A COMMUNITY OF CULTURE

The People and Prehistory of the Pacific

Edited by
Matthew Spriggs, Douglas E. Yen, Wal Ambrose,
Rhys Jones, Alan Thorne and Ann Andrews
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Department of Prehistory, Research School of Pacific Studies
The Australian National University, Canberra, Australia
In contributing this foreword to a volume containing papers by his colleagues to mark Jack Golson's retirement, it will be my aim to emphasise the qualities of mind and character that have enabled him to set the Department of Prehistory so firmly on its feet in the Research School of Pacific Studies of the Australian National University. During his tenure he has helped to make Australia a major centre of world prehistory and stimulate its study in many other Universities.

It is one of his characteristics that Jack has consistently drawn a clear distinction between means and ends. He has always regarded archaeology, ethnography and palaeoecology as fields of study that need to be cultivated, but he has never forgotten that the object of pursuing them was to advance a knowledge of what happened in the prehistoric past. When he came up to Peterhouse, the oldest college in Cambridge, he did so as a historian and he began his undergraduate career as a historian. On the other hand we would not be honouring him with this volume if he had not completed his degree in Archaeology and Anthropology. I can speak with some authority on this topic because I did the same thing. Moreover we both, though at different stages of our careers, profited from the influence of Michael Postan, then a fellow of Peterhouse, Professor of Economic History and for many years editor of the *Economic History Review*. Postan was a Russian by birth who came to Britain by way of Central Europe to avoid the impact of the Bolshevik Revolution. In Britain he graduated at the London School of Economics where he enjoyed the friendship of some of the leading social anthropologists of the day and in due course became a lecturer in Economic History. As editor and teacher Postan was convinced that economic historians ought to take account of prehistory, and that archaeology had a key part to play in contributing data to supplement written records. He frequently mentioned to me his regard for Jack Golson's abilities and I have little doubt that his influence was decisive in persuading Jack to move over to Archaeology and Anthropology to complete his degree. Again, it was under Postan's influence that Jack undertook his early researches into deserted medieval villages and among other things engaged in Axel Steensberg's field investigations into the early history of agriculture in Denmark. One thing Jack learned for certain was that archaeology in conjunction with the techniques of quaternary research was only of real value if it was used to throw light on the evolution of human society.

Jack's career as a professional prehistorian has already spanned some four decades. The first six years were spent in New Zealand. When he arrived at the University of Auckland, in 1954 he was the first lecturer in archaeology to be appointed in that country, if we except Dr Skinner of Otago who had passed through Cambridge before formal teaching in prehistory had been established there. It fell to Jack in Auckland as later at Canberra to introduce and develop prehistory as an academic discipline based on systematic teaching and sustained research. That is not to say that he introduced prehistoric archaeology to either country. In each, excavation and collecting had already been pursued to the point at which academic recognition was seen to be necessary. What Jack had to do was to ensure that fieldwork and excavation were directed systematically to the elucidation of prehistory. In New Zealand an important step was taken with the founding of the New Zealand Archaeological Association in 1955, the most important feature of which was the holding of annual conferences to promote the exchange of information and ideas, encourage discussion and above all to indicate the channels most likely to advance the understanding of Maori prehistory. In this he was greatly aided by Peter Gathercole, a fellow Peterite, who established teaching in anthropology and archaeology at Otago University in 1958. Before he left New Zealand Jack rendered two particular services. In conjunction with R.C. Green from Harvard who in due course was to succeed him at Auckland he prepared a handbook designed to assist the study of field monuments in New Zealand and in 1959 he published a masterly essay in which he defined some of the main problems facing prehistorians in New Zealand.

As a person – and even in these days of computers and collective decision-making, creative ideas still proceed from individuals – Jack has qualities with which his friends in Australia will
have become familiar over the years. To take a constructive lead in a field of such vast extent which lends itself to personal exploit and competition calls for exceptional qualities of judgement and compassion as well as an ability to identify and concentrate on significant problems and ensure that progress is made in their resolution. Above all it has to be remembered that the Research School of Pacific Studies confronts an immense geographical space and that as in New Zealand Jack had to confront its problems as the first fully trained and academically qualified prehistorian in the Australian National University. His task was not only to define the most profitable fields of research but to educate those who would cultivate them from Canberra and ultimately from a number of other university centres.

By the time he moved to Australia, he left New Zealand prehistory well established as a field of study in two universities. When he was invited to create the conditions needed before a separate department of prehistory could be established in the Research School of Pacific Studies at Canberra, the subject had yet to be properly recognised by other universities in Australia. It is a measure of his success and of others working in the field that in the course of time prehistory achieved academic recognition at a number of university centres in Australia. One of his main tasks was to produce many of the staff required. One obvious source of recruitment was the Cambridge department then already at the height of its development.

A no less daunting task was to define major projects for research. While at Auckland Jack had made a point of studying the Maori in Polynesian context and himself undertook excavations as far afield as Samoa and Tonga, as well as taking a sustained interest in Oceanic navigation. With this experience it is no wonder that he should have concerned himself with its geographical setting. He showed a keen awareness that Australia must originally have been populated from Southeast Asia. This led him to take a close interest in New Guinea and Tasmania which at the time of their original settlement formed part of the Sahul continent. In the case of New Guinea where Peter White undertook his doctoral research Jack has devoted many years to the investigation of early agriculture in the highland zone, work that must have reminded him of his student experiences in England and Denmark. In the case of Tasmania he had given sustained support to Rhys Jones' work in exploring the stone age succession. His interest in the palaeoecology of the continent led him with John Mulvany to co-edit Aboriginal Man and Environment in Australia, a volume to which twenty-four scholars contributed essays on a theme that still calls for more sustained and detailed study.

Jack Golson owes his success in part to his intellectual grasp of the problems facing Australian prehistorians and his understanding of what still needs to be done to solve them. On the other hand as his colleagues appreciate this is far from being the whole story. He owes much above all to his unselfish character. He has consistently worked to ensure that his department remains dedicated to a task of critical importance not merely to Australia but to the world at large.

Grahame Clark
36 Millington Road
Cambridge CB3 9HP
England
CONTENTS

Foreword
Grahame Clark

Acknowledgements

PART I  JACK GOLSON

Cambridge: History, archaeology and politics
Peter Gathercole

'Dig Up Those Moa Bones, Dig': Golson in New Zealand, 1954-1961
L.M. Groube

From Cambridge to the Bush
John Mulvaney

Professor
Matthew Spriggs and Rhys Jones

Jack Golson: A personal appreciation of his institutional role
Peter Ucko

The Golson Bibliography from 1953

PART II  FESTAL WRITINGS

Late Pleistocene Coasts and Human Migrations in the Austral Region
John Chappell

Are Your Fingerprints Destined to Become Part of Prehistory?
Barry Fankhauser

Prehistoric Organic Residue Analysis: The future meets the past
Thomas H. Loy

Voyaging
Geoffrey Irwin

Pacific Subsistence Systems and Aspects of Cultural Evolution
D.E. Yen

A Continental Reconnaissance: Some observations concerning the
discovery of the Pleistocene archaeology of Australia
Rhys Jones
PART II  FESTAL WRITINGS  (continued)

- Views of the Past in Australian Prehistory  
  Sandra Bowdler  
  (123)

- Notions of the Pleistocene in Greater Australia  
  Jim Allen  
  (139)

- Crossing the Wallace Line – with Style  
  Peter Bellwood  
  (152)

- Contradictions and Malaria in Melanesian and Australian Prehistory  
  L.M. Groube  
  (164)

- Island Melanesia: The last 10,000 years  
  Matthew Spriggs  
  (187)

- Pottery Raw Materials: Source recognition in the Manus Islands  
  W.R. Ambrose  
  (206)

- Tropical Polynesian Prehistory – Where Are We Now?  
  R.C. Green  
  (218)

- Issues in New Zealand Prehistory Since 1954  
  Janet Davidson  
  (239)

- Cultural Resource Management in Australia: The last three decades  
  Josephine Flood  
  (259)

- Museums and Cultural Heritage of the Pacific Islands  
  Jim Specht  
  (266)

Maps  
  Winifred Mumford  
  Australia  
  (281)  
  Pacific Region  
  (282)  
  Northern Melanesia  
  (283)  
  New Zealand  
  (284)  
  Southeast Asia  
  (285)

Index to Maps  
  (286)
The number of Jack Golson's former students and colleagues is immense – his influence wider still. In organising contributions to this fest­schrift volume, the editors decided it would be impossible to ask all but a small fraction of those he has worked with. It was therefore decided to invite people to contribute on particular themes of research with which Jack has been intimately involved, and we thank them all for their efforts. We aimed to cover particular topics rather than attempt to include a representative sample of colleagues and students. We realise that there are many more people who would have liked to contribute and who would have been appropriate authors for this volume. The fact that we could not include them all intends no slight.
PART I

Jack

Golson
CAMBRIDGE: HISTORY, ARCHAEOLOGY AND POLITICS

Peter Gathercole
Darwin College, Cambridge CB3 9EU, England

I first met Jack Golson at Cambridge in early October 1949. Still new to civilian life after two years' Army service spent mostly in Egypt, and very much a freshman, I was introduced to Jack by Alan (Max) Cole, both like myself History Scholars in Peterhouse and very soon friends and mentors in a new environment. Jack and Max had first come to Cambridge in October 1943 to spend a year reading for the Preliminary Examination in History before military call-up, both then aged 17. As it turned out, they were destined to undertake their military service as miners (the so-called Bevin Boys), though Max later transferred to the RAF. Both had returned to Peterhouse in 1948 to complete their degrees, Max from Germany and Jack from the Nottinghamshire coalfield. In the summer of 1949 he took Part I of the Historical Tripos, and when I first knew him was beginning work for Part II of the Archaeological and Anthropological Tripos, specialising in archaeology, which he took in 1951.

I mention these details to emphasise the fact that Cambridge was then still thronged with students whose courses had been interrupted by the war, or who had completed at least two years' national service after 1945. There were relatively few 18 year olds in their first year, and the prevailing attitude among both dons and students was that one worked hard to make up for lost time. Peterhouse had then about 240 undergraduates; its atmosphere was liberal, friendly and strongly academic, with good relations between fellows and students. The food was excellent.

I saw a lot of Jack, partly because until 1951 we lived on adjacent staircases in college. He helped to draw me to ancient and medieval history, which had been his particular interest before switching to archaeology. In fact, his initial attraction to the subject was to seek answers to questions concerning medieval economic history which the documents seemed unable to answer. He shared rooms with Charles Whitby, a law student with wholly non-dogmatic Tory views, who obtained much entertainment from the comings and goings of Jack's left wing friends, including myself. I would often find Jack prone on the floor, surrounded by books and journals. He sometimes worked through most of the night, and I had a standing instruction to get him up for 9 o'clock lectures, and on Saturdays, at times, for an early stint selling the Daily Worker. Much of our conversation during those years, whether at dinner in Hall (where at least one evening each week four or five left wing members would eat together) or in pubs, cinema queues, or a favourite café on the Market Place, was about medieval or 17th Century English history, archaeology or politics. Initially Jack was immersed in the archaeology of Roman Britain and of the Anglo-Saxon and Viking periods, and then, the next year, in European prehistory. The Faculty of Archaeology and Anthropology then had a small number of students who, according to his reports, worked in a very informal atmosphere and had good relations with the lecturers and the museum staff. Whatever the period concerned, archaeology was always dealing with new material. Compared to life in the History Faculty, with several hundred students and a well-trodden syllabus, this all sounded exciting, and led to my transfer in 1951 to read archaeology for my own Part II.

In retrospect our life seems to have been serious and rather narrow, at least to the extent that neither we nor our friends played sport, joined the Union or went out of our way to have a conventionally ascribed 'good time'. Nor were we interested in striking radical poses. We were interested in learning and scholarship, and we were conscious, as college Scholars, that we had got to Cambridge by our own efforts, with neither class position nor family wealth behind us. In so far as it mattered, we were different from most other students because of our membership of the Communist Party, much of our time being devoted to its activities, both inside and outside the University. The student branch had 33 members in 1949, having slowly but inexorably fallen in membership since the end of the war, and this fall continued thereafter. The Socialist Club, which was proscribed by the Labour Party because it did not exclude communists, who usually dominated its leadership, maintained a member-
ship of about 100. We took our share of routine work – selling literature, taking round the petition of the British Peace Committee, organising open meetings on political and theoretical issues. I particularly remember a vigorous, spontaneous campaign of protest in December 1950 against Truman's threat to use the atom bomb in Korea, which embraced almost all shades of political opinion, where freshly duplicated leaflets were distributed 100 yards away to people going into King's College Chapel for a Music Society concert. But most activities were more mundane, which, incidentally, only occasionally provoked open hostility. Sometimes they even had their comic side. Sometimes Jack and I spent time together during vacations. During part of the Easter vacation in 1950 we stayed with David Mulvany, then a second year economist in Queens' College, at his parents' home in Haywards Heath, Sussex. There we indulged in a parody of a 19th Century under-graduate reading party, with the emphasis on politics rather than literature (Fig. 1). More seriously, in 1951, there being several of us then reading archaeology or anthropology, we formed a study group which, inevitably examined Engels' Origin of the Family (from memory inconclusively), and Grahame Clark's Prehistoric Europe: The Economic Basis. We admired the latter's scholarship and elegant style, but found difficulty sorting out its theory. Childe's books were obviously very important to us, especially History, his survey of historiographical methods, and Social Evolution. But we were often baffled by his apparent preoccupation with the instruments of production. Only later did we appreciate that Childe was then searching for evidence for the relations of production in the European Bronze Age, hinging on the status of metal smiths, which he set out in the sixth edition of The Dawn of European Civilization, and more explicitly in The Prehistory of European Society.

In mid-1951 Jack began research into deserted medieval villages, particularly of Lincolnshire (Golson 1953). He and John Hurst, a Cambridge contemporary, helped to set up the Deserted Medieval Village Research Group, and they began an elaborate rescue excavation of part of the Norwich city wall at St Benedict's Gates (Hurst and Golson 1955). Jack spent much time between mid-1951 and the end of 1953 in the field, part of it digging with Axel Steensberg in Denmark, but he also worked in the Public Record Office in London and in the County Record Office in Lincoln. Meanwhile I had married Falmi Williams, switched to archaeology for the rest of my degree, and moved to London to take a postgraduate diploma under Childe. A peripatetic Jack would turn up from time to time at our tiny Hackney flat to sleep on our floor. As he would also stay with other ex-Cambridge friends, notably David and Lyndal
Mulvany, he was a welcome purveyor of news. Once in 1953 he arrived just before May Day, so he joined us on the march to Trafalgar Square as part of the local CP branch (Fig. 2). Much of our earlier relationship remained, but by then we were both deeply involved in archaeology, and although I was busy in the Student Peace Movement neither of us were often out 'on the knocker'.

Jack had the knack of suddenly materialising elsewhere than in London. During the Easter vacation of 1953 I was doing a lonely 'rescue watch' for the Ministry of Works at a building site adjacent to Oakham Castle, Rutland (Gathercole 1958). All sorts of pottery coming out of the moat-fill were new to me. Suddenly Jack appeared, having taken a spontaneous detour while on some cross-country rail journey in order to see what I was finding. We spent part of the night on the floor of my hotel room going through the finds and marking the bags. Before he left the next day we went over the stratification in detail. Such gestures were typical of him.

That summer I was his assistant for two months at Norwich, when the 1951 dig was extended (Fig. 3). After long hours on the site we had meals of steaks and Worthington ale and late walks in the medieval city talking architecture and Galsworthy's novels, which we were ploughing through at the time. Jack left me in charge for two weeks while he went back to the Lincoln Record Office. He returned with the news that Grahame Clark was urging him to apply for a lectureship at the University of Auckland. The prospect of such new horizons naturally excited Jack, although it would mean putting his research into storage at least for a time. I urged him to stay in England because of the shortage of medievalists, and sometimes have mused since on the possible consequences had he not gone to New Zealand. Only recently did he mention, almost in passing, that his strong second thoughts were stilled by Grahame's persuasiveness.

We corresponded from time to time over the next few years, although I cannot recall any
serious references to politics, despite the sig­
nificance of the events of 1956, particularly
Khrushchev’s speech to the 20th Congress of the
CPSU on Stalin and the cult of personality, the
Suez invasion and the Hungarian uprising. Even
our departures from the CP went unrecorded —
perhaps each of us was too angry and too
ashamed to discuss them. Jack wrote about
his teaching (taking students through Childe’s
_Dawn_ with no museum collections to study must
have been a severe methodological exercise), and
about the foundation of the University Arch­
aeological Society and of the New Zealand
Archaeological Association. He sent offprints of
his publications; the subject matter seemed a long
way from the archaeology and local history of
north Lincolnshire, where I became a museum
curator in mid-1956.

In the spring of 1957 Jack wrote a long letter,
partly about some of the deserted villages he had
investigated which I was keeping an eye on, but
mainly about a forthcoming job at Otago, half in
the University, half in Otago Museum. Would I
be interested? After much discussion with
Falmiai, mindful especially of the future of two
small sons, I applied for the position, with her
strong support. We arrived in Dunedin in early
July 1958. The move renewed collaboration with
Jack which, in different forms and places, has
continued ever since. But that is another story.

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Figure 3  Clearing the top layers of one of the trenches, St Benedict's Gates, Norwich, July 1953. Jack Golson using pick, Peter Gathercole trowelling [others unknown, including interested bystanders]. (Photo: A.R. Solt).

Leicestershire Archaeological and Historical Society 34:17-38.


This essay has been written in the tranquillity of Brittany, where Auckland is only known as that place where, in recent history, a certain ship was sunk. My task, as I originally saw it, was not only to review Jack Golson's seven fateful years in New Zealand, but to address the question why he was the right man, in the right place and at the right time. To provide answers, I found myself examining not only Golson's career, but the academic and cultural milieu in which he found himself in February 1954 when he disembarked from the 'S.S. Ruahine'.

To understand the importance of Jack's achievements it is necessary to appreciate the difficulty for anyone who was unfamiliar with the enormous wealth of documentation on the pre-European history of New Zealand to separate the purely academic from the quasi-academic, the speculative from the romantic, the authentic from the fraudulent. He must have been puzzled by much of what he read in his first few months, but it must have been obvious to him, despite being the first professional archaeologist in New Zealand, that he was not venturing into *terra incognita*. This was a country with an already established, articulate version of its prehistory with a lengthy scholarly tradition in support, and a number of well-ventilated problems. The outstanding one, the conflict between oral traditions and the authority of the spade, was already approaching crisis at the time of his appointment. Although in later years he was to join in this continuing controversy through his editorship of the *Journal of the Polynesian Society (JPS)*, his direct impact, by enhancing the image of archaeology through the rigour of its methodology, was to prove decisive. Indeed this was Golson's great contribution to the transformation of New Zealand archaeology from a nervous, self-conscious discipline to the confident professionalism of today.

Before looking at Golson's first influential adventures with the soils of North Island archaeological sites it is essential to review the 'state' of New Zealand prehistory and archaeology at the time of his arrival and the social and institutional milieu in which it had developed.

**THE STATE OF NEW ZEALAND PREHISTORY IN 1954**

Prehistory without archaeology

Unlike many countries colonised from Europe, where interest in local prehistory was only lukewarm and coloured by complex and often discriminatory views about the principal native actors in that prehistory, New Zealand enjoyed from the nineteenth century a lively and sophisticated appreciation of Maori history. The existing views on New Zealand prehistory, formulated largely by amateur scholars of genealogies and oral traditions with some input from archaeological sources, were persuasive and coherent, and a specialist archaeologist seemed unnecessary. It is to Golson's credit that he persuaded most of those involved in the study of Maori history that they *needed* archaeology; a considerable achievement in a country with a firmly established and widely believed version of its own prehistory and one, moreover, which was intimately embedded through land-ownership in the fabric of both Maori and Pakeha (white) society.

As the early generation of traditional scholars (Gudgeon, Tregear, Smith, Best and many others) died, the enthusiasm for accepting oral accounts as authentic history waned; a second generation of scholars began the burdensome task of re-assessing the vast libraries of material. Golson was to arrive as this reassessment was reaching its climax.

Prehistory with archaeology

Central in this reassessment were the discoveries and slowly accumulating evidence of
archaeology, particularly from the precocious outburst of fieldwork and excavation which had followed the exciting discovery of the bones of the extinct Moa and evidence that they had been hunted and eaten by the prehistoric inhabitants of New Zealand. This early but short-lived phase of intense archaeological activity during the 1870s laid the firm foundations of an alternative approach to New Zealand prehistory. The excavations of Julius Von Haast, although little known internationally, were extraordinarily innovative in the use of stratigraphy, section-drawing and particularly his remarkably astute faunal analysis which anticipated the methods of the 'bone-room boys' nearly a century later. Von Haast's pioneering work and that of many others who followed him would have become familiar to Golson on the voyage out to New Zealand, when he read the influential work The Moa-Hunter Period of Maori Culture (Duff 1950). The contradictions between the traditional and archaeological evidence is clearly spelt out in the introductory chapter. It should have had, to a medieval historian, an ominously familiar ring. And Duff's apology in the final paragraph of his Introduction should have warned of some of the academic debates which were to come:

I am well aware that to many traditionalists such methods (of comparing material objects recovered from the earth) are sacrilegious, but I trust such studies are to be regarded as supplementary, rather than hostile, to tradition.

Thus at the time of Golson's arrival the state of New Zealand prehistory was very much in flux. The prestige of the traditional story was beginning to ebb and the spade was aiding this decline. The foundations for an archaeologically sound version of part of New Zealand prehistory had already been laid; it was an ideal time to arrive and assert the authority of the spade.

**Archaeology through artefacts**

It is fortunate that there was, in the 'intellectual baggage' Golson (1986:3) claimed to have brought with him, a sensitivity to artefacts. Both in New Zealand and the Pacific, in the decades before his arrival, the main evidence summoned to challenge the authorised traditional story had been that of artefact-types and distributions. H.D. Skinner, one of the great pioneers in the study of Polynesian material culture, had led the way with a series of seminal papers on New Zealand and Pacific artefacts, including his dismissal of the myth of a Melanesian connection in the artefacts of the Chatham Islands. His students, included amongst whom Roger Duff was clearly the most outstanding, carried his influence into the 1950s. Golson was well equipped to understand and assess the evidence assembled in *The Moa-Hunter Period of Maori Culture*.

**Archaeology without archaeologists**

Unfortunately Skinner, the founder of a pioneering Museum group which undertook excavations in Otago, was not heir to the same traditions of fieldwork as Golson. The anthropology Skinner had acquired at Cambridge over three decades before still viewed excavation as principally a means of filling museum display cases. The fieldwork standards he promoted were not good, even for the time, for Skinner was also burdened by a particularly energetic local tradition of collecting inspired by the entrepreneurial spirit of gold-mining. In fact he was forced to employ one of the most talented of these free-lance 'diggers', David Teviotdale, to extract what information could be salvaged from the remaining sites. As Golson was to discover for himself, this tradition was also widespread in the North Island and was a major obstacle to the restructuring of New Zealand archaeology. Nevertheless the Otago Museum group was the only effective field research group in New Zealand archaeology for many decades working independently of traditional accounts. It also gave encouragement to another of Skinner's pupils, Leslie Lockerbie, who single-handedly resurrected some of the standards of excavation and reporting which had been so briefly witnessed in the work of Von Haast in the 1870s.

As Jack must have quickly appreciated, certain aspects of the state of archaeological practice in 1954 were not as healthy as its growing influence in the rewriting of New Zealand prehistory suggested. Field recording and site description was desultory, site protection and state interest in the conservation of archaeological sites was virtually non-existent, and excavations fell far short of the standards he had been trained in.

This situation was despite an excellent start to field recording which arose spontaneously in the early part of this century in many parts of New Zealand, particularly in the North Island which was dominated by a vast number of spectacular pa or earthwork fortifications. The great traditional scholar and anthropologist Elsdon Best led the way, with the superb maps of named pa, former villages and occupation sites appended to his *Tuhoe: The Children of the Mist* (1925). Best followed this up with *The Pa Maori* (1927), which remains one of the great studies of field monuments from anywhere in the world. Unfortunately, following the outburst of recording of
field monuments which centred on the Dominion Museum in Wellington where Best was Ethnologist, interest slackened after the 1930s.

Here then was yet another problem for Jack; to revive the flagging interest in site-recording and to strengthen site-protection. This was a task he had anticipated before arrival (1986:3), but he could not have appreciated the scale of the problem until he became familiar with the extraordinarily rich archaeological landscape of the North Island, where there were literally thousands of important archaeological sites under threat of damage or destruction. Few farmers showed interest in or concern for archaeological sites on their land; local bodies, government departments and the like were equally indifferent to the remnants of Maori history which could be found everywhere. Certain site types, particularly coastal middens, were becoming increasingly vulnerable. Jack appreciated the need for a national body to focus attention on the problem of site damage, the need for site recording and enhanced standards of excavation. Part of this aim was achieved within six months of his arrival, when the inaugural meeting of the New Zealand Archaeological Association was held in Wellington in August 1954. To understand the import of this development it is necessary to look in more detail at the variety and motivations of those already involved in some form of archaeological activity.

A prehistory divided

In the year of Golson’s arrival, the directors of the two major North Island museums were not archaeologists, whereas in the South Island, with a much smaller Maori population and outside the dominance of the all-powerful traditional Maori Fleet accounts, the Directors of the two major museums at Christchurch and Dunedin were both professionally involved in New Zealand prehistory. Neither Skinner nor Duff, however, could escape the conservative authority of the traditional story. One of Skinner’s most influential papers, Culture areas in New Zealand (1921) is remarkably close to being a statement of demarcation of professional interests. His Southern culture area was, in terms of his definition, the legitimate concern of ethnologists and archaeologists, whereas the North Island regions, defined in terms of accepted Maori anthropology, belonged to the traditionalists.

The demarcation lines of the museums were neither as well marked nor as jealously guarded as those of private collectors/curio-hunters and amateur excavators (‘diggers’). Sites rich in artefacts and sometimes whole regions were monopolised by individuals, locations were kept secret and private pacts for access were negotiated between land-owners and the avid artefact hunters.

Common to all the curio-hunters was the belief that excavation was merely a variant of potato-harvesting and that, with the land-owners’ consent, they had a God-given right to dig wherever and whatever they chose. For most of them, also, their enjoyment was private and they did not feel the urge to share their discoveries or expose what they had learnt of local history. They were even less inclined to reveal the location of their discoveries. How many such dedicated curio-hunters were to be found in the North Island in 1954 is impossible to know; it is clear that Golson, during his first few years, somehow or another got to know, or know of, many of them.

The amateur diggers were different. Some were merely hobbyists but others were extremely knowledgeable on local history, even spoke Maori and were friends with the local Maori community. Most had good, if sometimes nervous, relations with the local museum which was often a rival in their pursuits. Unlike the curio-hunter they were generally motivated by genuine curiosity about Maori history, although some, the most dangerous, had pet theories which their digging was designed to prove. They were, in many ways the artisanal equivalents of the genealogy-collectors of the previous century, anxious to share their discoveries but often lacking both the experience and rigour to interpret their results; enthusiasm often outweighed sound judgment. Some information flowed from amateur excavations, through Historical Societies, newspaper reports and occasionally in a prestigious journal such as JPS. They were also more consciously alert to excavation technique and most were widely read in archaeology. Material from their activities often finished up in the local museums or in display cases in schools and where there was a suitable outlet were even published. The amateur excavators were obviously a potential reservoir of talent and goodwill to be tapped and diverted into productive research.

The existence of curio-hunters and amateur diggers and their urge to secrecy and 'ownership' of both sites and the information gained from them can best be seen from this extraordinary disclaimer in Golson’s report (as the new Secretary) on the formation of the New Zealand Archaeological Association (NZAA) in August 1954:

The Association has no desire to curtail or direct every activity of those who join; on the contrary its wish is to stimulate and extend the efforts of
individuals. An erroneous impression has been formed in some quarters that membership of the Association makes it obligatory on a person to relinquish his claims to a site in which he is interested and hand over any material he may find to the Association (Golson 1955b:157).

This could be interpreted as a charter to ravage through all the archaeological sites in the country, but it is in fact an invitation to the numerous amateur diggers to join, without penalty, the new Association. On the whole this invitation and others which followed, were accepted and archaeology in New Zealand moved into a new phase, the institutionalisation of the God-given right to dig. Jack's role in this unusual submission to discipline of the independent-minded veterans of so many spoil-laden 'digs' was crucial, as we will see.

Whether the failure of the museums to appoint trained archaeologists was related to lack of funds or to the prevailing belief that excavation was relatively simple and did not require special training or aptitude, it proved fatal to the continued dominance of museums in New Zealand archaeology. In 1953 a North Island university at Auckland advertised for an archaeologist.

Archaeology at Auckland

This first archaeological appointment was in prehistoric archaeology to be housed in the new Anthropology department at University College Auckland. The department's founding professor, Ralph Piddington, already well attuned to a scholarly if critical view of the authority and authenticity of Polynesian genealogies and traditional history (see his Preface to Williamson 1939), and also Piddington (1956), must have been the principal inspiration in ensuring that the new appointee did more than teach the Part 1 syllabus (an introduction to world prehistory) but also actively prosecute local fieldwork and thus examine Maori history with the spade, was a bold move. To introduce the spade was to question, by implication, the authority and authenticity of Maori traditions as the legitimate and sole source of Maori history. This was potentially a jeopardy to one of the most important of Piddington's ambitions at Auckland, to attract Maori scholars and establish a strong section in Maori studies. Piddington, who was on the committee of the Anthropology and Maori Race section of the Auckland Institute and Museum, must have been aware that the appointment of an archaeologist was also a potential threat to the established territories of the influential museums which had become the foci of what little (official) archaeology was practised in the country.

Within the very limited budget and facilities then available Golson was encouraged to venture onto rich archaeological remains and eventually into the soils of the North Island. His was the first appointment of a professionally trained archaeologist in New Zealand. The timing of the establishment of the post at the University at Auckland (then a college of the University of New Zealand) was critical in both England and New Zealand.

When Golson took his degree the between-war advances in field methods, particularly in excavation, had flowed through to the universities. Mortimer Wheeler, one of Jack's lecturers, was still highly respected; his famous book, Archaeology from the Earth was published in the year of Golson's arrival in New Zealand; the Penguin edition (1956), under Golson's sway, became a New Testament for his students and the dedicated excavation teams drawn from the membership of the Auckland Archaeological Society. This can be best gauged from the words of a verse from one of the camp-fire songs from the Sarah's Gully excavations:

Watch your face, watch your layer,
Keep digging all the day,
Spade and trowel, brush down again,
For that is the Woolley/Wheeler way.

from 'To Delve into a Midden'

Not only was Wheeler influential in Golson's Cambridge days, but student involvement in fieldwork and excavation, under the inspiration of Grahame Clark's famous work at Star Carr, was actively encouraged.

With hindsight it can now be seen as very fortunate that the University of Auckland advertised for an archaeologist well before the dominance of laboratory archaeology for, as will be seen below, it was only someone with a wealth of field experience, particularly with open soil sites (in contrast to cave sites) and familiarity with the techniques of wresting elusive information from the stains and disturbances in intractable soils who could have led New Zealand archaeology into its next and decisive period, the exploration of the 'artefactless' sites of the North Island.

No-one, the candidate Golson or his sponsors, could have known how important that training in medieval field archaeology would prove to be in the following years in New Zealand archaeology. As there were few graduates specialising in medieval archaeology at that time, it is astonishing that he applied at all. The influence of Grahame Clark was surely necessary to push someone dedicated to Deserted Medieval Villages into the very un-mediteval landscape of New Zealand.
But in his first few years it was neither his Cambridge training in economic archaeology nor the disciplined scholarship of a medieval historian which was to be of benefit; it was his successful 'apprenticeship' (his own word, 1986:2) in two complementary crafts. The first, that of the craft of persuasion gained from his undergraduate involvement in politics, resulted in a revolution in the organisation of archaeology in the country, uniting the disparate, suspicious individuals and bodies each with separate and overlapping claims on various parts of the pre-European past of New Zealand. The second was the craft of archaeology in the soil, won from his field experiences on medieval sites and with a pedigree of excellence from Pitt-Rivers to Mortimer Wheeler with a final polishing by Axel Steensberg. He left behind him on his departure from New Zealand a blossoming National Association, the beginnings of a national site-recording scheme, and a new alertness to the problem of site-destruction and the need for effective protection of historic sites. This was in addition to a new tradition of excellence in 'archaeology from the earth' which he grafted onto the stubborn, resistant and proud local tradition. Just how he achieved this is described in the next section.

PERSUASION AND EXCAVATION 1954-57

Jack's first year at Auckland University was very busy. Apart from reading the extensive literature on Maori history and archaeology and preparing the inaugural lectures in prehistoric archaeology which would have taken up a major part of his time he managed to meet, correspond with or telephone a large number of those already working in New Zealand archaeology. It must have been from his reading and these contacts that he quickly sensed that one of the major problems he faced was not lack of interest in prehistory, or unawareness of the role of prehistory in the country, indeed quite the opposite, but lack of organisation and discipline. Thus although during 1954 he had field trips to the Bay of Islands, Great Barrier Island and perhaps others, his most important trip was not to an archaeological site but to Wellington, in August, for the meeting which formed the NZAA (Golson 1955b).

The appointment and arrival of the first professional archaeologist in New Zealand must have been viewed with trepidation by some and with pleasure by others. His presence, perhaps, was the catalyst needed to unite the various interests. The meeting duly elected a President, officers and council. Jack wisely became the first Secretary, aware as one schooled in politics that the Secretary in such a voluntary organisation was potentially in the most favourable position to influence events.

Whatever his role in initiating the first meeting, the policies which emerged from this and subsequent meetings have the signature of his concern for improving field standards which he clearly stated in his first published paper in New Zealand in the same issue of JPS in which he reported the inaugural meeting of the NZAA (1955b). This paper, 'Dating New Zealand's pre-history' (1955a), was the first independent commentary on the state of New Zealand archaeology. Already he was probing into areas beyond the experience of a medieval archaeologist but with the advantages of being a student of Zeuner: volcanic ash showers in the North Island, beach progradation, the technology of C14 dating and so forth. The thorough scholar is evident in that all of the influential publications on New Zealand archaeology available at that time had already been read and absorbed. The frustrated historian is also evident in his enthusiastic introduction of evidence from the earliest European explorers.

But there is evidence of his suppressed impatience with the quality of excavation and stratigraphic interpretation in the country. In his brief niggle at Duff's Waiwera evidence (p.114), he casts doubt on the widespread belief that stratigraphy, in New Zealand's brief prehistory, was virtually non-existent, thus giving excuse for poor techniques in the soil.

It is followed by a firm statement of the archaeologist's task in excavation:

The essence of the excavator's task is the painstaking examination and recording of limited stratigraphical evidence (such as found at Waiwera Bar). His interpretation of any site depends on the lie of the layers in the ground and the material of which they are composed, just as much as upon the nature of the material culture they contain.

His worry, however, was more fundamental than dismay at the lack of technical proficiency evident in some excavations. It was that with such a short prehistory and 'the typological unresponsiveness of New Zealand archaeological material' an extremely high standard of excavation was required, even with the new C14 dating methods which, he warned, the archaeologist 'must put to the best possible use'.

It is clear that this concern had already been formulated before the publication of this paper. The discussion at that first meeting, as reported
by Golson, seems to have centred principally upon excavation, the role of amateur archaeologists and associated ethics. This reflects probably the unease felt by some that their own activities might be restrained in some way but it is also the first hint of what was to become Golson's main concern in the next few years: improving field techniques particularly in excavation. The 'policy' of the new National Association of accepting 'the principle of joint annual excavations of key sites' offered the ideal forum for what was to be an adult 're-education' programme, as is evident from the only two occasions on which this policy was activated, with excavations at Moa Bone Point Cave, Sumner (Christchurch), in 1957 and at Pakotore, Rotorua, in 1959, both intended to be teaching excavations under the guidance of Golson. This campaign, of which such early notice was given in 1955, was to take up a great deal of Jack's time for the next four years and culminated in the Pakotore excavations in May 1959. The sometimes farcical outcome of these efforts at re-education was a measure of the inherent difficulty of teaching old diggers new tricks.

Archaeology without artefacts

During 1954 he also established the Archaeological Field Group in the University (soon to become the University Archaeological Society), from which came the nucleus of the work-force which was permanently to alter the direction of New Zealand archaeology. Pragmatically, of course, this Field Group was simply a means of attracting and organising a potential work force for local fieldwork. In practice, however, it became the political base from which his twin assaults upon the establishment in New Zealand archaeology, improved field methods and harmonisation of conflicting regional and institutional interests, were launched. The Society became a model of how local archaeology could be organised, reviving the spirit and intent of the earlier Otago Museum group by encouraging public participation and active fieldwork.

It was with his own excavation teams drawn from the ranks of the Archaeological Society at Auckland that the impact of the message Jack was trying to convey in his 1955 paper, that the recovery of material culture was not the sole aim of excavation, was to be felt first. This message, indeed, had probably already been influenced by his first adventures into the soils of the North Island, where artefacts proved to be very rare. He was anticipating what was to be clearly established over the course of the next few years, that North Island archaeology would not be defined by material culture but by more elusive structural alterations in the soil which could not be recovered except through careful stratigraphic excavation.

It was a recent graduate in historical geography, Bob Brown, who introduced him to the huge pa built on the slopes of the volcanic cones of the Auckland Isthmus which eventually led to his first excavation on New Zealand soil, on Taylor's Hill one of the smaller volcanic pa. It was being actively quarried for the valuable scoria, a sharp reminder of the parlous state of site protection in New Zealand. This excavation was to continue, intermittently at weekends for over two years. A report of this work was recently published (Leahy 1991). In the summer seasons of 1954-55 and 1955-56, using a work force from the Field Group, Golson started excavations on a small pa on Stingray Point, Mercury Island, where they recovered finely built interconnected sunken pits with drains and a complex picture of superimposed postholes. Other excavations followed at the Oruarangi swamp site in December 1955, and over the summer seasons of 1956-57 and 1957-58 at Sarah's Gully.

The new form of prehistoric evidence appearing from these excavations was registering a different aspect of prehistoric life in New Zealand. They were the result of specialised activities either within larger sites or in isolation identifiable only in the form of post-holes, pits, drains, ditches, fireplaces and the like. Such features were rarely associated with the diagnostic artefacts upon which the archaeological framework of New Zealand prehistory depended. The genealogies, adzes and fish-hooks of the earlier generation were being displaced by drains, pits, hollows, and a profusion of postholes. A very different picture of the Maori past was being exposed.

Within the Archaeological Society the campaign to upgrade the standards of local excavation was led by example, the only effective way of influencing independent-minded New Zealanders. Jack was the first to join in the heavy shovelling when dumps were to be moved, he was an expert turver, his trowel was constantly in use. If he was not allowed to share in the cooking it was not from lack of willingness but good sense on the part of the hungry teams. Every new recruit, whether student, housewife, or reformed fossicker, was individually tutored in the basic skills: trowelling, brushing, sieving, shovelling. As skills developed under his tutelage, rivalries and competition enhanced performance. Buckets and wheelbarrows were
jealously guarded, favourite spades coveted. At meal times and other convenient pauses Jack attempted to explain the overall stratigraphy; he encouraged everyone to get to know the whole site and not just the areas they were working on. He was an excellent trainer of apprentice excavators, but always through example.

The success of this strategy was recognised many years later, when on a return to New Zealand from Canberra, the Auckland Archaeological Society presented him with a vinyl pressing of the campfire songs from his excavations in the Auckland province, from which come the verses quoted in this paper. It was entitled, from one of the songs, 'Sweat in the Sun Mate', the common experience of all who participated in his excavations. These were not minor scratches in the soil. But the excavator, by all the skills at his command, had to relate the features, through stratigraphy, types of fill, shapes, dimensions and so forth. In many ways it was the archaeology of context without content. In emphasising method, Golson's training and experience in the excavation of medieval sites was asserting itself. It was clear from the camp-fire talks and his lectures that his Danish experience with Axel Steensberg had influenced him strongly.

Two further aspects of his approach to establishing a new tradition of excellence in the field require mention. The first was certainly the basis of his success. He had early recognised that prehistory in New Zealand had for a very long time, through the authority and widespread belief in the Fleet stories, belonged to the people. It was not then and was never during Golson's time, at least in Auckland, a specialised monopoly of the professional. The second was Jack's recognition that he would have to spread the burden of these intense, time-consuming excavations. At an early stage he was delegating responsibility to others; from this emerged the fine excavation team of Laune and Helen Birks and the introspective intensity of Ham Parker, who became the Director of the Opito excavation.

THE RESTRUCTURING OF NEW ZEALAND ARCHAEOLOGY 1957-59

For over three busy years, through his loyal work-force (known as 'Golson's gang' in early issues of the Newsletter) he had been the crusader of 'the Woolley/Wheeler way'. The Christchurch excavation at Moa Bone Point Cave in 1957 was his opportunity to extend the message beyond the Auckland Society, particularly to the South Island where it was clear, in the long tradition of rivalry between the two islands, the growing influence of Auckland was causing some unease.

This influence was not only through the activities of the Society. In his day-to-day contacts, over the telephone or a beer at his favourite pub 'the Kiwi', Golson was the persuader of many stubborn and proud archaeological mercenaries to disarm and abandon their long-handled shovels and private collections for the trowel and camera. Some of them joined the Archaeological Society and came to the excavations; others preferred to join the national body, the newly formed NZAA; others stayed stubbornly aloof. These grass-roots contacts were to pay handsome dividends at the Wanganui Conference where the contentious issue of site 'ownership', a central concern of many amateur diggers and curio-hunters, was to come under scrutiny. The regular Council meetings of the Association, usually at Wellington, brought him into contact with more and more people either sympathetic towards or already involved in New Zealand or local archaeology. Particularly important were the contacts in Wellington where the field recording tradition established by Best and the Dominion Museum was still alive and where there was keen interest in establishing a national site register. The quiet extension of Golson's influence into the national capital brought talented recruits from among the Civil Service and the Dominion Museum. They were to play an important role in the transformation of the infant Archaeological Association into a respected National organisation. His activities became well known, even in such unlikely journals as The New Zealand Woman's Weekly. There were many he had to contact, farmers and land-owners for permission to dig, local historians, petty officials, journalists, and to all he was the charmer who won over those suspicious and wary of any and all foreigners (particularly university-trained).

Moa Bone Point Cave, Summer 1957

The magnificent coastal cave at Moa Bone Point was already well known in New Zealand archaeology from excavations by Von Haast in the 1870s (Von Haast 1875). Less well-known was the scale of activity of curio-hunters and amateur diggers in the cave during the intervening years.

Under electric light supplied by the local authority a magnificent Wheeleresque grid was laid out within the large cave and 'Golson's gang' proceeded to demonstrate the precision of the
new techniques. Under the astonished eyes of local officials and journalists the flashing trowels recovered with fine precision the association of moa-bone artefacts, decayed leather boots, bottles, the odd broken stone adze, moa bones and rusting cans; the confused detritus of nearly a century of ransacking. I spent several days in a very dark square (with immaculately straight faces) in one of the deepest parts of the cave delicately exposing and preparing for photography strange mounds formed of concentric rings of dense, organic material. These proved, on identification of the Indian doab grass they contained, to be the droppings of circus elephants housed in the cave in the previous century.

It was in fact a fine stratigraphic record of the anarchy which the God-given right to dig had prescribed. The last verse added to one of the Sarah's Gully songs, (ironically entitled "To Delve into a Midden"), with words by Sue Bulmer, one of the 'gang' who had travelled to Christchurch, is a better record:

The Auckland group went down to Christchurch,
A bunch of enthusiasts,
And spent two weeks following
In the footsteps of Von Haast.
The digging it was delicate
The discoveries something grand,
We found iron bolts and moa bones
Six feet in natural sand.

The revival of field archaeology, Wanganui, 1957

The visible wealth of field remains, dominated by the impressive earthworks of pa had always excited a handful of enthusiasts, despite the general decline in interest in the twenty years before Golson's arrival. Many of these enthusiasts were attracted to the newly formed Archaeological Association and through their membership and the wide network of contacts Jack had established, many became known to him. From their enthusiasm and his own field trips came his definitive paper from this early period, 'Field archaeology in New Zealand' (Golson 1957a).

It was an outstanding contribution to New Zealand archaeology because, despite Jack's claim to be 'artefact-oriented', it shifted the grounds for study away from the exhausted adzes, fish-hooks and ornaments of the Skinner/Duff era towards the more formidable, diverse and extremely numerous field remains. As these were largely in the North Island it was a formal statement that the dominance of the South Island in the writing of New Zealand prehistory was drawing to a close. I suspect when the definitive book on the history of New Zealand archaeology is written this, rather than some of his later and better known papers, will be singled out as one of his most important contributions to New Zealand archaeology. The publication of the paper was also timely. The first two annual conferences of the NZAA, at Auckland in 1956 and at Otago in 1957, had been largely concerned with academic issues and reports of current work, but the next conference at Wanganui was to be devoted to field archaeology and the promotion and problems of site recording. Before the Wanganui conference it was important to restore the academic credibility and prestige of the evidence of that part of Maori prehistory which could not be transferred to museum display cases.

With the outstanding exception of the Dominion Museum's early initiatives and the local enthusiasts who followed them, institutional involvement in site recording was a national disgrace. The Auckland Institute and Museum, for example, in the centre of one of the greatest proliferations of visible field monuments in the world, sponsored the 'extraction' of artefacts from sites such as Oruarangi and maintained a small inventory of the more important sites from which collections had been recovered, but showed little interest in ensuring that the archaeological sites were preserved or the hundreds of others recorded. As in most of the rest of New Zealand this was left to a handful of enthusiasts, and it was their work which Golson's paper saluted.

The Wanganui conference, one of the great landmarks in the emergence of modern New Zealand archaeology, is poorly documented. It was well organised, and apart from a slide show on the Moa Bone Point excavations, artefacts were barely mentioned. I was one of the few students present and was not privy to the behind-the-scenes jostling which was obviously going on. Jack was often seen in earnest discussion or debate with one or other of the prominent people there; the Council had several meetings during the course of the Conference. After some of the more quarrelsome sessions he could be seen developing new strategies or rehearsing additional arguments with supporters, a technique which I got to know well in later years. At morning-coffee and other breaks various knots of differently aligned participants could be seen muttering and debating. I drifted, in some bewilderment, from group to group but everywhere the topics were the same, those most precious of all the legacies from the 'Do-it-yourself' tradition of New Zealand archaeology, the right to 'ownership' of site-locations, rights of access, and the God-given right to dig, the very issues on
which members had been given assurances in 1955 (above).

Following a paper by Jack introducing New Zealand field sites which supplemented and illustrated his published paper, there followed a notable address by J.D.H. Buchanan, a school teacher at Wanganui Collegiate. With the innocent title 'The Recording of Archaeological Field Evidence' it was actually a blueprint for a national site recording scheme. He presented a carefully argued paper on organisational issues which was the beginning of the national site-recording scheme and in the long-term of the legislation to protect and preserve sites.

As most members of the Association were also members of the Polynesian Society, it is probable that Golson's paper which had been published before the Conference had been read by many of those attending. It gave the field monuments a setting and academic respectability they might not have achieved otherwise among many of the artefact-dominated members of the Association. Astonishingly, many who had expressed fears that their private interests in sites might be threatened succumbed meekly to the possibility of a National Register; the promise of restricted access to files was a consolation, but on the whole the concept of private 'ownership' of sites, access to sites and the information they contained, appeared to wither away. There were some strong feelings expressed with some resignations, but on the whole most accepted that a national register of sites was now inevitable. The ownership of the past was passing into institutional, and eventually through legislation many years later, national control. When, in the Newsletter, it was reported from the meeting at Wanganui that Council would expect new members to agree to the Association's conditions which included (words by Ron Scarlett 1958) 'never, never dig just to obtain artefacts', even the God-given right to dig appeared to have been ceded to authority.

The remarkable and on the whole amicable agreement from this meeting to establish a site register and promote field recording was the achievement of many. Golson, by the timely publication of his paper, his astute management of the discussions, his lobbying and command of the issues, ensured that the purpose of the Conference, which could have been so divisive, was achieved.

Pakotore, 1959

Golson, undeterred by the somewhat hollow demonstration of excavation techniques at Moa Bone Point Cave, made one more attempt at adult re-education. At the Wanganui conference it was agreed that the next conference in 1959 would be devoted to 'archaeological method, techniques and ethics' (Golson and Brothers 1959:29), and a suitable venue at Rotorua with a nearby important archaeological site, a magnificent large pa named Pakotore, was announced early in 1959 (Golson and Stafford 1959).

Although the 're-training' aspect of the Pakotore conference was not a failure, some of the earlier generation of diggers to whom the whole operation was directed did not join the two-days of excavation. Nor did it result in a new wave of Wheeler-style excavations throughout the land. It must have been evident to many of the old hands that the technical support and manpower required to mount excavations on the scale of the Pakotore demonstration were beyond the resources of individuals and most local groups. New Zealand archaeology, regardless of whether new techniques were accepted, was becoming professional by default. The destruction of the majority of artefact-rich coastal sites by curio-hunters, as the Moa Bone Point Cave excavation demonstrated, forced attention towards larger and more difficult sites where individual enterprise had little place. In addition, access to scientific support such as C14 dating was closed to the individual; the era of 'Do-it-yourself' archaeology was drawing to a close.

Golson's role in the change in 'ownership' of New Zealand prehistory, might appear, from the previous discussion, to be largely as a supervisor of the inevitable. But his policy at both Moa Bone Point Cave and Pakotore of large-scale excavation, Wheeler-with-a-vengeance, was decisive. The same basic techniques could have been demonstrated on much smaller excavations within the resources of the individual amateur; there was no such concession. The Sarah's Gully excavation, on completion, was a large area-excavation, no mere test-pit, and the excavations which followed at Kauri Point were also massive. By such demonstrations, Golson made obvious that archaeology in New Zealand now required a large investment of labour and resources. It is significant that legitimate techniques of small-scale sampling, which would have given excuse to the amateur and curio-hunter, were never demonstrated. It was a form of academic bullying essential to discipline and redirect the energy of the amateur. But an alternative outlet for this energy had already been proposed at the previous year's conference in Wanganui, site recording, and some who were at Pakotore were to be prominent in the scheme in later years.
There was another sign of 'the changing of the Guard' at Pakotore to which we can trace Golson's influence. Two new faces were seen at that Conference. The first was that of Peter Gathercole, newly appointed to a joint position at Otago University and Museum. As an old friend of Jack and a Cambridge graduate, the appointment of Gathercole appeared to me at the time, and I suspect to many others, as a decisive pincer movement, isolating the old guard between the two University teaching centres and shattering the traditional North/South divide. Gathercole, from the same school of political training as Golson, but with an entirely different style, managed to resurrect and strengthen the teaching of anthropology and archaeology at Otago. My appointment there in 1963 completed the encirclement. Wheeler the military man would have enjoyed his victory in the distant Antipodes. The result is well known; two excellent teaching departments, enjoying high international reputations, are now in command of New Zealand archaeology. Some of the credit for this must also go to the other newcomer at that Conference, Roger Green, soon to be Golson's successor. That Green was in New Zealand and based in Auckland was clearly due to Golson's growing stature outside New Zealand, particularly his involvement in the TRIPP programme of Pacific archaeology. His presence was a clear indication that New Zealand archaeology was becoming internationalised and that international standards, not those developed in the isolated tradition of the past, were now required.

Pakotore then, through the success of the new university-trained archaeologists, signalled the birth of professional archaeology. The Council of the NZAA made no further attempt to bring the earlier generation of amateur diggers in from the cold; the new generation of students and graduates began slowly to dominate the membership of the Association. The change of ownership, however, did not result in the permanent loss of the energy and enthusiasm of the amateur, for much of this was re-directed into the more productive and less destructive activity of site-recording.

THE STATE OF NEW ZEALAND ARCHAEOLOGY IN 1960

By the end of 1959, in the year before Golson's departure for the Australian National University (ANU), the state of archaeology had been transformed from that of his arrival in 1954. There was a firmly founded National Association, and many of the divisive territorial rivalries between individuals and institutions were beginning to recede, although a new one between the university-trained professionals and the rest was emerging. Field recording was showing a healthy revival and although state involvement in site protection was still some time away, the existence of a unified National Association and a site-recording scheme were first steps. Although efforts to improve the excavation techniques of the older diggers were unsuccessful, the expansion of archaeological teaching in the Universities which Golson's academic achievements and influence did so much to promote, ensured that the teaching of sound field methods was firmly established.

It is improbable that many today will have recognised in Golson's 1955 paper (Golson 1955a) the foundation charter for professional archaeology in New Zealand. In the oblique wording necessary at the time it warns that the circumstances of New Zealand archaeology, with such a brief prehistory and with so much of the surviving information trapped in large intractable sites, required that the extraction of that information through excavation should be in the hands of fully trained professionals. Within five years this was coming about, but only those who were at the Wanganui conference could know how close to failure came the handing over ceremony. The timely publication of his 1957 paper, skilful lobbying and the promotion of open debate eventually allayed suspicions of a foreign takeover. Jack was skilled at finding and recruiting those of goodwill and commonsense who saw that the preceding years of archaeological anarchy must come to an end. He also had the wisdom to pass the responsibility for developing and shaping future policy, the detailed operation of the site recording scheme and so on to these people. Few of the participants at that Conference appreciated the irony that the slides of the Moa Bone Point excavation of six months before, shown on the final evening, displayed a perfect stratigraphic record of the archaeological anarchy which their Conference had brought to an end.

The success of the Wanganui conference alone, rather than his better known academic contributions, is the real measure of Golson's years in New Zealand. Few were better equipped to help change the organisation and ground rules for archaeology in New Zealand; few would have had his ability to persuade, cajole and bully the devotees of a long-established tradition of archaeology to reform and accept the discipline of institutionalised archaeology. What is surprising is that most of this was achieved within five years of his arrival.
If the flourishing community of professional archaeologists in New Zealand, his direct lineal descendants, are barely aware of the significance of these few years it is because by 1959 the changes during the Golson years were so profound and the emergence of a fully professional New Zealand archaeology so rapid that it is possible now to write about New Zealand prehistory, apart from ritual acknowledgment of a few pioneering works (e.g. Best 1927; Duff 1950), without referring to any publications before 1959. In truth, apart from a handful of seminal studies of artefacts, such as those of Skinner, there is not much worth reading or quoting before 1959. A glance at the bibliography in Janet Davidson's book The Prehistory of New Zealand (1984) will confirm this. There are only four of Jack's publications in that bibliography and none earlier than 1959. Why this should be may be more apparent in the next and final section.

THE STATE OF NEW ZEALAND PREHISTORY ON JACK'S DEPARTURE

I have discussed Jack's academic contributions only where relevant to his role in the transformation of New Zealand archaeology, but there are some academic threads which must be untangled if other aspects of his unpublished activities in New Zealand are to be fully appreciated. One was his role, complementary to his activities within the archaeological community, of persuading and encouraging others to join battle in some of the important academic debates of his time by publishing their work and ideas.

Although the authority of the traditional story of the settlement of New Zealand was weakening at the time of Golson's arrival, it was by no means dead. The New Zealand public's appetite for these stories seemed inexhaustible; Buck's Vikings of the Sunrise was reprinted in New Zealand in 1954, Duff's Moa-Hunter Period of Maori Culture was revised and reprinted in 1956 but with the introductory chapter dealing with traditions unchanged, and the continued popularity of Buck's The Coming of the Maori is evident from the reprinting in 1958. In all these books the Great Fleet, the central focus of the traditional story, and the basis of contemporary Maori social organisation and emotional identity was still intact.

It is significant that Golson in his 1955 paper does not appear to question the Fleet either:

Cultural diversity in New Zealand prehistory is on present evidence limited to moa-hunter and Fleet Maori, while after 1350 the latter were sealed off completely from cultural influence from outside (Golson 1955a:115).

It is certain, however, that by that time he must have been aware that the Fleet traditions themselves were soon to be openly challenged.

By 1956, the editorship of JPS was shared between Wellington and Auckland, and the editors were adopting a policy of publishing more contemporary Polynesian anthropology; its former role as the official organ of the traditionalists was changing. That year the Society published Andrew Sharp's Ancient Voyagers in the Pacific (Sharp 1956a). From the acknowledgements in the book it is clear that Golson had been consulted. In the controversy this book created, Sharp's lesser known paper claiming the Fleet canoes were merely internal movements within New Zealand (Sharp 1956b) escaped serious attention. It was the first scholarly claim that the fabled Fleet did not exist.

In the important contest which followed, Golson wisely stood aside and played the role of linesman, umpire and scorer. As the silent ignoring of Sharp's arguments by the majority of dedicated traditionalists and most of the Maori community was effectively silencing debate, Golson, who became joint editor of the Journal in 1957, encouraged other players to enter the contest, for example J.B.W. Robertson (1957, 1958). Throughout 1959 and 1960 the debate simmered down; the Fleet was still afloat and Jack either was not successful in soliciting further contributions as sole editor of JPS from January 1959 to June 1960, or was already planning a different, non-confrontational approach, organised after his departure to Australia, which resulted in the excellent Symposium on Polynesian Navigation which he edited (Golson 1962).

Jack was well aware, as were others who stayed remarkably silent throughout, that direct involvement by archaeologists in these debates would not only harm the long-term prospects for unity within the archaeological community, many of whose members were very committed to the traditional accounts, but would also cause irreparable damage to long-term relations with the Maori who were, after all, the principle actors in the prehistory the trowel and spade were revealing.

He tried a different approach. The buoyancy of the Great Fleet was partly maintained by archaeological and oral historical arguments that the tribes, descended from the Fleet, established their supremacy through the introduction of agriculture. Aware all the time of the pits discovered at Sarah's Gully and their probable function as tuber-stores, Jack enlisted the aid of agricultural
botanist Douglas Yen. Yen's convincing argument that a period of experimentation and adjustment to the colder climate would be required before the sweet potato could have become a staple, neatly demolished the arguments of Duff and Buck (Yen 1961). Although this important paper was published after Golson's departure, it was clearly solicited, debated and devised in many discussions before he left, probably in the unofficial seminar room of New Zealand archaeology, the bar of 'the Kiwi' pub.

An upsurge in the numbers of articles with archaeology-related themes during Jack's period of editing of *JPS* was to be expected, but without his coaxing and persuasion it is certain that many important papers, including Skinner's (1959) classic on Murdering Beach would never have been published. The editing of eight issues of the *Journal* between 1958 and 1960 took a great deal of his time. Nevertheless he managed to write his most widely quoted paper 'Culture change in prehistoric New Zealand' (Golson 1959c) during this period, but its content owes little to the theme discussed here, Jack's role in the transformation of New Zealand archaeology.

I have tried here to recover some of the unrecorded aspects of Jack's years in New Zealand. I have been forced to leave out the contributions of many others who supported him and, after his departure, carried through the changes which he had directly or indirectly initiated. As I was asked at the last minute to contribute this paper, there will also be errors because I have had no opportunity to check with others who will have better memories of his crucial early years. This will apply particularly to members of the University of Auckland Archaeological Society whose sweat made the Golson years possible. I will close with one of the few records we have of his personal impact during those excavations many years ago:

> Jack's let his hair down  
> Digging in his singlet.  
> My what a lily white back he's got.  
> Dig up those moa bones, dig,  
> And shovel, and trowel,  
> and shovel...  
> from 'The Sarah's Gully Song'.

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'Dig Up Those Moa Bones, Dig'
'Dung carts may be able to tell more of economic life than chariots and hearses'; a challenging assertion this, for discussion on my 1952 Cambridge Tripos paper. It symbolised the democratic, anti-establishment-archaeological approach to prehistory promoted at Cambridge by the non-egalitarian Grahame Clark and, at London, by the more radical but less field oriented Gordon Childe. Jack Golson, already a student celebrity before my arrival in Cambridge forty years ago, was immersed in graduate research exemplifying the merits of uncovering the lifeways of obscure Medieval peasants. Over coffee in the library, or beer in a pub, his sense of excitement proved infectious.

Membership of the Deserted Medieval Village Research Group involved Jack Golson in fieldwork in Lincolnshire and excavations at Wharram Percy, on the Yorkshire wolds (Hurst 1956:258, 272). During the 1952 summer many students accompanied Grahame Clark to dig on another notable Yorkshire site, Star Carr, Seamer; I headed for Libya's Hau Fleah with Charles McBurney's expedition; Jack Golson crossed to Denmark, to experience fieldwork with Axel Steensberg. His training there on the lost village site of Store Valby proved rewarding and formative. It also became legendary following his return, when he related the saga, methodology and significance of excavating the bones of a medieval cow, to the student archaeological field club at which members reported summer fieldwork activities.

Our fortunes intersected socially on a memorable departmental field trip to Kent early in 1953, where under the informal staff-student relationships existing on such tours, Glyn Daniel entertained Jack together with his students, (Sir) David Wilson and myself, over a convivial meal in Canterbury. A few weeks later our paths crossed more significantly. Grahame Clark had been consulted by the University of Auckland concerning the appointment of its first prehistorian. He assumed that I would accept the post, possibly on the common British assumption that New Zealand and Australia are identical and that cities such as Auckland and Melbourne are close neighbours. (When Clark visited both countries in 1964, the impact proved a revelation). After I declined the proposal, he sent for Jack Golson. Jack only commenced his Wharram Percy excavation in June 1953 (Beresford 1954:74), so it meant abandoning his rewarding career as one of the first archaeologists of Medieval Britain. He migrated a few months later, to shape the future of antipodean archaeology in a country which was settled only around the period when Wharram Percy village was deserted.

In a thoughtful and humane essay Jack Golson (1986:2-4) has written about myself in a manner that is equally applicable to himself. He drew attention to 'the parallelism of our careers', both historians turned prehistorians, 'and for some while (we) remained the only professionally trained archaeologists of our region'. When we commenced teaching duties at Auckland and Melbourne respectively, early in 1954, our intellectual baggage typified 'the archaeological tradition in which we had been schooled'. It stimulated us to action, both in the field and the classroom, with a 'striking similarity of approach', which Jack traces so objectively.

This was the era in which Harold Macmillan coined the 'winds of change' simile. Neither Jack nor I realised the extent to which gales would blow through archaeological theory and practice during the later 1960s, to drastically alter the presuppositions and explanatory basis of our proud new discipline. It reflects the strength of Jack Golson's intellectual powers, that he adapted so rapidly to those winds blowing strongly both from America and from a more ecologically aware and numerate Cambridge. His department has been in the forefront of new archaeological applications, although seldom garnished with the high-sounding, pseudo-scientific jargon which became trendy elsewhere.

From our isolated archaeological outposts, Jack and I exchanged news through the fifties. I envied the manner in which he brought system and many students into New Zealand prehistory.
By contrast, it was 1957 before I introduced a course of Pacific Prehistory as a final year honours History option at Melbourne university. There were only six students, but they were keen. As a class exercise they reviewed Andrew Sharp's recently published *Ancient Voyagers in the Pacific* (1957). Their project was coordinated by Gregory Dening (1958) and published in *Historical Studies Australia and New Zealand*. Dening later completed a Master's thesis on Pacific voyaging, which was examined by Golson. The significant results of Dening's research were incorporated by Golson (1962) in the important Polynesian Society memoir, *Polynesian Navigation*, which he edited.

The Australian National University wisely decided to include Prehistory within the ambit of its Research School of Pacific Studies. Amongst others, my advice was sought by John Barnes, professor of Anthropology. I responded emphatically on 23 July 1959: 'Concerning prospective appointees, Jack Golson ... is undoubtedly the key man to approach.' Fortunately Jack was appointed to the Fellowship. Prior to taking up his new post, he took some leave in England during 1961. He briefly was our guest in London, where I held a Nuffield Foundation Fellowship. As the rules precluded boarders in the Foundation's Regent Park flats, the Fellowship's supervisor was told that the large person in the spare room was our children's nanny. (In those expansive times, that was the use which the room was expected to serve). These were heady times in London. In addition to celebrating Jack's translation to Canberra, I also met Richard Wright and Vincent Megaw, both soon to depart for Sydney appointments, and my former student Ian Crawford, who had completed a course at the Institute of Archaeology and returned as curator to the Western Australian Museum. Back in Australia, Isabel McBryde had begun fieldwork in New England, and I prepared a paper for the inaugurating conference of the Australian Institute of Aboriginal Studies (AIAS) while in London.

After returning to Melbourne I received a welcome Nuffield Foundation grant of 3300 pounds, riches indeed in that era, to assist fieldwork between 1962 and 1965. My application had emphasised the requirement for interstate and inter-disciplinary co-operation if prehistory was to achieve maximum results. Jack Golson and I proposed some joint systematic research project to achieve that end.

There were many reasons in 1963 why we travelled so far afield to the Northern Territory's Top-end. I had dug sites at Fromm's Landing and Kenniff cave; Jack proposed initiating research in the Riverina and on the New South Wales (NSW) south coast. Richard Wright and Vincent Megaw already were active around Sydney, while Isabel McBryde was mastering the New England region. Rhys Jones was commencing research in Tasmania. Consequently, it seemed that the general pattern of southeastern prehistory was being sketched and other regions required attention. Richard Wright ventured to Cape York around this time and Ian Crawford was working in the Kimberleys. The potential of Arnhem Land had been demonstrated in 1948, by the National Geographic Expedition, particularly in the Oenpelli region. During December 1960, I had been visited by Bob Wren, a Commonwealth Scientific and Industrial Research Organisation (CSIRO) officer at Katherine, who excited my curiosity by showing me artefacts and talking of limestone caves in his area. Another officer at the Katherine CSIRO research station was Wally Arndt who published two articles on local rock art in *Oceania* during 1962 (1962a; 1962b).

Although these studies may not attract much notice today, we were both attracted by the use which Arndt made of local Aboriginal informants, to explain the mythological significance of the art. Such approaches were then rare.

I suspect, also, that all of us were drawn by the romance of distant tropics and their traditional indigenous people. We were all moderate diffusionists in those times, so the prevailing assumption was that evidence for routes of migration and the earliest occupation sites would be found there. Generally, the emotional message seems to have been 'go north, young archaeologist'. That Jack's graduate student, Carmel White (Schrire) excavated the world's oldest known ground and grooved hatchets near Oenpelli during 1964-65, surely justified this optimism and was a foretaste of even earlier discoveries, only confirmed in 1990.

Golson and I were presented with an unexpected opportunity to plan our expedition when, in May 1963, the interim council of the AIAS invited us to visit the Gove Peninsula. Our brief was to assess the potential risk to archaeological sites, should the proposed harbour facilities relating to bauxite mining be constructed. This was the period when the Yirrkala community presented its bark petition to parliament. The Minister for Territories, Paul Hasluck, made a long statement in Parliament on 9 April 1963, promoting the policy of assimilation and the advantages to the Aboriginal people of facilitating mining. It was a feverish atmosphere for two new chums to evaluate archaeological relics.
and, understandably, the compassionate Yirrkala missionary, the Rev Edgar Wells, had other priorities.

Jack joined me in Melbourne and we flew to Darwin on 20 May, making our first venture into tropical Australia. Darwin in 1963 retained much of its wartime ravages and its pre-war tempo. Yet it seemed a place with a future, when we judged a Smith Street cafe to offer Australia's best meat pies. It was disillusioning when the next meal time arrived, because Jack ascertained that the pies were flown in regularly from Brisbane.

Rereading the report on our field survey to the AIAS which Jack drafted, it is surprisingly comprehensive for such a brief but exhausting tour. Although we stated that in those areas covered by our brief, there appeared little threat to archaeological sites, we were painfully aware that the potential impact upon the Aboriginal community was much more significant. Back in Darwin, there was time for discussions with officials concerning site protection (or lack of it), and a hurried drive to Katherine with the enthusiastic young field naturalist Bill Walsh (later Strider). There we met Bob Wren, visited sites, and discussed the logistics of our proposed expedition.

In our wide-ranging report to the AIAS we recommended urgent action to protect Millin-gimbi shell middens, Port Bradshaw Macassan sites, and highlighted 'flagrant violations of conservation ethics' evident in some places. Writing as concerned archaeologists, we emphasised the need for site protection and education in the Territory. These were challenging comments for 1963.

Because of its isolation, we had assumed that it would be possible for expeditions to select an area and attempt systematic field record and collection in virgin territory. In many areas, already, this is impossible because enthusiastic but misguided amateurs have carried out considerable field activity. Not only is their work of no scientific value, but it is positively detrimental to future research. For example, caves have been fouled, while holes have been dug in floor deposits; large but selective collections of artefacts have been made on surface sites; finds have been dispersed, unlabelled and uncatalogued; some material has been removed from Australia; rivalry between individuals has led to concealment of evidence and misleading announcement of discoveries; in one instance, a large collection of artefacts was buried in a hole because the owner no longer wanted it.

We feel that it is urgent that this activity is directed into purposeful channels, and that a more sympathetic interest in aboriginal antiquities be awakened throughout the Territory.

We immediately began preparation for our return expedition and our respective parties arrived in Katherine, by road or air, during July 1963. Our personnel included, amongst others, Wal Ambrose, Diana Howlett, Dermot Casey FSA, Edgar Waters, and Jim Bowler, who was experiencing his first contact withprehistorians. With our base camp in a large shed at the CSIRO experimental farm, we agreed to divide our relatively large labour force and to mount two field parties. Apart from assisting my team at Kintore cave, Jack's party excavated at Sleisbeck and at a rockshelter near the Katherine airfield (Plate 1).

Plate 1 Jack contemplating his excavation at the site which he designated NTS, just outside the perimeter of the Katherine airport. Date: August 1963.

In light of recent misconceptions concerning our activities, I emphasise that neither of us visited Coronation Hill, while Jack retained the permanent services of a Djauan elder, amongst other direct contacts with Aboriginal people. It is an embarrassment for both of us that, for various reasons, we failed to publish detailed excavation reports. However, all material has been freely
available to researchers, and several theses, doctorates included, have used it extensively. The direct sequel to Golson’s literal trail-blazing expedition to Sleisbeck, was Carmel Schrire’s research in the following year.

Memories of that distant season on what, for 1963, were remote Australian frontiers, include the excitement of visiting superb art galleries and occupation sites which were ‘unknown to science’. They were known to Aboriginal people, however, and the impact of their cultural values on Jack Golson’s thinking was profound. So, also, was the perception of racial prejudice and deprivation, sadly evident in Katherine.

There were numerous memorable incidents of a nature for which Jack Golson is renowned, from Polynesia to Port Moresby. One was the occasion upon which my group vacated Kintore cave for our CSIRO base, arriving there in the heat of the dry season noon. Opening the refrigerator for welcome cool refreshment, we discovered that its sole contents was a barrel of anchovies.

Before he left Canberra, Jack had ordered a large number of ranging poles, intended for other projects. They were airfreighted to Katherine by mistake. These surplus poles and further brand-new surveying equipment were stowed on the tray of the four-wheel drive utility. When Jack was driving, a straying cigarette butt set fire to the equipment on the tray. Fortunately Wal Ambrose plucked a petrol can from the flames and the fire was extinguished. For years afterwards, fieldworkers made do with charred surveying gear. As I had hired the vehicle from the Territory administration, I was presented with an unexpected repair bill from my Nuffield Foundation funds.

During August, Jack’s party received dishonourable mention in the Northern Territory Legislative Council. The robust speaker was ‘Tiger’ Brennan, Member for Katherine, who had met Jack’s team in the Katherine pub. Whatever their actual conversation, Brennan’s speech on the subject of Aboriginal Reserves and bauxite mining, distilled from that encounter local prejudices and racial perceptions (Northern Territory Legislative Council Hansard 1963, p.903):

I might mention that at Katherine I was talking to several people from the National University at Canberra. They were not anthropologists they were archaeologists – they all come out of the same bin, though, and they spoke about Yarrkala. They said ‘It is such a peaceful village, why go and disturb them?’ Mr President, if we do not do something with this country we won’t be disturbing them but someone else from overseas will be disturbing them. And that is what we have to get down the necks of those dumb-clucks down south.

Jack Golson’s role in facilitating research into Australian prehistory during the sixties has received insufficient recognition, possibly because it became overshadowed later when he moved to investigate New Guinea prehistory, while the pace of later discoveries obscured his pumm-priming and problem-posing role. Although he received expert advice and assistance from his two imaginative technical support staff, Wal Ambrose and Ron Lampert, he was the sole academic staff member until I joined him in 1965. Because the fledgling Australian Institute of Aboriginal Studies was located in Canberra, Jack provided considerable counsel before it became formally constituted in November 1964, and thereafter he served on its Prehistory advisory committee for several years.

With AIAS support, Jack Golson inaugurated a fieldwork programme in two regions. The first was the NSW south coast, where his research was continued so ably and successfully by R.J. Lampert. Classic excavations by Lampert, with Golson’s enthusiastic support, included Burrill Lake, Durras North, Murramarang and Currawong. Golson also undertook widespread preliminary surveys in the Riverina. Amongst valuable contacts established, were those with Simon Pels, a Research Officer at Deniliquin, with the NSW Water Conservation and Irrigation Commission. Pels stimulated interest at the 1962 ANZAAS congress with his aerial photographs and related ground surveys of ‘prior streams’, ‘ancestral rivers’ and ‘source bordering dunes’ (Pels 1971). The archaeological implications of changing Riverina landscapes through time were well taken by Jack Golson and his geomorphological colleague, Joe Jennings. Jim Bowler’s discoveries in the Willandra Lakes system owed much to their encouragement.

In this instance, as elsewhere, Jack Golson showed much deeper awareness of the inter-relationship of environmental and ecological factors with human culture, than he allows in his reflective self-criticism of his assumptions and perceptions around this period (Golson 1986). An outstanding and pioneering example of his comprehension is provided by his detailed paper, ‘Australian Aboriginal food plants: Some ecological and culture-historical implications’ (Golson 1971). It was prepared in 1968, before Rhys Jones, Richard Gould, Sylvia Hallam and Nicholas Peterson all contributed variously over the next few years to a truer appreciation of ecological factors in the human past. His study
remains one of the most scholarly investigations of Aboriginal plant exploitation. It also was significant in directing attention to the relevance of Southeast Asian flora in assessing the impact of the Australian flora upon prehistoric migrants and their adaptive advantages. It also undermined the simplistic Eurocentric division between hunting-collecting (Paleolithic) and farming (Neolithic) economies.

Jack Golson's research was achieved within the context of the 1968 symposium which we organised jointly and which was funded by the Research School of Pacific Studies. *Aboriginal Man and Environment in Australia* was not published until 1971, but it reflects knowledge and research directions at the end of the sixties. Although funding did not extend to inviting a few distant scholars, the series involved interaction between researchers in many disciplines, some of whom previously had little contact with prehistorians. It produced useful state-of-the-art summaries from various disciplines, together with the first major stock-take of ongoing archaeological discovery. In our preface we made claims which time has confirmed (Mulvaney and Golson 1971:VI): 'It constitutes a landmark in Australian prehistoric research, in its attempt to re-evaluate evidence critically, in its interdisciplinary approach, and in its expression of some contemporary trends in research'. Jack played a major editorial role, supervised index preparation and ensured that it appeared in such a pleasing format. He had considerable input, also, into a subsequent interdisciplinary meeting, the 1971 symposium *Bridge and Barrier: The Natural and Cultural History of Torres Strait* (Walker 1972).

The great need at that time in Australia was for a major radiocarbon dating facility. That the Australian National University (ANU) established a laboratory, when and how it did, owed much to Jack Golson's energy and powers of persuasion. When in New Zealand Jack had forged firm contacts with Athol Rafter's pioneering radiocarbon dating laboratory and as early as 1955 expressed keen awareness of its importance (Golson 1955). After his arrival at the ANU, he found that the priorities amongst relevant scientists lay elsewhere. Even after my arrival in 1965, I recall attending meetings with earth scientists, where one object was to convince them of the supreme importance of dating samples which seemed trivial by their recent ages. The funding compromise which was agreed upon was largely Jack Golson's solution. The Research School of Physical Sciences (later Earth Sciences) would house and fund the facility, but staff were to be on the establishment of the Research School of Pacific Studies. For an initial period staff salaries were paid by the AIAS. This result was a considerable achievement, because the Prehistory section within that School only gained the status of a Department in 1970, when Jack was appointed Professor.

Henry Polach arrived to establish the outstanding ANU radiocarbon dating facility in 1965 and the first dates were produced a year later. As he came from Athol Rafter's pioneering New Zealand laboratory, Jack Golson's previous positive connection continued. Age estimations are so standard an item in any fieldworker's repertoire a quarter of a century later, and estimations so readily obtained (for example funding is included in research grants), that modern archaeologists may not appreciate the difficulties before the ANU laboratory existed. In 1956 I solicited six free samples from an American laboratory through the good offices of Hallam L. Movius; in 1961 I carried some Kenniff cave samples to England and begged for some to be accepted; in 1963 I had funds sufficient for four samples at an American commercial laboratory.

Once a trickle of dates flowed from the ANU facility, the AIAS received many requests from archaeologists for their samples to be submitted within the Institute's quota, granted in return for the staff salaries. It was evident that some field-workers were unaware of the correct field procedures for sample collection, or their optimum size. Difficulties also were encountered in interpreting the resulting age estimation and its standard deviation. Polach and Golson took the lead and prepared a manual, which was published rapidly during 1966 by the AIAS. Although Golson was the junior author, he was the initiator, and *The Collection of Specimens for Radiocarbon Dating and Interpretation of Results* (1966), made a major contribution towards accuracy in the field and objectivity in publication, a concern already expressed by him in New Zealand (Golson 1955). This was a unique publication in 1966. An indication of the manual's standing is provided by the action of the commercial Geochron Laboratories Inc, Cambridge, Mass., USA. Complimentary copies were sent to their clients with the covering advice:

The analytical techniques employed in our laboratories differ in some respects from those described in this publication, but we feel the general information herein will be of great value to our clients.

While teaching in New Zealand, Jack Golson read and synthesised recent theoretical and methodological writings from both the Old
World and the New. His major 1959 contribution, 'Culture change in prehistoric New Zealand', is an individual and penetrating statement, but drawing upon two major recent books, Gordon Childe's *Piecing Together the Past* (1956) and Willey and Phillips', *Method and Theory in American Archaeology* (1958). In retrospect, Golson (1986:4) stated that his purpose was to clear the stage of inadequacies and confusions of cultural nomenclature which distorted interpretations of the evidence, and preached the need for a regional approach as opposed to long-distance correlation and wide generalisation.

Conflicting nomenclature and idiosyncratic interpretations were so rife in Australia, that one of the first conferences initiated by the AIAS took place in April 1963. This 'conference on nomenclature of implements and cultures' was attended by the handful of archaeologists then working in Australia. Jack Golson presented 'a model for the organization of archaeological data into a culturally and historically significant series of categories'. His paper, 'Space and time in Australian archaeology' was, sadly, never published. It drew heavily upon his New Zealand experience, and cited Childe and Willey and Phillips. It rates amongst the first systematic attempts to introduce theoretical constructs and model building into Australian prehistory. Its influence upon my own conceptual framework is evident in the original edition of my *Prehistory in Australia* (Mulvaney 1969:91, 105-11, 132), written during 1967-68.

My suggested terminological models were outmoded before my book was published. Influenced by my British training and Golson's Antipodean adaptations, I was unaware that the New Archaeology was clearing the slate. Ironically, David Clarke's *Analytical Archaeology* (1968) was published around the time of my *Prehistory*, while Lewis and Sally Binford's *New Perspectives in Archaeology* (1968) appeared a few months before it. Jack Golson (1986:7) has expressed succinctly the context and the weaknesses of our approach during the sixties:

The culture-organised, artefact-based frame of archaeological reference which John Mulvaney and I brought with us from Britain in the early 50s was not the best equipped to deal with the features of Australian and Pacific prehistory ... It is not that traditionalist archaeology was necessarily insensitive to issues of this kind. It is that they were seen as high-order questions requiring the existence of an adequate and properly ordered corpus of data before that could be judged archeologically valid, let alone addressed.

If Australian prehistory could not be moulded into Jack Golson's culture-historical framework, he found other ways of assisting its recognition outside departmental confines. In 1964 he was the prime mover in establishing the Canberra Archaeological Society, together with Helmut Loofs-Wissowa, of the Department of Asian Civilisation. That Society celebrated its twenty-fifth anniversary by granting them honorary membership. Those who were early members will treasure those informal long-weekend tours of coastal sites. On reflection, the archaeological fraternity of those times seemed more carefree and excited by the latest discovery. How else to explain those memorable camping visits to the current Ron Lampert coastal dig along with the families of Golson, Wright, Megaw and Mulvaney?

One common interest which Jack and I shared was a deep interest in cricket, even though we supported opposite sides in Test matches. During the sixties, cricket contests between our department and the Sydney University anthropologists became a celebrated event on the social calendar, with the victors bearing away in triumph for the year, the Jeremy Beckett Memorial Nut. Venues alternated between cities, and informal comradeship prevailed under Jack's genial captaincy. When I decided to write a book describing the 1868 Aboriginal cricketing tour of England, to commemorate its centenary, Jack encouraged the idea. As it nowhere concerned archaeology, I worried that it fell outside my terms of appointment. The prefatory acknowledgement of the 1967 edition of *Cricket Walkabout* is therefore very meaningful: 'It was a cricketing enthusiast from Rochdale, Mr Jack Golson ... who ensured that this book was written'.

Jack Golson's Australian triumph surely must be his preparations for the 1971 International Congress of Orientalists and the affiliated Far-Eastern Prehistory Association meeting in Canberra. It made Australian archaeology widely known and brought scholars together from many countries. Jack's contribution was threefold: to raise funds for visits by overseas scholars, to arrange meetings which transformed the Far-Eastern Prehistory Association into the more appropriately named Indo-Pacific Prehistory Association (of which he became the first secretary), and to organise a legendary post-congress coach tour of archaeological sites.

Late in 1970, Jack Golson succeeded in obtaining a grant of $3000 from the AIAS to assist in the costs of mounting a coach tour to archaeological sites across southeastern Australia. On
9 December 1970, he and Rhys Jones wrote in prophetic vein to intending congress participants, inviting them to ride an archaeological bandwagon:

It is unlikely that such a group of archaeological scholars will ever again meet in Australia in our lifetimes or at a time when archaeological discoveries in Australia are being made, with not only profound significance for the story of man in this continent but revolutionary implications for the study of early man in Southeast Asia as well. It seemed a pity in these circumstances that discussion of the exciting issues involved should be restricted to the lecture room and the coloured slide in Canberra, when the sites, collections and institutions involved might actually be seen. An archaeological tour to accomplish this was planned ... A brief itinerary is attached. Travel will be by Pioneer bus, with Ansett-ANA charter for the return flight Melbourne/Wynyard. This restricts participation to 45 individuals, of whom we expect 20-25 to be from overseas. These overseas visitors will get all travel and accommodation free.

The Golson omnibus tour ranged from northwestern Tasmania to Keilor, Kow Swamp, Mungo, Roonka, Devon Downs and Fromm's Landing, Panaramitee, Mootwingee, Cobar, Lapstone Creek and sites around Sydney and the New South Wales south coast. Apart from the archaeological riches visited, by air to Tasmania and on some 3000 road kilometres, there were such culinary delights as mutton birds and barbecues, while exotic drinking places included the Marrawah school in northwestern Tasmania and the Menindee pub, famed for its association with Burke and Wills. The exploits of their misguided exploring expedition was the subject of a coach microphone talk on the drive to Menindee, when the temperature outside rose above the century and the coach air-conditioning failed. When the coach also broke down on the scorching plain, the Golson and Jones expedition looked set to emulate Burke and Wills. Passengers also survived an enforced delay at the Blanchetown pub, when the coach became bogged at the Roonka site, and endured the rigours of spending a night 'sleeping' on the floor of the Marrawah school, or under the trees. Jack's tour belongs to the Dreaming of Australian archaeology.

I departed Jack's department upon the safe return of that fabled expedition, to establish the new teaching department in the Faculty of Arts. Consequently the nature and extent of our long and close association changed. It is more appropriate for others to trace subsequent years. We were both proud to reunite our partnership in 1980, however, when we recommended to the university that it award Honorary Doctorates of Science to two outstanding figures from the time before ourselves and the academic recognition of prehistory in this nation. On 18 April 1980 our university conferred that honour upon Frederick C. McCarthy and Norman B. Tindale. When the citation was published in Australian Archaeology (1980:96), the editor happily titled it 'Two remarkably parallel careers'. That designation seems even more appropriate for Mulvaney and Golson.

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Orientalist Tour, Devon Downs, South Australia. Date: January 1971. From memory, and with help from others, this photo shows:
Mulvaney


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Plate 3

PROFESSOR

Matthew Spriggs and Rhys Jones

Department of Prehistory, Research School of Pacific Studies, The Australian National University, Canberra, ACT 0200, Australia

In December 1991 Jack Golson retired from his post as foundation Professor of Prehistory in the Research School of Pacific Studies at The Australian National University (ANU). Having joined the University in 1961, Golson took the leading role in the development of prehistory over a period of thirty years. Appointed to the Chair in 1969, he had earlier founded a semi-independent prehistory section within the Department of Anthropology and Sociology, as described earlier in this volume by John Mulvaney.

