 2 April 1796 (Dawson 1958:476) he begins:

> Dear Sir Joseph
>
> I had the pleasure of your letter last post, and sent it to my Brother-in-law at Dolecothy [Dolaucothi] to desire he would make the offer in your letter to recover as many of the Golden or/naments as possible - They were ploughed up in a field on the opposite side of the River … . I forgot to mention that the stone which has been forced out of this ring was of a green colour.
>
> Whether these monuments were Roman or Druidical I am too ignorant to determine but the work appears to me superior to any thing I have ever seen of the Druids - The Gold from its great ductility seems extremely fine .-

One should note that here one is in an era before the periodisation of past time as we know it; C.J. Thomsen (1788–1865) was yet to promulgate his Three Ages system, first devised for ordering the collections of the National Museum of Denmark (Daniel 1967:92–7). We have already noted John Frere's first tentative claims for extreme antiquity but archaeological discoveries made in Britain were largely categorised as Druidical (or Celtic), Roman or Danish.

As to the gold, in the absence of any more detailed description or illustration one can only hazard a guess; not infrequently such objects would be exhibited at the Ordinary Meetings of the Society of Antiquaries, a practice which continues to this very day. For example, Thomas Pennant, made a Fellow in 1754, showed a 'Golden Torques' as it is noted in the Minute Book of the Society (8 November 1770 to 18 March 1773, v.12, f.261). The drawing which notes the account given 'by the famous Antiquary Mr. Lloyd' is of a ribbon or flange-twisted torc characteristic of the Middle Bronze Age in the West of Britain and Ireland (Megaw and Simpson 1979:252–3, fig.6.4:5; Taylor 1980:60–4, Map 4).

Another London-based antiquary who corresponded frequently with Banks was the Reverend Samuel Lysons FRS FSA (1763–1819). One may forgive Lysons his promulgation of the theory that Hebrew was the language of the Ancient Britons (Piggott 1989:56). Lysons was a pioneer in the systematic study of Roman Britain; his excavations between 1793 and 1796 of the huge complex of buildings at Woodchester, Gloucestershire — some 64 rooms arranged round three courtyards — and his reconstruction of the façade of the temple of Sulis Minerva at Bath, remain key documents. Lyson's many publications culminated in his massive *Reliqiae Britannico-Romanae* which were published in three volumes between 1813 and 1817, and illustrate in meticulous detail many mosaic pavements now long since destroyed (q.v. Rivet 1969:61 ff., plates 3.11, 13–14). On 2 October 1797 we find Lysons on a visit to the Fens and writing from Lincoln to Banks at Revesby (Dawson 1958:561):

> … I stayed about a fortnight in Dorset-shire, when I quite finished my investigation of the Roman antiquities near Frampton; there was little more to be discovered, but it was satisfactory to ascertain that point, which I did completely, and also explored the substrata etc.- I had a *grand encampment*, three tents, an artillery Waggon, and two & twenty men. I took a copy of my Woodchester work to his Majesty (the only one that had been completed) with which he seemed much pleasd - All the Royal family went over one morning to Frampton to see the Mosaic pavements.

Lysons continues to describe his visit to Horkstow just southwest of Barton-upon-Humber, 'where I was received with great hospitality by Admi / ral Shirley unluckily he was very busy … we found nevertheless one Mosaic pavement, besides that of which you saw the drawing.' After commenting unfavourably on a Mr Fowler's drawings of mosaics, Lysons continues: 'I hope you will some time or other see the original pavement, which the Admiral says that he shall be very happy to show you. I have prevailed on him to erect a building over the most cu / rious & most perfect part of it.'

Part of the Horkstow mosaics are preserved in the British Museum (Smith 1969:102–7, fig. 3.2) but, apart from the visions conjured up by Lysons' letter of an excavation of positively Near Eastern proportions, one notes with approval his concern to preserve the site.

As seen, Banks had considerable interest throughout his life not only in ancient and modern coinage but also, particularly in later life, in many aspects of contemporary monetary systems; thus his correspondence is full of discussions as to how the price of gold should be fixed by the Bank of England. His ideas are never less than ingenious; writing from Soho Square to John Pinkerton on 9 May 1798, Banks, in thanking Pinkerton for the advice he has been given on silver coinage, believes that hard coins rubbed together will wear as quickly as soft ones; indeed he believes that Romans did not carry coinage in pockets and this accounts for the good condition of the Roman coinage found!

We have already noted the coin collections made both by Banks and his sister Sarah and his correspondence shows how he continued to collect and to dispose of his coins as he grew older. On 6 June 1808 Banks replies from Soho Square briefly to an unnamed benefactor as follows (Figure 8; the letter was purchased in 1973 by the present writer from Francis Edwards Ltd. Autograph letters and manuscripts, Cat. 971, item 37):

> Sir
>
> I beg of you to accept my thanks for the Ancient Coins you have been so good as to send to me, similar ones have been found on Jersey on Guernsey on the Coast of Devonshire & on that of France but seldom if ever in the inland this has induced some antiquaries to suppose it is the Coin of the Phoenicians who traded with our Early ancestors for Tin as however none of these Coins have letters on them the Question of to whom they originaly belonged must remain forever unsetled.
>
> If when you Come to London you will allow me to thank you in Person I Shall be much Flattered being Sir
>
> Your Obed Hble Servt
>
> Jos: Banks

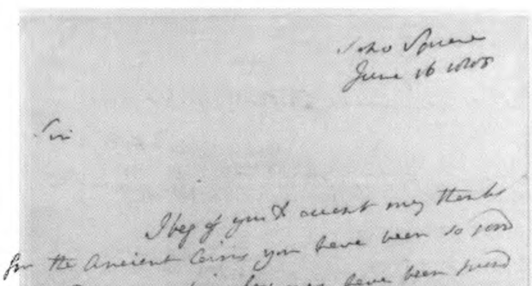
Soho Square
June 16 1808
Sir
I beg of you to accept my thanks
for the Ancient Coins you have been so good
as to send to me, similar ones have been found
on Jersey on Guernsey on the Coast of Devonshire
& on that of France but seldom if ever in
the inland this has induced some antiquaries
to suppose that it is the Coin of the Phoenicians
who traded with our Early ancestors for Tin
as however none of these Coins have letters
on them the Question of to whom they originaly
belonged must remain forever unsettled
If when you Come to London
you will allow me to thank you in Person I

Figure 8. Sir Joseph Banks. *Recto* of a letter dated 'June 16 1808' to an unknown correspondent referring to receipt of 'Ancient Coins'. Private collection.

What is interesting here is not so much the reference to Phoenician traders which was one of the longest lasting antiquarian myths about ancient Europe beginning in the late 16th century (Piggott 1989:esp. 100-2; see also Oscar Montelius' robust denial of such flights of fancy at the end of the 19th century: Daniel 1967:123-6). Here it is Banks the numismatist who is to the fore. Banks was not the first to interest himself in such things; that pioneering antiquarian, and onetime Headmaster of Winchester School, William Camden (1551-1623) illustrates his *Britannia* published in 1600 with a plate showing various inscribed ancient British coins — *Nummi Britannici antiqui* — which clearly show coins of the immediate post conquest period, especially coins struck by the Catuvellauni at Colchester (*Camulodunum*) (Daniel 1967:35–7, plate I). More than a century later another great figure of early antiquarian thought, the pioneering field archaeologist William Stukeley, found himself, as Secretary of the Society of Antiquaries, partly responsible for the British coinage section of a project entitled *Metallographica Britannica* (1687–1765). Stukeley's coin identifications were at times both rare and curious, though on other occasions he can be found making sensible comparisons between British coins and their putative

Roman prototypes (Piggott 1985:71, 140–2).

Figure 9. Brittany. 'Coriosolites'. Billon stater obverse and reverse. Late first century BC. Diam. 20mm. Wt. 6.6g. Acc. no. 6692, Cabinet des Médailles, Bibliothèque Nationale, Paris. Photo: Bibliothèque Nationale.

But to return to our letter. From the description, brief and un-illustrated though it is, there can be little doubt that the coins referred to must have been billon or base silver staters struck around the time of Julius Caesar's campaigns in Gaul in the mid-1st century BC, if not also somewhat later. These are ascribed to the Armorican tribe, the Coriosolites, and some 30,000 coins have been found in northwestern France and on Jersey (Figure 9) (de Jersey 1994:esp. 95–9). Idle even to surmise but it would be good to identify whether the coins mentioned in Banks' letter survive, for example in the British Museum, but there is much concerning this fascinating man and his multifarious activities which would repay more work than this brief encounter with him has allowed.

ENVOI

While it is easy to belittle much of what was undertaken in 18th century Britain as mere uninformed dilittanteism, one must remember that we too look at the past through the *camera obscura* of our own times. It is Stuart Piggott, who was, with our honorand's compatriot and teacher Glyn Daniel, the supreme scholar of antiquarian thought in Britain of modern times. Piggott comments (1985:11–12):

> To appreciate the eighteenth-century antiquarians and their work, we must remember that there are associations between surveying stone circles and landscape gardening, Druids and Deists, sketching in watercolours and excavating tumuli. We are dealing with an antiquarianism that was partly involved in the search for the newly discovered picturesque in nature; prehistory was non-Roman, excitingly barbarian and so scarcely to be distinguished from Gothic in its appeal.

In her account of Banks' expedition to Newfoundland and Labrador, Lysaght (1971:76) has written of Banks 'it is in his innumerable personal letters that … [Banks] shows the qualities that won him so many friends, and enabled him to make such good use of men of varying qualities'. While obedient to few and never humble, our honorand has often preferred the spoken to the written word but above all he has been a servant to scholarship, good old-fashioned scholarship where hours spent in the field vie with other long hours in the laboratory, the study and the library; sometimes he sits and thinks, sometimes he just sits (compare Jones 1982). Banks and Jones must vie for the title of the most travelled savant. In 1963 Jones the £10 migrant to New South Wales met his first Australian excavation (Figure 10); a decade later, and two centuries after Banks was elected President of the Royal Society, Jones was back in Old Europe examining with his mother the classic sites of the Dordogne, following in the footsteps of that other hungry archaeologist, Glyn Daniel (1967: 78–85, 1963; for an illustration see Jones 1978).

Figure 10. Curracurrang Cove, Royal National Park, New South Wales. Rhys Maengwyn Jones (bottom right) with (centre) F.D.McCarthy (Australian Museum, Sydney) and (left) Andrian A.Gerbrands (Rijksmuseum voor Volkenkunde, Leiden) Photo: J.V.S.Megaw.

Twenty-six years ago Rhys Jones concluded a review with the words: 'In today's turgid times we badly need our prehistoric

Touchstones' (Jones 1975). Fortunately for world prehistory, here in Australia we do not have far to look.

Pob hwyl, i'r teulu i gyd!

Acknowledgements

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Homo Mobilis

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marrambay abirrinybona

'flown like a pair of ducks'

Gidjingarli expression for elopement

RHYS JONES'S masterly address at the Harvard Australian Studies Symposium in 1997 made me conscious for the first time of the speed with which humans had apparently spread through the virgin continents of America and Australia. It was not until the following year, when he gave an after-dinner talk on the same subject at the Rockefeller Center in Bellagio, that I began to think seriously about some of the implications and problems. The present paper is a toast to our long friendship, our enduring links with Frank Gurrmanamana and the Gidjingarli people of Arnhem Land, and our commitment to the study of human evolution.

The archaeological evidence in the American case can be stated concisely and with some confidence. In 1933 a flaked stone point was discovered in association with mammoth bones at Clovis in New Mexico. The site was subsequently dated by the ^{14}C method at between 10,900 BP and 11,200 BP. Although in the ensuing years broadly similar projectiles were found distributed throughout North and South America, no claims for human occupation earlier than Clovis have yet been authenticated. Surprisingly, however, there is good evidence that palaeo-Indians reached the Straits of Magellan by 10,000 BP. If we assume that the founder population entered through Alaska as ice sheets melted 12,000 years ago, the colonisers would seem to have moved down the north-south axis of the hemisphere at an average rate of 4 miles per annum (Jones 1998:17–18; Diamond 1992:ch. 18. Diamond (1992:306) suggests that 'within a millennium of emerging from the ice-free corridor at Edmonton, humans had spread from coast to coast and over the entire length of the new world.' His estimate of the average speed of north-south colonisation is 8 miles per annum).

Radiation of this speed is unlikely to have been achieved merely by a combination of population growth, band fission and nearby re-location of splinter groups. The most plausible hypothesis proposed so far is that high levels of mobility arose as a function of large-game niche specialisation. The early settlers, pre-adapted to a meat diet by the Arctic crossing of their forebears, initially found themselves surrounded by an abundance of naive and readily-obtained land animals. As fauna became wary, scarce or extinct in any particular area, some hunters stayed put and modified their food-seeking strategies, others moved on in search of easy pickings. Clovis-type archaeological sites, widely distributed and occurring within a narrow time span, thus represent the remains of America's earliest fast-food chains (Meltzer 1995:26).

Statements about the rate of colonisation of Australia are necessarily more tentative. Until recently the evidence suggested that humans arrived somewhere between 35,000 and 40,000 years ago. As ^{14}C dates of this order were distributed widely throughout the continent, it has been argued that colonisation (at least around the coastal perimeter) proceeded rapidly. However, recent discoveries by Rhys Jones and his colleagues, using new luminescence dating methods, have suggested the presence of humans in the far north of Australia between 53,000 BP and 60,000 BP. Whether the new techniques will produce dates of this order elsewhere on the continent remains to be seen (Jones 1998; O'Connell and Allen 1998; Bowdler 1990).

For the sake of argument I shall assume that the colonisation of Australia was rapid, like that of America and for similar reasons (the exploitation of readily-available large game). The question whether species were extinguished in the process is not one that I wish to take up (see Diamond 1992; Flannery 1996). My primary concern, rather, is with breeding strategies within the parent population and their possible implications for the formation of colonising units. Using data from modern non-human primate species, I shall argue that sexual competition within early human communities is likely to have constituted a potential for fission and colonisation of runaway proportions in the resource-rich and predator-free environment of Australia. The argument might also apply to America. Should the hypothesis be sound, it would add a non-trophic dynamic to the forces and circumstances currently but perhaps insecurely deemed to have been sufficient.

SYNOPSIS OF MATING PATTERNS AMONG CONTEMPORARY APES

The systematic observation of wild monkeys and apes by professional fieldworkers in the second half of the 20th century is one of the signal achievements of biological science. In conjunction with the data of archaeology and anthropology, the results form the best basis we are likely to have for speculations about early human history.

The superfamily Hominoidea includes humans, great apes, and lesser apes. The great apes are nowadays taken to comprise four species: bonobos, chimpanzees, gorillas, and orangutans. The first three are found in Africa, the fourth in southeast Asia. The lesser apes are the gibbons of southeast Asia, including siamangs.

Mating patterns characteristic of great and lesser ape species form an interesting continuum from promiscuity to monogamy, as set out in Table 1.

Table 1. Mating patterns of great and lesser apes. Data are based mainly on Kano 1996 (bonobos); Tutin and McGinnis 1981, Goodall 1986 (chimpanzees); Galdikas 1981 (orangutans); Harcourt et al 1981 (gorillas); Ellefson (gibbons).

Species	Mating pattern	Description
Bonobos	Promiscuity	Promiscuity within and between neighbouring groups. Mainly ventral-ventral copulation.
Chimpanzes	Quasi-promiscuity	Promiscuity within territorial groups qualified by (a) dominant-male possessiveness and (b) subdominant-male elopements. Dorso-ventral copulation. Females have individual core areas within male group territory, remain apart from males except when in oestrus. Hostility between neighbouring male groups.
Orangutans	Quasi-polygygny	Lone territorial male solicited by several lone females whose home ranges overlap with his. Ventral-ventral copulation. Consortships last about three days, terminated by female. High levels of male conflict. Females subject to rape attempts by subadult males.
Gorillas	Polygyny	Dominant male forages with several females who solicit copulation from him during oestrus; dorso-ventral position with male sitting upright; tolerance of related males when females sexually quiescent; savage fighting with males outside group.
Gibbons	Monogamy	Male/female pair mates and reproduces within its own defended territory. Dorso-ventral copulation.

Chimpanzee mating strategies

The plurality of male chimpanzee mating strategies became apparent only during the course of the long term project undertaken by Jane Goodall and her colleagues at Gombe on the shores of Lake Tanganyika. Initially Goodall described sexual relationships as promiscuous (1965:451). At the height of oestrus, a female chimpanzee was a resource to be enjoyed by all adult males, and they by her. Twenty to thirty mountings per day were commonplace. Flo, a female of mature years, was credited with 50 copulations in a day with a dozen or so males. The sight of copulation seemed to generate excitement through the community, and males became eager to take their turn. It has been suggested that since the chimpanzees at Gombe at this stage were provisioned with bananas for observational purposes, the high levels of promiscuous matings were a consequence of attempts by females to induce males to give them food. However that may be, promiscuity is by no means confined to experimental provisioning situations (Ghiglieri 1984:160).

At this stage of her observations, Goodall had detected no evidence of male dominance hierarchies (1965:415). In the next decade, however, her co-workers presented evidence suggesting that individuals can in fact be ranked meaningfully in linear order (Bygott 1979; Tutin 1979:30). At any rate it was clear that a good deal of fighting occurs and that from time to time aggressive and physically-powerful individuals become ascendant. The alpha-male of a community, moreover, may exhibit unmistakeable signs of sexual possessiveness in relation to a female in the final stage of oestrus, such as aggression towards any male seeking access to her, or towards the female herself to deter her from eliciting sexual interest elsewhere or from responding to it.

When measured against costs, the gains from such a strategy seemed uncertain. On one occasion observers noted that the top male in a hierarchy of eight was maintaining surveillance over the only female currently in oestrus. When a lower-ranking male moved towards her, with hair and penis erect, he charged and attacked him. While they fought, three other males copulated with the oestrus female in quick succession.

Maintaining constant vigilance is not easy. When alpha-male Figan allowed his attention to wander briefly in order to watch some monkeys, three rivals quickly mated with his current partner (Goodall 1986:472). Distractions during hunting forays, meetings with strangers and other occasions of social excitement, not to speak of group foraging movements through dense bush, are likely to entail similar consequences. Given the porous nature of his hegemony, the most that a realistic alpha-male could hope for would be a high percentage of the total copulations occurring at the time of ovulation (last day of maximum tumescence and first day of detumescence).

To what extent guarding oestrus females in crowded places improves paternity scores could not be determined. It became evident, however, that while high-ranking males pursued this strategy, certain individuals lower in the hierarchy were enticing females to leave the group and cohabit with them for varying lengths of time on outlying parts of the home range. Goodall estimated that almost 40% of total conceptions over a 20 year period at Gombe occurred during consortships of this kind.

The usual prelude to an elopement is solicitousness on the part of a male towards a particular female, to whom he provides food, grooming services, and protection throughout her sexual cycle. If, following these attentions, she responds to his invitation to leave the group, their departure must be silent, swift and inconspicuous. Any vocalisation from her would quickly attract community males to the scene.

The main risk once a getaway is successfully achieved is discovery and attack by parties of hostile neighbouring males. Goodall (1986:86) reports an episode in 1974 when males of the Kasahela community invaded the territory of the Kahama community, killed a lone Kahama male and appropriated the female with whom he was consorting. There is also a risk to the elopers, though not so serious, of aggression from community males upon their return.

Oestrus

Oestrus is a period in which a female displays a readiness to mate and during which she ovulates. It is generally taken to coincide with maximal tumescence of the perineal area. The menstrual cycle in apes is slightly longer than in humans (Table 2). For captive animals the available figures for the menstrual cycle are: chimpanzee = 37.3 days, gorilla = 31 days, orangutan = 30 days (Graham 1981a:2-7; cf. Tutin and McGinnis 1981:244, who report 36 days for wild chimpanzees). The mean menstrual cycle reported for gibbons is 29.8 days (Roonwal and Mohnot 1977:323).

The evidence suggests a correlation between magnitude of oestrus and distribution of male access (cf. Tables 1 and 2). Prolonged and conspicuous oestrus is associated with promiscuity, as in bonobos, while absence of gross signals is associated with stable polygyny (orangutans, gorillas) or monogamy (gibbons). Chimpanzees represent an intermediate case in which oestrus of lesser magnitude than among bonobos is associated with a fraught co-existence of promiscuity, possessiveness, opportunism and elopement.

Table 2. Oestrus in great and lesser apes. The data are based on Graham (1981b); Kano (1989); Keeling and McClure (1972).

Species	Oestrus
Bonobos	Maximal tumescence for 20 days; three-quarters of copulations occur during maximal tumescence; oestrus is continuous at adolescence, continues after conception, and reappears about a year after parturition.
Chimpanzees	Maximal tumescence ranging from 9 to 13 days; oestrus is semi-continuous at adolescence (males less interested than in bonobos), continues after conception (but not as long as in bonobos), and reappears about 4-5 years after parturition.
Orangutans	No perineal swellings or other gross anatomical signs. Inferred duration of oestrus in the wild is 3 days.
Gorillas	Labial swelling visible to close inspection in captive females but not to observers in the wild, except in the case of nulliparous females. Inferred duration of oestrus in the wild is 2 days.
Gibbons	No gross signs, though some change in sexual skin observed in captive females when most receptive.

Infanticide

The production and rearing of offspring require high levels of maternal energy investment in all ape species. Gestation is only slightly shorter than in humans (e.g. gorillas 256 days; chimpanzees 228 days), and mothers may carry infants for up to 5 years. Live births per reproductive lifespan range in the wild from about 3 for orangutans to 5 or possibly more for gorillas.

It goes without saying that the death of even a single offspring represents a serious setback to a mother's reproductive success. Although the young of arboreal apes are not normally endangered by predators, one of the surprising discoveries of recent primatology is that they may be at significant risk from attacks by conspecific adults, particularly males from outside their own natal groups. The most plausible explanation advanced so far is that, when a male attempts to appropriate an alien female, he may kill her offspring in order to terminate infant care and thus bring about a more rapid return of oestrus (Hrdy 1979).

Goodall's (1986) review of chimpanzee infanticide, though documenting the phenomenon in dramatic detail, raises some problems for this hypothesis. Between 1975 and 1982 males of the Kasakela community were observed on some 20 occasions attacking anoestrus mothers with infants. Four infants were killed in the course of struggles and three of them eaten by the assailants. However, in no instance was any attempt made to appropriate the mother. Indeed the attack seemed to be primarily on her, rather than the infant. Goodall attributed the episodes to xenophobia.

Even more mysterious is the case of an adult female and her daughter who, between 1974 and 1977, attacked nursing mothers in their own community, wrested babies from their mothers and cannibalised them. Three such incidents were observed and seven other infants disappeared without trace. The killing spree left only one infant alive in the group and in 1977–8 was followed by a baby boom.

Whatever the reasons, infanticide on this scale helps to explain female participation in consortships. While the reproductive advantage of elopement to the male partner is clear enough (especially if he is a sub-dominant), one might have supposed that a more secure option for a female would be fertilisation by community males well inside their home range. The attraction of a suitor, however, is that even during her anoestrus phase he remains near at hand and is nice to any dependent offspring she may have, thus acting as a deterrent to infanticidal attacks. Reciprocity on her part in the form of elopement and short-term monogamy during her oestrus phase may be worth the risk.

Infanticide by males has been reported for gorillas by several observers (Fossey 1984; Watts 1989, 1996). Most cases occur when the mothers are unaccompanied — typically, after the death of their dominant male consort. Infants in such circumstances are almost certain to be killed. The data suggest that male protection against infanticide is more important than protection from competitors for resources (Watts 1996).

Infanticide has not been observed among bonobos, orangutans, or gibbons.

The masking of oestrus

In circumstances where long-term male protection is reproductively advantageous to females, strategies to obtain it will tend to evolve. These could include a prolongation of oestrus and rapid resumption after parturition, resulting in the sustained attentions of numerous males; or, paradoxically, the contraction and masking of oestrus, resulting in a narrowing of focus of individual male mating effort. The former pushes the system further into promiscuity (as in the case of bonobos), the latter moves it towards polygyny and monogamy. As tell-tale signs of ovulation disappear, promiscuous opportunism from a reproductive viewpoint becomes a game of chance increasingly against the odds. Hence investment tends to become more concentrated over longer terms and, simultaneously, aggressively guarded against loss. If males also expend energy in nurturing their partners' offspring, exclusivist strategies intensify further in order to forestall cuckoldry (for more detailed analysis of the concealment of ovulation and cuckoldry prophylaxis, see Alexander and Noonan 1979; Turke 1984; Hiatt 1989, 1990).

REPRODUCTION IN EARLY HUMANS: AN HYPOTHESIS

Hominid adaptation to a terrestrial habitat necessarily entailed exposure to higher levels of danger from predators. An assumption of kin-based social formations dependent upon co-operative male defence seems plausible, with female exogamy as a corollary (Wrangham 1987; Ghiglieri 1989). Although prolonged oestrus and bonobo-type hedonism may promote intra-group harmony, it is probably incompatible with high levels of vigilance and military discipline as exemplified by baboon troops on the savannah. Any existing trend towards masked oestrus engendered by the advantage of male protection against conspecific aggression is likely to be reinforced by vulnerability to predators. In combination with male rank order, it would tend to produce breeding units within each community ranging from stable harems led by dominant males to short-term consortships initiated by subdominants.

Male aspirations for exclusive access to a plurality of female partners typically generate problems for male solidarity since, if sex ratios are equal, one man's polygyny is another man's bachelorhood. Kin altruism, whereby brothers share the same women, may achieve some degree of compromise. Another solution, classically exemplified by the Australian Aborigines, is to promote solidarity and polygyny simultaneously in the senior male cohort by systematically prolonging bachelorhood in the junior cohort. Such an objective may be facilitated by a combination of coercion and sublimated reward, as manifested in male puberty rites and ensuing induction into religious mysteries and warrior cults.

The masking or muting of oestrus gives females greater control over their own sexuality and hence more freedom to use it as a bargaining point. Recent work on human evolution indicates that 'between 500,000 and 100,000 years ago there was an exponential increase in brain size which would have escalated female energetic costs [for reproduction] far beyond those found in *Homo erectus*' (Key and Aiello 1999:25). A reduction in the size of the human gut suggests that meat began to play a crucial role in meeting such costs. We may reasonably infer that, as male hunting skills developed, females increasingly selected mates able and willing to provide them with game.

A further possibility is that at some stage females began selecting males as their daughters' mates. Taking ethnographically-described Australian Aborigines as a model, the selection would be in the form of a promise made soon after the daughter's birth, on the understanding that gifts of meat from the future husband to his mother-in-law must begin immediately and continue indefinitely. The supply to the latter of foodstuffs vital for her reproductive success is thus increased and prolonged. The advantage to the gift-giver is that he gains a competitive edge over potential rivals in a polygynous market where demand always exceeds supply.

To gain maximum benefits, a mother with a newly-born daughter should seek a son-in-law with the following qualifications: (a) proven hunter, (b) youthful, (c) in good health, (d) having no other commitments, (e) reliable. In order to fulfil her side of the bargain, she must condition her daughter to accept the arrangement — a task facilitated by well attested mother-daughter solidarity (Hamilton 1970) — and, specifically, by ensuring that she receives a share of the food gifts sent by her future husband. Whatever the means, if daughters can be induced to be obedient, the reproductive benefits to mothers will tend to drive the arrangement towards universal adoption.

Systematic infant bestowal in hunting societies for the purpose of obtaining meat would produce regular age discrepancies between husband and wife of at least 12 to 15 years. This is likely to be increased in a percentage of cases by the fact that young men seeking their first bestowals would be competing with older men seeking second or third bestowals. Assuming females marry at puberty and males are not regarded as competent hunters until about the age of 18, a man would be lucky to co-habit with a wife before he reached 30.

An officially deprived class of young bachelors would represent for a woman a further source of protein, albeit a risky one. If lucky as well as astute, she might at various overlapping stages of her life succeed in obtaining meat supplies from husbands, sons-in-law (prospective as well as actual), and lovers.

Because a mother's interests are not the same as her daughter's, the pre-emption by the former of marital choice on behalf of the latter could readily lead to conflict. The most likely time for tensions to come to a head would be at adolescence, by which stage the daughter might well have developed sexual preferences in directions quite different from those planned for her. By now, however, much meat has been handed over, deep obligations have been incurred, and great expectations put in place. The scene is set for trouble.

Elopement and colonisation

As we saw earlier, elopers among wild chimpanzees are at risk both from foreign males and males of their own community. If we add predators as a third source of danger, it would be fair to describe elopement as a high-risk strategy. Wherever communities become havens surrounded by hostile forces, members tend to conform to their norms. If these happen to include marriage contracts depriving the young of freedom of choice, the young rebel at their own peril. On the one hand, within the community they face not only particular injured parties but an alliance of senior males and females whose generational and generative interests are protected and enforced by a range of gerontocratic sanctions. On the other hand, if they try to beat the system by eloping, where can they live in reasonable safety?

In a virgin continent free of dangerous predators, such as Australia, the answer would be as far as feasible ahead of the frontier of human expansion. In other words, when travel and isolation ceased to be life-threatening for individuals, regimes of infant bestowal and gerontocratic polygyny would no longer be able to contain the disaffections of youth and instead would generate a series of impulses for exodus and colonisation. If the old order re-appeared as the new communities matured, so too would the colonising impulses. The speed of colonisation would thus have been faster than might otherwise have been expected, even taking into account the abundance of previously unexploited food supplies. By the time all the empty spaces were filled, the old order had probably re-asserted itself everywhere. With communities locked in place until the cataclysm of European expansion, elopement was once again hazardous. For a long time, between the golden age of travel sometimes known as the Dreamtime and the appearance of the emissaries of Christ, youth had little option but to respect the reproductive interests of the elders.

The ethnographic and analytic basis for some of these speculations is set out in Hiatt (1996:chapter 4).

CODA: A GIDJINGARLI SCENARIO

In 1960 Frank Gurrmanamana, a Gidjingarli tribesman, devised for my benefit a series of texts on 'custom-law' (*joborr*). Two of the texts deal with elopement. One of them, 'The Lover's Accomplice', describes the elopement of a man and a young woman who has been promised in marriage to somebody else. A third man is involved, a special friend of the lover who sends the elopers on their way.

In the opening scene, the girl makes an assignation with her lover. Before they meet, the lover takes his friend into his confidence. Subsequently they make a pact in terms of which the friend agrees to play the role of accomplice. To seal the arrangement, he has sexual intercourse with the girl and the lover gives him a pair of spearheads. When the elopers have flown, the friend returns home. At first he denies knowledge of what has happened but later stands ready to take the brunt of the betrothed husband's anger.

Here the Gidjingarli original on which the English translation is based has been omitted, but is available in *People of the Rivermouth: the Joborr Texts of Frank Gurrmanamana*, (Hiatt and McKenzie forthcoming).

A LOVER'S ACCOMPLICE

by Frank Gurrmanamana

Cast

A young woman
Her betrothed husband
Her lover
His male friend
An old woman
An old man
Female food-gatherers

A young woman talks to her lover as she is preparing to go out gathering food with other women.

YOUNG WOMAN: Later today I'll be over that way. Come there.
LOVER: Where are you all going?

YOUNG WOMAN: We'll be going to collect vegetable food. You'll hear us calling out to each other as we move along.

The young woman's lover and his friend set out later.

LOVER: Let's go hunting.
FRIEND: Right, let's go.

They walk for awhile.

LOVER: I'd like to tell you something.
FRIEND: Go ahead.
LOVER: We are going this way so that I can meet a woman. She is ready to come away with me. She yearns to.
FRIEND: If you want me to, I'll send you off.
LOVER: Yes, I would like you to. I'll be indebted to you. When we get there, you hide yourself.
FRIEND: I'll stay back and watch you, and she'll think you're alone.

The young woman is not gathering food but watching for her lover.

FOOD-GATHERER: Get a move on. There's a lot of food over here.
YOUNG WOMAN: In a while! I'm still getting this lot.

The lover throws a pebble and hits her.

YOUNG WOMAN: Don't be afraid. Come up quickly.
LOVER: I want to talk to you. Where are the other women?
YOUNG WOMAN: They're collecting food.
LOVER:. I shall propose something to you. Perhaps you'll agree, perhaps not.
YOUNG WOMAN: I'm ready to listen. What is it?
LOVER: Let's elope today.
YOUNG WOMAN: I want that too. Let's go right now.
LOVER: There's a man over there. We came here together.
YOUNG WOMAN: Can he be trusted?
LOVER: Completely. We've already talked about it. Let's go to him.

They join the lover's friend.

FRIEND: So that is the woman you're taking?
LOVER: Yes, she's the one. Everything all right?
FRIEND: You must start off at once.
LOVER: First let's go over that way. Later you can send me on my way.

The lover's friend goes ahead.

LOVER: I would like to give you to him so that he can have sex with you. Later he will send us on our way. Are you agreeable?
YOUNG WOMAN: I'll do it. Because then you and I will be able to go.
LOVER: Exactly.

The lover rejoins his friend.

LOVER: I have told her that you will have sex with her, if you want to.
FRIEND: No, I'd be embarrassed because of you.
LOVER: It's fine with me, because you and I are special friends.

The lover returns to the young woman.

LOVER: I'll go and sit over there and wait for you. I'll talk to him afterwards.

He leaves, then his friend appears. She doesn't speak to him but lies down.

FRIEND: I will make love to you. Then I will send off my friend. He'll leave today.

He copulates with her. Later he addresses the lover.

FRIEND: Let's not talk. Go immediately. If trouble arises, no one will be more fierce than I.
LOVER: I have left some spearheads for you. Two of them. If you have trouble while I'm away, I'll come back. I'll be thinking of you.
FRIEND: Yes, now go, good friend! Be on your way!

The lover and the young woman elope The food-gatherers are still foraging and realise the young woman is missing.

FOOD-GATHERER 1: Which way did she go?
FOOD-GATHERER 2: Perhaps she went home.

The foragers return to the camp. An old woman speaks to them.

OLD WOMAN: You've all been to that place away over there?

They tell her about the young woman's disappearance.

FOOD-GATHERER 1: I said to her: 'There's a lot of food here', that's what I said, 'Come quickly. There's lots here.' She said to me: 'Not yet. There's still some here.'

The lover's friend returns to the camp.

OLD MAN: Didn't you and your friend go off together?
FRIEND: Yes, but I went a lot further than he did.
OLD MAN: Ah.
FRIEND: So?
OLD MAN: No reason - I was just asking.
FRIEND: Come on, tell me. I'm listening.
OLD MAN: Perhaps your friend has taken a woman and eloped with her.
FRIEND: I see. Is that what the women are saying?
OLD MAN: That's right, that's what they said.
FRIEND: Right. Well, it's news to me!
OLD MAN: You and he did go off together this morning.
FRIEND: Yes, we walked off together. Then we put down our spears and swag, and sat for a while.
OLD MAN: You got up and then what?
FRIEND: Well, he said to me: 'I'll go that way, and if I spear a wallaby we'll cook it back here. And if you spear one, wait here and we'll cook it.'
OLD MAN: Then?
FRIEND: I hastened away, speared a wallaby, carried it back, lit a fire, roasted it, and kept watching for him. He left these spearheads behind.
OLD MAN: I see. But these spearheads? Some night he'll come back quietly and wake you.

People discuss the matter and suspect that the lover's friend is lying. Later he speaks to the young woman's betrothed husband and tells him the truth.

FRIEND: My friend wanted to take her. He gave me some spearheads, which I've brought back. I sent him off, and he's gone. He'll return soon and talk to you.
HUSBAND: Why did you come back instead of going with him?
FRIEND: Because she's his girlfriend. It's their affair But if you pick a fight with me, I will take you on.
OLD WOMAN: Don't fight! That girl is damp between the legs! She knew what she was doing.

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Russians and the Australian Aborigines (200 Years of Observation, Research and Speculation)

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RUSSIAN STUDIES and perceptions of the Australian Aborigines remain hardly accessible to Western scholars because of the language barrier. Still, for the Russian mind it has always been an important field to which Russians contributed their original views and attitudes.

In spite of distance Russians were the third nation after the English and the French to make a significant contribution to the studies of the Australian indigenous population in the early post-contact period. Russians were prompt to translate for wide readership diverse early accounts describing Aborigines, such as those of Joseph Banks (translated in 1772), James Cook (1786, 1796, 1805), Watkin Tench (1790), John Hunter (1793), Arthur Phillip (1793), George Barrington (1803), and François Péron (1809–1810). In the next decades they were followed by the travels of Jacques Arago (1823), Jules Dumont d'Urville (1837), George Grey (1838, 1842), Thomas Mitchell (1839), Daniel Tyerman and George Bennet (1834) (for details, see Govor 1985:65–70). Fifteen Russian expeditions visited Australia between 1807 and 1835. Russian direct contacts with the Aborigines started in 1807 when the *Neva* visited Sydney on the way to the Russian colony in North America. In Sydney the visitors acquired a collection of Aboriginal weaponry. The records of their encounter with the Aborigines did not survive, but the following expedition on the *Suvorov* (1814) and especially on the *Otkrytie*, *Blagonamerennyi*, *Vostok*, and *Mirnyi* (1820) resulted in an extensive original material — travel journals, articles, books, drawings, collections. That is not surprising as studying the indigenous population was a part of the task set to these expeditions by the Naval Ministry. Observant officers and experienced naturalists, professional artists and just curious onlookers, they had many personal contacts with the Aborigines of Port Jackson area and became the last European team to provide a portrait of this community before its disintegration under the influence of colonisation. The factual results of these early expeditions have been described by Barratt (1981) and I just provide a specifically 'Russian' aspect to their contribution.

At this early stage Russian perceptions of Aboriginal society as a whole were determined by the belief common among Europeans that these natives belonged to the lowest stage of human culture. Due to Eurocentrism the Russian visitors invariably focussed their attention on the issues that in their view seemed to be the dominating features of human society. The set of Russians' expectations was blunt: clothing, dwellings, 'kings', and hard toil for subsistence. The Aborigines did not fit into this model. For Russians it was the first encounter with such a human society - the traditional societies which inhabited the territory of the Russian Empire, be it the peoples of the Caucasus, Siberia or Far East, could boast all these attributes. Encountering the Aborigines the Russians obviously experienced an initial shock — half-naked natives living on fish and mussels, sleeping by a fire under the open sky, dancing corroborees and enjoying life were obviously a society in its own right. And Russians did their best to reconcile their model with the reality.

Many of them persistently repeated that they had heard that the Aborigines constructed bark huts, and expressed regret that they themselves managed to see only windbreak brushwood fences around fires. The idea that the dwelling was an integral part of human settlement reached its apotheosis in Pavel Mikhailov's drawing 'Natives of New Holland'. Depicting Bungaree's group, in whose camp on the Kirribilli Point at Port Jackson the Russians had seen nothing but windbreak fences, Mikhailov nevertheless chose as the dominant element of the picture the stick-and-grass hut. It is obvious that he used not his personal observations but someone's description or drawing, as the hut is disproportionately large in comparison with the human figures near it (Barratt 1981:76).

Similarly they paid an exceptional tribute to 'King Boongaree'. While the colonists, although awarding him a brass chest plate, would perceive him with a degree of humour — on the portraits of the local artists he would appear with a grim and cunning mien dressed in ridiculous European cast-off clothes, — the Russians seem to treat him with genuine sympathy. Mikhailov's paintings of Bungaree and his wife are even distinguished by a romantic-heroic idealisation of their appearance. Moreover, the Russians were apt to treat Bungaree as a personification of their image of a 'noble savage'.

Symptomatically, as the Russians' contacts with the Aborigines developed, — and many of them had opportunities for this, living in a tent camp on Kirribilli Point close to Aborigines or going there for excursions — their initial perception of the Aborigines as 'repulsive' 'half-demons' gave way to the image of a kind and friendly community. Russians, probably more easily than other Europeans, inclined to enrich the image of a 'primitive native' with a specific Russian emphasis on a compassionate attitude towards the 'younger brother'. The Naval Ministry's instruction to Bellingshausen advised: 'When you are in foreign countries or amongst natives, deal kindly with all and observe every courtesy and politeness, instilling the same into the minds of your subordinates' (Bellingshausen 1945:21). Russians indeed had a good reputation in the Pacific as their contacts with natives, as distinct from many Europeans and Americans, have never been violent.

Although finding nothing attractive in the traditional way of life of the Aborigines, the Russians did not doubt their capacity to reach higher standards of culture through education. Novosil'skii (1853:67) wrote: 'New Holland natives are considered through their ignorance below all other peoples of the world, yet their children made progress in schools equally with Europeans', while Bellingshausen (1945:338) noted that 'the results have proved that the natives of New Holland are capable of being educated, notwithstanding the fact that many European armchair professors declared them to be utterly devoid of intelligence'. With such a belief it was not surprising for Russians discussing Aboriginal — settler confrontations to take side of the former. Actually colonists' atrocities towards Aborigines were the only point of criticism of the Russian visitors when writing about Australian life. Although Eurocentric in the notions of land ownership Russians were aware that 'The natives remember very well their former independence. Some expressed their claims to certain places, asserting that they belonged to their ancestors. It is easy to understand that they are not indifferent to having been expelled from their own favourite localities. Despite all the compensation offered to them, a spark of vengeance still smoulders in their hearts'. Noting that the Aboriginal 'dislike of the English has almost entirely vanished'. Bellingshausen emphasised that 'the Europeans themselves often [have been] the cause of quarrels' (Bellinghausen 1945:331, 337). The Russian visitors, when telling about the tragic resistance of Tasmanian Aborigines to the European invasion, blamed the latter, observing that the English started the perpetual hostilities when the first settlers fired grapeshot at a friendly group of Tasmanians (Lazarev 1993:79). The most tragic picture was given by Berens, who wrote after a visit to Australia in 1829: 'I was told by one of the officers, who served in Van Diemen's Land in a detachment, about the means they use to move the native inhabitants off the colony. Usually such a detachment sets out for the bush, as if to hunt game; on seeing the natives, they surround them and kill without any regret' (Berens 1903:55-6). And finally the last expedition of this period brought grim news to Russia: 'Native New Hollanders, it seems, disappeared completely. Since the settlement began, they gradually died out of the misuse of alcohol' (Zavoiko 1840:60).

Atrocities towards the Aborigines were a constant theme on the pages of Russian periodicals of the time. *Northern Bee* in 1829 published the account by Henry Widowson, an agricultural agent in Van Diemen's Land, who said that the colonists there deliberately provoked the natives' aggression and sometimes shot at them just to satisfy their inclinations to brutality. *Muscovite* in 1843 wrote that settlers corrupted Aborigines, treated them as brutes, addicted them to alcohol, deprived them of their last means of existence and, finally, exterminated them. But the Aborigines, argued *Muscovite*, 'possess worthy qualities that are not always inherent in those who enslaved them: they love their mothers, children and freedom!' *Son of the Fatherland* wrote in 1847: 'Since the conquering of Mexico and Peru there has been no event more monstrous than the extermination of the natives of Van Diemen's Land. In order to excuse themselves from any pangs of conscience, English colonists [then] began to preach that the savages were not people and that one could treat them as brutes' (Widowson 1829; Anon 1843:554; Anon 1847:49). Although experiencing similar collisions while colonising the newly gained frontier regions of the Russian Empire such as Siberia, Russians believed that they had grounds to criticise the English as in general their own colonisations tended to be more tolerant and place greater stress on assimilation than in Australia.

Being keen observers, early Russian visitors left accurate descriptions of their contacts with the Aborigines. Their evidence now is a valuable source for social-economic reconstruction of Aboriginal society. For instance in 1814 they witnessed an important social event in the life of the Aborigines on the south shore of Port Jackson - a crowded pre-planned gathering for the ritualised settlement of a conflict, which ended in a bloody fight. There are two independent descriptions of this battle by the *Suvorov's* crew members, Aleksei Rossiiskii and Semen Unkovskii. Unkovskii gave the exact place where it was held: 'behind the new hospitals in the place known as Hyde Park'. The Russians' descriptions provide evidence that this was a traditional territory for gatherings and rituals, including, as in this case, rites to settle inter-group conflicts. If my supposition is correct, it reveals the importance of this part of Sydney's territory in the social life of the Aborigines of the southern shore. Rossiiskii gave the number of participants in the fight: initially 50 and later on ca. 100. Unkovskii noted that the two parties of fighters formed a circle of one *verst* [1 km] in circumference. Though there were many white onlookers, the large number of Aboriginal participants and the bloody ferocity of the fight (emphasised by both Russians) shows that the gathering was not provoked by the whites but had a traditional cause. The following remark of Unkovskii also confirms that in 1814 such fights were still common. He wrote that the Aborigines in Englishmen's service 'have to be present at fights of their fellows and precisely fulfil every native practice; the master of such a servant never prevents him from going to a slaughter' (Rossiiskii 1993:24–5; Unkovskii 1944:100). Supposing that only male warriors took part in the fight (approximately 50 on each side according to Russians), we can estimate that their groups (probably bands, or even remnants of tribes) were several times larger in number – ca. 150–200 people each.

Ivan Simonov, who lived in a tent on Kirribilli Point in 1820, described two groups of Aborigines using the same part of the northern shore in the area of Kirribilli. A member of one group, a man called Burra Burra, was very friendly towards the Russians and even invited them to put their tent near his own camp. His group, which joined him at the seashore, consisted of 'a great number of natives, men and women; the women went off to fish, but the men remained with us, they fashioned various fishing implements with small iron axes, and smoothed them down with glass'. ... . 'Shortly after this crowd, one more family of natives came up', Simonov tells, led by the 55-year-old Bungaree. Later on he, 'accompanied only by his family, continued to wander in our vicinity; all the rest [i.e. Burra Burra's group] soon ... moved away into the wood. However they appeared near us from time to time' (Simonov 1829:48–52, 1993:48–50). It is possible to make the following reconstruction on the basis of these data. The 'great number of natives', who joined Burra Burra on the seashore, could be a core of the community (local group) who owned the northern shore of Port Jackson. Bungaree's family was probably an extended family, perhaps a foraging group, that was wandering at the time separately from the community's core. Bellingshausen mentioned that, during Bungaree's first visit to the *Vostok*, he had been accompanied by his wife Matora, daughter and son. Bungaree, pointing to his companions, said: 'These are my people'. Then, pointing to the whole north shore: 'This is my land' (Bellingshausen 1945:163).

Bellingshausen collected further evidence on the social organisation of the Aborigines: 'All live in communities of 25, 50, 60 or even more, each with its own name. In one, called Burra Burra, there were last year [i.e. 1819] reckoned to be as many as 120 people' (Bellingshausen 1945:330). Novosil'skii (1853:69) and Simonov (1993:61) provide similar data. The Russians' evidence of the numerical composition of the bands is very interesting. Although by the time of their visits in 1814 and 1820 the numbers of Aborigines of the Port Jackson area had dramatically reduced, and the initial bands ceased to exist, the Russians describe what can be considered as bands with numbers close to those in traditional society. The only explanation is that the remnants of the initial bands regrouped and formed new bands close in numbers and lifestyle to the pre-contact bands. That was the Russians' remarkable discovery of the amazing ability of the Aborigines to restore their traditional social structure even in the most hostile environment and adapt to changing conditions.

As the nineteenth century progressed the first Russian original anthropological studies of the Aborigines appeared. While most of the Russian anthropologists wrote in the tradition of evolutionism, N. I. Ziber, a Russian Marxist, in his work *Essays on the Primitive Economic Culture* (1883) creatively used the materials of Australian anthropology to show the role of the economy and socio-economic relations in the life of primitive society. He was a pioneer in anthropology, revealing the importance of the main socio-economic unit of traditional society - the community - at a time when Western anthropologists, following the tradition set by Morgan and the English anthropological school, devoted their attention mainly to kinship relations.

Nikolai Miklouho-Maclay pioneered the field of Russian anthropological studies in Australia, mainly in physical anthropology. Visiting Mabiak Island in Torres Strait in 1880 he recorded the custom of skull deformation of newborn children by their mothers. While in Queensland he got interested in a group of 'hairless' Aborigines and travelled as far as Gulnarber Station on the Balonne River in Southern Queensland to describe them. He also conducted a number of anatomical studies of the brains of Aborigines. He had constant interest in the field of sexual customs of the Aborigines, in particular he wrote several articles about 'operation mika' (sub-incision) in central and northern Australia, gathering evidence from reliable locals. But his greatest contribution to Aboriginal issues were his passionate appeals to the officials against extermination of the Aborigines of Australia and Oceania. Colonists of northern Australia, he wrote with indignation, as punishment for a stolen horse, 'killed as many blacks as they could manage'. In spite of his popularity at the time in Australia he realised that his exhortations to spare natives 'for the sake of justice and philanthropy' looked like 'an appeal to sharks not to be so voracious!' (Miklouho-Maclay 1990-96 vol.3:187–8, vol.4:50–77, vol. 5:222–3).

In Russian popular literature the tendency to compassion took the upper hand. For instance Cherniaeva's *Tales of Australia and Australians*, whose publisher shared Leo Tolstoy's spiritual ideas and addressed a working-class audience, depicted Aborigines with considerable sympathy, as pitiful but happy in their own way. Cherniaeva did not spare 'the English' for whom 'it was not enough to exterminate or drive away the black native; it seems that they want to change the Australian land itself into England'. She, like other Russian critics, said nothing about the situation of the natives of the Russian Empire. But, significantly, a reviewer in the influential journal of opinion *Russian Thought* criticised Cherniaeva because 'the comparison of the Russian colonisers with the English suggests itself' (Cherniaeva 1896; Anon 1897:32).

Atrocities against the Aborigines and their 'extermination' (as Russians put it) by colonists became, in the second half of the nineteenth century, the focal point of writings by both visitors and armchair travellers. S. V. Eshevskii (1862:549), a liberal historian, argued that the cause of the extinction of the Tasmanian Aborigines was the whites' atrocities, 'rather than the natural inability of these natives to adopt Christian-

European civilisation'. V. Linden, a naval visitor to Tasmania in 1870, criticised Robinson's relocation of the remaining Tasmanians to Flinders Island: 'Robinson, in describing his exploits, claims that he used only his powers of persuasion on the natives. It was hardly so. It was not persuasion that made the indigenous Tasmanians leave their native forests - it was their hopeless situation. If they had known what awaited them, they would doubtless have chosen to die of hunger like hunted beasts in their dens, rather than yield to Robinson's persuasions. Their life on Flinders Island was no better than imprisonment' (Linden 1871:134–6). Ten years later E. Tsimmerman, an observant Russian traveller, would write prophetically: 'One is in doubt as to who were the barbarians - the English administration with its free settlers and convicts or the uncivilised black indigenous peoples. Before the court of history, the latter will certainly be acquitted ... because their actions were in self-defence, protecting themselves and their families and the land they had peacefully occupied for many centuries before the uninvited civilised invaders'. Yarning with drovers around a campfire near Morgan, Victoria, he discovered that the atrocities were still going on. Telling of drovers' boasting how they had got rid of importunate natives using poisoned flour Tsimmerman remarked with indignation: 'All this was told with extreme cynicism, as if it were a matter of poisoning gluttonous rats' (1882, 12:461-2, 467–9, 1883, 8:351-4).

Vsevolod Rudnev, another naval visitor, in 1882 collected chilling evidence in Albany in Western Australia: 'We were told that the civilised conquerors of Australia, if they need to enlarge their properties, proceed in parties ... to the nearest [native] villages to clear the land in accordance with established custom, which consists in the following: members of the expedition descend on the villages, burn them, and without exception shoot the inhabitants, young and old alike. The combination of the agreeable with the useful — a picnic and land clearance' (Rudnev 1909:118). It is likely that such impressions disseminated in Russia by these naval visitors echoed in one of the first Russian short stories about an Australian kangaroo hunt (Al. L-va 1901) published in one of the most popular magazines. The anonymous author, although aware that Russian natives 'have far from a sweet life' indignantly concluded: 'But still in [Russia] even in the most extreme times no one stooped to "hunt for two-legged game", as the English say when they talk about chasing the poor savages'. Criticism of the English who, Russians believed, considered Aborigines 'even lower than animals' (Vitkovskaia 1915:281) was due to a specific ideology of Russian intelligentsia with its compassion to the 'younger brother', to the underprivileged.

The early 20th century brought the diversity of Russian approach to the field of Aboriginal studies. Some Russian anthropologists continued to treat Australian Aborigines as a perfect example of the initial stage of human evolution. For instance K. M. Takhtarev chose Australian Aborigines as a basis for the analysis of early forms of primitive social organisation and culture; his book *Essays on the History of Primitive Culture* (Takhtarev 1907) targetting the educated reader, ran to four editions between 1907 and 1924. The Russian Prince Petr Kropotkin, a scholar of broad-ranging interests and a famous theorist of anarchism, was especially interested in the development of ethics and related issues. As a political emigre in England he wrote *Mutual Aid among Animals and People*, which ran to at least three editions in Russia at the beginning of the century (1904, reprinted with slight title variations in 1919 and 1922). Australian Aboriginal materials available to him confirmed his theory that mutual aid had been the cornerstone of ethics, justice and harmony in human society since the early stages of mankind. The Russian sociologist and anthropologist Maksim Kovalevskii, in his book *Clan Life* (1905, reprinted in 1911), used Australian materials for a creative development of Marxist ideas, combining them with an evolutionist approach. Aleksandr Maksimov was one of the first to depart from evolutionism and Marxism. He argued that each people had its unique way of development and that the social organisation of Australian Aborigines did not confirm the evolutionists' ideas (Maksimov 1997). A new development was the interest of Russian educated readers in Aboriginal folklore. Poet Valerii Briusov wrote two poems imitating Australian Aboriginal songs. The folklorist P. N. Sakulin in his book *Primitive Poetry* (1905) made extensive use of Australian materials. Also such important Western works as Andrew Lang's *Mythology* and K. Langlo-Parker's *The Australian Legends* were translated into

Russian. All these publications, particularly *The Australian Legends* which was directed at juvenile and general readers, contributed to the more enlightened perception of the Aborigines as a people with an ancient, elaborate culture, as opposed to the idea of 'pitiful natives' which dominated in the previous century (see bibliographical details in Govor 1985:88, 98-9, 107-8).

The Russian naturalist Aleksandr Iashchenko came to Australia in 1903 as an envoy of these enlightened views. Unlike many of his contemporaries he never perceived Aborigines as a 'pitiful' race destined to die out in the face of European civilisation. He would describe the Aborigines whom he met as 'good-natured', 'good-humoured', 'cordial', 'with handsome and intelligent face' (1959:67, 99, 167, 169). Going with Dieri Aborigines to the bush in Killalpaninna (or Bethesda, southeast of Lake Eyre) Iashchenko was amazed to discover how well their society was adjusted to life in the harsh semi-desert conditions. Although they were already significantly influenced by European culture, he managed to see and describe many features of their traditional lifestyle. Killalpaninna mission station, where Iashchenko incidentally met the famous pastor and anthropologist Carl Strehlow, was headed by Pastor Johann Reuther who for years gathered materials about local Aborigines. Iashchenko was the first scholar to whom Reuther showed his materials; understanding their outstanding scientific value, Iashchenko tried to persuade Reuther to publish them (1959:94-110). With sorrow Iashchenko (1959:129-31, 160-68) observed the consequences of European destruction of the traditional Aboriginal society in Victoria and northern Queensland where he visited the Yarrabah mission near Cairns.

The poet Konstantin Balmont who visited Australia in 1912 elevated the interest in traditional culture to a symbolic level; for him Australian natives and nature a *priori* symbolised harmony, while the whites were agents of slavery and destruction:

> Rails cut into the waves of yellow hills,
> All calculated space is fettered and coupled.
> Where the blacks had composed harmonious dances, -
> There is a lonely white-faced shepherd.

In his public lectures read in Russia he strongly condemned the English who 'exterminated the beautiful dark-complexioned Tasmanian tribes... . The savagery of the English exceeded even that of the Spaniards in their subjugation of the last Mexicans. The creators of political freedom were unable to comprehend simple human freedom' (Balmont 1993:289, 1913:717). But Russians could adopt a quite different approach: agriculturalist Nikolai Kriukov, visiting Australia in 1903, depicted Aborigines as 'ferocious cannibals' ... 'unable to adapt to any elements of culture'(Kruikov 1906:35, 80–84). Such views could have been formed under the influence of his contacts with Australian farmers.

The Russian pre-revolutionary tradition of equality of races and compassion towards any downtrodden people, whatever their nationality, culminated in the case of the Russian émigré family of the Illins who came to Australia in 1910. The father, Nicholas Illin, an intellectual and eternal rebel who failed in the practical application of his ideas, did succeed in raising his son Leandro in accordance with his democratic beliefs. In 1912, exploring the suitability of the Northern Territory for a Russian colony, Leandro Illin made perceptive remarks about the Aborigines, in particular about the social and numerical composition of Aboriginal 'camps', their traditional diet, and the role of hunting and gathering of wild rice. But the main issue of interest was a shocking picture of European exploitation and cruelty in relation to the Aborigines recorded by Illin in his journal, particularly by Thomas and Roberts, the owners of Glenavon plantation near Daly River. When Illin wanted to pay the Aboriginal guides, who had brought him to the plantation, with some provisions, he was reproached by the hosts for 'spoiling the blacks for them. "They would not get as much from us for a year's service", said Mr Thomas. When I wanted to give them tea he became really angry and said "There is plenty of water, don't you spoil my blacks!" ' Illin constantly heard from the locals that they could use natives' labour for nothing, witnessed chained, imprisoned Aborigines and watched how they were tried without understanding the nature of their trial. He sympathised with several farmers who treated Aborigines more kindly (Illin 1912:14, 21, 34, 68, 73, 77–8; Govor 1997:210–12).

Continuing the traditions of the Russian intelligentsia, Leandro Illin himself passed the test in practice a few years later when he, after a long battle with officials, married an Aboriginal woman in 1915, raised the Aboriginal family of six after his wife's tragic death and became a champion of the equal treatment of Aborigines. 'I fought for every black that I seen wronged', Leandro wrote, reflecting on his life. One of his first public appeals in defence of equal treatment for Aborigines was as early as 1925 when in the *North Queensland Register* he declared his principles and attitudes to his 'dark brother' to be ones based on equality rather than corrupted paternalism. He never stopped demanding a similar stance from official Australia until his death in 1946 (Govor 2000).

Russian post-revolutionary studies of, and attitudes towards the Australian Aborigines might provide enough material for a special article. Here I outline only the main tendencies of this period.

After the revolution of 1917 formal defence of racial equality became the state policy in the Soviet Union as if continuing the pre-revolutionary democratic traditions. On the part of official Russia, which at that time brutally exiled whole nations (like Chechens or Crimean Tartars) and gradually replaced national cultures by uniform socialist 'culture' among its national minorities, it was predominantly hypocrisy. It reached its apotheosis in Vyshinsky's defence of the Australian Aborigines at the United Nations Assembly at the time of Cold War. Russians themselves considered Vyshinsky, the main prosecutor at Stalin's show trials of the 1930s, a personification of hypocritical brutality. Still, propaganda of racial equality had its positive effect on the Russian population at large. Russians, although possessing some prejudices towards their neighbours — Jews or 'blacks' from Central Asia and the Caucasus, did sincerely sympathise with the indigenous peoples abroad. Australian Aborigines were least known and thus most attractive. The tradition was set in 1928 when the Russian writer N. Moguchii wrote a novel about heroic resistance of the Aborigines to the white invasion. In the following decades Soviet journalists, writers and visitors to Australia produced over a hundred books and articles in which condemnation of the 'Australian colonisers' treatment of the Aborigines became a common theme (Govor 1985:113–7, 1989:31–3). Although for some of them it was propaganda, many writers took it to heart. I, for instance, as a teenager was deeply touched by an unpublished poetry cycle by Galina Usova 'The Perished Tribes'. My chance acquaintance Alla, an anthropology student brought up in an orphanage, told me how in the 1980s she literally wept with joy sitting over an article in the library when she suddenly 'discovered' that Aborigines had not 'died out' but were reviving. The Aboriginal problem also became an excuse for vast translations into Russian of Australian fiction dealing with Aborigines (Govor 1985:254–314). As for the Aboriginal 'revival' and the positive role of the Australian government, it was held suppressed as long as possible. Anthropologist Vladimir Kabo, while visiting Fred Rose in Berlin in 1985, received from him Australian newspaper cuttings concerning land claim developments in connection with official recognition of Aboriginal spiritual ties with their land and told about it to a surprised audience at the conference on Australia and Oceania in Moscow in 1986 (Kabo 1986).

By that time 'perestroika' and freedom of speech were gaining pace. The State did not insist on compulsory propaganda of racial equality any longer. Journalists were not obliged to follow the old traditions. And they did not. Two of them, visiting Central Australia in 1994, created a telling example of ignorance and prejudice in their depiction of encounter with the Aborigines 'as they are' which was eagerly published by a popular Russian newspaper (Kucher and Berestov 1994). V. Kabo and O. Artemova's protest to the editor fell on deaf ears. Indeed it was freedom of speech! Years of propaganda, it seemed, had no lasting effect on public mentality. Nearly half of Russian Internet sites with the words 'Australian Aborigines' are dirty jokes about them. On the other hand Aborigines begin to attract attention of the Russian parascientists, for instance as inheritors of a mysterious lost civilisation.

Soviet anthropologists writing about Aborigines for years had no chance to conduct field work in Australia and based their research on Western sources. Aborigines attracted them as a perfect example of a primitive human society, as a field for theoretical speculations. As such Aboriginal studies experienced the pressure of Marxist-Leninist dogmatism in the most ridiculous ways. Up to the early 1930s there still was some diversity of opinions, for instance Maksimov, who never accepted either evolutionism or Marxism, continued his original studies till 1930. A number of talented scholars — S.A. Tokarev, P.F. Preobrazhenskii (repressed), A.M. Zolotarev (repressed), M.O. Kosven, A.B. Piotrovskii — wrote fruitfully in that period. By the 1930s, as Stalinism progressed, Marx's and Engels' remarks on the Aborigines — which they drew mainly from L. Morgan's works of the 1870s (see Spriggs 1997) — were elevated to Holy Script. A new group of militant 'proper' Marxist anthropologists unleashed a struggle for the purity of Marxism in Aboriginal studies (E.Iu.Krichevsky, V.I.Ravdonikas, I.N.Vinnikov) (Govor 1985:11, and section on 'Ethnography'); some of them were repressed later too.

In spite of the spirit of Marxism itself which considers that the basis (economic relations) determines the superstructure (all other relations) in the case of Aboriginal studies their clan and kinship relations were placed over their basic socio-economic unit (band or local group). It happened because Engels believed clan and kinship relations to be the main institutions of primitive society (local groups had not been studied at that time). After World War II, when facts about the role of the local group in Aboriginal society had reached Soviet scholars, Sergey Tolstov, the head of the Institute of Ethnography, declared that the local group emerged only under the influence of colonisers and labelled it as 'the cancerous cell on the body of the primitive society'. Moreover, it was considered axiomatic that in prehistory matriarchy preceded patriarchy. This idea derived from Engels and was later consolidated with the authority of Stalin, who happened to refer to it in one of his articles and thus it became a Holy Script of Soviet prehistory, including Aboriginal studies (see interesting discussion of it by Rhys Jones in the section 'State dogma and Joseph Stalin' in afterword to Kabo 1998:296–7; Kabo 1990).

Up until the 1980s Soviet anthropologists belonging to the anthropological establishment were under the spell of these postulates. The younger generation of the 'rebels', whose 'crime' was just to return to the idea of Marxism versus Soviet style dogma in the field of Aboriginal studies, were handicapped in their research and careers. By the late 1980s the old guard had lost its battle, but by that time Soviet anthropology as a whole discovered itself at the edge of the mainstream of Western anthropological studies. Still the efforts of Soviet scholars aiming to convey the truth even under the totalitarian regime was not in vain; in their own way they contributed to the final collapse of Soviet ideological dogmatism. Vladimir Kabo produced a profound and complex study of the origin of Aborigines; later he published his comparative book *Primitive Pre-agricultural Community* — the result of his long battle with dogmatism in social issues, and for years he was writing 'in desk' about Aboriginal religion as well (Kabo 1998). Olga Artemova, drawing mainly on Aboriginal society, revised a widespread concept of egalitarianism in primitive society arguing that in reality Aboriginal society was more complex than previously believed, and that the personal qualities and abilities of an individual are of utmost importance in that society. Later she became the first historian of Russian Aboriginal studies (Artemova 1987, 1991). Levon Abrahamian from Armenia, a 'clandestine' follower and original interpreter of prohibited Western ideas, managed to publish his 'heretical' masterpieces on Aboriginal spiritual culture in obscure collections (Abrahamian 1983). Hopefully Russian anthropologists of the new millennium will draw from the aspirations, mistakes and tragedy of their less fortunate predecessors.

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'Knowing' and 'Being' in Place in the Western Desert

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ATTACHMENT TO place is a major theme in discourses by and about Aboriginal Australians. Appeals and demands by Aboriginal people for land rights are usually couched in terms of spiritual and emotional attachment, with economic considerations often being secondary or unstated. The strength of Aboriginal people's connection to the land and their knowledge of it impress observers as exceptional, and today phrases such as 'they belong to the land, rather than the land belonging to them' or 'the land is our mother' have considerable currency in popular discourses. In addition, most scholars who have written of Aboriginal societies have attempted to describe and explain their relationship to places in the landscape. Rhys Jones (1985, 1991), for example, has observed that Aboriginal people often define themselves in terms of the landscape with which they are associated: 'Thus the core of one's geographic perception was kin-based, its centre the country belonging to one's own people. This was often expressed by people defining themselves according to the special characteristics of this core territory' (Jones 1985:190).

The notion of 'totemic geography' has long existed in the anthropological literature (cf. Strehlow 1970; see also Moyle 1983), and there is frequent overstatement in popular literature about the mnemonic knowledge of country held by Aboriginal people via ancestral 'tracks', mythology and song (e.g. Chatwin's ***The Songlines*** 1987). Swain (1993:35) somewhat controversially removes both history and time from Aboriginal ontology in favour of 'an uncompromising insistence on the immutability of place'. Given abundant evidence to the contrary, one need not go this far yet still concede the centrality of place in Aboriginal world view.

Despite some excesses in the accounts of observers, there is considerable evidence that, among many Aboriginal Australians, attachment to place transcends physical presence in the land. In addition to multiple linkages of people, as spirit and flesh, to place, and through place to the spiritual realm of the Dreaming, there are also the many ways in which the living in every generation become 'imprinted' onto place. Among the Western Desert people with whom we have worked, for example, ordinary places assume special significance for individuals because of small happenings that may prove revelatory, as when a parent or parents of an as yet unborn child encounter the animal or plant that is their child's conception totem (cf. Berndt 1970:218). Personal identity is closely tied up with one's place of conception and/or birth. Such sites are loci for the transformation of objects in the landscape into human subjects. Today, as signs of the times, the sites of car crashes or similarly significant happenings, or even frequently used camping spots, become embedded in the consciousness of local people as mnemonics for commemorating both happiness and misfortune. The popular and scholarly understandings of attachment have also influenced the development of land rights laws; for example, the concept of 'spiritual affiliation' contained in the Aboriginal Land Rights Act (Northern Territory) 1976. A major requirement for native title in the current Australian legislative regime is that Aboriginal claimants be able to demonstrate their continued physical association with the land they are claiming. This is a new development in response to the land rights struggle and raises interesting questions about the complex nature of both 'knowing' place and 'being' in it.

In this paper we explore a number of these inter-linked themes, but particularly the meaning of 'being' in country. The Aboriginal people with whom we have worked in the Western Desert claim that their occupation and care of homelands have continued uninterrupted, despite their migration to settlements on the desert fringe in the assimilation era. They assert that, not only have they maintained such links to their homelands but that they continued to carry out the necessary guardianship and culturally reproductive functions consonant with physical presence. They have done this via the medium of dream-spirit journeys (known as *partunjarri*), during some of which elders would visit major waterholes and other sites, perform increase rites, and 'look after' their land (Tonkinson 1970). Another significant medium of connection is the performance of songs, stories and ritual by people where they happen to be, about and for the benefit of the distant place. Also extremely important are the manufacture, care and use of a range of portable sacred objects that symbolise and embody specific places as well as the associated ancestors and their imprinting activites in creating the landscape during the Dreaming epoch.

These phenomena raise significant questions for native title. While, emically, there is no doubt as to the 'physical reality' of spirit journeys, it is unlikely that these communications would qualify as continued 'physical' association in Western legal reasoning and argument. These are potentially conflicting understandings of attachment to place that are not only intellectually challenging but have consequences for the resolution of land rights claims. Such divergent perspectives raise interesting questions about the complex nature of both 'knowing' place and 'being' in it.

'TRADITIONAL' ATTACHMENTS TO LAND

In the world of nomadic hunter-gatherers, Aboriginal Australians stand out not only for the complexity of their social organisational forms and religious life but also for the emotional strength of their anchorage to land and its intimate relationship to identity, religion and belonging (Berndt and Berndt 1964). As Williams (1986:18) aptly notes, the 'sacred endowment' or religious charter that establishes and validates Aboriginal relationships to, and rights in, land is also historical and economic; these three categories are complementary rather than mutually exclusive. Over the past century, many scholars have contributed significantly to our understanding of the relationship of land to local and social organisation. Stanner (1965a), in particular, clarified this relationship by making a distinction between the 'range' and the 'estate'. Nomadism, necessitated by both ecological considerations and the mode of adaptation, coexisted with multiple cultural mechanisms, religiously validated, which ensured the development and maintenance of very strong emotions of attachment and deeply felt obligations and responsibilities towards significant sites and home estates (cf. Barker 1976; Hiatt 1984). Strehlow, who rated the absence of a division between Time and Eternity as Aboriginal Australia's great contribution to religious thought, stressed that ritual efficacy in Central Australia depended on specific site-based performance, in a society where everyone had a role to play in keeping nature functioning harmoniously (Strehlow 1970:132).

A major contribution to our understanding of the logic of attachment comes from Munn (1970), who described how desert Aboriginal religious worldview has at its centre a series of subject-object transformations, entailing metamorphoses, imprinting and externalisation, initiated by the creative ancestral beings of the Dreaming epoch. She showed how 'country' (the object world) both anchors the human subject's consciousness and identity and mediates relationships between the individual agent and the Aboriginal collectivity. Another valuable exposition of the nature of connectedness has been that of Rumsey (1989), who discussed the often misunderstood relationship between dialect (or language), people, and country. He emphasised that the person-language linkage is indirect, arising from the fact that both derive from the same stretch of country. The Dreaming ancestors originally left behind *both* language and the human inhabitants in pre-totemic life-essence form.

The scholars just cited, along with many others, have provided a coherent picture of the probable nature of Aboriginal attachment to land at the time of the European incursion. A complementary corpus of archaeological data, to which Rhys Jones (e.g 1980, 1991) has been a notable contributor, now exists. Theorising from the excavated record, it underpins significant regional variation, and important contentions concerning both continuities and innovation in the many millennia of Aboriginal occupation preceding colonisation by the British, beginning in 1788. The archaeological record affirms very strongly both the fact of Aboriginal attachment to place and of the imperative of movement to maintain the subsistence economy in the face of a ubiquitous 'imminence of diminishing returns' (Sahlins 1972). The existence of 'relative plenty' in parts of Aboriginal Australia, such as northeast Arnhem Land, has also been confirmed both archaeologically and anthropologically (e.g. Meehan 1982), as has its converse, for the Western Desert, characterised as one of the most marginal environments for human survival on earth (Gould 1969, 1980).

Attachment to land throughout Aboriginal Australia was derived from multiple sources yet, in an emic sense, ultimately came from the Dreaming, the creative epoch in which, through the actions of innumerable superhuman beings, landscape and culture ('the Law') took on their current forms as immutable (cf. Stanner 1979). Humans, as objects of ancestral creativity, are also subjects, whose essence is held to derive from the Dreaming via the land. Moreover, the humans imprint themselves on the ancestrally created landscape, investing it with new significances that have the potential over time to become stripped of the status of human acts as they are seamlessly absorbed into the realm of the Dreaming (cf. Munn 1970; Tonkinson 1991). Each new generation of people thus invests the land they live on with culturally significant new meanings and associations.

Hiatt and Jones (1988) have argued that among Aboriginal people there is a seamless merging of objective knowledge of the natural world and subjective revelation. Two main forms of 'knowing country' emerge from the ethnographic data. Firstly, existential knowledge derives from the experience of actually living on the land, being physically in country, seeing landscape. The second kind of knowledge derives from the religious base; it results from being taught by one's elders to 'see' the transformations in the landscape achieved by the creative beings of the Dreaming. Also derived from the exposure of males and females to the Dreaming is a body of knowledge, transmitted principally via mythology, songlines, stories, and ritual, about places not directly seen and experienced but dramatised in religious speech and performance. Adults may thus vicariously experience the travels of major creative beings and their adventures at particular places en route. This kind of knowledge enables people to talk authoritatively about places and events unseen directly but nonetheless apprehended - 'known' through story and song. By these means one may take a journey of discovery and enlightenment without leaving the campfire. Country is thus intellectually known and assimilated, in advance of, or even without, actually visiting it.

A major aim of the male initiation process in the Western Desert, for example, is to maximise the achievement of both kinds of 'knowing'. Traditionally, this was effected through extended trips by novices, accompanied by a small group of elders, following the paths (*yiwarra*) of prominent creative beings as they meandered far and wide across the face of the land in the Dreaming. Today, a similar imperative exists but the journeys, over long distances, are undertaken in vehicles, rather than on foot. By this protracted process young men are introduced to neighbouring peoples and their countries, and acquire an experiential base for 'knowing' the land in addition to what they may already intellectually know of it. Their participation in ritual with the many local host groups encountered during their extensive travels entails more than merely becoming knowledgeable about a stretch of country and the stories of its creation. In special male-only rituals, they are initiated into secret lore, entailing ceremonial feasts given by local hosts in reciprocity for the young men having hunted meat for their male elders. The young men eat ritually prepared 'dampers', but are also symbolically 'ingesting' the country of their hosts. Henceforth, they carry this country within them as an intrinsic component of their identity, and are thus assured of unchallenged access to its food and water resources. In this way, the territory is 'opened' to them as future participants in major rituals performed there.

Traditionally, attachment to land was underwritten by a strong religious imperative to nurture country, which was effected by both ritual and 'practical' means. The former is exemplified in the widespread institution of 'increase centres', from which multitudes of a particular flora or fauna would be 'brought up' in response to ritual performance (cf. Meggitt 1962:220–1). In the Western Desert, for example, the wide scatter of such increase centres ensured a sense of interdependence among constituent groups, the survival and reproduction of whose societies were deemed, at base, to inhere in a ritual mode of production (cf. Bern 1979; Hamilton 1982:90–92; Tonkinson 1988:160). The latter, or ecological, dimension of nurture or 'management' is most clearly demonstrated in burning strategies, whose importance for human impacts on the Australian landscape was clearly recognised by Rhys Jones (1969) in his now famous article, 'Fire-stick farming'.

As Peterson (1972) has suggested, a major reason for the widespread patrivirilocal bias in local group membership among Aboriginal Australians would have been the strong sense of responsibility, inculcated in adult men, for the care and maintenance of important sites and ritual paraphernalia in their heartland estates. Also impressed on these men was the necessity to organise and host the periodic performance of major rituals in those estates. In the Western Desert, at least, highly permeable boundaries, very low population densities, high levels of mobility, and an ethos of inclusivity combined to ensure that, through repeated attendance at 'big meetings' (*japal*) held at different major sites, people's experiential and intellectual knowledge of country was maximised over time (Tonkinson 1988). Repeated participation thus ensured not merely the acquisition of knowledge, but the assumption of obligation and responsibility, and the conferral of a range of rights in country (cf. Moyle 1983:87). In the expansive worldview of Western Desert people, 'society' always exists at a level beyond that of its periodic manifestation in the aggregation here labelled 'the big meeting', and a high rate of diffusion of new rituals and religious lore around the region reinforces this outward-looking orientation. Because people often sing about and act out ancestral exploits in places way beyond their mere physical acquaintance, their worldview is necessarily open-ended. It is shaped by the expectation of more to be known and assimilated, not merely by passive absorption, but through an active and enthusiastic embrace of the new - in the guise, however, of the immutably old.

TO 'BE' OR NOT TO BE IN PLACE?

Following colonisation, displacement and marginalisation became the rule for Aboriginal experience of domination, and many people were forced by historical circumstances from their ancestral domains. Thus the lands groups live on today may not traditionally have belonged to their forebears, yet their derivation of personal identity from it (via totemic connections, for example; see Stanner 1965b) and their imprinting on it occurs in ways that are nonetheless utterly 'traditional' in form and cultural significance. In some cases, the original owning groups have died out, or have long since moved permanently elsewhere, and, via established principles of succession and caretakership, neighbours or immigrants have come to assume a status akin to 'regents' (as Sutton suggests), but one which according to common understanding becomes permanent (1994:29). In some areas, such as among the Mardu in the Western Desert, the assumption of 'owner-like' responsibilities is formally acknowledged through ritual. In terms of current native title legislation, then, those who dwell on the land in much of the interior and north of the continent will normally have developed over time certain 'rights and interests' that arise from 'traditional laws and customs' (Merlan 1997:19). Particularly in the more rural and remote regions, large numbers of Aboriginal people no longer living on their ancestral homelands have generated sets of connections to their adopted country that in their view justify their claims to be considered as owners or co-owners of that country. Traditionally, there appear to have been no clear-cut hierarchies of entitlement criteria; land ownership was typically not at issue, so there would have been no need to prioritise and rank such fundamental connectedness to land.

In the case of the Mardu Aborigines with whom we have worked, the period of time that the homelands were 'empty' following the movement of the last nomadic groups into settlements on the western fringe of the Western Desert was brief (less than twenty years). However, there is no suggestion that what outsiders would construe as physical absence constituted abandonment by these people of religious and 'economic' obligations and responsibilities towards their ancestral lands. On the contrary, adult Mardu who were born in the desert attest that vital communication with their ancestral homelands continued uninterrupted, via dream-spirit travel, and that their connection to place, like their emotional attachment, remain undiminished. Even though the generation of old people who were active 'dream travellers' between settlement and ancestral estates has now largely disappeared, the beliefs remain. For example, major journeys remain part of certain rituals during which the elderly 'bosses' (*nyamparli*) and diviner-curers (*maparn*) would take all participants on such journeys to visit the relevant religiously significant sites. To take the example of rainmaking rites, the travellers would 'bring back' rain to the area in which the ritual was being performed.

Although such cultural institutions as dream-spirit journeys are likely to be greeted with suspicion and resistance in the native title era, they exemplify the dynamism that is, and was traditionally, inherent in Aboriginal religious life. Altered states of consciousness, and particularly dreaming, are closely associated with highly valued communication that is sometimes initiated by spirit intermediaries of the withdrawn ancestral beings of the spiritual realm, resulting in the flow of new knowledge into human society. Such dynamism, especially the possibility that previously unrecognised connections and sites may be revealed, challenges the dominant society's stereotype of an immutable Aboriginal tradition locked into the pre-colonial past, frozen and resistant to change, especially if that change includes post-contact elements carrying the marks of European infiltration. Cultural change is, of course, universal but the fact that non-literate cultures, based as they are on oral traditions, are eminently susceptible to change over time sits uncomfortably with many official and popular views of 'traditional' cultures. In addition, and paradoxically, Australian legal principles, especially in relation to heritage and land tenure issues, place Aboriginal systems at odds with the dominant society's view of legitimate claims of land ownership (cf. Tonkinson 1997). In this view, boundaries once drawn are intended to be fixed, and to change only in accordance with codification in law that maintains the fixity, as foundations on which subsequent legal judgments can be made.

In Aboriginal Australia, however, not only is dynamism a constant, but cultural variation means that the construction of boundaries rests on differing principles in different parts of the continent. In the interior deserts, for example, territories of constituent groups are rarely bounded or delineated by reference to topographical features, and perceptions of belonging and homeland centre on points in space, specific sites and constellations of sites rather than bounded entities. Religious and cultural salience rests in focal points and multiple chains of connected sites (often commemorating or emulating the routes of creative ancestral beings in the Dreaming) rather than peripheries, which are conceptualised as zones rather than lines, reflecting the essential permeability of whatever geographical boundaries are held to exist. Underlying ecological imperatives, a small but highly mobile population, and a variety of cultural mechanisms, in combination, ensure a flow of people across the boundaries that do exist, restricted only by the observance of the proper etiquette. This situation makes for an open-ended social system, where cooperation is enjoined to ensure survival under very harsh conditions, affiliative criteria are multiple, and an inclusivist ethos dominates.

Attachment, then, is to sites of personal and/or religious significance to individuals and groups, because care and proprietorship of land, plus associated responsibilities of nurture, custodianship, ritual performance, and guardianship of sacred fixed and portable objects are site-focused. The Aboriginal understanding of attachment to place and knowledge about land that we have described has practical application as well as academic significance. Because the Mabo decision and subsequent enabling legislation stress the centrality for native title of indigenous 'law and custom', the emic perspective should be accorded full recognition as fulfilling the continued association test. For people like the Mardu, dream-spirit contacts with homelands are as 'real' as visiting the places in waking life. One could also argue that the existence of portable sacred objects that symbolise and embody country ensure that, wherever they are, as long as Western Desert men and women have such objects under their care they are continuing to 'hold' country and to exercise their responsibilities of guardianship towards it through the manipulation of such objects and the performance of relevant rituals.

The concept of 'being in place' is thus more complex, at least as manifest among the Mardu, than it appears at first sight to be. We must entertain both the idea that there is a metaphysical sense of being in place that may be as real from the emic perspective as physical presence, and the possibility that portable representations or embodiments can 'be' or function as place, no matter where they are physically located. The latter conception suggests that when people departed from their homelands traditionally, and suffered enforced and protracted physical absences from their estates because of drought conditions, they nevertheless remained at least doubly bound to them. First, via dream-spirit visits and, secondly, because of the assured presence of portable sacred objects, symbolising sites and ancestral events in their homeland, located in the territories of their neighbours. Having one's place in the midst of others' territories is a legacy of an institutionalised reciprocity that today continues to lie at the heart of abiding political and social relationships among Western Desert people.

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The Great Flood: Eustatic Change and Cultural Change in Australia during the Late Pleistocene and Holocene

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> I am going to bring floodwaters on the earth to destroy all life..., In the six hundredth year of Noah's life, on the seventeenth day of the second month — on that day all the springs of the great deep burst forth, and the floodgates of the heavens were opened..., The waters rose and increased greatly on the earth..., (Genesis 6:17, 7:11, 7:18, 7:20).

> Thousands of spirit birds moved south from the desert sands, across the treeless interior of the Nullarbor Plain to the face of the sea. The Spinifex People call these spirits Sun People and Shadow People. They carried huge numbers of desert spears. The Sun People faced the encroaching water from the cliff tops. The Shadow People blocked the valleys and gorges. Between them they built massive ramparts with the spears. They dammed the sea, leaving the spiritual monument of the Nullarbor Cliffs and Hampton Escarpment as testimony to their great victory (a story of Pila Nguru, the people from the spinifex; see Cane 2000a).

DOES THE biblical account of the Great Flood represent a mythological history of the last interglacial rise in sea level? The thought is tempting, bound tentatively to a biblical adaptation of the Sumerian water god, Enki, and Noah himself — thought to be Ziusudra, a priest king of southern Babylon who ruled ca. 4900 BP (Johnson 1996:9). Equally, one wonders if the scatter of Aboriginal mythologies describing catastrophic inundation of the coast around Australia are an historic memory of the same phenomenon. Some archaeologists entertain the possibility and cite linguistic and anthropological evidence in the Gulf of Carpentaria, Arnhem Land, Port Phillip Bay, Kangaroo Island and Rottnest Island to that effect (Mulvaney 1975; Flood 1983; Dixon 1972; Isaacs 1980; Berndt 1940; Tindale 1987; Dortch and Morse 1984; cf. Sutton 1991:251).

How should one conceive an event as catastrophic as the inundation of the Australian coastline? Would an event of such magnitude have had a profound impact on human behaviour and a corresponding cultural expression that transformed itself into folklore? The environmental impacts were such that it is hard to conceive that there would not have been some proportional cultural response. Eustatic change saw the loss of a seventh of the continent, over 2,500,000 km^2 of foraging land, the drowning of river valleys, the creation of islands within sight of the mainland. While the average annual incremental vertical rise in sea level was minimal, and possibly erratic across the long period of inundation, the lateral encroachment of the sea was significant where the terrain was flat. The Nullabor, for example, saw the sea move inland at up to 1 m a week (Wright 1971:13; Mulvaney 1975:137), and other horizontal landscapes in northern Australia experienced rates of inundation as great as 5 km annually (Flood 1983:183).

In such settings the rate at which the vertical increase in sea level occurred meant that the lateral spread of the sea was substantial in time and space: it took place within the span of generational memory. People are likely to have known the sea was rising and, presumably, were affected by it. Effects must have included loss of territory, alterations to subsistence regimes, loss of ceremonial and spiritual locations and the search for physical and metaphysical explanations for the changes. How did human society respond? Were people physically displaced from their homelands in any appreciable sense? Did human subsistence and settlement have to change substantially to accommodate an environmental change that was spread over some ten millennia? Was there a culturally significant increase in population as those from the drowning coasts fell back onto the coastal hinterland? Would human society have changed to the extent that its cultural response could be detected archaeologically?

Perhaps the first question to be resolved is whether or not the changes in sea level between 10,000 BP and 6000 BP were sufficiently radical to create an identifiable social adjustment. Until recently answers to this question have been speculative - but generally in favour of large scale, even catastrophic, social change. Mulvaney (1975:136) considered that the 'consequences for human migration and land settlement were far reaching', that the impact on Pleistocene settlement must 'have been dramatic', and that regions larger than tribal territories disappeared, requiring 'adaptive efforts and a consolatory philosophy' as huge tracts of land were submerged (also Mulvaney and Kamminga 1999:120). Flood (1983:179) considered that the drowning of the coast 'must have had a profound effect on those who lived there'. Blainey (1976:90) stated that 'language, marriage patterns, genetics, religion, mythology and warfare - all must have been affected by the rising seas around Australia' even though 'the archaeological record so far has not even whispered about these effects'. Bowdler (1977:209) considered such pronouncements unnecessarily dramatic.

Archaeologists cite evidence for a broad range of cultural responses associated with climatic change at the close of the Pleistocene (O'Connell and Allen 1995) as well as a range of behavioural responses in coastal areas after the rise in sea level (Hughes and Lampert 1982; Lourandos 1997). However, there is little archaeological evidence and contingent speculation as to the impacts of the rising sea on human populations and culture. Actual archaeological evidence for any such impacts are, of course, hard to come by because the archaeological material associated with that era is generally underwater. Dortch (1984:80) thinks the archaeological record at Devils Lair between 16,000 BP and 12,000 BP may provide 'a local record of a population increase resulting from sea level rise'. Taçon and Brockwell (1995) propose social and economic changes across the coastal plains of Kakadu resulting from demographic change in the late Pleistocene and early Holocene — a general response to the post-glacial change accepted by others (O'Connell and Allen 1995:857; Frankel 1995:654; cf. Beaton 1985:16; 1995). Barker (1991) believes that coastal societies coped comparatively easily with the economic and cultural demands associated with a rising sea level. Taçon and Chippendale (1994) provide evidence for considerable social change and territorial conflict during, and attributable to, the final phases of eustatic change.

This paper seeks to explore the impacts of the post-glacial rise in sea levels on human populations around the Australian coastline. It presents archaeological evidence from the Nullarbor Plain for a concentrating effect on the coastal populations and associated diversification of human behaviour across the period of eustatic change. The evidence from the Nullarbor Plain suggests that the loss of territory associated with the rising seas had a discernible impact on human behaviour, notably on the development of marine economies around the Australian coastline.

THE DROWNING OF THE NULLARBOR PLAIN

The Nullarbor Plain is one of the landscapes most often quoted in the context of Australian post-glacial sea level rise. It has a large flat coastal plain which is vulnerable to even a minimal rise in sea level. For example, Koonalda Cave is currently 22 km from the present coastline but at 16,000 BP the coast was 180 km away. The present sea cliffs were then an inland escarpment backing a long flat coastal plain:

> On such level ground a slight vertical rise in sea level would have produced massive horizontal encroachment resulting in vigorous removal of territory available to man … land was being lost at the rate of three to four feet a week between 14,000 BP and 13,000 BP. … the constricting effects of this eustatic change must have had economic and social consequence. Individuals thirty years old might have lived through the destruction of a mile of their coastal territory (Wright 1971:15).

Koonalda Cave, however, does not provide evidence for the impact of eustatic change on general patterns of human behaviour. It was occupied sporadically ca. 16,000 BP and used specifically as a flint quarry. The behaviour captured within it is thus task-specific and related to the procurement of raw materials and artistic endeavour rather than settlement in the course of regional domestic subsistence activities. Allens Cave, 80 km to the west of Koonalda Cave, was occupied in a more conventional manner.

Excavated in 1969 (Marun 1969) and re-excavated by Jones in 1989–90 (Cane and Jones 1995) the site was first occupied ca. 39,000 BP (Murray and Roberts 1997) and is the oldest known site in the Western Desert (cf. O'Connor et al. 1998). During the last glacial maximum, the period when the sea was most remote and aridity highest, occupation was intermittent with average discard rates of ca. 0.5 artefacts per kg of deposit. Around 16,000 BP this rate increased to 1.5 artefacts per kg of deposit. These compare with figures from the Western Desert and Central Australia which suggest ca. 2 artefacts per kg. of sediment (Smith 1989; Veth 1993a).

The archaeological evidence thus suggests a minimal human presence across the Nullarbor Plain during the period of glacial aridity. The scale of occupation is consistent with a model of occasional short term settlement, not unlike that seen in the Western Desert today, where opportunistic bands of people use periodic climatic events, such as heavy rainfall, to exploit the infertile desert heartland. In such circumstances extended forays into inhospitable environments are typically aimed at key subsistence resources such as large game, stone tools (as in the case in Koonalda Cave) or sacred materials (such as ochre, pigments and particular kinds of wood).

The Pleistocene archaeology in Allens Cave points to the presence of a rudimentary nomadic society in an arid terrestrial landscape that was probably not enclosed by tight territorial controls. It is likely that the marginality of the Nullarbor environment encouraged the development of an inclusive social system and open economic networks of the kind found throughout the Western Desert today (Berndt 1959; Tonkinson 1978; Peterson and Long 1986; Cane 2000b). The art work found at Koonalda Cave, and another unnamed cave 250 km to the east (Cane 1996) has a stylistic homogeneity indicating open social and economic networks and is characteristic of groups with low population densities and high mobility (Gamble 1982; Smith 1992).

Evidence for human occupation at Allens Cave increased and diversified as the sea rose between 14,000 BP and 10,000 BP. A fragment of abalone (*Haliotis laegivata*) was found in the site at a stratigraphic level equivalent to ca. 13-14,000 BP, when the sea rose rapidly from 130 km to ca. 65 km from the shelter (Wright 1972:13). The fragment appears to have been transported to the site as a decorative item, possibly a pendant (Shepherd and Thomas 1989; Nicholson 1994; Akerman pers. comm. 1989). Two fragments of a bivalve (*Katelysia* cf. *scalarina*, P. Colman, Australian Museum, pers.comm.) are also located at a stratigraphic level equivalent to ca. 10,000 BP, when the sea was rising up the face of the present Nullarbor cliffs, 4 km to the south (Wright 1971; Short et al. 1986). The shell either entered the site as a derelict collected from the beach or as a small item of food.

The cockle fragments sit below an exceptionally large hearth and together are associated with a significant change in the cultural character of Allens Cave. Two carbon samples provide ^{14}C dates of 9530±190 BP (ANU-6849) and 9270±140 BP (ANU 6850), an OSL date of 10,100±600 BP and a TL date of 11,100±800 BP have been obtained just below the hearth (Murray and Roberts 1997). Marun (1969) reported an earlier ^{14}C date of 8750±140 (ANU-1040) for approximately the same stratigraphic level.

This hearth is associated with the greatest concentration and diversity of artefacts in Allens Cave. Here discard rates are three times higher than the site average and the range of raw materials changes significantly. Limestone decreases from 50% to 8% of the assemblage, flint increases to 77% and other materials, including silcrete and tektites (15%), occur. The increased proportion of flint is predictable and accompanies the exposure of flint nodules by the rising sea at the base of the newly forming Nullarbor cliffs. The presence of silcrete and tektites presumably reflects cultural change as silcrete is not found on the Nullarbor Plain and has to have been transported at least 200 km from the nearest source in the desert (to the east or the north). Tektites are rare on the Nullarbor, and are typically collected by initiated men, and are associated with trade, magic and sacred rituals in the Western Desert today.

Exotic items such as silcrete, shells and tektites suggest three possible cultural scenarios: either the movement of people with these materials from outside the local area into Allens Cave; the transportation of these materials within new cultural systems (which, more accurately, are likely to be re-organisations of the pre-existing systems); or both: i.e. the movement of people and the development of new cultural systems in response to the rising sea level. All three mechanisms involve the movement of cultural materials from the sea (shell) to the south and the desert (silcrete) to the north. What activated such cultural change? One possibility is that the rising sea in the late Pleistocene created new environmental circumstances which initiated cultural change. It is conceivable that the rapid inundation of the broad flat Nullarbor coast led to a loss of foraging territory, a proportional increase in population (through migration), increased pressure on available resources and the development of different behavioural mechanisms, such as new trade routes and extensive religious networks, to accommodate demographic and territorial change and improve social and economic opportunities in the context of these changes.

It seems feasible that the openness of the social and natural environment of the Nullarbor Plain during the late Pleistocene would have accommodated either a migratory coastal people or the development of a system of trade and exchange in a manner that was not catastrophic, but which would nevertheless have necessitated some modification to the customs and traditions of the current residents as competition for resources (both social and economic) increased. One means of coping with such change would have been to develop a more diverse and effective subsistence system: one which utilised the landscape more fully, involved the exploitation of a wider range of resources and obtained resources from further afield. Such changes have a potential archaeological correlate in Allens Cave. Thus migration might be implied by the presence of sea shells in the site. Inclusive territoriality may be suggested by the appearance of tektites which were presumably collected from the larger Nullabor area and carried to the site at this time. Extended economic networks are suggested by the appearance of desert silcrete, which was transported to the site from the north. Some form of demographic change - population increase, more frequent cave use or longer visits to the site - may be implied by the increase in artefact numbers at 10,000 BP. More effective use of available resources may be evidenced by the exploitation of local flint, the unusually large hearth, presence of shellfish and abundant burnt bone in the site at the close of the Pleistocene.

Similar evidence in southwestern Australia

Parallel changes in material culture have been noted in similar geographic and chronological settings across southern and western Australia. At the Seton Site on Kangaroo Island one finds limited evidence of human presence between 16,000 BP and 11,000 BP. Coastal flint was found in the site throughout this time, when the coast was up to 50 km away. At 11,000 BP there was 'an intensive phase of occupation' marked by a 'large ash lens' and a 'handful' of marine molluscs — when the coast was 16 km away from the site (Lampert 1981:103,108,114).

A similar association between coastal cultural elements and terrestrial cultural settings is seen in other terminal Pleistocene sites in Western Australia. At Cheetup Cave occupation is documented between 13,200

BP and 8200 BP (and again at 2500 BP until the present). At 13,000 BP the site reveals 'seaweed associated with c.100 human neonatal fragments' at a time when the sea was 80-100 km away (Smith 1996:6). Tunnel Cave was occupied from 19,000 BP to 12,000 BP and contains 'a few fragments of marine mollusc shell' (Dortch 1994:45) at a time when the sea was at least 35 km from the site. Devils Lair was occupied at least between 40,000 BP and 12,000 BP. It contains traces of fish at 16–17,000 BP and marine mussel at 12,000 BP, when the sea was 25–35 km from the site (Balme et al. 1978:60; Dortch 1984 suggests an earliest date of ca. 30,000 BP but new unpublished claims suggest humans in the area of the cave ca. 48,000 BP). Devils Lair also reflects increased intensity of human occupation through the late Pleistocene, in a manner that is reminiscent of Allens Cave — there is 'a thin but archaeologically rich occupation floor' dated to between 12,000 BP and 13,000 BP (Mulvaney and Kamminga 1999:178). Dortch thinks the nature of the cultural material 'hints' at a 'population increase' and 'regional intensification of human occupation' that may 'well provide a local record of a population increase resulting from sea level rise' (Dortch 1984:81; cf. O'Connor et al. 1993).

The concentration of cultural material, including the presence of marine materials at these sites during the terminal Pleistocene may provide genuine archaeological evidence for the translocation of coastal people and marine traditions across the arid hinterland as the post-glacial seas rose. The cultural momentum at these sites is accompanied by a comparable variation in cultural materials at other sites in the area. Quininup Brook, for example, was occupied between 18,000 BP and 10,600 BP, and displays a peak in artefact discard rates 'sandwiched in a 30 cm deep zone' across the Pleistocene-Holocene transition (Smith 1993:58; Ferguson 1981). Collectively these changes point to considerable cultural change and adjustment at the close of the Pleistocene. These changes are concurrent with, and may be a direct consequence of, the rising seas.

In the case of the Nullarbor Plain the sea was still rising at 10,000 BP but the land mass had stopped shrinking as the sea was climbing up the base of the Nullarbor cliffs and no longer swamping the horizontal plains. Aboriginal territories on the eastern Nullarbor were, in a sense, secure from aquatic invasion from about 10,000 BP onwards. Coincidentally, once the territorial contraction caused by the rising seas across the Nullarbor Plain ceased, the process of cultural change seen at Allens Cave also appears to have ceased. While there was an erosional event ca. 6000 BP which removed early Holocene deposits, occupation hearths survive at 5460 BP and 3720±150 BP, at 2800±70 and then at 210±30 BP, so that the record extends from the mid to late Holocene. Exotic stone disappeared from the site and the intensity of artefact discard and occupational events declined. There were no more shellfish, even though Allens Cave was by then closer to the sea than it had ever been before. The decrease in the diversity and density of cultural material after the sea level stalled against the Nullarbor cliffs suggests that whatever motivated the Pleistocene hunters to collect and carry shells, silcrete and tektites to Allens Cave between 14,000 BP and 10,000 BP, failed to maintain its momentum during the Holocene. The Seton Site, Tunnel Cave and Cheetup Cave show the same reduction in occupational intensity and diversity. Occupation effectively terminates at these sites at the close of the Pleistocene and marine materials disappear from the cultural record. (Evidence for human occupation is absent from Devils Lair after 12,000 BP as the cave was blocked by roof fall.)

While the sea level continued to rise elsewhere in southern Australia until 6800 BP (Short 1988), it was contained by the Nullabor cliffs near Allens Cave ca. 10,000 BP. The correlation between the 'geographic' stabilisation of the sea caused by the Nullarbor cliffs and the contraction of cultural materials at Allens Cave after that time suggests that population levels returned to a sustainable equilibrium and subsistence strategies consistent with those of the original terrestrial economy pervaded the migratory coastal culture once the increasing eustatic pressure on the regional landscape was stabilised. The nomadic patterns of desert subsistence seem to have re-established themselves over the arid face of the Nullarbor Plain during the Holocene. The elements of marine culture glimpsed archaeologically at Allens Cave in the terminal Pleistocene, and at the other sites across southwestern Australia, fade from view, even though each of these sites became closer to the sea as the Holocene dawned and the sea moved to its present position between 6000 BP and 7000 BP.

Absence of marine economies

The absence of marine foods in Allens Cave after 10,000 BP is consistent with the nature of Holocene archaeology throughout the region. Investigations in western South Australia conclude that 'sea foods appear to have made no more than a minor, irregular contribution to the subsistence economy' of southern Australian people during the Holocene (Nicholson 1994:124). Coastal sites indicate a selective emphasis on larger shellfish and some scavenging of sea mammals as part of a regional subsistence economy that focussed on a 'wide range of terrestrial resources'. The occupants of the arid coast appear to have been 'a desert culture living in a coastal environment' (Nicholson 1994:124–130).

Only four sites located along a coastline of over 700 km resemble what east coast archaeologists would call 'midden' sites and possibly provide evidence for a marine economy, defined by Bowdler (1977:223) as a 'heavy dependence on marine resources, especially scale fish and shellfish, and little systematic hunting of terrestrial mammals.' Reference here to 'marine subsistence' implies a more general subsistence regime which incorporated marine resources as a significant, but not dominant part of the diet. Three of these middens (at Point Drummond, the Granites and Acraman Creek) date to 6700 BP and 6800 BP and coincide with the final stages of the post-glacial sea level rise, presumably when there was still pressure on the geographic space of the residents of the coastal hinterland. The fourth 'midden' (at Point Brown) is just 700 years old and is associated with more intense settlement near a large quarry (Table 1) (Nicholson 1994).

Sites along the entire western and southern coastline of Australia reflect a subsistence economy reminiscent of that in arid South Australia (Cane 1998). Approximately 1200 sites have been recorded along the 1300 km coastline of southwest Western Australia. Just 60 of these sites contain shell material and less than a dozen contain enough to be called 'middens'. On average, the southwest of Western Australia contains a site with some humanly transported shell every 30 km but only one 'midden' every 100–180 km. There are no middens for almost 300 km in the vicinity of Cape Naturaliste. Interestingly, the majority of 'middens' that are noted in the literature were occupied before, or close to the Holocene sea level stabilisation - at a time when populations were presumably still in a state of flux and when subsistence regimes were still adjusting to the demands of an encroaching sea. Eight of the dozen 'midden' sites date between 8000 BP and 4000 BP (Table 1) (Kendrick and Morse 1982; Dortch et al. 1984; also O'Connor 1990; Smith 1996).

Table 1. Antiquity of middens in southern and southwestern Australia.

Site Name	Date (BP)	Comment
South Australia		
Granites	6900	Clusters of periwinkles
Pt. Drummond	6800	Largest midden site on west SA coast
Acraman Creek	6700	Clusters of periwinkles
Bales Bay	6500	50 shells (on Kangaroo Island)
Point. Brown	670	Mound of periwinkles — up to 300 shells per mound
Western Australia		
Walyunga	8000	Numbers of artefact decline significantly after initial occupation
Moorlinup Crs	7700	Significant decline in artefact numbers
Warroora Stn.	7000	Sea urchins, crabs and fish, site three km. inland
Rainbow Cave	6000	Little marine material
Middle Head	6000	Sparse scatter of marine shells, 300 m. inland
Guilderton Brdg.	5000	Limpet shells and Eocene chert, oldest site in s.w. W.A.
Sandland Ild.	5000	Contains 'thousands' of shells, 300–400m. from beach
Calgardup Brk.	4300	Artefact scatter and some whelks.

Most archaeologists agree that 'shellfish were never a major food source' (Smith 1996:15) in southwestern Australia even though fish, shellfish, sea birds and scavenged mammals were incorporated in the coastal diet. Some archaeologists suggest that the dearth of middens is a real reflection of the unimportance of shellfish in local economies (Hallam 1987; O'Connor 1990: 395–404; Nicholson and Cane 1991:3). Some consider that the people of the southwest practised a 'terrestrial rather than marine or littoral orientation in their past behaviour patterns' (Lilley 1993:40), an observation consistent with the absence of fish nets, fishhooks and water craft throughout the area (Lampert 1981), although fish traps were used in the region (Dortch 1997).

The evidence from Allens Cave suggests that a long process of cultural change, initiated by the post-glacial rise in sea level, led to the absence of marine economic activity on the southwestern coasts of Australia. People first occupying sites such as Allens Cave were 150 km from the coast and had a terrestrial subsistence economy. As the sea began to rise in the late Pleistocene, people with marine traditions migrated into those inland areas, introducing a marine subsistence. These traditions were, in part, incorporated into the existing terrestrial economy as people sought to develop cultural systems which could cope with the environmental changes confronting them, before ultimately being subsumed within the resident terrestrial culture. As the rising seas stabilised the economic traditions attached to the land into which the displaced coastal people migrated pervaded, leaving, in effect, people with a terrestrial subsistence economy living by the sea (see Dortch et al. 1984; O'Connor 1990; White and O'Connell 1982; Nicholson 1991; Nicholson and Cane 1991; Nicholson 1994; and Smith 1996, for other views regarding the causes and nature of Holocene coastal subsistence along the southwestern shores of Australia).

EUSTATIC AND CULTURAL CHANGE

An entirely different situation to that found at Allens Cave and proposed for the southwest coasts of Australia is described at Mandu Mandu Creek, on North West Cape, Western Australia. There, evidence has been reported for subsistence activity 'which included fish and shellfish' at 32,000 BP (Morse 1988:87, 1993, 1996) and two fragments of Baler shell at 30,000 BP and 20,000 BP. Then, as the sea level dropped, the site was abandoned and only re-occupied at 2500 BP. Other sites in the region indicate the resumption of shellfish collection as the sea rose between 11,000 BP and 5000 BP (Table 2) (Morse and Kee 1985; Warren 1993; Morse 1992, 1996; Turner 1985; Randolph and Wallam 1986).

Mandu Mandu Creek is located adjacent to the steepest edge of the Australian continental shelf so that when the Pleistocene sea level was at its lowest the shoreline was only about 10 km from the site — quite the reverse of Allens Cave. Not surprisingly, the archaeological evidence suggests little real difference in the economies of the Pleistocene and Holocene inhabitants of the site (Morse 1988: 87). The regional population appears to have been adapted to, or at least familiar with, maritime traditions from the early Pleistocene. As a consequence coastal subsistence traditions remained unchanged. Sites in the region of Mandu Mandu Creek reveal continuous use of the sea, unlike those in the region of Allens Cave.

Table 2. Holocene dates from Cape Range Peninsula.

Site Name	Site type	Date (BP)
Yardie Creek	rock shelter	10500
Pilgramunna Bay	rock shelter	9900
Warroora	midden	7800
Mandu Mandu Creek	rock shelter	5900
Tulki Well	midden	5600
Coral Bay	midden	5600
Turquoise Bay	midden	5300
Low Point	midden	4800
Mangrove Bay	midden	100

The difference between the cultural experience at Mandu Mandu Creek and Allens Cave suggests there is a correlation between the environmental circumstances in which the post-glacial rise took place and the cultural consequences of that environmental circumstance. More specifically, the distance between each site and its Pleistocene coast seems to have been a determinant in the existence, maintenance or development of cultural traditions. At Allens Cave the distance from the coast was extreme and significant cultural change is seen in the archaeological record. At Mandu Mandu Creek the distance between the site and the sea was small and the pattern of human behaviour seen during the Pleistocene remained unaltered throughout the Holocene. A similar pattern is seen along the north coast of New Ireland where the coastline is similarly steeply contoured. Here a number of sites contain substantial marine middens from 35–40,000 BP onwards, but show periods of abandonment during the height of the last glacial, with shell fishing continuing upon re-occupation (Allen 2000:148–9,152).

The difference in human behaviour in these sites suggests there is a correlation between the breadth of the coastal plains at the last glacial maximum and the nature of cultural change in the post-glacial period. The correlation suggests there is minimal cultural change, at least as far as such change is reflected by the subsistence activities of the inhabitants, at locations where the distance between the present shoreline and the last glacial shoreline was comparatively small. Conversely, there appears to be a measurable degree of cultural change where the lateral extent of eustatic change was significant. In such circumstances the economic activity of displaced coastal people appears to be subsumed within the incumbent terrestrial economy. Such cultural change might be expressed archaeologically as a general absence, or at least the late development, of marine economies in those significantly altered coastal environments.

Beaton (1995:798) has recognised that there is a difference in the nature of subsistence on 'precipitous' compared to 'procumbent' coasts around Australia and suggests the differing environmental characteristics of these coasts may have led to differing expressions of human behaviour. He suggests that 'precipitous coasts maintained a comparatively stable environment' and may have led to 'strong coastal adaptation' whereas 'procumbent zones experienced dynamic environmental changes' and may have led to 'weak coastal adaptation' (Beaton 1995:800-1). The effect, as implied by the different cultural histories of Allens Cave and Mandu Mandu Creek, is that the determinant factor in the expression of 'weak' or 'strong' coastal adaptation is the nature of cultural exchange rather than the nature of environmental change which took place on the coasts as a consequence of sea level rise. The emphasis here is on secondary cultural adaptation, rather than secondary environmental modification, after the primary effect of eustatic change. The correlation between the proximity of the Pleistocene shoreline and the contemporary shoreline of Australia appears to be a causal factor in that regional cultural expression.

Near shores

The 150 m contour, or approximate location of the Pleistocene shoreline during the height of the last glaciation, is about 30 km away from present western Victoria, eastern South Australia and southeastern Tasmania, and generally within 20 km of eastern Australia (between southern and northern New South Wales). In this region one finds the 'strongest coastal adaptations', or most intense evidence of marine economic activity. Marine economies date consistently from the earliest phases of the Holocene and continue uninterrupted, and with increasing intensity (and variability) throughout the Holocene. Antiquities range from over 8000 years in southeast Tasmania, western Victoria and the eastern margin of South Australia, and from ca. 5000 BP along the NSW coast (Tables 3–4) (Reber 1965; Lampert and Megaw 1979; Sullivan and Hughes 1982; Sullivan 1984; Frankel 1986; Brown 1986).

This regional characteristic is thought to be a consequence of the proximity of the Pleistocene and Holocene coasts which encouraged an easy accommodation of the migratory marine subsistence strategy as at Mandu Mandu Creek. The sea was, in a sense, always within walking distance in southeastern Australia,

Table 3. Antiquity of marine economies in southeastern mainland Australia. (Western Victoria, eastern South Australia: Bednarik 1989, Bird and Frankel 1991, Cann et al 1991, Egloff et al.1991, Frankel 1986, Godfrey 1989, Luebbers 1980, Wood 1995. Eastern Victoria: Bell & Clarke 1977, Bell et al. 1977, Coutts et al. 1976, Coutts and Witter 1977, Hall 1991, Hotchin 1986a& b, 1989, Lomax 1996, Schell 1993, Thompson 1985, Webb 1995. New South Wales: Attenbrow 1990, 1992, Attenbrow and Steele 1995, Bowdler 1976, Campbell 1969, 1972, Connah 1972, 1974, 1975, Hughes and Lampert 1982, Lampert 1971a & b, Lampert and Megaw 1979, Starling 1973, Sullivan 1981, Sullivan and Hughes 1982).