In this period of organisation, Jack Golson was to forge interdisciplinary links within and outside the University—just as he had in New Zealand. Those early personal contacts, with Donald Walker, the late Joe Jennings, Harold Brookfield, Marie Reay, Paula Brown, John Calaby and Jim Bowler to name a few were to be lasting influences on his own work. They would also set the tone for the eclectic approaches to archaeology that have characterised the development of the Department.

There is a coherent pattern to Golson's research vision, employed over the entire terrain of the Australian continent, Papua New Guinea, the Islands of Melanesia and Polynesia, and the region of Southeast Asia fronting the Pacific world. In conveying it, we can do no better than to quote his own statement of the research mission of the Department as presented in the various Annual Reports of the Department which are held in the departmental library. The example given is from 1979:

The major themes of departmental research into the prehistory of Australia, New Guinea and the nearer Pacific Islands have been established now for some years. They have been chosen because they are seen as decisive for the personality of the region in prehistory and look to take advantage of the unique opportunities it offers for archaeological research. The region itself forms a distinctive part of a wider Indo-Pacific province, the nature of whose place in the early history of man is only now coming to be appreciated. Initial colonisation, effected over 40,000 years ago by sea from tropical regions north and west and made in conditions of climate, environment and geography different from the present, opened up the vast new diversified continent of Greater Australia—Australia plus Tasmania plus New Guinea—to human settlement. Subsequent colonists from the same regions brought domesticated plants and animals into New Guinea and in the more recent past participated in the great maritime expansion, based on highly efficient sea-going vessels and navigational techniques, that led to the settlement of the Pacific Islands and the organisation of wide-ranging systems of trade and economic exploitation between them.

Questions arising from such situations which are the subject of established projects by staff and scholars of the department include the human palaeontology and archaeology of Australia's earliest inhabitants; changes in the physical anthropological, archaeological and faunal record consequent on the one hand on changes in climate and environment from late Pleistocene into modern times, on the other on human adaptations to and alterations of environments previously unaffected by man; the effects on human populations of the 'islanding' by rising sea levels of territories previously parts of landmasses; the history of human populations on small, truly oceanic islands discovered and settled by sea-going societies bringing new plants and animals; the origins and development of plant-based subsistence systems in Melanesia; the history of specialised trading systems in the same region and their more precise definition by both accurate sourcing of the raw materials and technological study of the manufactured products involved in the processes of exchange.

More accurate and widely applicable chronologies to underpin the historical aspects of such work are sought by departmental research into obsidian dating and cooperative work with the radiocarbon dating laboratory. In addition because traditional ways of life of the indigenous inhabitants of our research area were alive everywhere until relatively recently and in some form in some areas are alive still, often in environments unaffected by Western man, there exists a great deal of non-archaeological data to extend and control the direction and conclusions of archaeological research. All our projects aim to make use of historical and ethnographic sources and, where possible, of direct observations in the field. Some projects have been specifically set up to record the behavioural patterns that determine the nature and distribution of archaeological evidence in and on the ground, as a basis for better controlled
Spriggs and Jones

archaeological investigations in those areas but also as contributions to the methodology of the discipline as a whole.

... we have been more active than hitherto in the developing field of contract archaeology. We see this as one way of maintaining the research impetus that has been built up in the department over previous years and of creating employment for trained archaeologists in a situation where staffing establishments in Universities are frozen or falling.

We have also become increasingly aware of the need to make our work, and that of Australian archaeologists in general, more widely known in areas beyond the professional circles that are our immediate audience. These include the Australian public at large, from whom ultimately our finances are derived, the Aboriginal community amongst whom some of our members work and with whose history and culture much of our activities are concerned, and the overseas countries where we work as non-nationals.

From his appointment to his retirement, the Department produced a total of 39 successfully completed Ph.D theses, and these are listed in an Appendix. At the time of his retirement there were a further 11 Ph.D. scholars pursuing their research. In addition, Rhys Jones and Alan Thorne as Departmental staff members wrote Ph.D. theses on Rocky Cape archaeology and human skeletal remains from Mungo and Kow Swamp respectively, the result of earlier field research at the University of Sydney.

Golson felt the need to establish a publishing vehicle for basic archaeological texts, especially those derived from Ph.D. theses of this and other departments within the Australian and Pacific field. Accordingly he organised resources from the Department to publish a series of monographs, the Terra Australis series, as well as Occasional Papers and conference volumes. Starting in 1971 a total of 13 volumes of Terra Australis were produced at the time of Golson's retirement. Other Departmental publications, lately regularised as the Occasional Papers in Prehistory series now number 20.

As Editor, Jack Golson had a certain notoriety. Nearly every thesis produced by Departmental scholars bore the tacit marks of his standards of syntax, etymology and cognition, spelling and punctuation. Protests by authors were minimal. The same applied – perhaps even more so – to Terra Australis volumes that he was asked to edit. He built a department characterised by egalitarianism among the small permanent staff and a moving population of students, visiting scholars and research fellows. The envy of some professors in the School, the bane of others who prized hierarchical structures,
the Prehistory Department could present a confusion of identity to the visitor: which was the student, which the fellow? The departmental seminars, with their free exchanges, offered little clarification; nor did their continuations at the ANU Staff Centre. In the office, he was never the distant professor disengaged from the research of junior colleagues.

Following in the tradition of the Research School of Pacific Studies, Golson as Professor developed his own major research programme. This was, of course, the Western Highlands project in Papua New Guinea that centred on the site at the Kuk Swamp of the Wahgi Valley. In the mid-1980s this research on the origin of agricultural systems was extended to the Eastern Highlands with the Yonki Project in the Arana Valley, conducted with colleagues from the University of Papua New Guinea (UPNG).

Golson took an active part in two of the major fieldwork projects carried out by the Department during the 1980s. He played an important role in the initial organisation of the Kadakou Project funded on a consultancy by the Australian National Parks and Wildlife Service and in a sense brought to a conclusion his own pioneering field research in the Alligator Rivers region carried out during the early 1960s. With the Lapita Homeland Project, Golson's part was the excavations at Lasigi in New Ireland Province. In his final years in the Department, he returned to New Caledonia, with John Chappell and Daniel Frimigacci, to attack again the still obdurate problem of the origins of the 'tumuli' or mounds of the Ile des Pins and adjacent areas.

Internationally, the Department's reputation was enhanced by Jack's dissemination of its research results in Europe and the United States of America. Gale Sieveking, a long-time friend and colleague, has this to say about 'The Golson Effect':

Some of his most influential papers -- as far as Europeans were concerned -- were those given at international consultancies: Beyond the Wallace Line, for example, though one can see its shortcomings today, had a revelatory effect when earlier circulated in samizdat form. They demonstrated that it was possible to change the value that was put on data, by looking at it from the Pacific or from Greater Australia in terms of the problems of a reception area where certain external influences, though expected, did not always arrive. This all seems rather old-fashioned today! But one must remember that in immediately post-Second World War days it appeared obvious that what was missing was simply archaeological research to fill in the blanks on the map. When increasingly early radiocarbon dates in Australia began to document the existence of microlithic industries of mesolithic date in terms of the European and Near Eastern chronology, this merely confirmed our suspicions that the local development sequence would follow the pattern established in Western Asia. Jack very wisely threw his net much wider than the Childe model, using any weapons that came to hand to demonstrate an absence of fit between the data and conventional thinking. His demonstration that the modern version of the three age system was unsuitable to the prehistory of the Pacific is an early example of this approach. Subsequent papers and international lectures and seminars picked up and expanded on the theme of regional cultural autonomy, later increasingly echoed by prehistorians in other regions.

Jack's open-mindedness and sustained originality of approach have been one of his most valuable assets. From the point of view of the world prehistorian, he was among the first to appreciate the relevance for archaeology of the new discipline of ethnobotany and to demonstrate its applicability both to the Australian data and to that of New Guinea.

The international archaeological community received the news of the Golson fieldwork at Kuk in much the same spirit. It is perhaps difficult for regional prehistorians to appreciate the impact made by the announcement of a complex horticultural and water control system in a tropical rainforest country antedating 9000 BP, earlier in fact than the earliest known examples of irrigation channels in the classic region of the Tigris and the Euphrates. Here was, for the first time, evidence that the claims of tropical Southeast Asia and Melanesia to be centres for the prehistoric domestication of root-crops, often dismissed by botanists and archaeologists alike, had to be taken seriously. From that day, New Guinea Prehistory entered the world community.

As Professor, Golson served Australian archaeology in many ways through three decades: serving on committees in many capacities, including leadership roles in the Australian Institute of Aboriginal Studies, the Australian Archaeological Association, Australian Academies Project on the Preservation of Rock Art and Canberra Archaeological Society.

He was elected to the Australian Academy of the Humanities in 1975, as a Fellow of the Society of Antiquaries of London (FSA) in 1987 and in 1991 was elected as President of the World Archaeological Congress at its meeting in Colombia.

In March 1992 Golson was awarded an honorary doctorate by the University of Papua New Guinea, a fitting recognition of his academic achievement. In closing we give part of the speech delivered by UPNG Vice Chancellor, Joseph Sukwianomb, at the presentation ceremony on March 6, as an appropriate summing-up of that achievement in relation to his New Guinea research.
Spriggs and Jones

Jack Golson is best known in Papua New Guinea for his work on the ancient agricultural systems in the Highlands. He was the key figure in the research in Kuk near Mt. Hagen in the Western Highlands. It was from his work that Papua New Guinea is now seen as an area where agriculture was developed independently. His work shows that by 9000 years ago people living in the Wahgi Valley were practising agriculture.

Being able to discover something is one thing. The next part of any discovery is to convince the sceptical world of fellow archaeologists and prehistorians about the significance. Dr Golson is no stranger to the international forum and over the years he has convinced the world of the antiquity of agriculture in Papua New Guinea.

As head of the Prehistory department at the Research School, Dr Golson has been able to help students doing research in Papua New Guinea not only in Archaeology but also in other related disciplines like Anthropology, Sociology, Geography, Geomorphology and other related sciences, in particular Biology and Quaternary research.

Indeed, if one were to look at the work that has been done in Papua New Guinea in terms of research in the study of the past, one will inevitably see the hands of Dr Jack Golson somewhere in there. He is tireless in the field and he has been the principal supervisor of everyone who has done archaeological research through the Department at the Australian National University.

ACKNOWLEDGEMENTS

We thank Gale Sieveking for his contribution to this appreciation of Jack Golson, allowing us to quote at length his perceptions of Golson’s international achievement. Douglas Yen added his editorial skills and further insights into this period of Jack’s career.

Plate 2  The ecology of retirement: Jack, with his thoughts, after a great innings. (Department of Prehistory Interdepartmental Cricket Archives)
Appendix

Ph.D. theses in the Department of Prehistory, Research School of Pacific Studies, Australian National University.

1965 Matthews, J.M. The Hoabinhian in South East Asia and elsewhere
1968 White, C. Plateau and plain: Prehistoric investigations in Arnhem Land, Northern Territory
1968 White, J.P. Taim Bilong Bipo: Investigations towards a prehistory of the Papua-New Guinea Highlands
1969 Allen, F.J. Archaeology, and the history of Port Essington (Northern Territory)
1969 Poulsen, J.I. A contribution to the prehistory of the Tongan islands
1970 Macknight, C.C. The Macassans – a study of the early trepang industry along the Northern Territory coast
1970 Specht, J.R. Prehistoric and modern pottery industries of Buka Island, PNG
1971 Lauer, P.K. Pottery traditions in the D'Entrecasteaux Islands of Papua
1972 Egloff, B.J. Collingwood Bay and the Trobriand Islands in recent prehistory: Settlement and interaction in coastal and island Papua
1973 Allen, H.R. Where the crow flies backwards: Man and land in the Darling Basin
1973 Glover, I.C. Excavations in Timor: A study of economic change and cultural continuity in prehistory
1973 Vanderwal, R.J. Prehistoric studies in central coastal Papua
1974 Crosby, E. A comparative study of Melanesian hafted edge-tools and other percussive cutting implements
1977 Irwin, G.J. The emergence of Mailu as a central place in the prehistory of coastal Papua
1978 Beaton, J.M. Dangerous harvest: Investigations in the late prehistoric occupation of upland south-east central Queensland
1979 Bowler, S.E. Hunter Hill, Hunter Island: Archaeological investigations of a prehistoric Tasmanian site
1979 Luebbers, R.A. Meals and menus: A study of change in prehistoric coastal settlements in South Australia
1980 Johnson, I.R. The getting of data: A case study from the recent industries of Australia
1980 Lamport, R.J. The great Karran mystery
1980 Rhoads, J.W. Through a glass darkly: Present and past land-use systems of Papuan sagopalm users
1980 Ward, G.K. Prehistoric settlement and economy in a tropical small island environment: The Banks Islands, insular Melanesia
1981 Spriggs, M.J.T. Vegetable kingdoms: Taro irrigation and Pacific prehistory
1982 Gollan, J.K. Prehistoric dingo
1983 Brown, P.J. Coobool Creek: A prehistoric Australian Hominid population
1984 Stockton, J.H. The prehistoric geography of Northwest Tasmania
1985 Burton, J.E. Wahgi Valley stone axe production and exchange, Papua New Guinea
1985 Cane, S.B. Archaeology and ethnography of arid zone landuse by Aborigines of the Great Sandy Desert, Western Australia
1985 Webb, S.G. Palaeopathology of prehistoric Australians
1986 Pardoe, C. Prehistoric human morphological variation in Australia
1987 Lilley, I.A. Prehistoric exchange in the Vitiaz Strait, Papua New Guinea
1990 Green, M.K. Prehistoric cranial variation in Papua New Guinea
1990 Matthews, P.J. The origins, dispersal and domestication of taro
1990 Spennemann, D.H.R. 'ata 'a Tonga mo 'ata o Tonga: Early and later prehistory of the Tongan Islands
1992 Thompson, G.B. Archaeobotanical investigations at Khok Phanom Di, central Thailand
1992 Witter, D.C. Regions and resources
1992 Mountain, M.J. Highland Papua New Guinea hunter-gatherers from the Pleistocene: Nombe rock shelter, Simbu

MA Graduates
1968 Coutts, P.J.F. The Archaeology of Wilson's Promontory
1968 Flood, J.M. Archaeology of Yarar shelter

31
It has been said that one of the worst things to happen to the archaeology of Australia, from the point of view of its long-term development, was Jack Golson's original decision in 1962-63, not to deal with Australia at all, but to concentrate on the rest of Australia, Melanesia and the Pacific. In a recent interview with Clive Gamble (Current Anthropology, in prep.), Jack's version of events suggests deliberate planning to this end from the moment he decided to leave New Zealand, particularly in anticipation of the imminent arrival of John Mulvaney to take up the Chair of Prehistory in Canberra, and the latter's acknowledged Aboriginal interests. According to Jack, the only reason that he ever worked in Australia at all was because he was unable to get a work permit for Papua New Guinea in 1966. More revealing, perhaps, is his statement that he felt 'unequipped to deal with the Australian [archaeological/hunter-gatherer] situation'. Whatever the reasons, his work with Australian Aboriginals, and his investigation of Australian prehistory, has been limited.

There is a double irony in this situation, especially striking in view of his current international role (see below). The first irony is that Jack Golson must have been, at the time of his arrival in Australia, the only practical, yet academic, archaeologist in Australia with direct experience of what came to be known as the 'reburial issue'. His earlier excavations in New Zealand had uncovered human burials, and had consequently come up against Maori opposition. His reaction to this conflict was one of disappointment but, unlike most archaeologists at that time, he was also sympathetic: 'I attempted to talk to them, I attempted to talk to their parent communities...'. What a difference it would have made if other Australian archaeologists had followed this example, set in 1963; instead of lagging some ten years behind, as they did. In answer to further questions about his attitude to Maori involvement in New Zealand archaeology, Jack stresses that, faced with the hitherto unenvisaged situation in which Maori views decided the fate of his excavation, his reactions were spontaneous: 'to him, 'There was no question about it ... you try to involve...'; if you fail, the archaeological enquiry fails.

Formally, the Jack Golson of Australia had little to do with Aboriginal affairs – the closest that he had ever been associated with the (then) Australian Institute of Aboriginal Studies (AIAS) was to have arrived in the country just a few months after its creation, and to have been unsuccessfully proposed as a potential Governor General's nominee to its Council. In 1975, however, he was elected to the small committee that was formed to examine the nature, scope, function, academic jurisdiction, role, composition and membership of the AIAS.

Once he had accepted this task, Jack proceeded in the only way that he knows how to do anything: with genuine curiosity, enthusiasm, dedication and a commitment to even-handed handling of all issues. Only Jack Golson can transform what others would inevitably make disagreeable, frustrating and irksome into a positive, enjoyable and constructive 'learning experience': thus, for example, the potentially deadening sessions in which Jack would demand to have the AIAS Act and Statutes explained to him became, in the event, fascinating explorations into the background history and socio-cultural contexts of these documents.

It was in this context, witnessing Jack's interviews (or, more often, conversations) with the very wide variety of Europeans and Aboriginals involved in AIAS affairs, that I saw for myself that quality which others - from all over the world – had frequently mentioned to me. It is not easy to express this quality in words, since it is a mixture of different elements: part deriving from curiosity about everything (except, perhaps, the personal) – from dissertation topic to brand of beer to cricket score to deserted Medieval village – part deriving from his view of himself still as a young enthusiast and still in the business of acquiring knowledge, and part deriving from
a kind of wonderment about his fellow human beings.

Of course, over the years, Jack Golson has become a master of the committee, the enquiry and the review. His mastery is mainly based on three of his characteristics: meticulous preparation from briefing papers, enthusiasm for the matters under discussion and courteous, equal treatment of all his fellow committee/review/enquiry members.

Jack is also a true member of the international community, equally 'at home' in many parts of the world, and in any sort of conditions: Australia, New Zealand, India, the United Kingdom, Papua New Guinea, Scandinavia and France, for example, each have their own particular fascination for Jack. It is easy to imagine that, other things being equal, he could make his base in any of these places, happily concentrating on their specialities, curiosities and their differing cultural 'norms'. Any lack of fluency (as it is normally understood) in a foreign language appears not to be a problem; Jack Golson's communications are self-evidently successfully achieved through personality.

In 1990, three years after its inception, the World Archaeological Congress (WAC) elected Jack as President. WAC is devoted to the equal participation in its affairs of all those with a genuine interest in the past: its worldwide membership ranges from the most illustrious professor of archaeology to the undergraduate student to those with no formal education. In 1990 WAC needed to elect a visionary such as Jack for its first four-year Presidency under its new constitution – albeit a visionary with an international reputation as an outstanding archaeologist, albeit a visionary whose world was the proverbial oyster and albeit a visionary with a sympathetic understanding for such indigenous concerns as the 'reburial issue'. Here is the second irony in the present situation: Jack's understanding of the latter issue derived not from Australia, currently claimed to be the one of the two main springs (with the Americas) of such issues, but from New Zealand.

I can best end this short piece by paraphrasing my impromptu statement to the WAC Council on the occasion of Jack Golson's successful nomination to the WAC Presidency:

WAC has become a reality which no one can succeed in destroying – its accomplishments are too evident for its opponents to be able to overcome it: more than 20 One World Archaeology books already published; two successful World Archaeological Congresses, a third one offered for 1994 in India, a WAC-sponsored Workshop in 1992 in Puerto Rico, an Inter-Congress in Kenya in 1993 – and, already, offers are coming in to host WAC-4 in 1998... But even if I had not been convinced that WAC's future was, in any case, unstoppable, I would have been nominating the same person to be the (next) WAC President. WAC depends on its fearlessness to confront archaeological issues of whatever kind, and at whatever moment of international affairs. What is needed by those who believe in WAC is a President of deserved archaeological eminence and respect – of course – but also someone who is, by his or her own constitution, irrevocably sworn to a non-judgemental comparative approach to all human social activity. In addition, preferably, that person should have the freedom – and desire – to serve WAC's large aims. As far as I know there is only one person to fit this job advertisement and he sits – however unaware – in the room with us today. Professor Golson is an internationally recognised archaeological figure, with personal contacts throughout the world. He is also one of the few outstanding senior archaeological individuals of the world who has the trust of all his colleagues, who recognise and appreciate his truly ethical nature – and beyond all else WAC must feel itself to be ethically concerned, and must be seen to be involved in the wider, ethical aspects of archaeological endeavour. If we need someone who combines exceptional academic and organisational talents with inherently 'a-racial' attitudes, then Professor Jack Golson is WAC's person to be its President.
A DAWNING REALIZATION

Wake up Jack! You've got to get up to give a lecture.

My God! It's my birthday. I'm 35 and I haven't published anything. I'm a failure!

Wait a minute!

It's all right. I'm only 34.
THE GOLSON BIBLIOGRAPHY
FROM 1953


PART II

Festal Writings
Because the Sahul shelf is so broad, it has seemed clear to many writers that earliest human migration to Australia or New Guinea occurred when sea level was much lower than at present. Archaeologic results make it equally clear that humans did not wait until the most recent episode of very low sea level, about 18 ky ago (ky = 1000 years), before wittingly or unwittingly making the journey. Birdsell (1977) showed that some of the passages would have been out of sight of land, even at lowest Pleistocene sea levels, which raises interesting questions about methods of the early migrations. Results from the Solomons (Wickler and Spriggs 1988) demonstrate that overseas migration well out of sight of land occurred at least 28 ky ago, very close in time to earliest sites in New Ireland (Allen et al. 1988). The coincidence might imply the first appearance of a maritime culture which, to extend the fancy further, may have been a component of a newly emergent Austral culture with other novel elements which appear at about the same time, such as the cremation burials found in southeast Australia (Bowler et al. 1972), and art found elsewhere (Jones 1989). However, because sites are few and dating of late Pleistocene deposits is sometimes problematic, these innovations may not be as coeval as they appear, and it may be that their seeds had existed since the time, considerably earlier, when humans first arrived.

The recent demonstration by Roberts et al. (1990) that humans were in Australia about 55 ky ago breaks the devilish radiocarbon 'event horizon', which was a great obstacle to the estimation of human antiquity in our region (cf. Allen 1990), and also presents a conundrum for those who hold that first arrivals coincide with low sea levels. Moving backwards in time from the Last Glacial low, most sea level curves show an undulating but progressive rise towards the Last Interglacial, when the sea stood at about its present position. Hence, roughly speaking, the greater the antiquity of humans in the region, the longer and thus more difficult would have been their first passages. This difficulty would vanish if first crossings occurred at the penultimate glacial maximum about 140 ky ago, when the sea was again very low, but such an early date raises other, apparently greater, problems. No Austral archaeologic sites remotely approaching this age have been found, and, as shown in the review by Jones (1989), traces of modern man of this antiquity have not yet been found in nearby Sundaland. Pre-modern ('Solo') hominids were in Java at about that time but it is not believed that these were the primary founders of the Austral peoples (Thorne and Wolpert 1981; Groves 1989; see also Jones 1989). The idea of humans in Australia at 140 ky jars against the accumulated evidence – palaeontologic, genetic, and chronologic – for the descent of the modern species, reviewed by Stringer (1990). On the other hand, there is the well-known suggestion by G. Singh (Singh and Geissler 1985) that man-made fires have affected the vegetation at Lake George (southeast Australia) since the Last Interglacial, and the possibility of very early human arrival should be entertained, at least until an alternative explanation or a different chronology is proven for the Lake George pollen and sedimentary charcoal record.

Before the radiocarbon barrier was broken, writers, such as Birdsell (1977) and Flood (1983), examined the upper Quaternary record for very low sea levels somewhat earlier than 40 ky BP, to identify a likely moment for first arrivals. The sea level curves (e.g. Chappell and Thom 1977) used by these authors have since been revised (Chappell and Shackleton 1986), and the putative very low level around 50 to 55 ky has disappeared because it is not substantiated by closer examination of field evidence. I apologise for the confusion caused! However, relatively low occurrences remain around 70, 90, and 110 ky. This short essay attempts to set the record straight and also reconsiders Birdsell's (1977) scenarios of the geographic fluctuations – both regional and coastal – which must have affected humans as they made their way into the region.
Austral region. If justification is needed, it now seems clear that at no time between the Last Interglacial, at 120-130 ky, and occupation at Malakunanja II at 55 ky, was sea level sufficiently low for Australia to be reached in one relatively short hop from Timor.

The region, its continental shelves, and pathways suggested by Birdsell (1977) is shown in Figure 1, as an aid to discussion.

**LATE QUATERNARY SEA LEVELS: THE RECORD**

Until rather recently, those who required information about past sea levels to assist their palaeoenvironmental studies, usually sought for a 'standard curve' issued by a specialist in vogue at the time. The situation has changed for sea levels of the last 6000 years, as it is understood that there is no universally portable curve, covering the relatively small changes of this period. For late Holocene studies, the best advice is to discover the local sea level curve from local evidence — or at least to find some evidence confirming a curve which has been predicted for the site by a specialist. For the Pleistocene the situation is as it has ever been; the individual usually accepts a fashionable curve, sometimes supported by advice from its author to the effect that other, different, curves are not trustworthy. It is with due humility that I note that the upper Quaternary sea level curve, used more widely than any other in Australia in the last 15 years, derives largely from the work of myself and colleagues at Huon Peninsula, Papua New Guinea (Chappell 1974; Bloom et al. 1974).

Through detailed work at a section known as Tewai Gorge, the Huon Peninsula curve was revised in 1983 and was further adjusted by Chappell and Shackleton (1986), who tabulated ages and sea level values for all turning points on the curve for the last 150 ky. A quite independent sea level record has been extracted from oxygen isotope data from deep-sea cores by Shackleton (1987). Figure 2a shows these two curves. More should be said about Shackleton's curve, because for over 25 years oxygen isotope series have been widely used as a guide to Quaternary sea level changes although they actually register effects of temperature as well. Relative contributions of the two factors were reassessed several times (Shackleton and Opdyke 1973), before the curve in Figure 2a was derived by composite analysis of both planktic and benthic isotope records. The interested reader will find that this resembles many of the recent deep sea oxygen isotope series except that the heights of full interglacial peaks are reduced, relative to interstadial peaks, reflecting changes in deep ocean temperatures identified by Chappell and Shackleton (1986).

The two curves in Figure 2a agree rather closely, except that the isotope based curve gives somewhat lower levels than the Huon Peninsula...
version between 30 and 75 ky. These differences (which may be reduced after isostatic and other small geophysical corrections are completed) will not greatly affect our discussion, as both curves yield similar moving pictures of Late Quaternary paleogeography. For those who seek low sea levels as potential times of human passage to Australia and New Guinea, earlier than the 55 ky date of Roberts et al's (1990) Malakunanja II site, there are candidates at about 70 ky (isotopic sea level around -75 m, Huon Peninsula level about -68 m), 90 ky (about -45 to -55 m), and 110 ky (about -60 m). During the first and last of these, coastlines of the northern and northwest Australian shelves and of the Aru shelf would have been closer to the islands of Wallacea than at any other time between 130 and 30 ky. These low points lie between relatively long episodes when sea level was within 30 to 40 m of present, during which the probability of chance migration to Australia or the island of New Guinea would have been lowest, according to conventional expectations.

LATE QUATERNARY SEA LEVELS: THEIR EFFECTS

Birdsell (1977) identified several stepping stone routes through the island studded seas of eastern Indonesia (Fig. 1), and carefully tabulated distances and the dimensions of landfall targets along each, for a maximum sea level lowering of 150 m. He also examined interisland distances with sea level about 50 m below present, finding that most passages are little affected by variations between this level and -150 m, because they involve islands without submerged shelves or aprons. However, there are three corridors whose widths are highly affected, viz., Kalimantan to Sulawesi, Tanimbar to the Aru shelf, and Timor to the Sahul shelf. Examination of recent editions of the charts (Admiralty Charts 1990) confirms Birdsell's results, which are virtually unchanged if we take -130 m as the lowest level (Fig. 2a), rather than -150 m. It is also apparent that in many cases the interisland distances with sea level at -20 m are not much greater than at the -50 m level, with the same main exceptions as before. Birdsell (1977) noted that a number of additional straits, relatively small, appear if sea level is at -50 rather than -150 m, and that sizes and visible distances of islands are reduced. Although both effects are more pronounced at -20 m, the geography of interisland distances is relatively constant – with exceptions discussed in a moment – for levels between -20 and -75 m, which covers the period between 60 and 115 ky.

People arrived in the Austral region when none of the three broad and sea level-sensitive passages – Kalimantan to Sulawesi, Tanimbar to Aru shelf, and Timor to Sahul shelf – were at anything like their minimum distances. Between 60 and 115 ky, Kalimantan to Sulawesi would have varied between about 75 and 120 km, and Tanimbar to Aru shelf from about 130 to 200 km (with a wide sea appearing between Aru and New Guinea at levels higher than -40 m). Passage from Timor to Australia is affected most severely. Although distances to landfalls on the northwest Sahul margin, such as Karmt Shoal, are not much affected until they submerge, new detailed bathymetric maps of the Sahul shelf (National Mapping 1980-) show that stepping stones across most of this very broad shelf are small and sparse at -75 m and virtually vanish when sea level is within about 30 m of present. Taken overall, a southern Sunda migration route – which looks like a royal road as far as Timor – is the most problematic at all lowerings less than -75 m, while in terms of distances and targets the northern corridors through Sulawesi to the island of New Guinea (Fig. 1) were most favourable throughout the period of interest.

Because interisland distances along the most favourable routes are rather insensitive to sea levels in the relevant range, other factors which may have influenced early passages are considered. Some coastal habitats are more conducive to the regular use of simple watercraft than others, because they positively invite local travel on rafts, for instance, as an aid to food gathering. Birdsell (1977), Thorne (1980) and Jones (1989) suggest that some such craft were implicated in first human voyages to the Austral region, as...
natural drift-voyaging had had negligible impact on zoogeographic differences across Wallacea over millions of years. Although inadmissible as evidence, one notes that simple rafts as well as the ubiquitous canoes are used around New Guinea today, in mangrove estuaries and coral lagoons where people cross and recross stretches of water to gather shellfish and other foods. Cliffted rocky coasts and turbulent freshwater river mouths, on the other hand, are exploited directly from the land, when weather and tides are favourable. Such differences exist today between the people of the Siassi reefs and islands, on the northern side of Vitiaz Strait, and those of the cliffted and rocky coast of Huon Peninsula, on the southern side. It seems likely that watercraft, if they existed at all in the Pleistocene as tools of coastal subsistence, would have coincided with these geomorphologic distinctions.

Tropical coasts and coral reefs of rising versus falling sea level are very different. Contemporary coasts register effects of the large Postglacial sea level rise, which have been widely studied geomorphically and stratigraphically. Effects of falling sea level are less accessible as they are buried by younger transgressive deposits or are preserved some distance offshore. Exceptions are provided by areas where there is rapid tectonic uplift (such as the Huon Peninsula) because, during the last 6000 years when sea level itself has been approximately stable, emergence of the land simulates a falling sea. With reference to a few examples, the following generalisations can be made. Drilling studies in the Great Barrier Reef (Hopley 1982; Davies and Hopley 1983), and outcrop examinations of raised reefs at Huon Peninsula (Chappell 1974), show that rising sea level favours coral reef development, sometimes at rates equalling even the most rapid sea level rise (Chappell and Polach 1991), which leads to enlargement of lagoons and diversification of reef environments. With falling sea, corals contract to a narrow fringe on the steep margin of the former forereef, and lagoons become emergent land.

Through several research projects in northern Australia, the effects of sea level upon sedimentary tropical coasts are better known now, than when Chappell and Thom (1977) discussed some aspects of the matter. Streams, which under rising seas develop estuaries with meandering mangroves and waterways, often with fresh backwater swamps at their floodplain margin, become entrenched when the sea is falling and their littoral swamps contract dramatically. Coastal lake and wetland systems which develop during and after a sea level rise, such as the present Murik system adjacent to the Sepik rivermouth, will convert to freshwater swamps and then dry out as sea level falls. Studies in northern Australian tidal rivers (e.g. Woodroffe et al. 1986) show that estuarine mangrove systems can achieve their greatest extent at or shortly before the climax of sea level rise and afterwards are overtaken by freshwater floodplains if sea level then remains stable for a few thousand years. These differences in river-fed coastal environments depend very much on the fate of sediments under rising versus falling seas. When sea level rises, sediment is trapped within or even moves landward into the system, contributing to expansion of swamps seeded with waterways, while it tends to bypass the estuarine system when falling levels cause river entrenchment.

There will be exceptions to the above generalisations, in big tide dominated deltas with a very large fluvial sediment supply such as the Fly in Papua New Guinea, for example. However, the coastal types outlined above are likely to have been widespread, and to have alternated as Pleistocene sea level rose and fell. It is on this basis that Figure 2b is drawn in phase with Figure 2a, sketching a sequence of 'aquatic' coastlands alternating with 'rocky' coasts. We note that 'aquatic' coasts are likely to have reached their maximum extensions at or near the end of each sea level rise; coral reefs, which may lag behind sea level at its highest rates of rise, catch up and lagoons achieve their maximum size and diversity when or soon after the level stabilises. Similarly, some estuarine mangrove systems achieved their maxima when Holocene sea level stabilised (Woodroffe et al. 1985; 1986), while others prograded or continued expanding for several thousand years afterwards (Grindrod and Rhodes 1984; Chappell and Grindrod 1984). Hence, environmental switching in Figure 2b lags behind the sea level curve, and each 'aquatic' phase can be thought of as progressing from incipient through waxing to maximal conditions. It is noted that the 'aquatic coastal' phases in Figure 2b correspond with proven periods of reef building at Huon Peninsula and other tectonically rising islands; it is reasonable to infer that similar processes occurred at tectonically stable coral coasts.

It is a small step to suggest that the occurrence of early watercraft would have fluctuated in sympathy with Figure 2b. With watercraft as a precondition for oversea travel, whether by accident or design, we are led to suggest that first passages to the Austral region may not have been at times when sea level was low, but when it was rising or even during relative maxima. Certainly,
this seems most likely for those crossings where distance is little affected by sea level.

Two other factors favour this hypothesis, viz. correlated variations of weather, and island sizes. Insofar as tropical wind systems may have been affected by the majestic Pleistocene fluctuations of sea level and ice ages – and strength or duration of the northwest monsoon, which assists southward and eastward passages in Figure 1, may have diminished when large areas of the Sunda shelf were exposed – the patterns are likely to have been most similar to present during the phases of higher sea level. Further, if migration involved staging at islands which became smaller or even vanished whenever sea level rose (and there are several of these on or near Birdsell’s northern routes), the potential for population displacement which Jones (1977) identified as having acted during the Holocene in Bass Strait comes into play – but is absent under lowest sea level scenarios.

**DISCUSSION**

A hypothesis has little value unless it can be tested, and this is difficult in the present case. The hypothetical association of rising seas, aquatic coastal environments (in some instances with drowning islands), and watercraft is suggested to favour migration at particular times. Within the period of interest, Figure 2a shows that such culminations occurred about 60, 80, and 100 ky ago. If prehistoric chronology of first appearances in eastern Wallacea, New Guinea, and Australia could be sufficiently resolved, one might find in favour of this model. Success with this test is unlikely, not only because at least some of the evidence will lie below present sea level, but more importantly because each of these high sea levels follows less than 10 ky after a low level. Confident differentiation between 80 and 90 ky, say, is unlikely to be achieved with present dating methods (except in the very unlikely case of a site sandwiched between coral formations, which could be dated within ±1 ky by U-series mass spectrometry). Although not directly relevant to the ‘first arrivals’ problem, our model could be examined with more recent archaeologic age data. It may not be accidental that first appearances in New Ireland and the Solomons closely follow a sea level rise which was relatively small but sufficient to have promoted growth of a well formed reef at Huon Peninsula (Reef Complex II, Chappell 1974). Moving forward to Post-glacial time, rising seas and expanding aquatic systems may partially have promoted development of maritime technology which several thousand years later enabled migration into the Pacific. However, even if this was demonstrated by future research it would have no bearing on the first migrations problem, interesting though it might be otherwise.

Chronology provides another insight. If passage through Wallacea occurred after the Last Interglacial high sea level and any time up to occupation at Malakunanja II, then all island-hops were accomplished in less than 55 ky. We recall that this was far from a single step journey, as sea level was never below -70 m and was mostly within -30 m of present. The next 40 to 50 ky saw man apparently move no further onwards than the Solomons – a not inconsiderable step but little greater than those taken earlier through Wallacea – until the great Pacific expansion in Upper Holocene times. Probability would seem to favour the rather widespread existence of a littoral aquatic economy, energised by the effects of rising sea level. This would be supported if future research shows that humans appeared simultaneously in Wallacea and the Austral region – at least within a 10 ky interval which embraces dating uncertainties. Accordingly, favoured times would be at or shortly before the high sea levels which occurred about 60, 80, and 100 ky ago, or even around the Last Interglacial peak at 120 to 130 ky. Present data favour a 60 ky phase. Migration to the Austral region during any one of these episodes implies that humans developed watercraft, and were set for their first great maritime adventures, surprisingly early.

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Chappell


ARE YOUR FINGERPRINTS DESTINED TO BECOME PART OF PREHISTORY?

Barry Fankhauser

Department of Prehistory, Research School of Pacific Studies, The Australian National University, Canberra, ACT 0200, Australia

In 1988 a residue analysis laboratory was set up in the Department of Prehistory, Research School of Pacific Studies. A high performance liquid chromatograph was installed and a gas chromatograph was borrowed from the Department of Chemistry, The Faculties. At the time these were dedicated to amino acid analyses and fatty acid analyses, respectively. Having sophisticated equipment is one thing, but being able to get something meaningful out of it is another matter. The techniques involved in residue analysis must be extremely sensitive and reliable.

Proteins and fats have been shown to be stable over long periods of time often in archaeological contexts (Derbyshire et al. 1977; Rottlander and Schlichtherle 1978; Hill et al. 1985; Nelson et al. 1986). Residue analysis can (or has the potential to) provide information on stone tool and pottery functions as well as on paleodiet.

Blood residues have been detected on stone tools using microscopy and chemical techniques (Loy 1983; Newman and Julig 1989; Hyland et al. 1990). Microscopy has mainly been used to detect other organic residues on stone tools (Shafer and Holloway 1979; Anderson 1980; Hall et al. 1989). Other than blood, very few chemical analyses have been applied to residues on stone tools and most of these have been simple chemical tests with varying degrees of success (see, for example, Briuer 1976). Chromatographic methods have been applied to food residues on pottery (Passi et al. 1981; Patrick et al. 1985; Needham and Evans 1987; Hurst et al. 1989; Deal 1990) and similar techniques should be applicable to residues on stone tools.

I looked for a first 'project' which would involve the development of the required micro-methods. Also, all of the equipment had to be made to work. Certainly, if one can reliably analyse fingerprints then the methods are alive and well. And looking for residues you already know is a good way to start. So, I decided to look at the contamination produced by fingerprints on and along with known residues on obsidian. Clearly, this would answer several questions: can fingerprints be detected and do they interfere with residues on stone tools? Are the methods sensitive? Can the procedures be repeated? Are the extraction methods effective and repeatable? Can all of the amino acids and fatty acids (esters) be separated and quantified? Can minute amounts of residues be detected?

The methods developed to answer the above questions are given in detail below, but the many experiments required to develop them are not. The methods as given were found to be reliable and gave repeatable results. Indeed, minute amounts of residues from stone tools can be detected and quantified. These residues may include fingerprints of those who handle artefacts carelessly.

MATERIALS AND METHODS

Reagents

Ultra pure water (MQW) was obtained from distilled water through a Milli Q purification system. All solvents were HPLC grade and chemicals of analytical reagent grade. Nitrogen was high purity grade.

Obsidian preparation

Flakes of obsidian approximately 2 cm² were obtained from a large obsidian core. From this point flakes were handled only with rubber gloves or tweezers. Flakes were washed with detergent and water, rinsed with water and MQW and then heated overnight in a muffle furnace at 500°C.

Deposition of residues on Obsidian

Except where noted, flakes were handled only with tweezers or while wearing plastic gloves. A minimum of six pieces was used to slice through each of taro and red sweet potato (yam type). The tuber residues were left to dry on the
For each analysis (amino and fatty acid) the samples consisted of:

1. Three pieces of obsidian containing only tuber residue;
2. Three pieces containing tuber residue plus fingerprints;
3. Three pieces containing only fingerprints.

**Lipid analysis**

Useful guides to lipids and lipid analysis include books by Gurr and James (1980), Christie (1982), Gunstone et al. (1986) and Kates (1986).

**Extraction procedure**

1. Put pieces of obsidian separately into 50 mL beakers.
2. Dispense 2.0 mL of chloroform, methanol and MQW (1:2:0.8) into each beaker.
3. Use the ultra-sonic probe (Sonifier Cell Disrupter B-30, Branson Sonic Power Company) on each sample for 1 min. per side. Add 0.5 mL of chloroform and homogenise for 1 min. Add 0.5 mL of MQW and homogenise for 1 min.
4. Pour extract into small test tubes.
5. Centrifuge at 3000 rpm for 5 mins.
6. Pipet chloroform layer (bottom layer) into Reacti-Vials. Note: Reacti-Vials, Reacti-Vap and Reacti-Therm are products of Pierce Chemical Company.

7. Evaporate chloroform to dryness at room temperature using high-purity nitrogen and Reacti-Vap with needles just above the surface when evaporating.

**Transesterification procedure**

1. To the dried samples in Reacti-Vials add 100 µL of BF₃ in methanol (20% w/v).
2. Screw on caps containing septa and heat at 100°C for 5 mins on Reacti-Therm. Cool to room temperature.
3. Add 200 µL of hexane and 100 µL MQW. Replace caps.
4. Mix by using a vortex mixer and centrifuge to separate layers.
5. Remove upper hexane phase carefully with Pasteur pipettes and transfer to sample vials.
6. Repeat steps 3-5.
7. Completely evaporate hexane layer at 40°C using N₂ on the Reacti-Vap.
8. Dilute the sample with 10 µL of chloroform and analyse by gas chromatography.

**Gas chromatography analysis**

**Instrumentation**

Gas chromatographic analysis was done using a Perkin-Elmer Sigma 2000 Gas Chromatograph equipped with a flame ionisation detector. Peak areas were determined with a Hewlett-Packard 3390A integrator-recorder. Separations were done on an SGE 12 m bonded phase (BP20 polyethylene glycol) fused silica column with an inside diameter of 0.22 mm and a film thickness of 0.25 microns; helium was the carrier gas. Temperature program used was 100°C for 1 min., 20°C/min to 150°C, 10°C/min to 180°C and hold for 1 min., 10°C/min to 240°C and hold for 2 min. Injection volumes were 0.5-2 µL. A minimum of two injections was done for each sample and standard.

**Standards**

The following fatty acid standards were available (all from Sigma): 12:0, 14:0, 14:1, 15:0, 16:0, 17:0, 18:0, 18:1, 18:2, 19:0. Standards were dried in a vacuum oven at 35°C, weighed and made up in dichloromethane to a concentration of 0.5 nmol/µL. Methyl esters were prepared by dispensing 20 µL of fatty acid standard directly into Reacti-Vials (triplicate) and evaporating to dryness using high purity N₂ at 40°C. Fatty acids were esterified along with samples using the same procedure (see Transesterification procedure above).

**Calculation of results**

The esterified standard fatty acid ratios from peak areas were corrected to actual mole ratios by considering mass and purity. Fatty acid mole ratios for samples were calculated from the peak areas of the methyl esters. These ratios were corrected with a correction factor from actual mole ratios of standards to give absolute ratios so the sample mole ratios were not dependent on instrument conditions. This results in mole ratios being comparable regardless of gas chromatographs or columns used.

**Amino-acid analysis**

**Extraction of amino acids from fingertip**

Amino acids were obtained by rinsing a finger in 2 mL of MQW in a beaker. The extract was evaporated to dryness in a Reacti-Vial at 80°C using N₂ on a Reacti-Vap. This extract was then hydrolysed according to the procedure below.

**Extraction procedure for protein and amino acids**

1. Put pieces of obsidian separately into 50 mL beakers. Dispense 2 mL of MQW:methanol (4:1) into each beaker.
2. Use the ultrasonic probe on each sample for 1 min., turn the obsidian flake over with tweezers and homogenise for an additional
minute. Remove obsidian flakes with tweezers.

3. Pour extract into Reacti-Vials and evaporate to dryness at 80°C using N₂ on a Reacti-Vap.

**Hydrolysis procedure**

1. Add 200 µL of 6 M constant boiling HCl (doubly distilled) to each sample in a Reacti-Vial. Screw on caps hard with septa.
2. Put vials into a beaker and add water to above sample level. Put beaker with samples into an ultra-sonic bath for 3 mins.
3. Cool samples in freezer for 10-15 mins.
4. Evacuate vials using hypodermic needles attached to vacuum system. Introduce dry N₂ into the vials.
5. Repeat step 4 and re-evacuate.
6. Incubate at 145°C for 4 hours on a Reacti-Therm.
7. Evaporate the acid to dryness at 60°C with N₂ flowing over samples from a Reacti-Vap.
8. Add 200 µL of 0.1 N HCl to each vial and dissolve samples by using a vortex mixer at high speed for a minimum of 20 secs.
9. Centrifuge for 5 mins.
10. Transfer supernatant into HPLC vials using a 100 µL micropipet. Store samples at 0°C until analysis.
11. Analyse the samples by HPLC.

**High performance liquid chromatography analysis**

**Instrumentation**

Amino acid analysis was done using a Hewlett-Packard Model 1090L Liquid Chromatograph with a DR5 solvent delivery system, variable-volume autoinjector and ChemStation (Schuster 1988). A diode-array detector (DAD); measurement wavelength – 338 nm, reference – 390 nm and an HP 1046A fluorescence detector at excitation and emission wavelengths of 230 and 455 nm were used for detection. A HP Hypersil ODS column (5µm, 100 x 2.1 mm) with a SGE guard column (10GLC4-ODS 2) were used for separation of amino acids.

Column temperature was constant at 37°C. Flow rate was 0.35 mL/min.

**Derivatisation procedure**

The derivatisation was automated by means of an injector program shown in Table 1. In the injection sequence, specified volumes of sample and reagents were drawn (step = draw) from different vials. Mixing (step = mix) of sample and reagents was done inside the injection capillary.

<table>
<thead>
<tr>
<th>Step</th>
<th>Function</th>
<th>Amount (µL)</th>
<th>Details</th>
<th>Substance</th>
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<tr>
<td>1</td>
<td>Draw</td>
<td>5.0</td>
<td>From vial 3</td>
<td>Borate buffer</td>
</tr>
<tr>
<td>2</td>
<td>Draw</td>
<td>0.0</td>
<td>From vial 0</td>
<td>Water for needle wash OPA</td>
</tr>
<tr>
<td>3</td>
<td>Draw</td>
<td>2.0</td>
<td>From vial 1</td>
<td>-</td>
</tr>
<tr>
<td>4</td>
<td>Draw</td>
<td>0.0</td>
<td>From vial 0</td>
<td>Water for needle wash OPA</td>
</tr>
<tr>
<td>5</td>
<td>Mix</td>
<td>7.0</td>
<td>Three cycles</td>
<td>-</td>
</tr>
<tr>
<td>6</td>
<td>Draw</td>
<td>2.0</td>
<td>From sample</td>
<td>Amino acids</td>
</tr>
<tr>
<td>7</td>
<td>Draw</td>
<td>0.0</td>
<td>From vial 0</td>
<td>Water for needle wash Derivatisation</td>
</tr>
<tr>
<td>8</td>
<td>Mix</td>
<td>9.0</td>
<td>Six cycles</td>
<td>-</td>
</tr>
<tr>
<td>9</td>
<td>Inject</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

Table 1  Injection program for derivatisation of primary amino acids with OPA-2ME.

**Preparation of O-phthaldialdehyde/2-mercaptoethanol (OPA/2-ME) derivatising solution**

For DAD detection, 50 mg of OPA was dissolved in 1.5 mL of methanol. 2-ME (50 µL), 2 N boric acid (H₃BO₃, 4 mL) and Brij-35 (150 µL, Atlas Chemical Company supplied by Pierce) were then added and the solution mixed (Roth 1971; Jones et al. 1981; Jones and Gilligan 1983). This solution was filtered using 0.22 µm GV Millipore filter and stored at 0°C. Every two days 10 µL of 2-ME was added. Aliquots of this solution were withdrawn, put in HPLC vials, and stored overnight at room temperature before use. For fluorescence detection the concentration of the reagents was 40% of the above formulation.

**Preparation of amino acid standards**

Amino acid standard solutions were purchased from Sigma: A-9531 (2.5 µmoles/mL) and A-2161 (25.0 nmolcs/mL) both in 0.1 N HCL. For DAD detection, amino acid standard solutions were diluted with 0.1 N HCl to the following concentrations: 100, 250, 500 and 800 pmol/µL. Fluorescence detection standards had the following concentrations: 2.5, 5, 10, 25, and 50 pmol/µL. These standards were used for calibrating the instrument. 'Hydrolysed' standards were also prepared along with samples to correct results for hydrolysis effects. These were prepared by dispensing 20 µL of amino acid standard A-9531 (2.5 µmol/mL) into Reacti-Vials (triplicate) and evaporating to dryness using N₂ at 40°C on a Reacti-Vap. The hydrolysis method was the same as that for samples (see Hydrolysis procedure above).
Mobile phases and gradient

Solvent A was 0.1 M sodium acetate adjusted to a pH of 7.2 with 1 N acetic acid. Solvent B was methanol and tetrahydrofuran (97:3 by volume) (Rajendra 1987). Both solutions were filtered through 0.22 µm GV Millipore filters and degassed under vacuum in an ultrasonic bath. The gradient program applied is given in Figure 1.

![Figure 1](image1)

**Figure 1** Chromatogram of amino acids from fingerprint extract. Standard three letter abbreviations are used for amino acids. Top line indicates solvent gradient program in % Solvent B.

Calculation of results

Amino acid concentrations in pmol/µL were automatically calculated with the ChemStation. A minimum of three standards at different concentrations was run to allow the calculation of linear regression equations for each amino acid from peak areas. Sample concentrations were then calculated by ChemStation from peak areas. 'Hydrolysed' standards were analysed with samples to correct sample concentrations for hydrolysis effects. Concentrations were then in absolute terms and not dependent upon hydrolysis conditions. Amino acid mole ratios were calculated by dividing by the glutamic acid result (Glu=1).

RESULTS AND DISCUSSION

The chromatogram of the amino acids found in the extract from rinsing a finger is shown in Figure 1. In general, the amino acids are well resolved. Fingerprints are high in serine, glycine and ornithine compared to other amino acids. Proteins in plants and animal meat do not show this characteristic. Ornithine does not occur in proteins, however it has been found in the free state in small quantities in some plants. Although ornithine is identified in the chromatogram, it was not quantified in the amino acid standard and hence not included in further results. Amino acid results are more easily compared visually by setting one of them equal to unity (dividing by itself) and comparing the others to this value. This will be done in further results and chromatograms will not be shown. This amino acid profile will be unique for particular proteins from plants and animals and can be used as a basis of identification.

Figure 2 gives the amino acids present in fingerprints and sweet potato protein as found on obsidian flakes. Results are given as mole ratios of amino acids with glutamic acid equal to one. Serine and glycine in fingerprints have a higher ratio to glutamic acid than sweet potato. (Ornithine was also present.) The mole ratios of the amino acids in Figure 2 for fingerprints are different to those in Figure 1 where the amino acids were washed from the fingertips. Although this result was unexpected there is obviously a difference between fingerprint extract and leaving fingerprints on an object. These differences were also found from an analysis of whole sweet potato and that left behind as a residue. If one is attempting to identify residues by comparing known and unknown amino acid or fatty acid profiles then it would be better to base standard analyses on known residues which have been extracted from the tools themselves.

![Figure 2](image2)

**Figure 2** Amino acid mole ratios of combined fingerprint/sweet potato (SP) residues extracted from obsidian flakes.

Figure 3 gives the mole ratios of amino acids present in the combined fingerprint-sweet potato and sweet potato residues. They appear similar but to what extent were the sweet potato results affected when the obsidian containing the sweet potato residue was handled by bare fingers? I made a comparison between the three results obtained in the experiment (fingerprints only,
sweet potato only and the two combined) by doing a stepwise regression using Minitab, a statistical computing system. When a mixture was made by Minitab of the fingerprints only and the sweet potato only to match that of the fingerprint-sweet potato result, it was found that the fingerprints contributed 11.1% to the analysis. This is an average of the results because the experiment was done in triplicate. In a situation where residues are analysed from an archaeological artefact, fingerprints would make up a larger portion of the analysis because the residues themselves would be in a lower concentration having been reduced through soil burial processes. Fingerprints will in time also deteriorate (they are in the form of free amino acids), so it is unlikely that the fingerprints of the original user of the tool are present. No 'ageing' studies of fingerprints were done, but one research project indicates that fingerprint amino acids change fairly rapidly over time although the individual amino acids were not identified (Dikshitulu et al. 1986).

If an amino acid analysis is done on residues from a stone artefact and high levels of serine and glycine are found, then the artefact is probably contaminated with fingerprints. The presence of ornithine should confirm this, but presently the stability of ornithine is not known. Interestingly, a study by Broderick (1979) using paper chromatography to separate amino acids identified ornithine amongst others on a stone tool. No doubt he proved that amino acids can be analysed, but he also may have been unwittingly analysing someone's fingerprints.

Comparison of triplicate analyses of the same samples for amino acids gave correlation coefficients from 0.98-1.00 indicating that the methods used are reproducible. The amino acids were well separated to allow accurate integration of peak areas. Amino acids down to 1 pmol/µL were readily and reproducibly detected. This detection limit can be extended to the femtomole range.

The results of analyses of fatty acids from taro, fingerprints and taro-fingerprints are shown in Figure 4. There is a large difference between the two results and this is reflected in the amount that the fatty acids from fingerprints contribute to the taro-fingerprint result - an average of 51%. (A similar result was obtained for the analysis of fatty acids in sweet potatoes and fingerprints where fingerprints contributed 49% to the mix.) The relatively higher contribution from fingerprints for fatty acids compared to amino acids is due to the lower concentration of fat in taro and sweet potatoes (0.10% avg. for C. esculenta and 0.17% for sweet potatoes, Bradbury and Holloway 1988:84) than protein in sweet potatoes (1.43% avg., Bradbury and Holloway 1988:52). Certain fatty acids in Figure 4 are missing because they are either not present, are below the limit of detection, or cannot be positively identified. The first two possibilities are the usual. Only five fatty acids were common to all three analyses. Fatty acid 14:1 was not present in fingerprints or taro and 15:0 along with 12:0 were not present in taro. Interestingly, 15:0 which is not commonly found in high levels in fats is fairly abundant in fingerprints. Its presence in the analysis of fatty acids from residues on stone tools may be an indicator of fingerprint contamination.

A minimum of two injections of each sample was made of the original triplicate analyses for each constituent on obsidian. Comparisons of the peak areas for comparable samples had correlation coefficients of mainly 0.99-1.00. (Three analyses had correlation coefficients of 0.97-0.98.)
The standard peaks all had baseline separation. Extraction of residues from obsidian using an ultra-sonic probe was efficient and would result in less degradation of fats than the use of soxhlet extraction. Also, very small amounts of solvent are needed in the extraction process.

These experiments indicate fingerprints can influence results of both amino acid and fatty acid analyses of plant residues on obsidian. If flakes have been handled with bare hands, it is probably not worth the considerable effort in analysing residues. Fingerprints will most certainly deteriorate over time and lend less contamination to residues. Also, they may possibly be removed from artefacts with an organic solvent-water rinse, but this may remove the residues too. No doubt the best solution to possible future residue analysis of stone artefacts is to avoid fingerprint and other contamination. My recommendations for collecting stone tools include:

1. Collect artefacts along with soil directly into plastic bags.
2. Take soil samples from within the site and also outside of the site. These are controls.
3. Do not expose collected samples to direct sunlight.
4. Do not wash artefacts!
5. View artefacts by handling with plastic gloves which do not contain starch and, if residues are detected by microscopy, do not contaminate. Save for analysis.

These same recommendations should be applied to potsherds where the possibility of analysing for food residues exists. This probably includes the majority of excavated pottery. Even if burned residues are not apparent, residues will be located within the pottery matrix giving clues to a vessel’s function. Unlike stone tools, fingerprint contamination can be removed from sherds by scraping off the outer surface. Also, if considerable burned residues are present, fingerprints may contribute a negligible amount of amino acids and fatty acids.

This study reveals that it is possible to quantitatively analyse organic constituents on stone tools. It also indicates fingerprints can be a source of contamination. When handling artefacts with residues, handle with care. What is ignored today may some day be an enterprising Ph.D. student's thesis research!

REFERENCES


The analysis of blood and tissue residues is currently standing at a threshold in the application of a new approach to molecular and genetic analysis, but this does not mean that many of the analytical methods used in the early period of residue analysis are outdated. The fusion of the latest technology with research methods that are at the low end of a technological continuum is resulting in a body of methods that permit a broad, and at times highly specific, understanding of the past. In this chapter I will briefly outline the early research leading to the discovery of the widespread occurrence of blood residues and from that perspective, contrast the older methods and research applications with the latest methods of molecular biology that clearly outline the direction of organic residue analysis in the next few years.

THE RECENT PAST: DISCOVERY OF BLOOD RESIDUES

The discovery of the widespread occurrence of blood and other organic residues began some 10 years ago as part of an archaeological research project intended to analyse stylistic change in utilitarian tools from sites in the southern portion of the Northwest Pacific Coast of North America. The generally accepted view among archaeologists in the region (Mitchell 1971) was that the native cultures of the Northwest Coast had probably evolved in place over at least the last 4000 years; and that by at least 500 years ago the unmistakable material culture aspects of Pacific Northwest Indians had been firmly in place. The development of the highly elaborate social order of coastal tribes appeared to be linked with the archaeological material culture. There is no durable pottery record and, except at waterlogged sites, few artefacts containing 'stylistic' information have survived in coastal shell middens. My aim was to analyse the range in stylistic variation of 'projectile points' through the past 4000 years to ascertain, if possible, the ideational aspects of changes in morphology of this functionally constrained class of tools. I had hoped to investigate one aspect of the continuity model of Northwest Coast culture by choosing a specific artefact type, projectile points, which have often been used to define archaeological cultural phases. Published artefactual data from the Gulf Islands area lying between Vancouver and the east coast of Vancouver Island were interpreted by Mitchell (1971) to indicate that projectile points evolved in morphology from the early period when they were large lanceolate and basally-shouldered or notched to the late period forms that were small, triangular and had no notching or indentation. The suggestion was one of continued evolution in shape from large to small and compound outline to essentially triangular.

Changes in the form or, more precisely, the outline shape of projectile points has traditionally been used to define prehistoric cultural periods based upon the assumption that changes in the form of projectile points are linked with larger changes in the prehistoric culture (cf. Dunnell 1978). Changes in styles might indicate new human populations or the diffusion of new ideas into another culture, depending upon the exact nature and rate of change. A diffusionist paradigm applied to stylistic analysis overlooks the important fact that changes in style can result from a new or different tool function or a change in the perception of the suitability of an artefact's form within a culture as the makers and users of specific tools change their ideas over long periods of time (a kind of 'ideational drift').

The strictly morphological analysis of diagnostic artefacts is limited as a strategy for uncovering ideational aspects of style because it relies on a fundamental assumption that artefacts are always intentionally formed to a specifically observed shape. The concept of a 'mental template' is intuitively satisfying, suggesting as it does that there is a uniform and consistent relationship between ideas and culture, form and function. Nevertheless, the identification of these templates by observation of morphology and coarse-grained spatial analysis of tools can fall into a circle of tautology unless there is some
independent identification of specific function(s). Accidental breakages, re-sharpenings, intractable raw material or inept artisanship are only a few of the factors that can contribute to observed morphological variety. Morphological analysis alone can only indicate the degree to which there are recognisable classes based upon shape. To discuss changes in culture based upon artefact analyses presupposes that one knows the function or task association for each artefact and each group of artefacts. The largest limiting factor in the use of the artefact in archaeological cultural reconstruction has been an inability to accurately determine tool function.

Analysis of the styles of functional, utilitarian artefacts as opposed to decorative applications of various media is a complex undertaking. First, the initial shape of the tool must obviously conform to specific technological requirements; a tool must have the right shape to do the required job. But, there are adjunct functions that can be performed with any tool that are not precluded by the specific shape of the functional tool itself. Not only are there functional constraints upon the shape of a tool that reflect specific technological requirements, there are other ideational considerations that affect the form of the tool. It is easy to imagine a situation where a prehistoric group of hunters might have believed that a long and narrow point was better for killing deer, and a short and wide point was better for killing elk, for example (cf. Friis-Hansen 1990). Technologically speaking, either one propelled with sufficient force would kill either the deer or the elk; ultimately the tool maker/user interpreted specific formal and functional attributes and perceived causal chains that resulted in the final stylistic decision.

Stylistic analysis of utilitarian artefacts must therefore always confront the question of function: primary technical functional requirements, adjunct functions, ideational aspects of function, and the stylistic correlates of these concepts. The first step in the study of stylistic change in the assemblages chosen for analysis lay in a description of the morphology of the collection of points and a classification into a morphological typology. This classification needed to be independent of function and was based only on shape, for in the beginning it could not be said with any certainty that the objects in question were really projectile points rather than knives or other related functional types. By applying a method for the morphological description of a wide variety of artefact types (Loy and Powell 1976), 27 different morphological classes were recognised in the set chosen for study. A fundamental assumption had to be questioned however: did the set of putative projectile points in reality function primarily as projectiles or knives?

Ethnographic information from the Northwest coast contained little information on the ordinary material culture of the people. There is so much that is extraordinary about those cultures that the utilitarian has often been overwhelmed. However, the ethnographic literature (for example, Barnett 1955) suggested the possibility that some projectile points and knives were used for more than the simple arrowheads and spear points and hafted knives of a minimal archaeological assemblage: for example, there were traditionally recognised classes (emic classes) that included salmon harpoons with triangular slate points, stone-tipped barbed harpoons, whale harpoons and lances, copper and iron-handled chipped stone knives. In contrast, apart from the entire set of highly variable functionally and morphologically indistinct objects within the study assemblage, there was little prior information that could be used in the correlations of style, morphology and function.

A variety of statistical approaches were used, including analysis of length, width, thickness, and various morphometric ratios, in a search for natural divisions that might indicate larger classes of morphologically defined projectile points, but none were evident. David H. Thomas (1978) demonstrated that a discriminant analysis approach could separate arrow from spear/dart points. Applying his formulae it was indeed possible to separate small, notched points from big, indented points, but a residual, ill-defined set still remained. Statistics alone appeared not to be able to identify strictly functional or stylistic groups within the set of morphologically defined projectile points.

Direct experimentation including analysis of breakage patterns and edge damage was used in an attempt to distinguish between high impact damage from projectile point use, low energy twisting or other breakage from knife-related usage and manufacturing failure. It was in fact possible to distinguish two types of damage: high velocity, high energy impact fractures; and low velocity twisting fractures and shock failures from cutting and or manufacture (cf. Kamminga 1982; Cotterell and Kamminga 1979). Unfortunately not all the projectile points were broken, but breakage analysis could be used to suggest morphological correlates with unbroken points. But this offered little hope of securely identifying individual artefacts as either 'knife' or 'projectile'.

While the experimental research was being pursued, Lawrence Keeley published his land-
mark article in Scientific American (Keeley 1977; see also Keeley 1980). Keeley maintained that he could detect microscopic wear polish that gave specific functional identifications; that it was possible to distinguish one type of polish from working wood, another from bone and yet another from working meat. Keeley's technique appeared to be a new method of analysis that would allow the unequivocal assignments of functions to the morphologically, mathematically and experimentally classified tools. This research method went directly to the crucial point: in order to proceed with any stylistic analysis, the function of each tool and each tool class had to be held as a constant in order to isolate the strictly stylistic (i.e. nonfunctionally driven) variation. It was from attempts to replicate Keeley's work that eventually led to the discovery of the ubiquity of blood and other organic use-residues.

Nevertheless, Keeley had not proposed a mechanism that would cause the polish. Here a central question had been unasked: what did Keeley mean by polish? The word 'polish' itself became a problem. A fundamental element in the identification of minerals is the concept of relative hardness. Moh's scale (from soft 1-talc, to hard 10-diamond) reflects the relative hardness of various minerals. To find the hardness, the unknown mineral is scratched with a material of known hardness in order to see whether it scratches or not. The rule is: equal or greater hardness results in no visible scratching, and softer materials result in scratching. Simply put, to polish a glass mirror, a substance harder than glass (e.g. carborundum grit) is used to scratch or grind the surface smoother and smoother until the minor pits and variations of the surface reach very small dimensions. Finally, when the grit cannot be made smaller, various laps (pads of fibre and pitch) are used to mechanically and thermally alter the glass surface in a process (that is still not clearly understood) to finally reduce the surface variation to less than half the wavelength of light (Comish 1961). On the other hand, substances can be polished by adding a medium that smooths the surface to the requisite half or less the wavelength of light. If the polish Keeley was observing was reductive polish, then there should be some observed flattening of microscopic surface irregularities and mineral grains at least and the substance doing the polishing must be harder than the substance polished, or it might act in conjunction with heat and mechanical pressure as if it were a lap. If the polish were additive, then no surface reduction would be seen and the added surficial material should be microscopically visible.

A series of experiments were undertaken[1] to investigate the possibility that Keeley was observing reductive polish and various contributors to reductive polish were investigated. The results at the end of these experiments were that:

1. soil polish was unlikely and, at any rate, distinctive.
2. quiver wear might produce polish but hafted knives would be prone to the same quiver wear if carried in a sheath.
3. in the most optimistic of circumstances it would take an inordinate length of time for one tool to be polished in a reductive manner, even against material as 'hard' as the chrome tanned leather. It was not possible to observe reductive polish from dry leather in less than 300 m of continuous cutting.

It was evident that Keeley observed some sort of polish – his photographs demonstrated areas having more highly reflective surfaces. The problem remained: what was the mechanism whereby polish was formed? The logical alternative to reductive polish was that the polish was additive. To investigate that alternative, the experimental tools were observed again under the microscope. New tools were made and used to investigate additive polish. Microscopic examination revealed that there was always a reflective surface produced after tool use on animal products and some plant materials when compared with unused basalt material. The tools were washed in cold water, in hot water, in soapy water (the soapy water itself left a shiny residue and was abandoned for that reason). Keeley had used mild acid and alkaline conditions to clean the tool surfaces. Hydrochloric acid (1 Normal) made little difference and 3% hydrogen peroxide and 10% ammonia only acted to reduce the reflectivity. Through this process of replicative experimentation it seemed that there must be some organic residue left from working the experimental materials that was additive, and the organic material must have contributed to polish formation by reducing surface relief.

From this experimental base, a group of artefacts consisting of obsidian microblades from sites in the Gulf Island region were examined. They were at least 1000 years old and had never been washed or cleaned following excavation. What appeared to be a red blood cell in association with other organic residues was observed on one tool. Acting on the assumption that the feature was a blood cell, I began looking

[1] The full accounting of the experimentation that led to this discovery is in preparation for publication elsewhere and I will only summarise those aspects directly pertinent to this article.
for more red cells on the tool, and the more I looked the more I found. They were the right shape and size – they had no nucleus and they did not appear to be spores, pollen, diatoms, fungal, algal or bacterial bodies. The appearance of all of those possible identifications were checked against known control samples of thick and thin blood smears made on microscope slides and pieces of obsidian. From the microscopic appearance of the dried blood smears, other features that had been observed on other microblades in the collection were seen to be suggestive of blood films. But looking at imputed red cells or crazing patterns was not the same as proving that there was a blood coating, and it became obvious that another avenue of identification was necessary.

This avenue was provided by Brier’s (1976) report of plant matter adhering to tool surfaces even after several thousand years. This was the first thorough documentation of plant remains on prehistoric tools. These tools had been recovered from dry cave sites in the Southwestern portion of the United States. As the artefacts came from dry caves, it appeared that special circumstances might be required to preserve plant remains. The significance of Brier’s work however lay in his use of biological stains (commonly used in plant histology studies) to bring out the cellular patterns and identify starches and plant oils on these tools thus confirming the plant types actually cut and processed by those prehistoric people. His study suggested that a similar approach with biological stains and other reagents sensitive to proteins, especially blood, might provide the extra dimension of proof in establishing the presence of blood on the surface of the tools from the archaeological assemblage under study. This marked the beginning of using a wide variety of chemical methods to identify organic residues.

The action of biological stains are complex and, for many, stain reagents and combinations are incompletely understood; nevertheless there are a number of stains and stain mixtures that react in predictable ways with specific organic compounds. Three common stains were chosen: Aniline blue (water soluble) for protein, Iodine potassium iodide (IKI) for starch, and Sudan IV (alcohol soluble) for fats and oils (Bruier 1976; Humason 1978; Florian 1984). The assemblage of microblades and point/knives that had been made and used in controlled circumstances in previous experiments were used and the stains were applied to their edges, then washed and examined. When staining tools that had been used to cut meat, it was easy to see the brilliant blues of stained tissue indicating proteins and red-stained patches indicating fats and, significantly, no blue-black and purple grains that would have been starch. On tools that had cut plants, there were blue areas, some red fatty areas and, occasionally, some blue-black grains; on tools that had been used to cut tubers (potatoes and the rhizomes of cattails), the tool was covered with the blue-black results of the IKI stain for starch.

At least for modern tools then, it seemed that the three biological stains could serve to indicate in a broad way the type of material the tool had been used upon. Eventually it was found that Aniline blue (WS) stained both protein and cellulose and so could not be used to positively identify blood or meat proteins. But, by constructing a small table one could, with some assurance, identify the major category of the worked material (Table 1).

<table>
<thead>
<tr>
<th>Aniline Blue</th>
<th>Sudan IV</th>
<th>IKI</th>
<th>Worked Material</th>
</tr>
</thead>
<tbody>
<tr>
<td>+</td>
<td>+</td>
<td>-</td>
<td>meat</td>
</tr>
<tr>
<td>+</td>
<td>-</td>
<td>+</td>
<td>plant</td>
</tr>
<tr>
<td>-</td>
<td>-</td>
<td>+</td>
<td>starchy plant/root</td>
</tr>
<tr>
<td>+</td>
<td>+</td>
<td>+</td>
<td>meat and plant</td>
</tr>
</tbody>
</table>

At the end of the experiments there was sufficient evidence to believe that biological stains and microscopy constituted a reliable system that would permit the distinction between projectile points and knives, by directly identifying the worked material. At this stage Keeley’s use of polish appeared largely irrelevant, elusive as it was and subject to so many different causes. The opportunity to determine tool use through the direct identification of the worked material had presented itself.

This research was not the first to observe organic remnants of past tool use, nor was it the first to have used biochemical techniques to investigate ancient organic materials: for example, the use of antibody techniques in establishing ABO blood groups of ancient people by testing human bone and mummified tissue (Lengyel 1975; Connoly 1969; Harrison et al. 1969; Boyd and Boyd 1937). Some classes of residue were recognised: silica glosses and phytoliths on sickle blades (Coughlin and Claassen 1982), hafting resins, especially in the Australian context (Kamminga 1982), and tissue remains from both plant and animal origins (Bruier 1976). Organic residues on stone tools were occasionally observed during lithic use-wear studies but
they were not systematically pursued. As mentioned above, Briuer (1976) undertook an analysis of plant remains on stone tools excavated from a cave site in the southwest of the United States of America. Anderson (1980, 1981, 1983) used metallographic and scanning electron microscopes to examine residues on stone tools from a variety of sites in Europe and the middle east. Shafer and Holloway (1979) advanced the use of organic residue analysis in the determination of tool function and Brodrick (1979) suggested that amino acid analysis of tool surfaces might be useful in determining tool function. Mansur- Franchomme (1983) suggested that organic residues become entrapped in silica gels on the surfaces of stone tools. As Fullagar (1986) observed, use-wear analysts generally considered it to be unusual for organic material to survive except in special environmental circumstances. This assumption was based upon a further untested assumption that organic materials were easily broken down and would not survive the hostile environment of typical site soils.

THE DISTANT PAST: ARTEFACTS AND THE TRACES OF USE

At any point in the history of human use of tools, traces of that use are preserved in the organic materials on which the tools were used. It is an axiom of modern forensic science that whenever a material contacts another material, some transfer is bound to occur, whether it be the contact of two automobile fenders that leave traces of paint on each other, or a knife used in a homicide. If we remove the act to a non-forensic setting, the fact of material transfer remains. If a hunter makes a tool from stone and uses it to disjoint an animal, there will be traces of the manufacturing event (stone or bone fragments from the fabrication tools and his own blood if he inadvertently cuts himself in the process of making the tool) and all the subsequent contact with the animal carcass during the butchery. Blood, muscle, collagen from sinew, skin and bone, hair, pollen and insects in the animal's fur or on the skin will all find their way onto the tool in varying amounts, thereby reflecting the exact process of butchery. When the tool is eventually discarded it may have been used only once, or many times and each use will be represented by residues on the tool. Finally, the tool begins its residence in the soil where soil particles, microorganisms, humic and mineral compounds come into contact with the surface of the residue on the tool.

To summarise the process, when an animal is cut by a stone tool, blood comes into contact with the tool and spreads across that surface. Exposure to air and the subsequent evaporation of some of the water substantially changes the ionic composition of the blood by increasing the relative amount of salts resulting in changes to molecular and amino acid constituents. The changes extend from rearrangement of composition or conformation at a local level to more drastic quaternary and tertiary denaturation of each type of molecule, depending upon local chemistry and other micro-conditions. Many of the chemical bonds that hold the molecule in its proper functional and biologically active shape are broken or redistributed and the molecule expands into a less organised configuration. However, the primary structure is maintained with the stronger covalent bonds and peptide bonds linking the primary amino acids together. Immediately, the exposed charged binding sites begin to form new bonds with nearby reactive sites on other molecules; and, importantly, with some of the exposed positively and negatively charged sites on the surface of the tool material. The unravelling of portions of tertiary structure, or more simply a general expansion of the molecule exposes interior areas where the hydrophobic residues are normally buried, acts to increase the degree of insolubility.

The stable residue state is characterised by low solubility and occurs when the protein structure is modified from the native state to one where internal chemical bonds that are stronger are reformed, or in structural positions (e.g. hydrophobic areas) that cannot be easily re-broken or reformed within the given burial environment. Simple aggregation and more complex processes of polymer formation link the changed molecules together and prevent, or slow, the processes of resolubilisation or renaturation. The stability of this process is largely dictated by the environment at the time of the denaturation, and the stability of that environment over time. Different molecules react to a greater or lesser degree to the same denaturing conditions; if those conditions are the same for all molecules, some will be only slightly affected and some will be profoundly changed by the conditions. This fact creates difficulties in the analysis of dried blood residues. It can mean that it may be impossible to put some proteins back into solutions so that they resemble their prior native, functional shape and activity. This in turn makes for difficulties in documenting the changes that have occurred since deposition of the residue. But it also means that some molecules are preserved virtually intact.
and unchanged and thus are available for analysis in a condition that is indistinguishable from modern controls.

With burial and partial rehydration of the surface of the residue, ionic crosslinks are made with soil particles, especially clays and silts. These bonds are quite stable given normal site environments and soil chemistry. The ionic crosslinkage with clay/silt particles and the generally desiccated state of the residues, especially the rather more random arrangement of subsequent intra- and intermolecular crosslinkages, reduces or eliminates the ability of bacterial enzymes and groundwater to degrade the residue beyond that reached in the stable state. The end result is a kind of sandwich: at the bottom is a firmly bound interface between the stone tool and the residue, the desiccated and aggregated residue itself; and at the outer interface with the soil — a layer of firmly bound fine soil particles.

Through this set of molecular and taphonomic processes the organic residues form a highly stable complex that can resist degradation in even the most harsh environments. Central to this preservation are the interactions between proteins and the stone tool surface and soil particles. Rapid dehydration of the protein, changes in the ionic composition of proteinaceous substances and the chemically charged nature of stone and soil particle surfaces act together to form an almost insoluble complex. This complex is able to withstand groundwater and microbial degradation over what seems to be astonishingly long periods of time.

### EARLY METHODS OF ANALYSIS

This background sets the stage for a discussion of a few of the analytical techniques which have proved useful in residue analysis. The early research period relied upon microscopy of both replicate and excavated tools and the use of biological staining reagents to indicate the broad class of residue type, plant, animal and mixtures of the two. The early research period was also concerned with an initial understanding of the physico/chemical mechanism for the formation and longevity of proteinaceous and plant residues. Much research was expended simply looking at collections for the presence of residues and documenting the class of residue; in fact that activity still remains an important aspect of residue analysis. Nevertheless, as soon as the ability to identify the class of residue was confirmed, the inevitable extension to the identification of specific materials and ultimately taxonomic identification became a research imperative. That imperative demanded the application of biochemical and molecular biological methods.

A rather long period of evaluation of specific methods ensued and two key areas were addressed: the demonstration of any putative blood residue that actually contained proteins and specific blood molecules (e.g. hemoglobin, serum albumin, immunoglobins); and the identification of species of origin. There are three broad classes of techniques that were investigated: colourimetric detection of specific molecules; protein separation methods; and immunological techniques. Out of the range of potential techniques for each class of method, specific protocols were investigated for their suitability.

Two aspects of the research setting became germane to the assessment of technical suitability: 1) availability of funding and 2) access to specialised equipment and specialist researchers. These considerations, coupled with specific constraints of certain methods acted, and still do act, to limit the variety of techniques that are actually possible to apply to residues. The final limiting factor lies in the small size, relative insolubility, and the mixture of both cultural and natural components of the residues. For example, the demonstration of the presence of blood molecules can be made using colourimetric tests for both hemoglobin and serum albumin and there are straightforward clinical and forensic methods readily available. But the admixture of soil components complicates the application of such methods by introducing the potential for false-positive reaction in some tests that stem from a wide variety and somewhat unknown set of potential contaminants. Early on it became obvious that no single test could be used as a totally reliable indicator of the presence of specific residues. Eventually a strategy evolved that saw the development of a series of complimentary and confirmatory tests that are sufficient to build a weight of evidence: much like building a courtroom case based upon circumstantial evidence.

Colourimetric methods for the quantification of proteins were a logical starting point because they were inexpensive and generally quite straightforward. In addition, they were conceptually related to the initial testing with biological stains but were more quantifiable, and the reactions could be more easily controlled. There were a number of possibilities that could yield information about the presence of proteins in general (Biuret and Lowrey type, for example), and a few that are suitable for the determination of the presence of specific molecules. It ap-
peared, after some exploration, that the most unique molecule present in large amounts in blood residues was hemoglobin, and the exploration of detection systems concentrated on that molecule.

For many years forensic scientists have used a screening test based upon the peroxidase reaction using diaminobenzidine. In recent years it has been implicated as a carcinogen and, although there are other substitute chemicals, benzidine remains one of the most sensitive. Phenolphthalen is a well recognised substitute (Higaki and Philp 1976) but it is slightly less sensitive. In response to the clinical need to screen large numbers of urine specimens for the presence of trace amounts of hemoglobin which indicate the presence of blood in urine, commercial 'dip stick' tests, such as the Ames Labstix or Hemastix, or the Chemstrip 5 were developed. These tests require only small amounts of liquid (20 µL or less) and are, within the limitations of the method, quite reliable.

The Ames Hemastix test, in common use clinically as a urinalysis screening test for blood in urine and, forensically, for testing suspected blood droplets or splatters, uses a benzidine substitute. This chemical is impregnated into a small pad that, when wetted with the unknown test solution either remains a light yellow orange if there is no blood or, in the presence of blood, changes to shades of green: the deeper the colour, the greater the concentration of blood. The sensitivity of these pads is very good, reacting positively to the presence of the hemoglobin contents of as few as 5 red cells per microlitre, or about 0.5 ng of hemoglobin/myoglobin per microlitre of eluted residue. Importantly, the Ames Hema­stix test, although subject to false-positive reactions with plant chlorophyll, will not render a false-negative. That is, it will not indicate a positive reaction in the absence of chlorophyll, hemoglobin, myoglobin or strongly oxidising compounds as listed on the product sheet. Thus, if one gets a positive reaction, then it is a simple matter to distinguish between possible causes. If the test is negative, then either there is no blood, chlorophyll or other compound that will participate in the chemistry, or they are there in amounts that lie below the sensitivity of the testing pad.

Colourimetric protein detection tests commonly in use can be subject to false-positive results. I noted this at the outset (Loy 1983) and it has also been commented on by Gurfinkel and Franklin (1988:89) and Custer et al. (1988:343-5) who encountered this problem in their own attempts to detect residues. The Ames Hemastix test and other related peroxidase-sensitive tests can react with vegetable and bacterial peroxidases and the pseudo-peroxidases of hemoglobin/myoglobin, cytochrome C and chlorophyll (Loy 1983). Culliford (1971:47-58) discusses potential false-positive results of the benzidine test on modern residues in the crime laboratory and notes that false-positive reactions from vegetable sources or peroxidases can be easily discounted by microscopic examination. Vegetable peroxidases are contained in plant and bacterial tissue and do not survive long. In addition, a brief heating to 100°C will destroy vegetable and bacterial peroxidases but will not affect hemoglobin and myoglobin. It is not sufficient, however, to rely solely upon colourimetric tests to unequivocally demonstrate the presence of blood and, hopefully, one would not use this as the only analytic technique. Potential false-positive results, however, do not diminish the value of clinical strips as a highly useful, inexpensive and informative screening step in identifying prehistoric use-residues.

**SPECIES OF ORIGIN DETERMINATIONS USING IMMUNOLOGICAL METHODS**

Species of origin determination became a research goal for me almost as soon as it was realised that organic residues are commonly preserved. Initial observations of hair and feather barbules in some of the residues (Loy 1985) allowed an identification well beyond the level of animal or plant, or of identification of blood on the presumptive evidence of the Hemastix test. From the beginning of this research, microscopic examination revealed the rather common occurrence of red blood cells and, simply by the presence or absence of a nucleus and the shape (circular or ovate), it is possible to distinguish mammal from non-mammal blood (Andrew 1965; Stast 1989; Loy 1983, 1987).

Attempts to determine species of origin led immediately and directly to the use of the hemoglobin crystallisation method (Loy 1983; Loy and Wood 1989; Loy and Dixon submitted). This technique has now been proven through years of use and blind testing as a reliable and inexpensive method to identify blood residues at the species level. The method needs only a few picograms of oxygenated hemoglobin to produce diagnostic crystals. The major drawbacks lie in the fact that it is identificatory at the species level only, and requires the exact species blood as a reference to identify unknown species; the method also requires extensive microscopic analysis of both the slide preparations and the crystals
themselves to achieve reliable and consistent identifications.

Immunological methods were investigated as possible ways to corroborate hemoglobin crystals during the testing phase and as alternatives for identifying selected species. These methods are based upon the immune system which protects against disease and is responsible for rejection of tissue grafts and transplants, and generally defines what substances belong to the biological 'self'. Advances in understanding of this system has resulted in a variety of methods for the study of proteins (see Weissman et al. 1978). Fundamental to the immune system is the body's production of specific molecules, the immunoglobins. Chief among these is Immunoglobulin type G (IgG) which is composed of stable and variable regions in its biologically active shape. That shape is basically like the letter 'Y' with the stable, invariant region in the 'tail' of the Y and the immunologically reactive hypervariable and variable regions at the end of the two upper arms of the Y. The variable regions are determined by the exact sequence of amino acids from which the IgG molecule is constructed. The three-dimensional arrangement of the amino acid sequence in the hypervariable region at the ends of the 'Y' and the spatial distribution of ionic charges and non-covalent bonding sites, trap and hold complimentary arrangements of amino acids on target antigens. (The targets of antibody binding are called antigens.)

The largest single difficulty with accurate immunological identification of species has been the lack of antiserum specific to archaeologically common subsistence species. Rat, human, sheep, rabbit, goat, horse and bovine antiserum can readily be purchased because of their clinical research importance. For the most part, however, any other antibodies must be custom made which is an expensive proposition. Nevertheless, forensic scientists have been using broad spectrum antibodies for some time to identify, with a certain probability, the family, as well as genus and species level taxonomic identity of blood residues. Hyland et al. (1990) have reported on the use of antibody techniques (principally enzyme linked immunosassay) on prehistoric residues and they note the problems of cross-reactivity and the difficulty of obtaining or producing specific nonmonoclonal antibodies. He notes (pers. comm. 1990) that a limiting constraint on species identification is the non-availability of a range of antibodies against archaeologically important animal species. This aspect was a major limitation in the beginning of residue research and continues to be so.