Site name	Date (BP)	Site name	Date (BP)
Western Victoria, eastern South Australia		**New South Wales**	
Koongine Cave	9000	*North Coast*	
Mt Burr	8500	Clybucca	5500
Cape Martin	8500	Wombah	3200
Noble's Rocks	8490	Yowie Bay	2600
Noble's Rocks	8390	North Creek	1700
Noble's Rocks	8340	North Head	1500
Sutton Rocks	8200	Watering Place	1330
East Mobong	7960	Gymea Bay	1220
Noble's Rocks	6870	Terranora	650
Thunder Point	6710	*Central Coast*	
Pickering Point	6590	Sydney district	4500
Noble's Rocks	6410	Connection Ck.	4000
Piccaninnie Cave	5700	Curracurrang	4000
Armstrong Bay	5680	Norah Head	1200
Noble's Rocks	5320	Maguires Cross'g.	1000
Armstrong Bay	5120	Birubi Pt.	500
Noble's Rocks	4850	Broughton Island	400
Eastern Victoria		Cumberland St	300
Jack Smith Lake (estuarine shells)	3000	*South Coast*	
Gippsland Lakes	3000	Currarong	4000
Sp. Whale Head	2500	Pambula Lake	3200
Captains Point	2500	Bass Pt.	3000
Dog Island	2400	Burrill Lake	2500
Point Hicks	1400	Boat Harbour	1930
Jack Smith Lake (marine shell)	1000	Cullendulla Creek	800
Gippsland Lakes (pipi)	1000	Murramarang	500
		Durras North	500
		Disaster Bay	500

so the people on the Pleistocene coastal hinterland, in contrast to the situation in southwestern Australia, were familiar with coastal customs and readily accommodated the subsistence economy of the migrating coastal populations as the sea encroached upon them. The migratory processes initiated by the rising seas in southeastern Australia may even have assisted the establishment of marine economic activity as populations were compressed into comparatively 'constrained' hinterland environments. Here the populations of the coastal plain may have become 'compressed' against the steep terrain of the Great Dividing Range as the sea encroached. This range presents a physical barrier that may have put additional pressure on territorial space and available resources which in turn encouraged more intense exploitation of the sea.

When one examines the regional landscape in greater detail it is possible to visualise the migratory drift of people across the sinking Bassian Plain. The movement of coastal people is captured in marine economies in the Furneaux Group from 9000 BP until the islands were abandoned or depopulated (Table 5) (Sim 1989, 1991, 1994, 1994; West and Sim 1995; Anderson et al. 1996). To the west, the presence of a migratory coastal people is encapsulated in a 'thin but dense layer of shellfish' on Hunter Hill ca. 7000 BP (Bowdler 1984a:30). This discrete concentration of cultural material coincides with the appearance of cultural material at Cape de Couedic on Kangaroo Island between 7300 BP and 6800 BP, when the sea was

Table 4. Antiquity of marine subsistence in southeastern Tasmania (Brown 1986, Dunnett 1992, Jennings 1983, Kerrison and Binns 1984, Neal 1981, Reber 1965, Stockton and Wallace 1979).

Site name	Date (BP)
Carlton Bluff	8700, 7500
Apollo Bay	7500
Kelly's Point	6000
Old Beach	5800
Fishers Hill	5800
Porter's Bay	5400
Shag Bay	5300
Dennes Point	5200
Botanical Gardens	5200
Risdon Cove	4900
Barnes Bay	4500
Alum Cliffs	3,800
Jordan River	3,600
South Arm	2,000
Ventenant Pt (2 sites)	2,000 and 230
Cloudy Bay	1,100
Huon Point	1,800
Flat Topped Bluff	1,500
Cape Sorell	1,120

Table 5. Antiquity of marine subsistence in the Furneaux Group, Bass Strait (Anderson et al. 1996, Jones & Lampert 1978, Orchiston & Glennie 1978, Sim 1989,1991,1994, West and Sim 1995).

Site name	Date (BP)
Badger Island	9600
West End North	6700
Boat Harbour South	6200
West End	5900
Cave Beach	5600
Old Man's Head	5100
Palana	4700

approaching its present position (Draper 1987:4) and seems reminiscent of the thin but dense bands of cultural material deposited at the close of the Pleistocene in other sites in south and western Australia (Allens Cave, the Seton Site, Tunnel Cave and Devils Lair).

Distant shores

Unlike the situation on the southeastern side of Tasmania (and the southeastern seaboard of Australia), the coastline across northwestern Tasmania is long and flat. These Pleistocene coastal plains were more reminiscent of those fronting Allens Cave than of those adjacent to Mandu Mandu Creek. Thus the Pleistocene shoreline at 18,000 BP was some 150 km west of West Point, and the original occupants of Hunter Hill at 22,000 BP were terrestrial hunter-gatherers. The rising sea appears to have led to a brief mixing of the terrestrial and marine economies at ca. 7000 BP but it was not until ca. 4000 BP that a genuine marine economy emerged in northwest Tasmania — some 4000 years after southeastern Tasmania (Tables 4 and 6) (Bowdler 1984b, 1995a; Stockton 1981, 1982; Jones 1984; Lourandos 1970, 1983b, 1988; Macfarlane 1993).

The late cultural adaptation to marine economies in northwest Tasmania is similarly reflected in the archaeological record in comparable 'procumbent' coastal environments elsewhere around Australia. Wherever there is a considerable distance between the Pleistocene shoreline and the contemporary Holocene shoreline there is a lag time between the stabilisation of the sea and the emergence of a marine economy. This is apparent on the eastern side of the Bassian Plain (eastern Victoria) where the Pleistocene shoreline was between 85 km and 100 km south of the present East Gippsland coast. In this area there is evidence for the collection of estuarine shellfish ca. 3000 BP- but sites displaying a marine economy only appear from 2500 BP (Hotchin 1986a and b, 1989; Hall 1991; Lomax 1996; Schell 1993: Table 3). Marine coastal sites are rare in Western Port Bay and the marine environment is described as 'incidental' to the economy of the local coastal inhabitants (Gaughwin 1981, 1983).

Table 6: Antiquity of marine economies in northwestern Tasmania (Bowdler 1984b, 1995, Jones 1984, Lourandos 1970, 1983b, 1988, Macfarlane 1993, Stockton 1981, 1982).

Site name	Date (BP)	Comment
Islands		
Cave Bay Cave	22750	lowest hearth
	6600	base of midden
	4000	upper midden
Little Duck Bay	900	upper occupation - rich
Mutton Bird Midden	1000–900	base & top of midden
Rookery Shelter	1600–400	base & top of midden
	4600–870	base & top of deposit
Huts		
West Point		
	1880	base of hut
	1330	top of undisturbed deposit
Sundown Point	modern	hut
Nettley Bay	600	one hut
Little Duck Bay	1000	one hut (also above)
Mainwaring River	740	southwest region
Middens		
West Point		
	2600	south of lighthouse
	1580–550	other middens
Flat Topped Bluff	1500	40 cm deep
Woolnorth Point	3890	base of midden
Arthur River		
	1560	midden
	4050	base of sample
Greenes Point	modern	shallow midden
Nettley Bay	1750-modern	twelve middens dated
Shelters		
Rocky Cape North		
	5425	base of cultural material
	450	near top of unit
Rocky Cave South		
	8120–7465	base of cultural material
	3800	30 cm from surface
Sisters Creek	6050	base of midden

The archaeological representation of marine economic activity along these 'procumbent shorelines' seems to mirror the archaeology of similar shorelines across northern Australia. In southeastern Queensland, human presence has been documented from 18,000 BP (Neal and Stock 1986). At that time the resident population was based on a terrestrial economy and the sea was some 70 km away. Moreton Bay formed ca. 6000 BP but the archaeological evidence for a marine economy belongs only to the last 2500 years. A particularly interesting aspect of the development of the marine economy in this area is that fish bones have been found in sites 40 km inland prior to 6000 BP — implying the movement of marine adapted people inland as the sea level was peaking. A similar situation occurs at a 4000 BP site in the Hervey Range 30 km west from Townsville, where fragments of tidal shellfish have been recovered (Campbell 1978; Brayshaw 1974, 1977; Kelly 1982). Marine foods were also being consumed at coastal sites that are now locked within the prograded estuarine shoreline about 4000 BP, but marine economies did not appear until after 2500 BP (Table 7) (Hall 1982; Hall and Robbins 1984; Morwood 1987; Walters 1992a and b).

A similar lag time in the emergence of marine economies is recorded to the north. Between Noosa and the Keppel Islands human occupation is dated from 4400 BP to 5500 BP but midden material only enters sites in the last 1000 years (McNiven 1985, 1991a and b, 1992, 1993). Permanent marine based settlement of the offshore islands also seems to date from around 1000 BP (Rowland 1980 and 1982).

The best documented time lag is that investigated by Beaton (1985) in Princess Charlotte Bay. Aboriginal occupation of Cape York is demonstrated from the late Pleistocene (Morwood and Trezise 1989) and in the Barrier Reef province from 8000 BP (Barker 1991; Rowland 1996) yet marine exploitation, seen at Bathurst Head, is confined to the last 4700 years (Beaton 1985) — 1,500 years after the sea had stabilised. It was not until 2000 BP that significant amounts of midden material accumulated - indicating a late intensification of coastal occupation in the area. Here, as in the case of eastern Victoria, northwest Tasmania, southeastern Queensland and southwestern Australia, the Pleistocene shoreline was some distance from the present shore. The coastal dwellers of the Pleistocene coast of Princess Charlotte Bay had at least 60 km to translocate inland as the sea consumed their foraging territory. Beaton associates the eventual drift towards marine economic activity with demographic factors after the stabilisation of the sea. The time lag preceding that economic change may be due to the process of cultural readjustment necessary as the migratory coastal culture adapted to and blended with the resident terrestrial culture during the period of eustatic change.

Barker (1991:107) notes that occupation at Nara Inlet, near the Whitsunday Islands, demonstrates continuous exploitation and use of the sea from 8150 BP, stating there is 'no lag time in occupation of the coastal fringe and the use of marine resources' and, implicitly, that there is no negative impact from eustatic change on marine economic activity during the early Holocene. Despite this claim, it is difficult to ascertain what cultural processes are at work in Nara Inlet. Barker notes the presence of 50 g of shell (equivalent to one gastropod) in stratigraphic unit 3 between 8150 BP and 3990 BP, but the stratigraphic section places that shell at the top of the unit - above the date of 3990 BP. Barker notes that 'all the characteristics of intensified marine use' emerge after 3000 years, suggesting that marine economies were not operating for a least 5000 years after the site was first occupied. Beaton considers that Barker's evidence for early marine adaptation is 'unconvincing' (Beaton 1995:804), a view also held by Rowland (1996:195). Recent dating of a midden near Gladstone has further confirmed the emergence of 'focused marine resource exploitation' from an open coastal site in central Queensland some 3500 years ago (Ulm 2001:66).

The correlation between the distant Pleistocene shoreline and the late emergence of marine economic activity continues across northern Australia. The Pleistocene shoreline is generally over 100 km from the present coastal foraging zone and archaeological evidence for marine economic activity belongs to the last few thousand years (Table 7). The shell mounds of Cape York and the Northern Territory appear to have been established from 2500 BP with a major phase of mounding building between 1500 BP and 500 BP (Peterson 1973; Bailey 1977; Meehan 1982, 1983, 1991; White et al. 1990; Paton 1991; Roberts 1991, 1994; Bailey et al. 1994).

One site on Croker Island has been dated at 3500 BP (Mitchell 1994) and sites near Broome have been dated from ca. 3000 BP (Veth et al. 1993; O'Connor and Veth 1993). Middens are rare along much of the Kimberley coast, but this may be due to environmental impacts as much as cultural choices (O'Connor 1989:102, 1990:407—8). In the Pilbara, the evidence varies from recent, unstratified and sparse middens, to stratified mounded sites of some antiquity (Rosenfeld 1975; Strawbridge 1993; Veth et al. 1993; see also Harris 1988; Kendrick 1978; Bradshaw 1995). Both the Pilbara and the Kimberley have more complicated cultural and environmental histories which are relevant to understanding the nature of cultural change at the end of the Pleistocene.

OTHER ENVIRONMENTAL AND CULTURAL CONSIDERATIONS

In the past researchers have attempted to explain regional variability in the Holocene coastal record with a number of competing and complementary environmental and cultural explanations (see Rowland 1999).

Table 7. Antiquity of marine subsistence in northern Australia. #Site reflects 120% increase in marine foods after 2,500 BP, *149 carbon dates have been obtained from sites in Moreton Bay (Queensland: Hall 1982, Hall & Hiscock 1988a & b, Hall & Lilley 1987, Hall & Robbins 1984, Morwood 1987, Satterwaite & Heather 1987, Ulm et al. 1995, Walters 1986, 1989, 1992a & b, Walters et al. 1987. Northern Territory: Bailey 1977, Bailey et al. 1994, Bradshaw 1995, Harris 1988, Kendrick 1978, Meehan 1982, 1983, 1991, Paton 1991, Peterson 1973, Roberts 1991, 1994, White et al. 1990,. Pilbara: Smith 1993, Strawbridge 1993, Veth and OBrien 1986, Veth et al. 1993).

Site name	Date (BP)	Site name	Date (BP)
Queensland		**Northern Territory**	
S.E. Coast		Croaker Is.	3500
Sanctuary Cove#	4000	Macassan Well	2400
Brisbane Airport#	4000	Blyth River	1500
Morton Bay*	2500	Millingimbi Is.	1500
Morton Island	2000	Haycock Reach	1200
Toorbul Point	2000	Garrki	1000
St. Helen Island	1700	Alligator R. mouth	800
Polka Point	760	**Pilbara**	
Blue Lake Creek	500	Noala Cave (base)	27,000
Blue Lake	500	(lens)	8,500 to 7500
Maroochy River	500	Wadjuru Rockpool	6440
Capricorn Coast			6530
Nara Inlet	3500		6840
Curlew Island	3000		8520
Cape Palmerston	2300	Clarke's Cave	1470
Middle Percy Island	2000		7140
Freshwater Bay	1000	Anadara mound	2270
Teewah Beach	900		4270
Beachmount	800		6290
Keppel Island	700		6510
Stockyard Point	640	Anadara shelter	4240
North Coast			6380
Albatross Bay	3500	Nickol River	1780
Bathurst Head	3400		2480
Stanley Island	2400		3280
Naghi Island	2000		4090
Hinchinbrook Island	1700	Not-so-secret Shelter	6080
Pr. Charlotte Bay	1700		
Wellesley Island	1000		
Cooper Point	500		

These regionalised and compartmentalised explanations work at the local level but fail on a continental scale. They do not explain the broader disparities in the archaeological record, such as why marine economic activity was so dominant in temperate southeastern Australia and so unimportant in arid southwestern Australia. Nor do they explain why southeastern Australia has marine economies dating from 6—8000 BP while those of northern Australia belong to the last 3000 years. At a regional level there is no explanation as to why adjacent areas such as southeastern Tasmania and northwestern Tasmania, or northern New South Wales and southeast Queensland, show such widely divergent marine economic traditions and antiquities. In addition the regionality of most analyses leads to unintentional contradictions in interpreted behavioural outcomes: the same environmental explanations are used for differing human responses in different regions. Coastal progradation, for example, is used to explain the presence, absence, modification and reversal in subsistence behaviour on the coasts of southeast Queensland, central Victoria, northern New South Wales and central Queensland respectively (Hall 1982; Hall and Robbins 1984; Gaughwin 1981, 1983; Sullivan and Hughes 1982; Border 1985, 1991, 1994).

The fundamental problem for regional archaeology in coastal Australia is that human responses differ while the archaeological explanations remain essentially the same. The one constant variable in the relationship between the people, their habitat and their economic choices in all these areas is the distance between the Pleistocene coast and the stabilised Holocene coast. The proposition put here is that this variable geographic space had a proportional effect on the variability of human responses and processes of adaptation and subsistence. The proposition is that the greater the lateral extent of eustatic change the more likely it was for the traditions of the incumbent population to dominate those of the displaced population over that period of eustatic change - thus the greater the cultural change.

The acceptability of such a continental explanation for human behaviour is seductive, but nonetheless subject to regional qualification. Sites in the Kimberley and the Pilbara, for example, are described as containing evidence of marine subsistence from the early Holocene (Bradshaw 1995; O'Connor 1999) yet according to the model proposed in this paper both regions feature environmental settings that should see a late or minimal emergence of marine economic activity.

In the case of the Kimberley, the Koolan Shelter contains small amounts of shell when first occupied ca. 27,300 BP. At the time the sea was only a few km from the shelter. The site was then abandoned throughout the glacial maximum and reoccupied at 10,850 BP when the sea had returned to the 'northern face of the range that today forms the north coastline of Koolan' (O'Connor 1989a:102, also 1996, 1999). The site again contains small amounts of marine shell at this time (O'Connor 1989:100; see also Beaton 1995:803). The limited cultural material at the Koolan Shelter is supplemented by evidence of marine subsistence at Widgingarri 2, dating to 7500 BP (O'Connor 1999:61,90, 122). Both sites are interpreted as reflecting generalised hunting and gathering in which marine resources were exploited consistently as part of the subsistence regime throughout the last 10,000 years (O'Connor et al. 1993:98). The exploitation of marine foods at these sites appears more reminiscent of the pattern of subsistence described at Mandu Mandu Creek than at Allens Cave — even though the pattern of eustatic change more closely resembles that seen at Allens Cave.

The situation is similar in the Pilbara. Late Pleistocene midden material is documented in the Monte Bello Islands at ca. 27,000 BP and later there and on the Pilbara coast between 10,000 BP and 6000 BP (Table 7) (Bradshaw 1995; Veth 1993b; Veth 1995). Thus the pattern of cultural change identified for the Kimberley and Pilbara coasts seems to contrast the pattern proposed in this paper. In these areas there appears not to have been a period of cultural change across the transition from the Pleistocene to the Holocene, nor any indication that the coastal subsistence traditions of the regional inhabitants were subsumed within, or significantly altered, by those of the resident terrestrial population throughout the period of post-glacial sea level rise.

An obvious question thus emerges. If the case can be made for a degree of cultural incumbency in resident terrestrial cultures in the face of eustatic change on procumbent coasts elsewhere in Australia, why is that cultural incumbency not seen in the archaeological record of the Kimberley and the Pilbara? If the model of cultural change proposed for the large flat coasts of southwestern, north and northeastern Australia has merit, why is that model not reflected on similar coasts in western and northwestern Australia? One reason for this disparity, given the available evidence for marine subsistence through the Holocene in these areas, might be that the terrestrial environments of the Kimberley and Pilbara districts were not populated during the late Pleistocene and, as a consequence, contained no resident terrestrial cultural tradition that could confront or challenge that of a migrating post-glacial coastal people. Research by O'Connor et al. (1993) proposes that 'population declined' in both areas during the glacial maximum and that after the post-glacial amelioration there was demographic 'expansion into previously abandoned areas' (O'Connor et al. 1993:102–3). If this was the case, then any coastal cultural translocation resulting from post-glacial sea level rise would not have to deal with the cultural traditions of a resident terrestrial population. There would be, in effect, no competing terrestrial cultural domain confronting the displaced coastal people.

The emerging archaeological evidence from the Pilbara also raises another possible cultural process which may have affected the maintenance of marine subsistence traditions in areas where one might have expected land based traditions to dominate. In the Pilbara archaeological evidence indicates that the oldest middens are dominated by mangrove dwelling shellfish (Bradshaw 1995:37). This raises an obvious question. What happened when the sea level rose in areas where there were large river systems populated by people with riverine and estuarine subsistence traditions? How did these areas accommodate the cultural and demographic changes associated with the inundation of the coastal plains at the close of the Pleistocene? The archaeological evidence suggests that such changes were accommodated more easily in these environments, and that marine economies developed earlier and more steadily in situations where the encroaching seas moved coastal people into areas where the resident population already subsisted on riverine and estuarine resources. In such circumstances one has a sense of Bowdler's 'transliteration' in reverse, ie. rather than having a 'coastal economy "transliterated" to a freshwater situation' as proposed for the Willandra Lakes in the early Pleistocene (Bowdler 1977:223), one seems to have a freshwater economy 'transriverined' to a saltwater environment in the Holocene.

This 'estuarine effect' appears to be active on the coastal plains near Kakadu where estuarine shell is found in a rockshelter 15 km from the present day coastline at 7000 BP (White 1967; White and Peterson 1969; Kamminga and Allen 1973; Schrire 1982; Jones 1985; Meehan et al. 1985). Evidence from the East Alligator River suggests that shellfish were supplementary to the subsistence economy on the coastal hinterland during the mid-Holocene (Woodroffe et al. 1988:99, 102), and that there was a shift from a riverine to an estuarine economy as the sea level rose across the transition from the Pleistocene and Holocene (Taçon and Brockwell 1995). The recent establishment of coastal middens is thought to be a product of the 'gradual seaward displacement of the estuarine zones' (Woodroffe et al. 1988:101). There is, in a sense, a continuum from freshwater to saltwater subsistence activity throughout the Holocene as a consequence of eustatic change, rather than a change in tradition as is proposed where coastal people were pushed into a terrestrial cultural environment. Virtually the same scenario is described for estuarine settings in eastern South Australia where the regional culture has much in common with the riverine culture of the Murray (Berndt et al. 1993), and may have evolved from it over the last 10,000 years. Here, like the Alligator Rivers and the Pilbara, the oldest 'coastal' midden sites contain estuarine cockles and are located along inland lagoons (8000 BP: Cann et al. 1991; also Luebbers 1978, 1980 1981, 1982).

A more complicated situation, with a similar cultural outcome, seems to have transpired where eustatic change forced people back into a confined geographic and cultural space. This 'peninsula effect' seems to have occurred where the subsistence traditions of a migrating coastal population were compressed into a relatively limited foraging space. The archaeological evidence suggests that where there were geographic limitations to the movement of people following eustatic change the economic traditions of the displaced population were necessarily incorporated into the economy of the resident population. In areas such as Wilsons Promontory and Rocky Cape, for example, one sees sustained diversification in the subsistence activities of the inhabitants throughout much of the Holocene. At Rocky Cape marine economic activity is recognised at 8000 BP (Table 6), 4,000 years before the rest of northwest Tasmania (Bowdler 1984b, 1995; Stockton 1981, 1982; Jones 1984; Lourandos 1970, 1983b, 1988; Macfarlane 1993). The disjunction in behavioural chronologies has been recognised for some time: 'A great archaeological problem that now faces us is what use if any was made of the coast between c. 6000 BP and 8000 BP when the sea came up to approximately its present level and the period c. 2000 BP when it was utilised in a manner compatible with the ethnographic situation' (Jones 1984:59).

The archaeological record at Wilsons Promontory captures human occupation and marine economic activity 6500 BP compared to 3000 BP in the rest of eastern Victoria (also Great Glennie Island: Coutts 1967a and b, 1970; Hope and Coutts 1971; Coutts et al. 1976; Jones and Allen 1979; Head et al. 1983; Fullagar 1984; van Waarden 1989; Gaughwin and Fullagar 1995; McNiven 1995; Webb 1995). A similar antiquity for marine economic activity is seen from 5200 BP on Phillip Island and on the Nepean and Bellarine Peninsulas (H. Sullivan 1981; Gaughwin 1981; Coutts 1982; Rhodes 1986; Goulding 1988; Craib 1989; Tulloch 1995).

Rocky Cape and Wilsons Promontory are longitudinal equivalents - both central to, but on opposite sides of Bass Strait. Initial occupation of both regions coincides with the arrival of the sea and the appearance of marine economic activity. It seems possible that the Pleistocene marine economies seen across the Furneaux Group at 8000 BP, and captured at 7000 BP in Cave Bay Cave, survived in these peninsula environments, but were subsumed within a more broadly based terrestrial economy in neighbouring, 'procumbent' foraging landscapes until processes of intensification led to their development in the late Holocene (Lourandos 1977, 1980a and b, 1983a, 1996; Bowdler and Lourandos 1982).

CONCLUSION

Between ca. 13,000 BP and 10,000 BP there was a phase of concentrated and diverse cultural activity at Allens Cave on the Nullarbor Plain. For this period Allens Cave contains more artefacts, diverse and exotic raw materials, intense domestic activity and fragments of marine shell. Other late Pleistocene sites across southwestern Australia reveal similar concentrations of diverse cultural materials during the same period. All contain elements of marine fauna, despite the sea being between 30-100 km from these sites at the time of their occupation. This concentration of material is explained as a cultural response to the encroaching sea after the last glaciation. The archaeological record suggests the migration of people in the face of the rising sea which caused a diversification of cultural activity as communities sought to accommodate the combined effects of loss of foraging land, higher local population densities, and the infusion of migratory cultural practices. This cultural diversification then appears to fade from the archaeological record — presumably as the resident terrestrial culture dominated the traditions and customs of the displaced people. Most noticeable in this process of cultural readjustment is the virtual disappearance of marine economies from the archaeological record in this area.

The process of cultural evolution proposed for southwestern Australia is thus in contrast to that documented in other parts of the world. Elsewhere, post-glacial climatic change is associated with a diversification of cultural activities and sees the appearance of ceramics, domestication and agriculture (Hope and Golson 1995; O'Connell and Allen 1995; Jochim 1996; Allen 2000). The cultural contraction exhibited in the southwestern Australia seems at odds with the general evolutionary trend of human society and points to other forms of cultural response in the face of global environmental change at the end of the Pleistocene. Jochim (1996:360; also Trigger 1991) notes that 'when people confront adaptive challenges, they do so carrying considerable cultural baggage and they fashion responses out of the contents, often trying to minimise the changes they must undergo'. This circumstance might fit the experience in southwestern Australia. Here it is possible that the resident population of desert hunter-gatherers had a system of customs and traditions that did not accommodate, and perhaps could not accommodate (because of existing norms of behaviour and established parameters of social control and political power), the cultural changes presented by the movement of the sea into desert areas at the close of the Pleistocene. Thus the cultural processes documented at the end of the Pleistocene at Allens Cave may be consistent with, if not a predictable consequence of, the interaction of environmentally motivated social change in the context of conservative cultural traditions of the kind seen in the Western Desert of Australia today.

Resistance to external cultural change is consistent with Western Desert cultural practice. In Western Desert society human behaviour is underpinned by spiritual rather than economic imperatives: a person's worth is gauged by their willingness to submit to a preordained 'sacred purpose' (Stanner 1965) not by their willingness to act with creativity or innovation. Orthodox Western Desert culture is noted for its 'genius in accommodating the inevitability of constant change... to a pervasive Aboriginal ideology of non change' (Tonkinson 1978:14). In contemporary contexts Western Desert people are described as welcoming change only 'insofar as it would fit their forms of permanence' (Stanner 1966:168). This paper proposes that such cultural permanence overcame the cultural changes associated with the post-glacial sea level rise that are glimpsed in the archaeological record between 13,000 BP and 10,000 BP, when desert adapted hunter-gatherers found themselves living by the sea.

This cultural outcome can be contrasted with the experience of the occupants of North West Cape in Western Australia. Here there is evidence for the exploitation of the sea from the late Pleistocene and throughout the Holocene. There does not appear to have been a cultural diversification between 13,000 BP and 10,000 BP and the subsistence economies glanced in the archaeological record remained basically unaltered across the span of human occupation, despite the rise in sea levels. The reason for this uniformity of behaviour seems to be that the movement of the sea did not encourage any large scale migratory process and consequently saw no significant period of cultural adjustment. At North West Cape the landscape was steeply shelving and the coastline was never more than about 10 km from the present shoreline. Unlike the culture of the Nullarbor Plain and southwestern Australia more generally, the regional culture of North West Cape was always familiar with marine subsistence traditions.

Clearly there are differing human responses in these two areas despite exposure to the same environmental event. The human responses seem to vary according to the scale of environmental change, in a manner that suggests the greater the area over which the post-glacial seas rose, the greater the cultural impacts on the displaced and resident populations. As a general principle one might conclude that the greater the area of inundation, the greater the dislocating effect on human culture and the more likely it was for the migratory culture to be absorbed, following some minor cultural adjustments, into the culture of the resident population. Conversely, the smaller the area of inundation the smaller the cultural change, and the more likely it was for contiguous cultures to evolve without significant cultural change. So, one finds the maintenance of marine economies in south and southeastern Australia where the marine transgression was modest (less than 30 km), but the absence, or late development, of marine economies on the south, west and north coasts, where the marine transgression was significant (between 50 km and 200 km). This general correlative principle appears to hold true across Australia unless there was no resident terrestrial population at the time of eustatic change (as may have been the case in the Kimberley and the Pilbara). In such circumstances the coastal traditions apparent during the Pleistocene continue unchallenged and unchanged into and through the Holocene.

The archaeological evidence in coastal Australia implies an interplay between environmental change, environmental context and cultural characteristics to produce cultural change following the last ice age. In the case of Allens Cave, one sees global environmental change (the rising seas), in a distinctive environmental context (a huge flat arid plain) exhibiting particular cultural characteristics (the existence of an inclusive, conservative terrestrial hunter-gatherer tradition), combining to effect cultural change (the loss of marine based economies) during the Holocene.

Because of the variable cultural and environmental conditions into which the primary environmental event played, the rising sea is a global environmental event which has variable cultural outcomes (see also O'Connell and Allen 1995). Variable cultural conditions might be as simple as whether the resident population (towards which the sea rose) was conducive to change during the Pleistocene–Holocene transition (such as the 'marine adapted' cultures of North West Cape), nonconducive to cultural change (the desert adapted cultures of the Nullarbor Plain) or broadly conducive to cultural change (such as the riverine/estuarine cultures of the Coorong and Alligator Rivers). The environmental conditions might vary from a flat open landscape (as in southern and northern Australia), a steep shelving coast (as in parts of southeastern and northwestern Australia), a large estuarine environment (such as the Coorong), a constrained landscape (east of the Great Dividing Range) or a constrained peninsula (Rocky Cape and Wilsons Promontory).

The notion of large scale environmental change in variable environmental settings and filtered through variable cultural contexts is proposed as a better interpretive mechanism for understanding the nature of human responses to post-glacial environmental change and the nature of variability within the Holocene archaeological record than the current range of compartmentalised taphonomic, environmental, demographic and technological explanations. Examination of the primary influence of post-glacial sea level rise may be a useful foundation from which continental, as distinct from regional variability can be understood in the archaeological record of coastal Australia and may explain the absence of marine economies in southwestern Australia, the lag time between the development of marine economies across much of northern Australia (and in regional areas such an East Gippsland, Western Port Bay and northwest Tasmania), the maintenance of generalised coastal economies in the northwest Australia and the maintenance of marine economies in southeastern Australia.

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Archaeologist and Artist in Antarctica and Tasmania

Betty Meehan
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BEA MADDOCK'S concern with the human spirit has been the foundation of her work. Self-portraits and biblical mythological subjects are reflections of her concern with humanity in the broadest level. Since the 1980s her series of paintings, prints and illustrated books have as their subject the landscape, language and loss of the Aborigines of Tasmania. These highly original works are akin to poetry (Australian National Gallery 1999).

Rhys Jones' ideas in the form of conversations and publications have influenced several of Bea Maddock's works. Both share a deep interest in Tasmania and saw significant connections with Heard Island and Antarctica when they visited these remote locations in 1987:

> On 9 January 1987, the ice-strengthened ship *Icebird* left Hobart on the sixth Australian Antarctic resupply voyage of the 1986–87 summer season. Over the following six weeks the ship travelled across the Southern Ocean to subantarctic Heard Island and passed through pack ice to the Antarctic continent, visiting a number of stations and summer bases before returning to Hobart. Among *Icebird's* 68 passengers, which included scientists, tradesmen and other expeditioners, were the Australian artists Bea Maddock, Jan Senbergs and John Caldwell (Caldwell, Maddock and Senbergs 1988:4).

Rhys Jones was one of the scientists on that voyage.

We live in the meanings we are able to discern

This work consists of seven panels (see Kirker and Butler 1991:104–5 for the complete work). Two details are reproduced here (Figure 1).

Bea Maddock (1996:1–2) speaks of her experience in Antarctica in the following way:

> The impetus to work from the landscape after a gap of some thirty years, came from the voyage made to Antarctica in 1987. Having been invited under the auspices of the Artists in Antarctica Program, I had little idea what I would encounter or what work would emerge from the voyage. After sustaining a broken leg on my first step ashore on remote Heard Island I was compelled, from then on, to view the landscape from a fixed position, either on shore as I did from Atlas Cove, Heard Island or from the deck of the ship, or later from a mirror attached to a port window in the sick bay. In this way I was able to trace in a lineal fashion the panoramas before me.
>
> ...From the pages of many sketch books I later constructed the Antarctic Suite and the seven-panelled work, ***We live in the meanings we are able to discern***. Travelling on the ship to Antarctica was Dr Rhys Jones of the Australian National University. In a lecture aboard the ship he talked about Tasmania's glacial past in relation to the Aborigines. He likened Heard Island to the situation in

Figure 1
Bea Maddock, Australia, born 1934.
We live in the meanings we are able to discern (detail).
1987 pigment wash, charcoal, encaustic on canvas and cibachrome 115.2 × 527.1 cm (overall). Gift from the ANZ group 1991. Collection: National Gallery of Australia, Canberra. Reproduced with the permission of the National Gallery of Australia.

> South West Tasmania at the time of the last Ice Age, when Kuti Kina Cave was occupied. So a link with the prehistoric landscape of my home state was fixed firmly in my mind with the actual landscape of Heard Island ... .

And from Kirker and Butler (1991:105):

> ... Rhys Jones had a lot to do with that picture, because he used to talk a bit about how Heard Island today must be like Tasmania was in the last Ice Age, that was his reason for going to Heard Island.

Tromemanner - forgive us our trepass

This painting by Bea Maddock consists of four panels (see Kirker and Butler 1991:111–13 for an image of the work):

> ... In order to begin work on the panels for the painting that was to be exhibited in Queensland the following year, I stayed in the midlands of Tasmania, making journeys over the routes likely to have been taken by the Aborigines in their seasonal treks to and from the East Coast. The Salt-pan Plains at Tunbridge are in an area where three tribal boundaries met and where there was apparently much coming and going. I researched early written accounts of these movements and did much reading relating to the types of artefacts made in Tasmania and their use (Maddock 1996:3).

Artefacts from Tromemanner (Figure 2)

According to Maddock, some of this research about stone tools came from Jones (1977):

> And...I kept a little sketchbook going as I traced them around, and I worked from that original sketchbook to do the monoprint colouring. And I did all this analyzing of what sort of stones they

were, which was nice. Most of them are actually three or four purpose, and again Rhys Jones comes in because he was the one who set out the terminology for describing them...this one is for paring down — it's got a concave edge, and then the sharp end, you have a sort of plane, just a steep edge which again was used for making spears — and then we have a blade that was used for cutting flesh and so on. I mean they had various functions, so that's what the writing was (Kirker and Butler 1991:112).

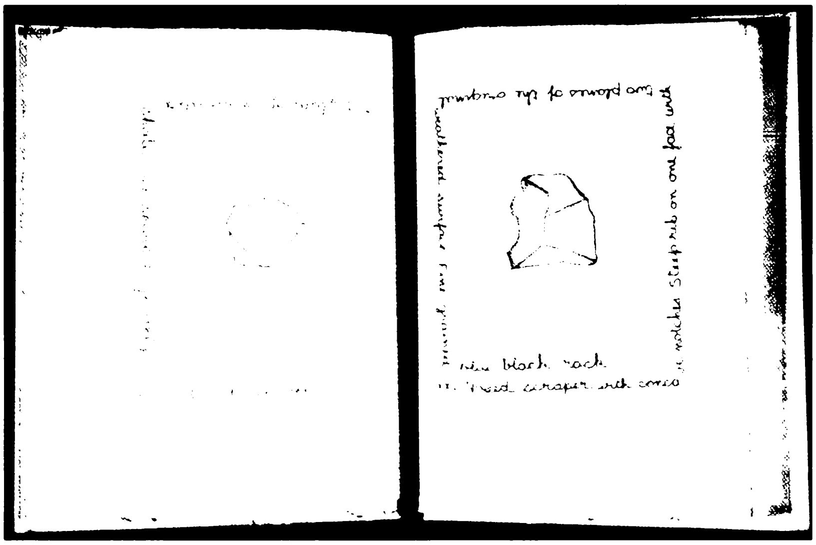

Figure 2
Bea Maddock, Australia, born 1934.
Artefacts from Tromemanner.
1990, artist's book of twenty eight pages; etching, engraving, colour monotype and letterpress 14.8 × 12.4; cover 24.6 × 21.4 × 1.4 cm. Edition 5/25
ANG Gordon Darling Fund 1991. Collection: National Gallery of Australia, Canberra. Reproduced with the permission of the National Gallery of Australia.

Terra Spiritus ... with a darker shade of pale

This work is now owned by the National Gallery of Australia in Canberra, and was on display during September-October 1999.

Terra Spiritus is the culmination of five years of preparation and work by Bea Maddock. The work consists of 51 panels extending some 39 metres from beginning to end. The medium is ochre over blind letterpress and hand drawn script:

> The last of the series of works reflecting on the ancestral lands of the Tasmanian Aborigines is titled *Terra Spiritus* and the work on this circumlittoral, editioned drawing of the entire coastline of Tasmania is still in progress. The initial idea was voiced to a fellow passenger on board the *Icebird* as as we sailed up the Derwent Estuary on the final day of the voyage from Antarctica. I had just spent several hours drawing the Southern Coastline as the ship approached Tasmania from the south and ran a course within sight of land from South West Cape to South East Cape before rounding Bruny Island. I talked to Rhys after I completed the drawing of the South Coast of Tasmania. He told me how Lesueur and Petit of the Baudin Expedition had done similar drawings, and promised to send me copies, which he did.
>
> ... At this stage I had decided to used red ochre as a colouring agent and means for reproducing the lineal drawing. ...The initial idea involved the use of white for the land forms. White according to Dr Rhys Jones writing in *Baudin in Australian Waters* (1988) was significant to the Aborigines in representing the spirit world (Maddock 1996:3–4).

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Climate before Agriculture

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The Australian National University

When gods alike and mortals rose to birth,
A golden race th' immortals form'd on earth
Of many-languaged men: they lived of old
When Saturn reign'd in heaven, an age of gold.
Like gods they lived, with calm untroubled mind;
Free from the toils and anguish of our kind ....
Theirs was each good; the life-sustaining soil
Yielded its copious fruits, unbribed by toil:
They with abundant good midst quiet lands
All willing shared the gatherings of their hands.
Hesiod, 8th century BC (quoted in full by Harlan 1975:3).

PROLOGUE

In reviewing the results of joint archaeological and palaeoenvironmental research in Kakadu National Park, Rhys Jones (1985) drew attention to a local expansion of human population, about 1400 BP, that apparently coincided with an ecological transformation of the South Alligator River plains from salty mangrove mud-flats to lush, freshwater wetlands. For Jones, this was an example of the way in which populations are dynamically dependent on their local habitat and its changes, a theme to which he has several times returned in the course of his writings in the larger theatre of Australian prehistory. Furthermore, Jones noted that this local expansion at Kakadu illustrates the point that prehistoric populations were not simply in equilibrium with the broad-scale 'carrying capacity' determined basically by rainfall, as suggested by Birdsell (1953), but also were subject to environmental changes. This is an important point, because human habitats fluctuated and changed throughout human prehistory. On time scales of years to decades, seasonally-repeating patterns of climate and resources were interrupted by savage events - floods, cyclones and droughts. On time scales of thousands of years, environments changed as a result of the advance and retreat of ice ages and sea level. Intermediate between these ends of the spectrum lie changes of climate or environment that took place over a number of human generations, such as those in Kakadu discussed by Jones, or the historical 'Little Ice Age' in Europe, or the sudden re-advance of ice in the Younger Dryas episode, about 13,000 BP.

Ethnographic studies ranging from the subarctic to the tropics suggest that both the magnitude and frequency of environmental disturbances have affected the ways of life of prehistoric humans, firstly through effects upon ecological diversity of their habitats, and secondly by affecting the behaviour of the peoples themselves. As regards habitats, the diversity of species and the variety of reproductive strategies tends to be highest at intermediate frequencies of ecosystem disturbance — at least, for rainforests and coral reefs, and probably for other environments dominated by relatively long-lived plants and animals (Connell 1978). It also seems likely that human subsistence strategies are broadened by environmental disturbances; illustrated specifically, for example, by Meehan's (1982:162–3) account of alternative food sources used by the Anbarra hunter-gatherers, following disruption of major shellfish sources by storms, or more generally by a variety of human adaptations to unstable sub-polar environments. Accounts of the latter indicate that all available resources are utilised at different times and that modes of subsistence change with environmental fluctuations (Laughlin 1963; Binford 1980; Krupnik 1990). In summary, responses depend on the nature and frequency of disturbance or change. The responses of plants and animals to inter-annual climatic variability are adaptive and evolutionary, whereas at ice age scales the principal response is to migrate. Human responses seem to be similar: adaptation to inter-annual fluctuations but migration in the face of a deepening or retreating ice age.

I turn now to agriculture and the climates of the Pleistocene. Agriculture commonly is seen as the child of the Holocene, when climates became milder than the preceding ice age (O'Connell and Allen 1995); however, the timing of its onset, the number of ways in which it developed and the number of theories to account for it are so various that the role of climate change *per se* often is obscure or seems to be of minor importance. Concerning the factors that led to agriculture, Joseph Harlan (1975:57) wrote some years ago: 'I am inclined to develop a no-model which leaves room for whole arrays of motives, actions, practices and evolutionary processes ... . A search for a single overriding cause for human behaviour is likely to be frustrating and fruitless'. Although the situation does not seem to have altered, there remains the tendency to implicate the shift from Pleistocene to Holocene climatic regimes. It is well known that in terms of averages, for example, the climate at the peak of the last ice age was very different from that of today: great ice sheets covered North America from about the 45th parallel to the Arctic Ocean and another sheet covered Fennoscandia; temperatures in the southeast Australian inlands were 7°–9°C. colder; the howling deserts of today were even more howling, and so on.

When changes across the Pleistocene-Holocene divide are invoked as determining the context for agriculture, comparisons should not be made simply in terms of climatic averages, because if averages were all that matter then the climate and presumably the ecosystems of almost any part of today's world could be found *somewhere* in the Pleistocene world, even on the same major land-masses as they are found today. However, the fossil record of the Pleistocene reveals plant assemblages that lack exact counterparts today (Overpeck et al. 1985), which suggests that factors other than climatic averages influenced community composition. As it happens, climatic differences of the last glacial period went beyond expansion of the ice itself. Firstly, atmospheric carbon dioxide was as much as 25% to 35% lower than in the Holocene (Barnola et al. 1991) and, in turn, plant growth may have been reduced globally (Street-Perrott et al. 1997), although the extent of the effect is debated and only the threshold at which major ecosystems became water-limited may have been raised. Secondly, almost every millennium of the late Pleistocene was punctuated by large and very rapid shifts of climate, at least throughout large regions of the northern hemisphere. These fluctuations were very much faster than those explained by the Milankovitch theory of climatic change, which invokes varying seasonal receipts of solar radiation as a trigger for icesheet growth and decay on time scales of about 20,000 to 100,000 years, and now are known to have been a major feature of climatic behaviour in the Pleistocene.

This paper later considers the possible effects of climatic fluctuations at millennial and shorter scales on ecosystems and human behaviour, but first the nature of pre-Holocene climates and their variability are reviewed.

THE SPECTRUM OF CLIMATIC CHANGE AND VARIABILITY

Throughout much of the mid-latitude world, the climatic difference between winter and summer is about as great as the mean difference between the ice age and the Holocene. The same also can be said of monsoonal regions, where annual differences between the wet and dry seasons are as great as the differences between Holocene savannahs and ice age aridity. For humans and the biota, it is the duration of climatic condition that is important: as remarked earlier, the responses to seasonality have been adaptive and evolutionary but the dominant response to the ice ages was migration. However, within the spectrum of variation that ranges from the seasons to the ice ages, lie other climatic disturbances, ranging from decadal and sub-decadal events such as El Niño and its global concatenations, through to millennial-scale 'megaflickers' found in cores from the Greenland ice sheet and other high-resolution records of prehistoric climate.

Millennial megaflickers

To offer an overview in advance of the details, conditions during the Holocene (the last 10,000 years) were unusually constant - in contrast with to-and-fro climatic battering in the Late Pleistocene. Although shifts of weather patterns did occur in the Holocene, such as the Little Ice Age and the Medieval Warm Period when mountain glaciers modestly advanced or retreated (Lamb 1977; Grove 1988; Crowley 2000), there were no fluctuations to compare with those of the last glacial age cycle. The late Pleistocene situation was radically different, not only because the world was in the throes of an ice age but also because the climate in some regions fluctuated wildy, every few millennia.

To illustrate the advances in knowledge of the pattern and tempo of Late Pleistocene climatic changes over the last decade, Fig. 1 compares the SPECMAP marine oxygen isotope record developed in the 1980s (Martinson et al. 1986) with the oxygen isotope record from the Greenland GISP2 icecore (Hammer et al. 1997; Stuiver and Grootes 2000). The ice-core record shows repeated rapid fluctuations, not visible in the SPECMAP record, that are nearly as large as the switch from glacial to inter-glacial conditions. Before taking the comparison further, the paleoclimatic signals in these records should be explained. The main signal in the marine isotope SPECMAP curve represents changes of the total volume of continental ice sheets, whereas oxygen isotopes in GISP2 largely reflect atmospheric temperatures above the Greenland ice sheet. Differences between the two can be expected as it is almost inevitable that the retreat of a great ice sheet lags behind the climate change that induces its decay, owing to its

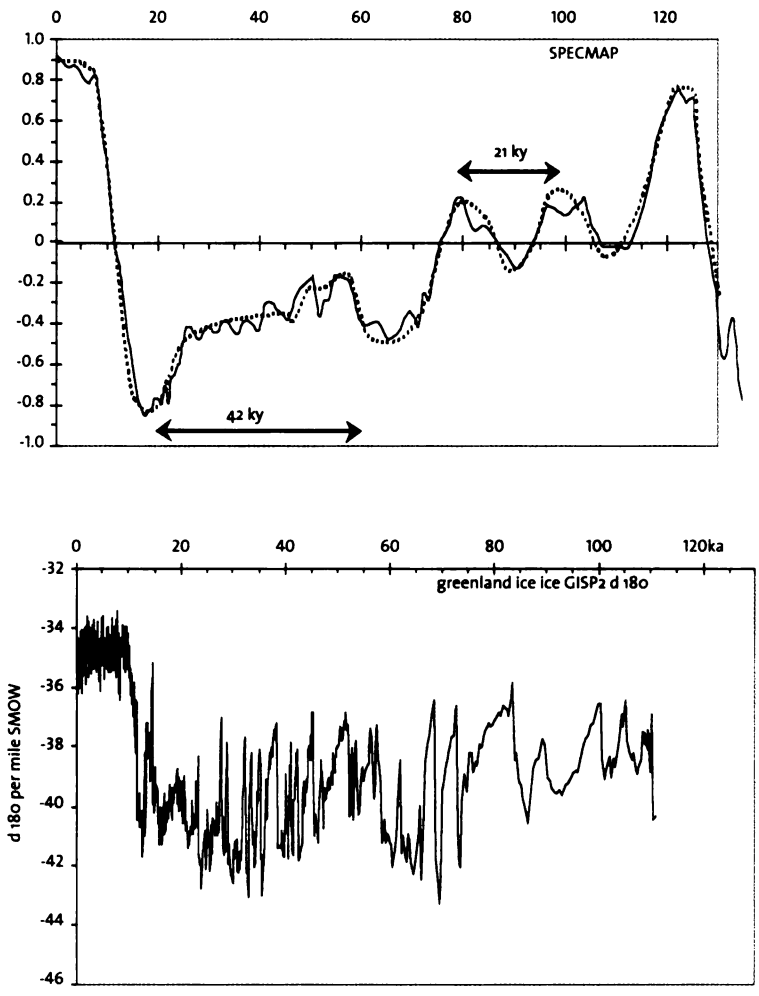


Figure 1. (a) The last glacial cycle: the view in terms of ice-volume (SPECMAP stacked marine-core oxygen isotope records: Martinson et al. 1987). Major changes of global ice volume and sea level within the 100,000 year glacial cycle were dominated by 'Milankovitch' oscillatory periods of ~20,000 and 40,000 year, although there is a hint of higher-frequency fluctuations. (b) The last glacial cycle: a subarctic climatic view (GISP2 icecore: Hammer et al. 1997). Oxygen isotope ratios in ice cores from Greenland reflect snowfall temperatures and show that the subarctic climate switched abruptly every thousand years or so, through the later part of the last glacial cycle.

massive thermal inertia. However, millennial-scale fluctuations comparable with those in Greenland ice cores in the last ~80,000 years have been detected rather widely in the northern hemisphere, including north Atlantic and Mediterranean sea surface temperatures determined from isotopic and foraminiferal data (Bond et al. 1993; Cacho et al. 1999; Shackleton et al. in press), and in high-resolution pollen records from Lago Grande, central Italy, which show repeated shifts of vegetation (Allen et al. 1999). Fluctuations in ocean circulation off the California coast also correlate very closely with Greenland temperatures (Cannariato and Kennett 1999), and particle size variations in Chinese loess sequences reveal comparable fluctuations in atmospheric circulation over central Asia (Porter and An 1995).

Overall, the records suggest that in temperate and colder northern latitudes, at least, large switches of climate occurred repeatedly from ca. 90,000 BP to 11,000 BP. These were rapid changes: recent analysis of the Greenland ice-cores shows that, on average, warmer intervals lasted about 700 years (although with significant variation) and that the switches from cold to warmer conditions typically happened in less than 50 years while the warm-cold transitions took about 80 years (Stuiver and Grootes 2000). These rates are more rapid than forecast for greenhouse climate change in the 21st century (Crowley 2000) and are much more rapid than the post-glacial retreat of continental ice sheets or sea level rise.

European archaeological records contain traces of similar climatic fluctuations. Well before the publication of the Greenland ice-core records, the imprint of repeated climatic changes was recognised in the sediments and pollens of rockshelter deposits in the Perigord. Climatic reconstructions indicated repeated cycles, each of a few thousand years or less, nested within the last two ice ages (originally referred to the Würm and Riss glacial periods). A scheme developed by Laville et al. (1980), for example, organised the sedimentary series previously described by a number of authors from more than 15 rockshelters, each with artefacts of successive palaeolithic industries and many supported by pollen diagrams, into multiple climatic cycles within each phase of the Riss and Würm glaciations. Climatic inferences drawn from pollen and from sediments did not always agree, as, for example, in layer 5 at the Abris Pataud where pollen indicates a cold climate (Donner 1975), while a relatively warm, humid climate was inferred from the sediments (Farrand, 1975). As shown shortly, the correlations and chronology proposed by Laville et al (1980) have not been entirely supported by subsequent dating; nonetheless, the inferred climatic patterns bear an intriguing resemblance to those detected in the Greenland ice cores and other records cited above.

Precise chronologies based on modern dating methods do not appear to have been established throughout all the rockshelter sequences, but the tempo of climatic oscillation has been established in several cases. For example, layers D to H at Peche de l'Azé, inferred to indicate seven or eight climatic cycles (Laville et al. 1980) were deposited between 90,000 BP and 65,000 BP, according to thermoluminescence dating of burnt flints and electron spin resonance dating of mammalian teeth, while zones G to J at Le Moustier, where Laville et al. (1980) recognise three sharp climatic oscillations, were shown by similar dating methods to have accumulated between ca. 38,000 BP and 45-50,000 BP (see review by Grün and Stringer 1991). Radiocarbon dating indicates that beds 2 to 10 at Abris Pataud, embracing 3 or 4 climatic cycles, accumulated between 22,000 BP and 32,000 BP (Movius 1975). Although dating uncertainties at present are such that the inferred climatic oscillations cannot uniquely be correlated with the Greenland or Monticchio sequences, there are strong resemblances in terms of amplitude and frequency of change, as illustrated in Fig. 2. With further advances in dating, relationships undoubtably will become better defined.

Turning from the northern realms, it is not yet clear whether the rapid climatic oscillations illustrated in Fig. 1 occurred in the southern hemisphere. The most recent event — the Younger Dryas cold episode and glacial re-advance at 11,200–13,000 BP — is a case in point. Although sharply defined in Europe and North America, from glacial, pollen, ice-core and marine sedimentary evidence (Wright 1989) and identifiable in China (An et al. 1993) and in marine cores from the Sulu Sea (Linsley and Thunell 1990), there are few southern hemisphere sites where a simultaneous cooling has been clearly established. A glacial re-advance in southwestern New

Zealand apparently coincided with the Younger Dryas in Europe, according to close ^{14}C dating of prominent moraine deposits west of the Franz Josef Glacier (Denton and Hendy 1994) and simultaneous cooling in South America is suggested by pollen evidence, although the interpretation in open to question (Markgraf 1993). The Younger Dryas and several of the earlier oscillations are represented in an ice core from the Taylor dome, Antarctica (Grootes et al. in press) but the Antarctic and Greenland records are far from identical. In summary, at present the southern hemisphere footprint of the northern hemisphere millennial-scale oscillations is unclear. Although it has been suggested that climatic shifts in both hemispheres might have been be linked by deep ocean circulation (Broecker et al. 1999), it has yet to be proved that the climates of both hemispheres danced in a millennial-scale lockstep.

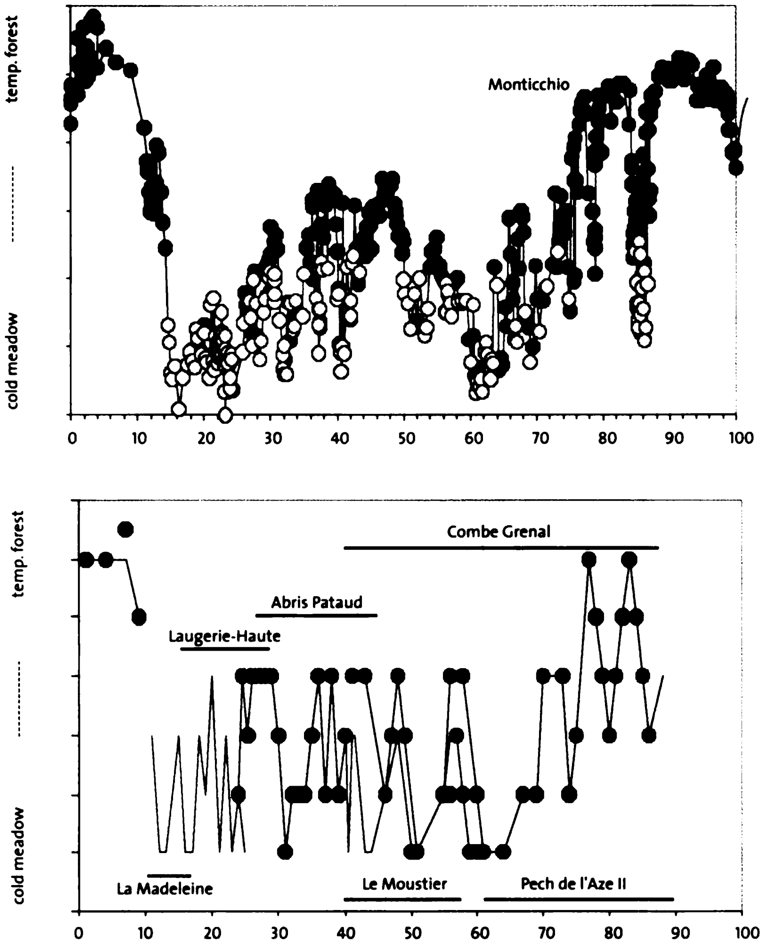


Figure 2. (a) Vegetation changes in central Italy during the last glacial cycle (90 m core from Lago Grande, Monticchio: Allen et al. 1999). Rapid alternations of vegetation communities (biomes) resemble abrupt fluctuations in the Greenland ice core (Fig.1b). (b) Rapid climatic changes in the Perigord, France, determined from rockshelter sediments and pollen (compiled from sources cited in the text and sources cited by Laville et al. 1980), resemble those at Monticchio although correlations suggested here are tentative. Dark gray spots = forest assemblages; pale gray/white = cold meadow/steppe vegetation.

Inter-annual to decadal disturbances

In most parts of the world it is rare that successive years are climatically identical. Events such as droughts, floods, or exceptionally cold winters are likely to occur a few times in 10–20 years, and hurricanes or tropical cyclones may devastate a coastal region once or twice in a lifetime. Climatic variability and disturbances at these frequencies are well determined from meterologic records and have been teased out from historical archives of the last several centuries (e.g. Lamb 1977; Quinn 1993). Probably the best-known quasi-periodic disturbances are those associated with the El Niño, which is heralded by the onset of a weak warm ocean current off northern Peru and a breakdown of the normal circulation from the relatively cool east equatorial Pacific to the warmer west Pacific. El Niño can occur at intervals from a few to more than ten years and is associated with droughts or floods in many tropical and subtropical regions (Quinn et al. 1987; Allan et al. 1996). The pattern varies owing to interactions with other fluctuations in the world's climatic system, such as the North Atlantic Oscillation described by van Loon and Rogers (1978). The statistical correlation is high between El Niño and droughts in Australia, India, Ethiopia and Java, for example, but patterns differ between individual cases, as illustrated by the 1997–1998 event, which was accompanied by severe droughts in New Guinea and Southeast Asia while the South Asian monsoon was barely affected (Grove and Chappell 2000).

The processes such as El Niño, the North Atlantic Oscillation and other fluctuations in the global climate system, not to mention seemingly random occurrences of tropical cyclones and large volcanic eruptions (which can significantly affect the weather: Lamb 1977; Crowley 2000), produce complex climatic fluctuations on times scales from a few years to many decades. To illustrate, Fig. 3 shows annual rainfalls in

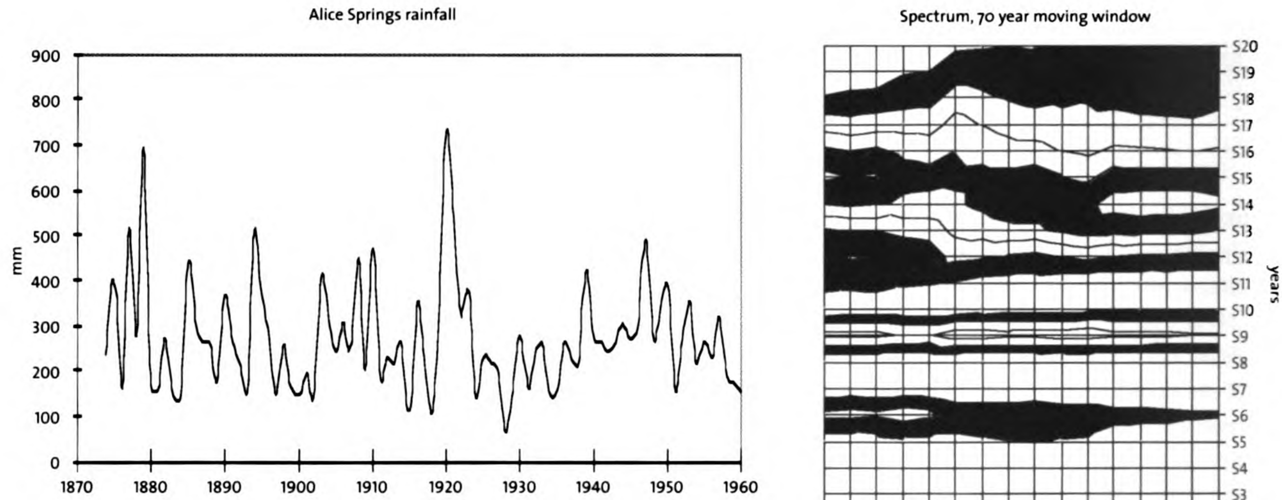


Figure 3. Interannual and decadal climatic variation: Alice Springs rainfall. Left: Annual rainfall 1875–1960 shows considerable variation, from as low as 60 mm to over 700 mm. Right: Spectrum of rainfall variability seen through a moving 70 year sampling window shows several different periodicities, which change through time (dark bands represent dominant periods in years).

Alice Springs, central Australia, for the last 120 years, together with a spectrum of variability. Spectrum peaks at 5–6 years, 9–12 years and 14–20 years indicate that there were recurrent patterns of rainfall at about these periods. However, the spectrum in Fig. 3 is but one way of characterising variability, and there are many other indices that vary in definition or meaning from one climatic regime to another; for example, 15 days without rain constitutes a drought in England and spells of 30+ days without rain may occur only once or twice a century (Manley 1970:119), whereas in semi-monsoonal Katherine, Northern Territory (where the mean annual rainfall is similar to that of England), spells of 100+ days with almost no rain occur annually and a severe drought is several years with little rain (Gentilli 1971). In this case the definition adjusts to the climatic and cultural differences between cool, temperate, populous England and the tropical savannahs of Katherine. For the moment, we pursue definitions no further and let it rest thus: between the time scales of annual seasons and the ice ages, there exist a range of climatic perturbations — some more abrupt than others — that have their own regional characteristics.

Finally, we come to the issue of whether the frequency and intensity of these intermediate-frequency fluctuations were different during late Quaternary periods of different global climate. It may be expected that a steeper meridional temperature gradient during the ice ages led to greater mid-latitude climatic instability, but that lower sea surface temperatures led to fewer tropical cyclones. Confirmatory detail from actual records is meagre, however. For the last few hundred years, tree-ring records from Java and subtropical north America suggest that the Southern Oscillation (a measure of El Niño) has intensified since waning of the Little Ice Age towards the end of the 19th century, but historical records show otherwise: El Niño-related droughts were as frequent, widespread and just as intense in the 18th and 19th centuries as in the 20th (Grove and Chappell 2000). Earlier in the Holocene, when the climate was somewhat warmer and wetter than today in many regions, El Niño and its climatic accomplices may have been less active, according to evidence from lake sediments in Ecuador and annual growth bands in corals from the Great Barrier Reef (Rodbell et al. 1999; Gagan et al. 1998). Before then, in late Pleistocene times, isotopic series for a number of intervals within the last 120,000 years, derived from corals from Huon Peninsula, Papua New Guinea, indicate that inter-annual to decadal scale variability in the late Pleistocene was similar to that of today (Tudhope et al. 2001). A similar result is shown by rate-of-change analysis of high-resolution pollen data, which suggest that lowland ecosystems in southern Chile were subjected to more frequent disurbances in late glacial times than in the early Holocene (Haberle 2000). In short, pollen data together with the results from corals and other sources, cited above, suggest that intermediate disturbances may have been a little less frequent in many regions in early to mid-Holocene times than before or since.

SUBSISTENCE UNDER FREQUENT DISTURBANCE

We now return to the question of whether human subsistence as well as ecosystem diversity was markedly affected by the nature and frequency of disturbance or change. The Holocene probably was the first interval of prolonged climatic stability experienced by modern humans, considering that migrations from their core region(s?) of origin mostly took place since the last Interglacial — i.e. within the last 100,000 years or so. (Note: usage here of the term 'modern humans' follows Stringer and McKie 1996.) Regarding the effects of climatic disturbances and millennial-scale changes, evidence from western Europe seems to offer more detail than do Pleistocene archaeological sites in Australia or elsewhere in the southern hemisphere. While there are hints of climatic shifts in detailed archaeological records from the Australian late Pleistocene, such as those from southwest Tasmania described by Cosgrove and Allen (this volume), the imprint of millennial-scale climatic oscillations has so far only been recognised in records from northern hemisphere sites, both sedimentary and archaeological. Pollen and faunal data from the Perigord rockshelters, in particular, indicate that the biota responded at least as much to millennial-scale climatic changes, as to the slower advances and retreats of the northern European ice sheet.

Fluctuations of flora and fauna

The Perigord sites generally lie within sheltered valleys, particularly those of the Vézere and the Dordogne with their high limestone cliffs, where the climate even at the glacial maximum was relatively mild and has been likened to that of the Massif Central of France today (Wilson 1975). Pollen diagrams from the cave deposits provide windows within the last 100,000 years, which (as noted earlier) reveal sharp, millennial-scale climatic changes, similar to those recorded in the Lago Grande sedimentary record from central Italy. Together with other records from western Europe, they also show that the footprint of millennial-scale climatic change varied from place to place. Pollen spectra of both the rockshelter and Lago Grande records are dominated by mixed temperate forest types during the earlier warm episodes (75,000–95,000 BP) but, whereas Lago Grande is dominated by steppe biomes during cool episodes later in the glacial cycle, forest elements are preserved in valleys of the Perigord. The Abris Pataud and Combe Grenal are contrasting cases in point: in the former, Donner (1975) found trees including oak present in all beds except level 5 (ca. 26–27,000 BP) throughout the descent towards the glacial maximum, whereas forest pollen become sparse at Combe Grenal significantly earlier (Paquereau 1975). Collectively, the evidence from different sites suggests that millennial-scale shifts of climate induced a changing mosaic of flora and fauna across the region, varying with topography, exposure, and soil properties but retaining mixed forest elements in protected valleys of the Dordogne, rather than migrations of vegetational zones on a continental scale (Donner 1975).

Comparable patterns of animal movements are revealed in the archaeological record of hunted prey. Once again turning to the upper series at Abris Pataud for illustration (beds 2–5; ca. 22-28,000 BP) Bouchud (1975) shows that while local reindeer (which differed in diet from the modern tundra-dwelling form) comprise the dominant element, the proportions of horses plus aurochs and bison, on one hand, alternate sharply with red and roe deer, on the other. Together with birds, small animals and montane elements such as ibex, fluctuating proportions amongst the faunal elements indicate significant switches of climate, although not exactly as had been deduced from the sedimentology by Farrand (1975): as Bouchud (1975:148) remarks, 'pour des raisons purement methodologique, les données du géologue ne recoupent pas toujours celles du paléontologiste.'

Extended disturbances

The very brief account, above, falls far short of being a review of late Pleistocene fluctuations in the Perigord but it does serve to illustrate that communities of animals, trees, shrubs, herbs and grasses fluctuated and regrouped in the valleys, meadows, crags and plateaux of the region, in response to Late Pleistocene millennial-scale climatic changes. The pattern was one of repeated, oscillatory change, where every so often the vegetation was reduced to scrubs and cold meadows, punctuated by copses of trees such as birch, oak and pine in the most sheltered places — only to be followed by onset of a new warm phase and renewed woodland expansion.

Such fluctuations resemble intermediate disturbance writ large: episodic taking-over or invasion of one vegetation assemblage by another, on the landscape scale. Of course, these patterns differ in both temporal and spatial terms from those 'intermediate disturbances' argued by Connell (1978) to have a major effect on species diversity in rain forests and coral reefs, such that the greatest diversity tends to be found where disturbances occur at intervals approaching the life-span of the space-filling organisms (i.e. trees, in the case of forests). In the temporal sense, the frequency of millennial-scale events is low, although each climatic shift appears to have been very rapid. Spatially, compared with forest clearings caused by storms, the scale is much larger. In the craggy Perigord, the distance between forest (or woodland) margins in their expanded and their contracted phases is similar to the scale of plateaux and valleys — but was probably much greater in the broad rolling country further north, for example. Thus, the effects of repeated competitive invasions across comparatively large spaces, although relatively infrequent, may not have been so different from Connell's 'intermediate' disturbances. In this light, rapid and radical climatic oscillation during the ice ages did not necessarily reduce species diversity; indeed, by sustaining inter-species competition, genetic variation within species if not diversity as a whole may have increased.

Inhibition of agriculture

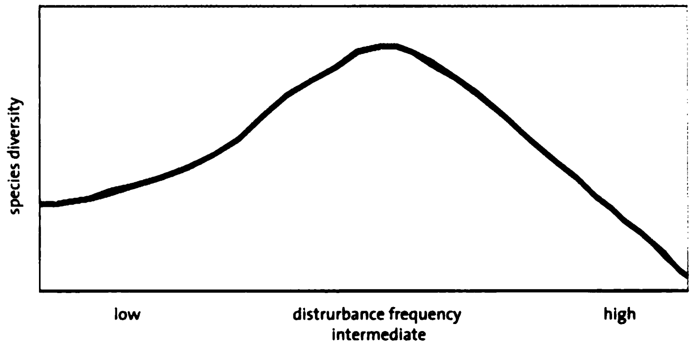
We now move briefly to the ethnographic present, in Australia. As Yen (1989) observes, Australian Aboriginal culture has been widely regarded as representing the hunter-gatherer estate from which agriculture emerged in earlier Holocene times. Interestingly, for many writers the Aborigines seemed to be on the brink of becoming agriculturalists (e.g. Chase 1989) (or so one might infer from the attention given to the question of why they weren't!). Let us suppose that in some sense this was so, and draw a parallel with Late Pleistocene peoples of the Perigord — from the Noaillien to the Magdalenian cultures, say. Other than the Perigordian preference for rockshelters, there is a notable parallelism, as Rhys Jones has suggested (1990:48): sophisticated toolkits, traded items from distant sources, personal ornaments, a diet substantially based on a large variety of animal foods — ca. 130 species including about 30 molluscs for the modern Gidjingali (Meehan 1982); ca. 50 in the deposits at the Abris Pataud (Bouchud 1975) —and a highly developed tradition of rock painting. If the Aborigines were in some sense 'ready' for agriculture, so too may have been the Perigordians - or at least so too may have been those amongst their contemporary societies in Late Glacial Europe or Asia whose estates included potential domesticates such as emmer and einkorn.

Suspending judgement on this scenario, the onset of (hypothetical) agriculture would have been inhibited by the millennial-scale climatic 'megaflickers', owing to the rate, magnitude and frequency of these events. To discuss this, it is useful to have a model in mind, which may be relaxed later. For convenience, we adopt Harris' (1989) three-stage model, where stage 1 is foraging leading to protective tending, stage 2 is deliberate production of wild plant foods culminating in soil modification, and stage 3 is cultivation of domesticated crops. In this light, the inhibiting effects of the climatic events and related vegetation shifts seem clear. Stage 1 behaviour embraces resource substitution and would have been a robust strategy under climatic changes; but stage 3 could only have persisted by the rapid substitution of one set of crops and cultivation practices for another, particularly as the rates of temperature change were several degrees per century, which would challenge any agricultural system - subsistence or otherwise (as the concern over today's global greenhouse shows). Furthermore, the time between abrupt climatic shifts, which ranges from a few hundred to a few thousand years in the interval from 40,000 BP to 13,000 BP (Fig. 1), is as short as or shorter than the timescale of *de novo* establishment of agriculture in the Holocene. Data, reviewed by Harlan (1995:86–93) from the Near

East indicate that passage from stages 2 to 3 in the Harris scheme took about 2000 years, for example. Thus, the probability of establishing agriculture on a continuing basis seems to have been very much higher in the Holocene; not because Holocene climates were warmer but because they were more stable.

Summary: the realms of climatic variability and agriculture

The above ideas concerning climatic variability, ecological responses and human subsistence are summarised in Fig. 4. Higher-frequency variability (times scales from inter-annual to several decades) is represented by the horizontal axis, which is divided into three classes: low, intermediate and high. Low-frequency or millennial-scale climatic switches are represented on the vertical axis, which is divided into two classes, low and high. Although the graph therefore resembles a 6-cell matrix, the axis variables should be regarded as continuous from low to high. Magnitude of event is not readily represented on this two dimensional graph, but the millennial-scale events have generally large amplitudes, as seen in Figs 1 and 2. Variability represented by the horizontal axis includes events such as floods and droughts, which generally have inverse relationships between magnitude and frequency (e.g. a fifty-year flood occurs one tenth as often as a five-year flood but is considerably larger). To give some sense to this axis, the climatic disturbances at the low end either occur very infrequently or are never large, while events at the high end are both frequent and large (although not on the same scale or duration as the millennial 'megaflickers'). To represent the ecological import of the horizontal axis, a curve showing species diversity according to Connell's (1978) 'intermediate disturbance' theory is shown above this matrix-like graph.



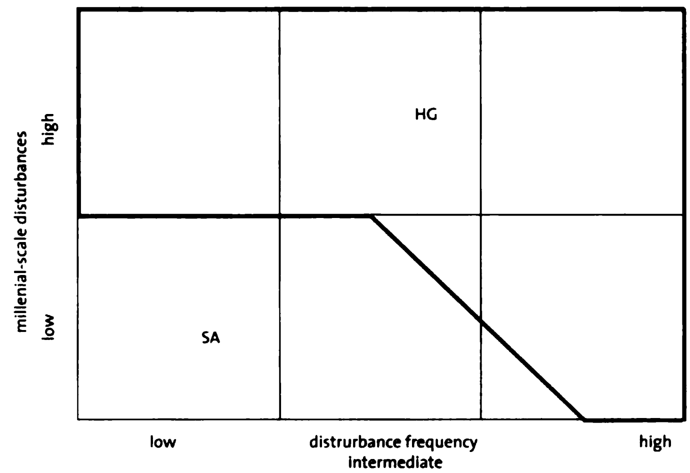


Figure 4. Top graph: species diversity as a function of frequency of disturbances (after Connell 1978). Main graph: subsistence in terms of two classes of climatic variability. Horizontal axis, higher-frequency variability (times scales from inter-annual to several decades); vertical axis, millennial-scale climatic changes. HG = hunter-gatherer realm; SA = subsistence agriculture realm..

The matrix is divided into two realms, labelled hunter-gatherer (HG) and subsistence agriculture (SA). The HG realm embraces all the high millennial-scale cells and a region in the low millennial-scale cells that represents a high frequency of disturbance events. A degree of interchangebility of the impacts of millennial-scale and higher frequency events is suggested by the oblique boundary separating the HG and SA regions. At one level, Fig. 4 can be seen as an empirical mapping of HG and SA cultures of the both late Pleistocene and Holocene, in terms of two forms of climatic variability. It can also be regarded as a theoretical and predictive diagram, open for testing, which might be overthrown by the discovery of a late Pleistocene hunter-gatherer culture of long standing (i.e. not an adventitious HG culture such as the moa hunter of New Zealand), whose climatic realm lay squarely in the SA region of Fig. 4.

RAPPROCHEMENT

The era of millennial climatic 'megaflickers' has rolled into the past; the historical Little Ice Age of western Europe and other such passages were but minor perturbations in the even sway of Holocene climates, disturbed only by El Niño and its cousins. In a world where agriculture was realised, the Australian

Aborigines continued in their hunter-gatherer way — despite the fact that agriculture was established by their immediate neighbours in highland New Guinea, before Torres Strait was breached by rising seas; despite that certain significant classes of plants domesticated in New Guinea lay within the estate held in common with the Australians (Golson 1989, 1991; Yen 1989). In reflecting on this contrast, Jones and Meehan (1989:133) wrote, 'the Australian system was one of great stability - these were successful hunters. As a model for the type of economic or social systems that historically led to the earliest practices of systematic horticulture in New Guinea or Southeast Asia, we may need to think of societies quite different from these - perhaps systems of inherent instability....'

Seen thus, the cultural and contextual differences between the Aborigines and the New Guineans stand in the sort of pairwise-opposites pattern that was beloved of the structuralist school of anthropology: inherent cultural instability under climatic stability (New Guinea) *versus* persistent cultural stability under climatic instability (Australia, where the climate is characterised by very high inter-annual variability; significantly more so than orographically-dominated Papua New Guinea). Undeniably, extreme events and foraging responses occur in New Guinea, as events surrounding the serious El Niño-induced drought of 1997 testify (Allen 2000; Ballard 2000; Bourke 2000). However, climatic and hydrologic variability in Australia rank higher. This is the land of flood and bushfire; the land of Goyder's Line and droughts that last a decade. For those who live not by the plough, the bush can be a land of plenty. The argument that millennial 'megaflickers' suppressed agriculture in Late Pleistocene times may apply with only modestly reduced force to Holocene Australia (and to other Holocene hunter-gatherer peoples living under climates or ecosystems of high variabitity, as in southwest Africa or the sub-Arctic). In this light, no explanation for persistence of Australian hunter-gathering need be sought in the structuralist terms proffered by Levi-Strauss (1962:81ff). Indeed, Levi-Strauss (1962:91), in writing quite inaccurately that 'the various seasonal changes [of rainfall in far northern Australia] are so regular that ... they can be predicted almost to the day', seems to have been unconcious of the factors to which this essay draws attention.

Whether Australia's peoples were progressing in the Late Holocene through intensification towards the brink of agriculture doubtless will be long debated, over endless coffee spoons. But, in closing, let us return to where we entered - to Kakadu and the lands of Australia's Top End, to the goose-camps and saratoga, the wallaby and whistle-duck, to where in the words of Betty Meehan (1982: 159), 'man does not live by calories alone.' Parallels between the Australian Aborigines and the Pleistocene Perigordians were drawn earlier in terms of their complex subsistence and artistic cultures; more figuratively, the parallel must be extended to all who 'willing shared the gatherings of their hands', even those conjured by Hesiod in a high and far-off golden age. In examining ecological aspects of the Aborigines' subsistence base and, in passing, exposing the historical roots of Progressionist views on the matter, Rhys Jones (1990:49) concluded that the ways of life of traditional Aborigines were 'adequate responses to the ecological challenges posed by the variability of the Australian continent.'

Variability has been the keynote of this essay, and in terms of climatic variability Australia maps squarely into the lower right-hand sector of Fig. 4. Furthermore, there are some regions in which geomorphological transformations in Holocene times had effects not very different from the millennial disturbances represented by the vertical axis in Fig. 4. Indeed, the culturally-significant transformation from salt mangrove to lush wetlands at Ki'na examined by Jones (1985:292 ff) was not a singular event, but more recently was shown to have been part of a landscape-wide series of transformations, set in train by rising sea in the earlier Holocene and continuing from when sea level stabilised until the present day. Great mangrove swamps flourished 6000 BP throughout the tidal-river lowlands in the Top End and transformed progressively, but in places very quickly into the seasonally flooded plains and wetlands of today (Woodroffe et al. 1986; Chappell 1993; see also Brockwell this volume). Mediated by sedimentation and a slow fall of relative sea level (by 1–2 m), the change has been shown by ^{14}C dating to have occurred rapidly around 5–6000 BP in middle and downstream reaches but was delayed in other areas, particularly in upstream reaches such as Ki'na. At the same time, coastal plains expanded seawards, although sedimentation often was dramatically punctuated by cyclonic waves that built beach and chenier ridges. This was a landscape of on-going geomorphological change, punctuated by floods, cyclonic disturbances and El Niño droughts, and thus would not be low-rated on either axis in Fig. 4.

As a final reflection, one test of the role of climatic variability may be found in Papua New Guinea. This stems ultimately from the idea, explored by Jack Golson elsewhere in this volume and expressed with characteristic verve by Rhys Jones in a conversation with me in 1986 after he and Paul Gorecki saw rock art with north Australian affinities in the Karawari gorge (Sepik River region), that Australia and Papua New Guinea in the period of Late Pleistocene low sea level embraced a single cultural region. If so, the advent of horticulture in highland New Guinea marked a parting of the ways, culturally, in early Holocene times. From Fig. 4, one would expect that climatic and environmental disturbances should have become smaller or less frequent around the Pleistocene-Holocene transition in highland New Guinea but to have changed little in Australia. Whether this was so, only future research will tell.

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New Guinea, Australia and the Sahul Connection

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EVEN ON a short chronology of 40,000 BP for their initial settlement, Australia and New Guinea spent the first four-fifths of their human history together as part of the enlarged Pleistocene continent that we call Sahul or Sahulland. They were finally separated by the rising postglacial sea at Torres Strait some 8000 BP (Torgersen et al. 1988:259–60), the rough equivalent of 8500 calendar years. As Gosden and Head have recently emphasised (1999:232–3, 234–5), interest in the implications of this situation has come to concentrate almost exclusively on the question of why Australian Aborigines did not become gardeners as some New Guineans had by the time Torres Strait was formed and as the inhabitants of the Torres Strait Islands were, to a varying extent, at European contact (Harris 1995; see also Saggers and Gray 1982–3). Gosden and Head (1999:234–5) say the effect of the focus on this single question has been to ignore the possibility of 'long-term historical continuities which still have some influence today', of which they cite the existence of Dreaming tracks in New Guinea as well as in Australia as a possible example (1999:244, 247).

A similar point about historical continuities in the two regions has been made by Gorecki and Jones (1987) in an unpublished report on a new province of rock art in the hills bordering the southern edge of the Sepik floodplain. The closest resemblances for this they see as lying, not with the rock art of New Guinea as known, but to the south, in Australia, especially the Carnarvon Gorge region of southern Queensland (1987:8–9). They go on to cite instances of cultural elements reminiscent of Australia that other scholars have noted as present in the Sepik grasslands and the hills bordering them to the south (e.g. Laycock 1973:58), for example the spearthrower (e.g. Swadling et al. 1988:Fig.238). Rhys found this some help in explaining the general sense of familiarity that, as a student of hunter-gatherers of the tropical savanna of Arnhem Land, he felt with the hunting and fishing sago eaters of the south Sepik (Jones pers. comm. 2000). 'Can we not start to look for similarities between northern Australia and New Guinea,' Gorecki and Jones conclude (1987:9), 'instead of always stressing the differences?'

This is the task I have set myself as a contribution to the volume honouring Rhys and his work. The arguments that I develop do not depend on precise chronologies, so the reader will find that I refer to dates in two different ways. Thus 'BP' (= before present, strictly speaking before 1950) means that the statements in question are based on ^{14}C ages which have been left uncalibrated to facilitate their recognition in the sources from which they have been taken. All other dates, expressed as 'years ago' or the equivalent, are in calendar years, being provided by methods of dating other than ^{14}C or from the calibration of ^{14}C results.

SETTING THE SCENE

While there is little doubt that the earliest stone tool traditions of the Sahul continent had their origins in the late Pleistocene of Sundaland, the predominantly flake industries in question on both the Southeast Asian and Australasian sides are unspecialised in nature (Bowdler 1990:16–17; Veth et al. 1998:164-6), making relationships between and within them difficult to demonstrate (Mulvaney and Kamminga 1999:209). As has been variously observed by different authors (Bowdler 1992:570; Holdaway 1995:789–90; Veth et al. 1998:164; Mulvaney and Kamminga 1999:219–21), what *is* distinctive about the Sahul industries is the occurrence of axes or hatchets with edge grinding; of large 'waisted' tools indented at the sides presumably for hafting; and of implements where hafting aids are found together with edge grinding on the same piece.

As regards Asian parallels, Bellwood (1997:174) describes early edge grinding as 'widespread ... but ... spotty' in distribution 'around the eastern fringes of the Old World', the oldest occurrences being in Japan from 30,000 BP, at Kota Tampan on the Malay Peninsula in an even older context and in Vietnam in Hoabinhian contexts apparently back to 18,000 BP (1997:173, 160, 161). Waisting in the broad sense is less in evidence in Pleistocene contexts in Sundaland. Bellwood (1997:190) notes occasional waisted forms among Hoabinhian implements and illustrates some with 'incipient waisting' from Gua Cha in Peninsular Malaysia (1997:159, Fig. 6.2), where the Hoabinhian does not appear to go back beyond 13,000 BP (1997:161).

Bellwood (1997:190) says that waisted tools have not yet been found in the Indo-Malaysian islands, but, as Groube (1986:169) tells us, Zuraina (1982:63, Plate V.3 top left) records a broken tool with side notches from the West Mouth of the Niah Caves in Sarawak, which she suggests may have been hafted for use. Rather confusingly, she classifies it in her category of pebble implements, which are pebbles with no sign of modification except through use. Groube (1986:169) describes it as 'the only tool from Southeast Asia approaching the dimensions and proportions of [the waisted axes] from Sahuland' and says that it cannot be older than 20,000 BP. In fact, it could be younger than 15,000 BP, since the class of axe-adze to which it is allocated within the category of pebble implements occurs no deeper than 48–60 inches (Zuraina 1982:Table V.1) and there is a ^{14}C date of around 15,000 BP for a depth of 52–54 inches (1982:Table IV.3, GX-4839).

The edge-ground axes from Niah are classified as axe-adzes in the category pebble tools (Zuraina 1982:58, 63), which are pebbles with intentional modification (1982:56). They are not found deeper than 72–84 inches (1982:Table V.1), which translates into a rough age of 20,000 BP from a slightly older ^{14}C date at a depth of 84–90 inches (1982:Table IV.3, GX-4834). Interestingly, there is a subclass, called short axe-adzes, said to have a flaked butt, possibly as a hafting aid (1982:58, 63): an example in Plate V.2 (second from the left in the bottom row) could properly be called 'waisted' in a general sense, despite having one side broken, and would not be out of place in Arnhem Land, though smaller than the axes there (e.g. Schrire 1982:Figs 27, 42). These short axe-adzes are only found in the lower levels of axe-adze pebble tool distribution, which is given as 48–72 inches (1982:58), but should properly be 48–84 inches (see 1982: Table V.1, under Pebble Tools, Axe-Adze). This represents an age range of around 15,000–20,000 BP (1982:Table IV.3, the bottom two dates).

In Sahul both edge grinding and waisting are well in evidence by 20,000 BP.

CLEARING THE DECKS

The term 'waisting' needs some discussion. It has been used to describe prehistoric New Guinea, and to some extent Australian, axe-like implements with a variety of modifications of the butt generally thought to have had the purpose of facilitating their hafting. Les Groube (1986:168–9, Fig. 1) identifies three basic forms to which the term has been applied, restricting his own use of it to implements whose width at the waist is less than that at both the butt and the blade (my Fig. 1a), in which he is followed by John Muke (1984:82) and by myself. I make a distinction in Figure 1 between axes with a closed (1a(i)) and an open (1a(ii)) waist.

I differ from Groube in the terms I use for the other two types: his 'stemmed' axe (Groube 1986:Fig. 1b), where the width of the butt is about the same as that at the waist and less than half of that of the blade

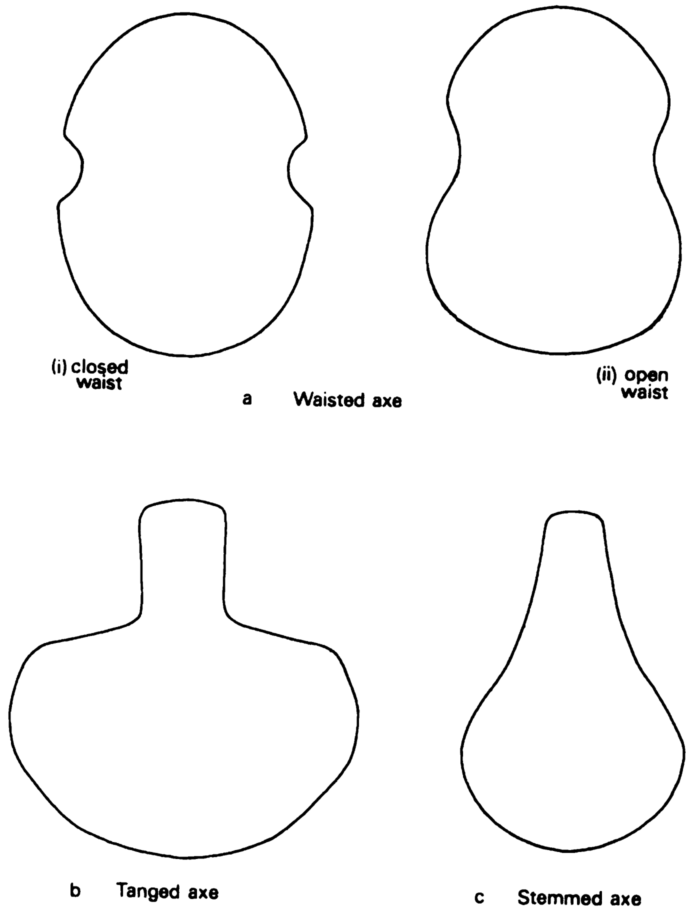

Figure 1. Terminology for the basic forms of axe-like implement conventionally called 'waisted' (after Groube 1986:Fig. 1).

(Fig. 1b), I call 'tanged', while his 'handaxe shape' (Groube 1986:Fig. 1c), where the poll is the narrowest part of the implement and the line between it and the widest part at the blade is generally concave (Fig. 1c), I call 'stemmed'. Groube and Muke exclude tanged and stemmed axes from their analyses. Bulmer (1977:55), who did the pioneer work, was less restrictive, seeing waisting and tanging as forming a continuum (1977:58). Indeed, her analyses include 'all tools with a working edge on one end, with flaking to shape the implement on the sides, whether or not it has butt modification' (1977:45, Fig. 1).

I use the term 'butt-modified' to cover the three different forms under discussion, acknowledging that they are broad categories with much variation within and overlap at the edges. Where there is information and it is relevant, I refer to the presence of axe and axe-like tools with no butt modification.

In the review that follows I am interested in butt modification and edge grinding as attributes in their own right and my focus is on Australia and mainland New Guinea and on axe-like tools. Thus I do not deal with the thin tanged implements of slate found in small numbers in the Sepik and the Highlands, which I have briefly discussed elsewhere (Golson 2000:243–4). However, I include some reference to the Bismarck Archipelago and the Solomon Islands chain, whose settlement took place in the late Pleistocene as a flow-on from that of Sahulland and which, after Green (1991), we call Near Oceania to register their distinctive place in the human colonisation of the South Pacific. Figures 2a and 2b map the localities with which I shall be dealing.

BUTT MODIFICATION IN MAINLAND NEW GUINEA

This section reviews the distribution of the different kinds of butt modification on the New Guinea mainland site by site.

Bobongara, Huon Peninsula

The Bobongara site was discovered in the early 1980s at the eastern end of the suite of uplifted coral terraces for which the Huon Peninsula is well-known (Groube et al. 1986). The collection from there falls into two parts. One comprises a small number of artefacts excavated *in situ* in association with three tephras lying on top of reef complex IIIa. An age in excess of 40,000 years for these discoveries is based on dates for reef IIIa, for the next younger reef in the sequence, IIIb, which seems not to have emerged when the last of the three tephras fell, and for the three tephras themselves (Groube et al.1986:453–4; see Chappell et al. 1996:232–3, Table 1 for newer dates for the two reefs). Three waisted axes were among the excavated finds (Groube et al. 1986:454–5). One of them has a distinctive hammer-dressed groove on both faces between its two waisting notches (Groube et al. 1986:Figs 3a, and 3b; see also Flood 1983:Colour Plate 2, shown with working end up), a clear indication that the implement was hafted for use, and hafted as an axe. This hafting groove becomes important in later discussion.

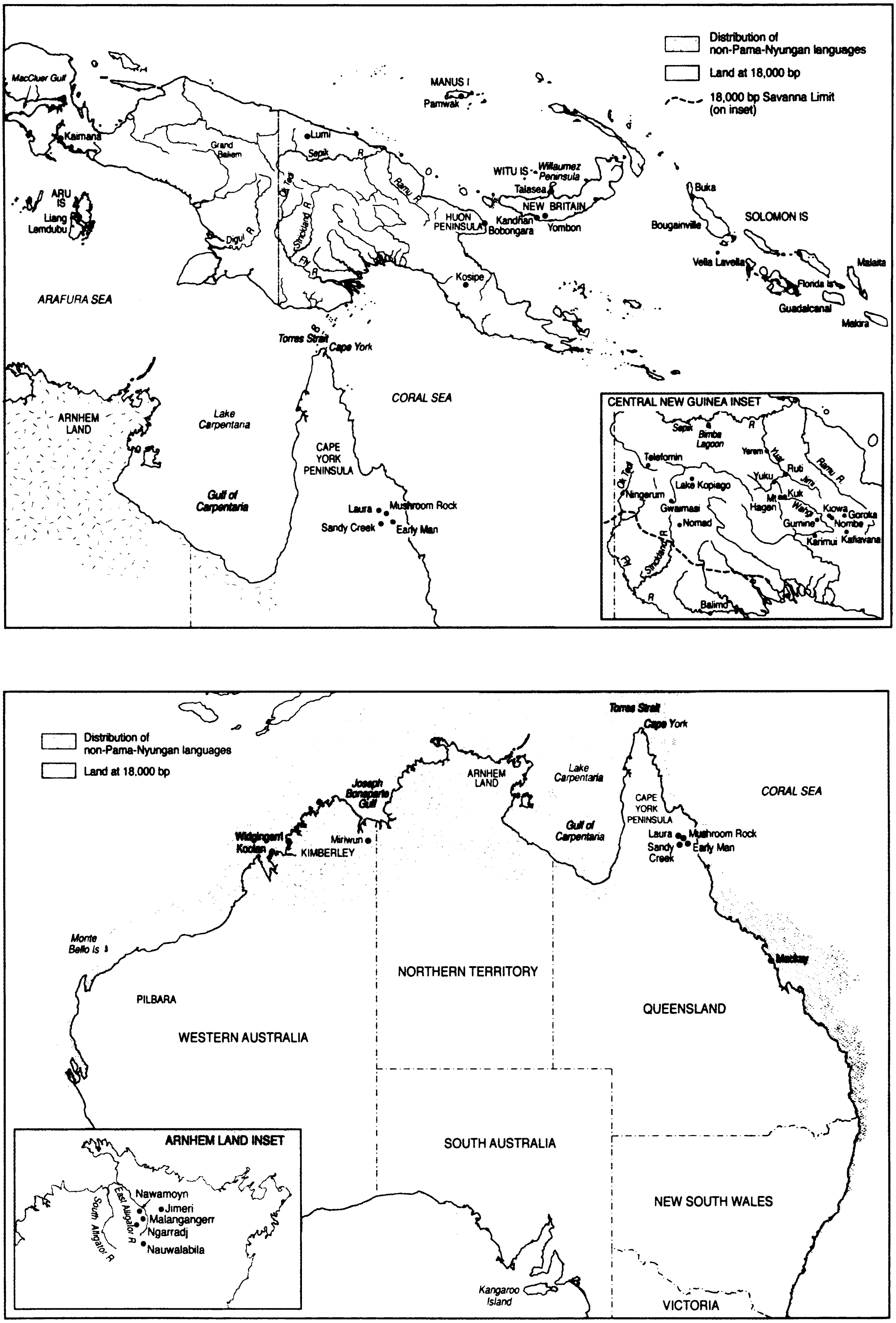

Figure 2. Locality maps
a) New Guinea and adjacent islands: the savanna limit at 18,000 BP shown on the inset is after Swadling and Hope 1992: Fig. 3.3.
b) Australia: the distribution of non-Pama-Nyungan languages is after Morwood and Hobbs 1995b:Fig. 1.

The other and much larger part of the Huon collection consists of artefacts picked up in the general area of the excavations, predominantly at or somewhat above reef IIIa, loose in the bottom of gullies that have been cut by creeks through the coral terraces and overlying tephras where these existed (Groube et al. 1986:455). Groube (1986:Fig. 2) provides a view of the situation, where his terraces 9 and 10 are crests of reef complex IIIa. It seems likely that most of the gully collection is of a similar age to the excavated finds. Its butt-modified component consists of over a hundred waisted axes, none of them with a hafting groove, as well as a stemmed axe and a 'knobbed' one (Groube et al. 1986:455). As I have indicated earlier, Groube's 'stemmed' is my 'tanged', while the term 'knobbed' appears nowhere else. Since Muke makes no mention of these two forms in his discussion of the Huon material, they are likely not to have been collected at the time he wrote. John Chappell tells me (pers. comm. 2000) that he later found a tanged specimen exposed by a track at the base of shallow tephric soil at about the same level as the Bobongara IIIa reef complex and some 3 km southeast of the excavated site there.

The Huon collection that Muke (1984:83) had available for study consisted of 72 waisted axes, 39 of them complete. They are large and heavy tools, broad in relation to length. The complete specimens range from 11.0 cm to 22.1 cm in length, with a mean of 17.7 cm, and from 350 g to 2450 g in weight, with a mean of 1330 g (1984:113–4). For the most part the waisting is closed rather than open (Fig. 1a(i) rather than Fig.1a(ii)) (1984:156–7, Fig.19). On 21 implements the position of the waisting falls between half and five-eighths of the distance along the length axis from the working end (indeed, on four implements it falls at less than half) and on the remaining 18 between five-eighths and three-quarters (1984:164–5). The mean angle of the working edge on the 29 implements on which it could be measured is 75°, with a standard deviation of 9.22° and a range from 58° to 109°(1984:123, 125, Fig. 14).

Muke (1984:72, 74, Plate 6) also makes reference to a class of unifacially flaked pebbles morphologically similar to waisted axes but lacking the waist.

Kosipe, Owen Stanley Range

Kosipe is an open site at ca. 2000 m altitude, where archaeological material was found in association with a sequence of volcanic ashes. The excavators identify a concentration around level 5, which means the bottom of level 4 and the top of level 6 as well, where six of the 11 butt-modified artefacts recovered during the investigations were found (White et al. 1970:167 and Table 4). The date is argued to be ca. 23,000–26,500 BP. Another two specimens came from higher up in level 4, but below Natanga ash, which is dated 17,000-18,000 BP (1970:167–8, where the age range is given as 17,000–18,500 BP, and Fig. 2).

These eight butt-modified specimens comprise five waisted axes (White et al. 1970:Figs 3a, 4a, 4d and S.2 and S.4 reconstructed from measurements in Table 5); one tanged specimen (Fig. 3b), which Muke (1984:151–2) squeezes into his analysis as a 'waisted stemmed axe' because it is marginally waisted at the base of the tang; a damaged piece (White et al. 1970:Fig. 4c), which is likely to have been stemmed; and a fragment (Fig. 4b), about which I can say nothing.

The remaining three items are from terminal Pleistocene and early Holocene levels (levels 3 and 2 respectively) (see White et al. 1970:159 and Fig. 2, who argue that of the two ^{14}C dates for level 2, the older is to be preferred because that sample was more precisely collected). The three items comprise two waisted axes (Fig. 3e, the other reconstructed from measurements for number 5 in Table 5) and an unfigured item, whose measurements (Table 5, number 2.2) suggest that originally it might have been stemmed.

Six axe-adzes are described (1970:166, Tables 4 and 6, Figs 3d and 3f), including one probable specimen (level 6) and one definitely edge-ground (level 2, Fig. 3f). Four of them (levels 5 and 6) are fragmentary. The two with measurable length (levels 2 and 4) and the four with measurable breadth fall within the range of the butt-modified implements. The edge-ground piece is discussed later.

All the above artefacts are what Bulmer (1977:45–6) calls end-edged tools, with bevelled working edges formed by unifacial or bifacial flaking (cf. Lampert 1983:145).

Yuku, Western Highlands Province

Yuku is a rockshelter at 1280 m altitude with a maximum depth of deposit of some 3.5 m, in which butt-modified artefacts were found from layer 3 of mid-Holocene date to layer 7 at the base, which is about 150 cm below a collagen date of ca. 12,000 BP in layer 5B (Bulmer 1975:29–31, 1977:43–4). Bulmer (1964:257) reports 21 butt-modified implements (Bulmer 1977:44 says 23), four in layer 3, 11 in layer 4, three in the top part of layer 5 and two in layer 7 (making 20). Nine of these are said to be 'shouldered', which is my 'stemmed', and waisted and stemmed were found together in layers 3, 4, 5 and 7. Muke (1984:94) had access to 18 of the Yuku butt-modified for his analysis, of which 10 were waisted axes. Six waisted and two stemmed axes are illustrated by Bulmer and Bulmer (1964:Fig. 3; c and d are the stemmed axes).

The four butt-modified specimens found in mid-Holocene layer 3 are all edge-ground. Two of them are waisted (Bulmer and Bulmer 1964:Figs 3a and 3f; 3a is illustrated by Muke 1984 as Plate 13 right and said there and elsewhere to be from layer 4A), one is stemmed (Muke 1984:Plate 13 left) and the fourth is not described. I return to the question of edge grinding below.

Bulmer (1977:Table 1) lists four unwaisted end-edged tools for Yuku. They belong to subgroups N, O, P and Q of her Figure 1, but only two of these subgroups appear in her Figure 2. These are O and P, belonging respectively to level 4C-E, probably early Holocene, and level 7, definitely late Pleistocene (1975:30).

Muke (1984:134) says that of the 10 Yuku waisted axes analysed by him all but two were bifacially flaked at the working edge. The angle of this edge could be measured on nine axes (1984:Table 4) and ranged from 41° to 80° (mean 66.1°, standard deviation 11.77°).

Bobongara, Kosipe and Yuku Compared

These three sites are a focus for attention because they provide the largest collections of butt-modified tools, with Bobongara supplying the oldest and Kosipe and Yuku two stratified series running from the Pleistocene into the Holocene. Both Groube (1986) and Muke (1984) have been interested in comparisons between them, Groube with an emphasis on regional relationships, Muke more concerned with the local implications.

The Bobongara collection consists of waisted axes and so is more homogeneous than the Kosipe and Yuku collections which combine stemmed and waisted types, with Kosipe adding a tanged specimen. It is true that a stemmed axe and a 'knobbed' one have been reported as surface finds at Bobongara (Groube et al. 1986:455) and a second tanged specimen has been found by John Chappell (pers. comm. 2000), but we do not know their relationship to the main collection, nor have they been described. The following comparisons therefore are restricted to the waisted axe component at the three sites.

Size

Muke (1984:Table 4) gives a series of measurements for the waisted axes in the three collections, of which I use total length as a measure of size. The range of lengths at Kosipe (n=7) is from 8.3 cm to 15.0 cm (mean 12.66 cm, standard deviation 3.21 cm) and at Yuku (n=10) from 7.4 cm to 21.5 cm (mean 12.7 cm, standard deviation 4.17 cm). For Bobongara the range for 39 waisted axes is from 11.0 cm to 22.1 cm (mean 17.7 cm, standard deviation 2.49 cm). This contrast holds equally for weight where the figures are available, which they are for Bobongara and Yuku. Muke (1984:115) says that of 10 waisted axes at Yuku, seven weigh less than 400 g, whereas this is true of only one at Bobongara out of 39.

The two largest of the Yuku axes (Bulmer and Bulmer 1964:Figs 3e and 3g) fall well within the Bobongara range for weight and length (Muke 1984:Fig. 11) and one of them (Fig. 3g) comes from the undated but ancient base of the shelter. The four largest waisted axes of the seven from Kosipe fall at the lower end of the Bobongara range for length and breadth, no weights being available (Muke 1984:120, Fig.12). On the basis of the measurements provided by White et al. (1970:Table 5), they belong to the lower part of the site, for which, as we have seen, a date of 23,000–26,500 BP is favoured.

Shape

The hint that size and weight might be important chronologically (Muke 1984:118) encouraged Muke to look at the shape of waisted axes from the same point of view (Muke 1984:Chap. 6). It is necessary to point out that for his analysis Muke divides Kosipe and Yuku differently from the way the excavators do for theirs. Thus where he combines levels 2-4 as the upper part of Kosipe (n=4) and levels 5-6 as the lower (n=3) (Muke 1984:Table 10), White et al. (1970:160-1, 167-8) look upon level 4 as belonging with levels 5 and 6. As for Yuku, Muke (1984:Table 10) puts layers 3 and 4 together as the upper part of the site (n=6) and layers 5-7 as the lower (n=4), whereas Bulmer (1975:30) combines layers 4 and 5 as phase 3 of site history. Despite this, and the smallness of the samples and the large amounts of space and time involved in the comparisons, some interesting indications result from Muke's analysis.

As regards overall shape (Muke 1984:169–70), the waisted axes from the older levels of Kosipe and Yuku tend to have the same squareness of shape as the Bobongara specimens. In the younger levels at the two sites the widths of the butt and waist are significantly narrower in relation to total implement length than at Bobongara, and while the blade is narrower also, it is not as markedly so. Thus, in Muke's view (1984:176), 'change occurred over time in the attachment part of the tool, while the blade retained its width for functional reasons'.

At the same time there are differences in the shape of the waist (Muke 1984:176–7). The notching that tends to be pronounced on Bobongara specimens (cf. Fig.1a(i)) is less so in the older levels of Yuku and especially Kosipe, while open waisting is the norm in the upper levels of both sites (cf. Fig.1a(ii)).

Differences are not as clear in the matter of the position of waisting on waisted axes (Muke 1984:177–8), though Muke sees a tendency for it to be nearer the butt end on younger implements.

As regards the angle of the working edge, while there is some overlap between Bobongara and Yuku and there are no measurements for Kosipe, the figures support Matthew Spriggs' (1997:59) statement about the contrast between the large blunt Huon specimens and smaller and sharper forms.

CONCLUSION

The Kosipe and Yuku collections of butt-modified implements are more diversified than that from Bobongara because of the clear presence of other forms than waisted axes. At the same time Muke's analysis of the waisted axes at the two sites shows that they have similar differences from the Bobongara series in size and shape, suggesting a chronological significance in the changes. As Muke (1984:94) acknowledges, Bulmer suspected something of the kind even before the discovery of the Bobongara site. Talking about the 150 cm of undated deposit in Yuku below the collagen date of ca. 12,000 BP, she says (Bulmer 1975:30): '... on the basis of the close similarity between the large stone implements in the early layers at Yuku and Kosipe ..., it is expected that Yuku may have been occupied as early as Kosipe.'

OTHER EXCAVATED FINDS

Kiowa, Simbu Province

The site of Kiowa at an altitude of 1530 m in Simbu Province was utilised from about 10,000 BP (Bulmer 1975:35–6), but butt-modified axes did not make an appearance until levels 5 and 3 in the middle Holocene (Bulmer 1964:262 for implements, 1975:35 for dates). The artefact from level 5 (Bulmer 1964:Plate 2 top left; Bulmer and Bulmer 1964:Fig. 2e) is stemmed (or perhaps even tanged), that from level 3 (Bulmer 1964:Plate 2 bottom left; Bulmer and Bulmer 1964:Fig. 2d) waisted. Both are said to be comparable in size to the smallest Yuku examples. Unlike the butt-modified implements from the mid-Holocene at Yuku, these Kiowa artefacts of the same age are not edge-ground.

A fragment of a polished blade from level 2 in the process of being reworked to produce a waisted axe is not further described (Bulmer 1964:261–2).

Nombe, Simbu Province

This rockshelter, only a few kilometres from Kiowa but 190 m higher in altitude, has received attention from two different workers. Because of the complex stratigraphy Peter White encountered in his excavations of the 1960s, he did not date either of the two horizons into which he grouped the excavated materials for description and publication (White 1972:Chap.VI, under the name of Niobe). He reports two butt-modified implements from the lower horizon (White 1972:132), one described as 'clearly waisted but ... broken just above the waist', the other a stemmed example some 7.5 cm long (from the illustration, Fig. 23d). This piece is edge-ground and the subject of further comment below.

Mary-Jane Mountain, who resumed excavations during the 1970s, thinks that all White's finds are Holocene in age (pers. comm. 2000). She herself recovered the first signs of human occupation at the site, in Stratum D, just above a thin and vuggy sheet of calcite with a date of ca. 25,000 BP (Gillieson and Mountain 1983:55–6). Among those first signs was a tanged axe, ca. 13.9 cm long from the illustrations (Mountain 1983:94, Fig. 7; White with O'Connell 1982:Fig. 3.10d) and an associated edge-ground axe with no butt modification, as well as a butt fragment that may also have originally belonged to a tanged axe (Mountain pers. comm. 1995). The edge-ground specimen is referred to again below.

Kuk, Western Highlands Province

Kuk is a swamp in the upper Wahgi Valley near Mt Hagen, which was periodically drained for cultivation from the middle and possibly the early Holocene. The swamp deposits are marked by a widespread change from brown and red-brown formations rich in organic detritus to black organic clays. This transition, for which a date of 20,000 BP is now preferred to one of 30,000 BP previously favoured (see Hughes et al. 1991:Fig. 3), is likely to represent the original position of the upper part of a stone artefact with modified butt thrown up by workmen digging a drainage ditch in connection with archaeological investigations. This represents a modification of a recently published statement (Golson 2000:242) that the artefact came from the top of the underlying organic-rich swamp.

The tool fragment in question is likely to have come from a waisted axe. The open waisting is high on the butt, as with the waisted axes from Gwaimasi in Western Province discussed below (Minnegal 1991:Figs 2A and 2B; Swadling and Hope 1992:Fig. 3.5), to which its intact measurements are very similar. The 'knobbed' axe reported by Groube et al. (1986:455) from Bobongara may have been something like these specimens in shape.

SURFACE FINDS

With butt modification

Muke discusses a waisted axe (1984:97, 101, Plate 14 right) in the collections of the Papua New Guinea National Museum from Karimui on the southern fall of the central highlands in Simbu Province. It is uncertain from his descriptions whether it is ground (1984:134, 176). It is rather large (length 14.0 cm) and heavy (1090 g), but Muke (1984:176) thinks that in shape it resembles the younger waisted axes from Kosipe and Yuku. The working edge angle is 72°(1984:134).

There are records of waisted axes from the dissected piedmont country south of the central ranges in Western Province, in the area of Ningerum on the Ok Tedi (Swadling and Hope 1992:22, Fig. 3.4 bottom left), at Gwaimasi on the Strickland (Minnegal 1991:512, Table 1, Figs 2A, and 2B; Swadling and Hope 1992:22, Fig. 3.5) and in the Nomad area (Swadling and Hope 1991:22, Fig. 3.6 top). Minnegal (1991:Tables 1 and 2) gives measurements for the four examples from Gwaimasi and (Table 4) compares the means with those for Bobongara, Kosipe and Yuku provided by Muke. While this shows that the Gwaimasi tools fall with the smaller members of the Kosipe and Yuku collections (see also Muke 1984:Table 7 and 4 for size and weight at Yuku), the major difference between the Gwaimasi implements and the others is the closer placement of their waisting to the butt, something they have in common with the 20,000 BP fragment from Kuk, as I have noted above. The waisted axe from Ningerum and that from Nomad, both 12.5 cm long from their illustrations, are similar in size to those from Gwaimasi (range 11.0 cm to 13.6 cm).