The methods of immunology available in the beginning of the research were not very sensitive and required relatively large amounts of residues. Precipitin tests then in common use rely upon the formation of an insoluble precipitate when large amounts of antibody and antigen combine. The precipitate is normally visible as a white opaque line although, in some micro-scale tests, the precipitin line must be stained to be clearly observed. Typically, a precipitin test involves the diffusion of antibodies and test sample through a gel as in the Ouchterlony technique where a pattern of holes (wells) is punched into a thick gel of agarose and filled with antigen and a variety of antibodies; interactions are recorded as positive when a line of precipitate is observed lying somewhere between the antigen and antibody wells. Prager et al. (1980) used this approach to determine the taxonomic relationship between frozen mammoth tissue (Mammuthus primigenius) and modern elephants. There is a minimum amount of antigen/antibody solution needed to form a visible precipitin line. Even using thin agar gels and sensitive stains to visualise the precipitin line, the method generally requires milligram amounts of antigen/antibody, which is much more than usually present in a typical blood residue.

Immobilised labelled-antibody tests have now almost completely replaced precipitin tests because of the advantages of speed, small sample size and small reagent requirements. In this group or related procedures, the simplest involves immobilising the antigen on support; the antibody is labelled (i.e. has had a marker attached to it) and when it combines with the antigen, the antigen is labelled and thus detectable. Radioimmunoassay (RIA) was the first to be commonly used in immunological testing as it offered versatility not possible with precipitin tests and sensitivities in the low nanogram and mid-picogram ranges. The presence of antibody-antigen complexes are detected and the strength of the reaction is measured in the radioimmunoassay method by simply putting the bound sample into a radioactivity measuring chamber and calibrating it against a known concentration of antigen. The use of radioactive chemicals demands compliance with a complex set of safety rules that precludes RIA analysis on a routine basis in most laboratories. RIA has been extensively used by Lowenstein (1981; Lowenstein et al. 1981; Lowenstein 1985) to identify the species of origin of bone, tissue extracts and some tool-use residues (Nelson et al. 1986; Newman and Julig 1990).

As a refinement of the RIA technique, de-
developed partially to avoid the use of radioactive chemicals, Enzyme Linked Immunosorbent Assays (ELISA) were developed in the 1970s. ELISA tests are now a routine method of immunological testing in laboratory and clinical settings. In this group of procedures, various enzymes are attached to the Immunoglobulin G molecule. Most are colour labels that turn intense colours upon the addition of suitable colour development reagents. Miyai (1981) provides an excellent overview of the variety possible with ELISA type assays. Because each antibody molecule is labelled, the depth of colour is proportional to the concentration of original antigen that is bound by the reacting antibody. Most ELISA assays are done in transparent plates with 98 wells that can hold about 0.5 mL (500 µL); the coloured reaction can be read and scored directly by eye with reference to wells that contain the known concentrations of antigen, or in automated readers that produce corrected settings. In this group of procedures, various concentrations automatically. The support can also be a nitrocellulose or other membrane that firmly binds the antigen; this type of test is usually termed a 'dot-blot' test. Ascenzi et al. (1985) used the dot blot test to detect the presence of hemoglobin and hemoglobin breakdown products in extracts of prehistoric human bone. A typical ELISA test used to identify the presence of human Immunoglobulin G (the antigen) from tool use residues using a monoclonal antibody, takes about 3.5 hours to perform and requires routinely only nanograms of antigen to give a positive reaction. Some dot-blot tests have been shown to detect picograms of specific proteins.

Two specific immunoassay methods have been used in residue analysis. In one test, a specific protein derived from the cell walls of the bacteria Staphylococcus aureus, designated protein A (SpA), has been found to recognise and bind with a region of the invariant portion of the Immunoglobulin G molecule, the Fe portion of most mammalian Immunoglobulin G (IgG) (cf. Weismann et al. 1984). The SpA molecule is most often used as a substitute for specific anti-animal second antibody in routine immunological work (cf. Forsgren and Sjöquist 1967; Wright et al. 1977; Harper et al. 1982). To circumvent the potential for uncertainty inherent in colourmetric tests, and to augment microscopic examination, this molecule was used in a dot-blot ELISA type test as antibody to determine the presence/absence of mammalian IgG (Loy 1983, 1985, 1989). In 14 separate tests, 38 different animal, bird and fish species' blood, preserved as dried films, have been subjected to probing with SpA. To date, the only exception to the universality of exclusive mammalian binding is a false-positive reaction to domestic chicken blood. Included in the testing panel were samples of proteins extracted from ten samples of desiccated tissue or bone from known species. The oldest control samples, some 37,000 years old, including Alces latifrons, Bison priscus and Mammutthus primigenius, all reacted positively. SpA has been used with success to quantify the preservation of IgG in blood residue extracts from tools that are between 90,000 and 100,000 years old (Loy 1990).

In contrast with the SpA immunological test which is useful because of its non-specific bind with all mammalian IgG, a very sensitive and selective antibody has been developed for the Federal Bureau of Investigation (FBI) in the United States. It is designed to bind with a unique portion of the amino acid sequence (an epitope) of the human serum albumin molecule (Benjamin et al. 1987) and is now offered commercially by Humagen Inc. who market a test kit, 'Human ID™' (Charlottesville, VA). The use of a monoclonal antibody is desirable because the probability of cross-reaction is negligible. This test was developed for forensic use where previously the unequivocal identification of human blood has been problematic using monovalent or polyvalent antibodies and various other electrophoretic tests. The test is a complex double antibody sandwich type. The first antibody, designated HSA-0 (an anti-primate serum albumin antibody) is bound to the walls of a plastic microtitre plate; addition of the unknown sample results in binding of the serum albumin if it is primate in origin. Following washing, the second monoclonal antibody designated HSA-1 (an anti-human serum albumin antibody) is introduced into the washed well. This antibody then binds with any human serum albumin already bound by the HSA-0 antiprimate antibody. The final step is to develop the colour label system associated with the second antibody. The colour development stage presents the only difficulty using this product: the colour reaction continues to increase in strength and there is no way of stopping the reaction. Thus the reaction must be read at a uniform time before the negative control begins to develop any colour. There are other colourmetric detection reagents available that can be stopped and may perhaps offer greater sensitivity and ease of quantification using optical densitometry (Sigma Chemical Company, St. Louis).

In one series of tests, two tools from the Barda Balka site (Loy 1987) designated BB-1
and BB-2, along with controls of human serum albumin and horse albumin, extracts of rock art pigments from Tasmania and Northern Australia (Loy et al. 1988) and a series of extracts of bone from human and non-human sources were reacted with the monoclonal antibodies. All human standards were positive and all non-human ones were negative. BB-1 was positive, indicating human blood, and BB-2 was negative, consistent with all other tests previously carried out on this material. The reaction of the monoclonal antibodies with the Barba Balka tool I is significant because it is at least 100,000 years old. It was on the strength of this test that the presence of human serum albumin was documented in rock art paints (Loy et al. 1990).

Extensive testing by the Humagen Company and the original developers have found no false-positive reactions with either blood or common forensic materials that can cause false-positive reactions using non-monomonal antibodies. The test, because it is complex and first screens out all non-primate serum albumin, means that the second antibody recognition of the human serum albumin epitope has a very high probability of deriving from human blood sources. Put another way, the probability of a false-positive reaction is minute. This test was admitted into evidence in trial cases almost immediately following its release by the FBI (D. Zauner, Pennsylvania State Crime Laboratory, pers. comm. 1988).

**GENETICS AND THE FUTURE OF THE 'HIGH-TECH' APPROACH**

Advances in technology in the fields of protein chemistry, especially protein separation, have permitted the almost routine verification of the presence of specific blood molecules in organic residues; of these the most useful have been HPLC and IEF. Their strengths lie in the fact that very small amounts of residue (in the order of a few nanograms) can be routinely examined and the actual protein molecules present quantified (Loy and Hardy, submitted). It has proved impossible at the present time to use (HPLC) as a species identificatory method. IEF has been successfully used for species of origin determination (Nelson et al. 1986) but the adaptation of the method for routine screening of large numbers of samples remains in the development stage as yet.

All of the techniques of species identification used for the analysis of residues take advantage of genetically driven variation in physical attributes of protein molecules. DNA is a structure that contains in its three-dimensional conformation, all of the information needed to make all the proteins and organic structures of the body. Each group of DNA base pairs that form the coding regions of genes are the starting point of the manufacturing system that eventually results in the production of a variety of nucleic acids and protein molecules. The specific identity and order of the base pairs of certain regions within the DNA genome are eventually expressed as specific amino acids which, in turn, comprise specific molecules. Changes that result from mutations and rearrangements of coding elements in the expressed regions of DNA themselves are expressed as substitutions within the basic composition and structure of viable molecules. Substitution of amino acids can change a variety of physical and structural aspects of protein molecules; changes in mass, charge and three-dimensional conformation are exploited by such techniques as microscopy, ultracentrifugation, electrophoresis, chromatography, immunology and others. Prior to the most recent developments in molecular genetic analysis, these derived indicators of changes at the DNA level were the only way to obtain genetic information from blood residues, given the small size of samples and the unknown effects of aging on the DNA molecule.

![Figure 1](image-url)

**Figure 1** Nucleated cell removed from within cancellous bone from the skull of a human burial designated WLH50, Wilandra Lakes, Australia, estimated to be in excess of 25,000 years old. Nuclear structures and mitochondrial bodies are clearly visible. Wrights stain, approximately 900 diameters magnification.
During the course of the examination of bone powder for radiocarbon dating and species identification applications, some cellular structures other than red blood cells were observed. The most interesting observations were of nucleated cells with the cell membrane still intact. As with blood residues, occasional red blood cells are observed in microscopic amounts of bone powder, but consistent with the experience from blood films, most red cell membranes are lysed in the process of initial dehydration and subsequent change in solution parameters. Recent modifications I have made to the methods of Wright's stain, and fluorescent stains specific for DNA, have clearly shown the presence of nuclear and cytoplasmic features. The cell illustrated in Figure 1 is significant, not only because it has identifiable structures, but because it has been extracted from a largely collagen-depleted calcified human skull (designated WLH 50) from the Lake Mungo area of Australia and estimated to be at least 20,000 years old (cf. Flood 1983).

In 1985, Pääbo (1985) reported the successful isolation and cloning of DNA from mummified human tissue. Although both interesting and very suggestive of new research possibilities for blood residues, the cloning technology described and the necessity for relatively much larger samples appeared to put this type of analysis well beyond the scope of application for the analysis of ancient residues. Since then Pääbo has demonstrated not only the ability to obtain microgram quantities of DNA from mummified tissue, but has proceeded to demonstrate the feasibility of using ancient tissue to investigate genetic history. Pääbo has concentrated on the extraction of DNA from mummified tissue and, to date, the oldest tissue with which he has worked is about 13,000 years old (Pääbo 1989). In the latter part of 1989, Hagelberg et al. (1989) announced the successful extraction of mitochondrial DNA from human bones from two sources, the British Civil War and a 5000 year old burial from Wadi Mamed in the Judean Desert.

DNA obviously promises a vast amount of information for both population and individual genetics. Jefferys et al. (1988) has devised a method of identifying the individual by examining the bands produced when DNA from a single nucleated cell is cut into fragments by a specific enzyme. This technique has profound implications for forensic science, both for criminal investigations and for questions of paternity. Analysis of this type will allow identification of the individual in a prehistoric setting, a compelling theme throughout the history of archaeology but not possible until now. Earlier attempts to identify the individual in prehistory have necessarily been based on secondary evidence. For example, Gunn (1977) used fourier transforms of diffraction patterns created by the shape of artefacts in an attempt to identify individual manufacturing 'signatures'. Now, with DNA finger printing methods, blood from flakes produced at a quarry or manufacturing site can directly identify an individual and thus also provide a concrete basis for projections about the number of individuals who produced or used a particular assemblage.

**Figure 2** Polymerase chain reaction (PCR) products from 25,000 to 35,000 year old *Bison priscus* muscle tissue taken from four different individual mummies recovered from near Fairbanks, Alaska. DNA from the tissue was extracted using the standard phenol/chloroform Centricon 30 procedure (cf. Hagelberg et al. 1989). PCR primers, CP16 and CP22 were chosen to amplify a 250 base pair region of a highly repeated minisattellite specific to the Bovinae. PCR was run through 35 cycles, the product was separated in a 2% agarose gel and subjected to Southern blotting and probed with the CP16 oligonucleotide that had been labeled with radioactive 32P and exposed to X-ray film overnight at -70°C.

The application of DNA analysis to ancient blood residues did not seem possible however, considering the large amount of DNA required. Mitochondrial DNA was traditionally purified from human placental tissue and nuclear DNA from purified and concentrated cells (blood and tissue). In 1988, I attempted to extract and identify DNA from bone powder as part of my research to extract noncollagenous protein for radiocarbon dating. Having seen nucleated cells...
in bone extracts, it seemed reasonable to suspect that some intact DNA would survive. The results of a preliminary study to investigate the potential of extracting DNA from bone using standard techniques of phenol/chloroform extraction and SDS gel electrophoretic separation of DNA (Maniatis et al. 1982) were inconclusive; if DNA was present in the extracts, it lay below the limits of detection sensitivity. Within months of that attempt however, the technology of Polymerase Chain Reaction Amplification (PCR) and DNA was widely publicised (Ehrlich et al. 1988). In its most recent form it provides an automated technique that enables a single fragment of single genes to be accurately copied millions of times in a short period without the errors involved in cloning (Higuchi et al. 1988; Vigilant et al. 1989) and Jeffreys et al. (1988) appear to be well on their way to the DNA fingerprinting of the nuclear contents of a single cell. Obviously when dealing with such small amounts of material, modern contamination remains a risk, but by using the precautions described by Sarkar and Sommer (1990) and Holding and Monk (1989), contamination can be avoided.

PCR amplification technology has two immediate and major advantages to the study of ancient DNA. First, and of most importance is the fact that very small amounts of original DNA source material is required for analysis. Considering that amplification can be done on as little DNA as a few tens of picograms per microlitre, it is feasible to undertake genetic analyses on the very small amount of nuclear DNA in blood residues. Valuable and/or scarce material can be analysed without apparent destruction. With respect to the size of the sample, the technique is similar in AMS dating, i.e., high precision from small samples. Thomas et al. (1989) used small amounts of muscle tissue and hide from the extinct Australian marsupial wolf to investigate its phylogenetic relationships; they found it to be more related to Australian marsupials than any other animal group, and Pääbo et al. (1988) used PCR to amplify mitochondrial DNA sequences from human brain tissue, well preserved in a 7000 year old skull recovered from a Florida swamp. The second advantage is that, although contamination (e.g. from hairs and skin) can be a problem when doing research on human DNA, it is important to note that the amplification is highly specific: if a sample of bone or tissue has high loadings of bacteria, the PCR amplification procedure will be completely insensitive to that DNA as long as the primer chemicals are made to recognise specific sequences of the target DNA. Well chosen primers can be highly selective, and the probability of false amplifications is very low. As Thomas et al. (1989) observed, curatorial handling of skin and tissue of the extinct marsupial wolf resulted in detection of both human and marsupial wolf DNA, but judicious decisions about their PCR primers resulted in eliminating the amplification of the human DNA contaminants. Molecules from blood extracts have been found to inhibit the PCR amplification reaction and, until better understood, may prove to be a problem; some treatments can reduce this inhibition of the enzyme that accomplishes the amplification (cf. de Franchis et al. 1988).

The recently published findings of Pääbo (Pääbo 1989; Pääbo et al. 1989) document the preservation of DNA in a variety of ancient animal tissue. However, it was found that the DNA was fragmented into smaller units and that the most sophisticated techniques of gene amplification are needed to reconstruct and identify specific genes or fragments of genes. Nevertheless, it appears that the majority of damage to DNA strands comes in the first stage of preservation and stabilisation, not unlike blood proteins. Parallel research has been undertaken on genetic material from plants (Rollo 1985). These studies establish the fact that DNA can survive for long periods if properly stabilised in the first place (Pääbo 1989).

In the course of applying DNA analysis to ancient organic residues, my colleagues and I began with replication of the methods of Pääbo and Hagelberg and have subsequently extracted DNA from mummified tissue of bison dated to some 30,000 years ago (Loy et al. 1990). Using PCR amplification of the extracted DNA (nanogram quantities per milligram of bulk tissue) with primers specific for cattle minisatellite repeats of nuclear DNA, we were able to obtain specific amplifications from *Bison priscus* tissue that has been radiocarbon dated to between 25 and 36,000 years old (J. Dixon pers. comm. 1990). In addition, using male specific probes, we were able to establish that two of the four animals were male (Fig. 2). This identification was later strengthened upon learning that sample 4 (University of Alaska, Fairbanks catalogue V61) was muscle tissue from 'Blue Babe', a bison that was clearly identified as male on the basis of preserved genitalia (Guthrie 1990; J. Dixon pers. comm. 1990). We are currently evaluating newer methods for extracting and manipulating DNA to replace the suitability of certain classes of nuclear DNA repeats which we anticipate will provide finer resolution and compliment the information now becoming available through mitochondrial DNA analysis.
The pursuit of this avenue of inquiry obviously has great promise: previously all genetic information about human or other evolution was gathered at the level of gross morphology or through the analysis of protein molecules. Most research on molecular evolution has relied on using the changes in amino acid composition between species of extant animals where the proteins and their amino acid sequences represent changes at the DNA level. The study of human evolution from bone morphology has proved even more difficult. Changes in bone structure, especially the conformation of the skull, certainly provide evidence of great genetic changes through the history of mankind, but the smaller and more subtle changes within the major different species and subspecies are largely lost with this strategy. If it proves feasible to locate, extract and isolate genes and gene fragments from intact nucleated bone marrow cells, and if these bones can be directly dated, then the way is open for the study of evolution in the most direct and informative way, through fossil DNA itself.

A joint research programme currently in progress between the Department of Prehistory, Research School of Pacific Studies and the Department of Plant Industry, CSIRO, has demonstrated the ability to recover microgram amounts of DNA having fragment sizes ranging from a few hundred base pairs to over 20 kilobases in length. We have used PCR technology to amplify a specific region of human mitochondrial DNA that lies within the 'D loop' region; the same portion as used by Hagelberg, Stonking and others.

My colleagues and I propose (Loy et al. 1991) that PCR technology, when used on ancient organic residues, bone and tissue, constitutes a kind of molecular microscope with a powerful level of resolution. The questions surrounding the analysis of ancient DNA parallel those isolated early in the analysis of blood residues: how widespread and abundant are these genetic fossils; by what means do they survive; and where best to look for them. Using this molecular microscope in conjunction with restriction enzyme mapping, specific hybridisation probes and direct sequencing, there is sufficient technological resolution to unequivocally identify extinct and extant species, thereby determining evolutionary and phylogenetic relationships in revolutionary ways.

Ultimately the application of highly sophisticated technical approaches to the analysis of proteins and nucleic acids and the weight of evidence gained from the past ten years of examination of stone tools for functionally related use residues goes well beyond the initial requirements for the determination of function in stylistic analysis. At the lowest level of discrimination, colourimetrically based and biological stain reagents can be used to identify animal and plant origins of the constituents of organic residues (e.g. proteins, heme units and fats/lipids). Microscopy can be used to identify specific and morphologically distinct structures which again can be used to discriminate between animal and plant origins of the organic material (e.g. cellulosic tissue, starch grains, collagen, muscle tissue and blood cells). In a recent publication, Fullagar (1991) has demonstrated the ability to distinguish between plant and animal origin of silica-mediated polish formation on quartz and obsidian based tools. Nevertheless, it remains to be seen what the effects are in the process of silica-mediated polish in the case where proteins, fats, resins, aminosugars and other organic compounds are also present. These 'low-tech' approaches can serve as screening steps for later analyses, or can serve as the end-point in general functional analyses. The only caveat is that no single colourimetric or staining based test alone can provide unequivocal evidence of the identity of residue type.

The demonstration that a residue is indeed an organic compound associated with tool use must rely upon the identification of specific structural properties (e.g. nucleated cells and hairs) coupled with additional evidence demonstrating the presence of specific molecular compounds (e.g. proteins and nucleic acids); and that those compounds are not part of the natural or cultural soil from which the artefact was removed. The range of methods available for such a demonstration is growing with the increasing sensitivity and specificity of chromatographic, immunological and nucleic acid analytical methods. The investigation of ancient DNA from bone and mummified tissue is now following the pattern of early research into proteinaceous residues; questions of environmental influences upon preservation, the state of degradation and ultimate longevity and understanding of contaminants and the background of extraneous DNA are now the focus. Polymerase Chain Amplification (PCR) and direct sequencing of the base pairs from PCR products are powerful techniques that will ultimately permit another approach to species identification, sexing of the individual, genetic history and evolutionary processes, and the history of certain genetic diseases. At this stage however, the techniques themselves must be adapted from more mainstream applications using modern, purified known materials for use on prehistoric and fossil materials.
Almost 10 years have past since the first steps were taken that led to the discovery of the ubiquitous occurrence of blood and other organic tool use residues on prehistoric stone tools. The implications of such a discovery were evident from the very beginning; implications for the possible range of analytical techniques that might be brought to bear on the residues, and especially for applications to archaeological questions of longstanding that included function, subsistence and economy and evolutionary questions (Loy 1983; Loy and Nelson 1986). Many of the technical methods of analysis have actually been applied to blood residues including determination of species of origin, and using Accelerator Mass spectrometry to obtain the direct dating of both tools and rock art paints; the extraction, amplification and identification of DNA; microscopic research has extended the range of materials, other than blood, that can be identified. The discovery of prehistoric organic residues occurred at what has been both a fortuitous and a frustrating time in the development of biochemical and molecular biological methods. Fortuitous in the sense that there has been an explosion of analytical methods applicable to residue research. The frustrating aspect is common to all new lines of research and reflects variable access to new technology and the fact that experts are scattered throughout the world. Nevertheless, residue analysis has and will continue to be an exciting foray into the frontiers of the newest science and knowledge of the distant past.

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**Table 2**

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Key: = probably PCR: Polymerase Chain Reaction Amplification
X yes Immun.: Immunological method
XI exact IEF: Isoelectric Focussing
X% statistical/counting Hb crystals: Hemoglobin microcrystallization
? exact RBC: Red blood cell
* doubotful IgG with SpA: Immunological identification of Immunoglobulin type G using Staphylococcal Protein A
microscopic


When Europeans first entered the remote Pacific Ocean they were surprised to find that other people had arrived before them. After some centuries of debate about how the Pacific was settled, the literature on the subject is now very large. In all of this discussion most attention has been given to whether voyaging was accidental or deliberate, what routes it took through the regions and islands of the ocean, when it happened, and who did it. In popular belief, the people involved have ranged from mythical navigators able to sail the ocean, discover new land and return home with sailing directions to find it again, to accidental travellers or exiles who made lucky, and unlucky, one-way passages in an ocean they could not map, leaving behind islands to which they could not return.

In 1962, when he was editor of the *Journal of the Polynesian Society*, Jack Golson played a significant role in the developing argument by editing *Polynesian Navigation*, a book described in its subtitle as a symposium on Andrew Sharp's theory of accidental voyages. The first edition in 1962 was a supplement to the *Journal* and the second, in 1963, was *Memoir No. 34* of the Polynesian Society. As a collection of articles written by a number of specialists, it took stock of contemporary evidence and theories of Pacific voyaging and colonisation. In his Foreword to the symposium, Golson noted that Sharp's thesis was not new 'but never previously had such a wealth and variety of evidence been assembled in its support'. Golson also remarked that the symposium 'does present material of importance for future discussions of the course of Oceanic settlement' (1963:9). Afterwards Sharp restated his position in his *Ancient Voyagers in Polynesia* (1963) but, much more importantly, the course of research took some new and constructive directions. In little more than a decade more real progress was made than had been made during the previous century.

THEMES IN THE HISTORY OF THEORIES OF VOYAGING

Observations by European voyagers in the South Pacific more than 200 years ago had recognised many issues on the subject of origins, which have remained until this day. Examples show that it was already realised that the people on many Pacific islands were related to one another and probably had an origin in the west, not the east, in spite of the prevailing easterly winds. It was known that these were interrupted seasonally, and that westerly winds could be used strategically for sailing east; that Polynesian specialists had wide geographical knowledge as well as a detailed understanding of astronomy, tides, weather and other matters; that navigators could maintain their sense of direction at sea, as shown by Tupai a on his voyage with Cook; that the means of colonisation of Pacific islands could range from systematic and intentional voyaging to unintended and largely undirected accidents (e.g. Banks 1962; Beaglehole 1967, 1968; Cook and King 1785; Dening 1963).

Information and theories accumulated through the 19th century. The possibility of an American origin was raised, not for the first time, by Ellis, a Hawaiian missionary (Ellis 1831) but John Williams, who was head of the LMS mission at Raiatea in the Society Islands, from 1817-39, explicitly disagreed and it is interesting to note some details of his argument. He was 'convinced ... of the practicality' of west to east colonisation (1837:512). He acknowledged that among the objections to an Asian origin were 'the prevalence of the easterly trade winds within the tropics', and the great distance from the Malay coast, and that 'it is thought to have been impossible for the natives to perform such a voyage with their vessels and imperfect knowledge of navigation' (1837:506). But he suggested 'if we can show that such a voyage may be performed by very short stages, the difficulty will disappear' (Williams 1837:506). On the question of the trade winds, he said:

''after some observation, I am satisfied that the direction of the wind is not so uniform as to prevent the Malays from reaching the various islands and groups, in which their descendants are, I believe, now found (Williams 1837:509).''

He continues with an explicit account of westerly winds which were known, including their Tahitian names, the months they blew, and their
Irwin described various of his own voyages including one from Rarotonga to Tahiti: 'although it blows from the E. almost constantly in those latitudes, we were favoured, during our voyage of 800 miles, with a fair wind' – which is described as light and the sea smooth (Williams 1837:169).

At the same time, however, evidence continued to grow of voyages of a different kind. In 1797, Wilson of the Duff made a very explicit case about migrations which occurred under stress of weather when canoes were driven from island to island and from one group to another (Parsonson 1963:18). Many others mentioned involuntary voyages of an unsophisticated kind as well as one-way voyages of exile. In 1866, W. Pritchard said:

It cannot be doubted that the early migrations of the ancestors of these islanders were involuntary rather than the result of roving dispositions, or of the pressure of limited and over-populated homes; that, in fact, they were blown away from their earlier homes in their frail canoes (Pritchard 1866:402).

Pritchard, however, noticed an anomaly in that the pattern of recorded drift voyages was from east to west (1866:402) which, of course, was against the direction of colonisation. J.D. Lang, who sailed in the Pacific in the mid-19th century, emphasised accidental storm drifts and believed Polynesians might have reached America; however, he did allow adventurous spirits and forced voyages of exile as possibilities, as well (Lang 1877).

Towards the end of the century the theory of deliberate voyaging became more acceptable. For the origins of Polynesians, most writers looked to the west to Asia and some looked even beyond. Formander in Hawaii, and Tregear and Smith in New Zealand developed grandiose migration theories which drew uncritically on voyaging traditions (Finney 1979a:323). However, not all were willing to take this kind of view (Sorrenson 1979:45).

This century has continued to favour a western origin with Heyerdahl (1952) providing another American diversion. It began with the view held by scholars such as Smith (1921) and Best (1923), that the Pacific was explored by navigators able to discover new land and return to their homes with sailing directions which others could follow. Buck's (1938) view was similar but his idea of voyaging more realistic, and he allowed some centuries for settlement to take place.

Andrew Sharp overreacted to the excesses of this position by arguing that voyaging was one-way and discoveries were accidental (1957, 1963). In many respects his argument restated an earlier position. While there was ethnographic evidence for return voyaging of up to about 300 miles in a few surviving contact areas in Micronesia, Fiji/West Polynesia and between the Society and northwestern Tuamotu Islands, Sharp believed that prehistoric navigation methods were not good enough to allow returns over greater distances. A considerable debate followed. Some authorities like Hilder (1963) and Akerblom (1968) took a position similar to Sharp's, while others, such as Heyen (1963), took a more positive stance. This is where Golson's symposium (1963) played its part by gathering contributions on the history of ideas about colonisation (Parsonson 1963), the feasibility of non-instrument navigation (Heyen 1963; Hilder 1963), the geographical knowledge of Polynesians (Dening 1963) and an early experimental study of the sailing characteristics of model Oceanic canoes (Bechtol 1963).

Two major developments followed and these were already discernible in Golson's Foreword to the third edition of his symposium published in 1972. The first was a computer simulation by Leison et al. (1973), which took account of recorded weather conditions, and persuaded most scholars that the major colonising voyages of the Pacific could not have occurred by drift, even if the methods which had been used were unknown. At the same time other scholars including Alkire (1965), Gladwin (1970), Lewis (1972), Finney (1977, 1979b), Siers (1977) and others since, have re-established the reputation of ancient voyagers by historical and ethnographic studies, and many experimental voyages in replica canoes and other craft have made successful landfalls (although not all). This position does not exclude the possibility of some accidental colonisation, but no longer sees it as necessary.

The modern view of the first settlers is that they were competent sailors. However some prehistorians believe they may have suffered high losses at sea, that their range of return voyaging was restricted, that they may have harboured unrealistic ideas about their island world, and may have been transported across parts of the ocean by conditions beyond their understanding or control. These views follow from continued uncertainty about the methods of deep-sea exploration and the geographical circumstances of colonisation.

Until recently there has been no explicit navigational theory of colonisation (Irwin 1989; Irwin et al. 1990). Instead, theories of colonisation have usually been rationalised from patterns seen among modern Pacific peoples in...
respects of language, biology and culture as if these were largely the product of diversification which accompanied colonisation. However, it is now clear that they are not (Irwin 1990). There is still much to be learned about the navigational methods of the first colonists which must have been different in important respects from the traditional techniques described since the time of European contact. By then the islands of the Pacific had been found and mentally-mapped.

NAVIGATION

We know colonisation was deliberate because explorers took with them the plants and animals, women and men necessary to establish viable settlements. As for navigation, computer simulation (Levison et al. 1973) has shown that the major voyages of settlement were the result of some kind of directed navigation. Detailed ethnographic research on traditional Pacific canoes and navigation which included experimental voyages at sea established that voyaging canoes were large, fast and safe with double hulls (like catamarans), or with single hull and outrigger. Practical skills widespread in the Pacific were to steer an accurate course at sea, to maintain a running fix of position by dead-reckoning, and to detect destination islands from beyond sighting range by the use of sea-signs and, where possible, create broad overlapping target-island screens.

Esoteric skills most probably included estimating a conceptual equivalent of latitude by the night sky without instruments. While longitude could not be controlled as such without time-pieces, it is possible and likely that the positions of islands was fixed by a method which combined elements of both astronomical and geographical knowledge (Irwin 1989). Island locations could be learned, in part, by dead-reckoning from elsewhere. A fairly precise estimate of latitude is available in the configuration of the sky. The directions to and from prior places can be known by the whole gamut of course-finders, such as horizon stars, while the distance between them is given by the speed and elapsed time of a voyage, all of which could be checked when a track was re-crossed.

Sailing between known and unknown islands has its risks, but sailing into empty ocean is fatal.
Some theories of Pacific colonisation prefer many explorers to die at sea, but there is nothing to show they were unconcerned about their lives. Without a doubt, it is safest to sail first in the direction which is normally upwind because one can expect the easiest trip back. The hard way is really the easy or safe way and this simple paradox is one of the keys to explaining the trajectory of human settlement. What has always seemed most intriguing about the settlement of the Pacific is that it went against the prevailing winds but now it seems that, for people interested in staying alive, it was the very ease of return which made sailing upwind possible. Practically every radiocarbon date in the remote Pacific supports the view that colonisation went first against the prevailing winds and only then across and down them.

Sailing upwind also provides the means to find the way home by latitude sailing. This was probably developed at some point in the settlement of the Pacific and simply involves returning to the latitude of one’s origin island, while still upwind of it, and then running with the wind along the latitude. Experimental evidence shows that the error in estimating latitude from the stars, without instruments, is matched by the ability to detect the presence of land from offshore.

**SOME NAVIGATIONAL ISSUES IN LONG-DISTANCE EXPLORATION**

Long voyages were made at some risk but evidently not successfully until there was sufficient experience, knowledge and skill. Voyages beyond the tropics, especially those downwind in high latitudes were made last of all. Islands both hard to reach and return from, can be expected to have been settled late and to show the influence of remoteness in their subsequent histories.

It is important to be clear about the factors which made for delay. As Lewis explains: (1972:223)

Navigational accuracy is not a function of the length of voyage (if anything the longer passages providing the greater opportunity for random sea effects and judgement errors to cancel out). Thus if a 15° arc of accuracy, for example, can be attained over 300 miles, it is just as navigationally feasible over 1000. The special problems of the longer journey concern such factors as food supply, manpower, motivation, and the strength of the vessel – not navigation.

Lewis (1972:158) says of modern Micronesian voyaging that the longer voyages are regarded as tests of endurance rather than especially difficult navigational exercises, a point also made by Gladwin (1970:61). In fact, Lewis says that the length of a voyage is regarded as less important than the size of the island target screen. Making a landfall was the vital thing in exploration too even if this was only back at the point of departure. Thus, ease of return affected the order of long-distance settlement and the elapsed time taken to settle various parts of the Pacific involved other variables, including accessibility of targets, the area of ocean to be searched, latitude and weather patterns, and development of suitable sailing strategies. The archaeological evidence implies these were all systematically related.

**Sailing by reaching and running**

The simplest case of a return voyage across a steady wind between two points, requires averaging a direction of 90° or less to the wind over both legs. Since traditional canoes could make good a course of about 75° this does not appear to be a problem but, allowing for adverse wind shifts and currents, being becalmed in a current or caught in bad weather, in practice it is not so easy to maintain a course which requires keeping the wind on the beam or ahead of it. It is a fair inference that voyages across the wind became more sure when expanding geographical knowledge allowed a return to land downwind of the point of origin. In theory and practice, cross-wind voyages around a triangular course are easier than out and back between two points because they need less on-the-wind work. As an extension of this reasoning, downwind voyages into the unknown involve the rather desperate risk of being unable to return over the same track in the event of not finding land, or else an intention to return by a different route through a more favourable weather system, or to a different place, or both. In other words, downwind exploration could be as much a matter of round-trips as direct return-trips and to be successful would require a sophisticated knowledge of both navigation and geography. The circumstances of exploration changed in the remote Pacific as knowledge grew, and the range of feasible options increased. Increasing experience and skill were needed to manage the long exploratory probes eventually made into the difficult higher-latitude extremities of Polynesia, and to South America. There has been a long and rather fruitless debate between one-way and two-way voyages, but it now seems that we have to allow for various kinds of 'three-way' voyages.

**Return voyaging**

In 1963 Andrew Sharp observed that:  
The idea of systematic exploration involves the presumption that explorers were prepared to go
twice as far as any island they happened to find, and to do so many times without success, for an explorer cannot hope to find new land more than very occasionally, if at all. (Sharp 1963:7-8)

Sharp saw this issue very clearly, especially as it relates to more distant and difficult destinations, but because of his negative view of prehistoric navigation, he thought a theory of one-way voyages of simultaneous exploration and colonisation was more economic and realistic (1963:74). The tide of information and opinion has moved against many of Sharp's views but his point that systematic exploration requires returns still deserves respect. We can expect many return voyages of non-discovery when people were probing the margins of Polynesia. The more difficult it became to find the remaining land, and the more empty ocean there was to traverse, the more they would become conventional.

Moreover, we should remember that return voyages could be made just as easily in the event of finding land or not. There are several reasons for thinking this happened. One relates to finding the position of new-found land. Interruptions to the trade winds useful for exploration are commonly accompanied by deteriorating weather and worse conditions for celestial navigation and dead-reckoning. South of the tropics the passage of cold fronts would make things even harder. On the other hand, return voyages in or near the tropics, instead of leaving during unsettled weather, could head off in steady conditions to intercept and run along the latitude of their starting island. Discoveries can certainly be made by one-way voyages but their positions are known better when the track is covered again. To be fair to Sharp, when he said he did not believe that people returned, already 'having noted their courses on outward voyages to distant islands which they did not know existed' (1963:74) without instruments, he had half a point. The other half, which he did not see, was that the return could locate the discovery more securely than the outward voyage could.

This raises the question of the general structure of the two-way voyage. To Sharp (1963) and others, it contained these elements:
1. discovering a new island
2. fixing its position
3. returning home
4. sailing again to the new island to demonstrate (mainly to modern prehistorians) the ability to find it again.

In other words, the structure was A to B to A and then back to B again. However, a more likely sequence is actually:
1. discovering a new island,
2. establishing some estimate of its position,
3. returning home and securing the position of the new land. This structure is A to B to A and any further outward leg was simply a matter of choice, not necessity.

The structure of the 'three-way' voyage is from A to B to C to A, the essential point being it is normally not feasible to return directly from B to A. There are many variations; for example, in a voyage of exploration if C was already known, B need not be, but turning towards C was essential for survival. The final stage from C to A would be optional. Indirect voyages around known tracks were probably necessary, as well.

Multiple settlement, tradition and
a distinction between discovery and settlement

One of the most compelling reasons for the view that the colonisation of islands involved multiple settlement is statistical (Law 1988). Briefly, he took the case of Hawaii, Easter Island and New Zealand and assessed the probability of all receiving a first voyage before any received a second or third voyage and showed that multiple settlement of one or two islands before all were reached was the likely solution, all other things being equal. However, given the varying difficulty of reaching different islands, we can be fairly confident of the multiple settlement of many, and a body of archaeological and linguistic evidence already points to examples.

These issues bring us close to oral traditions of Polynesian settlement which have been often misused or maligned during more than a century of European scholarship. To quote Sharp once more (1963:15):

Over the past eighty years most people have imagined that the farther islands of Polynesia were discovered by prehistoric navigators who sailed back to their home islands and organized colonizing expeditions to their discoveries.

Although such scholars often did describe return voyages in unrealistic terms and with unnecessary cultural associations, they are navigationally plausible. As mentioned already, if we are right to suppose that mounting a colonising expedition to the far reaches of Polynesia was a costly venture for a community, especially in the knowledge that the chance of finding land was slight, it might be simpler for explorers not colonists, to go first. In fact, most colonising voyages could have been to already-explored destinations. The point strikes a chord with some oral traditions and it would be of interest to see what insights a careful reanalysis of them might bring.

There are other reasons for making a distinction between discovery and settlement.
One is that the arithmetic of human reproduction says that a founder population cannot send off another in less than a generation, while the navigational arts of deep-sea sailing are founded in knowledge and extensive practical experience which had to be used by every passing generation. Only a minority of voyages could be expected to show up in the archaeological record as a new colony. Another insight into the amount of unsuccessful exploration comes from the statistical requirement to explore much largely-empty ocean, to find what habitable islands there were, especially in marginal Polynesia.

Colonisation was probably preceded by a frontier of exploration. Many voyages could have been made to established settlements and new islands did not have to be settled as they were found. This second possibility may account for observed anomalies and apparent delays in colonisation. It might also account for evidence suggesting the fleeting use of islands, some time before settlement is archaeologically assured. For example, Kirch and Yen (1982:312-4) estimate the settlement of Tikopia at c.900 BC, but report a radiocarbon date of mid-second millennium BC age. The explanation they prefer is that there was sporadic human activity on the island before permanent settlement.

In the early days we can envisage that only a small number of voyages were elaborately equipped for colonisation and those that were could have sailed directly towards known destinations. Moreover, establishing a new colony could have taken several trips over some elapsed time.

On all sea passages canoes would presumably have carried stored food, but there was a relative abundance of unexploited wild food on empty islands which were already known, and on those which were newly-found, including turtles, fish, shellfish and endemic birds. Today, these items still provide a reason for visits to satellite and empty islands. They were a resource for oceanic exploration as well as a shock-absorber for founder populations in what Kirch (1988:252) refers to as a colonisation-agriculture transfer stage.

Many general issues of long-distance voyaging can be applied to specific cases.

THE COLONISATION OF HAWAII, NEW ZEALAND AND THEIR NEIGHBOURS

North to Hawaii

A triangular Marquesas-Hawaii-Tahiti track is one long proposed in the literature. Rodman's theory (1927) called for determining latitude by measuring the altitude of Polaris with a 'sacred calabash' which held water to provide an artificial horizon and had a sighting aperture to check the angle of the star, but other aspects of his theory were more plausible. Makemson (1941) suggested the use of the star Aldebaran as a zenith star for Hawaii, but it has taken modern experiments to show precisely how zenith and other stars can actually be used for telling latitude. Akerblom (1968) followed Sharp (1963) in seeing voyages to Hawaii as one-way with no control of longitude 'and probably none of latitude,' either but accepted that the prevailing winds were relatively favourable for voyages in both directions and that the islands formed blocks which could compensate for some error in longitude (Akerblom 1968:81). Levison et al. (1973:53) concluded, from simulation, that people intended to go to Hawaii and Lewis (1972:302) agreed saying:

> it is in precisely this cross-wind direction that an explorer would choose to sail, so as to penetrate far into the unknown while, at the same time, being sure of a fair beam wind to speed his return.

Both Hawaii and the Societies/Tuamotus present substantial targets; Lewis thought the Tahiti-Hawaii leg not very difficult navigationally and that it might take three to four weeks to sail. As for a voyage from the Marquesas, Lewis thought that would have to be one-way, as they lie so far upwind, unless the return was made to Tahiti (Fig. 2).

There is a world of difference between predicting and doing as shown by the building of the Polynesian replica canoe Hokule'a, and assembling the distinguished navigators who sailed her over the Hawaii-Tahiti track in 1976 (Finney 1977, 1979b; Finney et al. 1986) which she later re-crossed. Since then she has voyaged to New Zealand (Babayyan et al. 1987), north to West Polynesia and from there across to East Polynesia (Finney et al. 1989). These voyages were not intended to duplicate conditions of first discovery but showed their feasibility and, together with experiments by Lewis (1972), Siets (1977) and others, shifted the issue of voyaging in Pacific canoes from the realm of theory to experience. They also provided remarkable insights into methods of navigation which could have been used by the first explorers and, in particular, measuring latitude, dead-reckoning, and the strategic use of predictable weather, which are crucial for understanding how explorers could have found islands and made their way home.

Hawaii, which straddles the Tropic of Cancer, presented diverse landforms and a 'graded set of opportunities for agricultural Polynesians' (Kirch
EXPERIMENT NUMBER: HA26
VOYAGES TOOK PLACE IN JANUARY
THE ORIGIN OF THE VOYAGES WAS HAWAII
CANOES USED STRATEGY NUMBER: 4
ANGLE OF SEARCH WAS: 150.0

LENGTH VOYAGE: 72
TACKING METHOD: 2
SPEED OF CANOES: 4.00
TURNING DAY: 20
HORIZ. FRACTION: 0.30
NO. OF VOYAGES: 10

Figure 2
Links between central East Polynesia, and Hawaii were probably made by 'three-way' voyages. A likely starting point for Hawaii was in the Marquesas, furthest upwind. However, the tracks of 10 canoes as generated by a computer simulation are unable to maintain a course to the Marquesas when they reach the southeasterly trade winds. A landfall in the Tuamotus or Societies is more feasible.

1985:32). There is some archaeological evidence for settlement as early as the fourth to the fifth centuries AD and by then there may have been such different sites as the rich Pu'u 'Ali'i (H1) fishing camp in an arid leeward region of South Point, Hawaii, with no permanent streams and low agricultural potential but adjacent to deep-sea fishing grounds, and the Bellows Site (O18) in a fertile and well-watered area of windward O'a hu, with its evidence of dwellings, a pavement and an absence of typologically-distinct Archaic artefact forms. Kirch (1986) suggests the Marquesas as a plausible source, but that sampling considerations could permit suitable early sites elsewhere in central East Polynesia.

The earliest known sites in Hawaii suggest it was settled with imported food crops, and the pig, dog and fowl, but the very earliest sites are probably still unknown. In fact it is possible that the first Polynesian discoverers of Hawaii did not carry these items on board, for reasons discussed already, but the first colonisers did so. However, it is not very likely that these two events, if they were different, could or will be distinguished archaeologically.

South to New Zealand
Reaching New Zealand involved a more complex voyage across the trade winds, through a belt of variables to the latitude of prevailing westerlies. These weather systems shift south in summer, north again in winter, and in summer variables reach New Zealand. The settlement of New Zealand is often spoken of in the same breath as other islands of Polynesia but, navigationally, it was very different. In the matter of age, this gives a good argument for New Zealand to be late, quite apart from the internal evidence which currently says the same thing.

Figure 3 shows detail of the belt of variables and westerlies south of the tropics where there is a regular passage of high and low pressure sy-
systems, from west to east, with ridges extending from highs, and troughs from lows and meridional fronts. The figure shows also that these wind systems are rotational – the lows clockwise and the highs anticlockwise (anticyclonic) – in the southern hemisphere.

With regard to sea routes to New Zealand, there are a number of ways of sailing south. One method predicted by Heyen (1963:74) is to sail close to the rhumbline course using easterly tail winds blowing across the top of summer high pressure systems as shown in Figure 3. It is fairly well known among sailors in this part of the world that the best time to do this is in early summer, especially November, before the start of the cyclone season and *Hokule'a*, by good management and good luck, sailed to New Zealand like this in November 1985 (Babayan et al. 1987). Yet at almost the same time Greg Brightwell and his crew on the canoe *Hawaiki Nui* struck more difficult conditions on her voyage south from the Cook Islands. In November-December 1965, David Lewis made an experimental voyage to New Zealand navigating the catamaran *Rehu Moana* without instruments, and he recorded 64 wind shifts in a month-long voyage to New Zealand through variables (Lewis 1972:3-5, 93). On a return passage from Australia in my yacht *Rhumbline* in November-December 1986, we experienced fairly steady easterly or northeasterly winds for 11 days in a row which originated from a high pressure system to the east of us and, while these were headwinds for us, they would have made for good sailing from Polynesia. The early summer high pressure system route to New Zealand is a good one, but it works best with foreknowledge, which the first explorers would not have had, as such.

Another way of getting south is by the northerlies on the leading edge of an advancing front and behind a high, where the two systems rotate against one another and the pressure gradient can cause strong winds. This happens regularly throughout the year sometimes as often as once a week and seems quite a likely explanation from my own experience. We can imagine that, sooner or later, a canoe crossed the trades, into the variables and arrived at a suitable position at the right time to pick up freshening northerlies approximately 200 miles ahead of a typical front and then flew south before them, initially from choice and then perhaps even gale-driven for a day or a night as the front closed in. After the front passed through the canoe would be well-placed to make a landfall in clearing weather. The most likely place would be on the east coast of Northland. Alternatively, because there is a strong north-easterly component in the gales which cross the northern North Island, a canoe caught just north of New Zealand could be blown west of North Cape and, in the moderating westerlies behind the gale, make its landfall on the
northwest coast. Landings south of East Cape or Cape Egmont would almost certainly be secondary ones from further north, even though their impact might be the same.

To sail so far south where the weather is often bad seems very deliberate but there are various sailing directions to New Zealand in the settlement traditions including ones which specify the easiest period from the end of November to February (Best 1923:28-9; Smith 1921:216), however, these may have been affected by a century of European navigation. While Sharp (1963:116) reports flotsam from the Pacific on Ninety Mile Beach in the far north he found no reports of historic drifts either to or from New Zealand, which is a measure of its isolation in prehistory. Why the first canoes came this way is a matter of conjecture but it is quite possible, as Lewis says (1972:171-2), that migrating birds had been noticed, again as mentioned in traditions. The long-tailed cuckoo comes from tropical Polynesia to New Zealand in September and shearwaters fly south in October. The golden plover goes from Tahiti northward. People could infer that land lay in a particular direction although at an unknown distance.

In the wider context of Polynesia, Hawaii provides a maximum age for New Zealand because Hawaii is the easier voyage. To reach it from central East Polynesia meant sailing north out of the southeast trades, through the doldrums and then on across the northeast trades, whereas to reach New Zealand involved a more complex voyage south of the trades to the variables and westerlies. The Big Island of Hawaii straddles 20° N. Latitude whereas Easter Island and Rapa, at contact the southernmost settled islands at the fringes of the tropics, are 27° S. or more than 400 nautical miles further from the equator, than Hawaii is. The northern tip of New Zealand at approximately 34° S. is still another 400 sea miles south of the closest inhabited land. If Hawaii was settled around 300-400 AD New Zealand, so much further from the equator, might well have been substantially later.

High latitude appears to correlate with late settlement and the Chathams provide a minimum age for New Zealand, lying 550 sea miles below the latitude of North Cape in a dangerous downwind direction from New Zealand, the closest land and most likely source. So far, there are no archaeologically-controlled dates for the Chathams older than 1500 AD. Navigational and geographical factors suggest that the first settlement of New Zealand should fall somewhere in the interval between the settlement of Hawaii and the Chathams. Orthodox archaeological opinion, as well as versions of tradition, places that event rather neatly, in terms of this argument, some 1000 years ago.

The issue of back-dating the settlement of East Polynesia (Irwin 1981; Kirch 1986) has been invoked for New Zealand recently by Sutton (1987) who postulates a date of 0-500 AD, but while the gap in the evidence between West and East Polynesia has closed a little, the same is not demonstrated for New Zealand and the arguments which suggested East Polynesia should be earlier than previously thought, do not in turn necessarily make New Zealand earlier. Much of Sutton's argument for early settlement rests not on archaeological evidence, but on his (1987) reinterpretation of palynological and geomorphological evidence. It has not however found support from specialists in those fields (Enright and Osborne 1988).

Rather less secure than the age of New Zealand's settlement is the origin of its founder population in Polynesia. The Society Islands have been considered a possible source because of the material recovered from the Vaito'otia-Fa'ahia site on Huahine and the Maupiti burial site which has striking similarities (and some differences) to New Zealand sites such as Wairau Bar. The late first millennium AD dates from the Societies sites fit as well. However, Davidson points out other possible sources, which are archaeologically less well known (1984:94-6). Walter (1988) reports that recent excavations in the Southern Cooks support their role as a possible source of New Zealand settlement. During the 800-1200 AD period there were a number of village-sized communities in the group; some were engaged in offshore communication and trade and he notes their proximity to New Zealand. Comparative ethnology and language have always invoked close links between the Southern Cooks and New Zealand (Walter 1988), and there may be a biological link with respect to New Zealand DNA (Hill and Serjeantson 1989). However, a recent review of other biological affinities of New Zealand Maori does not necessarily point in the same direction, although a general East Polynesian origin is indicated by the evidence. This is in spite of the fact that New Zealand lies closer geographically to islands in Melanesia, Fiji and West Polynesia, than to the nearest part of East Polynesia. It is also much closer to the source of Lapita than any other part of East Polynesia. Were it not for cultural and navigational considerations, this would be a real anomaly.

New Zealand is such a large archipelago it would have been hit again and again, almost as if for the first time, as maritime settlers shifted on
the coast, and this model has appeal for the North Island where the evidence is too sketchy to exclude it. The associated archaeological sites should be found near to where a canoe could be beached in shelter and with good access to a range of resources.

Given the general case which has been made for return voyaging and multiple settlement, we can conceive of one or more colonising groups operating in the early years of settlement and very possibly supplemented by a few others in the first few centuries. At some quite early stage their activities would become archaeologically visible, and sites showing the movement of distinctive industrial stone could signal their presence much as pottery does more conspicuously in other parts of Oceania. At present, archaeological evidence does not sustain the idea of a long pre-visibility tail to the prehistoric population curve and, in particular, the striking evidence of bird extinctions is itself an implicit argument against a 'pre-Archaic' New Zealand.

It is a nonsense to regard New Zealand as being settled by one event, and there is the possibility of a few last 'Hawaikis' on the New Zealand coast to which people returned in an early period of exploration and settlement. This could include places able to keep alive the climatically-fragile introduced tropical food plants which were lost or not taken on forays elsewhere, and particularly to the cooler south. Many possibilities come to mind and these include the larger northern offshore islands. Great Mercury, for instance, has a mild climate (Edson 1973), an all-weather harbour offshore from the exposed beaches of Kuaotunu, a major focus of early settlement and an important stone source. There is abundant secondarily-processed Tahanga basalt to suggest Great Mercury's role in the interaction system of an early time.

If the conventional settlement model turns out to be wrong, New Zealand could have been settled as early as Lapita, however, it appears that those seaborne explorers had reasons of their own not to search in this direction.

A return voyage to East Polynesia

A voyage to East Polynesia from New Zealand might have been a rather more difficult proposition than the outward one (Lewis 1972:305-6). Variables and westerlies could carry a canoe north some way, but then to make casting on a long voyage against the trade winds would be as hard as it usually was to sail that way. One consequence is that voyages from New Zealand could be expected to reach islands to the north, especially the Kermadecs and even Norfolk which, while small, are high and close to New Zealand while north of them again, lie islands of Melanesia or West Polynesia. Heyen (1963) suggested that canoes going from New Zealand to Tahiti could sail east at the latitude of the westerlies and then turn north, however, this would raise the difficult problem of knowing when to turn.

The cultural divergence of New Zealand from its East Polynesian relatives makes sense given its distance and the quite difficult return.

THE KERMADECS, NORFOLK, THE CHATHAMS AND LINE ISLANDS

With the exception of the Chaths, these islands were among the 'mystery' islands of Polynesia having been reached in prehistory but abandoned by historic times (Bellwood 1978). They contribute to the case made for Hawaii and New Zealand's settlement because they stand to intercept traffic on its way in and out. Norfolk is 400 nautical miles northwest of New Zealand approximately half-way to New Caledonia, the nearest major island and also the closest part of Melanesia. The Kermadecs are centred some 500 nautical miles to the northeast, half-way to Tonga, the nearest part of tropical Polynesia. The Chaths lie some 350 sea miles southeast of the southern North Island of New Zealand but at the latitude of the central South Island. The Line Islands are spread, in clusters, between central East Polynesia and Hawaii and several have evidence of interest.

Navigational predictions

(1) The Kermadecs and Norfolk

Norfolk Island is closer to New Caledonia than any part of Island Melanesia is to Fiji. The Kermadecs are closer to Tonga than Tonga is to East Polynesia. Both Norfolk and the Kermadecs, while fairly small targets, are high and often easily seen from sea. If the colonisation of the Pacific was undirected they were within easy range of Lapita or early post-Lapita settlement.

However, in terms of my general voyaging argument, these islands would not have been settled until crosswind voyages were made from East Polynesia into sub-tropical latitudes. Norfolk and the Kermadecs are still well north of New Zealand but, because they present much smaller targets, would not necessarily have been settled first and could have been reached almost as easily on a navigated voyage from New
Voyaging

Zealand as from anywhere else. Of all the islands to the north, the Kermadecs and Norfolk are the ones easiest to reach from New Zealand; for instance, the Kermadecs are less than halfway to the Southern Cooks, the closest part of East Polynesia, and it has been noted already that canoes returning north from New Zealand could have found it difficult to sail east in settled trade wind weather and could easily have found themselves west enough to find these islands. One would expect then, that the Kermadecs and Norfolk should have been settled at much the same time as New Zealand, or not long after, and that they could show signs of multiple contacts, from New Zealand and from elsewhere in East Polynesia. We might expect the Kermadecs too, from their more easterly position to have been the more likely stepping stone to or from Polynesia, if one existed, than Norfolk which lies more isolated in the west. Further, because we know that New Zealand was effectively cut off from the rest of Polynesia by European times, and that both Norfolk and the Kermadecs had no human inhabitants then, one would expect archaeological evidence of contact with these small islands to be earlier rather than later in their prehistories, in the increasing isolation of this part of the Pacific.

(2) A model for the Chathams

The Chathams are a different case. At more than 43°S, they mark open-ocean voyages at the highest-known latitude sailed by Polynesians, made in the westerly wind belt and exposed to regular bad weather of frontal low pressure systems. It is not difficult to suffer from exposure while ocean sailing in much gentler latitudes than this and it does not require particularly bad weather to make conditions very wet, cold and unpleasant. Even allowing the possibility of past variations in weather systems, it is difficult to conceive of conditions being much different. In terms of my argument, this voyage should have been the last successful voyage of discovery in Polynesia. It was more difficult than reaching South America and very much more difficult than getting to Australia.

Because few sailors could, or willingly would, push into westerly headwinds at high latitudes (although Captain Bligh tried to unsuccessfully on his way to the mutiny on the Bounty), New Zealand is the only obvious origin for Chatham Islanders. The probabilities are that a number of voyages left New Zealand for one or more to have hit land. On the other hand, from a sailing point of view, it is unlikely that many navigated voyages ever returned to New Zealand from the Chathams, unless it was by a reach across the southwesterlies to the North Island. In fact the tropics may have been more accessible than New Zealand and any canoe which missed the Chathams and sailed very far past them would have to head north to survive.

(3) The Line Islands

The Central and Northern Line groups, in particular, are to Hawaiian settlement what Norfolk and the Kermadecs are to New Zealand. In terms of expected settlement dates, Hawaii is a large target which stands at a great distance whereas the Lines are small low targets at various shorter distances so, again, there is really little to choose between the two in time. The Lines lie generally to leeward of the optimal track to and from Hawaii. One or more of them could have been encountered on a search directed west of the track needed to reach Hawaii or, quite possibly, they could have been found when a canoe on its way to or from Hawaii was unable to hold to the optimal windward track.

Archaeological evidence

(1) The Kermadecs and Norfolk

The archaeological evidence from the Kermadecs and Norfolk, although sparse and patchy, does not contradict the voyaging argument and could not be expected to appear to conform well by chance. A summary of the evidence from the Kermadecs is provided by Anderson (1980). Raoul, the largest island, is the only one on which he would expect prehistoric occupation; it is forested and generally 'precipitous and ravin ed' (Anderson 1980:131). The Low Flat site, at a beach facing north, is quite extensive and in one area has two levels dated to approximately 1030 BP and 620 BP respectively. There is evidence of the dog and the Polynesian rat (Rattus exulans), but not the pig or fowl, which is the same pattern as for New Zealand. Anderson concludes that Raoul was occupied by people from central East Polynesia at the end of the first millennium AD, that it is hard to tell whether occupation was continuous after that, but that the radiocarbon dates and styles of adze types and a bone pendant all suggest two periods of occupation with the later one also originating from central East Polynesia or from New Zealand. Anderson notes the canoe Aotea, of New Zealand fame, arrived at the Kermadecs in legend, and also the correspondence of the dates with 'major events in prevailing versions of Maori settlement legends' which invite speculation.
but all that can be safely asserted is that the archaeological evidence comfortably provides a cultural stepping stone between early East Polynesia and New Zealand, without prejudice as to which way, or how often, it might have been used (Anderson 1980:140).

This conclusion fits voyaging expectations neatly.

Specht (1984) reviewed the miscellaneous information on Norfolk Island prehistory although, as yet, there has been no excavation of in situ archaeological deposits. One group of adzes found at Slaughter Bay suggests an East Polynesian origin. Another group of Duff Type 2B (Duff 1956), a common New Zealand form but not one generally associated with early prehistory, was found in other parts of Norfolk, and made of a kind of stone which could be, but may not be, from the South Island of New Zealand. Groves of bananas found growing by Captain King in 1788, were evidently not planted by Captain Cook who rediscovered Norfolk in 1774 some time after Polynesians abandoned it. Specht's conclusion is that a likely initial East Polynesian source of settlers could have been the Cooks, Societies or New Zealand in the period 1000-1400 AD, and New Zealand a second possible source after c.1400 AD. These dates are obviously rough estimates but find some support in evidence of the age of the imported Polynesian rat (Meredith et al. 1985) which is widely regarded as a commensal of humans and was distributed throughout the Pacific by voyaging canoes. In Unit C4 of a test excavation at Cemetery Beach, four radiocarbon dates cluster around 800-900 BP overlying bones of *Rattus exulans* and providing a minimum age for them while, in the upper part of the same unit, evidence for extensive burning is possibly the result of forest clearance by humans. With respect to origins, Specht (1984) refers to some of Levison et al's previously unpublished data which showed 65 landings on Norfolk from more than 8000 simulated drift voyages, 22 coming from New Zealand, 41 from the Kermadecs and two from Rapa. This information sits well enough with the archaeological conclusions, and confirms what has been said about conditions for voyaging in the general area, but the likelihood that the actual voyages were by drift is low, which is a conclusion Levison et al. (1973) themselves subscribe to.

It is interesting to note the parallel structure of the tentative prehistories of the Kermadecs and Norfolk which include initial settlement perhaps a thousand years ago and in the same general time range as New Zealand, multiple settlement from East Polynesia over some centuries in which New Zealand plays some part and finally, abandonment or extinction at some time in the last 400-500 years.

(2) *The Chathams*

The situation for the Chathams is more contentious. Sutton argues that one-way drift voyages were the most likely form of first settlement (1980:70; 1982:167) and, while possible, it is inconsistent with the arguments regarding order of settlement, etc., presented here. He estimates the time of initial settlement between 1000-1200 AD (1980:87) or 800-1000 AD (1982:167) and suggests there were no further arrivals after 1400 AD (1982:167). However, both he (1980:74) and Davidson (1984:24) note that no excavated sites of this early period exist. McFadden, on the other hand, from a study of coastal geomorphology, believes the Chathams were still unoccupied 500 years ago (pers. comm. 1988) and first settled after then. The voyaging situation favours some elapsed time between the first settlement of New Zealand and the Chathams and, whatever the position, as yet there are no published radiocarbon dates from controlled archaeological excavations older than the 16th century AD.

Clark (1988) confirms an earlier suggestion by Bruce Biggs, that the Moriori language was most similar to South Island or eastern North Island dialects of Maori. Chatham artefact collections from sites at Owenga, Kaingaroa and Pitt Island are said to duplicate ornament forms found at Wairau Bar in New Zealand which is dated to approximately 600-700 BP on collagen (Caughley 1988) as well as adze forms shared with early New Zealand (Sutton 1980:74). This material supports a New Zealand origin, but does not supply a precise timetable for its arrival because such items are not closely dated in New Zealand and the Chathams material inventory includes other items which are not regarded as particularly early. The only human-introduced animal was the Polynesian rat and, unlike New Zealand, the pig and fowl are unknown. The pattern of evidence is consistent with the suggestion that the Chathams were settled after New Zealand and probably from New Zealand on one or more occasions; my estimate is after approximately 1300 AD. A hunting and gathering technology developed in the Chathams based especially on abundant marine resources but without the capacity for intensification (Sutton 1980:83, 87) in this marginal habitat for isolated Polynesians. There is no archaeological evidence in New Zealand for a return voyage from the
Chatham's and Davidson reports that Maori and Moriori apparently did not know of one another's existence at the time of European contact (Davidson 1984:24).

New Zealand obsidian in the Kermadecs and Chathams

A recent paper by Leach et al. (1986) examines in detail the origins of prehistoric obsidian artefacts from the Kermadec and Chatham Islands. In spite of difficulties of analysis and interpretation, the authors attribute the probable sources as follows: six of 11 Kermadec samples to Mayor Island (New Zealand) and the rest to Raoul Island (Kermadecs); 77 of a total of 81 Chathams samples to Mayor Island (New Zealand), one to the central North Island of New Zealand, another to Awana (New Zealand) while the remaining two are designated as 'cf. Rapanui', which means that they are most similar to an Easter Island source, although not necessarily from it (Leach et al. 1986). Thus the New Zealand connection with the prehistories of the Kermadecs and Chathams is confirmed and the suggestion is also made that this was prior to 1400 AD, although for the Chathams that point would be contested by McFadgen.

The authors then proceed to discuss the nature of that contact and leave as an open question whether contact between these islands was by one-way accident or two-way navigation (Leach et al. 1986). However, if it was not by navigation, there is no reason why these islands should not have been settled in any order or at any time in the three thousand years since the colonisation of Remote Oceania began. The archaeological situation is too systematic to allow that. The authors also examine the detail of some simulated drift contact probabilities, but these actually provide no good reason why any one of the Kermadecs, Chathams or New Zealand should have been settled at all, by drift, let alone all three.

(3) The Line Islands

Fanning Island, lying approximately half-way between central East Polynesia and Hawaii in the North Pacific Ocean, provides another independent example although the only one, from this region. Archaeological investigation of Site FAN 1-7 gave a corrected radiocarbon age of 1560±85 BP, in association with artefacts including one-piece fishhooks, trolling gear and porpoise tooth pendants (Sinoto 1973) which compare closely to early Hawaiian examples, of much the same age.

SUMMARY

Evidently the systematic colonisation of the Remote Pacific extended to the large islands at the margins of Polynesia. Their small, abandoned satellites provide a secondary level of archaeological corroboration. Further, the fact that the large islands were still occupied at Western contact but effectively marooned by then, implies other structured similarities in this aspect of their subsequent histories.

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Williams, J. (1837) *A Narrative of Missionary Enterprises in the South Seas.* London: J. Snow.
When Jack Golson and I began our association in New Zealand in the early 1950s, it was not founded as some might have it, on our continuing, partisan and often conflicting analyses of the state of the game of cricket. Rather was it focused on the problems of agricultural prehistory in the Pacific and the possibilities of interdisciplinary (a word of limited application at the time) research approaches that were to become our career-long interests and an important bonding of our friendship. Since then there has been a burgeoning of agricultural studies through archaeological and ethnobotanical methodologies by investigators in the Pacific radiating from research centres in Hawaii, New Zealand and Australia. Important has been the emergence of Australian prehistory, with one of its major emphases on hunter-gatherer production modes to which Golson (1971) made a major ethnobotanical contribution. Initially the effect was simply the acknowledgement of the antiquity of the continuous, integral hunter-gatherer systems in Australia from the Pleistocene that contrasted with the more recent agricultural subsistence modes of the Pacific islands. And although archaeological studies repeatedly demonstrated the importance of the exploitation of natural resources (with little or no agricultural evidence) in the early cultural phases of the late Holocene island sequences, the subsequent appearance of dominant agriculture discriminated against any connection with the Australian situation. That is until recently, when the evidence for mainland New Guinea hunter-gatherers in the Pleistocene (White and O'Connell 1982) was enjoined by increasing examples of equivalent radiometric datings for the foraging colonists of the island archipelagoes to the immediate north and the east. Thus there has been the independent building of a temporal background against which the hypothetical modelings of the origin of agriculture in the western Melanesian region could be set. These models were based on constructions of evidence for domestication of endemic plants in the region (Yen 1973, 1985), and for the development of intensive highland agricultural technology by Golson (1977, 1990) beginning some ten thousand years ago. The long human prehistory of the region endows it with developmental time; the divergence, in both subsistence and cultural terms, of the old Sahul continent into northern (New Guinea) agriculturalists and southern hunter-gatherers of Australia only approximately corresponding with the physical separation 8000 years ago by sea level change (Jennings 1971). That agriculture was purely a cultural introduction into the region is the earlier contention that cannot be sustained on present archaeological evidence. The more complex prehistory of Melanesia however, precludes the application of the simple hunting-to-horticulture derivation of Oceanic agriculture.
productive capacities. While the emphasis has been on Polynesia and its relatively complex stratified social systems, the derivative theoretical applications reflected on Australia in its hunter-gatherer modes of production and implication for prehistoric intensification related to social elaboration (Lourandos 1980, 1983). Similarly for New Guinea, Modjeska (1982) and Gorecki (1986) among others cited the significant research of Golson on the elaboration and progressive sequencing of drainage systems in the agriculture of the Highlands to illustrate inferential relationship to complex social consequences.

The developmental nature of the processes of change in society and social environment in Oceanic prehistory has adopted as its theoretical base the concept of evolution, e.g. Kirch (1984) for Polynesia; Feil (1987) for the New Guinea highlands. The differentiation from biology however, has been tacit in the analogous term 'cultural evolution,' and indeed predated Darwin and his development of the theory of natural selection. Dunnell (1980) has indicated the difficulty in distinction from Darwinian evolution of the natural sciences in terms of 'confusion between reason-giving and scientific cause.' This is a principal criticism by Dunnell (1987) of Kirch and Green (1987) in their interpretative use of Darwinism to explain Polynesian colonisation. The latter paper marks the first attempt to apply scientific evolution to wider ranging Oceanic archaeological evidence, and beyond similar applications to local development sequences that incorporate subsistence systems development.

Kirch and Green's vision (1987:451) of Darwinism as 'continuously changing phenomena in the process of becoming' seems to be only a part of the Oceanic story. Dunnell's charge (1987) of essentialism (and therefore Lamarckianism) might have been sustained rather than denied. For if subsistence has a role in evolutionary constructs of Pacific, and specifically Polynesian cultures, the biological or genetic units of transferred species do not show the evidence of true Darwinian evolution such as speciation or geographical race formation. Rather it is the technology and subsistence system that display divergence or convergence with sometimes clear ecological correlates that have inferences for (1) the widely adaptive nature of genotypes, (2) the transmission ('inheritance') of information on which systems of new colonisations are built and (3) alternative options of technology allowable by flexible genotypes or genetic variability of the species. The succeeding discussion revolves largely on the latter two points, and the element of volition with which they endow situations of processual development.

**SUBSISTENCE AND CULTURAL TRANSMISSION**

Subsistence systems appear to fit with the evolutionary game models that recently have claimed the attention of biologists in addressing cultural transmission. The strategies employed, whether hunter-gather foraging or agricultural cultivation, are transmitted culturally rather than inherited biologically. Indeed, 'the tendency of natural selection to increase high fitness strategies may be counteracted by the effects of cultural selection influencing strategy adoption. Alternatively, strategies conferring low biological fitness may be culturally selected.'

It may be recognised that I have appropriated the terminology of a 1989 paper by Scott Findlay, Charles Lumsden and Roger Hansell (shorn of the eloquence of algebraic abstractions), for its compact presentation of the issues that I wish to address. I propose to continue in this vein, interpolating subsistence system as the game.

Leaving aside for the moment the element of oblique or horizontal cultural transmission, Findlay and his associates recognise three departures of vertical cultural transmission in the biocultural model from 'simple' biological games (with my interpolations in parenthesis):

1. Conditions for evolutionary stability are different; unlike biological states, biocultural states need not be evolutionarily stable (relative stability can be a product of successful environmental control).
2. Biocultural games can evolve to different equilibrium states depending on initial conditions (ecological dictates, especially in marginal environments, and the state of the game rules at the time)
3. Equilibrium strategy diversity is greater on the average (the choices for human action may be more broadly based – control of varied environments; intra-island variability of ecology, and the choices for intensification/technological development; social control of consumer population numbers).

Thus the rules of the game provide for the engendering of diversity from common roots of subsistence systems in the far Pacific, the technologies responsive to variable island environments that result in landscape modifications. On this agenda, human decision has at least as much of a determinate role through the application of alternative responses to environment as the environment itself. Prehistory provides us with portraits of the progress of the game itself.
ORGINS OF OCEANIC SUBSISTENCE SYSTEMS

The Pleistocene hunter-gatherers reached Oceania some 40,000 years ago, colonising the environments of New Guinea and Australia, spread over the latitudes of 45 degrees S. to the modern equator. Until the rise in sea level at 8000 years ago, the two land masses together formed the continent of Sahul. Recent archaeology has shown at least 20 Pleistocene sites in Australia (summarised by Jones 1987) with its central desert region penetrated by 22,000 years ago. Again, Golson's studies in the agricultural arch­

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ology has shown at least 20 Pleistocene sites in

Australia (summarised by Jones 1987) with its

central desert region penetrated by 22,000 years

ago. The fewer dates so far from main land New

Guinea also range from 40,000 years, but the

northern Sahul colonisation was extended to the

Bismarck Islands and as far as Bougainville to the

east at nearly 30,000 years (Allen 1989).

Again, Golson's studies in the agricultural arch­

aeology of the New Guinea highlands showing

agriculture to be 10,000 years old, have led to the

common assumption that it must have been

earlier in the valleys and mid-altitudinal rain

forests. This evidence of early agricultural drain­
age of swamps is supported by a rare sequence

on the same site of increasing technological

complexity of cultivation. It followed earlier

generic studies on sugar cane and the fehi (Aus­

tralimusa) banana that placed the origin and
domestication of these pan-tropic Pacific culti­
gens in New Guinea. More recently, the results

of our chromosome (Coates et al. 1988) and

ribosomal DNA (Matthews 1990) studies at the

Australian National University on the important

Pacific staple plant taro (Colocasia esculenta)

have shown that the wild forms in Australia

(foraged, and embedded in the totemic world of

some northern Aboriginal groups) are identical

with the cultivated and wild forms of New

Guinea. Taxonomically and cytologically, they

are the same as those that reached Hawaii and

Easter Island in prehistory.

The emergence of the northern agricultur­

alist from the Sahulian hunter-gatherer was the major

developmental divergence from common ances­
try that was to remain permanent. The Australian

Aborigine was never to adopt cultivation despite

irregular contacts with peoples with agricultural

backgrounds (including New Guineans) in pre­

history – and after European colonisation.

DIVERGENCE AND CONVERGENCE 1:
SAHUL

The Sahul replication of the emergence of

agriculture in world prehistory allows us to in­

spect the processual aftermath through the study of

the ethnographic endpoint of development of the northern cultivators and the southern

foragers.

At the gross level of comparison, this situation

may be viewed simply as divergent 'evolutionary'

progression in technology. However, in terms

of the two contemporaneous systems stemming

from common human ancestry, the major com­

ponents of environmental modification and social

change appear to have some of the characteristics

of convergent development (Yen 1989). The Aus­

tralian Aborigines share with the New

Guinea agriculturalists the deliberate use of

controlled fire for clearing and the fostering of
desirable species, and a series of 'agronomic' yet

incidental parallels such as the replanting of tuber

species after harvest; the tillage effect on soil

through the quest for subterranean resources

(plant roots, insects, aestivating and hibernating

animals); aggregative harvesting methods of

seeded plants that maintain and spread the

species to produce field-like stands; water control

in the conservation of some fish species; special­

ised production of superior 'strains' through the

recognition of differential geographic distribution

of non-toxic forms of a drug-producing plant.

The social institutions for which these envi­

ronmental aspects of production might be con­

sidered fuelling forces are the periodic congres­

ses of groups identifiable by lineage or political

alliance, and the related establishment of trans­

continental trade networks through which valu­

ables (including drug plants) were ceremonially

exchanged. The division of the continent into

tribal territories breachable by treaty or intrusion

had been established well before the European

era. Indeed, the development of tribal and sub­

tribal boundaries required the intensification of

production, for with each new territory struck,

there was a progressive narrowing of the radii of

potential hunting-gathering cycles for subsistence

and for the surplus that we call social production.

At the level of comparison with which we are

dealing, the Australian hunter-gatherer mimicked

the New Guinea agricultural system. The mis­

sing element was plant domestication in the sense

of genetic modification guided by artificial

selection. In New Guinea, plant domestication

would produce some 27 species of cultigens (Yen


THE AUSTRONESIAN COLONISATION
IN MELANESIA AND AGRICULTURE

Unfortunately the eventual Oceanic systems

of subsistence were not derived solely from New

Guinea. Six thousand years ago by linguistic

projection and 4000 years by archaeological

90
evidence to date, the New Guinea region was colonised secondarily by Austronesian speakers out of Asia (Pawley and Green 1973; Tryon 1985). They were also agriculturalists, bringing with them some of the cultigens of Southeast Asia. The Austronesian colonists probably carried domesticates of some of the same genera, e.g. *Dioscorea* yams, and in a few cases, the same species, as the New Guinea cultigens, e.g. *taro*, *Colocasia esculenta*. However, they also brought previously unknown cultigens to the region such as the betel nut palm for which we now have evidence from 6000 years ago (Swadling et al. 1988) from the earlier estuarine environment of the Sepik River system. The latter has the implication that those earlier secondary colonists of the New Guinea region were Austronesians not bearing Lapita pottery, later to be a major artefactual signal for the earliest migrants out of New Guinea to Eastern Melanesia and Polynesia. However, the further archaeobotanical evidence of Kirch (1989) from the northern New Guinea islands indicates that the later colonists were to adopt the unique New Guinea tree crop domesticates.

The New Guinea region was thus the site of the blending of two independently developed agricultures. The coming together of Asian and New Guinea species was the basis for the confluence of genetic materials and cultural ideas of environmental adaptation that were given expression in Oceanic subsistence systems.

**DIVERGENCE AND CONVERGENCE II: THE PACIFIC ISLANDS**

The migratory travels eastward of the Austronesians bearing Lapita pottery were accompanied by losses of species from the compounded agriculture. Some cultigens of New Guinea were not to be found elsewhere, some were to expand no further than its northern islands; others like the *Canarium* nut tree reached into Western Polynesia. The Asiatic betel palm found its easterly limits in the eastern Solomon Islands, but the other drug plant *Piper methysticum* or *kava* of Melanesian origin was to become ritualised throughout most of tropical Polynesia. On the temperate islands, Easter and New Zealand, and on the smaller coral atolls, the loss of species was more marked, more clearly a function of unfavourable environments. The inconsistency of correlation of *dominance* of species with edaphic and rainfall conditions however, reflects something of the early decisions in the applications of alternative agronomic measures — adaptive environmental modifications. In the ethnographic record, the islands show diversity of production intensity of dominant crops through techniques of storage of root and tree crops; periodic intensification of animal husbandry — particularly pigs for ceremonial purposes — and agronomically, through the lengthening of rotations of dryland swidden cycles and mulch application. Important because they leave archaeological imprints were water control measures expressed as field drainage of swampy areas, irrigation terracing of stream margins and the exploitation of subterrenean water sources on coastal plains and on coral atolls.

The individual cultigen species (including animals) with their corresponding alternative husbandry techniques, together with the exploitation of wild fish, bird and plant species with a wide range of technologies, make up the flexible units of Oceanic subsistence systems. They offer a portrait of discontinuity or divergence with some environmental correlates. It is in the organisation or structure of individual systems, the assemblage of the variable units into systemic components, whereby the commonality of Oceanic systems are recognisable. For regardless of the settings, high or low island, coraline or volcanic, large or small, the systems at their apogees of development were constructed of the interdependent components of hunting-gathering, annual cropping, arboriculture and animal husbandry.

Archaeological sequences in the Pacific generally convey componential changes through time. Initial colonisation is generally characterised by hunting and gathering of a wide range of indigenous fauna, while the markers for later agricultural dominance are usually the remains of intensive cultivation such as irrigation. The developmental processes by which they are achieved however, are generally covert, but they do imply human population increase and the organisation of labour. Thus it is at the structural level of subsistence systems that we may recognise the development of similarities and the convergence of their histories. The segregation of plants and animals with their corresponding ideational sets that accompanied the early Pacific island colonisers were to be reassembled into structurally similar systems. The differences then may be of scale, for in marginal situations like small atolls the potential for production, as for human population and presumably for complexity of social organisation, is reduced. Such systems, miniaturised as they were, did not have the potential in themselves for expansion, with indeed, one component unstable. In atoll archaeology, the evidence for animal husbandry is
inconsistent between islands (e.g. Intoh 1986, reviewing variability in Micronesia; accounts of excavations in the Tuamotu Islands in Hatanaka and Shibata 1982) and within local sequences, disappearance and reappearance of pig, dog or chicken bespeaks of the maintenance of outside contact to maintain the component, and an unwillingness to break with a culturally important aspect that was naturally unadapted (Yen 1990).

The convergent processes of intensification of subsistence production have been one of the cornerstones of evidence for the emergence of similar patterns of development of the highly stratified, hierarchical societies in Polynesia, with population density changes and competition for land as contributing systemic factors. Can this be testable as a developmental principle? Taking a preliminary comparative step, we can select three irrigation societies of Austronesian speakers in the Pacific; the Hawaiian of Polynesia, the New Georgian of Melanesia, and the Ifugao of the Philippines. All constructed stream and river margin terraces, although Ifugao architecture is considerably more elaborate. They also share all the other components of production of Oceanic subsistence systems. All three underwent population increase in the past, and all three have histories of warfare that involve territorial competition. Apparently however, the Melanesian (Yen 1976) and Philippine (Maher 1973) systems were developed independently and convergently well after the beginning of the Christian era without the organising force of hierarchical societies. Nor did this convergence lead to highly developed social stratification or the emergence of centralised political power. The contrast with Hawaii needs no elaboration (see Kirch 1985). The question may be asked however, whether progress in subsistence systems is a separate parallel developmental pathway from social systems, with intersections at intervals. e.g. demographic change, or if the two constitute a single line, there are other socio-environmental dictates responsible for such bifurcation.

SUBSISTENCE AS BIOCULTURAL GAME

There are two kinds of biocultural games identifiable in the prehistory of Pacific subsistence:

Firstly the formative confluence of exotic Asian agriculture with the indigenous systems of the New Guinea coast, with a certain home team advantage. Exemplifying horizontal cultural transmission, in which the requirement of strong or repeated exposure 'during socialisation', resulting in 'amalgamation and coalescence across lineages', this is only now being demonstrated archaeologically.

Secondly vertical transmission that is best exemplified in island Polynesia. The structural components of subsistence systems were all in place in Western Polynesia; they were the set of qualitative structural constants whose rearrangement was the function of the segregational effects of variable environments on the material transmissions of species; and the extent to which the sets of knowledge carried that could provide the artificial conditions through the development of technology for intensive production, be it initially for fishing or later intensification of agriculture. Highly intensive agriculture appeared relatively late in most Polynesian (and indeed Melanesian) archaeological sequences, and these widely dispersed systems were essentially contemporaneous. Thus transmission/diffusion could not have been a direct transfer of technology. Rather, with the acceptance of the developmental sequences that have the advantage of archaeological verification, we may see the 'sets of knowledge' not merely in terms of correlative ecological requirements of species, e.g. taro – wet; yam – dry (Barraud 1965), but knowledge of more fundamental rules by which the adaptive biocultural game may be played. Such knowledge goes beyond the ciphered identification of the broad ecological requirement of individual species, into the qualities of perceived edaphic classes, and the sacred and secular controls of wind and weather. For out of these complex relationships come the technologies of production – and the extent of local control of environment.

The advances made in ethnoscience in the last decades have indicated something of indigenous concepts of environment, e.g. colour categories, plant and animal classification systems. Less in evidence are studies of 'laws' which, by their pragmatic applications, result in environmental modifications. The indigenous concepts of sex, asexual reproduction and facultative sexual reproduction in plants form the basis of selection and indeed continuing domestication of useful species in Polynesia. The effects of inbreeding in animals are widely known. The properties of native classes of soil with their association of water-holding capacities or richness as metaphorical reference to human diet often translatable as 'fat', hydrological concepts that result in mulch application, artificial conduction of water, the lowering of water table and the exploitation of underground sources are further examples that I have encountered in regrettably unstructured enquiries over many years of Pacific fieldwork...
Ethnoscience at such levels may be the basis for independent appearance of geographically and temporally separated, complex technologies *late in individual cultural sequences*, in which the direct transfer of adaptive information by horizontal transmission could not have been possible. Convergence in such cases may thus be viewed as coincident invention based on 'ethno-theoretical' premises in common. These comprise the 'information about the strategy set (that) is transmitted rather than the individual strategies themselves' (Findlay et al. 1989:571).

Biocultural game theory, as applied to subsistence development, has outcome similar to the theoretical derivations first addressed in this paper. Biocultural stability is dependent on human responses to the human condition, and the demands on production are dependent not only on the perceived requirements of subsistence, but the necessity to provide for social production in developing cultural institutions that are not necessarily hierarchical. Strategy diversity and different equilibria are embodied not only in separated systems stemming from common ancestry, but within sequences of change in individual examples of prehistory of individual archipelagoes. On this reading we are still in agreement. 'The tendency of natural selection to increase high-fitness strategies may be only counteracted by the effects of cultural selection influencing strategy adoption' may be most graphically translated in our terms to the atoll example. Natural selection favours strategies favouring the edaphically adapted, drought and wind resistant native flora, but cultural selection favours transported species artificially adapted, which without the maintenance of human intervention, could never become a part of the natural flora.