From Ningerum and Nomad there are stemmed axes as well as waisted (Swadling and Hope 1992:Fig. 3.4 bottom right and Fig. 3.6 middle respectively), 11.4 cm and 14.0 cm long, as measured from the illustrations.

Stemmed axes are also illustrated from Lumi in the Torricelli Mountains in Sandaun (West Sepik) Province and from Bimba lagoon just east of Ambunti in East Sepik (Swadling et al. 1988:Figs 53 and 51 respectively), the former 15.0 cm, the latter 12.7 cm long, again from the illustrations. Also in East Sepik Province, at Yerem village, where the Yuat River enters the Sepik plains from the south, Gorecki (1989:174–5, Fig. 8.23, Plate 8.12; see also Swadling et al. 1988:Fig. 54) recovered five stemmed axes from an extensive exposure of pebble artefacts and workshop debris. Ranging in length from 7.4 cm to 12.0 cm from Gorecki's illustrations, they bear a general resemblance to the stemmed axe from Layer 7 at Yuku rockshelter (Bulmer and Bulmer 1964:Fig. 3d), further south in the same drainage basin, which is 11.0 cm long from its illustration. Layer 7, we may recall, lies at the base of the site, with 1.5 m of undated deposit below a collagen date of ca. 12,000 BP.

Between Yerem and Yuku are the Ruti flats at about 500 m altitude, where, when the manager of the cattle station had a trench dug in a small swamp, three unifacially flaked pebbles were thrown out with the spoil (Gorecki 1989:148). Two of them were butt-modified axes, one of them (1989:Fig. 8.11 bottom) stemmed, the other (top) better described as tanged rather than stemmed. The stemmed piece, broken across the blade, is now 13.48 cm long, the tanged one 15.73 cm. Gorecki says that stratigraphic indications give the finds a minimum age of 5000 BP.

Swadling (1983:95, Fig. 41a) records a chert tool from the Telefomin area of the central highlands in Sandaun (West Sepik) Province, somewhat similar to Gorecki's tanged tool from the Ruti flats. However, it is better called stemmed than tanged because it is narrower than the Ruti piece in the blade, though from the illustrations about the same length. It is mentioned in the same context as a tanged piece and a waisted one from the Lake Kopiago area of Southern Highlands Province (Swadling 1983:33 for both, 95 for the former), about which there is no further information. Swadling's Figure 46bA reproduces a sketch of the tanged piece from a photograph that had no scale.

Some distance to the west comes the only butt-modified axe I know of from Indonesian New Guinea, a surface find by Chris Ballard from the floor of the Grand Baliem Valley in the central highlands. It is properly called tanged, even though the tang expands somewhat at the butt. Measuring about 14 cm from a photograph, it is similar in size and shape to the tanged specimens from Nombe and Kosipe.

Without butt modification

Minnegal (1991:Table 1) describes six tools without butt-modification from Gwaimasi, two of which she illustrates (1991:Fig. 2 bottom row). Some tools described as waisted in the literature that I have been reviewing do not meet the criteria for butt modification that I have been applying. Two artefacts from Ningerum (Swadling and Hope 1992:Fig. 3.4 top row) and one from Kini near Balimo, also in Western Province (1992:Fig. 3.6 bottom), fall into this category.

CONCLUSIONS

On the New Guinea mainland waisting and stemming are the commonest forms of butt modification, widely distributed in time and space and strongly associated.

Waisted axes are present at the oldest known site of New Guinea settlement, Bobongara on the Huon Peninsula, at more than 40,000 years ago. They are prominent in the sequences at Kosipe, which spans a period from ca. 25,000 BP to the early Holocene, and at Yuku, where they go back to the beginning, well before 12,000 BP, and end in the mid-Holocene. Along the central cordillera between Kosipe to the southeast and Yuku to the northwest, they are known at Nombe and Kiowa in Simbu Province and at Kuk in Western Highlands Province. There is a reported find from near Lake Kopiago in Southern Highlands Province further west. To the south of the mountains waisted axes have come from Karimui in Simbu and from three general localities in Western Province.

Stemming is almost certainly represented by a damaged implement at Kosipe dating to the early period at the site, ca. 25,000 BP, and there may be another stemmed example in the early Holocene level there. However, the type is in greater evidence at Yuku, where examples partner waisted axes through the sequence. In the central highlands between Kosipe and Yuku they are known from Kiowa and Nombe in Simbu Province. An artefact from the Telefomin area in the mountains near the Indonesian border has been called stemmed rather than tanged. More definitely stemmed pieces have come from two of the areas south of the central ranges in Western Province that have produced waisted axes. Between the mountains and the Sepik stemmed axes have been reported for Ruti and Yerem in the same drainage basin as Yuku. To the north there is a record for Bimba lagoon close to the Sepik and one for Lumi in the mountains north of it.

Tanged axes are not frequent either in excavated or surface contexts. Though they are reported from Bobongara, they are surface finds there and undatable. The oldest dated example is from ca. 25,000 BP at Nombe, where a stemmed and a waisted axe have been found in later but undated levels. There is a tanged axe at Kosipe which is of the same age as that from Nombe or somewhat younger. A butt-modified artefact from Ruti, downstream from Yuku, has been called tanged rather than stemmed. A more decisively tanged example of unknown size is reported from Lake Kopiago in Southern Highlands Province. Finally there is the tanged axe from the Grand Baliem Valley in West New Guinea.

Grooving is known on a waisted axe excavated at Bobongara more than 40,000 years old and does not recur.

BUTT MODIFICATION IN NEAR OCEANIA

For the time being I restrict my discussion to the Solomon Islands, where surface finds of unifacial waisted axes, in the strict sense, some with grooving, are of great interest, given the geographical position of the

Solomons at the oceanic margin of Near Oceania. Groube (1986:172) makes a mention of two waisted axes which he describes as unifacially worked and of similar proportions to Huon axes. His informant, Alec Rukia (1989), adds detail to the story.

In his text Rukia (1989:39) lists a specimen from Nggela Pile, the easternmost island of the Florida group, another from Malaita and an unspecified number from Marau Sound at the eastern end of Guadalcanal, as well mentioning the report of a second example from Nggela Pile. Rukia's Figure 5 illustrates three Solomon Islands specimens, from Nggela Pile (Fig. 5a, the first of the artefacts referred to above), Guadalcanal (Fig. 5d, but not apparently from Marau Sound) and possibly Makira (Fig. 5c, the former San Cristobal). Interestingly, all three illustrated items are grooved. Two of them (Figs 5a and 5c, 18 cm and 19 cm long respectively, the latter measurement from the illustration) fall around the mean length of the Huon collection. The third illustrated piece (Fig. 5d) is 12.9 cm long (from the illustration).

BUTT MODIFICATION IN AUSTRALIA

The Australian situation with regard to butt modification is very different from that described for the New Guinea mainland. Firstly, in Australia butt modification is much more often combined with edge grinding than in New Guinea and I shall look at the details of this in the next section. Secondly, what Lampert (1983:151) calls 'definite suites' of butt-modified implements comparable to the New Guinea cases are very few. Indeed, there are only two, both of them comprising surface finds. They are from Kangaroo Island, South Australia, and Mackay on the central Queensland coast and they are separated by some 2000 km distance in a country much better archaeologically known than New Guinea.

Kangaroo Island, South Australia

The 24 examples from Kangaroo Island are all waisted axes. Because of their loose association with Kartan sites, their manufacture on the same rock types as Kartan tools and their similar massiveness, Lampert (1981:78, 1983:148) considers them to be part of the Kartan industry and Pleistocene in age. With access to 14 specimens for study, Lampert (1981:190–1, Figs 77–80a, 1983:145, 148) gives their mean length as 20.62 cm (standard deviation 2.87 cm), with 70% of the functional ends bevelled by bifacial flaking, the rest being naturally flat or rounded.

Mackay, Queensland

First put on record by McCarthy (1949) as waisted hammerstones, the artefacts from Mackay took Lampert's interest (1983:148-9) because they had similarities of size and shape to the Kangaroo Island artefacts. He inspected a total of 80 items from restricted areas northwest and west of the town, but in his analysis did not break the collection down into the categories that he recognised in it.

Some of the pieces are truly waisted (Lampert 1983:Fig. 1a) and others are stemmed (Figs 1b, 2a, 2b). However the range extends 'through blocks on which the notches, though still opposed and bifacial, are not deep enough to alter the rectangular outline of the tool … to blocks on which the notches are similarly subdued, but which … are elongate and have a squarish lateral cross section' (Lampert 1983:149).

Lampert (1983:148–9) supports McCarthy in the view that the artefacts were hafted for use as heavy hammers or pounders, based on the blunt nature of their working ends and the use wear associated with them.

Australian and New Guinea Compared

Lampert's statistical analysis (1983:149–53) came up with significant differences not only between the Australian collections and Kosipe, the only New Guinea site at the time with comparable data for his purposes, but between Kangaroo Island and Mackay as well. Groube (1986:170) was of the opinion that the heterogeneous nature of the Mackay collection, as indicated above, was a relevant factor in Lampert's conclusion about Kangaroo Island and Mackay. As regards the Kosipe comparison, this ceased to be relevant following the Bobongara discoveries, when it was seen that artefact size and position of waisting, which marked Kosipe off from the Australian collections (Lampert 1981:190–1; Groube 1986:170), marked it off in the same way from the 40,000 year old material from the Huon (Muke 1984:120–1, 169, Tables 4 and 5, Fig. 20).

As a result of their analyses Groube (1986:174) and Muke (1984:178) concluded that the waisted axes of New Guinea and Australia belong to the same population (Groube) or tradition (Muke). This conclusion can be seen as supporting an early date for the two Australian collections.

EDGE GRINDING IN MAINLAND NEW GUINEA AND AUSTRALIA

New Guinea

On butt-modified axes at Yuku, Nombe and Karimui

A few of the New Guinea butt-modified implements discussed in the last section were mentioned as having edge grinding. There are four mid-Holocene examples from layer 3 at Yuku, two waisted, one stemmed, the fourth not described. A stemmed axe with edge grinding from White's lower horizon at Nombe is undated. Among the surface finds the waisted axe from Karimui is not certainly ground.

At Kosipe and Kafiavana

Two butt-modified artefacts from Kosipe are said to be possibly ground (White et al. 1970:165), one from level 2, which I have argued to be early Holocene, the other from level 5, the oldest part of the site, which the excavators date at 23,000–26,500 BP. There is no specification of which pieces these are.

There is unequivocal edge grinding at Kosipe on a flaked axe-adze without butt modification from level 2 (White et al. 1970:Fig. 3f), around 9000 BP. White's excavations at the Kafiavana rockshelter near Goroka in Eastern Highlands Province recovered flaked- and ground-axe fragments to the very bottom of the site (White 1972:95), dated to around 10,000 BP (White with O'Connell 1982:67).

Nombe - the oldest occurrence

As the situation stands, there is only one definite case of edge grinding in New Guinea at all commensurate in age with the well-known examples from northern Australia shortly to be discussed. This is the axe without butt modification found, as noted previously, in association with a tanged axe in the earliest occupation at Nombe at ca. 25,000 BP (Mountain 1983:94; White with O'Connell 1982:67 footnote, where the date quoted is 'about 26,000 years ago'). Mountain (1991:63) notes that it shows deep striations at right angles to the cutting edge.

Australia

Edge-ground axes, or, much more commonly, flakes from them recognisable by the evidence of grinding and/or the special nature of the raw material, have been found in Pleistocene and later contexts across northern Australia from west Kimberley to southeast Cape York Peninsula.

Axe flakes

At Widgingarri Shelter 1 on the west Kimberley coast axe flakes were excavated in spit 17 dated to around 28,000 BP (O'Connor 1999:75) and their stratigraphic distribution at Shelter 2 can be seen in O'Connor's Table 5.8. At the now-drowned Miriwun shelter in the Ord Valley of east Kimberley an axe flake came from the lower part of a layer with a date around its middle of about 18,000 BP (Dortch 1977:121, Fig. 4; see also

Dortch and Roberts 1996:32), while a complete edge-ground axe with deeply pecked groove was found in a late Holocene level at the site (1977:121, Fig. 9.1; see also Dortch and Roberts 1996:32).

At Nauwalabila I in western Arnhem Land (Jones and Johnson 1985:216-7, Table 9.6) flakes with signs of grinding were found throughout the deposit to a depth of 1.5 m, perhaps corresponding to a date of 15,000–16,000 BP. However, the discovery further down of highly weathered and decomposed pieces of the porphyritic dolerite used for axes raised the possiblility of their presence 25,000–30,000 years ago by reference to the age-depth curve for the site (Jones and Johnson 1985:Fig. 9.12). Direct dating of the lower sediments subsequently by the technique of optically stimulated luminescence would push the estimate even further back in time (Roberts et al. 1993).

Allen (1989:Table 5) records the earliest presence of 'ground axes' (in what form is not specified) at the western Arnhem Land shelter of Ngarradj Warde Djobkeng in layers V-VI/VII, which are interpreted as falling within the period 7000 BP to 18,000 BP. Layer IV at Ngarradj is actually dated to around 8500 BP (Allen and Barton 1989:Table 4a) and all that we are able to say on the evidence presented is that the axe evidence there is older than this.

There are three sites near Laura in southeast Cape York Peninsula to be mentioned. Four fragments of edge-ground axe were found at Sandy Creek 2 in sand deposits with a basal age of around 15,000 years ago extrapolated from thermoluminescence and calibrated ^{14}C dates, while a fragment of an edge-ground axe came from basal rubble beneath (Morwood et al. 1995:83-5). Rosenfeld (et al. 1981:26) reports the discovery of small rock fragments with grinding marks throughout the deposit at the Early Man shelter, the lowest thought to date around 10,000 BP. At Mushroom Rock shelter, which the excavator discusses under the more prosaic label of L-1 (Wright 1971:136), there are ground flakes from axes to the base of the deposit (1971:138), which is nearly 4 m deep and dated at a depth of 2 m close to 7000 BP (1971:179).

Whole axes: Arnhem Land

Schrire's excavations of the mid-1960s at two rockshelters below the escarpment in western Arnhem Land are notable not only for the fact that she established the Pleistocene age of edge-ground axes there, but also because she found them whole and in quantity.

At Malangangerr five axes, and a possible unfinished sixth, were found in the basal sands (Schrire 1982:106, Fig. 27), where dates range between ca. 18,000 BP and 25,000 BP (1982:84). The five finished specimens have lengths of 8.1 cm, 8.2 cm, 10.9 cm, 11.0 cm and 13.0 cm (1982:262).

At Nawamoyn there are 11 finished axes and two probable unfinished ones from the shell midden of Level I (1982:133, Fig.36), the base of which is dated ca. 7000 BP (1982:118–9); two from Level II (1982:138, Fig. 39), a transitional zone of mixing between the Level I midden and the Level III sands; and seven definite and two possible examples from these sands (1982:142, Fig. 42), for which a date of around 21,500 BP is thought to fall late in their accumulation (1982:118). The 11 finished axes from Level 1 range in length from 7.5 cm to 13.7 cm (1982:263), with a mean of 9.7 cm. The two from Level II are 9.7 cm and 10.3 cm long (1982:264). The seven finished examples from Level III range from 7.2 cm to 10.4 cm in length (1982:264), with a mean of 8.34 cm.

Schrire also recovered intact axes from two sites beyond the escarpment with occupation in the Holocene, five from Jimeri I (1982:186, Fig.70) and 12 from Jimeri II (1982:213, 218, 224, Figs 87, 89, 94). The former range from 5.9 cm to 9.9 cm in length (1982:265), with a mean of 8.56 cm, the latter from 5.5 cm to 12.5 cm (1982:265–6), with a mean of 9.0 cm.

Some of the whole axes excavated by Schrire are provided with butt modification of the kinds that we have met with in New Guinea, with the exception of the tang. All but one of the examples in question come from the Pleistocene levels at Malagangerr and Nawamoyn, the exception being a stemmed axe of Holocene date at Nawamoyn (Schrire 1982:Fig. 36e), which Schrire (1982:133, 263) calls 'waisted'.

There are two grooved axes, one from Level IIIb at Malangangerr (1982:Fig. 27c) with a marked groove on one surface and both sides (1982:106, 262), the other from Level IIIb at Nawamoyn (1982:Fig. 42e), where the grooving is on one face and one side (1982:142, 264). A second axe from Malangangerr Level IIIb (1982:Fig. 27d) is described as 'waisted' because one side is 'indented' (1982:106) or 'grooved' (1982:262), but this may be stretching the term.

The other examples of Pleistocene butt modification come from Level III at Nawamoyn. There is a waisted axe from Level IIIa (1982:142, 264), with open waist (1982:Fig. 42a). There is also a stemmed axe (1982:Fig. 42c), which is smaller and thinner than other axes. These characteristics, and the narrowness of the butt compared to that of the cutting edge, link it with two other specimens from the same stratum, Level IIIb (1982:142), which, however, cannot really be called stemmed, as Schrire (1982:264) implicitly appreciates, because their outlines from blade to butt are convex, not concave.

Schrire (1982:137, 142) says that 10 of the axes in Level 1 at Nawamoyn and four of those in Level III could have been cached for subsequent recovery because they were found in squares of the west excavation close to the shelter wall, meaning I0-2 and J1-2 (1982:Fig. 29). This is a high proportion of the total axes recovered, 13 in Level 1 (including two that are only probably axes, both of them found in the relevant squares) and nine in Level III (including two that are only possibly axes).

Although Schrire does not make the point, the implication is that the axes in question could be intrusions from higher levels, so that specimens found in the Pleistocene sands might in fact be Holocene in age. At Nawamoyn this would implicate the waisted axe and the grooved axe mentioned in previous discussion (J1/6-5 and J1/7-8 respectively of Schrire 1982:264). On the other hand, the waisted? axe and the grooved axe from Malangangerr (5A/8-452 and 2A/7-428 respectively of Schrire 1982:262) were recovered from the Pleistocene levels in squares some 5 m from the nearest shelter wall (1982:Fig. 15).

Whole axes: Cape York Peninsula

The only other intact edge-ground axe for which a Pleistocene age has been claimed is from Sandy Creek 1 Rockshelter, southeast Cape York Peninsula (Morwood and Trezise 1989:77-81). The excavations which recovered the axe were carried out more than 20 years earlier than the re-investigation of the site by Morwood that provided a minimum ^{14}C age of 32,000 BP for its alleged findspot. In the intervening period the axe, together with most of the other finds, had been lost. In the light of these circumstances Sutton (1990) questioned the validity of the claims made for the Sandy Creek 1 axe. This elicited a convincing reply from Morwood (1990), which provided further information from the records of the original excavation to back up his statements about the existence, provenance and characteristics of the axe.

The specimen is described as having been both waisted and grooved (Morwood and Trezise1989:Fig. 3; Morwood et al. 1995:Fig. 6.17f). It is a squat piece, 8.7 cm long (Morwood and Trezise 1989:Fig. 3 caption).

HAFTING

Australia

There is no reason to think otherwise than that the stone axe heads excavated in Pleistocene contexts in northern Australia were provided with what Dickson (1981:55–60) calls the wrap-around handles described by McCarthy et al. (1946:46) as the 'universal' method of hafting their Australian ethnographic counterparts. These were strips of split vine or cane, bark or wood, bent around the stone head and held in place by binding, adhesives or a combination of the two. Dickson (1981:60–5) discusses various additional methods that were employed to help the bonding of head and handle on Australian implements: the natural or artificial tapering of the stone head in breadth and/or thickness towards of the butt; 'shouldering', which is the equivalent of my stemming; waisting, said to be uncommon but exemplified in Dickson's Plate XI; and grooving, more frequent in the eastern part of the country from Cape York to Victoria than elsewhere and described (Dickson 1981:150) as 'the most secure of all the mechanical methods of fastening a handle to a hatchet head.' All of these devices are known in Pleistocene levels in northern Australia, as we have seen, but they do not occur on all axe/hatchet heads there.

Mainland New Guinea

Since the same mechanical devices as in the Australian case are present in Pleistocene contexts in New Guinea, we can suggest (as do Bulmer 1977:58 and Lampert 1983:151) that wrap-around hafting was employed there too. If so, the practice did not survive in mainland New Guinea. The butt-modified implements with which it was associated ceased to exist and different techniques came to be used for ground-stone axe heads, involving their mounting, directly or indirectly by means of a socket, either in a perforated wooden handle or, more commonly, against the front of an elbow-shaped wooden helve (Crosby 1977:87, Fig. 4).

Near Oceania

If wrap-around hafting did not survive in mainland New Guinea, it did so in the Bismarks and the Solomons (cf. Groube 1986:172). Here it was seen in use a hundred years ago by the German trader Parkinson, in New Britain at the Witu (formerly French) Islands, the Willaumez Peninsula and the Kandrian region of the south coast (Parkinson 1907:239, 1999:102–3) and in Buka and Bougainville of the northern Solomons (Parkinson1907:498–500, Fig. 81, 1999:217–8, Fig. 81).

The lashing on the New Britain blades was held in place by a sort of waisting below an expanded butt, illustrated by Parkinson (1907:Fig. 39, 1999:Fig. 39) with an example from the Willaumez Peninsula. Riesenfeld (1955:Fig. 89) shows a corresponding example from the Witu Islands. A Solomon Islands axe shaped on the same principle, though the butt here might be called a tang, is from Vella Lavella, described by Rolston (1945), who speculates (1945:121) that it might have been hafted like the Australian Aboriginal axe.

Parkinson (1907:498–9, Fig. 80, 1999:217–8, Fig. 80) discusses the hafting of axes in north and south Bougainville, on which there is grooving to retain the rattan binding. Golson (1968:7, Fig.1.5) illustrates an example with grooving from a collection made by Ann Chowning and Jane Goodale (1966) during ethnographic fieldwork in inland south central New Britain in the early 1960s.

In contrast to the New Guinea mainland, there are numerous finds of ground stone axes from New Britain and the north Solomons that exhibit not only hafting aids like waists and grooves as discussed above but others like lugs, used alone or in a variety of combinations with the others, with regional differences. Though his emphasis is on Buka and north Bougainville, the fullest treatment is by Jim Specht (1969a:271–8, 1969b:Figs XII.10-13), where eight types are provisionally recognised, in addition to one for axes with no butt modification. Examples illustrating the range can be found in Riesenfeld (1955:Figs 85–9) and Golson 1968 (Fig. 1.5-7). With specific reference to the northern Solomons, Specht (1969a:272–3) emphasises that the characteristic tool forms of the area are axes and that he knows of no adze hafting there.

A narrower range of forms of butt modification is known on flaked stone implements, of chert in south central New Britain inland of Kandrian and of obsidian at the central north coast around the Willaumez Peninsula. At Yombon in the south Christina Pavlides (1993:56) found butt-modified tools between two tephras, WK-1 and WK-2, dating to around 5900 and 3600 years ago respectively (Torrence et al. 2000:Table 3). At Talasea in the north their obsidian counterparts made their appearance before WK-1, but they all disappeared, chert and obsidian alike, following the WK-2 eruption (Torrence et al. 2000:235).

Formally, the butt-modified chert tools of the south are closer to the mainland New Guinea examples that I have reviewed than the obsidian tools of the north. Waisted, stemmed and tanged specimens, well-known from the Chowning and Goodale collection (Golson 1968:7, Fig. 1.3–4, 1971:133, Fig. 2J-M), have all been recovered in archaeological context by Pavlides (waisted, 1993:56, Fig. 3C; stemmed, 1993:56, Fig. 3B, where the stem is said to be broken, also figured Pavlides and Gosden 1994:Fig. 3B and Torrence et al. 2000:Fig. 7B; and tanged, Pavlides 1999:Plates 9.19–20, 31–2, 33–4). Richard Fullagar thinks that the wear on the modified butts of these tools is consistent with their hafting in handles of rattan or bamboo (Pavlides 1999:297).

Fullagar (1993:334-5) is of a similar opinion about their obsidian counterparts from the Talasea region of the central north coast of New Britain. Araho (1996:26) recognises two main types. Type 1, made on blades, is provided with a variety of true tangs at one end (Torrence et al. 1990:460 bottom row, second and third from left, both snapped across the blade; see also Casey 1939:148–9, Fig. 7a). Type 2, made on a large flake, has a long curved cutting edge and a projection for hafting in the form either of a tang or of stemming (Torrence et al. 1990:460 top and bottom left, the piece at bottom left also illustrated as Torrence et al. 2000:Fig. 7A).

Examples of both types have moved far from home, into other islands of Near Oceania and to the New Guinea mainland (e.g. Swadling et al. 1988:19-20, Figs 50 and 52, Types 2 and 1 respectively, for the Sepik). These occurrences, as well as mainland examples of Type 2-like objects not made in obsidian, are a subject of attention in the forthcoming published version of a paper delivered at the November 2000 conference on *Papuan Pasts* held at Australian National University.

I conclude this inevitably patchy review of butt modification in Near Oceania with what are likely to be its oldest examples, the surface finds of waisted axes from its eastern margin in the Solomons. The illustrated specimens (Rukia 1989:Figs 5a, 5c, 5d) have the proportions of the Bobongara artefacts, as Groube claimed (1986:172), and all three are grooved, in the manner of the more than 40,000 year-old Bobongara example. By the argument developed in this section, the Solomons waisted tools, grooved or not, would have been hafted as axes with wrap-around bindings of rattan or the like. The association of this style of hafting with butt modification of other kinds is indicated by the results of use wear and residue analysis on mid-Holocene chert and obsidian artefacts from New Britain. These other forms of butt modification, as well as grooving and new features like lugs, appear, alone and in combination, on a wide range of undated axes of ground stone. The use of wrap-around handles with these axes, strongly suggested by the previous evidence, is directly attested by the historical and ethnographic record.

THE RELATIONSHIP OF EDGE GRINDING AND BUTT MODIFICATION

I have noted previously that while edge grinding and similar forms of butt modification occur on early axe-like implements in New Guinea and Australia, there are significant differences in the distributions and associations of the traits in the two areas. Nowhere is this clearer than in the case of their combination.

Butt-modifying devices like waisting, stemming and grooving have been described for a number of edge-ground axes of Pleistocene age in northern Australia. By comparison, edge grinding is a much less noticeable feature both in Pleistocene contexts in New Guinea and on New Guinea implements with modified butts in general. With one exception, all examples of butt-modified tools in the New Guinea area with definite edge grinding and reliable dates belong to the mid-Holocene, the exception being a terminal Pleistocene waisted axe from Pamwak rockshelter on Manus Island (Fredericksen et al. 1993:148; Fredericksen 1994:Plate 7 top left).

On the mainland, edge grinding is known on tools without modified butt around the Pleistocene to Holocene transition at Kosipe and Kafiavana. The oldest edge-ground axe from New Guinea is the ca. 25,000 BP specimen with edge wear and no butt modification from Nombe, which was found with a tanged axe without edge grinding. As indicated by Mountain (1991:63), this association suggests the presence of two different and complementary types of tool. These would be a) one with butt modification, no edge grinding and typically a bluntish edge, b) the other with edge grinding but not necessarily butt modification.

Some support might be lent to this view by the fact that, in general, type a) tools, characteristic of New Guinea, would have operated in a rainforest environment, those of type b), charactersitic of northern Australia, in open forest or woodland. Groube (1989:297-300) has suggested a role for waisted axes in rainforest management, opening up the dense canopy by thinning, trimming and ringbarking to facilitate the growth of useful plants, with the site of Kosipe providing evidence of the association of butt-modified and unmodified axes with forest disturbance of an expectable scale and type (Hope and Golson 1995:822-3). The edge-ground blade, we may suppose, formed part of a cutting tool that was the counterpart of the multipurpose hatchet of the ethnographic period, characterised by versatility, portability and durability (Dickson 1981:6–9).

Obviously the waisted axes of Kangaroo Island in the temperate south of the Australian continent need a different explanation from that offered above for New Guinea tools of the same general class, and one that would also account for their isolated occurrence, but I am unable to offer one. However, it might be possible to explain the Mackay artefacts, of unknown age, in Groube's terms, as tools of the rainforest edge, since that is where they are found today (Lampert 1983:148). On the other hand, Cosgrove's report (1996:905) of the discovery of many thousands of edge-ground axes in the northeast Queensland rainforest seems to run counter to the argument being developed here. They are said to be found in concentrations and likely to have been associated with the clearing and maintenance of large open spaces for campsites and ceremonial grounds, such as are described in the ethnohistorical and ethnographic literature. They are thus the mark of a rainforest adaptation different from that proposed for the Pleistocene period in New Guinea and one that is probably quite late. There is no evidence for occupation of the Queensland rainforest before about 5000 BP (Horsfall 1996:178), though there has not been much archaeological attention paid to the question. In any case, there are doubts as to the likelihood of any continuous occupation of it from the Pleistocene (Horsfall 1996:175), given the restricted and fragmented distribution inferred for it during the last glacial period and its apparent penetration by sclerophyll vegetation (Kershaw 1994:409–10).

Both flaked and edge-ground axe fragments were found to the base of the Highlands shelter of Kafiavana around 10,000 BP, the first record of edge grinding on the mainland after the ca. 25,000 BP instance at Nombe. Evidence of increased erosion at Kuk swamp in the Wahgi Valley attributed to vegetation disturbance has led to claims for the entry of a new subsistence mode into the Highlands, that of shifting agriculture, as temperatures achieved modern levels at the beginning of the Holocene (Hope and Golson 1995:824–5). Such a development might give a context for the Kafiavana edge-ground tools, though hardly for the early Holocene edge-ground axe from Kosipe, ca. 600 m higher in altitude.

In this section I have been developing an argument that differences in the deployment in New Guinea and tropical Australia of shared devices relating to stone axes and their hafting may have resulted from the different requirements of rainforest and savanna situations. Were this the case, it might be proposed that they are witness to the regional differentiation of an ancestral culture. If so, that ancestral culture would appear to have excluded Australia south of the tropics (cf. Jones and Johnson 1985:217).

A CULTURE AREA ACROSS THE SAHUL SHELF?

Without doubt, the most striking feature of the various distributions of hafting devices and edge grinding under review is the restriction of edge grinding in Australia during the Pleistocene and early Holocene to the north and its appearance in the rest of the continent only after about 4500 years ago. Morwood and Trezise (1989:83–4, Fig.1; Morwood and Hobbs 1995b:747, Fig. 1) map the situation and discuss various 'resource-oriented' explanations for it (the quotation is from White with O'Connell 1982:67; see also Dickson 1981:7, citing Tindale), but Mulvaney and Kamminga (1999:221) remain perplexed.

Perhaps the explanation lies as much in the realm of culture as in that of resources. The Pleistocene and early Holocene edge-ground axes of northern Australia have an interesting distribution in this respect (see Fig. 2b). On the one hand they show a considerable overlap with the complex figurative rock art styles of the Pilbara, the Kimberley, Arnhem Land and southeast Cape York Peninsula (Morwood and Trezise 1989:84; Morwood and Hobbs 1995b:747), some with claims to go back into the Pleistocene (Morwood and Hobbs 1995b:754; Taçon and Brockwell 1995:684; Lewis 1997:15; on recent advances in rock art dating in northern Australia see Watchman and Jones 1998; Watchman this volume; O'Connor and Fankhauser this volume). On the other hand they substantially overlap the distribution of the so-called non-Pama-Nyungan language families of the Kimberley, Arnhem Land and the Gulf of Carpentaria, which give those areas a higher level of linguistic complexity than the single language family, Pama-Nyungan, does for the other seven-eighths of the continent (Morwood and Trezise 1989:84; Morwood and Hobbs 1995b:Fig. 1).

For Morwood and Trezise (1989:84) the suggested association between technology, art and language supports the idea of 'an interaction sphere across northern Australia during the Pleistocene and early Holocene, from which the rest of the continent may have been effectively isolated.' Lewis (1997:1, Fig. 1) argues in particular that stylistic similarities between Arnhem Land and Kimberley rock paintings suggest that these two regions were part of a late Pleistocene and early Holocene 'information network', physically linked at times of low sea level across the Bonaparte Gulf. The question in the present context is to what extent such an interaction sphere or information network might have included New Guinea.

There would have been no physical or ecological barriers. The flat bed of the Arafura Sea, in the main between 40 m and 80 m deep and slanting westwards to a shelf edge between 120 m and 200 m below present sea level (Galloway and Löffler 1972:21), was exposed to varying extents between the end of the last interglacial about 120,000 years ago and the formation of Torres Strait (Chappell et al. 1996:Figs 1 Lower and 4). Over the critical period for human entry and early settlement, say from 75,000 to 25,000 years ago, sea level oscillated between 50 m and 90 m below present. Oscillations of the scale and frequency depicted in the Chappell et al. graphs (1996:Figs 1 and 4) would, of course, have been a cause of instability at the margins of the shelf. More importantly, they could be a pointer to marked fluctuations in climate and vegetation of the kind that characterise the late Pleistocene in the northern hemisphere, as discussed by John Chappell (this volume).

At the height of the last glaciation, when sea level fell to 120–130 m below its present level and associated aridity was pronounced (van der Kaars 1991:295), a savanna-like environment was in existence in the Carpentaria Basin towards the eastern margin of the landbridge (Torgersen et al. 1988:21). Close to its western margin, the archaeological site of Liang Lemdubu in what are today the Aru Islands (Veth et al. 1998; Veth, Spriggs et al. 1998) has provided similar information. Now in dense rainforest with 5000 mm annual rainfall (Veth et al. 1998:80), the cave has produced a 'phenomenal quantity of land fauna', with the agile wallaby, *Macropus agilis*, and the bandicoot *Isoodon* indicating the presence of extensive savanna and the forest-edge pademelon, *Thylogale brunii*, that of gallery forests along waterways (Veth et al. 1998:78). The period of this exploitation extended from ca. 26,000 years ago (Veth et al. 1998:78) until terminated by the advance of the rainforest in the early Holocene (O'Connor pers. comm. 2000).

There are other interesting features of the Pleistocene levels at Liang Lemdubu. The stone industry is percussion flake-based and generalised in nature, with some flakes modified by retouch or utilisation. The technology is said to be superficially very similar to that of Pleistocene sites in northern Australia (Veth et al. 1998:79: Veth, Spriggs et al. 1998:173–4), though there is no report of edge-ground axes, such as are well-represented there. In addition, shell of the robust mangrove mudwhelk *Geloina coaxans* was occasionally found, presumed to have been traded in from the coast (Veth et al. 1998:79), which may have been as much as 40 km distant at the time (Veth et al. 1998:80).

Geloina shell was present in levels dating between ca. 24,000 BP and 26,500 BP at Koolan Shelter 2, at the present northwest Kimberley coast, when the sea was less than 20 km away, and there was a piece of pearl shell (*Pinctada* sp.) in one of the same levels (O'Connor 1999:27, 30). This combination O'Connor (1999:30, 39–40, 121) sees as suggesting that the shells did not come into the site as dietary items, but *Geloina* as an artefact and pearl shell as an exchange valuable. In Holocene levels at Koolan 2 there are *Geloina* shells with edge damage showing their use as scrapers (1999:36, Table 4.11), but the Pleistocene fragments are too weathered for this to be established in their case (1999:121). The manufacture of pearl shell ornaments on the Kimberley coast in ethnographic times and their extensive distribution in the course of exchange are well known (McCarthy 1939a:435, 1939b:96-8; Mulvaney and Kamminga 1999:97). At another coastal northwest Kimberley shelter, Widgingarri 1, pearl shell was found around the time of the last glacial maximum, when the sea was up to 200 km and more away, while baler shell (*Melo* sp.), ethnographically also an object of wide exchange (Mulvaney and Kamminga 1999:97), came from a level dating around 28,000 BP when it was some 70 km distant (O'Connor 1999:59–60, 61, 121). The story is repeated further west along the Western Australian coast, where Noala Cave at the Monte Bello Islands, now ca. 100 km from the Pilbara coast, has produced a *Geloina* valve from the earliest level of occupation at 27,000 BP, when the sea was ca. 35 km distant (O'Connor and Veth 2000:113).

For O'Connor (1999:119–21), the evidence of the shells lends support to the concept of a 'culture area' suggested for the wider northern Australian region by the character of the stone industry and the presence of edge grinding on axes.

Perhaps the occurrence of imported *Geloina* shells with a similar stone industry in the Pleistocene levels at Liang Lemdubu marks its presence on the Sahul Shelf, despite the absence of the edge-ground component, which, of course, is not universal in the Pleistocene levels of northern Australian sites.

The argument that I am developing carries with it an expectation that the early artistic activity of northern Australia which I have referred to above might also be represented in New Guinea. The only suggestion that I know of in this connection has been made by George Chaloupka, the leading student of Arnhem Land rock art, in respect of sites on the Vogelkop of west New Guinea, at the MacCluer Gulf and in the Kaimana area further south, both of which he has visited. Chaloupka (1993:246) talks of stencils of acutely angled boomerangs, hand stencils and x-ray paintings at the MacCluer Gulf sites and of a body of rock art in the Kaimana area in whose complexity he sees some kinship with the art of the Arnhem Land plateau.

These are tentative suggestions about what is for the most part a poorly known situation. The MacCluer Gulf is an exception by virtue of the exemplary record made of rock art there by a Frobenius Institute expedition of 1937–8 and the comprehensiveness of its subsequent publication by Röder (1959). In what follows I rely on Rosenfeld's (1988:121–3) discussion of the work. The paintings occur along a 30 km stretch of the southern shore of the Gulf and their location, together with the frequent depictions of fish and ship motifs, points to a marked marine orientation on the part of their authors. Röder recognised four successive styles of red paintings on the basis of superimpositions, followed by a phase of black paintings. He dated the second style to the late Southeast Asian bronze age because of its inclusion of certain decorative designs, and this meant for him around 1000 years ago. In the light of Röder's suggestion that stencilling was exclusively a technique of the first style, Rosenfeld puts the entire art complex at no earlier than the second half of the third millennium BP since there are stencils of artefacts resembling Dongson bronze axes (Rosenfeld 1988:123). However, in an unpublished paper of 1991, Chaloupka says that on the basis of his field observations the stencilling technique was clearly used in west New Guinea, as in Arnhem Land, throughout the rock art sequence.

Ballard's (1992) contribution to the subject is compatible with Rosenfeld's conclusions. He presents evidence suggesting the possibility of identifying a locationally distinctive class of rock art site in the wider New Guinea region, situated in areas associated with Austronesian-speaking settlement and dated as late as 2000 BP by motifs shared with bronze artefacts and pottery, and he says of this distinctive class that the painted sites of the Vogelkop and especially the MacCluer Gulf represent its 'clearest expressions and greatest concentrations' (Ballard 1992:98). Ballard goes on to say that the validation of the case he has argued on largely locational grounds requires the comparison of the elements of the art itself, something not possible for many of the sites in his catalogue.

This is where Chaloupka's identification of such elements in the form of stencils of 'acutely angled boomerangs' (Chaloupka 1993:246) or 'angled boomerang-like objects' (Chaloupka 1994:107) becomes potentially important, particularly since he mentions them in the context of Röder's oldest painting style (Chaloupka 1994:107; see also Chaloupka 1991). In his 1991 paper Chaloupka says that it was the presence of boomerang-shaped objects in Röder's publication that first suggested to him the possibility of Australian-New Guinea contacts and motivated his visit to the Bird's Head. He notes (and see Chaloupka 1993:246) that boomerangs were not used by, or known to, the local populations of the area, a situation which, as he points out, parallels that in both Arnhem Land and the Kimberley. Here boomerangs are absent ethnographically except as imported items (see also Evans and Jones 1997:40–3 for Arnhem Land and McCarthy 1939a:435, 1939b:81 for the Kimberley), though associated in early rock art with the Dynamic figures of Arnhem Land and the Bradshaw figures of the Kimberley. This suggests to Chaloupka (1991) that the antiquity of some of the rock art images on the Bird's Head is greater than that proposed by Röder and Rosenfeld for the MacCluer Gulf, though this particular complex at least does not appear to him to go back as far as the art of Arnhem Land.

Further in the matter of Australian-New Guinea relations, Chaloupka (1993:110) says that while the items of attire of the male figures of his Arnhem Land Dynamic Style, particularly the pubic apron and bustle, have no ethnographic parallels in Australia, they are very similar to the traditional dress of adult males among the Asmat of southwest New Guinea and elsewhere. Welch (1993:29) refers to ethnographic parallels in New Guinea as well as Australia when discussing ornamentation in the attire of the Bradshaw figures of Kimberley art.

There is a final point to be made about the boomerang. Its 'gradual disappearance' from Arnhem Land art is thought by Taçon and Brockwell (1995:689) to reflect a vegetation shift under the ameliorating climate at the end of the Pleistocene from more open to more forested country, where the weapon would have been less effective. This would have been even more the case at the MacCluer Gulf, had the boomerang been present at the appropriate time. The vegetation reconstructed there for the last glacial maximum around 18,000 BP is open forest and woodland, which would have disappeared by the early Holocene with the advance of the closed forest that characterises the region today (van der Kaars 1991:295, 299, Figs 2 and 20). Further south, as we have seen, the faunal evidence at Liang Lemdubu, in what was to become the Aru Islands, shows large areas of savanna to have been present by 26,000 years ago, but to have disappeared by the early Holocene.

What I am suggesting on the basis of the evidence from the western end of New Guinea is that the proposed interaction sphere (Morwood and colleagues) or information network (Lewis) or culture province (O'Connor) in northern Australia in the late Pleistocene would have incorporated the Sahul Shelf and extended into southern regions of New Guinea as the savanna moved northwards in the lead-up to the last glacial maximum and remained there in its immediate aftermath. There is no doubt that this extension would have included the great southwards bulge of the island occupied by the lowlands across which the Fly and the Digul flow, whose southern part today is characterised by savanna reminiscent of northern Australia (Paijmans et al. 1971:12, Fig. 2). Swadling and Hope (1992:21–2, Fig. 3.3) suggest that in this region rainforest gave way to savanna up to about the present 3000 isohyet ca. 25,000 BP (see my Fig. 2a inset), but then, under conditions of post-glacial maximum warming, moved back to its present position (Hope 1996:Fig. 2) after ca. 12,000 BP. There is no archaeological evidence directly related to this situation, but it is likely that settlement here during times of late glacial dryness was favoured by the presence of perennial rivers running down from the central ranges.

THE LAST ACT

While savanna was being replaced by rainforest in this way in southern New Guinea, the shelf behind was being inundated by the emerging Arafura Sea. According to O'Connor et al. (in press), the elevated land that became the Aru Islands separated from Australia ca. 14,000 years ago and from New Guinea ca. 11,500 years ago. Working in ^{14}C years because the calibration curve did not then extend back far enough in time, Torgersen and colleagues (1988:259–60) estimated that a permanent connection was established between the late Pleistocene lake in the Carpentaria Basin and the rising sea to the west ca. 12,000 BP, or roughly 13,500 calendar years ago, while the waters of the Coral Sea began to cross the higher land at Torres Strait from the east ca. 8000 BP, or about 8500 calendar years ago. Because of the low gradients on the Sahul Shelf the loss of land would at times have been marked: Galloway and Löffler (1972:24–5) suggest that often the shoreline must have retreated by as much as a metre a day. The effects on plant and animal distributions and on human fortunes must have been dramatic.

It seems likely that circumstances would have been more straitened on the New Guinea side where populations were caught in an ever narrowing corridor between advancing rainforest and rising sea. On the Australian side the loss of land should to an extent have been offset by the increasing carrying capacity of the fall-back country to the south as climate improved after the stress of the last glacial maximum (van der Kaars 1991:295, Figs 2, 5, 20, 22, giving palynological support to the modelling of Nix and Kalma 1972:83–90, Figs 5.9–12 and Jones and Bowler 1980:8–11, Figs 4–6). A growing body of archaeological information about sites and their settlement histories in the tropical north has encouraged the raising of questions of this kind (see regional reviews by Morwood and Hobbs 1995b, mainly for southeast Cape York Peninsula, Taçon and Brockwell 1995 for Arnhem Land and Veth 1995 for northwest Australia, together with O'Connor 1999 for west Kimberley). However, there are gross deficiencies in the available evidence, due to the erosion and burial of early sites and the lack of survival of faunal remains older than 7000 BP in Arnhem Land (Allen 1989:107), the almost complete disappearance of plant and animal remains from sites in southeast Cape York Peninsula (Morwood and Hobbs 1995a:180) and problems of stratigraphic resolution bearing on continuities and discontinuities in rockshelter occupation generally (O'Connor et al. 1999).

We have no archaeological evidence at all to track developments on the New Guinea side after the Arafura Sea cut across the great plain that had stretched from tropical Australia to the foothills of the New Guinea cordillera, putting an end to the culture province or interaction sphere or information network to which I have argued that it had belonged. Effectively the water barrier must have been in place by the end of the Pleistocene and at its full extent across Torres Strait a couple of thousand years later. At some subsequent stage the practice of horticulture spread south, eventually meeting the extensive coastal and lowland swamps that developed as a result of the marine transgression that formed the Arafura Sea. The chronology of this development is unknown, but we may expect the process to have been as complex and protracted as the parallel one that has been investigated on the coastal plains of the Alligator Rivers region of northern Australia (Hope et al. 1985; Woodroffe et al. 1986).

In the New Guinea swamplands the staple is sago, supplemented by standard New Guinea field and tree crops like yam, taro, sugarcane, banana and coconut, with gardening the main subsistence activity only where sago does not grow wild (Saggers and Gray 1982–3:108–10). Swadling and Hope (1992:19) call the absence of sago from northern Australia an enigma. Indeed it appears to be so, given the land connection provided by the Sahul Shelf and the social networks that I have argued were established across it, the first in theory making possible the palm's natural expansion along waterways, the second its human transfer. Perhaps circumstances on the Australian side were not ecologically favourable for its establishment, when opportunity was there. Rhys Jones has strongly argued this in the case of horticulture, citing the constraints of the savanna zone, with its long dry season and poor laterised soils. For him the Arafura Sea is the southern frontier of the Indo-Malayan horticultural province as present in New Guinea (Jones 1980:138–42; Jones and Bowler 1980:22–3; Jones and Meehan 1989:130–2).

SALUTATION

Put another way, the Arafura Sea is the northern frontier of the continent of hunter-gatherers which Rhys chose for his field of study and to which he has devoted his professional life, in its own right and for the light it is able to shed on wider issues in human culture and history.

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William Blandowski's Fish: An Ethnohistorical Account near the Junction of the Murray and Darling Rivers

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I FIRST heard of Blandowski through reading Krefft's (1866a, 1866b) papers on the fauna and Aboriginal customs of the country between the Darling and Murray Rivers. This followed my introduction to the Willandra Lakes in 1969, in the company of John Mulvaney, Jim Bowler, Con Key, and Rhys Jones amongst the party of soil scientists examining Bowler's claims that the Willandra represented a fully Pleistocene lacustrine landscape. It was on this trip that certain 'kangaroo bones', heavily encrusted in calcrete, turned out to have unmistakably human features. A party consisting of Rhys, Jim Bowler, Betty Meehan and myself returned to map the location of the Mungo 1 skeleton and to collect the stone artefacts and burnt faunal remains which dotted the shoreline of Pleistocene Lake Mungo. John Calaby identified the fauna at Lake Mungo and mentioned that some of the species we recovered were presently extinct but had last been recorded in the area by Krefft in 1857.

William Blandowski was the leader of an expedition to the junction of the Darling and Murray Rivers from December 1856 to December 1857. This scientific expedition was set up by the Government of Victoria at the prompting of the Philosophical Institute of Victoria. Blandowski left the expedition's camp at Mondellimin, on the northern side of the river opposite present day Yelta, on August 6th 1857. Two weeks after his return, on September 2nd 1857, Blandowski presented a 'Preliminary Report on Recent Discoveries in Natural History of the Lower Murray' to the Philosophical Institute and the members of that society gave a complimentary dinner in his honour the following evening. Blandowski's moment of glory, however, was short lived. On the 16th of October, he was directed to recall the remaining members of his party from the field, and although the expedition was not formally disbanded until December 15th 1857, Blandowski was informed that the wages of the party and the allowance for his own expenses would cease on the 31st of October (Paszkowski 1967:153–7).

The reason for this abrupt change in circumstances is as follows. Between the 3rd of September and early in October, the officers of the Philosophical Institute had a chance to read Blandowski's preliminary report. They discovered that Blandowski had taken the opportunity to name a number of species of freshwater fish after members of the Council and they took exception to some of the descriptions he provided. The report was duly published as Volume 2 of the *Transactions of the Philosophical Institute of Victoria* (1857) minus the illustrations and descriptions of the fish, 'omitted by order of the Council'. The missing plates and pages (pages 131–4) exist in a single copy of Volume 2 of the '*Transactions*' held by the State Library of Victoria.

The episode has been noted as a colonial curiosity. The fish descriptions, the Council's umbrage and the ensuing Commission of Enquiry into Blandowski's appointment are discusssed in a number of published accounts (Hoare 1967:23–4, Iredale and Whitley 1932:94–96, Paszkowski 1967: 155–61, Pescott 1954:14–15). The collection itself, the identifications and descriptions made by Blandowski and the Aboriginal names he recorded, however, are of interest. They throw light, firstly, on Blandowski's abilities as a naturalist, and, secondly, on an Aboriginal taxonomy of the freshwater fish for an area where little information of this type has survived.

BLANDOWSKI'S FISH

In order to draw the information together and to allow for further discussion, the species of fish and Blandowski's identifications are listed below. The commonly accepted species names, in their modern form, are taken from Iredale and Whitley (1932:95–6) and Lake (1978). The order in which they are presented below follows that of the original manuscript and the accompanying illustrations (see Plate 1). I have retained Blandowski's spellings and usage, e.g. 'billybongs' or 'Billibongs', but have included metric conversions of measurements.

1. *Tandanus tandanus* (Mitchell), identified by Blandowski as *Plotosus tandanus*. Eel-tailed catfish. Now a vulnerable species.

Blandowski's description: An olive-green coloured fish, with 8 long feelers round its mouth. An Asiatic form of fish, which lives here in the Murray and in Billibongs. It grows to the size of two feet (61 cm), weighing from 7 to 8lbs (3.2 kg to 3.6 kg). It lives principally on very small shells, and muddy spots are its favorite places of abode. It is not scaled.

2. *Nematolosa erebi*. Identified by Blandowski as *Megalope caillentassart* and by Iredale and Whitley as *Nematalosa richardsonii* (Castelnau). Bony bream, sometimes called hairback herring.

Blandowski's description: A fish found in the neighbourhood of Boston (?). It is of a silvery colour, and has on the back, behind the dorsal fin, a very elongated and elastic backray. It leaps frequently out of the water. It is most numerous in the Darling, but is also found above and below the junction of the Murray and Darling Rivers. It is remarkable that this fish contains an uncommon quantity of small soft bones. It grows only from 10 to 14 inches (25 cm to 36 cm).

3. *Bidyanus bidyanus (Mitchell)*, identified by Blandowski as *Cernua bidyanus*. Silver perch, also called black or silver bream. Iredale and Whitley identify the fish as a 'young' specimen.

Blandowski's description: Sir Thomas Mitchell has already given a good drawing of this fish. It grows to about 18 inches (46 cm) in length.

4. *Bidyanus bidyanus (Mitchell)*, identified by Blandowski as *Cernua eadesii*. Silver perch. Iredale and Whitley identify the fish as 'adult'.

Blandowski's description: A fish easily recognised by its low forehead, big belly and sharp spine.

5. *Bidyanus bidyanus (Mitchell)*, identified by Blandowski as *Cernua nicholsonia*. Silver perch. Iredale and Whitley identify the fish as 'half-grown'.

Blandowski's description: Lives on crawfish. Fishes 3, 4 and 5 are all difficult to distinguish from each other. They live in the Murray and its Billybongs. It grows to a length of 14 inches (36 cm).

6. *Bidyanus bidyanus (Mitchell)*, identified by Blandowski as *Cernua ifflaensis*. Silver perch. Iredale and Whitley identify the fish as 'young'.

Blandowski's description: This is a little fish from two to three inches (5 cm to 8 cm) in length, and found only in the waters of the Billybongs. Colour dirty greenish; irregular dotted lines running over the upper part of the body; body silvery.

7. *Ambassis castelnaui* (Macleay), identified by Blandowski as *Cernua (?) wilkiensis*. Western chanda perch.

Iredale and Whitley (1932:95) suggested that this fish belonged to a new family for which they proposed the generic name *Blandowskiella*, Blandowski's perchlet. Endangered.

Blandowski's description: This fish is very small and lives in the Billybongs.

8. *Craterocephalus fluviatilis* (McCulloch), described and named by Blandowski as *Kohna mackennae*. Mitchellian freshwater hardyhead, Murray hardyhead. Now rare.

Blandowski's description: A fine little fish, which seldom grows to the length of three inches (8 cm).

9. *Retropinna semoni* (Weber), described and named by Blandowski as *Turruitja achenson*. Australian Smelt. Lake (1971:19) notes that *Retropinna* occurs as one of the three species making up 'whitebait'.

Blandowski's description: This fish is found in the Murray and adjacent Billybongs.

10. probably *Nematocentris fluviatilis*. Identified by Iredale and Whitley as *Melanotaenia nigrans* (Richardson), described and named by Blandowski as *Jerrina dobreensis*. Crimson spotted rainbowfish, freshwater sunfish. Still common.

Blandowski's description: This fish has a pinkish breast and dark greenish body, with twelve intense bluish stripes, running along the body, commencing a little beyond the middle and pointing towards the tail. The dorsal and ventral fins are of a yellow colour. The tail is orange. This fish seldom grows larger than 5 inches (13 cm). The Billybongs are the principal abode of this fish.

11. *Galaxias truttaceus* (Cuvier), described but not identified by Blandowski. Spotted mountain galaxias, whitebait. Not recorded for the lower Murray-Darling catchment (Lake 1971:20).

Blandowski's description: This little spotted trout is a delicious eating fish, and not only found in the Billybongs and the Murray River, but also observed by me in the Yarra Yarra, near Melbourne. It is nearly transparent, of a slightly greenish colour, with blackish spots of large size on the back, and of a smaller description on the belly. It becomes a fat, plump little fish, seldom larger than six or seven inches (15 cm to 18 cm).

12. *Galaxias maculatus*. Identified by Iredale and Whitley (1932:96) as *Austrocobitis attenuatus* (Jenyns). Common galaxias or jollytail, 'whitebait'. Named by Blandowski as *Uteranka irvingii*.

Blandowski's description: This long thin fish seldom grows larger than seven inches (18 cm). It is caught by the boys in the Yarra Yarra, in great numbers, and is considered a very fine eating fish, but appears to be rare in the Murray, and only serves to support the Murray Cod which principally feeds on it.

13. *Maccullochella peeli* (Cuvier and Valenc.), identified by Blandowski as *Gristes macquariensis*. Murray cod. Iredale and Whitley identify the fish as an 'adult' specimen. Vulnerable.

Blandowski's description: This fish is of a dirty green colour and has less spots than *Gristes peelii*. In both the scales are small and covered by an epidermis. Both are characteristic forms of the Murray River and its tributaries. They grow from 36 to 40 inches (91 cm to 102 cm) in length.

14. *Maccullochella peeli* (Cuvier and Valenc.), identified by Blandowski as *Gristes peelii*. Murray cod. Iredale and Whitley identify the fish as 'young'.

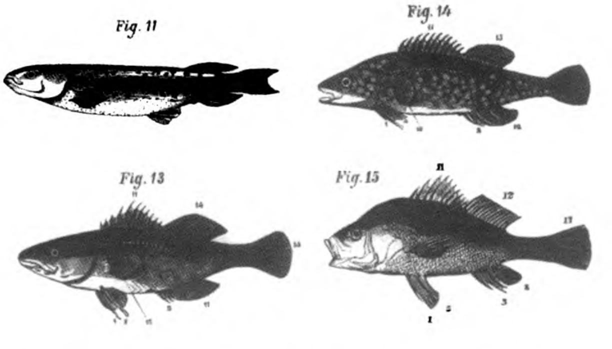

Figure 2. William Blandowski's fish. Fig.11. *Galaxias truttaceus*, Fig. 13. *Maccullochella peeli*, Fig. 14. *Maccullochella peeli*, Fig. 15. *Macquaria australasica*
After Blandowski 1857, plate iii.

Blandowski's description: This fish, as well as the preceding, No. 13, have both been already observed in America. I may as well allude here to Mr. Edward Wilson's experiment of transferring these fishes to the rivers joining the sea on the southern side of the range, and I believe them likely to prosper, if they find ample food in those rivers.

15. *Macquaria australasica* (Cuvier and Valenc.), identified by Blandowski as *Tilka wilsonia*. Macquarie perch. Now vulnerable.

Blandowski's description: A fish of middling size, grows from 14 to 18 inches (36 cm to 46 cm) and is finely scaled.

16. *Philypnodon grandiceps* (Castelnau), identified by Blandowski as *Collundera muelleriana*. Big-headed gudgeon.

Blandowski's description: This fish does not grow above three inches (8 cm), and lives principally in the Billybongs. It is of an olive-green colour, and has white eyes, and has large scales for its size.

17. *Hypseleotris klunzingeri* (Ogilby), drawn but not identified by Blandowski. Western carp-gudgeon. Of uncertain status today.

Blandowski's description: The smallest sized fish, which I have observed in the Australian waters. It lives in the Billybongs, and is only two inches (5 cm) in length and rarely grows larger.

18. *Mogurnda adspersa* (Castelnau), described and named by Blandowski as *Kurrina macadamia*. Southern purple spotted gudgeon. Now endangered.

Blandowski's description: A bluish-green small fish, with dark green stripes on the head and spotted with darker dots, particularly visible on the tail and fins. It lives principally on little crawfishes and takes its abode in the hollow banks of the Billybongs, there watching for its prey.

19. *Gadopsis marmoratus* (Richardson), named and described by Blandowski as *Brosmius bleasdalii*. River blackfish. Seriously threatened.

Blandowski's description: A slimy slippery fish. It lives in the mud and is of a violet bluish colour on the belly. The whole upper surface is of a dirty olivish-green colour, with numerous irregular dark patches. Principally found in Billybongs, but also found by me in the Yarra Yarra River. It grows to about 7 inches (18 cm) in length.

BLANDOWSKI'S FALL FROM GRACE

Not a great deal has been written about William Blandowski since 1970, though at that time he continued to have supporters who thought his poor reputation was undeserved. Paszkowski (1967) describes him as 'The First Government Zoologist of Victoria' while Iredale and Whitley (1932:93) comment:

> Blandowski was a man of parts, as, throughout, notwithstanding a little ego, there is considerable evidence of his great ability. The number and variety of animals collected, the time occupied, and the distance travelled are sufficient to stamp him as a successful worker.

Pescott (1954:20) compliments Blandowski on his far-seeing vision for the National Musuem of Victoria, and for his initiative and perseverance, in spite of many and varied forms of difficulties and opposition, in commencing the national collections. Finally, Hoare (1967:15–16) notes that commentators have been unable to agree about Blandowski's competence but adds that he belongs to that industrious group of German-speaking, and largely German trained scientists, who made such a contribution to science in the new colonies. He includes Blandowski in company along with Ludwig Becker, Gerard Krefft, Ferdinand Mueller and Georg Neumayer.

Paszkowski (1967:154) states that Krefft was 'without doubt prejudiced against Blandowski.' Krefft was certainly attempting to secure his own reputation as a zoologist and distancing himself from Blandowski would have been an advantage. Blandowski's blustering manner, however, laid himself open to ridicule. When he left the camp at the junction of the Murray and Darling Rivers, he claimed (1857:126) that he took with him:

> the valuable collection of specimens of Natural History, which had been accumulating in my tent during my stay at Mondellimin, to the extent of twenty-eight boxes and parcels, containing in all about 16,000 specimens, registered under 2,000 different numbers.

Krefft, however, noted (1856–7, 12th Feb, near Echuca) that some thousands of shells of the Orders Lymnea ? and Succinea ? were collected from the swamps, with each being accorded a different specimen number in order to make up the 20,000 specimens Blandowski claimed he would collect. The *Illustrated Journal of Australasia* (December 1857, no 18:244), in a description of the National Museum, confirms Krefft's opinion, noting:

> In this room also, are the 18,000 specimens of shells lately brought by Mr Blandowski from the Murray; and sadly they disappoint the naturalist who has been attracted by the published notices of them; for the whole number only includes about half a dozen species, and the contribution might be roughly described as a bushel of mussels and another of periwinkles.

The fish incident irreparably harmed Blandowski's reputation in Victoria. Hoare (1967:20) notes that, on one occasion, Ferdinand von Mueller had 'flattered the Institute with his proposal to dedicate some plants to certain of its members for their disinterested fostering of science.' He adds (1967:29, fn 87) that Wilkie and Macadam were so honoured. Blandowski, rather obsequiously, sought to repeat the exercise but it backfired badly. Table 1 lists the personages that Blandowski attempted to honour through his fish names.

Table 1. Proposed names for fish from the Murray River and the personages Blandowski was attempting to honour.

Species	Name Proposed	Named after	Comment
4. *B. bidyanus*	*Cernua eadesii*	Dr Richard Eades	Physician, Later Mayor of Melbourne.
5. *B. bidyanus*	*Cernua nicholsonia*	Mr Mark Nicholson	MLC, promoter of National Museum of Vic.
6. *B. bidyanus*	*Cernua ifflaesis*	Dr Solomon Iffla	Melbourne practitioner and magistrate.
7. *A. castelnaui*	*Cernua (?) wilkiensis*	Dr David Wilkie	Physician, promoter of clean water for Melbourne
8. *C. fluvialatilus*	*Kohna mackennae*		
9. *R. semoni*	*Turruitja achenson*	Frederick Achenson	An engineer, member of the Institute
10. *N. fluviatilus*	*Jerrina dobreen ensis*		
12. *G. maculatus*	*Uteranka irvingii*	Professor Martin Irving	Professor Classics at University of Melbourne.
15. *M. australasica*	*Tilka wilsonia*	Professor William Wilson	Vice-president of the Institute
16. *P. grandiceps*	*Collundera muelleriana*	Dr F. (later Baron) von Mueller	Botanist, Chairman of Philosophical Institute
18. *M. adspersa*	*Kurrina macadamia*	Dr John Macadam	Later secretary of the Royal Society
19. *G. marmoratus*	*Brosmius bleasdalii*	Rev Dr Bleasdale SJ	Vice-president of St Patrick's college.

It seems unlikely that Blandowski intended his fish descriptions as a gibe. To have done so would have put himself offside with the most powerful men of science in the colony, including those, such as Mark Nicholson, who had helped him secure employment at the National Museum. In any case, the entire committee of the Philosophical Institute had assisted him in his ambition to lead the expedition.

It is unfortunate that the most derogatory of the descriptions, and the great majority are quite innocuous, happened to be attached to Dr Eades (*Cernua eadesii*) is described as 'A fish easily recognised by its low forehead, big belly and sharp spine'- and the Reverend Bleasdale (*Brosmius bleasdalii*) is described as 'a slimy, slippery fish. Lives in the mud'. Blandowski had inadvertently made two formidable enemies. Blandowski claimed that Dr Eades had seen the illustrations and had pencilled his name beneath No 4., a claim which Eades denied 'upon his honour as a gentleman' (Paszkowski 1967:158).

The descriptions are accurate, even literal. Adult forms of silver perch do show a low forehead, while the river blackfish is also known by its common name 'slippery'(Lake 1978:65).

A sense of humour appears to have been missing from Dr Eades and Father Bleasdale as well as from Blandowski. Once Professor Wilson and the Reverend Bleasdale had resigned from the Institute over the issue then events took their course. In the debates which followed, Blandowski was unable to concede a mistake. Instead he claimed for himself the right to name species after any person he thought proper, and refused to amend the paper as submitted even though he admitted it was 'purely a preliminary one' (Paszkowski 1967:158).

The Philosophical Institute found itself the subject of ridicule. The *Argus* of 27th of March 1858, in a long and comic editorial, concluded 'who would ever thought about Dr. EADES'S stomach and forehead, in connection with the peculiarites of the unlucky fish, if his (the Doctor's) friends had not so agitated the matter, and insisted upon the resemblance.'

Even if, as seems likely, Blandowski contributed to his own downfall, he paid a steep price for this folly of fish naming. He left the colony, on the 17th March 1859, with his scientific career in tatters. This was an unfortunate outcome for an important scientific expedition. Apart from Blandowski's preliminary report to the Institute, and two articles by Gerard Krefft in Volume 1 of the *Transactions of the Philosophical Society of New South Wales* (1866a, 1866b) little was published at the time. Reassessments of the scientific contribution of the expedition have been made by Wakefield (1966) and Iredale and Whitley (1932). It remains the case, however, that the journals and illustrations of the expedition are unpublished and scattered between Australia and Germany. The collections and observations, however, continue to be significant as much has changed in northwest Victoria and southwest New South Wales since 1857. Wakefield (1966:372) notes that there are 140 mammalian specimens from the Blandowski expedition at the Museum of Victoria, representing 22 species, of which half have not been recorded in Victoria since they were collected.

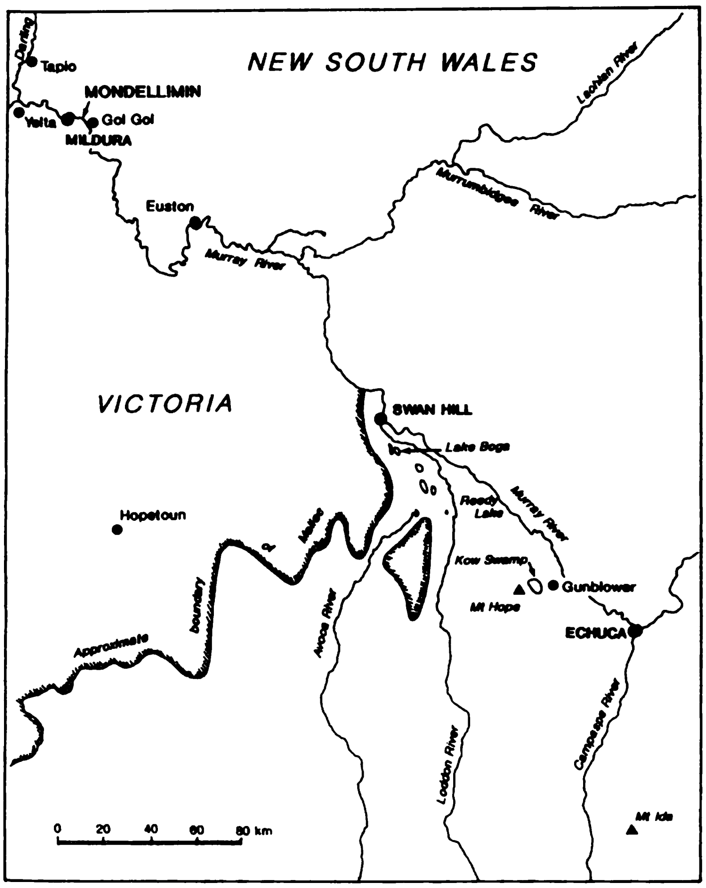

Figure 1. Locations of the Blandowski Expedition, 1856–57.
After Wakefield 1966: 376 (Fig. 1).

Blandowski began his description of the fish (1857:130) by claiming that there were only three kinds of fish known to exist in the Murray prior to his expedition, and continued, 'I beg to lay before you nineteen different forms of fish living in the waters of the Murray and Billibong'.

Despite the authorities on Australian fauna Blandowski took with him into the field, the number of misidentifications, and the failure of any of Blandowski's proposed names to enter the zoological record, make it clear that he was not a great taxonomist. Not even Iredale and Whitley's (1932:95) attempt to honour Blandowski by proposing a new genus for the perchlet he recorded, i.e., *Blandowskiella*, has survived for the fish in question is *Ambassis castelnaui*, though Blandowski has been honoured by a genus of marine fishes *Blandowskius*.

However, Blandowski collected 15 different species of fish, including some of the smallest in the Murray system, he provided descriptions of them and their habitats, and also observed their usage by Aboriginal people and collected the Aboriginal names for them. If we add these achievements to those of Krefft, for the mammals listed below, then the Blandowski expedition made a considerable contribution to colonial science.

BLANDOWSKI'S OBSERVATIONS OF ABORIGINAL NAMES AND FISHING TECHNIQUES

The preoccupation with the names of prominent individuals in Blandowski's nomenclature has drawn attention away from the fact that his taxonomy largely followed Aboriginal lines.

Blandowski distinguished a new species whenever his Aboriginal informants provided a new name, but he was misled by the fact that Aboriginal taxonomies can give separate names for different growth forms of the same fish (Table 2.). Thus different Aboriginal terms are provided for young and adult forms of the silver perch. His informants provided a clue that this was happening when they told him that one of the small fish was a *bipe purritjall*, or 'baby buruitjall', but his ear was not up to the task of interpreting the difference. The exception to this might be the term barnta for an adult Murray cod. Hercus (pers. comm, Jan 2000) suggests that this is likely to be a borrowing from a *Paakantyi* (Darling River) dialect in which *parntu* is the word for cod. She continues 'it also gives an inkling that Blandowski transcriptions might be pretty good, because he writes -rnt- having heard the retroflex sound'.

Table 2. 'Yarree Yarree' names for Murray River fish collected by William Blandowski near Yelta in 1856.

Species	Common name	Aboriginal name
'Yarree Yarree'		
1. *Tandanus tandanus*	Eel-tailed catfish	*Kenaru*
2. *Nematalosa erebi*	Bony bream	*Manur*
3. *Bidyanus bidyanus*	Silver perch silver bream	*Baggack* - young
4. *Bidyanus bidyanus*	Silver perch silver bream	*Buruitjall* - adult
5. *Bidyanus bidyanus*	Silver perch silver bream	*Karpa* -half grown
6. *Bidyanus bidyanus*	Silver perch silver bream	*Bipe Purritjall* - young
7. *Ambassis castelnaui*	Western chanda perch	*Mallupit*
8. *Craterocephalus fluviatilus*	Murray hardyhead	*Kohn*
9. *Retropinna semoni*	Australian smelt	*Turruitje*
10. *Nematocentris fluvialitilis*	Crimson spotted rainbowfish	*Jerrin*
11. *Galaxias truttaceous*	Spotted mountain galaxias	*Poke*
12. *Galaxias maculatus*	Common galaxias	*Uterank*
13. *Maccullochella peeli*	Murray cod	*Yaturr* –adult
14. *Maccullochella peeli*	Murray cod	*Barnta* - cf Paakantyi *parntu*.
16. *Philypnodon grandiceps*	Big-headed gudgeon	*Collundera*
17. *Hypseleotris klunzingeri*	Western carp gudgeon	*Loetj*
18. *Mogurnda adspersa*	Southern purple spotted gudgeon	*Koerin* or *Kurrin*
19. *Gadopsis marmoratus*	River blackfish	*Palkt*

Hooper (1994:189), in discussing Polynesia fish names, notes that growth stages are often differentiated for species that are economically important whereas fish of little or no significance might be grouped under a single descriptive term. Similarly, Rhys Jones (in Hiatt and Jones 1988:5) observes that the Gidjingarli of northern Australia have different names for different life stages of the Barramundi (*Lates calcarifer*) even though they are aware they are dealing with young and adult forms of the same species.

Blandowski was also a pioneer in using Aboriginal names for fish families e.g. *Kohna, Turruitja, Jerrina, Uteranka, Tilka, Collundera,* and *Kurrina*. This is a further compliment to the observational powers of Aboriginal hunter-gatherers, one which is emulated in the names *Bidyanus bidyanus (Cernua bidyana)* and *Tandanus tandanus (Plotosus tandanus)* which Mitchell (1839:95) gave to the silver perch and eel-tailed catfish respectively.

Blandowski (1857:136) notes he paid the Yarree Yarree £200 worth of flour, tea, sugar, blankets, clothing and other small presents. Krefft was critical of Blandowski's largesse in paying his Aboriginal collectors at Gunbower Creek a shilling (one twentieth of £1, — ca. 10 cents) for every mammal skin they brought in (Pescott 1954:13), but this, as can be seen below, ensured the scientific success of the expedition. Krefft notes elsewhere (1866b:360) that Aboriginal men on the Loddon River were offering to loan their wives 'for a small number of hooks and lines.'

LOCATING THE YARREE YARREE

Establishing the itinerary and permanent camping places of the expedition is not an entirely straightforward task. Blandowski's choice of men and equipment was bad, and, as he notes (1857:126) 'Krefft and Manson were all that held out my cause from eighteen persons, successively employed by me for Government service.' Added to the changes in personnel was the fact that Blandowski kept leaving the party to make side trips of his own. On one occasion, he noted that after he had pushed forward alone, the rest of the party arrived 'in a most deplorable condition' (1857:126). The expedition made the following points of call:

1. Melbourne to Kow Swamp, 6th December - 27th December 1856
2. At Kow Swamp/Gunbower Creek, 27th December - 3rd March 1857
3. Blandowski to Lake Boga, 3rd March 1857
4. Kow Swamp to Mondellimin, 3rd March - 8th April 1857
5. Blandowski to Moorundee, 8th April - 20th April 1857
6. Blandowski from Mondellimin to Mount Murchison (Darling River), 27th May - 20th June 1857 1857
7. Blandowski to Melbourne via Adelaide on a steamer, 6th August - 18th August 1857.
8. Expedition disbanded, 15th December 1857.

Blandowski notes that he left the camp at Mondellimin (on the northern side of the river opposite present day Yelta) in the charge of Krefft and Manson on the 6th of August, taking the collections with him.

Though Krefft spent much longer at Mondellimin (April to end November 1857), all told, Blandowski spent about two months there, the longest that he spent anywhere. Krefft, in his journal (1856-7), states that no new forms of fish were found at Gunbower except for a few specimens preserved in spirits for the collection. It seems likely that most of the fish collecting and the observations Blandowski made of Aboriginal fishing took place at Mondellimin.

The *Illustrated Melbourne News*, for February 6th, 1858, published an engraving of the camp at Mondellimin, noting that it was located twelve miles (ca. 19 km) by land above the Church of England Missionary Establishment at Yelta, near to the place where the waters of the Darling join the Murray. Krefft (1865:364) notes that Mondellimin is 'a few miles from Milldura', while Wakefield (1966:376) locates Mondellimin near present day Gol Gol, derived from GallGall the name of William's station where most of the mammals were obtained.

The majority of Aboriginal names Blandowski provides for his fish are attributed to the 'Yarree Yarree' tribe, but he provides no definite location or identity for this group. The use of the term *manur* for the bony bream, *Nematalosa erebi*, provides a clue as both Krefft (1866b:366–7) and Blandowski (1857:131) mention that bony bream, with this Aboriginal name, was an important source of food, and a prohibited one for the young, of the tribe living near the junction of the Murray and Darling Rivers. Krefft notes:

> Here, at Yelta, or rather on the New South Wales side of the River, the natives had always assembled in large numbers for the purpose of feasting upon fish and bartering their famous Myall-spears for reeds, Wongal-twine, and nets the produce of other parts of the country; .... About this time of the year, in the month of July, a similar gathering had taken place; and one night I visited the camp, ... and following our guide, we soon found ourselves in the midst of about two hundred natives, stretched around their camp-fires, which formed a semi-circle, the middle being occupied by 'Old Jacob', the famous chief. ... . Some tried to straighten young shoots of the Myall ... others were occupied knitting nets, using the same instrument as our fishermen do, and working with their hands and feet; the women were cooking fish, of which a large supply had been obtained during the day, - carefully reserving the taboo'd fish called Manor, for the use of the aged, no youth or lass being permitted to partake of it; - carving their waddies, or preparing opossums' skins for their rugs, kept others busy ... .

Blandowski (1857:136) notes that his collection of quadrupeds were entirely the results of the exertions of his friends the Yarree Yarree Aborigines and this is probably also true of the fish collection. Furthermore, he (1857:136) observes that 'Near the junction of the Darling ... the graves are huts covered with the 'Manur' nets of the deceased.'

Luise Hercus (1986:Maps 1, 2, 3 and 4) states that the Kureinji were located north of the Murray River from near its junction with the Darling River to the Big Bend area. She reports (pers comm. Jan 2000) that the area just to the east of the junction of the Murray and Darling was inhabited by people speaking Kureinji - Yitha-Yitha, a language belonging to the Upper Murray language family. Jamieson in Smyth (1878:74) records that 'Yerre Yerre' was the dialect spoken near Mildura and Krefft visited Jamieson's station Milldara (1886a:6, 1866b:359). Krefft (1866a:18-19) also recorded *bullucur* as the name for kangaroo amongst the 'Murray natives'. In confirmation of this, J.A. MacDonald, one of Curr's correspondents, (Curr 1886, Vol 2:288, Hercus 2000, pers. comm) notes that the Yit-Tha language at the junction of the Murray and Darling Rivers had the name *boolyoker* for kangaroo.

Hercus (1989:58) includes the Yari Yari as a part of the Kureinji - Yitha-Yitha language group. Though of great linguistic interest, little is known of the language of this group and only a few word-lists survive (Hercus 1989:59). To complete the list of Yari Yari animal names, Krefft's (1866a) word-lists for mammals collected by the Blandowski expedition are reproduced below.

Blandowski provided a few descriptions of fishing for the larger species but only Aboriginal names for the smaller fish. These are listed in Table 2. The descriptions of fishing are as follows:

2. *N. erebi*, bony bream, *Manur* of the Yarree Yarree.

This fish is easily caught by its elongated ray in thin fine nets, laid by the natives horizontally on the water. The fish gets entangled in the twine, and cannot escape. In June and July (Winter) it is considered a delicacy by the natives and forms their principal food during these two months. The young women are not permitted to eat them, from a belief, that if they did, all fish in the river would die; but in reality, because it is thought to be an aphrodisiac, this fish being very fat and nourishing. It is also placed on the top of graves, to point out the direction in which he lives, who caused the death of the inmate. Therefore, this fish is esteemed.

1. *T. tandanus*, eel-tailed catfish, *kenaru* of the Yarree Yarree.

This fish is very much esteemed by the natives as food and prohibited to their young men. It swims with great rapidity, even in shallow water. It ploughs the water with its powerful dorsal fin, and is therefore easily recognised and speared by the natives. They often hurt their fingers on the sharp back fin, and then say it is a 'saucy fellow'. It is unquestionably the best eating fish in the Murray.

10. *N. fluviatilis*, crimson spotted rainbowfish, *jerrin* of the Yarree Yarree.

This bright coloured fish soon attracts the attention of the little black children. The fish is roasted together with the other little fishes in the following manner: they take a few hot stones and some clods of clay, throw in the whole lot of fishes, turn them round for a few minutes, then take out again the hot stones and eat the whole mess like 'bubble and squeak' from a piece of bark, on which these little fishes have been previously prepared.

13. *M. peeli*, Murray cod, *yaturr* of the Yarree Yarree, (also fish 14 where the name *barnta* is given).

These are the principal fish on which the natives subsist during the greater part of the year. In winter, when the river overflows its banks, the natives spear them at night by firelight, while sleeping behind an old log; in the summer season, from January to June, when the river is low and the water clear, this fish sleeps in the river behind a log or stone. The native, spear in hand (now an iron rod of about six feet (ca. 1.8 m) in length), dives, head foremost, to the bottom of the river, where the fish sleeps and there spears it, an exciting sport even to the white man.

A mythological account from the adjacent Maraura (Paakantyi) tribal area recorded by Tindale (1939:255-6) describes Kingfisher teaching Crow how to dive down and find Murray cod hiding in hollow logs.

Both the method of netting bony bream and the cooking of whitebait are useful additions to our knowledge of the Murray River Aboriginal fishery. Blandowski's list of Yarree Yarree names for large and small fish in the Murray River is unmatched for the Murray Valley and elsewhere. Further confidence in his observations may be found in the fact that he provides names for *M. australasica*, Macquarie perch, only for the Gunbower Creek area, i.e. *pollugunder*, and for the Loddon River, i.e. *birnnett*. It seems likely that the Macquarie perch was not seen further down the river and hence no name was recorded. The absence of a record for *Macquaria ambigua*, golden perch or yellow belly is significant. The expedition was camped at Mondellimin during the winter months, when the Murray River was low, and golden perch may have been locally absent.

KREFFT'S WORD-LISTS FOR MAMMALS FROM NEAR THE JUNCTION OF THE MURRAY AND DARLING RIVERS

Wakefield (1966) discusses the mammals collected by the Blandowski expedition using the specimens and register from the Museum of Victoria, Krefft's published source (1866a), and the catalogue of specimens Krefft maintained. Krefft's Lists VIII-XX, specimen numbers 755–3000, collected between April and November 1857, came from Mondellimin. Krefft's Aboriginal names listed as 'of the natives', 'native name' or 'of the Murray natives' are all likely to be Kureinji- Yitha-Yitha terms. The order presented below follows Wakefield (1966), while the numbers relate to the order presented by Krefft (1866a). Krefft numbered placentals and marsupials separately, hence the duplication of numbers.

In discussing the collection of mammals, Krefft (1866a:6) noted:

> two thirds of the smaller mammalia collected and examined by me on the Murray were new to many old residents, and even the natives, who in many parts, have acquired habits different from their former mode of life, had almost forgotten the existence of some of these species. With the aid of Messrs. Williams and the natives, I succeeded in procuring every species known to exist in that part of Australia; and in finding also a number of animals of this order which hitherto had been only known to frequent Western and South Australia.

Tachyglossidae

24. *Tachyglossus aculeatus*. Echidna or spiny ant-eater. No Aboriginal name given, natives further down the river (at the junction) did not appear to be aware of the existence of such an animal as the echidna. It has since become common in the area between the Darling and the Murray.

Dasyuridae

5. *Antechinus flavipes*. Yellow footed marsupial mouse. *Warum*, 'common near the camp on the Murray.'

2. *Phascogale calura*. Red tailed phascogale. *Kultarr*, captured near William's station, Gol Gol Creek.

7. *Sminthopsis crassicaudata*. Fat-tailed marsupial mouse. *Mondellundellun*, Gall Gall Creek, Lower Murray.

6. *Sminthopsis murina*. Common marsupial mouse. *Tram trammit*, or *ram rammit*, Murray and Darling scrub.

4. *Antechinomys laniger*. Woolly phascogale, Kultarr (Ride 1970:126). *kultarr*, obtained through the natives at Gol Gol Creek. The natives informed me that the animal was very rare, ... had a dispute about its name and called it *kultarr*, while some asserted they had never seen the animal before, plains of the interior of NSW etc.

1. *Dasyurinus geoffroii*. Native or Tiger cat. *Kettrie*, along the scrub on the banks of the Murray.

8. *Myrmecobius fasciatus*. Banded ant-eater or numbat. No Aboriginal name provided. Not found close to the Murray river, Tapio Station.

Peramelidae

12. *Isoodon obesulus*. Short-nosed bandiccot. *Pirrikin*, Murray scrub.

11. *Perameles bougainville*. Barred bandicoot. *Thill* or *moncat*, the natives have two distinct names for this animal, Gunn's property, common on all parts of the Murray River.

10. *Macrotis lagotis*. Rabbit-eared bandicoot. *Wuirrapur* -Murray tribes, *jacko* or *jecko* - Darling tribes, long ago retreated to the north of the Murray (cf. *yacoo* Blandowski 1857:136. Krefft 1866a:15 makes a scathing comment about Blandowski's observations regarding this animal's habit of locating its nests in burial mounds).

9. *Chaeropus ecaudatus. Pig-footed bandicoot. Landwang*, Gall Gall Creek, Mondellimin, all specimens procured on the NSW side of the Murray.

Phalangeridae

14. Pseudocheirus peregrinus. Ring-tailed possum. *Pirrath* of the Murray natives, a rare animal on the Murray and Darling.

13. *Trichosurus vulpecula*. Brush-tailed possum. No Aboriginal name given, mallee scrub, sometimes 20 miles from water. Catalogue numbers indicate registration while at Mondellimin.

Macropodidae

20. *Bettongia penicillata*. Brush-tailed rat-kangaroo. *Pattuck*, Gall Gall Creek.

21. *Bettongia lesueur*. Lesueur's or burrowing rat-kangaroo. *Booming*, I have never met with this *Bettongia* on the Victorian side of the Murray, but on the NSW side it is found in abundance.

19. *Aepyprymnus rufescens*. Rufous rat-kangaroo. No Aboriginal name, not a single specimen was procured by the natives during my stay at the Darling Junction.

18. *Lagorchestes leporides*. Eastern hare-wallaby. *Turratt*, lower Darling, plains bordering upon the Murray, Murray and Darling.

17. *Onychogalea fraenata*. Bridle nail-tailed wallaby. *Merrin*, common in the northern parts of Victoria.

(?). *Onychogalea lunata*. Cresent nail-tailed wallaby. No Aboriginal name. Specimen misidentified by Krefft as a juvenile *O.fraenata*, taken west of the Darling (Wakefield 1966:384).

15. *Macropus rufus*. Red kangaroo. *Bullucur*, Krefft's list notes 'Native name ?', all the specimens procured were killed near to the Darling junction, very scarce upon the left bank of the Murray.

16. *Macropus major melanops*. Black-faced or mallee kangaroo. *Bullucur*, very common on both sides of the Murray.

Muridae

1. *Hydromys chrysogaster*. Water rat. No Aboriginal name given, I was assured by the natives that they had never seen it.

(?) *Leggadina hermannsburgensis*. Sandy inland mouse. Variously, *Lanango, kahlpere, kunnang*,or *kunang* (see Wakefield 1966:387).

5. *Pseudomys desertor*. Brown desert mouse. Variously, *Ramm ramm, ram ram, padorka, padack, pethack*, or *kunnang*, found in large numbers between Gol Gol Creek and the Darling. Wakefield (1966:388) suggests *padorka*, *padack*, *pasack* and *pethack* (with a stress on the first vowel) are evidently renderings of the one Aboriginal name.

3. *Leporillus apicalis*. White-tipped stick-nest rat. *Tillikin*, Gall Gall Creek, in great numbers on both sides of the Murray.

2. *Leporillus conditor*. Stick-nest rat. *Koel or kohl*, near Mount Lookout, a sandhill about 10 miles from the Darling junction, only a few nests were occasionally met with south of [the Murray].

4. *Notomys mitchellii*. Mitchell's hopping mouse. *Kahlpere*, very plentiful on the Darling. Wakefield notes additional names, *kirrie* and *muirruitang* where Krefft had misidentified specimens of *N. mitchellii* as a different species.

DISCUSSION

Krefft's list of mammal names from the junction of the Darling and the Murray Rivers is a useful one. Where a species can be demonstrated not to have been caught in that vicinity e.g. echidna, numbat, rufous rat-kangaroo and water rat, no Aboriginal name is provided. No name is given also for the brush-tailed possum or the dingo, possibly because Krefft thought these so common as to be not worth collecting. *Bullukur* may be a general name for kangaroo rather than for either the red or grey kangaroo as MacDonald (in Curr 1886) provides the name *boolyoker* for kangaroo. The names for the mice are the least satisfactory, or most interesting, depending on one's perspective. Krefft had difficulty in speciating them and this may have confused his informants. It is clear from the context of some of the answers that the informants were providing names for species they were unfamiliar with. *Tram trammitt, ram rammitt* and *ram ram* are probably the same name but have been applied to marsupial and placental mice respectively and may simply be a generic term for mouse. The echidna is now common in the area despite its rarity in 1857.

Krefft also noted the Aboriginal manner of counting, an observation made at Mondellimin. He records (1866b:365) that '*rangul* means two, and *meta* one, so that *rangul, rangul, meta* is equivalent to five, and so on *ad finitum*'. Macdonald in Curr (1886:vol 2:288, Hercus 2000, pers. comm) provides the terms for numerals in Yit-Tha as *mo* -one, *thral* - two, *thral mo* - three, *thral thral - four*, indicating that the recorded counting systems are very similar.

As noted previously, the Blandowski expedition to the Murray River and its junction with the Darling made scientific observations which have not been accorded the importance they deserve. The cloud which hung over the expedition appears to have assisted in the downgrading of the scientific objectives of exploration. This, however, did not wholly dampen the enthusiasm of members of the Philosophical Institute for exploration as Mueller, Wilkie and Macadam were prominent advocates of the Burke and Wills expedition (Serle 1963:367).

The fate of Blandowski's expedition should have meant that a great deal of attention would be given to the personal, managerial and bushcraft skills of any potential leader. This was not the case, however, and Robert O'Hara Burke, a cultured but erratic man with few bush skills, was selected to lead an expedition to cross the continent from south to north, with tragic results (Serle 1963:368).

Great changes occurred in the Murray-Darling region after 1857. Townships were created. Aboriginal communities have survived but many languages and a great deal of detailed cultural knowledge has not. The ecology and fishery of the Murray River has been greatly altered through land clearance, irrigation and the building of weirs to regulate flows. Many of the species observed by Krefft and Blandowski have become extinct across much of the continent. Blandowski and Krefft observed these changes at the very time that they were happening. Our knowledge of the area would be much poorer except for the work of the Philosophical Institute in promoting a scientific expedition to the junction of the Murray and Darling Rivers and for Blandowski and Krefft's leadership of it.

Whatever Blandowski's faults, it is clear that a lack of generosity of spirit was not one of them. His 1857 article concludes:

> the specimens were obtained by the assistance of the natives, to whom I am indebted for all the information and discoveries I have made, so that I can but claim a small share of the credit of having, with my party, been successfully exploring the desert of Australia for eight months.

Acknowledgements

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The Colonisations and Visitations of Wales by Neanderthals and Modern Humans in the Later Pleistocene

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RHYS JONES is known internationally for his work on the earliest human presence in Australia. For my part, I have worked on the issues of human visitation and colonisation of Wales throughout the later Middle and Upper Pleistocene. We have followed parallel research agenda on opposite sides of the globe: I, an expatriate Englishman living in Wales; Rhys, Welsh by birth but now Australian. My own research program 'Hunter-gatherers at the Periphery of the Pleistocene World' has been concerned both with the field investigation of sites in Wales and, increasingly with the interpretation of the context and nature of the settlement of one of the remotest areas of Europe by Pleistocene hunter-gatherers. It has been a particular pleasure to work with Rhys as a member of the research team for the publication of a definitive monograph on Paviland Cave in Wales. Rhys's contribution (Jones 2000) has given a perspective not only on the socially embedded meanings of the ritual associated with the Gravettian ceremonial ochre burial of a young male at Goat's Hole, Paviland, known perversely as the 'Red Lady of Paviland', but has also shown how the foundations of many of the concepts which inform the thinking of European archaeologists are actually of Australian derivation. None of the ^{14}C ages quoted here has been calibrated.

COLONISATION

The British Isles were first peopled around 500,000 years ago during the temperate phase correlated with Oxygen Isotope Stage (OIS) 13 of the deep sea core sequence. Settlement is attested as close as 80 km to the modern boundary of Wales at Westbury Cave (Andrews et al. 1999; Barton 1997:47) at this period. It is also known from a number of other sites in Britain, most notably at Boxgrove and High Lodge (Roberts and Parfitt 1999; Ashton et al. 1992). It would seem likely that there was a correspondingly early hominid presence in Wales but no evidence, or indeed potentially contemporaneous sediment traps, can currently be identified. Thus, the period of my concern here is the last 250,000 years. Colonisations of Britain in the later Pleistocene took place against a backdrop of certain natural constraints. The first was the sea whose interglacial rise restricted or even cut off what was then the British peninsula from the mainland of Europe (Preece 1995). The second was the presence of extreme cold and, at the height of the glacial periods, of extensive ice-sheets. To these natural constraints must be factored in the very small size of contemporary hominid populations.

Looking at later Pleistocene British evidence as a whole, the picture is one of hominid presence during OIS 7 with abandonment taking place as the extreme conditions of late OIS 6 began to bite. During OIS 5e, the last interglacial, no human presence is recorded. Indeed, there is no certain evidence that Britain was revisited until after the glacial conditions of OIS 4 had come to an end ca. 60,000 BP (Currant and Jacobi 1997, in press). We then find Neanderthal groups present (Fig. 1) in the context of the milder environments of OIS 3 (Van Andel

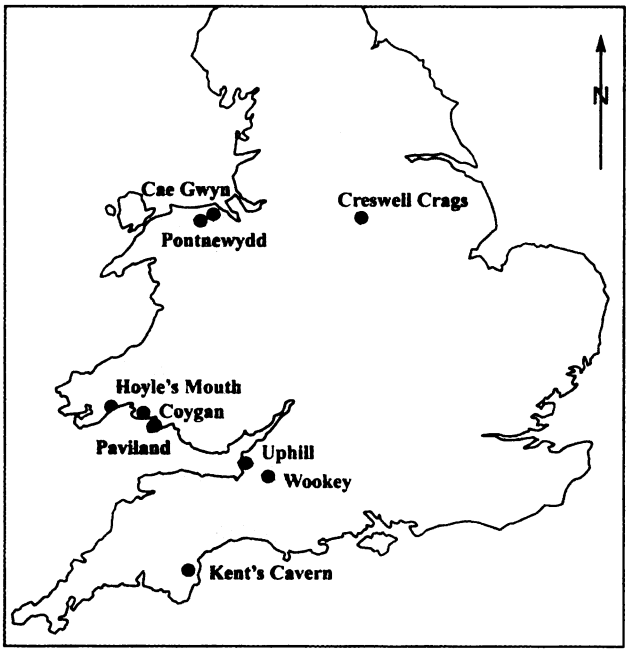

Figure 1 The British Isles. Neanderthal and Modern Human sites quoted in the text.

1998) both in southern England and, in Wales, at the sites of Coygan (Aldhouse-Green et al. 1995) and Paviland (Aldhouse-Green 2000a).

The earliest colonisation of Wales

Wales lies in a region which has been glaciated on a number of occasions. Accordingly, settlement evidence is limited, except for the southern littoral which lay beyond the limits of later Pleistocene ice, to caves whose deposits have been protected from later destruction and thus preserve a record of hominid presence in the form of artefacts and fauna, albeit emplaced by debris flow events and so in secondary context. Settlement evidence is also preserved for the Late Upper Palaeolithic period, ca. 13–10,000 BP after the waning of the Devensian icesheets following the glacial maximum at 20–18,000 BP.

Although the land bridge to Britain across the English Channel had been breached ca. 400,000 BP there is some evidence for a land connection having been present through part at least of OIS 7 (Sutcliffe 1995:136; Keen 1995:63–5). The first colonisation event which can be recognised in Wales falls within this stage. The evidence is provided by the single site of Pontnewydd Cave which, lying less than 12 km from the modern coast of North Wales, is the most northwesterly Middle Pleistocene site in Eurasia. The cave has yielded the remains of a number of individuals of probable early Neanderthal affinity (Stringer 1984) associated with an extensive industry characterised by handaxes, the products of Levallois technology and side-scrapers. The main phase of human occupation at Pontnewydd lies in OIS 7, possibly in part of the cold substage 7b and dated, therefore, to ca. 225,000 BP (Lowe and Walker 1997:284–5; Green 1984:211). This dating is based principally on two strands of evidence: firstly the Lower Breccia debris flow — in which the derived fauna, hominid remains and artefacts are preserved - underlies a stalagmitic floor dated to 225,000 BP and, secondly, the Lower Breccia contains a fauna (but potentially of more than one actual phase) which generally lacks explicit temperate or glacial elements (Currant 1984), and so plausibly belongs to the interface of OIS 7b/7a or 7c/7b. There is, therefore, no reason to suppose that the occupation was linked either to a fully interglacial or very cold climate context. A possible later occupation took place early in OIS 6 (Aldhouse-Green 1998), perhaps centred on 175,000 BP on the basis of TL determinations on burnt flint (Aldhouse-Green 1995:39, fig 3), and characterised by rare handaxes and by a greater frequency of Levallois products and side-scrapers. This occupation event is coeval with an episode of stalagmite formation at Pontnewydd (Schwarcz 1984) and, so, would seem to correspond to an early OIS 6 temperate event.

Pontnewydd Cave represents the most northwesterly settlement of its period in Eurasia. Accordingly, its very presence provokes the question 'why then and there ?'. Here, the distribution of the region of oceanic climate in Europe may be seen as a key factor, in combination with mosaic patterns of vegetation, in favouring the concentration of both animals and humans in western Eurasia (Gamble 1995:283–4). The strengthening of the oceanic effect in some oxygen isotope stages may, indeed, have acted as a pump in encouraging westerly movements of animals and humans. The clue to the timing of main phase of settlement at Pontnewydd in OIS 7 may lie in the fact that this was a low sea level interglacial, unlike OIS 11 and 9. Because of this the January 0°C isotherm, which forms the boundary between areas of oceanic and continental climate in Europe, will have moved west, and so have reduced the area of the milder western oceanic region (Lockwood 1974:fig 9.2; Shackleton 1987; Gamble 1999:230). Thus, some human populations may have been encouraged to move westward to follow the longitudinal shift in climate. A key factor in this will have lain with winter temperatures which will have been less severe in the west, thereby facilitating year round settlement. Caves are likely to have been an important resource almost regardless of climate. Thus, Pontnewydd Cave itself now maintains a constant all year temperature of 10°C: when there is deep snow outside, the cave seems delightfully warm; in summer, the cave seems beautifully cool. Accordingly, regions with caves, as indeed regions with less marked seasonal variation in climate, may have held an especial attraction for Middle Pleistocene hominids, as also with modern humans of the Early Upper Palaeolithic in Wales (Aldhouse-Green and Pettitt 1998). It may be relevant that OIS 13, when the first certain hominid presence in Britain is attested, was also a low sea level interglacial.

The late arrival and swift departure of the Neanderthals

A recent paper by Boquet-Appel and Demars (2000:548) has identified the appearance of Neanderthals in Britain ca. 40,000 BP followed by their departure several thousand years later, apparently leaving a hiatus in settlement before the arrival of anatomically modern humans in the time range 30-28,000 BP (Aldhouse-Green and Pettitt 1998; Jacobi and Pettitt 2000). This interpretation is based on the very limited evidence of ^{14}C determinations. However, a range of dating techniques is available. Thus, at Kent's Cavern, southwest England, speleothem dating by Uranium-series (U-s) has yielded a suggested maximum age of 74,000 BP for the Middle Palaeolithic industry (Proctor et al. 1996:253). At Coygan in southeast Wales (Aldhouse-Green et al. 1995) Neanderthals would seem to have been present certainly post-64,000 BP and probably ca. 50,000 BP on the basis of a full assessment of the site radiocarbon and U-s determinations, taken in conjunction with wider archaeological comparative evidence. The evidence of Uranium series, ESR and ^{14}C dates from the Creswell Crags site of Pin Hole (Jacobi et al. 1998) indicates an age there of <64,000 BP and probably 50-38,000 BP for the Middle Palaeolithic industry. At Robin Hood Cave, Creswell, an ESR determination of ca. 51,000 BP on a woolly rhino tooth gives a maximum age for the Middle Palaeolithic finds (Jacobi et al. 1998:41). At Rhinoceros Hole, Wookey, speleothem dating by U-s suggests a maximum age of 50,000 BP for a bout coupé handaxe and associated material (Proctor et al. 1996:253). The contiguous site, the Hyaena Den, has yielded a ^{14}C age of 40,400±1600 BP (OxA-4782) on a cut marked red deer tooth which must relate to some phase of the Middle Palaeolithic occupation there (Jacobi and Hawkes 1993).

The approach of Boquet-Appel and Demars is deliberately 'broad brush' and concerned with the interpretation of Neanderthal contraction and modern human expansion in Europe. Their synthesis of the data suggests that this contraction had begun by 37,500 BP and that the areas first to be abandoned included regions of eastern Europe, northern Italy, Spain's northwest coast and Britain. By 32,500 BP, the Neanderthals were confined to refuge areas in southwestern France and southwestern Iberia, but, by contrast, modern human settlement was continuous. By 27,500 BP, the Neanderthals had disappeared even from these refuge areas which had, perhaps, taken on more the aspect of 'reservations'. In these broad terms the analysis is compelling. However, the authors would seem to have overlooked the scanty evidence for early modern human presence in Britain, among which the direct date of 30,900±900 BP (OxA-1621) on a *Homo sapiens* maxilla from Kent's Cavern is pivotal. It is would seem that Aurignacian expansion into Britain took place during the period 30-28,000 BP. This is based on the evidence of the Kent's Cavern maxilla; determinations on unmodified fauna from debris flow contexts at Kent's Cavern which indicate a possible 29,000 BP or younger age for the Aurignacian there (Jacobi 1999:37); a direct date of 28,080±360 BP (OxA-8408) on an Aurignacian bone point from the site of Uphill on the English side of the Bristol Channel (Jacobi and Pettitt 2000); and inferential evidence from Paviland and Hoyle's Mouth (see below).

There is evidence for a possible late OIS 3 Neanderthal presence in both Wales and England which would be in harmony with the picture of Neanderthals surviving in regions along or proximal to the Atlantic seaboard of Europe. A sole ^{14}C determination, on charred bone, of 34,900±1450 BP (OxA-4113) from the Hyaena

Den at Wookey plausibly refers to this horizon, as does a result of 37,200±1300 BP (OxA-3417) on a cut marked reindeer tibia provenanced only to 'Creswell Crags'. In Wales, as more widely in Britain, leaf points plausibly represent a late Middle Palaeolithic industry executed by Neanderthals. Even so, none can actually be shown to be older than the earliest Aurignacian in Britain and may be coeval with it (Aldhouse-Green and Pettitt 1998:763-4). It remains therefore an open question as to whether Wales was a *terra nullius* following range contraction by Neanderthals or whether, conversely, it was the presence of Neanderthals beyond an 'English Channel frontier' — mirroring the Iberian Ebro frontier (Zilhao 2000) — that led to a late arrival of anatomically modern humans in the British peninsula. It is interesting that the move by modern humans south across the Ebro Frontier, occurred at about the same time as the movement north into Britain. The crossing of the Ebro has been related (Zilhao 2000:120) to worsening climate in northern Europe. Climate may have been the cause of both movements, with the move into western Britain, where the British Aurignacian is distributed, arising from the attraction of its maritime climate. A more compelling reason for Britain may, however, be seen in the biomass explosion which clearly took place there 29–28,000 BP (Aldhouse-Green 2000b:232).

Some light has been thrown on the question of the chronology of the leaf points by a new ^{14}C determination from the cave of Cae Gwyn which, together with its neighbour Ffynnon Beuno cave, is situated near Tremeirchion in the Vale of Clwyd, North Wales. The two sites, although not apparently interconnecting, are physically contiguous. Both were excavated in the 1880s by Dr Henry Hicks (1886) and together yielded a hyaena den fauna of 'Coygan' type (Currant and Jacobi 1997) interstratified with a leaf point and Aurignacian artefacts. The latter comprise a busked burin and an end-of-blade scraper on a characteristic Aurignacian blank with typical fluted retouch. A gnawed chunk of the shaft of a mammoth long bone (identification: Kate Scott, Donald Baden Powell Quaternary Research Centre, University of Oxford) has been dated to 41,800±1800 BP (OxA-8314). This find, from an unknown context in Cae Gwyn cave, comes from a collection originally housed in Tremeirchion School and now in the care of Denbigh Museum (find ref. no. 41). A result, obtained 30 years ago, on mammoth bone from Ffynnon Beuno and not from Cae Gwyn cave (as variously published) gave an age of 18,000+1400/-1200 BP (Birm-146) (Rowlands 1971; Green 1991:41). The latter date is actually not far removed from the range of mammoth ivory determinations at Paviland (Aldhouse-Green and Pettitt 1998:764). The context of the finds seems to have been debris flow (Collcutt 1984:58) and, accordingly, the wide divergence of the two dates remains wholly plausible with a potentially long phase of accumulation followed by input of the flows around the time of the last glacial maximum, the latter given an age of 22,800 sidereal years BP (equivalent to a ^{14}C age of ca. 20,000 BP) by a ^{36}Cl age from a glacial erratic in Gower on the southern edge of the Devensian ice sheet (Bowen 1994:210). The 18,000 BP result from Ffynnon Beuno cave, with a 2s range of 20,800 BP to 15,600 BP, is clearly consistent with this. What is important about the 41,800 BP date is that it demonstrates the existence of reworked Middle Devensian sedimentary elements and so indicates the former existence of deposits, potentially of Middle Palaeolithic age, which could have been the context of the leaf point from Ffynnon Beuno. It now seems less easy to sustain the view that 'at smaller sites ... in particular Ffynnon Beuno ... it is hard to escape the impression that Aurignacian elements and leaf points really do belong together' (Allsworth-Jones 1990:208).

VISITATIONS BY MODERN HUMANS

The first dates unequivocally associated with modern humans in Wales are the results on the bones of the 'Red Lady' of Paviland and date as recently as 26,000 BP (Aldhouse-Green and Pettitt 1998). As we have seen, it is likely that the Aurignacian settlement of Wales had taken place by 29-28,000 BP. It is plausible, although not ultimately demonstrable, that accumulations of ^{14}C-dated burnt bone at Paviland relate to this Aurignacian phase of settlement (Aldhouse-Green and Pettitt 1998:765). The presence of burnt Aurignacian artefacts in the cave is consistent with this (Swainston 2000:103). If the dating of this phase is correctly inferred, it can be linked to the Greenland Ice Core Project (GRIP) interstadial event 5 (Bowen 2000). That event was 461 sidereal years long and, as interstadial events occupy only 17% of time between ca. 35,000 BP and 15,000 BP, the association between a phase of milder climate and human occupation is likely to be significant. Whilst the Aurignacian presence at Paviland is small by continental standards, the site nonetheless presents the richest assemblage in Britain. The events that follow during the Gravettian phase of the Early Upper Palaeolithic, from ca. 28,000 BP to 21,000 BP, are defined by a series of five episodes which are not represented by anything which could be called an occupation. The evidence consists firstly of one flint artefact only, the tang of a probable spear point of 'Font Robert' affinity (28–27,000 BP), plausibly a hunting expedition loss or discard; secondly, the ceremonial burial of a young male, possibly interred in a burial suit of leather clothes stained blood-red with ochre, ca. 26,000 BP (the 'Red Lady' of Paviland); thirdly, the manufacture of an ivory pendant on site ca. 24,000 BP; fourthly, the deposit of three bone spatulae interpreted as anthropomorphic figurines ca. 23,000 BP; and finally, a phase of ivory working ca. 21,000 BP (Aldhouse-Green 2000b; Aldhouse-Green 2000c).

The Gravettian events took place, except apparently for the pendant, during or very close to interstadial events represented in the Greenland ice cores. They would seem, from both their timing and content, to represent visitations rather than colonising events. They could be seen as the work of Gravettian task groups, perhaps in pursuit of ivory whose availability in Europe was variable (Aldhouse-Green 2000b:232), and this is doubtless part of the story. I would argue, however, for a link between the ritual burial of the 'Red Lady' and the subsequent visits during the climatic downturn that followed. Here, one may invoke sacred quests, perhaps even pilgrimages, to the Goat's Hole cave at Paviland, a site which may have been viewed as a *locus consecratus*, a place of oneiric power (Aldhouse-Green 2000b:243-5).

It is perhaps not surprising that the colonists, who established what was plausibly largely continuous settlement in Britain during the warm conditions of the late glacial interstadial (13–11,000 BP), seem to have abandoned it for part at least of the intensely cold Younger Dryas stadial phase which followed (11–10,000 BP) (Barton and Dumont 2000). It is interesting that in Wales the only evidence of human activity dating to the latter period takes the form of art objects — notably, a decorated horse mandible and incised and perforated teeth - from the site of Kendrick's Cave, Llandudno. The site is located on the North Wales coast and human burials there, the only others known from Upper Palaeolithic Wales, are dated several millennia earlier (Aldhouse-Green 2000d:21). In this way, the well documented late glacial period and the site of Kendrick's Cave may provide a potential analogue for earlier events during the Upper Palaeolithic.

CAVES IN PALAEOLITHIC WALES: ISSUES OF LOCATION

Reconstruction of the settings of palaeolithic sites, whilst difficult to achieve, offers insight into aspects of their roles in the social landscape of the period. Here, I have taken four key Welsh sites, as follows.

Pontnewydd

The Pontnewydd cave system is situated at a height of 90 m above mean sea level (msl) on the northeastern scarp face of the Elwy Valley (Fig 2A). The main cave faces due west and its entrance offers both shelter and sun-trap even when snow is lying outside. The cave looks directly across the narrow valley, about 300 m wide at the level of the cave. At present, the valley floor lies ca. 50 m below but the palaeo-Elwy may have flowed closer to the level of the cave at the time of the occupation (Embleton 1984:29). It would seem unlikely, however, the valley floor was higher than 75 m above msl, because the much larger Cefn cave system, located at a distance of only 700 m downstream, is situated at that height and contains a Middle/Upper Pleistocene depositional sequence which in many respects mirrors that of Pontnewydd

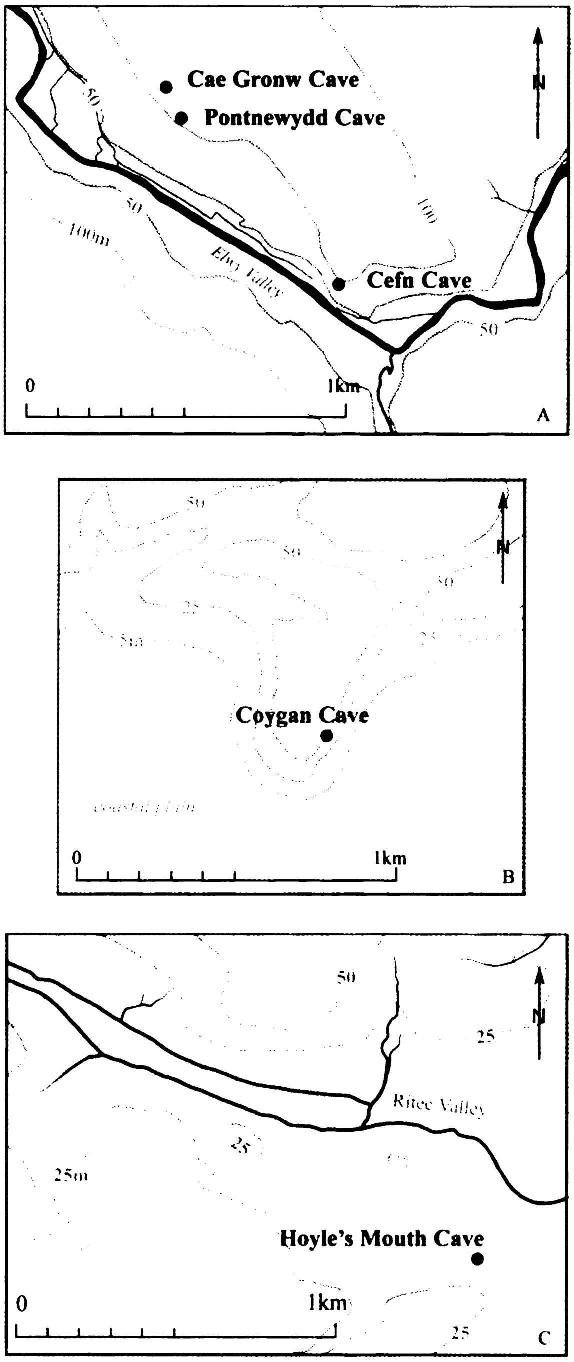

Figure 2 Wales. The topographic settings of selected caves: A Pontnewydd, Cefn and Cae Gronw; B Coygan; C Hoyle's Mouth

(Green and Walker 1991:44–7). The Cefn caves are, however, located half way up a cliff which rises precipitously from the valley floor, 35 m below. The caves were clearly accessible then (as they are now by means of a narrow ledge) for they were used by hibernating bears. Yet, the absence of coeval settlement evidence suggests that shelter was not a sufficient reason for their use and that they lacked therefore one or both of two other clear advantages offered by Pontnewydd, namely copious supplies of raw material for artefact manufacture and/or ready access to the valley floor to intercept game (Aldhouse-Green et al. in press). Few British caves had such possibilities for raw material collection and so it would seem reasonable to conclude that the role of caves as invisible vantage points and intercept sites was generally important. This interpretation, based on the positive and negative evidence presented by these two Elwy Valley sites is lent further substantiation by the absence of evidence for coeval human occupation at a third, the site of Cae Gronw, also with later Middle Pleistocene sediments and situated on the hill above Pontnewydd (ca. 110 m above msl), at arguably too great a height for effective valley floor intercept hunting (Green 1986a).

Coygan

Coygan Cave is an east-facing cave situated at ca. 50 m above msl high above the coastal plain (Fig. 2B). The last glaciation Middle Palaeolithic finds it yielded, whilst important, were scanty. John Clegg, who had directed excavations at the site with Charles McBurney in the 1960s, even went so far as to suggest that there had been no actual occupation but that the handaxes from the site had been introduced 'in the pockets of the hyaenas' breakfast' (in Aldhouse-Green et al. 1995:47). Clegg may be right: the cave has small entrances and lacks high sheltering cliffs. Its ready access, moreover, is to the top of the plateau in which it is located rather than to the very open plain over 40 m below. The destruction of the cave by quarrying followed an unsuccessful campaign, in which the young Rhys Jones (2000:247) played a noble role to save the site. The cave was shorn of its status as a scheduled ancient monument leading eventually to it being finally blasted away ca. 1980.

Paviland

Goat's Hole, Paviland, is located on the north shore of the Bristol Channel at 15 m above msl and is within reach of storm tides. South-facing, it lies at the foot of Paviland cliff which towers 30 m above it. At the top of the cliff the hill has the form of a level promontory which extends out from the Gower plateau. It would have made an excellent jump site. The occupants of the cave, moreover, would have had ready access to this plateau via a valley known as Foxhole Slade. Thus the residents of the site could have sat in the shelter of their cave awaiting an 'air-drop' of convenient mammoth-shaped food parcels. But they also had spectacular views southwards. The modern visitor, viewing a vast sea from Goat's Hole might be forgiven for imagining a once level plain. It was not so. The cave does not command a narrow valley, as do Pontnewydd and Hoyle's Mouth; nor is it a plateau-edge site like Coygan. Instead, the cave overlooks a limestone-based shelf (David Lowe pers. comm.; Wallingford 1997) 30 m below the cave which runs 15 km west to east from beyond Worm's Head to Port Eynon Point where it converges with the land (Fig.3). In front of Goat's Hole, the shelf is some 2.5 km wide. At this point migrating animals would have passed Goat's Hole in order to reach or leave the Gower plateau, using Foxhole Slade. Goat's Hole was well sited, therefore, as both a jump and intercept site.

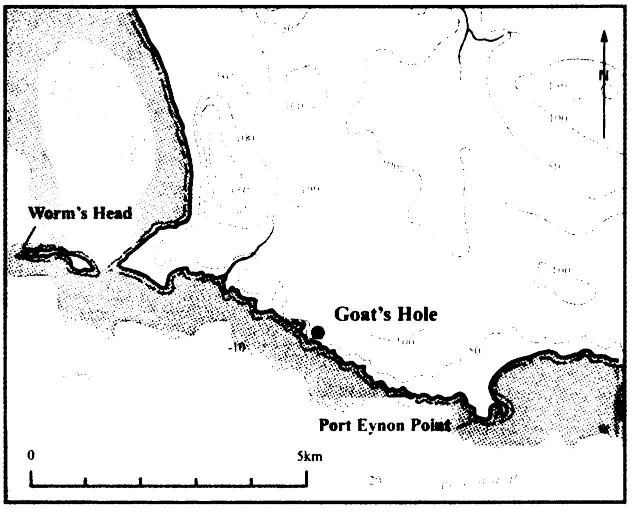

Figure 3 Goat's Hole, Paviland. Topographic context. Based on Admiralty Chart 1076. With kind permission of the Ordnance Survey © Crown Copyright.

Hoyle's Mouth

Just as Paviland is the richest early Upper Palaeolithic (30–21.000 BP) site in Wales, so the cave of Hoyle's Mouth is the richest site of the late Upper Palaeolithic period (13–10,000 BP) which followed the period of the last glacial maximum when the British peninsula was abandoned (Green 1986b; Aldhouse-Green 1996). This north-facing cave is located 19 m above msl on a former embayment of the sea which drains the Ritec Valley (Fig 2C). Apart from a reasonably commodious entrance chamber where it was considered that the flint knapper 'might be supposed to have seated himself to take advantage of the light' (Smith 1863:48), the cave is composed of a narrow horizontal fissure, not easy of access and mostly lying in darkness. The entrance is situated below 4 m high cliffs above which lies the surface of the local plateau. The valley floor, here 500 m in width, lies only 17 m below the cave entrance from which it is immediately accessible. The cave, as might be inferred from its aspect, tends to be cold and sunless. It should be seen as the largest of a series of small local caves of which it is the most productive.

These caves fall into two periods, Middle and Upper Palaeolithic. The synthesis of data by Mellars (1996:245–54) relating to Mousterian caves in southwestern France shows that both Pontnewydd and Coygan fit into the pattern seen there. Pontnewydd was a site of importance: a south-facing cave located in a tributary valley situation which game might favour both for the shelter it afforded and, perhaps in the case of horse (for which there is butchery evidence), as a seasonal migration route to the uplands of Mynydd Hiraethog. Like the French sites it was also situated close to good supplies of raw materials and, if not in a valley floor situation, at least had ready access to it. Some of the Dordogne sites were located, like Coygan, high above the valley floor with access to a plateau. Such sites, in Wales and southwestern France alike, tend to display only limited evidence of human use. The selection of Goat's Hole meets few of the Middle Palaeolithic criteria, but it does command a possible migration route from the lowland, which it abuts, to the Gower plateau. Its south-facing aspect may have been important also. It was clearly a site of major importance during the earlier Upper Palaeolithic for reasons that were at the same time topographic, exploitational (related to the site or its environs as a source of ivory), or sacramental (Aldhouse-Green 2000b). Hoyle's Mouth, as a small north-facing cave, does not make good sense as a settlement site. It has yielded one characteristic Aurignacian busked burin and a ^{14}C date of 27,900±600 BP (OxA-1024) on unmodified bone of indeterminate species could be coeval with this. There was, however, a relatively rich Late Upper Palaeolithic occupation of the 13th millennium BP when temperatures were comparable with those of the present day. The occupation seems explicable in this mild climatic context. The discovery of artefacts deep within the cave system is suggestive, however, of a ritual aspect to the site also.

DISCUSSION

As we have seen, Britain was normally a peninsula of Europe. There is no evidence that the hominids of the last quarter of a million years, or indeed before, made substantial water crossings to reach Britain during interglacial periods when sea level may have been a factor. By the time that modern humans arrived in the last glacial period, sea level lay at around –80 m and no such crossing was necessary. Wales, then, did not receive a single colonisation event as perhaps Australia may have done. Rather, it was repeatedly colonised, visited, and abandoned.

As part of Britain's highland zone, Wales was also repeatedly colonised by glacial ice. Such glacial events destroyed much of the evidence for archaic human presence, whilst sometimes tidying away hominid settlement debris and carnivore accumulations into convenient caves. This evidence is necessarily coarse-grained but it nonetheless offers us the opportunity to ask, if not always answer, some key questions relating to human peopling of the globe. I have in mind here particularly the issue of the climatic contexts of human presence. During the early Neanderthal period, ca. 200,000 BP, the implication is of colonisation events, relating neither to fully interglacial nor glacial environments, taking place in the far west by hominids migrating in pursuit of a westward moving region of oceanic climate. Later Neanderthal expansion into both westernmost Europe and the Middle East (Schwarcz and Rink 1998:65) seems to have been taking place at or after 60,000 BP and it may be that such territorial movement were fostered by the neothermal conditions of OIS 3. During the modern human period of the Upper Palaeolithic, there is evidence more of visitation rather than colonisation and these quests — whether sacred or secular — seem generally to have favoured interstadial periods. These were also the periods when biomass increased and game was plentiful. Indeed, my brief analysis of the setting of selected cave sites suggests that their primary purpose was not for shelter but lay rather in their strategic positioning to intercept the resources of the land, principally game but also raw materials.

The calibration of radiocarbon dates with the Greenland ice core sequence (Bowen 2000) has made possible the ascription of modern human presence events to their wider environmental context. I believe, however, that the powerful evidence of continuity at Paviland - of which the ^{14}C record probably offers no more than a fragmented vestige - (Aldhouse-Green 2000b; Jones 2000:263) is suggestive of visitation continuing even during the increasingly cold stadial events of the climatic downturn of 27–21,000 BP (Gamble 1991). One of the surprising outcomes of the recent Paviland dating program was the realisation that the 'Red Lady' is the earliest, in terms of direct dating of human remains, of the well known series of Gravettian ceremonial burials which extend from Wales to Russia and as far south as Portugal. Even allowing for indirect dating, it remains as early as any (Aldhouse-Green 2000b:241). Taken as a group the burials may reflect complex dynastic societies but it is interesting that the dated child burials of Sungir, Russia, and Lagar Velho, Portugal - which permit the inference that status may have been ascribed rather than achieved - seem to be late in the series (Pettitt and Bader 2000; Duarte et al. 1999). The Paviland burial was that of a young male, 'healthy, if dead' — to quote Erik Trinkaus (2000:191) — who may have been a person of status in his own right, perhaps a warrior hero, a successful hunter, an 'oikistes' (founder of a new colony) or shaman. The 'Red Lady', excavated in January 1823, represents the world's first scientifically recovered human skeleton. He has now revealed his age, his DNA, and much more besides (Sykes 2000; and papers in Aldhouse-Green 2000a). This early colonist now has a part to play in unravelling not simply matters of chronology but the very fabric of the society of earlier palaeolithic Europe.

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Who Cares What Time It Is?
The Importance of Chronology in Pacific Archaeology

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The oldest part of the General Cemetery, which was a few dozen yards behind the administrative building, was the one preferred by archaeologists for their investigations. These ancient stones, some so worn by time that you could only make out a few barely visible marks that could as easily be the remains of letters as the result of scratches made by an unskilled chisel, continued to be the object of intense debate and polemic, in which, with no hope, in the majority of cases, of ever knowing who had been buried beneath them, archaeologists merely discussed, as if it were a matter of vital import … the probable date of the tombs. Such insignificant differences as a few hundred years here or there were the motive for long, long controversies, both public and academic, which almost always resulted in the violent break-up of personal relationships and even in mortal enmities. Things got still worse, if that were possible, when historians and art critics decided to stick their oar in, for while it was relatively easy, in the circumstances, for the corporation of archaeologists to reach agreement over a broad concept of antiquity acceptable to all, leaving aside actual dates, the matter of truth and beauty created a veritable tug-of-war amongst the men and women of aesthetics and history, each pulling for their own side, and it was a not uncommon sight to see a critic suddenly changing his opinion simply because the changed opinion of another critic meant that they both now agreed.

José Saramago (1999:195)

BACK TO ARCHAEOLOGICAL BASICS: WHO, WHY, WHAT AND WHEN?

Having recently completed a joint paper with Rhys Jones for the British Academy where we summarised what we saw as some of the major issues in Australian and Pacific archaeology (Jones and Spriggs in press), I am confident we are of one mind on the importance of answering some basic and perhaps unfashionable questions of culture history before we all get too excited about easy speculation on the who, why, and what questions of our region's archaeology. Of the who, why, what and when questions, 'when' is the most basic.

It is often perceived that in this part of the world there is too much obsession with claiming the earliest - the earliest evidence of boat travel, the earliest humans in Australia, the earliest agriculture in New Guinea, the earliest pottery in the Pacific, the earliest metal in Southeast Asia, and so on. This obsession does have its unhealthy side — the Jinmium farce did not do Australian archaeology any service at all — but there is a reason for this seemingly exaggerated concern with when things happened. It is a reason that goes beyond ego, attention-grabbing and talking up projects to attract grant funding — common motivations for all of us though these might be. Knowing when something happened, even if only in the relative sense of before or after something else, is a vital component of understanding what happened, and hopefully explaining why. You will never know the why of a region's history if you haven't pinned down the when.

In the archaeologies of better-studied regions of the globe, such basic questions have perhaps already been worked out - although in our paper Rhys and I questioned whether this is in fact so in relation to the timing of such a seemingly well-studied 'event' as the Middle to Upper Palaeolithic transition in Europe. But in the Australian and Pacific regions, it is patently not the case that we have got the dates right in providing a framework for the study of culture change. This paper seeks to illustrate that the basic task of chronology-building remains to be achieved in the Pacific, and that different possible answers to the 'when' questions produce vastly different histories for the region.

I take here a consciously conservative position and a consciously non-post modern one. I don't believe that there are an infinite number of equally legitimate histories to be constructed for the Pacific from archaeological evidence. Also I believe we can often judge between those on show simply by examining the chronological underpinning which is offered in their support. To paraphrase Julian Barnes in his *History of the World in Ten and a Half Chapters*, although absolute truth may not be obtainable, 43% absolute truth must be better than 41% (Barnes 1989:246). If we don't believe this we are leaving the world to the charlatans and patent medicine sellers. Although the choices I make about what chronologies to accept for particular historical phenomena may turn out to be wrong, they are at least falsifiable as more evidence comes to hand.

'IT IS LATER THAN YOU THINK': TWO VERY DIFFERENT NEW GUINEA ARCHAEOLOGIES

The question of the age of pottery in northern New Guinea is clearly relevant to understanding the relationship of the pottery traditions of the area to those to the east and west, and more widely to views of New Guinea as an independent centre of important cultural developments or more as a receiver of diffused ideas and practices. The earliest author to claim a high antiquity for pottery manufacture in the region was Sue Bulmer (1977, 1985). At the time cultural sequences for any part of New Guinea or Island Southeast Asia were few and far between. A supposed association of pottery with early dates at Wañlek could be noted but not really evaluated in the absence of any regional chronological framework. And so it remained until further archaeological research in north New Guinea was undertaken in the late 1980s and early 1990s by Pam Swadling, Pawel Gorecki and a team led by John Terrell (see for instance Swadling 1997; Swadling et al. 1989, 1991; Gorecki 1992; Gorecki et al. 1991; Terrell 1998; Terrell and Welsch 1997).

Meanwhile, basic pottery sequences were being established and evaluated for regions to the west in Island Southeast Asia and to the east in the Bismarck Archipelago. Slowly but surely a regional overview was becoming possible. What is now the conventional view of the spread of pottery through the region from the Taiwanese and other Southeast Asian neolithic cultures to the Lapita sites of Tonga and Samoa was taking shape (Bellwood 1997; Spriggs 1989, 1990, 1996a, 1997a). The north New Guinea results seriously challenged the conventional picture, by claiming — initially at least —that pottery was in fact earlier in New Guinea than it was in areas immediately to the west and the east. Sue Bulmer had suggested the possibility of a link to the Jomon pottery tradition of Japan, on technological and chronological grounds (Bulmer 1985:130). Supporters of an indigenous origin for the Lapita culture of the Bismarck Archipelago were happy to see an immediate origin in New Guinea for this cultural complex rather than a derivation from the Southeast Asian neolithic. Pam Swadling seemed content to maintain a link to the Southeast Asian neolithic but only if that was of uniformly early date across the region (1996:52).

It all comes down to the dates. We are asked to choose between histories postulating very early and presumably direct links between Japan and New Guinea, or a widespread very early cultural horizon across Southeast Asia and New Guinea, or the independent invention of pottery making in New Guinea and its subsequent spread to the east (and possibly to the west too into Southeast Asia, according to Terrell (1998)), or the view that pottery in northern New Guinea resulted from contact with the Southeast Asian neolithic cultures which spread through the area from west to east ca. 3500 BP to 3000 BP. There are implications too which flow from these different scenarios for the history of initial human settlement in areas further out in the Pacific, such as Vanuatu, New Caledonia and Fiji.

I have discussed elsewhere the problems with the association between radiocarbon dates and pottery assemblages in these northern New Guinea sites due to unrecognised site disturbance (Spriggs 1996a:43–4, 1996b: 329–34, 1999:17–18). Such problems of site disturbance are also found in some Solomons and Bismarck archipelago sites such as Kilu (Wickler 1990, 1995) and Pamwak (Fredericksen et al. 1993) but have not caused interpretive problems there as the pottery sequences were already well-understood, and so spurious associations of early dates and late styles are easily recognisable.

Terrell had at first claimed that a typological sequence of styles he developed from his research along the northern New Guinea coast started with Sumalo ware. This was seen as part of a widespread red-slipped pottery complex found in Southeast Asia and the Western Pacific and in its latest phases related to Lapita (see for instance Terrell 1998:207–9, 1999:57; also his original press release of 22 October 1996 cited by Shutler 1999:528). The suggestion was made that 'the plain and red-slip industries of Island Southeast Asia are either older than Spriggs, Bellwood and others believe (only 4,000 to 4,500 years) or else the art of pottery making in the plain and red-slip tradition got started in northern New Guinea and later spread to southeast Asia and the Bismarck Archipelago' (Terrell 1998:208 — text references removed). His excitement was infectious, leading Swadling (1997:9) to suggest that the claimed association of Lapita pottery and Austronesian languages 'may soon change' on the basis of a 1996 personal communication from Terrell that he had evidence 'likely to confirm the pre-Lapita antiquity of pottery on this coast.' A few pages later, quoting Terrell again, this became 'pottery sites predating Lapita were present on the Sepik north coast' (Swadling 1997:12).

But it was not to be. The radiocarbon dates in the case of Sumalo ware turned out for once to be consistent between sites and laboratories, but instead of being of pre-Lapita age as anticipated, they centred on 1220 BP rather than 5000 BP (Terrell and Welsch 1997:560–1). If ever there was a classic illustration of the need to get the dates right first, this is it. According to Terrell, the local pottery sequences begin with Sumalo ware - as well they might if there is an association with other dated north coast wares thought perhaps to be associated with an east to west spread of Austronesian languages along that coast from the Vitiaz Strait in post-Lapita times (cf. Lilley 1999).

Lilley suggests that time is the critical variable in the seeming variability of the various north coast trade pottery industries compared to the spread of Lapita or the south coast Papuan pottery styles: 'It is the speed with which populations moved, not the distance over which they moved, that seems to be critical to the way issues of identity affected the archaeological expression of migration in the cases in focus' (Lilley 1999:31). On the basis of the radiocarbon dates Lilley suggests a slow process compared to the Lapita and Papuan examples. He proposes a need for newly-established migrants in the north coast case to have differentiated themselves from their source communities in order 'to establish the new interaction networks they use to survive and prosper' (Lilley 1999:31) by establishing emblems such as distinctive pottery forms from the outset. By such means they were able to insert themselves into previously established networks of interaction with which they already had links. Whether this explains all of the north New Guinea pottery industries is unclear. There may well have been earlier industries in the region (post-Lapita but pre-Sumalo). After all, there are the two sherds of Lapita pottery known from Aitape which must date to earlier than 2700 BP (see below).

Another factor in pottery variability as recorded ethnographically in north New Guinea has been discussed by Pétrequin and Pétrequin (1999). Apprenticing themselves to a series of potting communities using a wide range of techniques to produce somewhat similar vessel forms, they found that there are important differences in terms of length of apprenticeship needed to learn particular techniques of potting. Some of the techniques seen as the most ancient — recall Bulmer's suggestions of links to early Holocene Jomon pottery of Japan — turn out to be those most easily learned from a brief acquaintance with pottery manufacture. It is hardly coincidental that the inland non-Austronesian pottery makers are the ones using these techniques. They represent a straightforward transfer of technology. But the techniques most difficult to pick up, passed on after long apprenticeship from mother to daughter, are precisely those likely to have been associated with direct migrations of Austronesian communities along the north coast. It is these different forms of transmission noted by the Pétrequins and the process of differentiation of form from source communities as an emblematic strategy during migration as argued by Lilley, which are likely to have produced the diversity of present-day pottery styles and techniques in north New Guinea. These factors seem adequate to explain current patterns rather than the great antiquity of pottery production which Swadling (1997:12) and Kaufmann (1999) argue for.

TAIWAN TO TONGA: TAKE THE FAST TRAIN

As the corpus of radiocarbon dates increases for neolithic sites in Southeast Asia, regular reconsideration is needed of the body of data and its interpretation (cf. Bellwood 1997). In 1989 I presented a list of dates then available for island Southeast Asia which I considered showed a cline in age of neolithic sites from Taiwan to eastern Indonesia, and by extension further out into the Pacific (Spriggs 1989). The start of the neolithic in Taiwan was taken to be before 5500 BP, its spread to the Philippines and Sulawesi to have taken place by 5000 BP, and — in the absence of directly dated assemblages in much of eastern Indonesia — its further spread to Maluku and west New Guinea to have been accomplished ca. 4000 BP. This would provide time for its spread further east as the Lapita culture of the Bismarck Archipelago to have taken place by 3500 BP.

I considered the spread of the Lapita culture explicitly in presenting the available dates in 1990 (Spriggs 1990), and subsequently updated the dates for both Southeast Asia and the western Pacific (Spriggs 1996a). Most recently (Spriggs 1999) I have reviewed the evidence again in the light of the second edition of Peter Bellwood's *Prehistory of the Indo-Malaysian Archipelago* (Bellwood 1997). The quality of the island Southeast Asian dates, in terms of presentation of enough context to allow some evaluation of their likely accuracy, is generally poor, as I noted in 1996. Were one to mete out the degree of chronometric hygiene to which the East Polynesian radiocarbon corpus was subjected by Spriggs and Anderson (1993) then few determinations would survive and no outline chronology would be possible.

As more dates are available a degree of screw-tightening becomes possible on this recalcitrant body of evidence. Bellwood has done this in his 1997 publication, and a reconsideration of three previously accepted and seemingly contradictory island Southeast Asian radiocarbon determinations supports his viewpoint (Spriggs 1999:19). Bellwood now believes that south of Luzon in the northern Philippines, the earliest neolithic assemblages in Sulawesi and Maluku are derived from the Yuanshan culture of Taiwan and date to 4000 BP rather than 5000 BP as previously suggested (Bellwood 1997:219–36). The beginnings of the Taiwan neolithic still date to considerably earlier than 5000 BP, although the earliest phases there are not dated, and plausibly derive from earlier assemblages in mainland China.

Along with a reconsideration of the spread of the island Southeast Asia neolithic has come a re-assessment and further dating of sites associated with the spread of the Lapita culture from the Bismarcks to Tonga and Samoa: Specht and Gosden (1997) for the Bismarcks, Bedford and Spriggs (2000; see also Bedford et al 1998) for parts of Vanuatu, Sand (1997) for New Caledonia, Anderson and Clark (1999) for Fiji, and Burley et al. (1999) for Tonga, and, by extension, Samoa. The current position (summarised from Spriggs 1999) would have Lapita starting in the Bismarcks at ca. 3300-3200 BP (although Kirch, pers. comm., would still argue for a start as early as 3550–3450 BP), spreading to the Reef-Santa Cruz Group at 3150 BP, Vanuatu and New Caledonia by 3000 BP, Fiji at 2900 BP and Tonga by 2850–2800 BP. Over the whole range of its distribution, Lapita dentate-stamped decoration on pottery would have disappeared at about 2700 BP.

The refining of chronologies for the island Southeast Asian neolithic spread and its further expansion into the Pacific as Lapita leads to a very different view of cultural and linguistic change in the region than previously appeared to be the case. How can one demographically model a migration from Luzon to Tonga in just over a thousand years, with regular and continuing settlements along the route? How does one explain the seeming thousand-year pause in Luzon before this explosive settlement push? Interesting here is the attenuation of material culture and the disappearance of rice from the crop roster as settlement spread south of Luzon. The spread from China into Taiwan of this culture is also not well-dated. Certainly it would seem to be the case on at least linguistic grounds that there was a considerable pause in Taiwan before the further push on to Luzon, given the large number of primary Austronesian sub-groups on the island and the large number of early neolithic assemblages there, which although undated must belong to a period pre-5000 BP. After that time there are radiocarbon dated assemblages which are clearly descended from them.

Until the recent re-dating of key western Pacific sites the time span of Lapita was usually given as 3500 BP to 2500 BP (Kirch 1997), with some arguing for an extension of the manufacture of dentate-stamped Lapita pottery until about the time of Christ or even later (Galipaud 1990:140; and most recently Frimigacci 1999). Thus it seemed that Lapita was a phenomenon of the longue durée with a time span of 1000 or even 1500 years. At one stage Kirch and Hunt (1988) championed an instantaneous spread of this culture from New Britain to Tonga around 3500 BP. The current picture is of a spread from the Bismarcks to Tonga and Samoa which took 400–500 years — thus very different demographics need to be invoked here than with the earlier model! As well, it now seems that whatever the Lapita design system meant to its pottery makers and users, its meaning ceased after some 500 to 600 years in the Bismarcks, 300 years in Vanuatu and New Caledonia, but after only ca. 100 years following settlement of Tonga. Again one would have to imagine very different social processes at work to those where a much longer period of use of decorated Lapita pots had occurred, as with the earlier models. Again, correcting the duration of Lapita pottery use, requires a change the models used to explain Lapita.

But the process continues. An argument has been made that dentate-stamped pottery continued later on the island of New Britain than elsewhere (Specht et al. 1991:287; Torrence and Stevenson 2000; see also Anson 1999). What was a general argument for Lapita has now become a local one. But given the work of Summerhayes (2000) in defining early, middle and late phases of Lapita with general application to the entire Lapita region, those who argue for continuing production of such pottery in New Britain alone until perhaps the time of Christ will have to demonstrate the continuation of the stylistic sequence beyond Summerhayes' 'late Lapita' which indeed seems to finish everywhere ca. 700 years before this date. So far they have not done so, and one can only suggest that the relevant sites are disturbed, with earlier Lapita ceramics mixed in with more recent materials. This would certainly explain the presence of Lapita, fingernail-impressed and applied-relief in the same (2315 BP to 1950 BP) levels at Watom without needing to invoke models such as two separate but contemporary populations, as Anson (1999) does. The occasional heirloom piece retained by later generations, as suggested earlier by Green and Anson (1991:175) for Watom, can also be invoked — perhaps more convincingly at other sites — to explain the odd piece of Lapita pottery in post-Lapita contexts. In Vanuatu, for instance, single Lapita sherds have been found in immediately post-Lapita contexts at Ponamla on Erromango and Malua Bay on Malakula in the absence of any deposits of Lapita age (Bedford et al. 1998).

CONTEMPORARIES OR SUCCESSORS? WHERE MANGAASI FITS

When Garanger (1972) defined the Mangaasi or incised and applied-relief pottery style of central Vanuatu and stressed its links to other regions of Melanesia, his chronology was pinned to very few absolute dates. He produced the first coherent cultural sequence for any part of Vanuatu. It seemed to show, on the basis of two radiocarbon dates from different central Vanuatu sites, that the Mangaasi style in that area pre-dated a second style of pottery called Erueti by ca. 300–400 years, and started at ca. 2600 BP. This was a surprising result as Erueti was clearly derived from the Lapita style of pottery, dated elsewhere to about 3000 BP or even earlier. The only possible interpretation based on the radiocarbon chronology was that the Lapita/Erueti style and the Mangaasi style represented migrations of different populations to Vanuatu, one perhaps Polynesian in appearance and the other 'Melanesian' and possibly linked on grounds of similar pottery style to populations in the Markham Valley of PNG. At the time, of course, radiocarbon dates were hard to come by. Another problem was that the Mangaasi and Erueti styles were abstractions from assemblages that were less clearly differentiated than they seemed. There was a minor component of Erueti style vessels at the Mangaasi site, but Erueti itself produced a quite mixed assemblage. Unique at the site were a range of vessel forms and lip decoration — these are what define the Erueti style — but, in addition, a range of Mangaasi-style decorated pottery was also recovered.

As further research took place in the Island Melanesian region, other such 'mixed' assemblages were coming to light. In 1984 I raised the possibility that instead of stratigraphic mixing, another possibility might be that the sites were in fact transitional between Lapita and Mangaasi (Spriggs 1984). I noted that non-ceramic artefacts at sites such as Mangaasi were in fact very similar to those first found on Lapita sites and could represent cultural continuity. Along with new sites were coming new dates for assemblages comparable to Mangaasi and Erueti assemblages and the dates for the start of Mangaasi were appearing more and more anomalous. A statistical comparison of various 'Mangaasi-like' pottery assemblages by Wahome (1998) demonstrated similarities between several of them, but these seemed obviated by very different ages of the assemblages, particularly that of the Mangaasi type site itself.

It was decided to return to Mangaasi and collect further dating samples to check the original determinations and to prospect for earlier assemblages in the immediate vicinity of the type site. Systematic test pitting from 1996 to 1999 has revealed that there is considerable horizontal as well as vertical stratigraphy represented at Mangaasi. As the coast prograded because of tectonic uplift, the coastal settlement shifted to follow the shoreline. Garanger had excavated at the seaward edge of the pottery-bearing deposits and earlier components of the site were found inland and to the southwest across a small creek. By mid-2000 an additional 47 ^{14}C dates had been processed from the site and a very different picture has emerged of the pottery sequence for central Vanuatu. The two early dates for the Mangaasi pottery style are now seen to be anomalies. At 2550 BP the part of the site which Garanger was to excavate had not yet emerged above the waves and was in fact part of the reef flat out in front of the village at that time.

Also at that time the pottery style in use was early Erueti, itself derived from a Lapita assemblage whose plainware component we have found at the immediately adjacent site of Arapus dating to about 3000 BP (Bedford and Spriggs 2000). Later Erueti pottery developed a greater component of incised designs executed on an increasingly limited range of vessel forms. A convenient tephra layer separates this material from what becomes recognisably Mangaasi-style pottery with the addition of applied-relief decoration. All this occurs before the emergence of the part of the site excavated by Garanger, which thus must date to about 1800 BP rather than 2600 BP. Thus the earliest date from Mangaasi of Garanger was too early, whereas his single date for the Erueti site turns out to be a reasonable ballpark figure. The chronology is thus reversed: the Lapita-derived Erueti style transforms over time into Mangaasi, seemingly the latest pottery style in central Vanuatu, with pottery going out of use somewhere around 1500 BP to 1200 BP. This is well before a further marker tephra blanketed the Mangaasi site, associated with the eruption of the Kuwae volcano in or around 1452 AD.

Thus two migrations become one, given that there is no evidence of pre-Lapita settlement of Vanuatu, Fiji or New Caledonia, despite research programs targetted at examining the possible locations of earlier sites. The sorting out of the pottery chronology at Mangaasi and at other Island Melanesian sites now reveals a different but equally interesting picture, one foreshadowed in my 1984 paper which itself built on earlier research by Golson (1972), Kennedy (1983), Specht (1969) and others. As well as Lapita pottery changing 'in step' across its range — the point made by Summerhayes' (2000) re-analysis of Lapita sub-styles — the post-Lapita styles also seem to display a common sequence across island groups. There are exceptions — areas drop out of contact, and on occasion drop in again in the ceramic equivalent of linguistic levelling — but in broad terms we can see clear similarities in pottery styles from Manus to Mangaasi, and less certainly perhaps on to New Caledonia and Fiji, for over 1000 years after the end of the Lapita pottery style. It is really only after about 1500 BP that cultural drift really sets in and we start to see the amazing diversity of pottery and other cultural styles for which Island Melanesia is celebrated today. There are important implications of these findings in terms of continuing cultural interaction over large areas of the western Pacific in post-Lapita times, with its attendant genetic and linguistic consequences.

Polynesia drops out of this interaction sphere first, somewhere before the development of a major component of incised decoration in post-Lapita styles, say 2300 BP or so. Fiji shows intermittent contacts to some later date (Best 1987), but Clark (2000) argues that this is not reflected in the pottery styles. New Caledonia appears isolated from changes in pottery style further north in the immediately post-Lapita period, probably because of the cessation of pottery making in southern Vanuatu sometime prior to 2000 BP. But there appears to be at least fleeting contact between its potters and those of central Vanuatu after 1900 BP. This is reflected in the occasional presence in New Caledonia of Mangaasi decorative motifs and the use of particular kinds of handles on pots which occur at the Mangaasi type-site at the same time (the 'Plum tradition' - Sand 1996:53).

It is tempting to see the genetic distance between Polynesia and Island Melanesia being in part a product of the progressive breakdown of regular contacts, and the transitional position of Fiji as a result of the further contraction of regional interaction and diminution of gene flow across the region. Two migrations can therefore be replaced by one, but with continuing gene flow from the Bismarck Archipelago and Solomons population centres in the north. Detailed stylistic comparison has not yet been extended to the north coast New Guinea pottery assemblages, although the proponents of early dates for them discussed above seem convinced they are culturally related to Mangaasi assemblages (e.g. Gorecki 1992). It could be that they too participated in this post-Lapita 'community of culture' over much of Melanesia.

'THE EMPEROR HAS NO CLOTHES': THE SETTLEMENT OF EAST POLYNESIA

Until 1993 there was a seeming consensus that the dates for settlement of Eastern Polynesia needed to be pushed back to avoid a long pause following Lapita settlement of Western Polynesia. All new early radiocarbon dates were seized upon as evidence for the missing settlement phase and older determinations in support of the model were accepted uncritically. Spriggs and Anderson (1993) sought to cast a critical eye over this trend and concluded that East Polynesia had indeed been settled after a long pause, of over 1000 years, in Western Polynesia. The two competing chronologies again produce very different histories for the region (Anderson 1995). Just as in the well-known tale alluded to in the title of this section, once we challenged what everyone thought they saw, suddenly the lack of substantive raiment on the early chronology hypothesis became evident to (nearly) all. At the least a great deal of caution has ensued in presenting radiocarbon dates (or rather perhaps unwarranted extrapolations from them) for early settlement sites in East Polynesia. Subsequent re-dating of claimed early sites in East Polynesia has supported the later chronology (Rolett and Conte 1995; also Anderson pers. comm. on projects in French Polynesia; Higham et al. 1999 for New Zealand). There are also sound linguistic arguments for a substantial pause between the settlement of West and East Polynesia (Pawley 1996).

Irwin, long a proponent of the early settlement model (e.g. 1992:80–82), has recently considered various contrasts between Island Melanesia-West Polynesia and Eastern Polynesia (Irwin 1998). He notes that as one moves east, except at the Hawaii and New Zealand margins of East Polynesia, there is considerably more ocean to be searched and much less land. He illustrates this by drawing an imaginary circle around Rarotonga in the Cook Islands of 800 km radius, incorporating some two million square km of ocean. Within this radius there are 10 habitable islands with a land area of 240 km^2. Irwin contrasts this with a circle of the same radius drawn around the Ha'apai Group of Tonga to the west, which incorporates hundreds of islands in the Tongan, Samoan and Fijian archipelagoes, which combined make up a land area of 24,000 km^2 (Irwin 1998:124). Irwin notes that similar results would be obtained by contrasting many other East Polynesian islands and island groups with Western Polynesian ones. He concludes (Irwin 1998:132) that the early East Polynesians:

> made more dangerous ocean passages in a wider range to discover more distant islands. Their circumstances were expanding ocean area, increasing navigational experience and skill, and more elapsed time during the period of colonisation. Yet the implied need for on-going communication among related communities would not have lessened. Adjustments had to be made to the changing social and environmental circumstances of markedly increasing isolation. Again, more space implies more time.

Later in the same paper he notes that 'marked increases in social isolation and in the costs of communication may have acted as constraints for establishing settlements. It took time for colonists to adjust to changing geographical circumstances and to develop social strategies for living in greater isolation on islands of the Pacific Plate' (Irwin 1998:136). After all this it is most surprising to see him retreat in the conclusions to his earlier position (Irwin 1992:88–9) favouring the continuous settlement model for East Polynesia. The 1998 paper was written before the new dating evidence of Burley et al. (1999) for Tonga. This would suggest that the 500 year 'pause' Irwin would allow before settlement of Eastern Polynesia, based on Kirch's Mangaia dates which were criticised by Spriggs and Anderson (1993), would have to be reduced to less than 350 years. Time enough to make the adjustments Irwin believes were necessary before the move further east?

Irwin gives the best arguments yet for the social basis for the pause in settlement between West and East Polynesian settlement, then rejects the almost inevitable conclusions. One is reminded of Erik Satie's comment on the composer Ravel, that 'he has refused the Légion d'Honneur but all his music accepts it' (quoted in Cohen and Cohen 1980:298)!

NATURE OR NURTURE: THE TIMING OF PACIFIC ENVIRONMENTAL CHANGE

A clearer picture of the environmental impacts of human colonisation of the Pacific Islands has developed over recent years. Reviewing the current state of knowledge, I have written (1997a:85–6 ; 1997b) of a pattern of pioneering agricultural settlement of the Pacific resulting in immediate massive environmental impacts. Forest clearance through fire for agricultural and other purposes led to greatly increased erosion rates, sometimes necessitating the abandonment of an area for many hundreds of years. Initially this was not a problem as new settlement areas on islands, indeed new islands, were not in short supply. Later, in more fully occupied environments such a land use pattern was unsustainable, and so conservation practices such as terracing had to be developed to allow continued settlement. I argued that the evidence suggested that this occurred immediately upon settlement by agriculturalists, Lapita or later, throughout the Pacific. I noted that the evidence for environmental change from previously settled parts of the Pacific in Near Oceania (New Guinea, the Bismarcks and the main Solomon Islands - Green 1991) seemed to be no earlier than that in areas not settled pre-Lapita. Continental New Guinea would of course provide an important exception here. It was thus not the presence of people in the islands, but the presence of a particular lifestyle which caused the major impacts.

An effect of forest clearance, direct predation by humans and predation by and the other impacts of introduced vertebrates such as pig, dog, chicken and the Polynesian rat appears to have been the mass extinction of many species. Pacific land and sea birds were particularly affected, as well as some other vertebrates such as lizards, land crocodiles, turtles and various endemic species of rats and bats.

This general model of the environmental impact of agricultural settlement has recently been questioned in a review of Spriggs (1997a) by White (1999:14):

> One claim of some dubiety is the supposed impact of Lapita settlers on local environments, about which Spriggs makes a considerable deal. However, except for one set of sites in the Arawe Islands, these impacts are in Remote Oceania, where Lapita pottery marks the first settlers. Noticeable too is that evidence of the impact is frequently geomorphic and not faunal, even where Lapita settlers were primary. Faunal impacts are recorded, but occur before, during, and after Lapita in Near Oceania at least.

White rightly stresses that the timing of all this is crucial, and more on this below. But there was in fact more information drawn upon at the time than he suggests from the Near Oceanic parts of Island Melanesia, all indicative of a major phase of human impact on the environment. This dates to around 3000 BP for New Ireland on pollen evidence (to which White does not refer at all), and ca. 2100 BP for Guadalcanal in the Solomons which is when we first get evidence there of agricultural activities taking place (Spriggs 1997a:86–7, 148). Although not reported in my 1997 book, for Manus too there is pollen evidence of a major episode of forest clearance some time later than a date of about 3600 BP (cited in Spriggs 1996a:40, 42).

Greater effort clearly needs to be expended on dating relevant pollen cores and conducting further palynological research in Near Oceania as well as Remote Oceania. The cores we do have generally show extensive vegetation change around the time we would expect agriculture to have been introduced to the various islands of the Pacific. In Remote Oceania this coincides of course with initial human settlement. As Hope et al. (1999:396; see also Stevenson 1999 for a detailed case study for New Caledonia) conclude for the western Pacific: '… it seems that people have been the main cause of deforestation and the spread of grasslands and Talasiga. The site records suggest that clearance soon after 3000 yr BP was fairly common, but not universal.' Hope et al. admit that problems of interpretation are often caused by inexact dating of the cores: 'most problems experienced by archaeologists in assessing these data sources are due to insufficient resolution of both pollen and time. Careful dating is needed to accurately find the time of initiation of these regimes … . The lack of precise chronologies diminishes the contribution that sedimentary sequence studies have so far made to archaeological reconstructions' (Hope et al. 1999:397).

The timing is also crucial for the assessment of faunal impacts. Did the extinctions occur gradually over a long period — as White (1999) seems to be implying — or was there a 'blitzkrieg' situation as described by Martin for the extinction of the American megafauna (e.g. Martin 1984)? Evidence from the Ha'apai Group in the Tongan Archipelago suggests that such mass extinctions occurred within '300 years or less' of Lapita settlement (Steadman 1999:381), but in some other archipelagoes the chronology is as yet uncertain. For Eastern Polynesia the answer depends on the model used for the timing of human settlement. As Steadman writes (1999:382 – text references removed): 'Many species of birds became extinct in East Polynesia from 900-600 BP. The discrepancies in estimates of the rapidity of extinction arise from differing opinions on when humans first arrived in East Polynesia, ranging from 2500–1000 BP. The younger estimates of human arrival are compatible with a blitzkrieg model of extinction. The older ones are not, and invite explanations of how species could survive so long in human presence.'

For Near Oceania we do not yet have the evidence to assess whether there is a similar wave of extinctions associated with the start of agriculture in the area at about 3000 BP. But long faunal records from 35,000 BP to the early or mid-Holocene in the islands of Near Oceania do not record any clear evidence of vertebrate extinctions (with the exception of *Rattus sanila* on New Ireland, apparently rendered extinct by competition with the humanly-introduced *Rattus praetor* (Flannery and White 1991)) for at least the first 25,000 years of human occupation.

Steadman et al. (1999) are somewhat equivocal as to the timing of land bird extinctions on New Ireland, in part because of hiatuses in site occupation during the critical mid-Holocene period. They did not provide bands for extinction of particular species narrower than within the last 10,000 or 6000 years. Despite White's earlier quoted gradualist view of extinctions before, during and after Lapita in Near Oceania, the data presented so far on the New Ireland birds certainly make it possible that we are looking at a Lapita/agricultural phase of extinctions there too, with few earlier extinctions. More dated assemblages from the usually missing early to mid-Holocene period will enable resolution of the issue. The fact that the birds of the Bismarcks and Solomons had evolved in the presence of various predators, such as endemic species of rats, would likely have made them less susceptible to the wave of extinctions which engulfed the birds of many of the islands of Remote Oceania, where rats were first introduced at initial human settlement (Steadman et al. 1999:2566–7).

Other studies in the same Near Oceanic region are suggestive of significant faunal impact around 3000 BP, such as Flannery and Wickler (1990) and Wickler (1995) for Buka at the northern end of the Solomons, and Roe (1992, 1993) for Guadalcanal in the central Solomons. Although Flannery and Wickler (1990) could not tie down the date of the extinction of two endemic rat species more exactly than to a range of 6670 BP to 1860 BP because of a hiatus in occupation at the Kilu site, it is noteworthy that the major change in the local economic system identifiable on Buka occurred with the beginning of Lapita occupation at 3000 BP. There are other sites on Buka and adjacent islands dating to about 2500 BP onwards with faunal remains and none show any evidence of the now-extinct rats either (Wickler 1995:631-53), thus further narrowing down the time frame in which they are likely to have become extinct. Bird extinctions have also occurred on Buka, but the sample is too small to delineate a time span for them. For Guadalcanal in the main Solomons the evidence is that the later spread of agriculture there around 2100 BP had an equally significant impact on the fauna of another long-settled region (Roe 1992:94–6, 1993:119).

CONCLUSIONS

It was my contention at the start of this paper that chronology-building, while unfashionable at present, is absolutely basic to theory-building in Pacific archaeology. We will never get to answer the 'why' questions or even the more-than-basic 'what' questions unless we resolve a whole series of chronological issues of timing and duration of archaeological phenomena in the region. I have tried to illustrate my point by giving a range of examples of key questions in Pacific archaeology where different chronologies lead to very different interpretations of the causes and consequences of cultural change. The reader does not have to agree to my particular 'take' on any of these questions to appreciate the point being made. As Rhys and I have done, you have just got to take the time.

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The Terminal Age of the Fijian Megafauna

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THROUGHOUT THE Pacific many vertebrates, especially land birds, became extinct during the period of prehistoric human settlement — the late Holocene in the central and eastern Pacific islands — and the general plausibility of cultural causation is hardly in doubt. But what may be generally true need not be true in particular instances, or of particular groups of taxa or islands. Each new case needs to be assessed according to its evidence. The latest case, and a substantial one in terms of the diversity of sites and remains, is from Fiji.

The Fijian archipelago dates back ca. 40 mya to the Eocene. In more recent times, it has been several times its current landmass (18,000 km^2) as the result of lowered Pleistocene sea-levels. Of considerable size and older than any Oceanic landmass east of the Solomons except for New Zealand and New Caledonia, it ought to contain remains of a diverse terrestrial fauna. Yet, until very recently, there had been almost no evidence of the fossil fauna of Fiji. However, research in the caves of Viti Levu, as part of the Indo-Pacific Colonisation Project at the ANU, directed by Atholl Anderson, is now beginning to document the former existence of a rich terrestrial vertebrate fauna. The fossil sites are described in Worthy and Anderson (1999) and there are preliminary faunal descriptions in Worthy et al. (1999). Fuller details of provenance and formal descriptions of the taxa will appear in due course. The purpose of this paper is to outline current efforts to determine the terminal age of the fossil deposits and to consider that evidence in relation to the human settlement of Fiji.

THE FOSSIL FAUNA

A project to locate remains of the pre-human vertebrate fauna of Fiji began in 1996 with the systematic searching of caves in the western area of Viti Levu, mainly in and about the Sigatoka Valley. It was later extended to eastern Viti Levu and Vatulele Island. The more important sites discovered to date are at Volivoli in the lower Sigatoka Valley where there are two fossil sites, Volivoli Fossil Site 1 and Qara-ni-Vokai, and in the Wainibuku area of eastern Viti Levu where there are important deposits in Wainibuku Cave and Udit Cave (see Worthy and Anderson 1999 for locational and stratigraphic data). Another site (Qara-I-Oso II) was found in the limestone outcrop at Tau, southwest of Nadi (Fig. 1).

Remains of at least nine taxa, unknown within the historical period in Fiji, have been recovered from these sites. The megafauna, a comparative term, comprises a land crocodilian, estimated from specimens to have reached up to 2-3 m in length, a giant iguana estimated to have reached 1.5 m in length, a tortoise, probably of meiolanian type, a giant megapode (one of the largest known from the Pacific), and a giant flightless pigeon, comparable in size and probable weight to the dodo and solitaire of the Mascarenes. Also in the investigated faunas are another, smaller, megapode similar to but stouter than the widespread *Megapodius freycinet*, one or more new species of fruit pigeon, a rail, and a giant frog. This list excludes several species which became extinct historically: the whistling duck, *Dendrocygna arcuata*, and a species of barred-wing rail, *Nesoclopeus poecilopterus*, plus the mastiff bat, *Chaerephon bregullae*, which is now restricted to Vanua Levu and Taveuni (Worthy et al. 1999, Worthy and Anderson 1999).

We describe the chronological research on the Fijian megafauna by site and technique.

Figure 1. Viti Levu, Fiji, showing the location of fossil deposits and archaeological sites mentioned in the text.

THE AGE OF THE FOSSIL FAUNA

At Volivoli there are two cave sites, both parts of the same complex, but at different levels in separate chambers. Volivoli Fossil Site 1 is ca. 100 m along the main cave shaft, which angles gently downward, while Qara-ni-Vokai is a small chamber at the base of a 7 m deep sinkhole. The chronology of these, plus Qara-I-Oso II, and Udit Cave is discussed here. There is no chronology as yet for other fossil sites in Wainibuku or on Vatulele Island. Anderson, Sorovi-Vunidilo and Worthy conducted the fieldwork and collected samples for the various dating techniques employed. Ayliffe undertook the U-series dating and Questiaux and Spooner the OSL measurement.

VOLIVOLI FOSSIL SITE 1

From the consolidated nature of the deeply weathered red clay, which could only be pecked and levered out in small pieces by trowel, and the apparent remnant position of the deposit as a vertical slab on the cave wall, it was thought that this site was likely to be of considerable age. It contained fragmented crocodilian and tortoise remains as well as those of giant megapode, giant pigeon, giant iguana and giant frog, none of which was in position of articulation.

Radiocarbon dating

Two bone samples from Volivoli 1 were submitted to Beta Analytic in July 1997 for AMS dating (Table 1): a 3.9 g piece of a bird femur (VOL-ABIRD), and a 3.5 g piece of crocodile bone (VOL-ACROC). Pretreatment disclosed that these samples contained no collagen suitable for dating. A charcoal sample (0.0113 g), of charred fibres was collected at approximately 10 cm deep in the clay wall from amongst bone fragments, and at the same level as remains of a giant extinct pigeon. The sample was submitted to Beta Analytic for AMS dating (FIJICHAR-1). The laboratory stated (16.7.1998), that the sample "provided plenty of carbon for AMS counting." It had an expected $\delta^{13}C$ value of -24.5 per mill. and returned the conventional radiocarbon age of 350±50 BP (Beta-118682).

This was the only charcoal sample seen in the sediments at this site and it is clearly far too young to refer to the fossil bone remains. It is possible that the near vertical face of the damp clay containing the fossil bone fragments has slumped periodically, and in doing so has enclosed material of much later origin which

Table 1. Radiocarbon determinations on fossil remains and sites in Fiji.

Sample	Provenance	Age BP	$\delta^{13}C$	Lab. No.
Charcoal				
FIJICHAR-1	VV-1, 10 cm	50 ±50	-24.5	Beta-118682
12QnV	V-Term, 0-25 cm	20,020 ±660	-24.0	ANU-11010
7 QiO	QiO-II,TP-130 cm	660 ±60	-24.0	ANU-11014
Organic Sediment				
15 QnV	QnV-Term, 0-25 cm	25,540 ±630	-24.0	ANU-11011
14 QnV	QnV-TP-1, 80-90 cm	25,880 ±410	-24.0	ANU-11012
13 QnV	QnV-TP-1, 60-70 cm	26,340 ±390	-24.0	ANU-11013
Bone		**Material**		
VOL-ABIRD	VV-1, 1-15 cm	Bird femur shaft, 3.9g		Beta
VOL-ACROC	VV-1, 1-15 cm	Crocodile bone, 3.5g		Beta
FIJIFOSS-1	QnV-1, 10-20 cm	Giant iguana femur frag. 6.5g		Beta
FIJIFOSS-2	QnV-Term, surface	Giant iguana humerus frag. 2.7g		Beta
TAU-IGO1	QiO-II, 50 cm	Giant iguana corac.-scap. 4.0g		Oxford
R24484/1	Udit Cave	Megapode n. gen. Tib/tar.		Rafter

adhered to its surface or fell into a crack in the sediments. In the present case it is possible that the charred fibres came from a recent native torch. The site has been known to the local people for many years and they walk through the cave from time to time. In addition, there is plenty of late period pottery, some adze pieces and midden distributed about the cave entrance and up to 20 m into the shaft.

Optical dating

One sediment sample from Volivoli cave was processed by optical dating. The principle of this technique is that a light-sensitive population of trapped charges (Aitken 1998) accumulates in quartz crystals when stored in the dark, i.e. when buried, and that this population is proportional to the total radiation dose accrued since the crystal was last exposed to light: this dose, termed the palaeodose (P), is measured in the laboratory. The intensity of natural irradiation, the environmental dose rate, must also be measured in order to calculate the luminescence age.

Despite the sensitivity of the OSL signal to light (Spooner 1994), complete pre-depositional bleaching could not be assumed for this sample on account of its deep-cave location. Consequently, of the range of optical dating procedures that have been introduced over the preceding decade, only methods applicable to single-aliquot and hence single grain dating were considered here. We chose the 'improved' single-aliquot regenerative-dose (SAR) procedure (Murray and Wintle 2000) which has developed from that presented by Murray and Roberts (1998), because despite some recent reservations regarding this procedure (Spooner et al. in press) we consider it to be the most reliable of the existing optical dating protocols applicable to single-aliquot and single grain dating.

Sample preparation

Quartz grains in the size ranges of 180–212 mm and 350–425 mm were extracted from the sediment under low-intensity red light in the laboratory by a procedure including HCl acid digestion, sieving, heavy liquid flotation (collecting < 2.68 g/cm^3 fraction), and lastly, etching in 48 % hydrofluoric acid for 40 minutes to remove surface discoloration and the outer 6–8 mm alpha-particle irradiated shell.

Although the particle size distribution of this sample was dominated by silt and clay-sized material with only a very small proportion being quartz grains of sufficient size for practical single grain optical dating, the number of grains successfully isolated proved adequate, and hence the improved SAR method could be used.

Single-Aliquot palaeodose measurements

The single aliquot OSL measurements were made using a Type TL-DA-15 Risø 'Minisys' TL/OSL reader, with optical stimulation of 420-560 nm provided by an optically-filtered halogen lamp. Emissions were detected by an EMI 9235QA photomultiplier tube shielded from the scattered stimulation light by two 2 mm Hoya U 340 filters, and irradiations were performed using a 40mCi ^{90}Sr/^{90}Y beta plaque incorporated in the apparatus.

Initially, measurements were made on multiple grain aliquots of 180-212 mm quartz grains attached to the central 7 mm diameter stainless steel discs using silicone oil and analysed using the SAR protocol. The samples were illuminated for 125 s at 125°C, and in each case, the OSL signal was determined by subtracting an integral over the final 20 s of exposure (as 'background') from an integral over the first 20 s of illumination. A pre-heat temperature of 240°C for 10 s was used prior to all OSL measurements, and a 'cut-heat' to 160°C was given after each test dose. In this study, a test dose of 3.0 Gy was used to track sensitivity changes through the procedure, for which corrections were subsequently made, and five different regenerative dose levels ranging from 1.5 to 20 Gy were given.

All the multiple grain aliquots emitted usefully intense OSL and gave a near-Poisson distribution of P, as shown in Figs 2a and 2b, indicating ages ranging from ca. 10,000 to 30,000 BP. Such a distribution form is generally interpreted as evidence of contamination by a mixture of partially-bleached grains in the sample (Olley et al. 1998), with the rising edge of the histogram shown in Fig. 2b giving the best indicator of true age, and hence this age distribution was interpreted as showing a depositional event near the start of the Holocene. However, this date was presumed a maximum age on account of the presence of partially-bleached grains.

Consequently, to get a better age estimate, 48 single grains of quartz were measured grain-by-grain using the same SAR protocol with the aim of identifying a hypothetical younger population of grains believed to be present and to represent the true age of the deposit. However, a larger grain size, 350–425mm, was now used, as a pilot study had showed no detectable OSL from any of 16 individual 180–212mm grains; even so, all the 350–425 mm single grains exhibited either faint or undetectable OSL and only 13 gave useable intensities. P was determined for each of these in an interpolative procedure which takes account of non-linearities of the luminescence dose-response curve; the results are shown in Figs 3a and 3b. The uncertainties on P are based on counting statistics and also incorporate calibration uncertainties for the laboratory beta source.

The ages were evaluated using a modified version of an age computation program authored by Murray, and incorporating the updated dose rate conversion factors of Adamiec and Aitken (1998). None of these single grains gave ages approaching the ages found from the multiple grain aliquots, and so appear to reveal either a more recent influx of material, or to represent an artifact induced by the SAR protocol in otherwise insensitive grains.

One explanation why the single grains showed no comparable older ages is that the random sampling of 48 single grains for measurement by chance included none of the 'older' or less-well-bleached grains which apparently dominated the OSL emission from the multiple grain aliquots. It is also feasible that because of the smaller size of the 180–212 mm grains, a proportion had undergone aeolian transportation into the cave prior to burial, and hence were more reliably bleached. This suggests that the smaller grains possess different OSL characteristics from the less readily transported 350–435 mm grains, which may be largely derived from the cave walls or bedrock.

Dose rate determination

Radio-isotope activities were measured using high-resolution gamma-spectrometry (CSIRO Division of Land and Water, Canberra). The uranium chain appears to be in disequilibrium, with excess Ra-226, a not uncommon situation in the zone of groundwater influence. In the case of the younger set of ages, this U-series disequilibrium has an insignificant effect on the effective total dose rate. The older ages are potentially more severely affected but to an extent which can only be modelled, not analytically-determined. If the assumption is made that Ra-226 was incorporated in excess in the sediment at the time of deposition and that the present-day excess is the remnant of the decay of this excess component, then an unrealistic initial activity of the order of 2000 Bq/kg is required. Consequently we favour the realistic alternative assumption of steady-state conditions pertaining since deposition. The optical age is calculated under this assumption.

The concentration of radio-isotopes contained within the quartz grains was assumed to be 10% of the bulk sediment concentration, and the efficiency with which resulting internally-emitted alpha-particles induced luminescence within the host grain is assumed to be 0.05 ± 0.02, based on previous results for quartz-rich sediment (Questiaux 1990). The location of the sample within the cave resulted in strong attenuation of the cosmic-ray flux by the overlying sandstone, and consequently this dose rate component, calculated using Prescott and Hutton (1994), made only a trivial contribution to the total dose rate.

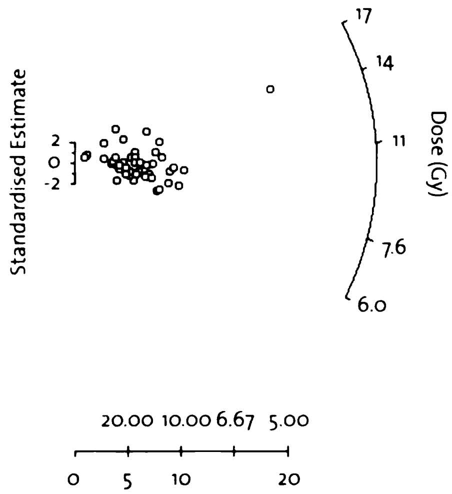

Fig. 2 (a) Radial plot (Galbraith et al, 1999) showing the measured P values from the 48 multiple-grain 7 mm diameter aliquots, incorporating test-dose OSL correction.

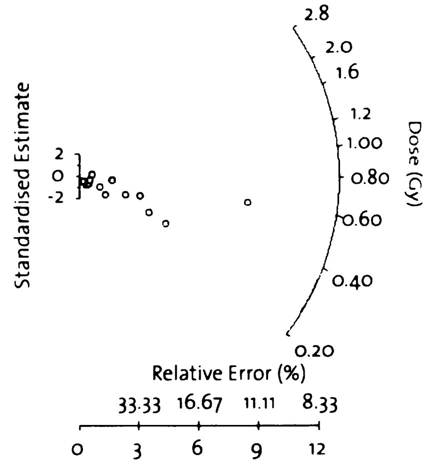

Fig. 3 (a) Radial plot showing the measured P values from 12 of the 13 grains which gave detectable OSL. The pooled P for the shaded set of grains is 0.31 ± 0.05 Gy. The grain lying at P = 0.6 Gy is excluded from the determination for P shown, as it is attributed to a separate population - that of partially-bleached grains. (b) Alternatively, the grain lying at P = 0.6 Gy is now included, giving a pooled value of 0.52 ± 0.08 Gy.

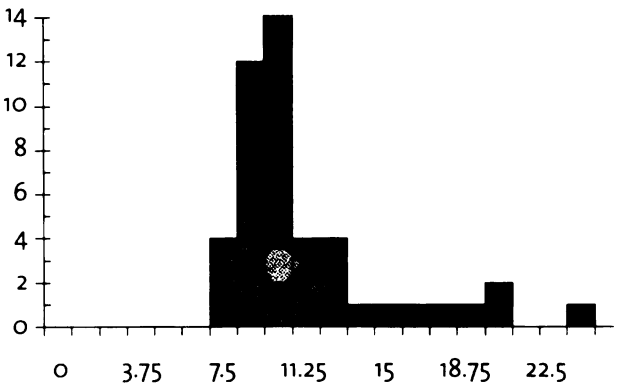

Fig. 2 (b) Histogram showing the measured P values (Gy) from the 48 multiple-grain 7 mm diameter aliquots, incorporating test-dose OSL correction. The best estimate for P is taken as the weighted mean of the 4 lowest P aliquots and is 7.07 ± 0.54 Gy.

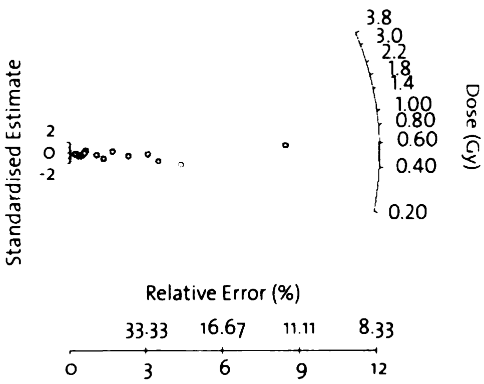

Fig. 3 (b)

Discussion

The single grain results taken at face value indicate that there are two alternative young ages for this sample, depending on which subsets of the 13 detected single grains are selected. An inclusive assessment, incorporating all 13 grains in the pooled P value, gives an age of 650±110 years ago, a second scenario, excluding the brightest individual grain observed, gives a pooled age for the 12 remaining grains of 390±60 years ago. The rationale for excluding the bright grain in the latter case is the possibility that it was not completely reset prior to burial (Murray et al. 1995) and hence would contribute excess age to the ensemble. The multiple grain measurements, conversely, suggest a much greater age for this deposit.

In assessing which of the two options is the more likely to be correct, the older ages measured from the multiple grain aliquots must be accorded the greater validity. The consistently low-intensity of OSL seen from the single grains, all of which produced low palaeodoses using the SAR protocol, has elsewhere been shown to be an artefact of the SAR protocol itself. As it is not possible to show unambiguously whether that is the case here, or whether these grains are truly of young age, limited credibility must be assigned to these young ages. Conversely the older ages, calculated from the multiple grain measurements, are likely to be overestimates due to incorporation of incompletely-bleached grains in some of the aliquots. Therefore, the rising edge of the histogram is assumed to provide the best estimate for P. A weighted mean of the 4 lowest P aliquots gives 7.07±0.54 Gy, which, with a dose rate modelled for steady-state U-series disequilibrium, corresponds to an age of 8530±850 years ago.

Conclusion

The single grain OSL results are counter to those expected from the sample location and deposition conditions, being younger than the unconsolidated, 80 cm deep, archaeological sediments in the Voli Voli III site (which on ceramic type is at least 1000 years old) in the doline directly above the Volivoli 1 fossil site, though which any slumping of material from the doline surface would have had to pass (Worthy and Anderson 1999). Given that the single grain results are unacceptable on this stratigraphic ground as well as for technical reasons described above, and that the single charcoal date refers to modern contamination, then the multiple grain OSL results offer a more plausible result. Further research is planned to see whether uncertainty concerning this result can be reduced.

QARA-NI-VOKAI

The Qara-ni-vokai remains (including giant iguana, giant megapode, giant pigeon, and giant frog) were found on the surface of, and down to ca. 30 cm within unconsolidated sediments extending into a small chamber below a talus slope which reached up to a vertical entrance. The absence of any cultural material in these sediments, notable in the light of pottery and midden accumulations near the vertical entrance, suggest that they probably predate human occupation. However, the occurrence of bones on the surface or in the loose surface sediment down to a maximum of 30 cm, and the existence on the sediment surface of small stalagmites, whereas much larger stalagmites are growing elsewhere in the cave, suggest that the age of the deposits might be quite young, perhaps within the period of human settlement in Fiji.

Radiocarbon dating

From this site, two bone samples were submitted to Beta Analytic in May 1998 for AMS ^{14}C dating (Table 1): the shaft of an iguana femur (FIJIFOSS-1) from the 10–20 cm level in Square 1, and a piece of iguana humerus (FIJIFOSS-2) collected from the surface of the terminal chamber. Both weighed ca. 5 g. As in the case of the Volivoli Fossil Site 1 samples, pretreatment disclosed that these samples 'have not yielded a separable collagen component for analysis' (Beta Analytic 7.10.1998).

Two charcoal and several sedimentary samples were submitted for conventional (liquid scintillation) ^{14}C dating by the ANU laboratory. The charcoal sample of 4.9 g from the upper 25 cm of friable red soil around the fossil bones returned a conventional ^{14}C age of 20,020±660 BP (ANU-11010). This sample consisted of small (<1 mm) pieces of charcoal extracted from the friable, red sediment containing fossil bones in the upper 25 cm of the stratigraphy. The sediment appears identical to other soft sediments in the cave, varying in colour from red to black, which look and smell like guano. It was not possible to extract all of the sediment adhering to the grains of charcoal, so this was a mixed sample which was collected from an area of ca. 1 m^2. It returned a conventional ^{14}C age of 25,540±630 BP.

A test-pit (TP-1) excavated to 130 cm below the fossil deposit cut through a series of similar friable organic sediments interleaved with thin deposits of fine silt, to reach an impenetrable deposit of heavily weathered limestone at the base. Two sediment samples were submitted for dating. ANU-11013 (26,340±390 BP) was from a thin deposit (layer 4, upper black), at 46–48 cm depth. ANU-11012 (25,880±410 BP) was from level 80–90 cm in a thick black layer (layer 6, lower black, 60–90 cm).

These results indicate that fossil deposition had occurred during the Pleistocene, but they do not show when it ceased. To obtain an estimate of that age required Uranium-series dating on samples from two small speleothems which we recovered from the surface of the terminal chamber area of the site. These were clearly stalagmites sitting in growth position with their bases situated approximately 1 cm below the upper surface of the sediment.

Uranium series dating

Background

The U-Th dating method is based on the decay chain of the naturally occurring isotope ^{238}U, as follows:

^{238}U - a- > ^{234}U, $t_{1/2} = 4.47 \times 10^9$ yrs

^{234}U - a- > ^{230}Th, $t_{1/2} = 2.45 \times 10^5$ yrs

^{230}Th - a- > ^{226}Ra, $t_{1/2} = 0.75 \times 10^5$ yrs

Most terrestrial near-surface waters contain dissolved uranium in trace quantities but no dissolved thorium because of thorium's high affinity for adsorption onto particle surfaces. Therefore ^{238}U and ^{234}U but not their daughter product, ^{230}Th, have the potential to be incorporated into the crystal lattice of autigenic phases that may precipitate from near-surface waters. Precipitation of $CaCO_3$ from waters percolating into a limestone cave to form speleothems (stalagmites, stalactites and flowstones) is an example of such a process. Immediately after their formation, speleothems will be found to contain uranium isotopes (from their formation waters) but no daughter product thorium isotopes. The amounts of ^{230}Th then start to accumulate in the solid phase via decay from its parent uranium isotopes. Up to a certain maximum age limit, of usually around 500,000 years (governed principally by the half life of ^{230}Th itself), the ratio of daughter ^{230}Th to parent ^{234}U and ^{238}U isotopes gives a direct measure of the time that has elapsed since formation of the calcite phase.

With the aid of a Thermal Ionisation Mass Spectrometer (TIMS) it is possible to measure the concentration of these uranium and thorium isotopes contained in a sample with very great precision. For samples in the range of a few thousand years to a few hundred thousand years in age, for example, uncertainties on the age measurement are commonly around a few percent, or less. As for all dating methods certain criteria need to be met in U/Th dating to ensure the best possible outcome. The main one concerns the occurrence of 'extra' ^{230}Th that is associated with clay particles or sediment present in a sample. This is why obtaining very clean samples for U/Th dating is crucial to the success of the method. Fortunately it is possible to gauge the level of such detrital thorium contamination using another isotope of thorium, ^{232}Th, which is also associated with the contaminating detrital phase. If the levels of such contamination are found to be low enough, it will be possible to correct samples for contribution by detrital ^{230}Th by analysing some of the sediment associated with the sample and accurate ages will still be obtainable (Ivanovitch and Harmon 1992).

Analytical Procedure

Samples of $CaCO_3$ are cleaned by agitation in an ultrasonic bath in alternate solutions of Milli-Q water and AR Grade acetone. The samples are dissolved by the stepwise addition of HNO_3, and when dissolved, a mixed $^{233,236}U/^{229}Th$ spike is added. This sample-spike mixture is refluxed overnight in the presence of 1–2 ml of H_2O_2 to ensure complete equilibration of sample with spike U and Th isotopes, and the oxidation of any organic compounds present in the samples. Chemical procedures for U and Th involve co-precipitation of U and Th with $Fe(OH)_2$ at pH 7.0, followed by separation and purification of U and Th by standard anion exchange techniques (Stirling et al. 1995).

Isotopes of U and Th are measured using a Finnigan MAT 262 solid source mass spectrometer equipped with an ion counting device. U and Th separates are loaded onto zone refined Re filaments between two layers of colloidal graphite. The respective U or Th isotopes are then measured by cycling each mass into an ion counter. The spike $^{233}U/^{236}U$ (= 1) is used to correct for machine mass fractionation effects during U measurement. No attempt is made to correct Th isotopes for mass fraction. The $^{233,236}U/^{229}Th$ of the mixed spike solution used in the U and Th isotope analysis was established by calibration against the uraninite standard HU-1 assumed to be in secular equilibrium, and against standard solutions of U and Th.

The details of our U and Th isotopic measurements made on the speleothem samples from Qara-ni-Vokai are given in Table 3. Age estimates are given in calendar years BP.

Samples

The TIMS U-Th dating method available at the Laboratoire des Sciences du Climat et de l'Environment was used to obtain ages on the two small stalagmites. Sample F-QnV-2a came from 2.5-3.0 cm below the upper surface of stalagmite F-QnV-2 and sample F-QnV-2b from the base of the same stalagmite at 5.0-6.0 cm depth below the surface. Sample F-QnV-3a is from the top (0.0-1.0 cm) of stalagmite F-QnV-3a. All were samples of clean calcite, but an additional fraction constituted mainly of detrital grains associated with F-QnV-3a was also measured to enable correction for detrital Th isotopes as explained above. This involved construction of two-point isochrons using the results from the 'clean' fraction of F-QnV-3a and the fraction dominated by detrital grains (Table 3) and follows the procedure outlined in Ku and Liang (1984). In this case the correction was very small, shifting an initial result of 4570±80 BP to 4490±160 BP. Given the higher $^{230}Th/^{232}Th$ determined for the other speleothem samples (indicating less contamination by detrital thorium), and the likely similar isotopic content of the detrital phases associated with these samples, a correction for detrital contamination was not considered necessary for these samples.

Conclusion

With no direct age on the bones available, the contextual ages indicate that the sedimentary deposit is of late Pleistocene age, and perhaps laid down quite rapidly at that time, judging by the overlap of radiocarbon determinations on samples down to 90 cm. The U-series ages indicate, equally clearly, that deposition had ceased by ca. 4500 BP. These measurements bracket the age of the bone deposit in the upper sedimentary layer. The single exception was constituted by remains of a single giant iguana whose localised and associated bones lying on the surface of the site indicate that since its deposition it had not been disturbed by subsequent water flows. A bone of this specimen was sample FIJIFOSS-2, which did not have any collagen in it, but its surface position allows the possibility that it could be of contemporary age to the stalagmites - indeed it could be younger.

QARA-I-OSO II

This is a large rockshelter at ca. 200 m altitude in a limestone outcrop at Tau, approximately three km inland. A small test excavation was taken to 80 cm depth. The upper layers consisted of a brown silty sediment containing shell midden, bone and pottery. The latter included paddle-impressed and leaf-impressed wares of the middle phase of Fijian prehistory, beginning at 30 cm depth where it is dated by ANU-11014 to 660±60 BP. At the base of the cultural deposit, on a sloping boundary at 49–65 cm depth, was one piece of Lapita pottery (Anderson et al. 2000), which elsewhere in Fiji would have an age of 2600–2800 BP (Anderson and Clark 1999). This is underlain by a 4 cm thick layer of soft calcite and limestone pebbles which, in turn, rests upon a very deep and undifferentiated deposit of soft brown sediment that is probably almost entirely from guano. Within the calcite band, and extending down to the interface with the brown layer there was a deposit of large landsnail shells (*Placostylus* sp.) and amongst them some pieces of bone from the giant iguana. The iguana bone was dark brown in colour and had ragged damage to its (normally) stout dorsal and ventral sections, conducive with chewing by a predator. It was submitted to Oxford Radiocarbon Accelerator Unit for AMS dating. They advised (7th September 1999) that the sample 'failed to produce sufficient collagen to enable dating to take place'.

Whether the calcite and pebble layer is a cultural feature, e.g. a living surface, or represents a period of non-habitation is unknown. There was no other midden or other cultural material within it, but dark staining, probably from charcoal, indicates that it might represent the earliest habitation in the site. Alternatively,

Table 2. The OSL analytical and field data, along with the dose rate and the ages.

Sample		$ANU_{OD}187a$	
Latitude (degrees)		18 S	
Longitude (degrees)		177 W	
Altitude (m)		250	
Sample burial depth (m)		12 m sandstone	
Quartz grain diameter (mm)	387.5	±	37.5
Palaeodose (Gy) Incorporating all 13 grains	0.52	±	0.08
Palaeodose (Gy) Incorporating 12 of 13 grains	0.31	±	0.04
Palaeodose (Gy) Weighted mean of 4 youngest grains	7.07	±	0.54
In-situ water content (%)	27.3	±	3.0
Saturation water content (%)	54.0	±	5.0
In-situ fraction of saturation	0.51	±	0.07
Alpha particle efficiency	0.05	±	0.02
High-resolution gamma spectrometry (activity, Bq/kg)			
U-238	37.24	±	2.16
Ra-226	67.12	±	0.74
Pb-210	42.79	±	2.52
Th-228	5.94	±	0.26
Ra-228	6.55	±	0.57
Th-232	6.05	±	0.24
K-40	40.51	±	2.60
High-resolution gamma spectrometry (concentration)			
U (ppm)	5.43	±	0.06
Th (ppm)	1.48	±	0.06
K (%)	0.13	±	0.01
Cosmic ray dose-rate (Gy/ka)	**0.044**	±	**0.007**
Total dose rate (Gy/ka)	**0.79**	±	**0.06**
Age (ka) (Incorporating 13 detected single grains)	**0.65**	±	**0.11**
Age (ka) (Incorporating 12 of 13 grains, excluding oldest)	**0.39**	±	**0.06**
Age (ka) (from weighted mean of 4 lowest P aliquots)	8.53	±	0.85

Table 3. The U-series results on Qara-ni-Vokai samples.

Sample	U (ppm)	$^{234}U/^{238}U$	±	$^{230}Th/^{238}U$	±	$^{230}Th/^{232}Th$	±	Age (yrs)	±	Age* (yrs)	±
Speleothem Calcite											
F-QnV-2a	0.141	0.9958	0.0041	0.0353	0.0009	130.97	3.47	3930	110		
F-QnV-2a	0.145	1.0137	0.0057	0.0411	0.0014	164.38	5.47	4500	160		
F-QnV-3a	0.120	1.0457	0.0032	0.0430	0.0007	115.47	2.30	4570	80	4490	160
Detrital Fraction											
F-QnV-3a		1.0366	0.0088	1.7014	0.0108	1.80	0.01				

* corrected for detrital contamination using the method of Ku & Liang (1984). Ratios given are activity ratios. Errors are 2s.

the landsnail shell and iguana bone could have been deposited in the calcite layer during a period, of unknown length, which preceded any human occupation. Cultural and faunal material is entirely absent in the underlying brown sediment, except for a few pieces of swift bone.

Further planned excavations at the site have been postponed because of political unrest in Fiji.

UDIT CAVE

This is the longest cave (790 m) presently known in the Wainibuku area. A very small deposit (1 m long by 0.5 m across and 0.3 m deep), designated the Udit Cave Pitfall Site (Worthy and Anderson 1999) contained fossils of several extinct taxa: the giant megapode, another new species of megapode, the giant pigeon, a rail and the giant frog. A proximal tibiotarsus from the giant megapode was submitted to the Rafter Laboratory (New Zealand). Pretreatment indicated that it had no collagen yield.

THE PERIOD OF DISAPPEARANCE

No collagen suitable for radiocarbon age measurement remained in any of the bone samples. It is probable that collagen leaches rapidly from bone in tropical climates, especially in humid caves where the temperature is close to 30°C, as in the Fijian cases, so the general inability to obtain collagen suitable for age measurement is not necessarily an indication of great age, as it might be in samples from temperate sites. At Volivoli 1 Fossil Site, the deeply-weathered clay seemed unlikely to be much younger than Pleistocene in age, and the early Holocene OSL date is plausible without being as secure as we would like. Conversely, we thought that the Qara-ni-Vokai bone deposit would be of Holocene age, but it turns out to be in Pleistocene sediments. However, the U-series ages on the speleothems allow the possibility that some of the fossil deposit accumulated until about the mid-Holocene, and one surface specimen could be younger than that. At Qara-I-Oso II the giant iguana was in a layer at the base of the cultural stratigraphy and association with humans is equivocal, but ragged breaks preserved on the stout bone are possibly from human consumption.

At Bukusia, in the Sigatoka Valley, two bones of the giant iguana were found on the surface in a rockshelter containing abundant midden deposits which may have accumulated during the occupation of an adjacent hill fort, built during the Kai Colo uprising of 1875–76 (Worthy and Anderson 1999). While it seems improbable that giant iguana survived until the late 19th century, the bones are in good condition and do not appear to be re-exposed fossils of great antiquity.

Association of remains of the Viti Levu crocodilian with humans is as yet unknown, and those of the giant birds of Viti Levu is restricted to a single Lapita age site on the nearby island of Naigani, which was part of greater Viti Levu during Pleistocene low sea levels, from which four bones of the giant megapode and some remains of an undescribed species of pigeon were recovered in calcareous sands. The pattern of breakage on them is consistent with their having been killed and eaten by the site inhabitants. All other Lapita sites in Viti Levu either have no faunal associations or very limited faunas preserved in them. This may reflect unsuitable geochemical conditions in the predominantly non-calcareous soils of the main Fijian islands, for example at Sigatoka, but other factors must also have been involved because Lapita sites generally are located in calcareous deposits, often with additional marine shell midden, as at Natunuku.

In regard to the offshore islands, it is unlikely that the unique endemics discovered on Viti Levu could have dispersed over water. Vatulele, for example, is presently only 30 km from Viti Levu but has never been connected to it. It has abundant caves suitable for preservation of fossil faunas, but no remains of any of the distinctive Viti Levu megafaunal species and it also lacks the frogs (*Platymantis* spp.). Its arboreal iguana and boa, however, could have been transported on floating logs, and all of its birds are widespread, highly dispersive species. Yet Vatulele has still suffered extinctions. The Friendly Ground Dove, *Gallicolumba stairii*, and Swamphen *Porphyrio porphyrio* have both become extinct in the period following human arrival. A Lapita age site on Lakeba Island contained many bones of one of the smaller megapodes (*Megapodius alimentum*), and those of a large pigeon (*Ducula* sp.) and a tooth-billed pigeon (*Didunculus strigirostris*) amongst some other taxa yet to be clearly identified (Worthy and Anderson 1999). Limited faunas recovered from excavations at Votua Lapita site, on Mago Island, also resulted in the recovery of a bone of *M. alimentum*.

More extensive excavation of the early Fijian sites might turn up more extinct taxa, as has occurred in Tonga, where there are butchered and burnt remains of a large extinct iguanid (*Brachylophus* sp., about half the size of the extinct Fijian species) in early sites on Lifuka and Tongatapu (Pregill 1993; Pregill and Dye 1989), as well as bones of extinct megapodes and other birds (Steadman 1993). In New Caledonia, a human association is claimed for *Ducula* sp., the giant galliform *Sylviornis neocaledoniae* the crocodilian *Mekosuchus inexpectatus*, and the horned tortoise *Meiolania* sp., for example at the Pindai caves, where there are radiocarbon determinations as late as 1750 BP (Balouet and Olson 1989:4; Balouet 1991). In Vanuatu, as well, a recent discovery places crocodilian remains in cultural association at what appears to be an early Lapita site (Bedford pers. comm.).

However, we do need to be wary about arguments by analogy. Of the Fijian megafauna, only the giant megapode seems to be culturally associated, at Naigani, on reasonably strong but not impregnable grounds. The association is less secure, so far at least, of giant iguana at Tau. The Vanuatu evidence is very slim, and still not confirmed beyond doubt. The New Caledonia megafauna is mostly from sites which are, at least partly, of pre-human natural origin and none of the radiocarbon ages are directly on megafaunal bone. The few instances of *Sylviornis* and crocodilian bone occurring in Lapita age sites are open to question because the remains come from basal deposits at the interface with pre-human Holocene sediments (Sand pers. comm.). As in Fiji, it remains to be demonstrated that the megafaunal remains are in unequivocal cultural association.

CONCLUSIONS

At a recent Centre for Archaeological Research (ANU, June 9 2000) workshop on megafaunal extinctions, Jared Diamond offered some useful concluding remarks. Amongst them was the argument that since we have great difficulty in determining the causes of modern faunal extinctions, we may never be able to do so satisfactorily for those in antiquity, and therefore perhaps the best we can expect to show is a chronological concurrence of extinction events and human advent. He went on to suggest that we apply a "smell test" to the Oceanic evidence, i.e. if it is accepted that there is substantial human causation in various other and better-known cases of island extinction in the Pacific, then that is very probably the case in Fiji as well. However, while this seems like a sensible approach to the evidence as a whole, and one to which we are inclined, it still remains necessary to document what we should refrain from merely assuming. On a lighter note, Jared Diamond has offered to share with one of us (AJA) a bottle of his 1975 Penfolds Grange Hermitage if it is not apparent by 2010 that the extinction of Fijian megafauna was culturally initiated.

In this paper, we have described current progress in documenting the period at which the Fijian megafauna became extinct. Our results show, so far, that we can track the subfossil remains from natural sites into the late Pleistocene, ca. 20,000 BP by radiocarbon dating, at one site (Qara-ni-Vokai), and to the early Holocene at another (Volivoli Fosssil site 1), given the accuracy of current OSL results on difficult samples. The U-series results from Qara-ni-Vokai show that, with one possible exception, fossil deposition had ceased by the mid-Holocene, still significantly earlier than human settlement, although there are enigmatic data from other sites where remains, undated, occur surficially. The archaeological data indicate that the giant megapode may have survived into the Lapita era and possibly also the giant iguana. But these cases are not beyond argument on associational or chronological grounds, and the same may be said of similar cases in New Caledonia and elsewhere.

Consequently, in addition to the hypothesis that megafauna on the large islands of the tropical central Pacific went extinct through human agency of one kind or another, as seems sufficiently certain in New Zealand for example (Anderson 1989), we should retain for investigation the proposition that megafaunal extinction occurred before the arrival of people — possibly as a direct or indirect consequence of the approximately 50% reduction in land area in each of Fiji, New Caledonia and Vanuatu at the beginning of the Holocene and/or through developing climatic instability by the mid-Holocene. Further excavation of Lapita age sites and of subfossil bone deposits is clearly required, with meticulous attention being paid to the stratigraphic associations of megafaunal remains, and it is necessary also to keep looking for samples of sufficient quality to enable the obtaining of direct ^{14}C ages on the bones. Given the scarcity of material from archaeological sites, attention might be more profitably directed at subfossil deposits where the material is more diverse and abundant. Robust ages of 3000 BP or younger from these would still demonstrate human contemporaneity, and if we must abandon any more complex line of argument about the causes of extinction, then everything depends upon an unimpeachable chronology.

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Edge of the Trees

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TOGETHER WITH Fiona Foley, Janet Laurence created the award winning sculptural installation, *Edge of the Trees*, outside the Museum of Sydney (Fig. 1). This museum is situated on the corner of Phillip and Bridge Streets, the site of Australia's first government house, in Sydney.

According to Anne Susskind (1999:114), Janet Laurence is one of Australia's most successful artists who 'finds inspiration in science and history as much as conventional art'. Davina Jackson, editor of *Architecture Australia*, maintains that Janet is a 'serious candidate for the role of Australia's leading public artist' (quoted in Susskind 1999:114). She has created many public sculptures throughout Australia including a 100 m × 50 m 'atmospheric zone' at one end of the Olympic Boulevard at Homebush Bay.

The following text accompanies the installation:

> ...the discoverers struggling throught the surf were met on the beach by other people looking at them from the edge of the trees. Thus the same landscape perceived by the newcomers as alien, hostile or having no coherent form, was to the indigenous people their home, a familiar place, the inspiration of dreams (Rhys Jones 1985).
>
> *Edge of the Trees: A Sculptural Installation by Janet Laurence and Fiona Foley.*
>
> *Edge of the Trees* is about contact. It acknowledges the indigenous place and people of Sydney, home of the Eora, and the many layers of occupation since 1788. Materials — stone, wood, steel — represent interface of natural and built environment. Substances — shell, hair, ochre, ash, bone — represent human presence and passing. Names of Eora men and women, First Fleeters, plants and Koori callings of place — represent shared and separate custom, memory and knowledge. A place to enter, explore, contest anew; perhaps reconciliation.

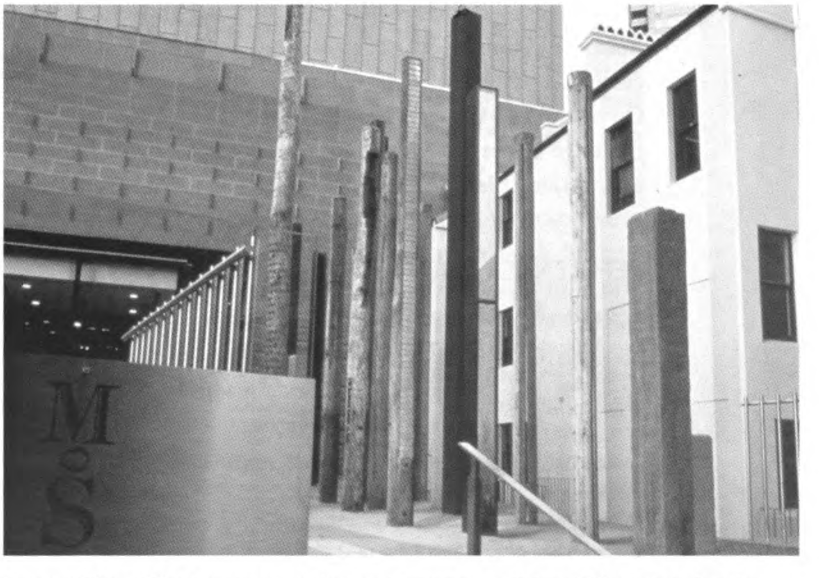

Figure 1. *Edge of the Trees*, 1994–95, by Janet Laurence and Fiona Foley. Located at the Museum of Sydney on the site of first Government House. The quotation from Rhys Jones, carved in stone, lies adjacent to this installation. Photograph: Ray Joyce. Reproduced with the permission of the Museum of Sydney.

Edge of the Trees was inspired by an article written by Rhys Jones — 'Ordering the landscape', the significant section being (Jones 1985:185):

> To me, the most salient aspect of the 'Age of Discovery' was that almost everywhere in the temperate regions, with the exception only of certain small islands such as those of Bass Strait and the Falklands, the discoverers struggling through the surf were met on the beaches by other people looking at them from the edges of the trees. Thus the same landscape perceived by the newcomers as alien, hostile or having no coherent form, was to the indigenous people their home, a familiar place, the inspiration of dreams. The fifteenth-century Welsh poet Tudur Penllyn expressed this dichotomy well, as from his 'shady dismal wood' he contemplated the manicured red-soil plains of England, but advised his country-man, the guerilla Dafydd ap Siancyn (David Jenkins) that:
>
> Dy gastell ydyw'r gelli
> Derw dol yw dy dyrau di
> Da yw ffin a thref ddinas
> Gorau yw'r glyn a'r graig las;
> A'r tyrau o'r tu arall

The poem, reproduced in Parry (1962:169–70) has been translated by Rhys Jones as follows:

> Thy castle is the copse
> Oak glade thy towers
> Good is the urbane edge of cities
> Better the glen and the shining rock
> Watch those towns, formal and fat,
> And turrets from inside out.

Janet Laurence and Rhys Jones did not actually meet until April 1995, after the installation at the Museum of Sydney had been completed, when they both presented papers at an Australia ICOMOS event entitled 'World Heritage Convention and Cultural Landscapes' at the Sydney Opera House. Rhys' paper was called 'Imagined landscapes: Aboriginal and archaeological perspectives'; Janet's was titled 'Inspirational value of cultural landscapes.' The conference proceedings were filmed by Channel 4 for Llewelyn's television series, Gwyn and His World. A chapter in the book of this series, entitled 'Hen linach cwr y coed' ('The old line of the edge of the trees') includes a discussion about the Edge of the Trees installation and Rhys' role in its conception (Llewelyn 1996:44–53).

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Australian Aboriginal Art and Russian Icon Painting

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TWO ARTISTIC WORLDS: FEATURES IN COMMON

Medieval Russian icon painting and Australian Aboriginal art - what comparison could be more unexpected, more paradoxical? They belong to worlds very different in cultural history and in art. Yet comparison and apposition are feasible and acceptable, provided that one goes into the essence of both forms of art. Both Russian icon painting and traditional Aboriginal art — at least to a significant degree - belong to religious art, to religious creativity. They are both conceptual and the notion of the sacred is central to each of them. Their affinity, their common features are central for both — it is their place and function in social life, their content ('sacred history') and their language of symbol and metaphor. Their common themes indicate that they are based on universal archetypes of consciousness.

Traditional Aboriginal art, in its sources and fundamental forms, is religious and esoteric, although that does not imply that its content extends no further. It performs other functions: communicative, informational, educational and aesthetic, as well as philosophical and cognitive. However, they all are organically interwoven, and religion plays a dominant role in this complex. For the Aboriginal artist and the icon painter, abstract notions very often lie behind a material visual image, the world around is a great metaphor, full of symbols and concealed meanings. When it appears to us that the Aboriginal artist or icon painter seeks to depict the real world, actually they are drawing a world of meaning, a world of things, beings and happenings replete with meaning. For them the universe is an infinite system of signs. For both, their art, in the words of Saint Paul on faith, is 'the evidence of things not seen' (Hebrews XI, 1).

The philosophical, symbolic and metaphorical content of Aboriginal art, with its religious content, also give it an affinity with icon painting. One of the main topics of traditional Aboriginal art is the cycle of birth, life and death. An example of this is the theme of the morning star, a symbol of the transition of the soul from one state to another and in its final impetus towards the land of the dead. Widespread in the myths and art of the Aborigines is the theme of swallowing and subsequent ejection, for example of those undergoing initiation, as a metaphor for the transition from one metaphysical state to another and as a solution to the problem of transformation. An example is the myth of the Wagilag sisters, reproduced in the rites and in the art of Arnhem Land. The sisters and their children were swallowed and then vomited out by the Great Serpent. The myth is a metaphor for transition from one level of existence to another, for spiritual transformation. This is the inner meaning of the form of burial, widespread in the north of Australia, which consists of two stages separated in time. Burial is understood as the transfiguration of the body, as the liberation of the soul and its departure for the land of the dead (Hiatt 1975; Caruana 1993:48, 72–4, fig. 59). The same theme is developed on another plane and by other means as one of the main themes in icon painting.

The theme of the death and resurrection of a hero is significant in Aboriginal myths and art. An example is Laindjung, a cultural hero of Arnhem Land, the originator of laws, customs and sacred rites. He was killed by envious men, but was resurrected in another form. We see him murdered, speared by enemies, in the lower part of a bark painting. In the upper part he rises from a waterhole transfigured, with sacred emblems on his body (Allen 1975:64). The myth and the painting which illustrates it are remarkable in a number of ways. In the first place, we have a metaphysical transformation of the hero following his murder by treacherous enemies; sacred signs on his body are the visible expression of an inner, spiritual transformation. Do these signs not recall the stigma on the body of the resurrected Christ? It is also interesting that water appears as an element connected with resurrection to a new life in another, spiritually transformed shape. These themes reproduce the Christian paradigm and are evidence of the existence of a universal archetypal subject. Finally, attention must be drawn to the combination in one painting of two episodes occurring at different times — a device familiar in icon painting. Moreover, death and resurrection are in a metaphysical sense just two aspects of the one event.

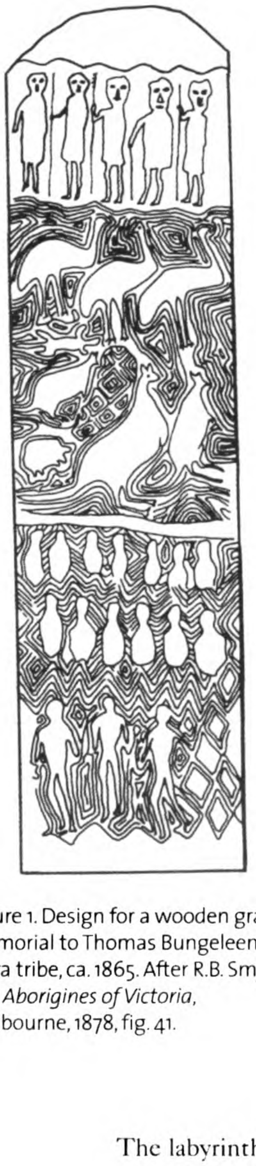

Figure 1. Design for a wooden grave memorial to Thomas Bungeleen, Yarra tribe, ca. 1865. After R.B. Smyth *The Aborigines of Victoria*, Melbourne, 1878, fig. 41.

Take the symbol of the labyrinth as an image of the world of the dead, as a reflection of ancient archetypal notions of death and resurrection to a new life. The Museum of Victoria has a wooden board made by an Aboriginal artist and placed on the grave of a member of his tribe who died in 1865 (Figure 1). Men with spears are carved in the upper part of the board. In the middle part there are emus, kangaroos and other animals. In the lower part there are three rows of anthropomorphic figures. The middle and lower parts of the board are totally covered with a design in the form of a labyrinth within which the animals and anthropomorphic figures are set. R. Brough Smyth offers the following interpretation, based on information from Aborigines, who, he says, no longer remembered the exact meaning of the images. The upper part of the board depicts friends of the dead man investigating his death; the animals in the middle part indicate that he did not die of hunger (?); below are the spirits who were the cause of his death (Smyth 1878:288, fig. 41). This interpretation is silent on the representation of the labyrinth, yet this latter is the key to decipher the meaning of the whole composition. The image of the labyrinth, since Palaeolithic times one of the most ancient and universal archetypes of human consciousness, is a symbol of the land of the dead, to which people and animals go, and from which they return, recalled by rites and invocations; I discuss this in more detail elsewhere (Kabo 1966, 1972). With this meaning, the visual image of the labyrinth is widely represented in Aboriginal art. It was in the south-east of the continent, where the board from the grave originated, that designs in the form of a labyrinth were carved on the trunks of trees surrounding graves, or sites where initiation ceremonies were performed (Figure 2); these images were also formed on the ground at initiation sites (McCarthy 1956:23-4, figs 10, 20). The labyrinth design was a sacred symbol connected with initiation ceremonies and funeral rites, both of which ritual systems are understood identically by traditional consciousness as transfiguration, as a metaphorical passage from one state to another. Hence I believe that the space on the board from the grave, filled with the design of the labyrinth, indicated the home of the dead, inhabited by the people and animals of the middle and lower parts of the board who have departed our world. The upper part depicts members of the dead man's tribe, witnesses to his last journey. Finally, it is significant that two rows of the anthropomorphic figures in the lower part are limbless: they are incomplete beings, the souls of people inhabiting the lower world, not yet born or not resurrected to a new life, an image known in Aboriginal mythology.

The labyrinth design is extremely rare in Russian icon painting. Hence *'The Path to Paradise'*, an icon dating from the seventeenth century in the Museum of the History of Religion in St. Petersburg, is of particular interest (Figure 3). The path to paradise is depicted as a labyrinth. This subject is quite atypical for Orthodoxy, being evidently influenced by secular culture. However this may be, the ancient symbolism of the labyrinth is transformed by the icon painter in the spirit of Christian philosophy. Each entrance to the labyrinth bears the name of one of the deadly sins: murder, slander, envy, fornication and so on. Beneath is Hell in the shape of a monster with gaping, all-consuming jaws, in wait for the straying human soul. Above is Christ radiant and enthroned, judging sinful mankind. On the right is a sinner on his deathbed, with the devil in the shape of a winged beast lurking to bear him off to the underworld. On the left is a righteous man, with an angel descending to bear him away to the land of the blessed. In the centre is a man in the Garden of Eden, where the tree of knowledge grows; this is possibly Adam, embodying sinful mankind (the legend above him is 'How I wish to go into the darkness'). The sense of this image and the notion of the path to paradise as a labyrinth in which the human soul is lost are quite clear. Nevertheless, here we have the ancient, universal image of the road to the home of the dead and the home itself as a labyrinth — an image connecting the picture on the Russian icon, permeated with the Christian conception of the world, with the mythical symbolism of traditional Aboriginal art.

Figure 2. The design on the carved tree from the Dubbo district, New South Wales. F.D. McCarthy, *Australian Aboriginal Decorative Art*, Sydney, 1956, fig. 10.

Traditional Aboriginal art and icon painting are one in their aim to capture in their images, not the time in which the events of everyday life occur, but a transcendental, sacred time actuated in a ritual or mystery which reproduces events in sacred history, known by Aborigines as the Dreaming (Berndt and Berndt 1977:228–30; Elkin 1979:210; Stanner 1972). As Elkin has put it, the Dreaming is 'the ever-present, unseen, ground of being-of existence ... . The concept is not of a "horizontal" line extending back chronologically through a series of pasts, but rather of a "vertical" line in which the past underlies and is within the present' (Elkin 1969:88, 93). The same concept of time is found in icon painting. The notion of space in the two artistic traditions is also a dual one. Icon painting and Aboriginal art recognise two categories of space: the space of tangible, material reality and that of a reality spiritually visualised. The latter category includes mythological geography, an image of the earth in its sacred dimension, sacred history imprinted on a locality.

Sacred time and sacred space obey their own laws, their visual embodiment requiring specific devices. Icon painting and Aboriginal art use similar devices to convey the development of action and the movement of time. Such devices include repeating the figure being depicted, such as representing several images of the Virgin on one icon; or placing in one and the same space events which take place at different times. An example in Aboriginal art is the depiction twice on one painting of the great mythical hero Lumaluma, as a living being and as a spirit in the form of a skeleton, both being in sacred space and emitting radiance. The hero's wanderings, his

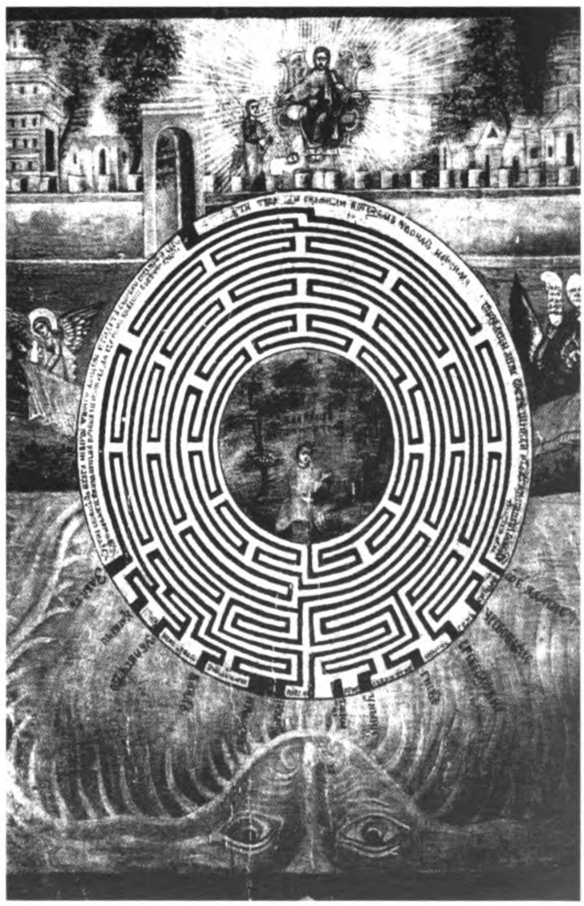

Figure 3. 'The Path to Paradise', 17th century icon in the Museum of History of Religion, St Petersburg, Russia.

persecution and death, and the sacred objects given by him to his descendants are all represented in the one painting (Ryan 1990:76, 95, 114, plate 55). The device is widespread in medieval Russian icon painting, for example in the cycle *'The Assumption of the Virgin'*. We see the body of the Virgin laid out, her soul in the hands of the Archangel Michael standing beside her, and then her soul born off to Heaven by angels, all in the one icon. In an icon on the same topic, the twelve Apostles are also seen both descending on clouds and surrounding the body of the Virgin. The fifteenth century icon *'The Conception of John the Baptist'* juxtaposes two periods of time, separated not only by an interval of several decades, but also by a miraculous event, the birth of the prophet. It shows the parents of Saint John meeting, rejoicing at the news of the miracle of the advent of the prophet to be, and, beside them, the already adult John the Baptist. The fifteenth century icon *'The Beheading of John the Baptist'* shows John, as yet unharmed, with head bowed under the executioner's blade, and with his head, cut off, at his feet. This is one of the most striking examples of the juxtaposition of non-contemporaneous events in Russian icon painting. A plurality of non-contemporaneous events and happenings filling the field of the icon is one of the most typical devices of Russian icon painting. The icon painter, naturally, knows that the head of a victim of execution cannot lie at his feet until the fatal blow is delivered, or that a grown man cannot stand beside his parents as they await his birth. Yet he knows equally that earthly laws do not apply in the world of sacred entities.

A bright light, an aura, is associated by the Aborigines with the power of their mythical ancestors (Morphy 1991:194). The great Rainbow Serpent, regarded in northern Australia as the most powerful of mythical beings, is sometimes depicted in the radiance of a rainbow like the halo of a saint in icon painting (Allen 1975:71). The most remarkable phenomena of this kind are the representations of the Wandjina, anthropomorphic mythical beings, on the cliffs and cave walls of the Kimberleys. Halos around their heads are typical features. The Aborigines associate the Wandjina with rain and fertility. To renew them in fresh, bright colours is magic contributing to restoring their strength and bringing about the return of the rainy season. To control the rain is to control the elements; it is the fertility of the earth, the increase of plants, animals and humankind itself. According to the Aborigines, the halo around the head of a Wandjina is normally taken to be a rainbow accompanying the rain or a thunderclouds of the northwest monsoon, riven by lightning. All who have written about Wandjinas agree on this. I would like to take up two features of these reports. The first is the association of halos with the rainbow (Elkin 1979:224) and lightning (Crawford 1968) - phenomena which impress by their brilliance. Moreover, Wandjinas were the focus of a kind of cult: some rituals were performed in conjunction with the paintings, while the paintings were regarded as the creation of the Wandjinas themselves (Crawford 1968:31, 37). Hence I would like to suggest that some ancient archetype underlies the paintings of halos of Wandjinas, connecting them with the images of the saints in icon painting. The halos around the heads of these beings, so important in Aboriginal religion, are the same aura which surrounds the heads of the saints in Christian and Buddhist icon painting. It is an emanation characteristic of something numinous. The white faces of the Wandjina, mouthless and with huge, black, hollow eyes, bring to mind human skulls. They are ancestral faces, the faces of the creators of the world, who have left their images on the rock faces.

Rock paintings of this kind are found beyond the Kimberleys. On a cliff in the Victoria River region there was discovered a full length drawing of an anthropomorphic being with arms raised as in a pose of adoration, with a white, mouthless face, large, black eye sockets, and a head surrounded by an aura (Lewis and Rose 1988:plate 4).

In the rock paintings the dead eyes of the Wandjina are circled by red rays; these radiate the magic, life-giving power latent within. At the opposite end of the continent, in New South Wales, a cliff face bears a picture of the mythical being Baiame. Like the Wandjina, he is mouthless, but with huge, brilliant eyes in a dark face; light emanates from his sacred inner being (Godden and Malnic 1982:plate 28).

In icon painting, halos encircling the heads of saints, apostles and prophets are so much a tradition that more need not be said. Christ and the Madonna appear with an aura in icons and church frescoes. Icons of the Transfiguration show a brilliant radiance emanating from Christ, his figure painted on the background of a circle of light. The analogy is clear; it derives, probably, from the mysterious depths of religious consciousness, from a perception of figures from the sacred world exuding an aura.

SYMBOLS: THEIR ORIGIN AND ESSENCE

Amongst the abstract symbols most frequently encountered in Aboriginal art are the circle, the concentric circle and the spiral, and, derived therefrom, the concentric arc, together with the zigzag and the wavy line. The cross is less common. All these symbols are significant and are polysemic; each carries multiple meanings. The most productive are, however, the circle, the concentric circle and the spiral. The genesis of these symbols can be traced from the Palaeolithic age: the earliest images relate to the Middle and even Early Palaeolithic. Alexander Marshack describes a concentric arc carved on a stone plate; the tablet is ca. 54,000 BP and originated in Quneitra in the Near East. Marshack supposes that the Quneitra design is prompted by the rainbow and indicates its probable connection with seasonal rites. Another set of symbolic designs from the Middle Paleolithic are the zigzags carved on a bone fragment from Bacho Kiro (Bulgaria) and dated to ca. 44,000 BP (Marshack 1996). The zigzag motif, however, appeared much earlier - ca. 300,000 BP: a bone fragment from that time from Pech de l'Azé (France) shows perhaps the earliest carving in the form of a zigzag or meander (Marshack 1977). Objects analogous to the *tjuringa* are known in the Palaeolithic: one of such Palaeolithic *tjuringa* from Predmosti (Czech Republic), made of mammoth bone, is covered with a pattern of concentric arcs. The mysterious bone batons from the Late Palaeolithic cave at Isturitz (France) bear a relief forming a complex system of concentric arcs and spirals. There is a very interesting bone plate from Malta (Siberia), one side of which is decorated with a spiral design and the other with zigzags, possibly representing snakes (Jelinek 1976:429, fig. 683, 449, fig. 722, 451, fig. 729, 452, figs 732, 733).

A stone disc with a carved cross was found in the Middle Palaeolithic complex of the Tata Cave (Hungary). This is not the only representation of a cross found in the Palaeolithic. A limestone plate with a clearly cut four-pointed cross was found in the Middle Palaeolithic layer of the Tsona Cave (Georgia). There are crosses carved on a bone fragment from Vilen and on a statuette of a mammoth from Vogelherd (Germany) (Stolyar 1985:125, 127, figs 71, 91, 92, 93). Nevertheless the cross is rare in the Palaeolithic, while concentric arcs, spirals, zigzags and similar designs are frequent; only a few examples are given here.

Orientation in space, the ability of the mind to conceptualise space as a whole, the notion of the centre of a space and its vectors, were all achieved by mankind in the Palaeolithic, as primitive communities occupied territory, constructed dwellings and settlements, went on hunting expeditions and migrations, with the community and the individual coming more and more to feel themselves the centre of the cosmos. The spatial organisation of social life, from as far back as the stone age, can be expressed geometrically: basically it is circular in form (a constructed dwelling, often on a circular plan, and the territory occupied by the people around it) and radial (the community's movement from its home base and back). This found graphic expression in the symbol of the circle and concentric circle embodying the notion of the social group and the universe around it. It was expressed also in the symbols of the wavy line, the zigzag and the labyrinth as ideograms of movement and the occupation of space and of wanderings in this and in another world. Finally, it was expressed in the symbol of the cross as the centre of the universe and the four basic vectors which made it up. All these symbols are rooted in human consciousness as its most ancient archetypes.

Such is the age of some basic archetypal symbols which have then passed through many millennia of the history of visual symbols and absorbed a number of images created by mythological consciousness. These ancient archetypes and the notions connected with them are unbelievably stable, capable of being reproduced during the lives of many generations, in the Palaeolithic, in Aboriginal art and even in icon painting.

Tindale (1974:38, 1978:157) states that the circle and the spiral express in graphic form the idea of home or dwelling place. In the Western Desert, concentric circles usually represented camping places or sanctuaries and the lines between them the paths and tracks of Aborigines or mythological beings. Among the Warlpiri, the circle, one of the basic symbols, most often conveys the idea of a camp site or a waterhole in the centre of a camp. It also has the meaning of a maternal bosom, which is one of the universal archetypes of human culture (Munn 1973a:68, 117, 138, 1973b:197, 213, fig. 8). In both meanings, the circle symbolises the place where the life force is to be found.