**CONCLUSION**

For this discourse I have used a work with strong sociobiological roots as a text. Findlay et al. (1989), through mathematical modelling, have pointed out comprehensively the radical departures of the cultural game from the standard biological model for evolution under natural selection. Furthermore, one conclusion is that cultural transmission, in contrast to biological inheritance, is 'often Lamarckian' (Findlay et al. 1989:572). In considering the transmission of subsistence systems it has always been the obvious and thus often unsaid implication. The dynamics of the application of composite technologies demonstrable in archaeology as environmental responses and indeed as purposeful changes in those environments have the characteristics of use and disuse, and their transmission, the inheritance of acquired characters, but without the material genetic base. The subsistence basis as a major feature of the environment for culture and cultural change becomes modified so that the directions and intensity of selection become increasingly governed by the requirements of the consumer rather than by the natural selective factors, edaphic or climatic, or even necessarily maximal reproductive success or natural survival value of cultivars. In the dynamic nature-nurture equations that are agricultural history, the genetic units of species tend towards conservatism under artificial selection. Guided by gastronomic preference, acceptable yield levels and sometimes aesthetic considerations, the indigenous selection process maintains, nevertheless, variability at sub-specific levels in the major staple food plants. The preservation of heterozygosity in agricultural clonal propagation is expressed then when the occasional sexually-produced seedlings allow for selection or rejection of new gene recombinants (Yen 1985). Among exclusively seed producers, especially tree species, selection is exercised on the same criteria producing a plethora of what we might call varieties, but without vegetative reproduction (Polynesian breadfruit, *Artocarpus altilis*, being one of the major exceptions), there is no attempt at mass selection towards uniformity. This is indeed the major difference from the market (including supermarket) farmer - uniformity and the consequent loss of germplasm that we have been facing in todays' crop plants. In other words, the selection of 'fitness' has changed on the changing criteria of the social rather than natural environment. Thus while evolution is apt enough as analogy in the progress of human affairs, the efforts to fit it into a Darwinian template is nearly as difficult as a complete embrace of Lamarck. Rindos (1989) has defined his view of 'cultural selectionism', positing 'a strictly Darwinian model for cultural function and change', earlier applied to domestication and agricultural origins (Rindos 1984), with emphasis on accident or chance. Just as the Newtonian apple was not the first recognition of the force of gravity, domestication was accompanied by a prior empirical environmental consciousness. This, translated into early technology, allowed the provision of appropriate artificial environments, and provided the forerunners of the immediate conditions for selection of species that otherwise would have largely succumbed to competition in the game of natural selection.

It is difficult to see how non-genetically transmitted phenomena, even agronomy with its gene-
tic, species units, can be accommodated as strictly Darwinian (Rindos 1989:28), as it is to attribute natural selection as the sole ordering force for human organisation, be it subsistence, language or kinship systems. It is interesting that some geneticists in addressing human action in selection hold a differing view, e.g. Dobzhansky (1962:11) has described artificial selection exercised by humans as antonymous with natural selection: Maynard Smith (1975:130) states that 'A domesticated population has been largely removed from the action of natural selection'. Where we have investigated the subject of variability of plant or cultivation method among Pacific subsistence people, we have not been able to avoid the implications of cognition of genetic variability and its suspension and release or of choices in environmental adaptation and the alternate pathways for the organisation of system components. Indeed, as the philosopher Anthony Flew (1984) has emphasised in addressing Darwinism in human history, it is the element of choice that discriminates between evolution in biology and the social sciences. The plasticity of choice has produced convergence and divergence even in mundane human activity like subsistence, and in terms of componential structure, has produced similar systems regardless of environmental and individual technological diversity. The domestication of environment by man leads from his environmental control as hunter-gatherer, through agriculture, citification and world trade to the conquest of space. The mis-choice of other environmental controls could interrupt the last of this great sequence.

Perhaps we have been over-zealous in going beyond the data in our pursuit of neat coordination of utilitarian choices in prehistory with the development of the more esoteric and ecologically independent institutions of culture. Usually, archaeological indications of motor forces for social (and environmental) change, such as demographic fluctuations, are more often inferential than evidential. As Colin Renfrew (unpublished) seemed to suggest, we may need to sideline derivative theory accounting for what we know and, with the results of rapid increase in archaeological research, construct a developmental theory of cultural evolution without strictures from other disciplines.

With the advancement of discussions of the application of Darwinian evolution to institutional development, it becomes increasingly a matter of historical analogy or literary metaphor. It is an irony that as biologists are currently exploring the discriminations between biocultural 'evolution' and 'standard' biological evolution, some anthropologists are stressing the Darwinian parallels. One hopes that the latter trend does not lead to another cardboard Darwinism against which Stephen Gould (1988) has warned.

ACKNOWLEDGEMENTS

This paper is an attempt to recall my side of some of the later discussions on agricultural origins with Jack Golson over the last decade (and understating the importance of that other game), when I was a member of his Department. No doubt Jack will recognise most of it, even if he does not subscribe to all or any the points that I raise (ditto for that other game). My appreciation of our times together in over thirty-five years is terribly understated, my career debt impossible to express.

A version was presented to a Cold Spring Harbor Laboratory Centennial Symposium, 'Evolution: Molecules to Culture', in September 1990. I wish to acknowledge the stimulation of my symposium colleagues, Kent Flannery, Patrick Kirch, Colin Renfrew and Henry Wright, without implying their responsibility for anything written here.

REFERENCES


In 1917 the Senate of the University of Sydney refused to ratify the appointment of one V.G. Childe, already a graduate of both Sydney and Oxford, to a Tutorship in History in its Department of Tutorial Classes, clearly on grounds of his political affiliations. One wonders but idly what might have happened to archaeology in Australia had Sydney's decision been otherwise (Megaw 1963:301).[1]

There is a special melancholy about the return of Gordon Childe to Australia 'just to look at my native land' as he put it to the Melbourne Herald on his arrival at Essendon Airport on 19 September 1957 (Anon. 1957). It is not merely the sadness that such a great scholar should have chosen prematurely to end his life, spinning towards oblivion through the gum-scented air, but there is also some indefinable sense of loss of what might have been, had he lived to witness the great expansion of knowledge about the prehistory of the continent upon which, for complex reasons, he had turned his back for the bulk of his brilliant creative life (Green 1981:142-54). It was ironically in that same year 1957, that Norman Tindale published the synthesis of his views concerning prehistoric cultural succession in southeastern Australia, and John Mulvaney had initiated the only course on the prehistory of Australia taught in any Australian university as part of a Pacific prehistory option in final year honours in the Department of History at Melbourne University (Mulvaney 1990a:29). A year previously, one of the examination papers for the pass and honours course in Ancient History had nine out of eleven questions devoted to issues of world prehistory, including references to Grahame Clark's work on palaeolithic and mesolithic economic systems, the recent uncovering of the Pilton Skull fraud, and Childe's theories concerning the neolithic and urban transformations (University of Melbourne 1956). These questions reflected Mulvaney's experiences as a recently returned graduate from Cambridge University's palaeolithic course, with expedition experience at Hauf Fatah in Libya with Charles McBurney (Mulvaney 1986:98). Nevertheless even before his Cambridge training, Mulvaney referred back to 1950-51 when he had been influenced by his co-tutor, John O'Brien and had gradually shifted from the archaeological history of Europe to questioning the extent and nature of research into pre-European Australia ... [so that] ... Australian evidence and problems kept straying into Ancient History tutorials (Mulvaney 1990b:v).

The materials available were sparse, consisting of stone tool typologies by Mitchell (1949) and McCarthy, Bramell and Noone (1946), field reports from Gill's work in western Victoria and Keilor (1954) and Hale and Tindale's account of their excavation at Devon Downs (1930). To place these issues into a broader intellectual context, it is worth remembering that it was only in 1946, again at the University of Melbourne, Department of History that Manning Clark initiated the first year-long course on Australian history in any Australian university; a move described by historian Don Baker in his eulogy at Manning Clark's funeral in 1991 as having been a 'revolutionary step' (Cotton 1991). Prior to that, Australian history had been subsumed under British or Imperial histories.

It is also fair to note that in Clark's Short History of Australia published in 1963, the entire pre-European history of the Aborigines was confined to three paragraphs. Following a pastiche of Tindale's theory of 'three migration waves', he wrote that

While the inhabitants of most of Asia and of the islands from Sumatra to the Moluccas and Timor gradually progressed from barbarism to civilisation, the aboriginals retained their primitive Stone Age culture. The absence of suitable seed-bearing plants and animals suitable for domestication probably were the main causes of
this changelessness, though their cosmology also contributed to it (Clark 1963:14).

That they were not called upon to respond to externally induced change, was partly due to isolation – 'chance protected the aborigines from ... an invader' (Clark 1963:14). John Mulvaney (1986:100) recalled ruefully that despite a rural childhood in Gippsland and later in the Wimmera of western Victoria, it was not until his mid-thirties that he had actually met an Aboriginal person. In those days, not only was Australian prehistory almost invisible, so too at least in southeastern Australia, were the Aborigines themselves.

On 13 October 1957, Childe was a 'Guest of Honour' at the Australian Broadcasting Commission and he referred to a time almost 40 years previously, when he had left Australia to study European archaeology. At that time the received wisdom was that the antecedents of European history existed only in the written texts of the Middle East 'while the natives of Europe stagnated in illiterate barbarism'. Now returning to Australia, Childe found 'the same old dogma being repeated with different names'. This time the 'true prelude' to the prehistory of Australia was postulated as being written in Europe 'while the Aborigines stagnated in illiterate savagery' (1957a in 1990:26-7). One can note in this apparent equivalence of metaphor, that Childe like Manning Clark still maintained a hierarchical nod of the hat towards Morgan's distinctions between the cultural status of farmers and hunters. Nevertheless, Childe (1990:27) pressed on with a devastating observation that 'the archaeological sources for Australia's prehistory are less well-studied in 1957, than the sources for European prehistory were in 1857'. Other archaeologists before him had also had the same sense of pessimism about the possibilities of creating a coherent sense of a past in Australia from the materials available. The American D.S. Davidson, with experience in material culture studies and having done some pioneering archaeological research himself in the Katherine district of the Northern Territory, wrote in 1935, that 'there is so little archaeological information available at present that it is difficult to outline any strictly archaeological problem' (1935:148).

While Davidson was probably influenced by his own northern experience, this was a trifle unfair on pioneer excavations carried out by Hale and Tindale in 1929 at Devon Downs on the banks of the lower Murray River, which had set up a stratigraphically controlled set of super-imposed assemblages of artefacts, believed on typological grounds to be distinctive as 'cultures'; given the terminology of the day (cf. Mulvaney 1961:66-88; and Bowdler this volume). However in his major review The Stone Age of Australia, John Mulvaney, having referred to Governor Arthur Phillip's excavation of what he thought was a burial mound, within a few months of his arrival in Sydney Cove in May 1788, could still write that 'Australia remains the dark continent of prehistory even after the passage of 170 years' (1961:56). Glyn Daniel's A Hundred Years of Archaeology (1950), passed Australia by, with only part of a single sentence devoted to it in a chapter called 'The development of world prehistory' where it was stated that Australia shared the post glacial Hoabinhian culture with Southeast Asia and Japan (p.86). Finally in this set of considered opinions, Grahame Clark from his vantage point while writing the first edition of his World Prehistory in 1961, said that 'the scientific pursuit of prehistory is still in its infancy over large parts of the vast Australian territories' and Mulvaney even as late as 1964 in a review paper in the Australian Journal of Science, announcing secure evidence for terminal Pleistocene occupation of Australia, still has a refrain that 'Australia is the last inhabited continent to discover its prehistory and this provides an opportunity to learn from the errors of others' (1964a:42).

Yet less than thirty years after these Jeremiahs, there is a different message, one of confidence, even of triumphalism. Paul Mellars wrote in a review of Josephine Flood's book Archaeology of the Dreamtime, that 'the discovery of Australian prehistory must be one of the most significant achievements in archaeology during the present century' (1984:232) and Renfrew and Bahn in their recent textbook of world archaeology have written that

the transformation in our knowledge of prehistoric Australia and Southeast Asia over the course of the last 30 years has been one of the most exciting developments to have taken place in modern archaeology (1991:438).

Clearly something had happened.

A potential trap in writing history is to view the whole process as leading inexorably up to the present. This tendency towards a 'Whig Interpretation' seems to be especially strong where the subject matter is a realm of human endeavour such as science, which purports to discover the 'truth' about the natural world or about past events. One could write a history of Australian archaeology in this way. There are few concepts or approaches currently at the edge of enquiry for which one could not find some antecedent if one searched the literature diligently enough. One could make a case that there had been no
'revolution', but that the subject had developed with ever increasing knowledge and sureness of touch from a core, rooted within the milieu of mid-nineteenth century scholarship. David Horton, in his recent book *Recovering the Tracks* (1991), has attempted to write the first sustained historical account of Australian archaeology since Mulvaney (1961). His thesis is that there has been a broad continuity of purpose and research aims within Australian archaeological research from a period since the last few decades of the 19th century until the Mungo discoveries in 1969-72; and his tale, looking back from that vantage point, ends where others might have begun it. There are also some anachronisms in his groupings of papers and of themes. For example he generously included the first serious paper that I wrote on the archaeological sequence of northwest Tasmania (Jones 1966), within the same section entitled 'Classic Archaeology Begins', as papers by Hale and Tindale, McCarthy, Gill and Mulvaney; the birth-dates of some authors spanning more than forty years. The next section called 'New Syntheses' was restricted to works either published or written in the 1950s, including those of Birdsell on the colonisation of the continent, Tindale and McCarthy's regional cultural sequences for southeastern Australia, and Mulvaney's 'Stone Age of Australia' critique. Only the latter pointed towards the archaeological research methodology of the next decade.

Horton has however done a great service to the historiography of Australian archaeology in bringing to contemporary notice the concerns and some of the key papers of an earlier period, such as those of Etheridge, Howitt and David, which had considered the possibilities of a high antiquity for the human occupation of the continent. It is to this question that I now wish to turn.

**GEOGRAPHICAL REMOTENESS AND THE LIVING STONE AGE**

Even some of the earliest European geographical commentators on the Australian continent had also speculated as to the deep antiquity of its Aboriginal inhabitants. The French Enlightenment philosopher J.-M. Degréando, having been asked by the *Société des Observateurs de l'Homme* to give advice to the Baudin expedition, about to explore the coasts of western Australia and Tasmania, wrote in 1800 that

> The philosophical traveller who sails to the extremities of the earth, traverses in effect the sequence of the ages; he travels into the past; each step he takes leaps a century (1978:131, my translation).

François Péron who had coined the term 'anthropologist' in his mission to be allowed to join the expedition, wrote the official account of it after Baudin's death at sea on the return voyage (Jones 1988a:38-9). In 1804 he was invited to address the Class of Physical and Mathematical Sciences of the Institut National concerning 'certain zoological facts applicable to the theory of the Globe'. He stated that of all the results gained from the long voyage (1800-4) which he compared with the travels and travails of his contemporary Alexander von Humboldt, 'the most striking without doubt, the most important, and perhaps also the most inexplicable, is the absolute difference between the two races which people each of the two lands' namely Tasmania and the mainland of Australia (1804:5). These differences were spelled out in terms of culture; i.e. customs, technology of the chase, habitations, canoes, languages, and also in terms of physical characteristics; the shape of the face and cranium, skin colour, and above all the texture of the hair. There were also no dingoes in Tasmania, yet they were ubiquitous on the mainland. How could one have a totally different race, whose physical characteristics he considered to be more appropriate to a tropical climate, living on a totally isolated island off the cold southern edge of a continent which was itself inhabited by another group of Aboriginal people? Had the lands been joined in recent times, they would have been inhabited by a common race of people, which without doubt would have been those who occupied 'the immense coasts of New Holland (from Wilsons Promontory) through to the burning beaches of Arnhem Land and the great Gulf of Carpentaria' (1804:6). These ethnological facts demanded a geological answer, that the separation of Tasmania from the Australian continent did not, according to Péron result from the 'modern operations of nature', but 'must belong to an epoch much more ancient than one can at first suspect' (1804:18). The cultural separation between the two peoples and the physiological observations of what seemed to be tropical physical characteristics of the Aborigines of Tasmania, were to constitute for Péron 'new proofs concerning the imperfection of our systems of (explanation) concerning the communications of peoples, their transmigrations, and the influence of climate on man' (1804:18).

Sixty-five years later, at the Third Meeting of the International Congress of Prehistoric Archaeology held in Norwich and London, Thomas Huxley felt that the geographical distribution of what he considered physically to be a closely related group of peoples, namely those who had
inhabited parts of New Guinea, New Caledonia and Tasmania also implied a great antiquity for their initial colonisations. His argument was that these peoples were quite different to the Aborigines of the Australian mainland and that they had arrived in Tasmania along a now sunken chain of islands situated to the east of the continent, and he concluded that if this were so the distribution of the Negroid and Australian races of man is as strong evidence of his antiquity as the occurrence of his works in the gravels of Hoxne and Amiens (1869:97).

By the turn of the century, A.W. Howitt was arguing a more plausible scenario, that people had got to Tasmania by dry land from southeastern Australia. However he thought that this had happened a long time ago, before a great geological event had caused the tectonic sinking of a graben that had formed Bass Strait (1904:24). It must be remembered that a unified theory of cyclical glacial periods with their consequent effects upon sea levels due to the periodic locking up of so much of the Earth's atmospheric water circulation was only fully enunciated by R.A. Daly (1934) in the early 1920s. We can see from these examples spanning a century, that based on principles of the geographical distributions of what were deemed to be racially and culturally different peoples in the Australian and southwest Pacific region, scholars were not inhibited from speculating that the original colonisations had occurred a long time ago.

As knowledge of geological time-depth became more refined during the nineteenth century, so also was a demonstration of human fossil skulls, artefacts and camp debris within deposits that could be investigated using geological methods; and which on sedimentary grounds and also in terms of associations with extinct faunas had antiquities which must have been measured in terms of tens and even hundreds of thousands of years. Even as early as in Lyell's Geographical Evidence of the Antiquity of Man (1863:69, 298) there were scattered references to Australian data; to archaeological sites such as shell middens, artefacts such as ground stone axes and comparative measurements of skulls. Huxley's first description of the Neanderthal cranium (in Lyell 1863:69) compared its longitudinal profile with that of a skull from near Adelaide. It was in the next phase of synthesis, namely the marrying of artefact-based archaeological 'cultures' with ethnographically derived 'societies' that Australian data were brought into the centre of theory building.

Lewis Henry Morgan in his Ancient Society (1877) devised a scheme of what he called 'ethnic periods', whose terminology and definitions according to their subsistence and technological bases followed closely the pioneering schemes of Montesquieu and Adam Smith more than a century previously (Jones 1992:745-8). Morgan's construction of three sub-stages of what he called 'savagery', three of 'barbarism' and the final one of 'civilization' was tied loosely to the emerging archaeological record, but the main basis for its content was derived from comparative ethnographic studies. The people of the palaeolithic stages of savagery were believed to have lived by hunting and gathering for their food within units of society comparable in scale and density with those seen ethnographically amongst contemporary hunters. Neolithic barbarism began with the discovery of pottery and the use of edge-ground axes, and it continued with the domestication of animals and plants, in societies comparable either to pastoral herders or subsistence horticulturalists such as Indians of south eastern United States of America, village Indians of Peru and the Ancient Britons (1877:9-12).

Both the Australian and Tasmanian Aborigines were centrally incorporated into this and similar schemes such as those of Lubbock (1865), Tylor (1865, 1893), and later Sollas (1911) as living exemplars of early stages of society which had been superseded in other more advanced parts of the world and which there, existed only in the archaeological record. In 1860 Tylor specifically compared a round-edge stone scraper from Tasmania which had been brought to the Taunton Museum in Somerset, with other tools found in the French Pleistocene 'drift', i.e. river terrace gravels (1865:195). Tylor felt that the similarity in typology indicated 'the persistence among these modern savages (i.e. Tasmanian Aborigines) of a state of stone implement making comparable to that of mankind in their remotest acknowledged antiquity' (1893:142). In a later paper, he put forward the view that because of the information gained from a study of Tasmanian and Australian Aboriginal societies, 'Man of the Lower Stone Age ceases to be a creature of philosophic inference, but becomes a known reality' (1899:ix).

Viewed from a contemporary perspective, such theories may also have had the unintended political consequences of regarding the Aborigines as 'living fossils', and thus numbing into indifference the European moral response at seeing the collapse of their societies at the edge of the colonial advance; the process could conveniently be posited as being part of the inexorable workings of a historical law of nature (Golson 1977:3-4). Restricting ourselves how-
ever to the narrower concerns of this essay, it can be argued that by assigning the Aborigines to a static palaeolithic stage, this also had the effect of paralysing field research into their prehistory, since it was felt that they were people unchanged since ancient times and thus such investigations would be futile. However this was not the only implication that could be drawn from such theories. If the Tasmanian Aborigines really were equivalent in some way to the 'earliest relics of palaeolithic man', and those of the mainland were in any way 'the Mousterians of the Antipodes' as Sollas put it (1911:91, 170), then this should have galvanised action into investigating the deep prehistories of these peoples on the Australian continent. There should at least have been an effort to document their past migrations, which on the theoretical premises of the day, might have rivalled that of European terms of antiquity. That this did not happen was partly due to the different sociology of research in a colonial and sparsely inhabited country; but it was also due to the very different geological and stratigraphic contexts on the Australian continent which proved to be beyond the technical capacity of the researchers of the time.

TALGAI, TASMANIA
AND EDGEWORTH DAVID

I wish briefly to illustrate these difficulties by reference to the Talgai skull and the attempts by Edgeworth David to establish a Pleistocene chronology for Australian archaeology in the early part of this century. The carbonate-encrusted Talgai Skull was discovered on the bed of Dalrymple Creek in the Darling Downs of south-eastern Queensland as a chance find by a boundary rider sometime about 1886 (Macintosh 1969:189). Technically it may have been the first Pleistocene human fossil to be found outside of Europe, but its significance was only recognised after the discovery by Dubois of his 'Pithecanthropus erectus' on the banks of the Solo River in Java in 1891 and published by him in Batavia in 1894. The Sydney Mail, in its issue of 10 October 1896, described the Talgai find as a petrified skull... The specimen is heavy and has become a solid mass... The shape of the skull — a protruding jaw and low retreating forehead — indicates a nature of almost exclusively animal propensities (quoted in Macintosh 1969:194).

It was eventually acquired by the University of Sydney and presented to the general scientific world by Professors Edgeworth David and J.T. Wilson to the eighty-fourth meeting of the British Association for the Advancement of Science held in Australia in August 1914; only the second time that it had been held outside of the British Isles. With the shadows of war looming, it was not an auspicious time for an international conference. The German glacial geologist A. Penck was a guest of honour and was given special dispensation for the duration of the conference. Five years previously in Die Alpen im Eiszeitalter (1909), he and E. Brückner had proposed the classic Pleistocene glacial sequence of Würm, Riss Mindel and Günz, named after rivers running from the northern slopes of the German Alps. Penck accompanied David to look at evidence of Carboniferous glaciation in the Hunter Valley and opposition to this collaboration by xenophobic elements of the press and the New South Wales Government led David to say that 'the bond of scientific fellowship is so strong that not even the storm and stress of a mighty war could break it' (M. David 1937:207). At about the same time the Secretary of the Royal Society of Tasmania, the prolific archaeologist and geologist Fritz Noetling who had published extensively in German journals and who had presented important stone tool collections to the Leipzig Museum, was interned as an alien (W.L.H. Crowther pers. comm. 1977). It was the end of a world.

David and Wilson made their announcement of the fossil skull to the Association on 21 August 1914. They acknowledged that it had been discovered many years before as a surface find. Nevertheless, they pointed out that numerous bones of extinct mammals of presumed Pleistocene age had been found in nearby creek beds, and that the skull showed a degree of fossilisation at least as well developed as bones of Diprotodon and Nototherium in adjacent regions. While they felt that there is a strong probability of this fossil being of Pleistocene age, perhaps early Pleistocene, its exact age obviously cannot be determined until further evidence can be adduced which may directly connect it with the mammalian bone-bearing clays of the Darling Downs. Certainly it is far older than any aboriginal skulls that have ever been obtained in Australasia, and it proves that in Australia man attained to geological antiquity (1915:531).

Although there was a high degree of probability that some of these conclusions were correct, one can also stress that the actual evidence was circumstantial as the caution in the title of their paper acknowledged (see also David and Wilson 1914a and 1914b).

The first anatomical description of the Talgai Skull was made by Smith in 1918 and he commented on what he thought were primitive
Jones

morphological characteristics, some even having an echo of the Ptiltdown *Eoanthropus*. The Ptildown skull, now known to be a forgery, was then the sensation of the age, and the features which the forgery was intended to display, i.e. a primitive ape-like jaw and teeth, were those which Smith drew attention to in the Talga fossil. 'In the light of the anatomical facts here set forth ... the claim to high geological antiquity – the assignation of this fossil to the Pleistocene – is very strongly supported' (1918; quoted in Macintosh 1969:190). Subsequent analyses of the teeth of Talga by several senior anatomists, including Dubois himself (1920) and Campbell (1925), indicated that Smith's interpretation could not be sustained, and there were also problems to do with the fracturing of the jaw and palate of the fossil which Smith had failed to appreciate. However in the later opinion of Macintosh (1969:190), these criticisms were so much accepted by general scientific opinion that they masked Dubois' and others' confirmation of Smith's view that 'the teeth, jaw, and palate were of a generalized primitive human type', and thus 'The significance of the cranium was therefore not only reduced, but reduced excessively' (1969:190) [2]. It is worth noting however that Dubois, when he came to writing his definitive monograph on the Javanese Wadjak hominid, made explicit comparison with the Talga fossil. Indeed the very title of the monograph – *The Proto-Australian Fossil Man of Wadjak* (1920) implied a direct biological and chronological link between the ancient prehistoric peoples of the two regions.

Edgeworth David visited what was believed to be the find site in 1914 and was convinced both from the geological field evidence and from the degree of carbonate encrustation covering the skull, that it had an antiquity of at least terminal Pleistocene age [3], and this reinforced his view of a high antiquity for the human occupation of the Australian continent. In 1924 he drew together various types of evidence to propose that the human occupation of Australia went back to Pleistocene times. Concentrating his argument on Tasmania, he was keen to refute a view put forward by Noëting (1911) amongst others, that based on a study of the putative rate of accumulation of coastal shell middens in Tasmania, the maximum length of time of occupation of the island extended back only some 5000 years. Whereas this estimate of the age of the middens concerned, namely around little Swanport has since proved remarkably accurate given all the assumptions of the original calculation, David also pointed out correctly that signs of any previous coastal occupation would have been along now drowned glacial age coastlines. He sought direct geological evidence and thought that this had been provided by the Tasmanian State geologist W.H. Twelvetrees' (1917) account of a well made stone scraper which had purportedly been found 3.5 m below the ground within consolidated gravel at the Doone alluvial tin mine in northeastern Tasmania. This 'drift' was thought by David to be a glacial outwash, belonging either to the Würm or even the preceding Riss glaciation and thus dating to between 20,000 and 100,000 years ago, which was the time range that David (1924) believed bracketed the antiquity of Aboriginal arrival in Tasmania. Other arguments were adduced to support a high antiquity, such as the fact that since the Tasmanians only had limited water-crossing ability, they had to have walked across a glacially exposed land bridge. This would have to have occurred before the arrival of the dingo on the mainland. There were also typological arguments derived from the unilinial evolutionist theories of the previous century, concerning the identity of the Tasmanian stone tool technology as belonging to the 'palaeolithic stage of culture'. The Tasmanians were seen as the first colonisers of the continent, being replaced on the mainland by the Australian Aboriginals; thus any age for the latter would imply a significantly older one for the former. Finally, in his own mind, having established an Aboriginal presence in Tasmania during the time span of two glacial periods, Edgeworth David drew upon his Antarctic exploring experience;

![Image](https://via.placeholder.com/150)

One can visualise [he said] the tiers of Tasmania under their snowy mantle with great glittering ice-fields between: can see the glorious sapphire blue of the deep crevasses where the ice-sheet plunged down the steep mountain escarpments of the West Coast, and with the waning of the last ice age;

view the highlands alone snow-covered, the white of the corrie glaciers, framed in dark rock, while all the rest of the isle is under a living garment of green. What changes the Tasmanian man must have witnessed. Probably some of these glacial phases with the gradual drowning
of the Bass land bridge which so effectively checked the Australian Pharaoh and his hounds on the Victorian bank. What difference, if any ensued in his culture as the result of this isolation from the mainland? (1924, in Horton 1991:136)

Seeing field research in Tasmania as being the key to an investigation of the prehistory of the continent, he urged exploration of the 'oldest and largest kitchen middens, like those of Swanport (and) the Derwent Estuary...; raised beaches and older dunes of King and Flinders Islands; peat deposits such as Mowbray Swamp; 'cave deposits, like those of Mole Creek'; older terraced river gravels, dating back into the Pleistocene; and stone quarries and 'places where they dug out lumps of haematite for pounding into raddle for colouring their hair' (1924, in Horton 1991:135).

I admit to a certain feeling of déjà vu that this field research agenda and scale of thinking might have been a blueprint for the past thirty years archaeological research in Tasmania. Yet the scientific base of David's poetic scenario was unfortunately as flimsy as a house of cards. The Devonport school teacher Meston (1937) scathingly pointed out that the Doone Mine find had been done with the aid of an industrial hydraulic nozzle – a California Gun, located at the base of an excavation 'half a chain wide' and 15 feet below the lump of gravel dislodged by the force of water and to which the stone tool was said to be adhering. In appearance the chalcedony was also fresh, with no sign of having a weathered skin nor being water-rounded, despite being found in the gravel. Meston also dismissed the claims of another geologist Lewis (1935), who had referred to a midden situated on a raised beach terrace in southern Tasmania which he claimed was dated to the Yolande-Margaret i.e. equivalent to the Riss-Würm Inter-glacial period. Meston pointed out that once the terrace had been formed, it would have been a suitable surface for camping upon even to the present day; and analytically, he showed that the species of shellfish within this particular midden were the same as could be found in a nearby contemporary shell bed. Meston had no particular theory to pursue, but in this paper he dispassionately dismissed any direct evidence then available for a Pleistocene antiquity for man in Tasmania. He felt that Tasmanian watercraft were capable of traversing the inter-island gaps of eastern Bass Strait, and that people had arrived across water to Tasmania in Postglacial times from the Australian mainland.

ROCKY CAPE AND TINDALE

Interestingly, at the time of writing this paper, Meston was himself carrying out several excavations in the midden deposits at Rocky Cape, which aimed at establishing an archaeological sequence for Tasmania. Although these were not conducted with any particular finesse concerning stratigraphic control, and were never published beyond a few notes, the exposed sections in the South Cave and the excavated stone artefacts were inspected by N.B. Tindale, on a field visit in April 1936, when he had been invited by the Government of Tasmania to referee the authentication of the recently discovered rock carvings at Mount Cameron West. His observations at that time became the basis for his postulated division of the Rocky Cape sequence into two typologically differentiated assemblages, one the 'Older Tasmanian series' which he thought were patinated and the 'Newer series' which he thought were not (Tindale 1937). He thought that there was also an older industry which had been found in Tasmania as collections of surface tools only, but which were similar to the flaked-cobble Kartan implements from Kangaroo Island off the coast of South Australia. He thus set up a threefold sequence for the southeast portion of Australia and its adjacent large continental islands, with a basal 'archaic Kangaroo Island industry' which he believed was typologically related to what was then referred to as the Malayan Upper Palaeolithic (cf. Hoabinhian) and 'may thus represent the type of implement culture which the first visitors to Australia brought with them' (Tindale 1937:55). This scenario was further refined twenty years later in his synthesis of 1957, with the Rocky Cape Tasmanian series being correlated with his Tartangan industry of the mainland sequence, and with the tools putatively made by the first colonisers of Tasmania still being assigned to the Kartan. Using arguments derived from the chronology of land bridges and the post glacial sea-level rise, together with geomorphological observations as to the presumed age of deposits from which artefacts had been found, Tindale placed this part of his sequence into the terminal Pleistocene and early Post-Glacial period. The later Tasmanian industries were seen as having been derived in isolation from the Tartangan, whereas on the mainland it was succeeded by a series of other industries characterised by the manufacture of bifacial points and other specialised small tools as defined in the Devon Downs sequence (1957; see also Bowdler this volume).

Impressive though this synthesis initially seemed to be when viewed as a totality, like that of Edgeworth David before, it also proved brittle when subjected to rigorous analysis of its
supporting evidence. It was to do this that Mul-
vaney devoted the bulk of his 1961 'Stone Age of
Australia' paper, concentrating on problems of
dating, stratigraphic isolation of typologically
defined stone tool industries and the distinctive
definition of some of these as justifiable separate
entities. When I came to excavate at Rocky Cape
in 1965, I found that there were no differences in
patination of implements down the sequence;
rather that there were differences in the raw ma-
terials used. My analysis (1966 and later 1971)
stressed cultural continuity and that the typo-
logical changes might have reflected the varying
mechanical characteristics of the stone materials
used, the different locations of quarries and other
economic changes that had occurred at the site
during the 8,000 years of its occupation. My new
interpretation, which I first delivered in summary
form at a lecture to the August 1965, Hobart
ANZAS was graciously acknowledged by Tin-
dale from the floor of the theatre. The genera-
tional succession of archaeological ideas and
method thus codified in that exchange, lay not
in differences which we might have had at the
broadest synthetic level, but rather that the new
data were based on a degree of stratigraphic
control and typological rigour previously unavail-
able.

The theory that Tasmanian Aborigines had
preceded Australian Aborigines also played an
important role in archaeological thinking on the
mainland. J.S. Falkinder led an excavation at the
southern New South Wales coastal sandstone
This work was organised by the Anthropological
Society of New South Wales and its main pur-
pose was to discover stone artefacts that had been
left by the first inhabitants who were believed
to be related to ancestral Tasmanian Aborigines
(Falkinder 1931/2). It is interesting to note that
Mulvaney, in his first descriptions of the flaked
stone artefacts from the lower levels at Kenniff
Cave, Queensland referred to them as 'Tasma-
noid', saying that they 'possess definite affinities
with the stone culture of the extinct Tasmanian
aborigines' (1964b:265; Mulvaney and Joyce
1965:186). The idea that the basal industries in
Australian mainland sites have Tasmanian affin-
ities has a long history.

THE OBDRATE CONTINENT

The Melbourne meeting of the British Asso-
ciation in 1914 also received two reports on
excavations being carried out at the large
palaeolithic cave site of La Cotte de St Brelade,
Jersey (Marett et al. 1915; Marett 1915). The
excavations by modern standards seemed more
muscular than careful, with 'the upper portions of
the cave-filling being 'demolished by a success-
ful piece of blasting which brought down some
eighty tons' (Marett et al. 1915:230). The artefact
and bone-bearing deposits thus exposed, proved
remarkably rich in Pleistocene fauna and Mous-
terian stone artefacts. There is a certain historical
irony in these reports being published at the 1914
Australian meeting, since fifty years later at this
same site, new and for their time exacting exca-
vation, stratigraphic, chronometric and typologi-
tical techniques were pioneered in the early 1960s
by McBurney of the University of Cambridge
(Callow and Cornford 1986). Taught to under-
graduates in field schools at La Cotte, these
became transferred as part of the core research
tools of a new generation of archaeologists who
took part in pioneering scientific prehistory on
the Australian-New Guinea continent. Between
1915 and 1960 in Europe, the discipline of pre-
historic archaeology, especially that branch
devoted to the archaeology of Pleistocene and
Holocene hunting and gathering societies, under-
went a series of technological transformations,
which resulted in vast improvements in excava-
tion strategies and analytical techniques which
were deployed on probably thousands of sites,
some of which have become the classic locations
of our discipline. In Australia on the other hand,
over a land mass of roughly the same area, al-
most no work of comparable quality was carried
out. There are the honourable exceptions of the
cave-excavations at the mid and late Holocene sites of
Devon Downs and Lapstone Creek, but it is also
worth emphasising that apart from exploratory
work at the Keilor river terrace (Gill 1954), not a
single Pleistocene stratified site was excavated
during this period. There is the additional para-
doxy that for a small colonial society, Australia of
the first half of the twentieth century, produced or
nurtured a number of distinguished archaeolo-
gists and human palaeontologists of international
repute. Apart from Childe, there were also
Grafton Elliot-Smith, Raymond Dart, James
Stewart and Dale Trendall, all of whom worked
outside the Australian-Pacific region.

To a certain extent, this indifference to the
prehistoric archaeology of Australia and Melan-
esia during the first half of this century reflected
a more general lack of interest in, or even con-
tempt for, the indigenous peoples of the region.
However it is important not to underestimate the
sheer brute difficulty of carrying out meaningful
field prehistoric research in Australian conditions
unless one has the full panoply of modern
scientific methods at one's disposal. It is not, I
believe, a mere co-incidence that Australia was
the last continent to have its deep prehistory
systematically investigated. The Australian land-
scape is in general terms ungenerous to the
archaeologist. Firstly it has been an extremely
stable continent, so that its topography over vast
areas has been worn down under an erosion
pattern established an immense time ago. Alec
Hope said it well in his poem 'Australia',
referring to the hills;

... those endless, outstretched paws
of Sphinx demolished or stone lion worn away.

There are entire river systems such as that of
the Finke in central Australia which had already
established their drainage patterns in pre-Tertiary
times (Mabbutt 1967:153). On the entire Austra-
lian continent there are no active volcanic land-
scapes and only limited areas of recent tectonic
instability with consequent erosion revealing
depth; a foot of laterised rubble separating the
original spindly grass of today from a rock forma-
tion peded since. This soil measures a mere 25 cm in
depth; a foot of laterised rubble separating the
spindly grass of today from a rock forma-
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depth; a foot of laterised rubble separating the
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depth; a foot of laterised rubble separating the
spindly grass of today from a rock forma-

The northern third of the continent has a
tropical monsoonal climate and elsewhere in the
arid hot interior regions, climate and soil combi-
ne to destroy the chemical structure of buried
bones, pollen and other organic materials. Charcoal
usually does not survive more than a

few centimetres below the surface of the soil. If
archaeologists and geomorphologists had wished
to search for locations where there had been a
regime of accumulation of deposits during the
Pleistocene, they could have found them — in
river terraces, sand dunes, alluvial plains, slope
deposits etc, much as Edgeworth David had indi-
cated in 1924. The problem remained however
as to how to date these using conventional geo-
morphological or palaeontological methods. The
still undeveloped state of Quaternary geochron-
ology in Australia as recently as the end of the
1950s was spelt out in depressing detail by the
then doyen of Pleistocene geological studies in
Australia, W.R. Browne (1963). The tectonically
active northern edge of the Australian Plate is of
course New Guinea, with its active orogeny and
in the outer chain of islands, with vulcanism.
However in the period under review, physical ac-
tess to many parts of New Guinea was difficult,
and it must be remembered that the first
European exploration of the highland valleys of
New Guinea was only made as recently as the
1930s (Sinclair 1988).

The European palaeolithic archaeologists had
of course their own independent methods of com-
parative dating, namely those based on the dif-
ferent typological characteristics of worked stone
and bone tool assemblages. In Australia how-
ever, as has been alluded to previously, stone tool
assemblages older than mid-Holocene times in
most parts of the continent were of such uniform
form as flakes, scrapers and core-tools, that little
chronological trend in their typologies could
be discerned, and thus they were of limited use
in trying to set up any seriated chronological
sequences.

THE RADIOCARBON TIME MACHINE

'A new time machine has been invented' wrote
John Mulvaney to the Melbourne-based literary
journal Twentieth Century in 1952: no doubt his
readers appreciating the allusion to the title of
H.G. Wells' futuristic novel [4]. This prescient

[4] How Jack Golson might have heard about radiocarbon
dating has been told by Gale Sieweking. They were both
working as student volunteers at the base of a waterlogged
trench in Star Carr in 1950, when they overheard Gordon
Childe who was visiting the site, telling Graham Clark,
standing on the edge of the excavation that 'this new dating
method, well they've tried it on the Pharaohs' tombs and it
works'. Apocryphal though it might seem, this story was
confirmed to me by both Sieweking and Clark, during a
lunch at the Royal Society, 26 February 1992 in conjunction
with the meeting on 'The origin of modern humans and the
impact of science-based dating'. There is a photograph of
Childe and Clark together at the 1950 Star Carr excavation
reference was to what is now recognised as having been the key technical development in 20th Century archaeology, namely the discovery of radiocarbon dating by Willard Libby and his colleagues at the University of Chicago in 1946, and which had come directly out of cosmic-ray research associated with the 'Manhattan Project' for the development of the atom bomb (Arnold and Libby 1949). In his final formalised 'Val­
ediction' to his discipline written from Australia two weeks before his death, Childe expressed a sense of anxiety that the entire intellectual method­ology of archaeology, which to him meant the system of typological analyses and the cross corre­lations of assemblages, might become subverted to the non-cultural mechanisms of the natural scientist:

In other words, archaeologists will abandon responsibility for chronology or themselves become nuclear physicists. In any case every prehistorian must master enough mathematics, physics and chemistry to appreciate the limitations of the information the latter can provide (Childe 1957b).

Radiocarbon dating however, acted to liberate archaeologists from the shackles of their typological past and in no other part of the world did the technique transform the field more than it did in the Australian and Melanesian theatres (Jones 1982:27). As early as 1952, E.D. Gill, geologist at the National Museum of Victoria in Melbourne had submitted a carbon sample from a coastal shell midden near Warmambool, Victoria to Lib­by's dating laboratory in Chicago. This yielded a value of 538±200 BP (C601, Gill 1955a:51-3); a result that Mulvaney commented upon, somewhat sardonically that it was 'not a very promising date, but it is a beginning' (1952:23). Leonard Adam (1954) reviewed Libby's book Radio­carbon Dating in Mankind in the Journal of the Anthropological Society of New South Wales. In his 1957 paper, Tindale included four radiocar­bon dates, some of the samples for which had been collected and stored before the method had even been invented. In 1961, Mulvaney pub­lished a table containing a list of 17 radiocarbon dates then available and which were deemed to be relevant to the question of the human occu­pation of the continent. Two of these were purely geological, and five came from Mulvaney's own Fromm's Landing excavation, and spanned a period between about 3000 to 5000 years ago. The others were merely spot dates with no coherent cultural association.

A REVOLUTION IN METHOD

Rather than seeing the development of Aus­tralian archaeology as a continuous one from these antecedents, I would argue that a genuine scientific revolution occurred, albeit on a small scale, of ideas and method, which not only transformed the field of enquiry, but also marked a sharp break with the past. This revolution occurred over a relatively short period of time, within the early 1960s, and was effectively over by the end of that decade, or at least by about 1972. This is not to say that crucial changes were not occurring before that time period, and more to the point that major discoveries were not still being made to the present day, but rather to use the terminology of the historian of science Thomas Kuhn (1962), the 'paradigm shift' was made during that brief period. We may consider these changes in terms of research technology, and field methods and also in terms of personnel and the institutionalisation of research and teaching support.

Radiocarbon dating, critical though it was, was but one of a plethora of scientific investiga­tive techniques which began to be deployed onto archaeological problems during the late 1950s. This was especially the case within the field of palaeolithic archaeology, where the links with geological and biological sciences have always been the strongest. The sub-discipline of archaeometry was being developed with its concentration on varied absolute and relative dating methods, from thermoluminescence and uranium series dating to the study of bone and soil fluorine levels or obsidian hydration rim dating (Aitken 1990). Chemical and physical techniques were used to analyse metals or ores and to source the quarries for axes and other stone tools. Perhaps the most important influence was at the scale of the landscape itself, with pollen analyses, developed initially in Scandinavia, being deployed onto British sites especially for the Late Glacial and early Post Glacial periods. Key develop­ments in this palaeoecological field were made by the Quaternary Research Group of the Uni­versity of Cambridge; a study which epitomised this integrated approach being carried out at the swamp-edge mesolithic site of Star Carr, Yorkshire (Clark 1954). Here the pollen analyses were done by Donald Walker who later was to take up the Foundation Chair in Biogeography and Geomorphology at The Australian National University and with his fellow member of the Cambridge Quaternary group, the geomorpholo­gist J.N. (Joe) Jennings was to play a key role in establishing a sound empirical base for palaeoen­vironmental studies in Australia and New Guinea during the 1960s and 1970s.

Within this school of archaeological thinking centred round the teaching of palaeolithic and
mesolithic studies in the Department of Archaeology at the University of Cambridge, and in Wheeler's and later Child's Institute of Archaeology at the University of London, human behaviour was predicated as having been profoundly influenced both by the technology and means of production of a society, and also by the ecological interaction of that society with its environment. This 'economic prehistory' as Clark was to term it (1952, 1989:v) came to dominate the thinking and practice of the new field researchers in Australia during the 1960s (Golson 1986:7, 10; Jones 1982:24-5).

SIEVES, SPITS AND SILICON CHIPS

Don Brothwell and Eric Higgs in the preface to their influential book *Science in Archaeology* (1963:15), stated that

not to use the scientific methods now available to archaeology is to commit the worst of archaeological crimes, to ignore available evidence or during excavation to destroy it.

Paradoxically, perhaps the most potent piece of equipment that transformed the practice of field archaeology during the late 1950s and early 1960s was not some boffin's delight but rather the prosaic garden sieve. Sieves of various mesh sizes began to become systematically used at British prehistoric sites during this period by archaeologists working within the 'ecological' tradition outlined above (cf. Mulvaney 1986:98), and especially on sites such as cave deposits where there existed a large amount of faunal and other environmentally significant finds. Their analyses shifted from being peripheral specialist's reports to attaining a central place in the interpretation of a site. Golson (1986:9) expressed this development precisely when he said of Eric Higgs' 'bone room' at Cambridge University's archaeological department in the late 1960s that 'in a real sense (it was) the nerve centre of research operations'. Later developments such as flotation (Jarman et al. 1972) and wet sieving (Johnson and Jones 1985) were to bring to systematic archaeologically attention whole sets of materials, such as plant fragments, which to a previous generation of archaeologists might have seemed beyond their capacity to investigate.

Side by side with retrieval went excavation itself. Whereas the excesses with dynamite that the British Association supported at La Cotte were no longer in favour, it still remained a problem as to how precise one was to record the three dimensional co-ordinates of the finds being excavated, especially in the case of a complex, deeply stratified cave deposit. One approach developed in the late 1950s by French and American archaeologists working on rich palaeolithic sites whose basic sequences had already been established, was to set up grids so that all recognisable artefacts could directly be assigned to their three-dimensional spatial co-ordinates. Logical though this system might be, there was the problem that without the aid of computers, the task of documenting and analysing these data, if the number of finds in a site measured hundreds of thousands of pieces or more, became prohibitive. An alternative approach exemplifying a British empirical sense was that of the 'spit', namely a flexible unit of excavation, the three dimensional parameters of which, could easily be measured and which could easily be aligned to stratigraphic boundaries. The excavations of McBurney at Haua Fteah and at La Cotte de St Brelade, refined these field methods, both in terms of recording and of recovery.

Probably the most significant development of all, has crept up on us slowly and almost unremarked, yet its influence has been transformational. I refer to the invention of electronic counting machines which led later to the computer revolution. In the July 1953 issue of the British science magazine *Discover* there was an advertisement for the first university post in prehistory anywhere in Australasia, namely a lectureship at the University of Auckland to which Jack Golson was appointed. On the opposite page there was an advertisement by the London Office Machine company of what then was the state of the art counting machine, namely a hand-cranked mechanical device that was said to have had 15 figures in its register and was useful 'for all statistical calculations such as summing squares, Mean Deviations etc.' Mathematical handling of archaeological data was beginning to become systematised during the late 1950s, mostly in the form of tabulations, but increasingly statistical tests were beginning to be applied, as can be seen in the review of the subject by Spaulding (1960). A central concept was that of a total population of attributes which approximated to a normal distribution and which thus could be described conveniently in terms of a mean and standard deviation and further subjected to such comparative tests as the T-test and regression analyses. A problem was that as recently as the middle 1960s all calculations had to be done by hand, aided by the use of mechanical counting machines such as the one described above. To calculate the mean and standard deviation of a population of say 100 measurements, involved several hours work, and
the difficulties can be appreciated by reading the methods proposed by the standard practical text book of the time, namely the highly influential *Facts from Figures* (Moroney 1956:66-81). As access to university valve-based computers became available so also some archaeologists tried to use the new technology to explore some of the statistical characteristics of their assemblages. McBurney (1967:360-73) devoted an entire section of his Haua Fteah report to an attempt at fitting log-normal curves of various characteristics onto the size parameters of some of his stone artefacts, and he employed his university’s Edsac Computer to do so. At Sydney, the university installed its first computer, a Silliac thermionic valve digital machine in 1956 (University of Sydney Gazette 1978:8). Hailed at the time as being able to do ‘three month’s work in ten minutes’, it is worth remembering that its computing power was equal to only a single microprocessor of 1978, and I do not know what the comparison might be with today’s silicon microdot technology.

Mulvaney followed McBurney in employing the concept of a population of attributes when he tabulated the size distributions of some parameters of his Kenniff Cave artefacts and these were presented as raw figure histograms with no further statistical treatment (Mulvaney and Joyce 1965:179-81). Carmel Schrire (1982) and Peter White (1972), carried out excavations during 1964-65, respectively in the Kakadu region of the Northern Territory and in the New Guinea highlands, and in the Ph.D. theses which emanated from this work, they presented extensive data sets on the distributions of various dimensions of their artefacts. Again like Mulvaney, these were presented as raw data in tables setting out the frequency distribution within each size interval, the operations being carried out mechanically with punched cards and knitting needles. By the end of the 1960s, using a desk calculator with a single memory function, I was able to present essentially the same information concisely in terms of means and standard deviations (Jones 1971). By 1970, using the ANU’s Dec 10 computer, fed with manually-coded IBM cards, I was able to carry out systematic bi-variate analyses of one attribute against another, and to measure the significance of each. Two or three years later, Lampert (1981:56-8, 73) carried out sophisticated multivariate discriminant analysis, comparing one assemblage with another. In this context, one must not forget Ian Glover’s (1969) pioneering factor analysis on Bondaiyan and other geometric backed blades from NSW; a study on only about 100 pieces, but which tested the capacity of the University of Sydney’s computer to the full. Even these analyses are commonplace today, and can be carried out on a home computer.

The development of computer-controlled excavation data bases had to wait until the availability of the portable machine in the late 1970s and early 1980s. Some of the excavations in the early 1960s were carried out in a way compatible with their data being entered onto the modern systems, although the precision of the older excavations was less rigorous than ones carried out today. It is also true that the amount of material obtained by using systematic retrieval methods in the field far outweighed the capacity to analyse them, and there was a feeling of being swamped by the material, much of which has not been systematically analysed. The computer data-base linked intimately with the excavation and laboratory is the way to proceed, as Johnson and I (1985) tried to do in the Kakadu Project of 1981.

All of the field and analytical techniques outlined above were becoming available to the science-based archaeologist during the late 1950s and early 1960s. They pointed inexorably in a single direction that there had to be a total integration of all of the materials obtained from an excavation; that artefacts had to be dealt with as total assemblages, and that the cultural material had to be integrated with the environmental data, to investigate human actions within the contexts of past environments. All of these developments constituted a radical new technology of archaeological investigation and it was armed with this that the first systematic exploration of the prehistory of the Australian continent was carried out.

**INSTITUTIONS**

Vincent Megaw (1966:306) called 1961 an *annus mirabilis*. This was the year not only when full university courses in Australian prehistory began to be taught, but it also saw the establishment of the Australian Institute of Aboriginal Studies. Childe in 1957 had seemed to show no particular interest in the finds that Mulvaney showed him from his recently excavated rock shelter at Fromm’s Landing – ‘no incisive comments about the artefacts or the nature of the site’ (Mulvaney 1990a:31). However at Mount Pleasant, the Bathurst home of James Stewart then professor of Middle-eastern Archaeology at the University of Sydney, Childe spoke enthusiastically to the young Laila Haglund about the great potential for research in Australia, and gave her advice that she should
gain skills in the scientific infrastructure of archaeological methodology, then being taught by Zeuner at the London Institute of Archaeology, from which Childe had just retired as Director (Haglund 1990:33). To his life-long friend, Mary Alice Evatt, the wife of 'Doc' Evatt, leader of the Australian Labour Party, he wrote about the prehistory of Australia in August 1957 that

'I could not at my age work up an interest in it myself, but I'm sure it's something worth studying and preserving. There are only 3 or 4 people working on it at all seriously with rather inadequate training and hopelessly inadequate resources. One university – probably ANU – ought to have a professorship or at least a readership in Australian or Oceanic archaeology. And antiquities ought to be preserved – particularly the Aboriginal rock pictures (Green 1981:149).

Both Australian and Oceanic archaeology were fortunate that the person selected to fill that founding ANU position in 1961 was Jack Golson. He was joined there in 1965 by Mulvany (1986:101), whose departure from Melbourne University marked the abandonment by that institution of any further meaningful role in the discovery of the prehistory of the continent. Isabel McBaye began teaching prehistory as part of the Ancient History Department at the University of New England in Armidale, and Richard Wright joined the Department of Anthropology at the University of Sydney to commence an option in world prehistory, which was based around a research programme in Australian prehistory. Sydney's well-established Archaeology Department had remained aloof from the prehistory of this part of the world, though on a personal basis individual lecturers, in particular Vincent Megaw (1966:311) and Judy Birmingham carried out important research here. Many students took advantage of the parallel courses in both departments and either officially or otherwise, availed themselves of the opportunities to gain a genuine perspective on world prehistory. Students in the 1960s included Jim Allen, Betty Meehan, Ian Glover, Leslie Maynard, Alan Thorne, Emily Glover, Anne Bickford, Harry Allen, Harry Lourandos, Johan Kammenga and Sandra Bowdler.

In May 1961, the Commonwealth Government, with the direct support of the Prime Minister, Sir Robert Menzies (1963) organised a conference on the current state of knowledge within a wide range of disciplines concerning Aboriginal culture and history (Sheils 1963). Issues of prehistory and physical anthropology were formally addressed by Mulvany (in absentia), McCarthy, Browne and Abbie. There was a general discussion on the issue of the 'antiquity of man in Australia', and a perusal of this somewhat rambling debate indicates starkly the poverty of hard facts upon which it was based. By the end of the year, the visionary and sometimes eccentric Liberal Parliamentarian, W.C. Wentworth had persuaded Menzies to set up the Australian Institute of Aboriginal Studies under an Interim Council chaired by the Classical archaeologist Dale Trendall. High on the agenda for the new Institute, which appointed F.D. McCarthy as its first Principal, was the fostering of fundamental research into Australian prehistory. Generous field work support by the Institute for young workers was a decisive factor in the subsequent explosive growth of knowledge in this field, and there were few important projects over the next decade which were not supported in some way by the Institute.

THE 1963 CONFERENCE ON THE NOMENCLATURE OF IMPLEMENTS AND CULTURES

Anzac Day 1963 saw the opening of a four-day conference at the Australian Museum in Sydney sponsored by the Institute, concerning the topic of the 'Nomenclature of implements and cultures', and which brought together for the first time, most of the archaeologists working in Australia (Megaw 1963:299-300). It had originally been planned as a forum for the ratification of, or adjudication between, fixed typological terms and concepts – the 'fossil indices' of a palaeontologically-derived view of the human past – and one reflecting the museum-based experience of the few scholars who had been employed professionally as archaeologists during the previous decades. A series of prepared papers presented to the conference showed however that this could not be done, not only because of the inadequacy of the primary excrated material, but also because the relationship between material form, type and culture became a forum for a free-flowing debate on new concepts concerning the relationships between archaeologically derived information on the one hand, and the cultural realities which may have been responsible for their formation on the other (Megaw 1963:299-301). The final session, according to the only published rapporteur of the event (Megaw 1963:300), was dominated by a paper presented by Golson (1963a), entitled 'Space and time in Australian archaeology'. Golson's aim was to 'present a model for the organization of archaeological data into a culturally and historically significant series of categories' and to examine the available Australian
data according to precepts which he had earlier developed in his synthesis of New Zealand prehistory (1959) which had been greatly influenced by both Childe (1956) and Willey and Phillips (1958). In Australia he saw a different order of scale both in space and time.

In Australian prehistory, with its vast area, its long duration and its varied (and changing) environments occupied exclusively by nomadic hunters and gatherers exhibiting no great variety of cultural origin, the (archaeological) culture promises to have considerable dimensions in both respects, and considerable internal diversification as a result (Golson 1963a:5).

For these reasons, he considered that the concept of the archaeological 'culture' such as was being used in the contemporary discourse was inadequate for the conceptual task in hand, and proposed instead the concept of 'phase' and 'aspect', the former being 'the temporal variant of the culture' and the latter 'the local variant of the phase' (Golson 1963a:6). Golson's approach at that time (1963b) was firmly within what he has referred to since as a 'culture-organised, artefact-based frame of archaeological reference' (1986:7).

This however was no naïve measurement, but one which referred on a technical level to new methodological developments such as the histograms of Bordes and Bohmers; a concept of type being based on an assemblage of traits, developed by Gardin in Paris; and to the seriation and matrix analyses devised independently by Brainerd (1951a, 1951b) and Robinson and by Tugby. The latter, from the Anthropology Department of the University of Queensland and a participant at the conference, had based his analysis on Australian flaked pebble tools and had published his results in *American Antiquity* (1958), thus entering the key American theoretical literature on the subject.

Golson felt that in Australia so little systematic research had been carried out that 'we are only just beginning to win the data that will permit of the formation of the basic units or aspects out of which the superstructure of cultures and culture phases will be built' (1963a:14). Furthermore, he stressed the primacy of the stratified archaeological site as being the basic unit for study, and that 'Fieldwork techniques should be strictly geared to the recovery of assemblages' (Golson 1963a:14-15). There was a need for a regional perspective and at that time, only in the southeastern part of South Australia, namely along the lower Murray River were there local assemblages from more than a single site.

Reading the literature of the 1963 conference indicates how radically the nature of conceptual thinking about Australian archaeology had changed, even in the three years since Mulvaney was writing his 1961 'Stone Age of Australia' paper, which he himself with ironic self-deprecation had described as a 'dead-and-gone world viewpoint of a European archaeologist' (Mulvaney 1986:100). Here was no longer a theoretical tradition derived solely from a British, and specifically a Cambridge source, but it had been transformed by social and analytical perspectives derived from the very place within American thinking from which by the late 1960s, the self-styled 'New archaeology' would be derived (e.g. Binford and Binford 1968). I think it is significant that many of the new recruits to the Australian academic scene as Golson himself had been, were all teaching or researching prehistory within anthropology departments, and through this experience gained the same disciplinary perspective as our American colleagues. The new Australian archaeology from this time onwards became strongly influenced by an anthropological perspective which marks an important break from the previous primary links with history and classics. Jim Allen and I (1983:166) have previously pointed out that second year undergraduate courses in prehistory at the Anthropology Department in the University of Sydney taught by Richard Wright in 1963, took as their basic texts the books of Taylor (1948) and of Willey and Phillips (1958). In the following few years not only were the key Americanist theoretical papers being taught, but also the basic fieldwork reports upon which they had been based, such as those of Griffin, Ford, McNeish, Jennings, Rouse and what seemed to us to be a distinctive school of Californian archaeology carried out by Heizer, Cook, Meighan, Treganza and others. What was attractive in this work was that the field situations seemed to be much closer to our Australian experience than were those from Europe. The influence of an anthropologically informed processual approach came to Australian archaeology not as a secondhand version of a trans-Atlantic exchange of ideas, but directly as a trans-Pacific one.

An analysis of the attempts within Australia during the mid 1960s and early 1970s to integrate or test the Aboriginal ethnographic record against an emerging archaeological database, and the impact that these comparisons had on the emergence of an indigenous theoretical perspective within Australian archaeology, is beyond the scope of this essay, but I hope to be able to develop the argument elsewhere (cf. Allen and Jones 1983; Golson 1986:5-6; McBryde 1986:20-3).
BEGINNING THE RECONNAISSANCE

Golson's brief at the ANU was to develop primary prehistoric research in Australia, Melanesia and neighbouring regions of southeast Asia. Although the bulk of his own active involvement was to lie in the second two areas, during the early stages of his Fellowship he also carried out fieldwork in Australia which was greatly to influence the course of future research. He quickly grasped the potential significance of geomorphological work then being initiated on the physical evolution of the Riverina Plain of the Murray-Darling, and he carried out a series of exploratory visits there in the company of soil scientists and other geomorphologists. Work in this region by Jennings (1968), Pels (1971) and Bowler (1971), eventually resulted in a co-operative venture with archaeologists which led to the Mungo discoveries. On the south coast of New South Wales in 1964, Golson directed staff archaeologist Ron Lampert to initiate a research project, which led to a re-investigation of the Pleistocene basal levels of the Burrill Lake rock shelter (Lampert 1971).

However Golson's own major personal commitment in the field to Australian archaeology was in the joint expedition that he and Mulvaney carried out to the Katherine-Kakadu region of the Northern Territory in the dry season of 1963 (see Mulvaney this volume). Funded by a grant from the Nuffield Foundation, they decided to split their research areas. Mulvaney worked to the west of the Stuart Highway, investigating sites such as Delamere which had previously been reported by Davidson (1935) and excavating the limestone cave of Kintore and the sandstone rock shelter of Yingalarn (= Ingaladdi). Golson had contacts with Wally Arndt, an officer at the Agricultural Research Station at Katherine who had carried out ethnological field research in the then little-known headwater valleys of the Katherine River on the southwestern edge of the Arnhem Land plateau (Arndt 1962, 1966; Macintosh 1950). Golson was accompanied by Arndt's main Aboriginal research partner, the senior Jawoyn man 'Soupy' (or Kewpie) Marapunya (Arndt 1962:301) to the vicinity of a key ceremonial site associated with the restricted Bula cult near the uranium prospect of Sleisbeck in what is now known as the 'Sickness Country'. Excavations were carried out at a stratified rock-shelter which, despite a general proximity to the nearby religious site was a secular location and was in any case fully supervised by senior initiated Aboriginal men. The site yielded the same general two-fold typological sequence that Mulvaney had isolated stratigraphically for the first time in his Kenniff Cave excavation of 1960 and 1962 and which was still under analysis. At Sleisbeck, the upper industry had a distinctive characteristic of containing many short rectangular 'Leilira'-type blades, retouched along the sides and also transversely, in some cases having a hollow transverse end. Golson and Marapunya then proceeded northwards from Pine Creek, through what is now the Kakadu National Park, past Cahill's Crossing on the East Alligator River and on to Oenpelli; all the while following a traditional trading route. They met other Aboriginal men and some trading activities were casually carried out (Golson pers. comm.). Near Oenpelli were the rock shelters on Inyaluk and Arguluk sandstone outliers which had been previously excavated by McCarthy and Setzler in 1948 during the American-Australian expedition to Arnhem Land. Using the terminology of the day, based on the artefacts that they had found there, they thought that they had an industry consisting of elements of the Bondaian, Eloiuran, Kimberleyan, Pirrian, Mudukian and Munudian (McCarthy and Setzler 1960:286). Golson had re-analysed this report for the 1963 Nomenclature Conference and had found marked differences in the percentages of different artefact types in the two sites (White 1967a:7). He suggested that there were 'two distinct entities presented as a single "culture"' (Golson 1964:5). There existed therefore a research problem to be resolved by applying the new field methods. Having made requisite contacts with the Aboriginal owners, he suggested to Carmel Schrire (White) that she embark on her research project there in 1964.

In the dry season of 1962, Richard Wright made his first exploration in Cape York and discovered a deep sandstone shelter called Mushroom Rock near Laura. This site also contained a basic two-fold typological sequence, with an upper assemblage containing burrton-type adze slugs and a lower one with steep edge scrapers and other forms similar to the bottom half of the Kenniff Cave sequence. There were radiocarbon dates back to about 7000 years ago for the upper part of the deposit, but the basal levels, which contained numerous stone artefacts, had no charcoal at all. Isabel McBrady (1974) was carrying out her integrated regional work in the New England area. Vincent Megaw began his site surveys on the coast south of Sydney and excavated the Curracurrang coastal rock shelter; Ian Crawford was working in the Kimberley; and Dr Alexander Gallus was exploring karst caves on the Nullarbor Plain. These workers were
reinforced by a new generation of professionally trained archaeologists, some in their early twenties, who took up research scholarships or junior university research and teaching posts as they became available. It is historically the case that in Australia at that time, most of the active researchers had been trained either fully or partially at Cambridge, a situation commented upon from somewhat different perspectives by Clark (1989:11-20) and Mulvaney (1990c).

Peter White did a disservice both to his own finely conceived and conducted doctoral research project in the New Guinea highlands (White 1972) and also to the work of his contemporaries, when he and Murray (1981:257) said that

There was no real research design in this period: digs were conducted close to home bases because of easy access ... or in Arnhem Land, central Queensland, Cape York and Tasmania because there was money and time to go there.

For my own Tasmanian reconnaissance, my research aims were set out explicitly (Jones 1965, 1966, 1971:17-66). These were to find sites with undisturbed stratigraphic integrity; to carry out excavations at these in order to set up chronologically controlled sequences of well provenanced assemblages of both artefacts and faunal material; to choose a set of sites within a region so as to be able to attempt to investigate the interactions of a society with its environment over the long time span afforded by the prehistoric perspective; and finally to consider how this regional archaeological history illuminated the broader question of the initial colonisation of Tasmania and the relationships of Tasmanian to continental Australian societies within the time frame and changing ecological conditions of the Pleistocene. Wide though these aims might have been, the actual attack in the field was conducted along a narrow front, concentrating on specific field problems raised by the previous work of Tindale (1957) and Mulvaney (1961).

Equally, Schrire for her Alligator Rivers work set out a similar field programme within the different ecological context of tropical Australia (White 1967a). Her final synthesis led her to one of the key theoretical issues of palaeolithic archaeology in the 1960s, and one that would have been close to Gordon Childe's heart, namely to what extent, were typologically distinguishable stone tool assemblage sequences the products of diachronically maintained cultural traditions, and to what extent did they reflect seasonal or other functional aspects of the same culture. This formed the basis of the great debate between Bordes and Binford at the time, concerning structural and functional variability in the Mousterian industries of Europe; and Schrire's joint article with Nic Peterson, albeit presenting a different conclusion to the one she had previously reached in her thesis, was published in the leading theoretical anthropological journal of the day (White and Peterson 1969). When I used the term 'cowboy archaeology' (Jones 1979:447), to describe this phase of fieldwork and research, was there not also a touch of irony?

GANING THE PLEISTOCENE

The explosion of knowledge that occurred concerning Australian prehistory during the 1960s and 1970s has already been well documented (Mulvaney 1964a, 1964b, 1969; Jones 1968, 1973, 1979, 1989; Golson 1971a; White and O'Connell 1982; Flood 1983). I will concentrate here upon one significant index of that research surge, namely the crucial question of the antiquity of human occupation of the Australian continent. During the 1958 ANZAAS Congress held in Adelaide, Mulvaney gave a lecture concerning his Fromm's Landing excavation and stated that the oldest date for stone artefacts within a reasonably secure stratigraphic context was one of 8700 BP at Cape Martin on the coast of South Australia (Mulvaney 1971:368). In 1962, he announced the first secure date which broke through the Pleistocene barrier, with a value of 12,900 BP (NPL 33) from a depth of seven feet in his excavations at Kenniff Cave in southern Queensland (Mulvaney 1962:137). At the Canberra ANZAAS Congress in January 1964, a further date of about 16,000 BP (NPL 68) was obtained from a depth of 7 feet 5 inches to 7 feet 8 inches at the same site with artefacts stratified within older deposits (Mulvaney 1964a:42, 1964b:265). By 1967, there were confirmed dates of just over 20,000 years from the debris of flint mining in the underground Koonalda Cave on the Nullarbor Plain (Wright 1971); and also dates of 26,000 (L.J. 204) and 18,000 (GaK 335) from an unit on the Lunette dune of Lake Menindee in western New South Wales which seemed to have a stratigraphic association between stone flakes and the bones of extinct giant marsupials (Jones 1968:187). In New Guinea, Bulmer (1964) and White (1972) had obtained dates of between 10,000 and 11,000 years BP for stratified cultural deposits in the caves of Kiowa and Kafiavana respectively.

In the Northern Territory, Schrire (White 1967b) obtained dates of between 18,000 and almost 25,000 BP from sands underlying shell midden deposits at the rockshelter site of...
Malangangerr and 21,500 at Nawamoyin. Remarkable though these dates were in themselves, their significance was enhanced by the fact that they were associated with small edge-ground axe heads several of which were grooved and made from volcanic or metamorphic rocks. This caused something of a typological storm, that in the continent of hunters there were edge-ground axes, some three times as old as any in western Asia or Europe. Confirmation of these dates and their stratigraphic association was provided by the geomorphologists J.N. Jennings and M.A.J. Williams with further geochronological support from the recently established C14 laboratory at the ANU. Between 1964 and 1967, White and his geomorphological colleagues K. Crook and B. Buxton (1970) obtained carbon dates ranging between 23,000 and 26,500 years for a level within the open site of Kosipe in the New Guinea highlands which contained a number of large lenticular core-tools with well-made opposed notches in their sides, and thus referred to as 'waisted blades'. The rock type from which they were made consisted of metamorphosed phyllite or basalt and their presumed use was as hafted axe-like tools. Similar waisted blades and ground stone axes were also found in the Yuku rockshelter in the New Guinea highlands, within levels which in 1967 were still undated but which were presumed to be of late Pleistocene age (Bulmer 1964:256-7). Golson (1971b) in a paper written early in 1968, compared the similarities in form and probable function of these late Pleistocene edge-ground axe heads from tropical Australia with the equivalent waisted blades from New Guinea. He also drew attention to similar artefacts within the presumed late Pleistocene and early Holocene archaeological record of Southeast Asia, even as far as Vietnam. He considered that the general technology of grinding and also hafting, using techniques of waisting, stemming or grooving the axe heads, had a high antiquity in this part of the world; and the fact that this had not previously been recognised was due to a typological 'burden of Europe' (1971b:129).

In a review paper which I presented to the Christchurch ANZAAS conference in January 1968, concerning the available archaeological and palaeoenvironmental evidence which pertained to the question of the arrival of people on the continent of Australia and New Guinea and to the subsequent geographical colonisation, I referred to a total of ten sites with reliable evidence for human occupation over 10,000 years ago; the oldest date being about 25,000 years ago (Jones 1968:190). By 1973, a similar survey showed 28 sites older than 10,000 years with the oldest date being almost 33,000 years ago (Barbetti and Allen 1972; Jones 1973:279). A recent systematic survey of the literature carried out by Sharp and Smith (1991), indicated that there were 170 sites with human occupation greater than 10,000 years for the region, which now also included parts of island Melanesia; and that the rate of discovery of such sites was increasing steadily.

**LIMITATION OF THE RADIOCARBON DATING METHOD**

Concerning the question of the oldest dates; values in the mid thirty thousand range had been achieved by the early 1970s, with Bowler (1976:59-60) reporting to the Australian Institute of Aboriginal Studies conference in 1974, a date of between 34,000 and 37,000 BP (N 1665) for a shell midden layer of transported freshwater mussels near Lake Mungo in western New South Wales. During the last fifteen or so years, there has been no significant increase in this order of magnitude, at least as revealed by radiocarbon methods (Jones 1989:760-5). What has happened is that well dated sequences going back to the early or mid thirty thousand mark have been established in a wide range of ecological zones on the Greater Australian continent and the closer oceanic islands to its east. Key areas from which these facts have been gained over the past decade have been the central deserts, the northwest coast of western Australia, southwest Tasmania and on the islands of the Bismarck Archipelago, such as New Ireland and even Buka in the northern Solomon Group (cf. Jones 1989:756-9, 766-71). These facts have led to a division of opinion; one group of scholars, including Allen (1989) taking the data at face value and arguing that indeed the human colonisation of the continent took place in the time period of the order of 35,000 to 40,000 years ago.

I have long been suspicious of radiocarbon dates of this order of magnitude, that they are close to the theoretical limits of the method in practical terms. For a sample of infinite age, it would only require contamination by one percent of modern carbon to give an apparent age of about 38,000 years (Aitken 1990:86). My own view, put forward in successive publications over the past decade has been that the time of human arrival on the continent lies beyond the limits of the radiocarbon method, and that we are in danger of being trapped by the very method which first gave us access to the human Pleistocene world of Australia (Jones 1982:30). New
developments in the thermoluminescence (T.L.)
dating of naturally deposited sands, give us for
the first time, a tool to investigate directly the
time period beyond 40,000 years. A series of
T.L. dates from the sand column at the Kakadu
site of Malakunanja 2, have indicated that the
first stone tools were stratified within sands dated
to between 50,000 and 60,000 years ago; with
culturally sterile sands below, dated back to
110,000 years ago (Roberts et al. 1990a).
Critiques of both the method and the associations
of the sand with the artefacts were made by
Hiscock (1990) and Bowdler (1990) and replied
to by ourselves (Roberts et al. 1990b, 1990c).
Current research into the T.L. and optically
stimulated luminescence [O.S.L.] dating of the
cultural sand deposits at the nearby Lindner Site,
Nauwalabila I in Deaf Adder Gorge, within the
Arnhem Land escarpment, also indicates that the
oldest stone artefacts were made by
Bowler, Jennings and Crook, and the archaeo-
lologists Mulvaney, H. Allen and myself, showed
that the remains were those of a person whose
bones had been cremated and then buried in a
small pit. Identified later as being those of a
young woman, the shattered and cemented bones
were reconstructed by Thorne into a calotte,
which proved to be that of a gracile individual.
These funeral procedures were similar to those
that had been documented ethnographically for
both Tasmania and parts of the southeastern coast
of mainland Australia. Meehan (Hiatt 1969), had
already postulated that this distribution implied
an antiquity greater than the time of the flooding
of Bass Strait and she predicted that cremation
of the form indicated above would be one of
the disposal methods practised in the terminal
Pleistocene in southeastern Australia.

Field research at the Mungo site by Bowler,
Allen, Meehan and myself a week or so after the
original discovery found stratified stone tools and
small circular patches of charcoal. The latter
contained the burnt remains of animal and fish
bones and of fresh water mussels, which we
interpreted were hearths. In the first report on
the site (Bowler et al. 1970:55), Allen and I
explicitly took as our explanatory point of refer-
ence, Allen's ethnographic land use thesis for
western New South Wales (1968) as a model
against which to test the archaeological material.
Our conclusions were that with the exception
of grass seed grinding (cf. Allen 1974), the
economic evidence from Mungo did not refute an
hypothesis of a flexible mixed foraging strategy
using the fish and mollusc resources of the lake
edge with emu eggs and small mammals from the
scrub behind. We envisioned people living in
hearth groups along the centre of the dune close
to and parallel to the lake shore.

The stone tools at the site consisted of steep-
edge scrapers, many having noses and notches,
and of dome-shaped horse-hoof cores (or core-
tools), most being manufactured from silicified
quartzite (Bowler et al. 1970:47-52). In general
terms, they were typologically similar to the
basal and presumably late Pleistocene and early
Holocene assemblages at Kenniff, Mushroom
Rock near Laura, Capertee, Burrill Lake, Yinga-
larri (Ingaladdi), Malangangerr (without the axes)
and several other described sites on the mainland of Australia. They were also similar to assemblages such as at Kafiuana in the New Guinea highlands, and those found in Tasmanian sites throughout the Holocene up to historic times (Jones 1971). Mindful of the strictures of the Nomenclature Conference, Allen and I did not seek to clutter up the literature with a ‘Mungonian’ culture, but chose instead to view all of these assemblages as manifestations of a single, broad ‘Core-tool and scraper tradition’ [5]. The term ‘tradition’ was seen in the sense proposed by Willey and Phillips (1958:37). We thought that the assemblages within this tradition had sufficient typological ‘character’ over the geographical spread of the continent, to be ascribed the prefix of ‘Australian’ (sensu lato). We saw it as being deployed on the sort of spatial and chronological scale as such historical entities as the European Upper Palaeolithic blade-based industries, the Hoabinhian of southeast Asia, or the southern African ‘Middle Stone Age’, all of which might have shared some chronological overlap with the Australian industries in question. Childe put it that the usefulness of an archaeological type is proportionate to its improbability (1956:35-7). Whether or not the flaked stone tool assemblages of the Australian Pleistocene and of Tasmania following its post glacial isolation have sufficient diagnostic ‘character’ in this sense, is a question for future research (cf. Allen and Barton 1990:113). It is an interesting irony that of all the excavated Pleistocene assemblages now recovered from Australian sites, the ones which differ greatest from this somewhat generalised and typologically encompassing group are those recently found in the upper levels of sites such as Kutikina, Numamira and Bone Caves from western Tasmania which are dominated by tiny thumb-nail scrapers and which are dated, at least in the former site, between about 17,000 and 14,000 years ago (Jones 1990:279-81; Cosgrove 1991; McNiven et al. in press).

Despite the break implied by the appearance of the various ‘small tool’ assemblages in mid-Holocene times on mainland Australia, my opinion in the early 1970s was that there existed broad patterns of historical and cultural continuity linking the ethnographically observed Aboriginal societies and their presumed ancestors, even as far back as the time of the Mungo site. In an address to the INQUA Congress in Christchurch in 1973, I said that

The Aboriginal economic system was flexible enough to encompass all the variety of modern-day Australia, and there is no a priori reason why it could not also have been able to cope with differences of the same order of magnitude which may have affected any particular region over the past 30,000 years. My own view is that this time ago, the distinctive Australian economy was already in train and that the major adaptations to the continent had been made (Jones 1975:28).

CONCLUSION

My analysis is that by the late 1960s or the early 1970s a ‘modern’ approach towards the prehistory of Australia had been produced. By modern, I mean that works produced from this time onwards seem totally intelligible when considered from the perspective of today. Works written in the late 1950s or even in the mid 1960s seem to me to be strange and foreign – my own papers included; whereas the best works of the early 1970s do not. Using the Kuhnian terminology, a paradigm shift had occurred and there was a genuine scientific revolution in both technical methods and theoretical perspectives within the field of Australian prehistory (see also Golson 1986:6). We are still operating substantially within that same paradigm today. To illustrate my point, consider the following works; the best Ph.D. theses and site reports of the late 1960s and early 1970s; the papers at the ‘Aboriginal Man and Environment in Australia’ conference (Mulvaney and Golson 1971); the papers and the general research ambience at the 1971 Orientalist Congress in Canberra; the three week field excursion, funded by the Aboriginal Institute which followed it; and reportage of the results of archaeological research to the general media (Manning 1971). By the time of the major Aboriginal Institute conference in 1974, with international participation including such creative archaeologists as Bordes, Binford and Isaacs, the course of Australian prehistory had already been firmly set, as illustrated by the books of essays edited respectively by Kirk and Thorne (1976), Ucko (1977) and Wright (1977) which emanated from it. It is indeed possible that the key intellectual changes had already been made by 1968, which implies that the period of the most intense changes spanned a brief period of only some six

[5] The industries of the Australian mainland during the mid and late Holocene had already been dubbed ‘the Australian Small-Tool Tradition’ by Gould (1969:233), partly as a result of a typological misunderstanding. Gould (1968), in his description of worn-down flake ‘slugs’ recovered from his excavations at Puntutjarpa near the Warburton Ranges in Western Australia, had referred to these as ‘microliths’, a term which in Australian parlance, following that of European prehistory was reserved for backed geometric forms such as the ‘Bondi Point’. This problem was pointed out by Glover and Lampert (1969:224), and was neatly resolved by Gould who coined a portmanteau term to cover all of the tool types found in the late Holocene assemblages.
or seven years. Given this interpretation, a new technical and analytical perspective was deployed which quickly transformed the database concerning the prehistory of the Australian-New Guinea continent. I am not for the moment trying to say that fundamental discoveries have not been made in this field over the past twenty years; indeed as I stated in a general review in 1989 (p.759), 'the expansion of the database is still outstripping our capacity to incorporate it into our synthetic historical frameworks'.

It is not every generation of archaeologists who have had the privilege of engaging in the primary exploration of the prehistory of an entire continent. We have, I sense, already reached the metaphorical 'corners of the room'; we know the outline. The work into the next few decades will consist of the deeper analyses of problems already perceived, though not yet resolved. Childe in his Hobhouse Memorial Lecture delivered at King's College London in 1949, said that

The domain of rational human action is not a world apprehended, nor even apprehensible, by the senses of any single individual, but a world of ideas, a collective representation built up by the unconscious co-operation of millions of individuals over thousands of generations, an aspect of culture transcending each individual who participates in and contributes to its formation. (1949:7)

Here is the very essence of Australia's prehistory.

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There are many ways of viewing the past. In this chapter I want to look at some of the ways in which archaeologists working in Australia have viewed the prehistoric past. Obviously this is not intended to be an exhaustive or definitive view, and it will certainly not be an objective one. It is hoped however that some real flavour of what we have been about in the last thirty or so years will emerge, along with an idea of where we might go in the future.

In a very broad sense, there are two ways of viewing the past. On the one hand, as once expressed at a conference by my colleague Annie Bickford, one may consider the past to be the same as the present, only old. On the other hand, one may consider the past to be virtually unknowable. One school of thought suggests that classical Greek drama 'remains desperately foreign', 'very alien', not 'recoverable by us' (Finley 1972:13). If this is a judgment on a culture from which ours is conventionally thought to derive, what price our understanding the prehistoric past of another culture altogether?

What is it about the past we wish to find out? The usual textbook response is to list three aims of archaeology as a discipline, as follows. Firstly, archaeology may be concerned with the construction of past lifeways; secondly, it may concern itself with culture history; and thirdly, it may aim to answer questions of 'process', that is, address itself to the underlying mechanisms which dictate culture change, and hence, culture history (e.g. Fagan 1980:3). Nowadays, we have also a self-proclaimed 'post-processual' archaeology, whose aims have not, in this sense, been clearly defined, but appear to involve 'reconstructing the past' (e.g. Hodder 1986:169).

I do not intend to discuss any consciously theoretical contributions to Australian prehistory; I wish rather to look at the substantive writings of archaeologists working in this region, and try and see what 'views of the past' both dictated their approach, and emerged from their research and analysis. I would contend at the outset that most archaeologists who have made substantial contributions to Australasian prehistory have had as their underlying aim one or both of the first two possible aims adumbrated above, namely the reconstruction of past lifeways and the delineation of culture history, but with an ecological emphasis. In more substantive terms, they have wished to understand how people lived in mainland Australia, Tasmania, New Guinea and associated islands before the arrival of Europeans; they have concerned themselves with the place of origin of the earliest migrants to the region, and the origin of new cultural traits discernible in the archaeological record; and they have wished to understand how people have adapted to new and changing environments.

Within these various areas of concern, different positions have been occupied on the spectrum from 'like the present, only old' to 'desperately foreign'. In this connection, it is important to understand the use that has been made of this region's rich ethnographic record. This in turn relates to the extent that archaeologists in Australia have been conscious of investigating the Aboriginal past, or alternatively have simply dealt with a subject called 'Australian prehistory'.

The establishment of a time depth and of a measure of human antiquity has been crucial. I will not deal with that topic per se, since it will be covered by my colleague Rhys Jones elsewhere in this volume. Before time could be controlled as a dimension, and partly due to the perception of Aboriginal society as 'primitive' and unchanging, what passed as archaeology in Australia was generally only an interest in artefacts, a form of stamp-collecting. The first real attempt to calibrate the Australian past was made by Tindale in the 1930s (Hale and Tindale 1930; Tindale 1937, 1941). This was pretty much a one-person effort, until the advent of radiocarbon dating in the 1950s. Only subsequently does it become fruitful to consider Australian archaeological views of the past.

The 1960s saw the firm beginnings of professional archaeology in Australia, with the appointment of several prehistoric archaeologists to university posts, including Jack Golson. Certain strands of enquiry were established during this period. The most obvious one is of course the question of antiquity, which I have said I will not
address here specifically, but it is a perennial and legitimate base-line question, underpinning most Australian prehistoric research.

Other enduring interests were the relevance of the ethnographic data base, the importance of an 'economic' approach in Australian prehistory, the problems of stone tool typology with decidedly unEuropean assemblages to work with, and the question of Asian connections, both originally (and crucially tied to antiquity) and more recently. What underlies much of our enquiry has been, and remains, a concern with the nature of prehistoric Aboriginal society.

THE 1960S: ARCHAEOLOGY AND ETHNOGRAPHY

During the 1960s, I think it can be suggested that the future and enduring character of Australian archaeology was influenced by input from the disciplines anthropology and history, specifically Aboriginal anthropology and history. This can be economically demonstrated by referring to Jones's first published paper on Tasmanian archaeology (1964-5). This paper was written after Jones's first field trip to Tasmania in 1963-4. The object of the research was 'to try and isolate total industries for the purpose of setting up descriptions which might be compared with mainland sites, and also to investigate any ecological or geographical variations and adaptions [sic] within the island' (Jones 1964-5:191). His conclusions to that paper included the following. The conservation of materials in some sites provided 'an opportunity of applying the economic approach to prehistory', while 'the stone industries are complex enough to enable a typological study to be made'. Furthermore, 'the discipline of archaeology has a special contribution to make to Australian anthropology. In Tasmania some examples where archaeology can correct or supplement ethnology are in the discoveries of the stone arrangement, the multiplicity of bone tools, and the abundant fish bones' (Jones 1964-5:200).

In his next relevant paper (1966), Jones's conclusions were considerably more sophisticated. No longer were typological studies and the economic approach seen as separate entities, but what people ate and what they did with stone tools were seen as part of an overall integrated economic system. Jones also recognised that the archaeology was not in some sense a 'corrective' of anthropology, but in fact an aspect of it; fish bones and bone tools in the archaeological record did not contradict ethnographic observations, but supplemented them and demonstrated change through time. In one sense, obviously, the change in view was due to further analysis, wider reading and more considered thought. In another sense, however, I think we may be permitted to see a step away from a more European attitude to the investigation of a remote past, towards a more Australian one of investigating the lives of people but recently gone (in a cultural sense) from the scene.

In the 1964-5 article, the only ethnohistorical references were to the 19th century compilations by Bonwick and Ling Roth. In the 1966 article, there were a plethora of references to primary sources: Labillardière, Péron, and particularly George Augustus Robinson.

It has often been said that the publication of Robinson's journals by Plomley in 1966 marked a milestone for Tasmanian studies. It seems to me however that the significance lay in the recognition of that significance. In this sense, just as important was the Honours dissertation written in 1965 by Hiatt, entitled 'Some aspects of the economy of the Tasmanian Aborigine' (University of Sydney, subsequently published as Hiatt 1967). This work made extensive use of the Robinson journals to address the question of Tasmanian economy at the time of European contact. It did not however just look at simple questions of economy in the European sense, virtually synonymous with 'diet', but addressed wider questions about Tasmanian society, such as the division of labour, the size and movement across the landscape of groups, and the nature of Tasmanian religion. To do this, Hiatt used the more detailed ethnographic literature about mainland Australian Aborigines to provide a model framework for her inquiry. This was, in short, a very anthropological work, as well it might be, since it was carried out within a Department of Anthropology. I believe this work, and this approach, were very influential, both on the way Tasmanian archaeology developed, but also in laying groundwork for similar approaches elsewhere.

Meanwhile, simultaneously at the University of New England, a similar but more historical approach was initiated under the guidance of Isabel McBryde. Similar theses were prepared within the context of her regional archaeological study of New England, some of which addressed questions of immediate significance to archaeological research such as economy and subsistence, while others were forerunners of the now flourishing subject of Aboriginal history (Sullivan 1964, 1970; Belshaw 1966; Brayshaw 1966; Harrison 1966; Campbell 1969; Lane 1970;
when however a symposium was convened in day, no such voice was heard (Meehan and Jones protest, in favour of a mor e 'processua l' approach. ethnohi storical record to interpret or amplif y their the south coast of New Sout h Wales represents historic societies generally accept that there is direct exists. The occasi onal voice has been raised in perhaps the first time, an attempt was made to publish a fully-integrated Australian site report, which described in detail not only the excavation, stratigraphy and artefacts, but also brought a detailed description of faunal remains into the body of the text (rather than relegating them to an appendix), and furthermore used this data in conjunction with ethnohistorical records to present a rounded picture of Aboriginal life at this place in the recent past.

Similarly, in 1967, White (1967b) drew on her Ph.D. researches to present a short 'prehistory of the Kakadu people'. In this instance, however, the anthropologist Peterson suggested that White had interpreted her ethnohistorical sources somewhat too literally. They subsequently published a joint paper (White and Peterson 1969) which modified White's earlier conclusions in the light of Peterson's detailed knowledge of contemporary Aboriginal life in the Northern Territory: at this stage, a wider anthropological model of Aboriginal behaviour was preferred to the apparent specifics of the ethnohistorical record (but see also Schrire 1982). Peterson himself (1971) further explored the question of the anthropological contribution to Australian archaeology.

At this time, there was of course a certain amount of international literature which discussed the use of 'ethnographic analogy' (e.g. Ascher 1961; Binford 1962; Chang 1967). Much of this was addressed to ethnographic analogies transported across rather considerable gulfs of time and/or space. Ascher (1961) and others were rather more tolerant of what he called the 'direct historical analogy', where there was continuity between the archaeology and ethnography in time and space. In general, Australian archaeologists have accepted that, for them, such continuity exists. The occasional voice has been raised in protest, in favour of a more 'processual' approach. When however a symposium was convened in 1983 specifically to allow such dissenters their day, no such voice was heard (Meehan and Jones 1988:ix).

It seems therefore reasonable to assert that Australian archaeologists working with prehistoric societies generally accept that there is direct cultural continuity between Aboriginal society as we understand it to have been in the period of first contact with European society, and the immediate prehistoric past. There are however some problems which can be raised, even given that consensus.

**THE QUESTION OF CHANGE**

The main question with which we must deal is the question of change. This has wide ramifications, as we shall see. In the first instance, however, we need to consider to what extent the 'ethnographic present' has any reality at all. It has been argued (e.g. Rose 1987) that the anthropological concept of Aboriginal culture in the ethnographic present is a construct with little reality. It has also been suggested that Aboriginal society in what is considered the ethnographic present is in fact Aboriginal society in a peculiar state, that of being impacted by alien cultures. These are to some extent questions of anthropological and historical methodology.

What concerns us more as archaeologists is the question of change in prehistoric time. Put one way, if we rely on ethnographic analogy to reconstruct past lifeways, to what extent might that mask our understanding of culture history, or indeed the process of change? In 1970, I attempted an exercise like Lampert's, in an analysis of a south coast shell midden intended to integrate faunal, artefactual and ethnographic evidence. The sequence I was dealing with was considerably longer than that at Durras North, and (like Burring Lake and Currarong, Lampert 1971) showed evidence of artefactual and dietary change through time. I argued at that time that 'we can push ... interpretations of behaviour back into the past only so far as the artefacts appear unchanged' (Bowdler 1970:1), but that if change was restricted only to a single artefact (the particular case in point was shell fish hooks), then minor behavioural change might be interpreted within a context of general continuity (Bowdler 1970:2; see also Bowdler 1976).

This however avoided the wider issue: to what extent has Aboriginal society changed, or not changed, since the colonisation of Australia? What do the changes in the archaeological record really seem to indicate about past behaviour? I think it can be suggested that, until recently, the Aboriginal past was thought of as like the ethnographic present, only old. Only more recently has it been suggested that change may have been profound.
AUSTRALIAN ARCHAEOLOGY: CHANGE THROUGH TIME

Change in the archaeological record was first demonstrated by Tindale, with the excavation of Devon Downs and his construction of a general sequence on the basis of that evidence, and other sites including surface sites on Kangaroo Island (Hale and Tindale 1930; Tindale 1937). The changes he documented were changes in the kinds of stone artefacts found at different periods. On the basis of his primarily South Australian evidence, he suggested an Australia-wide scheme of cultural phases. Another pioneer in Australian archaeology, Fred McCarthy, drawing on his primarily New South Wales experience, took issue with Tindale's pan-Australian claims, and argued for different regional sequences (e.g. McCarthy et al. 1946).

Tindale envisaged five cultural phases (Table 1). The Kartan was argued to be of Pleistocene age because it was found on Kangaroo Island off the South Australian coast, not occupied or visited by Aborigines during the ethnographic present. It was named after a local mainland Aboriginal term for the distantly visible island, and this phase was characterised by the artefacts found there, large horsehoe cores and pebble choppers. The Tartangan phase was named after a site on a Murray River island. The Pirrian was named for an artefact type, pirri points, found in the older levels of Devon Down rockshelter; the Mudukian was named for a kind of bone artefact found in the middle layers at Devon Downs; and finally the Murundian was the historically known culture of Aborigines in the Murray Valley (represented in the most recent levels of Devon Downs). By 1957, with some radiocarbon dates now available, Tindale was able to calibrate his scheme to some extent.

<table>
<thead>
<tr>
<th>Cultural Phase</th>
<th>Suggested Age</th>
</tr>
</thead>
<tbody>
<tr>
<td>Murundian</td>
<td>6-8000 BP</td>
</tr>
<tr>
<td>Mudukian</td>
<td></td>
</tr>
<tr>
<td>Pirrian</td>
<td>12,000-2000 BP</td>
</tr>
<tr>
<td>Tartangan</td>
<td>≥ 11,000 BP</td>
</tr>
<tr>
<td>Kartan</td>
<td></td>
</tr>
</tbody>
</table>

Table 1 Tindale's scheme.

McCarthy (1948, 1964, 1967b; McCarthy et al. 1946) worked on rockshelters on the western outskirts of Sydney and in the mountains somewhat further northwest. He argued that Tindale's sequence did not apply there, and referred to it as the 'Tula Regional Sequence'. To incorporate his own evidence, he constructed an 'Eastern Regional Sequence' (Table 2). The earliest phase, the Capertian, was named for the Capertee Valley in the mountains where some of his sites were, and where this phase occurred in the oldest levels of one of the rockshelters. The middle Bondaian phase was named after an implement type, the Bondi Point, an asymmetrical backed blade[1], which characterised this middle phase. The Eloueran[2] was named after another artefact which McCarthy believed characterised his most recent phase.

<table>
<thead>
<tr>
<th>Cultural Phase</th>
<th>Suggested Age</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eloueran</td>
<td>1000-200 BP</td>
</tr>
<tr>
<td>Bondaian</td>
<td>2000-1000 BP</td>
</tr>
<tr>
<td>Capertian</td>
<td>12,000-2000 BP</td>
</tr>
</tbody>
</table>

Table 2 McCarthy's eastern regional sequence.

Both researchers argued vigorously that their scheme was the right one, Tindale on the one hand that his sequence was applicable everywhere and McCarthy that there were different regional sequences. It can be seen however that while they are incompatible as presented by their proponents, there are in fact parallels (Table 3).

<table>
<thead>
<tr>
<th>McCarthy Phase</th>
<th>Tindale Phase</th>
<th>Suggested Age</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eloueran</td>
<td>Mudukian</td>
<td></td>
</tr>
<tr>
<td>Bondaian</td>
<td>Pirrian</td>
<td>from 4-3000 BP</td>
</tr>
<tr>
<td>Capertian</td>
<td>Kartan</td>
<td>from ca. 12,000 BP</td>
</tr>
</tbody>
</table>

Table 3 McCarthy and Tindale compared.

This was brought out more clearly by Mulvaney's (1961) critique, which effectively demolished any standing of Tindale's Tartangan and Murundian phases by pointing out that no particular criteria had been defined for them.

[1] Itself named for the famous beach where it once apparently occurred in vast numbers, now only matched alas by a proliferation of hypodermic needles.

[2] The word *elouera* was selected by Towle (1933) to designate an artefact previously described as a 'chipped back knife'. Towle took exception to this functional designation, and proposed a non-committal title, which will not suggest usage, but which will serve to distinguish this type from other flake work types. It is proposed to call the implement the 'Elouera', which is the aboriginal word for 'Illawarra' (Towle 1933:8). I understand another variant of the term is 'allowrie', which is of course the Aboriginal word for butter.
Mulvaney's excavation of Kenniff Cave (Mulvaney 1964; Mulvaney and Joyce 1965) not only established a Pleistocene antiquity for the occupation of Australia, it also demonstrated that there was in fact one major change in the Australian archaeological record. Mulvaney recognised that the stone artefactual record at Kenniff Cave was basically one of continuity, with a mid-Holocene addition of new artefact types. He suggested (Mulvaney and Joyce 1965) that the difference was technological: that the old and continuing tool types were hand-held implements, but that the new types were intended to be used with a handle, as composite tools. He suggested that the basic division could be characterised as a non-hafted phase followed by a hafted phase. He argued further that there was in some places a third phase, which was not characterised by any new types, but rather by a falling off in number of the new hafted types. Thus the sequence at Kenniff Cave could be fitted with both Tindale's and McCarthy's sequence (Table 4).

<table>
<thead>
<tr>
<th>Mulvaney Phase</th>
<th>McCarthy Phase</th>
<th>Tindale Phase</th>
<th>Suggested Age</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hafted</td>
<td>Eloueran</td>
<td>(Murundian)</td>
<td>5-4000 BP</td>
</tr>
<tr>
<td></td>
<td>Bondaian</td>
<td>Pirrian</td>
<td>from</td>
</tr>
<tr>
<td>Non-Hafted</td>
<td>Capertian</td>
<td>(Tartangan)</td>
<td>19,000 BP</td>
</tr>
</tbody>
</table>

Table 4 Mulvaney, McCarthy, Tindale compared.

The existence of a basic two-fold division of the Australian archaeological record was confirmed by several other researchers in the 1960s (e.g. McIvor 1961, 1965, 1966, 1968). Megaw (1965, 1968) basically confirmed McCarthy's tripartite division for the Sydney region, but also concurred with Mulvaney that the main division was between the earliest Capertian and the succeeding Bondaian, marked by the introduction of new tool types, with the Eloueran representing a decrease in the latter. This view was also confirmed a little later by Lampert working on the coast south of Sydney at Burrill Lake and Curra­rong rockshelters (1971a, 1971b). He preferred different terms. He retained Bondaian, because it reflected one of the most common of the new types, and he suggested the terms pre-Bondaian for the earlier phase and post-Bondaian for the most recent. He argued particularly that the latter phase was not characterised by a predominance of eloueras, although they did continue in use after the cessation of other backed blades. The Bass Point sequence also conformed with these results (Bowdler 1970, 1976).

White (1967a) demonstrated that the major change in the archaeological record was indeed a pan-Australian phenomenon, as it occurred also in Arnhem Land. Here the new tools consisted of bifacial and unifacial projectile points, rather than backed blades. She took exception to Mulvaney's hafting versus non-hafting scheme however. In her earliest levels, dated to over 20,000 BP, she found edge-ground hatchets, several with a groove clearly intended for hafting.

Gould (1968) extended the phenomenon into the desert centre with his excavation of Puntutjarpa rockshelter. Again, he showed a basic twofold division in his sequence, with new tools introduced some time after 7000 BP. In an interesting exchange, Glover and Lampert (1969) published some criticisms of Gould's (1968) paper, including his use of the term 'microlith'. In response, Gould argued for 'an hypothesis which will include all small tools in Australia (that is, tools thought to be small enough to have required hafting). This would be referred to as the 'Australian small-tool tradition'. It includes, but is not limited to, backed blades' (Gould 1969:234-5). The term 'Australian Small Tool Tradition' is still in common currency. It would now generally be agreed that it includes backed blades (including eloueras), pirri points, projectile points of the kind found in the Northern Territory (and the Kimberley of Western Australia) and tula adzes as found by Gould at Puntutjarpa.

The one part of Australia where some aspect of the new tools did not appear during the Holocene was Tasmania (Jones 1968).

By 1969, in the first edition of The Prehistory of Australia, Mulvaney was able to talk about Australia-wide phases. Resiling from his original hafted versus non-hafted concept, he now spoke of an unnamed early phase, an Inventive Phase (equivalent to Bondaian, Australian Small Tool Tradition etc.), and a more regionalised Adaptive Phase, which he thought might reflect a move away from stone tools towards artefacts made of organic materials (Mulvaney 1969:106-26). In the second edition (Mulvaney 1975:210), he prefers the Gould term of Australian Small-Tool Tradition, but comments that 'the general validity of my twofold technological division has been borne out and its characteristics do seem to have been innovatory'. Meanwhile, the discovery of the Lake Mungo sites in the late 1960s, led to the
introduction of a new term for the older industry: the 'Australian Core Tool and Scraper Tradition' (Bowler et al. 1970).

By 1975 there was a clear consensus as to the nature of the evidence. The earliest colonists of Australia had an industry, or collection of industries, not terribly well defined, but characterised largely by somewhat amorphous scrapers, often with a steep working edge, and also by horsehoof cores and pebble choppers, with edge-ground axes in the north. Some time between 7000 and 3000 BP, a whole new range of generally smaller tools were introduced, and this was demonstrated for all parts of mainland Australia. The new tool types were not evident in Tasmania; nor were they found in New Guinea to the north, which, like Tasmania, had been joined to Australia in the Pleistocene. In some parts of Australia, primarily southern coastal locations, the new tools fell into disuse within the last 10,000 or so years.

THE MEANING OF CHANGE?

This is a convenient point at which to pause, and consider what was being made of this evidence. What was the meaning of the change? Were other changes also evident in the archaeological record? In the first instance, the changes in industries were seen as typological/technological, with no particular implications of wider change. The basic continuity of assemblages was emphasised, with the new tools being an addition. Tindale, for instance, saw no problem in applying essentially contemporary Aboriginal terms to his phases which were up to 11,000 years old; he envisaged Aboriginal culture as being essentially continuous throughout that time.

An obvious question arose from the new tool types: did they represent some form of contact with the outside world? Some of the new types, but not all, do have exact counterparts in other parts of the world. Backed blades are known not only from Mesolithic Europe and the Near East, but also from India and Indonesia. Could they have been introduced from Southeast Asia, and, if so, what would the implications of this be? Very little other evidence in the Australian archaeological record pointed to contact with other societies during the Holocene, with one possible exception.

By 1970 (e.g. Jones 1970), it had become evident that the introduction of the semi-domestic dog, the dingo, was also a Holocene event, post-dating the separation of Tasmania from the mainland. No dogs occurred in Tasmania in prehis-
cremation had been practised in southeast mainland Australia and also in Tasmania in the ethnographic present, and that this suggested it was a custom which predated the formation of Bass Strait (Hiatt 1969). The subsequent publication of the cremation burial found at Lake Mungo, and dated to at least 26,000 years ago (Bowler et al. 1970), supported the idea that cremation was of considerable antiquity, and continuity, in south-eastern Australia.

This apparent conservatism was also invoked with respect to the question of why agriculture was never invented or adopted by prehistoric Australians. These might be considered to be two slightly different questions. On the one hand, since other hunter-gatherer societies in other parts of the world had 'progressed' from food-gathering to food production, why had this not occurred in Australia? On the other hand, since it was evident by the 1970s (e.g. White 1971) that agriculture had appeared in New Guinea by at least 5000 to 6000 years ago, and since there was known contact between Aboriginal and New Guinean societies across Torres Strait, why had the former not adopted new economic practices from their food-producing neighbours? The most commonly offered answer to these questions was that, in the first place, there was no environmental pressure or other kind of necessity to induce such a change, but in the second place, Aboriginal society was basically conservative. Mulvaney (1975:239-41) for instance defends Aboriginal society against a charge of conservatism, pointing to an ethnographically-attested willingness of some groups to accept new 'non-material' items such as ceremonies and dances, yet argues that 'the Australian adaptation was made by the first colonists', and that there was a 'long established Aboriginal land use pattern'. This, I would argue, was until quite recently the basic paradigm of Australian archaeology, and it is one still held by many researchers.

This view can be usefully examined in the Tasmanian context. While the new mainland tool types did not appear in the Tasmanian archaeological record, other changes were evident. Jones's excavations at Rocky Cape, in particular (Jones 1971), but also the excavation of other sites in Tasmania (Lourandos 1968; Bowdler 1974), showed that about 3500 years ago, the Tasmanians stopped eating fish and making bone tools. The various interpretations and arguments engendered by this fact have been well enough aired elsewhere for them not to be canvassed again here in any detail (e.g. White and O'Connell 1982:Appendix 1). On the one hand, Jones (e.g. 1977) saw these events as evidence of a declining population, while others (Allen 1979; Horton 1979; Bowdler 1980) argued that they may in fact have had adaptive value, and that the Tasmanian population was, in the ethnographic present, not in decline, but, on the contrary, may have been expanding. What is of interest here is that all of us accepted a basic continuity in the Tasmanian culture, from Pleistocene times up until the nineteenth century (e.g. Bowdler 1977: 209, 218).

In a paper written for a conference in 1974, Jones (1977) examined the 'Tasmanian paradox': if the new stone artefacts which appeared in the Australian mainland during the Holocene had some adaptational, economic advantage, then this should be demonstrable in terms of population densities. A useful comparative baseline existed with Tasmania, where the new tools did not appear. His comparison of population densities between groups in reasonably similar, productive coastal environments, one in the Northern Territory, the other in northwest Tasmania, showed no significant difference. Therefore, he argued, either the new tools conferred no particular adaptational advantage, or (his preferred alternative) mainland groups invested such advantage as was gained into the realm of the spiritual life, in staging large, economically demanding religious festivals. The main aim of Aboriginal religious ceremonies was to maintain life as it was known, that is, to preserve the status quo. In this view, Aboriginal society changed only sufficiently to allow it to continue without change.

It is interesting to note that Aboriginal society presented itself as conservative, and saw conservatism as a value. It might be argued that this view has been perpetuated by anthropologists, who reflect the views of their subjects. This is still to some extent true of Aboriginal society today, a topic which will be pursued further below.

**INTENSIFICATION?**

In the late 1970s and early 1980s, some archaeologists began to challenge the view that Aboriginal society had been essentially unchanging since Australia was first colonised. Lourandos, in a series of papers (1977, 1983, 1985), questioned particularly the views of Jones (1977), on the grounds of both archaeological and ethnographic evidence, and also in terms of its interpretation. He argued that archaeological evidence from southeastern Australia showed increased food productivity, and increased levels of population density within the last 3000 or so
years. He also argued that ethnohistorical evidence from western Victoria in particular did show population densities significantly higher than those in Tasmania, thus refuting Jones's argument that no adaptive advantage was discernible. Lourandos aligned his work with that of international researchers investigating ideas about economic intensification (e.g. Bender 1978, 1981).

Lourandos argued that, during the Holocene, Aboriginal populations intensified their economies in various ways involving increased food productivity, such as eel-harvesting in western Victoria. He saw this as not due to simple mechanisms of population expansion, but rather as arising from specifically social causes. He argued that social organisation in mainland Aboriginal society was such that it carried within it potential conflicts which led, in a Marxist sense, to internal transformations. It is however not clear exactly what the specific mechanisms that lead to this are thought to have been, that is, the exact nature of the Aboriginal dialectic, nor what might have triggered them at this particular time. Nevertheless, Lourandos's views were important in identifying a significant phenomenon in the Australian archaeological record.

Others were quick to identify this phenomenon in other areas. Ross (1981), for instance, showed that, in the rather harsher environment of north-west Victoria, occupation intensified in the early Holocene in response to climatic amelioration, then declined with somewhat increased aridity. More recent evidence of increased population could not however be related to changes in the environment, and she interpreted it as due to increased populations in neighbouring regions, related to Lourandos's theories of intensification elsewhere in Victoria.

In 1981 also, I published a survey of the prehistoric use of the highland areas of south-eastern Australia. Tasmania was included for the sake of completeness, but I was primarily interested in mainland Australia, and my remarks here will only refer to that area. The main thrust of that argument was to show that these regions were only systematically occupied after the appearance of the new small tool tradition, within the last 4000-3000 years. Drawing largely on the archaeological evidence, it could be shown that, while there was sporadic occupation of a few sites earlier than this time, many more sites were subsequently occupied either much more intensively (in terms of density of occupational materials), or for the first time. In some regions it could be suggested that new food resources were being exploited (such as macrozamias), but no direct functional connection with the new artefacts could be shown. I argued rather that perhaps what enabled this increased effectiveness of occupation may have been a change in social organisation, allied with a change in religious practices reflected in large regular gatherings, and that the new stone tools might have had no direct part to play, but simply been a visible expression of a new order of society. I argued that we might be seeing here a thorough transformation of Aboriginal society, from something not really known, into the society we know from the ethnographic present.

THE EVIDENCE OF ROCK ART

Some support for such a view could come from changes in one aspect of culture which we generally connect with religious expression in Aboriginal society, namely rock art. There are of course problems with dating this sort of evidence, but various attempts have been made. The first attempt at an archaeological dating of rock art was made by Tindale (Hale and Tindale 1930), when he found in his Pirrian layer at Devon Downs a slab of engraved rock, which we now know to be dated to ca. 3000 BP.

Subsequently, various schemes have been constructed, which purport to order various rock art styles into chronological sequences. McCarthy (e.g. 1967a) developed an evolutionary sequence of styles, severely criticised by Maynard (1977, 1979), who then developed her own (see also McCarthy 1988, and comments). She had the advantage of the results of research carried out in the 1970s, of which the most important were probably the discovery and dating of finger markings in Koonalda Cave, left there some 19,000 years ago (Maynard and Edwards 1971) and the delineation of the Panaramitee engraving style by Edwards (1971).

Maynard defined art styles as being based on attributes of motif, character, technique and form. She identified three major, and sequential, styles in Australian prehistoric art, as follows (from Maynard 1977, 1979).

Panaramitee
This style consists of engravings, many of which were assumed to have a Pleistocene antiquity because of their generally weathered state, the amount of patination, the absence of tracks of dingoes, the possible depiction of extinct animal tracks, the support of the Koonalda evidence, the further (less spectacular) support of engraved fragments at Mulvaney's (still unpublished) Ingaladdi site stratified at depths dated between
5000 and 7000 BP (i.e. predating the Australian Small Tool Tradition), and the general distribution of such sites, including examples in Tasmania. The nature of the motifs is generally of a geometrical nature, but includes also realistic-looking animal tracks and also supposed faces.

Simple Figurative

This style includes engravings and paintings rendered, according to Maynard, with 'crude naturalism'. I am unaware of any particular archaeological credentials for it.

Complex Figurative

This style includes a broad swathe of Northern Territory paintings (Mimi and X-ray, see below), Kimberley paintings (Bradshaw and Wandjina, see below), Pilbara engravings and many others.

Much of this derives from an assumption of evolutionary progression from simpler to more complex, plus some evidence of superimposition. My impression is that the latter twofold division has not found much favour, but that there is still something of a consensus that Panamitee is an 'old' style. This has received support from Rosenfeld's excavations at the 'Early Man' shelter in Cape York, where engravings of a Panamitee nature occur on the back wall of the rockshelter where they were covered by an archaeological deposit, dated to 15,000 BP (Rosenfeld et al. 1981).

Engravings of a similar nature occur in Tasmania, particularly on the west coast. The best known site is Mount Cameron West, which was excavated some years ago. The evidence recovered showed that the carvings were done no more than 2000 years ago; Maynard however holds that the fact that the tradition is in Tasmania at all, points to a pre-Bass Strait common origin with the mainland.

Recent research has been published on the application of cation ratio dating to desert varnish covering engraved sites in the Oolay region of South Australia, on the edge of the arid zone. I am unable to assess these results; the statistical limits of confidence seem to me to be very large and yet played down by the authors (Nobbs and Dorn 1988), and it also seems to strain credulity that they have dated 24 motifs in a very restricted space, which are said to show little stylistic variation, and that their age range is from 1400 BP to 31,000 BP. There seems to be no associated or nearby occupational evidence.

Further efforts to develop sequences, regional rather than pan-Australian, have been made. In the Kimberley, it has been thought for some time that a definite sequence is observable, with small red 'lively' human figures called 'Bradshaw' figures preceding the multi-coloured larger, more 'static', Wandjina galleries. Not only are Bradshaw figures usually more faded and always beneath Wandjina figures where superimposition can be described, but also, while Aboriginal people today have an ongoing relationship with Wandjina art, they disclaim any knowledge of, or relationship with, Bradshaw figures (Crawford 1977).

Similarly, in the Northern Territory, what appears to be a parallel case occurs with earlier Mimi figures in relation to the later X-ray style. A most elaborate sequence of Arnhem Land art styles has been proposed by Chaloupka (1985). It is mostly circumstantial, depending very much on Chaloupka's interpretation of 'naturalistic' animal motifs, presence of a siliceous skin on paintings thus thought to be old, and what is known of Arnhem Land environmental history. Before the sea reached its present level, modern Arnhem Land was largely an inland plain. After the sea reached its present level, estuarine conditions prevailed, and sometime in the last 2000-1500 years, more freshwater conditions obtained. Chaloupka arranges his art style sequence as follows.

1. Pre-Estuarine 'Dynamic Figure style': this includes Mimi art, extinct animals such as thylacines, a predominance of other land mammals and dates to some time before 7000 years ago.
2. Estuarine: this phase marks the introduction of X-ray art, and motifs include a predominance of estuarine animals such as barramundi and crocodiles.
3. Fresh-water: this is also fairly self-explanatory; it is not so much another style, as simply a reflection of changing conditions; the most typical motif is a goose-wing fan.
4. Contact: a different kind of category again, reflecting historical changes; motifs include Macassan praus and European items like horses.

We can note that Chaloupka only really recognises one major stylistic change, between his phases 1 and 2.

One of the most ambitious archaeological attempts to seriate rock art is the work of Morwood (1980, 1984) in the Carnarvon Ranges of south central Queensland. Morwood carried out several excavations here, and generated a detailed archaeological sequence of artefacts, and attempted also to deal archaeologically with the art which occurs abundantly on the walls of the
Bowdler

rock shelters. He analysed his art sites in terms of the techniques used, the colours used, spatial distribution of these and consistent patterns of superimposition. He subjected this data to a principal components analysis and concluded that there was a three-part sequence, as follows.
1. Engravings, including pecked motifs and pecked and abraded motifs.
2. Paintings, including stencils, in a variety of colours.
3. Paintings, predominantly with white pigment.

Morwood here is in agreement with Maynard and others in finding engravings to be the earliest. He has no means of dating the subsequent changes, but argues that the appearance of paintings coincides with the appearance of the Australian Small Tool Tradition in his research area, at ca. 4000 BP. This would seem to be little more than an attractive hypothesis at this time. New techniques for dating rock art have been developing rapidly of late, so it is to be hoped that some of these sequences may be confirmed, refined or indeed refuted (Loy et al. 1990; McDonald et al. 1990).

Overall, it can be seen there is a certain amount of evidence to support the introduction of new art styles within the last 7000 to 4000 years, which may fit with arguments about transformation in the ritual/religious spheres ca. 4000 years ago. In general, there is now a reasonable body of evidence and argument to counter the idea of Aboriginal society as conservative and basically unchanging over the last 40,000 or so years.

CHANGE IN THE PLEISTOCENE?

Recent further argument pushes the idea of change even further back into the past. Following Lourandos's arguments about mid-Holocene intensification, it appeared as though the Pleistocene-early Holocene inhabitants of Australia were regarded as being possessed of a less complex social organisation, and of being relatively technologically and economically 'simple'. It was also suggested that intensification was only a question of slow and inexorable population increase since the time of first colonisation (Beaton 1985).

These views have recently been challenged by researchers working independently at opposing ends of the continent. For Tasmania, Cosgrove et al. (1990) argue that their evidence of intensive occupation of south central Tasmanian sites by 30,000 years ago shows that Pleistocene populations show considerable cultural complexity and diversity. O'Conner (1990) argues similarly from her evidence from the Kimberley in northwest Australia. She argues that there was a Pleistocene 'intensification', at least in population terms, by ca. 30,000 BP, which was followed by a population crash in many areas following the onset of full glacial aridity ca. 20,000 BP.

IMPLICATIONS

This new evidence, and its interpretation, is obviously still to be fully assimilated. It still leaves the way clear for two basically differing viewpoints about the Aboriginal past. On the one hand, it is possible to argue that the changes seen in the overall Australian archaeological record reflect adjustments of population and technology to allow a fundamentally conservative society to continue basically unchanged: a form of dynamic equilibrium, perhaps. On the other hand, it may be argued that this society has experienced at least one, possibly two, and perhaps more, fundamental transformations, and the continuities we do perceive are the result of a somewhat intractable archaeological data base, which is not amenable to reflecting profound changes in the non-material record in an obvious manner.

ABORIGINAL VIEWS

One group of people who have strongly asserted their right for a privileged view of the Australian prehistoric past is, of course, the very descendants of those we study (e.g. Langford 1983). Disagreements have arisen between archaeologists and Aboriginal people over who in fact has that right. This is not the place for a discussion of that issue in a political sense. It is however of interest to look at what such an Aboriginal view might be, in a substantive sense.

An American historian of archaeology, Trigger (1985), has suggested that native American people have welcomed an approach to archaeology which explains culture change in terms of adaptation and inventiveness in a context of cultural continuity, rather than relying on ideas of invasion and diffusion. For Aboriginal people, the idea of cultural change itself may be the stumbling block. Traditional Aboriginal belief holds that Aboriginal society has been essentially the same since the time of its creation by Dreamtime beings (e.g. Stanner 1965; Maddock 1974). As mentioned above, Aboriginal belief is in this sense conservative about itself. Contemporary Aboriginal people
also appear to express a view that Aboriginal society had been essentially unchanging since at least 40,000 years ago, until the devastating impact of the white invasion (e.g. Gilbert 1978; Bourke et al. 1980).

Perhaps the point at which archaeologists and Aboriginal people deviate most in their respective views of the Australian human past is at its beginning. To Aborigines, Aboriginal society was created by Dreamtime beings; Aboriginal people did not 'come from' anywhere. For archaeologists, their understanding of evolution and biogeography lead them to believe that it was not possible for the human species to have evolved in Australia.

AUSTRALIA AND THE REST OF THE WORLD

The scientific issues involved in the colonisation of Australia by Homo sapiens are complex and controversial, and are addressed elsewhere in this volume. There is however a consensus that Australia was first colonised, probably about 60,000-50,000 years ago, by seafarers from Southeast Asia, although their ultimate origin may lie beyond that area (Bowdler 1990a, 1990b, 1990c). Since that time at least, Southeast Asia as well as Australia and considerable parts of Melanesia have all been more or less continuously occupied by Homo sapiens. An interesting question which has received fluctuating attention over the last 30 or so years is that of the relationship between Australian societies and those in neighbouring regions, between that initial colonising period, and the recent European incursions.

The attentions of researchers have tended to be focussed on the two ends of the time span. On the one hand, to what extent could cultural relationships be identified between the first Australian colonists and presumed cultural forebears in Southeast Asia, or further afield? To what extent could physical forebears be identified in the fossil record? On the other hand, to what extent had the European invaders been anticipated by other, unrecorded European visitors (Dutch, Portuguese, Spanish etc), or by voyagers from other places, such as China (Mulvaney 1975:41-7)? And, given the historical evidence of Macassan visitors from Sulawesi (e.g. Matthew Flinders in 1803; Flinders 1814), what was the antiquity of those visits, and what were their effects? (Bermdt and Berndt 1954; Schrire 1972).

The question of early cultural relationships was investigated in some detail by both Tindale (e.g. 1937) and McCarthy (e.g. 1940, 1941), and not unnaturally, given the state of knowledge of the time, they turned particularly to the Hoabinhian industries of Malaysia (then Malaya) and Vietnam (then Indo-China). These early comparisons were further pursued, with newer statistical approaches, in the 1960s, by Matthews (1966) and McBryde (1974:245-6).

In the early 1970s, Jack Golson (1971a, 1971b, 1971c, 1972) published a series of papers with significant implications to which I will return. In particular, he showed convincingly that, while the Hoabinhian industries of Southeast Asia did indeed share some significant characteristics with early Australian industries, including edge grinding, they were in fact probably no older than those of Australia. This has to some extent reduced their interest to Australian archaeologists seeking antecedents. In more recent times indeed, the Hoabinhian sites have all been shown to be considerably younger than the early Australian sites, and only recently has a series of sites been excavated in Southeast Asia to which early Australian sites might usefully be compared (e.g. Bowdler 1990b).

The wider significance of Golson's papers however seems to me to lie in the way in which they firmly located Australian prehistory within the context of its geographical neighbours: New Guinea, Island Melanesia and Asia. To quite a surprising extent, Australian archaeologists of the later 1970s and 1980s have preferred to pursue a considerably more isolationist approach. Having got the continent of Australia firmly peopled by c. 40,000-30,000 years ago, most archaeologists have then concentrated on a scenario of internal development, fluctuation or continuity, depending on their bent. Relatively little consideration has been given to the possibility of external contact and possible influence.

In New Guinea, the work of Golson and others has revealed an unexpectedly early indigenus development of plant domestication (Golson 1977). In Melanesia within the last 5000-3000 years, extraordinarily sweeping cultural changes have taken place, with massive voyages being undertaken to fill up the last empty places on the earth. In Asia, equally vast cultural changes were taking place from 5000 years ago, culminating in the establishment of rich and sophisticated outposts of world civilisations on our doorstep: Kutei, Srivijaya, Majapahit. Yet Australian archaeologists have chosen not to take much notice.

There is a standard and unresolved debate amongst Australian archaeologists as to whether
the Australian Small Tool Tradition represents an introduction or an 'independent invention'. Without making any attempt to keep a running score, it would be my impression that consensus has favoured the latter option (e.g. White and O'Connell 1979). This seems to be partly due to questions of chronology (which I will not go into here, but which are also a matter of continuing argument), in that presumed possible 'ancestral' or parent industries in Southeast Asia may not be older than the Australian industries in question. Another counter argument is that only some of the types in question have external counterparts, although those which do have, have them very exactly. Mention of the dingo is usually met with the response that it is not necessarily connected with the appearance of the new tool types, either chronologically (which is debatable), or in any other way, and it is not discussed further. A lot of the reasons for rejecting the idea of an Australia continually subject to outside influence lie deeper than this, perhaps mirroring wider contemporary Australian attitudes: protectionist and isolationist.

Returning again to the near end of our time frame, we may consider the question of the Macassans. Detailed archaeological and historical research was carried out by Macknight in the 1960s (Macknight 1972, 1976). He concluded that the historically observed regular visits by these fishermen from what is now Indonesia had only been occurring within the last 300 years. This conclusion was based on historical evidence, and also archaeological evidence in the form of Dutch coins and bottles. Yet Macknight himself had obtained radiocarbon dates for Macassan sites on the Northern Territory coast of the order of 800 BP.

As I write these words, thirteen Indonesian fishing boats have been seized in Australian waters northwest of Broome, and 233 Indonesians are being held in a quarantine centre near Broome. Since the end of the Vietnam War, many Vietnamese boat people have managed the voyage from mainland Southeast Asia to Australia.

Australian archaeologists appear to believe that some 50,000-40,000 years ago, seafarers from Southeast Asia, in one or two 'waves', or even three, if we go back to Birdsell (e.g. 1967), crossed the Sunda Straits to Australia, but thereafter, all such travel ceased, until about 300 years ago. Alternatively to this Fortress Australia idea, some may concede that such voyaging has taken place from time to time, but its effects have been minimal.

Once again, the idea of Aboriginal society as continuously and infinitely conservative seems to underlie this attitude, an idea that Australia constituted an impermeable or at least immutable cultural mass, which absorbed outside influences, digesting what it could assimilate to its unchanging being, and rejecting the rest. This may well be true, but until some more serious attempts are made to investigate the possibility of the alternative viewpoint, Australian archaeology will continue to be as conservative as it supposes its subject matter to be. It seems not before time that, in archaeology as in economics and politics, and as Golson recognised twenty years ago, we firmly grasped the idea that Australasia is part of Asia.

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

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

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

intensification in the Australian mid- to late Holocene. This last argument leads me off the track and I leave it for the moment with the observation that if there was such a Pleistocene unity of undifferentiated, small, dispersed hunter-gatherer groups in Greater Australia, then the intensification claimed for mid-Holocene Australia pales into insignificance against the transformation which occurred in its separated northern half of Papua New Guinea at the beginning of the Holocene.

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

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

THE MELANESIAN PLEISTOCENE

The New Guinea Highlands

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

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

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

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

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

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

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

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

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

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

The Melanesian Lowlands

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

These changes in shell use in the terminal Pleistocene are accompanied by other changes in the archaeological record. Obsidian from the Talasea area of West New Britain occurs in small but continuous amounts throughout the 18-20,000 BP levels of Matenbek. In the adjacent Matenkupkum cave the published age for the earliest appearance of obsidian, c.12,000 BP (Allen et al. 1989:554) is currently subject to review following further excavations there in 1988; in the light of the apparent gap in the Matenkupkum sequence between c.21,000 BP and c.14,000 BP, already discussed, this discrepancy between Matenkupkum and Matenbek appears to be stratigraphical in nature and likely to be resolved. The same argument pertains to the earliest occurrences of phalanger in these sites, discussed next. Three points are to be made about the distribution of Talasea obsidian in the New Ireland sites. The first is the simple fact that a useful raw material was being transported over a straight line distance of c.350 km at least 18,000 years ago. The second is that this movement involved water transport between New Britain and New Ireland at this time – itself unremarkable in the context of Pleistocene sea travel, apart from the fact that it is the earliest known demonstration in this region of repeated and systematic canoe transport rather than accidental movement, and thus illustrates patterning in another dimension of human behaviour. The third point is that Talasea obsidian occurs in the Pleistocene levels of neither of the northern sites, Balof 2 and Panakiwuk. Whether this is a product of our small sample sizes or a real regional or site functional difference is presently unclear. However, Talasea obsidian does appear in the Holocene levels of these sites (certainly at 7-8000 BP at Balof 2 and probably at the same time in Panakiwuk) and thus signals a definite change of some sort. On the single site samples from Manus and the Solomons so far available, Talasea obsidian reached neither of these places in the Pleistocene.

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

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

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

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

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

The Tasmanian Pleistocene

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

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

The Southern Forests Project has sought to test the geographical extent and cultural variability present in the Pleistocene sites of the Southwest. Initial excavations were carried out on the eastern fringes of the Southwest, at Numamira Cave in the Florentine Valley, Bone Cave in the Weld Valley and at ORS 7, a sandstone rockshelter overlooking the Shannon
River. (This last site, on the edge of the Tasmanian Central Plateau is, strictly speaking, east of the Tasmanian 'Southwest', but it has provided an important contrast in this research, particularly in respect of defining the Pleistocene cultural boundary between Southwest and Southeast Tasmania.) During the 1990-91 summer the project excavated sequences on the western side of the region, at Warreen Cave in the valley of the Maxwell River, formerly called M86/2 (Harris et al. 1988; Allen et al. 1990); at a rockshelter on the Acheron River, labelled ACH/84/1 (Jones and Allen 1984); and at an unnamed cave on Lake Mackintosh near Tullah discovered by a project survey in 1990. There are thus six new major sequences plus several minor ones, together with the open sites data from the King Valley, to supplement the information from Kutikina. Overall, these sites occupy an area of c.15,000 km², with Bone Cave and the Mackintosh cave separated by c.150 km.

ORS 7, Nunamira, Bone Cave and Warreen Cave are currently the four oldest human sites in Tasmania, each having extensively dated sequences extending back to about 30,000 radiocarbon years before the present (Cosgrove 1989; Cosgrove et al. 1990:66). Warreen Cave has now yielded ten C¹⁴ dates in sequence, spanning the period from c.16,000 BP to c.27,000 BP. This latter date is, however, only two thirds of the way down the cultural sequence, with depth/age curves predicting dates in excess of 32,000 BP at the base of the excavated deposits. Further excavation was blocked by rocks before sterile deposits were reached. Warreen is thus conservatively as old or older than ORS 7, Nunamira and Bone Cave. While dates on the early Warreen levels and on the other sites are still awaited, this general antiquity of c.30,000 BP for four sites in the region has come as something of a surprise, since none of the earlier dated sites from further west, including Kutikina, had exceeded 20,000 years in age (Jones 1990:276-7).

Like Kutikina, these sites are mostly extremely rich in artefacts, rivalling the densities of the richest Palaeolithic sites anywhere in the world, and thus, to date, our resources have only permitted minimal sampling. Notwithstanding this, the excavation at Nunamira of about 1 m³ of deposit recovered some 30,000 stone flakes and 200,000 pieces (or 30 kg) of bone from animals eaten at the site; in Bone Cave, 0.8 m³ produced a similar amount of bone and more stone. Impressionistically, Warreen Cave is equally rich. In addition to this richness, the quality of the data recovered is also high. Many of the bones are whole or nearly so, enabling both a more certain identification of the species involved and also an accurate quantification of the body parts present.

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

Southern Forests Archaeological Project member, Richard Cosgrove has taken advantage
of the extensive palaeobotanical, palaeoclimatic and geomorphological research previously undertaken in Tasmania to construct a palaeoecological model to accommodate the Tasmanian Pleistocene archaeology (Cosgrove et al. 1990). If this model holds (and currently it accommodates the evidence quite well) it will continue to indicate quite structured human behaviour which concentrated on the exploitation of a limited range of animals in discrete and rich grass patches scattered along limestone river valleys. Cosgrove sees the predictability of game animals in these patches as the factor which outweighed the environmental harshness of the region and kept people there through the climatic excesses of the last glacial maximum.

A second project member, Brendan Marshall, together with Cosgrove, is painstakingly reconstructing the faunal data into a picture of hunting strategies, prey species compositions and butchering and bone disposal patterns which equally reflect the long-term structuring of this regional Pleistocene economy.

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

CONCLUSION

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

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

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

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

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

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

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In the past twenty-five years Jack Golson has been one of the few Oceanic archaeologists to look west for certain of the inspirations which have affected the course of Oceanic prehistory[1]. In this paper I wish to follow partly in the footsteps laid by Jack in his seminal paper ‘Both sides of the Wallace Line’ (henceforth termed BSWL; Golson 1971, 1972, 1974). That paper was almost entirely archaeological in content, but since Jack first prepared it for a conference held in 1967 not only has the archaeological record for Southeast Asia and the Pacific Islands increased by leaps and bounds, but the linguistic and genetic records have also done likewise. We can now look at the prehistoric cultures, languages and peoples of the region from independent and well-documented vantage points. The time is ripe for a reassessment of BSWL in the light of the past twenty years of discovery.

This re-assessment is not, of course, intended to be merely a paragraph-by-paragraph updating of Jack's paper. I will point to some of the main features of Island Southeast Asian prehistory since the emergence of modern humanity, and in doing so I will emphasise those aspects which seem to be pertinent for an understanding of Oceanic prehistory. My own view here is that many Oceanic archaeologists have in the past erroneously regarded Southeast Asia as a cultural region of no relevance for Oceanic affairs. In some cases this reflects a basic ignorance of the finer details of Southeast Asian archaeology and anthropology, in others it reflects a basic desire to stand Oceania on its own feet as an area of independent cultural and biological evolution. The latter view commands my respect. But human reality surely demands that both externally and internally generated causes of cultural and biological change have always been in operation, as amongst almost all human populations.

[1] In Jack’s own words written (presumably) in 1967: ‘New Guinea was exposed to influences from seminal areas in Asia and its archipelagoes which it had the opportunity to absorb, to transmute, and perchance to transmit’ (Golson 1974:537).
this issue), might have been the local descendants of Javan Homo erectus ancestors (Wolpoff et al. 1984), the descendants of immigrant modern hominids (via replacement radiation; Groves 1989:295-7), or perhaps a combination of both ancestries. Whatever the answer, and whichever grade of sapiens population – archaic or modern – first made the sea crossing to Sahulland, we cannot avoid the conclusion that the archaeo-
logical record of the Upper Pleistocene in Island South-east Asia must contain the technological roots of the first stone tool assemblages in the new lands to the east; Australia, New Guinea and the nearer islands of Melanesia.

It is now assumed by most scholars that stone and bone tool making in Java began with the late erectus or archaic sapiens population represented by the Ngandong (Solo) crania (Simanjuntak 1984; Bartstra and Basoeki 1989), recently given an early Upper Pleistocene age, between 100,000 and 40,000 BP, by Bartstra, Soegondho and van der Wijk (1988). Claims for earlier erectus tool use in Java are controversial and of no direct concern here, but the Solo population merits closer attention given the possibility that it might have been able to migrate along the Nusa Tenggara (Lesser Sunda) Chain to as far as Alor and possibly even Timor (and Australia?), long before the appearance of fully modern hominids in the region. This suggestion was made by Maringer and Verhoeven in 1970 and has been given implicit support from time to time since then by claims for typologically 'early' stone tools in Lombok, Flores and Timor (e.g. Glover and Glover 1970; Soejono 1987), often in apparent association with the bones of stegodons. Sondaar and Dermitzakis (1988) have recently claimed that Homo erectus and the Javan full-sized Stegodon trigonocephalus entered Flores together in the Middle Pleistocene to replace an earlier fauna of dwarf stegodons and giant land tortoises. However, stegodons might well have survived until very late Pleistocene times in the isolation of Flores. One cannot, without fuller presentation of the data, automatically accept the former presence of an erectus or archaic sapiens population in Nusa Tenggara.

Despite this, I must confess to two hunches in this debate. Firstly, the stone tools glossed by many authors as 'Pacitanian' are in my experience much larger on average than those found in Island Southeast Asian (but not Mainland Hoabinhian) cave deposits of the past 30,000 years. I am not entirely convinced that all Pacitanian-grade tools are the handiwork of morphologically-modern sapiens grade hominids [2], as recently argued by Bartstra and Basoeki (1989). Secondly, the 3726 metre Rinja ni volcano on Lombok is so easily visible from neighbouring Bali, across the Wallace Line no less, that one can hardly deny early humans the desire to float or swim across. Once they reached Lombok their descendants would probably have been able to walk to Alor during periods of glacial low sea level. In light of this, Butlin's (1989) arguments in favour of a route to Australia via the Lesser Sundas and Timor seem to me to be very attractive. The Wallace Line between Bali and Lombok was obviously an irrelevancy in the dispersal of fully modern (post-50,000 BP?) humans; perhaps its was too for some of their more archaic ancestors or antecedents.

Early Technologies

There can be little doubt, given a spate of recent discoveries, that stone tool industries, focused on the use of flakes and occasional core or pebble tools, occurred all the way from Mainland Southeast Asia to Australia by perhaps 50,000 years ago (Roberts et al. 1990), and onwards to the Solomon Islands (Wickler and Spriggs 1988) and Tasmania (Cosgrove et al. 1990) by at least 30,000 years ago. These early industries reveal considerable chronological and regional variation. Twenty years ago the focus of interest as expressed in BSWL lay in the distributions of and dates for edge-grinding and hafting, the latter via the provision of waists or tangs. Recent discoveries suggest that these two technologies are indeed very widespread and ancient in the eastern Old World, and for edge-grinding there are now dates back to between 20,000 and 30,000 years ago for northern Australia and Japan (Oda and Keally 1990), and possibly for Vietnam (Anon. 1988:87; Nguyen 1990), Peninsular Malaysia (Zuraina 1990:83) and New Guinea (Mountain 1983). There are also many archaeological sequences going back this far in time in which Pleistocene edge-grinding is totally absent, as in temperate Australia, Palawan, Sabah (despite its apparent occurrence at Niah in Sarawak; see Golson 1974: 544) and south Sulawesi. The technique would seem to have been by no means universally favoured. Perhaps one may wonder why.

One reason may, of course, relate to the occurrence of suitable raw materials, but this may not be all. Other late Pleistocene technologies
Bellwood

seem to have been similarly localised in spatial extent. For instance, a prepared core technology was present in south Sulawesi prior to 20,000 BP (Bellwood 1981). Bifacial points and knives were being manufactured in the Tingkayu basin in Sabah at or before 18,000 BP (Bellwood 1988a; 1988b). During the Holocene the slightly more widespread 'microlithic' industries of south Sulawesi and parts of Java can now be dated from about 7000 years ago and onwards (Glover and Presland 1985). All of these technologies, like the edge-grinding, seem to have been regionally specific. The Tingkayu bifaces, perhaps coincidentally, are similar in shape and size to those characteristic of other Upper Pleistocene and geographically-localised industries such as the Szeletian of eastern Europe (Allsworth-Jones 1986) and the Dyuktay of the eastern USSR (Michael 1984). The Sulawesi microliths resemble those of late Holocene Australia, despite the puzzling fact that none have yet been found in Nusa Tenggara.

What does all this local variation mean? This is a fundamental question for future researchers to address. Either we are witnessing a human capacity to innovate independently several times, even in the complex realms of bifacial, prepared-core and microlithic technologies, or there are channels of population movement/diffusion which still remain invisible to us. Given the essential mobility of hunter-gatherers the latter explanation should not be dismissed out of hand, even if it is almost impossible to demonstrate in cases of disjunct distribution such as those listed here.

Early Agriculturalists

The viewpoint on early agricultural dispersal into the Pacific taken in BSWL reflected general opinion during the 1960s and 1970s. Fruit and tuber agriculture (not cereal cultivation, which at that time was believed to be secondary in Southeast Asia) had been introduced, together with pigs, dogs and chickens, from Island Southeast Asia into New Guinea and the rest of Oceania at some time during the Holocene[3]. Agriculture was considered to be a development which had taken place in many regions in parallel, and in the Indo-Pacific region it was considered to have begun as a generalised Hoabinhian development in Mainland Southeast Asia, datable in its early stages to the terminal Pleistocene. In Jack's own words:

Whatever the specific period of its birth, the cradle of Indo-Oceanian horticulture ... lies in the area occupied at the appropriate time by communities of Hoabinhian-Bacsonian and allied type. (Golson 1974:554)

Since BSWL was written Jack has, of course, changed his opinion considerably with respect to New Guinea. In his Peanblanca paper (Golson 1985) he positively accepted New Guinea as a separate and independent centre of domestication of certain species of pandanus, yams, sugarcane, Australimusa bananas and possibly taro[4]. He was unable to find any palynological indications that agriculture had developed in Island Southeast Asia as early as the evidence suggested for New Guinea (c.9000 BP), and nothing to my mind changes this picture now. Pollen sites between 900 and 1525 m altitude in Sumatra show only limited signs of forest clearance from about 8000 BP onwards (Maloney 1980; Flennery 1988; Newsome and Flennery 1988), with no signs of major permanent clearance until some time approaching 2000 years ago, when the archaeological record suggests a considerable expansion of Early Metal Phase population. There is fairly good evidence for forest clearance for agriculture by perhaps 3000 BC in Taiwan[5] as expectable from its proximity to the locus of early rice domestication in China, but this hardly challenges the observation that New Guinea agriculture has an impressively high chance of being of independent origin. Having stated this, however, I must confess to a number of reservations about the overall agricultural scenario presented in BSWL for Southeast Asia (rather than New Guinea), many doubtless now shared by Jack himself.

For instance, the past twenty years of research have produced no good evidence to suggest that any late Hoabinhian populations practiced systematic cultivation. Yen (1977) has indicated the absence of evidence for early Holocene cultivation in the Hoabinhian of northern Thailand, a view emphasised more recently by Higham (1989:59-61). I have recently drawn the same conclusion for the Malayan Peninsula (Bellwood in press, b). In addition, I feel increasingly certain that the apparent presence of pottery in 'late

[3] By 1985 Jack was clearly more willing to entertain Chet Gorman's suggestion that tuber (especially taro) and rice cultivation had coeval rather than successive origins in eastern Asia (Golson 1985:307-8).

[4] In his 1985 paper Golson still regarded taro as an Asian introduction into New Guinea. The possibility that native wild forms grew and were independently domesticated in New Guinea is now being taken far more seriously (e.g. Matthews 1991).

[5] Tsukada (1967) gives a date of 4200 uncal BP for major forest clearance in the Jh Tan core from the Taiwan highlands.
Hoabinhian levels in many Mainland Southeast Asian caves is a result of continuous stratigraphic perturbation, not noticed in the days before the conjoining of matching sherds became common practice (cf. Matthews 1965 for an early suspicion along the same lines). Stated bluntly, there is precious little evidence for agriculture or pottery anywhere in Southeast Asia, Mainland or Island, prior to perhaps 4000 BC, a date which may be a full three millennia later than the appearance of systematic rice cultivation in central China[6].

If the Hoabinhian shows no signs of early agriculture, then neither do the island cultures of mid-Holocene Sundaland and Wallacea. Here there seems to be an equally sharp transition from foraging into fully-fledged Neolithic cultures with pottery, ground stone adzes, an impressive range of stone and shell ornaments and, quite often, traces of rice husk temper in pottery; the latter now known from third and second millennium BC contexts in southern Thailand, Taiwan, the northern Philippines and Sarawak[7]. Seen from the vantage point of the 1990s there are, in fact, many arguments to be made against the view of the 1960s and 1970s that innumerable populations of foragers around the world all progressed in parallel and probably independently of each other into a glorious early-middle Holocene state of agriculture.

For most mobile foragers the shifts in scheduling demanded by a successful (i.e. population-sustaining) move into systematic cultivation would always have been daunting and probably unpalatable, as one can surmise from recent ethnographic commentaries on forager 'settling-down' in Southeast Asia (Kuchikura 1988; Headland 1985, 1986; Eder 1988) and Australia (Chase 1989). A similar resistance can be detected in Africa (Clark and Brandt 1984:5), despite the current heated debate about the 'pristineness' of ethnographic foragers there (Solway and Lee 1990)[8]. Mobile foraging and seasonally-sedentary agriculture can only be combined successfully into one economy in rare situations, otherwise the mobile would-be cultivators never achieve any worthwhile agricultural yield. Furthermore, to refer to surviving foragers as somehow 'atypical' of the early Holocene world is not an acceptable escape from the significance of this observation. The ethnographic foragers of Southeast Asia and Australia are/were only atypical in the geographical sense that they occupied environments far removed from heavy agricultural competition (but not of course from all contact with agriculturalists; Headland and Reid 1989) until very recently. There is no reason why they should have been any more resistant to agricultural adoption than long-vanished early Holocene foragers adjacent to core agricultural zones. The primary developments of agriculture on a world-wide canvas probably occurred in a very small number of locations, each the independent result of an early Holocene situation of cultural stress in an environment with suitable potential cultivars (cf. Suggers and Grey 1987 for a thought-provoking comparative application of this principle to Australia and New Guinea). Early agriculture, I would like to suggest, was something special. In the Indo-Oceania world the only hints of its independent presence occur in southern China and highland New Guinea.

The adoption of this stance leads to another. The expansion of agriculture must have been in large part (although certainly not entirely) associated with an actual demographic and geographical expansion of existing cultivators. The high demographic potential of cultivation when compared to foraging is self-evident in most environments where successful cultivation is possible. It is difficult to regard agricultural expansion as mainly or even significantly the result of emulation by pre-existing foragers consequent upon contact with neighbouring cultivators.

In a recent paper (Bellwood 1991) I have discussed these observations at greater length and have attempted to relate them to the linguistic record. In short, all the major Old World language families which can be posited as having

[6] Two points should be added here to avoid confusion; I do not accept without question the older dates for pottery from Spirit Cave in Thailand; but I am willing to accept a presence of pottery in late Hoabinhian/Bacsonian contexts in northern Vietnam from about 5000 BC onwards (i.e. the Da But shell mound and other sites of this period, including Quynh Van). For the beginning of rice cultivation in China see Yan 1991.

[7] A sharp transition from foraging to agriculture need not, in itself, imply that agriculture has been introduced. For instance, it could be argued that the change from Natufian to PPNA in the southern Levant was a sharp transition, even though the archaeological record makes it clear that this region was a primary centre of agricultural development. In the case of Island Southeast Asia the sharp transition simply matches my view, derived from the archaeological and linguistic records, that this area was not a primary centre of agricultural development, even though a number of important plants (Eumusa bananas, yams, aroids, etc) might have been locally domesticated there.

agricultural proto-language vocabularies, such as Indo-European, Bantu, Austronesian or Sino-Tibetan, seem to have evolved initially in and spread rapidly from areas of primary agricultural development (in these instances, western Asia, central Africa and China). The records of such expansions are enshrined in the structural histories of the language families themselves, and also in the associated archaeological records.

For Austronesian, many linguists and archaeologists (Blust 1988a; Bellwood 1988b; Spriggs 1989a:608-9) now accept a homeland in the southern China/Taiwan region, followed by a break-up of Proto-Austronesian (c.3000 BC) in Taiwan. Proto-Malayo-Polynesian later broke up in the southern Philippines, to be followed by its subsequent differentiation of Western, Central and Eastern branches in Sundaland, eastern Wallacea and western Melanesia respectively. The break up of Eastern Austronesian was evidently underway by 2000 BC in the region from Halmahera to the Bismarcks. Early Austronesian societies, according to linguistic reconstructions, were squarely in the Neolithic mode of subsistence with agriculture, domestic animals, timber houses, canoes, pottery and so forth (see Blust 1976; Bellwood 1985). It is not my intention here to repeat all the archaeological and linguistic details since they are so freely available in the literature, but I would like to emphasise certain points.

Firstly, the overall expansion of the Austronesian languages – to Madagascar, throughout Island Southeast Asia and right across Oceania – was surely for the most part tied to an expansion of primary speakers. The peculiar hypothesis that early Austronesian somehow spread as a trade language through unmoving and linguistically unrelated populations (e.g. Meacham 1988:93-4; Solheim 1988:81) has no precedent on such a massive scale anywhere in human linguistic history. The bilingualism and trade languages ethnographically characteristic of parts of Melanesia are on extremely local scales by comparison. Languages of great geographical extent, such as English, Spanish and Arabic, have spread predominantly through substantial colonisation rather than by mere trade or conquest. Indeed, in those historical situations where foreign conquest was not accompanied by substantial colonisation the introduced languages either disappeared with the withdrawal of the conquering agency (as with Persian and Mongol) or were maintained only by elites, as in the modern case of the English language in India and Malaysia[9]. Amongst scattered and non-integrated Neolithic tribal societies over as large an area as Island Southeast Asia such an elite- or trade-based spread is impossible to imagine.

It is therefore my view that people and their natal languages, at least in the Austronesian case, have spread predominately together. In support of this view there are now many genetic data, especially for HLA genes and mitochondrial DNA (Hill and Serjeantson 1989; Hertzberg et al. 1989; Stoneking et al. 1990), which suggest that the Austronesian-speaking populations of southern Mongolia physical appearance in Island Southeast Asia, Micronesia and Polynesia share a very high degree of common genetic ancestry. The Melanesians, especially in the west, share a different common ancestry with the Aboriginal Australians, despite the fact that these two groups today seem to be quite distinct in terms of many genetic markers and to have experienced remarkably little genetic contact for many millennia (Kirk 1989; Stoneking et al. 1990).

This situation of almost no genetic contact between Australia and New Guinea contrasts markedly with the very complex patterns of intermarriage in western Melanesia between the Austronesian and Papuan speaking groups, both of whom appear already to have had agricultural economies when initial contacts occurred about 4000 years ago[10]. The original (pre-Austronesian) Melanesian population, presumably by virtue of greater size and overall density in the long-term, seems to have absorbed the phenotype of the early Austronesian settlers. Despite this process of absorption, however, it is interesting that the members of both linguistic groups, indigenous Papuan and intrusive Austronesian, can still be differentiated in many areas in terms of genetically-determined features such as fingerprints (in Bougainville; Friedlaender 1987:351-62) and Gm polymorphisms (Kelly 1990). It

[9] Today, only 2% of Indians are literate in English, despite the fact that 50% of the books published in the country are in this language (Warthaugh 1987). Despite this, English shows absolutely no sign of replacing Hindi, Bengali, Tamil or the other Indian vernaculars, just as it shows no sign of replacing Malay in Malaysia. However, had English speakers been able to settle in large numbers throughout these two countries in the same way that they were able to settle North America, Australia and New Zealand the picture would undoubtedly be totally different.

[10] The existence of a pre-Austronesian agricultural economy in New Guinea seems almost certain given Jack Golson’s evidence from Kuk (Golson 1977), and also the new evidence for tree-crop exploitation (coconut, pandanus, canarium, candlenut, areca and many others, including perhaps sago) in the Sepik Basin at about 6000 BP (Swadling, Araho and Iyuyo 1991).
is also worth remarking that a recent claim, based on cranial analyses, that Polynesians and Micronesians share their closest common ancestry with Jomon Japanese (Brace et al. 1991), rather than with Island Southeast Asians via Melanesia, is very hard to square with the linguistic and genetic evidence as it is currently interpreted.

These digressions into the linguistic and genetic fields are today crucial for any rounded form of prehistoric interpretation. My memories of the late 1960s are that archaeologists in most parts of the world were generally unaware of their significance, whether in Europe, Australasia or North America; hence perhaps the almost total lack of reference to them in BSWL and the reliance on artefacts and absolute dates in that article for the writing of prehistory. With linguistic and genetic inputs, however, the expansions of agriculture, Austronesian languages and people in Island Southeast Asia and Oceania can be reconstructed more precisely.

In the field of economic prehistory, it is now clear that the earliest Austronesian cultivators grew swamp rice (dryland rices probably being later developments; Chang 1988:71), millet and sugar cane (probably Saccharum sinense; Daniels et al. 1989), as well as a range of tubers and fruits. Basically, there is no sign in the Austronesian record of any hypothetical pre-cereal stage of tuber cultivation. Indeed, there is no sign that such a stage existed anywhere in Asia, despite its obvious significance in New Guinea. Neither can rice any more be posited as late in its date of domestication since it is now (with millet) the earliest domesticated plant known in east Asia (c.8500 BP; Yan 1991). The gradual fading of rice in the eastern Indonesian equatorial zone, documented in the simplification of plant vocabulary (Revel 1988) as well as in distribution, can instead be related to environmental factors such as unsuitable climate and the possibility that the early cultivars were highly photosensitive and thus adapted to intermediate tropical latitudes rather than equatorial ones (Spencer 1963; Bellwood 1985)[11]. Hence the likely derivation of some aspects of the agriculture of Oceanic Austronesian communities from a zone of fruit and tuber cultivation in equatorial eastern Indonesia, doubtless enriched by contributions from the coeval and independent centre of fruit and tuber exploitation centred on New Guinea.

As far as animals are concerned the major domesticated triad of pig, dog and chicken are undoubtedly of immediate Island Southeast Asian origin (and the dog and chicken ultimately from China). However, the pre-Austronesian introductions of phalangers and wallabies to New Ireland and perhaps beyond (Spriggs 1989b) should alert us to the possibility of similarly-ancient animal introductions further west. For instance, Glover (1986) has reported the appearances of phalangers, civet cats and macaque monkeys in the Timor Cave record at about 4500 BP. Musser (1981) also lists deer and Javan porcupines as ancient introductions into the Lesser Sundas (see also Groves 1984). Glover's dates do not automatically imply an association of these introductions with an agricultural population; indeed, it would make more sense to postulate that these presumably undomesticated animals were introduced by pre-agricultural hunters, intent on providing a living in the zoologically-impoverished islands between Bali and New Guinea.

CULTURAL CONTACTS BETWEEN ISLAND SOUTHEAST ASIA AND OCEANIA – LAPITA AND ONWARDS

Lapita

The major part of the text of BSWL was concerned with a comparative survey of Southeast Asian and western Oceanic pottery traditions, particularly with those mainly-Melanesian traditions still classified as Lapita, paddle-impressed and incised. Golson believed that Lapita, as the most widespread of these traditions, might have been derived, together with Solheim's Kalanay tradition in the Philippines, from a common ancestor represented by earlier Neolithic pottery excavated in Taiwan (see Golson 1974:576-81). His conclusion at the end of this comparative exercise ran as follows:

As a result of a growing body of archaeological materials from the South Pacific the cultures of that region are more clearly seen to have their ultimate origins in Southeast Asia, in circumstances, however, that have yet to be much more closely defined (Golson 1974:581).

Twenty years later this conclusion still stands, although the contribution of the pre-Austronesian populations of western Melanesia must now be accorded a much greater significance. We also know far more about Southeast Asian prehistory than in the 1960s, such that many features once thought fairly unique to Lapita (dentate stamping on pottery, certain kinds of shell ornaments, trade

[11] The significance of photosensitivity in the spread of rice is less certain than I once thought since the early Chinese rices are now known to have included both indica (photosensitive) and japonica (non-sensitive) varieties (Li 1985; Bray 1986; Yan 1991).
access to Talasea obsidian) are now known to have been much more widespread in Southeast Asia (see reviews in Bellwood 1985, in press a, in press c)[12].

Nevertheless, the question of Lapita origins is still not entirely answered and obviously we must allow for a substantial amount of generative cultural change within the Bismarck Archipelago, the area which now ranks fairly decisively as the 'Lapita Homeland'. It is clear that the bulk of Lapita material culture, together with the early Oceanic Austronesian languages, which must surely have been associated to a very high degree with the Lapita population, can be traced back into Indonesia, the Philippines, Taiwan and ultimately perhaps into the early rice-growing cultures of southern China[13]. However, the precise origin for a cultural entity such as Lapita will never be traceable to a point on a map. Many geographical and chronological threads have gone into the weave – a point which I have emphasised elsewhere (Bellwood 1989a:2).

Dongson and Friends; the Early Metal Phase

From the archaeological record of the last 2500 years there are lots of sites and artistic complexes, especially in Melanesia, which hint at 'Bronze Age' influence in the western Pacific (see Golson 1974:581-8 and Bellwood 1978:266-9 for general overviews). Yet the linguistic evidence indicates quite clearly that major Austronesian colonising activity from Island Southeast Asia into western Melanesia had ceased well before 2500 years ago. There is no evidence for language intrusion into the Melanesian region from Southeast Asia during this period, except perhaps for the faint influence of Malay. We are therefore looking at brief episodes of trade and other kinds of contact, rather than any large-scale settlement by new peoples.

[12] Talasea obsidian, recently reported from the site of Bukit Tengkorak in Sabah during the first millennium BC (Bellwood and Koon 1989), is now reported from a second undated site near Bukit Silam, 80 km northwest of Bukit Tengkorak (discovery by Peter Koon, Sabah Museum; obsidian sourcing by Roger Bird, ANSTO, Sydney).

[13] For instance, the material culture of Hemudu In Zhejiang Province (5000 BC; Liu 1985) includes stepped paddles (and a pottery model canoe), elaborate pottery (including spouts, handles, lids, and what may turn out to be a very important class of pottery stoves; see Bellwood 1989b:141-3), bone pieces carved with spirals and concentric circles, matting, rope, spindle whorls, large quantities of rice remains, and the bones of pigs, dogs and cattle. The significance of this list for many later Southeast Asian and Oceanic cultures hardly needs emphasis.

In recent centuries, prior to the imposition of colonial rule over New Guinea, trade with the Moluccas seems to have been mainly confined to the Bird's-Head region (Doberai and Bomberai Peninsulas) and the western part of Cenderawasih Bay (Forrest 1969:Chapter 8; Seiler 1985). This trade involved a limited spread of Malay, as well as other Moluccan languages such as Ternate and Tidore, but it seems not to have proceeded far eastwards beyond Cenderawasih Bay until the later nineteenth century. However, the influence of Malay on western New Guinea languages could go back in a sporadic way for many centuries, given that the Ambon and Bacoan dialects of Malay might have been established by traders during the first millennium AD (Blust 1988b:8; see also Gonda 1973:38 for Sanskrit loans, via Malay, into Numfor). As I will indicate, the idea that trade connections might have penetrated quite far from Indonesia into Melanesia within the last 2000 years does have its attractions.

Unfortunately, the issue of 'Bronze Age' influence in the Pacific has to date been bedevilled by two problems. The first, of course, has been the extreme prominence of diffusion as explanation for all observed Southeast Asian-Oceanic parallels. Without going into details, many of these parallels in the realms of art and material culture look much more comfortable today as the results of shared inheritance rather than direct contact[14]. The second major problem has been the reliance on the concept of 'Dongson' as a prime mover. Surely, the idea that all Early Metal Phase influences in Melanesia, whether real or imagined, must have come directly from the Dongson culture of northern Vietnam is no longer tenable. The Dongson drums themselves, found throughout the Sundal Islands and even in western New Guinea, were perhaps traded long after the demise of the Dongson culture and almost certainly by Indonesian traders rather than by Vietnamese. Local Indonesian metalworking traditions were well established by 2000 years ago, as indicated by the finds of drum moulds of this approximate date in Bali (Ardika and Bellwood 1991) and socketed axe moulds of the

[14] I state this despite the fact that I am often amazed at how close some forms of Indonesian art are to, for instance, Maori art. Compare the openwork and heavily spiral-ornamented canoe prow from Tanimbar shown by De Hoog (1980:146; see also McKinnon 1988) with the similar canoe stern drawn in New Zealand by Sydney Parkinson (Bellwood 1978:299). Or again, witness the Maori-like spirals and interlacing in the doorframes of Ngada in central Flores (Azis and Awe 1984:60; Erb 1988:109). In such cases one might feel persuaded to cry diffusion, but the reality suggests common origin in part, and perhaps even sheer coincidence in part as well.
first millennium AD in Java, Borneo, Talaud and the Philippines (Bellwood 1985: Chapter 9).

Therefore, to describe these potential influences into western Melanesia as 'Bronze Age' or 'Dongson' rules out the possibility that many undoubtedly derived from the trade in spices and other products (hardly a Dongson phenomenon) which penetrated far into eastern Indonesia in the search for cloves, nutmegs, sandalwood and other precious items from perhaps 2000 year ago and onwards, well into the ages of Hindu-Buddhism, Islam and even Christianity. Indeed, the search for sandalwood might have been very important, despite the fact that this topic has been approached in mutual isolation by Asian and Pacific historians. Miller (1969:61), for instance, indicates that the sandalwood traded to China and the Mediterranean came from the Sunda Islands and southern India. Yet we know from Shiningberg (1967) how important the sandalwood trade was in the nineteenth century Pacific. The plant itself knows no boundary between the Moluccas and New Guinea and there seems no reason to assume that ancient traders necessarily ceased their activities on the western border of Melanesia.

Actual evidence for contact between Southeast Asia and Oceania within the past 2500 years has always been rather elusive, despite the confident stance of some art historians. However, there are a number of ethnographic and archaeological entities pertinent to this question which I think may be worth greater attention. The first must surely be the Southeast Asian backstrap loom, used for weaving abaca fibre in parts of Micronesia and Melanesia (Riesenberg and Gayton 1952). The absence of the backstrap loom beyond a restricted region of the western Pacific suggests that it is almost certainly post Lapita in its date of introduction, although since abaca fibre preparation does not require the use of spindle whorls it is hard to see how direct archaeological evidence on this topic can be recovered, except perhaps from dry cave situations.

The second situation, this time more positively archaeological, revolves around the occurrence of bronze and double-spouted pottery vessels in the Admiralty Islands at c.2000 BP (Ambrose 1988; Kennedy 1982). The latter are almost identical to specimens of the same age or earlier from burial caves in the Niah complex and at Lubang Angin in northern Sarawak (Harrison 1971; Datan and Bellwood 1991). While these similarities may be entirely coincidental, the discovery of Talasea obsidian at two sites in Sabah (see footnote [12]) suggests that two-way trade between the Borneo-Philippine region and northern Melanesia might have been underway during the first millennium BC.

Thirdly, we have the pottery of generalised Indonesian-Philippine Early Metal Phase type (including high-necked flasks and openwork pedestals) from contexts of the (late?) first millennium AD in Collingwood Bay and the Trobriand Islands (Seligman and Joyce 1907[15]; Egloff 1979: Ceramic Group P; Golson 1974:584-6). Perhaps the jar burial traditions of some regions of the Massim District (Lauer 1971) are also related to this ceramic complex in origin.

Fourthly, we have from Indonesia itself the new knowledge that direct trade with India was underway, to at least as far east as Bali, by the first century AD (Ardika and Bellwood 1991). Archaeological remains of boats indicate the existence in Southeast Asia of substantial lashed-lug and dowelled plank vessels (Manguin 1989), those found in the Butuan site on Mindanao (one perhaps dated as early as AD 300) being of local hardwood timber (Ronquillo 1987:73). So it is clear that ocean-going trading vessels capable of carrying sizeable cargoes were being manufactured almost on the borders of Melanesia by the early centuries AD. Such vessels, given a knowledge of the monsoonal shifts in wind direction, would have been perfectly capable of sailing the short distance into Melanesia from the southern Philippines or the Moluccas.

**BOTH SIDES OF THE WALLACE LINE IN RETROSPECT**

The sub-headings within BS WL give a clear idea of the major topics which were of concern in the late 1960s in the general field of Southeast Asia-Oceania relationships (Oceania, of course, including Australia). These topics included the edge-grinding and hafting of stone tools; the development and spread of horticulture and pottery; the origins and relationships of the Lapita, paddle-impressed and incised pottery complexes of Island Melanesia; and the issue of 'Bronze Age' influence. The most surprising archaeological discoveries since BS WL was written relate to the antiquity of human settlement beyond Wallacea and to the antiquity of agriculture/aboriculture in New Guinea. On the other hand, despite the filling in of much detail, the chronological and geographical frameworks of the pottery-based prehistory of the western Pacific have not changed so drastically.

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[15] Plate X no.13 of Seligman and Joyce shows a spout identical to Island Southeast Asian Metal Phase specimens.
What have changed, of course, are interpretative stances. The origins of modern humans, Austronesian languages, Lapita pottery and Melanesian agriculture can all be interpreted across a range of views from outright diffusionism to blinkered indigenism. Hopefully, I find a place somewhere in-between these two poles, as did Jack Golson with BSWL. It is not my place to suggest here what the correct stance must be, and indeed I doubt that there is a correct stance. All situations require consideration in their own right and no amount of theoretical posturing will ever change this basic fact of prehistoric life. I do believe, however, that the discipline of prehistory must be at least multidisciplinary, and if possible truly inter-disciplinary as well. The equation of prehistory solely with the archaeological record is no longer sufficient to convince those who want to know about the whole past, despite the great strides ahead which archaeology sensu stricto has made in the past two decades.

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Bellwood


The inexorable and unprejudiced honesty of the radiocarbon and thermoluminescence clocks shows that Melanesia and Australia were among the earliest colonisations by the newly emergent *Homo sapiens sapiens*. This great antiquity of human settlement exposes a number of contradictions in our traditional views of the past of this region, including an unconscious clinging to models of prehistory derived from other parts of the world. The sophistication and rigour of techniques used to uncover this remarkable prehistory are not always matched by our explanations, many of which show a persistence of an earlier addiction to viewing the past as the history of the migrations, successes, failures and replacements of various 'races'. When these explanations fail, as they often do, the 'new' archaeologists seek 'processual' explanations which are often little more than heavily disguised Malthusian overcoats. This paper looks at only one set of these emerging contradictions, those concerning the relationship between Melanesia and Australia during prehistory and offers an alternative approach which does not, perhaps, require so many contradictions.

Thus currently, as archaeologists, we are quite happy to acknowledge the spectacular and precocious maritime colonising skills of the initial settlers of Sahuland, evident from the recent discoveries in New Guinea and Island Melanesia (Groube et al. 1986; Allen et al. 1988; Wickler and Spriggs 1988). Yet simultaneously we accept that the gradually emerging ocean between New Guinea and northern Australia 30,000 years or so later proved to be a significant barrier to continued human contact between Melanesia and Australia. Why were descendants of the colonisers of Sahuland, who found little difficulty in bridging ocean gaps greater than 100 kms during initial settlement, inhibited by the tiny (though expanding) ocean gaps which must have existed for many hundreds of years during the post-Pleistocene sea-level recovery?

The apparently profound biological, cultural and technological divide between the inhabitants of Australia and those of the island of New Guinea is the most obvious reason for accepting this contradiction. Explanations for biological differences which do not require an effective ocean barrier in late prehistory fall back on theories of separate origins of the two populations, such as multiple origins for the Australians and/or later recolonisations of the island of New Guinea, (the Asian 'rubbish-dump' theories, Groube 1985:65). It is also claimed that the ecological limitations in the south, particularly the aridity of most of the sub-continent, could account for many of the cultural/technological differences regardless of the existence of an ocean barrier.

Few seem aware that this latter 'processual' explanation involves a delicate irony. Malthus, to whom all ecological theories can be ultimately traced, recognised that the low population densities of the Australian Aborigines was a central issue in developing his Principle of Population. He says of them:

> by what means are the inhabitants of this country reduced to such a number as they can subsist? (Malthus 1806)

Flew (1985:22) considers this to be Malthus's 'master speculative question' from which his theories of population 'checks' developed. Models developed from the ideas of Malthus, particularly those of 'carrying capacity', are re-employed today to explain the resurrected problem which Malthus identified with the Australian Aboriginals.

Theories of multiple origins, although not rejected, are increasingly unrealistic. The convergence of the earliest settlement dates in Australia and Melanesia, the demonstration that the two regions share at least one distinctive stone tool, the 'waisted axe' (Groube 1986), which in Melanesia at least is clearly dated to the initial settlement period, and the persuasion of Occam's razor of seeking the simplest hypothesis, recommend that it is not improbable that these two populations shared a common parentage in the Pleistocene. Having occupied a single land for
over 30,000 years they were finally separated a mere 7000 years ago. This possibility would favour a clinal variation from north to south. This is evident in some genetic data, particularly connecting New Guinea Highlanders and Australian Aborigines (Kirk 1979; Serjeantson and Hill 1989:292), but not in cultural/technological development where there appears to be a sharp separation.

Another reason for accepting the effectiveness of a late ocean barrier is the continuing archaeological silence from the surviving remnants of the former connection, the south coast of the island of New Guinea, the Torres Strait islands and Cape York, in the millennia preceding and following the formation of Torres Strait (Beaton 1985; Barham and Harris 1987). The significance of this silence is weakened, however, by awareness that coastal changes initiated by the sea-level rises will have destroyed much of the crucial evidence, and more importantly by the fact that the apparent emptiness of these three regions appears to be much older than the onset of sea-level rises. If the poverty of archaeological remains from this period is registering the existence of a barrier then it is clear it was in operation long before the rising of the Arafura Sea and the formation of Torres Strait.

Further contradictions about this crucial region come from the failure of even the simplest of the intensification strategies practised in Melanesia to penetrate northern Australia during the Holocene. Our new awareness is that much of the agriculture practised in Melanesia was not borrowed recently from Asia to the north (as was formerly thought) but was developed from local plants within Melanesia (Yen 1982). Some of these plants which became staples in Melanesia were also present in northern tropical Australia during the Pleistocene (Golson 1971:199; Jones and Meehan 1989). There is also the possibility of swamp manipulation for agriculture in the New Guinea Highlands before the Torres Strait was formed (Golson 1977). All this has removed the most obvious of the explanations, that agriculture was developed (or introduced) in New Guinea long after the ocean barrier was in existence. Claims that the trend towards forest-plant manipulation and intensification were already underway during the Pleistocene in New Guinea (Groube 1989), may have little relevance in Pleistocene Australia because the more abundant marsupial fauna in the south made forest plant resources less important. The issue has a Pleistocene sting, however, for northern Australia (or as it then was, 'middle Sahuland') differed little ecologically from the area to its north.

The contradiction, however, is not in the absence of 'Melanesian' intensification strategies in northern Australia (this is merely a fact) but in the simultaneous acceptance by many archaeologists that there were some Melanesian influences which were adopted. There is a widespread belief, for example, that more 'primitive' watercraft of Pleistocene Australia (reed and bark coracles or rafts?) were supplanted by 'Melanesian' dug-out canoes in recent prehistory. If canoes, why not the two vegetable adjuncts to the canoe and lagoonal lifestyle of coastal Melanesians, banana and coconut? Why was the pig, an oft-times passenger in Melanesian canoes, held in such disfavour by the northern Australians? We can be fairly confident that all three were present on the South Coast of New Guinea by at least 2000 years ago during which period (returning to our archaeological 'double-talk'), we are happy to accept that there was trade between Cape York and New Guinea via the Torres Strait's Islanders (White 1971:187-8). Geneticists are also quite happy to explain any similarity between northern Australians and coastal Melanesians in terms of contact rather than founder effect or shared experience of the same selective pressures (e.g. Kirk 1976): blood flow without ideas? What has happened to the diffusionist's basic premises? And if all this conflicting information is true, what is the basis of the apparent 'resistance' of Australians to the intensification strategies used by their trade and marriage partners to the north?

This obvious question has been posed many times (see for example White 1971). An explanation for the absence of any intensification strategies (apart from 'fire-stick farming' and communal fish-traps) in sub-tropical and temperate Australia has been reasonably sought in the unyielding climate (particularly aridity) of most of Australia. Their absence from northern tropical Australia, however, is not so easily explained.

Underlying all these contradictions specific to the Australian-Melanesian region are much broader demographic issues which emerge from the greater time depth of initial colonisation established by absolute-dating. Malthus, as a Jesus College Wrangler, would have been the first to appreciate the implications of the great antiquity of the settlement of Sahuland upon his 'master speculative question' quoted above: with such small populations in the region natural increase must have been miniscule. Powerful population checks were obviously in operation over a very long period. If, for example, the initial founding population was as small as 100,
the average rate of natural increase over 40,000 years would have been only 1.00025 per annum to account for a total population in the Australo-Melanesian region of 2,000,000. If we increase this terminal figure to an optimistic 10,000,000 the average rate of natural increase would be only 1.00029. In addition, the widespread early dates recommend that during the period of initial exploration and colonisation, rates of natural increase must have been well above these average figures. Rapid colonisation on this scale must be funded from a healthy surplus population. Thus we must assume that population expansion in prehistoric Sahuland followed the classic sigmoid curve and that, for at least the latter half of the prehistoric period (20,000 years), it was at a virtual standstill if not declining. The contradiction is obvious: the exciting early dates imply that the settlers of Sahuland practised a form of birth restraint and/or suffered excessive mortality on which the archaeological and ethnographic records are totally silent.

To archaeologists these contradictions (and others) essentially represent 'failures' in expected performance in terms of some current models of the past. We would expect, for example, following the balanced arguments of Hassan (1981:200-3) a natural increase rate during the initial population phase of Sahuland prehistory of at least 1.001 per annum, a rate which (with a starting population of 100) would have left over 2,000,000 descendants after only 10,000 years. Clearly the average annual growth rate even during the first ten millennia was less than this. But why? In the ecologically 'tough' conditions of much of arid Australia, standard arguments about population limitations because of low carrying capacity appear valid, but for northern tropical Australia and Melanesia, particularly in the coastal mangrove areas, it is improbable that carrying capacity was ever approached during prehistory.