The ancient sacred symbols of the circle and the concentric circle had their place in the culture of the Slavs. One of the most stable forms of Slavonic pagan sacred places were circular structures with two concentric earth walls, in the centre of which an idol was placed (Rybakov 1987:223–5, fig. 44).

Taking their place in medieval icon painting, the circle and concentric circle brought a rich legacy of pre-Christian notions. In icon painting, these ancient archetypes acquired new meaning, while retaining something deep and fundamental. In icons on *'The Assumption of the Virgin'*, the Madonna risen to Heaven is frequently painted in a concentric circle. The circle here is a symbol of Heaven as of home, a metaphor of the soul's return to its eternal abode. In this connection, we recall the symbol of the circle as home in Aboriginal traditional art, the symbol of the circle as a place from whence come and whither go the heros of the Dreaming.

The concentric circle of the icon is a symbol of sacred space, frequently observed also in Aboriginal art. Christ as king of the world is represented in a concentric circle as the sacred centre of the cosmos. In the sixteenth century icon *'The Fiery Ascension of Elias the Prophet'*, a concentric circle symbolises Heaven as a sacred space filled with fire (Alpatov 1984:plates 7, 25, 64, 79).

The cross symbol can be traced through the whole history of humankind, acquiring in Christianity a deep sacred meaning. It is possible that Palaeolithic carvings in the form of a cross were connected with spatial or cosmic symbolism. The symbolism of the cross is also developed in Aboriginal art. The *waninja*, or thread crosses, ritual objects, took the form of a cross in central Australia. The cross controls the composition of many sacred works of Aboriginal art (e.g. Elkin et al. 1950:66-67, plate 11a; Sutton 1988:figs 36, 39, 40, 44; Ryan 1990:viii, 105, 111, plates 2, 31). The cross may be an abstract representation of sacred ceremonial ground (Ryan 1990:25, fig. 11). One painting in traditional style shows a composition, typical of certain icons, in the form of a cross, with a human figure placed at each side, though in the centre, in place of the crucifixion, is a large circle resembling the sun (Sutton 1988:fig. 43).

The Aborigines themselves may understand the cross motif differently; their interpretation is most frequently taken from material objects around them. This is just one of many instances when ancient archetypes and the corresponding ideograms, through successive millennia and cultures, acquire a new meaning, when a new, rational explanation replaces one forgotten. In some instances, however, a composition with a cross dominant has, in addition to the overt version, a covert one known only to the initiated (Sutton 1988:104, 118-19, fig. 144). This confirms our supposition that the cross symbol rests on a system of notions and associations with sacred meaning accessible only to those initiated into the secret versions of the myth.

The female figure with upraised arms, in a pose of adoration, is another link between Aboriginal painting and icon painting. In this way, Aboriginal art presents a woman as mythical ancestor of a clan and cultural hero — for example in Narritjin Maymuru's painting (Ryan 1990:27, 107, plate 8).

The image of the Madonna has its source in the ancient cults of the Great Mother, traces of which have been retained by the Slavs and date back to the primitive farmers of Tripolye, to Neolithic Greece and Near Asia, and perhaps even further, to the most ancient layers of human culture. There are archaic traces of the image of the Great Mother in Aboriginal culture, not only in cults and myths, but also in art.

POPULAR CHRISTIANITY AND ANCIENT ETHNIC RELIGIONS

The current religious situation in Aboriginal society in Australia has something in common with what happened in Kievan Russia at the time of the adoption and spread of Christianity. When it arrived in Russia from Byzantium in the 980s, Christianity encountered a developed, original culture with its own mythology, pantheon of gods and popular beliefs and rites. This culture in turn had its roots in even more ancient layers of human religious and mythological consciousness, which, I believe, was the source also of the images and spiritual culture of the Aborigines. Arriving in Russia, Christianity did not destroy this culture, but absorbed and reworked it. In time there came to obtain a system of 'dual belief', a compromise between paganism and Orthodoxy.

Russian icon painting, while retaining much in common with the Byzantine, differed from it in reflecting the syncretic nature of popular Orthodoxy. The Christian Son of God absorbed features of the Slavonic pagan god Dazhbog the Sun and icon painters reflected the image of Christ the Sun. Perun, another cosmic divinity, god of thunder and fire, was transformed into the biblical prophet Elias. Icon painters showed Elias against a background of heavenly fire, emphasising, as it were, his fiery nature, his connection with storm and tempest. The wheel seen on icons of Saint Elias is a symbol of the sun and of fire. In the icon *'The Fiery Ascension of Elias the Prophet'* (see above), the wheel appears as the huge disc of the sun drawn by horses of fire. This is the prophet's chariot, in which he drives across a flaming sky.

The cult of the Slavonic divinity Volos (or Veles), which may have arisen as early as the Palaeolithic as a cult of the master of beasts and later as the patron of domestic animals, was, as Christianity spread, transferred to Saint Vlas. Adopting the functions of Volos, Vlas became patron of cattle and as such he appears in icons, normally surrounded by the flocks and herds under his protection.

Features of Khors, the sun god on his horse, were transferred to Saint George. In icons, Saint George was shown on horseback in a flying scarlet cape reminiscent of his solar nature. At the same time George embodies the life-giving spring, having acquired features of Yarilo, the Slavonic god of vernal fertility. Saint George's day in Russia was celebrated in April, the traditional day for cattle to be driven out onto the fresh grass. The horse was the symbol of that day. Saint George's association with the horse, so striking in icon painting, has a pre-Christian origin. In the legend Saint George appears in combat with the serpent, vanquishing the Chtonic monster. In icons, he is shown spearing a dragon or a serpent of fantasy. Interestingly, in Aboriginal myths and art we see a triumphant serpent, while in Christian legend, reflected in icon painting, the serpent is defeated and humiliated. In Aboriginal myths and art the serpent appears as a rainbow in the sky; Saint George strikes down a creature of the underworld. Its sad fate seems to symbolise the defeat of paganism and the triumph of Christianity. Even where Russian icon painting is permeated with pre-Christian, folk motifs, its bears witness to the triumph of the new vision.

Russian popular Christianity has much in common with the popular Christianity of the Aborigines and their work on Christian subjects. As earlier in Kievan Russia, in Australia Christianity encountered the original and complex culture of the native population with a history going back for many millennia. Christianity not only absorbed the ancient, pre-Christian system of religion and myth, but itself experienced its powerful influence. The Aborigines absorbed Christianity, transforming it in accordance with their traditional notions, introduced it into their own system of ideas and images and made it *their* religion (for a typical manifesto of this new religious awareness see Rainbow Spirit Elders 1997).

Modern Aboriginal art is clear evidence of the manner of their acceptance and absorption of Christianity. It shows the Christian legend and teaching refracted in the minds of the artists, while traditional means are put to use in their expression, resulting finally in a remarkable organic fusion.

Let us look at how Jesus Christ's Road to Calvary is depicted by Miriam-Rose Ungunmerr-Baumann in her painting *'Stations of the Cross'* (Crumlin 1991:plates 20–21; see also Derrington 2000). The Daly River artist boldly introduces into her work elements of traditional symbolism. The heads of Christ and other characters are presented as concentric circles; the faces and bodies of the people burying Christ are painted in ceremonial fashion. Beside Jesus there is a huge serpent, a mystical being which plays a fateful role both in Aboriginal myths and in the Bible, the Rainbow Serpent and the mysterious creature of Eden. Introducing this image, the artist wished to express the idea of the death of Christ overcoming the forces of evil. In other terms, she attempted to convey one of the key notions of Christianity by means of pre-Christian symbolism held in common by Aboriginal culture and the creators of the Bible.

In traditional culture, the Rainbow Serpent is not only the bearer of destruction, but also a symbol of rebirth and transfiguration, as it is in a Yirrkala artist's painting, Mawalan Marika's *'Crucifixion'* (Crumlin 1991:plate 9). In the centre is Christ crucified; above him is the serpent; on both sides of Jesus are the robbers on their crosses. In the upper part of the painting is another figure of Christ ascending to Heaven on wings. In the top corner he appears in the tomb. In the one painting we see the juxtaposition of events which take place at different times and the combination of two dimensions - the real (Golgotha) and the metaphysical (the soul of Jesus ascending to Heaven). As we know, this is typical of Russian icon painting too. The serpent in the painting is the traditional, archetypal symbol of rebirth and transfiguration. Its inclusion in the crucifixion subject has, as in the previous painting, deep symbolic significance.

'The Crucifixion of Jesus', a painting by Groote Eylandt artist, Naidjiwarra Amagula, shows Christ on the cross surrounded by an aura (Crumlin 1991:plate 10). The subject of the death and the immortal, divine nature of Christ is dominant in the painting, a subject close to icon painting and expressed in the same language of symbols - the aura emanating from Christ. The same artist has a painting entitled *'The Ascension of Jesus'*, but which in reality, taking the evidence of its basic content, represents the Transfiguration (Crumlin 1991:plate 12). In the centre is the figure of Christ surrounded by an aura, as he appears in Russian icons on the Transfiguration.

Tony Swain wrote down a Warlpiri Aborigine's account of his dreams. In one he saw the heavenly Jerusalem and Jesus Christ seated in Heaven and observing the world below. Red rays, 'like torches', emanated from his throne and coloured the world red. In another the Aborigine saw the earth again coloured red; he felt lightning strike and with it Jesus entered into him. Christ, as the Aborigine sees him in his dream, is seated on his heavenly throne exactly as he is depicted in Russian icons, and from his throne emanate the same brilliant rays as in icons - symbols of Christ's divinity and power (Swain 1988:462). Christ sends this power to the Aborigine in the form of lightning striking him, exactly as spirits penetrate the body of an Aborigine undergoing initiation as a sorcerer, placing within him magic crystals which sparkle like lightning. In this connection I would like to recall that certain Abriginal groups associate the brilliance in their art with ancestral power. For the Yolngu, the brightness in their paintings emanates from the *wangarr* beings themselves and is imbued with their essence (Jones 1990:28-9; Morphy 1996). Christianity and traditional religion are organically combined in these dreams, reflecting some syncretic religious state typical of an adherent of Christianity who has not yet cast off the religious legacy of his ancestors. It is embodied in images typical of Russian icon painting, so remote in spatial terms, yet typologically so close.

Aboriginal artists' works on Christian subjects are like waking dreams, the dreams of an Aboriginal who has only the day before become a Christian. His work and icon painting, a reflection of Russian popular Christianity, can occupy the same page in the history of art. What has been happening in Aboriginal communities in Australia in the twentieth century and what happened in Russia centuries ago are two processes, typologically identical, by which ancient ethnic religions are overcome by a new religion aimed at all people and nations equally, for which 'there is neither Greek nor Jew', a religion striving to cross ethnic, cultural and racial bounds and encompass the world. In the process specific, ethnically coloured forms of Christianity take shape. Only in these forms, perhaps, can it survive.

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Erlikilyika: Arrernte Ethnographer and Artist

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'BYRNE WIRED today asking if I wanted a first class black boy [who] will go right through with us', Frank Gillen wrote cryptically on 18 February 1901 to Baldwin Spencer (Mulvaney et al. 1997:315).

Taciturn Paddy Byrne, an officer at the Charlotte Waters Overland Telegraph station, thereby became responsible for the Southern Arrernte man, Erlikilyika (known to Europeans as Jim Kite, or Kyte) accompanying Spencer and Gillen on their 1901–2 cross-continental expedition. In prevailing imperial paternalistic idiom any black male servant was a 'boy'; this intelligent man was born probably around 1865.

Erlikilyika is remembered today as a sculptor, woodworker and sketcher, an early artist to adapt to white purchasing interests, but he also has forgotten claims as a man of diverse abilities including acting as a virtual research assistant to Spencer and Gillen. Possibly he merits the title of Aboriginal anthropologist or ethnographer. This is an appropriate theme in this festschrift because Rhys Jones has done much to achieve an objective understanding of Aboriginal culture; and not less because Byrne's telegram was sent to Gillen a century to the week before Rhys Jones reached his sixtieth year.

It should be noted that in 1972 Jones and Betty Meehan witnessed the historic return of Anbarra people to their country following almost fifteen years at the Maningrida government station. They shared in the communal excitement and observed its social and ecological significance. 'The prospects seem good,' Rhys wrote to me from there on 26 July 1972, 'and we are picking up the language, and the names of edible plants and animals.' Ten years later they both were prime movers and participants in the Canberra ROM ceremony, at which the Anbarra people honoured the (then) Australian Institute of Aboriginal Studies. Their informants were treated with dignity and equality, contrasting with the racial barrier and social mores involved in the following episode.

The Spencer and Gillen expedition commenced at Oodnadatta on 18 March 1901 (Figure 1). In addition to Melbourne biology professor Spencer, and Gillen (then postmaster at Moonta), Mounted Constable Harry Chance was in charge of the stores wagon and horses (like Gillen he had served at Alice Springs and was also stationed at Moonta). Before the partners arrived at Oodnadatta, Chance had engaged a Southern Arrernte 'boy' whom they called Warwick, but whose real name was Parunda or Purunda, and whose homeland also was the Charlotte Waters district. Gillen (1968:80) described Parunda as 'a character', with 'a keen eye for the ladies of his colour', upon their departure bearing a scar from a recent reprisal by 'a jealous husband'. Within a day along the track Parunda proved his reliability by recovering two horses which strayed during the night (Spencer 1901:20 March).

Upon the expedition's arrival at Charlotte Waters, just across the Northern Territory border, Paddy Byrne provided convivial hospitality and the services of Jim Kite, at that time a telegraph stationhand. Gillen (1968:28) carefully recorded their new recruit's name in his diary as Erli-kil-yi-ka, while Spencer's journal reference was to Erlikiliakina, and once to Erlikiliaka (Spencer 1901:10 April, 30 August). As an Arrernte word for emu was 'erlia', and a totemic group of the Southern Arrernte emu people related to the Charlotte Waters area,

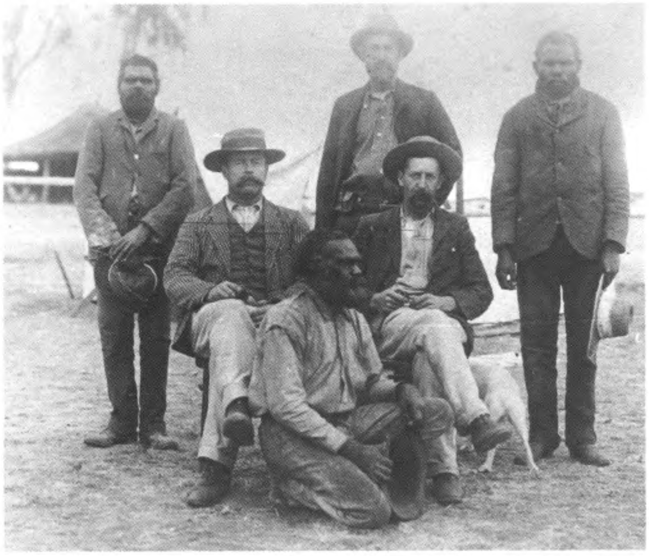


Figure 1. The 1901 expedition at Barrow Creek. Standing: Erlikilyika at right, Parunda at left, M/C Chance in centre. Spencer is seated at right, Gillen left. Tungalla is kneeling in the foreground (Royal Geographic Society, S.A. Branch, obtained 1971).

his name probably relates to emu (Luise Hercus pers. comm.; Mulvaney et al. 1997:502). Across the years Spencer remained remarkably unsure of the name, referring in publications to Erlikiliakirra (Spencer and Gillen 1904:viii), Erlikiliaka (Spencer and Gillen 1912:492), and Erlikiliakirra or Erlikilliaka (Spencer 1928:586, 406). In his field journal 'Jim' usually sufficed. Gillen sometimes wrote 'Jim', but elsewhere retained his original spelling, though occasionally adding 'c'.-.Erlickilyika (e.g. Gillen 1968:95, 100, 335). Usually, however, he used the nickname 'The Subdued', evidently a term of respect. Presumably his quiet and orderly bearing contrasted with Parunda's extrovert character.

During their seven months with the party these two Arrernte men undertook varied responsible and invaluable duties, briefly summarised here from a reading of the field records. They were literal hewers of wood and drawers of water, routinely finding water, at times digging in soaks. There was a morning schedule of harnessing often restive horses and tracking them if they had strayed. This was not always easy, as when Erlikilyika was badly kicked by a horse, or when Parunda was thrown from his mount (Gillen 1968:40, 120). An evening duty when on the move involved the construction of a bush wurley as a sleeping shelter for the anthropologists, or shades for daytime socialising with informants. Parunda became a specialist in cutting branches and erecting shelters, for which he was praised as 'a great hand' by Spencer (1901:26 October). He also was the camp baker of damper. Erlikilyika accompanied Chance, and drove the wagon team at times. He proved a resourceful marksman, shooting brush turkeys and other food supplements. On one occasion the two men combined to shoot six wallabies, while in northern watered areas Erlikilyika demonstrated abilities as an angler.

Their dedication was severely tested when duties separated them from the party in tribal lands with which they had no affiliation. On 29 May, for instance, Erlikilyika rode alone searching for two lost horses. He successfully returned the following day, having covered about 100 km. In July Parunda carried mail 170 km from Barrow Creek to Ryans Well on two packhorses. During August Erlikilyika made a 450 km round trip with three packhorses from Tennant Creek to Barrow Creek, returning with mail 'quite alone and just got off and had his tea as if he had come home by a suburban train' (Spencer 1901:30 August).

Although these solitary journeys were made along the telegraph track, now over thirty years in use, they still travelled within unfamiliar language territories, in which they had no traditional acceptance. Their intrusions must have caused them spiritual anxiety, if not fear of physical violence. Parunda apparently betrayed his state of mind at Tennant Creek during Erlikilyika's absence on the mail run. As reported by Spencer (1901:20 August; Gillen 1968: 233), 'we heard a few shots fired and found that our black boy had been aiming his revolver at what he supposed was a strange blackfellow [kurdaitcha] coming up in the dark'. Local concerned armed warriors assisted Parunda in a search for the invisible evil intruder, but an unsympathetic Spencer concluded that it was a 'matter of imagination on the part of our boy.' His cynical reflection — that with 'at least 200 dogs about and 50 or 60 fully armed' men, any Kurdaitcha man needed to be brave — completely misunderstood Parunda's fear, as an Arrernte in Warramungu land some 1100 km from home. He already had covered a distance across the interior probably exceeding the movements of any traditional inland society member in pre-European times.

Any assessment of the contribution of these two men should take these psychological factors into account. And they still had vast distances to cover, involving prolonged absence from their country and people. Spencer (Spencer and Gillen 1904:28) underestimated their resolution on solitary journeys when he observed that they 'were particularly careful to keep close to camp unless well armed, when they got amongst absolutely strange tribes ... east of the telegraph line.' Spencer also forgot that they bravely returned from Borroloola to Charlotte Waters by themselves.

This brief recapitulation of the activities of the Aborigines on the expedition to Borroloola establishes their crucial role during this classic cross-continental anthropological episode. Spencer acknowledged their contribution in print, but this was a brief, restrained and paternalistic comment. Without their presence the famous expedition would have encountered many difficulties, while their daily reliability and resourcefulness was an outstanding characteristic. Yet Erlikilyika's role acquired a further unique dimension during the six weeks spent recording Kaytej traditions at Barrow Creek during June and July 1901, when he assisted research beyond the linguistic capabilities of Gillen or Spencer. It is necessary to examine this episode in as much detail as the sources permit.

Erlikilyika was born during the 1860s, a few years before the overland telegraph disrupted traditional life. During his youth it is believed that he was employed in some capacity along the telegraph line, because by 1901 he could speak the Kaytej language (Kimber 1990:92). That a Southern Arrernte man could converse in the variants of the Arrernte language might be expected, but also to speak Kaytej was unusual; Parunda was 'ignorant' of that language (Gillen 1968:174). Byrne may have realised what an asset Erlikilyika's linguistic abilities could prove when he recommended his inclusion in the party.

As the expedition neared Barrow Creek on 6 June, they met a Kaytej elder known to Gillen (1968:105) as 'an old friend' from his Alice Springs days. This man was Tangalla (Tungulla), a senior man of the grass-seed totem, who became the most important of several informants at Barrow Creek. 'When questioning Tungulla,' Spencer recalled (1928:406), 'we always used to have one of our boys with us to help us. Tungulla knew a good deal of the Arunta dialect and Jim, or Erlikilliaka, to give him his native name, knew a good deal of Kaitisha.' As will be discussed later, Erlikilyika proved more than simply an interpreter for the anthropologists.

Satisfied upon meeting Tungulla that he was vital to their fieldwork, Spencer (1901:7 June) followed a frequently cited formula, and 'attached old man Tungulla to staff. Sent out natives to bring others in'. Three aspects of these developments merit comment. Both Spencer and Gillen considered Tungulla to be 'old'. Certainly he was a senior elder and had been involved in the 1874 Aboriginal attack on the Barrow Creek telegraph station, but photographs suggest that he was little older than themselves. (His photo is identified as Spencer and Gillen 1904:59; Gillen 1968:144; Spencer 1928 fig. 243. His ceremonial name was Arabinya-urungwina, Gillen 1968:1686, Spencer 1928:401.) Becoming a staff member simply meant 'three meals daily and a liberal supply of tobacco' and the 'farewell' gift of pipes, a tomahawk and a knife (Spencer and Gillen 1912:318; Gillen 1968:168).

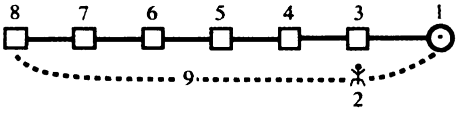
It is significant that people were lured by largesse into their Barrow Creek camp as informants. Spencer (author of Spencer and Gillen 1912:322) proved rather disingenuous in his astonishment that various ceremonies were performed for them there: 'Amongst the Arunta ceremonies belonging to different totems are never performed on the same spot or in rapid succession, but to our surprise we saw at Barrow Creek emu, grass-seed, water and other ceremonies performed one after the other in spots separated by only a few yards.' What should have surprised Spencer more was the fact that ceremonial life remained ongoing, though possibly modified, following the terrible reprisal massacres which followed the 1874 attack on the telegraph station which killed two operators (Mulvaney 1989:119–22).

Spencer was to repeat his account of Erlikilyika's dealings with Tungalla on various occasions (1912:325; 1928:407), together with a diagram. Perhaps the most authentic version is that written at the time (Spencer 1901:11 June):

> We have a splendid blackfellow with us — Jim by name ... and he helps us much and acts as an interpreter. He is by way of being an artist and is very fond of drawing on bits of paper so we give him

a sheet of paper and a pencil and he makes what he calls notes when asking Tungala questions. Of course he has not the remotest idea of reading or writing but is very proud of acting as our interpreter and as we make notes thinks that he ought to do the same also.

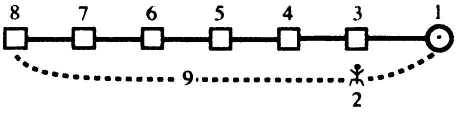
This is a copy of one of his 'notes'



1 represents a water hole where a man (2) came into existence long ago. Three is a big tree which arose to mark the spot close to the water hole. Then one day a man walked away to 4 and looked back and said hallo I can see the water him close up yet. Then he walked to 5 and 6 and made the same remark and then to 7 where he said hallo that one water long way off. Then he walked on to 8 and there he died and the black blot represents another tree where his spirit lives though it often goes back to the waterhole along the track marked 9. We are always getting odd bits of information like this and by and by shall have to piece them all together.

Spencer and Gillen's final printed version (1912:325–6; 1928:407) of this episode is more literary and merits comparison:

Tungulla himself knew a good deal of the Arunta dialect and Jim or Erlikilliaka ...knew a good deal of Kaitish. Our boy was immensely proud to help us, and as he noticed that we always took notes, evidently thought that it was the proper thing for him to do so too. Though he could not write a word, he always had a pencil and paper on which he made marks that much impressed Tungulla. His 'notes' consisted of hieroglyphics such as the following, which, apart from the numbers that we have added, is an exact copy of on of them:



1 represents a waterhole where a man, 2, came into existence in the Alcheringa: 3 is a big tree which rose to mark the spot close to the waterhole: one day the man walked away to spot 3 and looked back and said, 'I can see the water, it is close up yet'; then he walked on to 4, 5, and 6, each square indicating a resting-place, at which he made the same remark. At 7 he said 'That water long way off now,' then he walked on to 8, much further away, and there he died, the black square indicating a tree in which his spirit now lives, though it often goes back to the original waterhole along the track marked 9. The narration reduced to this concise form sounds very commonplace; but each such narrative, though it may only recount how an old man walked over the country and finally died, occupies a long time in the telling and is accompanied by much pantomimic representation of the actions of the old man - peering back with his hand shading his eyes to see if the waterhole was visible, or sitting down exhausted and breathing deeply after his hard trudge over the country. Each square indicates some natural feature such as a rock, large tree, or waterhole, that arose to mark the spot where the ancestor came to a halt.

On 14 June Spencer noted 'morning with Tungulla,' and as he drew a characteristic Erlikilyika line with numbered observation posts, it is likely that these are his direct notes of Erlikilyika's explanation involving

Tungalla's Dreaming. Below the line numbered 1–9, Spencer wrote (with some abbreviation):

1 = Tungalla a little boy in Alcheri

2 = Old man Marunda grass seed (Thungalla gave churinga to Tungalla ...)

3 = Churinga under his head when he slept

4 = Where he walks to + looked back - too close

5,6,7 still too close. then to 8 where he stood up churinga on ground then he said lubras might see it so he laid it on ground + went back along path marked 9.

Because Spencer made infrequent reference to Erlikilyika's field presence and Gillen (1968:120,152) only referred twice to him as their interpreter, the extent to which he interrogated informants, 'recorded' their stories and explained them to the anthropologists cannot be gauged. It seems reasonable to equate Erlikilyika's use of pencil and paper with a traditional message stick, upon which the markings serve to remind the messenger of the information or story which he needs to pass on. Consequently he was communicating traditional material using a traditional device, but utilising new technology.

When a newcomer arrived in camp on 16 June Tungalla's interrogation ceased abruptly until 2 July. This man was Unmatchera Jack (Ulpailiurkna), who would be known today as one of the Anmatyerre people, whose language is closely related to that of the Kaytej. It seems likely, therefore, that Erlikilyika acted as the intermediary with Ulpailiurkna during the period from 16 June to 1 July, when Spencer compiled 78 pages of notes. The only clue that this was so, however, was an entry on 24 June accompanied by another of the tell-tale mnemonic devices.

Whether or not Erlikilyika worked with this co-operative Anmatyere informant, he presumably assisted with the interrogation of other Kaytej informants, including Imbarkwa, head of the rain totem, another of Gillen's 'old friends' (1968:128, 143). Imbarkwa provided important esoteric information which, as communicated by Spencer and Gillen (1912:323–4), is reminiscent of a reconstruction based upon Erlikilyika's presentation.

The minimum time in available records which he spent recording Tungalla's stories ran from 11 to 15 June inclusive and 2 July. It is particularly revealing to consider Gillen's concise comments on 2 July (Gillen 1968:152):

> Erlikilyika has entire charge of the Ethnological branch today and is on his own account digging up a Kaitish tradition which he is carefully recording on paper in his own peculiar fashion tomorrow we will check and record it in the official journal ... .

That 'tradition' fills three pages of Gillen's diary for 3 July (1968:152–5).

The anthropologists surprisingly seemed oblivious to the reality that Erlikilyika's activities involved potential danger both for himself and his informants. This is implicit in Spencer's rather patronising account of the revelation of magical practices by Tungalla, when they cajoled him into demonstrating the practice of pointing a stick at a victim (Spencer and Gillen 1904:462; 1912:326; Spencer 1928:408). 'After a good deal of persuasion we induced [Tungalla] to show us how they were used,' they wrote. 'Our boy Erlikilliaka promptly retired to what he considered a safe distance, which was out of sight'. Ritually pointing the stick, but lacking any victim to absorb the magic, Tungalla induced dizziness in himself, 'and he declared that the evil magic had gone into him and that he felt, as he looked, very bad.'

On another occasion (Spencer 1901:15 June) they 'had a try with Tungula', to encourage him to recount sacred ancestral stories, obviously involving the assistance of Erlikilyika. 'The old beggar didn't even want to tell us things,' Spencer remarked imperiously, 'and we could not help smiling when he said he was too young to know these sacred things - seeing that he is at least 60 years old. However he soon relented and once started we could not stop him'.

Such demands upon the informant must have imposed considerable psychological pressures, particularly revealing sacred lore to inappropriate people. Consider, also, the unease of the interpreter. Erlikilyika was the vehicle for prying into the secret and sacred myths of a people with whom he had no kinship or spiritual relationships. That he and Parunda returned a few months later through Kaytej territory suggests their bravery, if not foolhardiness.

A close reading between the lines of the sources suggests that the anthropological success of the Barrow Creek leg of the expedition involved Erlikilyika to a much greater extent than the anthropologists revealed in their publications. In a sense, it was he who was the ethnographer of the Kaytej society. Was Erlikilyika the first Aboriginal anthropologist?

Weary and suffering under the humid tropical conditions, the party reached Borroloola on 2 November 1901, to be marooned there until February. While Spencer and Gillen always intended to sail home from some northern port, it was assumed that the Arrernte men would return overland to Charlotte Waters. With that intention food supplies had been cached for them en route, although there is only one such record in the journals, of a capacious hollow tree concealing tinned meat and jam (Gillen 1968:306, 335). Obviously, also, they were known to telegraph line staff who would have sustained them.

For the Arrernte, Borroloola proved a foreign, threatening place, with its unnatural crocodiles and sticky heat. Parunda was seriously upset because the tidal river flowed upstream, totally against nature as he had experienced it. More importantly, however, Spencer (1901, 14 November) grasped their predicament. 'They cannot mix with these blacks as the members of one tribe do not like these of a far distant tribe and it would not be safe for them to go far away from our camp.'

On 16 November 1901 the pair set forth, on horseback, with two packhorses carrying their gear, and each man armed with a revolver. Four strenuous months later they safely reached Charlotte Waters 'and delivered their horses and impedimenta, all in perfectly good order' to P.M. Byrne (Spencer 1928:586-7). Their eager kin must have been regaled with tales redolent of the Dreaming, the returning heroes gaining prestige through the telling. Yet for all their services to anthropology their material reward was limited. According to Spencer (1928:586), 'they left us with possessions enough to make a black boy happy for the rest of his life.'

Whether Parunda enjoyed his subsequent life is unknown, for he vanished across the threshold of unrecorded history. Not so Erlikilyika, who embarked upon the artistic career for which he gained considerable praise from white Australians during his lifetime.

Gillen (1968:75–6) was responsible for encouraging his sketching potential when, camped at Alice Springs, he asked Erlikilyika 'to fill in the pages with examples of his artistic skill', when he accidentally left two blank pages in his field diary. Erlikilyika was so enthused that his eleven sketches filled the book, much to Gillen's annoyance, although he agreed that as 'an example of original drawing by our Australian natives the pictures are not without interest.'

Three weeks later, at Titree Well, Gillen (1968:102) 'supplied Erlikilyika with a book which he is going to fill with original sketches for my boys.' Those 27 drawings were preserved by Brian and Jack Gillen, and through Jack's son, Dr Robert Spencer Gillen, they have been acquired by the National Museum of Australia (Figures 2–5). The Alice Springs sketches were reproduced in the 1968 publication of Gillen's diary.

At some undated period, circumstances unknown, Erlikilyika filled 24 pages of a sketchbook, now in the South Australian Museum. These are bold, carefully arranged representations of trees, the name of each plant provided in both English and Arrernte. They represent an unusual botanical study (Sayers 1994:78–9). This approximation to 'scientific' presentation may have been a by-product of his experiences on the ethnographic expedition, but these botanical drawings reflect a more precise and balanced vision than the rough, though sometimes whimsical, sketches of people, animals and places depicted in the Gillen artworks.

Erlikilyika's skill in sculpting or carving figurative items from kaolin clay (gypsum) and wood developed across the two following decades. His chief medium was the kaolin available in quantity near the Charlotte Waters telegraph station, where it was quarried in small lumps. His stock-in-trade was carved meerschaum tobacco pipes in various motifs and shapes, his craftsmanship attracting favourable comment. A report in the *Advertiser* (24 March 1910) stated that he 'has become so proficient in the art of pipe-making that he devotes nearly the whole of his leisure time to the work, always having more orders on hand than he can carry out'.

Covetous eyes turned towards his kaolin source as a potential quarry for china clay. The same *Advertiser* report contained the information that a clay sample had been given for analysis to H.Y.L. Brown, the Government geologist. It was pronounced of 'excellent quality ... unusually white and superior' to other sources. In view of subsequent analyses, discussed later, this was a surprising finding. In any case access problems ensured no positive outcome, so Erlikilyika and some other Charlotte Waters men were left to pursue their craft with sole access to the kaolin.

In 1913 Erlikilyika was brought to Adelaide by H.O. Kearnan, then stationmaster at the Charlotte Waters telegraph station (Figure 6). He succeeded Paddy Byrne in that post in 1909, and as he was stationed there between 1896 and 1918, he must have approved and promoted Erlikilyika's craftwork. It was reported late in his life, that 'he was taken once to the city to be trained, but ... the experiment was a failure' (*Daily Telegraph*, 9 August 1926). As he was interviewed by the press in Adelaide during 1913, in Kearnan's presence, it must have been at this time that he resided briefly in Adelaide.

The most detailed description of Erlikilyika's production techniques followed his 1913 exhibition in an Adelaide hotel (Figure 7). 'I saw

Figure 2. 'The Professor': From Erlikilyika's sketch book for Gillen's son (Dr R.S.Gillen).

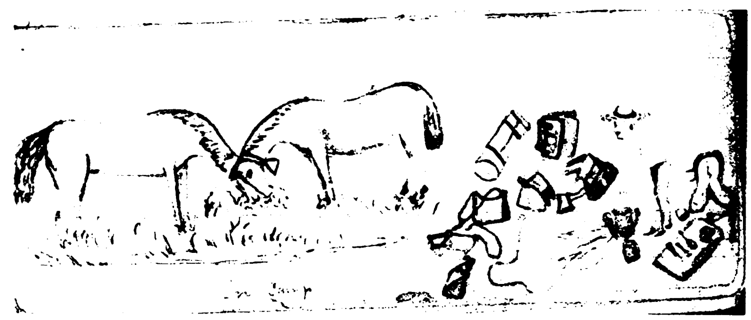


Figure 3. 'In Camp': From Erlikilyika's sketch book for Gillen's son (Dr R.S.Gillen).

Figure 4. 'Fixing the tent' Gillen and Parunda? : From Erlikilyika's sketch book for Gillen's son (Dr R.S.Gillen).

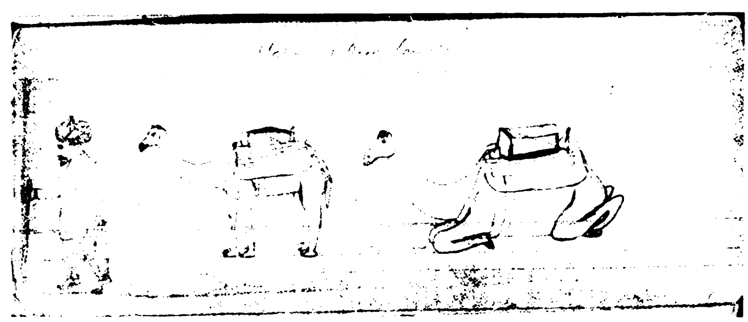


Figure 5. 'Afghan and pack camels': From Erlikilyika's sketch book for Gillen's son (Dr R.S.Gillen).

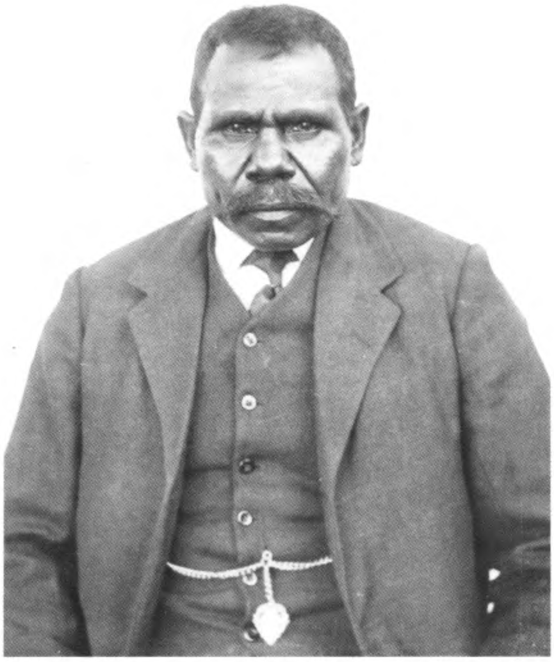


Figure 6. Erlikilyika, presumably in Adelaide, 1913 (South Australian Museum, H.Basedown, AA22/15/11).

ABORIGINAL ART CARVINGS ON KAOLIN.

Figure 7. Erlikilyika's art exhibited in Adelaide, 1913. Several of these items are in the Basedow Collection, National Museum of Australia (Observer, 26 July 1913, courtesy State Library of South Australia).

the triumph of the aborigine's handwork,' a reporter for the South Australian *Register* (18 July 1913, p. 7) enthused:

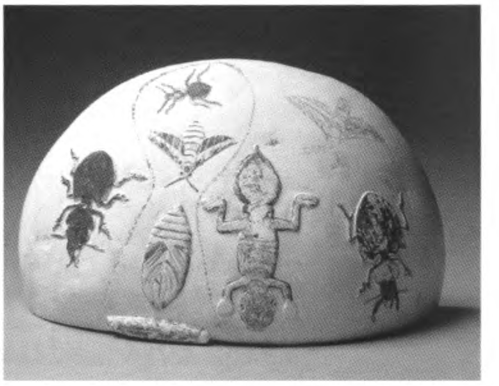
They ought surely to be purchased for the national collection .... The table was spread with perhaps a score of valuable and diversified creations ... . I saw marsupial rats with life so realistically suggested ... snakes twisted in dark-patterned ribbons ... and locusts almost hopping ... . aborigines in the picturesque raiment of corrobborees [sic] ... . Jim Kite's graphic knife had carved birds with such delicacy of form and such a superb sense of rhythm ... spread on the ground and wondrously ornamented boomerangs ... .

Presumably it was at this time that W. Ramsay Smith (1925:245) examined 'some beautiful specimens of carving in kaolin ... of truly artistic character.' J.R.B. Love (Kimber 1990:92) praised Erlikilyika's carving. '... native birds, insects and animals he has carved with astonishing accuracy, and further completes his work by colouring the creatures with ochre and various pigments he obtains from plants'.

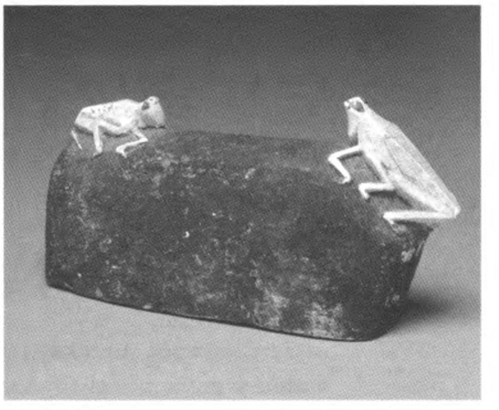
In 1917 *The Northern Territory Times and Gazette* (1 November) reported the gift of some kaolin pipes to the Administrator of 'very creditable' workmanship by an Aboriginal who also 'does some fine woodcarving'. Apart from a period when Erlikilyika lived in Adelaide, associated with his 1913 exhibition, most of his sales came from passing travellers along the overland track. Captain S.A. White (1914:56) inspected 'Jim Kite's work' at Charlotte Waters in 1913. Herbert Basedow (1935:108–9, 127) called there in 1920 and 1923 and judged 'my old friend Orknadinja, alias Jim Kite — an unusually talented person'. Basedow attended a ritual ceremony (a 'little pantomime'), when Jim 'rendered the legend in the poetical way it was given to the initiate'.

Fortunately for posterity Erlikilyika's sculptured figurative kaolin objects and decorated boomerangs and spearthrowers are prominent in institutional collections, particularly the South Australian Museum and the National Museum of Australia (Figure 8). The latter houses Herbert Basedow's extensive collection, much of which he acquired from the 1913 Adelaide exhibition. Some pieces were illustrated in a remarkably clear montage in the *Adelaide Observer* (26 July 1913:30).

Some objects are life sized carved grasshoppers or cicadas, others are small slabs decorated with human activity scenes, plants or insects; scorpions were popular. Wooden artefacts have realistic lizard, dingo or other animal motifs. Observers saw the artist work with simple tools - a broken axehead or shearing blade gripped with some rag and gum, a penknife or piece of wire (Tindale 1964:1087; Lindsay 1968). Traces of ochre on many items indicate that they were painted, although others relied upon the whiteness of the clay for their visual impression.


a)

These productions indicate a great improvement in Erlikilyika's artistry when compared with the Gillen sketches, or alternatively, the medium of carving better suited his traditional skills. It provides evidence for his clever exploitation of the tourist trade, through applying traditional skills, and his untutored expertise as an indigenous naturalist to art forms which appealed to European taste. He remained a collectable artist into the 1920s, but then his name disappeared for half a century. His work has been recognised as significant only during the past decade, by Sutton et al. (1989:198-9), Sayers (1994:78-9) and Morphy (1998:263-5).


b)

Early Aboriginal artists in contact situations, including Albert Namatjira, have conventionally been labelled producers of assimilated fine art. Morphy (1998:266–73, 355) cautions about glib categorisation of artists from diverse times and localities. He emphasises that the Arrernte artists typified by Namatjira 'can equally be seen as Aboriginal representations of land that challenged both European occupation and European preconceptions of the centre as an arid wasteland.' It is relevant to this re-appraisal, that like the Hermannsburg artists whose landscapes excluded a European presence, Erlikilyika's oeuvre chiefly embraced the fauna, flora and indigenous people.

'The Subdued' as Gillen nicknamed Erlikilyika, was a dignified, reliable and adaptable character. His many years of productive sculpting of figurative, naturalistic objects was a remarkable individual response to his experience and circumstances. Such was his fleeting expeditionary celebrity, however, that the *Register* journalist who stressed the vigour and naturalness of his artistry, evidently was unaware that 12 years previously Erlikilyika had been a member of the Spencer and Gillen expedition. It possibly was an echo of a misunderstood conversation, however, that led the writer to state that the artist had visited Sydney and Darwin, both unlikely destinations at this period, before Aboriginal art became popular with collectors, galleries and the discriminating public.


c)

However, Erlikilyika's influence produced an unusual sequel. Around 1919, Lieutenant C.W. Gray AFC visited London where he was observed smoking a distinctive Jim Kite pipe. It prompted 'several enquiries' concerning its origin. An English porcelain manufacturer expressed interest in the source of the clay. In March 1920, therefore, the Secretary of the Commonwealth Department of Home and Territories requested that the Charlotte Waters postal officials obtain a sample of the kaolin for testing for its industrial potential (Australian Archives CRS A3 NT 1922/831).

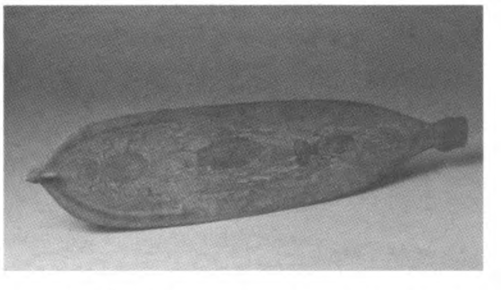

d)

Figure 8. Selected items in the Basedow collection, NMA.a,b,c. Sculptured figures from white kaolinite with ochre overlay (nos. 1985. 60. 797,–797,800). d. Wooden spearthrower (no. 1985. 60 .598). (National Museum of Australia, photographer George Serras).

Within two weeks a sample of the 'white stone as used for carving by Aboriginal Kyte' was sent to Melbourne. A geologist subsequently visited Charlotte Waters and surveyed and described the rather nondescript deposit, which lay concealed over most of the area and which was retrieved only in small blocks by Aborigines. He referred to a vase at the telegraph station about 30 cm high and 15 cm across, 'artistically carved by Aboriginal Jim Kyte.' The South Australian Museum holds three comparable vases with incised decoration (especially SAM A41325).

Hopes for a viable industry were soon dashed. Unlike the optimism of the 1910 report on the clay, transport to the Oodnadatta railhead was estimated to cost £4 per ton, while the scientific analysis of the sample at the Commonwealth Institute of Science and Industry laboratory pronounced the kaolin as possessing 'no value as a pottery material'. A Home Affairs officer bleakly made a file note: 'it looks as if this is not likely to be much use commercially,' and there the matter ended (Australian Archives CRS A3 NT 1922/831—1, 13 March, 1 September 1920; 17 September 1921).

In parallel with the waning fortunes of Charlotte Waters, Erlikilyika's life edged towards obscurity. The telegraph station was bypassed when the railway was extended from Oodnadatta to Alice Springs between 1927 and 1930. The telegraph station closed in 1930, because telegraphy no longer required the repeater facility. By 1940 the impressive building was a ruin and the sales outlet had vanished. In 1935 the local policeman was farewelled with an Aboriginal 'corroboree send-off'. Although Jack Kite participated, there was no reference to the presence of his brother Jim (Luise Hercus pers. comm.). He had been described in 1926 as 'old' (*Daily Telegraph* 9 August 1926).

At the centennial of Erlikilyika's epic anthropological expedition it is appropriate to acknowledge more fully his role as anthropologist and artist. He represents a role model from the past for Aboriginal Australians. It is a sad reflection on white society's attitude to indigenous people early this century that he was treated as a passing curiosity. Even Baldwin Spencer's references to his research assistant reflect poorly on Spencer's understanding and racially superior outlook. It is worth reflecting that progress has been made in racial relations, because today such an Aboriginal would be a celebrity whose artworks would be keenly sought; yet even so, his kin would be unlikely to have the respect of the white community.

Acknowledgements

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Art at 40,000 BP? One Step Closer: An Ochre Covered Rock from Carpenter's Gap Shelter 1, Kimberley Region, Western Australia

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A SMALL slab of painted rock from Carpenter's Gap Shelter 1 in the Napier Ranges, Central Kimberley, has been dated to ca. 40,000 BP. This find adds to a growing body of data that indicates the widespread use of ochre — and by implication, art — as an aspect of the earliest human occupation of widely separated and environmentally diverse regions in Australia (Jones and Johnson 1985:219; Smith and Fankhauser 1996; Smith et al. 1998).

The rock slab is limestone which is the parent material of the shelter, and is covered in a deep red pigment. This is on two sides and one edge, suggesting that it was painted while attached to the parent rock as a ledge, joined at the remaining unpainted edge. If this interpretation is correct, the act of painting must be as old and probably older than the excavation level from which it was recovered.

The slab and a piece of red ochre from the same excavation unit were subjected to elemental analysis to determine if the red coating resembled ochre and whether it had the same composition and likely source as the piece of ochre found alongside it. A description of the slab and the results of the elemental analysis are presented. The significance of the find is discussed within the context of other Pleistocene evidence for art in Australia and ethnographic symbolic expression in the Kimberley region.

CARPENTER'S GAP 1 ROCKSHELTER

Like many of the rockshelters in the Napier Range area, Carpenter's Gap 1 (Figures 1 and 2) contains spectacular painted and engraved art panels on its roof and walls. In the upper section of the shelter the low overhanging roof is covered with red, yellow, brown and white ochre paintings and charcoal drawings in the most recent Kimberley art style which is associated with the ancestral creative beings, known as Wandjina (Crawford 1968, 1977; Walsh in press).

The shelter is in Bunuba country and the Bunuba and other Kimberley people who share this ideology believe that the Wandjina experienced 'creative journeys which left the land and all living matter in its present form' (Vinnicombe 1992:10). Following the creation of the clan estates they put themselves in a shelter within each clan estate. 'Each Wandjina has a name, a moiety and a set of totemic symbols from which each clan is directly descended' (Vinnicombe 1992:10) and for which the members of that clan are responsible. Carpenter's Gap 1 contains a large Wandjina, indicating that this shelter is prominent in the ideology of the Bunuba, as attested to by the traditional owners of the site who continue to visit it. The painted art in this site is not 'open', hence no

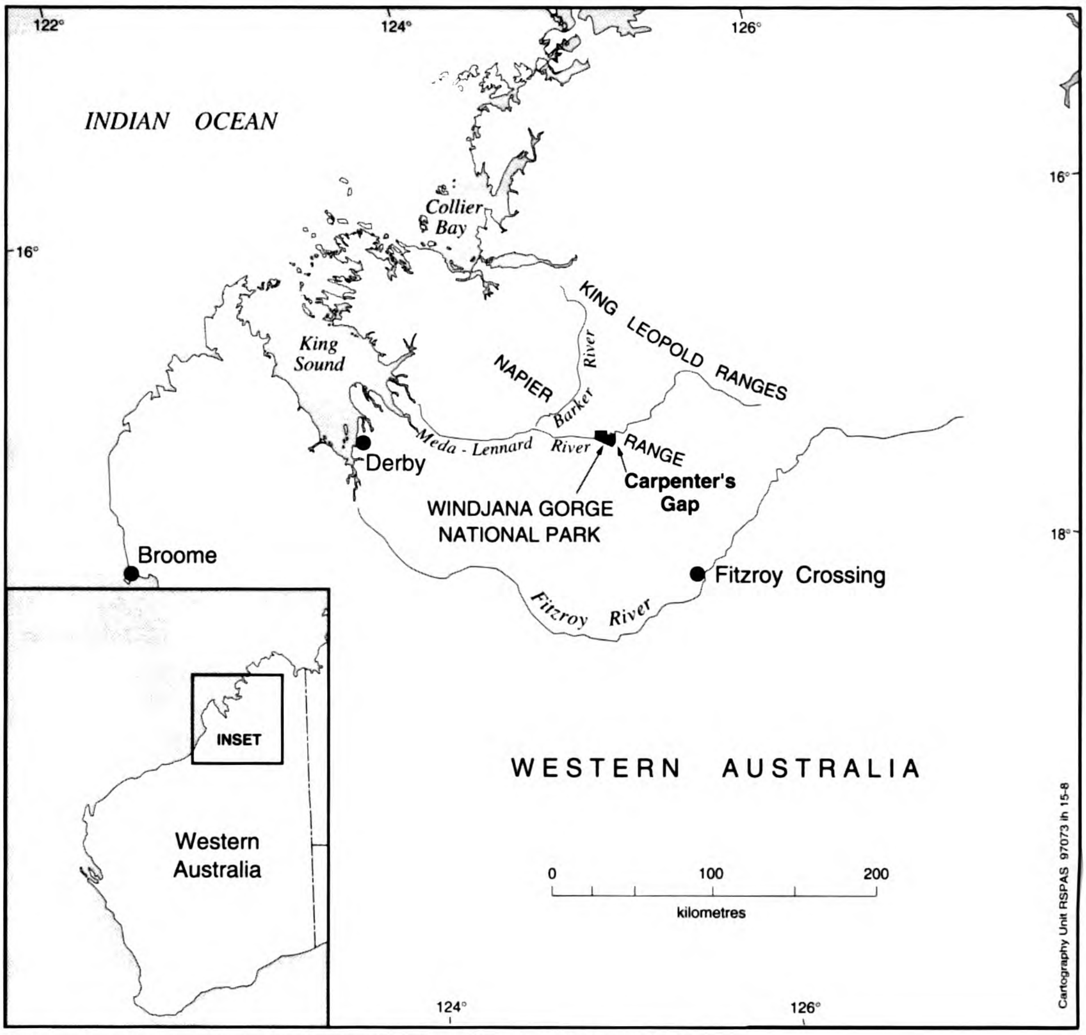

Figure 1. Location map showing Napier Range and Carpenter's Gap 1 Shelter.

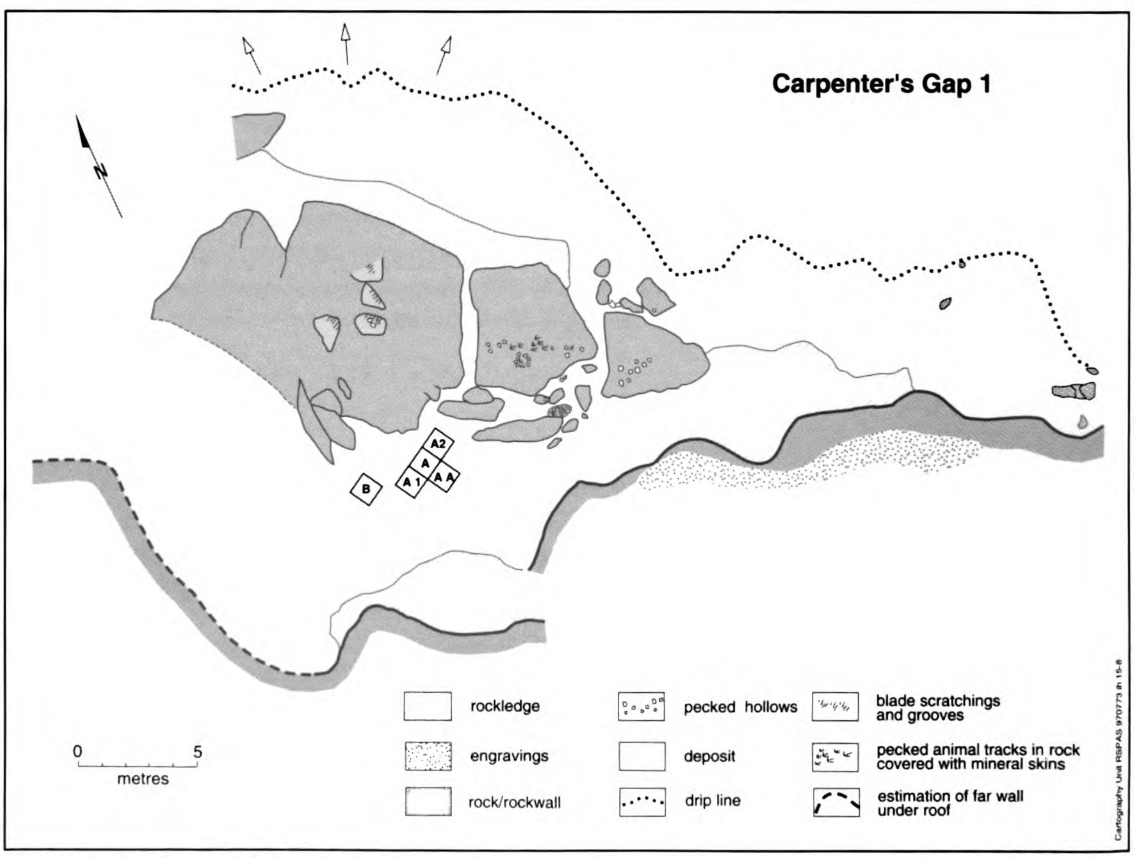

Figure 2. Plan of Carpenter's Gap 1 Shelter.

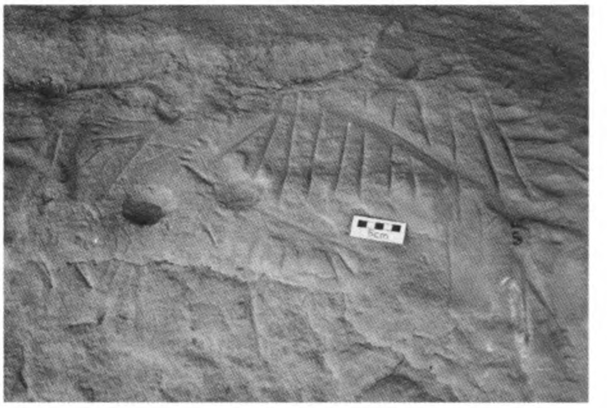

Figure 3. Engravings: geometrics, human footprints, grooves and cupules.

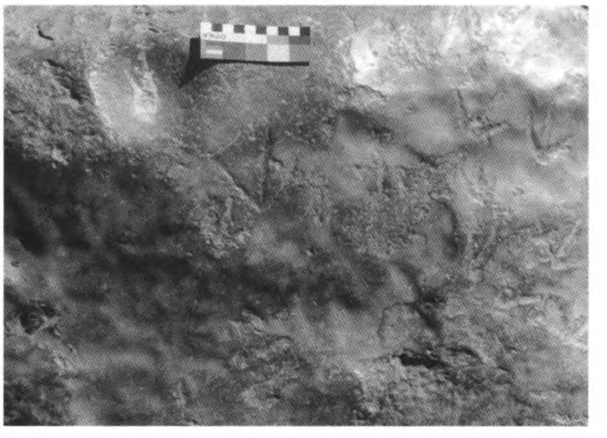

Figure 4. Petroglyphs of bird and macropod tracks covered in mineral skins.

the earlier Bradshaw style (Walsh 1994) were seen in this shelter or any of the adjacent shelters in the Napier Range.

Extensive engravings are found at the base of the gently sloping back wall of the shelter and on large boulders in the central area of the shelter inside the dripline. Those on the base of the back walls are abraded and incised and include a variety of abraded grooves, geometrics, animal tracks, human footprints (Figure 3) and a large figurative crocodile in plan view. In the central upper portion of the shelter massive sandstone and limestone boulders have served to trap the deposit and it is in this area that the deposit has greatest depth and the excavation was undertaken (Figure 2) (O'Connor 1995). These boulders are also covered in engravings which are pecked and abraded and comprise bird/trident and macropod tracks (Figure 4) and deep circular depressions or pits. The upper surfaces of these boulders are covered in oxalate-rich crusts that are no longer forming under present climatic conditions. While no dates are available for crusts over the engravings, basal dates on the crusts from off-art locations on the boulders indicate a Pleistocene age for the beginning of crust formation at this site (Watchman et al. 2001). There is little doubt that the engravings covered by the crusts are considerably older than the paintings on the shelter roof.

In all, five 1 m × 1 m squares were excavated, Squares A and B in the 1993 field season and Squares A1, A2 and AA, adjoining square A, in the 1994 field season. The limestone slab and ochre pellet were recovered from close to the base of Square A in the first excavation season. Despite enlarging the excavation in the following season no further fragments of ochre-covered rock were found.

The oldest ^{14}C date (charcoal AMS) from the excavation, from Spit 48 of Square A, indicates that this site was initially occupied 42,800±1850 BP (OZD-161). The slab, which comes from Spit 47 in this square, is bracketted by this Spit 48 date, only 2 cm below it, and another of 33,600±500 BP (ANUA-7626, ABOX-880c) (Fifield et al. submitted) which dates Spit 45, ca. 5 cm above it. Spit 40, previously dated to 39,700±1000 BP (Wk-3078) (O'Connor 1995) has been re-dated by two other charcoal samples to 25,390±370 BP (ANU-11163) and 23,140±620 BP (ANU-11241). The radiocarbon dates would suggest a minimum age of ca. 40,000 BP for the slab. Fuller discussions of the radiocarbon results have been presented elsewhere (O'Connor 1995, McConnell and O'Connor 1999, Wallis in press) along with a preliminary description of the excavation (O'Connor 1995). While samples of the sediment from the deposit were taken for OSL dating, the background radiation in the lower sediments was high and the OSL signal was saturated (Roberts pers. comm.).

DESCRIPTION OF THE LIMESTONE SLAB AND ANALYSIS OF THE PIGMENTS

The limestone slab has maximum dimensions of 20.5 cm × 7.5 cm and is 2.8 cm thick (Figure 5). Ochre appears to be present on both faces and an edge. An unweathered surface indicates where it was presumably broken from the shelter roof or wall. The slab does not appear to be have been used for grinding ochre or as a palette, as the ochre is thinly applied and there are no striations or visible signs of abrasion on the slab. The slab surface is uneven and quite unlike a fragment of any type of grindstone, even of the informal or

Figure 5. Limestone rock slab with ochre from Carpenter's Gap 1.

expedient type (Smith 1985, 1986) and there are no areas where ochre has lodged in low points or crenulations on the surface. Rather, the ochre seems to have been applied by a method resulting in a thin even coating, possibly by blowing of wet pigment (Flood pers. comm.).

The walls of the Carpenter's Gap 1 Shelter have no evidence of iron or other mineral staining. However, elsewhere, most particularly where shelters are composed of sedimentary rocks such as sandstone or siltstone, haematite may be found naturally occurring in the bedrock, and water leaching through the rock can lead to red staining on the surface of the shelter walls.

An elemental analysis was carried out to determine if the red coating could be analysed while still attached to the surface of the slab (hereafter *in situ*) and if this coating was consistent with ochre. The analysis of the red substance on the rock slab presented an analytical problem because the substance was intimately associated with the rock slab and therefore the analysis had to be done *in situ*. Not only was the red layer thin but on a microscopic level it was uneven with a blotchy appearance. Red staining due to leaching of iron rich minerals would likely produce a more even coating than that observed. Samples containing red stains from leaching were not available for comparison, but it is likely that they would have variable compositions dependent upon parent material. Energy dispersive x-ray analysis (EDXA) penetrated the red layer, simultaneously analysing this layer and the rock substrate to different degrees dependent upon the thickness of the red layer. Determining if the substance resembled ochre involved a comparison of elemental analyses between the 'background' (limestone slab) and 'background' with red coating. Coatings of other ochres with known elemental concentrations on the same background were analysed for comparison with the slab. Tests were used to determine if the red coating on both sides of the slab could reasonably be said to come from the same ochre and if the ochre pellet found alongside the slab matched that on the slab surface or could conceivably have the same source.

EXPERIMENTAL

Samples

Sediment clinging to the rock slab was blown off with high pressure air. Samples were cut directly from the rock slab using a diamond saw with water rinse. Cut pieces were then rinsed with distilled water and dried at 115°C. The following samples were prepared for analysis: (1) three blanks - one with a smooth surface from the saw cut (Saw Blank), and the others from the top and bottom of the rock slab with the original surface of the slab from which the red coating was removed using a toothbrush (Top and Bottom Blanks); (2) rock with pigment from top and bottom (Limestone + Top and Bottom Layers); and (3) further saw blanks coated with ochres: CG1-A-47, Bookartoo (#67979 sample from Bookartoo quarry, South Australia, Jones 1984a, 1984b), and Karrku (#6 sample from Karrku quarry, Peterson and Lampert 1985, Smith and Fankhauser 1996; Smith et al. 1998). In addition, finely ground thick samples of these ochres were analysed. Since the analysis was done *in situ*, the thin coatings of ochres on pieces of the limestone slab would furnish information on the combination of slab background plus ochres. Ochres from Bookartoo and Karrku quarries were picked for analysis because they offered quite different major element compositions, especially in their silica, aluminium, and calcium contents.

Energy Dispersive X-Ray Analysis

A JEOL JSM-6400 scanning electron microscope fitted with an Oxford 138 eV SATW 10-mm^2 detector was used for EDXA. The analysis system was an Oxford ISIS using SEMQUANT. Analyses were done at 120–160X magnification using 15 keV at 1 nA for 200 seconds live time.

Regression Analysis

A multiple regression algorithm (Minitab 1989:Ch. 7) was used to determine the best estimate (in a least squares sense) of the percentage of ochre on limestone. A stepwise regression algorithm was used to choose the ochre closest in composition to the rock slab ochre sample. Stepwise regression uses the maximum F-statistic to identify a useful subset of the predictors (Minitab 1989:Ch. 7). This is equivalent to minimising a goodness of fit statistic of an ochre with the estimated ochre given a least squares estimate of the proportion. The constant (intercept) term was omitted from equations. A database of EDXA results for 43 ochres was used for stepwise regression.

RESULTS AND DISCUSSION

The results for the analysis of the slab blanks and pigment layers on limestone are presented in Table 1. They are presented as element percentage as determined by EDXA. The analyses do not total to 100% because two important elements, oxygen and carbon, could not be analysed accurately and therefore are not included. In addition, sample porosity lowers the total percentage analysis (Goodhew and Humphreys 1988). Often analyses of this nature are presented as oxides and totalled to 100%, but to do so here would introduce errors in the actual compositions because calcium, especially in the limestone, is a carbonate rather than an oxide. Calcium of course could be mathematically converted to a carbonate (Ca and C are in a ratio of 1), but ochres, though often having calcium in the form of carbonate, have calcium in a variety of minerals. For example, Bookartoo ochre has calcium in the form of calcite, $CaCO_3$, dolomite, $CaMg(CO_3)_2$, and ankerite, $Ca(Mg_{0.67}Fe_{0.33})(CO_3)_2$ (Jercher et al. 1998). Also, note that Mg and Fe are not only oxides but carbonates as well.

The rock slab (Saw Blank) is a limestone given its high calcium and low silica content. The original surfaces of the limestone slab (Top and Bottom Blanks) are similar to the cleanly sawn surface (Saw Blank) although the Top and Bottom Blanks have higher concentrations of some elements which may be from residual pigment and a mineral skin which forms on rock surfaces (Watchman 1990). An *in situ* analysis requires the analysis of the rock surface with pigment removed because of the possibility of differences between a fresh surface and the surface with a pigment layer.

The red pigment on the rock slab is thin, resulting in a proportionally greater importance for the elemental concentration of the background. The limestone background cannot simply be subtracted from the *in situ*

Table 1. EDXA elemental analysis (%) for limestone blanks and in situ pigment layers. The numbers in parentheses indicate the number of analyses making up the mean. S is the sample standard deviation.

Element	Limestone Saw Blank		Limestone Top Blank		Limestone Bot. Blank		Limestone + Top Layer		Limestone + Bot. Layer	
	Mean (9)	S	Mean (4)	S	Mean (4)	S	Mean (2)	S	Mean (2)	S
Na	0.10	0.03	0.07	0.01	0.05	0.03	0.12	0.02	0.03	0.02
Mg	0.14	0.03	0.25	0.03	0.09	0.01	2.79	0.05	2.09	0.02
Al	0.02	0.01	0.17	0.03	0.21	0.15	3.21	0.01	3.17	0.27
Si	0.11	0.05	0.53	0.02	0.33	0.25	8.92	0.03	8.14	0.40
P	0.03	0.02	0.15	0.01	0.07	0.02	0.71	0.04	0.55	0.01
S	0.09	0.02	0.05	0.01	0.02	0.02	0.24	0.01	0.12	0.02
Cl	0.11	0.03	0.05	0.01	0.03	0.02	0.07	0.03	0.09	0.01
K	0.14	0.03	0.11	0.02	0.08	0.03	1.11	0.01	0.83	0.06
Ca	34.97	0.21	31.33	1.41	35.55	0.14	15.06	0.04	15.71	0.17
Ti	0.01	0.01	0.01	0.01	0.02	0.02	0.15	0.03	0.13	0.01
Mn	0.05	0.03	0.06	0.01	0.06	0.05	0.05	0.01	0.06	0.01
Fe	0.04	0.05	0.32	0.06	0.08	0.05	10.64	0.00	8.60	1.44

analysis of the red coating plus background because the relative contributions of each are unknown and ochres often contain significant amounts of calcium as indicated by ochres CG1-A-47 and Bookartoo (Table 2). However, the limestone has only minor concentrations of elements other than calcium and some general conclusions can be made from the elemental analysis of the Top and Bottom Layers compared with the Limestone Blanks. The elements Mg, Al, Si, P, K, and Fe are in relatively high concentrations in the red layer. Other than P these elements are often found in high concentrations in ochres, for example see CG1-A-47, Bookartoo, and Karrku (Table 2). Visually, at both macro and micro levels, the red substance resembles an applied ochre. The elemental analysis results do not conflict with these observations.

The most desirable red ochres have the highest Fe content from the mineral haematite, Fe_2O_3. Both Bookartoo and Karrku ochres are examples of this. The 'dilution effect' of the limestone for elemental analysis of *in situ* layers of ochre can be seen by comparing the analytical results for CG1-A-47, Bookartoo and Karrku ochres with their coated analyses (Table 2). (Note that the sample Blank + Bookartoo was thickly coated so the background contributed only 1.7% to the analysis.) Multiple regression of the Blank + Karrku on two predictors, Karrku ochre and Saw Blank, resulted in the equation, Blank + Karrku = (0.393)(Saw Blank) + (0.672)(Karrku), for which the ochre contributed 63.1% [(0.672 x 100)/(0.393 + 0.672)] and the limestone contributed 36.9% to the elemental analysis (Table 3). With a thinner coating these figures were 26.0% and 74.0% for ochre CG1-A-47. From the results in Table 2 it can be seen that the major element concentrations (Mg, Al, Si, Fe for CG1-A-47 and Al, Si, K, Fe for Karrku) are diluted to quite an extent by the limestone. This dilution would be more pronounced for the red pigment on the rock slab as it is visually thinner than those coatings applied onto limestone in the laboratory. This indicates the red pigment has relatively high concentrations of Mg, Al, Si, and Fe and again is consistent with an ochre.

The elemental analysis of an archaeological ochre sample from Kutikina is given in Table 2. Stepwise regression of the EDXA results for the Top Layer on the Top Blank and 43 ochres picked sample A3 SW from Kutikina (Kiernan et al. 1983) as the best match with close agreement for most of the major elements. Note that elements in higher concentration have a large influence on the calculated regression equations used to give the predicted values in Table 3. Comparison of predicted compositions for Kutikina Top and Bottom Layers in Table 3 with the Limestone + Top and Bottom Layers in Table 1 shows large disagreement for Mg, P and S. While it is extremely unlikely that Carpenter's Gap and Kutikina ochres are identical given that the Kutikina site is in Tasmania, the composition of the ochre on the rock slab may be similar to Kutikina despite the Mg, P and S concentrations of the ochre on the rock slab being larger. To give

Table 2. EDXA elemental analyses (%) for ochres and ochre coatings on limestone. The numbers in parentheses indicate the number of analyses making up the mean. S is the sample standard deviation.

Element	Ochre CG1-A-47		Limestone + CG1-A-47		Ochre Karrku		Limestone + Karrku		Ochre Bookartoo		Limestone + Bookartoo		Ochre Kutikina	
	Mean (13)	S	Mean (3)	S	Mean (13)	S	Mean (3)	S	Mean (9)	S	Mean (2)	S	Mean (2)	S
Na	0.17	0.05	0.08	0.02	0.18	0.04	0.27	0.01	0.14	0.06	0.20	0.04	0.07	0.01
Mg	0.54	0.06	0.19	0.03	0.28	0.03	0.28	0.01	2.50	0.07	1.66	0.06	0.87	0.06
Al	1.44	0.17	0.18	0.02	6.32	0.20	3.40	0.06	0.91	0.05	0.56	0.00	3.30	0.13
Si	2.49	0.29	0.38	0.05	14.66	0.28	9.16	0.21	3.52	0.14	2.15	0.04	9.96	0.27
P	0.07	0.03	0.05	0.01	0.25	0.05	0.12	0.02	0.09	0.05	0.04	0.01	0.23	0.00
S	0.13	0.03	0.11	0.02	0.10	0.03	0.22	0.03	0.05	0.03	0.07	0.01	0.03	0.01
Cl	0.13	0.06	0.13	0.01	0.10	0.03	0.22	0.01	0.04	0.03	0.11	0.04	0.02	0.03
K	0.67	0.15	0.24	0.02	2.61	0.08	1.66	0.03	0.18	0.02	0.18	0.03	1.18	0.01
Ca	19.37	1.23	31.01	0.35	0.19	0.15	13.89	0.02	7.29	0.36	6.12	0.08	9.27	0.20
Ti	0.07	0.04	0.03	0.02	0.17	0.03	0.10	0.02	0.02	0.03	0.03	0.01	0.23	0.01
Mn	0.02	0.02	0.04	0.04	0.03	0.03	0.01	0.02	0.18	0.06	0.16	0.01	0.06	0.04
Fe	22.91	1.84	6.05	0.66	30.19	0.74	20.84	0.06	39.19	0.55	30.46	0.01	15.16	0.40

Table 3. Predicted elemental compositions (%) from multiple regression equations.

Element	Blank + CG1-A-47	Blank + Karrku	Blank + Bookartoo	CG1-A-47 Top Layer	CG1-A-47 Bot. Layer	Kutikina Top Layer	Kutikina Bot. Layer
Na	0.12	0.16	0.11	0.10	0.08	0.07	0.06
Mg	0.24	0.24	1.94	0.32	0.24	0.72	0.59
Al	0.39	4.25	0.70	0.75	0.64	2.57	2.21
Si	0.73	9.89	2.73	1.34	1.10	7.75	6.59
P	0.04	0.18	0.07	0.06	0.04	0.21	0.17
S	0.10	0.10	0.04	0.07	0.05	0.03	0.03
Cl	0.12	0.11	0.03	0.07	0.06	0.02	0.02
K	0.28	1.81	0.14	0.36	0.29	0.93	0.79
Ca	31.01	13.89	6.12	15.22	15.80	15.09	15.73
Ti	0.03	0.12	0.02	0.04	0.03	0.18	0.16
Mn	0.04	0.04	0.14	0.02	0.02	0.06	0.06
Fe	6.00	20.30	30.39	11.57	9.51	11.67	9.91
Ochre %	26.0	63.1	98.3	74.2	65.5	74.9	70.5
Correlation	1.000	0.999	1.000	0.901	0.910	0.990	0.988
F-statistic	28379	2228	10562	26	28	291	265

a ranking of fits, Pearson's correlation coefficients between the predicted values from the regression and the observed percentages are shown. As a further indicator of fit, the F-statistic for the regression is reported. It should be noted however that the statistical significance of the F test is not used. The correlations and F-statistics given in Table 3 for the multiple regression equations also indicate the fit of Kutikina ochre with the *in situ* pigment on the limestone slab (note the larger correlation and F-statistic values for the experimental coatings of Blanks + ochres CG1-A-47, Karrku and Bookartoo). The predicted compositions for the Kutikina Top and Bottom Layers have the ochre making up more than 70% of the analyses. This is unlikely given the thin appearance of the pigment layers and the actual ochre on the rock slab must have a higher Fe concentration.

Separate multiple regression analyses (Tables 1 and 3) of Limestone + Top and Limestone + Bottom Layers on two predictors, ochre CG1-A-47 and associated Blanks, give a poor fit (note the correlations and F-statistics) with large concentration discrepancies for Mg, Al, Si, P, S, K, and Ti. This indicates that ochre CG1-A-47 is not the ochre on the rock slab.

The analyses of the Limestone Slab + Top and Bottom Layers in Table 1 are similar and it can be concluded that the pigment on both sides is the same ochre. Furthermore, the similar predicted values in Table 3 for the Top and Bottom Layers of CG1-A-47 and Kutikina, respectively, also indicate that the pigment on both sides is a single ochre. (Multidimensional scaling of several analyses from the top and bottom layers was also closely grouped.)

A high concentration for phosphorus in the pigment is unusual for ochres as P is generally in concentrations less than 0.25% although it has been found up to 2.4% (Smith and Fankhauser 1996). Phosphorus concentration is low in the limestone, but its presence in the ochre layer could come from contamination by the surrounding sediment. Phosphorus levels are often high in archaeological sites (Bakkevig 1980, Bethal and Mate 1989). This problem was addressed by analysing the sediment from several depths in the site including the depth where the ochre slab was found. The percentage of P varied from a low of 0.28% at a depth of 62 cm to a high of 1.55% at 16 cm and a P concentration of 0.52% in the sediment surrounding the rock slab. We do not know if the phosphorus concentration in the pigment layer on the rock slab has been enhanced, but if an ochre is found which matches this pigment, the phosphorus level should be carefully considered.

SUMMARY OF THE PIGMENT ANALYSIS

It is possible to analyse an *in situ* pigment on a rock substrate and gain useful information. This study has shown that the composition of the red substance on the limestone slab is consistent with that of an ochre. The composition of this ochre is different from that of the ochre recovered from the same stratigraphic level. The top and bottom pigment layers of the limestone slab appear to be the same ochre.

EMERGING EVIDENCE FOR PLEISTOCENE 'ART' IN AUSTRALIA

To quote Beaton (1994:160) 'no rock art seems to be speaking more loudly these days than the rock art of Australia' and its message has far-reaching implications for modern human origins and behaviour. Unfortunately, several recent claims have been contentious and subsequently disproved. These instances threaten the credibility of those rock art dating endeavours which have a sound basis in archaeological science.

By far the oldest and most controversial of the claims for art were those made by Fullagar et al. (1996:771) for the Jinmium rock shelter in the Keep River area of the Northern Territory. Here pecked cupules were claimed to be > 50,000 BP on the basis of TL ages of the sediments overlying a slab of rock containing pecked cupules found in the deposit. Redating of the samples by OSL suggested that they were contaminated with older saprolite, thus producing dates significantly older than the true age of the sediments (Roberts et al. 1998). Independent AMS dating of comminuted charcoal from the sediments covering the slab supports this interpretation, producing dates in the order of 3,000 BP and suggesting that the entire deposit is of Holocene age (Roberts et al. 1998). The oxalate crusts overlying three pecked cupules on the shelter walls have recently been dated and have produced dates ranging between 1430 BP and 11,000 BP, with most of the 16 dates obtained being in the order of 2000 to 6000 BP. A mid-Holocene age is favoured for the beginning of crust formation over most of the cupules. As these dates provide only a minimum age for the cupules, they may be much older; but how much older is unknown (Watchman et al. 2000:7).

Petroglyphs located in the arid Olary region of central Australia were dated to 30,000 BP by measuring the cation-ratios in desert varnish formed over the surface of the motifs (Dorn et al. 1988). The methodology of this technique has been extensively criticised (Bednarik 1988; Harry 1995; see also Watchman, this volume). Dorn and Nobbs' (1992) replication of the cation-ratio results with AMS dates on organic matter trapped in the skins covering the petroglyphs was found during independent testing made on the crust samples submitted for dating, to contain contaminants of coal and wood fractions of greatly disparate ages (Beck et al. 1998:2135). Dorn (1997) has withdrawn the AMS C14 dates associated with the petroglyphs.

Several excellent reviews of direct dating of rock art in Australia have been published over the past 10 years (e.g. Rosenfeld 1993; Rosenfeld and Smith 1997) and Watchman (this volume) provides a regional overview of the background and results of rock art dating in Australia. The following discussion serves only to place the Carpenter's Gap 1 ochre-covered slab into a pan-Australian perspective.

One of the main criticisms directed at recent attempts to date rock art concerns the anthropogenic derivation of materials dated and the validity of the results as a record of the time of the painting or engraving event (Flood 1997; Rosenfeld 1993; Rosenfeld and Smith 1997). For example, carbon-containing components of the pigments used in rock art, and protein in the pigments have been dated using AMS (Loy 1994; Loy et al. 1990) but serious doubts have been raised as to the anthropogenic origin of the substances dated. Other scientists have been unable to replicate Loy's results on Laurie Creek or Judds Cavern (Nelson 1993; Gillespie 1997). This problem has been discussed by McDonald et al. (1990) who demonstrated that vastly disparate dates could be achieved from paired AMS samples from a single painted motif in the Sydney Basin region. The most likely cause of this inconsistency is 'contamination by organic carbon from sources other than that associated with the painting event...potential sources of contamination include naturally occurring organisms on the rock face, such as algae, lichen, fungi, bacteria', as well as charcoal bearing dusts and sources or inorganic carbons (Rosenfeld and Smith 1997:406) which can be incorporated in the paint surface and can potentially pre-date or post-date the painting event (Gillespie 1997:436; Rosenfeld and Smith 1997).

Watchman and Campbell (1996:418) have recently produced a series of AMS dates from a stratigraphic sequence of oxalate-rich crusts encasing pigments at Walkunder Arch Cave in North Queensland. This sequence spans ca. 28,000 years with no indications of chronological inversions. However, the fact that the dated pigments are rendered 'invisible' by a pentimento of later art and natural skins, leaves some room for doubt about the origin of the pigment and, if anthropogenic in origin, whether it is art. Lewis-Williams, leading rock art researcher from the University of Witwatersrand, was quoted in *Science* as saying 'It may only mean that someone wiped his or her hand on a wall after rubbing their body with ocher' and 'there's a great difference between that and making a symbol' (Morrel 1995:1909).

The weakness in all of the AMS dates on rock art lies in linking the carbon trapped in the pigments or mineral accretions with the human action being investigated. Dating organic materials in oxalate skins overlying engravings does not have the problems of image 'concealment' associated with pigment, but still relies on the assumption that the combined carbon will reliably date the time of the skins' formations. This problem can be overcome if a sufficient number of dates are obtained in a micro-stratigraphic sequence and if the results are replicated.

Roberts et al. (1997) have used the OSL dating technique to date mud-wasp nests overlying a faded human figure with headdress in a west Kimberley shelter. This figure belongs to the Bradshaw tradition, a figurative painting tradition which occurs throughout the Kimberley and is thought to be connected with the Dynamic paintings in Arnhem Land. The significance of these images is unknown to contemporary Aboriginal people (Flood 1997:292). The dates indicate a minimum age for this figure of ca. 17,000 BP (Roberts et al. 1997). Of significance to these results is the fact the Bradshaw style motifs overlie an entirely different stylistic painting tradition of Large Naturalistic Animals (Flood 1997:292). This has not yet been dated but must be earlier. The OSL dates on the mud nests overlying the Bradshaw figure have been achieved by examining small aliquot samples whereby any older contaminating silica grains would have been detected. They should therefore not be subject to the mixed age problem that plagued the Jinmium samples. However, AMS radiocarbon determinations reported by Watchman et al. (1997) from Bradshaw pigments have produced mid to late Holocene ages and it seems unlikely that this style prevailed for 15,000 years. While more dates are necessary, at this stage it seems the OSL date should be favoured as it does not have the problems of contamination with younger or older materials which might effect direct dating of the pigments.

The ochre 'crayons' from Malakunanja II and Nauwalabila I shelters in western Arnhem Land (Jones and Johnson 1985:219) are also contenders for art dating back to the earliest evidence for occupation of the continent. These crayons are dated by association with the sediments which have a luminescence age between 50,000 BP and 60,000 BP (Roberts et al. 1994). Jones' and Johnson's (1985:219) view that the ground iron ore pieces from Nauwalabila give 'proof of a Pleistocene antiquity for artistic activity in this region, and the strong presumption is that some of these high-grade ground iron ore 'crayons' were used to prepare paint for the rock art' has met with mixed response. Some critics have claimed that ground haematite pieces at Malakunanja II and Nauwalabila I do not in themselves constitute evidence for 'art', nor are they *prima facie* evidence for a symbolic referential system. Klein (in Morrel 1995:1909) notes that ochre fragments of a similar age have been found associated with 'many Neanderthal sites and sites of comparable age in Africa without evidence for art' and that utilitarian uses for the ochre are conceivable. Bednarik (1992:385), however, believes that the distinction of facets and striations on ochre pieces is a meaningful one, suggesting their use was not utilitarian.

Another criticism concerns the association of these finds and the sediments dated. In many sites in Australia and elsewhere where conjoining studies have been undertaken, vertical movement of cultural materials into lower levels has been demonstrated. Richardson (in Schultz 1995) raises the possibility that the haematite 'crayons' from Malakunanja II and Nauwalabila I may have been displaced downwards in the deposit in just the same way that artefacts have been demonstrated to move (Richardson 1992).

In short, direct dating of rock art has met with a number of problems and some inconsistent ages have resulted. The most secure dates for art are much as they were two decades ago and are provided by association with dated deposits. For example, engravings in the Early Man Shelter, North Queensland have a minimum age of 13,000 BP established by association with the dated deposits covering them (Rosenfeld et al. 1981:12). However direct dating studies on rock art are in their infancy in Australia. As the methods are refined and the results replicated, many of the inconsistent results which arise from problems of small sample size will disappear and a better understanding of changes in regional artistic traditions through time will emerge.

THE OCHRE-COVERED SLAB FROM CG1

'But is it art?' (Layton 1978:25) ... and does it matter?

There is no doubt that systematic use of ochre in Australian sites is as early as the earliest evidence for occupation. The Carpenter's Gap 1 find adds to this growing body of data. In view of the size of the slab and the fact that it was horizontally bedded in the deposit, the possibility of it being derived by vertical movement from higher in the deposit can be discounted. We now address the issue of whether finds such as the ochre-covered slab from Carpenter's Gap 1, constitute evidence for 'art'.

All the earliest Australian evidence for art falls into the non-iconic category. The earliest dated iconic art is the faded human figure in the Bradshaw style underlying the wasp nest dated to the last major phase of glacial aridity, 17,000 years ago. Layton (1978) draws the distinction between art which combines 'the symbolic and the aesthetic', and visual communication which merely signifies, but admits that neither the symbolic nor the aesthetic are universal, nor that they necessarily always occur in conjunction with each other.

Forge (1991) and Rosenfeld (1993:77) apply such a distinction to body and object stencils, prints and finger marking. Forge (1991:40) argues that stencils are not art in that 'they are not mediated through any symbolic system, they are not part of culture.' Rather, they are 'the equivalent of signing the visitor's book', telling us about an individual's presence and perhaps an individual's connectedness to place. Rosenfeld (1993:76) similarly queries whether finger flutings are 'qualitatively the same cultural phenomenon as other rock art'.

Stencils and finger flutings may thus be understood as marks that manifest a person's relationship to place in the gesture of their execution. Both gestural and referential graphic systems of marks are visual manifestations that carry meaning. However, the personalised mark mechanically executed in hand stencil or finger fluting stresses the individual relationship of person to marked place, while the use of stylistic graphic units marks a corporately encoded relationship to place. There are no *a priori* reasons why gestural systems, such as hand stencils or finger flutings should not operate in parallel with a referential graphic system (Rosenfeld 1993:77).

Others would argue that whether we wish to eschew the use of the word art and call it marking, imaging or any other thing, what we are interested in identifying in prehistory is evidence of a system for communicating these corporate cultural meanings and that the certain identification of this in prehistory requires the presence of iconics.

In the context of ethnographic and contemporary Aboriginal views, however, such distinctions are not meaningful. The ethnoarchaeological experience of one of us (S. O'C) in Western Australia and particularly the central Kimberley, indicates that while the stencils placed in shelters are certainly put there for the reasons given by Forge, to see them as only this is simplistic and inappropriate, given the framework within which they are made. Indeed stencils are as mediated through culture as any associated iconics *because* they exemplify the individuals connectedness to place, the Dreaming and the country. There is a fluidity and synergy in such concepts that does not sit comfortably with the dichotomy drawn by Forge (e.g. Mowaljarlai et al. 1988).

The Wandjina images discussed above illustrate this well. These are not iconic images of the creative ancestral figures, painted by people; they are the creative beings who placed themselves on the rock after the creative process was completed (Utemara and Vinnicombe 1992:25). Mowaljarlai (in Mowaljarlai et al. 1988:691) sums this up elegantly:

> We have never thought of our rock-paintings as 'Art'. To us they are IMAGES. IMAGES with ENERGIES that keep us ALIVE - EVERY PERSON, EVERYTHING WE STAND ON, ARE MADE FROM, EAT AND LIVE ON.
>
> Those IMAGES were put down for us by our Creator, Wandjina, so that we would know how to STAY ALIVE, make everything grow and CONTINUE what he gave to us in the first place. We should dance those images back into the ground in corroborees. That would make us learn the story, to put new life into those IMAGES.

Of course, the human origins of some paintings in some shelters are recognised by Aborigines (Watchman 1992:29), but it is doubtful whether they apply the concept of 'art' to such images. Jones (1990:29) captures the difficulty of labelling 'art' or 'not art' in his analysis of the multivalency and non-linear nature of the qualities of spiritual essence or *marr* and brilliance or *bir'yun* based on Morphy's (1989, 1992) work with the Marra-larr-mirri people of eastern Arnhem Land.

The concept of *marr* is similar to that of *bir'yun* - the sensation of shimmering light that could sometimes be created by an artist when he painted the cross-hatching designs, *rrarrk* on a bark sheet or a hollow log coffin (Morphy 1989). This *bir'yun* was seen to be an emanation of the inherent power of the design itself, which since it depicted ancestral beings or elements associated with them, was imbued with their essence. It is this that gave the design its shining and sometimes blinding quality. Such paintings were referred to as being *mali Wangarr* - literally 'shades of the ancestral world'.

Similarly some natural products also contained this shining essence (Morphy 1992:196). In such cases the pigment used does not simply represent certain qualities, it possesses its own power. This power is momentarily transferred through its application as well as through the application of the design. Blood, fat, beeswax, rainbow lorikeet and cockatoo feathers quintessentially had this quality (Morphy 1992:196). When 'applied to the bodies of key actors in the great ceremonies' they were said 'to imbue these men with the essential internal *marr* power appropriate to their proximity to the *Wangarr* forces' (Jones 1990:29). Similarly, the moiety songs of the Djan'kawu myth describe how the Ancestral Wawilak sisters' dillybags, made of brilliantly coloured lorikeet feathers, had this power. When discarded they changed to stone. Today these rocks on the coast near Milingimbi still radiate *bir'yun* and continue to unite the past and the present, the Ancestral and the Natural worlds (Morphy 1992:197).

Morphy (1987:21) explains that 'painting a design on a person enables him or her to be placed in direct contact with the Ancestral Past - the paintings themselves, as controllable manifestations of the Ancestors, providing people with the means of tapping the sources of the Ancestral power'. In this respect the use of ochre body decoration in ceremony, the use of ochre to cover the bones of the dead in secondary burial or to place a stencil or iconic on the wall of a shelter may equally be considered 'art' or 'not art'. The ochre crayons at Malakunanja II and Nauwalabila I are as likely to have functioned as part of such a system as any iconic. Whether any of the Australian archaeological evidence could be said to constitute 'art', is probably not a meaningful question in the context of the past, or even the present. In contemporary Aboriginal society the notion of 'art' had little meaning prior to the creation of the commercial art market (see, for example, Layton 1992).

CONCLUSION

When we factor in the effect of taphonomic processes on symbolic production of the Pleistocene (Bednarik 1994:69), it is hardly surprising that evidence for it has been slow in emerging. However, as it accumulates it is changing our perspective on the nature of Pleistocene occupation of Australia.

The ochre-covered rock at Carpenter's Gap 1 takes the evidence a significant step further. This is Australia's first find of a piece of painted rock buried in the Pleistocene, and is significantly older than the buried Pleistocene engravings in Early Man Shelter, for example. Here ochre has been deliberately applied to a rock surface which was probably part of the shelter wall or ceiling. Importantly, the association of the find with the date of ca. 40,000 BP is uncontestable. The research presented here has also demonstrated the potential for *in situ* analysis of ochre and shown that it can be separated from the background rock. This opens the way for future studies that will allow pigment from different art panels and superimposed motifs to be matched with ochre fragments in deposits. In cases where the ochre can be provenanced to particular quarries such studies also have the potential to identify changes in mobility, alliance networks and territoriality (see, for example, Smith et al. 1998).

Direct dating of rock art is an area of archaeological science which has significantly altered our perception of life in the Pleistocene in Australia. As yet few direct dates have been obtained for the Kimberley region and the dates we have are somewhat contradictory. Such problems can only be overcome by long term research projects firmly rooted in the methods of archaeological science. A good example is the Rock Art Dating Project of Watchman and Jones which aims to provide context for dates in that it also investigates the processes which lead to the preservation and loss of pigment over time, and in the case of the formation of oxalate-rich surface crusts over pigment and engravings, the past environmental conditions under which they formed (see Watchman 1990, this volume; Watchman et al. in press).

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Najombolmi's People: From Rock Painting to National Icon

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AUSTRALIA IS home to over 100,000 rock art sites — petroglyphs, paintings, stencils and, in the north, designs made from beeswax. A great diversity of 'styles', subject matter and ages can be found and in some parts of the country knowledgeable elders can inform us as to the meaning of recent rock art (Flood 1997; Layton 1992; McCarthy 1979). One of the last great rock art painters to have been prolific across a vast region was Najombolmi, a renowned painter from what is now Kakadu National Park (Haskovec and Sullivan 1989). By the time he passed away, in 1965, he had left elaborate and striking imagery of people, Ancestral Beings, fish and other animals at dozens of rockshelter sites. Because of his distinctive manner of depiction, Aboriginal elders and rock art experts alike can identify much of his work, with newly discovered Najombolmi sites continuing to come to our attention (Figures 1 and 2).

Since the establishment of Kakadu National Park in 1979, Najombolmi's work has been included on the 'must see' list of most Kakadu visitors, with the main gallery of Nourlangie the most accessible venue (Figures 3 and 4). More recently, rock art images from this and related sites have appeared on tea towels, T-shirts, jewellery, greeting cards (Figure 5), key-rings, cigarette lighters, coffee mugs, drink coolers, letterheads, postage stamps, paper money (Figure 6), stickers (Figure 7), buildings and a wide variety of mass-produced tourist souvenir items. As well, it has appeared in films, magazines and tourist literature around the world. Sometimes the rock art imagery has been reproduced by Aboriginal artists or their delegates, from the area where the rock art originals are located, and sometimes they have been reproduced by Aboriginal or non-Aboriginal persons or organisations, with permission; sometimes, however, the designs have been illegally appropriated. Najombolmi's paintings are, like all original art under Australian law, protected by copyright until 50 years after the painter's death. Anyone is free to make images evocative of Najombolmi's, but the images he painted himself are only lawfully reproduced if copyright permission to use them is granted. However, this has not been an issue, as the traditional owners of the Park are more concerned about the replication of imagery from other locations, and most users treat Najombolmi's paintings in a spirit of respect or admiration. Furthermore, his descendants have sanctioned visits to the Nourlangie sites, along with associated photography, promotion and reproduction. It is also likely that people reproducing Najombolmi's imagery have not been aware that a question of copyright exists.

Copyright is not, however, the concern of this paper, especially since the Kakadu Aboriginal community has sanctioned the public presentation and use of Najombolmi's Nourlangie designs. Instead, we explore the transformation of Najombolmi's creative expressions of individual and cultural experience into national icons or symbols that have meaning for Aboriginal and non-Aboriginal Australians alike. In a sense, the

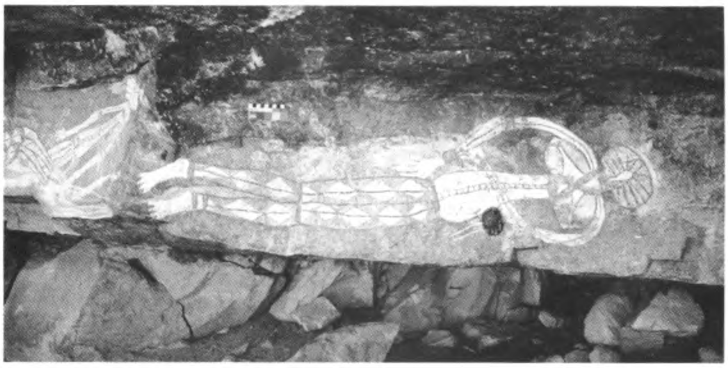

Figure 1. A number of previously undocumented paintings of horizontal women attributed to Najombolmi by the authors and Aboriginal elders were found at a site in Arnhem Land as recently as 1995.

Figure 2. An 'X-ray' painting of a woman next to a pig nose turtle was 'discovered' near the Arnhem Land/Kakadu border in 1992. They are similar to paintings at the Blue Paintings site, Nourlangie, and were made by either Najombolmi or one of his contemporaries.

Figure 3. The main gallery of Nourlangie is the largest composition attributed to Najombolmi.

Figure 4. Close-up of the main gallery's female figures which have been copied by or inspired numerous Indigenous and non-Indigenous artists.

Figure 5. One of many greeting cards with a Najombolmi-like female figure (Bu-ngarl/Totem Designs).

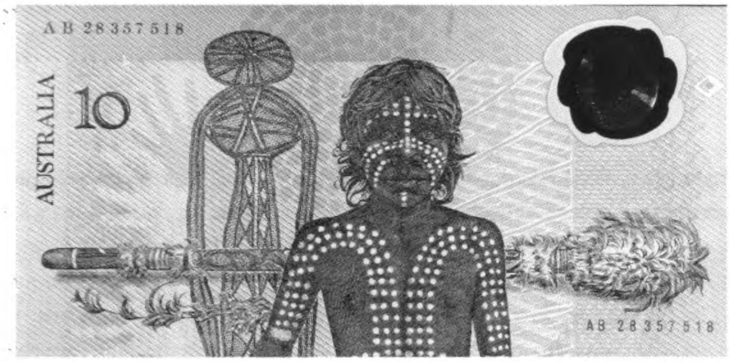

Figure 6. The bicentennial ten dollar note has a likeness of one of the main gallery females in the background.

Figure 7. Souvenir items with Najombolmi's people, such as this sticker, can be purchased throughout Australia.

adoption of Najombolmi's people and other rock art imagery is symptomatic of multicultural Australia's search for national symbols in place of those of the British Empire, and often with indigenous roots, with the centenary of Federation now reached. The widespread reproduction of images can trivialise and devalue, but also it can heighten power, potency and popularity, as Lewis-Williams (1995) has shown for South African rock art and Poikalainen (1995) has demonstrated for northeast Europe. In other words, the appropriation, reproduction and re-contextualisation of rock art imagery is not always clear-cut in terms of motivation, purpose or result, as Lewis-Williams (1995:322) points out: 'Certainly, each instance and context of appropriation must be considered separately; it is impossible to make sweeping generalisations, positive or negative.'

NAJOMBOLMI'S 'X-RAY' PEOPLE

Anbangbang, one of Australia's most famous rock art galleries, lies at the foot of Nourlangie Rock, in th heart of Kakadu National Park. Familiar to many through personal visits, scholarly reading or a bombardment of advertisements, this frieze of very recent paintings is now a tourist 'Mecca'. Hundreds of thousands of national and international travellers visit the site annually. For example, it is estimated that in 1990 over 238,00 people visited Kakadu, and of these 86% visited Nourlangie Rock (Sullivan 1995:82). But it was not always that way - the site's fame initially grew slowly, climaxing only in recent years after much promotion, publicity and study.

People camped and made rock paintings at Anbangbang and nearby sites for many thousands of years (Jones 1985; Taçon 1989b) but it was not 'discovered' by non-indigenous Australia until 1963, when buffalo shooters Alan Stewart (1969:53) and Fred Hunter stumbled upon the rock art, as well as burials and a 'swag' full of traditional material culture items, along with an old tobacco tin. The last frieze, of family groups, fish and powerful Ancestral Beings such as Namandjolk, Nabulwinjbulwinj and Namarrgon the Lightning Man with his wife Barrkinj, was painted soon after by Najombolmi, who was also known as 'Barramundi Charlie'. George Chaloupka (1982:22–5) recorded that:

> In 1964, only a year before his death, he camped for the last time in this shelter … . In his swag he carried ochres which he had collected on his travels. He took them out, prepared the pigments and painted the people back into the shelter. There are two family groups, men standing amidst their wives, some of whom he depicted with milk in their breast, as if he really wished them to be alive, to procreate and to people the land again. He built a platform and from this he painted the mythic beings … . This painted wall is unique, the last work of a great artist.

The site is dominated by the two family groups of men and their many wives that Najombolmi repainted. It is versions of these images that are most often seen elsewhere. By the usual conventions of 'X-ray' art, he emphasised some internal anatomical features of his people, such as backbones, adding painted body designs and other items of adornment. In keeping with 'X-ray' human figures painted by artists elsewhere in

Arnhem Land (e.g. Basedow 1928; Chaloupka 1993; Taçon 1993a:fig. 30), he added complex lozenge-shaped patterns on their legs, and sometimes on torsos. The designs are similar to body painting designs reported by Spencer (1914:151, fig. 47), said to represent a kangaroo backbone. These designs, rarely found on animal paintings, occasionally are found on paintings of fish - Najombolmi applied them to various depictions of Ancestral Beings as well. The lozenge pattern is common to X-ray paintings of women but in actual ritual use the comparable designs are worn only by men. The paradox is explained by the many myths recording how men's rituals originally belonged to ancestral women. These rituals were given to men or stolen by them so that now they are practised only by men.

Paintings of women with these body designs emphasise the power of the ancestral past as well as a direct connection to the present. The paintings also illustrate the transsexual nature of creativity, as do paintings and beliefs about Rainbow Serpents and many other Ancestral Beings that have both male and female characteristics (Taylor 1990; Taçon et al. 1996; West 1995).

Many of Najombolmi's paintings seem to reflect a desire for children, as a large number are concerned with fertility and aspects of childbirth. Several of his depictions of women, for instance, have areas with coloured dot infill where the uterus is located. These are said to represent the developing foetus. Other paintings of women have dot infill on the breasts; Aboriginal elders say this was Najombolmi's way of indicating that the women he painted were lactating (Taçon 1989b:160), but Chaloupka (1993:241) sees this as a more widespread convention used by a number of artists. A large 'X' features between the breasts of most of Najombolmi's women and a band also can be seen across their chests. This may reflect either body painting or clothing or both as it is similar to string breast girdles reported by Basedow (1907:43) and painted breast girdles illustrated by the Berndts (Berndt 1950:76; Berndt and Berndt 1951:Plates 14 and 20). Hodgson, who conducted an exhaustive review of Alligator Rivers region material culture, notes (1995:83) 'A thin skein of string was worn around a young girl's chest to indicate her first menstruation'. In this way the 'X' and chest band also denote fertility. Some of Najombolmi's women wear skirts but all have large, bared breasts, usually with one on each side of the body.

Najombolmi often painted pairs of women (possibly his two wives), sometimes shown as if reclining or sleeping (Figure 8), domestic scenes of men and women with characteristic items of material culture, men hunting and 'maternal scenes of a larger female figure lying above a smaller male figure with the arms of the female surrounding the male in the protective pose' (Haskovec and Sullivan 1989:69). Many of his human figures are linked with crossed arms, hold hands or reach out to each other. He also painted pictures of couples engaged in coitus and women in positions 'inviting' sexual intercourse. But Najombolmi never had children of his own, even though he had two wives at the same time: 'Although it was said of Najombolmi that he was possessive "like a policeman" never leaving the wives out of sight, they didn't have any children' (Chaloupka et al. 1985:176). Perhaps he was concerned with the fertility that was eluding him, expressing both his personal experience and that of his people, devastated by introduced sexual diseases, dispossession

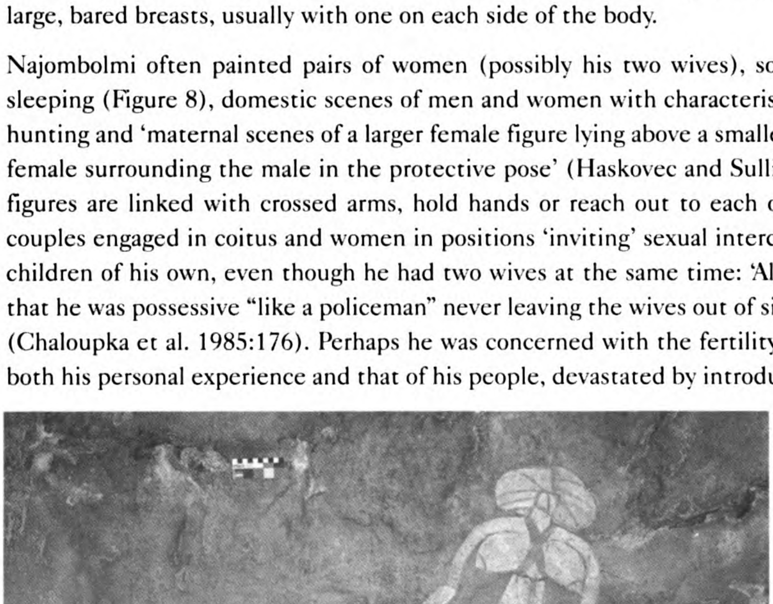

Figure 8. A reclining woman attributed to Najomblomi from the Arnhem Land site documented in 1995.

and malnutrition, through art. Whatever the case, his painted people soon caught the imagination of those that saw them, including Kakadu National Park archaeologists Hilary Sullivan and Ivan Haskovec.

In the mid-1980s Haskovec and Sullivan (1989) undertook the most comprehensive study of Najombolmi's art, resulting in a unique data base of rock art attributed to a known individual. They attributed 604 paintings from 46 sites, spread over an 1800 km^2 area, to Najombolmi and more precisely defined the characteristics of his art. They identified Najombolmi's paintings in three ways (1989:60):

> First, there were sites that could be directly identified by people who had actually witnessed Najombolmi doing the painting. Secondly, possible Najombolmi sites were identified by us and then people were taken to the sites to confirm the identification. Thirdly, people were shown photographs of sites which, for a variety of reasons, it was impossible to take them to. Obviously, these three methods of identification vary in the level of accuracy achieved, the first being the most accurate, the last the least (1989:60).

Among other things, Haskovec and Sullivan (1989:67) found that fish (36%) and people (32%) are Najombolmi's most common subjects, although Chaloupka (1993:241) disputes some of their attributions, noting at least two other artists who painted in the same manner at the same time. Fish and human figures are common in both the recent 'X-ray' and solid/stroke infill figurative rock art of Kakadu and Arnhem Land (Taçon 1987, 1989b) but Najombolmi appears to have been particularly concerned with depicting ***bininj bininj*** and ***daluk daluk***, the Aboriginal men and women of his country, with people depicted two to three times more often than the average for all recent artists (32% versus 12% for all 'X-ray' infill paintings and 23% for all solid/stroke infill art — see Taçon 1989b). As Chaloupka (1982:25) notes, after witnessing the impact of European culture on Aboriginal lifestyle, including death, dislocation and dispossession, Najombolmi 'painted the people back into the shelter'. Since then others have been so inspired by the paintings at Nourlangie Rock that they have continued 'painting the people back' but on other media.

INSPIRING IMAGES INSPIRING OTHERS

A variety of celebrated archaeologists and naturalists were so taken by the paintings of Anbangbang they soon began to publicise the gallery intensively. Their own growing fame also increased the profile of the site for non-Aboriginal people unable to visit what was once a remote location. The earliest widely-circulated photographs were published in 1963 by naturalist and documentary film producer David Attenborough who profiled the site in a film along with a companion book (Attenbrough 1963:opp. 48, 49; also see drawing on dust jacket). But the original paintings of people published by Attenborough were fading; soon after Attenborough's visit, Najombolmi re-painted the site.

Najombolmi's reinvigorated panel was first published by Edwards and Guerin (1969). When a National Geographic team visited in 1972 the site was growing in popularity: 'where tribesmen once met in solemn ritual, tourists now gather at Nourlangie Rock. Busloads of visitors from Darwin regularly reach the stone outcropping to view one of the most beautiful collections of rock art yet discovered' (Breeden and Breeden 1973:184).

Interest grew when the article in National Geographic duly appeared. In 1975 most of the Nourlangie panel appeared on the cover of a book dedicated to the preservation of Australia's Aboriginal heritage (Edwards 1975:cover, Plate 19). In 1977 a large photograph of the gallery was published in Fox et al. (1977:32). In 1979 the gallery was published again, commencing with a paper by Harry Allen (1979:277). One of the clearest and largest early colour reproductions of the Nourlangie frieze appeared on the cover of the fourth edition of Fred McCarthy's (1979) *Australian Aboriginal Rock Art*. It was reproduced from a photograph by Robert Edwards, part of a series from his own volume published the same year (Edwards 1979:68, 160). In 1982 George Chaloupka published a small book on Nourlangie Rock (Burrunguy) rock art with Najombolmi's paintings prominent on the inside cover and throughout the book (Chaloupka 1982:2—4, 8, 10–11, 13–14). As Chaloupka (1982:16) notes:

Although nearly all the men at times executed a rock painting, some men were considered to be more skilful than others and their paintings were much admired. One such artist was Najombolmi of the Badmardi clan who painted numerous designs not only in his own land but also in the territories of the adjoining clans. Of these paintings his large frieze of humans and spirit figures at Nourlangie Rock is the most famous.

Chaloupka (1982:25), who had visited many thousands of Australian and overseas rock art sites of varying ages, worried about the rapid deterioration of the gallery:

> Yet this nation, which at great expense buys works of other cultures and builds mausoleums to house them, ignores the genius of its own land. Unprotected, not only from natural weathering but also from buffaloes which rub their bodies against the painted wall and from the fingers of curious visitors who cause the fragile pigment to flake, the lower half of this magnificent frieze is nearly defaced.

This concern was taken up by the newly formed Kakadu National Park Board of Management and park ranger Danny Gillespie (1983; see esp. 72, 83, 117, 122 for photographs and photogrammetric plots). A conservation and management program was begun that would not only better preserve the paintings but also would lead to the site becoming one of the most visited rock art sites of Australia — a national gallery of World Heritage status.

The program began with an Australian National University research project led by Rhys Jones in 1981. The brief was to investigate the region's prehistory, with recommendations for the management and interpretation of key sites of scientific and tourist interest a priority. As a result, the gallery floor was partially excavated, along with several other sites nearby. With the publication of Jones' report in 1985 more attention was brought to the gallery; another photograph was published (Jones and Johnson 1985:70) together with a lengthy archaeological analysis of the deposits in it.

In June 1985, Paul Taçon (1989b) began PhD field research on the more recent rock painting tradition of western Arnhem Land, selecting Najombolmi's Nourlangie gallery as a starting point. Since the mid-1980s, dozens more photographs of this gallery's rock paintings have been published by Taçon and others (e.g. Balodis 1987:22; Berndt et al. 1992:19; Brockwell et al. 1995:33; Brody 1984:13; Breeden and Wright 1989:32, 37, 135; Chakravarty and Bednarik 1997; Cole 1981:front and back cover; Lambert 1989:cover; Parker 1997:2, 3; Pfeiffer 1982:iv; Taçon 1988b:8, 1989b:figs. 53, 61, 87, 96, 1989c:323, 1991:194, 1992:16, 1993b:115; Taçon and Cassis 1993:8; Rosenfeld 1985:52; Welch 1982:73; cover of the June 1998 issue of *The Australian's Review of Books*) while other authors have reproduced what are believed to be Najombolmi's people and animals from other sites (e.g. Chaloupka 1993; Haskovec and Sullivan 1989; Taçon 1988a:24, 1989a:fig. 10.9; Walsh 1988:251).

During the 1990s the trend to exhibit images of Najombolmi's people, Ancestral Beings and animals continued. Chaloupka devoted an entire oversized page to the frieze (1993:239) and remarks on its importance. Layton (1992:22) discussed Najombolmi's contribution and illustrated his depiction of Namarrgon, the Lightning Man (1992:81). Flood (1997:290) also acknowledged his contribution, having earlier published Namarrgon on the cover of another book (Flood 1990:cover, 96-97). Remarkably, the whole panel was faithfully reproduced by Kelvin Smibert for a semi-permanent world rock art exhibition at the Indira Gandi Rashtriya Manav Sangrahalaya (National Museum of Man), Bhopal, that opened in 1994 (see Bednarik 1994:140) - an exact replica half way across the world.

Najombolmi's paintings of people have featured in dozens of documentaries, television advertisements and feature films (eg. *Quigley Down Under* 1990). They can be found on the internet (e.g. Photovault has some for sale) and versions have been adopted as institution, state and national logos and symbols. When the Northern Territory was granted statehood in 1978, one of Najombolmi's women was chosen for the central shield of the Coat-of-arms (Figure 9). Later, another Nourlangie woman appeared on the 1988 bicentennial ten dollar Australian note (Figure 6). About the same time the Darwin-based Aboriginal Areas Protection

Authority, which protects Aboriginal interests in the Northern Territory's significant and sacred sites, adopted Najombolmi people as a logo, so that they regularly appear on letterheads and business cards. The Public Trustee for the Northern Territory, within the Northern Territory's Attorney-General's Department, uses one of Najombolmi's women holding a set of balanced scales as a logo. The Maningrida Progress Association, an organisation based in central Arnhem Land, also includes female figures based on Najombolmi women in its logo. For many years Imparja, an Aboriginal controlled television station based in central Australia, introduced movies with a collage of Aboriginal rock art, including stencils, a Wandjina and Najombolmi's Anbangbang people. Recently, the Australian Museum (Sydney) nominated a Najombolmi person to become a symbol for Aboriginal people in its multi-media interactive sites, developed in 1997 for the semi-permanent exhibition *Indigenous Australians: Australia's First Peoples*. Furthermore, the Museum's *Research Centre for Materials Conservation and the Built Environment* features a photograph of Najombolmi's people on its advertising flyer.

Figure 9. The Northern Territory armorial ensign includes a Najombolmi-like female figure as its centre-piece.

Also in the exhibition is one of the many forms of contemporary urban Aboriginal art inspired by Najombolmi, a 1991 painting *Dubai*, which means 'woman' in artist Rosalee Quinlan Jaluka's Dunghutti language (Figure 10). *Dubai*, in turn, was used in 1992 to introduce a book on Aboriginal women's issues, especially reproduction (Gosden 1992). Many other versions of Najombolmi people, by both Aboriginal and non-Aboriginal artists, have been exhibited and published (e.g. Davis-Hurst 1996; Monger 1996:10; George Chaloupka's 1972 oil on canvas interpretation of an 'X-ray' woman was used to introduce a new book on the archaeology of rock art - Taçon and Chipindale 1998:3). In the Kimberley region of Western Australia a pearl shell was incised by an unknown Aboriginal artist with copies of Najombolmi's Anbangbang people (Sotheby's 1994:Plate 7). Photographs of Najombolmi's designs or variants of them can also be found on tourist souvenirs, postcards, pamphlets, brochures, maps, guide books and all manner of Northern Territory tourist literature. It is almost as if Najombolmi's people begat children who since have had children of their own! And both they and their offspring are now recognisable to millions of people throughout the world. In this sense they have become both a national Australian icon and a symbol for and of the country's first peoples.

Figure 10. This 1991 acrylic-on-board painting by Dunghutti artist Rosalee Quinlan Jaluka was inspired by Najombolmi's Nourlangie paintings. Titled *Dubai* or 'woman' in the Dunghutti language, it is meant to honour the spiritual power of all Aboriginal women.

FROM INDIGENOUS ROOTS TO NATIONAL SYMBOLS AND ICONS

In recent years, the Australian nation has returned to its native fauna and Aboriginal roots in its search for national icons - things recognisably unique to the land and its people - and there is still much debate about what those icons should be. But kangaroos, emus and koalas, long identifiable as Australian faunal icons, were usurped by an echidna, a kookaburra and a platypus as mascots for the 2000 Olympics. The first Aboriginal-inspired Australian icon to be adopted by 'Europeans' was the boomerang. As Philip Jones (1996:4) notes 'in the European imagination, the boomerang has always expressed a unique quality associated with Australia as a land of paradox. This remarkable object, both a weapon and a toy, which can return to strike its thrower as easily as its target, seems of a kind with the duck-billed platypus, the egg-laying echidna, the bounding kangaroo, or plants which spring into life after bushfires.' In recent years, other Aboriginal objects, such as the drone pipe or didgeridoo, or designs, such as concentric circles, have been elevated to near icon status, along with the all-powerful Rainbow Serpent Ancestral Being and European-derived things such as Vegemite, bronzed Aussie lifeguards, brands of beer, and the barbecue, inevitably shortened in Australia to 'barbie'. Australians and tourists alike recognise these as both distinctly 'Australian' and of national cultural significance. These symbolise themes of the national psyche, what Panofsky (1972) might refer to as 'humanistic themes', not mere subjects. Often we do not consciously chose them as icons; they become icons by virtue of the power they attain through replication, transmission and adoption by an increasingly larger population. They acquire meaning beyond that which they signify, like symbols but larger - they are venerated, almost sacred, but also can be open to interpretation, even stereotyping, as Jones (1996:4) notes with boomerangs:

> Since their arrival in Australia in 1788, Europeans have tended to reduce the many forms, styles and names of Aboriginal boomerangs into a single, inaccurate stereotype. The same process has applied to Aboriginal people themselves, overlooking the great cultural differences which apply across the country.

In many ways Najombolmi's people have become stereotypes of the Aboriginal conception and representation of self, for both Aboriginal and non-Aboriginal Australians. Their common currency, uniqueness and recognisability makes them both icons of Aboriginality and of the Australian nation — the Aboriginal roots of a unique land of diverse people united in nationhood. The constant and consistent reproduction of Najombolmi's imagery could have been an exploitation or appropriation; rather it has been a happier story of respect and reconciliation - an attempt to come to terms with the continent's unique, Aboriginal past. Najombolmi's people, increased in potency through their reproduction, have power as icons and ambassadors for Australian culture: 'the use of interlocking arms and the depicting of human beings in a protective stance toward other human beings is particularly interesting, demonstrating the relationship between the people in the paintings and, at least subjectively, conveying strong emotional connotations' (Haskovec and Sullivan 1989:69).

In this sense, Najombolmi's people also are important icons of land rights: they are both of and owners of the land - the original people who called the place home, protecting both the land and each other. Symbols of the past, they stand also for the future. Exploring the painted shelters of Arnhem Land, especially in the high stone country where very few people go, we feel a thrill when we see an original Najombolmi painting. Each picture, ancient or recent, is the trace and the proof a person was in this place before; that thrill is more special when you know just who that human person was, as you do with the distinctive, personal and immediately recognised look of a Najombolmi figure.

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Nourlangie Cave at Night

Mark O'Connor

—Utter stillness of warm air among rocks
as if ancestors held their breath.

Each niche has held its dilly bag
of cracked thigh-bones, feared and untouchable
till the brutal four-wheel drives,

—and Barramundi Charlie with his Reckitt's Blue
painting his people back into the country
under the great faulted cliffs of Arnhem Land
where the line was held, and the farms stopped short.

From dry walls the Rainbow Serpent thunders:
without her lashing explosion
no life, no food, no worshipper.

The Lightning Man and Woman have no mouth,
—their speech is in the spark
that leaps from stricken stones.

Timeless,
primal Wallaby takes the spear in his lung,
drips blood and baby-making fat.

A horseshoe bat loops softly past my ears,
harmless unless to moths;
the vast bulk of stone a blackness
and comfort from to-morrow's sun.

A python, heat-seeker, lazily unwinds
from the blinding heat of day,
nosing out into the blood-warm night.
The crisp chill before dawn will turn it murderous.

Wargata Mina to Gunbilmurrung: The Direct Dating of Australian Rock Art

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UNTIL RECENTLY, Aboriginal rock paintings and engravings, generally called rock art, could not be directly dated, so that study of this art had no reliable chronological framework. With the superpositioning of different styles as the only measure of relative age, the integration of rock art and other archaeological evidence was also problematic. Artefacts and other materials in rockshelter deposits could only rarely be related to rock paintings and engravings on nearby walls.

Development of accelerator mass spectrometry for measuring small amounts of isotopes, particularly ^{14}C, enabled the dating of carbon-bearing substances directly associated with rock art (Litherland 1980). Whereas the optical dating of sediments has been known for fifteen years (Huntley et al. 1985) it has only been three years since luminescence dating was applied to quartz grains in mud wasp nests associated with rock paintings (Roberts et al. 1997).

Dating of rock art is therefore a new field of research and while many of the initial age determinations have changed the way in which archaeologists are viewing and using rock art, uncertainties and scepticism exist about the reliability of some age estimates. The problems relate to the use of a range of carbon-bearing substances, some of which may not be directly associated with the production of a painting or an engraving, but with natural processes of sedimentation instead. Whereas large scale processes of sedimentation in a rockshelter deposit may be well understood, the similar accumulation at a microscopic scale, of dust and salts over a painting or an engraving, is less well grasped. A further problem is that rock art researchers, for so long without a reliable chronology, have traditionally viewed some paintings and engravings as extremely old. Now, when direct dating indicates the contrary, young dates may be questioned or even rejected. Despite this, the existing archaeological evidence is gradually being re-evaluated in the light of the probable younger ages for some well known painting and engraving styles.

Only a handful of archaeological researchers are focussing on the challenging science of dating rock art, with Rhys Jones providing a pivotal role in accelerating research by generating innovative teams, disseminating results to the broad community and linking rock art data to other archaeological information. Jones has been at the forefront of rock art dating research, making one of his major goals the determination of the ages of major recognised Australian rock art styles, particularly the Bradshaw Figure paintings of the Kimberley and the Dynamic Figure paintings of the Arnhem Land area. As this review demonstrates, Rhys Jones continues to activate, catalyse and engage in rock art dating projects. Rhys was a team member that first dated blood in hand stencils in Wargata Mina in Tasmania (Fig.1), and he is currently on the team using single grain OSL for the dating of quartz grains in mud wasp nests associated with the Dynamic Figure style paintings at Gunbilmurrung and other sites in western Arnhem Land. In documenting the young history of rock art dating achievements in Australia this paper recognises and pays tribute to the considerable contributions made by Rhys Jones in this rapidly developing field.

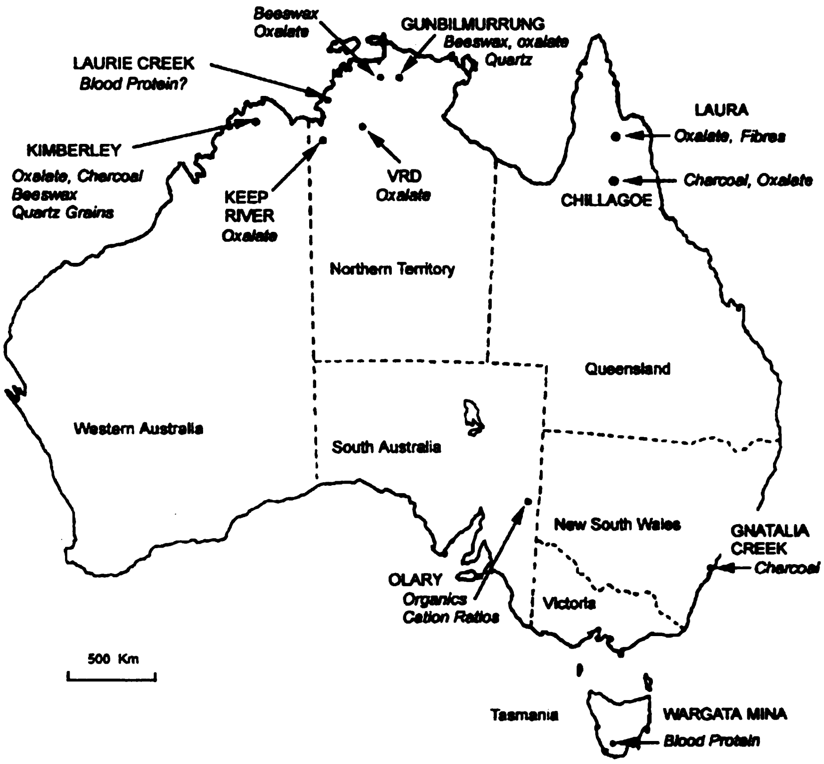

Figure 1. Map of Australia showing the locations and the materials used for the direct dating of rock paintings and engravings.

THE REGIONAL DATA

Tasmania

Wargata Mina, formerly Judds Cavern, is a limestone cave in the World Heritage area in southern Tasmania and in 1987, during an archaeological survey by Rhys Jones and colleagues, fifteen hand stencils were discovered (Jones et al. 1988). Subsequently Rhys formed a team to investigate the hand stencils in more detail. Following initial positive blood screening tests using Ames Hemastix, which indicated the presence of human blood protein in tiny samples of naturally exfoliating pigment, samples were collected for analysis and dating (Loy et al. 1990). Two ^{14}C measurements were obtained using AMS ^{14}C: 10,730±810 BP (RIDDL-1268) for paint from a hand stencil and 9240±820 BP (RIDDL-1269) from a smear of red paint. These results were described as preliminary because 'the taphonomy, diagenesis and stability of the proteins in blood pigment mixes in rock art settings' were not fully understood (Loy et al. 1990:114). While the research team was confident of the presence of some human blood protein in the samples dated they were not absolutely sure that only carbon from blood was dated (Nelson 1993). The measurement of a δ^{13}C value of -27.4 per mil indicated that carbon from the underlying limestone had not affected the age determination, but it did not provide unequivocal evidence that only blood protein was present in the dated sample. The subsequent dating of another site, Laurie Creek in the Northern Territory, by the same team raised concerns about the possible contamination of the samples by other carbon-bearing substances (Nelson 1993), probably associated with rock surface microorganic activities.

The hand stencil panel at Wargata Mina was found to support low numbers of microorganic colonies, including *Streptomycetes* spp., *Flavobacterium* sp., *Bacillus* sp., *Pseudomonas/Alcaligenes* sp., and *Acinetobacter/Moraxella* spp. (Brown and Line 1993:56). Unidentified filamentous and other fungi were also found in swabs taken from the painted surfaces. These micro-organisms are capable of producing a range of biomolecules, possibly including oxalic acid, and such compounds can also provide carbon for dating (see later). Notwithstanding the possibility of biomolecular contamination of this first blood protein dating at Wargata Mina the procedure provided 'an avenue for its [rock art] systematic dating by direct radiometric means' (Loy et al. 1990:115).

If the age estimates for the hand stencil and red smear at Wargata Mina are accurate, then they are discordant with substantial evidence of human occupation in the mountainous southwest of Tasmania where intensive occupation during the late Pleistocene had effectively ceased by 12,000 years ago (Allen 1995; Cosgrove et al. 1990). Occupation of caves along the river valleys to the north occurred much more recently (Harris et al. 1988), and the hand stencils could conform to a scenario of infrequent transitory visits to the cold wet caves. The age estimates may also be unreliable because of doubts about the identity of the extracted carbon and presence of young contaminating materials. Given the retraction by Nelson of his support for the dating of similar material at Laurie Creek (see later) the ages for Wargata Mina should be regarded warily.

Coastal New South Wales

A regionally important sandstone rockshelter on the New South Wales south coast, Gnatalia Creek, contains an assemblage of over 200 painted motifs or motif remnants, including a large black and red, curvilinear non-figurative drawing. The dark pigment in the drawing, identified as charcoal that was firmly bonded and silica coated, was collected for dating using AMS ^{14}C (McDonald et al. 1990). Two age estimates were obtained for samples from the large black and red motif; 6085±60 BP (AA-5850) and 29,795±420 BP (AA-5851). These conflicting results were attributed to contamination that emphasised the difficulties in attempting to date small samples from rock art panels. An additional problem was that the sample used for dating was not cross-sectioned to examine the association of paint and charcoal with the silica which bonded the paint to the rock. An adjacent piece of painted rock was sectioned and this gave no indication of dust or other likely contamination.

Unbeknown to these researchers micro-organisms, mainly bacteria, fungi and algae, are usually trapped within amorphous silica films that are formed by the precipitation of polymerised silicic acid from seepage water. Physical remains of biological materials and fatty acids, as well as small particles of charcoal that are naturally transported in surface water are bound within amorphous silica coatings (Watchman 1992a, 1994). Surface and basal layers of fossilised micro-organisms in natural coatings not associated with Aboriginal paintings at the same site were found to range in age from 825 BP to 11,235 BP (Watchman 1994). The potential use for dating these natural carbon-bearing fossils in silica covered rock paintings and engravings has been investigated and shown to be worthy of further study (Watchman 1996).

The inconsistent age estimates of two samples from one motif at Gnatalia Creek highlights the needs for careful sample selection and rigorous laboratory examination to identify the nature of the materials used for dating even when the readily dated material, charcoal, is apparently the only carbon-bearing substance present. Presuming that only one source of carbon exists in a sample may lead to an erroneous age estimate and so the pre-treatment steps should be preceded by chemical and mineralogical analyses. A mid-Holocene age may be more likely for the Gnatalia Creek painting because it is only covered by a thin amorphous silica film compared with the much thicker 11,000 year old natural deposit nearby.

Olary, South Australia

Dating results from the Karolta site, near Olary, have been controversial because of concerns about the reliability of rock varnish dating methods. Manganese-rich rock varnishes that have formed on natural exposed pavements and over engraved tracks, circles and abstract designs have been dated using cation-ratios (Nobbs and Dorn 1988, 1993; Watchman 1992b) and AMS ^{14}C methods (Dorn and Nobbs 1992; Watchman 1993a). When the cation-ratio dating method was initially published it attracted great attention because it seemed a unique, non-destructive way to date Aboriginal engravings. On the initiative of Rhys Jones, who was on the scientific committee of the Australian Institute of Aboriginal and Torres Strait Islander Studies at the time, Dr Ron Dorn (who developed the cation-ratio dating method), was brought to Australia to describe his method. Subsequently the Institute, prompted by Rhys, provided research funds to validate the cation-ratio dating method (Watchman 1992b).

Cation-ratio dating provided sensational ancient age estimates for varnished rock engravings near Olary in South Australia (Nobbs and Dorn 1988), but the apparent brilliance of the procedure gradually faded as methodological, technical and practical problems were identified (Bednarik 1988; Clegg 1988; Reneau and Harrington 1988; Clarke 1989; Watchman 1989, 1992b). Following some seemingly supportive AMS ^{14}C age determinations (Dorn and Nobbs 1992; Nobbs and Dorn 1993) for petroglyphs at Olary the original cation-ratio age determinations were withdrawn (Dorn 1996). Uncertainty about the veracity of the ages of petroglyphs in the Olary area was heightened with the revelations of Beck et al. (1998, but also see Dorn

1998). The finding of coal and charcoal together in sub-samples of varnishes submitted for dating has meant that at best the age determinations are the weighted means of the two components whose individual ages are widely disparate. The debate about possible tampering and manipulation of rock varnish samples, including those from the Olary province has created great uncertainty about the age range for the engravings there, and the published rock varnish dating results are considered highly unreliable (Watchman 2000a).

Chillagoe, Queensland

Charcoal was scraped from some Aboriginal rock drawings in the Chillagoe area of Queensland and subjected to plasma oxidation (Russ et al. 1990) to produce carbon dioxide in a preliminary step before undergoing AMS ^{14}C dating. David et al. (1999) were able to measure ages ranging from modern to 3350±350 BP (OZB-783). The charcoal drawings included infilled geometric designs, concentric ovals, elongate net-like structures, irregular and sub-parallel lines, concentric arcs and star shaped radiating lines (Armitage et al. 1998). Potential problems with the dating of charcoal are that the charcoal could either have been lying on the ground for several hundred years or come from the burning of wood from slow growing trees. More research is needed into the ages of dead standing trees and surface charcoal.

Oxalate minerals have also been used to date rock engravings and paintings in the Chillagoe area. One crust was systematically excavated at Walkunder Arch and the resulting ten samples containing oxalate minerals were dated within the range 29,700±500 BP to 3340±60 BP (Watchman and Campbell 1996). The earliest evidence of painting in that crust was found at 28,100±400 BP (OZA-391). On another surface in the same shelter, an engraved 'star burst', 'asterisk' or radiating line design had been painted with red paint before being naturally covered with oxalate-rich dust and salts. The basal oxalate-rich layer above the painted engraving was dated at 7085±135 BP (Watchman and Hatte 1996). These results, and others from Laura, Kimberley and western Arnhem Land, are age determinations that use carbon in oxalate salts to indicate the time when photophobic micro-organisms colonised regularly moist and dusty rock surfaces. A research project, initiated by Rhys Jones and funded by the Australian Institute of Aboriginal and Torres Strait Islander Studies, aiming at confirming this proposed mechanism for oxalate formation in northern Australia is currently underway (Watchman and Jones 1998), with imminent publication of the results from several sites.

Laura, Queensland

Persistent research of the remarkable body of rock paintings and engravings on Cape York Peninsula (Flood and Horsfall 1986; Cole 1988, 1999; Cole and Watchman 1992; Cole et al. 1995) has realised only a few occasions where reliable ^{14}C age determinations could be obtained. Plant fibres found in coarse grained white paint at the Yam Camp site, Laura, were dated at 725±115 BP (AA-7679) and 730±75 BP (AA-7719, Watchman and Cole 1993). The two age determinations are considered reliable measurements because the fibres are presumably from a juicy orchid tuber, alive just prior to the time of painting, which was crushed and mixed with the coarse white paint. The two age estimates are for female human figures that represent some of the most recent paintings in the region. They overlie heavily painted and engraved motifs and confirm a long history of rock marking production.

Paintings and engravings associated with oxalate have also been dated in the Laura region. The oldest rock painting lies buried in an encrusted shelter wall deposit; its age is 24,600±220 BP (NZA-2570, Watchman 1993b). The ancient date for this buried rock painting corresponds with evidence of occupation at that site extending from 30,000 BP (Morwood and Hobbs 1995).

Similarly, the carbon in oxalate-rich encrustations has allowed for the determination of a range of minimum ages for coatings covering engraved tracks, radiating forms and abstract friezes. A thinly coated bird track motif at the Kennedy River complex was dated at 1210±245 BP (AA-9221), a radiating form at the

Quinkans B6 shelter at 2850±115 BP (AA-9222, Cole et al. 1995) and a rectilinear abstract design at Sandy Creek 1 at 9160±70 BP (OZA-405). These age determinations have shown that some paintings that were made more than 3000 years ago are no longer visible on the surface, but can only be seen as remnant red and yellow layers in cross-sectional studies of rock surface coatings (Watchman 1992c). Other examples of buried paint layers reflecting periodic painting can be seen in the Northern Territory.

The Victoria River District (VRD), Northern Territory

The time consuming and highly specialised micro-excavation technique that was used in the Chillagoe and Laura regions for progressively removing layers of oxalate-bearing encrustations from rock surfaces was improved with the development of the focussed laser extraction method in Canada (FLECS-AMS, Watchman 1993c). The FLECS-AMS method was refined further at James Cook University and used to extract carbon from layered crusts at Yiwarlarlay, as part of a dating project in the VRD (Watchman, David et al. 2000). The base layer of laminated oxalate and fine charcoal dust in an encrustation covering an engraved macropod track at Yiwarlarlay was dated after extracting carbon using the laser system (3160±60 BP, OZD-454, Watchman, David et al. 2000). The conventional acidified permanganate method of extracting carbon from an oxalate salt (Gillespie 1997) was used to extract carbon to date the base layer of crust adjacent to the engraving (off-art, 4080±50 BP, CAMS-45675). Twenty five paint layers were recognised in four cross-sections of painted encrustations at that site indicating that the tradition of rock surface painting was well established from at least mid-Holocene times.

Questions have been asked about the relative merits of the three carbon extraction methods from oxalate salts: combustion, laser decomposition and acidified permanganate oxidation. Combustion (heating to 1000°C in the presence of oxygen, usually with cupric oxide) is used to oxidise all carbon-bearing substances, including oxalate salts, in powders obtained by micro-excavating rock surface coatings. The assumption in this approach is that all the carbon-bearing components in the crusts, for example oxalate salts, micro-organic substances, flecks of charcoal, insect remains and small pieces of leaf and plant litter are contemporary. The focussed low energy of the laser not only decomposes oxalate but also induces the oxidation of all volatile carbon-bearing substances within a cross-section of a laminated crust. The difference between these two methods is that for the laser a section is made in the laboratory from a partly detached flake of rock crust collected in the field, whereas powder is obtained by using a portable engraving tool when a flake cannot be removed. The rapid, low temperature, acidified permanganate method ensures that only the oxalate minerals in a powdered sample are oxidised to yield an age estimate for when the oxalate formed. Recently completed research has shown that the three extraction methods provide comparable results (Watchman 2000b).

Kimberley, Western Australia

While visiting Sue O'Connor's excavation of the floor deposits at Carpenters Gap 1 in the southern Kimberley of Western Australia, Rhys Jones observed heavily encrusted boulders within the dripline and similar ones that were intersected within the trench. His initiative in taking samples of the encrusted boulders opened the way for a detailed collaborative investigation of oxalate crust formation. The thick oxalate-rich crusts formed on the boulders are providing information about changes in the nature and extent of processes leading to crust formation across the shelter space and through time (Watchman et al. in press). This work is also shedding light on the physical evolution, microclimatic changes and human use of this rockshelter; information which will assist in dating circular depressions and engraved bird track and other motifs at the site.

Whereas thick oxalate-bearing crusts have formed in some rockshelters in the southern Kimberley (and in northern Australia generally), the northern Kimberley region is characterised by thin coatings associated with red and purple (mulberry) rock paintings. Typically the rock painting style of Gwion paintings, known as Bradshaw Figures (Walsh 1994), is on the relatively unweathered ceilings and back walls of elevated rockshelters containing shallow Holocene floor deposits. Three thin oxalate-rich accretions associated with rock paintings have been dated in the Kimberley (Watchman 1997). A minimum age of 3140±350 BP (OZB-125, Watchman et al. 1997:25) indicates that the Irregular Infill Animal painting is about the same age as the Bradshaw Figure painting (dated at 3880±110 BP, OZB-126). The dated paint layers in both these figures were associated with a thin surface salt deposit containing oxalate carbon and it was impossible to determine whether the paint had been applied to the accretion or the accretion had formed after the paint had been applied to bare rock.

The age estimates for intimately associated oxalate and paint are substantiated by an inferred age based on fossilised micro-organisms in a thin siliceous coating (Watchman 1997). Unfortunately the carbon in a layer of algal matter (the material that was dated) occurred in a lens lying above the silica coated paint. The Bradshaw painting had been applied to a rock surface 'laminate' that had been covered by a film of amorphous silica up to 0.3 mm thick. The 'backing board' for the paint, consisting of white amorphous silica contained only extremely small amounts of carbon-bearing substances and could not be used to determine a maximum age for the painting. However, the near surface lenses making up a discontinuous layer of carbon-rich silica above the paint gave ages of 1430±180 BP (OZB-351) and 1490±50 BP (CAMS-16755). Taking the total silica thickness into consideration and calculating a rate of silica deposition it is possible to estimate an age for the painting of approximately 3000 years. This computed age for the Bradshaw painting, considering the thickness of the silica layer above the paint, is slightly younger than the age determined for the oxalate-coated paintings. This indicates that the Bradshaw Figure and Irregular Infill Animal styles, as described by Walsh (1994), are probably mid-Holocene or slightly younger in age.

These minimum ages gave the first insight into the probable age of these exquisitely painted figures. At the same time as these results were released Rhys Jones, Richard Roberts, Mike Morwood and Grahame Walsh were sampling mud wasp nests associated with similar rock paintings in the Kimberley. The oxalate and silica dates are controversial because they are inconsistent with the observed relative ages of the Irregular Infill Animal and the Bradshaw Figure styles (Walsh 1994), and with a much older luminescent date for an anthropomorphic figure with Bradshaw-like attributes. The optical dating of quartz sand grains cemented in the core of a mud wasp nest gave an OSL age of 17,500±1800 BP (Roberts et al. 1997). The remnant stub of the nest, fixed to the sandstone back wall of a rockshelter near the King Edward River Crossing, was over a weathered purple paint layer and therefore gave a minimum age for the painting. Another debate has arisen in the dating of the Kimberley painting by OSL because the painting is so weathered and superimposed by later figures that the typical Bradshaw Figure attributes, thought to be present by some team members, can also be regarded as indiscernible and problematic. The fundamental question is whether the faded purple painting is actually a Bradshaw Figure or another, as yet unknown, category of presumably older paintings.

Archaeological evidence of young occupations from the shallow excavations at rockshelters where Bradshaw Figure style paintings persist, together with the presence of thin oxalate and other encrustations, and the angular and relatively unweathered walls and fallen slabs provide supporting evidence for recent rather than ancient Bradshaw paintings. A mid-Holocene antiquity for the Bradshaw Figure style of paintings is therefore likely to be demonstrated by future work that involves using different dating methods and collecting duplicate samples from the same figures.

Much better knowledge of the age of Wandjina and Argula Figures is obtained because many more samples have been dated. Scrapings of charcoal from the black eye of a Wandjina Figure gave a modern age whereas a small burnt stem from an Argula Figure has an age of 430±130 BP (Watchman 1997). The basal layer of charcoal under 35 distinct black and white paint laminations from a Rainbow Serpent figure at a site west of the Drysdale River homestead was dated to 300±85 BP (OZC-549). The sequence of paint layers making up this Wandjina style painting indicates intensive repainting during the last several hundred years.

Beeswax resin stick figures in the northern Kimberley region of Western Australia have been dated using the acid insoluble components and AMS ^{14}C (Watchman 1997). Eight anthropomorphic figures made with beeswax resin range in age from 1510±80 BP to 550±100 BP. One figure in the design of a macropod track was dated at 1680±50 BP. Unpublished results on other beeswax figure samples obtained by Walsh and Morwood (pers. comm.) indicate that in the Kimberley the use of beeswax for making rock art extends through the Wandjina period, from ca. 4,000 years ago. These results parallel those obtained for beeswax designs in the Northern Territory (for example at Gunbilmurrung, see later) indicating widespread use of that medium for rock painting beginning in the mid-Holocene and extending to the recent past.

Keep River, Northern Territory

The thinly encrusted and shallow cupules on the sloping wall of the Jinmium rockshelter contain the oxalate mineral whewellite and a series of age estimates for formation of the coating has been obtained. The pooled mean age for carbon in oxalate from the base of the crust is 2240 BP (Figure 2 Watchman, Taçon et al. 2000). This estimate of the minimum age for cupule production agrees with the OSL age for sediment that covered a spalled rock fragment bearing weathered cupule marks (Roberts et al. 1998). Other rockshelters in the same region with encrusted oxalate-rich surfaces gave minimum ages for circular depressions that are similar to the measurement at Jinmium; 2280±50 BP at the KR24 site (CAMS-42879) and 4320 BP at Granilpi (Watchman, Taçon et al. 2000). Fieldwork done in June 2000 included the re-sampling of encrustations in the area to identify carbon-bearing substances other than oxalate in the crusts. This was necessary because preliminary gas chromatographic analyses had indicated that at least one large siliceous biomolecule as well as fatty acids are also present, and these may interfere with age determinations. Whether these molecules are related to the production of oxalate minerals by micro-organisms that had been living in the shelters is yet to be determined, but it seems highly likely.

Laurie Creek, Northern Territory

Proterozoic sandstone and siltstone shelters in the Wingate Mountains of the Northern Territory provided the backdrop for Wagiman and Nanggiwumerri paintings and engravings. One rockshelter and panel of faded paintings was selected by the same team that had dated the Wargata Mina site in Tasmania and blood screening tests were undertaken. The dating project, initiated by Rhys Jones, was only made possible after extensive and delicate negotiations between Betty Meehan and the traditional owners. After blood protein was detected in three samples from 'weathered dark red pigment, in many places capped by a thin silicate skin' (Loy et al. 1990:114), one sample was ^{14}C dated. The measured age of 20,320 +3100/-2300 BP (RIDDL-1270) was a momentous and controversial achievement. As the first Pleistocene date for rock art in Australia it not only showed that Aboriginal people had been painting on rocks for many thousands of years, but the new dating approach created opportunities for establishing rock art chronologies. However, a troubling question arose; how could blood protein remain stable on a rock face for so long? Are there other organic substances on rock surfaces that could affect the ^{14}C dating of blood protein?

After a critical re-examination of samples collected and analysed from the Laurie Creek site, Erle Nelson, one of the ^{14}C dating specialists on the original dating team, concluded that 'we no longer have a demonstrable association linking the material dated with human actions. There is no figurative representation on the surface that provides direct indication of human activity, and the layered structure of the 'skin' could be due to natural processes' (Nelson 1993:894). The original indication was that blood protein was present in the paint smeared rock surface coating and as it was assumed that because only high molecular weight material was selected for dating the measured age was most probably from proteinaceous carbon. However, the nitrogen and carbon concentrations were atypical of protein; C/N ratios exceeded 36, compared with 3 in protein. Repeated stable carbon isotope values of –21.8 per mil and –22.6 per mil were very similar to the original measurement (–22.3 per mil). The intriguing situation led Nelson to question the nature of the substance dated and he therefore withdrew his support for the original dating result.

The taphonomy of blood protein remains today an unexplored domain in rock art. Following the ambiguous result at Laurie Creek, involving blood and possibly other substances, a collaborative research project was established to find out more about the nature of carbon-bearing substances on rock art surfaces (Watchman and Jones 1998). The preliminary chromatographic discovery of large siliceous biomolecules in rock surface crusts from northern Australia indicates that the original dating of blood protein was probably unreliable. Until the research and dating are independently replicated the date from Laurie Creek is not secure.

This problem and the use for dating of carbon-bearing substances not found directly in paint, concerns archaeologists because numbers, reported as dates, are being produced for certain rock art styles without any apparently strong archaeological association between the material dated and the art. The archaeometrist aims to provide the best age determination from material that can be sampled with minimal impact to a painting and from close proximity to the paint, both spatially and chronologically. The archaeometrist may only be able to provide minimum or maximum age constraints from the material sampled and dated, leaving the archaeologists to interpret the results in the light of their detailed examination of motif characteristics, classification of style, superimposition sequence, relative chronology and other site and regional archaeological information. Controversial rock art dating situations have arisen either because there is a wide disparity in age estimates between preconceived notions of age based on speculative estimates and the physical measurements, or where on-site research has been unable to define categorically the archaeological association between the artefact and the datable material. Greater field and laboratory collaborations between archaeologist and archaeometrist are therefore essential if reliable age estimates for rock art styles are to be achieved.

Kakadu National Park, Northern Territory

Oxalate minerals associated with rock paintings were first identified on rock art panels in Kakadu National Park in 1987 (Watchman 1987), providing the first real opportunity to date rock paintings by direct physical means in Australia. Although the age estimates obtained by ^{14}C for the crystallisation of oxalate minerals in Kakadu National Park initially did not yield precise ages for any particular set of paintings, they demonstrate an innovative approach able to date previously undatable Aboriginal rock art (Watchman 1990, 1991). Extensive archaeological work in the Park has provided a substantial framework against which the dating of rock art can be compared (Jones 1985).

Two dates for oxalate carbon were obtained from a thick crust at Spirit Cave, Angbangbang at the site excavated in 1981. Evidence of haematite painting was observed at a depth of 2 mm in the laminated crust beneath the rock paintings and oxalate immediately above this layer gave an age of 12,250±105 BP (AA-9223, Watchman and Campbell 1996). Underlying the paint is a crust containing only small amounts of dateable materials, too low in concentration for dating. Traces of other paintings were also observed above that layer with the most recent buried red paint layer occurring at slightly younger than 3470±120 BP (AA-9224). The age determinations from the rock surface crusts all pre-date the charcoal obtained from the junction between the grey sandy sediment and the gravel base of the deposit (Jones and Johnston 1985:68). These rock art age determinations, like those from Walkunder Arch, clearly show that the marking of rockshelter walls often has greater antiquity than evidence in floor deposits of human occupation and that, in many cases, the adornment of rocks may have taken place at locations not normally used for habitation. In other cases, for example at Sandy Creek, Laura, and in the northern Kimberley the antiquity of both ancient and recent paintings matches excavated evidence of stone tool use and occupation. Each site must therefore be considered on its own merits when comparing painted and engraved surfaces with the discarded remnants of human occupation in the sediments below.

Oxalate-bearing encrustations covering engravings at two sites near Coronation Hill in the southern part of Kakadu National Park have been dated. The site had been previously documented during an archaeological survey of the Kakadu Conservation Zone in 1990 (Jones and Brockwell 1990). At the engraved boulder site, thin dark coatings cover the upper surface of a rounded boulder which contains more than 100 engraved animal and human tracks. The basal layer of oxalate from one of the engraved macropod tracks gave a preliminary age of ca. 7000 years (Watchman personal data) whereas at the other site the basal oxalate in an engraved 'rake' motif gave an age almost half that of the first engraving. As these preliminary determinations only provide a glimpse of the possible ages of engraved motifs on rocks in this area systematic dating work, using oxalate and other materials, is being undertaken to determine the chronological sequence of painted and engraved rock art.

Aboriginal people pressed native beeswax into pellets and thin strips and applied them to rock surfaces as rows of dots and as animal and human designs. The complex chemistry of the mixture of resins, gums, saps and waxes making up beeswax (Fig. 2) all contain carbon which can be ^{14}C dated. In western Arnhem Land the acid insoluble fractions of two non-figurative designs were dated by a team led by Erle Nelson; 1100±120 BP (CAMS-2280) and 1030±70 BP (CAMS-2298, Nelson et al. 1994). At Gunbilmurrung, a large rockshelter high up on the edge of a steep valley on the tributary of the East Alligator River in Arnhem Land, two turtle-like designs made of beeswax had been applied on a protected low overhang. These were dated at 4040±80 BP (CAMS-2300) and 3820±70 BP (CAMS-2301). Beeswax dots forming a line were less than 200 years old. These results confirm the general categorisation of this form of painting in the 'Estuarine' phase (Chaloupka 1984). The two old dates from the beeswax turtles appear anomalously old because most other beeswax figures are less than 1300 years old (Nelson et al. 1994). Independent sampling and dating of the beeswax from these figures is the only way to verify that the original results are reliable. Therefore, during an expedition led by Rhys Jones in 1997 aimed primarily at dating the Dynamic Figure style of rock paintings in western Arnhem Land, additional samples were collected from the older beeswax figure in order to validate its apparently anomalous age. This independent age determination of beeswax from the older turtle figure has confirmed the ancient age (Watchman and Jones in prep.) giving considerable reliability to age estimates for painted figures with which the beeswax figure is associated.

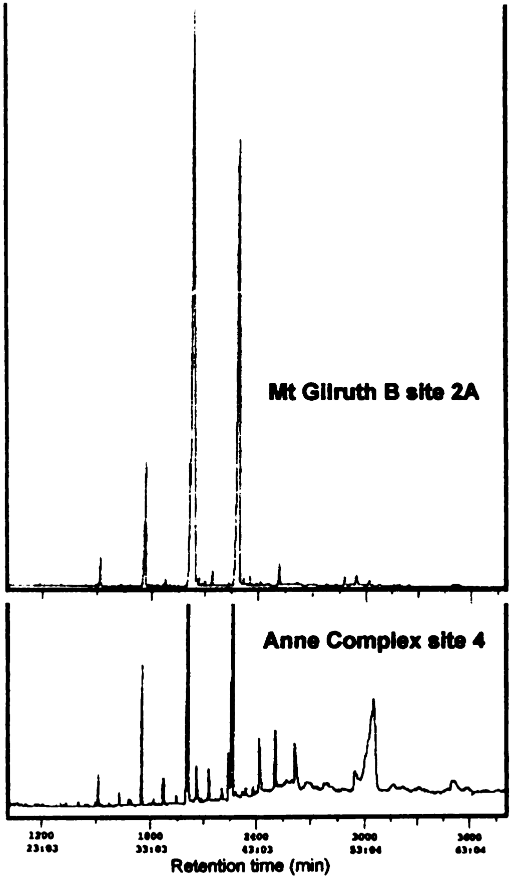

Figure 2. Spectra obtained using gas chromatography illustrating the similarity between the major volatile components in beeswax resins from the Mt Gilruth (western Arnhem Land) and Anne Complex (northern Kimberley) sites. Samples were dissolved in hexane and analysed using a Finnigan Mat GCQ gas chromatograph mass spectrometer.

Gunbilmurrung also provides a great opportunity to date mud wasp nests associated with the Dynamic Figure style of rock paintings (Chaloupka 1984). Extensive panels of finely painted anthropomorphic figures lie over and under hard, cemented mud wasp nests. George Chaloupka rediscovered the site and invited Rhys Jones, Richard Roberts and the author to examine the possibility of dating the paintings. One painted figure is also partly covered by a cream coloured oxalate-rich coating and a preliminary ^{14}C date from the layer immediately beneath the red paint indicates considerable antiquity for this rock painting style. Work is in progress to date individual quartz grains in nests associated with this and other similar figures to determine the likely timeframe when this style of paintings was applied.

CONCLUSIONS

The last decade has been a pioneering period in archaeology in relation to the direct dating of rock art. Major innovations and the use of oxalate salts for ^{14}C and quartz grains for OSL dating have increased perceptions of rock painting and engraving chronologies. As with all pioneering ventures there are some controversies as well as outstanding successes, but the way forward has been set to make more improvements and other innovations. The problematic dating results of rock varnish dating and disparities between analyses using different methods are targets for greater research effort.

The reliability of rock art dating results, like all archaeological age determinations, not only depends on a sound dating method but also on the selection and use of appropriate materials related to the event under examination. It is inevitable in the young and rapidly developing science of rock art dating that some failures will occur, but by reflecting on the sample selection, analysis and material identification only an improved dating process can result. This paper has attempted to reveal the reasons why some age determinations for Australian rock art are problematic whereas other results are reliable (Table 1). Acidified permanganate oxidation of micro-excavated powders containing oxalate provide a reliable means for establishing maximum and minimum ages for 'backing board' and 'laminate' layers in rock surface crusts.

The most promising of the new methods is the application of single grain OSL. The initial use of single grains has shown great promise, although the application has been limited to mud wasp nests. Potential may also exist, with refinement of the OSL method, for the dating of quartz particles smaller than sand grains that are entrapped in paints and rock surface coatings. New dating methods, probably relying on physical rather than chemical methods, need to be developed for the dating of unvarnished engravings because at present we have no way of directly measuring their ages. Research into the formation of oxalate minerals in rock surface coatings is progressing and recent studies aimed at identifying the biomolecules associated with oxalates are producing intriguing results.

One researcher stands out in this pioneering field because of his contributions to the dating of rock art across Australia, from Wargata Mina to Gunbilmurrung. Rhys Jones has combined his vast and intimately detailed knowledge of Australian archaeology with his thirst for scientific inquiry and archaeometric perceptivity and has become researcher, adviser, promoter and facilitator in the direct dating of rock art. Rhys has been the key to the direct dating of much of Australia's rock art, and as the brilliant maestro has orchestrated teams that have started to reveal the chronological mystery that has, until recently, veiled the immense archaeological resource hidden within rock art.

Acknowledgements

The direct dating of Australian rock art would not have been possible without Aboriginal endorsement of the various research proposals and the support of many local communities and individuals. Financial support has been provided by research grants from the Australian Institute of Aboriginal and Torres Strait Islander Studies, the Australian Research Council and the Australian Institute of Nuclear Science and Engineering. The Australian National University, James Cook University, La Trobe University and Melbourne University have contributed greatly to the creation of facilities needed by researchers for the dating projects. Laval University, the Australian Nuclear Science and Technology Organisation, and accelerator mass spectrometry facilities in Vancouver (RIDDL), Tucson (AA), Livermore (CAMS), Oxford (OxA) and New Zealand (NZA) and their staffs have made ^{14}C dating of rock art possible.

Table 1. Summary of some dating results for Australian rock art. The table identifies motifs, materials, extraction techniques and presents reliability comments.

Site location	Motif	Material dated	Extraction technique	Dating method	Age estimate (Years BP)	Comments
Wargata Mina	hand stencil	blood protein?	chemical separation	AMS ^{14}C	10,370±810	Doubtful separation of blood
Wargata Mina	red smear	blood protein?	chemical separation	AMS ^{14}C	9,240±820	Doubtful separation of blood
Gnatalia Creek	rectilinear grid	charcoal	combustion	AMS ^{14}C	6,085± 60 29,795±420	Fossilised micro-organisms and charcoal in amorphous silica associated with paint - unreliable
Olary	engraving	rock varnish	elemental analysis	Cation-ratios	1,400–31,500	Unreliable method
Olary	engraving	rock varnish	combustion	AMS ^{14}C	7,400–31,700	Probably contaminated with coal
Chillagoe	abstract paintings	charcoal	oxygen plasma	AMS ^{14}C	3,350±350	Reliable age estimate, but potential problem of old wood
Chillagoe	painting	oxalate	combustion	AMS ^{14}C	28,100±400	Reliable age estimate
Laura	female human painting	plant fibres	combustion	AMS ^{14}C	725±115	Reliable age estimate
Laura	painting	oxalate	combustion	AMS ^{14}C	24,600±220	Reliable age estimate
Victoria River	painted engraving	oxalate	laser oxidation	AMS ^{14}C	3,160±60	Reliable age estimate
Victoria River	off-art crust	oxalate	acidified permanganate oxidation	AMS ^{14}C	4,080±50	Reliable age estimate
Kimberley	Gwion or Bradshaw painting	oxalate	combustion	AMS ^{14}C	3,880±110	Reliable age estimate
Kimberley	faded purple painting	mud wasp nest stub	quartz grain	OSL	17,500±1800	Probably reliable, but motif identification problematic
Kimberley	Argula painting	charcoal	combustion	AMS ^{14}C	430±130	Reliable, but potential old wood problem
Kimberley	macropod track painting	beeswax	combustion	AMS ^{14}C	1,680±50	Reliable age estimate
Keep River	engraved cupule	oxalate	acidified permanganate oxidation	AMS ^{14}C	4,320± 80	Reliable age estimate
Laurie Creek	red smear	blood protein?	chemical separation	AMS ^{14}C	20,320 +3100 /-2300	Doubtful separation of blood
Angbangbang	under painting	oxalate	combustion	AMS ^{14}C	12,250±105	Reliable age estimate
Gunbilmurrung	turtle painting	beeswax	combustion	AMS ^{14}C	3,820±70	Reliable, independently verified

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Wetlands Archaeology in the Top End: Models, Mounds and Mobility

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IN 1981 the Kakadu Archaeological Consultancy, led by Rhys Jones, investigated open sites located on the floodplain margins of the South Alligator River wetlands in the Top End (Jones 1985a, 1985b; Meehan et al. 1985). This research followed up previous work undertaken on the nearby East Alligator River and Magela Creek systems. The data derived from the 1981 research added substantially to our knowledge not just of settlement and subsistence patterns on the wetlands but to the archaeology of the Kakadu region in general during the mid to late Holocene period. It was discovered that the occupation of the wetlands was linked closely to the geomorphologic evolution of the landscape, namely the spread of mangrove forests following sea level stabilisation in the mid-Holocene, through to the formation of the vast freshwater swamps which dominate the floodplains today. This scenario is common to the major river systems draining the coastal plains of the Top End, although the rivers themselves may be at different stages of evolution. Since the Kakadu work, studies of wetlands archaeology have been carried out along the floodplains of the Mary and the Adelaide Rivers, and some intriguing differences have been noted between settlement patterns (for example, Bowen 1996; Guse 1992; Hiscock 1999). On the South Alligator and the Mary Rivers, occupation is in the form of large open sites with little depth of deposit, containing numerous stone artefacts and virtually no organic remains. On the Adelaide River, earth mounds are a common feature. They are up to 2 m high and contain both stone artefacts and faunal remains. The evidence from the Adelaide River is based on the results of research contained in my forthcoming doctoral thesis.

This paper seeks to explain why there appear to be differences in settlement strategies between areas which are both similar environmentally and close geographically.

Although wetlands sites in the Top End date from the mid Holocene, the archaeology discussed in this paper will be limited to the last 2000 years, after the widespread establishment of freshwater wetlands on the floodplains of the north. Prior to this time different environmental conditions existed. While the interpretation of all these sites is linked to development of freshwater conditions, the differences between settlement patterns on the various river systems which have emerged make it clear that no single pattern of settlement fits all wetlands occupation.

ENVIRONMENTAL BACKGROUND

The region under discussion lies on the coastal plains of northern Australia, 12° south of the Equator in a sub-humid savannah environment. To the north it is bounded by the Van Diemen Gulf, to the east by the Arnhem Land plateau, to the west by Cape Hotham and to the south by low hills and rises. The climate is markedly seasonal with a long dry season, lasting from April to November and a shorter wet season from December to March. Several large rivers drain the Arnhem Land escarpment and southern hills, flowing through the coastal plains, floodplains and estuaries to dump into the van Diemen Gulf. From east to west the waterways discussed here are the Magela Creek, which is a tributary of the East Alligator River, the

South Alligator, the Mary and the Adelaide Rivers (Fig. 1). The floodplains of these rivers can be divided into three geomorphological units: the coastal plains (lower river and estuary), estuarine plains (middle river up to the limit of tidal influence) and the alluvial plains (upper river) (Chappell 1988; Clark et al. 1992; Hope et al. 1985; Woodroffe and Mulrennan 1983; Woodroffe et al. 1993). The sites discussed here are all located adjacent to the estuarine plains.

The hydrology of the coastal plains of northern Australia is regulated by the strongly monsoonal climate. The ground water builds up in the wet season and maximum run-off occurs late in the season (Chappell and Woodroffe 1985:90). Floodplain areas are inundated during the wet season, making many places accessible only by watercraft. These areas dry out progressively through the dry season. By the late dry season most of the floodplains have dried out, except for low-lying areas of back swamp and lagoons and billabongs which remain submerged for most or all of the year.

Geomorphology

During the post-Pleistocene sea level rise, down-cut river valleys in northern Australia were drowned. The various river systems of northern Australia responded differently to this event. Some, like Darwin Harbour, became deep water embayments. Others through processes of sedimentation formed mangrove swamps and, in the late Holocene, freshwater swamps (Chappell 1988:34). The rivers of the Kakadu region are examples of the latter type. When the sea level stabilised, mangroves rapidly invaded, leading to what has been described for the South Alligator River as the 'Big Swamp Phase' from ca. 7000 BP to 5300 BP (Woodroffe 1988; Woodroffe et al. 1986). This was followed by the 'Sinuous Phase' from ca. 5300 BP to 2000 BP. During this period the South Alligator increased its sinuosity and wandered across the floodplain. Meander channels were cut off and became palaeochannels. Siltation and coastal progradation meant the tidal influence was cut off and the mangroves retreated seawards and to channels on the river. Saline mud flats were common on the coastal plains during this period and freshwater clays began to accumulate (Woodroffe 1988). The 'Cuspate Phase' began after 2500 BP and is marked by the change in channel form to sharp inner bends (Woodroffe 1988:5). During this phase, the slowing of coastal progradation and continued sedimentation led to a final cut-off of the tidal influence and the ponding of freshwater behind the seaward chenier resulted in the formation of the freshwater floodplains. These were established on the middle reaches of the South Alligator River by 1400 BP (Hope et al. 1985:235). The same sequence has been established for the Magela Creek, and the Mary and Adelaide Rivers (Chappell 1988; Clark and Guppy 1988; Woodroffe and Mulrennan 1993; Woodroffe et al. 1993).

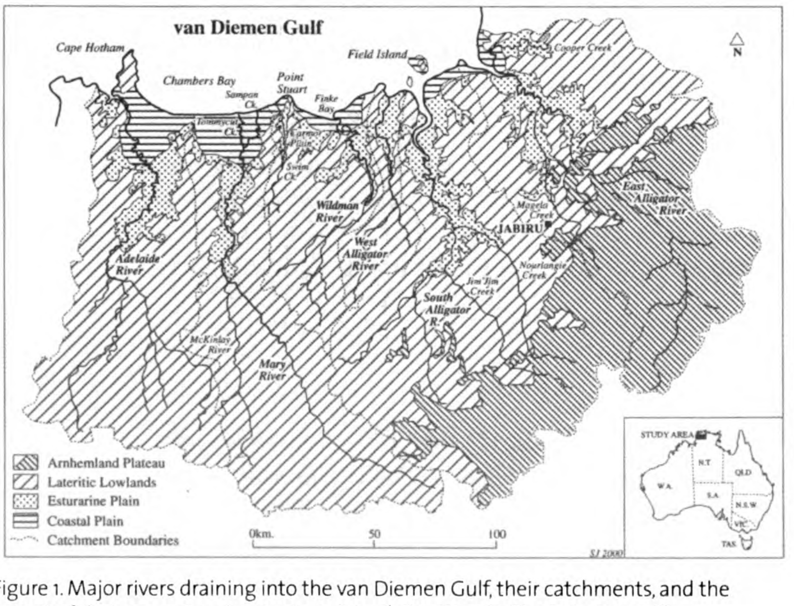

Figure 1. Major rivers draining into the van Diemen Gulf, their catchments, and the extent of their coastal and estuarine plains (after Woodroffe et al. 1993:258).

While there are similarities in the development of the northern rivers, Chappell has pointed out that there are also differences. Not all the rivers in the region have changed in the same way as the South Alligator. There is a spectrum ranging from the Adelaide River, which has a highly sinuous course and a very low rate of channel migration, through to the Daly River which has a highly mobile channel in which meanders cut off and reform continually. However all probably flowed through sinuous meandering channels while their floodplains were forming, ca. 5000 BP, and subsequent changes depend largely on catchment sizes and specific floodplain and tidal river systems (Chappell 1988:52).

The South Alligator River has the largest catchment and the Adelaide River has the largest estuarine plain (Table 1) (Woodroffe et al. 1993). The forms of their estuaries also differ. The South and East Alligator Rivers flow freely through funnel-shaped estuaries into the Van Diemen Gulf, while the estuary of the Adelaide River was diverted between 3500 BP and 2300 BP, and that of the Mary River is blocked (Woodroffe et al. 1993:267, 271). The South Alligator River, Magela Creek and Mary River all have perennial backwater swamps. However flooding on the Adelaide River floodplains is shallow and relatively short-lived, and areas of perennial swamp are much smaller compared to wetlands on river systems further east (Kingston 1991:32).

Table 1. Areas of estuarine plains and catchments of rivers (after Woodroffe et al. 1993:271).

River	Estuarine Plain (km²)	Upland Catchment (km²)	Condition of Estuary
Adelaide River	1100	6250	Diverted
Mary River	540	6350	Blocked
South Alligator R.	915	10315	Open
East Alligator R.	145	8800	Open

Resources

The floodplains of the northern rivers are abundant with fauna and flora that are useful to hunter-gatherers. The most important mammal inhabiting the freshwater wetlands is the dusky rat (*Rattus colletti*). Although this water rat was eaten by Aborigines in the past and was an important food in the late dry season, its presence is more significant for the fact that in large numbers it can support many carnivores that were also exploited by Aborigines, including birds and reptiles, especially the water python (*Liasis fuscus*). Two species of flying fox (*Pteropus alecto* and *P. scapulatus*) also inhabit these floodplain and riverine environments (Conservation Commission of the Northern Territory 1993:7; Russell-Smith et al. 1997).

The floodplains are a haven for bird life, especially water birds. Sixty-five species of these have been recorded and at least 16 breed there regularly, including the magpie goose (*Anseranus semipalmata*), which is an important resource for Aboriginal people. Other favoured foods at various times of the year are whistling ducks (*Dendrocygna arcuata and D. eytoni*), the Rajah shelduck (*Tadorna radjah*), green pygmy goose (*Nettapus pulchellus*) and several species of herons, ibises and spoonbills (Bowman and Wilson 1986:76; Conservation Commission of the Northern Territory 1993:6-7; Finlayson et al. 1988:110; Russell-Smith et al. 1997).

Reptile species of the floodplains important to Aboriginal subsistence include crocodiles, both estuarine (*Crocodylus porosus*) and freshwater (*C. johnstonii*); freshwater turtles (*Carettochelys insculpta, Chelodina rugosa,* and *Elseya dentata*); several species of monitor, including two species of sand goanna (*Varanus gouldii* and *V. panoptes*) and the mangrove monitor (*V. indicus*); and several species of snakes, including the Arafuran file snake (*Acrochordus arafurae*) (Conservation Commission of the Northern Territory 1993; Finalyson et al. 1988; Russell-Smith et al. 1997).

There are also numerous fish species. Although many species are eaten, the most important ones are the salmon catfish (*Arius leptaspis*) from the floodplains; black bream (*Hephaestus fuligunosus*), spangled grunter (*Leiopotherapon unicolor*) and archerfish (*Toxotes chatareus*) from the river; and barramundi (*Lates calcarifer*) and saratoga (*Scleropages jardini*) from the floodplains and the river (Conservation Commission of the Northern Territory 1993; Finlayson et al. 1988:112; Russell-Smith et al. 1997).

The invertebrate species from the floodplains and the river which are most important to the Aboriginal population are freshwater mussels (*Velesunio angasi*) and honey from the native bee (*Trigonia* spp.) (Russell-Smith et al. 1997).

Plant species that were important to Aboriginal subsistence and grow on the floodplains include several species of yams (*Amorphophallus* sp., *Aponogeton elongatus*, *Dioscorea* spp., *Ipomoea* spp., *Triglochin procera*), water chestnuts (*Eleocharis dulcis*), waterlilies (*Nelumbo nucifera*, *Nymphaea* spp.) and wild rice (*Oryza* sp.) (Bowman and Wilson 1986; Howe and Czachorowski 1979; Russell-Smith 1985, Russell-Smith et al. 1997; Smith 1980). There are also numerous other species that were edible and/or used in the material culture.

The annual distribution of flora and fauna of the floodplains is strongly influenced by the highly seasonal nature of the dry tropics. During the wet season, floodplain resources are flooded and dispersed. As the floodplains dry out, resources are concentrated around areas of permanent water, resulting in a high biomass located in a small area by the late dry season. The physical differences between the floodplains affect hydrology and mean that ecological components differ between rivers. So that, although the systems are broadly similar and may contain many of the same elements, different micro-environments exist on individual river systems at different times of the year. For example, magpie geese favour the corms of semi-acquatic spike rushes (*Eleocharis* spp.) which grow in water up to a 1.5 m in depth. The geese harvest from wet areas as their beaks cannot penetrate the black soil once it has dried out (Morton et al. 1990; Tulloch 1985:289). For nesting they favour the stems of *Eleocharis* spp. and *Oryza* sp. that flourish in the perennial back swamps of the floodplains. Different flooding regimes and floodplain conditions mean that magpie geese are distributed differently on each river system. On the South Alligator system (Nourlangie Creek and Boggy Swamp) the magpie goose population peaks in the mid dry season when the birds feed on vast *Eleocharis* sp. swamps. In the late dry season the geese move onto the Magela Creek floodplains where they see out the dry season on perennial swamps. During the wet season magpie geese numbers are substantially lower on both systems when they transfer to the floodplains on the East Alligator River to nest (Morton et al. 1990:307; Morton and Brennan 1991:143). The floodplains of the Mary and Adelaide Rivers are wet season habitats for magpie geese and contain the most important breeding grounds in the Northern Territory (Guse 1992:47–8). However, there are microenvironments involved here too. The floodplains where the archaeological sites are located are classified as high black soil grassland. They are important feeding areas in the mid wet season, whereas the low black soil swamps, located elsewhere on the floodplains, are the breeding grounds (Bowman and Wilson 1986:75-6).

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Occupation sites dating back to the Pleistocene are found in rockshelter sites in outliers of the Arnhem Land plateau on the floodplain margins of the East Alligator River and Magela Creek (cf. Kamminga and Allen 1973; Schrire 1982; Allen and Barton 1989; Roberts et al. 1990). However, this paper is concerned with the archaeology associated with freshwater wetlands.

Most sites occupied after the formation of the freshwater floodplains are open sites dating to post-2000 BP. Detailed investigations of these sites have been carried out on the South Alligator River (Brockwell 1983, 1989, 1996b; Guse 1992; Hiscock 1996, 1999; Hiscock et al. 1992; Kamminga and Allen 1973; Meehan et al. 1985), Magela Creek (Bowen 1996; Schrire 1982), Mary River (Baker 1981; Guse 1992) and the Adelaide River (Schrire 1968; Smith 1980; Brockwell 1996a, 1996c).

On the South Alligator River floodplains settlement tends to occur in a strip pattern on headlands that jut into the freshwater wetlands. These sites are flooded in the wet season. They consist of open scatters of artefacts up to a kilometre in length with a high diversity of stone artefact types and raw materials (Brockwell 1989; Kamminga and Allen 1973:10–17; Meehan et al. 1985). One of the sites, Kina, includes an earth mound with freshwater mussel shell (*Velesunio angasi*), excavated and dated to 280±140 BP (Meehan et al. 1985:152). The other surface sites had no datable material, but because of their association with the freshwater wetlands, it was concluded that these sites were occupied after the establishment of the freshwater conditions on the floodplains ca. 1400 BP (Hope et al. 1985:235; Meehan et al. 1985:152). Earth mounds littered with stone artefacts were also located at two sites on higher ground adjacent to the wetlands (Brockwell 1989:158-62; Meehan et al. 1985:126-7).

Subsequent surveys located surface scatters of stone artefacts, mainly quartz flakes, in the open woodland behind the floodplains (Hiscock et al. 1992; Guse 1992). These are mostly quartz quarrying and knapping sites. For this reason they often have a high density of artefacts, but are generally substantially smaller than the wetland sites, have fewer stone artefacts, and a smaller range of artefact types and raw materials (Guse 1992:92-3).

The surveys on the South Alligator River defined three classes of sites; open sites on the wetland margins, earth mounds on the higher ground adjacent to the wetlands, and open sites in the woodlands. The earlier surveys located 11 sites associated with the wetlands, and six earth mounds in two clusters on higher ground adjacent to the wetlands (Brockwell 1989; Kamminga and Allen 1973:10-17; Meehan et al. 1985). Guse (1992:78) located eight additional wetlands sites and 11 in the open woodlands (Table 2).

Table 2. Site types (%) on the floodplains of the Top End (post-2000 BP).

Site Type	South Alligator (n=37)	Magela (n=?)	Mary (n=13)	Adelaide (n=45)
Wetlands/Open	54%	2	100%	29%
Wetlands/Mound	16%	present		67%
Wetlands/Rockshelter		1		
Woodlands/Open	30%			4%

There are a number of rockshelter sites associated with the escarpment and outliers in close proximity to the Magela Creek and East Alligator River, that contain faunal remains from the floodplains; Malangangerr, Nawamoyn and Paribari (Schrire 1982); Malakanunja II (Kamminga and Allen 1973; Roberts et al. 1990) and Ngarradj Warde Jobkeng (Allen and Barton 1989). However most of these remains are of estuarine fauna associated with the Big Swamp Phase prior to 3000 BP. It is only Paribari on the Magela floodplains that is firmly linked to the freshwater period post-1500 BP and it has been argued that the other sites were abandoned prior to this time (Allen and Barton 1989:90-2).

Bowen (1996:155-7) carried out a number of systematic and purposeful surveys across the Magela Creek floodplains. His surveys also covered adjacent open woodland and 'mixed scrub'. He established that, apart from the rockshelters, sites were generally lacking in this area. Only two sites, both open artefact scatters on the floodplain margins, contained artefacts in any abundance (Bowen 1996:176-88). One of these sites was recorded previously by Kamminga and Allen (1973). Allen and Barton (1989:106) mention that some small earth mounds also occur in the vicinity of the floodplains sites, but do not state how many.

So three site types can be identified for the Magela floodplains; open sites, earth mounds and rockshelter sites (Table 2). The differences in settlement patterns between to the Magela and South Alligator systems is the paucity of floodplain sites on the Magela, despite the similarities between their resource bases. The rockshelter occupation on the Magela Creek can be attributed to the proximity of the escarpment to the floodplains in the northern sector of Kakadu.

On the Mary River, surveys by Guse (1992:94) revealed thirteen sites, all open artefact scatters, located on the floodplain margins in various ecological niches that included river levees, black soil plains and permanent water. No sites were located on high ground in the open woodland adjacent to the wetlands (Table 1).

A number of investigations have been undertaken in the adjacent Adelaide River region (Brockwell 1993, 1996a, 1996c; Crassweller 1996; Hiscock 1995; Smith 1980; Smith and Brockwell 1994; Schrire 1968). Along the floodplain margins, thirteen open artefact scatters were recorded, all of which were small in area and contained artefacts of mainly one raw material. Thirty earth mounds were located, six of which have been excavated. The mounds contained a high density of artefacts, made on a number of different raw materials, both local and non-local. There was also a variety of faunal remains from both the wetlands and the woodlands (Brockwell 1996a; Schrire 1968). One of the mounds dated to 4000 BP but the majority were less than 2000 BP. Only two sites were located in the open woodlands. One is a small scatter of quartz flakes, the other a seam of quartzite that has been quarried (Brockwell 1993, 1996c; Table 2).

DISCUSSION

Table 2 demonstrates that the overwhelming focus of settlement for all four rivers is on the wetlands rather than the woodlands. However within this generalisation, it is clear that the settlement patterns are different for each river. The South Alligator River has a significant number of woodland sites, while the Magela, Mary and Adelaide systems have none or few in open woodland. The Magela Creek floodplains lack sites generally and the Adelaide River is dominated by mound sites. So it seems it is not possible generalise about wetlands occupation. What can account for this variability of settlement patterns on the wetlands, all of which are similar ecologically?

Hiscock (1984:133) argues that when offering explanations for variability in the archaeological record, one must eliminate more simple reasons before accepting more complex ones. He (1984:133-5) has pointed out that post-depositional factors, such as research bias and taphonomic processes, must be considered first as these influences will affect data collection and analyses. These effects must be removed before explanations of human behaviour can be considered. Likewise, both Jones (1985a:294) and Head (1986:126) have pointed out that environmental factors must be considered before social explanations are assumed.

Bearing this advice in mind, I have identified a number of factors that need to be considered in relation to settlement patterns on the northern floodplains. I will address each of these issues separately and present the various theories that have been put forward to explain settlement patterns on the floodplains. These issues include sampling, taphonomy, chronology, environment and culture.

Sampling

It is possible that differences in settlement patterns between the various rivers reflect sampling bias, i.e. different sites were located because different survey methods were employed. However, in all cases random and/or purposeful sampling strategies were used both on the wetlands and the higher ground adjacent. These approaches enabled a representative sample of sites to be recorded. A further issue in sampling involves visibility. The data suggest that visibility did not affect the locating of sites. Surveys on the South Alligator River, Mary and Adelaide Rivers were undertaken in the late dry season when vegetation had died back and in some cases had been cleared by fire. Visibility under these conditions is usually good. Additionally, most surveys were undertaken over a number of years and the same sites were recognised. Magela Creek was surveyed over only one season, but Bowen (1996:174-6) argues against visibility being a limiting factor in site location in that case. Therefore there is little evidence that sampling bias is responsible for the different settlement patterns observed between the rivers.

Taphonomy

As a result of experimental work Gregory (1998:i) concluded that the biggest taphonomic impact upon sites in the Ord-Victoria River region are fluvial processes and that these processes are likely to have a similar impact on sites elsewhere in northern Australia.

Bowen (1996) considered this issue in relation to the general lack of sites around the Magela Creek floodplains. He also concluded (1996:173) that the greatest taphonomic force in the region was fluvial impact and that artefacts may have been moved or buried by flooding or run-off. However he dismissed taphonomy as the agency responsible for site distribution as he argued that fluvial forces affect the region as a whole and still two substantial sites occur on the southern margins of the Magela floodplains whereas they are completely absent on the northern side.

Meehan et al. (1985:103) drew attention to taphonomic forces operating at the South Alligator River floodplains sites when they pointed out that sites had been degraded by annual inundation and disturbed by buffaloes. However the affect that these conditions had on the sites was to expose them through erosion rather than bury them.

Conditions are the same on the Adelaide River where sites are exposed to heavy rains and flooding during the wet season. Animals have also had an impact as the sites are located on a former cattle station and, before the Brucellosis and Tuberculosis Eradication Campaign (BTEC) in 1989, buffaloes were present in high numbers. However like the South Alligator River, these forces are likely to have exposed sites rather than obscured them. Therefore I think it is unlikely that taphonomy is responsible for the differences in settlement patterns between the rivers.

Another taphonomic factor perhaps influencing the presence/absence of mound sites is the megapode (*Megapodius reinwardt*). Stone (1989, 1991, 1992, 1993) argued that these mound building birds were responsible for the creation of all mounds, even those containing cultural material. This proposal has been refuted by a number of researchers (cf. Bailey 1991, 1994, 1999; Burns 1994; Cribb 1991; Mitchell 1993; Roberts 1994; Rowland 1994; Veitch 1994; Williams 1994). In the case of the Adelaide River mounds, megapodes seem an unlikely explanation for their existence. Those mounds that were excavated contain cultural material throughout and their dates are in sequence - criteria that Mitchell (1993:183-4,190) used to distinguish humanly constructed mounds from megapode mounds.

Chronology

Although we are looking at the same phase of development for all rivers - the freshwater phase of the floodplains from 2000 BP until present – this event was not necessarily synchronous on individual rivers, as their floodplains became freshwater at different stages. This event is dated to ca. 1370±70 BP on the South Alligator River (Hope et al. 1985:235); ca. 1650 BP on the Magela Creek (Clark et al. 1992:47); and ca. 2000 BP on the Mary River (Woodroffe and Mulrennan 1993:61). It has not been dated directly on the Adelaide River. However, faunal remains from earth mounds on the edge of the Adelaide River floodplains link these sites to exploitation of freshwater wetlands. They are mostly dated to within the last 2000 years.

Hiscock (1999:99) suggests that, even though the environment may have been broadly similar to today over the last 1500-2000 years, changes have continued to occur at a local level, adaptations to which are reflected in the archaeological record. In western Arnhem Land where the South Alligator and Magela systems are located, he suggests a coastal focus of settlement from 1500 BP to 700 BP, based on dates from coastal middens (Hiscock and Mowat 1993; Mowat 1995). Subsequently, rapid environmental changes led to the disappearance of coastal resources. Between 1000 BP and 700 BP the focus shifted to sub-coastal resources on the floodplains of the river, for which the relatively recent date of ca. 300 BP from Kina on the South Alligator River (Meehan et al. 1985:152) fits well. However, the pattern seems to be different on the Adelaide River where the earth mounds from the freshwater phase date from ca. 2000 BP.

Environment

Environmental explanations for site distribution include arguments based on seasonality of occupation, and differences in site use and mobility strategies linked to the availability of water and other resources

Meehan et al. (1985:119,121,123) concluded that the South Alligator wetlands open sites were occupied at different times during the dry season and used for different activities according to resource availability. Brockwell (1983, 1989) concluded that the differences between the mound sites and the open sites on the South Alligator River floodplains reflected different seasonal occupation. While site distribution and ethnographic evidence indicates that the wetland areas were occupied year round, the large open sites were probably dry season base camps, while the mounds were occupied during the wet (Brockwell 1983:72–4, 1989:293).

Guse (1992:8) suggested that environmental factors explain the broad differences in site size, site artefact density and artefact assemblage composition between sites on the South Alligator and Mary Rivers. He proposed that the availability of raw material resources and utilisation of certain food resources might be responsible. Using definitions supplied by Thomas (1989:86) Guse (1992:66) interpreted the wetland sites as being 'long term residential areas' exploiting the wetlands resources. The open woodlands sites he described as 'logistic encampments' and 'diurnal use' sites that people used for specific activities, such as quarries and knapping sites (Guse 1992:93). Hiscock (1996:156) concurred with this assessment of low residential mobility along the floodplain margins and high mobility at the woodlands sites. He based this conclusion on the fact that the bipolar technique, which is a more economic method of producing flakes, is an indicator of low mobility among populations. Bipolar cores are common at the South Alligator floodplain sites and rare in the woodlands sites.

While acknowledging the differences between settlement patterns on the South Alligator River and the Magela Creek floodplains, Bowen (1996:195) does not offer any overt explanations, apart from hinting that it relates to the availability of perennial water sources.

Guse (1992:94) concluded that, on the Mary River, settlement was focussed mainly on the floodplains as there were no sites located in the open woodland, and that these open sites reflected both long term and short term activities.

As a result of his surveys on the South Alligator and Mary Rivers, Guse (1992) constructed a model of Aboriginal subsistence/settlement patterns that could be applied generally to wetland utilisation, similar to that proposed by Brockwell (1996). He (1992:96-97) suggested that wetlands use was year round. Wet season occupation was restricted to wetlands margins while goose eggs were harvested and geese taken during the laying season. At the same time there would be a dispersal of groups into other environmental zones, like the open woodlands, while other wetlands resources were either flooded or dispersed. There would be more intense occupation of the wetlands during the dry season especially late in the season when diminishing water sources would concentrate water birds and the geese were fat from a season of eating *Eleocharis* spp. and wild rice, carbohydrate sources also exploited by the humans.

Guse (1992) predicted that the archaeological signature of this pattern would be large open sites on the wetlands margins, close to sources of avifauna and plants. These sites would contain 'low average artefact density, high richness in artefact and raw material types and large total numbers of artefacts' (Guse 1992:97-98). Sites in the open woodlands would be smaller and contain higher densities of artefacts, reflecting stone procurement and manufacture of artefacts in these areas, as well as single use sites such as grinding hollows. Artefact and raw material type richness in these sites would be low, as would be total artefact numbers. Site frequency would be similar for both wetlands and open woodlands (Guse 1992:98).

Surveys on the floodplain margins of the Adelaide River and in the open woodlands, however, revealed quite a different pattern of occupation. The sites were located mostly next to the floodplains and conformed to

Guse's (1992:97–8) description of 'high richness in artefact and raw material types'. But mound sites rather than artefact scatters dominated and they have a high artefact density as well as high total numbers. The diversity of artefact types and raw materials indicate that they were base camps. The faunal remains suggest exploitation of both floodplains and open woodland. So the mound sites could have been occupied on a year-round basis (Brockwell 1996:56-7; Schrire 1968). A high presence of bipolar knapping techniques also suggests low mobility as outlined by Hiscock (1996).

Rowland (1994:155-6) is firmly convinced that mounds occur in areas of high biomass where there is a non-linear distribution of resources. On the other hand, linear distribution of resources will result in a pattern of linear occupation, such as continuous middens along open coastlines. It is also true that northern Australian mounds, whether of shell or earth, appear to be located at the junction of different resource zones (Bailey 1977; Baker 1981; Beaton 1985; Bourke in press; Burns 1999; Cribb 1986; Guse and Majar 2000; Meehan 1988, 1991; Peterson 1973; Roberts 1994). Peterson (1973:186) assigned a seasonal role to earth mounds from his observations of the movements of a group of people at the Arafura Swamp. The mounds were located on the edge of the floodplains at the base of a plateau containing monsoon forest and open woodland, and close to pools of water that persist well into the dry season. He noted that,

> Each area where the mounds are located is one where it is desirable to camp when the streams on the plateau have stopped running but where the ground is either flooded or remains muddy and damp under foot for several months. By the height of the dry season all the clusters of mounds are high and dry with no water close by (Peterson (1973:186).

The South Alligator and Adelaide River mounds are all located at the junction of the floodplains and the open woodland. From an examination of topographic maps it appears also that the mounds located on the South Alligator River (Brockwell 1989:158–62; Meehan et al. 1985:126–7), the Magela Creek (Allen and Barton 1989:106) and the Adelaide River are located adjacent to seasonal floodplains rather than permanent swamps, usually beside a lagoon or old river channel.

Culture

Cultural explanations for settlement patterns on the floodplains include population expansion, demographic shifts, and 'intensification' — increased sedentism and population growth resulting from changes in the social and/or technological mode of production.

Bowen alluded to a cultural explanation when he said that, in contrast to the South Alligator floodplains, site distribution on the Magela floodplain 'suggests that environmental bounty was not the only factor influencing the spatial organisation of Aboriginal people' (1996:206). But he does not elaborate on this point.

In southern Australia, Williams (1988) argued that earth mound sites in southwestern Victoria found in similar topographic circumstances to those in northern Australia, were the result of social rather than environmental change. She (1988:220–1) concluded that mound-building was not only an environmental adaptation associated with exploitation of swampy areas but also linked to changes in social networks which led to increases in production and allowed more sedentary occupation of sites and a consequent increase in population.

Jones argued that the increase of sites along the South Alligator River post-1000 BP signalled an increase in the population of the region overall. However he cautioned against explanations such as 'intensification' and said that environmental evidence such as the seasonal abundance of resources on the adjacent floodplains should be considered as well (Jones 1985a:294). Lourandos (1996:86), a proponent of 'intensification' himself, acknowledged that the model was not designed to be tested on unstable coastal landforms for the very reason that it was too difficult to separate environment from other factors. Rowland (1994:156) argued against mounds being indicative of population growth or intensification, though he did not discard either theory entirely. He also pointed out that it is still difficult to resolve whether mound sites are the result of repeated occupation over a long period or intense occupation over a short period. It may be a case of the latter at the South Alligator River site of Kina that is dated to ca. 300 BP (Meehan et al. 1985:147-52), but it seems that it is a case of the former with the Adelaide River where some mounds have dates spread over a 2000 year period.

Hiscock (1999:100) argued the case for changes in demography in response to the appearance of the freshwater floodplains. He argued that increased settlement of these areas post-2000 BP was merely the result of a relocation of population from elsewhere, rather than a uniform increase of population throughout the region. As well as the example of the South Alligator River (see Chronology above), he (1999:99) cited the case of Bayview Haven on Darwin Harbour where there was an intense period of coastal occupation in the form of Anadara shell middens from 1500 BP until 700 BP when mangroves colonised the open beaches and led to the demise of the Anadara beds. The fate of the inhabitants is uncertain but Hiscock (1999:99) suggests that they may have moved to sub-coastal resource areas, such as the lower Adelaide River, and there does indeed appear to be an expansion of occupation post-700 BP there. However Bourke (pers. comm.) suggests that the Darwin Harbour people merely shifted locally to alternative sites on the harbour that have yet to be dated.

CONCLUSION

This paper has highlighted the differences in settlement patterns between the freshwater floodplains of the Magela Creek and the South Alligator, Mary and Adelaide Rivers in the late Holocene. The discussion concluded that these differences are not the result of sampling bias, post-depositional taphonomy or megapode nest building. A closer examination of the environment has indicated that, despite broad similarities between the floodplains post-2000 BP, there are a number of spatial and temporal factors that may have influenced settlement strategies. Catchment and floodplain size and subsequent hydrological regimes have created microenvironments or sub-regions on individual rivers that have affected resource distribution. Even though the archaeology under consideration is contained within the freshwater phase of the last 2000 years, these microenvironments have not necessarily remained stable, and there have been local environmental changes that have affected each river system individually. Additionally, although the development of the floodplains was broadly similar, the chronology of these changes is different for different systems, for example the freshwater phase began earlier on the Adelaide and Mary than the South Alligator River and the Magela Creek. Topographic differences may also have been responsible for different types of occupation. The earth mounds under discussion are all associated with discrete seasonal water bodies located on floodplains, whereas the open artefact scatters exist adjacent to perennial backwater swamps. Therefore earth mound occupation may be associated with swamp resources that are distributed in a compact non-linear way, as suggested by Rowland (1994:155). Whereas open artefact scatters are the type of occupation associated almost exclusively with linear distributions of resources along permanent swamps. If such is the case, it may be that earth mounds and open artefact scatters are merely different topographic expressions of the same phenomenon of swamp exploitation. This model would explain the absence of large open artefact scatters on the Adelaide River floodplains, which lack the perennial backwater swamps of the South Alligator River. However, I think that the explanation goes further than this. Peterson's (1973:186) observations at the Arafura Swamp lend credence to a seasonal rationale for earth mound occupation. Repeated early dry season occupation of raised dry ground adjacent to freshwater swamps would have led to the build up of mounds. As the dry season progressed people would have abandoned the mounds and relocated closer to the retreating swamps, now able to camp anywhere on the dried out floodplains. The open artefacts scatters along swamp edges are the archaeological expression of this late dry season settlement strategy. Wet season occupation of mounds is not precluded from this seasonal model as they would make the logical base from which to exploit the wet season harvest of goose eggs.

How much the difference in settlement patterns was a result of cultural factors is unclear. 'Intensification' has been an argument used particularly in relation to mound occupation. However, as pointed out by various authors, the creation of the freshwater swamps was sufficient reason for an expansion of occupation, not necessarily preceded by social and/or technological changes. Population growth as an explanation is also difficult to demonstrate, and site expansion on the floodplain margins post-2000 BP may simply be a result of demographic shifts that reflect locally changing resource bases.

The model of wetlands settlement that emerges from these regional studies is one of sedentary dry season occupation of multi-purpose base camps located on the edge of the highly productive freshwater floodplains. It is possible that occupation of such localities is year round, with the resources of adjacent open woodland and monsoon rainforest being exploited in the wet season when the floodplain resources are flooded or dispersed. Where people resided at this time is unclear. Sites in the open woodland indicate high mobility, being either single purpose or short term encampments. The mound sites are possible wet season/early dry season base camps as they are located on high ground and faunal remains indicate exploitation of both floodplains and woodland resources. However, the archaeological expression of this model differs according to the factors discussed above, all of which must be considered when reconstructing patterns of wetlands settlement in northern Australia.

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High Places

Cathie O'Sullivan

JOHN MULVANEY wrote a reference for me, based on archaeological work I had done through the University of Sydney. John had not met me before but with his letter I approached Rhys, whom I had not met before either. It was a good letter I think, and perhaps Rhys already knew I played the Celtic harp because with Rhys and other people who became lasting friends, I ventured north into Kakadu in May 1981. I, who never sew, sewed pebbles into the bottom of mosquito netting walls to hold them down in our tarpaulin-based 'dining room' at Nourlangie where we entertained many visitors including 'Nugget' Coombs.

I sprained my ankle twice (and still Rhys did not send me home), created a laboratory out of the kitchen in a buffalo shooting lodge, dodged a bullet (someone was cleaning a rifle ... the same person whose drum kit sat near the 'dining room'), avoided pigs and buffalo, swam with crocodiles, climbed the Arnhem Land plateau — and was not the only one to get heat stroke — dug a very deep hole at Djuwarr, learnt 'mattock' archaeological methods from Jim Allen, found small and beautiful chert and crystal blades, mopped up 'Barcoo Rot' wounds, competed with a goanna for boiled water, ran away from 'good tucker' (snake) in spite of Betty Meehan's reassurances, was swooped on by murrawadi (sea eagle) and sang for George Chaloupka and Nipper Kapirrigi on the banks of Djuwarr.

I do many things with my life, and often I work with only one side of me showing. In Kakadu, Rhys accepted the music, the anthropology and archaeology — even the pharmacy when we needed to rethink hygiene during our five weeks at Djuwarr. I used all of me in Kakadu because Rhys is someone who is able to enjoy and wonder at all things people do. I had a smaller harp made before I left Sydney, and a canvas case, so it could hang on a tree when we camped. Kakadu always photographs well and I used to marvel at how such beautiful images came from a place so full of heat, mosquitoes and crocodiles (and pigs and buffalo and

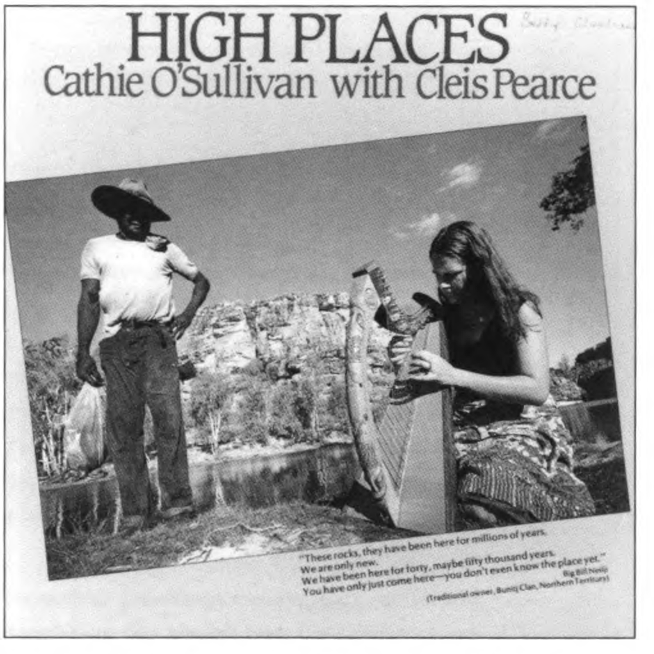

Figure 1. The cover of High Places, depicting Cathie O'Sullivan with George Namingam (now deceased) in Kakadu National Park. The cover text reads in part:

> The recording came together over a period of two years commencing with my involvement as a member of the Australian National University's 1981 archaeological team, led by Dr Rhys Jones, to the Kakadu National Park in the Northern Territory. The land is owned by the Aborigines of the Gagadju Association who lease it to the National Parks.'
>
> Our Aboriginal hosts showed us warmth and help and, through them, I felt the presence of that unfamiliar land. I would like this record to be considered part of an exchange which began with the hospitality of the Gagadju people - two such different cultures coming from lands so far apart and yet it seems sometimes, we can meet.

George Chaloupka (who took the original photograph) gave his permission for the image, including George Namingam, to be used. The photograph of the cover was taken by Darren Boyd, ANU.

snakes and...). This paradox of place filled my heart and filled my eyes with views of immense red and green valleys and distant plateaux. I came away and wrote songs and music, not about Kakadu, but certainly from Kakadu. For the first time I recorded songs with words of my own and I called this album *High Places*. But Kakadu remains with me and my music, a nub of memories - of warm people, heat, dirt, extreme colour and distance.

REFERENCE

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In Search of the Traditional Australian Aboriginal Diet — Then and Now

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THERE IS considerable interest in the subsistence behaviours and diets of early humans (e.g. Southgate 1991) for the reconstruction of prehistoric societies. Interest in these 'natural' diets (Southgate 1991:281) has also been stimulated by research on preventative diets for a number of chronic, or 'lifestyle', diseases prevalent in industrialised societies. As Eaton and Konner (1985:288) have argued, 'it is both intellectually satisfying and heuristically valuable to estimate the typical diet that human beings were adapted to consume during the long course of our evolution .... The diet of our remote ancestors may be a reference standard for modern human nutrition and a model for defense against certain "diseases of civilization." ' Research of this kind rests largely on ethnographic studies of recent and present-day hunting and foraging societies. Using 'subsistence data from fifty eight technologically primitive societies', Eaton and Konner (1985:287) claimed that the 'mean, median, and mode for recent foragers converge on a dietary ratio of 35% meat and 65 % vegetable foods'. While recognising that the 'paleolithic diet' was not fixed, they used these ratios to develop recommendations for the American diet.

In Australia there is also considerable interest in trying to reconstruct pre-contact diets not only to add insights into the processes of Aboriginal colonisation(s) and adaptations, but also to understand the causes of, and to develop preventative strategies for, nutrition-related diseases such as diabetes mellitus and cardiovascular disease. These diseases are a major cause of illness and death in Aboriginal communities across Australia. Here, too, we need to draw on the available ethnographic studies of traditional-oriented Aboriginal people. Without such information, we are forced back onto an interpretation of the sparse archaeological evidence.

Here I will summarise the observations and conclusions of some of the major studies of Australian Aboriginal subsistence. In addition to the published accounts of others, particularly those of Rhys Jones and his research colleague Betty Meehan for the Anbarra of central coastal Arnhem Land, I will draw upon observations which we have made over a period of almost 30 years in the remote Aboriginal community of Donydji, in northeastern Arnhem Land. Donydji is one of a number of Homeland Centres, or outstations, located in the lands of people known to anthropologists as the Yolngu (see e.g. White 1985, for background to both the community and the field research).

The overriding concern of our research over recent years has been to help the Yolngu people understand, and cope with their rapidly changing world. We believe that long term studies of subsistence, and an understanding of indigenous belief systems and food habits, are crucial for the interpretation of nutritional health in remote Australian Aboriginal communities such as Donydji (White 1985; White 1989; Polakiewicz et al. 1990). The value of such an approach became evident in 1985 during the initial phase of a long term study at Donydji to monitor the markers of nutritional status and the risk factors for diabetes mellitus and cardiovascular diseases (O'Dea et al. 1988). The considerable variation found among individuals in their cholesterol concentrations, triglyceride levels and fatty acid profiles, could only be understood through long term research into food beliefs and habits. Nutritional anthropological studies also enable us to predict which individuals and groups in Aboriginal communities are at greatest risk from nutrition-related disorders, particularly as a consequence of dietary and lifestyle change.

What then do we know of the diets of Australian hunters and foragers? Does the Australian evidence support the generalisation by Eaton and Konner (1985:287) concerning the macro-nutrient profile of 'the paleolithic diet'?

"Everywhere [vegetable foods] were of the order of 70 or 80 percent of the total food supplies"

This assertion was made by Meggitt (1964:33) in one of the most widely quoted papers in Aboriginal studies: from the central desert through the semi-arid steppes, the sub-humid grasslands and humid forests to the northern coasts with areas of rainforest 'vegetable foods collected by women made by far the greatest contribution to the diet; everywhere these were of the order of 70 or 80 percent of the total food supplies.'

While it is unclear whether this figure refers to gross weight or available energy or some other measure, the claim has become dogma in many anthropological studies stimulating considerable research and debate (e.g. Hiatt 1967/8; Meehan 1982a). Meehan considers that the belief that 'Aborigines were Arcardian vegetarians and that the women were the main providers of the food' are myths 'derived partly from what is stated to have been the economy of pre-European Australian Aborigines which is unsupported by any quantitative facts' (cited in Jones 1980:136). What is the evidence for Meggitt's assertion, and is there a 'typical' Aboriginal diet? Certainly, the accounts of early observers indicate substantial regional differences. For example:

1. the food of Aborigines living near Cooper's Creek in the arid southern central region of Australia 'may be described as consisting of everything having life ... [although] ... it is principally of fish and seeds, which are pounded and then mixed with water, and either eaten raw or baked in ashes' (Smyth 1878, 2:302);
2. Curr (1886, 2:46), referring to the 'Dieyerie Tribe' of this same region, wrote that 'their food is principally vegetable, animals being very scarce, if we except rats and their species, and snakes and other reptiles, of which there is an unlimited number ... . In a dry season they mainly subsist on ardoo seeds of *Marsilea quadrifolia*, but in a good season they have an ample supply of seeds which they grind and pound and bake in the ashes. They gather also then plenty of plants, herbs and roots';
3. the 'Kurnai' of Gippsland, in southeastern temperate Australia, 'depend upon their success in hunting, or in gathering plants, roots, fruit and seeds for their daily support of food' (Fison and Howitt 1880:208). There were 'grassy forests and plains stocked with kangaroos and other marsupials; forest trees harbouring opossums, native bear [koalas], and iguana [goannas]; rivers and lakes swarming with fish and eels; birds were plentiful in number and variety' and, finally, 'various plants, bushes and trees [which] afforded edible roots, berries and seeds;
4. Dawson (1881:22), recorded the traditions, organisation and beliefs of the Aboriginal people living on the relatively rich soils of western Victoria. While not providing great detail of the subsistence of these people, he places emphasis on animal species, especially shellfish on the coast and the seasonal exploitation of eels, although 'of roots and vegetables they have plenty' (1881:19), adding that 'the southern portions of Australia are remarkably deficient in native fruits' (1881:22);
5. in their work among the 'native tribes of central Australia' Spencer and Gillen (1899:22) paid little attention to the diet of the people other than the rules governing food distribution and proscriptions. They did write, however, that 'perhaps the most standard vegetable diet of the native in this part of the Centre ... is the seed of a species of *Claytonia* which takes the place of the Nardoo (*Marsilea quadrifolia*) which is the staple article of food in the Barcoo district and other parts of the interior of Australia'; and

6. for the people of the Burdekin River region of north-central eastern Queensland, there were 'innumerable articles of food, fish, flesh and fowl' (Curr 1887, 3:27). Further south in mid-eastern Queensland, the 'food of the tribes is very various. Amongst other articles they have emu, kangaroo, wallaby, opposum, snakes and birds: but each season of the year has its particular article of food' (Curr 1887, 2:473). Similarly, the Aboriginal people living in the Halifax Bay area of tropical north Queensland had for food 'besides marsupial game, ...fish, roots and fruits of several kinds. From some of the roots they extract, before they can be eaten, certain poisonous qualities by more than one ingenious process' (1887, 2:427).

Reading these early accounts leaves us with the impression that vegetable food was more important in the arid and semi-arid regions of Australia. It should be noted, however, that at the time ethnographers first began studying the Aboriginal people of southeastern Australia, considerable disruption to traditional life had already occurred following the arrival of European settlers and their stock. This included the displacement of people from their favoured habitats and damage to traditional plant food resources by sheep and cattle; the impact of grazing on the grasslands with their traditional plant foods would have been both rapid and dramatic (N. Scarlett, pers. comm.).

Meggitt's assertion received support from Richard Gould (1982:77) who concluded that the diet of Western Desert Aborigines 'is primarily vegetarian, with the women providing about 95% of the total diet about 90% of the time, mainly in the form of edible plant staples.' This is despite fleshy foods always being preferred over vegetable foods (Gould 1969; F. Walsh pers. comm.). Gould (1969:18) had previously noted that 'for all their talk about this or that kangaroo they once killed, or the pros and cons of a particular spot for hunting, the men contribute relatively little to the subsistence of the group ... one consequence of the preponderance of vegetables over meat foods is a tendency towards an unbalanced diet. There is generally enough to eat, but generally the emphasis is on particular staples, one or two at a time.' It should be noted that here a 'staple' was 'any plant species which singly or in combination with another (all vegetables) accounted for at least 50% of the total diet during the period it was collected and consumed' (Gould 1969:258), whereas later it became 'any food that constituted at least 30% of the total diet by weight at the time it was collected' (Gould 1982:77).


Figure 1. A Wagilak woman collecting *Cycas media* nuts, *ngathu*, an important source of carbohydrate for inland Yolngu. The fresh nuts require intensive processing to remove toxic alkaloids. (N.G. White collection 1983, F33 no. 21.).

What of the situation in the tropical north of Australia? Hart's observations in 1928–29 among the Tiwi of Melville and Bathurst Islands, near Darwin, led him to declare that they 'ate pretty well especially in larger households' but despite terrestrial and marine animals being 'very plentiful', these 'were extras or dividends; the staple every day foods were the vegetable foods gathered day after day in apparently unending quantities by the women' (Hart *et al.* 1988:38). Shortly after this, Thomson, who was the first anthropologist to work with 'traditional' Yolngu people living on the east coast of Arnhem Land, also found that 'contrary to the general idea, the main food supply among Aborigines, except at certain restricted seasons of the year, is not animal but vegetable ... but a comparatively small number which constitute staple foods, are gathered in great bulk' (1949:21). These foods included cycad nuts, '2 or more species of yam' (*Dioscorea* spp.), and water lilies of several species. Little had changed three or four decades later for Yolngu people in the Donydji area (Figure 1). The same point was expressed in a remarkably similar way by W.B. Chaseling, the missionary who established the Yirrkala mission on the Gove Peninsula in 1934, who wrote (1957:42): 'contrary to popular belief, Aboriginal foods are mainly of vegetable origin ... .'

During 1966–67 Peterson conducted anthropological research among a group of Yolngu people living near the Arafura Swamp, in northeastern Arnhem Land. (A number of families in this group are, or were, living at Donydji.) At that time, as is still the case in some remote communities, these Yolngu relied heavily on wild foods for their survival. Peterson concluded that 'at least 60% of the food by bulk is provided by the women, and at certain times of the year this rises to 90% and included animal proteins as well as vegetable foods' (Peterson 1973:22). This was also my experience of the productivity of women in the Donydji area, although, as suggested by Peterson, there are considerable seasonal differences, ranging from about 80% of the food by weight gathered by women to less than 20% during the 'hungry time' at the height of the wet season (White 1985). (The contribution of store purchased carbohydrate foods also varied greatly by season and according to the availability of cash and transport). The men's contribution of meat from large game, although much more prized than vegetable foods and small fish, is less certain. The Donydji study showed that men were successful in their hunting quest a little over 50% of the time on average, while women returned to camp with food on 95% of foraging trips, although this figure has dropped as store foods have become more readily available. (It should be noted that in my experience men quite commonly captured small game and consumed it on site. This was rarely declared back at camp). Apart from a food craving by the individual or band, hunters can be motivated by a desire for recognition as a successful hunter as well as to satisfy kinship obligations, particularly towards actual or potential parents-in-law. It would seem then, that, contrary to the claim by Meehan that it is a myth that women were the main providers of the food, in much of Aboriginal Australia women were the regular providers of the bulk of food eaten by Aboriginal people, although it needs to be considered more carefully in terms of the macro-nutrients obtained over the seasonal cycle, as well as the energy expended in the food quest. As noted by Peterson (1974) this gross quantification ignores the relative nutritional value of the foods provided by the two sexes.

Most subsistence studies of Aboriginal people have been qualitative, relying on inventories of edible plant and animal species, some providing considerable detail of hunting and foraging techniques and food preparation (e.g. Roth 1901; Levitt 1981; Rose 1987). Table 1a summarises Rose's data from Groote Eylandt presented with the inland Yolngu data from Donydji, shown for comparison. Such qualitative studies can be misleading, since they provide no information on the relative contribution of each of these species and food categories to the diet and take no account of their nutritional importance.

Unfortunately, there have been few detailed quantitative studies of Aboriginal diets and none from the arid zone, at least for people who relied largely on hunting and foraging for their subsistence. McArthur (1960) is justifiably seen as a landmark study. It was the first to look at 'food consumption and dietary levels of groups of Aborigines living on naturally occurring foods.' This research was carried out in Arnhem Land during 1948, as part of the American-Australian Scientific Expedition. Table 2 summarises the bush foods consumed at four Arnhem Land localities visited by McArthur, together with an estimate of the mean daily consumption of major nutrients. The dietary variation, by food type, locality and season, is considerable.

The most detailed study of food intake in a traditional-oriented Aboriginal community was that of Meehan and Jones who, for a full year, lived with the Anbarra Gidjingali community of central coastal Arnhem Land (Meehan 1982a, 1982b; Jones 1980) (Figures 2 and 3). The results of their dietary study are summarised in Tables 3 and 4. Taken together with McArthur's observations (Table 5) they are at odds with Meggitt's generalisation. Over a 4 month period, the contribution of animal flesh (mainly shellfish collected by women, and fish) to the diet of about 30 Anbarra people was nearly 50% in terms of energy, and 80% (66% from marine food) as protein by weight. In 1972–73, vegetable foods contributed only to 31% of the total gross weight of all food available to the Anbarra (Table 3). It is also of interest here to note that a similar figure for the contribution of marine protein to the diet was obtained by Collier and Hobson (1987) who analysed human skeletal remains from southern coastal Queensland, using stable-isotype analysis. Jones and Bowler (1980:21), concluded that 'the basic fact that emerges from the studies we have of the Australian tropical savanna, is that they were substantial meat eaters as defined in the broadest sense.' This conclusion

Figure 2. Man the hunter fails again. Rhys Jones attempting to spear fish off Gupanga beach in the late dry season of 1972. (Photograph: Meehan-Jones collection.).

Table 1a. Number of varieties of foodstuffs consumed by Aborigines of Groote Eylandt and the Donydji region, compared with the Dieri of the south central region of Australia (adapted from Rose, 1987).

	Groote Eylandt		Dieri		Donydji region	
	No. of varieties	% of total foodstuffs consumed	No. of varieties	% of total foodstuffs consumed	No. of varieties	% of total foodstuffs consumed
Land animals (incl reptiles)	15	5	40	33	37	14
Birds (land and sea)	76	25	55	45	73	27
Freshwater and marine animals and fish	97	31	3	2	31	11
Shellfish, crabs, etc.	39	13	2	2	3	1
Plants	82	26	22	18	130	47
Total	309	100	122	100	274	100

Table 1b. Details of Donydji region foodstuffs

	No. of varieties	No. actually observed being eaten	Varieties most commonly eaten
Land animals			
Reptiles	17	8	4
Mammals	20	12	4
+ Introduced	3	3	2
Birds	73	18	5
Freshwater Fish and Animals			
Fish	25	13	5
Tortoises	3	3	2
Snakes	3	2	1
Shellfish, crabs and custaceans	3	3	1
Honey	?4	?4	3
Plants			
Roots	46	18	9
Fruits	51	14	6
Seeds	9	7	3

Figure 3. Successful foragers, wet season 1973. Fortunately Betty Meehan did not have to rely on the hunting skills of Rhys Jones. Here she is seen collecting the bivalve *diyama* (*Tapes hiantina*) with Anbarra women on the shell fish beds located adjacent to a wet season coastal home base of Lalarr Gujirrapa. (Photograph: Meehan-Jones collection.).

Table 2. Summary of foods consumed at the four Arnhem Land locations visited by McArthur in 1948 (adapted from McArthur, 1960).

Foods (weight in pounds)	**Hemple Bay 4 days; 13 people early May 1948**	**Bickerton Island 3 days; 15 people May 1948**	**Port Bradshaw 4 days; 20 people July-August 1948**	**Fish Creek 11 days; 9 people mid Oct. 1948**
Vegetable foods				
Fruit	3		1	7
Roots, etc.	87	76.5	92.5	28
Honey		3.5		
Animal foods				
Fish	137	46.5	120	116.5
Turtle	75			
Turtle eggs			7.5	
Shellfish			33.5	
Crabs			4	
Reptiles			8	
Macropods			16	390
Offal	3.5	0.5	5	16.5
Total	**305.5 lb**	**127 lb**	**287.5 lb**	**565.5 lb**
% Vegetable	**30**	**63**	**33**	**7.5**

Mean Daily Consumption as Percentage of Recommended Dietary Allowances

Camp	Calories	Protein	Iron	Calcium	
Ascorbic acid					
Hemple Bay	116	444	80	128	394
Bickerton Island	74	172	135	41	234
Port Bradshaw	79	300	131	490	220
Fish Creek	104	544	33	355	47

Table 3. Contribution made by vegetable foods to the Anbarra diet (based on Meehan 1982a).

Vegetable food contribution	%
Gross weight (kg)	31
Weight protein (kg)	20
Energy equivalent (k'cal)	57

Table 4. Vegetable foods eaten by the Anbarra Community 1972–73 (based on Meehand 1982b)

	Percentage total food eaten
Traditional	
Gross wt (kg)	37.4%
Energy (k'cal)	10.9%
European	
Gross wt (kg)	62.6%
Energy (k'cal)	89.1%

Table 5. Relative gross weight of meats and vegetable foods hunted and gathered by some Arnhem Land groups (adapted from Jones 1980 based on Meehan 1975, 1977, and McArthur 1960).

Consumption/day	Anbarra Gidjingali			Hemple Bay (Groote Eylandt)	Port Bradshaw	Fish Creek
		1972–1973				
	Early Wet	Late Dry	Dry	May 1948 (Early Dry)	Dry 1948	October 1948 (Late Dry)
Consumption/day						
Animal flesh/head (kg)	0.61	0.61	0.75	1.60	0.91	1.77
Vegetable food/head (kg)	0.31	0.65	0.24	0.78	0.52	0.19
Protein/head(kg)						
(animal& vegetable)	0.14	0.16	0.17	0.23	0.15	0.30
k'cal/head	1620	2400	2090	2160	1380	2130

would seem to rest on the observations of the coastal Anbarra. For the Yolngu people living in the hinterland to the southeast, the contribution of fleshy foods to the diet in terms of bulk, was substantially less over most of the year, varying from about 25% by weight to about 70% in the wet season (White 1985). According to elderly Yolngu women, the wet season was a 'hungry' time since they were largely dependent on the hunting success of the men. Elsewhere, Jones (1980:136) postulated that 'perhaps only in the grassy areas of the central deserts could there have been a tendency for a vegetable based diet … . Certainly farther south in Tasmania the Aborigines ate proportionally even more meat than did the Arnhemlanders.' The importance of meat to the diet was also claimed for Tasmanians in the late Pleistocene (Cosgrove et al. 1990), although some of the dietary conclusions drawn from the archaeological evidence are contentious. The archaeological study of early diets is, after all, 'a little like navigating in the vicinity of an iceberg: more than four-fifths of what is of interest is not visible' (Isaac 1971:280).

Despite the undoubted quality of the field research of Meehan and Jones with the Anbarra, I do not think the available evidence such as that cited here, justifies Jones' (1980:136) generalisation that the Aboriginal people of the tropical savanna consumed a diet in which 'some two thirds of the gross weight of food was in the form of meat of all sorts, and that even in calorific terms possibly half of the food came from meat'. This is not to deny that for *some coastal communities* such as the Anbarra, the amount of animal flesh including the shellfish collected by women may indeed have formed the bulk of the diet for a good part of the seasonal cycle. Bernhard Schebeck (pers. comm.) draws attention to the use of words for vegetable foods, rather than those for meat or game, to mean food in the wider sense. For example, in Yolngu, *ngatha* ('vegetable foods') is used for food in general rather than *wayin* ('game'), or *dhaanggu/warrakan* ('meat'). Similarly, *mai*, which means vegetable food in Yura from the Flinders Ranges, South Australia, is used there to mean food in general. I do not think this is a coincidence. In my experience, the amount of meat that people desire is not always matched by the amount that they actually eat. Similarly, the emphasis given to hunting, especially big

Figure 4. Clubbing an Agile Wallaby (*Macropus agilis*), *barranggal*, which had been wounded by spear during a late dry season *Wurrk*, or fire-assisted communal hunt.
(N.G. White collection 1990, F9 no. 35.).

Figure 5. A Ritharrngu hunter at *Wurrk*: lighting a late dry season fire as part of a communal hunt, Mitchell Ranges, northeast Arnhem Land.
(N.G. White collection 1981, 73620.).

game hunting, in general conversation among men, and the significance of kill sites in their personal histories give the impression of a society of meat eaters; however this is not confirmed by observation.

More recently, Cosgrove and Allen (this volume) provide a convincing case for regular (probably seasonal) targetting of Bennett's wallaby (*Macropus rufogriseus rufogriseus*) by hunters in southwest Tasmania between 35,000 BP and 14,000 BP, principally during the periods 24–23,000 BP and 16–14,000 BP. The evidence which they present for the selective use of body parts and the scenario of co-operative seasonal hunting is strikingly reminiscent of the late Dry Season hunting of agile wallabies (*Macropus agilis*) which I have observed in the Donydji region. Here groups of related Yolngu men use hunting fires, or *wurrk*, to drive these and other prey animals into river junctions, defiles or onto stony rises (Figures 4, 5 and 6). These hunting sites were deliberately preserved for late season fires, unlike other parts of the landscape which were subject to early, 'cool', patch burns. The macropods were carefully butchered, with certain portions eaten by the hunters on site, while other body parts were selectively distributed to their kin back at the base camp (White 1985). Cosgrove and Allen argue that their data point to substantial differences in the quantity and quality, and hence nutritional value, of wild flesh and plant foods over the seasonal cycle. This seems to have been characteristic of the diets of Aboriginal people throughout Australia in the past as it is in some parts of remote Australia today.

Although the studies of McArthur, as well as Meehan and Jones are exceptional, both in terms of the quality of the data and their implications for our understanding of traditionally based Aboriginal subsistence, there are some methodological limitations that need to be borne in mind when interpreting their results. For example, in McArthur's study:

1. there were sometimes problems obtaining satisfactory interpreters;
2. visited groups were those which could be reached by the transport available. All had access to store foods or were returning to their traditional lands after long periods on the missions or settlement. People were told not to eat store foods during the study periods, even though, in the case of Fish Creek, some of the people had gone to Oenpelli Mission to obtain flour and sugar 'because they were sick of meat';
3. all people living nearby gathered at the site that was chosen for the survey;
4. the composition of the groups differed; at Fish Creek there was a preponderance of males, all of whom were excellent hunters, while at Port Bradshaw there were 11 children;
5. the groups were studied over short periods (3–11 days) at different times of the seasonal cycle. The Fish Creek study took place in the late Dry Season (the worst time for vegetable foods) while

the people at Hemple Bay were visited in the early Dry (the best time for edible plants). Furthermore, the habitats in which the groups hunted and foraged were very different, and,

6. McArthur accompanied hunting and foraging groups where possible, but at other times she had to question people about what they had eaten. From this she was forced to 'simply allow' for foods eaten. My own field experience has shown that the dietary recall method is highly unreliable in this situation.

Figure 6. A party of *Ritharrngu* hunters in the Mitchell Ranges with a bustard (*Ardeotis australis*), *walpurrunggu*, speared during a late dry season *Wurrk*. (N.G. White collection 1981, 73618.).

The Anbarra people, who were the focus of Meehan's and Jones's study (1972–73), had access to store foods, especially flour and sugar. These 'European' foods accounted for about 60% of the gross weight of all the vegetable foods eaten, providing nearly 90% of the energy over the study period (Table 4). In addition, the hunting success of men may well have been substantially increased by the use of fence wire tips for fish spears and a 'few' 12-gauge shotguns.