Underachievement in the sense of failure to conform to archaeological expectations is also typical of the developments following colonisation. For the Sahuland 'southerners' it is in their failure to bridge the narrow ocean gap in late prehistory. Their failure to adopt the intensification strategies of the 'northerners' thus incurs the ultimate prehistoric condemnation, failure to emerge from the 'lower-class' of the Palaeolithic. Our archaeological constructs which include the evolutionary imperative of 'progress', falter with the Australian case. The 'northerners', now the Melanesians, had their failures in expectations also, the most obvious being, with the advantages of agriculture, their failure to oblige the Marxist-leaning archaeologists and anthropologists by developing some of the tendencies towards centralised authority and 'state-formation' found in so many other regions with comparably rich resources and a Pleistocene prehistory (e.g. Mesoamerica). The set of contradictions arising from the plethora of small-scale societies and the immense diversity of Melanesia cannot concern us here, but is amenable to the same explanation: that something went 'wrong' in Melanesian and Australian prehistory, something which disturbed the anticipated pattern of prehistoric development.

That something, I would suggest, was the role in Sahuland prehistory of the 'predators within', the parasites of infectious diseases, particularly the presence in Melanesia, and probably northern Australia as well, of that most ancient of human-specific diseases, malaria.

That parasites may well have played a significant role in evolution is increasingly concerning many biologists. For a general review of this see Rennie (1992), or the more detailed discussion in Keymer and Read (1990). Although controversial, it is claimed that the evolutionary role of parasites has been underrated by biologists. Archaeologists also have tended to ignore these hidden manipulators of human behaviour and achievement.

That malaria may have played a major role in Melanesian prehistory is not a new idea. Its impact is too obvious in coastal regions in Melanesia today for archaeologists and anthropologists to fail to wonder at its long-term effect, but it is seldom mentioned by prehistorians. Indeed in recent years only Gorecki has recommended that intermittent malarial introductions may have influenced Highlands prehistory (1979). Webb (1990) has suggested that certain pathological features on some Pleistocene human remains in Australia might reflect the impact of malaria.

A probable reason for this is the misunderstanding (but not misreading) of the views of Livingstone whose seminal study, _Abnormal Hemoglobins in Human Populations_ (1967), although now outdated, is usually the first consulted by those curious to learn about malaria in Melanesia. It is worth quoting what he says so that this misunderstanding can be exposed.

After reporting on the 'spotty' distribution of some blood dyscrasias such as G6PD deficiency and the thalassaemias in malarious areas of New Guinea, he went on to say:

(This is) what one would expect if malaria had only recently become an important selective
Only a careful reading of the arguments preceding and following this statement makes it clear that the emphasis is upon the recency of selection rather than the mere presence of malaria, although the paragraph in which it is contained is ambiguous. It is much clearer in his later book (Livingstone 1985) where he clearly identifies the main selective pressure, in common with most authorities (below), with only one of the plasmodia, *Plasmodium falciparum*, which, as discussed in the next section, is clearly late on the Melanesian scene.

To avoid similar misunderstanding it is necessary to clarify the use of the term *endemic* diseases in this text. There is considerable variation in the use of 'endemic' in various disciplines. In its broadest sense, 'endemic' is used by biologists and biogeographers as equivalent to 'indigenous', 'native' or 'original' when referring to plants or animals of a particular region, the opposite of exotic. But when used in reference to diseases by the medical profession or parasitologists it is closer to its original meaning of 'belonging to a people' but is also extended to a region/country. The statement that a disease is endemic in a particular region means that the disease is long-established with permanent local causes. It does not necessarily mean that it was never exotic, nor that it is widespread with everyone infected during their lifetime. A disease can be endemic where only one sector of the population by age, sex, hygiene habits and the like is affected.

Where a (density dependent) disease is long-standing, however, it will have become, through a natural disease 'progression' as the population rises, a childhood affliction, with adults (experienced from childhood) enjoying partial or total immunity. When the population becomes large enough with the supply of non-immune 'fresh susceptibles' in the form of the newborn (or other sorts of recruits) greater than the threshold to maintain the disease, infection will be continuous with no discernible epidemics and this pattern will remain stable. Endemic disease is often used in this dynamic sense, particularly by parasitologists, as a contrast to epidemic. It is this finer distinction which I intend throughout this text, meaning specifically a disease which has progressed (through host population increase) to the youngest sector of the population to which its transmission characteristics give access.

Some confusion also exists, particularly in the archaeological literature, about the use of 'stable' population. It is essential not to confuse a stable population with a stationary population. In a stable population the internal dynamics remain constant. The proportions of people of different ages, sexes (determined by age-specific mortality and an unchanging reproductive effort) remain constant, but a stable population can be either increasing, decreasing or be stationary. It does not refer to natural increase but to the shape of the population 'pyramid' or more exactly life-table values.

Many questions about malaria are of relevance to the earlier prehistory of Sahuland but we will look at two principal questions: the antiquity of malaria in Melanesia and the nature of its impact, if it was present, on the prehistory of the region connecting what is now the island of New Guinea with Australia.

Some of the facts (with some key references) necessary to examine these questions are listed below. Unfortunately most issues in malariology abound with 'ifs', 'buts' and contrary views, and it is not possible in this context to give a balanced review of these. The following statements, to the best of my knowledge, would be acceptable to the majority of specialists.

**THE ANTIQUITY OF MALARIA IN MELANESIA**

1. Human malaria is as old as the hominids; the complex inter-relationship between the four plasmodid parasites, their intermediate host (*Homo sapiens sapiens*) and definitive host (certain species of the mosquito genus *Anopheles*), recommends a very long period of co-evolution (Garnham 1966). It is highly probable that the rainforest cousins of Lucy would have been familiar with plasmodia (certainly *P. malariae* and possibly *P. ovale*) and would have suffered from malaria.

2. The original homeland of the plasmodiids (as of the anopheline mosquito and the hominids) was humid rainforest, and co-evolution with their vertebrate and mosquito hosts could have started as early as the middle Cretaceous (Mattingly 1973). Although Garnham (1966) claims that even the most recent and lethal hominid variety, *P. falciparum* is probably no younger than the Miocene, recent analysis by RNA typing (Waters et al. 1991) suggests that it was originally an avian malaria acquired by man through lateral transfer (or zoonosis) within only the last 10,000 years, a view coincident with all other evidence suggesting the recency of its malignant role in human history (see 4. below). *P. malariae, P. ovale* and *P. falciparum* are clearly of African origin.
P. vivax, the most widely distributed form, is considered to be of Southeast Asian rain-forest origin by Sergiev and Tiburskaya (1965). In the taxonomic revision referred to above (Waters et al. 1991), vivax was the only plasmodium of human malaria close to the one simian plasmodium in their sample, P. fragile found in macaques in southern Asia (Sri Lanka, India).

3. The plasmodiids vivax and ovale can lodge in the liver causing a relapse up to two years after the initial infection (Bray and Garnham 1982). P. malariae allegedly has an even longer relapse period (Harwood and James 1979:196). All three could thus be easily transported within their human hosts over great distances; vivax is found as far north as Sweden and Russia and is throughout the tropics, malariae extends into the Mediterranean and may have been as far north as England and Holland, but ovale, the mildest of the malarias is largely restricted to north and East Africa.

4. P. falciparum, the parasite of malignant tertian malaria, is in contrast, non-relapsing and requires larger numbers of human hosts than the benign malarials to be maintained as a continuous infection. More significantly, as the parasite could not survive within its host beyond the duration of its initial infection, it also required large host populations moving rapidly for successful migration, one of the reasons for considering that its expansion out of West Africa was during the late Iron Age and its present pan-tropical dominance did not start until later Roman times.

Livingstone noted that Hbs (sickle cell) frequencies were not in equilibrium in East Africa and wrote 'the most reasonable interpretation of these non-equilibrium frequencies is the recent spread of falciparum malaria into these regions' (1967:91). In 1985, he reviewed from compendious haemoglobin data the evidence that Hbs is still spreading across into East Asia. As Hbs is strongly disadvantageous in homozygotes, the gene will persist and rise in frequency in a population only in response to a disease as lethal as falciparum malaria. Although the spread of a genetic response to such a lethal disease will always be slower than the spread of the disease itself, the evidence that Hbs is still spreading today does not recommend a great antiquity for the parasite in Asia.

In addition, prior to the African Iron Age, there were probably too few human hosts to move the parasite out of its West African homeland. It would also have required frequent and fast movements of large numbers of infected people to establish it in new areas. Zulueva (1973) and Bruce-Chwatt and Zuleta (1980), from comprehensive surveys of the literature, are adamant that P. falciparum was unlikely to have been present in Classical Greece (c.300 BC). In addition they failed to find any credible description of malignant (falciparum) malaria until AD 200. Along with other historians they are certain that the early Roman Imperial Armies suffered little from malaria but by the late Empire/Byzantine times falciparum was almost certainly present and having a major impact upon military campaigns. The abandonment of Roman Campagna in the last centuries of the Roman Empire (Celli 1933:12-13) may be another witness of the arrival of falciparum.

The progress of the parasite across into East Asia is probably masked in the epidemiological record by the newly emergent 'city diseases' which were wracking Asia at this time (McNeill 1976). Following its progress were genetic responses: in Southeast Asia Hbe, and still spreading from out of Africa the more effective Hbs (Livingstone 1985).

The date of the arrival of falciparum in Melanesia is unlikely to have been much before a thousand years ago. It is very important to note that this most lethal of malaria parasites had no role in the early prehistory of Sahuland.

5. For the expanding plasmodiids to become established in new areas also required the presence of large populations of receptive anopheline mosquitoes (Livingstone 1967:73-4). The additional requirement for mutual adaptation between the parasite and a new potential vector must also have affected the rate of expansion.

6. With several species among the rich coastal anopheline fauna of New Guinea receptive to plasmodia (below); little seasonal variation in climate in most of coastal New Guinea allowing all-year infestation; numerous rich mangrove and swamp breeding areas; and in most years the annual monsoons able to blow infected female anophelines the short distance from Southeast Asia, conditions for the transfer of plasmodia were ideal once their intermediate host was present in northern coastal New Guinea.

Five species of Anophelines have been identified as vectors of malaria in Melanesia.
The most widespread are *A. farauti* and *A. punctulatis*, both important on the Papuan South Coast but with the former the only vector in northern Australia. *A. farauti* generally breeds in saline/mangrove environments close to the shoreline whereas *A. punctulatis* extends further inland. Other species, *A. subpictus*, *A. koliensis* and perhaps *A. bancrofti* are also incriminated in other regions. In contrast to *A. gambiae*, the most important African vector, the Melanesian anophelines are relatively inefficient vectors in that they can be readily diverted to an animal blood feed, an important fact in malaria 'management' in coastal Melanesia in later prehistory but a theme, unfortunately, which cannot be covered in this paper.

7. For malaria to become permanently established, however, there has to be more or less continuous enroachment by the intermediate host (the crucial haemoglobin food supply) into or close to areas of anopheline concentrations, a situation which would probably not arise until coastal populations had grown, some time after initial colonisation. If, however, the original colonists were dependent on mangrove/swamp foods for survival and were frequently within range of anophelines, and the (relapsing) malarial parasite they brought with them was able to adapt rapidly to the new species of anophedes present, then it is possible that the parasite could have been successfully implanted from initial colonisation.

8. The speed and success with which the malarial parasites *vivax* (and possibly *malariae*) followed human colonisation of Sahuland depends crucially upon the numbers of the human intermediate host required to maintain the disease. Unfortunately, because malarial research is largely in the tropics, where lethal *P. falciparum* is now pandemic, many facts about the milder malarias (including thresholds and even case mortality) are difficult to abstract from available data. If, as I suspect from northern European evidence, where for climatic reasons *falciparum* was never present, the number of human intermediate hosts required to maintain *vivax* or *malariae* infections is low (<100 living or regularly visiting within the normal flying range of the local vector/s [Groube in prep., a, b]), successful introduction of these parasites into Sahuland could have been very rapid. It may have been only a few generations, with a succession of introductions and fade-outs, for the parasites to be successfully implanted in their new home. The ease with which malaria infection can be transformed from epidemic to endemic occurrence, discussed in the next section, because of the failure of the immune system to mount any form of long-term defenses against the parasite, is relevant here.

It seems more reasonable, however, that it would have taken a few millennia after initial colonisation for the minimum conditions for the successful introduction of malaria and its continuity, a human population of the right size in the right place at the right time, to emerge. Thus malaria may have been of little significance during the first millennia of colonisation of Sahuland, but like the situation across Torres Strait in late prehistory (below), the risk of introduction must have been ever present. Alternatively, if its introduction was rapid, it could have been constantly dogging the expanding colonists who could not escape from its threat until they entered regions where the crucial anopheline vector was not present (parts of Island Melanesia and arid Australia).

On the whole it would be incredible, considering the proximity of Southeast Asia (the possible homeland of *P. vivax*), if this parasite was not introduced before the end of the Pleistocene. Thirty thousand years is a very long time for insulation from this notably mobile parasite with conditions so favourable for its transfer. If it failed to become established during this time, it would imply either coastal populations were very low and/or highly mobile with a consequent lack of continuity of fresh susceptibles; the favourable areas for malaria transmission, swamps and mangrove environments were avoided, or both. The implications of this for the mangrove/swamp dominated coasts of Melanesia are obvious.

The oft-repeated claim for the absence of malaria in pre-European times in anopheline-infested northern Australia is clearly relevant at this stage but is addressed below after clarification of the nature and intensity of malarial infection which could be expected during the Pleistocene in New Guinea. The discussion concerns only *P. vivax* and *P. malariae*; as argued above, the most lethal of the malarial parasites, *P. falciparum*, played no role in this period.

**THE IMPACT OF MALARIA**

**Quantitative issues**

Malaria is a density-dependent disease, where the numbers of hosts (intermediate and defini-
tive) determine its transmission efficiency, proliferation and morbidity. Density dependent diseases are the principal agents of recurrent epidemics when numbers of hosts and parasites achieve critical levels. The mathematical inevitability of such sudden intensifications has been ably demonstrated from the 'threshold theorem' of Kermack and McKendrick (1927, 1932, 1933), and has been further developed for malaria by several epidemiologists (e.g. Bailey 1957; Aaron and May 1982; Bailey and Norman 1982) who have exposed the complex quantitative parameters of malaria occurrence. Some understanding of this 'numbers game' is essential to appreciate the significance of malaria in prehistory. Unfortunately, as Anderson and May (1979:361) point out, most epidemiological models (including those for malaria) assume a constant (human) population density (i.e. stationary). Long term demographic trends, the concern of historians and archaeologists, tend to be ignored. The reason for this is the concentration of most epidemiological studies upon applied aspects, the prediction, medical control and avoidance of epidemics. The epidemic time scale for such studies is almost instantaneous against the backdrop of prehistory.

The following discussion owes a great deal to these important works but is concerned more with long-term effects than epidemics and thus departs from them in many places. To avoid tedium, only a few references are supplied as most are available in any standard text on malarialogy and others are given in my forthcoming book (Groube in prep., b).

1. Despite the narrow medical focus of most models, the essential variables are well explored. For instance, vector concentrations are clearly critical; the dependence of the plasmodiids upon their definitive host is absolute, for it is within the mid-gut of a female *anopheles* mosquito that the crucial sexual stage of the complex life cycle of the parasite is enacted. Unfortunately for the parasitic invader, the average life span of any mosquito is only a few days, insufficient to complete the sexual stage of the life cycle (ten days or more, depending on the temperature and species), so that large populations of anophelines are essential to ensure that there are sufficient survivors not only to incubate the developing oocysts but to successfully re-infect another intermediate host. Thus malaria control measures which succeeded in reducing anopheline mosquito populations were effective (e.g. the UNRAA programmes in Sardinia and Greece) and sometimes eliminated the disease. In contrast, chemical therapy targeted at various stages in the life cycle of plasmodia within their intermediate host (e.g. chloroquin) has never eliminated the parasite.

2. Where there is an abundance of anophelines, the most important additional factors are the blood preferences and breeding habits of the local vectors. Complications such as the average longevity of the local anopheline mosquito species, their weekly 'bite-rate', and so forth, contribute to the difficulty of assessing the degree to which a population will be affected.

3. Critical minimal numbers of both hosts are required to ensure continuous malaria transmission even when all other conditions are ideal, but it is usually the mosquito definitive host which determines parasite loads. Thus the intensity of malaria parasitaemia and its morbidity/lethality is dependent on the number, density and transmission efficiency of the local anopheline vectors. Even when the human intermediate host population is very large, small and/or inefficient mosquito populations can infect only a small proportion of the population. In the reverse situation, a small human population (but larger than the minimum threshold for maintenance) with opportunity to be infected by a plenitude of efficient anopheline vectors will suffer serious continuous malaria. An over-abundance of both hosts can result in extremely high parasite loads which will be continuous if these populations remain stable (stable endemic malaria, as in many parts of Africa). If, usually for climatic reasons, the anopheline population is unstable, parasite loads will vary, reaching epidemic proportions when conditions are favourable, resulting in the unstable endemic malaria of Sri Lanka or parts of India. But if the human population is unstable for reasons other than the disease and fluctuates in numbers/density over long time periods, endemity (or status as a continuous, non-epidemic childhood disease) will not be affected until the population falls below the threshold for maintenance of the disease except where, as discussed below, there are major inputs of fresh susceptibles usually in the form of migrants (Bailey 1957:135-8).

4. Epidemics of malaria, typified by sudden increases in parasite loads in the human population, are largely determined by fluctuations in the numbers and or conditions for transmission of the vector. In West Africa,
for example, where the most efficient malarial vector Anopheles gambiae is prolific, there are few 'epidemics' of malaria. This is the classic 'stable' malaria of McDonald (1957). In contrast, the 'unstable' Malaria of Sri Lanka is typified by recurrent epidemics (Bailey 1957), the most serious of which are caused by random climatic events such as a poor monsoon which allow rapid proliferation of the relatively inefficient local anopheline vector. The severe epidemic of 1935 had such a cause (Briercliffe 1935; Groube in prep., b).

Fluctuations in the numbers of human hosts can alter levels of parasitaemia in an existing population with stable malaria but only if there is a rapid increase in the supply of fresh susceptibles such as by the arrival of large numbers of immigrants or visitors, the importation of indentured labour and so on. The sudden increase in available human haemoglobin, unencumbered by an already alerted immune system, allows a sudden growth in parasite numbers without a change in vector density. This resulted in the so-called 'immigrant malaria' formerly common amongst indentured labour on colonial plantations in Africa and Melanesia (McDonald 1957). In this form of epidemic the resident, malaria-experienced population is also at some risk because of the 'partial' nature of the individual immune response to malaria, as discussed below.

The immune response

Unlike the strong individual immune reactions provoked by many serious infectious diseases (particularly viral infections such as measles) the human immune response to all forms of malaria (as with many other protozoan diseases) is relatively inefficient (Wakelin 1984:35). Apart from the genetically endowed Duffy negative blood group, which appears to give effective lasting protection from vivax malaria in adults in West Africa (Livingstone 1984), the benign malarial (vivax, malariae) appear to have had little selective impact despite their long association with humans. One reason for this is probably the relatively low case-mortality of vivax and malariae (but see below) as the most frequent genetic response is the conservation of a variety of inherited blood abnormalities which disadvantages the developing parasite, but which are in themselves also disadvantageous to the individual host. The balance in advantages (balanced polymorphism) may not, with low malaria mortality, favour the retention of the inherited blood abnormality. Thus the widespread inherited blood dyscrasias now widely recognised as malaria suppressants (including HbS, HbE, Hba2, G6PD deficiency and a number of thalassaemias [Livingstone 1985]), high frequencies of which are almost certainly responses to the more lethal falciparum malaria, and like that malaria, recent in human history.

The relative impotence of the relapsing forms in promoting selection is supported by the conviction of most epidemiologists that the dyscrasias are principally a response to the selective pressure of lethal falciparum malaria. Luzzatto et al. (1983:160) write:

There is now overwhelming evidence that the three genetically determined pathological conditions that are most prevalent in the human species in absolute terms, namely sickle cell anaemia, the thalassaemia syndromes and G6PD deficiency are all related to selection by P. falciparum. (See also Luzzatto 1979).

Even falciparum malaria may leave little genetic imprint when it is not stable and endemic. The gross number infected over a long time period is much larger in stable endemic conditions than with epidemics. At certain levels of mortality and epidemic regularity the number infected can be too small to have a long-term selective effect. Thus the gene frequencies of the blood dyscrasias from the relatively poor data available from Sri Lanka (Livingstone 1985:80-1) do not even hint at the fact that this country has suffered some of the worst (recorded) malaria epidemics in history, with falciparum a principal culprit.

Despite the failure of the human immune system to develop an effective long-term response to the invasion of plasmodia, the short-term response by the individual immune system is rapid and continuous. The details of the immense 'battle' which takes place in the blood stream are too complex for this paper but they are on the whole effective, reducing the activity of the parasite and lowering the number of parasitised red blood cells, thus reducing the number of gametocytes, the final sexual forms in the human phase of the plasmodial life-cycle. With fewer gametocytes available for transfer to another mosquito from each infected individual the chances of eventual re-infection of another human host are lowered.

When everyone in a population experiences continuous malaria infections during the first five or so years of childhood, the survivors acquire a form of partial immunity or inurement called 'premunition' (Sergent 1963) which requires regular re-infection throughout adulthood as it
fades rapidly. This form of protection is only efficient if everyone in the community experiences malaria and develops individual premunition thus lowering parasite activity for everyone. Premunition (or 'seasoning' as Defoe's informants in Essex succinctly described it [1742, Letter 1:9]) does not protect the individual from re-infection but markedly reduces its lethal impact so that the parasite is rendered relatively benign, and few adult deaths will occur. High infant mortality during the five or so years required to establish acquired partial immunity is inevitable, with high risks to those with other medical stress such as malnutrition or other blood disorders such as anaemia or hookworm infection. Pregnant women, particularly in first pregnancies, are the most vulnerable adult targets, as discussed below.

The fact that there is no development of true individual short-term or long-term immunity following a malaria attack and everyone is susceptible to re-infection through life (although when premunition has developed, with greatly diminished risks after about five and a half years old) has important implications for prehistory:

a. The maintenance of continuous malaria in a population is not solely dependent on a supply of fresh susceptibles (the new-born or migrants), as for instance required in measles where life-time immunity develops after an attack. This is very important in understanding why small and often isolated populations with low birth numbers can still maintain malaria, as for example with the 'non-endemic' malaria in the interior forests in Borneo where it is transmitted by the forest mosquito *A. leucosphyrus* (Livingstone 1967), or in the 'Malaria houses' in Holland (Groube in prep. b). The critical threshold to maintain the disease is the whole population not just the population of fresh susceptibles, as with so many serious infections, although the numbers of fresh susceptibles (those under five and a half years old) able to support higher levels of parasitaemia than inured adults would still be very important. What this threshold population is for *vivax* and *malariae*, however, is unclear but from the European evidence isolated from the complications of the presence of *falciparum* it appears to be very low, perhaps less than 100 persons (Groube in prep. b).

b. The lack of development of true individual immunity has another important implication: the transition of the disease from epidemic occurrence to continuous infection (i.e. from epidemic to endemic) could be extremely rapid when the population experiencing the disease for the first time (a 'virgin' population) is already above the threshold level. If, as claimed above, this threshold is <100 for the benign malarials, there would have been frequent opportunities in the early prehistory of Sahul and for such encounters.

In theory at least, if the population was at or above the threshold numbers for continuous maintenance of the malaria, the transition (or progression) to endemicity need be no longer than the five years required to develop premunition in the population. Following the morbidity and fatalities of initial introduction to this 'virgin' population, which would be in the form of an epidemic lasting five years (as premunition developed), only those younger than five and a half years would still be at risk. If the high levels of parasitaemia in this 'at risk' group plus the lower levels from adult infections in the rest of the population achieved or were greater than the threshold, then the disease would be at endemicity following a single epidemic.

If the threshold conditions were not met in the aftermath of the initial five-year epidemic, then the malarial parasite would be unable to maintain itself and die out. As the inured sector of the population could no longer 'top-up' their immunity with occasional attacks, the communal premunition would fade and the whole population would become susceptible once again and vulnerable to another five year epidemic if they came into contact with the parasite again.

c. The communal premunition enjoyed by a population with endemic malaria can, as mentioned above, be disturbed by the arrival of migrants or other such fresh susceptibles. Obviously in a stable endemic state, there must be an equilibrium between the activity levels of the parasite and the sum of the dampening effect of the individual immune responses in the form of premunition. With the level of protection developed in individuals in the community adjusted to the local (probably quite modest) levels of parasitaemia established by the equilibrium between parasitic activity, communal premunition and the efficiency, numbers and habits of the local mosquito, the sudden influx of new supplies of unprotected haemoglobin in the form of migrants, indentured labour, slaves or invading armies from non-malarial areas would cause a rapid expansion in parasite numbers with a consequent epidemic. Although this would obviously have a severe effect upon the newcomers, it could also, from the vastly
increased numbers of parasites in circulation in the population, overcome the existing individual immune protection, particularly in vulnerable children and pregnant women. It would thus destabilise the communal pre-munition developed over many generations of mutual adjustment between the parasite and its intermediate host. The protected population would again be at risk.

This aspect of malaria impact, in many ways the reverse of the pattern of 'virgin soil epidemics' which have been of such interest to historians (McNeill 1976; Crosby 1986), has received little notice but is very important in prehistory. Perhaps the otherwise inexplicable replacement of the Sumerians (obviously well adjusted to their malarial environment in Southern Mesopotamia) by the Akkadians, apparently initially encouraged to settle as labourers but coming from the non-malarial highlands, is a prehistoric witness of the fatal effects upon the resident (inured) population of systematic migration into an endemic malarial region.

Thus the nature of malarial tolerance, the equilibrium established by pre-munition, encourages population replacements of the resident inured by the immigrant uninured, assuming of course, that there is a continuous availability of replacement immigrants from a non-malarial source. This has implications for the expansion and success of some of the large language families (e.g. Indo-European [Groube in prep., a, b]). In addition, the classic 'virgin soil' epidemics are also encouraged by the fact that pre-munition is developed against the local species/strains of plasmodia (Walliker 1982), and movements from malarial regions with different strains can also have fatal consequences.

d. Thus, in theory at least, the presence of endemic malaria should over a long time period discourage movements between and into infected groups, particularly the movement of vulnerable women as brides. Perhaps it is no coincidence that two of the regions dominated by matrilocal marriage (where the woman does not move), West Africa and parts of Island Melanesia (particularly Bougainville), are also highly malarious. A trend towards isolation and a community size adjusted to the flying range of the local anophelines vector could be expected where endemic malaria has been established for a long time (below). The relevance of this observation to the situation in Melanesia is obvious.

The mortality of the pre- \textit{falciparum} malarias

All the above discussion appears to be begging the question. If \textit{vivax} and \textit{malariae} infections are 'benign', as their medical labels suggest, then there would be too few deaths for much effect. Because so few of the intensively studied malarial regions lack the more lethal \textit{P. falciparum}, mortality rates for \textit{vivax} and \textit{malariae} alone are difficult to establish but there are reasons for thinking that 'benign' may be a misnomer for the effect of these diseases before \textit{falciparum} made its appearance.

The only glimpse we have of the mortality of malaria in the absence of \textit{falciparum} is from accounts of Europe north of the Mediterranean. These are on the whole unsatisfactory, but where they can be trusted, they do not suggest that the local strains of malaria were at the time 'benign'. To gain some impression of this it might be useful to read Defoe's description of 'the strange decay of the female sex' in the English coastal marshes (1742, Letter 1.8-9; cf. Smith 1956) and the chilling confirmation of his observations by the historical geographer Dobson (1980). The mortality from the persistent epidemics of malaria in Holland [Groube in prep., b] also, does not justify the label 'benign'.

The contradiction between this label and what facts can be assembled on malaria mortality in Europe are reviewed in Groube (in prep., b), but it will be useful for the region under review, where \textit{vivax} and \textit{malariae} are the only possible candidates for malaria impact during most of prehistory, to expose an essential irony inherent in the problem.

The term comes, as do most in malariology, from medical experience in Africa. In West Africa the label would be well deserved, for there, as discussed above, the only known effective genetic defence against \textit{vivax}, the absence of the Duffy receptor on the red blood cell (Duffy negative) has made \textit{vivax} relatively innocuous. But in the remainder of Africa, as throughout the rest of the tropics, where the Duffy negative blood group is absent, \textit{vivax} infections still cause fewer deaths than \textit{falciparum}. But if, as all evidence suggests, \textit{falciparum} is a latecomer on the world scene and is the specific disease which caused the emergence of many balanced polymorphisms such as \textit{Hbs} (sickle cell anaemia), we have a delicate but statistically verifiable irony. The higher lethality of \textit{falciparum} which promoted the conservation of otherwise harmful blood dyscrasias which are equally (if not more) effective against the milder malarials would have thereby reduced their lethality. Perhaps the
'benign' malarias owe their reputation to the blood dyscrasias selected by the mortality pressure of *falciparum*. The lethality of both *vivax* and *malariae* must have been reduced by the increase in the prevalence of the various haemoglobin abnormalities which followed the introduction of the more lethal *falciparum* parasite. Thus their pre-*falciparum* mortality could have been much higher, but not high enough to promote by themselves the conservation of inherited haemoglobin abnormalities.

Despite this possibility it would be unwise to assume that the average annual case mortality of *vivax* malaria in the pre-*falciparum* era was higher than 1% per annum. As the discussion on population regulation by endemic malaria in the next section suggests, case-mortality is only one of the relevant variables needed to assess the long-term impact of an endemic disease such as malaria. Before looking at this, however, two further aspects of malaria mortality must be discussed: placental malaria and the impact upon the individual immune system of the repeated bouts of malaria experienced in an endemic region.

Placental malaria is a particularly sad and demographically crippling aspect of the disease. The parasite lodges in vast numbers in the placenta of the developing foetus to escape the immune system, particularly the circulation of blood through the spleen where parasitised red blood cells are identified and removed. The enormous parasite load can overcome the existing immune defences of the woman, particularly in first pregnancy, causing anaemia, foetal loss and death. Perhaps the best way of documenting the scale of this risk are the grim statistics from the Ceylon epidemic of 1935 where between 20% and 30% of cases were of *vivax* malaria. In one hospital 13% of pregnant women died of malaria, in another 24%; of 253 deliveries, total foetal and infantile mortality was 67% (Groube in prep., b). Perhaps Defoe's informants referred to above were not exaggerating.

It has been well known for many years that endemic malaria depresses the individual immune system, making individuals who have experienced malarial attacks more vulnerable to other, less lethal unrelated infections (Wakelin 1984:46). Thus in Africa today childhood measles is still a killer in malarious areas (Grist et al. 1987:205). The morbidity of other blood disorders is also enhanced: hookworm infection with malaria is a serious killer of pregnant women (Wickramasuriya 1937) and so forth. Complications in assessing the scale of this secondary impact upon unrelated diseases, such as the prevalence of malnutrition and anaemia (itself often of malarial origin) in many of the afflicted populations, make it impossible to assess the impact of 'malaria-related' mortality in prehistory. Indeed, how effectively *vivax* alone suppresses the immune system is unclear because of the presence of *falciparum* in these regions, but the impact of immune suppression plus the associated anaemia of endemic malaria must have been additional health hazards which would have increased mortality in crises such as temporary food shortages, injuries and other infections.

**Population regulation by pre-*falciparum* malarias**

For the historian and prehistorian, epidemics, despite their trauma and often indelible records, are of importance because of their overall impact on that most crucial of demographic facts, natural increase. The less spectacular effects of endemic (childhood) diseases are often ignored. If the Plague, for example, had killed only children instead of a third of the English clergy, it might not have made the history books, indeed would have been lost even in folk-memory within a few generations. The social cost of epidemics with the loss of adults in whom society has already invested heavily is obviously greater than endemic infant deaths (Hassan 1981:157-9). The latter can be replaced more cheaply, albeit at an additional cost to the crucial breeding females.

But because endemic diseases have continuous (although lower) mortality, their long term biological impact can be more serious than occasional high mortality epidemics. To give a relatively trivial example, an epidemic disease with a case mortality of 20% but with an average interval to the next epidemic of 40 years has an overall annual mortality of only 0.5% which is probably less than the average annual case-mortality of endemic *vivax* malaria before the appearance of *falciparum*. But any old people who died in the epidemic would have no effect on long-term natural increase, being beyond the reproductive age, whereas in endemic *vivax* malaria all the deaths would be of potential breeders. In assessing long-term impact upon natural increase the regularity of infection and who dies is as important as case-mortality.

This can be more formally expressed from the following equation which excludes deaths of those beyond reproductive age. For any disease where immunity develops (either immediately or delayed) the impact upon long-term natural increase can be estimated from the following equation:
months when maternal immunity fades. With the deaths months to five and a half years.

Maintenance and the partial nature of the immune mosquitoes have ready access to infants, malaria mission conditions for the short-lived virus occur settles down to endemity after the age of six years can become endemnic due to its low threshold for maintenance and the partial nature of the immune response (above), the complexities of slowly changing population shape and natural increase characteristic of such a disease progression need not concern us further here. This period of unstable epidemics, however, can be lengthy and traumatic with most other human diseases; it is usually ignored in standard epidemiological modelling where interest is focused on the actual epidemic. Anderson and May, for example, in their important dynamic approach to disease impact 'discard the transient initial population values en route to the steady state' (1979:363). For prehistorians, however, the demographic stress of this adjustment period can be vital. It has left, I believe, indelible records, particularly in the form of population displacements in some important prehistoric regions (such as the Middle East), an issue discussed elsewhere (Groube in prep., a, b).

But whether death occurs during infancy or later, such as between five and eight years as with endemic measles, the biological effect is the same: the loss of individuals before they have reproduced. The social costs, however, will be different; the 'calorific cost' of children in the age-range of measles deaths is over 30 times that of infants (Hassan 1981, Table 9.2). In addition, a death in the first year can ease birth-spacing stress, making replacement less costly to the crucial breeding population (below).

The interval of disease recurrence (d) is preferred to 'epidemic interval' because of the variety of definitions of 'epidemic' in current use. This is the area of great haziness in terminology which the definition of endemic recommended above was trying to clarify. Thus a disease which is technically endemic such as measles or chicken pox (i.e. has settled down to the youngest age group to which its transmission characteristics allows) may still occur as periodic intensifications (or epidemics) because of other factors such as changing transmission conditions or simply because of the rate of production of fresh susceptibles of the requisite age.

Thus employing (d), a classic endemic disease which is continuous or seasonal (annually) such as malaria, would have a value of 1 whereas chickenpox, similarly endemic but influenced by other non-density factors, might have an average recurrence of 3 years. It is the different values of (d) which have the greatest impact upon long-term natural increase, as the examples below make clear.

There are many simplifying assumptions in equation (1). Some of the more important are: 1. The only source of fresh susceptibles are the cohorts of new-born (no immigrants).
2. Reproductive effort (= number of children per woman) remains constant. The effect of changed reproductive effort is discussed below.

3. In the long period required to reach stability (see below), variations in climate, birth numbers, sex ratio and so forth are random and do not affect the long-term result.

Thus to give some hypothetical values for equation (1) which would be acceptable in the early prehistory of Sahuland:

a. a target population with an original natural increase \( n_i \), of 1.001 (Hassan 1981),
b. an average age for the birth of the last child (\( \beta \)) of 37 years, which represents healthy first settlers (cf. !Kung, 34 years, [Howell 1979]),
c. vivax malaria with a case-mortality (m*) of 1.005 (1/2%) which has settled down after the natural disease progression to an average disease recurrence (d) of 1 year (endemic), with 100% infectivity of those who have not yet developed premunition (\( t = 1 \)).

It will eventually stabilise after a period of dampening oscillations in natural increase at:

\[
n_i = 1.001 - \frac{(.005) \times (5 \times 1 + 1)}{37 \times 1}
\]

\[= 1.00019 \quad \text{(a reduction of 80% of } n_i)\] ........... (2)

If, however, the same population met up with a disease of the measles mortality pattern with a higher case-mortality (1.02, 2%), but (unlike measles) with a low enough threshold to allow it to settle down to an epidemic recurrence pattern at an average value for (d) of 4 years and infecting the five to seven year olds only (\( p = 2 \)) and with 100% transmission efficiency, we would have:

\[
n_i = 1.001 - \frac{(.02) \times (2 \times 1 + 1)}{37 \times 4}
\]

\[= 1.00059 \quad \text{(a reduction of 40% of } n_i)\] ........... (3)

If a high threshold viral infection like measles did invade Sahuland it would rapidly vanish as it would not be able to maintain itself and would have to be reintroduced. Any occasional visitsations of high mortality diseases despite the trauma of the actual epidemics, could have only a small effect on long term natural increase. If for example, during the long period of Pleistocene prehistory there were occasional introductions from Southeast Asia of a smallpox-like disease with a case-mortality of 20% (m* = 1.20); a medium level of infectiveness (\( t = .5 \)); and an average recurrence rate of only 500 years (d = 500) its long term impact would be:

\[
n_i = 1.001 - \frac{(0.20) \times (37 \times .5 + 1)}{37 \times 500}
\]

\[= 1.00079 \quad \text{(20% reduction throughout the period)}\] ........... (4)

If the smallpox was a more frequent intruder, with an average recurrence of only 100 years, the \( n_i \) would then be .9999 or permanent population decline. Thus the argument presented here is not that epidemic diseases are unimportant, as the above examples illustrate, but that endemic diseases such as malaria are equally if not more important. If the hypothetical values for the population were considered unrealistic (e.g. \( n_i, \beta \)) and changed throughout, the ultimate values of \( n_i \) would change but not the order of severity of long term impact; vivax malaria would still have the most depressing effect upon long-term natural increase. The derivation of equation (1) and the more complex expressions which cover other disease patterns are given in Groube (in prep., b).

The calculated example for vivax (equation 2) shows that, given the generally accepted natural increase rates and parameters of small-scale, pre-agricultural populations in prehistory (as for instance well-documented in Hassan 1981), a low case-mortality disease like vivax malaria can, when endemic, have a marked impact upon long-term natural increase.

The length of the period following the initial introduction of the disease until stability is reached at the new \( n_i \) can be estimated for vivax malaria because of the quick transition to endemicity that is possible. After approximately \( \beta^2 \) years the natural increase between each age grade in successive years will be the same as that of the population as a whole at \( n_i \) (i.e. the population will be stable). But for nearly half of this period the natural increase will be close to a new value. In the example given above, where \( \beta = 37 \), it would take 1369 years to reach stability. For most other diseases, where endemicity is preceded by a lengthy period of natural disease progression from epidemic to endemic recurrence, the calculation is more complex and depends crucially on the specific age mortality structure of the population before the introduction of the new disease.

Given the number of simplifying assumptions and the hypothetical values used in the above calculations it is hardly likely that the above
values for \( n_i \) have any reality; what is important is that the reduction in natural increase even at the low mortality rate used for \( vivax \) malaria is of the order of 80%. If, for example, malaria-related deaths and maternal mortality were added to the mortality rate used here, the case-mortality of \( vivax \) might be as high as 1.01: at this value the population above would go into permanent decline (.99994). The nature of the partial immunity to malaria is crucial. If immunity was established within the first year instead of after five years the reduction would be only in the order of 40% as in the measles mortality pattern (equation 3) above. That long-term natural increase is influenced by the failure of the immune system to develop full immunity against malaria is an essential element in understanding how \( vivax \) malaria, even with a low mortality, can regulate populations.

The situation looks grim except for one of the above assumptions: that the reproductive effort remains constant throughout this period. As the increased infant mortality with malaria (unlike, for instance, measles) would allow some relaxation of the existing birth spacing because of the more rapid restoration of fertility, it would not be sufficient to compensate for the deaths from malaria, particularly as in the above calculations I have excluded foetal loss and maternal mortality, both of which as has already been documented can be very high.

Obviously if the breeding sector uplift their reproductive effort to match the cohort mortality in equation (1) the original natural increase will be restored and all will be well. But lifting the number of births per woman by 3.0% annually (which would mean only a reduction of about 2 weeks on average birth-spacing) may not be easy (= \( m^* - 1 \) x (p + 1)). It will depend upon whether the already low natural increase (1.001) is due to social and other artificial population checks, ecological constraints such as shortage of protein (with resulting ill-health and mortality), or to existing high mortality rates from other diseases, trauma and the like. Social checks on population such as infanticide or enforced celibacy could be abandoned and the former natural increase restored, but with ecological constraints or existing high mortality the additional maternal stress of increased reproductive effort, which has to be in the form of reduced birth-spacing, might introduce other hazards. Not the least of these could be increased morbidity, foetal loss and mortality from placental malaria (above). More regular pregnancies would obviously aggravate this problem.

It is pointless to speculate too closely on unknown demographic variables in such a remote period. What is clear is that the natural increase rate appears to have been very low throughout the prehistory of Sahuland. Infections of \( vivax \) and \( malariae \) offer one possible check upon natural increase which could have been in operation from an early stage in the prehistory of the region. But, as the previous paragraph makes clear, this check would not by itself regulate natural increase. It could too readily be compensated by increased reproductive effort if mortality from other causes was low.

An important additional pressure which may have reinforced the regulatory role of malaria does not involve speculation. This is the important birth-spacing threshold of four to five years Lee and his colleagues (see for instance Lee 1979:324) have exposed from studies of the !Kung and other ethnographic hunter-gatherers. For a mobile group, short birth-spacing intervals can present problems because of the additional workload and physiological stress on the mother. Their arguments are particularly apposite for the colonisation phase of Sahuland prehistory when vast areas of land were being traversed. If, in the rigours of this period, birth-spacing was already close to the threshold where additional infants would be a problem, then compensating for the long-term impact of malaria would have been difficult without a change in life-style toward a more sedentary settlement pattern and a subsistence strategy which allowed more time for child-bearing and rearing: in essence the pressures for intensification. In the malarial coasts of Melanesia, even where populations are not fully dependent on agriculture as with the sago-collectors of the Fly and Purari deltas, subsistence strategies are still very intensive and groups are not mobile. The contrast with non-malarious Australia may not be a coincidence. In the arid south of Sahuland a pressure to sacrifice mobility for increased fertility did not have to be made.

The possibility that the groups settling Sahuland in remote prehistory may have already been close to the five year birth-spacing threshold is reinforced by what little (and inadequate) data we have on the demography of pre-agricultural populations and by theoretical considerations. From an examination of published mortality figures from both cemetery and ethnographic sources it is evident that in order to have a birth-spacing greater than five years, the survival rate for the population at age \( \beta \) (above) must be close to or greater than 50%, unless the breeding group is
abnormally efficient, e.g. high rate of re-marriage of widows, low incidence of ageing sterility, a very young age for birth of first child and so on (Groube in prep., b). We have little evidence from archaeological sources (cemetry remains) to support such a high survival rate at any time in prehistory; the closest, from the Sahaba Terminal Palaeolithic (Hassan 1981:Table 7.1.1.) is of dubious validity, as Hassan himself admits (1981:111). Howell's outstanding study of the Dobe !Kung (1979) established natural birth spacing intervals of between three and four years, below the theoretical threshold for hunter-gatherers. It would be unreasonable to expect the early colonists of Sahuland to be more healthy and fecund than anyone else in that remote period, but with such a vast country to explore and for many thousands of years little pressure on resources, it is probable that birth-spacing hovered close to the threshold where mobility inhibits or threatens fertility. It was the existence of such a threshold which made vivax such an important population check.

The arguments presented above essentially repeat the claim of Anderson and May (1979: 364) that at certain critical mortality/natural increase rates a disease can actually regulate population density/sizes (see particularly their equation 13). I do not believe this has been fully appreciated by archaeologists.

Population regulation by malaria, however, is not only quantitative. In a malarial landscape, the opportunities for inoculation by the plasmodial-carrying mosquito and thus, eventually developing premunition, vary with distance from the mosquito breeding grounds, the direction of the prevailing winds and of course any cultural controls such as clothing, nets and the like. The most dangerous part of the landscape is where malarial mosquitoes are only intermittent and rare visitors. Disrupted transmission would inhibit the development of premunition and from time to time serious epidemics could occur. It is not surprising, perhaps, that the intermontane zone on the island of New Guinea between the malarial coast and the non-malarial interior is so thinly populated.

The importance of such disease gradients in more recent prehistory and history is well exposed by the historian McNeill in his important book Plagues and Peoples (1976). Unfortunately too few archaeologists appear to have read, or perhaps understood, the book. The fact that what is now tropical northern Australia was a malarial gradient zone during the Pleistocene is an important part of our story. It is the gradient zones and not the areas of intensive infestation which would be avoided in prehistory. Like Sri Lanka, they would be areas of unstable and dangerous malaria, where endemicity could never develop and adult deaths were always a risk. Child mortality, as is evident from everywhere in the world, can be tolerated but adult deaths can have social and psychological impacts far outweighing their strictly biological importance. The death from malaria of an elder or leader beyond the age of breeding is biologically irrelevant but could be remembered for many generations. The association of such deaths with the swamps in the gradient zone is very likely although the role of the mosquito would never have been suspected.

The tendency of plasmodia to develop different regional and local strains (very evident in falciparum, less so in vivax and malariae (Walliker 1982)), and the fact that acquired partial immunity is strain and species specific are also important in population regulation. An obvious need is to keep a population down to the size/density where the entire population can be inoculated with the local strain and develop communal premunition. In fact a population which expanded beyond the range of the local vectors would eventually acquire adult susceptibles with the risks this entailed. They could induce a local variant of 'immigrant malaria' (above). This form of localised density/size control was probably a more important regulatory pressure in prehistory than the gross quantitative effects raised above.

In addition to all these factors, and also of great importance to archaeologists are those aspects of the 'life-style' of the intermediate hosts which affect the frequency of incursions into anopheine infested swamps. A population which shunned such swamps could avoid infection and might eliminate the parasite. A population with the inconvenient habit of returning irregularly (but within two years) to the same mosquito-infested area could maintain it but suffer epidemics from time to time. The population which entered the swamps and lived permanently there would develop premunition and survive quite well despite high infant mortality and anaemia. This important strategy of 'learning to live with malaria' was, as argued in a forthcoming paper (Groube in prep., a) one of the major turning points in Melanesian coastal prehistory.

Many other quite subtle behavioural and cultural adaptations to the presence of malaria in Coastal Melanesia are reviewed in that paper. One is relevant to the theme here: the difficulty of recruiting brides from non-malarial to malarial areas or even between malarial areas with different strains. As will already be evident from
the descriptions of placental malaria given above, a bride from a non-malarial area was taking a frightful risk in moving into an endemic malarial village. Defoe recounts the local men's cynical view of this form of marriage traffic into the Essex marshes (Defoe 1742, Letter 1:8-9). The mortality as Dobson (1980) documents was of epidemic proportions. We shall return to this issue in the final section of the paper.

Thus, in summary of this rather lengthy section on the impact of malaria, it is probable that:

1. Malaria was present during the Pleistocene in New Guinea, possibly from within a few millennia of initial settlement. It would have rapidly become endemic in favourable coastal areas.

2. As the evidence for the presence of *P. ovale* in Melanesia is dubious (George Nurse pers. comm.), the pathogens were *P. malariae* and *P. vivax*.

3. The most lethal of the plasmodia, *P. falciparum*, and the suite of genetic responses it provokes were not present at this time. Its impact in coastal Melanesia is another story (Groube in prep., a), but it has much to do with the puzzling linguistic and genetic diversity of the present populations which are almost certainly still adjusting today to the recent arrival of this lethal parasite.

4. Unless other (non-malarial) mortality of the migrants was unreasonably low, a birth-spacing close to the threshold of between four to five years for mobile hunter-gathers identified by Lee and his associates (above) can be expected. The long-term impact of endemic malaria in suppressing natural increase might, therefore, have been difficult to overcome by increased reproductive effort.

5. From the time of its first appearance malaria would have imposed a number of 'choices' in life-styles (particularly settlement strategies) if the stress of unstable malaria was to be avoided. Such 'choices', it must be added, would not be based on awareness of the nature and cause of the illnesses and deaths but on the survival success of groups with different life-styles. Such accidentally developed strategies must have ranged from abandonment of infectious areas through a number of customs, such as visiting swamps only during daylight which minimised vector opportunities, to the suite of adaptations required in 'learning to live with malaria'. These divergent strategies have left a permanent imprint upon the human geography of Melanesia and Australia.

6. There should have existed throughout the Pleistocene a disease gradient zone determined by the prolificity of the *anopheles* vector which would have included what is now northern tropical Australia. This would have been a region of unstable (epidemic) malaria.

**A CASE FOR VIVAX AND MALARIAE IN NORTHERN AUSTRALIA?**

With Australia and New Guinea a single land mass during the Pleistocene, it follows that these parasites should also have been present in coastal regions of what is now northern tropical Australia throughout the period when these choices in life-styles were being developed. It is essential, therefore, to review the controversial issue of whether malaria was ever present on what is now continental Australia. The issue has recently been revived by Webb (1990) and should be reassessed on the basis of modern evidence, particularly the fact that *falciparum* malaria could never have been present.

Although it is clear that the increased contact between Papua and northern Australia during the last 200 years provoked a number of serious epidemics of malaria (reviewed by Cilento and Cooling n.d.; Mackerras and Sandars 1954; Black 1955), the evidence for the pre-European presence of the disease is, to say the least, dubious. If, as is generally thought, the slowly shrinking isthmus connecting New Guinea with Australia became increasingly insular with the rising sea levels, it may have served as a barrier to continuous transmission of the parasite. The absence of the crucial anopheline vector in some of the Torres Strait islands during the historic period (Mackerras and Sandars 1954) reinforces this possibility. The disease could be easily eliminated if the vector's density fell too low.

A small shift in the frequency with which the intermediate hosts stray within range of the mosquitoes, particularly if this results in the desertion of anopheline infested areas for two or more years could spell doom for the human plasmodiids. Although the monsoonal climate of northern Australia would not inhibit relapsing *vivax* or *malariae*, seasonal abandonment of the coastal and low lying areas during the wet season could be sufficient to break the fatal chain of transmission. A greater availability of food resources away from the monsoonal coasts and a reduced requirement to exploit lagoonal and estuarine environments during the peak activity of the mosquitoes would favour elimination of the disease. Nevertheless, if malaria has *never*
been present in monsoonal Australia before historic times, the case for its presence on the island of New Guinea during the late Pleistocene is greatly weakened.

One of the reasons many authorities reject the possibility of malaria being a prehistoric disease in Northern Australia is the total absence of any genetic witness of the disease amongst Australian Aboriginals. Blood dyscrasias, such as abnormal haemoglobins (sickle cell, HbA, HbE), G6PD deficiency and many thalassaemias (Livingstone 1967, 1985), high levels of which are now considered to be responses to the selective pressures of *P. falciparum* malaria, are missing in all tested Australian populations (Livingstone 1985:35-6). But as the preceding arguments make clear, the appearance on the world scene of *P. falciparum* is long after the severance of any link between New Guinea and Australia, and so there could never have been sufficient selective pressure to promote the conservation of any favourable abnormalities. In addition, although endemic where it did occur, the disease may have been affecting only small pockets of the population. Unless such pockets remained isolated biologically, any genetic responses which did start to emerge would become diluted and lost in the larger gene pool which the systematic exogamy of the Australian marriage system prescribed.

In northern Europe where *P. vivax* and *P. malariae* were endemic in many areas (possibly for thousands of years), such blood dyscrasias are also virtually non-existent despite widespread testing (Livingstone 1967:74, 1985). This does not give confidence that the absence of malaria-selective genetic witnesses in native Australians has any relevance to the question of whether *vivax* and/or *malariae* were present during prehistory. A similar argument is used to support the absence of malaria from pre-Columbian America (Dunn 1965), a claim which is disputed by some authorities (Bruce-Chwatt 1965; Zulueta 1980) on other grounds, including failing to differentiate between the different plasmodial genera.

If the argument proposed here that malaria withered out in northern Australia from lack of host numbers in the crucial anopheline areas after the ocean barrier was in position, has any validity, even mildly advantageous dyscrasias with few clinical symptoms such as Alpha-thalassaemia would tend to disappear as malaria declined. Thus the hints from a globin-gene survey of Australian Aboriginals of enhanced levels of α-thalassaemia deletions in northern Australians may also be relevant, particularly as one of the alleles, α*3,4* is also known on the south coast of New Guinea (Yenchitsomanus et al. 1986). Other genetic systems may also indirectly reflect that selection was once more important in the north. Thus in a number of studies of the Human Leucocyte Antigen (HLA) system there appears to be a clear separation of northern Australian populations from those in central or western Australia in the HLA A, B, and C systems (Serjeantson 1989: Tables 3.1, 3.3, 3.5). Although selection pressures are accepted as an influence upon the HLA system and none of the Oceanic reports appears to mention malaria as a possible agent, Piazza et al. (1973) and Osoba et al. (1979) report possible associations between malaria and HLA in Sardinia and Tanzania. As it is widely acknowledged that continuous malaria infection depresses the individual immune system, thus increasing susceptibility to other infections, some reflection in the genes controlling the immune system is not unlikely. Thus the consistent genetic separation of Northern Australians from those in the south reflected in so many systems (Lie-Injo 1976:Tables 4 and 5; Simmons 1976:Table 1), although at about the level of clinical variation one would expect in such a large land, may also reflect differential selective pressures between the north and south in the not too remote past.

It is obvious that the evidence for the presence or absence of malaria in prehistoric northern Australia is (and will probably remain) ephemeral, but one stray record from Prince of Wales Island off Cape York Peninsula does hint at the presence of localised malaria. On this large island, apparently most terrestrial animal resources had been exhausted. During the wet season the inhabitants, the Kaurareg, retreated annually to a favoured location in the mangroves where they built long-houses and subsisted on the edible parts of a local mangrove species (Moore 1979:275-6). This was obviously ideal for the establishment of *vivax* or *malariae* malaria, so that it is interesting in the testimony of Barbara Thomson, who lived with the Kaurareg for five years on Prince of Wales Island, that there are several references to persistent bouts of 'doopoo' during the wet-season.

Then I got bad with doopoo, began all shivering and then perspiring. It lasted a fortnight, sometimes bad, sometimes better... (in Moore 1979:176)

If this was malaria (rather than scrub typhus or perhaps dengue fever) it is clear that the Kaurareg were sufficiently familiar with it to have developed a remedy (Moore 1979:127), and as there are no reports that they died from it, were probably inured to the infection from
childhood. This does not suggest that it was a recent European introduction. It could be a classic example of endemic vivax/malariae infection maintained by a seasonally sedentary group living closely together within range of anopheline breeding areas, a model, perhaps of one of the many ways the earliest colonists could have coped with the disease.

The case of the Kaurareg gives added caution about claims for the absence of blood dyscrasias in Australian Aboriginal populations. In 1869 they were virtually 'annihilated' in a 'reprisal' by whites and in 1898 Haddon had difficulty finding any survivors (Carroll 1969). Any witness of genetic tolerance to malaria which they may have conserved in their blood was annihilated with them.

The contradictions revisited

To return to the problems about this volatile region with which the paper opened, it is clear that the possible presence of malaria offers some relief from the burden of the apparent 'failures' in archaeological expectations. Some of the more obvious possibilities are spelt out below. These, however, are necessarily speculative, opening up the opportunity for a more serious examination of the issues by Australian and Pacific archaeologists.

1. The demographic dilemma of a long prehistory but only modest population sizes at the time of European discovery is afforded some relief. The pre-falciparum malarias could, in theory, effectively slow down population growth if the pre-disease mortality was close to that of comparable excavated prehistoric populations. When endemic they would certainly have imposed local checks on the size and distribution of populations in or near to anopheline concentrations. This alone, combined with the standard checks imposed by carrying capacity in the more southerly regions, may have been sufficient to account for the apparently low natural increase during prehistory. Certainly if these malarias were present in early Sahuland prehistory the contradiction between demographic expectations and performance is greatly reduced.

2. If the arguments developed in this paper have any validity then it is clear that the barrier between New Guinea and Australia was initially not primarily caused by the bottleneck of the narrow isthmus nor encroaching ocean but determined by the marked disease gradient in malaria between the humid tropical north and the arid-temperate south. It was a formidable region of unstable malaria long before the end of the Pleistocene.

3. The contradiction of (potentially) rich tropical coastal resources with a profound archaeological silence during the millennia before and after the rising of the Arafura sea is also lessened. The attractions of the coastal resources may not have compensated for the trauma of epidemic fatalities. The strategy of 'living with malaria', plunging into the midst of the mosquitoes and suffering the initial epidemic and continued infant mortality and adult morbidity after endemcity is established, is neither obvious nor pleasant. It also required a more sedentary way of life than any groups at the time were willing or able to tolerate.

4. During a lengthy period of unknown duration, then, the population of the gradient zone could have been extremely low, not only because of the unstable conditions typical of a disease gradient zone, but because unexpected deaths, particularly of important adults, generate in most societies the forms of social hysteria which become transformed into beliefs in witchcraft, sorcery and the like. Thus fear and a bad reputation could have continued the avoidance of these areas long after the parasite, starved of its food resources, had died out.

5. For Homo sapiens sapiens, the intermediate host of the plasmodia, the rising seas may have been merely a physical confirmation of a long existing impediment to human movement and contact from the north to the south, but for the parasite it was fatal, disrupting, in the more southerly regions of its distribution, the crucial chain of transmission. Thus the impact of the ocean barrier on mosquito populations may have been as significant as the restriction on human movements. Some of the Torres Strait islands lack the essential definitive host, Anopheles mosquitoes (Mackerras and Sandars 1954), possibly a response to the general impoverishment in the diversity of land fauna and blood meals as the environment became increasingly insular. In the Pacific, Anopheles is not found beyond Vanuatu where the diversity of terrestrial species falls off sharply.

6. One probable result of this, despite the abundance of Anopheles on the Australian mainland, was a gradual reduction in parasite numbers and extermination of the disease in most areas, particularly as favoured swamp and mangrove environments disappeared.
Here the evidence for a 'Big Swamp' phase during the period following the rise in sea-levels between about 7000 and 5000 BP (Woodroffe et al. 1988:101) is significant. If malarial pockets persisted during this period, much of the coast would have been intolerable for a significant human presence despite the many rich coastal environments which may have been developing during this time. This might correlate well with the 'delay' in the exploitation of coastal resources following the stabilisation of sea-level identified by Beaton (1985). Perhaps the presence of the parasite and accelerating swamp and mangrove formation was too much for the northern Australian (and probably Papuan South Coast) populations, as the archaeological record (or rather lack of it) testifies.

This would allow the possibility of a late (post 5000 BP?) recolonisation (or repopulation) of many parts of northern (coastal) Australia, presumably from the south and west, as the malaria menace diminished with the vanishing swamps some time after the stabilisation of sea levels. This is not without some linguistic support, for apart from most of the languages in Arnhemland and the Kimberley, Australian languages belong to a single large group, which, in terms of standard linguistic models, should be witness to a relatively recent expansion. It has always surprised me that Australian archaeologists take the linguistic evidence of one large family uniting most of the Australian languages with such equanimity in the light of the dates for the settlement of Australia. The New Guinea situation, with dozens of confused and apparently unrelated language families, is closer to expectations with such a lengthy prehistory.

That the end of the 'Big Swamp' phase ushering in this possibility of recolonisation of the northern coast from other regions coincides with other changes which took place in Australian Holocene prehistory about this time (e.g. in stone tools), is interesting, although any causal connection between these changes and the extinction of the threat of malaria in the north eludes me.

The contradiction with which this paper opened, that the great mariners who had discovered and colonised Sahuland over 40,000 years ago had apparently lost their skills (or enthusiasm for travel) by the end of the Pleistocene and did not bridge the tiny ocean gap between New Guinea and Australia, may have been a consequence of the loss of the parasite in northern Australia. The former disease gradient was now reduced to a precise and inhibitive boundary marking the presence or absence of the parasite. The gradient zone, if it existed at all, may have persisted in some of the Torres Strait Islands and Cape York. At least in later prehistory, with coastal populations on both sides of the ocean divide equipped with canoes and with annual monsoons and regular hurricanes able to blow blood-engorged female anopheles great distances, this barrier could have been and probably was, frequently breached.

This shrinking of the disease gradient between New Guinea and Australia during the later Holocene and its replacement in many parts by a sharp presence/absence boundary, had a more profound impact upon the frequency of contact between the two countries than the size of the ocean gap.

All trade, for instance, would have to be through 'intermediaries' who still lived in the shrunken gradient zone where there was some development of immunity (i.e. the Torres Strait). A brave mainland Aboriginal trader/sailor who got to Daru would probably never return; a Torres Strait Islander, however, with some native experience of the disease and additional inurement from frequent visits to coastal New Guinea would probably return, although perhaps introducing an outbreak of malaria if the vector was present on his island home. A consequence of this is that the population of the intermediary traders in the gradient zone would be small and unstable with irregular but debilitating epidemics from time to time as the parasite was reintroduced.

Obviously any trade partners on the Australian mainland where Anopheles farauti, the principal vector in Melanesia certainly existed, were also at risk, particularly if trade was conducted during the wet season at peak anopheline activity. Frequent trading, therefore, could have been disastrous for some mainland populations, as is evident from the severity and regularity of trade-port epidemics in Europe (McNeill 1976). The increase in malaria epidemics in the Torres Strait Islands and Cape York during the Colonial Period when contacts increased dramatically illustrates the vulnerability of the Australians to the northern menace.

The 'resistance' by northern Australians to the demographic advantages of Melanesian intensification strategies makes more sense with this narrowed gradient zone. If for
instance, some group did experiment with more intensive methods of food production (and there is some evidence that they were starting to do so in the Torres Strait Islands (Moore 1979:278-80) with a more sedentary life-style and denser population), the conditions for the accidental introduction of malaria across the narrow barrier would be ideal.

If such a group tolerated the epidemic following an accidental introduction and the disease became endemic, there would then be the Catch-22 of the situation for any 'traditional' Australian tribe: marriage. Wives brought in from non-malarial areas would suffer, as did those of Defoe's Essex informants, and the fecundity necessary to fund the new agricultural enterprise would be denied. The Australian marriage system promoting patrilocality and systematic exogamy, especially if it involves bringing wives in from great distances, would necessarily imperil the prospects of group survival if malaria was present.

Whatever the social system, however, population expansion close to areas with high levels of a density-dependent disease is always a risk. This was the dilemma of northern Australians living within the regions of high anopheline concentrations and close to malarial coastal New Guinea. If they were not, as perhaps the Kaurareg were, familiar with the disease, a stray blood-engorged female anopheline blown by the monsoons from coastal New Guinea or a ship-wrecked Daru Islander could provoke a serious epidemic. A similar dilemma was solved by many of the expanding cities of southern Europe during the Middle Ages by the institution of quarantine. Perhaps the resistance of northern Australians to influences from further north was similar. Quarantine enforced by a spear would ensure that our unfortunate Daru Islander would not live long enough to pass his plasmodia to the local mosquitoes.

Thus, with a small but ever-present risk of 'natural' reintroductions by the monsoons or hurricanes, any north coast Australian population which achieved a high density would be especially vulnerable. Thus a sea gap which was relatively trivial when reinforced by this pronounced and narrow disease boundary gained in effectiveness. Moreover, the very region of Australia which had the natural resources and climate to promote the development or acceptance of intensification strategies, was too close for comfort to the endemic malaria of Melanesia. This was particularly so during the last thousand years as the lethality of the parasites had been dramatically enhanced by the belated appearance of *P. falciparum*, causing a major demographic convulsion throughout Melanesia. The relatively late addition to the existing plasmodial genera of this parasite of malignant tertian malaria is relevant to a further set of contradictions which will be reviewed elsewhere (Groube in prep., a).

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The definition of Island Melanesia can be taken minimally as purely geographical: archipelagoes that stretch from north-west to south-east, adjacent to New Guinea at their northern end and far from anywhere at their southern end though part of an undersea ridge which ultimately leads to New Zealand. The archipelagoes in question are the Bismarcks (currently part of Papua New Guinea [PNG]), the Solomon Islands (the northernmost main island, Bougainville in rebellion against PNG rule, the rest forming the independent nation of Solomon Islands), Vanuatu (independent) and New Caledonia (controlled by France).

Is there any other unity to these archipelagoes? An important biogeographic boundary separates New Guinea from the Bismarcks. This marks the end of the distribution of primary division freshwater fish, some 265 bird species found on the east coast of New Guinea are reduced to 80 in New Britain, and very few terrestrial mammals crossed the same gap unassisted by humans. The flora is also considerably depauperate compared to New Guinea (van Balgooy 1971; Green, in press, a). There are two further major biogeographic boundaries within Island Melanesia, however. These are between the main Solomons chain and the Santa Cruz group to the south, and between Vanuatu and New Caledonia.

Beyond the main Solomons there are no non-volant terrestrial mammals which have not been humanly-transported, 30 genera of land birds and 162 genera of seed-plants find their eastern limits, and major disjunctions occur in the natural distribution of other fauna and flora. This has led Green (in press, a) to name this boundary as that between Near and Remote Oceania, with implications for early human dispersal across it. Even more significant in terms of seed-plant distribution is the boundary between Vanuatu and New Caledonia (van Balgooy 1971) but no similar importance in terms of possibilities for early human colonisation has yet been canvassed.

At present the boundary between Near and Remote Oceania is also the boundary between the island groups known to have been settled by humans in the Pleistocene and those not settled until the Lapita expansion at about 3200 BP. While there are reasons why this boundary may have formed an impenetrable barrier to hunter-gatherer settlement (Spriggs in press, a), an alternative explanation is simply that not enough archaeological work has been done in Vanuatu and New Caledonia to locate early sites. In addition, if it were established that pre-Lapita populations in the Bismarcks and Solomons were agricultural, then some of the earlier constraints to settlement of the nearer parts of Remote Oceania may have been relaxed by the early to mid Holocene. Some level of pre-Lapita cultural unity for Island Melanesia, therefore, remains a possibility. Human settlement at this time across the much larger water gap to Fiji seems much more unlikely (but see Southern 1986).

A Lapita unity is more firmly established for Island Melanesia. With the exception of sherds of a single Lapita vessel from Aitape on New Guinea, Lapita sites in the region are limited to Island Melanesia and Fiji – West Polynesia. An Early Western style of Lapita pottery (previously called Far Western by Anson (1983, 1986)) appears to be limited to the Bismarcks, the suggested Lapita Homeland. The slightly later Western Lapita style (Green 1990) encompasses the entire study area and can be distinguished from the Eastern Lapita style of Fiji – West Polynesia.

Whether this apparent Lapita unity is continued post-Lapita is open to debate, depending on more detailed analysis of the incised and relief pottery styles found from Manus to New Caledonia which superficially seem to be related. These styles are found outside the study area as well, in Fiji to the east and in parts of coastal New Guinea to the west.

The lower boundary in time which I have been set is most certainly an arbitrary one, the conventional boundary of the end of the Pleistocene. While it may form a meaningful climatic divide at high altitude or in more temperate latitudes, at close to sea level in the tropics...
10,000 BP is a non-event. Sea level post-17,000 BP continued to rise steeply until at least 8000 BP, levelling off around 6000 BP. That might be a good time to divide up the prehistoric sequence because for coastal areas from that date (possible mid-Holocene high stands notwithstanding) there is less reason to believe that evidence of coastal occupation has disappeared beneath the waves. From 6000 BP too we can be certain that island sizes were not orders of magnitude above the present. Evidence for agriculture at Kuk in the New Guinea Highlands (Golson 1989) at 9000 BP, consonant with the earliest agricultural dates from around the world, might suggest 10,000 BP as a useful break. Whether this date has any relevance for Island Melanesia, however, is yet to be established and should certainly not be assumed.

It would be possible to present a narrative history of the last 10,000 years and such might be 'read' from the discussion below. But the main effect of the explosion of archaeological research in Island Melanesia in the last ten years has been to show how little we know. In addition, because archaeology in the region is still very much in a pioneer phase, we are data-led rather than theory-led at present. Not that theories don't abound, but the pace of empirical research is such that they are usually wholly or partially obsolete by publication date or are simply unpersuasive given a total lack of evidence bearing on the issues they address.

One important question already alluded to is whether Vanuatu and New Caledonia were settled pre-Lapita. Not only does this affect the question of the unity of our area in having a common pre-Lapita as well as Lapita history, but it has important implications as to the degree of constraint imposed on human settlement in relation to the above-mentioned biogeographical boundaries, boundaries representing increasingly depauperate natural fauna and flora.

The purpose of this paper is to demonstrate how little we know of the area's prehistory by identifying important unresolved questions and examining some of the implications which would flow from particular answers to them. A rough temporal framework is given to these questions by a series of divisions between pre-Lapita (10,000 BP to 3600/3200 BP), Lapita (3600/3200 BP to 2500/2000 BP), post-Lapita (2500/2000 BP to 750 BP), and late Prehistory (750 BP to sustained European contact at about 150/100 BP).

**PRE-LAPITA**

As Jim Allen (this volume) has discussed, the Pleistocene settlement of Island Melanesia was undertaken by hunter-gatherers. What can we say about them at our arbitrary starting date of 10,000 BP (±1000 years)? There are five sites of relevance: three on New Ireland, one on Manus and one on Buka, most of which have not yet been published in detail. All of them provide information on the fauna being hunted. On Buka there was a concentration on the rich endemic rat fauna, including the noble Solomys spriggsarum (Flannery and Wickler 1990) as well as bats, reptiles, birds and fish. At Balof 2, Panakiwuk and Matenkupkum Phalanger orientalis, introduced during the Pleistocene, was prominent and again bats, reptiles, birds, and fish occur (Allen et al. 1989; Marshall and Allen 1991). Rattus mordax and, at least at Panakiwuk, the introduced Rattus praetor are also found but the species range and dietary importance of rats in the New Ireland sites do not approach that at the Kilu site on Buka with its five endemic species. The Pamwak fauna from Manus has not yet been examined in detail but prominent in it is the introduced bandicoot (Echymipera cf. kalubu) as well as rats, bats, reptiles and fish. At all sites except Panakiwuk, marine and/or mangrove shellfish are present.

The plant component of the diet is much less certain but the nut tree Canarium was important on Buka and in Manus, identified from macroscopic charcoal remains[1]. The work of Douglas Yen (1990:262, 268) strongly suggests that the species in question originated on the island of New Guinea and were humanly-transported and planted in the Bismarcks and Solomons. The evidence is that this occurred in the terminal Pleistocene, and it is possible that Canarium was an important storable food resource at this time. It was a major food source in some areas of the Bismarcks and Solomons in ethnographic times (see Spriggs 1990a for details).

Recent advances in residue analysis (see Loy, this volume) can be expected to increase our knowledge of the plant component of hunter-gatherer diet in the region. Study of the Kilu stone tool assemblage has suggested that many of the flakes were used for cutting and scraping Colocasia taro prior to cooking (Loy et al. n.d.). Taro and other wild roots would have presumably formed the vegetable basis of subsistence prior to the development or adoption of

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[1] A single fragment of a possible 'wild form' of Canarium has also been identified from Panakiwuk on New Ireland in early Holocene levels (Marshail and Allen 1991:88).
agriculture.

It has recently been suggested (Bailey et al. 1989) that hunter-gatherers cannot live in tropical rain forests in the absence of agriculturalists with whom to trade. It may indeed be hard to subsist in a full-on closed canopy forest but Bailey et al.'s definition seems far too restrictive as it excludes the presence of rivers, swamps, and lakes, proximity to the coast or to any non-closed canopy rainforest vegetation type. I doubt that such a landscape exists anywhere in Melanesia. As well as exploring microhabitats within the rainforest such as swamps and river margins where stands of wild taro might be expected, the evidence for the introduction of animals such as phalangers and bandicoots and of useful tree species such as Canarium suggest enhancement of the natural (harnessable) productivity of the rainforest. Selective clearing to encourage growth of useful plants is also a possibility on evidence from Balof (Allen et al. 1989:558) which future pollen studies might further address.

Did this forest 'management', wild food production in Harris' (1989) scheme rather than the simpler wild food procurement, develop into agriculture as appears to have happened at the beginning of the Holocene in parts of New Guinea? Some have raised the possibility that it did (Allen et al. 1989:558) based on the Kuk evidence, a common pattern of early-mid Holocene abandonment of rockshelter sites, the Balof evidence for short-term local forest clearance, and the single piece of Canarium at Panakiwuk at about 8000 BP. The Canarium and evidence of short-term clearance might be better accommodated as part of a model of non-agricultural forest management extending back into the Pleistocene. Agriculture in the New Guinea Highlands at 9000 BP need not imply its universal early spread over occupied Melanesia. The abandonment of certain rock shelter sites in this period too should not be considered apart from their total history of use, abandonment and reuse.

It is indeed the case that Matenkupum shows no signs of significant use after 10,000 BP although this is probably in part due to later human or natural disturbance. Panakiwuk is unoccupied between about 8000 BP and 1600 BP, Balof 2 may be abandoned between 7700 BP and 3500 BP, Matenbek between 6000 BP and the present apart from very ephemeral use, Pamwak between 5000 BP and about 1800 BP, and Kilu and Palandraku perhaps from 5000 BP to about 2500 BP (Allen et al. 1989; Wickler 1990). Balof 1 and Lolmo in the Arawe Islands off New Britain may give us some insight into the immediately pre-Lapita and Lapita periods but a continuous sequence even at these sites is by no means certain (Downie and White 1978; Gosden et al. 1989). The suggestion is presumably that a shift to agriculture meant less reliance on hunting and gathering in the forest as well as a shift to village settlement, hence abandonment of the rockshelters. What are not discussed in this context however are earlier periods of apparent abandonment of most of these sites during the Pleistocene: Matenkupum probably during some of the period between 21,000 BP and 14,000 BP (Gosden and Robertson 1991), Matenbek from 18,000 BP to 8000 BP (Allen et al. 1989), and Kilu between 20,000 BP and 9000 BP (Wickler and Spriggs 1988). The Panakiwuk report suggests that prior to 10,000 BP occupation was sparse and perhaps episodically non-existent (Marshall and Allen 1991:89). The lack of dated contexts between 13,000 BP and 10,000 BP raises the possibility of complete abandonment during this time period.

The causes which might be invoked for these earlier hiatuses include sea level depression effects of the glacial maximum around 17,000 BP which cut off easy access to marine resources from near-coastal sites (for Kilu at least: Wickler and Spriggs 1988), re-alignment of behaviour in the region leading to the omitting of particular sites from an otherwise continuing hunting and gathering strategy (paraphrasing Marshall and Allen 1991:90), and changes in cave micro-climate making particular sites less attractive for settlement. Local causes for each particular site need to be considered given the range of dates of abandonment for the Holocene as well as the Pleistocene hiatuses. If Holocene abandonment is related to the adoption of agriculture, one also has to ask what change led to re-occupation at various time between 3500 BP and 1600 BP?

If there is no convincing evidence of agriculture in this pre-Lapita period, is there evidence for an intensification of hunting and gathering, often linked to the development of agriculture as a 'Mesolithic Prelude' in Clark's (1980) term? The evidence is at best equivocal. Pamwak does seem to show a sequence of increased rates of shell midden and bone accumulation, and a wider range of formal artefact types including shell and edge-ground stone axes at about 10,000 BP. This can be interpreted, however, in terms of changing site function to do with easier access to marine and mangrove resources as sea level rose and the coast moved progressively closer to the site. A simple model of subsistence intensification over
time would be most misleading in this case if site function changed from occasional hunting camp in the forest to near-coastal base camp where a wider range of activities would be carried out during more regular occupation.

Gosden and Robertson (1991), although discussing the Pleistocene use of Matenkupkum, provide a general discussion of the problems involved in interpreting changes in shell size, species and quantities in midden sites. The range of these problems casts doubt on any simple interpretation of size-reduction as representing more intensive exploitation of shell beds. Such phenomena can have a variety of other natural (as well as cultural) explanations, particularly during periods of changing sea level.

The most sustained occupation at Panakiwuk occurred between 10,000 BP and 8000 BP, again possibly suggesting some form of intensification. But Marshall and Allen's subtle treatment of the evidence canvasses a number of possible explanations. Phalanger orientalis, although apparently present prior to 10,000 BP, only occurs in any quantity in the site from that date and, could only have served to increase the range of available prey in rainforest within the vicinity of Panakiwuk and perhaps provided greater opportunity for movement to, and more permanent occupation of sites, such as this one, away from the immediate coastline (1991:88-9).

The densest concentrations of all classes of artefactual material occur in these levels but whether this means more people occupying the site or the site being utilised more frequently is not clear. Marshall and Allen also note the possibility that the small excavated area of the site might have been used differently at different periods. The apparent incorporation of the site into an 'integrated system of coast and inland activities' (1991:90) at this time may be enough to explain these changes without necessarily representing a general subsistence intensification, particularly if sea level rise put coastal resources more within the range of people using Panakiwuk.

Balof 2 likewise shows changes after 10,000 BP and again the Phalanger is implicated. White et al. (1991) suggest increased sedentism linked to prey change to Phalanger. This assured hunters of a continuous catch over the medium term because of the animal's predictable habits. Disappearance of fine-grained (probably exotic) rock is linked not very convincingly to a localisation of people's horizons, perhaps consequent on the development of more intensive plant curation techniques (1991:57), for which no evidence is presented. Elsewhere in the text it is reported that obsidian (certainly exotic) replaces the fine-grained rock. This is said to be explicable by any combination of loss of previous stone sources through rising sea level, superiority of obsidian as a raw material, change in exchange networks, or for aesthetic reasons. Again, these changes need not imply intensification. Prior to the changes associated with the introduction of pottery at Balof 1 there is no evidence for subsistence change or intensification at that site (Downie and White 1978).

At Kilu on Buka there appears to be little difference between the Pleistocene and Holocene levels. Stone flakes are concentrated in Pleistocene deposits, the largest weights of bone occur near the base of the Holocene levels, and peaks for marine shell weight occur in both phases of occupation (Wickler and Spriggs 1988:704). Again, a Holocene intensification argument seems difficult to sustain. It is noteworthy that obsidian, present in some New Ireland sites, does not occur in any pre-Lapita deposits on Buka and Phalanger orientalis is represented pre-Lapita by a single bone at the base of the Holocene deposits at Kilu, suggesting that no breeding population was present in the Solomons at the time (Wickler 1990). Canarium is found only in Holocene levels but the date of its introduction is unclear given the 11,000 year hiatus in occupation below these levels and the absence of any macroscopic charcoal in the Pleistocene deposits.

The pre-Lapita evidence for subsistence has been examined here in some detail to determine whether there is evidence for Holocene development of agriculture or other subsistence changes which might represent an intensified form of hunting and gathering. There is no firm evidence for the former and for the latter it is at best equivocal. All in all a pattern of subsistence continuity from at least 10,000 BP onwards is attested for the pre-Lapita period, making the changes that occurred after that time all the more dramatic.

**LAPITA CONTINUITIES?**

An indigenist view of the development of Lapita stresses the continuities from pre-Lapita to Lapita and downplays the differences (Allen and White 1989; White et al. 1988). One of the models that the 1985 Lapita Homeland Project set out to test was the idea that:
Bismarcks cultures older than those making Lapita were developing in directions favourable to the emergence of Lapita cultures, including, in particular, a familiarity with and common use of the sea and its resources (White et al. 1988:412-3).

Such familiarity is particularly proved to the indigenists by sharks' teeth in levels at Balof 2 back to 7000 BP, 'familiarity with the sea and its denizens thousands of years before the settlement of more eastern islands' (ibid.), and by transport of New Britain obsidian to New Ireland, evidence which (in 1988) suggested first use of 'remote' islands such as Nissan and Manus about 1000 years pre-Lapita. The 'almost total absence' of Lapita in the vicinity of the Lou Island obsidian sources suggested that 'the obsidian miners were already established when the Lapita potters arrived, and were not interested in acquiring or making similar pots' (White et al. 1988:414). A strawperson idea that Lapita sites in the Bismarcks would represent short period transit camps en route to Polynesia if a migration hypothesis were to be sustained was dismissed by the indigenists because of the presence of Lapita sites occupied for periods of 1000 years or more in the area. While pottery is admitted as being introduced from the east, it is claimed that the question of the settlement of the Pacific has now 'been largely solved':

> It is now clear that the basic developments that lay behind the Lapita cultures occurred within the Bismarck Archipelago... There is, indeed, no need to believe in migrations at all: pottery technology may just as well have been acquired by Bismarck inhabitants in the course of their voyaging in the western Pacific 4,000 years ago (White et al. 1988:416; cf. Allen and White 1989:142).

It has already been noted that several of the cave sites important for our knowledge of the early Holocene go out of use before the Lapita period, so which sites in the 1000 years or so prior to Lapita might provide evidence of continuity? A series of sites near Talasea on New Britain dating to prior to the eruption of Mount Witori about 3500 BP are currently under investigation (Torrance et al. 1990). On the south coast of New Britain in the Kandrian area three sites appear to have deposits contemporary with or earlier than the earliest Lapita sites – Misisil (c.3700 BP), Yambon (c.4850 BP) and Alanglong (c.3900 BP). Very few details of these sites have yet been published (Specht et al. 1981, 1983; but see Brown 1988 for Yambon). In the Arawe Islands further to the west along the New Britain coast the lower unit of Lolmo Cave has produced dates of 6100-5250 BP and a flexed inhumation from the beach at Paligmete has given a date of 3950 BP (Gosden et al. 1989:565, 568, 583). In New Ireland only Balof 1 appears to contain deposits from the relevant period, but this may be an artefact of there only being two dates from the site, one from near the bottom and one near the top. In Manus two sites, Peli Louson (c.4850 BP) and Father's Water (4850 BP, 4500 BP) have produced dates of the right period (Kennedy 1983) but very few details are yet available and the dating samples come from stratigraphic depth ranges of 45 cm in the first case and 60 and 80 cm in the second and so can be used as giving only the most general indication of the period of use of these sites. Moving south of New Ireland, Lebang Takoroi on Nissan has produced dates for aceramic levels of 6150 BP to 3750 BP, Lebang Tatala has aceramic levels dating between 4600 BP and 3650 BP, and another aceramic site, Lebang Halika, has produced dates contemporary with the earliest Bismarcks Lapita sites starting at 3650 BP (Spriggs 1991). In Buka, the site of Palandraku has an aceramic deposit dating to about 5000 BP (Wickler 1990). Some of the Guadalcanal sites investigated by Roe have also produced pre-Lapita dates back to 6000 BP (David Roe, pers. comm.).

Many of these sites are still being investigated and so in general only preliminary reports are available. What do they tell us so far that will allow us to examine claims of continuity? Talasea area obsidian was clearly being exchanged widely, being found away from its source on the south coast of New Britain, in New Ireland and on Nissan. There is as yet no evidence for its use pre-Lapita in the Solomon Islands, nor of the presence of Admiralties obsidian outside of the Admiralties group. At Talasea an industry of stemmed obsidian tools is found in this pre-Lapita period but not in subsequent Lapita levels. Bifacially flaked waisted chert tools are known as surface finds from the Yambon area, and the assemblage excavated there fits within the same technological tradition. On present evidence there is no great difference in stone technology between Layer 6 (4850 BP) and Layer 4 (2750 BP) at Yambon. Pottery first appears in layers above this later date. Disturbance at the top of the lower cultural layer suggested that it may have been a garden layer (Brown 1988:16). At Talasea there is often continuity of site location from pre-Lapita to Lapita times, but this is not the case at other Lapita open sites with the exception of the pre-Lapita Paligmete burial in the Arawe Islands which underlies a Lapita site. Some continuity in the working of shell is
apparent across the pre-Lapita/Lapita divide (Spriggs in press, b) with *Tridacna* shell adzes, *Trochus* armrings, shell beads, and probably *Trochus* one-piece fishhooks, as well as a range of ground or flaked shell pieces found in both types of sites. Bone points occur in pre-Lapita levels at Balof 1 (Downie and White 1978) and are known also from Lapita sites.

The Nissan site of Halika contains some deposits contemporary with early Lapita but which lack pots. It produced a range of shell artefacts including pearlshell knives or scrapers which are typical of Lapita, contains a small amount of pig bone and a wide range of fruit and nut tree species, presumably domesticated. On this evidence I have interpreted this site as 'Lapita without pots'. The assemblage is quite different from that in the earlier Takoroi site which has no shell artefacts, no pig, no plant remains except *Cocos* and contains significant quantities of Talasea obsidian. There is only a single piece of obsidian in early Tatalene and none in Halika deposits. *Canarium indicum* and *Cocos* were identified with confidence in Lebang Tatalene aceramic deposits, as well as very tentative identifications of some other plant species. (See Spriggs 1991, in press, b for further discussion).

Given the weak evidence for continuity from the sites of relevant age, it is surprising to read statements like the following:

In relation to the traits listed by Roger Green (1979) as necessary for Pacific colonisation, shell and bone technology, watercraft and voyaging, probably domestic plants and perhaps domestic animals were all available to people in this area well before Lapita pottery was made (Allen and White 1989:14).

Many applications of Lapita shell technology were in fact innovations, bone technology before and during Lapita was very little developed, there is no evidence of voyaging range extension after the Pleistocene until Lapita and no evidence of regular voyaging to and from Manus, or south of Nissan during the pre-Lapita period. As we have seen, the only firm claim for a domestic plant appears to be *Canarium*, again already being moved around in the Pleistocene, and there is no evidence of pre-Lapita domestic animals. Similar methods of marine exploitation in pre-Lapita and Lapita speak more of common sense in nearshore fishing and gathering practices than culturally significant continuity. It may indeed be the case that the question of the settlement of the Pacific now has 'been largely solved' as argued by White et al. (1988:416) but they appear more in the role of Inspector Lastrade than Sherlock Holmes in this case! That said, however, the criticism that there are not enough immediately pre-Lapita open habitation sites available for comparison is unavoidable. Yambon and the Talasea sites contain no organic remains such as shell and bone for comparison, and few details are available concerning Father's Water in Manus which contains both, at least in the upper part of its aceramic sequence. This contrasts with the over 70 open Lapita sites in Island Melanesia known from the following period, several of them large village complexes.

**LAPITA DISCONTINUITIES**

If we can lay out what is on current evidence new about the Lapita period, then the evidence needed to counter a view of major cultural discontinuity is made plain. This will suggest what to look for and, perhaps, where to look for it. It is therefore worth considering what is new in Lapita:

1. The most visible attribute is the pottery, its very occurrence and its distinctive decorative system using dentate stamping, incising and some other minor techniques.

2. Lapita represents the first convincing evidence for agriculture in the region, indicated by macrofossils of a range of domesticated plants (Gosden et al. 1989; Kirch 1989), evidence for accelerated erosion consistent with gardening on adjacent hillslopes (Gosden et al. 1989:573; Hope and Spriggs 1982), the extension of settlement to areas where a non-agricultural subsistence base would be unlikely or impossible i.e. Remote Oceania (Polynesia in particular), and the very size and nature of Lapita settlements as large settled villages.

3. It marks the first appearance of the three Pacific domesticates: pig, dog and chicken, and therefore the beginnings of Pacific animal husbandry.

4. The already mentioned settlement pattern is itself new: large villages often consisting of stilts houses over lagoons or on small offshore islands. Lapita sites do not generally re-occupy previously used locations apart from where Lapita deposits occur in rockshelters. The settlement pattern suggests a defensive posture or avoidance of mainland situations where malaria could have been rife.

5. There is a distinctive Lapita stone adze kit not paralleled in previous assemblages in the area (Green in press, b). In addition, although edge-ground adzes occur pre-Lapita at Pamwak on Manus for instance, there are no examples of fully-ground stone adzes from earlier periods in Island Melanesia.
6. There is a distinctive range of shell ornaments, interpreted by Kirch (1988a:107) as shell valuables, again not paralleled by earlier forms. These include Conus shell rectangular units, beads, rings and disks, Tridacna rings and Spondylus beads and long units (Kirch 1988a:Fig. 3).

7. Lapita represents a major extension in the range of New Britain and Admiralties obsidian. Pre-Lapita, Admiralties obsidian is not found outside that island group. It is found in Lapita sites throughout the Bismarcks, Solomons and into Vanuatu. New Britain obsidian was distributed pre-Lapita to the west on the New Guinea mainland as far as the Sepik-Ramu basin (Swadling et al. 1988:19-20), and to the south and east in New Ireland and on Nissan. In Lapita times its spread encompassed Sabah in Borneo to the west (Bellwood and Koon 1989) and Fiji in the east (Best 1987), a spread of some 7,000 km.

8. South and east of the main Solomons Lapita appears to be the founding culture, representing initial human colonisation of Remote Oceania.

This list should help to deny the charge that Lapita is just pots, made by supporters of a continuity or indigenist model. A more sophisticated version of this argument is now being put forward that Lapita is just trade, trade in pots, shell valuables, obsidian and other stone (Terrell 1989). While there was indeed trade in all these items to varying extents it is their consistent co-occurrence in sites over a wide area which defines both an exchange network and a cultural complex.

In Remote Oceania Lapita represents initial colonisation by human groups, equipped with a fully agricultural economy. In the Bismarcks and Solomons it represents an intrusive culture, though one not isolated from the already existing cultures of the area. There is some cross-over of artefact types from pre-Lapita to Lapita assemblages, or at least a partially shared artefact inventory, and exploitation of the major obsidian sources of the region continued.

The ancestral links of Lapita are clearly much further to the west in Island Southeast Asia. The domestic animals and the commensal Polynesian rat (Rattus exulans) are all of Southeast Asian origin. Pottery and several other aspects of material culture are also earlier in Southeast Asia and a time cline in their distribution from Taiwan to the Philippines, Eastern Indonesia and thence to the Bismarcks has been recognised (Spriggs 1989). Pottery vessel forms and the use of red-slip link Lapita pottery to the west, but the Lapita design system and its application by dentate-stamping cannot be shown to be earlier in Southeast Asia (Spriggs 1989:607). It may be an innovation in the Bismarcks or at least a transfer to pottery of a design system previously used in other media such as barkcloth and tattoo (Green 1979a; Green 1985).

THE AGRICULTURE QUESTION

Island Southeast Asian neolithic cultures were at least initially rice-using and yet rice was not transferred across to Melanesia. The question then arises of the origins of the Oceanic agricultural complex spread by the Lapita culture to previously unoccupied parts of the Pacific. Traditionally the crops were seen as of Asian or Southeast Asian origin (Yen 1973) but more recently Yen (1982, 1985) has challenged this derivation, suggesting many as New Guinea domesticates and pointing to the evidence for early agriculture in the New Guinea Highlands as support for an independent centre of plant domestication in Melanesia:

A suite of plants was domesticated that included basic staples, vegetable and fruit species that were able to sustain human populations in their settlement of diverse and foreign ecologies from beginnings of hunting and gathering, and was a continuing process. However, this continuity was interrupted by further colonists out of Asia this time with agriculture and transferred domesticates, which were to dominate, in many cases, the earlier evolved cultivation of indigenous domesticates: these to become secondary, in Barrat's Phrase (1965) 'witnesses of the past'. (Yen 1982:291).

Basic questions raised by this are: how far in the islands to east and west of the mainland of New Guinea did the process of pre-Lapita domestication extend, how much further had the domesticates themselves spread, and could some of the plants in question have been domesticated independently in areas of Southeast Asia and New Guinea? Evidence for pre-Lapita agriculture east of New Guinea is lacking and the archaeology of Maluku, immediately to the west of New Guinea is virtually unknown (Spriggs 1990b). Linguistic reconstructions of Proto Malayo-Polynesian terms for nearly all of the Oceanic domesticated plants (Pawley and Green 1984:130) would suggest that they were known over much of Island Southeast Asia at an early stage of the neolithic in that area, prior to Lapita appearing in the Bismarcks. Hypotheses either of early westward spread from a New Guinea centre and/or independent domestication in Southeast Asia could equally be sustained for plants such as
taro and breadfruit. Most of the edible yams, Eumusa-section bananas, and at least some clones of domestic taro are more certainly of Southeast Asian origin (Yen 1982; Yen and Wheeler 1968).

To the extent (as I have indicated, unknown) that the Lapita crop complex was picked up in the New Guinea region, there is a wide area in which this transfer could have occurred — from Eastern Indonesia and along the north coast of New Guinea to the Bismarcks. There is no reason in the absence of direct pre-Lapita plant evidence to give primacy to the Bismarcks for this, a point overlooked by the indigenists.

If Lapita does represent in its initial stages the movement into the area of pottery-using agriculturalists these groups must initially have been small, gaining numbers over time by natural increase and recruitment from neighbouring groups. All Island Melanesian societies were agricultural by European contact. Agriculture, therefore, would have spread out from Lapita centres to non-Lapita groups across a 'frontier' by varying processes and at varying rates. Adoption of agriculture by hunter-gatherer groups could have allowed much increased population growth, perhaps in competition with Lapita populations. Such groups might themselves have adopted Lapita culture and produced Lapita sites, preventing any easy 'ethnic' classification for later Lapita settlements.

The spread of agriculture and the relations between Lapita and non-Lapita populations are major topics in Island Melanesia which are as yet barely defined and where the deficiencies of our data base are glaringly obvious. The comparative European material may be of relevance in modelling agricultural spread and the nature of relations across the frontier (Dennell 1983; Renfrew 1987; Sherratt and Sherratt 1988; Zvelebil 1986a; Zvelebil and Zvelebil 1988). For instance, Zvelebil has contrasted a colonization model and an indigenous adoption model for agricultural spread in Europe. The initial Lapita spread fits the colonisation model where,

...there is no evidence for the availability phase, where the introduction of farming was rapid, and where it was introduced as a complete set of techniques and domesticates, as a package. Furthermore, it best fits those areas where farming rapidly replaced foraging (short substitution phase), where there is a break in settlement between the mesolithic and neolithic sites, where the material culture of farming contrasts with that of the preceding hunter-gatherers and where it can be traced to a parent farming culture, and finally where there are no symbolic or ritual traces of ideology connected with hunting and gathering (1986b:182–3).

The contrast is with areas where 'the role of indigenous populations appears to have been predominate', which in our region would appear to correspond to all areas not immediately coastal in the Bismarcks, and in the Solomons perhaps whole islands such as Bougainville (but not Buka) and Guadalcanal. The archaeological pattern would include,

...selective and gradual, rather than complete and rapid, occurrence of farming; long availability phase and an extended transition from one form of economy to the other (long substitution phase); continuity in settlement; similarities in material culture between the neolithic and mesolithic communities; and retention of at least some aspects of hunter-gatherer symbolism and ritual into the Early Neolithic (Zvelebil 1986b:183-4).

That this neat set of contrasts cannot always be maintained, however, is admitted by Zvelebil in a subsequent passage where he notes cases of 'small-scale population shifts associated with secondary colonisation of less favourable environments, or situations with a mixed set of attributes, indicating neither of the two processes contrasted here' (1986b:184).

There are a few candidates for sites contemporary with and relatively near to known Lapita sites which are not obviously Lapita in affiliation: Yambon, Misilisil, Alanglong perhaps, Balof 1 and 2, and some Guadalcanal sites. The upper part of Yambon has produced a sequence starting at 2750 BP, with pottery first appearing in the site sometime later. The stone-working tradition, however, appears to represent continuity from the 4850 BP occupation, as of course does its ridgetop location. In the same inland area, reoccupation at Misilisil occurs around 3700 BP and in a subsequent level is a fragment of a polished adze, perhaps suggesting a transfer of one element of the Lapita complex. Alanglong too presents pottery in its upper layers but not enough information is yet available to suggest a cultural affiliation for this coastal site (Brown 1988; Specht et al. 1981, 1983).

When pottery appears in the Balof sequences, other significant changes occur at about the same time: the first Admiralties obsidian, the first pig and Rattus exulans, and ground stone adze fragments (Downie and White 1978). These represent significant parts of the Lapita package and it may be that the Balof shelters are part of a Lapita settlement system at this time, rather than merely occasional recipients of Lapita tidbits.

Detailed information on the Guadalcanal sites excavated by David Roe is not yet available.
As more sites in inland parts of islands and away from known areas of Lapita settlement are investigated then the moving agricultural "frontier" of the time will become visible.

ORGANISATIONAL PROBLEMS

The nature of Lapita sociopolitical organisation has been speculated upon at some length on the basis of linguistic clues, ethnographic analogy, general principle and common sense (Bellwood 1990; Friedman 1981; Hayden 1983; Lilley 1985; Pawley 1982). Largely absent has been any appeal to archaeological evidence, and for good reason. Very rarely have more than test pits been put into Lapita sites and in only one case, the RF-2 site in the Reef-Santa Cruz Group in the Solomons, are major results of area excavations available (Sheppard and Green 1991). This site has the largest contiguous area excavation in Island Melanesia (153.5 m²), and clear spatial differentiation in the distribution of particular artefacts and food remains can be demonstrated. Part of a large (7 x 10 m) structure was exposed with evidence of smaller structures around it. The main structure was used for food consumption and some food preparation, most food preparation occurring south of it. The central structure was also the focus for much of the non-food preparation activity at the site. Whether it was a living house, a men's house or a community structure is not clear. On the RF-2 evidence one can argue for spatial separation of activities but not for status differences within the community.

It is difficult to extrapolate from the RF-2 site except to state with confidence that large-scale excavation of other Lapita sites is the only way to get a handle on questions of village layout and organisation. RF-2 is not a 'typical' Lapita site. Its total area is about 1100 m², much below the common Lapita open site size range of 9500 to 13,000 m² and it is 'representative' of only a hamlet-sized level of settlement (Sheppard and Green 1991:100).

Other contiguous area excavations are all below 50 m²; although there are some sites such as Talepakemalai (Eloua) where a contiguous area excavation (here 24 m²) has been extended by test pit transects in various directions, giving further distributional information. The area excavation (Area B) revealed a still-house structure with a dense associated midden including a large quantity of dentate-stamped pottery. In contrast, an area (Area A) behind the former shoreline and claimed as contemporary by the excavator consisted almost exclusively of plain sherds (Kirch 1988a). Clearly, more evidence for the function of decorated as opposed to plain vessels is needed, as well as for the function of the still-house structure itself. In this and other known Lapita sites, the evidence is there to allow us to address questions of settlement and social organisation, but we need to dig a lot more of it up by area excavation.

Kirch's (1988b) interpretation of Lapita shell ornaments functioning as exchange valuables, based on an argument of continuity of function through to ethnographic times, has already been mentioned. The other artefact type with a suggested link to social status is of course the dentate-stamped pottery itself and suggestions as to how this pottery can be investigated to examine such a postulated link have recently been made (Spriggs 1990c). Burial goods are often used in evaluating status differentiation in archaeological contexts, though the relation between grave goods and status is never a simple one (Pearson 1984). Very few Lapita burials have been found, however, and the Lapita cemetery at Watom belongs to the latest phase of Lapita occupation when recognisably Lapita settlements had vanished from most of the Island Melanesian region. In addition, the sample is small (eight burials) and there are virtually no artefacts associated with the skeletons (Green et al. 1989).

That Lapita society was hierarchically structured seems a reasonable supposition, given the demands of voyaging and colonisation of the far-flung islands of Remote Oceania, but the details of its structure cannot at this stage be established on archaeological grounds (Kirch 1988a). We are even further away from understanding the transformations of that society over time, within the Lapita period and between Lapita and the succeeding post-Lapita period. What effects Lapita settlements had on adjacent cultures directly or indirectly through the availability of new technologies and subsistence possibilities, new exchange relations and so on, cannot even be contemplated with the current data base.

POST-LAPITA CONTINUITIES

In 1984 I argued for a basic continuity between Lapita and later cultures in Island Melanesia on the basis of a statistically indefensible but still informative Table showing continuity in artefacts such as various shell adze and ornament forms, fishhooks and stone adzes,
Spriggs

and a consideration of what I called transitional sites (Spriggs 1984). These transitional sites displayed a basic continuity in pottery but without the classic Lapita dentate stamping. This had dropped out to be replaced by decorative techniques such as incision and appliqué which were present but rare in Lapita assemblages and to become the major types of pottery decoration in later styles. In the 1984 paper I also looked at certain assemblages usually suggested as contemporary with but distinct from Lapita. Two of these remain pertinent here: Mangaasi in Vanuatu and the Podtanean or Paddle-impressed assemblages of New Caledonia. I argued that these were either facies of Lapita culture (Podtanean) or were themselves 'successors' in the sense of dropping out early from the Lapita network but deriving from it (Mangaasi).

Recently, Galipaud (1990) has demonstrated that in terms of fabric the Podtanean style, with dates contemporary with Lapita, is identical to local Lapita pottery. It would appear to have been made by the same potters, perhaps as the domestic ware of Lapita in New Caledonia. Galipaud notes that concentrations of one or the other style occur generally in adjacent sites which may turn out in fact to be different parts of single site complexes. Early plain pottery assemblages which occur near classic Lapita sites, and are contemporary with them, such as the Nôvlâo rockshelter on Santa Cruz (McCoy and Cleghorn 1988), may also be explained in similar fashion.

The detailed pottery sequences which are starting to appear from Bismarck Archipelago sites show that as dentate stamping declines, other more 'Mangaasi-like' decorative styles come into prominence: incision, appliqué and fingernail impression. Variety in vessel shape decreases over time within Lapita as well, often leaving only simple globular cooking pot forms which become the common post-Lapita vessel form.

These changes occurred earliest in the central Vanuatu Mangaasi sites which start around 2750-2700 BP. Other assemblages with strong resonances of Mangaasi, some still with a recognisable Lapita element as well, occur in Central Vanuatu, Erromango (Southern Vanuatu) and the Banks Islands north of Vanuatu by 2350 BP. Further north again, Vanuatu Mangaasi pottery is imported to Tikopia by 1950 BP. A similar style change in pottery occurs on Buka at the northern end of the Solomons by 1900 BP, sometime between 2700 and 2000 BP on New Ireland, before 2000 BP on Watom, at the late end of the Arawe Islands Lapita sequence in New Britain and in the Manus group by 2100 BP. Mangaasi-related pottery, the Oundjo style, starts in New Caledonia by 1700-1650 BP although some of its decorative elements occur in earlier Podtanean assemblages, as might be expected. Discussion of and references for these dates can be found in Spriggs 1990d.

The continuity argument was in reaction to earlier views of a major cultural break between Lapita and post-Lapita, sometimes seen as representing migration of a new group of people through the area. Localised population movements may explain the changes in styles in some areas, and the linguistic evidence (Ross 1989) might be seen to support this in the Bismarcks – Northern Solomons area. In general, however, a somewhat attenuated chain of connection (Spriggs 1984:217-8) which links communities from Manus to New Caledonia in the period 2500-2000 BP could have been enough to allow some form of continued communication throughout the region keeping changes in pottery style loosely 'in sync', without major migrations being necessary to explain them.

The end of Lapita does mark an important contraction in exchange systems. Obsidian, for instance, never again travels so far, and from about 2000 BP the processes of diversification seem stronger than those maintaining a degree of cultural unity. The ethnographic diversity of Island Melanesia may well turn out to be a product of the immediate post-Lapita era, although it must be remembered that Lapita itself was never a universal culture in the Bismarcks and Solomons. In some areas pottery ceased being made or traded in after 2000 BP, and everywhere the number of pottery making centres declined.

Important realignments of exchange networks occur in the post-Lapita period. In the Vitiaz Strait between New Britain and the New Guinea mainland, a 'protosystem' of the ethnographically recorded exchange system developed about 1500 BP (Lilley 1988). The post-Lapita trade links of the Mussau Islands were also quite different than those before (Kirch 1990). At about 900 BP pottery exchange to Nissan switches from a northern supplier (possibly on New Ireland) to a southern source (Buka) and from that time on the ethnographic patterns of settlement and material culture can be recognised on the Island (Spriggs 1991). Pottery making on New Ireland and/or adjacent islands off its east coast appears to have ceased at this time. The exchange connections of Tikopia undergo an important switch at about 1950 BP (Kirch 1986) from materials sourced to the north-west.
northern end of the Solomons (Specht 1969), a covering the last 2000 years from Buka at the last 2000 years on Manus (Wal Ambrose, pers. post-Lapita pottery tradition, and no evidence for few well-dated points but no sequence yet for the comm.), no evidence for New Britain of a local more than test excavations have been carried out (Garanger 1972a:57-8). Other than this little site had been a pottery manufacturing area at post-Lapita sites.

Ambrose has excavated at several locations on Lou Island, Manus, revealing well-preserved deposits under volcanic ash showers dating to about 2100 BP and 1650 BP. Two contemporary sites 500 m apart showed specialised site functions: the Emsin site was an obsidian point workshop while the Pisik School site appears to have represented a communal cooking area (Gosden et al. 1989:578). The earlier Sasi site showed evidence of point manufacture but also more general village tasks.

Garanger excavated just over 112 m² at the Mangaasi type-site on Efate (Vanuatu). From wasters and unfired clay lumps he suggested that the site had been a pottery manufacturing area (Garanger 1972a:57-8). Other than this little more than test excavations have been carried out at post-Lapita sites.

**POST-LAPITA CULTURAL SEQUENCES**

There is a continuous pottery sequence covering the last 2000 years from Buka at the northern end of the Solomons (Specht 1969), a few well-dated points but no sequence yet for the last 2000 years on Manus (Wal Ambrose, pers. comm.), no evidence for New Britain of a local post-Lapita pottery tradition, and no evidence for New Ireland manufacture of pottery within the last 1000 years. The earliest non-Buka pottery from Bougainville provides a 1500 year continuous sequence in the Kieta area of the south-central part of the island (author's data). An exactly parallel sequence, at least for its 'early' and 'middle' periods down to about 300 BP comes from the Shortland Islands off the southern end of Bougainville (Irwin 1972), is replicated in Buin in south Bougainville (Terrell 1976), and is also likely in Choiseul on evidence from surface collections (Miller 1979). Terrell and Irwin's (1972) suggestion of a Bougainville Straits interaction sphere seems confirmed by the more recent data. Prehistoric pottery has also been found elsewhere in the Western Solomons but no pottery sequence has been established (Miller 1979; Reeve 1989).

No post-Lapita pottery appears to have been manufactured elsewhere in the Solomons, and the Mangaasi pottery from Tikopia and Vanikoro has been sourced to northern Vanuatu, possibly the island of Santo (Kirch 1982; Kirch and Yen 1982). Further south, pottery is known from the Banks Islands (Ward 1979) but how long that pottery tradition continued is a matter of dispute (Kirch 1982:72-4; Kirch and Yen 1982:204, 206). Pottery was manufactured in northern Vanuatu up to ethnographic times but no prehistoric cultural sequence has been established for any of the northern islands. This area remains one of the most under-researched in the Pacific. For central Vanuatu there is the major work of Garanger (1972a), already referred to. Even here, however, there is dispute over the length of time pottery continued to be used. In some parts of Garanger's text he seems to suggest an end to pottery manufacture in the 18th century AD, while elsewhere 750 BP appears to be the *terminus ante quem*. Recently Ward has reanalysed Garanger's dates and suggests that pottery went out of use in the early centuries AD (Ward 1990). Ward himself, however, seems not to have taken into account the excavations by Shutler and Shutler (1965, 1966) at Fila Island (off Efate) where Mangaasi pottery was found in contexts dating to 1000 BP, 950 BP and 700 BP. The question thus remains unresolved, although it is almost certainly the case that pottery making had ceased in Central Vanuatu by about 700 BP at the latest.

In southern Vanuatu pottery is very scarce (it is commonly encountered in surface collections in central Vanuatu) and has only been excavated from the Ifo site on Erromango. Pottery with affiliations both to Lapita and Mangaasi traditions dated to 2350 BP at that site, and it is likely
that pottery ceased being manufactured in southern Vanuatu soon after that date (Spriggs and Wickler 1989). In New Caledonia pottery manufacture continued into this century.

Cultural sequences have been harder to establish in areas without pottery. In part this is due to a lack of distinctive material culture innovation prior to about 750 BP. Many individual sites are known in the Solomons and southern Vanuatu but in general little has been established except that people were occupying the sites.

One strand of available evidence is that for forest clearance and landscape change (Spriggs 1985, 1986). There is evidence for increased erosion rates, presumably associated with clearance for agriculture, from various Lapita sites (Gosden et al. 1989:573). Post-Lapita the most detailed evidence is from Aneityum in Southern Vanuatu, although Roe's work on Guadalcanal will provide a detailed picture from that island. On Aneityum increased sedimentation, valley infilling and coastal progradation were initiated at several locations by 1600 BP (Spriggs 1986:9-10). The use of the newly created valley flats for settlement and agriculture—initially dry land but later irrigated—began around 950 BP (Spriggs 1986:11). By the early European contact period (AD 1830), the valley floors were covered with a network of intensive irrigation systems, some tapping streams in adjacent watersheds to increase their water supply (Spriggs 1981).

There are other areas in Island Melanesia which were centres of intensive agriculture at European contact. Surface remains of irrigated agriculture systems have been recorded from elsewhere in Vanuatu, Guadalcanal (Roe 1989), the Western Solomons and New Caledonia (Spriggs 1990d). The archaeological investigation of these systems has barely begun but in concert with excavation of associated settlements will allow a detailed knowledge of the timing and processes of agricultural intensification in the region. The widespread distribution of pondfield irrigation in Island Melanesia and in Polynesia suggests a Lapita origin, and opens up the possibility of a link to some of the Island Southeast Asian systems in use for taro in some areas as well as for rice (Spriggs 1982).

**LATE PREHISTORY**

In some areas the material culture and settlement patterns of the period immediately prior to 750 BP seem broadly similar to those observed at European contact, but particularly in parts of the Solomons and Vanuatu major changes occur at about this date which mark it off from what went before. A series of oral narratives from Central Vanuatu recorded by Guiart (Espirat et al. 1973) have been linked convincingly to a series of archaeological sites of the last 750 years by Garanger (1972b). Kirch and Yen (1982) have also drawn upon a corpus of oral traditions collected by Firth (1961) on Tikopia. Similar collaboration between archaeologists and oral historians in other areas is likely to be as fruitful in elucidating details of the process that led to Island Melanesian societies as observed by the early European explorers and missionaries.

This period too is crucial as our area has provided the world with one of its great anthropological stereotypes, the 'Big Man' society. This was developed by Sahlins (1963) on the basis of Oliver's (1955) ethnography of the Siwai of southern Bougainville as observed during the late 1930s. It has led to a gross ethnographic oversimplification of Melanesia as having Big Man societies, contrasted with Polynesia having chiefly societies. Where chiefs were found in Melanesia, their presence has often been interpreted as a cultural borrowing under Polynesian influence and the presence of Polynesian 'outlier' islands along the eastern fringe of Island Melanesia has been invoked as the source.

Some areas do indeed show evidence of significant Polynesian influence, and in general these are areas which also have chiefly systems. But this would seem to contradict models for Lapita society, also suggested as hierarchical. Do the chiefdoms represent recent innovations in sociopolitical organisation under Polynesian influence or intrusion ('elite dominance' in Renfrew's [1987] term), or are they continuities from Lapita, perhaps with superficial Polynesian trappings?

At about 700 BP significant changes occur in some of the so-called 'Polynesian outliers' like Tikopia or in islands adjacent to such outliers such as Efate in Central Vanuatu. On Tikopia an identifiably Polynesian element in the material culture comes into prominence, one recognisable in the ethnographic assemblages (Kirch and Yen 1982:331). Some architecture takes on West Polynesian forms, particularly in the use of coral conglomerate slabs mined from the beaches. There are direct imports of stone adzes of oceanic basalt from Western Polynesia and some new artefact forms make an appearance: a West
Polynesian style of trolling lure, *Polinices* shell ornaments, bone needles and pig tooth beads. These latter are almost a *fossilis directeur* for this period in Tikopia and Vanuatu. Pottery imports from Vanuatu cease at this time on Tikopia. On Efate at the same period is the grave of Roy Mata, an important chief who came from 'the south', set up the modern system of Efate chiefly titles and was buried with attendant human sacrifices and 'voluntary' immolation by representatives of the many clans under his control (Garanger 1972a, 1972b). Garanger interprets Roy Mata as a Polynesian immigrant and his burial is indeed very reminiscent of chiefly burials found on Uvea (Wallis) in Western Polynesia (Sand and Valentin 1990). New elements of material culture came in at this time with a greater reliance on shell tools such as *Terebra* and *Lambis* adzes and the probable cessation of pottery manufacture.

Two chiefly graves on Anentityum, one discovered by villagers in the 1970s and one excavated by the author in 1983, display a very similar assemblage of ornaments to the Roy Mata burial and fit in with oral traditions of chiefly burial rites on the island. One of these burials dates 400-300 BP and osteological analysis, particularly tall stature and some non-metric traits, has suggested a Polynesian affiliation (Douglas ms.). There are several other cemetery sites of this period known from Efate and from Polynesian outliers in Vanuatu and the Solomons with very similar material culture (Garanger 1972a; F. Leach and J. Davidson pers. comm. for Taumako; Shutler and Shutler 1965).

The existence of Polynesian-speaking peoples on the 'outliers', Polynesian loans in New Caledonian and Vanuatu languages, local myths involving Polynesian culture heroes such as Mauitikitiki and Tangaroa, and oral traditions of 'Tongan' contact all point to a period of important Polynesian influence in the last 700 years of prehistory, in the more southerly regions of Island Melanesia at least. The nature of the contact and its effects clearly varied from place to place, but whether there is a strong Polynesian influence on the substance rather than merely the outward form of political organisation is not at all clear (cf. Spriggs 1986:18).

Contact-induced change of another kind is evident in the Bougainville Straits area where Austronesian-speaking Shortland Islanders and Non-Austronesian Buin speakers have been in contact for at least 1000 years (the archaeology prior to 1000 BP is unknown). Here the 'feudal' nature of Buin society is thought to come from Shortlands influence, either by elite dominance or 'mimicry' (discussed in Terrell 1986:222-40). The major ethnographer of the Buin, Richard Thurnwald, noted that between his two visits 1907-09 and 1933-34 there had been an important levelling of social distance between the chiefs and commoners. He attributed this to European colonial and mission pacification (Thurnwald 1951). The group adjacent to the Buin is the Siwai, studied by Oliver in 1938-39 and the model for the contrasting Big Man social type. The Siwai have no earlier ethnography equivalent to Thurnwald's 1907-09 observations and it is possible that the Big Man system is here a transformation under colonialism of a more hereditary, hierarchical state of affairs. Terrell found support for Thurnwald's ideas on 'devolution' in the archaeology of the Buin area. No archaeology has been carried out yet in Siwai but the results of such work could provide a significant commentary on the genesis of classic Big Man systems. The Big Man to Chief model is commonly invoked as an explanatory framework in European prehistory, particularly in neolithic and bronze age studies. It would indeed be ironic if the imperialist descend ants of these early Europeans are the real cause of this form of social organisation!

Pushing prehistory back from an ethnographic baseline, utilising evidence from oral traditions and continuities in lifestyle in the archaeological record, will be an increasingly valuable technique in Island Melanesia and one that could lead to a particularly rich description of the last 700 years or so in some areas. This should help to show us that this region is certainly not 'cold', unchanging and without history over long periods of time but presents a dynamic interplay of endogenous and exogenous cause and effect. Such research is important on a world archaeological scale in deconstructing the ethnographic parallels by which archaeologists 'think' prehistory in other contexts, parallels often directly or indirectly inspired by the recent ethnography of the area covered here.

**CONCLUSIONS**

This paper has attempted to identify some of the areas where we know little or nothing about important questions of Island Melanesian prehistory and has sought to suggest some avenues for further research. The coverage of problem areas is of course idiosyncratic and I have left out many unresolved problems to do with issues of the relationship between archaeological and linguistic, and archaeological and human biological prehistories for the region (but
see Spriggs, in press, c). The recent prehistory of some areas has barely been mentioned here, significant work having taken place in the Bismarcks and Southeast Solomons in particular. Also not considered have been the evidence for wide-ranging cultural connections shown by the distribution of painted and engraved rock art in Melanesia, issues of gender relations in prehistory (Marshall 1985), and the archaeology of early European contact.

In function, this paper is, in much more limited scope, an update of parts of Jack Golson's (1972) justly celebrated 'Both sides of the Wallace Line: New Guinea, Australia, Island Melanesia and Asian Prehistory', first presented in 1967. Different authors in this volume have reassessed what we do or do not know about other regions covered by Golson's vast canvas. The advances in our knowledge of Island Melanesian prehistory over the past two decades and more have been profound, but many of the problems raised by Golson in 1967 are still pertinent today.

If we run through his article we find a general theme of the relation between Pacific prehistory and Southeast Asian prehistory, stressing the contact between the two areas. Golson discussed the issues surrounding early waisted blades and stemmed tools and linked their occurrence in New Britain and the northern Solomons to a postulated early settlement of these islands. Certainly some prescience here. He gave extended coverage to the incised and applied relief wares which occur widely in our region, noting parallels not only between such wares in New Caledonia, Fiji and Vanuatu but bringing into the discussion surface collections from Bougainville as well. His analysis of regionalism in later New Caledonian pottery still stands today as an important contribution. The same Southeast Asian parallels with Lapita pottery design which he noted continue to exercise archaeological minds over 20 years on. Finally, he took seriously the suggestions by some art historians of Bronze Age (loosely Dong-Son) influences in New Guinea and Island Melanesia, bringing in information from pottery, rock art, occasional surface finds of bronze in West New Guinea and suggested bronze prototypes for various stone and pottery forms. Such an interest has recently been vindicated by the discovery of a bronze artefact in a 2100 BP context on Lou Island, Manus (Ambrose 1988). This find is contemporaneous with the earliest bronze artefacts in Island Southeast Asia (Spriggs 1989:607) and is a material witness to the influences Golson suggested in 1967. Recent studies of Melanesian rock art also show a clear connection between Eastern Indonesian, New Guinea and Island Melanesian styles (Ballard, in press).

This comparison might suggest that we have advanced our knowledge of Island Melanesia less than we would like to think. Some areas certainly remain very much under-researched, particularly the main Solomons chain and Vanuatu. Government attitudes to foreign researchers have prevented any archaeological research in Vanuatu since 1983, apart from that directly connected to environmental assessment of proposed development projects. Research in Solomon Islands has also been severely constrained since that time for similar reasons. Papua New Guinea has been more open to archaeological research in the last decade but increasing law and order problems in many parts of the country are likely to constrain research in the future. The author's own research in the North Solomons Province had to be abandoned because of guerilla warfare and the subsequent declaration in May 1990 of a rebel Republic of Bougainville which has de-facto seceded from PNG. The future of research in New Caledonia is by no means certain in the lead up to a planned referendum on independence for the French territory in 1998.

One significant change since Golson's 1967 paper is that the indigenous people of the region have become empowered to take an interest in their past and the first generation of Melanesian archaeologists are starting to make their mark in research. Let us hope that the area's current socio-economic problems and political instability can be overcome to allow them to continue to investigate and hopefully answer some of the questions posed by the current state of research.

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POTTERY RAW MATERIALS: SOURCE RECOGNITION IN THE MANUS ISLANDS

W.R. Ambrose
Department of Prehistory, Research School of Pacific Studies, The Australian National University, Canberra, ACT 0200, Australia

The Manus or Admiralty Islands are relatively remote from other island groups in Melanesia, the nearest neighbours being 170 km away in St. Matthias and New Hanover to the east, or the small cluster of the Hermit Islands an equal distance to the west, or the New Guinea landmass to the southwest over 200 km distance. The main high island of Manus and its smaller outlying islands form a significant land mass by oceanic standards, amounting to around 3000 km². The group includes low and high island terrains comprised of a wide range of volcanic and sedimentary geological substrates. For the long prehistory of this large, diverse and isolated island group there was the potential for local cultural development, relatively free from the adventitious impact of surrounding peoples. The local development of pottery in these islands over the last 3500 years will have been affected by this regional isolation, while the employment of local raw materials from the diverse Manus geology, offers an attractive area for the study of a key artefact in understanding Manus prehistory. On the other hand access within the archipelago to exploitable resources and transactable commodities required a well developed canoe technology, while general remoteness required ocean-going sailing canoes for any purposeful contact with outside groups; both internal and external factors would have reinforced the value of a maritime orientation within the archipelago. Despite a century of changing foreign administrations, a cash economy, more consumer oriented aspirations and the introduction of powered fibreglass vessels, some of the coastal people of the Manus Islands are still avid canoe builders and sailors, although pottery is no longer made.

Traditionally the Manus people were equally adept at living in coastal and inland settlements, with horticulture, arboriculture, fishing and hunting all being well developed. Cutting across this productively diverse resource base is a high degree of regional linguistic differentiation with a cluster of about 19 languages being recorded. The relationships between languages in this cluster indicates some complexity in their derivation from an ancestral Oceanic language; the differentiation being an outcome of dialect networks compounded by population movement within the archipelago (Ross 1988:319). Recent work on the prehistory of Manus shows that habitation took place during the Pleistocene, so that the linguistic prehistory of the archipelago is not simply related to the Oceanic language family of the last 4000-5000 years as Ross surmised (Ross 1988:315). Superimposed on the resources and linguistics fields was a system of socially observed proprietary rights in a range of product specialisations exercised at village and lineage level. The idea of proprietary specialisation was widespread and probably has a long history in the region (Ambrose 1978:329). It requires that the potential users of the goods and services should not set out to compete with the rights to the specialisms of other groups. A good example of complexity in rights to the ownership and access of important resources is the practice of the Ponam Islanders in controlling fishing in their surrounding waters off the north coast of Manus; this turned 'a relatively straightforward technical operation of fishing into a forest of rights and counterrights, each the jealously guarded property of one agnatic lineage or another' (Carrier and Carrier 1989:103). But the proscription against assuming the rights of other groups appears to have been an ideal that could be overtaken by coercion when an opportunity arose.

The most visible archaeological reflection of two widely circulated formerly important products is to be found in the ubiquitous surface remains of obsidian and pottery. The substitution of metals for these materials began a century ago; being quickly triggered by the German colonial administration earlier this century in the case of obsidian following a ban on spear production and, in the case of pottery finally phasing out only within the last decade. Most Manus people can name the former centres for pottery production, as M'buke Island off the south coast and Ahus Island off the north coast; obsidian is said...
to have been supplied exclusively from shaft mines on Lou Island to the south. Other specialist groups were said to be responsible for a host of items such as large canoes, wooden bowls, beds, pottery, obsidian, fishing nets, wicker oil containers, coconut oil production, eggs and so on (Mead 1930). The transfer of commodities and valuables would have been complex and extensive despite the potential simple self-sufficiency of most villages. While exchanges implicitly acknowledged and reinforced the exclusive rights of participating groups, their overall connectivity and integration leads Schwartz to the view that traditional Admiralty Islands society should be considered as a single areal culture (Schwartz 1963:68).

The main expression of the integration was in the active exchange of goods manufactured and produced in different places and brought together in markets, or distributed via intermediaries such as the Titan language group who were active in canoe-based exchanges along the south coast of Manus (Mead 1930), and people from the north coast islands who were similarly engaged (Carrier and Carrier 1989:66). The pervasive nature of exchange transactions and the observance of proprietary exclusions, could be expected to work against the domination of the system by any one group. At the same time group coherence necessary to establish a strong intermediary control in exchange transactions over several generations may seem an unlikely outcome given the stresses which have been noted in traditional Admiralty Islands social organisation (Mead 1937:234; Schwartz 1963:83; Carrier and Carrier 1989:70). Nevertheless the south coast Titan group appears to have gained a reputation this century for stability as middlemen who, lacking their own extensive garden lands, managed to extract their needs for produce by mediating the flow of goods between other groups. The extent of their mediation appears to have been geographically limited to the south side of Manus Island and the outer smaller islands but whether this amounted to a middle-man trader role is a moot point. The Titan produced quantities of seafoods and pottery for exchange with other villages, so that their major effort was production for direct transaction. It is not possible at this remove in time to determine the relative amounts of imported material destined for transfer to other end users in a pre-colonial setting.

The prehistory of this specialist Titan pottery industry is not known, but its locus probably changed over time with the flux of lineage dominance, from splitting and fusion of alliances. For instance the historically important pottery production centre on M'buque Island was established as a Titan preserve only within the 19th century, after the migration of disaffected Titan lineages from Pere, on the Manus south coast, and the assimilation of the local non-Titan M'buque inhabitants (Crocombe 1965:44). Yet field evidence shows there are large and stylistically diverse collections of potsherds from the island which seem unrelated to the recent Titan wares. Some of the prehistoric M'buque style pottery has been found 30 km away on the Manus south coast. In the case of the potters on Ahus Island on the north coast, one informant claimed that the island was only settled around five generations ago, by people migrating from their land on the main Manus Island. The question arises whether the prehistoric distribution of pottery arose by the same means as the historic cases with supplier-carriers based on M'buque and Ahus. It can also be questioned whether the transaction per se has been elevated to a specialised role in its own right by the recent activities of the Titan, and whether they were different in a major way from the usual sequestering of specialised skills, wares and products by other Manus groups. The additional geographical range of influence attendant on exchanges of pottery and sea products by the Titan with other groups can be seen as a result of a broadly accommodated notion of specialisation already existing in Manus Islands societies. On the other hand increased specialisation in goods production could be a corollary of specialisation as an intermediary as Irwin (1977) has shown for the Papuan south coast, where a number of dispersed pottery-making villages yielded their role over time to the monopoly production centre of the offshore Mailu Islanders.

In Manus, as the owners of rights to resources or production of goods are likely to change their allegiances, domicile and exchange relations with the changing fortunes of their lineages and villages over time, there would have been a state of flux in the emphasis, direction and location of product-based transactions. An archaeological manifestation of this is likely to be the changing distribution pattern of pottery and the raw materials assembled for its manufacture. The identification and dating of pottery manufacturing centres and the delineation of the distribution of the wares could contribute to an understanding of Manus within New Guinea prehistory.

At a broader level it is important to understand the history of the organisation of the specialist role system in New Guinea, for it has been claimed to be the precursor of a political align-
ment towards more centralised control of commerce generally (Allen 1977).

**PREHISTORY**

Manus Islanders have maintained their sea-borne contact with distant island groups for at least 3500 years. The most telling evidence for this is in the distribution of obsidian to early sites throughout the Melanesian island chains. The main point of the exploitation of Manus obsidian is that it appears to occur simultaneously with pottery, with the first settlement of many islands as far away as Vanuatu and indicates a major improvement in communication brought about by advances in boat-building and sailing expertise. The early extensive voyaging waned over time although Manus obsidian was being transported at least 200 km to New Ireland and the New Guinea mainland up to the beginning of this century.

The other important indicator of outside contacts is the presence of the distinctly decorated Lapita pottery in the Manus Island site of Kohin Cave (Kennedy 1981a). Further Lapita style sherds have been found at Baluan Island on inland gardening land and on the nearby islet of Mok (McEldowney and Ballard 1991). The introduction of pottery and the very widespread transfer of obsidian around the same time indicates that important changes were underway in Manus around 2500-3500 years ago.

Bearing in mind that up to 20 different languages are presently recorded for the Manus Islands, it is easy to appreciate that regional diversity of pottery forms may also have a long and complex history. The continuous tradition of pottery manufacture, the evidence for extensive distribution of the Lapita style wares 3500 years ago, the presence ethnographically of pottery specialists and the widespread distribution of their wares, all point to the value of detailed pottery analysis for understanding the prehistory of the Admiralty Islands.

Off the south coast of Manus, based on M'buke Island, the Titan people operated their proprietary rights for the manufacture and distribution of pottery as part of their overall emphasis on an exchange-based economic strategy. In the earlier years of this century German reports also locate Titan pottery production at Timoenai, a mainland south coast village, and on Tawi Island which was uninhabited in the 1980s. Off the north coast of Manus the tiny .5 km² island of Ahus was the base for another pottery industry by a different group, speaking a different language, and producing different wares with a different raw material. Their double mouthed pots have some parallel to the double spouted water vessels from a 1600-1700 year old site on Lou Island, off the Manus south coast. The thin light coloured ware of the prehistoric Lou double spouted water pots is very similar in appearance to fragments of pottery dating to 2100 BP from another site on Lou Island, where differently decorated thicker red ware is also present. It appears that at this early period two types of ware for the most likely specialised functions of cooking and water containers were present (Ambrose 1991). The need for dual purpose vessels has been described for the Papuan south coast, in pottery of the last 1000 years, where basic cooking vessels determined the composition of the ware, with specialised water container functions being achieved by post-firing modification of the basic ware (Rye 1976:119). Rye has also observed that the addition of salt, through the widespread use of sea water, modifies the chemistry and colour of wares when fired to temperatures between 800-900°C (Rye 1976:122). Here there is a contrast between the modification of a single fabric by post-firing treatment on the Papuan coast, and the presence of two different fabrics in the early Manus wares; and continued as two separate products between the Manus north and south coast to the present.

As well as questions regarding the distribution of pottery, the location of manufacturing centres, and the functional difference between vessels over time, there is a range of technological enquiries which could be addressed to Manus Islands pottery and other prehistoric wares throughout New Guinea. The analysis of pottery shape and decoration has traditionally provided the baseline for understanding the chronology and connection of archaeological sites; in an area with such a long continuous pottery tradition this typological contribution is a necessary basis for prehistoric research. A beginning to the study of the changes in decoration and form in the Manus Islands pottery industries has been made by Kennedy (1979, 1981a, 1981b, 1982, 1983).

**PHYSICAL ANALYSES**

Rye and Allen (1980:305) suggest that an analysis of morphology and decoration may not be able to distinguish the more complex details of pottery manufacture, nor differentiate the wares within the geographically confined area of the Papuan coast they studied. Any distributional
analysis of pottery based on form and decoration is necessarily based on arbitrary definition of the elements of style, where conventions may have a life of their own. For instance the widespread Lapita decorative system continued over several hundred years in a readily recognisable way while incorporating diverse raw materials from a very wide geographic range of sources. In this instance the decorative suite may not be as useful as the mineralogical suite in determining the manufacturing location and distribution of the ware. As well the broken and fragmented condition of pottery in archaeological collections from the southwest Pacific restricts the analysis of morphology and decoration to a small fraction of the sherd collections. This may not be a problem when a large collection can be investigated, such as that from Motupore Island with its 800,000 excavated sherds (Rye and Allen 1980:306) but it becomes extremely restrictive when only a handful of decorated sherds are recovered, such as those from Kohin Cave (Kennedy 1981a:757). In contrast the chemical and physical analysis of pottery composition and technology gives information for any sherd large enough to be analysed. As analytical techniques have improved, the information to be won, from both large and small pottery sherd collections, has increased.

TEPPER IDENTIFICATION

The most comprehensive petrological study of the sand temper component in Pacific Islands potsherds has been undertaken since 1965 by Dickinson (1979), who showed that valuable information can be gained from a standard microscopic examination of sherd thin sections. For example Dickinson's petrological analysis of sherds from the 3000 year old Lapita site at Mulifanua in Samoa, is quite definite in the identification of a local northwest Upolu source for the temper sands used in the pottery (Dickinson 1974:179). In his overall survey of Pacific potsherd tempers, Dickinson is confident when ascribing mineral suites which are compositionally incompatible with the local geology to exotic sources (Dickinson 1979:1689). Although Dickinson has examined less than 1000 prehistoric potsherds in detail from about 50 different islands sites, the ability to macroscopically differentiate between thousands of sherds, by using the mineral identifications recorded at the microscopic level, is a potential of Dickinson's work that has not been developed by others. One major disadvantage of standard petrology is that the chemistry of the clay component is not included in the analysis. In the case where exotic tempers have been identified, it is not possible to decide whether raw temper, clay, finished ware or all three were transported to the site (Dickinson 1979:1662), yet clay is the most important component in the manufacturing process.

The trade in clay by the Amphlett Islanders of Papua New Guinea required a day's canoe voyage to quarries at the northwest tip of Ferguson Island where two varieties of clay were gathered; from one, Lauer estimates that about 290 m³ has been removed (Lauer 1970:141). The effort expended in this case in acquiring a preferred clay can be compared with the casual acquisition of mineral fillers. Dickinson (1979:1663) believes that 'serviceable earthenware apparently could be made with essentially any handy material as temper, provided suitable clay was available nearby'. In many cases therefore, when the sources of the pottery raw materials are being investigated, a temper analysis will give a different result from a clay analysis. For the purposes of tracking materials sources and pottery distribution the two components of clay and filler are not equal. Mineral fillers may be derived from a wide range of depositional processes, including ephemeral ones, such as beach and stream sands, while the proportion present in pottery may vary up to one-third. This observation prompted Dickinson to question the value of bulk sample instrumental methods for the chemical analysis of pottery in favour of microscopy (Dickinson 1979).

The experimental work of Rye on the effect of firing temperatures and workable sand/clay mixtures, with and without sea water, and from three clay sources in the Papuan Gulf is instructive (Rye 1976:123). It shows that even the least workable clay, from Boera, has a tolerable workable range with the addition of sand temper between about 14-32% by dry weight. Successful experimental laboratory firing at low temperatures around 700°C was achieved for practically all mixtures of sand from 5-40%.

Since the essential element in pottery production is clay, with a greater latitude for compositional variation in the mineral filler, we should really consider these two components separately and expect each fraction to yield different information. Worthing (1982:80), aware of the likelihood of separate sources of clay and filler around the Port Moresby area of the Papuan coast, shows through standard microscopic petrological examination that the local lithology is reflected in the modal percentage of quartz-feldspar of both the clays and the sands used for
pottery there. Freestone (1982), using an electron microprobe for examining technological aspects of clay chemistry in pottery from western Europe, has been able to relate ceramics to a geological source and to provide basic chemical characterisation data on unprovenanced wares. The advantage of the microprobe is its ability to selectively analyse individual components such as single crystals of feldspar or pyroxenes or the surrounding clay matrix (1982:106). The system is an extension of the standard microscope mineralogy analysis and has been applied extensively to the ceramics of Melanesia by Summerhayes (1987). By comparing the analysis of wares by the bulk sample PIXE method, with the selective component analysis of the microprobe, Summerhayes (1989) was able to differentiate the relative contribution of mineral inclusions and determine whether they were contributed as part of the raw clay or the added filler. Anson (1983:166), after analysing for major elements in the clay of Lapita style pottery by microprobe, found that the wares from Watom overlapped at one standard deviation with a prepared clay sample from the area. A similar claim of compositional identity, on the basis of major element analysis of the clay matrix, is made by Hunt for sherd and clays from Mussau and the Manus Islands (1989:191); his caution in rejecting Na, Cl, Ca and P is well founded because all these can be included in the clay after it has been collected for pot making. The eight major elements used by Hunt, i.e. Al, Si, Cr, Fe, Ti, Mg, Mn and K, are not ideal for separating clays from diverse geological sources and further confirmation of claimed affinities is needed.

An alternative to petrology for examining pottery composition is instrumental chemical analysis, but the very wide potential range of clay-filler combinations used in pottery manufacture makes any direct chemical association of a sherd with a particular source very difficult. Hancock (1984) investigated the source of clay used in the Roman pottery of Cologne using neutron activation analysis. His study was based on the demonstration that modern wares contained more sand temper than the Roman wares, with Hafnium being a diagnostic element positively correlated with silica and introduced with zircon sands. The use of elements correlated with either clay or filler allows some separation of these components as Hancock has shown but in the absence of other diagnostic elements little differentiation can be expected. For example Barlow and Idziak (1989) attempted to differentiate between calcareous and non-calcareous clays combined in pottery but were restricted to the major elements Si, Al, Fe, Mg, Ca and Ti. The problem of clay-temper chemical differentiation has been addressed by Neff et al. (1989) in terms of the effect the tempering material has on the final mixture. They identify low effect tempers (for Neutron Activation Analysis) including silica and calcite and proceed to distinguish between the ‘inert diluent’ and other parts of a tempering medium having a relatively large effect on the final pottery composition. All these approaches attempt to use an undivided pottery sample for chemically differentiating the clay and filler from a mixture of the two components.

Rye (Allen and Rye 1982:110) sought to overcome the problem of determining a unique clay source chemistry from a range of temper-clay compositions by experimentally producing a series of filler and clay mixtures for each of the known clay sources in the Port Moresby area; the mixtures were then used as reference standards chemically characterised by PIXE analysis (Duerden et al. 1980). Certain element ratios were constant for the range of mixtures from each clay source and therefore allowed each quarry source to be uniquely identified regardless of its dilution by an admixture of filler. When the element ratios of archaeological pottery showed the same pattern over about 20 element combinations it was ascribed to the matching source. The same data can also be used to define body composition from the temper-clay proportions of the archaeological sherd (Allen and Rye 1982:111). This approach by Rye can be very productive when the clay quarries are located with certainty, as is the case with the recent potteries of the Port Moresby area. Difficulties may emerge if the potsherds have an exotic origin for which there are no reference materials available for comparison; the possibility of this occurring must increase for older prehistoric wares.

For the general corpus of pottery in the Melanesian archaeological collections there are no clearly known clay sources apart from those described in the recent ethnological literature. It would be unwise to presume that these recent sources supplied materials throughout the prehistoric record in the region. Therefore the approach suggested here does not require clay sources to be known as marks on a map, but rather sets out to characterise the unique properties of the clay sized fraction incorporated in the pottery itself; if a source can be located on the ground that matches the clay within the sherd then this is very useful additional information, but the absence of such an association should not impede the analysis. The main means for
implementing this approach is to separate the clay sized fraction from the filler before either is chemically analysed. This will then enable the two main components to yield separate analyses which would provide two independent lines of evidence approaching the chemistry of the original raw materials.

There is some question about the stability of discarded pot sherds to the effects of long term weathering; this is also a reason for caution in attempting to locate a raw material source in the ground for the matching chemical properties of sherds from an archaeological site. Franklin and Vitali (1985) have shown experimentally that sherds may be relatively inert to loss from leaching effects in the ground, but Freestone and others (1985) demonstrate the acquisition of phosphate in archaeological sherds through the porous nature of the wares. Knapp and others (1988:77) consider their difficulty in assigning sherds, analysed by PIXE and NAA, to specific clay sources may be due to both leaching and accretion of elements since their manufacture in Jordan several thousand years ago. There is therefore good reason to treat the archaeological assemblage as a discrete collection to be chemically analysed in its own right, separate from attempts to locate the sources of its raw material.

**MANUS POTTERY**

Pottery has been present in the Manus Islands for possibly 3500 years, allowing ample time for potters to gain a comprehensive knowledge of usable local clay sources. Over the same time changing alliances and population centres will have been reflected in changes in pottery manufacturing centres, technology and raw materials. It is the intention here to examine an alternative procedure for pottery fabric analysis as an instrument for examining these changes.

The possible degree of partitioning of pottery collections into useful groupings, on the basis of their chemical composition, will be to a large extent correlated to the diversity of the local geology. The more diverse the local lithology the more likelihood there is of defining a narrow raw material province, with a corresponding increase in the definition of possible fields of prehistoric interaction.

The Manus Islands have a diverse lithology ranging from olivine basalt through microdiorite, andesite, dacite and rhyolite to obsidian as primary volcanic deposits, with volcanic conglomerate, tuffaceous and lithic sandstone, silt-stone and limestone among the sedimentary suite. The oldest dated rocks range from 50 million years for the Tinniwi volcanics to 11 million for the Yirri intrusive complex (Jaques 1980). Tropical weathering has produced bauxite deposits in some areas, and zones of mineral enrichment occur in association with the intrusive complexes. The most recent volcanism saw the emergence of submarine rhyolite and pitchstone islets off the coast of Lou Island in 1953. The landscape of the Manus main island and some of the outer volcanic islands is rugged and dissected by steep drainage systems. Given the diverse geology of the region, the prospects of defining clay and mineral temper sources within a confined geographical area may appear to be fairly good. On the other hand the drainage regime of the main island taps a range of eroding rock types that yield their mixed materials to the many streams, rivers and coastal sands.

The only pottery clay sources the Manus people presently recognise are found in coastal deposits of Manus Island and M'buke Island. The north coast Karli Bay clay is quarried from sluggish stream-laid sediments near the present beach. The stream-laid quaternary clays derive from source rocks of tertiary volcanic conglomerates and sandstones. The south coast clay is quarried from redeposited sediments in the banks of the Watani River behind the village of Timoenai; the source rocks include a range of basalts, andesite, dacite and quartz microdiorite, with some tuffaceous greywacke and limestone, all early tertiary in age. Clay from Vogali island in the M'buke group is derived from quaternary basalts. The different lithology in these catchments would ensure that the clays and temper sands nearby will be chemically distinct. The ethnographically described pottery industry on the north coast (May and Tuckson 1982), was on the low coral island, Ahus, about 4 km directly offshore from the Karli Bay clay deposit. The industry described on the south coast was based in the M'buke Islands about 23 km directly offshore and also on Tawi Island nearer the main Manus island. The M'buke islands are basaltic volcanic remnant cones so that clay from the mainland will be different from the local material. For an examination of these two pottery producing centres over the recent past it would be possible to adopt the procedures of Rye since the raw material sources are generally known. But as a time range of at least 3500 years for local pottery making is involved and with the probability of presently unrecognised clay-temper deposits being used over this period, and with the inherent complexity in the basic lithology supply-
Ambrose

ing the stream sediments, an alternative approach to characterising the archaeological pottery clays and the definition of inherent groups is warranted.

Source matching

In cases where only one or two sherds have to be characterised and matched to a particular source, a primary need is numerous data on well defined sources. On the other hand, where many thousands of potsherds may be partitioned among a number of known and unknown sources, the entire pottery collection can be considered as the sample universe for an initial characterisation study without immediate reference to any source. A collection from which clay groups can be chemically partitioned can then be examined again by thin section petrology, or other means, in order to define lithological provinces from which clay or temper may be derived. Separating the chemical differentiation of separated pottery clay from the task of source ascription is necessary in order to test the underlying assumption of connection between the two. An experimental run for the analysis and characterisation of pottery from two adjacent islands off the south coast has been undertaken with the foregoing considerations in mind.

POTTERY ANALYSIS

The low firing temperatures achieved in the open bonfire technique used throughout the Melanesian region does not produce high quality ware since vitrification of the clay minerals is not achieved, although grain boundary sintering and transformation of most clay minerals will occur. The experimental results of Tite (1969) on the vitrification of ancient ceramics refers to the onset of vitrification for some clays as low as 700°C. The slow heating time of 200°C/Hr reported in Tite's experiments contrasts with the rate of 1477°C/Hr determined by Lauer (1970:150) for open firing by traditional potters in the Amphlett Islands. The Lauer rate is not only seven times faster than the Tite laboratory rate but the maximum bonfire temperature is maintained for about one quarter of the time necessary to achieve a sintering effect in the laboratory. Since sintering is a diffusion controlled reaction it is a function of both temperature and time (Kingery et al. 1976:420); sintering is likely to be poorly developed under normal traditional firing practices in Melanesia, while the temperature and time to achieve vitrification is absent. In general firing temperatures between 600-900°C are commonly reported from thermo-couple tests (Lauer 1970:150; Irwin 1977:235) with the period at the maximum temperature rarely exceeding 15 minutes. Even in windy conditions when the firing temperature may approach 1000°C, the short duration at this temperature should not greatly obscure the original chemistry and particle sizes of the pottery raw materials. Pottery sherds in the ground will rapidly absorb moisture and the decomposed clay minerals can rehydrate to their original form (Tite 1969:139).

A test of the proposition that the original chemical components are preserved after firing could be made by separately analysing the coarse (filler) and fine (clay) fractions of the archaeological ware. In the study reported here separated preparations from archaeological sherds were used, including two sets of potsherds from Lou Island and M'buke Island, to the south of Manus, as follows:

<table>
<thead>
<tr>
<th>Location</th>
<th>Site Name</th>
<th>Time</th>
<th>Sherds</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lou Island</td>
<td>Sasi site (GDY)</td>
<td>2100 BP</td>
<td>3 sherds</td>
</tr>
<tr>
<td></td>
<td>Emsin site (GEB)</td>
<td>1600 BP</td>
<td>2 sherds</td>
</tr>
<tr>
<td>M'buke Island</td>
<td>Surface site (GBT)</td>
<td></td>
<td>3 sherds</td>
</tr>
<tr>
<td></td>
<td>Surface site (GBU)</td>
<td></td>
<td>2 sherds</td>
</tr>
</tbody>
</table>

Method

A roughly 5 g piece of each sherd was used. Surface dirt was removed with a scraper or cleaned by abrasion on a carborundum plate. The piece was crushed without grinding and placed in a 50 ml beaker with 40 ml distilled water. The sample was disaggregated with a Bransonic B30 ultrasonic cell disruptor for 10 minutes. The resultant slurry was washed into a 250 ml flask with distilled water, agitated, then allowed to stand for 1 hour. The 10 cm depth of cloudy supernatant water was siphoned into an evaporating dish and allowed to dry in air over a hot plate at about 40°C. The dried sediment was regarded as the clay – fine silt fraction.

The coarser fraction was re-agitated in the ultrasonic disruptor for 5 minutes, again diluted into a 250 ml flask, agitated and allowed to stand for 1 hour when the liquid was siphoned off. The dried sand and coarse silt fraction was collected to provide samples of coarse, non-clay material. This procedure would keep the water soluble material such as salt with the fine fraction and remove it from the coarse fraction; this could account for the higher chloride level in the fine fraction as shown in Figure 1. A better procedure now being used is to remove the liquid by centrifugation.
The coarse fractions were ground in a tungsten carbide mill and, as with the fine portion, were compressed into flat surfaced 10 mm diameter discs mounted in aluminium caps. The 10 sherds, plus clay specimens from a traditionally known clay source at Timoenai on the south coast of Manus and an unknown one from Southwest Bay, comprised the analysed sample. Three of the M'buke sherds and the two clays were analysed twice to give a total of 17 analyses on the fine separations and clays. The coarse separations provided 13 samples with repeat analyses on two sherds, giving a total of 15.

The analyses were carried out at the Australian Nuclear Science and Technology Organisation (ANSTO) using the accelerator-based PIXE-PIGME system (Bird et al. 1983:435). The major advantages of the procedure are its speed at about 5 minutes per analysis, the large range of around 20 elements which can be routinely assayed, the small sample size required of less than a gram, the automated sample cycling of up to 60 specimens in a single batch, and the on-line computer storage and statistical analysis of the PIXE-PIGME spectra. Absolute percentage values were extracted for the major elements Na, Mg, Al, Si, Cl, K, Ca, Ti, Mn, Fe and µ/g determinations for Ni, Cu, Zn, Ga, As, Pb, Br, Rb, Sr, Y and Zr.

Results

The purpose of this initial study was to see whether the method of disaggregation was in fact separating the fine and coarse components into chemically different groups. This could be assessed in two ways. If the disaggregation was unselective, and simply reduced a solid sherd to a homogeneous mixture of fine powder and sand, then there should be no clear chemical difference between the fine and coarse fractions. Even in the case of pottery made from an unsorted primary clay with its natural fraction of coarse components, and collected at its original location in country rock, certain elements will have been preferentially removed by weathering in forming the clay; in this case the same distinction between selective and unselective disaggregation would apply in determining whether the sherd's reduction to powder has achieved a corresponding chemical separation of the original fine and coarse fractions. A successful separation should be accompanied by observable chemical differences reflecting the composition of the potters' original raw materials. A simple test of the disaggregation can be made by comparing the chemical composition of fine and coarse fractions of each sherd as a fine(a)/coarse(b) ratio (using a-b/a+b). A ratio of zero indicating no difference, with deviations from zero pointing to relative depletion or enrichment of the chemical element being analysed. Figure 1 shows the expected biasing effect in the fine/coarse fractions for elements determined from the sherds. Table 1, a two sample T-test on each element gives a better indication of the relative separation of fine/coarse parts of the whole sherd collection.

![Figure 1](null) Relative enrichment (positive values) and depletion (negative values) of elements in the fine and coarse fractions separated from sherd 12 from the Lou Island Sasi Site (2100 BP).

<table>
<thead>
<tr>
<th>Element</th>
<th>F Ratio</th>
<th>Prob.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Br</td>
<td>79.25</td>
<td>.000</td>
</tr>
<tr>
<td>Ca</td>
<td>16.22</td>
<td>.000</td>
</tr>
<tr>
<td>Cl</td>
<td>14.08</td>
<td>.000</td>
</tr>
<tr>
<td>Si</td>
<td>5.72</td>
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<tr>
<td>Mg</td>
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<td>.004</td>
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<tr>
<td>Ti</td>
<td>4.79</td>
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<tr>
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<td>K</td>
<td>3.61</td>
<td>.016</td>
</tr>
<tr>
<td>Ga</td>
<td>3.53</td>
<td>.014</td>
</tr>
<tr>
<td>As</td>
<td>3.40</td>
<td>.016</td>
</tr>
<tr>
<td>Fe</td>
<td>2.94</td>
<td>.035</td>
</tr>
<tr>
<td>Pb</td>
<td>2.06</td>
<td>.098</td>
</tr>
<tr>
<td>Cu</td>
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<td>.102</td>
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<tr>
<td>F</td>
<td>1.89</td>
<td>.127</td>
</tr>
<tr>
<td>Rb</td>
<td>1.81</td>
<td>.389</td>
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<tr>
<td>Al</td>
<td>1.74</td>
<td>.159</td>
</tr>
<tr>
<td>Zn</td>
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<td>.185</td>
</tr>
<tr>
<td>Ni</td>
<td>1.61</td>
<td>.207</td>
</tr>
<tr>
<td>Zr</td>
<td>1.60</td>
<td>.210</td>
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<tr>
<td>Y</td>
<td>1.19</td>
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<td>Sr</td>
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<td>.410</td>
</tr>
<tr>
<td>Na</td>
<td>1.06</td>
<td>.454</td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>Element</th>
<th>F Ratio</th>
<th>Prob.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Br</td>
<td>2.06</td>
<td>.098</td>
</tr>
<tr>
<td>Ca</td>
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<td>.210</td>
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<tr>
<td>Ga</td>
<td>1.19</td>
<td>.373</td>
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<td>.410</td>
</tr>
<tr>
<td>Na</td>
<td>1.06</td>
<td>.454</td>
</tr>
</tbody>
</table>

Table 1 T-test results for element difference between fine and coarse fractions, in descending order of magnitude.

The fine fraction shows relative enrichment or depletion of several elements compared with the fillers. Individual sherds show this effect to a variable extent and with a different ranking of the element fine/coarse ratios (Fig. 2). A second approach for testing the efficacy of the disaggregation procedure is through automatic multivariate clustering analysis. Multivariate analysis of the PIXE-PIGME data should distinguish between clays and fillers if they have a different chemical composition. Basically there should be two clusters in a case of chemical differentiation.
but as various degrees of similarity between an unlevigated primary clay and local filler sands would be expected to occur, the distance of the separation is likely to be variable. Nevertheless intermediate results will still be useful in distinguishing between separate clay or filler sources.

![Figure 2](image)

**Figure 2** Relative enrichment and depletion of elements in the fine and coarse fractions separated from Sherd 15, from the Lou Island site GEB (1600 BP). Although similar in fine fraction enrichment of elements to Figure 1, there is a greater depletion of Na, K and Sr.

A multivariate cluster analysis was used to partition the data set into a number of final groups, using the NCSS computational package of Hintze (1991). At a basic level, based on thin section identification of mineral inclusions there are two groups of sherds present in the analysed collection. These correspond to basalt, andesite and intrusive complex rocks on one hand and dacite, andesite, basalt, porphyritic microdiorite parent rocks on the other (Watchman 1982).

Considering the terminal groups from the cluster program it can be seen that five sherds from M'buke are separated from the five from Lou Island in both coarse and fine fractions; in terms of the petrology of the mineral inclusions the sherds from the acid volcanic island of Lou derive from elsewhere, where the basic rock suite included andesite and basalt. On the other hand the sherds from the basalt island of M'buke contain minerals typical of dacite, andesite, and basalt, again indicating a mixed non-local source for the tempering material. In general, diverse volcanic and intrusive geological environments are represented in the mineral fillers of the sherds (Watchman 1982).

At ten, nine or eight terminal groups all the Lou Island sherds are differentiated from the M'buke group in both fine and coarse fractions. Although the fine/coarse components of the five M'buke sherds maintain their distinctiveness down to eight groups, the five Lou sherds show a higher degree of affinity in their fine/coarse chemistry. At a nine group termination all sherds show a differentiation between clay and filler, but merge at eight groups in the Lou examples. This suggests that the fine/coarse differences in the M'buke sherds are more pronounced than those for the Lou pieces. At nine terminal groups there is a further contrast between the separations of the Lou and M'buke sherds; whereas Lou has three fillers and one fine fraction, M'buke has one filler and three fine fractions represented, one of which is clustered with the Timoenai clay deposit on the Manus mainland. It would be tempting to suggest that the Lou sherds were made from a single clay and separate fillers compared with the mixture of different clays with a single filler in the M'buke case. A larger number of analyses are required to test this observation.

In a separate set of computer runs the clustering program was applied to the fine and coarse parts as separate data sets, shown on Table 2 as groups six, five and four. The group numbers of the fine and filler fractions are therefore not comparable between the two columns. The results show a different partitioning between the Lou Island and M'buke sherds but the overall differences remain. Disconcertingly the Timoenai clay, previously clustered with the M'buke sherds in the combined ten and nine group terminations, is lumped with the Lou sherds when the fine fractions are clustered separately.

The Southwest Bay clay, from the Manus Island south coast is separated from all ten sherds at all the group clusters. Sherds H, I and J from M'buke were analysed twice and as expected there is no differentiation between the two sets of PIXE analyses.

**CONCLUSION**

These results have been achieved on sherd separations using a low level separation system. Work is now being undertaken using better separation procedures that ensure that only clay size particles are analysed. This is being done by using longer settling times, and centrifugation rather than evaporation to remove water from the separated samples.

The disaggregation of the low firing temperature pottery from the Admiralty Islands is feasible and yields consistent results on a 5 gm sherd. The inferences that can be drawn in the present study from the small set of ten sherds and two clays are necessarily ambiguous. It is clear nevertheless that a far more complete picture can be won by using, as our basic elements for analysis, the same separate raw materials of the prehistoric potters. In analysing the complex questions of pottery manufacture, and connec-
<table>
<thead>
<tr>
<th>Site</th>
<th>Sherd</th>
<th>Combined grouping</th>
<th>Separate groupings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>10 groups</td>
<td>9 groups</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Coarse</td>
<td>Fine</td>
</tr>
<tr>
<td>Lou Island</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>GDY</td>
<td>A</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td>B</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td>C</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>GEB</td>
<td>D</td>
<td>3</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>E</td>
<td>5</td>
<td>4</td>
</tr>
<tr>
<td></td>
<td>F</td>
<td>5</td>
<td>4</td>
</tr>
<tr>
<td>M'buke Island</td>
<td>G</td>
<td>6</td>
<td>7</td>
</tr>
<tr>
<td></td>
<td>H</td>
<td>6</td>
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<td>6</td>
<td>7</td>
</tr>
<tr>
<td></td>
<td>J</td>
<td>-</td>
<td>7</td>
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<td>-</td>
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<tr>
<td></td>
<td>L</td>
<td>-</td>
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</tr>
<tr>
<td></td>
<td>K</td>
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<td>9</td>
</tr>
<tr>
<td>Southwest Bay</td>
<td>L</td>
<td>10</td>
<td>9</td>
</tr>
</tbody>
</table>

Table 2 Results from partitioning the chemical analysis results into groups by automatic clustering. Groups 10, 9 and 8 are for all data combined. Groups 6, 5 and 4 are results from separately clustering the coarse and fine fractions.
tions in its dispersal over time, this approach applied to hundreds of sherds should provide a better basis for understanding both Manus and regional prehistory.

ACKNOWLEDGEMENTS

The PIXE-PIGMÉ analyses were undertaken at ANSTO, the Australian Nuclear Science and Technology Organisation, Lucas Heights, Sydney with assistance from AINSE, the Australian Institute of Nuclear Science and Engineering. I am particularly grateful to Peter Duerden and Roger Bird of ANSTO for their long term interest in the application of nuclear methods to archaeology, and their contribution in arranging access to the Lucas Heights facility and providing the chemical analyses for the samples used in this paper. Alan Watchman provided thin section identifications on several potsherds used in this study; I.E.M. Smith of the University of Auckland provided x-ray diffraction analyses on some of the fine grain separates used here.

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TROPICAL POLYNESIAN PREHISTORY – WHERE ARE WE NOW?

R.C. Green
Department of Anthropology, University of Auckland, Auckland, New Zealand

The modern era of archaeology in tropical Polynesia began in the 1950s at about the time that an earlier decade of work in New Zealand was being brought to completion through publication (Duff 1942, 1947, 1950; Lockerbie 1940, 1959). It started in 1950 excavations by Kenneth Emory in Hawai`i (Kirk 1985:15; Emory et al. 1959). It continued with the surveys and excavations of the Heyerdahl Expedition of 1955-56, principally on Easter Island, but also on Pitcairn, Raivavae, Rapa and the Marquesas (Heyerdahl and Ferdon 1961, 1965), with Suggs’ 1956 and 1957 excavations in the Marquesas (Suggs 1961), and with those of Golson in Tonga and Samoa in 1957, ending with a mound excavation in Tonga by Golson’s associates, H. and L. Birks, in 1959 (Golson 1959:10-11, 1962a). This decade in tropical Polynesian prehistory was very adequately summarised by two people, one focussing on Polynesia (Suggs 1960) and the other tackling the whole South Pacific (Golson 1959). The two perspectives were remarkably different: the one being popular, broad ranging, speculative – a synthesis drawing on linguistics, cultural anthropology, tradition, and ethnography to augment a sparse archaeological record; the other being largely archaeological in content, more descriptive of results and fairly cautious in its generalisations. In addition, Golson (1961) published a long critique of Suggs’ book which, not unexpectedly, drew a suitably robust rejoinder (Suggs 1963).

These events culminated during the 1961 Pacific Science Congress in Honolulu, Hawai`i. The research of the previous decade was summarised there in a series of papers (Green 1961) and, equally importantly, the possibility of a co-ordinated archaeological programme in the Pacific area was given concrete expression in a resolution adopted by the Congress. It was backed up by the development of an overall plan in which a detailed prospectus was drawn up for Micronesia, Melanesia and Polynesia, each setting out desirable research projects and priorities (Green 1961:479-80); that for Polynesia was implemented through a multi-institutional three-year research programme (News Bulletin 1962). This set the stage for the next two decades in Polynesian archaeology excluding Hawaii and New Zealand, which had their own local institutions and research agendas. Jack Golson was prevented from attending the Congress by the denial of a U.S. Government visa (Groves 1961:484). His paper was presented by a student, John Matthews. Golson’s involvement with tropical Polynesian archaeology was largely indirect, but his influence was felt at that time through people such as myself, and thereafter through students such as Poulsen, Groube and Davidson.

In 1958 my mentor, Douglas L. Oliver, enticed me away from Southwestern and Mesoamerican archaeology into the Pacific. On the way to Tahiti I was directed to Hawai`i to excavate briefly with Kenneth Emory and his young Japanese archaeological associate, Yoshihiko Sinoto on Kaua’i. A Fulbright Scholarship then permitted me to work with Jack Golson in New Zealand for nine months, before striking out on my own in Mangareva and the Society Islands. It was Golson, therefore, who helped to lay the basis for my knowledge of Polynesian and Oceanic archaeology in much the same way Oliver had done for its ethnology and social anthropology. That rewarding experience with Golson was the source of a heritage which has surfaced time and again in various forms over the years. I know of no better position from which to assess where we are now in tropical Polynesian archaeology than to use the baseline provided then, for myself and for many others. It had strengths and weaknesses, but proved to be something on which people could and did build.

GOLSON AND MODERN TROPICAL POLYNESIAN ARCHAEOLOGY

Fieldwork

Golson’s field experience in tropical Polynesian archaeology was brief, most of it during an initial 1957 expedition to Tonga (six weeks)
and Western Samoa (five weeks). Detailed publication of the Tongan work by Golson or the Birks, has never appeared. But those efforts formed a secure basis for the more extensive research of his Ph.D. student, Poulsen (1967), the revised report of which Golson laboured over at length before its final appearance (Poulsen 1987). Details of the Samoan work did appear (Golson 1969a, 1969b). Moreover, soon after my arrival in New Zealand in 1958 Golson spent a slide evening reviewing what he had done in Samoa, although at that point I understood only a little of its significance. However, under Golson's continued tuition, its importance became clear enough to inspire a 1960 return to the USA from Tahiti by way of Western Samoa with the intention of assessing that group's archaeological potential at first hand. Subsequently it formed the basis of a proposed three year programme in Western Samoa (Green 1961:481).

Golson's pioneering efforts in Western Samoa provided a foundation from which one of the better known Polynesian archaeological sequences has been built (Green and Davidson 1969, 1974a; Jennings et al. 1976; Jennings and Holmer 1980). Furthermore, those investigations involved extension of the excavations on the one site he had initially dug and dated. Here it should be noted that Golson's methods of excavation influenced what he conducted by myself, Janet Davidson, Les Groube and others. It was one legacy of our New Zealand experience, repeated on Moorea in the Society Islands, and again in Western Samoa, and by Groube in Tonga. As Golson (1986:3) said 'a top priority was to spread the message of stratigraphic excavation and to train people in its procedures'.

Interpretation of the field evidence

Golson's enduring contribution from his direct involvement in Western Polynesian archaeology was the recognition that the undecorated, crude, thick, coarse tempered pottery from early contexts in Western Samoa was in fact related to the thinner-walled, finer tempered plain ware of Tonga, which was itself securely associated with dentate-stamped decorated sherds (which he initially referred to as possessing a distinctive kind of incised decoration). He (1959:36-8) correctly correlated these with the early C14 dated sherd assemblages from site 13 in New Caledonia, and the undated pottery from the Vatcha site on the Ile des Pins (where he was later to excavate), the Rakival site on Watom Island off New Britain, the sand dune site of Sigatoka in Fiji, and McKern's previously published decorated pottery from Tonga. These comparisons allowed him to refer to 'some early community of culture' over the area (Golson 1962a:176), which preceded its later division into Melanesia and Polynesia. This breakthrough subsequently led to the recognition of the Lapita cultural complex (Golson 1971). Its Eastern Lapita section is now seen as the foundation culture of Fiji and West Polynesia out of which Ancestral Polynesian society developed (Kirch 1984a:41-69). As Golson (1959:38) noted with some vision the use of pottery as proof of the existence in the Pacific of wide cultural relationships is one of the principal results of the archaeological work carried out in Oceania during the last ten years, and shows what can be expected of this method in the future.

General theoretical and methodological viewpoints

Golson (1986:3-4) relates in a Festschrift article for John Mulvaney, that they both were true to their background and very strongly artefact-oriented. Thus it was through repeated associations of formal types that they expected to define cultural units whose prehistoric life and habitat they would then flesh out from other evidence. And in this context, as in most of his writings, 'artefact' typically means portable artefact, as this paragraph makes clear: The deceptive character of excavations carried out in Tonga and Samoa, especially with regard to artifacts, is not entirely due to Golson's very short stay. Neither in Tonga or Samoa do the contours, anymore than the geological structure, ever make up sites likely to reveal layers rich in artifacts. In the two groups the most numerous and striking sites are monuments whose type and size are of such a nature as to discourage or prevent excavation ... All this evidence written in the earth or the countryside obviously has a value equal to that of artifacts as far as the reconstruction of modes of life is concerned. And it is certain that it raises very interesting problems from the point of view of adaptation to the local environment and changes in the density and distribution of settlement. The research on artifacts which is so necessary must not make us forget these problems [Golson 1959:27].

In sum, a Childean concept of an archaeological culture as a recurrent assemblage of distinctive portable artefact types was fundamental. In contrast, the reality one faces is the total absence of pottery in many sites throughout tropical Polynesia, only limited amounts in a few early sites in East Polynesia and more in those of West Polynesia. When this restriction of ceramics to a few early sites is added the general prospect of small assemblages of adzes, fish-hooks and other formal items in sites of all ages
and types, it meant that portable artefact definitions of cultures were seldom going to provide a productive approach, as Golson himself (1986:2-5) was to recognise.

What was required, where portable artefacts were few, was the treatment of sites and their internal features as artefacts, and not simply as containers for portable items. This was the advantage of the settlement pattern approach, because its theoretical and methodological strategies corresponded more closely to surviving archaeological records. It also explains why there is currently a more complete cultural sequence for 3000 years of settlement in Western Samoa with a strong structural representation over the last 800 to 1000 years, and (except in Niuatuputapu) a pottery-based sequence for the first 1000 years of Tongan prehistory with only minimal archaeological definition of the last 1800 years (Davidson 1979:95, 102), until the recent thesis of Spennemann (1989b).

However, the comparative approach to portable artefacts was not without its benefits. Many of us acquired our basic knowledge about Polynesian adzes from Golson; the same applies to Polynesian fishhooks, about which I had begun to learn while in Hawaii. Both of these subjects often featured in his lectures as well as his writings (Golson n.d.). The theory (Golson 1959:17-9) that Eastern Polynesian adzes had different Asian origins, rather than being derived from those of Western Polynesia, has proved untenable. But again, Golson's carefully-posed theoretical alternatives were used by a number of us who continued working in this field. In the comparative discussion of pottery, adzes, fishhooks and other items of Polynesian and Pacific material culture, Golson was always in his element and a great many of us owe a lifelong interest in these matters to him, even though portable artefact studies unfortunately seem to have lost their attraction for today's students.

That Golson could handle structural evidence in a similar fashion is illustrated in tropical Polynesia by his revision of the Easter Island sequence (Golson 1965). There he inverted developments in the architectural sequence based on the image ahu, so the most elaborate forms appear toward the middle of the sequence and not at the beginning. Subsequent investigations strongly supported his interpretation (Mulloy and Figueroa 1978:122-37).

In his review of Suggs' book, Golson (1961:499-502) addressed two other theoretical issues in Polynesian prehistory. One was the proposition that Western Polynesia served as the 'Hawaiki' or dispersal point for all Polynesian cultures; the other was that Polynesian cultures derived from an early form, then poorly understood, present 2000 years ago in the Marquesas and in Tonga and Samoa at that period or earlier. As with the interpretation of a second Asian source of adzes discussed above, he was essentially concerned with 'cultural contact or secondary settlement within Eastern Polynesia after the initial period of colonisation' (Golson 1961:502). On both these points further evidence has altered our understanding (see below). Today Golson's reluctance to accept Suggs' two propositions without sufficient supporting evidence from archaeology has largely been overcome as the evidence in several fields, including archaeology, has accumulated, making possible an interpretation within a single unified framework.

Golson's other contribution to Polynesian prehistory was his editorship and subsequent revision of a volume on Polynesian navigation provoked by Sharpe's theory of accidental voyaging (Golson 1962b, 1972a). This controversy, which is fundamental to any theory of settlement for the far-flung islands of Polynesia, has swung from an initial pole of deliberate voyages of migration to whatever extent and in whichever direction was required by the comparative evidence, to an opposing one of almost random one way voyages subject only to the patterns of winds, currents, and island position. Recently it has stabilised around the concept of navigated voyages eastward into the wind as the safest and most effective strategy of exploration. This provides both for a return to base and the potential for subsequent settlement of newly found land (Irwin 1989, 1990). Once again Golson was an early participant in a debate centred on a theoretical issue of great importance in Polynesia, one only now approaching resolution, largely through the experimental efforts of Finney and his associates (Finney 1979, Finney et al. 1989) as well as the more wide-ranging theoretical analysis of Irwin (in press).

**CHANGES OVER THREE DECADES OF TROPICAL POLYNESIAN ARCHAEOLOGY**

On re-reading Golson's two overview papers (1959, 1961), it is evident that in the three decades since 1960 we have come a long way in our knowledge of and approach to tropical Polynesian archaeology, much further than I, at
least, had previously appreciated. Some of the older issues remain, others have now been satisfactorily resolved, and there is a great deal that is new and challenging. We have largely put aside the long-standing concern with the immediate origins of the Polynesians and moved away from an undue interest in the precise internal order and direction of the settlement of particular islands or island groups. The problems of sequence building, on which so much depends, are still with us, but the record is far more detailed and the coverage much wider. While there is still a need to fill in the gaps, the questions currently being explored in depth encompass a whole new range of topics: human and natural impacts on fragile ecosystems, reconstructions of production, subsistence and economic systems, settlement patterns and spatial studies, social and political developments towards new levels of complexity, the role of specific evolutionary models for explaining stability and change, and aspects of palaeodemography (Kirch 1982a, 1989). Moreover, in the consideration of these subjects, most investigators today explicitly, and often in the context of formal models, address a range of processes they believe on theoretical grounds to be operating in their explanations of what happened and why in tropical Polynesian prehistory (cf. Irwin 1990).

Origins

Golson correctly anticipated that there was an underlying cultural entity out of which the various Western Polynesian societies developed, but he remained ambivalent about an almost complete derivation of the early culture of Eastern Polynesia from that of Western Polynesia. Instead he posed the possibility that some of that region’s adzes, fishing gear and other items may have had a separate secondary source in Southeast Asia (1959, 1961, n.d.; Green 1961:477). Two of Suggs’s propositions which Golson queried (1961:499-502) [Western Polynesia as the dispersal centre for all Polynesian societies, and all Polynesian cultures derived from an early form] are now rather better supported through archaeology than Suggs (1960) was able to achieve. Suggs (1963:35) largely drew on linguistic evidence. Green (1971) has made the case using adzes and Kirch (1980) for fishhooks, for example. Recent discoveries in the To’aga site, American Samoa (Kirch et al. 1990:11-12) give the necessary definition of early Western Polynesian fishing gear with parallels in Eastern Polynesia which also has appropriate antecedents in the Lapita cultural complex. Moreover, the basis of Suggs’ and Sinoto’s Marquesan case for a direct West Polynesian origin have been critically examined by Rolett (1989:74-5) and the alternative of one in East Polynesia advanced (1989:96-7).

As a result of these and other studies, the prevailing model sees the development of Ancestral Polynesian society occurring within Western Polynesia from an antecedent Eastern Lapita cultural complex. Migration of Polynesians with their distinctive culture from elsewhere in Oceania or Southeast Asia, much less the New World, is no longer creditable. Rather, the basic Polynesian culture is viewed as having developed in place (Emory 1959; Green 1967; Groube 1971; Kirch 1984a:41-69) with little or no input from non-related sources. Sufficient explanations are currently found in the inheritance of some of its content from a Lapita ancestor, the loss of a number of items and the replacement of or addition of others, with newly innovated forms. In the course of these developments, longer term, functional and stylistic changes are also evident, especially in the process of adaptation to a truly Oceanic volcanic high island and atoll world. The rather different Lapita cultural complex is now seen in most models as forming the colonising society of Remote Oceania, including Fiji-West Polynesia.