REGIONAL DIFFERENCES IN THE TYPES OF PLANT FOODS EATEN

The ethnographic studies discussed above, a number of which are summarised for edible plant species in Table 6, highlight important differences in the types of vegetable foods consumed by Aboriginal people living in the arid and semi-arid parts of Australia, compared with those living in the northern tropical regions. As far as we can ascertain from the early accounts, the southeastern temperate region of the continent was similarly distinguishable from the more arid parts of the continent. There is an emphasis on roots on the coast, and seeds requiring pounding or grinding, in the interior. This form of plant processing 'has been around for about 30,000 years' according to Fullagar and Field (1997:306) from their research at Cuddie Springs in north-central NSW, although it may have been far more recent in the Western Desert region as the Aboriginal occupants 'focussed on the abundant seed species of the hummock grasslands' (Veth 1989:90).

Table 6. Comparison of edible plant species from Central and northern Australia (based on Cleland 1966, Cleland and Johnston 1939, Cleland and Tindale 1959, Crawford 1982, Gason in Smyth 1878, Levitt 1981, Meggit 1962, 1964, Rose 1987, Smyth 1878, Specht 1958, Veth and Walsh 1988, Worsley 1961 and White unpubl. for the Yolngu).

Locality (north coastal to central & southern Australia)	Total edible species/ "varieties" n (%)	Roots, Corms & Tubers n (%)	Fruits n (%)	Seeds & Nuts n (%)	Stems & Leaves n (%)	Miscellaneous [exudates, gum nectar, galls, honey] n (%)
"Coast"	152 (100)	41 (27)	68 (45)	12 (8)		31 (20)
Groote Eylandt	86 (100)	22 (26)	45 (52)	9 (11)	5 (6)	5 (6)
1. Donydji area	127 (100)	46 (36)	51 (40)	9 (7)	5 (4)	16 (13)
2. Yolngu pooled	183 (100)	65 (36)	85 (46)	11 (6)	5 (3)	17 (9)
Northern Qld	51 (100)	15 (29)	26 (51)	9 (18)		1 (2)
Northern Kimberely	101 (100)	48 (47.5)	36 (35.5)	13 (13)		4 (4)
"Inland"	91 (100)	10 (11)	32 (35)	34 (37)		15 (17)
Dieri	22 (100)	3 (14)	5 (23)	11 (50)	2 (9)	1 (4)
Western Desert (Martudjarra)	89 (100)	5 (6)	12 (14)	43 (48)		23 (26)
Arid South & Central Aust.	92 (100)	10 (11)	21 (23)	37 (40)	15 (16)	9 (10)

Golson (1971:205) noted that 'of 45 species used for seed in Central Australia, 14 are grasses and 19 are acacias [while] Arnhem Land has 40 species in 9 of the 10 genera of grasses that supplied seed food in Central Australia ... [but] not one is recorded as a food. [Similarly] Arnhem Land has 36 species of Acacia', (2 identical with Central Australian food species), but none was used as food. Golson also drew attention to the large number of food plants preponderantly from Malaysian genera, particularly near the northern coasts. These regional differences in the types of vegetable foods consumed have nutritional implications. Seeds, for example, are the most energy-rich and protein-rich plant foods, while, fruits are important sources of essential micronutrients, especially vitamin C and carotenoids (Southgate 1991; Brand Miller *et al* 1993). While tubers and some roots contain significant amounts of starch some contain toxic and/or unpalatable compounds that require treatment before they are consumed. This would suggest that more roots and tubers by weight are needed to satisfy energy and protein requirements than seeds and nuts. While the data on the composition of Aboriginal foods are informative, consideration must also be given to the spatial and seasonal availability of the different food types and species, as well as to the different costs in time and energy required in their collection and processing.

Variation in the quantity and quality of food resources throughout the year is a feature of the Aboriginal economy in both the arid and tropical zones (e.g. White 1985:fig. 3, for the inland Yolngu; Tonkinson 1991:fig 2.2, for the Mardu of the Western Desert). In the central arid region of the continent where rainfall is sporadic and localised, there is good evidence to suggest that drought and famine had a considerable impact on the Aboriginal people living in the area with, perhaps, '10% of the population perishing every two generations' (Kimber 1990:162). However, this does not appear to have been the case for other areas. While seasonality is particularly marked in the tropical woodlands and coastal areas of northern Australia, the resources are far more predictable in time and space than in the arid zones. Compared with parts of the arid interior of the continent, for much of the year food was considerably more abundant and more easily obtained, with the outlay of less time and effort, especially on the coast (McArthur 1960; White 1985). This apparent ease with which the material needs of Aboriginal people were satisfied was used by Sahlins (1972) in declaring that hunter-gatherers represent the 'original affluent society'. I say apparent, since there would, no doubt, have been occasions when greater time and effort were required to obtain sufficient food, and the existence of 'emergency' (or 'children's') foods suggests that the people of the Top-End did experience times of hardship. For instance, in the Donydji region, of the 253 plant and animal species said to be edible, only about 100 (40%) were actually observed being eaten in nearly two and a half years of fieldwork, with just 17% commonly eaten (Table 1b). That is, almost 60% of plant and animal species known to be edible appear to have sustained these Yolngu people only when their preferred foods were unavailable. For these inland Yolngu the 'hungry' times were the heavy Wet Season and the late Dry. A number of older women complained that they were often hungry during the rainy times because they had to rely on the men for their food (White 1985). On the coast there was less hardship since the Aboriginal people could collect shellfish and other marine resources (Meehan 1977a, 1977b, 1982a). These data also underline the range of choice available to the Aboriginal people in their food quest.

Apart from variation in diet by locality and by season, and indeed, day-to-day, there are considerable differences among individuals as a result of customary rules governing the distribution of food, particularly meat, fat and offal. These tend to favour older men at the expense of older women and children. In addition, food proscriptions ('taboos') applied at different times to varying degrees to all Aboriginal people during their lifetime (White 1985), although, with the possible exception of pregnant and lactating women, food taboos were probably of little nutritional significance.

ABORIGINAL VIEWS ON DIET AND HEALTH

We have noted elsewhere (Scarlett *et al.* 1982:166), that: 'sorcery and supernatural agents form an important, possibly the most important component of the Yolngu view of illness and death', although there is a recognition of the relationship between physical cause and biological consequences in the area of health and well-being. Examples of the perceived link between diet and health drawn from the Donydji study include: coughs and 'lung complaints' attributed to being 'slack inside' as a consequence of a diet deficient in meat; diarrhoea said to be caused by vegetable food that is 'too dry'; cycad bread made from old nuts; yams (*Dioscorea* spp.) eaten at the wrong time of the year, and headaches resulting from 'dry' food or excessive exposure to the sun. A Ritharrngu-speaking Yolngu man living at Donydji explained to me that if you 'want to feel good *(djaal-ngamadhirri)* you should eat a mixed diet (*miil manabarn*), a diet of meat and vegetable foods'.

Peterson (1973:184), makes a similar point: the Yolngu people in the vicinity of the Arafura swamp 'place a high value on a mixed diet containing both meat and vegetable food. To eat such a mixed diet, that is to eat well, is *dakarait'yun*. [Whereas] a diet of vegetable without meat (*gulu'yun*) for more than a day or two' leads to complaints from the old people; 'they say they want something to "relieve their tongues" (*matayal*)'. If, on the other hand, the diet is entirely of meat, 'the people complain that their head becomes heavy (*bukumuktun*)'. (Zorc 1986 offers for *dakarait'yun* the word *dhaakardatjun* 'eat vegetarian in contrast to meat; eaten for variety'; and for *gulu'yun* he offers *gurlurl'yun* 'vegetarian'. Schebeck (pers. comm.), suggests *matayal* derives from *matha yalyun* 'tongue to cool down'.) Recently, in pursuing the meaning of these terms with a middle-aged Ritharrngu man, great emphasis was again given to eating both vegetables and meat in order to have a healthy or 'happy' diet (Fieldnote book 1/1, March 2000). This is illustrated by his use of *dhaakardatjun*: *"nhakana wayin luki - ngatha gaana; ngaathili wayin ... ga gapu"*; literally, 'like eating meat - vegetable food separately, first meat ... and water'. That is, for a satisfying diet you need meat, vegetables and water. Another Ritharrngu man indicated that people get sick of eating *ngatha* ('vegetable food'), that is, they feel nauseous (*munyanggun*), and crave other foods, especially meat or fish, with fat. When asked to give the meaning of *gurlurl'yun* , the first man replied "then "*yalukmirri ngay*" then ('he hungry') ... too much bush tucker - *gugu* (wild honey) and *ngatha* (vegetable food) makes him sick". Here the meaning also seems to be that you would become unwell if you ate only vegetables and honey without meat. In both cases meat is emphasised. My experience of Yolngu living in small bush camps suggests a continual cycle of food 'crises', at least as perceived by the people — almost always men — based on food cravings. Here is the imperative for a mixed diet over the course of days and weeks if not meal to meal; this in turn leads to the shifting of camp, that is, to 'nomadism'. In other words, moving from place to place is not simply dictated by seasonal and other environmental constraints, it is also driven by a desire for variety in the diet.

Similarly, Meehan (1982:115) describes how the Anbarra:

> "prefer a diet that contains ample quantities of all the different food categories. If forced to eat white meat for too many days, they complain ... and want red meat. If they have lots of flour, yams or spike nuts they say how much they wish for ... any kind of white or red animal flesh. .... Sometimes they crave for [vegetable foods] especially during the wet season when traditional and European forms are in short supply and they eat fish and crabs constantly. On a more subtle level, people sometimes say they want to have shellfish because its flesh is 'wet' ...; they are tired of eating flesh that is 'dry' (i.e. usually fish and vegetable substance)."

This emphasis on a variety of food stuffs in the diet makes physiological sense. Effective food strategies in selecting a mixture of plant and animal foods would ensure, for instance, that the biological quality of the dietary protein would be satisfactory and also enhance the supply of inorganic nutrients which would be only marginally adequate from plant foods alone (Southgate 1991:287). To satisfy energy requirements, the most effective food selection in terms of amounts eaten requires a proportion of animal foods, especially those that contain fat. 'These have several advantages; firstly, in the amounts required; secondly, as sources of substantial amounts of protein of high biological value, free from natural toxicants; thirdly, as sources of essential inorganic nutrients; fourthly, as sources of the B-vitamins, especially B12 and, very importantly, as a source of vitamin A and the other fat-soluble vitamins'. Meat and fish would also 'provide long-chain polyunsaturated fatty acids of the n-3 series' (Southgate 1991:287).

PERSPECTIVES ON FAT

'Every drop [of fat] was treasured' (Smyth 1878, 2:306)

Fat, in particular, is a much sought after food which is differentially incorporated into the diets of men and women, and among the young and old, according to customary rules of sharing and avoidance (Figure 7). This reflects the place of fat in the indigenous belief system which finds expression in the language. Schebeck (pers. comm.) noted that in the Yolngu dialects of the Gove area, *djana'-mirri*, literally 'having fat', is used to mean also 'beautiful' or 'cute'. For example, somebody may be said to be *djana'mirri buku*, that is, to have a nice/beautiful face.

Figure 7. 'Every drop of fat was treasured.' An unwrapped bundle of golden emu fat (*Wurrparn djukurr*), which was distributed by the hunter to selected kin at Donydji, according to custom. (N.G. White collection 1983, F25 no. 3.)

'To the Yolngu [then,] fat is more than a rich source of energy; it can be endowed by '*marr*' or spiritual power - sometimes a positive force associated with happiness, strength, health and fertility. At other times, for certain people, it can have a dangerous dimension (Thomson 1975). In Yolngu religious art, animal fats are said to have properties of shininess and brightness (Morphy 1989)' (White 1990:225). Jones and White (1988:83) linked the concept of *marr* to the highly regarded stone spear heads (*ngambi*) from the Ngilipitji quarry in northeastern Arnhem Land: 'Thomson (1975) [drew attention to the] need to distribute in small bark bundles, the fat (*djukurr*) from certain species said to be endowed with *marr* "lest the owner" become contaminated by the spiritual power which had accumulated. It is interesting to speculate that the special *djukurr* [the shiny, grey-pink quartzite] from deep within the stone of Ngilipitji was regarded in the same way'.

The cultural value accorded animal fats by Aboriginal people throughout Australia has been noted by a number of researchers, including Meehan (1982b), for the Anbarra, and Devitt (1990), for the people of the Sandover River region of Central Australia, who writes (1990:226) that, like the Yolngu, 'their view is grounded in traditionally-derived notions linking animal fat with high-quality food, abundance and well-being - associations [that] are complex, multidimensional and often symbolic'. Contemporary dietary practices reflect the continuing influence of the high value associated with fat. A consequence of this is the incorporation of new and different fats into the diet.

In terms of nutrition, especially energy requirements, it was noted above that the selection of food providing fat had substantial advantages in reducing the amount of plant foods to be gathered. Furthermore, satiety is 'strongly influenced by the fat content of a meal, and thus a learned association of fatty foods in delaying the need to seek more foods is possible' (Rogers and Blundell 1990, cited in Southgate 1991:285).

The desire for fat may be *one* of the contributing factors to the increasing obesity in Aboriginal communities undergoing transition from a substantial dependence on hunting and foraging to a westernised lifestyle, with fatty foods accessible year round. I say may be, because at Donydji, where store-purchased foods are now consumed on a more regular basis and in increasing amounts, introduced fatty foods such as tinned meat do not figure prominently in the diet. Priorities are given to refined carbohydrates such as white flour and bread, sugar and sweet beverages. There has, however, been an increase in recent years in the number of introduced wild cattle and buffalo being captured, largely because of greater access to guns and off-road vehicles. In these feral animals there is a larger amount of depot fat than in most native animals, although the saturated fat component is less than in their domesticated state. In my experience, while these people, and especially the men, have access to more captured fat, they still prefer the fat from native animals such as wallabies, kangaroos and emus. All of these species have a substantially lower fat content and a higher proportion of polyunsaturated fatty acids than do introduced animals (Naughton *et al.* 1986).

CHANGING DIETS

The changes to subsistence behaviour and diet that have taken place throughout Aboriginal Australia following European colonisation, have occurred in a particularly dramatic way over recent years in the more remote communities of Central Australia and Arnhem Land. O'Connell and Hawkes (1984:509), working with the Alyawara (ca. 250 km northeast of Alice Springs) in 1974–75, observed that over a 260 day study period 'even the most active collectors went out only once or twice a week, and their take represented no more than 5% of their total diet'. The situation for the Mardu people of the Western Desert is the same, according to Fiona Walsh (pers. comm.). In addition, she noted that only ca. 40% of plant foods and less than 30% of animal species that were said to have been used at the time of European contact, were being used today. The changing role of women in the food economy, was also noted by Altman (1987), who lived with an outstation community of Gunwinggu in Central Arnhem Land. There, 46% of total kilocalories and 81% of total protein came from bush foods, with almost 90% of both contributed by males through hunting and fishing. The situation is similar in the Homeland Centre of Donydji. Access to off-road motor vehicles has led not only to a higher intake of meat and fat, but has also resulted in a growing dependence on store foods (White 1985).

As a consequence of these dietary and lifestyle changes, particularly the shift towards refined carbohydrates and fats, and a decrease in physical activity, there has been for many indigenous communities, an increase in lifestyle-related disorders, such as obesity, NIDDM (Non-Insulin Dependent, or type 2, Diabetes Mellitus), cardiovascular disease and nutritional deficiencies. There is now strong evidence that at least 'some of the major chronic diseases of Industrialised society are related to the typical Western diet' (Eaton and Konner 1985:288)

T.R. Guyula, is a health worker from Gapuwiyak near Donydji. He was raised in seasonal bush camps in the Arafura Swamp area, and when in his Homeland Centre still depends to a considerable extent on the bush and its resources for subsistence. TRG expressed his concerns about the impact of dietary and lifestyle changes on the health of his community in the following way. (This is part of his 'story' which was recorded on audio-cassette and given to me at Donydji on June 2, 1993.):

> '... Bush food made people feel good and look well. ... Now things are changing, now people go to shops and eat food that's got a lot of sugar in it — lots of greasy food. In the large settlements where there are shops people have lots of sores and are getting fat because they're using a lot of shop food. ... My father said that before the balanda [white people] came to this country, people looked healthy because they didn't have a shop and they only ate bush foods. Now that the white people have come the people are changing their bodies. ... Now some people are diabetics which is no good. That's the story I want to tell. ... We've got to start teaching our culture to those Yolngu; teaching

people how to live off the land once again. We need programmes to learn more from older people like my family at Mirrngadja and Donydji. It will be good for diabetics to eat bush food without fat and grease and sugar. We've got to teach the kids the ways of the old people: show them how to be healthy like our mothers, fathers, grandmothers and grandfathers. They've got to be taught not to eat too much shop food. ... I'm always happy to be back with my family eating bush food and getting exercise when I'm hunting. With this life they don't get any diabetic sickness and that's really good. I'd like to learn more about diet and sugar sickness. ... I've got to learn more from her [his mother] and we've got to talk more about how things are changing — look at what happened before and what's happening now, what was good for our body in the past and what's causing sickness today. ...'

NIDDM and cardiovascular disease are not yet problems for the Donydji community although according to O'Dea et al. (1988:177), 'their fasting insulin and triglyceride levels were inappropriately high for their very low body mass index and fasting glucose levels. The mild elevation of triglycerides and fasting insulin levels is consistent with insulin resistance and suggests that these Aborigines (in common with other Aborigines) may become susceptible to obesity and diabetes if they urbanised further.' The risks of these lifestyle diseases is greater in Aboriginal women than for women of European descent because of their android pattern of fat distribution (Rutishauser and MacKay 1986; Jones and White 1994). Such population-specific differences in fat patterning may, in part, be the result of biological adaptation to climate. 'If women with a central distribution of fat are more efficient at laying down and metabolising their fat deposits, then such a pattern in Aboriginal women living in tropical environments may well have conferred an advantage in thermoregulation, metabolic efficiency and reproduction. It may also help account for the rapid increase in adiposity that occurs in Australian Aboriginal people when they adopt a Westernised diet and sedentary lifestyle' (Jones and White 1994:222).

While on the subject of the biology of Aboriginal people, it must be noted that there are substantial genetic differences within and between Aboriginal populations (e.g. White 1997). Strong divisions exist at the population genetic level between groups living in northern parts of Australia and the people of the arid centre of Australia. Given the duration of Aboriginal occupation of this country, and the important differences in the nutritional environment across Australia, it would be surprising if genetically determined dietary adaptations had not evolved at the regional level. However, despite the very different selective regimes, the susceptibility to NIDDM of Aboriginal people of full descent across the different environments appears to be similar. A closer inspection of the dietary data and subsistence behaviour available for Aboriginal Australia appears to challenge the feast and famine hypothesis proposed for NIDDM (Neel 1963) in populations such as indigenous Australians. The impact of famine, rather than food shortages, was almost certainly greatest on those populations living in the arid central parts of the continent, being far less common, if it occurred at all, in the tropical coastal regions of Australia which were colonised earlier than Central Australia, perhaps by some thousands of years. It is generally accepted that the ancestors of Aboriginal Australians came from southeast Asia, having presumably adapted to tropical lowland and coastal environments where cycles of feast and famine would have been infrequent or less pronounced than in drier inland areas. Despite their different evolutionary histories, Aboriginal people in all regions of Australia who have adopted a 'western' lifestyle exhibit similar levels of NIDDM. The metabolic characteristics predisposing Aboriginal people to this form of diabetes 'may have been important to survival in the traditional lifestyle by favouring efficient fat deposition - the 'thrifty gene' - on a diet in which the carbohydrate content was seasonally variable' (O'Dea and Spargo 1982:498). An alternative hypothesis (Brand Miller and Colagiuri 1994), sees insulin resistance as an adaptation to diets high in protein and low in available carbohydrate, that is, foods with a low glycaemic index, such as wild plants which contain carbohydrate in a form which is slowly digested and absorbed.

CONCLUSION

Detailed quantitative accounts of the diets of traditionally orientated Aboriginal people are few, and none is available for groups residing in the arid central parts of Australia. The information that we do have for Australia as a whole, based as it is on impressionistic accounts of early European settlers and ethnographers, and on recent descriptions by anthropologists, many of whom spent considerable time with the Aboriginal people, suggest that there was no typical Australian hunter-gatherer diet. Important regional differences existed in the types of food available to the Aboriginal people. The seeds of grasses and Acacia formed a substantial part of the diet of desert Aborigines, while roots and fruits (at least the number of fruit-bearing species) contributed far more to the food intake away from the arid zone, especially in the northern coastal regions. The extent to which vegetable foods dominate the diets of Aboriginal people is debatable, although it does seem clear that animal flesh contributed far more to the diet of tropical groups than was asserted by Meggitt (1964). This was particularly so on the coast, where shellfish, most of which were collected by women, and fish, formed a major part of the diet. Apart from regional differences in diet, there were very considerable seasonal differences in the quality and quantity of food consumed in all regions, as well as differences among individuals according to gender, age and status. Such dietary variation recorded among hunting and foraging populations in Australia and beyond, calls into question the notion of a (or the) traditional diet, and the value of an average, mean or mode as used by Eaton and Konner (1985) to generalise pre-industrial or 'paleolithic' diets as a basis for current dietary recommendations.

Obviously, an adequate diet can be obtained in many different ways. Southgate (1991:281), in his discussion of the 'natural' diet of humans, believes that 'this characteristic of extreme fluctuations in the amounts and types of foods available for consumption is an important one ... and that the control of appetite and the choice of food evolved under conditions of alternating abundance and scarcity ...'. Introduced foods will have influenced the choice and amounts of wild foods that are hunted and gathered as it will have altered the appetite to varying degrees. For instance, at Donydji the introduction of sugar, almost invariably consumed as very sweet, black tea, is said by the Yolngu to quickly satiate their hunger and make them happy.

Throughout Aboriginal Australia today, dramatic dietary and lifestyle changes have occurred and are still occurring. In remote, traditionally-orientated communities it is the contribution of the women to subsistence that has changed most of all, with wild vegetable foods no longer providing the quantity and quality of nourishment that they did in the past. In the place of these foraged resources are the refined carbohydrate, salt and saturated fat of purchased food. An awareness of food beliefs and habits is crucial, not only for an understanding of Aboriginal diet and nutritional status both in the past and today, but also for the provision of effective health management programs for Aboriginal communities. Gould (1969), commenting on the lack of dietary information for desert Aboriginal groups, acknowledged that the time has probably passed when anthropologists can hope to carry out the kind of empirical quantitative studies of Aboriginal subsistence which have been carried out in other parts of the world, most notably among the Kalahari San. To quote from Gould (1969:273): 'It will be the sad task of historians of anthropology to ask how it was that anthropologists in Australia [with a few exceptions] failed to get this kind of information before it was too late. Anthropologists should use every means at their disposal to salvage whatever information can be gained about Aboriginal subsistence'. Rhys Jones and Betty Meehan are prominent among these exceptions, having contributed substantially to our knowledge of the subsistence behaviour and diet of Aboriginal hunters and foragers, particularly those living in tropical northern Australia.

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Seasonal Movement in the Prehistoric Human Ecology of the Alligator Rivers Region, North Australia

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IN 1969 Carmel Schrire (then White) and Nicolas Peterson published their landmark paper (White and Peterson 1969) presenting a model of seasonal movement and resource usage in late prehistoric western Arnhem Land. Their analysis placed archaeological evidence from local sites, itself constructed within a context of regional ethnohistorical data and comparative anthropological insights, into the human ecology of a wider region of north Australia. This allowed them to make sense of differences in Schrire's excavated tool assemblages by proposing an adaptive strategy of seasonal migration between two physiographic zones, each allowing access to a different localised resource base. Their interdisciplinary, regional and comparative approach thus enabled Schrire and Peterson to escape the interpretive confines of having to understand a site only in terms of itself, and thereby to develop a more dynamic and extensive notion of Holocene hunter-gatherer adaptation in a north Australian environment.

This paper reconsiders the model of seasonal migration more than three decades after the original paper. Like the original, it is multi-disciplinary. It combines the findings of archaeological research on the South Alligator River floodplains (SB), archaeobotanical research from sites in two other physiographic zones (AC), and regional ethnohistorical research and a critical analysis of Schrire and Peterson's argument (RL), and draws also on a substantial amount of other research carried out in the region since 1969. Much of this new work, in particular the archaeological research of Brockwell and Clarke, occurred as part of the major 1981 Australian National University research project led by Rhys Jones, investigating the archaeology of the then recently-declared Stage I of Kakadu National Park.

The Kakadu Archaeological Project spanned a period of seven years from the initial consultancy in 1979 (Golson et al. 1980) followed by the long fieldwork season in 1981, through extensive laboratory work in 1982 and 1983, to the publication of the results in 1985 (Jones 1985a). In all phases the Project drew together a large, multi-disciplinary collection of researchers; some such as Jones with a long history and experience of archaeological research in northern Australia and others including Brockwell and Clarke who were embarking on their first forays into the region. Levitus, who was conducting an independent research project with local Aboriginal people looking at their involvement in the buffalo shooting industry (Levitus 1982), arrived in the Park on the same day as the archaeological team. This paper, written in honour of Rhys Jones' contribution to the discipline of archaeology in Australia, is testament to the enduring personal and intellectual connections created by the research carried out in Kakadu National Park in the early 1980s.

The argument here proceeds in four stages, developing the seasonality argument and considering both its explanatory power and logical limitations, over areas that are successively larger and more ecologically and culturally varied. After reviewing the initial model of seasonal movement, we consider the findings of further research that has been conducted across the particular area in which Schrire's sites are located. In the subsequent sections, we then consider the implications for the seasonality model of expanding the focus to the wider region, thereby removing two of the limiting conditions that framed Schrire's and Peterson's analysis: the compression of ecological zones into a small transect of country, and the artificiality of the upland boundaries of their proposed migratory range. While our argument both criticises and contextualises Schrire's and Peterson's analysis, we preserve their central insight, that inter-site artefact variations do not attest unilaterally to corresponding cultural differences. The geographically expanded frame does, however, lead us to propose in the final section a new logic of seasonal migration in which resource exploitation is itself contextualised alongside other motivations within a more general cultural system.

THE STUDY AREA

The Alligator Rivers region is located approximately 200 km east of Darwin in the Top End of the Northern Territory of Australia (Fig. 1). It consists of the catchment areas of the East, South and West Alligator Rivers and is bounded in the north by Van Diemen Gulf. Moving generally south and east from the coast, six broad topographic systems are encountered: tidal flats, broad floodplains and wetlands, forested undulating lowlands, a long sandstone escarpment and its outliers, the Arnhem Land plateau, and the southern hills and basins (ANPWS 1986:5–9) (Fig. 2). Our argument extends the seasonality model to all except the first and, in large part, the last zone.

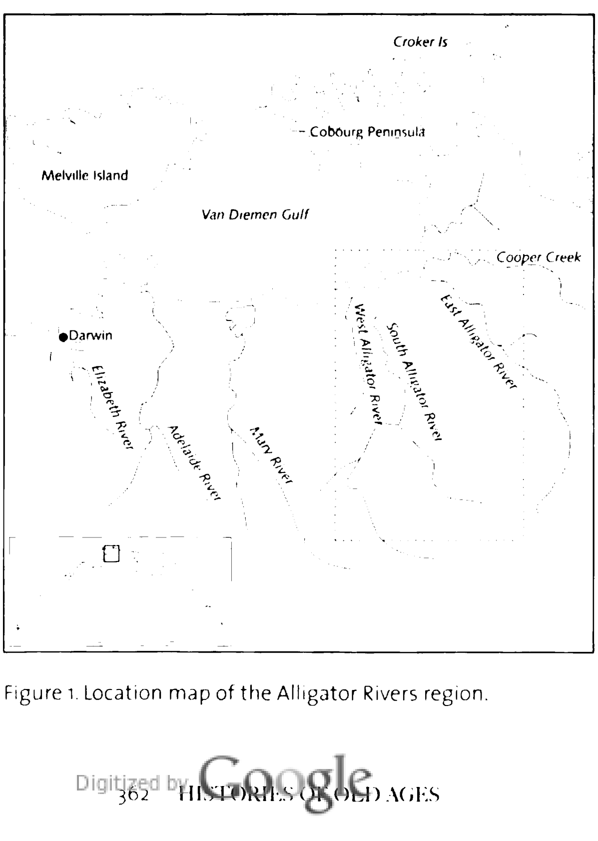

Figure 1. Location map of the Alligator Rivers region.

The region has a high biodiversity. There is a wide range of plant communities, including coastal mangroves, monsoon rainforests, sandstone escarpment communities, swamp paperbarks and grasses and eucalypt woodlands (ANPWS 1986:7–12). It is also an area known to contain one of the most abundant and diverse faunal populations in Australia (Fox et al. 1977:52). An estimated one third of Australia's bird species and one quarter of known Australian freshwater fish species have been recorded in the region (ANPWS 1986:40; Christian and Aldrick 1977:49).

The climate is characterised by consistently high daytime temperatures, but large seasonal changes in humidity. The year is broadly divided into two major seasons, from which the area in general is characterised as being within the wet/dry tropics. Aborigines of the region, on the other hand, distinguish six seasons in the year, according to changes in atmospheric conditions, floristic changes and faunal behaviour (Morris (1996:6, 14). The wet season is brought by monsoonal winds from the northwest in December and lasts

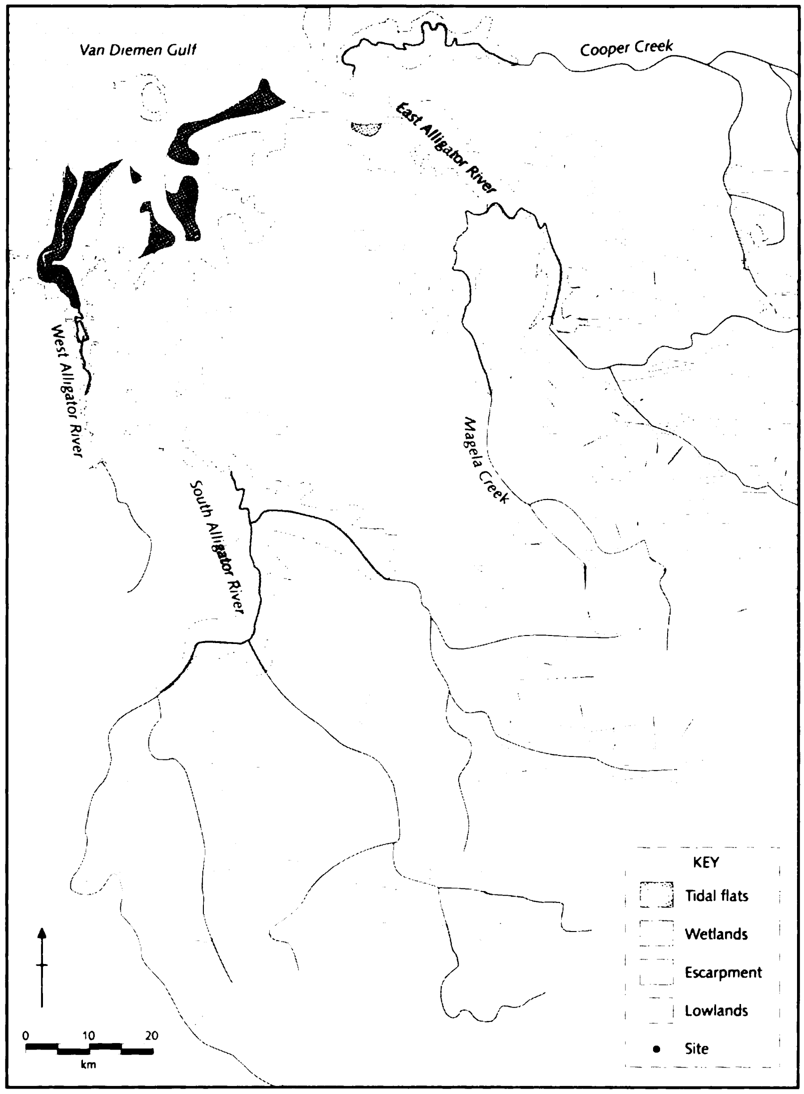

Figure 2. Physiographic zones in the Alligator Rivers region.

until about March. The almost rainless dry season is brought by southeast trade winds around April and lasts almost to the end of the year. This annual cycle is reliable but variable. The commencement and duration of the wet season changes, and inland areas receive less rainfall than coastal areas, with consequent changes in vegetation (Christian and Stewart 1953:29).

In the time since Schrire's pioneering research in the 1960s (Schrire 1982; White 1967a), the Alligator Rivers region has become a major focus of archaeological study. There is a considerable body of new data concerning the chronology, sequence and diversity of human settlement in the region (Allen 1987, 1989, 1996; Allen and Barton 1989; Bowen 1996; Brockwell 1983, 1989, 1996; Chaloupka 1993; Chippindale and Tacon 1993; Clarke 1987; Guse 1992; Haskovec 1992; Hiscock 1996; Hiscock and Mowat 1993; Hiscock et al. 1992; Kamminga and Allen 1973; Jones and Johnson 1985a, 1985b; Lewis 1988; Meehan et al. 1985; Mowat 1995; Roberts and Jones 1994; Roberts et al. 1990, 1993, 1994; Taçon 1989, 1991; Taçon and Brockwell 1995; Taçon and Chippindale 1994; Taçon et al. 1996; Woodroffe et al. 1988). A number of regional geomorphological studies have also established comparative floodplain histories for the South Alligator River and the Magela Creek systems, providing a detailed account of landscape evolution unavailable to White and Peterson (Chappell 1988; Chappell and Grindrod 1985; Chappell and Woodroffe 1985; Clark and Guppy 1988; Clark et al. 1992; East, Cull et al. 1987; East, Murray et al. 1987; Grindrod 1988; Hope et al. 1985; Russell-Smith 1985a, 1985b; Woodroffe 1988; Woodroffe et al. 1985a, 1985b; Woodroffe, Chappell and Thom 1985; Woodroffe et al. 1986; Woodroffe et al. 1987). We have drawn on much of this research in order to develop a new understanding of seasonality in the human ecology of the region. First, however, we set out Schrire's and Peterson's initial analysis of this strategy.

'PLATEAU AND PLAIN': ARCHAEOLOGY AND SEASONALITY

The original model of seasonal movement was derived from Schrire's excavations of five rockshelter sites located within a confined range around the northwestern edge of the Arnhem Land plateau (Schrire 1982; White 1967a) (Fig. 3). In interpreting the archaeological patterning, Schrire divided the sites into two groups. Her 'plateau' sites (Jimeri I, Jimeri II), were located on the edge of Tin Camp Creek valley, one of the broad valleys in the Arnhem Land plateau. Her 'plain' sites (Malangangerr, Nawamoyn, Paribari), were located on sandstone outliers separated from the plateau and found on the edge of the floodplains of the East Alligator River and Magela Creek.

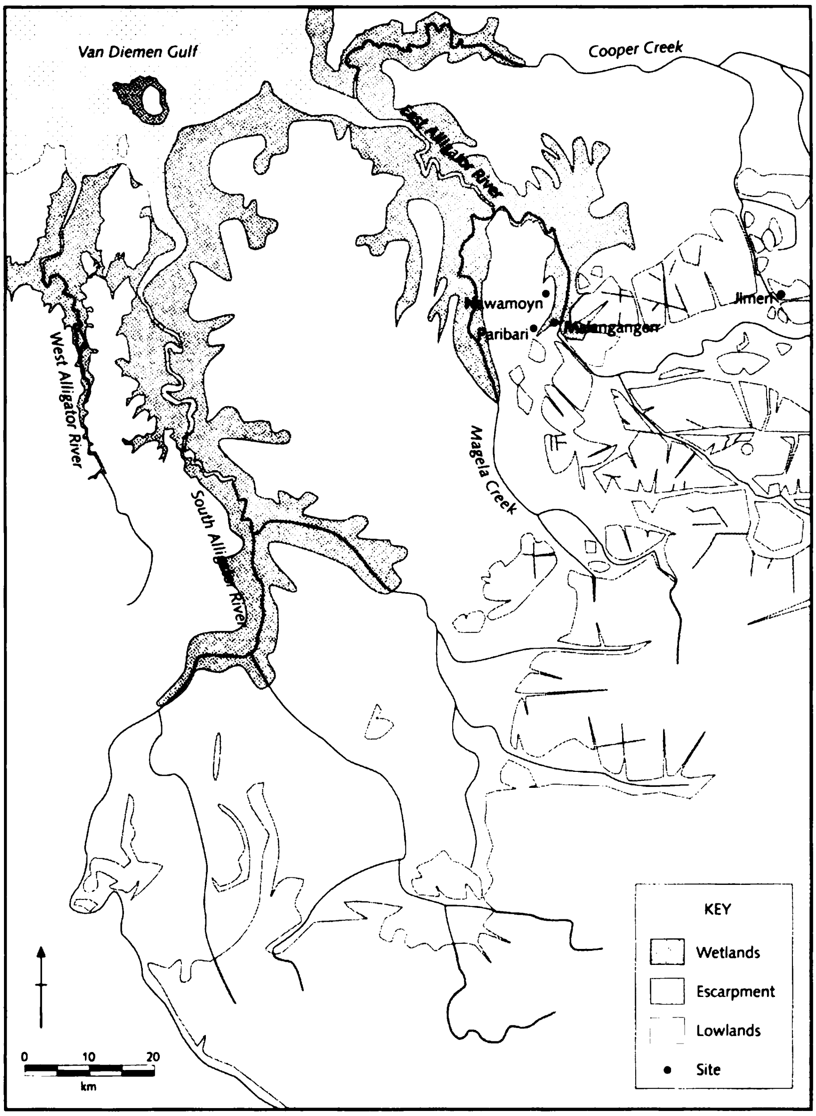

Figure 3. Location map of Schrire's excavated sites.

Schrire's analysis of artefacts from these five sites showed evidence for a dichotomy in the upper levels, from about 6000 BP. The plains sites contained very few stone artefacts, whilst those in the plateau valley produced evidence for on-site stone artefact manufacture. Conversely, the plains sites, notably Paribari, contained many examples of wood, bone and shell tools, while the plateau valley sites contained almost no organic remains. Schrire (White 1967a, 1967b) initially explained this dichotomy in terms of two distinct cultural groups; a stone-using plateau people and a wood/shell/bone tool-using plains people, who traded stone and organic materials between the two areas.

In 1969 Schrire and Peterson offered a different interpretation. They drew on early ethnographic sources (Spencer 1914, 1928; Thomson 1939) and from contemporary anthropological studies such as Peterson's (1973) own work in central Arnhem Land to argue that these material differences were indicative of seasonal changes in residence and resource use by the same sub-culture of foragers (White and Peterson 1969:58–61). They concluded that the archaeological patterning was a result of seasonal movement between the Arnhem Land plateau and the East Alligator floodplains, a change of habitat by members of a culturally undifferentiated local population.

In detail, they suggested that at the onset of the wet season some people moved from the flood-prone plains to the higher ground of the plateau valleys, where macropod hunting became a focus of economic activity. In terms of the technological and archaeological manifestation of this strategy, single-pointed stone-tipped spears formed a major item of the tool kit. Stone sources in the plateau valleys were quarried to obtain the raw material for artefact manufacture. At the end of the wet season people gradually moved back out onto the floodplains, where they concentrated on the exploitation of aquatic resources, swapping the stone-tipped spears for multi-pronged, composite spears with bone and hardwood points. In the archaeological record this resulted in dry season sites, around sandstone outliers on the plains, containing more wooden and bone points (White and Peterson 1969:60).

In 1982 Schrire presented a revised version of this model (Schrire 1982:250–1) in the light of observations of the recent historical impact of feral animals (water buffalo and pig) on floodplain ecosystems. She guessed that, prior to the introduction of these animals, seasonal stresses upon the swamps were less, such that 'the plain was more congenial to year round occupation … . One might well imagine minor retreats to higher ground at peak wet, rather than major movements into plateau valleys' (Schrire 1982:25).

Schrire thus moved her emphasis away from a general migratory movement, favouring now a more complex model dictated by a concentration on those resources available in the vicinity of the respective sites. A re-reading of the 1969 analysis shows that this was not a substantial reformulation. Schrire and Peterson (1969:62) had then made it clear that a range of habitats were viable in the wet season (and see further below). In her last statement on the issue, Schrire (1984:86) extended this more flexible view to dry season adaptations as well: 'the groups ranged widely throughout the year, depending on a multiplicity of circumstances, but were certainly not bound to the plateau in the wet season or to the plain in the dry season.'

FURTHER RESEARCH ON THE SCHRIRE AND PETERSON SEASONAL RANGE

Much of the extensive further research since 1969 in the Alligator Rivers region, though in disciplines other than archaeology and directed to other purposes, has a bearing upon the seasonality model presented by Schrire and Peterson. To begin, geomorphology has demonstrated a history of landscape evolution against which we may infer the antiquity of this Aboriginal seasonal adaptation.

Over the period of occupation of Schrire's rockshelter sites, the adjacent river floodplains changed markedly. Following post-Pleistocene sea level rise ca. 10,000 BP, down-cut river valleys were flooded. Mangroves rapidly invaded the newly formed inter-tidal zones. This initial phase has been dated to ca. 8000 BP to 6800 BP (Chappell 1988:41; Woodroffe et al. 1985a:3). From 6800 BP to 5300 BP sedimentation kept pace with sea level rise and inter-tidal deposits accumulated in a shallow estuarine environment (Woodroffe et al. 1985c:712). Mangroves, especially *Rhizophora sp.*, spread widely to dominate the floodplains when sea level rise slowed and approached its present level ca. 6000 BP (Chappell 1988:41; Chappell et al. 1982; Clarke et al. 1979; Thom and Chappell 1975; Woodroffe 1988:4; Woodroffe et al. 1985c, 1986:127–30). Continued sedimentation under stable conditions led to the retreat of mangroves and subsequently, to the formation of sedge and grass floodplains (Woodroffe 1988:25). The formation of freshwater floodplains on the middle reaches of the South Alligator River and Magela Creek has been dated to between 1500 BP and 1000 BP (Clark and Guppy 1988; Clark et al. 1992:92; East, Cull et al. 1987; East, Murray et al. 1987; Hope et al. 1985:235). It is these last dates that give the maximum antiquity for the seasonal strategy under discussion. It is an adaptation that is very recent in the history of Aboriginal occupation of the region, and much younger than the 7–6000 years estimated by Schrire (White and Peterson 1969:62; Schrire 1982:251).

We now turn to ethnographic research that bears upon the details of the Schrire and Peterson model. Schrire identified two of her 'plain' sites, Malangangerr and Nawamoyn, as places belonging to the *Erre* language group, which she suspected was part of Spencer's 'Kakadu [*Gagudju*] nation' (White 1967a:11, 125, 191). She also initially argued from Spencer's ethnography that the occupants of the third 'plain' site, Paribari, were *Gagudju*. Land claim mapping by the anthropologist Keen (1980:152) located two of these sites, Malangangerr and Paribari, within the traditional estate of the *Mirarr* clan of the *Erre* language group. Later linguistic mapping by Harvey (1992:xiii) showed *Mirarr Erre* territory to include all three of the 'plain' sites, and further identified Tin Camp Creek valley, where Schrire's 'plateau' sites lay, as making up part of the territory of the neighbouring *Urningangk* language group. Harvey's linguistic analysis shows further that *Erre* and *Urningangk* are dialects of a language that he labels '*Kimbi-yu*' (1992:xiv), from Spencer's '*Geimbio*', a group Spencer regarded as closely allied to the *Gagudju*. *Erre* and *Urningangk*, moreover, are both clearly associated by local Aboriginal people with the 'Swamp' or 'Alligator' groups who make up the core of Spencer's 'Kakadu nation' (Levitus unpublished field notes).

These findings appear to confirm Schrire's and Peterson's argument for cultural homogeneity across the range that includes both their 'plain' and 'plateau' sites. Within the geographical limits of their site sample, then, later research serves to confirm one of the underlying conditions of their model of seasonal adaptation. We turn now to consider the significance of those geographical limits.

TOWARDS A REGIONAL MODEL OF SEASONALITY

From analysing the logical framework of the seasonality model, and setting it against the wider topographical and cultural conditions of the Alligator Rivers region, it can be shown that the coherence of the

Schrire and Peterson model depends on two specific and limiting conditions. One of these arises from the local environment in which their five sites were located, and the other from an artificial boundary that they implicitly draw around their proposed seasonal range at the upland end. In order to propose a model of seasonal movement that may have a wider regional relevance, it is necessary first to consider the significance of those two conditions within their model.

Along the northwestern edge of the Arnhem Land plateau, where Schrire found her sites, sandstone cliffs directly overlook lowland swamps. There is a compression of the wetland and escarpment ecological zones which, especially in the wider Alligator Rivers region to the west and south, are separated by a broad zone of lowland savannah woodland (see Fig. 2). In that northwestern corner, the intervening woodland zone is very narrow at some points. As a result the territories of language groups identified with the swamp country cover terrain that includes both sandstone outliers and plateau valleys. This is not the case for most of the western Arnhem Land escarpment. Thus the cultural homogeneity demonstrable in support of Schrire's and Peterson's model is a function of the particular ecological conjunctions and the geographical scale associated with Schrire's site sample.

Further, the ethnographic evidence regarding language and clan affiliations of the sites is concerned with the question of traditional ownership. Schrire's and Peterson's model is about residence and usage, necessarily matters of greater flexibility than ownership (Hiatt 1996:23–2). Thus, while we might be satisfied that the Aboriginal owners of the sites were members of the same cultural grouping, we cannot presume the same about all the bands that may have used the sites. This then brings us to the artificiality of the upland boundary that Schrire and Peterson implicitly place around their proposed seasonal range.

To support their argument for seasonal movement, Schrire and Peterson (1969:58; Schrire 1982:250) point out that the carrying capacity of Tin Camp Creek valley is too small to have supported a distinctive sub-cultural group. But there is no reason that any such distinctive group would have had to depend solely upon that valley. Tin Camp Creek valley is surrounded by the Arnhem Land plateau, itself consisting of other valleys, gorges and stony uplands. Indeed, in her first interpretation of the archaeological evidence, when Schrire (White 1967a, 1967b) saw the different assemblages produced by her 'plain' and 'plateau' sites as evidence of different cultural groups, she cited ethnography suggesting that the 'plateau' sites were associated with the language groups of the stone country.

In her original explanation of the technological dichotomy, Schrire was guided by a consistent distinction drawn by Aboriginal people between plains, 'bottom end' or 'swamp country' - people of the Alligators Rivers lowlands, and plateau, 'top end' or 'stone country'people, a distinction marked by differences in material culture: 'They suggested that the plains people made and used bone, shell and wooden artefacts, but did not fashion stone tools. These they obtained by trade from stone working tribes who lived in the hills' (White 1967a:8). Schrire acknowledged Spencer's ethnography in corroboration of this, and confirmed, both from the ethnography and her own inquiries, the association of *Gundjehmi* and *Kunwinjku* language groups with the plateau or stone country south and east of Oenpelli (White 1967a:10–11). While Tin Camp Creek valley is *Urningangk* country, she records that her upland sites have a *Kunwinjku* name, Jimeri (her 'Tyimede') (White 1967a:254). She concluded that the differences in material assemblages between the 'plateau' sites in this valley and the 'plain' sites on the outliers were consistent with the cultural dichotomy reported in the ethnography and by local Aboriginal people (White 1967a:463–7; Schrire 1982:250).

In the light of these arguments, this was a matter Schrire and Peterson had to address. They thus treated the testimony of Oenpelli people skeptically, arguing that exchange relations between upland and lowland peoples were a recent product of contact with Europeans. They postulated that metal implements replaced stone tools in the lowland material repertoire, but that stone tools were still needed for ceremonial exchange. Thus, 'a dichotomy arose between the sophisticated people of the plains and the less acculturated people of the stony interior, who continued to be associated with such traditional practices as the continued use of stone tools' (White and Peterson 1969:58).

If by 'people of the stony interior' they referred to those occupying Schrire's local 'plateau' sites, then this argument contradicted the thesis of their paper, that the peoples of the 'plain' and 'plateau' were members of the same seasonally-migrating groups. If, on the other hand, they were referring to people from the more distant plateau hinterland, then their hypothesis did not address the point that Schrire took the Oenpelli people to be making, about 'two distinct groups living on the plain and in the hills near Oenpelli' (White and Peterson 1969:57). They further expressed 'some doubt, however, that a permanent population ever resided in the plateau valley south of Oenpelli, or in the northern and western borders of the tableland country' (1969:56–7).

They thus attempted to rule out the possibility that even on the wider geographical scale, distinctive sub-cultures may have existed and been responsible for contrasting material assemblages of the kind they were trying to explain. The ethnographic data cited so far, including that from Schrire's original findings, show that these northern and western borders were the countries of the *Kunwinjku* and *Gundjehmi* language groups respectively, and that both these groups were identified as 'top end' or 'stone country' people, in opposition to the 'bottom end' or 'swamp country' people of the Alligator Rivers lowlands. Moreover, while they might have maintained impermanent occupation of their stone-country estates, whether this would have prevented the maintenance of sub-cultural distinctiveness is unclear now that the geographical scale has been widened. The cultural homogeneity underlying the Schrire and Peterson model depends on the small areas and distances involved in Schrire's site sample. This reasoning loses its force over the wider Alligator Rivers region, where distances between upland escarpment and lowland swamp can be many times greater, and the enabling conditions of their model are thus absent.

The artificiality of the upland limits implicitly placed around Tin Camp Creek valley is most clearly evident in Schrire's and Peterson's discussion (White and Peterson 1969:57) of the report of the geologist Gray, who traversed the valley at the beginning of the wet season of 1914–15. They point to the fact that Gray, 'did not see any camps of people living in the main valley'. However, he did, they acknowledge, come upon a camp of Aborigines shortly before entering into the valley from the northeast. Reference to Gray's report (1915:29) and the 1:100,000 scale map (Oenpelli) for the area shows that the camp he encountered was on the headwaters of Birraduk Creek, only a few km over easy terrain from Tin Camp Creek valley itself.

So far, in moving beyond the geographical limits of the seasonal range proposed by Schrire and Peterson, we have encountered two sets of conditions that are different to those that framed their model. Firstly, we have a clearer differentiation of ecological zones. At the 'plain' end of their dichotomy, the compression that obscured the lowland forests from view is removed, so that the wetlands and floodplains of the major drainage systems and the broad lowland forests in which sandstone outliers occur, are identifiable as distinct topographic systems. At the 'plateau' end, by removing the artificial boundaries around Tin Camp Creek valley, we can distinguish between plateau valleys of that kind, and the surrounding upland stone country of the plateau itself. Secondly, their claim for cultural homogeneity is untenable on the larger scale.

Schrire and Peterson's analysis placed the phenomenon of seasonal migration on the research agenda of the Alligator Rivers region, but was unsuccessful in its attempt to exclude the cultural dichotomy between swamp country and stone country from the proper range of that analysis. The research by the Kakadu Project (Jones 1985b) in 1981 pursued the question of seasonal adaptation across a transect of country further south. This not only shifted the focus from estuarine to freshwater conditions and added swamp sites to the sample, but also moved to a larger geographical scale and incorporated locally recognised cultural heterogeneity within the research area. It thus cut away the logical conditions that had allowed Schrire and Peterson to remove the dichotomy between stone and swamp country from the purview of their model, and opened that dichotomy to ecological investigation.

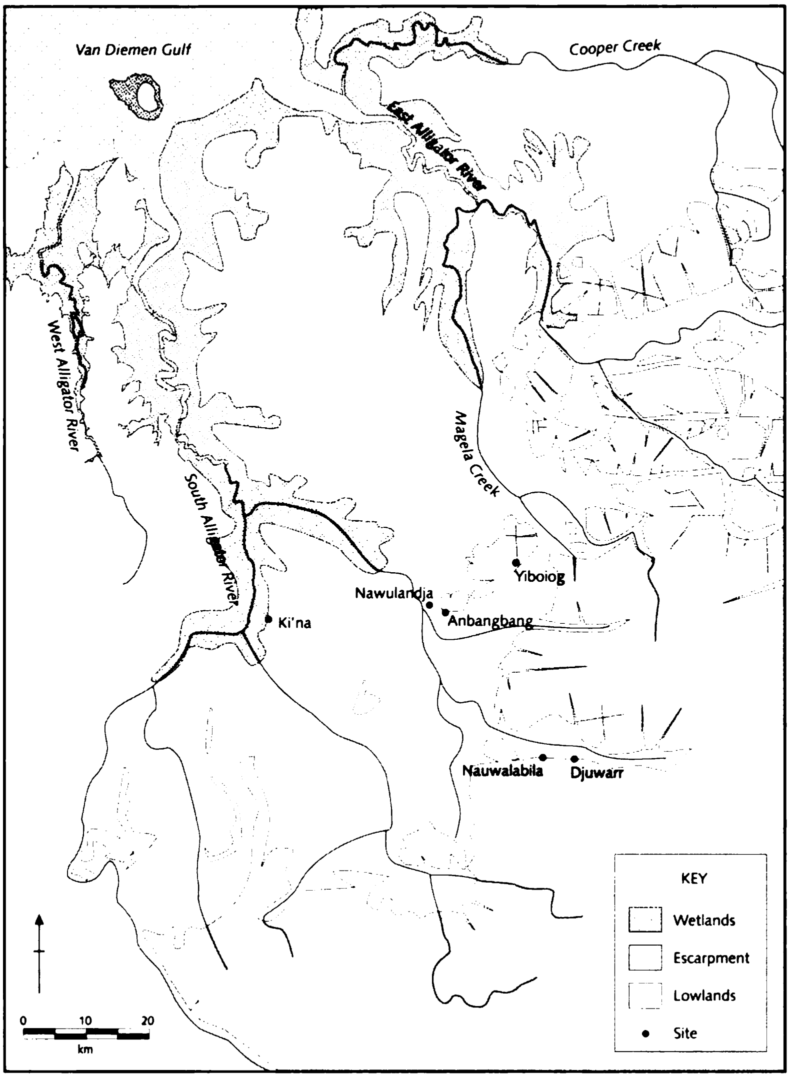

Figure 4. Location map of sites excavated during the Kakadu Archaeological Project in 1981.

THE SOUTHERN TRANSECT: 1. THE WETLAND SITES

Attention was first drawn to the archaeological potential of the wetlands in the early 1970s when Kamminga and Allen (1973) recorded some open sites on the East Alligator River floodplains. However, it was not until 1981 that the plateau/plain model was more thoroughly investigated by the Kakadu Project (Jones 1985b). A transect was designed which crossed a variety of landscapes, including plateau valleys, outliers and floodplains, 50–70 km south of Schrire's original field area (Jones 1985c:26). As well as excavations at rockshelter sites in the plateau valleys and at the outliers, open sites located along the floodplain margins of the middle reaches of the South Alligator River (Fig. 4) were also examined (Meehan et al. 1985). This wetlands research has enabled the 'plain' component of the Schrire and Peterson seasonal model to be examined in detail (Bowen 1996; Brockwell 1983, 1989; Guse 1992; Hiscock et al. 1992; Hiscock 1996; Jones 1985a:294; Meehan et al. 1985; Mowat 1995).

Because of the marked contrast between the wet and dry seasons in tropical Australia, wetlands are a seasonal but highly productive resource base. Given the enormous resource base represented by the wetlands, it seems appropriate to suggest that the formation of freshwater swamps ca. 1500–1000 BP would have had a major impact on the late Holocene regional economy (Brockwell 1983:66; Jones 1985a:293; see also Brockwell this volume).

In northern Australia, plant and animal resources of the wetlands are also highly seasonal in habit and availability (Crassweller 1996; Guse 1992). In the early dry season the wetlands are full and the resources dispersed. As the dry season progresses the water retreats and resources are concentrated in a few permanent lagoons and billabongs in the backwater swamps of the floodplains. The wetlands are highly productive in the late dry season when resources are often scarce elsewhere. During wet season flooding, the wetlands resources become dispersed once again. In the past, this regime would have influenced the way in which Aboriginal hunter-gatherers subsisted and moved around the landscape. The changing nature of the resource base would have necessitated a flexible approach to exploitation.

The existence of large open sites along the South Alligator River floodplain margins indicates that such was indeed the case. They provide evidence for occupation within the last 1500 years suggesting that wetland resources were a primary focus of prehistoric economic activities in the late Holocene. Twelve sites were identified along the middle reaches of the South Alligator River (Meehan et al. 1985:103–53; Brockwell 1989, this volume). They consist of continuous scatters of artefacts along wetland edges, mostly located on headlands of sand or silt, a metre or two higher than the surrounding floodplains. If prehistoric conditions were similar to those at present, such locations would have afforded well-drained positions for campsites strategically placed for the exploitation of swamp resources. Most of these sites are deflated with no depth of deposit, although there was a large midden at one site, Ki'na, which has been excavated, returning a basal date for occupation of 280±140 BP (Meehan et al. 1985:152).

The interpretation of regional sequences after 1500 BP also demonstrates the importance of the freshwater wetlands in the economy, and suggests that they allowed more intensive settlement of the region. Along the Magela Creek floodplains, the upper levels of the outlier site, Paribari, were dominated by freshwater shellfish (Schrire 1982:52), and the previously abandoned outlier sites of Malangangerr and Nawamoyn may have been re-occupied at this time (Allen and Barton 1989:105). In the south, the lowland outlier site of Yiboiog was occupied for the first time ca. 1000 BP and there was a dramatic increase in activity at the nearby site of Anbangbang I (Jones and Johnson 1985b:57, 60, 72).

Having established the importance of freshwater wetlands in the late Holocene regional economy, the questions that now need to be addressed are to what extent do the wetland sites represent seasonal activity, to what extent were the wetlands integrated within a regional economic system, and what does this tell us about seasonal migration and resource use?

The topography of the sites indicates that they are flooded in the wet season. As this is also a time of dispersed resources it can be concluded that the wetland sites were not occupied in the wet season. This raises the question of what alternative strategies were available to people at this time of year. It also suggests that our earlier ecological distinction between the wetlands and lowland forests is also significant archaeologically, and that these two zones were of differing significance for human ecology.

During the surveys along the South Alligator River wetlands, a series of earth-mound sites in open forest on the higher ground adjacent to the floodplains of the South Alligator River was identified (Brockwell 1989, this volume). These mounds are littered with stone artefacts. The mounds would have made suitable wet season sites as they are elevated above flood levels in open woodland, close to micro-environments such as monsoon rainforests which are productive during the wet season (Brockwell 1983; Russell-Smith 1985b). They would have also made good bases from which to exploit the harvest of goose eggs from the wetlands during the wet season. This distribution, of wetland sites and mound sites suitable for dry season and wet season occupation respectively, confirms Schrire's suggestion that residence could have been maintained in the vicinity of the wetlands throughout the year. Indeed, the stone artefact assemblages suggest that different activities were pursued at the mound sites and the wetlands sites, with an emphasis on land-based hunting strategies at the mound sites (Brockwell 1989:209, 1996:96, 100–1, this volume).

On the question now of the role of the wetlands in a regional economy, the exotic lithic raw materials (quartzite and chert) found at the South Alligator sites were obtained from up to 50 km away and their presence suggests that the wetlands population either maintained regional trade and exchange contacts, or moved considerable distances during their seasonal round (Brockwell 1989:211, 1996:99; Meehan et al. 1985:138). The presence of exotic stone materials implies direct or indirect regional contact with inland sites. To anticipate the next section, this interpretation is supported by organic evidence from outlier and plateau valley sites. In the light of this evidence, it is essential to examine the archaeology of the wetlands to understand the plateau/plain model and ascertain its validity over the wider region. A comparison between wetland stone assemblages and those from several rockshelter sites in the lowland outlier and plateau valley region was carried out to examine these questions (Brockwell 1989:213–24, 1996: 97–9). As the South Alligator wetlands sites are no older than 1500 years, only data from the upper levels of the rockshelter sequences were considered.

The results of the stone analysis suggest that each landscape unit had a distinct pattern of occupation, reflecting different environmental locations and economic bases. At the plateau valley sites of Nauwalabila

I and Djuwarr I there was a high proportion of stone points which were manufactured *in situ* from materials quarried locally (Jones and Johnson 1985a:188-90). At the South Alligator wetland sites, points are present only in low numbers and stone was mostly imported and only secondarily retouched. The exclusive occurrence of use-polished flakes at these sites suggests the processing of silica-rich plants that grow abundantly in the freshwater wetlands (Brockwell 1989:223, 1996:98). The situation is different again at the lowland outlier sites (Anbangbang I and Yiboiog) where there is a high proportion of stone points (Brockwell 1989:215), but the stone raw materials are not locally available and manufacture has taken place elsewhere, probably in the quartzite quarries of Deaf Adder Gorge, ca. 35 kms to the southeast. These outlier and plateau valley sites also contained a significant proportion of rectilinear scrapers in their upper levels, while the wetland and plateau valley sites contained only rare examples (Brockwell 1989:213–24; Meehan et al. 1985:138-40, 144, 145).

Thus the technological dichotomy observed by Schrire (1982; White 1967a, 1967b) at the northern sites becomes a technological trichotomy in the south. Points and rectilinear scrapers are manufactured at the plateau valley sites, imported to the outlier sites and are present in low numbers on the floodplain. Use-polished flakes and rectilinear scrapers also show a differentiated distribution between the three zones. The geographical compression of these zones in the north obscured the full complexity of the system.

THE SOUTHERN TRANSECT: 2. ARCHAEOBOTANY

Well-preserved (uncarbonised and carbonised) fruits, nuts, flowers and corms from seasonally-productive plant species were recovered from the deposits of two rockshelters, Anbangbang 1, a lowland outlier site, and Djuwarr 1, a plateau valley site (Clarke 1985:77, 1987, 1988, 1989; Jones and Johnson 1985b). The preservation of this material allowed aspects of the Schrire and Peterson model to be examined in specific detail. Recent ethnobiological research in Kakadu National Park (Russell-Smith et al. 1997) has also provided new data on the seasonality of plant and animal resources in the Alligator Rivers region.

Both Anbangbang 1 and Djuwarr 1 are located 70 km to the south of Schrire's research area. In this southern region the Arnhem Land escarpment and the South Alligator floodplains are some 50 km apart, separated by a lowland zone of eucalyptus woodlands. Anbangbang 1 occurs in a sandstone outlier situated in the intermediate zone of lowland woodlands. Deaf Adder Gorge, the plateau valley where Djuwarr 1 is located, opens onto the lowlands. According to Schrire's and Peterson's model, some people made use of such plateau valleys during the wet season, and then moved onto the lowland plains and down to the swamps as the dry season progressed.

At Anbangbang 1 most of the plant remains occurred in the uppermost, sandy excavation units. These are dated from modern to ca. 800 BP. Preservation of plant materials in the lower silty layers was poor and the plant assemblage consisted of wood charcoal only. At Djuwarr 1 only a surface collection was carried out. This also produced a large quantity of well-preserved plant remains. No dates are available for the occupation of this site but the material from the surface collection is likely to be modern in age.

According to the seasonal model Anbangbang 1 would be expected to contain evidence of dry season occupation as people moved out to the plains when the country began to dry. The presence of *Nymphaea sp.*, (water lily) and *Triglochin procerum* (ribbon plant) provided evidence that the site could have been occupied at any time during the dry season as these plants can be collected from the nearby Anbangbang lagoon throughout that period (Russell-Smith et al. 1997:Appendix 1). Fragments of *Pandanus spiralis* (screw palm) fruits indicated site use in the mid-late dry season. The plant resource base for the wet season was not particularly well-represented but *Syzygium sp.*, (bush apple) provided evidence for early wet season use of the site. There was a range of other wet season fruits preserved, but their presence in the deposit could not be unequivocally demonstrated to be the result of human activities (Clarke 1988:127–32).

The wooden artefact assemblage from Anbangbang 1 included hardwood points and barbed fishing spears, all items predicted in the seasonal model to be part of the dry season tool kit. In addition, fragments of *Bambusa arnhemica* (bamboo) and *Phragmites karka* (phragmites reed) were also recovered from the deposit. These materials, which derive exclusively from the floodplains, were used in the manufacture of composite spears used to hunt *Anseranus semipalmata* (magpie goose) in the billabongs and wetlands (Leichhardt 1847:505; Levitt 1981:26; Russell-Smith 1985b:248; Spencer 1914:358). In 1981 Aboriginal people told Jones and Johnson (1985b:63) that people used to travel from the plateau to the wetlands to collect both bamboo and phragmites reeds to make spear shafts. Magpie goose hunting is a late dry season activity and the presence of these plant remains can be interpreted as evidence for the manufacture and maintenance of goose spears whilst hunting in the adjacent Anbangbang lagoon during the late dry season.

The plant remains showed that Anbangbang 1 could have been used at almost any period of the year. This is not to imply continuous occupation but rather use of the site at times when particular resources were seasonally and locally abundant. It can be further suggested that the site was regularly used by people for whom Anbangbang I was a focal point in their clan estates. Links to the South Alligator River wetlands to the west are shown through the presence of *Bambusa arnhemica* and *Phragmites karka*. Similarly, links to the plateau country to the east is evident through the presence of quartzite artefacts.

In terms of Schrire's and Peterson's model, Djuwarr I would be expected to have been occupied in the wet season as people moved from the floodplains back into the plateau valleys. The evidence from the plant remains does not strongly support this view. A similar plant assemblage to that from Anbangbang 1 was identified. The samples contained fragments of *Nymphaea sp.*, *Triglochin procerum*, *Pandanus spiralis* and *Syzygium* sp. The wooden artefact assemblage also bore more resemblance to that predicted for the dry season tool kit, with hardwood points and barbed fishing spears. Some fragments of *Bambusa arnhemica* testify to the links between the plateau valleys and the wetlands.

As with Anbangbang I, it can be suggested that Djuwarr I was also occupied when particular resources were seasonally and locally available. The preservation of dry season plant species are evidence for the presence of people in the plateau valley of Deaf Adder Gorge precisely at that time of year when, according to the seasonal model, these valleys would have been emptied by migration to the wetlands.

The similarity between the organic remains at both sites reflects the fact that the major plateau valleys contain a similar range of resource zones as found in the open woodland plains. The plateau valleys contain seasonal swamps, monsoon rainforest and mixed woodland communities that have the potential to supply aquatic plant foods, tubers, fruits and nuts. These include the same range of plant foods found in the lowland woodlands to the west of the plateau. The plateau valleys would have contained sufficient plant and water resources in the early to middle dry season to support a human population in this zone.

The archaeobotanical evidence thus does not support the model of dry season movement of people from the plateau valleys to the floodplains as suggested by Schrire and Peterson. Rather, the evidence is more suggestive of the possibility for lowland outlier and plateau valley sites to be occupied at almost any time, according to seasonal and local resource abundance, combined with material provisioning from the wetlands by means of migration and exchange.

SEASONALITY REVISITED

These findings, following from the 1981 Kakadu Project, can now be combined with historical and ethnographic evidence to assess the coherence of a model of seasonal movement across the ecologically and culturally expanded range of the southern transect. The following sub-sections discuss particular components of such a model.

Occupation of the wetlands

The archaeology of the South Alligator wetlands reported above provides good evidence for year-round occupation of that zone, either on broad open dry season sites alongside remnant bodies of water, or on raised sites immediately above the reach of wet season floods. This is consistent with Schrire's and Peterson's model regarding the Magela Creek wetlands. Contrary to the view often attributed to them of uniform group migration keyed into seasonal change, they allowed for a dispersed wet season residence, and in particular pointed up the feasibility of continuous habitation around the wetlands. Thus, 'the whole plain population certainly did not move en masse to the high valley at the peak of the wet season. Some must have retreated to high plain outliers, other to encampments of stout bark huts above the swamps and small parties to the plateau valleys where they exploited the prolific sources of rocks and stony gravels to make tools' (White and Peterson 1969:62)

There is some evidence to support the suggestion of a year round Aboriginal presence on the plains and swamps in the vicinity of Schrire's outlier sites. The range of organic remains at Paribari led her to conclude that it was probably used sporadically throughout the year (Schrire 1982:73–4). Gillespie has also documented the great diversity of habitat types and food resources in the vicinity of another nearby outlier site at Ubirr (Gillespie 1982).

Historical observations are also useful here, although no explorers wrote wet season accounts of the Alligator Rivers region itself. McKinlay, for example, did not enter the South Alligator catchment until the wet season was substantially over. Spencer (1914:32; 1928:774–5, 823–4) described Aboriginal people exploiting the resources of the East Alligator River floodplains throughout the dry season of 1912, then when the rains came retreating to rockshelters in the nearby escarpment, and returning to the floodplains late in the wet season to harvest goose eggs. Warburton (1944) reported Aborigines living year-round in the vicinity of the Alligator Rivers wetlands during the early 1920s. In the wet season of 1934, Tom Cole crossed from the lowlands into the plateau valley of Tin Camp Creek, passing through the migratory range proposed by Schrire and Peterson. While camped between Magela Creek and the East Alligator River, he wrote (1992:80): '19 January 1934 – Friday … . Mobs of bush blacks camped about here. One lot have a tree down and are making a canoe. Heavy storm.'

Although this event occurred well after contact with Europeans, the note suggests not only significant numbers remaining on the lowlands immediately adjacent to the outliers, but also an intention and ability to move around that waterlogged terrain.

All these accounts refer to those northern floodplain areas in close contiguity to rockshelter sites in sandstone outliers or the escarpment. Brockwell's mound sites, however, suggest continued wet season residence even around those swamps that were a long way from convenient rockshelters. The best historical corroboration of this comes from the explorer McKinlay on the Mary River, which is located immediately to the west of the region and which also flows from the sandstone plateau through lowland forests to extensive downstream swamps. Travelling at the height of the wet season in 1866 he received a visit on February 7 from a few natives when camped apparently between Marrakai Creek and the Mount Bundey range, in an area he described as 'an awkward country to pass over — one moment bog, the next a stony ridge'. Later when his party was stranded for six weeks at Camp 14 in swampy country located on Marrakai Creek, they received a number of visits, firstly from three, then two, individuals, then on five consecutive days in the last week of March by a group numbering up to 33 (McKinlay 1866:4–7). These reports suggest an Aboriginal ability to live and move freely in the low-lying areas of the region throughout its season of inundation, unencumbered by horses, stock or stores.

Seasonal zones

The technological inventories established for the sites across the southern transect favour the differentiation of three zones of human adaptation. The relative frequency at different sites of stone points, rectilinear scrapers and use-polished flakes, as described above, supports the view that people concentrated on a different range of productive activities when camped on the wetlands, at lowland outliers, and in plateau valleys. This illustrates the point made earlier, that the particular topography of her study area caused Schrire to conflate these three zones into two.

These stone artefact inventories have a further implication when we recall the cultural geography of the region. Deaf Adder Gorge, where the plateau valley sites are located on the southern transect, and much of the surrounding lowland forest zone, is the traditional territory of clans of the *Gundjehmi* language group, which is associated with the stone country. The identification of three adaptive strategies across a transect of country that incorporates cultural territories in both swamp country and stone country, validates one of the central tenets of the Schrire and Peterson model, that variations in material assemblages are not necessarily co-incident with sub-cultural differences in human groups. Just as they argued for a technological dichotomy encompassed by a cultural homogeneity, so this evidence suggests a technological trichotomy encompassed by a cultural dichotomy.

These material inventories further suggest that the three zones were integrated by movement and exchange into a regional economy. The occurrences of stone points, chert, quartzite, bamboo and phragmites at different sites show that some of the materials that people used in each of the zones were either brought by them, or acquired by exchange with people, from other zones. If the bamboo and phragmites in Deaf Adder Gorge came from the South Alligator wetlands, then the lowland outlier sites in between were both a separate component in a tri-partite landscape (Brockwell 1983), and also way stations on the journey between swamp and plateau.

Seasonality and culture

Up to this point the findings of the 1981 project amount to a refinement of the Schrire and Peterson model. That is, while on this larger scale cultural homogeneity cannot be reasonably presumed and is in any event ethnographically denied, there is no reason in principle that the members of swamp country and stone country groups should not have participated to the same extent in seasonal movements between zones. The archaeological evidence taken alone suggests that the cultural opposition between plateau and plain was continually bridged in the course of routine material provisioning. Schrire, in her re-formulation of the model, emphasised the lack of congruence between attachment to country and environmental adaptation. Having noted from the ethnography the association between language and land, particularly the distinction between the language group estates of plateau and plain, she then dismisses them as ecologically irrelevant (Schrire 1982:24):

> "It is important to note here that although some congruence is found between land and language at one particular time, as Carroll claims, the individuals concerned were multilingual people who operated in small groups over a wide range of territory and resources. The systems that are recalled today imply a great deal of overlap bespeaking inbuilt flexibility. Consequently it is useless to try to delineate rigid linguistic and territorial boundaries within which to fit the prehistoric features of the region. Instead we need to retain a notion of shifting alliances, languages and people which approximates more closely to good sense than to tidiness."

Despite this denial, reference to other comments in Schrire's monograph suggest that affiliations to territory did influence economic behaviour. She interprets the refusal of the explorer Leichhardt's guides to go beyond a certain distance as an indication 'that they had reached the limits of their own territories', and she accepts oral testimony that the *Erre* place Malangangerr was 'a former camping place of the *Erre* people who had lived and hunted in that area' (1982:25, 75). If correct, both suggest that traditional attachment did exercise some degree of influence in daily practicalities.

Statements recorded from traditional owners by Brockwell and Levitus confirm a regular pattern of seasonal migration, lending credence to the interpretation of site assemblages presented here. Like Schrire, they were told of exchanges between stone country and swamp country peoples (Jones and Johnson 1985:167; Meehan et al. 1985; White and Peterson 1969:57). The important respect in which this ethnography goes beyond what can be discovered from the archaeological data, is in its disclosure of differences in the human ecology of swamp country and stone country groups. Not all groups moved around equally. The largest movements, between plateau valley and swamp, are attributed to people from the stone country. Nothing is said of swamp peoples traversing the same route.

More importantly, these groups traded. Curiously Schrire (1982:252) argues that this 'constant exchange of goods and of food' tended to blur the distinction between swamp country and stone country peoples. But exchange does not blur distinctions so much as presuppose them. To exchange, groups must be articulating with different kinds of country and producing from different resources, a point Schrire acknowledges in her next sentence about 'plains dwellers who were far more adept at reaching [goose] nests because they knew how to make rafts that could be poled over the swamps'.

Thus while the practicalities of provisioning did indeed cause people to bridge the distinctions of language groups and country, such processes were not generalised and boundless, but specific in purpose and direction. They provide a behavioural basis upon which to begin substantiating the cultural dichotomy between swamp country and stone country groups.

There is no suggestion here of a uniform annual migration by a unitary cultural bloc from the plateau. Undoubtedly some people with primary attachments to stone country estates did not routinely return there; indeed there is further Aboriginal testimony to that effect. Possibly some individuals from the swamp country accompanied migrant bands back to wet season shelters in the escarpment. The ethnography does, however, justify an interpretation of a locally-perceived congruence between the stone country/swamp country dichotomy on the one hand and contrasting material inventories and ecological strategies on the other. This, then, is a significant revision of the Schrire and Peterson model, in that cultural heterogeneity is identified as a factor influencing participation in seasonal strategies.

The western Arnhem Land Plateau

There remains, however, another difficulty. It must be admitted that the archaeobotanical findings reported above do not unequivocally support the separation of the lowland outlier sites from the plateau valleys. While there were notable differences in the tool assemblages, only one of these - the higher proportion of rectilinear scrapers at the lowland outliers — may be relevant to seasonal adaptation. The difference with respect to stone points was related to their manufacture, not usage. As well, Clarke found that the same broad suite of floristic resources were being exploited in both places, and that availability of these, and other resources indicated by the identified tool kits, would have allowed occupation of sites in both zones at most times of the year. In other words, while there appears to have been some difference in the adaptive strategies that were possible at the lowland outlier and plateau valley sites, these differences are much less than between those two kinds of site and the dry season wetland sites. It is therefore not clear whether we can properly consider the forested lowlands and the plateau valleys to be absolutely distinct zones in terms of their significance for human ecology.

It is relevant, then, to recall the second limiting condition that applied to the Schrire and Peterson model: the artificiality of the upland boundaries that they implicitly placed around Tin Camp Creek valley. The same limiting condition applies to the archaeology of the southern transect. It now remains to dissolve those upland boundaries, and consider the plateau. For the stone country of western Arnhem Land, we have no archaeological excavations, only history. In contrast to all the other zones, there seems good evidence of abandonment, or very sparse occupation, of the plateau areas during the mid to late dry season. Leichhardt, on his journey across the continent from Moreton Bay in Queensland to Port Essington on the northern coast, crossed the southwestern section of the Arnhem Land plateau and descended into the lowlands of the Alligator Rivers region via the Jim Jim Creek valley in the late dry season of 1845. By the time of his descent from the uplands on November 21st, he had recorded no sign of recent Aboriginal habitation for two weeks (1847:471–84).

Another account is provided by Stuart who in July 1862 (mid dry season) left the Katherine River valley, crossed a sandstone tableland and travelled down the length of the Mary River to the coast and back again on his north-south traverse of the continent. Though he saw no Aborigines at all on the plateau or along the Mary, the signs of their presence, in the form of camps and tracks, became more frequent and recent as he approached the coast (Stuart 1865:388–41). Similarly, Lindsay (1884:16–19) reports that he heard the sounds of Aborigines and saw their fires during his late dry season journey up the Liverpool River into the Arnhem Land stone country in 1883. Once having reached the plateau however, he recorded no signs of occupation during a 16 day transect of its western section until he reached a camp on the Katherine River on November 29th. In 1898 Knut Dahl, a Norwegian naturalist, arrived to collect zoological specimens for the Oslo Museum. In the early dry season, he crossed the same tableland as Stuart had crossed, this time from west to east. Like Stuart, he reported no sign of Aboriginal occupation on the tableland, but found ceremonial sites in the upper South Alligator valley immediately adjacent. A large group of Aborigines encountered below the northern edge of the tableland had no reticence about following him back across it (Dahl 1926:225–39). These accounts indicate that occupation of the western Arnhem Land stone country was subject to a marked seasonality, quite unlike the other zones we have so far considered.

Another source (Chaloupka 1981) gives an ethnohistorical account of the seasonal round of one band, and is discussed further below. If the seasonal order of movement that it gives is accurate, then it causes some revision in this view of the plateau. Chaloupka's account has this band following the water courses of the Upper Katherine and East Alligator Rivers across the western plateau, from south to north, in mid-dry season, perhaps three months earlier than the journeys of Leichardt and Lindsay. In contrast, the tableland across which Dahl and Stuart travelled, in the early and middle dry season respectively, is a much smaller portion of the plateau and has no significant watercourses. In other words, we have to allow for variation within physiographic zones to which the gross label 'plateau' is applied and contemplate a more complex seasonality of human access than the historical reports suggest.

A LOGIC OF SEASONAL MOVEMENT

These various data suggest an order of priority in the human ecology of the pre-contact Alligator Rivers region. As the dry season advanced , the wetlands became especially attractive. This much can be illustrated from Leichhardt's reports of the large numbers of Aborigines congregated around the South Alligator swamps, and the resources still available there at a time when the first storms had begun but the water bodies were still confined (Leichhardt 1847:492–9, 504–7). On November 28th he wrote (1847:496):

> Since the 23rd of November, no night had passed without long files and phalanxes of geese taking their flight up and down the river, and they often passed so low, that the heavy flapping of their wings was distinctly heard. Whistling ducks, in close flocks, flew generally much higher, and with great rapidity. No part of the country we had passed, was so well provided with game as this.

Similarly illustrative are archaeological observations of the large, open, high-density dry season occupation sites along the edge of the South Alligator wetlands (Meehan et al. 1985; Brockwell 1989:116–67). These attest to far higher numbers and densities of people in the mid to late dry season than any other locale at any other time. Moreover, the resources available there were of wide renown. Berndt (1951:171), in referring to the ceremonial trading relations maintained by Kunwinjku people at Oenpelli, states that serrated stone and shovel-nosed spears were provided by people from plateau areas up to 80 km away, in return for bundles of goose spears from the South Alligator River.

In the wet season, when the waters rose and resources were dispersed, no ecological zone offered any such compelling and centralised attraction. Rather, a range of habitats provided resources for smaller groups dispersed across the entire region. Only the availability of goose eggs for a brief period towards the end of the wet drew some people again to the swamps. This then raises the problem of accounting for movements during that large part of the year when there was no compulsion to be present for a major harvest in any one area.

We have noted the possibility of year round occupation of the lowlands, and the regular eschewing of that option by some groups returning to the escarpment and plateau at the beginning of the wet season. Their seasonal round, referred to above, has been documented from the memories of Aboriginal men, who lived in bush camps in Deaf Adder Gorge as children. Chaloupka's (1981) paper on the *Badmardi* year, recounts one band's movements, campsites and social interactions over a range incorporating most of the western Arnhem Land escarpment, the upper South Alligator River and Katherine River valleys, the East Alligator River, Deaf Adder Gorge, and the Woolwonga wetlands. As Jones and Johnson observe (1985a:167), this account is constructed from memories of a period in which the Aboriginal world had already been drastically affected by contact. However, the absence of any orientation towards non-Aboriginal places or purposes, except for one optional detour to Oenpelli (Chaloupka 1981:8), tends to affirm its veracity as a traditional reconstruction. Nevertheless, this narrative should not be accepted entirely in the terms Chaloupka (1981:2) proposes, as a single continuous journey supplemented by shorter trips and made every year by one *Badmardi* family. If we treat the account less as strict narrative and more in the nature of an inventory of places and purposes, its value becomes evident. Most notable is the overall geographical orientation itself, towards escarpment and plateau. Campsites, story places, ceremonial grounds, increase sites and dreamings, and opportunities for reunion and exchange are met with along many kilometres of escarpment cliffs and river valleys, testifying to diffuse stone country connections re-affirmed by reciprocity, reproduction and teaching. By contrast the wetlands connection is characterised as specific and predominantly utilitarian.

This *Badmardi* narrative, taken not literally but as an encoding of the cultural priorities of a stone country group, thus provides a fresh perspective on seasonal migration. It shifts our attention away from the ecological rationale for the descent to the wetlands and focusses instead on the cultural rationale for the return to the top country. Gillespie (1982) approaches this point when, in summarising the importance of the *Badmardi* year, he refers to 'an economy whose religious and political strength lay in the escarpment.' Attachment and responsibility to country and engagement with neighbours and partners appear as the governing criteria. Seasonal migration is a choice to attend to this range of values. It makes sense as an economic strategy once attachments and relations on the plateau are understood as a concern for these stone country people. Once instituted as strategy it may further inform thought on the symbolic ordering of the world.

Seasonal migration thus needs to be understood as a strategy that selects from among the possibilities for material provisioning according to the correspondences that can be made between those possibilities and the wider concerns of life. People travel into the plateau when seasonal conditions allow in order to attend to other forms of business appropriate to the places in that country. Seasonal migration expresses an exploration of the other possibilities that ecology creates in human affairs. Within those wider concerns, it is predicated in particular on territorial attachment operating at a level more generalised than that of clan or language group. It thus also allows us finally to ground the local categories of stone country and swamp country groups in precisely what those labels suggest, relations to land.

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Smiles at Deaf Adder Creek

For R. J.

Carmel Schrire

The parallels you draw are masterly. You,
Sprawled bare beside the darkening pool, while he
Wades through in boot and belt to clasp your hand.
A contrast to those sepiaed days, when he
Watched jodphured experts greet his naked Dad.
Now, Overlord, he watches as your mob
Explore his mother's belly for its yield
Of ochred bones and edge-ground stones
With enigmatic grooves.

So, mark him well:
He's treading water way beyond his depth.
Up to his neck in Titles and in Rights,
He smiles to see you raise your hands to him,
Encrusted with droppings of his past.

Bad Homburg and Princeton
June 1983

Reminiscences of Erith Island with Rhys

Ronald Lampert
Guernevan, Glomel
France

LOOKING BACK nearly 40 years I can remember being impressed from the very beginning by the way in which Rhys Jones' research projects were conceived. In the years when the study of Australian prehistory was gaining momentum and most of our efforts were like first hesitant brushstrokes on an almost blank canvas, Rhys displayed particularly broad vision. He saw from the start the importance of not merely digging up sites but of doing so within a context of investigating relationships between people and landscape and the potential effects of environmental change deduced from research by other scientists. At a time when the minds of many archaeologists were enthralled by typology and diffusion, Rhys was discussing such ideas as the effect of changing sea levels upon island populations and their responses to changing availability of food resources. So, while many of us set off into the field seeking to fulfil our pipe dreams of deep rich cave deposits, neatly stratified like examples in European text books, Rhys pursued a strategy that involved the investigation of middens and the resources of the shoreline around Tasmania and the Bass Strait islands.

A number of years were to elapse before Rhys found an opportunity to visit the Kent Group, a cluster of three small islands off the main shipping routes and rarely visited. Like all islands in the Bass Strait these would have once been pieces of elevated land on the Bassian Plain before the plain was inundated by rising post-glacial seas, beginning some 12,500 BP and with the sea reaching its present level around 6000 BP. An environmental change of this magnitude would have profoundly affected the pattern of human occupation. For islands like those of the Kent Group, too small to maintain a long term human population and too remote to have attracted visits by canoe, any human occupation must have occurred before the final inundation of the plain. It was the possibility of finding evidence for such occupation and perhaps more exactly dating its demise, that prompted Rhys' visit.

Although the Kent Group was not easy to reach, one the of the islands, Erith, had for some years been the holiday destination of a group of academics led by Stephen Murray-Smith. The group included Leonie Sandercock, Don Anderson and Ian Turner, all known to Rhys, which led to an invitation to join them on Erith Island during their 1978 summer vacation. I was delighted to learn that I too had been invited.

Our means of transport to Erith was by fishing boat chartered at Port Albert on the Gippsland coast. On our arrival at Port Albert we were told by the boat's skipper, Robert Gould, that because of a force 10 gale raging in the Bass Strait we would not be sailing immediately. We settled in for a comfortable stay at the local hotel which inevitably (given Rhys' gregarious nature) brought us into social contact with the town's residents. As readers familiar with the trivia of the history of Australian prehistory may know, Port Albert was the home town of John Mulvaney when his father was headmaster of the school there. While sitting in the hotel bar on our last evening at Port Albert we were surprised to hear a suspiciously too loud conversation between two other customers who referred frequently to their school days and what a stern disciplinarian 'old Mulvaney' had been, ever ready it seemed to wield the cane at the merest whiff of an aspirated aitch or a slipped dipthong. Obviously the situation had been contrived for our benefit, probably by one of the Erith Island vacationers. I even suspected some contribution from Rhys himself, knowing his reputation as humorist and raconteur.

With the waters of the strait reasonably calm on the following morning we set off and reached a sheltered cove on Erith at noon. There we were greeted by Stephen Murray-Smith who had ventured into the sea to meet us and rose above the waves like some popular depiction of Neptune: huge, bearded and naked. We were soon to learn that nudity was the norm among the vacationers. but Rhys and I managed to avoid disporting ourselves thus, reasoning that we were there for research, not to frolic on the beach. Rhys confided to me later that because of some residual sense of propriety resulting from his Welsh upbringing he could not possibly get his gear off in public.

There was some discussion between Murray-Smith and Don Anderson as to whether our camp site should be 'Kampong' or 'Q Camp', names suggesting a memory, among older members of the group, of World War 2 experiences in 'the islands' (as ex-servicemen referred to Melanesia and island Southeast Asia). Eventually we were directed to Kampong, a small clearing in tea tree scrub behind the beach, where we pitched our tent and prepared to begin an archaeological survey of the island. Because of the confines of the clearing we could not see the beach itself but had an excellent view of Murray-Smith's shack and his morning visits to a 'thunderbox', exposed on a prominent headland.

Our visit was prompted by the discovery of a stone flake eroding from a dune horizon by Robert Newton of La Trobe University during an earlier summer on the island. This site, known as the Swashway Saddle, is located just below a dip in the upland interior of the island where the wind sweeps through to both deposit and scour away windborne sands. As reported elsewhere (Jones and Lampert 1978), our investigation led to the discovery of an eroded dune face in which were stratified at least four depositional episodes, with a stone flake, charcoal and emu egg shell embedded in the bottom sedimentary unit for which a ^{14}C age of ca. 9500 BP was determined later (Jones 1987:36), while a number of flakes and bones of seal and wallaby lay scattered on the adjacent eroding surface. We intended returning to the site for a fuller examination at some future date so merely collected items then visible, which were prone to damage and dispersal by wind.

After reconnoitring the island generally without discovering any further site, we turned our attention to the Great Cave of Erith, so called because of its spaciousness, having a roof height of 10m, a depth of 23m and a mouth 8m wide. Formed during an old high sea level, its floor is ca. 20m above today's sea level. With treacherous waves pounding the nearly vertical cliff below it, access to the cave is decidedly dangerous, involving a dash across slippery rocks between waves before scaling the cliff to reach the cave mouth. For this reason we decided to make only one visit, carrying with us in rucksacks our equipment, sleeping bags and enough food for a few days.

The floor of the cave is nearly level and consists of accumulated sediments. Our aim was to seek subsurface archaeological materials by means of a small sounding. As reported elsewhere (Jones and Lampert 1978), our 1 m^2 test pit penetrated 2.2 m of varied waterborne grits and clays. In these deposits, dated later to ca. 7–9000 BP (Jones 1987:33), we found bones of several species of animal and an abundance of charcoal and one animal bone that was charred and calcined. There was enough evidence to raise our hopes but without the discovery of a definite artefact we could not say for certain that the cave had once been inhabited by humans. At a depth of 2.2 m in a 1 m^2 pit we had excavated downward as far as we could physically manage, even though our access was aided towards the end by a crude ladder made from driftwood. With considerable misgiving we had to abandon our excavation with an unknown depth of sediments still unexamined. We vowed to return at some future date, better equipped and with more time available to carry out the larger excavation that the site obviously called for. Our feeling of being cheated on this occasion was partly offset by the prospect of returning to the main camp for food a little more varied than the diet of damper and tinned sardines we had subsisted on in the cave.

The vacationers were extremely generous to us, inviting us to a meal and social entertainment most evenings. Somewhat grandiose names were given to several of these occasions. My credulity could cope with 'The President's Dinner', an excellent meal served by Stephen Murray-Smith and his wife Nita in their beachside shack, but I was flummoxed over an invitation to 'The Commodore's Ball'. On enquiry I was told that Don Anderson was Commodore of the Erith Island Yacht Squadron. Strange, I thought, when the only craft to be seen were Robert Gould's fishing boat on its regular visits to replenish (mostly liquid) supplies, and a chance visit by a yacht returning from that year's Sydney to Hobart race. However, on the evening all was revealed including the squadron's pennant, a small triangular piece of blue and red cloth prominently displayed. It transpired that the 'squadron' consisted of a single craft, a 14-footer sailed by Don Anderson on Lake Burley Griffin, but legitimately of Erith, because it flew the Island's colours.

The annual bowling tournament was another ancient Erithean tradition to which we were invited one Sunday afternoon. The bowling 'green' was an irregularly surfaced tract about the size and shape of a petanque court on the island's upland interior; the bowls any conveniently sized lump of granite that could be found lying naturally on the nearby surface. Luck rather than skill won the day and custom seemed to demand a great deal of posturing, arguing and downright deviousness. Neither Rhys nor I showed potential for the mastery of this game despite our experience with crude stone tools.

An invitation to a fancy dress party tested our inventiveness given the scant range of clothes and materials at hand. While I turned myself into a rather wan parody of a pirate, Rhys showed great ingenuity in transforming himself into his conception of a Celtic warrior. He made his hair stand out in spikes by applying a thick paste of flour and water and letting it dry; he lined his face grotesquely with charcoal; a groundsheet became a cloak; the lid of our camp oven became a shield; our machete a sword. Hard to know how such an appearance might have affected Rome's legions, but Rhys certainly impressed the guests that evening, not only with his appearance but also with his ability as a raconteur. It was as if adopting the outward guise of a silver-tongued Celt had also increased his already remarkable facility with the flow of words. A series of stories, reminiscences and histrionic enactments kept the party amused until late in the night.

With knowledge of two promising sites needing further investigation we vowed to return to Erith, and indeed ritually reaffirmed that promise, albeit with diminishing certainty, whenever we met during the years that followed. Our reasons for not returning are varied, and include the inevitable conflict with other research interests. There was also a series of tragedies involving our hosts on Erith Island. A year or so after our visit, Ian Turner called to see me at the Australian Museum from whence we repaired to the bar of the nearby New Zealand Hotel, as the Museum Tavern was then named. A few months later I was shocked to learn that Ian had died suddenly of a heart attack. I was visited shortly afterwards by Stephen Murray-Smith who in memory of Ian suggested a drink at the same bar, where we sat in the same seats and drank a silent toast to an absent friend. A few years afterwards Stephen was also dead, as indeed was the artist Fred Williams, another Erith Island vacationer whom we met there on our visit. I do not know how this spate of deaths affected Rhys but I became less enamoured by the prospect of a return visit, not so much out of superstition as from the knowledge that Erith Island would not be quite the same.

The artefact obtained from Swashway Saddle, with the associated ^{14}C date of c.9500 BP does show the presence of humans around the time that Erith was becoming finally isolated (Porch and Allen 1995:726). Too small to have supported a human population after isolation and too remote to have been visited afterwards by watercraft, there seems to be no possibility that later evidence will be discovered. Therefore the presence of charcoal and burnt animal bone in deposits dated ca. 7–9000 BP in the Great Cave of Erith must have occurred through natural causes. However, the presence of lower deposits that are as yet untested does raise the possibility that the cave will some day reveal evidence for early human occupation of the Bassian Plain.

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Raven Mad: An Analysis of Bird Bone from a West Point Midden

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THIS PAPER describes the results of the preliminary analysis of avian bone excavated from the West Point midden by Jones (1966). Bird types represented in the remains accord with the range of birds found in late Holocene cultural deposits in sites on Hunter Island although there is a marked increase in the proportion of raven bone in the West Point midden. Nearly one third of all the identifiable bird bone from the West Point sample comprised raven bone, an apparently wily and difficult bird to capture. Nevertheless, Aboriginal bird hunting strategies described in the ethnography from the west coast supports the high representation of raven in the West Point avian remains, suggesting a continuity in bird hunting practices over several thousands of years in this region. The ethnography in this case consists of the detailed journals of George Augustus Robinson, a government appointed conciliator charged in the 1830s with removing Tasmanian Aboriginal people from their homelands and re-settling them on the Bass Strait islands (Plomley 1966). His journals provide some intriguing descriptive accounts of Aboriginal bird hunting methods along the west coast of Tasmania; accounts which provide insights into the archaeological findings from West Point and in particular the predominance of raven in the bird remains.

BACKGROUND

In 1995 Darrell West spent several months as a Visiting Fellow in the Division of Archaeology and Natural History in the Research School of Pacific and Asian Studies at the Australian National University, engaged in the identification of bird bone. Prior to this he had spent some months sorting and identifying seabird remains from sites excavated by Robin Sim on the Bass Strait islands (Sim and West 1999; West and Sim 1995). Rhys Jones invited West to further utilise his skills by working on the West Point material, working in association with the late Jerry van Tets as he had on the previous project. van Tets was at that time employed in the Division and he guided West in general faunal identification procedures and those specific to birds.

As Jones intends to produce a comprehensive analysis of the faunal and other West Point remains, the preliminary findings presented here focus solely on the types of birds represented in the archaeological remains from this site. This analysis was to a large extent guided by the ethnography from the west coast region, which suggested people in the area in the nineteenth century were targetting raven, in addition to a range of seabirds.

With the exception of direct quotes from historic accounts the term 'raven' is used in preference to 'crow' when referring to the one species of the corvidae family present in mainland Tasmania, the Forest Raven (*Corvus tasmanicus*).

ETHNOGRAPHY

In the early 1830s George Augustus Robinson was appointed to conciliate the surviving Tasmanian Aboriginals and oversee their removal to a reserve on the Bass Strait islands. From 1829 and 1834 Robinson spent most of his time travelling through the Tasmanian bush in the company of the Aborigines, in this pursuit. During these years he kept a detailed journal much of which is devoted to describing the cultural ways of the different Tasmanian Aboriginal groups (Plomley 1966). Robinson's journals are the most expansive treatise on the Tasmanians from the contact and early post-contact period. Many of the more remote areas visited by Robinson had not been previously explored by Europeans, although the Aboriginal inhabitants were acquainted with the presence of the British in other regions of the island.

In the early 1830s British colonisation had yet to disrupt the Aboriginal lifestyle and customs in the west to the same extent that it had in the east and northeast of Tasmania. Despite knowledge of the European presence in Tasmania and of the associated dangers for their people, the economic and social organisation of Aboriginal people in the more remote western regions was similar to that in the past, before the intrusion of Europeans. Hence, Robinson's descriptions, along with the historic accounts from earlier European maritime explorers, offer valuable insights into Aboriginal customs prior to the disruption of their society by the British.

It should be noted however that Robinson was not an anthropologist and his journals are anecdotal and descriptive in character rather than ethnographic documents *per se*; as the editor N.J.B. Plomley has noted, Robinson suffered from the limitation of not being 'a trained observer' (Plomley 1966:3). Nonetheless, the journals include detailed accounts of Tasmanian tribes about which there are no other descriptions, and this is particularly true of the western regions where the rugged terrain and exposed coastline deterred non-indigenous explorers (Plomley 1966:3). It is from Robinson's accounts of his travels on the west and northwest coasts that information can be obtained concerning Aboriginal exploitation of birds that may be relevant to the archaeological remains from the West Point midden.

Robinson notes on a number occasions that birds constituted an important part of the Aboriginal diet and were a food of which 'all aborigines are exceedingly fond' (Plomley 1966:66; see also pp. 64, 70, 281, 330, 722). He also describes the west coast Aborigines as being more sedentary in winter, 'remaining chiefly in their huts on the sea coast' during which time terrestrial mammal hunting declined and the focus of subsistence activities shifted to marine and avian resources (Plomley 1966:736).

During the winter people congregated in a number of villages (such as West Point) which comprised clusters of dome-shaped huts located close to the sea and fresh water sources. Robinson's descriptions of the huts indicate that they were sturdy, weatherproof structures generally ca. 3 m in diameter with head-room of ca. 2 m or more (Plomley 1966:724). It is estimated that the size of bands inhabiting such villages was in the order of 40 to 50 people (Jones 1974). In Robinson's time, West Point was the main village and winter residence of the Peerapper, one of the eight bands of the Northwest Tribe (Jones 1974:332). Evidence from elephant seal and migratory bird bone in the West Point midden indicates that this particular area was being used year round, but probably less in the summer months (Bryden et al. 1999). Whatever the occupation pattern, Robinson's accounts suggest that the resident band congregating at West Point was relatively large (Plomley 1966:736). Clearly such a group must have been confident of its ability to maintain an adequate food supply given the scarcity of terrestrial fauna in winter months. As a complement to shellfish and other marine resources which were relied upon throughout winter, birds were undoubtedly a sought-after commodity.

Aboriginal methods for hunting birds

Robinson and other early explorers observed the Aboriginal Tasmanians exploiting a range of avian fauna including muttonbirds, petrels, penguins, pelicans, Cape Barren geese, ducks, black swans, currawongs and ravens (Plomley 1966, Hiatt 1967–8:111). From Robinson's journals it is clear that people collected seabirds such as prions, muttonbirds and penguins by hand from rookery burrows, and that the seasonal availability of the muttonbirds formed an important part of the movements of bands around the coastal regions in Tasmania (Jones 1974; Plomley 1966). Non-rookery birds however required greater ingenuity to capture and there are descriptions of the Tasmanian Aborigines employing missiles and waddies in conjunction with shepherding and ambushing techniques to obtain water birds such as ducks, swans, Cape Barren geese and emus (Plomley 1966:310, 393, 410, 722; Walker 1973:256). Although more difficult to capture, these other birds would have been present all year round, including times when rookery birds were unavailable.

The tree roosting habits of cormorants render them relatively susceptible to both stone missiles and shepherding methods of hunting (N. Mooney pers. comm.). Other smaller and faster birds such as gulls and ravens however would be virtually impossible to catch in reliable quantities using such methods. In the absence of firearms or bow and arrows, the only effective means of catching such birds would be traps of some kind; and Robinson's journals from his west coast travels provide some intriguing descriptions of traps being used to capture birds in these regions (Plomley 1966:722, 752).

> Saw on a point of a rock a trap which the natives had constructed to catch crows … . All the natives along this coast [west] and south make those machines…

> In walking along the coast saw numerous places where the natives had made traps to catch crows and ducks; hence persons passing along the coast may see sticks projecting from rocks. The natives erect a kind of hut under which they lay concealed. In front on a rock they place some fish fastened by stone, and when the crows come to feed they do nothing more than put out their hand and pull them in.

Plomley (1966:813) notes that there appear to be references in Robinson's journals to two different kind of traps; one a cage type which the birds enter and cannot escape, and the other a larger baited hide construction. According to Robinson the latter hide arrangements were also used to catch ducks although 'worms' rather than fish were used as bait; there are several references in the journal to the Aborigines 'trapping' ducks (Plomley 1966:752). Nevertheless, the bird traps are generally referred to as 'crow traps' in the journal, suggesting that in the main they were probably used for capturing ravens rather than ducks.

THE ARCHAEOLOGY - THE WEST POINT BIRD BONE

The West Point midden, located on the exposed northwest coast of Tasmania (Figure 1), was excavated by Jones in 1964–65. Of the numerous middens along this coast, this is one of the largest, currently forming a grassy hill of ca. 0.5 ha and comprising up to 2.5 m depth of midden deposit (Jones 1966:6). The elevated area has numerous circular depressions which indicate the locations of former Aboriginal huts and Jones' excavations encompassed one of these hut depressions (Jones 1966; Stockton 1982; Plomley 1966).

Radiocarbon dates obtained from the basal and upper levels of the excavated midden deposit indicate that the remains here were deposited sometime between ca. 1900 BP and ca. 1000 BP, that is over a span of about ca. 900 years. Both the radiocarbon dating results and the midden stratigraphy suggest that there were two phases of occupation at this part of the site, with a more rapid build-up of the lower midden unit (Jones 1966; Bryden et al. 1999).

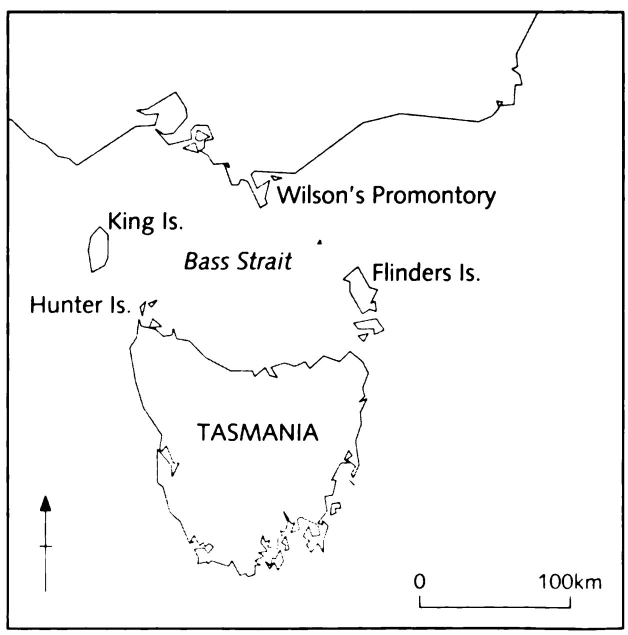

Figure 1. Site location map

Method

Avian remains analysed for this paper were all from square A - a 1 × 1 m square in the centre of the excavation with ca. 2 m of cultural deposit. In total, 349 individual bird bones with diagnostic elements, and weighing 399 grams were recovered from square A. These bird bones were distributed throughout the 12 excavated spits. Identifications were made using reference material from the Archaeology and Natural History osteological collection and the CSIRO Division of Wildlife Research and were checked by Jerry van Tets. Although the analysis described here mainly employs data concerning species (or family) identification and bone counts and weights, additional information such as skeletal element and body side data, where appropriate, was recorded during the sorting process.

Most bones were whole or substantially whole, and with few exceptions could be identified to species level. The bird bones were grouped as species or families according to standard ornithological taxonomy (Table 1).

Analysis and Results

The weight and number of bones in each of the family groups is listed in Table 2, along with the minimum number of individuals (MNI). MNIs were calculated for each species using the most numerous single element (for square A as a whole) and summing species MNIs within each family group (Table 2). The use of the same element for each species rather than most numerous element per individual spit avoided the possibility that individuals were counted more than once, should bones from a single individual have been present in more than one spit. This was a distinct possibility given that the midden was excavated in arbitrary spits where stratigraphic layers could not be defined.

From Table 2 it can be seen that raven was the most common bird, both in terms of NISPs (31%) and MNIs (20%). That raven bone weighed less than cormorant reflects the disparity in body size between these species, cormorants being generally larger and skeletally more robust birds. Similar disparities in size between other birds represented in the remains suggest that for analytical purposes, NISPs or MNIs are more useful for comparative measures than bone weights.

Table 3 categorises the birds represented in the remains according to their nesting habits as these relate directly to different hunting strategies required for obtaining birds. The nesting habits of the birds represented fall into three categories; a) oceanic birds (non-passerines) which nest on remote offshore rookeries and do not come to ground on the mainland except when accidentally washed up, drowned or weakened by storms, b) sea birds (non-passerines) which nest in burrows in rookeries, either on nearshore islands or in mainland coastal environs, and c) birds (non-passerines and passerines) which nest in trees or on the ground but which do not nest in burrows. This last category includes sea, estuarine, swamp and bush birds.

With the exception of the single diving-petrel bone, significant proportions of the bones came from birds with rookery-burrow nesting habits. This suggests that penguin, petrel, prion, and shearwater rookeries were accessible to Aborigines using the West Point area, and were all exploited (Table 2). Equally, the recovery of a single diving-petrel bone suggests that these birds did not inhabit rookeries close to West Point.

Overall however, rookery species represented 30% of the birds in the remains from square A at West Point; the remainder being species which generally do not nest in burrows. Thus the most commonly exploited birds appear to have been those that nested above ground, and moreover types that would require specific trapping or hunting skills and equipment to capture (Tables 3 and 4). Despite the apparent relative ease with which burrow-nesting sea birds could be obtained from rookeries, people were principally hunting birds which were not rookery sea birds.

The main target for predation appears to have been the raven, as the most common species amongst the bird bone remains was the Forest Raven (common crow, *Corvus tasmanicus*) (Table 5). Ironically, the raven is

Table 1: Species identified in the bird bone from Square A, West Point midden.

Bird Group	Species identified
Albatross (Diomedeidae)	Diomedea cauta (Shy or White-Capped Albatross)
Gannet (Sulidae)	*Morus serrator* (Australasian Gannet)
Penguin (Spheniscidae)	*Eudyptula minor* (Little, Fairy or Little Blue Penguin)
Petrel; Prion; Shearwater (Procellariidae)	*Pterodroma macroptera* (Great-Winged Petrel)
	Pachyptila turtur (Fairy Prion)
	Procellaria aequinoctialis (White-Chinned Petrel)
	Puffinus tenuirostris (Short-Tailed Shearwater; muttonbird)
	Puffinus huttoni (*Hutton's shearwater*)
Diving-petrel (Pelicanoididae)	*Pelecanoides urinatrix* (Common Diving-Petrel)
Cormorant (Phalacrocoracidae)	*Phalacrocorax fuscescens* (Black-Faced Cormorant)
	Phalacrocorax varius (Pied Cormorant)
	Phalacrocorax carbo (Black or Great Cormorant)
Egret; Bittern (Ardeidae)	*Egretta intermedia* (Plumed Egret)
	Egretta alba (Great Egret)
	Botaurus poiciloptilus (Brown Bittern)
Native Hen (Rallidae)	*Gallinula mortierii* (Tasmanian Native Hen)
Duck; Swan; Goose (Anatidae)	*Anas superciliosa* (Black Duck)
	Anas castanea (Chestnut Teal Duck)
	Biziura lobata (Musk Duck)
	Cygnus atratus (Black Swan)
	Cereopsis novaehollandiae (Cape Barren Goose)
Gull; Tern (Laridae)	*Larus pacificus* (Pacific Gull)
	Larus dominicanus (Dominican Gull)
	Larus novaehollandiae (Silver or Franklin's Gull)
	Sterna bergii (Crested Tern)
Oystercatcher (Haematopodidae)	*Haematopus ostralegus* (Pied Oystercatcher) or
	Haematopus fuliginosus (Sooty Oystercatcher)
Magpie; Currawong (Cracticidae)	*Gymnorhina tibicen* (Australian Magpie)
	Strepera fuliginosa (Black Currawong)
Raven (Corvidae)	*Corvus tasmanicus* (Forest Raven)

Table 2: Bone weights, numbers and MNIs from combined spits in Square A.

Bird Group	Wt (g)	MNI	MNI(%)	NISP	NISP(%)
Albatross;	43.06	2	3	13	4
Gannet	0.79	1	2	1	0
Penguin	32.21	6	11	38	11
Petrel; Prion; Shearwater	79.53	8	14	69	20
Diving-petrel	0.40	1	2	1	0
Cormorant	103.00	7	12	65	19
Egret; Bittern	3.59	4	7	4	1
Native Hen	0.27	1	2	1	0
Duck; Swan; Goose	27.76	5	9	9	3
Gull; Tern	29.67	5	9	26	8
Oystercatcher	0.44	1	2	1	0
Magpie; Currawong	3.80	4	7	12	3
Raven	74.31	11	20	109	31
Total	398.83	56	100	349	100

Table 3. Nesting habits and capture methods identified for various bird groups.

Nesting Habitat	Bird Group	Methods of Capture
Remote island surface nest rookeries	Albatross; Gannet	Scavenged when washed ashore
Burrow rookeries (mainland or near islands)	Penguin Petrel; Prion Shearwater Diving-petrel	Taken by hand from rookeries and burrows
Tree or ground-surface stick/reed nests	Cormorant Egret; Bittern Native Hen Duck; Swan; Goose Gull; Tern; Oystercatcher Magpie; Currawong Raven	Missiles, traps, ambush, shepherding

Table 4. Proportion of birds represented in each of the nesting categories.

Nesting Habitat	MNI	MNI (%)	Bone (No.)	Bone (%)
Remote island surface nest rookeries	3	5	14	4
Burrow rookeries (mainland or near islands)	15	27	106	30
Tree or ground-surface stick/reed nests	38	68	229	66
Total	56	100	349	100

Table 5. Proportions of different birds in non-burrow nesting categories.

Bird Group (Tree or ground-surface nesting birds)	Species Present (No.)	MNI (%) (n = 38)	Bones (%) (n = 229)
Cormorant	3	18	28
Egret; Bittern	3	10.5	2
Native Hen	1	3	0.5
Duck; Swan; Goose	5	13	4
Gull; Tern	4	13	11
Oystercatcher	1	3	0.5
Magpie; Currawong	2	10.5	6
Raven	1	29	48
Total	20	100	100

a bird commonly known these days for its artful cunning and yet, numerically raven represents nearly 50% of the bone in the non-burrow nesting group, and more than a third of the MNIs (Tables 3 and 4). Other well represented species or groups included cormorants, ducks swan and geese, gulls and terns, with cormorants being the most intensively exploited amongst these (Table 5).

Figure 2 shows changes in bird capturing methods from the basal to upper levels in the midden, that is over the span of about 900 years of human occupation represented at this site (Bryden et al. 1999). These data indicate that hunting methods other than rookery gathering have consistently been the principal method for obtaining bird resources (see also Table 2). Figure 2 does not include the minor component of the bird bone sample which represents opportunistic scavenging, i.e. the albatross and gannet which were presumably washed up by storms, and which numerically represent about 4% of the bird bone sample.

The proportion of birds gathered from rookeries appears to have steadily increased in the lower levels of the midden, that is from the basal spit 12 to about spit 8. This pattern could be explained by people becoming less reliant on bird hunting technology and skills as they became more familiar with the local rookery sources near the site. Interestingly this pattern does not continue through the upper levels (Figure 2), suggesting either opportunistic changes in exploitation patterns in the more recent phase or possibly variation in the timing of site use, as the majority of the burrow nesting birds are only available seasonally. Variation could thus result from changes in resource (bird) availability and/or human habitation patterns.

What is most clear however from Figure 2 is that overall, more birds were actively hunted than collected from burrows and rookeries. Surprisingly, of the birds being hunted, the raven was the predominant species in nearly half of the excavated spits (Figure 3). Moreover, raven was the only species represented in all spits. Three species of cormorants comprise the second most common bird group, but this group is not represented in 25% of the spits.

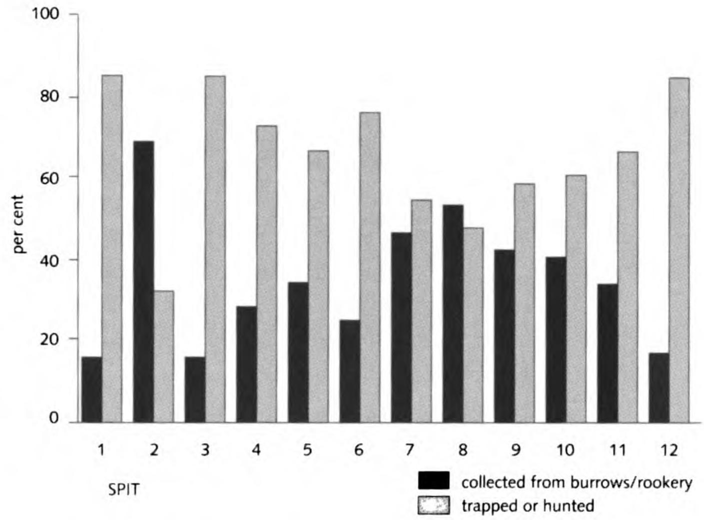

Figure 2. Burrow nesting birds (collected) versus other birds (hunted or trapped) in each spit in Square A - calculated using numbers of individual bones.

Summary of results

In summary, the analysis of the archaeological bird bone from the West Point midden sample indicates that there was consistent exploitation of a range of both sea and terrestrial birds over time at West Point. Because of the different nesting habits of birds represented in the remains, it is assumed that the birds were obtained either by active hunting methods (such as shepherding, missiles or traps) or by the relatively easy means of collection from burrows and rookeries. The analysis further suggests that despite the seasonal abundance of burrow nesting seabirds, birds appear to have been more commonly obtained by active hunting. The majority of birds in both the assemblage as a whole, and in individual spits, are types that would necessitate the use of missiles, traps or other hunting strategies to capture. The most common species through time in the bird bone assemblage was the raven, a bird that generally is considered to be wily in nature and yet one which the Tasmanian Aborigines quite clearly were adept at catching.

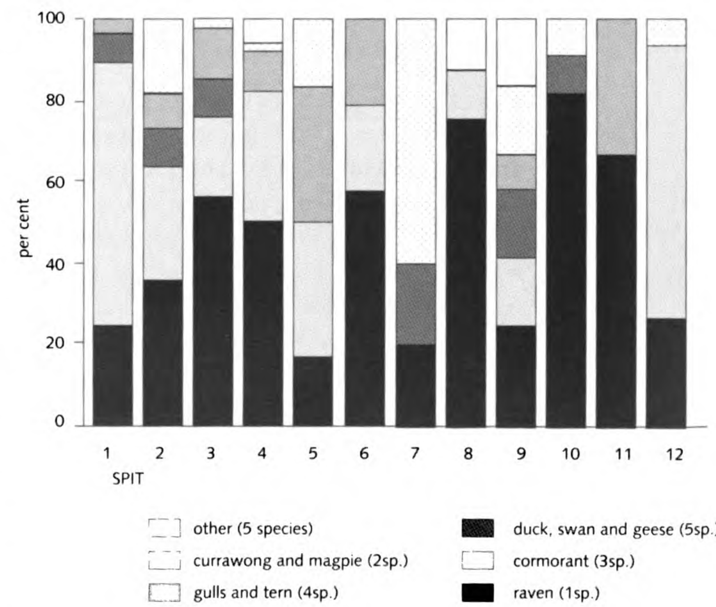

Figure 3. Bird group representation in non-burrow nesting birds (hunted or trapped) in each spit in Square A.

DISCUSSION

The range of birds represented in the West Point sample is similar to that which Bowdler (1984, see also O'Connor 1982) recovered from late Holocene deposits in sites on Hunter Island. Bird remains reflecting human exploitation found in the Cave Bay Cave and the Stockyard sites included penguin, albatross, diving-petrel, prion, shearwater, cormorant, gull, swan, duck, goose, sea eagle and raven (Bowdler 1984:63; O'Connor 1982:134). As some of the raven bone found by Bowdler in the Cave Bay Cave deposit was recovered from levels which did not contain stone artefacts, midden shell or other cultural remains, this raised the possibility that raven bone could have a non-anthropogenic origin. At the Stockyard site however the raven bone was unambiguously cultural in origin and comprised some 10% of the birds recorded (O'Connor 1982:134).

There is no explanation other than human activity that would satisfactorily account for inclusion of raven bone in the Stockyard or West Point open midden sites. Unlike cave sites, open middens do not provide the roosts or protection sought by birds of prey which arguably could deposit raven bone. Nor is there any non-anthropogenic scavenging behaviour to which the raven bone could be attributed in these contexts. Not only were the people living at West Point catching raven but the analysis indicates that raven was the most frequently captured bird species. This seems rather puzzling given the relative ease with which other birds could be gathered from rookeries and burrows. Why then were people expending time and effort to actively hunt or trap birds, and why would they target the raven?

Rookery gathering versus hunting and trapping

Arguably non-rookery birds may have been hunted because most burrow nesting birds are migratory and only inhabit the rookeries for the few months of the year during which they breed. Nevertheless, penguins do not migrate and thus would have been available from burrows all year. However, penguins comprise only a minor part of the bird component of the diet; more than 40% of the bird bones in the sample were from birds other than seabirds, that is bush and lagoon or swamp birds (Table 2). That people were choosing to exploiting birds other than seabirds suggests that there was a preference for variation in the types of bird flesh being consumed. Given the very distinctive and strong fish flavour of seabird flesh it is not surprising that other types of birds were also being sought.

Since ravens were the principal target species among the terrestrial bird bird groups, it is clear that the Aboriginal hunters had efficient techniques for capturing this species. Arguably the relatively high representation of raven could be due solely to these birds being more common than other bird types in the region. This argument however is not supported by the present-day avian regime of the northwest coast where gulls are as common or more common than ravens. And while gulls were also hunted in the past, the archaeological and historic accounts do not indicate gulls were exploited as much as ravens. Overall the archaeological evidence suggests that ravens were probably not difficult for the Aboriginal hunters to capture, although the reason for their predominance in the archaeological record might have remained somewhat enigmatic if it had not been for the 'crow trap' descriptions in Robinson's journals (Plomley 1966:722,752).

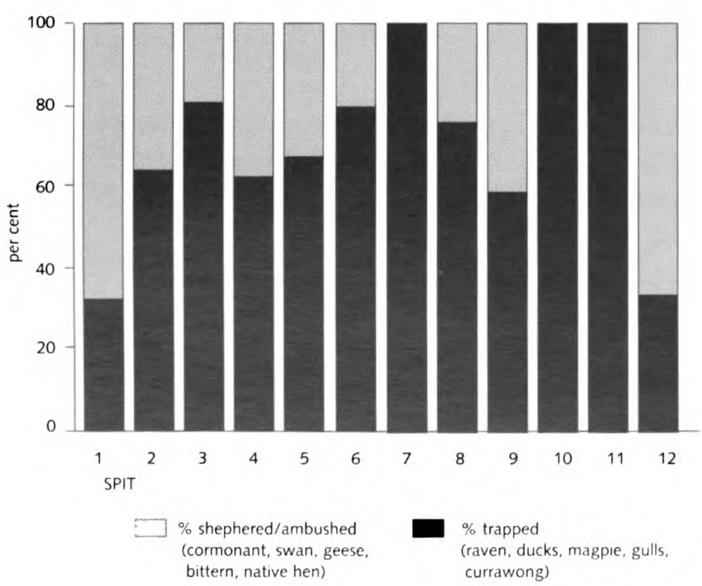

Figure 4. Proportions of non-burrow nesting birds obtained by trapping versus other active hunting methods (calculated using individual bone numbers).

Why ravens?

The evidence from West Point indicates that in the main, birds were being actively hunted rather than collected from rookeries, and that the most common hunting method was trapping (Figures 2 and 4).

This accords with historic accounts which describe west coast Aboriginal inhabitants trapping 'crows' and ducks, birds which could not be hunted readily on foot using shepherding techniques, and whose feeding habits made them susceptible to being caught in traps (Plomley 1966:722). Carrion scavengers are especially vulnerable to trapping, and on mainland Australia there are various types of Aboriginal traps used to catch kites, a scavenging bird of prey (N. Mooney pers. comm.). Kites however are extremely rare in Tasmania and other raptors generally uncommon in coastal environs of Tasmania. The exceptions to this are the Peregrine Falcon (*Falco peregrinus*) which prefers a cliffed habitat, and the White-breasted Sea-Eagle (*Haliaeetus leucogaster*) which are not numerous (Pizzey 1980; Slater 1979). Of the other scavengers, only ravens and gulls are very common on the west coast of Tasmania, with magpies and currawongs common but less numerous (A. McGarvie pers. comm.). Hence the only common non-seabird along the west coast which would have been susceptible to being caught in traps would have been the raven.

While not as common as ravens, ducks too were trapped using a variation of the 'crow trap' described by Robinson (Plomley 1966:722,752). The archaeological remains from West Point however suggest that ducks were not a major avian food resource at this site (Table 2). This is almost certainly a reflection of the lesser availability of duck in most of the west coast environs. At the Stockyard midden site on Hunter Island where duck is more common, the duck MNI was almost equal to that of raven, and there is no reason to expect ravens to be less common on Hunter Island than elsewhere in Tasmania (O'Connor 1982:134).

Hence it appears that Aboriginal people of the west coast were probably targetting crows for a combination of reasons; being scavengers these birds could be enticed into traps, they were common, available year round and provided dietary variation from seabirds. Both Robinson's west coast accounts of 'crow traps' and the archaeological record from West Point support the view that ravens were one of the main avian food resources, despite the abundance of more easily obtainable seabirds such as penguins and seasonal seabirds. It would therefore seem logical that the trapping technology was refined specifically to target ravens. Interestingly the radiocarbon dates from the excavated West Point midden indicate that Aboriginal use of this specific site probably ceased some thousand or so years ago, and yet the focus on raven as a principal avian food resource continued at least until the 1830s. Moreover the pattern of raven exploitation did not substantially change in the span of nine hundred years or so of human occupation represented in the excavated West Point remains. This evidence strongly suggests that bird traps, possibly of a type similar to those described by Robinson have been part of the west coast technology for several thousands of years.

Of interest here is how the historic accounts not only concur with, but enhance the archaeology. Instead of simply gaining an impression of the avian component of the diet, we have an image of people gathering sticks and branches, weaving dome-like structures, lying in wait, bagging the game and the pleasure of the recipients of the spoils from the bird traps. We also have an understanding of why particular bird types are being targetted: without the ethnography we would have no traps, and merely an interesting pattern in the archaeological remains indicating an apparent preference for ravens.

CONCLUSION

The disruption that European colonisation wrought on Aboriginal society in Tasmania means that without historical accounts such as Robinson's journals we would be hard pressed to explain some of the patterns we find in the archaeological remains. Although archaeologists are unlikely to find technology like bird traps, knotted grass wallaby snares, wooden-staked traps, game drive nets and watercraft *per se* in the archaeological remains, they can look for evidence of the use of such technology. Despite inherent limitations in the use of ethnographic and historic accounts in archaeological interpretation, such documents are nonetheless frequently useful. In this instance the ethnography and the archaeology together indicate that the targetting of ravens was a west coast practice that spanned several thousand years at least before the period of first non-Aboriginal contact on the west coast of Tasmania.

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Prey Choice and Hunting Strategies in the Late Pleistocene: Evidence from Southwest Tasmania

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IN THE early months of 1981 Rhys Jones and Tasmanian colleagues excavated at the Southwest Tasmanian limestone cave now called Kutikina. This began more than a decade of concentrated fieldwork in the region which has resulted in Southwest Tasmania yielding the most complete regional sequence of late Pleistocene archaeology anywhere in Australia (Allen and Cosgrove 1996:5–10). The single, small test square dug in Kutikina gave dates for occupation between ca. 15,000 BP and ca. 20,000 BP, indicating that people had used the cave through the height of the last glacial maximum. As well, it produced large quantities of stone tools and animal bones, the remains of human meals. Amongst these, one species of macropod, the Bennett's wallaby (*Macropus rufogriseus rufogriseus*) predominated.

Jones was immediately struck by the general parallel between these 'most southerly ice age hunters on earth', and their more famous relatives of similar antiquity in southwest Europe. This was drawn out in the first report of Kutikina in Nature (Kiernan, Jones and Ranson 1983). In particular, Jones saw a similarity between what he called the targetting of a single species of wallaby in Tasmania with the well documented occurrence of one or a few species dominating the faunal sequences of the southwest European Upper Palaeolithic: 'The specific targetting onto reindeer by the European hunters bears comparison with the similar emphasis on wallabies by sub-Antarctic palaeo-Tasmanians' (Kiernan et al. 1983:31).

The use of the term 'targetting' in this context is straightforward, indicating the deliberate capture of one species of prey among a number of possible prey species choices. While all predators, including humans, select particular prey species, the important distinction here is between opportunistic capture of any edible prey at one extreme and the deliberate selection of one or a few prey species at the other. Also, one implication of deliberate prey choice is that the ranking of prey species may change as various conditions (including environmental, technological and demographic ones) also change through space and/or time. However, as we will demonstrate, Bennett's wallaby appears to have remained the primary prey choice in the uplands regions of Tasmania through the Pleistocene and the Holocene. This is surprising, because this region clearly experienced changes in these conditions during this long period, with perturbations in any one of them influencing the distribution and quality of resources on the landscape. The expected behavioural responses of hunters to such influences might include (a) diversification of prey species, (b) changes in mobility strategies, (c) physical storage, and/or (d) evidence of exchange (Halstead and O'Shea 1989:3–4). The archaeological correlates of these changes might be, respectively, a larger range of species (even though, in our case, Bennett's wallaby maintained its primary ranking throughout), increased numbers of exotic stone artefact raw materials, a wider range of anatomical parts preserved, and foreign items such as marine shell or stone from distant sources.

Beyond this simple definition of prey choice are various implications of such a strategy, including specialisation and scheduling, which in turn can be seen as measures of modern human behaviour.

AIMS

This paper sets out to examine the degree to which prey choice was an economic strategy used by late Pleistocene hunters in Tasmania. To do this we employ the data from Southwest Tasmanian sites investigated as part of the Southern Forests Archaeological Project (Allen 1996a).

Put most simply, the question is whether Late Pleistocene hunter-gatherers in Tasmania merely took prey animals roughly in the proportions that they were encountered on the landscape, or whether these hunters deliberately by-passed the capture of some species in order to take one or a few other species. It is implicit here that the reason(s) for doing this would be amongst those economic considerations advanced by optimal foraging strategists: low pursuit time; ease of capture; ease of field butchery, transport and preparation; nutritional return; and incidental benefits such as raw material by-products — skins, sinews and bones for tool-making. For this reason we draw on aspects of optimal foraging theory (e.g. MacArthur and Pianka 1966; Charnov 1976; Stephens and Krebs 1986) which are appropriate to our ecological approach.

However the choice between these two propositions is not straightforward. Primarily it relies on an ability to reconstruct the community ecologies of all prey animals available on the prehistoric landscape at any given time. Obviously this is always difficult and frequently impossible. The use of natural sites such as drop pits, owl roosts and carnivore dens provide some evidence on past distributions of animal species independent of the biases of human sites, but these sites are themselves subject to other biases in their formation, as we discuss later in the paper. This situation prevents the obvious approach of directly comparing the proportional representation of animal species in archaeological sites with their proportional representation in the landscape. Instead, as often happens in archaeology, we are forced to approach the question in a less direct and less precise manner.

We begin by reviewing the Old World evidence for the specialised hunting of prey species as an Upper Palaeolithic characteristic behaviour, in order to develop some of the ideas that have emerged from Old World faunal studies for comparison with the Tasmanian situation. We then elaborate an environmental basis for hunter-gatherer hunting activities in Southwestern Tasmania during the late Pleistocene. We next consider the ecologies of potential game animals in the region, together with the faunal evidence from the Southwestern Tasmanian sites, in order to test propositions concerning both prey choice and the wider hunting strategies of the Southwest Pleistocene Tasmanians. This procedure enables us to narrow the possibilities and approach plausible conclusions that make the fewest assumptions.

We see three procedures appropriate to the data investigation. The first is to consider what species are and are not represented in these sites and the proportions in which those present occur. The second is to consider what little evidence exists for seasonality, since trying to estimate when, seasonally, humans used the Southwest Tasmanian valleys (and whether this changed through time) is directly pertinent to prey animal ecologies and therefore to any consideration of strategy. The last is to consider 'carcase management' (Binford 1978:94; Boyle 1997), in this case the body part distributions of Bennett's wallaby, to see, in addition to whether whole animals were returned to cave sites, whether the data at hand can inform on other aspects of logistical organisation across the landscape.

SOUTHWESTERN EUROPE

The move from the Middle Palaeolithic to the Upper Palaeolithic in Europe (and its equivalents in western Asia and Africa) represents one of the most dramatic changes in human behaviour seen in human history. Stone tool technology, the proliferation of bone tools and art objects, parietal art, dwellings, burials and other facets of material culture all reflect elaboration and variability unlike anything that had preceded it, implying concomitant changes in social organisation and ideology. This package comprises those things labelled 'modern human behaviour' (see Klein 1999:514ff for a recent review).

Within the objective of defining and researching modern human behaviour archaeologically, in recent times much more emphasis has been placed on analysing archaeological faunas from Upper Palaeolithic sites in Europe in terms of the social and economic systems they might represent (e.g. Straus 1983; Chase 1989; Boyle 1990; Jackson and Thacker 1997; Castel et al. 1998). These studies have examined frequencies of species, body element counts, butchery data such as cut marks, and seasonality patterns inferred from modern ecological studies of animals. Such a direction is quite different to earlier approaches where faunal data were used mainly as indicators of past climatic conditions, rather than measures of change in human behaviour, which might then be tested against patterns of climate change established independently of the archaeological record (Boyle 1990:29, 282).

In keeping with the notion that the hunted meat component of hunter-gatherer diets is generally higher in the higher latitudes, game seems likely to have been plentiful in late Pleistocene Europe where glacial conditions promoted the spread of grassy steppe that favoured gregarious herbivores such as the reindeer, an animal which reached as far south as the Pyrenees. Reindeer are common in Upper Palaeolithic French sites and dominate faunal assemblages there during the Solutrean and Magdalenian periods, between ca. 20,000 BP and ca. 11,000 BP. Elsewhere, single species of other large herbivores, such as bison on the south Russian plain (Soffer 1990) and red deer in northern Spain (Clark and Straus 1983) also dominated some regional sequences at this time. However, as Boyle's (1990:222-28) analysis of major French sites shows, single species dominance varied considerably across regions and through time, a finding supported by Burke and Pike-Tay (1997) whose analyses also reveal more species diversity than has been previously recognised.

At the time that Kiernan et al. (1983) proposed the 'targetting' comparison between late Palaeolithic Tasmania and southwest Europe, the view was widespread (eg Straus 1977; White 1982; Mellars 1989) that single species capture indicated hunting specialisation as a distinguishing characteristic of modern human behaviour which separated the Upper Palaeolithic of Europe from the Middle Palaeolithic. Since that time, however, it has been pointed out that the tendency for a single species to dominate some archaeological assemblages also occurs commonly in Mousterian sites in France, Germany, Poland, the Crimea and Russia (Gaudzinski 1996; Klein 1999:532–4; Burke 2000). It has also been argued (Rigaud and Simek 1990) that climatic downturn during the last glacial maximum virtually eliminated herbivores other than the reindeer in southern France, so that these assemblages actually reflect seriously reduced numbers of prey species amongst the available larger ungulates, rather than 'targetting' *per se*. Boyle (1990:279), on the other hand, doubted whether during the Magdalenian, reindeer ever represented 90% or more of the available prey species. While she rejected Chase's (1986) definition of specialist (single species) hunting as applicable to Upper Palaeolithic France, because her study had indicated the continuing importance of other minor species, she opted for what she called 'selective foraging' - the purposeful selection of a small number of prey species, normally fewer than four. In defending this view she pointed to the dietary deficiencies of using a single high protein/low fat prey species.

In an initial account of our own early excavations in Southwest Tasmania (Cosgrove et al. 1990) we raised some similar issues for the Tasmanian data. What is much clearer now is that we have an independent test situation with to investigate the dichotomy between environmental and behavioural explanations raised by the European data. Some factors are similar - early behaviourally modern humans, high latitude glacial conditions - while others, like the variety and ecologies of the prey species, are particularly different. While we do not have an equivalent of the Middle Palaeolithic for comparison, in the Tasmanian case any specialisations or efficiencies which might be apparent in the data cannot be related to the appearance of a more elaborate blade tool technology, as is done in Europe, because it is absent throughout the Tasmanian prehistoric sequence (Holdaway and Cosgrove 1997).

LATE PLEISTOCENE ENVIRONMENTS IN SOUTHWEST TASMANIA

In proceeding to examine presence/absence of prey species in the Tasmanian faunal assemblages, it is obvious that hunters could only take prey animals actually present on the landscape. It is therefore necessary to assess the sorts of landscapes in which these hunters operated, and how they might have changed through variable climatic periods. An immediate problem is the difficulty of recognising short to medium time scales and local variations in both the archaeological and environmental records for late Pleistocene Tasmania. As good as both records are, the available scales and levels of resolution conspire to obscure stochastic variability within them. Limited space precludes a fuller discussion of this point, but see Cosgrove and Allen (1996:23-4). In this paper we review both records in terms of five large-scale periods, before, during and after the last glacial maximum, defined more precisely for the archaeological data below.

On present evidence humans first entered the rugged mountains and valleys of Southwest Tasmania ca. 34,000 BP, during an interstadial period (Isotope Stage 3) when the climate was cooler and drier than at present. Colhoun et al. (1996:42) suggest that during the last glacial maximum 1,085 km^2 of Tasmania were glaciated, with limited cirque and valley glaciation occurring throughout the Southwest. Temperatures fell to 6.5° C below present minima at the last glacial maximum (Colhoun 2000). It is widely held that Tasmania at this time was dominated by grass, heath and shrub taxa, with pollen studies indicating alpine/sub-alpine moorlands and herbfields and some coniferous forests (Cosgrove 1995a; Porch and Allen 1995:716–17; Colhoun 2000), the general pattern being the altitudinal lowering of distinctly layered vegetation zones so that subalpine and alpine components dominated. Kirkpatrick (1986) suggested that grasslands and herbfields would have been extensive along fertile valley floors; however these vegetation types would have been absent on the more extensive infertile soils and quartzite substrates, where they would have been replaced by stunted woody taxa. Cosgrove (1991, 1995a), following this line of argument, developed the palaeoecological model described and used below.

Palaeoenvironmental reconstructions (eg Colhoun et al. 1990; Colhoun et al. 1991; Colhoun et al. 1992; van de Geer et al. 1991), indicate a warmer and wetter climate from ca. 14,000 BP or earlier, with the rapid expansion of arboreal taxa and the establishment of rainforest and wet sclerophyll; existing ice sheets seems to have fully retreated by 13-10,000 BP. At higher altitudes alpine communities were replaced by rainforest taxa between 12,000 BP and 10,000 BP. Markgraf et al. (1986) suggest that this period is marked by strong seasonal rainfall and cool to cold conditions. After ca. 17,000 BP the record reflects an increase in both the number of human sites occupied and deposition rates in them, but by ca. 12,000 BP all but one of the known Southwestern inland valley sites were abandoned and these valleys were apparently never re-occupied by humans. Nunamira Cave appears to be the most recently abandoned of the excavated sites, with limited human occupation between 12,000 BP and 10,000 BP. This site is in the easternmost valley of the Southwest zone; its river, the Florentine, drains eastwards, unlike most of the Southwest rivers. This valley would logically have been the easiest valley to continue to access from the east.

The total depopulation of the Southwest at the end of the Pleistocene is attributed to the return of the forests driving out the game animals on which the humans depended.

Ecological Patch Model

Kiernan et al. (1983) originally proposed that widespread steppe grassland existed in Pleistocene Southwest Tasmania and that this attracted the macropods that people in turn hunted. However, as stated, Kirkpatrick (1986, 1997:8; Kirkpatrick and Duncan 1987) argued that grasses would only survive on the restricted alluvial and limestone substrates during the last glacial maximum. Conversely the widespread siliceous soils in Southwest Tasmania would have supported only infertile shrub and buttongrass sedge, and these areas would have been large. No *Poa* spp. grow on these substrates today and only inhabit, as refuge plants, the well-drained fertile soils associated with limestone outcrops. Thus the last glacial maximum vegetation communities were most likely distinguished by limited grassland patches on fertile soils along the limestone valley bottoms, surrounded by infertile low shrub and heath vegetation growing on siliceous soils (Cosgrove 1991, 1995a; Cosgrove et al. 1990). These patches were attractive habitats for the Bennett's wallaby to gather, in much the same way as the marsupial 'lawns' are used by macropods today (Cosgrove 1999b).

Given the sedentary nature of Bennett's wallabies, reviewed below, concentrations of this species certainly occurred on these patches, in close proximity to caves within the limestone outcrops that could provide shelter to human hunters. Compared to eastern Tasmania, the Southwest had a relatively more stable ecosystem during the last glacial maximum. Eastern Tasmania had low precipitation and evaporation (Bowden 1983), an unpredictable moisture supply, was cold and drought prone, and possessed scattered and varied resources. Reduction in vegetation caused soil erosion, as indicated by lunettes and sand dunes thought to date to the last glacial period in northeast and southeast Tasmania (Sigleo and Colhoun 1982; Duller and Augustinus 1997). Overall the ecological data suggest that western and eastern Tasmanian ecosystems were different and this had an important effect on the structure and distribution of human prey animals and plant resources during the late Pleistocene.

The ecological differences between eastern and western Tasmania were modelled by Cosgrove and first published by Cosgrove et al. (1990). Fig. 1 is an upgraded version which schematically represents the dynamics of the two zones (Cosgrove 1999b) and attempts to highlight the human/landscape relationships, as well as suggesting possible links with zones outside the Southwest. The model is not intended to reflect an unchanging ecological structure for the entire late Pleistocene, because the magnitude and frequency of ecosystem fluctuations must have varied that structure over time. Indeed the ecology probably varied continually at small and medium scales over the long term. However, regardless of this variability, the general associations of geology, soils and plant communities are assumed to have maintained the grass patch/scrub mosaic at different configurations in the region throughout the late Pleistocene. Most importantly the model allows the archaeological record to be investigated within an ecological framework.

The model suggests that grassland patches clumped animal resources in a predictable way. Human groups moved between them, capturing ecologically 'tethered' wallabies which were consumed at nearby caves. When the returns from hunting fell below a certain point, people moved to the next patch. Group movements were not necessarily continuous, although we assume they involved either seasonal or intermittent contact with the coast and other areas outside the Southwest; however almost no evidence for this contact has yet been identified. Outside the Southwest valleys, and especially in the drier eastern part of Tasmania, people were likely more mobile and moved between resources which were relatively more scattered (Cosgrove 1999b).

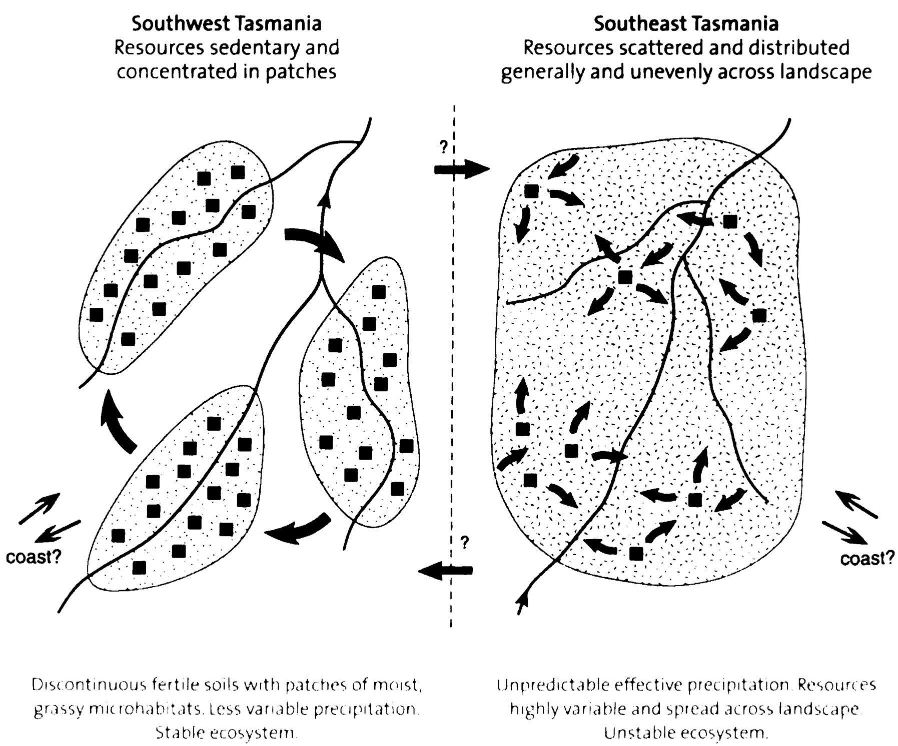

Figure 1.

Prey animal ecologies and behaviours in Tasmania

The degree to which it is appropriate to draw on modern animal studies to infer past behaviours of prey animals is an acknowledged problem, but we know of no satisfactory alternative as a starting point to explore the conditions which determined past distributions of species and their behaviours. As with our environmental reconstructions, assessing the absence, presence and densities of any species at different times in the remote past depends on our modelling of given situations using proxy data from ethnography, behavioural ecology and biogeography. In other words, our conclusions are constrained by the quality of the data and the theories used to articulate those data (Winder 1997:616).

In the descriptions which follow, this reservation is mostly unstated, but remains. That a large majority of the species under consideration are extant in Tasmania tends to ameliorate this problem, in contrast to Palaeolithic Europe.

Table 1 contains a list of terrestrial species which might conceivably have been human food prey during the Pleistocene in the study area (no marine food remains have been excavated from the Southwest sites, and coastal connections are not pursued further here). Excluding an ethnographic reference to the mouse, *Antechinus* sp., as food, these include all the Tasmanian non-marine animals recorded ethnographically as human prey (Hiatt 1967/8) and/or recorded archaeologically as human prey in Tasmania outside the study area. Since Tasmanian sites routinely contain faunal refuse from non-human predators as well as humans, we have arbitrarily excluded most species ambiguous as to predator (see, for example, Bowdler 1984:75–89), some of which we discuss further below. This relatively short list probably reflects the biogeographic principle that more species are found in areas of mild climatic conditions and higher primary biomass than in areas where extreme conditions apply (Boyle 1990:277). Rounsevell et al. (1991:714) observe that lower species diversity is also typical of islands, pointing out that diversity in Tasmania is considerably lower than in coastal southeastern Australia, where vegetation is comparable. Since Tasmania was connected to the Australian mainland during the late Pleistocene, our list could therefore perhaps be lengthened. However, apart from the claimed presence of the toolache wallaby (*Macropus greyi*) in Cave Bay Cave (Bowdler 1984:65) no terrestrial species extant in southern mainland Australia but absent in Tasmania has been recognised in excavated assemblages, so that this point is also omitted from further consideration.

Table 1 has excluded the following four groups of animals.

1. Amongst the marsupial carnivores, the now extinct thylacine (*Thylacinus cynocephalus*) and the Tasmanian devil (*Sarcophilus harrisii*) are only rarely recorded ethnographically as food, and in each case as hearsay evidence rather than direct observation (Plomley 1966: 527; Hiatt 1967/8:111-12). Bones of these animals are very rare in archaeological deposits in Tasmania; at Cave Bay Cave, Bowdler (1984:52,73) identified only a single tooth of each species in more than 20 kg of faunal material. While the detailed habitat requirements of the thylacine are unknown, the widespread distribution of the Tasmanian devil through most habitats and along river systems suggests it was probably common in the Pleistocene Southwest.

2. A range of small terrestrial mammals some of which were likely present in the Southwest but which, on size, are considered improbable as human prey; where they occur in archaeological sites, they are normally attributed to non-human predators (e.g. Bowdler 1984). These include *Antechinus* spp., *Sminthopsis leucomis, Cercartetus* spp., *Hydromys chrysogaster, Pseudomys higginsi, Mastacomys fuscus and Rattus lutreolus.*

3. Bats, snakes, lizards, birds, frogs and fresh water fish. Again, any presence in the archaeological record of some of these animals is ambiguous as to predator. Normally, these species form only minor components of inland faunal assemblages and this is the case with the Southwest sites. From the ethnography, Hiatt (1967/8: 111–12, 115) lists only lizards, pigeons and crows as human food (see also Sim and West this volume), and notes that frogs are not widespread in Tasmania.

4. The group of larger herbivorous extinct marsupials referred to as megafauna. These animals are known to have been in Tasmania in the Pleistocene, but not necessarily in the Southwest or contemporaneously with humans. We return to these animals in the discussion.

Table 1. A summary of principal human food prey animals in Tasmania. This list is not exhaustive and excludes various other potential food animal categories discussed in the text. Fuller discussion of the ecologies of some of these listed animals also occurs in the text. 'Preferred habitat' and 'cold/wet tolerant' refer to present-day conditions; 'sociability' to the propensity for animals to group in herds or remain solitary; 'Aboriginal food prey' to whether there is archaeological or ethnographic evidence for these animals forming part of Aboriginal diet *beyond the Southwest Pleistocene sites*; and 'Pleistocene presence' to our prediction of the probable presence of these animals in the Southwestern region of Pleistocene Tasmania based on the available evidence, *exclusive of presence/absence in the Southwest archaeological sites*. Abbreviations: m = male; f = female; Wi = widespread; E, W, N, NW, NE = east, et cetera; CM = central midlands; F = dry forest; RF = rainforest; WS = wet sclerophyll forest; OGW = open grassy woodland; DU = dense undergrowth; GC = grassy clearing; S = scrub; Se = sedgelands; H = heath; BGM = buttongrass moorland; FWLR = fresh water lakes and rivers; L = low; G = gregarious; So = solitary; A = absent; P = present; R = a restricted presence (either across space or through time); LL = a low level or scattered presence. Data from Cosgrove (1995a:70-76), Rounsevell et al. (1991), Caughley et al. (1987), Barker and Caughley (1990), Taylor (1993), Connolly and Obendorf (1998).

Common name	Scientific name	Average body wt. (kg)	Present distribution	Preferred habitat	Cold/wet tolerant	Sociability	Aboriginal food prey	Pleistocene presence
Tasmanian emu (plus eggs)	*Dromaius diemenensis*	m = 54 f = 45 egg = 0.65	extinct	OGW	L	G	Yes	R
Pademelon	*Thylogale billardiere*	m = 4–12 f = 2–10	Wi	WS/RF/F near GC	Yes	G	Yes	P
Bettong	*Bettongia gaimardi*	1.2–2.3	E	OGW	No	So	??	A
Long-nosed Potoroo	*Potorous tridactylus*	0.6–1.6	Mainly E; W/SW low	Wi/DU	Yes	So	??	LL
Platypus	*Ornithorhyncus anatinus*	m = 1.6–3.0 f = 1.0–1.75	Wi	FWLR	Yes	So	Yes	P
Echidna	*Tachyglossus aculeatus*	2–7	Wi	OGW/F	Yes	So	Yes	P
Tiger Quoll (Tiger Cat)	*Dasyurus maculatus*	4	NE/NW/W; rare in E/CM	WS/RF/S	Wet–Yes HA–No	So	?Yes	LL
Eastern Quoll (Native Cat)	*Dasyurus viverrinus*	1.3	Wi especially E	Various, not RF	Yes	So	Yes	P
Eastern barred bandicoot	*Perameles gunnii*	0.45–0.9	E/N	OGW	No	So	Yes	A
Southern brown bandicoot	*Isoodon obesulus*	1.2	E/NE/NW	S/Se/H/F	Yes	So	Yes	LL/P
Brushtail possum	*Trichosurus vulpeca*	4.5	Wi	Various, not BGM	Yes	So	Yes	P
Ringtail possum	*Pseudocheirus peregrinus*	0.7–1.0	Wi	F/WS/ wet S	Yes	So	Yes	P
Common wombat	*Vombatus ursinus*	22–39	Wi	Various, not RF	Yes	So	Yes	P
Eastern grey kangaroo	*Macropus giganteus*	m = 23–57 f = 10–36	E/NE/NW	OGW	No	G	Yes	A
Bennett's wallaby	*Macropus rufogriseus*	m = 15–27 f = 11–16	Wi	Various, not WS/RF	Yes	G	Yes	P

On the basis of Table 1, constructed without reference to the Southwest Pleistocene archaeological record, eight or nine human prey species able to cope with colder conditions and vegetation patterns generally like those proposed in the patch model above, were likely present in the region during the last glacial maximum. These are the Bennett's wallaby, the pademelon , the wombat, the platypus, the echidna, the brushtail possum, the ringtail possum, and the Eastern quoll. Given a lack of detailed knowledge of the ecology and cold tolerance of the emu, we suggest that it may have had a restricted presence when and where conditions suited. The tiger quoll remains ambiguous as human food. Hiatt (1967/8:115) identified the Eastern quoll but not the tiger quoll as food in ethnographic Tasmania. Both species are fairly common in Cave Bay Cave, where Bowdler (1984:77) regarded them as the likely prey of the Tasmanian devil. Jones (1965:115) also reported a minor presence of the Eastern Quoll but not the tiger quoll in the West Point midden. Two other potential food species, the southern brown bandicoot and the long-nosed potoroo may also have been present on a restricted basis.

Optimality models of foraging propose that faced with diet choices, a predator will move to maximise the rate of energy return against expenditure, such that some prey items encountered on the landscape may be passed over to continue searching for more profitable prey. This is because even when search time is zero (because the encounter has already been made), other costs, such as pursuit and processing time, may still yield a lower energy return than continuing to search for a different prey (Stephens and Krebs 1986:23). If such a case can be established for human hunters, then narrow prey selection has already been demonstrated in a general sense, since hunters in this situation were not merely taking prey animals as they encountered them on the landscape. It also follows that prey species may be ranked according to the various costs and benefits which make up their energy profitability. We can briefly list the most important of these.

1. Prey size. Other things being equal, large prey provide a better dividend than small prey. This primary criterion is tempered by those that follow.
2. Search time. Strictly speaking search time is rarely considered a component of rank (but see Madsen and Schmitt 1998). However, here we would argue that herd animals may be easier to encounter than solitary animals, and if multiple captures ensue, then per unit search time diminishes further. Technically speaking the herd can be considered as a single large prey item, making it more attractive to hunters than the individual animals which comprise it.
3. Pursuit time/ease of capture. Factors here include the awareness, speed, agility and endurance of the prey species; hunting technology (snares, pits, nets, spears), techniques (stalking, driving, tree-climbing); and group versus individual hunting.
4. Field processing and transport. Large animals might require partitioning in the field to transport to base camps; even when captured prey are small enough to transport whole, it might maximise energetic return to transport only the more edible parts of the carcases.

With these criteria in mind we can develop a crude ranking for the prey animals assumed to be present in the Pleistocene Southwest. On size, the three most highly preferred prey animals would be the emu, the wombat and the Bennett's wallaby, then the pademelon and brushtail, with the remainder significantly less important.

While the Tasmanian emu is now extinct, it was a little smaller than its mainland Australian relative (*D. novaehollandiae)* but is considered to have been behaviourally similar, preferring open grassy woodlands or plains (Cosgrove1995a:76). This flightless bird flocked in variously sized groups and if alarmed could run at speed to escape predators. For central Australia, O'Connell (2000) notes that the emu is always ranked high on size and fat content, but is rarely pursued by hunters on foot, once the bird is alarmed. In the desert the emu is normally ambushed at waterholes. Such limited and predictable ambush locations are unlikely to have occurred in Southwest Tasmania, where water sources were more abundant. Alternatively, given its preferred habitat, the Tasmanian emu may have been restricted to the easternmost valleys of the

Pleistocene Southwest, particularly the Florentine, which is a large open valley with direct access to the more eastern parts of Tasmania; and even there its speed and endurance perhaps made the emu a less attractive prey. If so, wombat and Bennett's wallaby were probably higher ranked prey species.

Although home ranges may overlap, wombats are solitary animals that avoid other wombats. While they are slow and easy to capture in the open, and would offer good nutritional returns to hunters, they are also nocturnal, normally spending daylight hours in their burrows. While it would be possible to dig animals from their burrows (having first determined that they were occupied), time and labour costs might argue against this capture technique. Night trapping at burrows is the technique often employed by zoologists studying wombats, and would have been the easiest capture technique available to hunters. Various studies (McIlroy 1976, 1977; Triggs 1988; Taylor 1993) suggest that wombat population densities may vary between 20 and 60 animals per km^2. Home ranges vary in size between ca. 5 ha and 24 ha.

While we have listed the Bennett's wallaby as gregarious in Table 1, it is in fact the least gregarious of the macropods. In a study of the closely related mainland species (*M. rufogriseus banksianus*), Johnson (1985) recorded average densities of 48 animals per km^2 and noted that groups larger than two or three animals rarely formed. Conversely, this species is among the most sedentary of the macropods. Female home ranges average 6–11 ha with some larger males extending to 20 ha. Within these ranges, over periods of several years, these animals may shift the centres of their activities no more than 30 m (Johnson (1987:131). Bennett's wallabies are intermediate feeders between purely grazing and browsing macropods and position themselves close to gullies and forest edges offering protective cover close to feeding areas. The home range of a female will be occupied by her female offspring for their entire lives, creating high density groups of related females with overlapping home ranges. Where resources are stable and clumped, as in the proposed patch model, predictable, high density accumulations of this prey animal would be expected (Cosgrove 1995a:100–104). This wallaby's greater predicability, low search time, and perhaps ease of capture would suggest that it should outrank the heavier wombat as a preferred prey animal.

Today, where Bennett's wallaby is common in open vegetation, the pademelon is also common if there is associated shelter. The latter species is, however, abundant in low scrub and rainforest, where the Bennett's wallaby is rare (Driessen 1988:8-9). Thus, although we list the pademelon as present in the Southwest during the Pleistocene, this presence may have been restricted to those areas providing suitable cover. This, coupled with its smaller body size, would likely have made it a lower ranked prey species until the spread of rainforest in the terminal Pleistocene altered the prevailing environment and species distribution. Thus we might predict a greater presence of this species in the upper, terminal Pleistocene layers of sites. Similarly, against the wallaby and wombat, the brushtail possum would be a lower ranked resource. Although known to travel and feed on the ground, it is mainly arboreal and nocturnal, nesting in hollow eucalypt trees, so that search and pursuit time might have been comparatively more expensive against potential return.

In summary, this assessment suggests that the Bennett's wallaby and wombat should be the preferred prey species, with the wombat taken when encountered, but with the wallaby as the primary quarry. According to whether it was present, the emu should outrank the pademelon and the brushtail possum, with other food species appearing irregularly or not at all.

Comparative species distribution data

We have previously discussed (Cosgrove and Allen 1996:24–5) the difficulties of merely adopting models from ethnographic Tasmania, which is removed from the Pleistocene Southwest both in time and space, since ethnographically the Tasmanians did not occupy the Southwestern valleys. Even so, it is useful here to examine the Aboriginal diet recorded from the alpine zone of Tasmania by Ross in 1823 and Robinson in 1831, since this region is the closest available analogue to the environments of the late Pleistocene

Southwest. Table 2 lists the number of observations of prey capture by species noted by these observers. It must be kept in mind that although the ethnographic data suggest that animal protein in the highlands formed a large part of the diet, there may be particular reasons for this pattern. Firstly, gathering was probably a low key activity performed by women away from camp and while on the move; thus Robinson may not have had many opportunities to record it, since there appears to have been 10 males and only 5 females in Robinson's party. In this circumstance co-operative hunting may have been a better strategy, and one employed more frequently and successfully. Thirdly all observations were made in spring/summer and are probably not a true reflection of the total seasonal animal protein intake.

Nevertheless, the species distribution in Table 2 can be compared with 2,040 pieces of bone collected from surface of Warragarra rockshelter under a low, protective overhang (Table 3). This site is in the upper Mersey Valley in the central highlands at an altitude of 610 m.a.s.l. The alluvial flats adjacent to the site are covered in *Poa* grassland, flanked by mixed forest. Radiocarbon dates from the upper part of the site suggest the shelter surface material is less than 400 years old. Every piece of bone in this surface collection was examined in an attempt to identify each to species and body part (Allen and Porch 1996:201–2, 212; McWilliams et al. 1999). Location, breakage patterns and the average large size of the bone pieces, together with a general lack of carnivore teeth marks suggested that this was wholly or mainly a humanly derived assemblage.

The Warragarra figures for Bennett's wallaby represent at least 44 animals, based on the presence of the distal epiphysis of the right tibia, a figure generally supported by the presence of other elements (e.g. the left femoral distal epiphysis = 31; complete mandibles = 28 left, 29 right).

Table 2. Frequency of prey species captured in the alpine zone of Tasmania as recorded by Ross in 1823 and Robinson in 1831 (Cosgrove 1984:17).

Species	Number and percentage	Season observed
Bennett's wallaby	27 (56.3)	spring 13; summer 14
Eastern grey kangaroo	3 (6.3)	summer 3
possum	8 (16.7)	spring 4; summer 4
wombat	3 (6.3)	spring 2; summer 1
quoll	2 (4.2)	spring 2; summer 1
platypus	2 (4.2)	spring 1; Summer 1
rodent	2 (4.2)	spring 2
thylacine	1 (2.1)	summer 1

Table 3. Distribution of Warragarra surface collection according to species. Data from McWilliams et al. (1999).

Species	NISP	%
Bennett's wallaby	1541	75.5
unident. macropod	224	11.0
pademelon	84	4.1
ringtail possum	72	3.5
brushtail possum	19	0.9
wombat	2	0.1
Eastern quoll	1	0.05
long-nosed potoroo	5	0.25
broad-toothed rat	1	0.05
thylacine	2	0.1
unidentified bird	6	0.3
unidentified bone	83	4.1

Both data sets indicate the dominance of Bennett's wallaby as a prey species for recent prehistoric and ethnographic hunters in the Tasmanian highlands. At Warragarra the minor representations of the pademelon and ringtail possum probably reflect nearby sclerophyll forest and rainforest. The very low presence of wombat in both samples perhaps reflects the search and pursuit time involved in its capture, despite its size as a prey animal; even so, its near absence is surprising. It, together with the thylacine, bird and small mammals, makes up less than 1% of the Warragarra assemblage. This distribution cannot be attributed to differential bone survival, as the wombat and thylacine skeletons contain large and dense bone; also the small number of unidentified bones reflect the general good condition and size of the assemblage, with 96% able to be identified to species.

Hunting techniques

We assume that in order to exploit any of these species, some hunting techniques would have been more appropriate than others, and that the effectiveness of any technique depends not only on the behavioural characteristics of the prey (solitary behaviour versus herding, small versus large ranges, nocturnal versus diurnal activity) and the nature of the vegetation cover, but also the available technology. Again, while the difficulties in adopting models from ethnographic Tasmania remain, we begin with a brief review of that material.

The simplicity of Tasmanian technology at the time of contact with Europeans is well known (Jones 1977a). Weapons consisted of wooden clubs and single-piece wooden spears, the latter fashioned using chipped stone spokeshaves and having their tips sharpened and then hardened by fire. Spear-throwers and composite weapons employing attached spear points were absent. While string was made using bark fibre or grass (Jones 1977b:323–4) the manufacture and use of nets is not reported. Ethnographically, the use of bone points is unrecorded, but they are present in the archaeological record, and these and other bone tools are relatively common in Southwest Pleistocene sites as well as early to mid Holocene sites elsewhere in Tasmania. Bowdler's (1984:126) suggestion that Tasmanian bone tools were used primarily as netting needles to make fishing nets because they were mostly found in coastal sites in the period when Tasmanians fished, is now superseded by the Southwest inland data, where usewear studies by Webb suggest that the bone points there were hafted and where the impact breakage patterns of their tips indicate their possible use as spear armatures (Webb and Allen 1990). Various trapping devices or weapons such as throwing sticks may have been used in the past, but no direct evidence for these exists, apart from bird traps (Sim and West this volume).

Robinson's journals make various references to hunting strategies. Robinson (Plomley 1966:531) documents a sexual division of hunting responsibilities, with women hunting possums and other small animals and men hunting kangaroos. Essentially, larger game such as wallabies and eastern grey kangaroos were speared or hit with waddies (Plomley 1966: 520, 564–5). Lacking dogs with which to hunt, Tasmanians also captured kangaroos by using sharp stakes embedded in kangaroo pathways through scrub which impaled these animals; fire drives and driving animals into narrow defiles where they might be speared is also reported (Hiatt 1967/8:205–6). As well, co-operative drives were undertaken to herd animals into the water where they could be clubbed to death (Plomley 1966:554–5; 875). In 1804 at Risdon Cove 300 Aborigines were seen in a semicircle driving kangaroos before them (Collins 1956, cited in Barker and Caughley 1990). Barker and Caughley believe that this hunt, disrupted when the Europeans opened fire on the Aborigines, was intended to drive the animals into the water. It is widely stated (e.g. Ross 1830:97, 99; Barker and Caughley 1990:158) that Tasmanian Aborigines created and maintained grassland patches in scrub by burning to promote regrowth and attract kangaroos which might be speared. Possums were knocked out of trees with waddies or caught by women using notches cut into tree trunks to climb to possum holes. It is possible that they were also smoked out of their nests (Plomley 1966: 533, 557; Hiatt 1967/8:206–7). Platypuses were dug from their burrows or speared while swimming (Plomley 1966:534, 544). Finally Tasmanians were known to scavenge as well as hunt (Plomley 1966:214, 900).

In her European study, Boyle (1990:96ff) differentiated between opportunistic hunting, where prey is taken when encountered and required, but without forward planning; systematic hunting, which involves techniques such as stalking and the swift capture of solitary animals; and co-operative hunting, where it is difficult to approach the prey unnoticed and several or more hunters co-ordinate their activities to capture game. Boyle (1990:100) observes that herd animals are best taken using co-operative hunting, as described ethnographically above for the capture of macropods in Tasmania. We assume that all three techniques could have been used in the Pleistocene Southwest, at different times and for different species, but that macropods were taken systematically and co-operatively.

Butchery/cooking procedures

The most common ethnographically recorded Tasmanian procedure for processing macropods was to throw whole animals onto a fire to remove the fur. Their forelimbs were then broken and hind legs cut, and a hole made in the belly with the fingers so that the entrails could be pulled out. The cavity was filled with ashes and the animal roasted in the fire (Plomley 1966: 507, 510, 519, 549; Ross 1830: 99). However another reference by Robinson (Plomley 1966:539) suggests that some systematic butchery also took place, where a kangaroo's hind legs were removed and kept and the remainder of the carcase discarded. Robinson (Plomley 1966:548) suggested that the singeing of fur was common to all animals except the possum, although Ross (1830:99) saw possums being prepared in this manner near Lake Echo: 'As soon as the opossoms were singed and well heated on one side the cook turned them to the other, and then dragging them by the leg from the fire he scraped off the fur, and with a sharp flint cut out the inside, and again threw it on the fire, from which it was soon after taken and eaten, without the trouble of knife and fork, in a half-raw state.'

Robinson (Plomley 1966:539, 564) also reported that certain parts of the wallaby and kangaroo were selected for processing as secondary products. Both tail sinews and skins were collected for cloaks and cordage.

DATA ANALYSIS

A fuller analysis of the fauna from all the Southern Forest Archaeological Project sites is currently underway; here the analysis is confined to data directed to those prey choice questions.

We use data from the five Southern Forests Archaeological Project sites for which the faunal assemblages were completely analysed before all project assemblages were withdrawn from study by the Tasmanian government for political reasons (see Allen and Cosgrove 1996:10–14). Between them, these sites cover every major period of human occupation known for the late Pleistocene in Southwest Tasmania. As well, Fig. 2 demonstrates that these sites represent a good geographical spread; they also range in altitude up to ca. 400 m above sea level. The Tasmanian Southwest can be subdivided into eastern and western zones (Cosgrove et al. 1990:74) for which the Mackintosh and Warreen sites represent the west, the other three the east. The Mackintosh cave is on the northwestern edge of the Southwest region, more than 150 km from Bone Cave and Stone Cave to the southeast. These latter caves are physically small and could not reasonably have housed more than ca. 6–8 people at any one time, while Warreen was a much more spacious rockshelter, which might potentially be seen as a 'home base'.

The approximately even time periods chosen to subdivide the data here reflect the best method available for temporal comparisons between sites, but nonetheless obscure important site differences. These are discussed in detail in the first volume of the project report (Allen 1996a); here we limit comments to a few more important observations that indicate the limitations of this approach. Bone Cave occupation spans a period from ca. 30,000 BP to ca. 14,000 BP, but on the radiocarbon dates was mainly occupied in two periods: between 14,000 BP and 16,000 BP and between 23,000 BP and 24,000 BP. It was effectively or

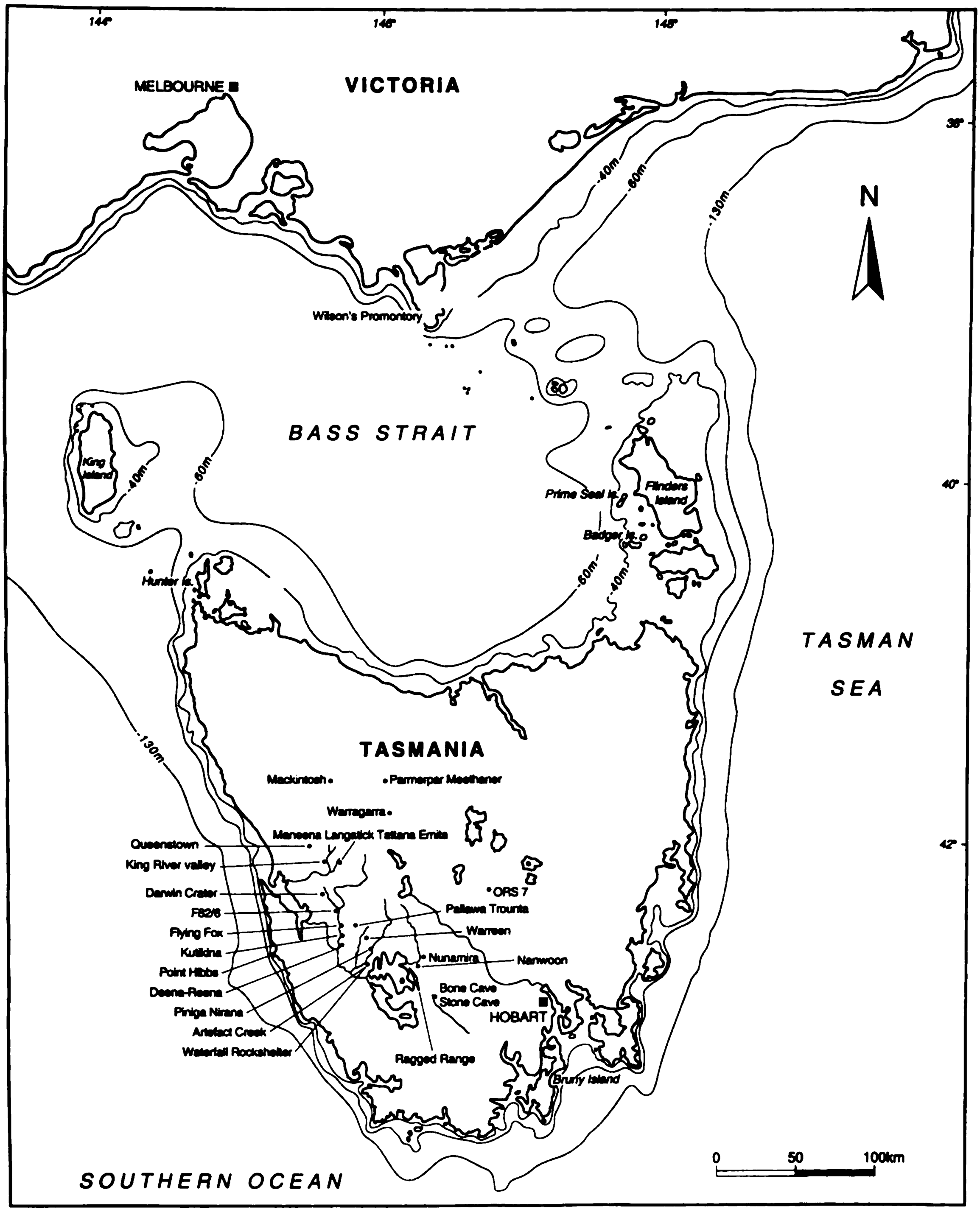

Figure 2.

totally unused for 7000 years through the period of the last glacial maximum. Thus a time slice like 19–24,000 BP obscures the fact that all the material in this unit was deposited between 23,000 BP and 24,000 BP. The data from Nunamira are less clear but show perhaps a similar downturn at the last glacial maximum. Warreen was at least sporadically occupied throughout this period, but faunal densities were exceptionally high between 22,000 BP and 24,000 BP. Because of a stratigraphic disconformity in Bone Cave square B, at least some of the remains attributed to the 25–30,000 BP time slice would actually derive from the 19-24,000 BP slice if we were to be guided solely by the radiocarbon dates. Various methods for analysing Bone Cave materials chronologically are discussed elsewhere (Allen 1996b:117–19).

The highest deposition rates for all sites except Warreen were in the period following the last glacial maximum. Warreen's apparently lower deposition rate in this period is again misleading, as this shelter collapsed between 16,000 BP and 17,000 BP and was not occupied after this event, although unexcavated sites nearby likely reflect the continued presence of humans in the area.

It should be remembered that different volumes of deposit were excavated in each site, so that faunal numbers differ between sites because of this. However, since comparisons of NISPs and MNIs between sites are not directly used in this analysis, we have made no attempt to apply correction factors which would be needed for such comparisons. Such correction factors are however available in each of the site reports (Allen 1996a).

Other potential limitations include the degree to which the sites are adequately represented by the generally small excavations in each, a question raised by Bowdler (1997). This was a consideration discussed by various authors in the first volume of the project report. Although Bowdler appears to agree that archaeological strategy is best dictated by the research aims, she failed in her criticisms to consider those aims, discussed in detail in that volume. Since our concern was and is with intersite variability rather than intrasite variability, excavating large pits in one or two sites and subsampling the assemblages for analysis - itself frequently a dubious strategy - was rejected in favour of obtaining as wide a regional overview as possible in an archaeologically pristine landscape. We believe that the robustness of the faunal sample numbers used here speak for themselves, and as previously remarked (Stern and Allen 1996:192), the similarities between the assemblages from the various excavations, further exemplified below, support the notion that the samples are representative of the sites from which they derive, and more importantly, for the region as a whole.

Prey species composition of the sites

Table 4 provides an overall view of the faunal assemblages from each of the major sites used in this study, separated into the time units discussed above. Warreen Cave reflects the only representation of pre-30,000 BP occupation available to us, but as discussed above, lacks an adequate sample from the period after the last glacial. Apart from Warreen, all sites show higher frequencies of bone disposal after the last glacial maximum. It is unclear whether the Mackintosh site has any Holocene occupation, but while we believe that all humanly derived fauna there is from the terminal Pleistocene after the last glacial maximum (see Cosgrove and Allen 1996:32–5 for discussion of this point), here we have erred on the side of caution and allowed that some might be early Holocene. Most importantly, this table shows the robust nature of the data base we are dealing with; beyond the numbers, the high levels of identification to species (Table 5) indicate the general intactness of these assemblages.

Table 5 contains essentially the same range of animals as Table 1, those terrestrial species which might have been human prey in the Southwest during the Pleistocene. As predicted, the bettong is absent, and there is only a single bone identified as Eastern barred bandicoot. The grey kangaroo is represented by four bones only from the Florentine Valley site of Nunamira. On this basis, much of the large macropod and the large and medium sized mammal bone unidentified to species is almost certainly from the Bennett's wallaby. Since we no longer have access to the collections, we are unable to verify whether these few odd attributions are correct, but suspect that they may be wrong. The potoroo may be human prey, although the brown bandicoot and ringtail possum are probably small enough to be introduced by large raptors such as the masked owl (Garvey 1999:89). Carnivores such as the Tasmanian tiger and devil may also be implicated although there is little evidence for these two animals in any of the sites. They may not den or have latrine areas like Old World carnivores (Marshall and Cosgrove 1990). The brushtail possum is larger and a potential human prey. In many sites the possum only occurs in the uppermost layers and probably heralds a return to forested conditions after the last glacial maximum. The low level presence of native hen and echidna probably also reflects inconsistent human predation.

Macropod and wombat dominate as human prey species in all sites. As can be seen from Table 5, bones which are identified as Bennett's wallaby represent between 16% and 35% of all bones identified to species and considered possible human prey animals, with this spread at least partly attributable to the differential identification abilities of the various analysts. Identified wombat bones represent between 3% and 15%.

If we take only those bones identified to species in Table 5, omitting unspecified groups such as macropod and large and medium mammals, wallaby percentages increase significantly, to between 46% and 73%, and wombats to between 13% and 26% as in Table 6.

These percentages might be further increased by allowing that large macropod is in fact Bennett's wallaby, which seems a legitimate assumption, and that 80% or more of large mammal and some of the medium mammal is also Bennett's wallaby. Such manipulations would serve to increase the percentage representation of Bennett's wallaby, but its predominance is sufficiently demonstrated for present purposes.

Table 4. Total bone distribution for the chosen sites by time periods discussed in text.

Time Unit	Bone Cave	Mackintosh Cave	Nunamira Cave	Stone Cave	Warreen Cave
Holocene	0	5549	0	0	0
10ka – 12ka	0	0	7600	0	0
13ka – 18ka	73273	5481	22857	10908	8085
19ka – 24ka	2792	0	5589	0	97988
25ka – 30ka	9204	0	8060	0	25565
31ka – 35ka	0	0	0	0	3698
Total	85269	11030	44106	10908	135336

Table 5. Potential prey species by whole site, numbers and percentages, for the chosen sites.

Taxon	Bone Cave		Mackintosh Cave		Nunamira Cave		Stone Cave		Warreen Cave	
	No	%	No	%	No	%	No	%	No	%
Dasyurus sp.	10	0.03	0	0	15	0.23	0	0	0	0
Dasyurus maculatus	3	0.01	0	0	6	0.09	0	0	0	0
Dasyurus viverrinus	84	0.30	5	0.20	8	0.12	66	5.69	212	2.22
Dromaius novaehollandiae	4	0.01	0	0	8	0.12	0	0	0	0
Emu egg shell	0	0	0	0	43	0.66	0	0	0	0
Gallinula mortierii	21	0.08	0	0	6	0.09	0	0	0	0
Isoodon obesulus	3	0.01	0	0	12	0.18	0	0	0	0
Large Macropod	1919	7.00	265	10.90	0	0	164	14.16	2251	23.67
Large Mammal	19086	66.10	537	22.10	4139	64.29	281	24.26	2211	23.25
Macropus giganteus	0	0	0	0	4	0.06	0	0	0	0
Macropus rufogriseus	4890	16.00	507	20.90	1015	15.76	379	32.72	3357	35.30
Medium Mammal	1196	4.14	507	20.90	823	12.78	66	5.69	338	3.55
Ornithorhynchus anatinus	55	0.20	1	0.04	47	0.73	14	1.20	21	0.22
Perameles gunnii	0	0	0	0	0	0	0	0	1	0.01
Potorous tridactylus	5	0.01	164	6.77	14	0.21	2	0.17	5	0.05
Pseudocheirus peregrinus	45	0.15	267	11.02	74	1.14	0	0	18	0.18
Sarcophilus harrisii	10	0.03	1	0.04	0	0	0	0	3	0.03
Tachyglossus aculeatus	0	0	2	0.08	0	0	0	0	3	0.03
Thylogale billardierii	34	0.11	20	0.82	34	0.52	0	0	18	0.18
Trichosurus vulpecula	13	0.04	0	0	3	0.04	17	1.46	27	0.28
Vombatus ursinus	1492	5.16	145	6.00	187	2.90	169	14.59	1044	10.97
Site Totals	28870	99.38	2421	99.77	6438	99.92	1158	99.94	9509	99.94
Grand Total										**48396**

Table 6. Identified Bennett's wallaby and wombat bones as percentages of all possible human prey bones identified to species level in the designated sites.

Species	Bone Cave	Mackintosh Cave	Nunamira Cave	Stone Cave	Warreen Cave
Bennett's wallaby	73	46	71	59	71
Wombat	22	13	13	26	22

Distributions of Bennett's wallaby and wombat through time

Distribution measurements of Bennett's wallaby and wombat through time employ two measures, NISP and MNI. For this exercise the large mammal and large macropod categories were concatenated with Bennett's wallaby data on the assumption that these bones derive entirely or in very large part from this species, as discussed above. Here we present the results from two of the best three sites in terms of data, Warreen and Bone Cave. The same data for Nunamira Cave have appeared elsewhere (Cosgrove 1995a:223–4).

In the calculation of MNI, teeth, vertebrae and ribs were excluded and only proximal, distal, complete and diaphysis specimens were used. We did this to avoid inflationary effects on MNI estimates of wallabies that would occur using fragments of left and right elements (Cosgrove 1999b). Comparisons between the sites follow the time periods discussed earlier. This gives some temporal control over the overall patterns and allows us to compare the patterns at, and on either side, of the last glacial maximum rather than merely pinpointing this extreme period as in previous discussions of Southwest Tasmanian archaeology (e.g. Jones 1990). In this way we attempted to identify any responses by humans (and their prey) to this event.

In Bone Cave, wallaby numbers are highest in the post-glacial period, 13,000 BP to 18,000 BP (Table 7 and 8). In Warreen Cave the highest densities occur between 19,000 BP and 24,000 BP before the roof of the shelter collapsed. Whether high density occupation would have been as sustained at Warreen in the post-glacial period, had the roof not collapsed, is unclear, but the presence of apparent post-glacial occupation in smaller shelters close by suggests that it is likely. Warreen certainly shows an increased tempo of occupation occurring just prior to the last glacial maximum, compared with much lower relative discard rates in other sites of the same time period, although, again, on the basis of the radiocarbon dates at least, a similar increased tempo has been argued for Bone Cave at this time (Allen 1996b:113).

Mean weights are consistent over both time and space, reflecting generally similar taphonomic histories even though absolute numbers fluctuate between time units. However, when individual elements are compared, some significant differences between breakage patterns emerge, as discussed further below.

Wombat NISPs and MNIs (Tables 9 and 10) are much lower overall than for Bennett's wallaby. However there are quite high numbers of particular wombat elements, especially cranial parts, teeth, mandibles, maxillae, ribs and vertebrae (McWilliams et al. 1999). For wombats, the wide representation of body parts indicates whole animals being returned to the sites, which may not be equally true of wallabies (see below). These data also parallel observations of the relative abundance and prey selection strategies of the two species recorded at the Warragarra rockshelter, discussed earlier.

Body part selection for Bennett's wallaby

Here and elsewhere (Cosgrove et al. 1990; Porch and Allen 1995; Cosgrove 1999b) we argue that humans selected both their prey species and (at least periodically) specific body parts as part of their high latitude economic strategy. One of the striking things about the Bennett's wallaby assemblage is the predominance of particular elements such as the lower limb bones, argued to reflect the deliberate extraction of marrow. Apart from the detailed analysis of this aspect at Nunamira Cave (Cosgrove 1995a) no comparable analyses have previously been attempted. Here we test this notion further with a comparison between Bone Cave and Warreen Cave distributions of wallaby elements and body parts.

Tables 11 and 12 show the range of elements grouped by body part in various time periods compared to the expected number based on the MNIs for the two sites. The total number and percentage for each body part has also been calculated, with the differences between the percentage observed and expected converted to a log scale of difference (Tables 13 and 14). This has been plotted for Bone Cave and Warreen Cave for each time period in Fig. 3. The general results reflect the over-representation of long bones and, to a lesser degree,

Table 7. Bennett's wallaby NISP and mean weight (gm).

Time Unit	Bone Cave	Mean Weight	Warreen Cave	Mean Weight
10ka – 12ka	0	0	0	0
13ka – 18ka	3660	2.18	77	0.62
19ka – 24ka	147	2.25	2236	2.14
25ka – 30ka	402	2.39	665	1.74
31ka – 35ka	0	0	62	2.11

Table 8. Bennett's wallaby MNI and the specimen used for calculation.

Time Unit	Bone Cave	Specimen	Warreen Cave	Specimen
10ka – 12ka	0		0	
13ka – 18ka	42	Right proximal tibia	2	Right Navicular
19ka – 24ka	3	Right proximal tibia	44	Left proximal tibia
25ka – 30ka	4	Diaphysis tibia	13	Right proximal tibia
31ka – 35ka	0		3	Left proximal tibia

Table 9. Wombat NISP and mean weight.

Time Unit	Bone Cave	Mean Weight	Warreen Cave	Mean Weight
10ka – 12ka	0	0	0	0
13ka – 18ka	1282	1.45	17	0.23
19ka – 24ka	65	1.47	728	3.38
25ka – 30ka	145	1.25	233	1.91
31ka – 35ka	0	0	66	2.03

Table 10. Wombat MNI and the specimen used for calculation.

Time Unit	Bone Cave	Specimen	Warreen Cave	Specimen
10ka – 12ka	0		0	
13ka – 18ka	3	Complete right clavical	1	Complete left ulna
19ka – 24ka	1	Complete left mandible	3	Right proximal ulna
25ka – 30ka	1	Complete left tibia	2	Left diaphysis tibia
31ka – 35ka	0		1	Right proximal humerus

cranial body parts in both sites, while there is an under-representation of axial, girdle, mes and pes body parts. This pattern is particularly evident in the period 19,000 BP to 30,000 BP in both sites. In the other two periods long bones are still over represented whereas cranial parts are about what would be expected given the calculated MNI. This suggests that in the two middle time slices, the fat contained in the brain was targetted in addition to the marrow cavities of the limbs. The brain is relatively small in the wallaby so this increased representation may also indicate cranial bone processing to obtain additional nutrients.

While the data are too coarse to reveal specific butchery events they tend to support previous observations on prey body part selection (Cosgrove 1995a; 1999b). Certainly the low frequencies of numerous body elements such as the hard carpals, tarsals, phalanges and caudal chevron bones suggest to us that differential field processing and transport are reflected in these sites. These bones are the hardest and most numerous in the macropod body and likely to survive better than almost any other element. Apart from identification error, possible explanations include the removal of feet and paws at butchery sites away from the caves and the removal of tail sinew, both associated with removing the skins. The data thus suggest at least two processes in wallaby utilisation, firstly the detachment of distal limb bones as riders on the skins,

Table 11. The observed and expected frequencies of Bennett's wallaby elements in Bone Cave according to time units.n/a = not applicable.

Bone cave		13ka – 18ka	MNI = 42	19ka – 24ka	MNI = 3	25ka – 30ka	MNI = 4
Body Part & Element	No. in body	Observed	Expected	Observed	Expected	Observed	Expected
Cranial							
Angle	2	1	84	0	6	0	8
Basioccipital	1	5	42	0	3	0	4
Bulla	1	18	42	3	3	6	4
Frontal	1	16	42	0	3	2	4
Interparietal	2	3	84	0	6	1	8
Lower incisor	2	36	84	2	18	14	8
Mandible	2	92	84	8	6	13	8
Maxilla	2	82	84	1	6	17	8
Molar	16	368	672	34	6	95	64
Nasal	1	9	42	0	3	1	4
Occipital	1	28	42	2	3	3	4
Orbit	2	3	84	0	6	0	8
Palatine	1	10	42	0	3	0	4
Parietal	1	24	42	0	3	1	4
Premaxilla	2	33	84	2	6	6	8
Premolar	4	20	168	0	12	6	16
Ramus	2	36	84	0	6	1	8
Sphenoid	1	5	42	0	3	0	4
Temporal	2	25	84	5	6	0	8
Zygomatic	2	35	84	4	6	1	8
Total		849	2016	61	114	167	192
Axial							
Atlas	1	10	42	1	3	2	4
Axis	1	18	42	0	3	0	4
Caudal vertebra	18	665	756	30	54	64	72
Cervical vertebra	5	17	210	1	90	3	20
Chevron vertebra	18	5	756	0	54	0	72
Lumbar vertebra	6	82	252	0	6	7	24
Rib	26	627	1092	28	78	135	104
Sacral vertebra	5	24	210	0	15	3	20
Sternum	6	9	252	0	18	0	24
Thoracic vertebra	9	67	378	2	27	9	32
Total		1524	3990	62	348	223	376
Girdle (Pectoral)							
Clavicle	2	33	84	0	6	1	8
Scapula acromion	2	3	84	0	6	0	8
Scapula blade	2	13	84	0	6	0	8
Scapula glenoid	2	8	84	0	6	0	8
Scapula spine	2	10	84	0	6	0	8
Total		67	420	0	30	1	40
Girdle (Pelvic)							
Acetabulum	2	26	84	1	6	1	8
Epipubis	2	50	84	1	6	7	8
Ilium	2	102	84	5	6	8	8
Innominate	2	39	84	0	6	3	8
Ischium	2	37	84	1	6	2	8
Pubis	2	25	84	1	6	0	8
Total		279	504	9	36	21	48

contnued over

Table 11. continued

Bone cave Body Part & Element	No. in body	13ka – 18ka Observed	MNI = 42 Expected	19ka – 24ka Observed	MNI = 3 Expected	25ka – 30ka Observed	MNI = 4 Expected
Mes/Pes							
Carpal	14	10	588	0	42	0	56
Carpal/tarsal	n/a	20		1		2	
Metacarpal	10	25	420	0	30	6	40
Metatarsal	n/a	9		0		0	
Tarsal	n/a	1		0		0	
Unidentified claw	n/a	141		8		14	
Total		206	1008	9	72	22	96
Longbone							
Astragalus	2	41	84	2	6	6	8
Calcaneum	2	93	84	2	6	5	8
Cuboid	2	42	84	1	6	1	8
Cuneiform	4	9	168	1	12	2	16
Ectocuneiform	2	22	84	1	6	1	8
Femur	2	306	84	8	6	34	8
Fibula	2	304	84	8	6	50	8
Humerus	2	117	84	5	6	18	8
Longbone epiphysis	n/a	1		0		0	
Metatarsal 2/3	2	151	84	5	6	16	8
Metatarsal 4	2	228	84	11	6	42	8
Metatarsal 5	2	121	84	6	6	23	8
Navicular	2	9	84	1	6	1	8
Patella	2	3	42	0	6	0	8
Phalange	44	239	1848	10	132	19	176
Radius	2	115	84	6	6	10	8
Sesamoid	n/a	2		0		0	
Tibia	2	488	84	24	6	54	8
Ulna	2	97	84	7	6	10	8
Total		2388	3234	98	234	292	312
Grand total		**5313**	**11172**	**239**	**720**	**726**	**1064**

Table 12. The observed and expected frequencies of Bennett's wallaby elements in Warreen Cave according to time units. n/a = not applicable.

Warreen cave		13ka–18ka	MNI=2	19ka–24ka	MNI=44	25ka–30ka	MNI=13	31ka–35ka	MNI=3
Body Part & Element	No. in body	Observed	Expected	Observed	Expected	Observed	Expected	Observed	Expected
Cranial									
Angle	2	0	4	1	88	0	26	0	6
Basioccipital	1	0	2	0	44	0	13	1	3
Bulla	1	1	2	23	44	3	13	1	3
Frontal	1	0	2	15	44	8	13	1	3
Interparietal	2	0	4	2	88	0	26	0	6
Mandible	2	0	4	41	88	7	26	1	6
Maxilla	2	2	4	49	88	12	26	1	6
Lower incisor	2	0	4	52	88	19	26	1	6
Molar	16	60	64	419	704	157	208	5	48
Nasal	1	0	2	2	44	0	13	0	3
Occipital	1	0	2	22	44	8	13	1	3
Palatine	1	0	2	2	44	0	13	1	3
Parietal	2	0	4	10	88	5	26	0	6
Premaxilla	2	0	4	30	88	11	26	0	6
Premolar	4	6	8	50	176	14	62	1	12
Ramus	2	1	4	60	88	20		0	6
Sphenoid	1	0	2	2	44	0	13	1	3
Unidentified cranial	n/a	0		41		33		1	
Unidentified incisor	n/a	32		57		0		0	
Upper incisor	n/a	0		74		45		1	
Zygomatic	2	1	4	28	88	3	26	1	6
Total		103	122	980	1980	345	569	18	135
Axial									
Atlas	1	0	2	1	44	2	13	1	3
Axis	1	0	2	2	44	1	13	0	3
Caudal vertebra	18	6	36	308	792	97	234	9	54
Cervical vertebra	5	0	10	6	220	9	65	0	15
Chevron vertebra	18	0	36	5	792	2	234	0	54
Lumbar vertebra	6	0	12	30	264	11	78	0	18
Rib	26	19	52	664	1144	244	338	14	78
Sacral vertebra	5	0	10	9	220	0	65	1	15
Sternum	6	2	12	8	264	3	78	0	18
Thoracic vertebra	9	0	18	44	396	15	117	7	27
Unidentified vertebra	n/a	4		64		7		0	
Total		31	190	1141	4180	391	1235	32	285
Girdle (Pectoral)									
Clavicle	2	0	4	16	88	3	26	0	6
Scapula blade	2	0	4	24	88	21	26	0	6
Scapula glenoid	2	1	4	34	88	9	26	0	6
Scapula spine	2	0	4	1	88	1	26	0	6
Unidentified scapula	n/a	0		17		0		0	
Total		1	16	92	352	34	104	0	24

contnued over

Table 12. continued

Warreen cave Body Part & Element	No. in body	13ka–18ka Observed	MNI=2 Expected	19ka–24ka Observed	MNI=44 Expected	25ka–30ka Observed	MNI=13 Expected	31ka–35ka Observed	MNI=3 Expected
Girdle (Pelvic)									
Acetabulum	2	2	4	11	88	11	26	0	6
Epipubis	2	0	4	26	88	9	26	1	6
Ilium	2	2	4	49	88	10	26	0	6
Innominate	2	0	4	6	88	0	26	0	6
Ischium	2	0	4	19	88	11	26	0	6
Pubis	2	0	4	24	88	5	26	0	6
Total		4	24	135	528	46	156	1	36
Mes/Pes									
Carpal	14	0	28	6	616	4	182	0	42
Carpal/tarsal	n/a	0		1		1		0	
Cuboid	2	1	4	23	88	6	26	2	6
Cuneiform	4	0	8	6	176	6	52	1	12
Ectocuneiform	2	2	4	14	88	1	26	0	6
Metacarpal	10	0	20	24	440	1	130	0	30
Phalange	44	16	88	215	1936	51		4	132
Unidentified claw	n/a	7		88		32		4	
Unidentified metatarsal	n/a	0		7		2		1	
Total		26	152	384	3344	104	416	12	228
Longbone									
Astragalus	2	1	4	25	88	11	26	1	6
Calcaneum	2	0	4	48	88	9	26	1	6
Femur	2	3	4	147	88	34	26	5	6
Fibula	2	0	4	195	88	51	26	7	6
Humerus	2	2	4	84	88	21	26	1	6
Metatarsal 2/3	2	5	4	180	88	76		7	6
Metatarsal 4	2	11	4	122	88	39		5	6
Metatarsal 5	2	4	4	78	88	14		0	6
Navicular	2	3	4	12	88	0		0	6
Radius	2	1	4	41	88	14		1	6
Tibia	2	5	4	291	88	74		10	6
Ulna	2	3	4	58	88	20		0	6
Unidentified longbone	n/a	1		5		2		0	6
Total		39	48	1286	1056	365	130	38	78

Table 13. Bone Cave. Bennett's wallaby body part representation. a = 13,000 BP – 18,000 BP; b = 19,000 –24,000 BP; c = 25,000 – 30,000 BP.

Table 13a.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	849	16.0	2016	18.0	-0.11
Axial	1524	28.6	3990	35.0	-0.20
Girdle (Pectoral)	67	1.2	420	3.7	-3.51
Girdle (Pelvic)	279	5.2	504	4.5	0.15
Mes/Pes	206	3.8	1008	9.0	-0.86
Longbone	2388	45.0	3234	29.0	0.43

Table 13b.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	61	25.0	114	15.8	0.45
Axial	62	25.0	348	48.3	-0.65
Girdle (Pectoral)	0	0	30	4.1	0
Girdle (Pelvic)	9	3.7	36	5.0	-0.30
Mes/Pes	9	3.7	72	10.0	-0.99
Longbone	98	41.0	234	32.5	0.23

Table 13c.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	167	23.0	192	18.0	0.24
Axial	223	30.7	376	35.3	-0.13
Girdle (Pectoral)	1	0.13	40	3.7	-3.34
Girdle (Pelvic)	21	2.8	48	4.5	-0.47
Mes/Pes	22	3.0	96	9.0	-1.09
Longbone	292	40.2	312	29.3	0.31

and secondly the selection of marrow bearing long bones and cranial elements for nutrient extraction. There is however a lower than expected number of ribs and vertebrae in both sites in most time periods. One explanation to account for the absence of vertebrae is that they were ground down to render the within-bone nutrients available for consumption. It is unlikely, however, that this explanation, whether or not true for vertebrae, could hold for ribs. The simplest explanation is that some wallabies were returned to the site whole, accounting for the presence of some ribs and whole vertebrae, but on other occasions they were butchered in the field.

If further substantiated, such patterns and changes through time may indicate different use of the landscape in the post-glacial and pre-glacial periods. Given all the indications of more intensive site use and/or higher population densities in the post-glacial period, as previously discussed, this apparent move away from the use of whole carcases at this time suggests a significant change in human behaviour and/or site use; the lack of resolution prevents us yet from identifying specific butchery events, but the changes through time certainly appear not to be random. Other more elaborate explanations are possible but not yet warranted.

Fragmentation

Although the mean weights of wallaby bones per analytical unit suggest little change in fragmentation through time, analysis of individual elements is a better indicator of specific differences. This is particularly true when a majority of small unidentifiable fragments contribute to the total, and also when there are grounds for believing that particular elements have been differently utilised, such as extracting or not extracting the brain. Here we simply compare pre- and post-glacial periods since they provide the most

Table 14. Warreen Cave. Bennett's wallaby body part representation. a = 13,000 BP – 18,000 BP; b = 19,000 –24,000 BP; c = 25,000 – 30,000 BP d = 31,000 – 35,000 BP.

Table 14a.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	103	19.1	122	22.1	-0.4
Axial	31	15.1	190	34.4	-0.8
Girdle (Pectoral)	1	0.4	16	2.8	-1.9
Girdle (Pelvic)	4	1.9	24	4.3	-0.8
Mes/Pes	26	12.7	152	29.1	-0.8
Longbone	39	19.1	48	8.6	0.7

Table 14b.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	980	24.4	1980	17.3	0.3
Axial	1141	28.4	4180	36.5	-0.2
Girdle (Pectoral)	92	2.2	352	3.0	-0.3
Girdle (Pelvic)	135	3.3	528	4.6	-0.3
Mes/Pes	384	9.5	3344	29.2	-1.1
Longbone	1286	32.0	1056	9.2	1.2

Table 14c.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	345	26.8	569	21.8	0.2
Axial	391	30.4	1235	47.3	-0.4
Girdle (Pectoral)	34	2.6	104	3.9	0.4
Girdle (Pelvic)	46	3.5	156	5.9	-0.5
Mes/Pes	104	8.0	416	15.9	-0.6
Longbone	365	28.4	130	4.9	1.7

Table 14d.

Body Part	NISP Observed	Percentage	NISP Expected	Percentage	Log Difference
Cranial	18	17.8	135	17.1	0.04
Axial	32	31.6	285	36.2	-0.13
Girdle (Pectoral)	0	0	24	3.0	0
Girdle (Pelvic)	1	0.9	36	4.5	-1.60
Mes/Pes	12	11.8	228	29.0	-0.80
Longbone	38	37.6	78	9.9	1.33

robust data sets. Table 15 shows the results of a series of ANOVA tests performed on selected macropod elements from combined data from Bone Cave and Warreen. As one example, Fig. 4 demonstrates the differences in Bennett's wallaby femur weights between the two periods. The comparison circles indicate a significant difference in weight between post-glacial and pre-glacial times.

The results in Table 15 indicate a significant statistical difference between the weight of individual skeletal elements in these two periods. Generally elements are larger in the pre-glacial period than in the post-glacial period tending to suggest either more intensive bone processing of wallaby bones, or their greater post-depositional fragmentation, after the glacial maximum.

In this test we combined the Warreen and Bone Cave data to increase the robustness of the results. However, since the bulk of the pre-glacial assemblage is from Warreen and most of the post-glacial one is from Bone Cave, it was considered that regional rather than temporal differences might be reflected in this result. We therefore compared the numerically uneven samples from the pre-glacial period and the post-glacial period in both sites. In each case we found no significant difference. The same tests on these elements were undertaken between these time periods within each of the sites and between the

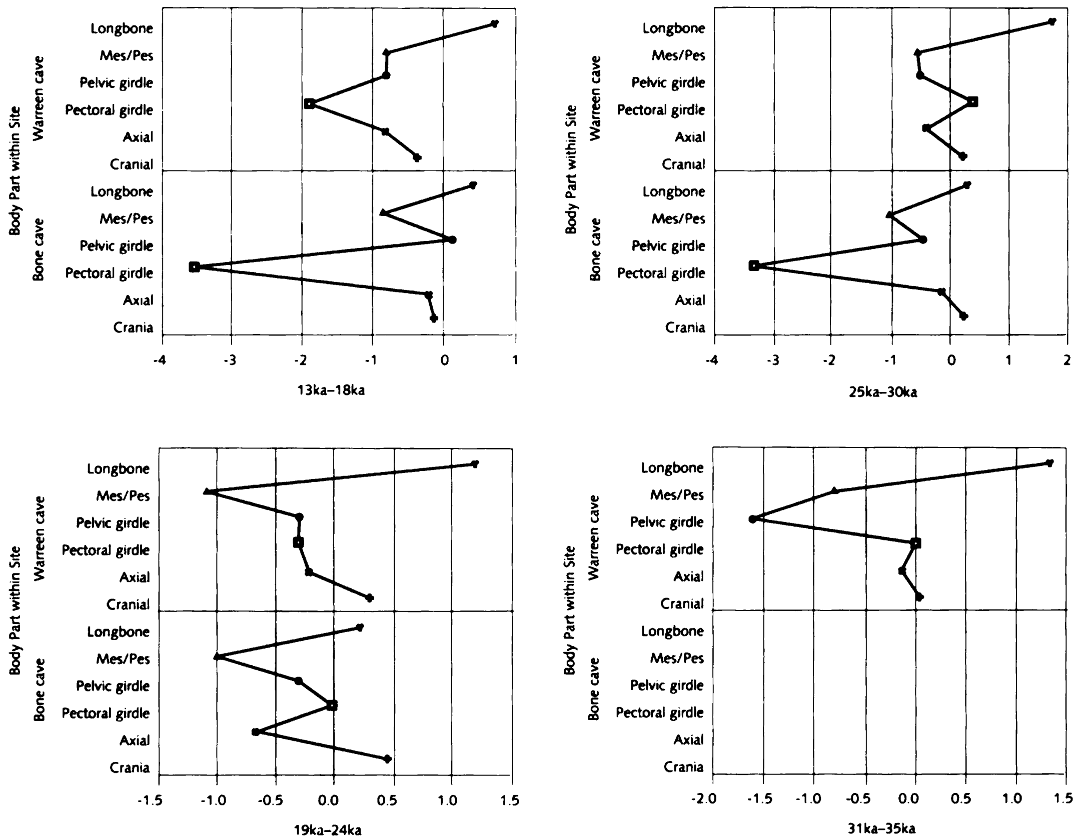

Figure 3. Ratio charts of Bennett's wallaby body part distributions in Bone Cave and Warreen Cave according to time period.

Table 15. The results of ANOVA tests determining significant statistical difference between the size of individual skeletal elements (taken on weight) before and after the last glacial maximum for Warreen Cave and Bone Cave combined data.

Element	Post-glacial	Pre-glacial	Significance 0.05 level
Cranial	Larger	Smaller	Significant difference
Maxilla	Larger	Smaller	No significant difference
Mandible	Larger	Smaller	Significant difference
Cervical vertebrae	Smaller	Larger	Significant difference
Thoracic vertebrae	Smaller	Larger	Significant difference
Lumbar vertebrae	Smaller	Larger	Significant difference
Caudal vertebrae	Smaller	Larger	Significant difference
All vertebrae	Smaller	Larger	Significant difference
Rib	Larger	Smaller	Significant difference
Humerus	Larger	Smaller	No significant difference
Tibia	Smaller	Larger	Significant difference
Femur	Smaller	Larger	Significant difference
Metatarsal	Larger	Smaller	No significant difference
Metatarsal 4	Smaller	Larger	Significant difference
Metatarsal 5	Smaller	Larger	Significant difference
Calcaneum	Smaller	Larger	Significant difference
Girdle (Pelvic)	Smaller	Larger	Significant difference
Girdle (Pectoral)	Smaller	Smaller	No significant difference

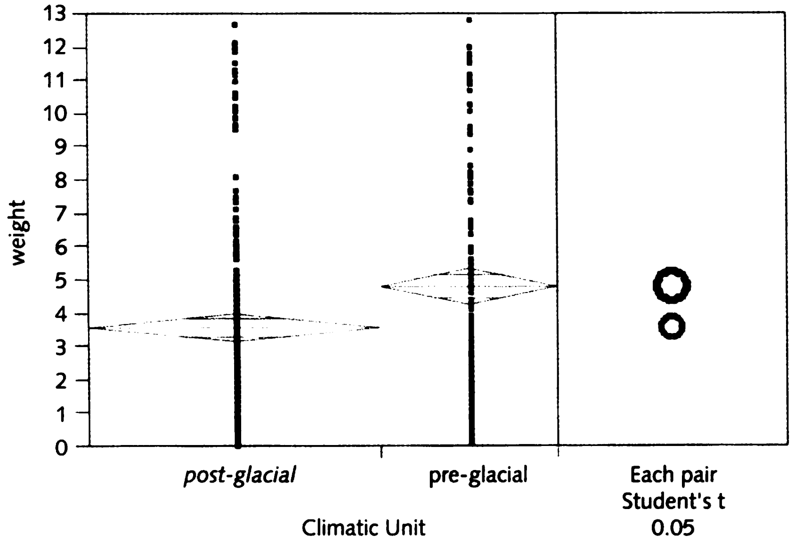

Figure 4. Weights of Bennett's wallaby femora in the pre and post-glacial periods, combined Bone Cave and Wareen Cave data. Positive values show pairs of means that are significantly different. Circles indicate a significant difference in weight between pre and post-glacial times.Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Ratio
Model	1	176.7181	176.718	10.1605
Error	535	9305.1024	17.393	Prob>F
C Total	536	9481.8205	17.690	0.0015

Comparisons for each pair using Student's t

Abs(Dif)-LSD	Pre-glacial	Post-glacial
Pre-glacial	-0.81722	0.454850
Post-glacial	0.454850	-0.63207

post-glacial deposits in the Macintosh Cave, Bone Cave and Nunamira to further test for the effects of regional differences. All these tests are available to interested readers but have been omitted because of lack of space. As far as these data allow us to judge, we are presently satisfied that the differences are totally or predominantly time related.

DISCUSSION: RESULTS FROM THE DATA ANALYSES

The patterns of the species distribution data from all Southwest Tasmanian Pleistocene sites are very similar. In every excavation Bennett's wallaby predominates as the principal human food prey species, varying in its percentage representation between sites, and through time within sites, in the maximum range of 33% to 85%. Although having a comparatively minor representation, wombat is consistently the next ranked prey species, after which no species has consistent representation across sites or through time. Platypus and the Eastern quoll, if indeed this latter is a human food prey, are present in small numbers. The total absence of the emu west of Nunamira and Bone Cave is taken to indicate that this animal was not present in the more western valleys. Emu egg shell occurs only in Nunamira, and emu bones are only very minor occurrences there and in Bone Cave. As suggested earlier, this animal appears to have been restricted during the late Pleistocene to the more open Florentine Valley on the eastern edge of the Southwest.

The argument that the emu was present, but by-passed by hunters, seems refuted by the absence of emu egg shell beyond Nunamira, since emu eggs would likely have been collected whenever encountered, and some at least returned to the cave sites.

In general, however, the uniformity between sites is remarkable, with the greatest differences being recorded between the Mackintosh site and those further south and east. During the post-glacial period this site, previously unoccupied, clearly came within the ambit of the Southwest system, but remained marginal to it, as did Warragarra. In the same region, the shelter site of Parmerpar Meethaner (Cosgrove 1995b) has a lithic assemblage which links it to the Southwest, but a distinctly different faunal suite; it is also a site which continues to be occupied through the Holocene. We currently see these three sites representing northern outliers of the Southwest system.

Outside the Southwest the faunal assemblages are remarkably different, reflecting the contrasting palaeoecological conditions prevailing during the glacial period. For example Bennett's wallaby bones occur at much lower frequencies in sites like Cave Bay Cave, ORS 7 and Mannalargenna Cave despite good preservational conditions.

Even so, the faunal analyses presented here show clear changes through time both in volumes and behavioural implications, consistent with changes noted in other aspects of the Southwest archaeology. Among various points of interest, while these hunters were more efficiently processing some wallaby body parts after the glacial maximum, they were not necessarily processing whole wallaby carcases intensively. If it be further substantiated that they were selectively returning the more energy-valuable body parts to sites at a time when there were more people on the landscape, it implies that neither they as hunters, nor wallabies as prey, were under increased pressure. We return to this question of resilience below.

While all the evidence suggests that wallaby was the most frequently encountered animal, the archaeological distributions of all the proposed prey species do not reasonably match the estimated range of proposed species and their likely proportions determined from the zoological and ethnographic records, if these animals were being taken opportunistically as encountered on the landscape. Even at the low estimate of 20 wombats per km^2, we would expect a greater number of these animals; equally the almost total absence of brushtail possums is inconsistent with this encounter model. After consideration, the faunal data and the wider ecological model put forward earlier still lend themselves to interpretation within a general strategy of optimal foraging, to which we turn.

Although evidence of seasonality in the human occupation of the Pleistocene Southwest is thin, we have always assumed that people moved seasonally in and out of these valleys (e.g. Cosgrove et al. 1990; Porch and Allen 1995). Preliminary studies of annuli displayed in wallaby teeth suggest a degree of seasonal capture in these sites. A silcrete flake from Nanwoon Cave in the Florentine Valley derives petrographically from an east coast quarry south of Hobart (Cosgrove 1996:51). But no other direct evidence for movement outside the Southwest has been recognised. Within the faunal assemblages, and extrapolating from the behaviour of the mainland emu, Cosgrove (1995a:76–7) has suggested that the taking of emu eggs at Nunamira indicates humans there at least in late winter/early spring. More tenuously, the Bennett's wallaby is one of only two macropod species to show obligate seasonal breeding, giving birth 2–4 months after the summer solstice, that is, from late February to late April. Assuming that this breeding pattern existed in the late Pleistocene (cf. Hinds and Louden 1977:44), the absence of very young wallabies in any of the assemblages may indicate that humans were not present in the late summer/autumn.

The point to make is that providing people used these valleys periodically rather than continuously, these early Tasmanian hunters were scheduling their visits, choosing to use these upland locations at particular times, likely determined by their wider activities (for example the availability of other seasonal foods on the coasts and elsewhere) and the dietary deficiencies of a prolonged high protein meat diet (Cosgrove et al.

1990). This in turn reflects a structured economic strategy involving specific prey selection. However, the use of this landscape at any time demanded choices and decisions. If our ecological patch model holds, then high on the list of decisions would be when, seasonally, to start visiting these upland patches, and, more importantly, when to abandon each patch to move to the next, given that food capture rates very likely decreased the longer the hunters remained in one patch. In this case, travel between patches can be seen as an important cost, particularly if the intervening country between patches held little food. Charnov (1976) has modelled this general situation in the development of his marginal value theorem, concluding (1976:132) that the predator should leave the patch when the marginal capture rate drops to the average capture rate for the habitat as a whole. In the Tasmanian case, if the contrast between available prey in grassy patches and those in surrounding scrub areas is as stark as we suggest, it may be better to see the decision more strongly reflecting the distance (= time) to the next patch, such that the hunters should leave a patch when the actual capture rate falls below the anticipated average capture rate in the next patch, calculated to include travel time to that patch.

Returning to the initial question of whether late Pleistocene Southwest Tasmanian hunters took animals in proportions in which they encountered them, or whether they deliberately selected particular species, we are forced to acknowledge that the data remain ambiguous because of our inability to delineate, with sufficient precision, the community structures of the prey species present during the late Pleistocene. This limitation inhibits the precise application of optimal foraging theory to questions of prey selection in the prehistoric past, even when the quality of the archaeological record is exceptionally good.

The Tasmanian data do however allow us to offer a reasonable archaeological compromise on this question. In terms of optimal foraging theory, we can be confident that these patches were highly ranked economic zones which were targetted among a selection of alternatives. Then, at a finer level, some prey choice is evident in the archaeological record within these patches.

These within-patch choices have largely been dealt with in the body of this paper. If optimality is measured in the time spent to obtain a unit of food, a theoretical consequence is that the number of prey species taken should normally be increased until adding further species results in an overall increase in cost, that is, an increase in the time spent to obtain that food unit (MacArthur and Pianka 1966:603). In the Tasmanian case the extremely low numbers of medium sized prey animals in the assemblages emphasise the overwhelming preoccupation with Bennett's wallaby. Contrary to ethnographic observations that women collected the smaller mammals while men hunted kangaroo, in these patches activities were directed to capturing and processing wallabies to the exclusion of other species. Then, rather than moving to lower ranked prey species as wallaby numbers dwindled, people moved to the next patch as a better strategy. A strong implication of this scenario is that site visits were short lived.

MacArthur and Pianka (1966:604) demonstrate that in a productive environment the optimal diet should become more restricted, because while search time decreases, pursuit time is a separate function of the abilities of the predator and the prey, and remains unaltered. In such cases low search/pursuit ratios result in a restricted diet whether the reason be high prey densities, as we argue here for wallabies, or the differential ability of some prey species to avoid capture (as might be the case here for emus). Our ecological model suggests that wallabies were present in high densities within and adjacent to the grassy patches, and that both the structure of the landscape and the behaviour of this species would have made it an ideal prey choice within the cost parameters of time spent in search, pursuit, transport and processing.

A further question concerns the sustainability of the predator/prey relationship, touched on above. On first principles we might question the long-term viability, at the hands of efficient human predators, of a prey species 'tethered' to small patches on the landscape. However the archaeological record in this case immediately disposes of any notion of 'overkill' in Pleistocene Southwest Tasmania. Bennett's wallaby remained the dominant prey species for more than 20,000 years. This may be unsurprising when we consider that modern wallaby populations in Tasmania are very resilient even in the face of modern gun technology. More than two million wallabies were shot between 1981 and 1986 with no appreciable effect on population size (Driessen 1988:22). Loring (1997) reports similar resiliency with caribou in northern Labrador. One early twentieth century observer noted that four men and four boys speared more than 1000 caribou in about a week (Loring 1997: 195) an apparently common level of predation.

Instead, this Pleistocene case demonstrates what Winder (1997:617) calls *active-predator* behaviour, where the growth of population of both predator and prey is each partially determined by the other. If the ecological patch model for the Pleistocene Southwest holds, two further associated observations of Winder's also follow: firstly, such an ecosystem was particularly resilient in ecological terms, assisting the sustained predator-prey interactions through a period of major environmental change (Winder 1997:620). The expectation that site densities be correlated in space and time with resilience (Winder 1997:633) is obviously supported in the Southwest case. Secondly, this resilience reflects the physically separated patches creating punctuated hunting/non-hunting periods in predator/prey interactions at each of these locations, similar to Winder's time lags in hunting pressure which created regional variation and refugia from predation pressure. A third element in this relationship is the 'stability' of a species. It appears that the two principal prey animals, Bennett's wallaby and wombat were ecologically stable (Cosgrove 1995a:95) — able to withstand high environmental variations in both frequency and magnitude that possibly led to greater variation in numbers of other species in these patches at any given time. If low numbers of medium size mammals like possums in these sites are reflecting their inability to maintain a consistent presence in the face of these perturbations, this changing availability might undermine our view that hunters deliberately by-passed some species for others. Equally, however, the very predictability of *Macropus rufogriseus* and probably *Vombatus ursinus* at a regional level might also have helped to reinforce a prey selection strategy which was aimed at animals always present at the expense of others with less predictable distributions at the regional level.

Optimal foraging patch theory also provides us with a rational explanation of why and when humans would likely abandon one patch for another. In Winder's terms a balance was achieved between dispersal mechanisms and carrying capacity which allowed 'predator and prey to play "hide and seek" indefinitely' (Winder 1997:625).

With regard to the absence of megafaunal species from the Southwest sites we limit ourselves to a few comments. While there has never been a clear resolution to the debate in Australia concerning climate or humans as a principal cause in the extinction of the megafauna, it seems that, by default, most Australian commentators now accept that humans were fundamentally implicated. This was the widely (but not universally) held opinion at the 1997 CAVEPS conference on faunal extinctions held in Perth, and only a minority of researchers (e.g. Horton 2000) appear now to favour a climatic explanation. Most recently, in a very selective review of the data, Flannery and Roberts (1999) revived the 'blitzkrieg' model, which suggests the rapid demise of the megafauna immediately following human arrival - a model out of favour because of the apparent long-term overlap of megafauna and humans on the continent. Flannery and Roberts propose that human arrival, megafaunal extinction, and widespread vegetation change towards fire-tolerant eucalypts all occurred 50-60,000 years ago, and add (1999:250): 'An implicit assumption is that people colonized the entire continent rapidly once landfall had been made… .'

The data from Tasmania currently fail to support this view. Data show that megafaunal species including *Diprotodon* and larger macropods were in Tasmania during the late Pleistocene. Early claims for overlap between humans and *Macropus titan* in Florentine Valley caves (Goede and Murray 1977:9; Murray *et al.* 1980:151–2) were subsequently retracted (Goede and Murray 1979:52; Goede and Bada 1985:161) and no other suggestion of overlap has come to light. Regardless of when humans first arrived in north Australia, the dating evidence (including OSL and conventional, AMS and ABOX-SC ^{14}C techniques) from more than a dozen Tasmanian Pleistocene sites rejects any human presence there before ca. 35,000 BP and the faunal assemblages reviewed in this paper consist of more than 637,000 bones, none of which is from an extinct megafaunal species (allowing that emu and thylacine are historic extinctions). It is difficult to believe that none of these animals would have been captured or scavenged at some time had they been in the Southwest valleys when humans arrived; and we know that at some time during the late Pleistocene one large macropod was at least in the Florentine, and that *Diprotodon* was also likely to have been present there at some stage, given its widespread distribution across many environments further north. Therefore, while we cannot yet rule out the possibility that megafaunal species were present in Tasmania when humans arrived in the late Pleistocene, and while we can also question whether humans may have passed over them as prey if they were encountered, the interpretation of the existing evidence which continues to make the fewest assumptions is that megafauna were already locally extinct in the Tasmanian Southwest — and probably further afield in Tasmania - before humans arrived (Cosgrove 1989). Certainly the hunting strategies reflected in the capture of Bennett's wallabies do not sit well with the overkill nature implied by 'blitzkrieg'. The real point here is that extinction events, their timing and their specific causes may have varied in different parts of the continent, and no single continent-wide explanation is demanded.

CONCLUSION

While the use of optimal foraging models in archaeological analyses elsewhere in the world has not been extensive, their use is increasing (see Broughton and O'Connell 1999 for a review of pertinent cases). Such models have been used to study contemporary hunter-gatherers in Australia (e.g. O'Connell and Hawkes 1984) but we are unaware of any archaeological application in Australia, nor do we know of other attempts to analyse the faunal composition of Pleistocene sites in marginal value terms.

The present approach has structured an argument which leads us to conclude that discrete grassland patches in otherwise scrubby vegetation in the upland Southwest valleys were targetted by Pleistocene hunters in Tasmania as highly desirable hunting localities. Even though Bennett's wallabies were likely the most numerous prey species in and around these patches, the data suggest that they were pursued more selectively than other potential prey species that were likely there. The strategies for their capture appear to have involved scheduled seasonal visits to the upland valleys, co-operative hunting techniques, and a regional strategy of moving between focal hunting locations to maximise returns. This in turn resulted in the continual rejuvenation of local prey populations so that a potentially vulnerable species continued to populate these grassland patches in the long term. It is beyond our data and our perceptions to determine whether this conservation aspect was a deliberate intention of these hunters. Advertently or inadvertently, these hunters provided themselves with a predictable and stable food source through a period of extreme climatic change.

Southwest Tasmania also provides us with a data base of sufficient robusticity to be able to reject the proposition (Lourandos 1997:252, 325) that Pleistocene Australians were always 'transient' hunters and collectors with an 'immediate-return type system' proceeding with the food quest almost without system or discrimination. The Pleistocene Tasmanians were as dynamic as their Holocene descendants. The quality of the data allows us to argue that these people scheduled visits into the upland valleys of the Southwest specifically to pursue a narrow range of prey animals, principally the Bennett's wallaby. The recurrence of this clear pattern in all the excavated Southwestern sites is due in part to the primary focus of limestone caves as residential locations for these hunters, behaviour which channelled unusually high density accumulations of archaeological debris into these normally small caves and shelters. It is also partly due to the exceptional preservational qualities of these sites, which remained undisturbed for the last 12,000 years. Despite these exceptional data, the resolution of events which they record does not yet permit the notion of periodic visits to be refined into seasonal ones. However, while this record lacks the clear seasonal indicators of the southwest European data, comparisons between the two regions, first drawn out by Rhys Jones, continue to fascinate us because of the common thread of early humanity that they entail.

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Er Côf Am Edward Lhuyd

Meini nadd a mynyddoedd —
  a gwaliau
Ac olion dinasoedd
A dail, dy fyfyrdod oedd,
A hanesion hen oesoedd.

John Morgan, 1688–1733

In Memory of Edward Lhuyd

Worked stones and mountains
  — and walls
And remains of cities
And leaves, your interests we
And histories of old ages.

(From Thomas Parry (ed.)
1962 The Oxford Book
Verse. Oxford: Clarend
As translated by Rhys J

HISTORIES OF OLD AGES

Essays in honour of Rhys Jones

PROFESSOR RHYS JONES is an internationally-distinguished archaeologist at The Australian National University. His research, particularly in Australia, has made a substantial and enduring contribution to the discipline, and to its public appreciation. The papers and tributes collected here were written by his colleagues and friends to honour Rhys' formal retirement in 2001, and to celebrate his many interests.

The papers cover an appropriately wide range of topics, but most take up themes that have loomed large in Rhys' research. Perhaps foremost amongst these has been his career-long concern with the age, nature and impact of initial human colonization in Greater Australia. Many papers here discuss cases and techniques of chronology, the fundamental issue, and one to which Rhys has brought a unique combination of technical expertise, archaeological knowledge and scientific imagination. Others take up Rhys' longstanding interests in the development of Australian Aboriginal art, the meanings of stone tool technology, the extinction of megafauna, and the historical ecology of Aboriginal subsistence practices. Others again, extend some of these themes into the western Pacific and further afield or acknowledge Rhys' delight in the antiquarian ideas and philosophies which gave rise to the emerging discipline of archaeology.

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