Remote Oceania carries a similar burden as the foundation proto-language associated with Lapita, out of which Proto-Central-Pacific and then Proto-Polynesian derived (Pawley and Green 1973, 1984). In this model, until recently, only the biological origin of the Polynesians posed a problem. This was because some analysts denied that Polynesian populations exhibited biological characteristics traceable to any of the diverse populations of ‘Old Melanesia’, which they regarded as forming a single basic, long resident and unrelated biological entity (Green 1989). Now this aspect of the puzzle too seems capable of resolution, (a) because it is increasingly evident that a number of different biological populations of various ages are likely to be identifiable in the area of Near Oceania (Green 1989; Stoneking and Wilson 1989:233-5 and Fig.2.7), and (b) because there is a reasonable case for the derivation of Polynesians from a late paleo-population associated with the Lapita cultural complex in Near Oceania (Houghton 1989; but also Pierusewsky 1989). To relate Polynesians to certain paleo-populations at one time resident in parts of Melanesia, however, requires allowance for sampling error, because the founding populations of Polynesia were very small and thus not fully representative of their parental
population. Small size of founding populations is assumed by the theories of Terrell (1986) and Houghton (1989) and attested by recent mtDNA studies (Hertzberg et al. 1989). The unique Polynesian body form also requires recognition of the effects of selection, one proposition being that it favours survival on long sea journeys (Terrell 1986; Houghton 1990, in press).

Attempts to specify fairly exact sequences for the settlement of particular islands or island groups (often in the form of Fiji, Tonga and Samoa in Western Polynesia and then of directional arrows for Eastern Polynesia), along with the designations of certain islands or island groups as primary dispersal centres (Tonga, Samoa, Marquesas, Society Islands), are now being replaced by an alternative model. This sees the settlement of the entire region of Fiji/West Polynesia as a more or less contemporary process in which any attempt to specify order more precisely is an unproductive exercise, given the present state of knowledge and dating techniques. Thus the A to B to C model has given way to the model of an early widespread settlement of the region followed by slow differentiation within it of those socio-cultural entities we employ in discussions today (Green 1981).

In the new model, the Western Polynesia 'homeland' is a region not an island or island group, and settlement of Eastern Polynesia is from somewhere within it rather than from a particular island, or even a group such as Samoa. Similarly, the Eastern Polynesian 'homeland' becomes a fairly quickly settled zone from the Southern Cook Islands through the Australs and Society Islands to the Marquesas (Davidson 1981:5, Map 1; Green 1983; Kirch 1986) rather than an individual island much less a group such as the Marquesas or the Society Islands, none of which are likely to have represented prehistoric political or other social units. The Hawaiian group may well have been first settled from the Marquesas (Kirch 1985:65-6). However, it is no longer possible to argue that either the Society Islands, the Southern Cook Islands or even the Marquesas constituted the main source for the settlement of New Zealand (Davidson 1984:23-4), or that it is even a very important exercise archaeologically to specify one or the other of these regions as the general homeland (Walter 1990:317-8). Rather, the region of central Eastern Polynesia as a source will do for most purposes. And finally, it is extremely doubtful that Easter Island was settled from the Marquesas; a more southerly origin in the Austral, Mangareva, Pitcairn region is far more likely both culturally and in terms of known voyaging possibilities.

Island sequence building

By the 1961 Pacific Science Congress, tropical Polynesian island sequences, only partially known to and reviewed by Golson in 1959, had been published for some of the Hawaiian Islands [based predominately on changes in fishing gear] (Emory et al. 1959), for Easter Island [based on developments in the architecture and function of religious structures (ahu)] (Heyerdahl and Ferdon 1961), and for Nuku Hiva in the Marquesas [based on a range of portable and non-portable artefacts] (Suggs 1961). Since then the sequence in the Marquesas has been further documented for a number of other islands in the group and expanded to include economic data (Sinoto 1979a; Kirch 1973; Rolett 1989:63-118). In a similar fashion the Hawaiian sequence has been extended to cover the entire chain of islands in that group and filled out to include change not only among several kinds of portable artefacts, but also in a whole range of structural forms, as well as in aspects of both land and sea based subsistence systems (Kirch 1985:284-308 and Fig. 239). Data on the Easter Island sequence today include not just better information on the development of its religious and associated image statues, but also on many other aspects of its culture from fishhooks to rock art (Ayres 1973, 1981; McCoy 1979; Van Tilburg 1986; Van Tilburg and Lee 1987).

Useful archaeological work has by now been done on a great number of the islands of tropical Polynesia, including the very small and remote, with nearly all investigations recovering some remains of prehistoric settlement. Despite these efforts, reasonably complete sequences exist in Western Polynesia only for the Samoan Islands (Green and Davidson 1974b; Jennings and Holmer 1980; Clark and Herdrich 1988; Kirch et al. 1990), for Niuatoputapu (Kirch 1988) but not Tongatapu or Ha'apai (Poulsen 1987; Dye 1987) in the Tongan group, and for Futuna and Alofi (Sand 1990; Frimigacci et al. 1988). A little is now known about Uvea (Kirch 1976; Frimigacci et al. 1982, 1983), Tokelau (Best 1988) and Tuvalu (Takayama et al. 1987), but much remains to be done in those groups.

In East Polynesia the Austral Islands and Tuamotus remain particularly poorly served by sequence oriented archaeology. For example little after Heyerdahl has been done beyond the definition of the Classic or Proto-historic period on Rurutu (Verin 1969), and in the Tuamotus a series of reports deals largely with late period
sites. Surprisingly the sequence for the Society Islands is not as well documented as might be expected, despite Golson's (1959:48) recognition of the need for such a sequence, and his anticipation that work there would lead to the discovery of an ancient layer in the islands of Tahiti which contains Western Polynesian adzes and pottery. What we currently have in the Society Islands is little more than a rather undifferentiated 'Period of the Marae' dating from after AD 1200 (Garanger 1967:387). The numerous marae and associated structures are preceded by three earlier Leeward Island sites, one a burial ground (Emory and Sinoto 1964), and another an important and now waterlogged village (Sinoto and McCoy 1975; Sinoto 1979b). These have, in contrast to later sites, assemblages of portable artefacts assignable to an earlier phase of East Polynesian culture and dating from AD 800 to 1200 (Sinoto 1983). In addition there is an unpublished sequence for Mangareva dating from AD 1200 (Green ms). Finally, the beginnings of a sequence are now emerging from the Southern Cook Islands (Bellwood 1978; Walter 1990:296-9; Allen and Steadman 1990), but as in the Society Islands, it still does not go back before AD 800.

Augmenting sequences from triangular Polynesia, are some from Polynesian outliers, especially Tikopia and Anuta (Kirch 1984b) but also Rennell (Chikamori and Takasugi 1985), plus an unpublished one from Taumako (Leach and Davidson n.d.). All raise the question of when in the sequence it becomes culturally Polynesian. One has to determine whether this status was reached by an addition to and dominance over a preceding cultural tradition, a replacement in large part of the earlier society and population, or simply impacts on particular aspects that lead us to classify it as Polynesian (Davidson 1974a).

Such distinctions are not easily made on the basis of a limited range of items which constitute the archaeological record of these small islands. Several interpretations are possible for each case, and several may apply. One is of early settlement from Western Polynesia, with subsequent contacts by already neighbouring cultures, and further contact late in the sequence by additional settlers from Polynesia. This is the current interpretation for Tikopia (Kirch and Yen 1982:337-8; Kirch 1988:188-9). Others involve additions with or without dominance by the incoming Polynesians. Still others may reflect total replacement of the previous inhabitants by them, perhaps because the island was abandoned for a time as is possible in the Anutan case (Kirch 1982b; Feinberg 1989:311).

A conclusion from the above overview is that with the partial exception of Tonga, and possibly the underwater Ferry Berth site in Samoa (Leach and Green 1989), founder period sites are almost unknown in most of tropical Polynesia. Even sites of the settlement period, defined by Rollett (1989:67-8) as those transitional sites exhibiting the initial changes inherent in the colonisation process during which settlers adapted to new environmental and social conditions, are relatively few in number. Examples could certainly be cited from Tonga, Samoa, Futuna, the Marquesas, Hawaii and perhaps Easter Island, but in most cases, it would be as one or two assemblages only from individual sites on a particular island which were identified. In Tongatapu, however, because of the highly visible nature of sites with pottery and an established sequence of ceramic change in these assemblages (Poulsen 1987), it is possible to see a few early sites clustered on the old beach ridge around the lagoon, a wider spread of coastal middle ceramic period sites, and then in the late ceramic period the distribution of sites over the entire island (Spennemann 1987:81, Fig. 1). Despite a paucity of coastal examples, the same spread of later ceramic sites inland after some 800 years of settlement is also documented for Western Samoa (Davidson 1974b:161-2, 1979:94-5), and interestingly is traceable in Fiji on the island of Lakeba (Best 1984:557-64) and slightly later in Beqa (Crosby 1988:219-26).

The implications of this are that a series of major coastal settlements prevailed in tropical Polynesia for some 800 to 1000 years following its initial habitation. During this period people moved to other coastal locations and indeed other islands. Thereafter populations became more widely distributed, often through rather more dispersed homesteads inland. This viewpoint has been explored in depth by Walter (1990:248-66, 298-315). The available evidence suggests (a) that the timing of these events was different in West and East Polynesia (1000 BC to 200 BC versus 200 BC to as late as 1300 AD), (b) that founding populations were always small (in the 50 to 100 range rather than 500 or more), and (c) that the annual growth rate was low and doubling times lengthy. A considerable interval passed before sizeable populations were reached. As a result the discernible impacts on the environment were patchy, and a continuing settlement process, especially in more marginal regions of larger islands and island groups, extended over time.
Finally, it should be noted that four different types of sequence building have been employed in tropical Polynesia. Kirch (1982a:71-2, 1989:28-31) identifies these as (1) a widely applied typological period approach (usually portable artefact oriented) that concentrated on stylistic change in a small range of items, (2) the use of more broadly conceived developmental sequence models in the Marquesas (Suggs 1961), Easter Island (Ayers 1973:120-34) and Hawaii (Cordy 1974; Hommon 1986; Kirch 1985:284-308; Dye 1989; Spriggs 1989), and (3) the construction as in Tikopia (Kirch and Yen 1982:317-34; 350-61) and Niutatoputapu (Kirch 1988:239-60), of a phase model similar to, but more inclusive than the typological one. In the last approach this is combined with a separate examination of trends, not always matching changes in the temporal phases, which occur in the various subsystems such as subsistence and population growth, and in environmental and socio-political transformations.

The fourth approach has only been attempted in Western Samoa. It resists the notion that in most tropical Polynesian sequences there are a series of readily identifiable stages, phases, or periods representative of closely correlated temporal changes in a range of portable and non-portable artefact forms reflecting different cultural subsystems (Green and Davidson 1974b:213). Rather, it stresses the basic continuity evident in the sequence (Green and Davidson 1974b:224) within which change occurs first in one aspect of culture then in another and not always in a causally related fashion. That approach, of course, requires a narrative account of the various changes and when they occurred, more like the various trends outlined by Kirch for certain subsystems in the examples from Tikopia, Niutatoputapu and Hawaii, and a variety of explanations for their occurrence. So far this probably more realistic but more challenging approach has not found much favour or application in Tropical Polynesia, not least because it is more difficult to teach or remember. However, in my view it may assist us in moving from an overly technological-economic-environmental and demographic orientation in sequence building to narratives exhibiting a deeper concern with identifying major shifts in the socio-political aspects of the culture, especially where these reflect new levels of organisational complexity. Such a narrative outline and exploration was attempted for the events in Makaha Valley from its initial coastal settlement, through a shift to an *ahu pua‘a* form of structure, followed by the integration of that unit into a broader and more complex Hawaiian polity (Green 1980:72-7).

### Human and natural impact on fragile ecosystems

Although the theme of human and natural impact on Pacific Island ecosystems had a thorough airing at the 1961 Pacific Science Congress (Fosberg 1963), it was not really until the late 1970s that the theme became a dominant one for archaeology in tropical Polynesia. Kirch (1984a:123-50, 1989:38-40) reviews this theme under two headings. The first is transformations of the environment wrought by natural processes, such as the short term impacts of droughts and cyclones and the longer term ones of tectonic events and climatic change. The second embraces human influences, by introduction of exotic biota to remote and fragile island ecosystems, by land clearance and deforestation, resulting in erosion and deposition, and by faunal depletion often resulting in extinction. Under the first heading, the effects of tectonic change are most dramatically demonstrated on Niutatoputapu (Kirch 1988), but also may apply to the early sites on Tongatapu (Spennemann 1987) and the Ferry Berth site on Samoa (Leach and Green 1989). The difficulty is separating these from regional changes in late Holocene sea levels (Clark 1990). This topic, of considerable concern in the recent non-archaeological literature, has important consequences for atolls and for identifying earlier shorelines on volcanic high islands. Under the heading of human influences, the role of inland erosion and coastal deposition on some volcanic high islands is only now receiving the attention it deserves, while the depletion and extinction of the bird fauna, long evident in New Zealand before being attested for Hawaii, is today recognised as a general phenomenon both in East and West Polynesia (Steadman 1989a, 1989b). The most outstanding case of deforestation with damaging consequences for the inhabitants, of course, is that documented for Easter Island (Flenley and King 1984; McCoy 1979; Kirch 1984a:264-78). The best examples of human influences on island building are known from the outliers of Kapingamarangi, Nukuoro and Tikopia (Leach and Ward 1981; Davidson 1971; Kirch and Yen 1982).

### Production, subsistence and other economic systems

Surprisingly, in view of the record in New Zealand, the recovery of evidence bearing on marine and land-based subsistence systems in
tropical Polynesian archaeology has been very uneven. Thus, while we have fully summarised sets of data reconstructing changing trends in maritime and horticultural components throughout the sequences of Hawaii (Kirch 1985:199-236) and Tikopia (Kirch and Yen 1982:274-310), elsewhere the analyses providing the basis for such syntheses are frequently insufficient for the task. In Western Polynesia a reasonable amount of evidence exists for the ceramic stage sites of Tonga. This reflects predominately inshore fishing in Niutopatapu (Kirch and Dye 1979; Kirch 1988:221-5) and lagoonal and reef shell-fishing in Niutopatapu (Kirch 1988:225-33) and Tongatatapu (Spennemann 1987, 1989a), the latter situation being affected by both predation and tectonic change. Comparable information relating to later periods, however, is sparse. For Samoa some less extensive but useful analyses of fishing and shellfishing are available for assemblages in association with Polynesian plainware ceramics (Janetski 1976, 1980; Kirch et al. 1990:10-11), but as in Tonga, the retrieval of samples for the later periods has been minimal.

In both island groups archaeological evidence permitting the reconstruction of horticultural practices has surfaced only on odd occasions, usually as garden soils under mounds and terraces or in the use of certain soil types. Thus most statements about the horticulture are inferential and dependent on food preparation tools, fermentation storage pits, or the recovery of pig and chicken bones. Even in Futuna where the ethno-archaeological potential is great because of its irrigated pond-fields (Kirch 1978; Frimigacci et al. 1988), the archaeological documentation of the development of these gardening systems is just beginning to emerge (Kirch 1976:47-9) although a time depth of more than 1000 years is indicated (Frimigacci et al. 1988:14). As Kirch (1988:281) remarks of the Niutopatapu sequence, reconstructions of developments in the prehistoric production systems of Western Polynesia 'depend more than one might wish on indirect evidence'. In fact, I see the information available for monitoring substantive changes in both land and sea subsistence strategies during most periods in any of these island groups as thoroughly unsatisfactory.

In Eastern Polynesia the situation is slowly improving in respect of the maritime component of the subsistence base. More recent Hawaiian studies have moved from the use of fishhooks, largely for chronological purposes, to establishing an improved understanding of the relationship between the fish catching technology and the fish being caught (Kirch 1985:207-11; Goto 1990). This technological-ecological approach has been extended to the Marquesas by Rolett (1989:203-87) with excellent results for a Tahuata island sequence dating from AD 1000, where change toward more inshore fishing without hooks appears late in the sequence. It also includes an analysis of the much less important molluscan contribution to the diet (Rolett 1989:185-98). Outlines of fishing and shellfishing practices are also beginning to appear for some of the earlier sites in the Southern Cook Islands, one on Mauke (Walter 1990:217-23) and the others on Aitutaki (Allen and Steadman 1990:30-3; Allen and Schubel 1990). Finally, there is an important analysis, indicating that in certain appropriate situations 'hunting' for pelagic gamefish was a fishing strategy responsible for some 42% of the fish at the early East Polynesian village site on Huahein in the Society Islands (Leach et al. 1984). However, archaeological remains indicating exploitation of marine resources during later periods in the Society and Cook Islands are still very poorly attested. In contrast, for Easter Island most of the information on the dietary contribution of fishing and shellfishing stems from the last 500 to 600 years in the sequence (Ayers 1981, 1985).

Although direct archaeological evidence of gardening systems has been recorded during settlement pattern surveys of prehistoric structures in the Marquesas, the Society Islands and the Southern Cook Islands, it has not been systematically investigated through excavation. The development of these systems remains largely undated, and nowhere has the data on them been assembled in a really coherent fashion. Most of this evidence is assignable to the later parts of sequences in these groups. For the earlier periods, as in Western Polynesia, the scarce data derive largely from food preparation tools, pits and the bones of pigs, so that again most statements on this subject are highly inferential.

In summary, while the overall record allows one to say more than in Western Polynesia about changes in certain aspects of the economic basis of tropical East Polynesian societies during the middle and late parts of various island sequences, in general (except for Hawaii), the data are insufficient to carry the kind of interpretive load usually assigned to economic change.

An increasing necessity when reconstructing tropical Polynesian economic systems, is to examine assemblages for items indicating trade or exchange and to look at production centres for certain trade items such as adzes. Here the
necessary studies in the sourcing of volcanic glasses (Sheppard et al. 1989; Weisler 1990) and Oceanic basalts (Best 1984:399-407; Walter 1990:228-37; Clegholm et al. 1985) are just getting underway. In a similar fashion, the study of adze production at major quarries such as Ma'uke Kea in Hawaii (McCoy 1977; Clegholm 1982, 1986) or Tataga-matau in American Samoa (Leach and Witter 1987, 1990) have just begun to appear.

Settlement patterns and spatial studies

Settlement pattern and spatial studies in Polynesian archaeology have largely been applied to the late end of island sequences. Little is known about the settlements dating to AD 1300 or before except for one intensively studied 600 year old village on Ma'uke in the Cook Islands (Walter 1990). Overviews of tropical Polynesian settlement pattern studies have been summarised several times, most recently by Kirch (1982a:78-81, 1989:40-3) and Green (1984:60-3). Such studies in tropical Polynesia began in earnest in the 1960s and continued well into the 1970s, but except in Hawaii, have not been a prominent feature of the later 1980s.

Important studies published at the end of the 1970s included Makaha on O'ahu (Green 1980), Kawela on Moloka'i (Weisler and Kirch 1985), and the northeast coast of the big island of Hawai'i (Cordy 1981), all in the Hawaiian Islands. Similarly important studies on Samoa demonstrate continuity in spatial organisation over some 500 to 600 years and in the form of household units and their definition, in the impact of rank on the disposition of households along paths, and in the presence of wards within larger 'village' (nuku) units, even though nuku did not become tightly nucleated entities until after European contact (Jennings et al. 1982; Jennings and Holmer 1980; Davidson 1974c). On Easter Island most of this work following McCoy's (1976) definition of the household cluster, has centred on changing political organisation as revealed by religious structures (Stevenson 1986), images (Van Tilburg 1986) and rock art (Lee 1986) with only the occasional consideration of the overall pattern in a particular region (Ayres 1988), despite efforts to map the entire island (Cristino et al. 1981). In Tonga, where such studies are badly needed on most islands, the only effort made to date to investigate this field systematically has been on Niutatoputapu (Kirch 1988:37-78). Work is also ongoing at Mata'ire'a Hill in the leeward Society Islands and in American Samoa, but so far it has only been published in a preliminary form (Sinoto and Komori 1988; Clark and Herdrich 1988; Clark 1989).

Although the identification of household clusters has been an important aspect of most such studies, extensive excavations that would reveal the internal features and arrangements within these units are still extremely limited. Instead the focus has been either on the settlement pattern, subsistence, and ecological/environmental dimensions of the distributions of various structural monuments, site and site clusters or on the spatial arrangements among them which reflect social groupings and status differentiation, or on both approaches together. Recently, symbolic aspects of spatial patterning have also been considered as part of the Kawela study (Weisler and Kirch 1985), in a restudy of the 'Opunohu Valley data (Descantes 1990) and in a study of the Marquesan house (Ottino 1990). In my view, studies of what I would now call landscape archaeology, including all dimensions from the environmental to the symbolic, will be necessary for many years to come in tropical Polynesia, expensive though it may be of resources to undertake the intensive surveys and areal excavations demanded by this approach.

Social and political developments towards new levels of complexity

The analysis and explanations of socio-political diversity in tropical Polynesia has long attracted the attention of social and cultural anthropologists (Sahlins 1958; Goldman 1970; Howard and Kirkpatrick 1989:60-6; Marcus 1989). Yet, as Kirch (1989:44) notes, until recently 'few archaeological studies have explicitly addressed the problem'. A number of issues require resolution before real progress is possible. One is the variation within the category 'typical', 'traditional' or 'simple' Polynesian chiefdom, as the most common and widespread form in the region is often termed. A second is how these developments are to be distinguished from the more complex chiefdoms, both in Polynesia and elsewhere. Another is, of course, how such characteristics can be traced and interpreted from the archaeological record.

Given the variability now evident worldwide in chiefdoms, and the difficulties of specifying their defining characteristics ethnologically or archaeologically (Earle 1987:280-91), it seems most useful to employ the interrelationships between three criteria in their analysis. These are the scale of integration, the centrality of decision making and the degree of stratification. On this basis a case could be made that in Polynesia simple chiefdoms consist of interacting social
units that act as polities, which involve only small populations (less than 1000 people) spread over limited territories (a single valley or a coastal-inland wedge of an island on the order of 10 or so square kilometres) and exhibit a minimal degree of social and economic differentiation. In Cordy's (1985:160) terms these are societies with two major decision-making levels (chief-producer) or what Sahlin's (1958) called simple ranked societies with only two social strata (chief and commoners). In this sense, as Earle (1987:279) has indicated, 'the term chiefdom is used to characterize social complexity in stateless societies'.

In this analysis, complex chiefdoms, a widely used term (Earle 1987:280), or Cordy's (1985:161) complex societies, become those societies in tropical Polynesia acting as polities (a) whose populations number in the thousands or in tens of thousands, (b) which usually include in their territories hundreds to thousands of square kilometres, and (c) which exhibit greater differentiation in status, rank, access to resources and three or four decision making levels in respect to them. For Polynesia, most writers seem to agree (whatever their scheme) that at the time of European contact the polities in Hawaii, Tonga, the Society Islands and Samoa qualify as examples in which social and political developments had advanced in prehistory towards added levels of complexity. This increasing complexity is the problem most archaeologists in tropical Polynesia have tackled, usually in reference to Hawaii (Cordy 1981; Hommon 1986; Kirch 1985), on one occasion in Tonga (Kirch 1984a:217-42), and in one overall instance in relation to distance from nearest occupied island and angle of target island (Irwin 1990:92-3).

The difficulty is that in three cases (Hawaii, the Society Islands, and Tonga) the stimulus of European contact led to the establishment of true states with kings, resulting in what Marcus (1989:197) calls compromise cultures. These are the first long-term adjustments of Polynesian cultures to Euro-American contact, in which Polynesian versions of Western institutions were created and older institutions and customs were censored, reorganized, and retraditionalized (Marcus 1989:197).

The same could be said of the other Polynesian chiefdoms which did not turn into states. Work in this area requires sophisticated studies of change during the contact period on the part of ethnographers, ethnohistorians, historians and archaeologists to define the social and political complexity that obtained before contact. Thus for the Society Islands, for example, we have Oliver's (1974) excellent ethnohistory, Newbury's (1980) detailed historical account, and brief essays by Dening (1986) and his students (Dening 1989), but the archaeological correlates only from the interior of the 'Opunohu Valley on Mo'orea (Descantes 1990:92-116, 145-54). Still the evidence is there to begin to tackle the problem in Tonga, Samoa, the Society Islands, as it is in Hawaii (Kirch 1990) and New Zealand (Sutton 1990).

At the other end of the scale, and at the early end of most tropical Polynesian island sequences with their much smaller, and at colonisation probably very small, populations, another problem faces us. As Earle (1987:288) observes, many of the societies used by Feinman and Neitzal (1984) in their survey of sedentary pre-state societies in the Americas have been called chiefdoms because of their inheritance of rank. Yet their small population sizes, often well below a thousand, perhaps requires them to be considered not as simple chiefdoms but as 'tribal' variants on a local-group level. One Polynesian example is Anuta where a population, probably always less than 200 people, occupied an island 4 km² in size, all living in one coastal village in which two ariki (chiefs) headed up two of the four kainanga (patrilineal units), the other two having only appointed formal leaders (Feinberg 1981:134-92). Certainly, on the linguistic evidence, terms for concepts of hereditary ranking were retained and distributed throughout Polynesian societies and have an origin in Ancestral Polynesian Society (Kirch 1984a:62-7). Whether these settlement period societies are better considered local-group variants possessing terms and personages reflecting nothing more than hereditary ranking, or simple chiefdoms as defined by their scale of integration, centrality of decision making, and stratification, is a question we cannot yet answer. However, it is only by exploring such questions that prehistorians can contribute statements about the development of levels of social and political complexity in various tropical Polynesian societies.

Evolutionary models, stability, change and explanation

General evolutionary models of band, tribe, chiefdom, state, or stagal models like palaeolithic, neolithic, bronze and iron age, used in China and Southeast Asia, have never held much attraction for the prehistorians of Oceania. For Polynesia, in fact, they possessed almost no analytical value; at best all one appeared to have throughout prehistory were neolithic chiefdoms.
Other finer grained distinctions were therefore required, and various less grand stagal models have been applied. These often take the following form: Settlement (Colonisation), Developmental, Expansion, Classic (Proto-historic) and Historic periods. The difficulty is that most reflect an assumed linear progression toward increased levels of sociocultural integration which may not be fully warranted, with nearly all significant change occurring between one stage and the next. The Groube (1967) critique satisfactorily reviewed the implications of such models in New Zealand prehistory, but his comments apply equally to tropical Polynesia, as he well recognised.

Rather than trying to fit Polynesian prehistory into an overall framework involving a comparable set of stadial or stagal units for each region or island group, as has so often been the case elsewhere in the Old or New World, it is also possible to treat the whole region as a case of evolution at the specific level. The concern here is to view Polynesian prehistory as an historically identifiable case of divergence where numerous cultures or societies have arisen from a common ancestor. Such an analytical approach permits one to apply a conceptual framework, the phylogenetic model, tried in various parts of the world with similarly suitable conditions (Kirch and Green 1987). An appeal of this model is that it allows one to formally combine the data of archaeology with those of linguistics, biological anthropology, ethnohistory, and ethnology in a series of explicit steps. It requires one to take advantage of the strong evidence for cultural continuity so evident, often right to the present, in Polynesian island sequences and to include in one's historical reconstructions the rich data from a number of fields in addition to archaeology. Such cultural reconstruction allows one to apply what I have called the tripartite approach (Green 1986; Kirch 1989:44), one used by Kirch (1984a) in his book on the evolution of Polynesian chiefdoms. However, because it requires archaeologists to handle a wide range of data beyond that recovered from the earth, and because it includes the concept of cultural as well as biological evolution within its framework (although only at the specific level), it is not without its critics (cf. comments by various individuals at the end of Kirch and Green 1987:443-50).

What a phylogenetic approach to the prehistory of tropical Polynesian archaeology assists with is to make more explicit the mechanisms which have maintained stability and similarities and those which have promoted various changes. The theoretical strategy here is to move away from an emphasis on one or a very few explanatory factors. Thus one can trace a shift from single variables such as diverse origins and successive migrations, competition for differentially distributed resources, or status rivalry, to a range of general processes, each having different effects as first one and then another or several together are employed in the interpretation of various sequences which archaeologists outline.

The major processes identified to date (Kirch and Green 1987; Kirch 1989:45) have been (1) generational and lateral inheritance of cultural practices, or the persistence of ancestral patterns (homologies), (2) internal contact between groups within Polynesia or the transmission of cultural materials and ideas between different societies, (3) isolation, the reverse of contact and a strong force within parts of Polynesia (Irwin 1990), (4) founder effect or sampling error in cultural transmission resulting from the movements of small populations from one island or island group to the next in the course of settlement, (5) colonisation processes resulting from adaptation to new and frequently radically different environments, (6) long-term environmental selection often as a result of humanly induced as well as natural changes in the environment, (7) external contacts with non-related cultural systems (South America and the sweet potato (Yen 1974), Eastern Micronesia and some fishing gear (Takayama 1987)), (8) demographic factors promoting population growth, decline, or restriction, (9) intensification of production, (10) competition and conflict, and finally (11) innovation, sometimes only once with subsequent spread, and in other cases, several times, resulting in convergence (analogies).

Some of these mechanisms, when operating over time, serve as processes that provide explanations for the systemic cultural patterns seen ethnographically which permit us to speak of them as distinctively Polynesian. Others of the processes help to explain the numerous divergences within the systems, while still others seem to reflect what are viewed as parallel general trends which underpin some of the developmental or stagal sequences. Finally, a few of the outcomes may be explained by processes which give rise to similarities that are the result of convergence. All those mechanisms, of course, must be treated as interrelated and differentially operating over time (which is what transforms a potential means of change into a process). Still with such a list of variables seen to promote stability and change, examples of
which can be documented in one or more specific cases, explanations of what happened in Polynesian prehistory begin to take on a more explicit and potentially richer perspective than that offered by origins and migrations which dominated the period before 1960. Thus most interpretations in Polynesian prehistory today, whether in an evolutionary or some other conceptual framework, employ a much wider range of explanatory devices than previously.

**Palaeodemography**

Beginning in the 1970s, archaeologists seriously began to attempt reconstructions of population sizes and trends in tropical Polynesian prehistory. The first attempts usually focussed on the size of population at contact, as in the Cook Islands (Bellwood 1971), the Marquesas (Bellwood 1972:45-7), Tonga (Green 1975), and Easter Island (McCoy 1976:141-2). This was in reaction to the rather conservative estimates provided by McArthur (1967) as a correction to the generally unreliable first estimates found in the earliest ethnohistoric sources. The problem is still with us, as for example in the Marquesas where McArthur (1967:284-5 and Table 54) estimated 19,300 from the first reliable historical sources of 1840, while Bellwood (1972:47) calculated it to have been between 16,000 and 35,000 at the time of contact on the basis of archaeological data, and a demographer has recently suggested 43,000 for that period (Rallu 1989:131).

Another example is McArthur's (1967:247) estimate of 13,000 to 19,000 for Tahiti during the period of early European contact, compared with Oliver's (1974:34) even more careful appraisal of 35,000, both of which are now contradicted by Rallu (1989:132) who suggested the figure should be on the order of 70,000 inhabitants. Similarly massive revisions upward from 250,000 to 300,000 inhabitants usually used by archaeologists and others, have also been suggested for contact populations of Hawaii (Stannard 1989:5, 78-80). Archaeologists have also argued for much larger prehistoric populations for Tongatapu (Green 1973) and Western Samoa (Green and Davidson 1974c:281-2) than have been allowed by the demographers. Clearly it will be some time yet before estimates for tropical Polynesian populations at the time of European contact are generally accepted as reliable.

At the other end of the time scale there currently seems to be a consensus that 'initial colonization was always by relatively small propagules' (Kirch 1989:32). This agreement stems not so much from estimates derived directly from the archaeological evidence of founder and settlement period sites in tropical Polynesia (which, as noted above, is limited), as from the various voyaging and colonisation models employed by prehistorians (Brewis et al. 1990; Irwin 1990:93) which often tend to incorporate findings from the simulation studies of demographers (McArthur et al. 1976; Black 1978, 1980). These imply that only 80 or less people are required to found island populations and in the course of a few thousand years at very low growth rates give rise to populations of the size found at contact. Golson (1972b:29) made the point early on when he wrote

> a more reasonable overall estimate of 1% per annum would require only about 850 years to bring a founding population of 20 up to the 100,000 level, which is the highest estimate made by any observer for the Marquesas with its 1500 years of prehistory and in excess of the indigenous populations of all but the largest island groups today.

Given that it is possible to take an approximate founding population size, the duration of a sequence, and a reliable estimate of population at the time of European contact, then various hypothetical demographic pathways for island population growth are available. Kirch (1984a:103) lists and illustrates these alternative models as extinction, exponential, logistic, overshoot, oscillating, and step. Data from archaeology and from biological anthropology, especially skeletal evidence, may then be used to determine which of these models is more likely in any given case. Kirch (1984a:103) has taken the position that 'some form of sigmoid or logistic process probably characterized population growth on all Polynesian islands'. He used Hawaiian data, archaeological and skeletal, as one case supporting this viewpoint, although his methodology has now been criticised by Sutton and Molloy (1990). The analysis and interpretation of the leeward and other Hawaiian archaeological data have also been challenged by Clark (1988). In addition Sutton and Molloy (1990) have challenged three propositions that underlie the model favoured by Kirch for Polynesia, namely that population follows a sigmoid curve, that the form of the curve is density dependent, and that mortality increased as density rose. Elsewhere, on the basis of close study of the New Zealand skeletal evidence, Brewis et al. (1990:352) have calculated a population growth rate (r) of 0.00875, or less than 1% per year, but suggested that 'the overall trend of population growth was more sigmoid than linear'.

Kirch (1984a:222-3) has also calculated that with an even lower r of 0.005 and a starting
population of 100, a 'land full' situation would occur on Tongatapu between 300 BC and AD 700, in which most of the arable land under shifting cultivation would have come into agrarian use and homestead type settlement (see also Green 1973:73). In my view that situation would thereafter have favoured an oscillating population model for the later periods of Tongatapu's prehistory (Green 1973:62, 73), following a long early period of exponential growth. Finally, no one would contest the fact that in tropical Polynesia some of the 21 'empty and abandoned' islands, such as Pitcairn and Henderson, constitute cases of population extinction (Irwin 1990). It therefore seems probable in Polynesia that several of the possible models which Kirch has outlined may in fact apply, and each case will require close examination.

From the above outline, it is evident that palaeodemographic studies in tropical Polynesia at present provide a fairly controversial area of study into which archaeologists have only ventured in the last two decades. Here one is frequently reminded of the trenchant criticisms of McArthur (1974), which could well be taken as a counsel for despair were it not for the efforts of Spriggs (1981), based on archaeological data, that overcome many of her objections in the specific Anceityum case which both studied. In no area, from population size at settlement to size at contact, or the multiple pathways in between, are the issues settled, at least not by substantive evidence from the cultural and skeletal evidence which archaeologists recover. However, Golson's observation that reasonable growth rates lie in the region of 1% or less may well prove to be prophetic.

CONCLUSION

Jack Golson was able to summarise the information on modern archaeology in Oceania after its first decade in one article, devoting little more than half of it to tropical Polynesia. Three decades later there is no hope of full coverage of the subject even within the compass of an entire article restricted to tropical Polynesian prehistory. One has to confine discussion to a careful selection of themes which will provide some idea of where we are now. In the interim between Golson's article and the present, books, monographs and theses have been written on individual islands or island groups, as well as the whole area, and articles on the region's prehistory now number in the thousands. An immense data base has been built in just 30 years. Viewed from the perspective of the major themes discussed above, however, a limited number of conclusions would appear to be warranted.

1. The cultural and linguistic (and perhaps the biological) origins of the Polynesians no longer pose a significant problem. Nor is the order of their dispersion to individual island groups necessarily a particularly important question.

2. Geographic and temporal gaps in cultural sequences for islands and island groups can be specified without much difficulty. Moreover, appropriate programmes using proven field strategies which take landscape change into account can be directed to filling them.

3. Methods for outlining island sequences have moved away from the simpler typological and developmental stagal approaches - where most of the change occurs between periods - to more complex portrayals of change through time, although entirely satisfactory approaches to dealing with the complexity have yet to be developed.

4. Humanly induced as well as natural impacts on the ecosystems of tropical Polynesian islands are now widely recognised and frequently documented by archaeologists in conjunction with colleagues from other disciplines. More importantly, changes in the ecosystem are seen as playing an active role in the cultural transformations that occur through a process of continuing adaptation.

5. Reconstructions of the subsistence systems and productive economic activities for most island groups have less substance than they require for the interpretive load usually placed on them. Much more needs to be done in this area along the lines of the analysis undertaken in New Zealand and Hawaii.

6. If we are not only going to study changes in the environmental/technological/economic aspects of Polynesian cultural systems, but also attempt to reconstruct developments in socio-political and symbolic domains, then settlement pattern and spatial investigations of structures accompanied by adequate excavation, will have to remain a major focus of our research objectives, costly though this is. This is especially true for the later periods of most sequences, while for the earlier periods areal excavations of a number of large coastal settlements in each of the island groups are absolutely essential. Until then we cannot really address issues such as increasing social complexity, particularly in its early stages, in a satisfactory fashion.

7. Attempts to trace and interpret social complexity in the archaeological record require
definition of hierarchical scales in relation to habitations, settlements, religious monuments, burials and their associated constructions, and a range of other public features, as well as certain classes of portable artefacts. The need, of course, is for scales which will accurately reflect degrees of social integration, centrality and cooperation at different levels of decision making, and stratification and segmentation amongst social groups in their access to and use of resources. At present our models and measurements of variables in this field are rudimentary.

8. Few general or formal frameworks exist for dealing analytically with Polynesian prehistory. Those that do, however, propose that it include not just the data of archaeology, but also information from historical linguistics, biological anthropology, ethnography and modern ethnology. How "evolutionary" such frameworks should be is a matter for debate, although serious alternatives to a phylogenetic model, other than the usual rather descriptive culture historical narratives, have still to be formulated and applied to an evergrowing data base.

9. A reasonable outline of a set of mechanisms that operate over time as processes designed to account for stability and change in Polynesian cultural systems has been developed in recent decades and is now being employed in many specific instances. The use of these has moved most explanatory accounts away from simple statements relying on origins and migrations or other kinds of monocausal analysis that prevailed in the 1950s and 1960s to ones that stress the interplay of a number of variables.

10. Essential though it may be as one of the variables underpinning our understanding of cultural change in tropical Polynesian sequences, none of the current palaeodemographic reconstructions enjoy full acceptance. Rather, most issues in the field (except perhaps the small size of founding populations and low initial rates of population growth) are matters of contention. This applies whether one talks about sizes of an island's population at contact, changes to growth rates and pathways by which they were achieved, or the levels of fertility, mortality and methods of population control that were involved.

These ten assessments of the current situation would appear to constitute a useful set of conclusions from which to launch another decade or more of work in tropical Polynesia. Moreover, for myself, as probably for Jack Golson, it would appear that archaeology in this region has undergone a transformation which makes it as fresh and promising a field of investigation today as it was at the end of that first decade when he attempted to pull together the initial pioneering efforts of a modern era in the field.

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ISSUES IN NEW ZEALAND PREHISTORY SINCE 1954

Janet Davidson
Museum of New Zealand Te Papa Tongarewa, Wellington, New Zealand

New Zealand prehistory has to do with the land-fall of a handful of voyagers from tropical Eastern Polynesia in a temperate homeland radically different in size, environment and resources from anything they had previously known, highly differentiated within itself and as effectively isolated from the outer world as it is possible to imagine. Change in New Zealand prehistory concerns adaptation to new circumstances, the impact of organised human activities on untouched ecosystems and the effects of significant regional growth of population within a short timespan. (Golson 1986:5)

In the relatively short space of seven years, from 1954 to 1961, Jack Golson had a great impact on New Zealand archaeology and prehistory. He has been heard to say that seven years is an appropriate length of time to devote to a major excavation, from fieldwork through analysis to publication. His seven years in New Zealand, however, were devoted to pioneering on a number of different fronts, rather than to the completion of one or more excavation reports. It seems appropriate here to review some of the issues in New Zealand prehistory with which he was particularly concerned, or whose importance he foreshadowed or predicted. It is obviousl y not possible to cover everything in equal detail, so the discussion is selective. In this respect it reflects the biases of its author as much as those of the person in whose honour it is offered.

Golson's succinct summary of the business of New Zealand prehistory, quoted above, makes it difficult to see what some of the controversies of the past 40 years have been about. Yet controversies there have certainly been. Forty years ago, one of the main issues was whether change in New Zealand prehistory was largely or wholly internal, or whether it was stimulated by new arrivals. Much of the subsequent debate has centred on the impetus for change, the rate of change, and whether the most significant changes occurred in one place at one time, or separately. Of particular interest have been economic change (and particularly the changing importance of big game hunting and agriculture[1]); social change, as manifested particularly in the development of fortification-based or pa warfare; and change in material culture. At the heart of most issues in New Zealand prehistory has been the question of the nature of culture change.

NEW ZEALAND ARCHAEOLOGY IN THE 1950s

For New Zealand archaeology, the 1950s were a time of excitement, marked by the cross-fertilisation of ideas from European and North American archaeology with a long established indigenous New Zealand tradition (Green 1972). The decade began with a major landmark – the publication of Roger Duff's Moa-hunter Period of Maori Culture (Duff 1950), in which an archaeological definition of early Polynesian culture in New Zealand was presented, and the question of culture change in New Zealand prehistory, so fiercely debated in the 1870s and 1880s, readdressed.

With the partial exception of Otago, where H.D. Skinner had long held a joint Museum and University appointment in Anthropology, museums were the leading exponents of archaeological fieldwork, although 'fossicking' – ransacking sites in search of artefacts – was a flourishing and still respectable pastime for a wide cross-section of New Zealanders. The serious practice of archaeology in New Zealand, however, was dominated by the South Island museums.

From his base in the northern North Island, Golson therefore flung himself into research into a New Zealand prehistory almost totally dominated by a South Island perspective. His enthusiasm for this research soon highlighted two important requirements: the need for a trained labour force to carry out excavations, and the need to draw together the many interested amateurs in order to make their knowledge available and channel their energy in useful directions. The Auckland Archaeological Field Group and the New Zealand Archaeological Society of New Zealand were born.

[1] Helen Leach (1976) has shown that the term horticulture is more appropriate for this aspect of Maori activity.
Davidson

Archaeological Association [2] were the result. The Association held its inaugural meeting in August 1954 (Golson 1955a) and its first annual conference in May 1956 (Golson 1955b, 1956). Jack Golson was the foundation secretary/treasurer.

During his first years in New Zealand, Golson carried out an extensive appraisal of the North Island field evidence (Golson 1957a). In the summer of 1954-55 he began an excavation programme which continued every summer, often at Easter and May, and sometimes also at weekends, until Easter 1961. Even during the summer (1959-60) he spent in New Caledonia, the excavation programme he master-minded continued in the Coromandel. At Great Mercury Island, Taylors Hill, Sarah's Gully and Opito, Pig Bay Motutapu, Mount Wellington, and Kauri Point he insisted on standards of excavation, recording and recovery previously unknown in New Zealand. In a recent tribute to John Mulvaney, he described the background of British archaeological scholarship from which he came and noted the need to 'spread the message of stratigraphic excavation' (Golson 1986:3). If the theoretical frameworks have changed dramatically since the 1950s, few today would disagree with the requirements for precise excavation techniques and interdisciplinary cooperation to expand the frame of interpretation (Golson 1986:2-3) that were part of the approach he brought to New Zealand.

The excavation programme in northern New Zealand led progressively away from the traditional raw material of New Zealand archaeology—coastal midden deposits rich in artefacts—and revealed a wealth of structural information, particularly postholes and pits. The latter at first seemed as if they might be amenable to typological analysis and thus compensate for the lack of artefacts, which on many of these sites were extremely rare. The recovery of these diverse new data and the need to take them into account in any synthesis might have changed the course of New Zealand archaeology, even without the new emphasis on economic prehistory and the ecological approach recently stressed by Golson in his own review of antipodean archaeology since 1954 (Golson 1986).

CULTURE CHANGE IN PREHISTORIC NEW ZEALAND

The question of the relationship between Moa-hunter and Maori was fiercely debated in the 1870s and 1880s. It has remained a central issue in New Zealand prehistory until the present day, although the way in which the question is posed has changed. In the 1940s and 1950s, Duff (1947, 1950, 1956) appealed to traditional and ethnographic as well as archaeological evidence to produce his theory of change in New Zealand prehistory. Although Duff appeared to regard Maori culture as largely the development in isolation of the Polynesian culture of the first immigrants, his appeal to the so-called 'Fleet' traditions for the stimulus of agriculture and warfare set the scene for a preoccupation with multiple origins of the New Zealand Maori which lasted through the 1950s and 1960s. Thus, at the Association's second conference in 1957, Golson argued that

The key question in New Zealand culture history is whether the differences between moa-hunter and Maori are the result of spontaneously generated culture change within New Zealand itself or whether they were sparked off by influence from overseas (1957b:282-3).

By the time he came to write his major paper on 'Culture change in prehistoric New Zealand', Golson clearly shared what he described as the orthodox view: that New Zealand Maori culture is 'wholly Polynesian' and

the unique features of the latter [Classic Maori] phase are due to developments within New Zealand partly stimulated by the new climatic and geographical conditions into which the culture of the tropical islands was carried (Golson 1959a:65).

Nevertheless, he made provision in his organisational framework to accommodate the alternative possibility of significant cultural intrusion.

The two-part paper by Golson and Gathercole (1962) in Antiquity provided an overview of the state of knowledge at the time Golson left New Zealand. The central problem was still seen as the difficulty of determining the relationship between Moa-hunters and Maoris at the two ends of the time scale. Golson's 1959 paper enabled the authors to 'define the areas of our present ignorance' (Golson and Gathercole 1962:271), which included two major areas of activity often regarded as later introductions: agriculture and warfare. Again the question was raised of whether all change was internal or the result of renewed migration.

That paper concluded with the confident prediction that 'New Zealand's status in the prehistory of Polynesia will emerge with greater certainty than is at present the case' (Golson and Gathercole 1962:277). And indeed the growing body of data from tropical Polynesia gradually laid to rest the idea that a later migration to New

[2] Referred to for convenience in the rest of this paper as the Association.
Zealand was responsible for some major characteristics of Maori culture (Groube 1968). That same body of data introduced a new variation – the idea of multiple origins for New Zealand’s earlier population (Green 1967). This resulted partly from what in hindsight seem obvious examples of sampling problems and partly from a continuing reluctance to accept that agriculture could have been introduced by the first settlers.

Golson’s paper on culture change in prehistoric New Zealand was a landmark in New Zealand archaeology. It sought to provide an archaeological framework within which archaeological evidence could be organised and it gave a valuable review of the existing evidence. It stimulated a great deal of controversy, much of which centred on terminology, rather than on the theory behind the names. Although it may now be remembered primarily for its proposal of the term Archaic in place of Moa-hunter, and for its definition of Archaic and Classic material culture (Anderson 1983:48), the impact in 1959 of its archaeological approach should not be forgotten.

The 1960s were the decade of serious preoccupation with theories of change in New Zealand archaeology. The relative merits of the terms Archaic and Moa-hunter continued to be debated, and new contributions added fuel to the flames. At the Association’s 1962 conference in Christchurch, Shawcross pointed out that two successive stages are insufficient for a more precise study of a thousand or more years of time. More seriously, it leads to the assumption that all the important cultural changes in New Zealand occurred in the brief transitional interval between the earlier and later stage and not as a series of steps over the entire sequence (Green and Shawcross 1962:212).

A diagram showing the approximate chronological distribution of 15 traits revealed a ‘tyranny of the many by the few’ in which the age of assemblages was determined by the presence of certain artefacts and remains of extinct birds. At the time, this was an important contribution. In fact, however, the efforts of the succeeding 30 years have shown that certain artefacts and the remains of extinct birds are almost the only traits distinctive of one or other of the two poles of New Zealand prehistory over wide areas of New Zealand and that little or nothing is unique to a middle period. Other ways of organising the data are needed.

Shawcross’s paper was a prelude to Green’s first presentation of his six-phase sequence for the Auckland Province (Green and Shawcross 1962:213-20), published in expanded form the following year (Green 1963). The theoretical basis of Green’s sequence was examined by Golson (1965) and Groube (1967), both of whom were critical of the evolutionary ‘agricultural model’ and ‘settlement patterns model’, while welcoming what Golson called the ‘colonisation model’. In fact, many of the apparently diagnostic traits used in the colonisation model have not withstood the test of time either, and Green’s sequence is probably most useful now as a summary of information on a number of important sites which have never been fully reported.

The consideration of theory in New Zealand archaeology can be said to have peaked with Groube’s paper on models in prehistory (Groube 1967), in which he examined stadal, stagal and strophic models, pointing out the usefulness of what he called the ‘strophic model’ because it emphasizes the points of change rather than the platforms of conservatism (Groube 1967:22). It was Groube’s view that future research should concentrate on the rate of change and discover whether, in fact, the strophic model could be applied to New Zealand. In a later paper he reviewed the perpetual problem, now characterised as ‘from Archaic to Classic Maori’ (Groube 1969), raising a number of issues about adaptation and rates of change without providing any clear solutions but advocating a merging of the rich evidence of Maori traditions and the types of evidence used by archaeologists (Groube 1969:10) through research on fortifications. He explored this topic further in his major paper on fortifications (Groube 1970). Both Simmons (1971) and Groube at this time were developing a scenario of New Zealand prehistory which saw agriculture, fortifications and Classic Maori culture developing in Northland and expanding from there. These scenarios, with their appeal to oral traditions, represented new variations on an old theme of which Duff had been the principal proponent. Simmons (1969a) also proposed a sequence of six periods, in which he attempted to accommodate both economic and artefactual evidence. He pointed to increasing regional variation until about AD 1600, after which the more uniform Classic Maori culture spread rapidly (Simmons 1969a:9).

A similar stance to that adopted by Groube and Simmons was taken more recently by Bellwood (1978a), who reaffirmed the view that what he called the Classic Maori cultural configuration, characterised by the construction of earthwork fortifications, the frequent use of storage pits for sweet potato, and a new artefact suite, developed in one area of the North Island (not yet identified) and spread by migration and conquest.
From the late 1960s, two factors probably influenced the trend away from theoretical frameworks for organising New Zealand prehistory. New Zealand archaeologists were concentrating more and more on regional studies and, as both Green (1963:31) and Golson (1965:90) had predicted, phases were giving way to aspects, although not necessarily thus expressed. Just as significantly, more radiocarbon dates were becoming available, and were being used to order the data of regional prehistory. As I discuss below, however, the move to regional studies has not necessarily always led to clearer thinking about ways of organising the data.

A number of issues concerning change in New Zealand prehistory were raised between 1959 and 1969. Some of them appear to have been resolved, most notably the question of whether the development of Classic Maori was stimulated by intrusion from outside. However, Allen (1987) has recently argued that this issue was essentially a ripple in an underlying continuum of archaeological assumption that New Zealand prehistory was the result of steady adaptation and change in isolation. It is interesting to note that while the concept of divergence in isolation is currently becoming unfashionable elsewhere in Polynesia, it continues to hold sway in New Zealand. At the same time, the notion of regular inter-island voyaging in central Eastern Polynesia 600 to 1000 years ago has led to a loss of interest in the possibility of multiple origins of New Zealand settlement because of the growing belief that colonisers from different islands within the same general area were probably archaeologically indistinguishable.

Opinion remains divided as to whether change in New Zealand prehistory was slow and steady or sudden and dramatic. Groube (1969) and Bellwood (1978a) have been the principal supporters of sudden and dramatic change. Allen (1987:16) cites Groube in support of his view that Duff, Golson and Green all used an implicit model of constant change to explain the development of Maori culture. Yet Golson (1965:89) wrote that ‘changes in different elements of culture should not of course be expected to take place at a uniform rate’ and Golson and Gathercole (1962:274) expressed the view that ‘a great deal of regional diversity is present in the Classic Maori phase and considerable complexity in its genesis is to be expected’. It is this last view I have followed in arguing that the changes that resulted in the Classic Maori culture of the eighteenth century took place at different times and in different places and spread largely by other means than migration and conquest (Davidson 1984:5, 194, 222).

As noted above, questions of terminology have been among the most hotly debated, and on occasion dictionaries have been thumped with some heat during Association conferences. Golson’s term Archaic and the less controversial Classic Maori have been most used, as noted below. Archaeologists have generally found it necessary to use a term other than Maori to characterise the people and culture of the earlier part of New Zealand prehistory (e.g. Duff 1947:313; Davidson 1983:291). Recently, however, Anderson has advanced the view that Maori, qualified if necessary by ‘early’ and ‘late’, ‘is a perfectly correct term for the entire pre-European period’ (Anderson 1989a:3). This is a fashionable, even politically expedient view in the 1990s. It is to be welcomed, however, if it heralds a serious revision of terminology, and hence of the concepts reflected by the terms.

**DATING NEW ZEALAND PREHISTORY**

One of the problems addressed by Golson at an early stage in his New Zealand work was that of ‘dating New Zealand’s prehistory’ (Golson 1955c). This paper, published in the same year that the first radiocarbon dates were obtained for New Zealand archaeological sites by Duff and Lockerbie, shows a clear grasp of the problem still confronting New Zealand archaeologists: ‘within the broadest of limits a New Zealand archaeological site does not by the material culture it discloses proclaim its relative and still less its absolute age.’ (Golson 1955c:115). And further ‘The conclusion must be that, outside the broad limits set forth ... the date derived by any means for a New Zealand archaeological site and its associated material applies solely to that site and the occurrence of the material there.’ (Golson 1955c:133). A wide range of structural and faunal components could now be added to material culture.

Golson’s review of dating methods was written at a time when he apparently accepted the overall timescale of New Zealand prehistory as laid down by Duff. His concern was not so much to date the beginning of the sequence as to establish its subdivisions. In the succeeding 35 years, New Zealand archaeologists have obtained a large number of radiocarbon dates. A number of publications have discussed the technical and more philosophical problems of using radiocarbon dates within the short and
recent span of New Zealand prehistory (e.g. Shawcross 1969; McCulloch and Trotter 1975; McFadgen 1982; Anderson 1989b:171-6). Archaeologists have implicitly, if seldom explicitly, accepted that archaeological data are to be ordered in a framework established by radiocarbon dates, despite the difficulties this entails. The high hopes originally entertained for obsidian dating (Green 1962, 1964; Ambrose and Green 1962) did not lead to the effective development of an alternative dating method in New Zealand. Current work on changes in the surface chemistry of obsidian flakes may yet fulfill that early promise (B.F. Leach, pers. comm.).

However, the dating issue that has constantly generated the most heat has not been the subdivision or description of the prehistoric sequence, which really needs addressing, but the date of first colonisation of New Zealand and the nature of that colonisation.

By 1959, a handful of dates were available for both North and South Island sites but, as Golson noted, they had done little to clarify what he saw as the central issue – the relationship of Archaic to Classic Maori (1959a:67). Indeed, they had already thrown up the paradox that the earliest known South Island sites appeared to be slightly older than the earliest known North Island sites (idem). However, both Golson and Lockerbie asserted that older sites were to be expected in the North Island, because of the small quantities of obsidian found in low levels of the southern sites (Golson idem; Lockerbie 1959:77).

These first radiocarbon dates provided a picture:

- at about 1200-1350 years of a number of established Archaic communities strung along the eastern seaboard of New Zealand from Auckland to the Bluff. This suggests an arrival of Eastern Polynesians in New Zealand back in the first millennium AD (Golson 1959a:70).

In the less guarded environment of an unpublished public lecture, this became, significantly 'well back in the first millennium' (Golson 1959b:9).

This scenario, or one very similar to it, has been repeated as the orthodox version ever since (e.g. Cumberland 1962; Groube 1968:144; Shawcross 1969; Davidson 1981:6), but it has always been accompanied by a parallel unorthodox version, which sees the first settlement of New Zealand as significantly earlier. This has given rise to both lunatic fringe outbursts, and more serious claims, and often the difference between the two has been blurred.

Both radiocarbon dates and geochronology have been invoked as indicating early settlement.

Important examples have included 'Grumpy's stump', an apparently adzed timber beneath the Taupo Pumice (and therefore likely to be about 1800 years old) and the Taumatawhana stump, another adzed stump from a swamp in the far north which returned a radiocarbon date of similar antiquity.

More serious was the claim of an amateur archaeologist (Price 1963, 1965), that human occupation debris was stratified between tephra deposits at several sites at Lake Poukawa in southern Hawkes Bay. This was taken seriously by the news media and by some members of the scientific establishment (Pullar 1965), if rejected by most archaeologists. The Poukawa situation was complicated by the presence of natural moa deposits which had been 'mined' by the inhabitants of a later prehistoric settlement on a low island in the lake. Eventually, Poukawa was laid to rest when McFadgen (1979) demonstrated the extent to which ploughing had disturbed the site.

In the last few years, the orthodox model of settlement of Eastern Polynesia, of which New Zealand is a part, has been severely questioned (Kirch 1986). This led Sutton (1987) to raise the question of significantly earlier settlement of New Zealand. Sutton's arguments were attacked on a number of grounds (e.g. Grant 1988; Enright and Osborne 1988). At an Association conference devoted to this question in May 1988, a poll of 83 delegates revealed that the majority still hold to the concept of first settlement towards the close of the first millennium AD. Two almost equally large groups supported AD 800 and AD 1000 (Anonymous 1988). As the reporter observed, this split 'presumably reflects different assessments of how long it would take a population to become established, and become archaeologically visible.' The conference proceedings (Sutton [ed.] in press) will, it is hoped, provide an indication of the actual evidence on which these assessments were based.

Closely related to the date of first settlement is the course of colonisation. Implicit in most interpretations of the last 30 years has been the notion of rapid coastal dispersal following initial colonisation. However, Caughey (1988) has recently proposed a model of 'patterned spread' and argued from a selection of radiocarbon dates that settlement began at Kaikoura in the northeast of the South Island and spread steadily both north and south. This follows an argument previously advanced by McCulloch and Trotter (1975) for the South Island only and repeated by Trotter (1982:101) for the northern part of that island, of a steady southwards spread over a period of...
200 years or more. Caughley's argument, in particular, comes dangerously close to fulfilling a prediction made by Shawcross more than 20 years ago that it is only a short step to prehistories constructed entirely from series of radiocarbon dates. Such would literally indicate that there is evidence for human settlement by the date of the oldest measurement and that from then onwards man persisted (Shawcross 1969:189).

Caughley offered two suggestions as to how his model may be falsified. One requires more radiocarbon dates, but the other concerns the presence of greenstone and obsidian in sites far from their sources. Although it has often been stated that obsidian occurs in early layers of southern sites, it is not easy to find indisputable evidence of this in the archaeological literature. The two examples given by Caughley do not really advance the argument one way or the other. It is worth noting, however, that obsidian was apparently present in the deposit where, according to Caughley, it all began, Avoca Point at Kaikoura (Trotter 1980), and that greenstone was present at the base of one of the other very early sites, Titirangi in Marlborough (Trotter 1982). On the other hand, Anderson (1989a) has recently shown that obsidian is not present in the basal layers of the major early site at the Shag River Mouth in North Otago, and taken this as possible evidence in support of a separate southern colonisation.

Just as most archaeologists declined to argue in print with Price and his supporters about the antiquity of human settlement in Hawkes Bay, most have not bothered to take issue with Caughley. However, Anderson and McGovern-Wilson (1990) have examined Caughley's hypothesis in terms of radiocarbon dates and argued that a model of 'chaotic spread' fits the data better. They also point out just how important the question is, for although the construction of sequences purely in terms of radiocarbon dates seems to most archaeologists absurd, as it entirely omits the cultural component, the choice of colonisation model has wide implications in terms of both human behaviour and moa extinction.

The extensive use of radiocarbon dating, despite the many problems it has posed, has relieved New Zealand archaeologists of the necessity of developing other ways of organising their data chronologically. It has enabled them to separate the introduction of agriculture from the development of pa warfare and the appearance of distinctive Maori artefacts. However, knowledge of the duration of New Zealand prehistory is little changed since 1959. Whichever dates one chooses, whichever material is preferred, whichever chronology is accepted, New Zealand prehistory has a span of about 700 to 1000 years, as traditionalists, working from genealogies, long ago decreed (cf. Shawcross 1969).

REGIONAL PREHISTORY

The existence of regional variation in New Zealand prehistory has long been realised, although the implications, particularly in subsistence, have not always been fully appreciated. Skinner (1921) defined seven culture areas in mainland New Zealand using mainly ethnographic features, but including some archaeological data. The geographers, Cumberland (1949) and Lewthwaite (1949), identified three major regions of differing potential for Polynesian settlement, emphasising the uneven distribution of the Maori population in the eighteenth century. Duff's model of New Zealand prehistory allowed for different courses of events in the North and South Islands and the Chathams, while Lockerbie, working steadily in the southern South Island over a long period and bringing controlled stratigraphic excavation to bear, built up a regional sequence there. In his 1959 paper, Golson explicitly made provision for different kinds of regional sequences.

The first coordinated regional archaeological programme in New Zealand was the Palliser Bay project directed by Foss and Helen Leach between 1969 and 1972 (Leach and Leach [eds] 1979; B.F. Leach 1981). This set a standard in coordinated regional research which has since been matched only by the Pouerua project in Northland, directed by Sutton (1985), one of the Otago students who took part in the Palliser Bay project.

Meanwhile, however, other researchers working over long periods in particular regions, albeit less systematically, were building up enough data for the beginnings of regional syntheses. When the Wairarapa work began, the Leaches could point only to South Otago, the Coromandel/Bay of Plenty and Motutapu as places in which regional investigations had taken place (Leach and Leach [eds] 1979:1). In the last two, of course, Golson had initiated investigations, which others had later developed further. In Otago, Lockerbie (1959) had developed a sequence 'From Moa-hunter to Classic Maori', but apart from Green's Auckland sequence, there had been no synthesis of the Coromandel/Bay of Plenty data. A tentative four-phase sequence was

244
proposed for Motutapu (Davidson 1972) but this was never extended, as by implication it might have been, to the wider Auckland area.

Regional summaries began to appear in greater numbers towards the end of the 1970s (Davidson 1978a, 1978b; Sutton 1980) and by 1982, the Association was able to publish a monograph on regional perspectives in New Zealand archaeology (Prickett [ed.] 1982). Regional reviews were also published in other outlets at about the same time (B.F. Leach 1981; Anderson 1983; Prickett 1983).

Few of these attempt formal subdivisions of the sequence, although most admit some kind of amorphous division into an early phase or period usually called Archaic and a later, called Classic. Most ignore, or at best treat very perfunctorily, the protohistoric period. The confusions besetting New Zealand archaeologists in organising their material were summed up by Trotter (1982:98):

Like "Moa-hunter" or its near synonym "Archaic", the term "Classic" does not in practice appear to cover a precise period or cultural aspect, but is loosely applied to sites and artefacts dating from about this time.

Trotter alone in the volume on regional perspectives used Moa-hunter in preference to Archaic, but he took care to reference the term firmly to Duff's original definition in terms of Wairau Bar.

The South Island with its rich artefact assemblages has, despite problems of poor provenance of many older collections, consistently lent itself more readily to subdivision of the artefactual sequence. The number of dated midden deposits, with their changing faunal content, has also enabled subdivision of an economic sequence which is essentially concerned with the decline in big game hunting. Thus several authors have proposed subdivisions characterised by either or both artefactual and subsistence changes.

An early venture in this respect by Simmons (1973) proposed four 'groups' of sites on the basis of various artefact types. Simmons then correlated these with four 'economic periods' he had previously defined: Early, Middle, Intermediate, and Late. Although this sequence has not been adopted by other workers, the artefact data have been found useful (e.g. Anderson 1982a).

The terms Late Archaic and Early Classic have also been used in discussions of southern regional sequences (e.g. Anderson 1982a, 1982b; Leach and Hamel 1981), variously referring to artefact assemblages and economic activity. Elsewhere, Anderson (1983) has suggested Early, Middle and Late chronological periods, the first two roughly corresponding to the Archaic Phase, so that the Middle Period presumably corresponds approximately to the Late Archaic.

A recurrent theme in these better developed southern sequences is the idea of greater regional diversity, perhaps reflecting greater isolation, in late Archaic assemblages, followed by greater uniformity in the succeeding Classic assemblages (Trotter 1982:101; Anderson 1982a:125). Trotter attributed this greater uniformity to better communication networks in the later period. The effect of the growing trade in greenstone, tentatively identified by Anderson (1982c) as responsible for settlement pattern changes on the West Coast of the South Island, may have had a lot to do with this apparent increase in uniformity.

The most finely divided of the regional sequences is that suggested for the southern Wairarapa by Foss Leach (1981), who proposed seven cultural periods, five of which are pre-European and two protohistoric. The sequence runs from first colonisation estimated at about AD 1000 to the beginning of formal European settlement in 1853. The divisions are based on radiocarbon dates, but the periods are characterised by a wide variety of data on material culture, economy, settlement pattern and environmental change. Eventually, regional sequences of this precision should be able to be developed for many parts of New Zealand. The task then will be to identify those trends that are of greater than regional significance.

More than thirty years after 'Culture change in prehistoric New Zealand', it is clear that the most satisfactory regional sequences, such as those for southern New Zealand and the Wairarapa, are based on radiocarbon dates as the primary means of establishing chronology, and draw on a wide variety of data to develop a picture of the changing fortunes of the inhabitants of the region in question. Each sequence is suitable only for that region, and may indeed be suitable only for the purposes of the researcher who devised it. Where data are fewer or less well controlled, archaeologists are forced back to the old two part division. Attempts to review New Zealand prehistory as a whole may be forced to explore the extent of regional variation at specific points in time, rather than tracing changes over wide areas (e.g. Davidson 1985).

PORTABLE ARTEFACTS

Up to the 1950s, New Zealand archaeology was dominated by museum ethnologists and curio hunters who concentrated almost exclusively on artefacts, with a small but significant
emphasis on the remains of extinct birds, particularly moa. The study of artefactual assemblages also loomed large in Golson's approach to archaeology, and he quickly became knowledgeable about the artefacts, particularly stone adzes, that had been the products of much of the archaeological endeavour in New Zealand till that time. His grasp of this issue is clearly seen in 'Culture change', which remains a standard reference on the kinds of artefacts likely to be found in excavations in New Zealand.

Yet the events which Golson set in train were at least partly responsible for a reaction against artefact studies which has affected New Zealand archaeology ever since. Complex structural sites like Kauri Point produced very few artefacts; the growing interest in settlement patterns and economy and frustration at the failure of artefacts to yield a more useful chronological framework combined with well meaning attempts to repress fossicking to produce an almost puritanical feeling in some quarters that artefact studies were of minor significance, if not actually immoral. The decline in interest in material culture was exemplified in an incident at Otago when a student in the laboratory was unable to establish the anatomy of a bone encountered in a midden sample. This proved to be part of a fishhook (B.F. Leach, pers. comm.).

In 1963, Golson and Gathercole in a reply to Duff, asserted that

Artifactual changes in New Zealand prehistory are sufficiently great to admit of a cultural subdivision beyond the established divisions of Archaic and Classic Maori at the beginning and end of the sequence (Golson and Gathercole 1963:128).

In fact, with a few notable exceptions, it has not yet been possible to establish such subdivisions, although it remains possible that the appearance (or disappearance) of individual items may prove useful chronological markers.

The exceptions have been mainly in the south, as noted above. In the North Island, the demonstration of stylistic change in wooden combs from the Kauri Point Swamp during the last few centuries of the prehistoric period (Shawcross 1964, 1976) showed the potential of some wooden artefacts in this respect, if enough examples can be recovered from controlled excavations.

Expectations about the nature of the changes in material culture have altered considerably. There has been an increasing realisation that all elements of the Classic Maori package (including agriculture and warfare) need not, and indeed could not, have developed at one time and in one place, and similarly that changes in material culture may have happened at different times and in different places. It thus becomes a case of laboriously documenting changes in regional sequences and then making inter-regional comparisons as appropriate (e.g. Davidson 1984:61-111).

As noted above, South Island artefacts have generally been better studied, and in some categories, particularly fishhooks, steady and progressive change has been well documented for a long time (Lockerie 1940; Trotter 1965; Hjarno 1967). North Island fishhooks have not been studied to the same extent, although Law (1984) has shown that they lend themselves to classification using metrical as well as formal study. Sewell (1988), has recently documented a change in material from bone to shell at the Cross Creek Midden at Sarah's Gully. These fishhooks differ stylistically from later examples from Kauri Point. Sewell's study shows that the potential exists for fishhooks to be used as chronological markers in the North Island too, at least in regional studies.

The description and classification of Polynesian stone adzes have a long history, and the use of adze typologies in culture historical/studies flourished from the 1920s until quite recently (see reviews by Cleghorn 1984; Park 1989). In New Zealand, typological studies reached their peak in the work of Duff (1950, 1959) and although Golson used adzes extensively in teaching and in discussing the development of Classic Maori culture, little progress was made in documenting actual changes, beyond the demonstration of hypothetical sequences of possible changes based on surface collections (e.g. Golson 1959a:67). However, strong dissatisfaction with adzes as an illustration of progressive change was voiced by Groube (1969). The emphasis in adze studies has moved away from typology into studies of function (Best 1977) and manufacture (Leach and Leach 1980; H.M. Leach 1981), paralleling trends elsewhere in Polynesia. At the same time it has become apparent that the so-called change from the varied Archaic adze kit to the Classic Maori '2B' style was a very complex business, due partly to the availability of raw materials and the technology to use them, partly to changing functional requirements, and partly to social factors involved in the distribution of products from manufacturing centres.

The early settlers quickly identified sources of stone in New Zealand best suited to their adze making technology and the products they desired. Adzes from a few major production centres were distributed throughout New Zealand in the early centuries and products from
particular manufacturing centres have been found in workshops at other centres. However, the original centres do not seem to have been able to supply the growing population in the North Island and stone workers turned increasingly to more local sources of stone, some of which had to be worked in different ways. At the same time, a new, superior and different product, the nephrite adze, came increasingly to dominate the distribution system. The change from Archaic to Classic Maori in adzes is probably best exemplified by the replacement of the argillite or basalt adze by the greenstone adze, rather than in hypothetical sequences of typological change.

Portable artefact studies are overdue for rehabilitation. While there will continue to be a place for typological studies, the investigation of the social context in which artefacts were made and used will probably contribute more to our understanding (cf. Anderson 1982b).

AGRICULTURE

If a single issue has most complicated the thinking of archaeologists about culture change in New Zealand prehistory it is the question of the introduction of agriculture. Duff had asserted that the moa-hunters were not agriculturalists, and attributed the introduction of kumara and other cultivated plants to the Fleet migration. This was the accepted position in the mid 1950s, but the excavation and dating of pits thought to be for the storage of root crops challenged that view. In his 1959 paper, Golson stated 'It is on present evidence impossible to say whether agriculture was practised...in the Archaic or not' (1959a:62), but he drew attention to the presence of pits at Sarah's Gully and raised the possibility 'that the Archaic structures at Sarah's Gully were such kumara stores' (1959a:45). This idea of Archaic storage pits for kumara was explored in a little more detail by Golson and Gathercole (1962:172), but by that time the question of agriculture had become entangled with the question of climatic change [3] following Yen's important paper of the previous year (Yen 1961). The question was taken up and elaborated by Green (1963), who made Yen's model of kumara introduction an important part of his own model of culture change. By 1965, the idea was well established that agriculture became more important as other resources, particularly moa, became scarcer, regardless of when it was first introduced. This thesis was woven into the settlement pattern component of Green's (1963) model. As Golson argued, however, 'To agree that it becomes more important in these circumstances is not to concede that it was unimportant before' (Golson 1965:89). And he pointed to the evidence from Opito, where 'the beach midden rich in fauna and the ridge settlement with food storage pits' appeared likely to be contemporary. He then proposed a break away from the assumed correlation between a rise in agriculture and a decline in hunting: 'Agriculture would allow stability of settlement, village organisation and the like independently of what was happening to other food resources.'

This view offered a quite different perspective of North Island culture history than was contained in the pan-New Zealand framework in which agriculture grew and spread as moa-hunting declined. Nonetheless, the idea that the adaptation of agriculture was intimately related to the origin and spread of Classic Maori became deeply entrenched (Simmons 1969a; Groube 1970), although its premises have since been seriously challenged (e.g. Irwin 1982).

By the early 1970s, acceptance that moa-hunters in some regions were also gardeners was gaining ground (Green 1972). However, evidence for agriculture during the Archaic Phase still rested largely on storage pits. A debate on the function of pits had raged during the previous decade, largely because so many pits and so few remains of surface buildings were being excavated. The arguments were sometimes extreme, refusing to recognise that the term pit could encompass many different items, embracing large ovens and houses that had been slightly set into the ground as well as definite storage pits. By the early to mid 1970s, however, most archaeologists had come to accept that most pits were for storage (Law 1969; Fox 1974) but this interpretation has never been fully accepted in Canterbury/Marlborough (Trotter 1982).

Archaeological investigations of gardens themselves began in the 1960s (Groube 1966: 111; Nicholls 1965; Peters 1975). From the late 1960s, major research programmes were undertaken in the Auckland area (Sullivan 1972) and in Palliser Bay in the southern North Island (H.M. Leach 1976, 1979a, 1979b). The work in Palliser Bay, particularly, which documented over 80 hectares of fields mostly dating to the earlier part of the sequence, quickly rendered much of the earlier debate about the age and function of pits irrelevant. A few more early

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[3] Space precludes a review of the extensive debate about climate change, which is no longer thought to have been influential in the establishment of agriculture in New Zealand. Recent opinions can be found in Leach and Leach (editors) 1979:229-40 and Burrows 1982.

247
dates have been obtained for storage pits on the Coromandel Peninsula (Davidson 1974; Harsant 1984), but these alone would have been insufficient to resolve the lingering doubts about early gardening. The interpretation of prehistoric life in Palliser Bay, drawn from many different lines of investigation, produced a picture not unlike that foreseen by Golson and quoted above, of small permanent villages at river mouths, close to gardens, from which people indulged in a variety of activities, including hunting and fishing, adapting their economic strategies to the changing circumstances of their particular region.

The archaeological investigation of gardens has proliferated in recent years, with studies in many parts of the North Island which are too numerous to list here (see reviews by Davidson 1984:119-21; H.M. Leach 1984:33-52; Barber 1989; Bulmer 1989). It remains true, however, that most garden systems studied belong to the middle or late parts of the respective sequences, and really early gardens are not easy to find. This is hardly surprising; it is in the most marginal areas such as Palliser Bay, where gardening was abandoned in the face of increasing difficulty, that early field systems are recognisable, while in more favourable areas, earlier gardens have been modified by later ones.

It is one thing to accept that gardening was probably always part of the way of life of people in most coastal regions of the North Island and parts of the northern interior; it is another to demonstrate the actual importance of gardening in the activities of a community or of garden produce in the diet. The relative importance of cultivated food, and particularly kumara, compared with gathered food, particularly fern root, was questioned by K. Shawcross (1967). The relative importance of gardening and hunting has been debated inconclusively for years.

In a review of the role of agriculture, Green (1972) considered various ways of distinguishing between the presumptive Archaic agriculturalists in the north, and apparent Archaic hunter-gatherers in the far south, without coming to any firm conclusion. However, he held to the view that agriculture was initially relegated to a minor role, since New Zealand offered far better opportunities for hunting and gathering than the Eastern Polynesian homeland (Green 1975:610). In his view, it was not until the later Archaic in some regions of the North Island that 'economies were evolving in the direction of economies characteristic of the 18th century Maori practices in that area' (Green 1975:615).

A similar view was expressed by Bellwood (1978a), who accepted that some pit storage of sweet potato was present during the Archaic Phase, but considered it to have been limited in distribution (and by implication in importance). Law (1982:56) also implied that agriculture may not have been important on the Coromandel Peninsula during what he called the Archaic Period, by stressing the hunting orientation of the site distribution.

Thus three main scenarios have been proposed for the introduction and spread of agriculture, and particularly kumara agriculture in New Zealand: it was introduced by the initial settlers and quickly became important in those areas where it was climatically feasible; it was introduced by the initial settlers but for some centuries was of only minor importance because of the wonderful opportunities for hunting; it was a later introduction and possibly a catalyst for other cultural changes.

Underlying these views are fundamental differences of opinion among archaeologists themselves about the relative merits of gardening and hunting. Would people reared in a milieu of intensive cultivation seek to perpetuate this way of life even in the face of undreamed of hunting possibilities, or would they abandon their gardening altogether or reduce it to the level of a hobby while the hunting opportunities lasted? In the southern South Island, of course, there was no choice; the question surfaces here in a slightly different guise. Was the eastern side of the South Island the preferred part of New Zealand to live during the early centuries when, as it appears, there were more moa there than anywhere else?

Until recently, there was no way of establishing the relative importance of different constituents of the diet. Now, however, isotope and trace element studies of bone aimed at revealing the relative importance of marine/terrestrial and plant/animal foods in past diets may at last provide a means of distinguishing between different populations in this respect and providing some answers, at last, to the question (Horwood 1989a, 1989b; Quinn 1990; B.F. Leach et al. in press). The answers, at least in the early stages of this research, will not be straightforward, however, and in the 1990s, it may be very difficult indeed to obtain the necessary bone samples and the approval to use them in this type of research.

WARFARE

Warfare was declared by Duff to be an integral part of the Classic Maori package. In his view, the moa-hunters abandoned the Polynesian
pattern of feuding and warfare and became for several centuries entirely peaceful, just as they abandoned a long established agricultural tradition in favour of a hunting and gathering economy. Moa-hunters did not build pa, did not have weapons, and did not practice cannibalism. Warfare in all its aspects developed subsequently in the North Island and spread south together with other Classic Maori characteristics (Duff 1956:11). These assertions had a considerable influence on much subsequent thinking, and the possibility of war without pa was seldom entertained. There are, however, two questions to be resolved: the nature and extent (if any) of warfare in the early centuries of settlement of New Zealand, and the reasons for the development and spread of fortified pa, with the allied problems of when and where pa originated.

Golson took the same cautious approach to the question of warfare that he took to agriculture. 'It is on present evidence impossible to say whether agriculture was practised or the pa present in the Archaic or not' (1959a:62). Similarly, although he noted that weapons were not documented for the Archaic apart from a bone patu of doubtful association from Horowhenua, he did not rule out the possibility of Archaic weapons. These views were repeated by Golson and Gathercole (1962:271) in rather more detail and they added the important point that weapons had not been found in recent excavations of undeniably fortified sites.

That pa were seen as vital to an understanding of the development of Classic Maori culture was shown by the selection of the pa at Kauri Point for Golson's last excavation in New Zealand (Golson et al. 1961). In the conclusion to this report, written after the first season of excavation at the site, Golson put forward the hypothesis that scarp and terrace defence might have been present during the Archaic Phase, with ring-ditch pa spreading during the later Classic Maori phase. Here he was probably influenced by the perplexing evidence from the volcanic cones of Auckland, as much as by the preliminary interpretation of evidence from Kauri Point, which did not survive the test of further work at the site. In respect of pa, however, as in respect of agriculture, Golson clearly showed himself willing to approach North Island prehistory without any preconceptions inherited from Duff. Fortified pa are the outstanding field monuments of New Zealand archaeology, as Best (1927) and Golson (1957a) both appreciated. Since archaeological finds of weapons and evidence of cannibalism are rare, and pa are exceedingly numerous, it is not surprising that pa have dominated archaeological discussion of warfare in New Zealand prehistory. Moreover, the central role of pa in later Maori settlement patterns shows them to be very much a feature of Maori life at the close of the prehistoric period. No wonder, then, that the question of their origin has been seen, since Best's day, as 'a fair field of enquiry' (Best 1927:320; see also review in Davidson 1987).

The course of pa studies has varied with the varying approaches to all aspects of New Zealand archaeology. The six pa excavations initiated by Golson set trends that continued for some time. Salvage excavations were undertaken on the Auckland volcanic cones of Taylors Hill and Mount Wellington and a training excavation was mounted for Association members during the 1959 conference at Rotorua. The research excavations on pa at Great Mercury Island, Sarah's Gully and Kauri Point included significant area excavations in all three as well as cuts through the defences at the latter two.

The 1960s were the decade of major pa excavations. Kauri Point was followed by Ongari Point (Shawcross 1966), Ngaroto (Shawcross 1968), Mangakaware (Bellwood 1971, 1978b), Otakamini (Bellwood 1971, 1972) and Waioneke (McKinlay 1971), as well as other smaller investigations. There were fewer pa excavations of any extent during the 1970s, although a higher proportion reached final publication (Fox 1978; McFadgen and Sheppard 1984).

The 1980s saw significant changes in the approach to pa studies (Davidson 1987) with an increasing focus on regional surveys of pa, and attempts to date a number of these using radiocarbon samples taken from exposed sections or obtained by small test excavations. The results have consistently suggested the appearance of pa at about the same time in many parts of the North Island: Northland, Auckland, Taranaki, Bay of Plenty, East Coast and Hawkes Bay. In some areas it is evident that fortifications were added to existing settlements, and thus did not represent a sudden and drastic change in settlement pattern. Pa appeared in about the fifteenth century, and there is no evidence to suggest the primacy of any one region.

Pa excavations have also continued, although they have generally been of more limited extent than those of the 1960s. An exception has been the Pouerua project in the inland Bay of Islands, which investigated many different aspects of a largely intact archaeological landscape, including undefended settlements, ancillary pa, and a major fortified volcanic cone (Sutton 1985, 1990). Much recent work on pa is not yet fully
published, and consequently it is difficult to summarise.

Running parallel to excavations of individual pa or regional studies of pa distribution and typology have been overall reviews of pa and warfare. Particularly influential was the work of Groube (1970), who developed a typological classification of pa, and wove it into an embracing theory of change in New Zealand prehistory, as noted above. More general reviews have been provided by Fox (1976) and Davidson (1984:181-94, 1987).

Present evidence suggests that pa warfare arose fairly suddenly and spread rapidly at an intermediate point in New Zealand prehistory. Pa building was a major characteristic of Maori culture at the close of the prehistoric period, and in this sense the beginning of pa warfare was certainly an important step in the development of Classic Maori culture. However, it now seems most likely that it was not correlated with the establishment of agriculture or the development of pit storage, both of which are significantly older. Nor need it have been accompanied by a sudden switch from one suite of portable artefacts to another, although it may have resulted in a great increase in weapons – mostly, if not entirely, wooden.

In the eighteenth century, pa warfare was endemic among people with very differing economies, from the far north at least to the northern South Island. It was not, therefore, directly correlated with agriculture or the need for garden land, however it had arisen. Certainly pa were extraordinarily rare south of Banks Peninsula, but the correlation is likely to be with the low density and high mobility of southern populations, rather than directly with agriculture.

The origin of pa is on present evidence earlier than the appearance of the short stone club known as patu onewa, which is documented only for late prehistoric sites (Davidson 1984:101), although the rarity of weapons of any kind in excavations means that this assertion should be treated with caution. No whalebone weapons of either patu or hoeroa form have ever been found in North Island excavations apart from one patu recovered by Adkin from a burial at Horowhenua and one found by Mantell at Waingongoro in the nineteenth century (Davidson 1984:101). Most, if not all, weapons for the first half of the pa building period may therefore have been of wood. Thus absence of weapons cannot be taken as an argument for absence of any form of warfare during the Archaic. Since there is some evidence for violence during the pre-pa period (Sutton 1979), it is reasonable to assume that the first settlers brought with them the propensity for quarrelling and violence that was characteristic of most Polynesian societies. The problem now is to explain why Polynesian warfare in New Zealand developed as it did.

ARCHAEOLOGY, TRADITION AND MYTH

In the 1950s, appeal to tradition when archaeological data was lacking was a common and accepted procedure. Indeed, the mutual reinforcement of archaeology and tradition had been a feature of New Zealand archaeology for many years. To Golson, however, the appeal from the archaeological evidence, or rather its deficiencies at the present stage, to tradition is unfortunate. The need is for full archaeological exploration of the relevant problems and the use of tradition for comparison with the archaeological hypotheses and not as a source of explanation of archaeological data (1959a:66).

His views on this matter were amplified in an extended critique of papers by Adkin (1960) and Keyes (1960) which had sought to use Maori traditions to provide a framework and nomenclature for cultural developments (Golson 1960). The introduction to this paper provides a clear statement of the relationship between archaeology and tradition. Each deals with different aspects of prehistoric activity with very little overlap. Precise tribal attributions can be made for many recent sites, but difficulties increase with greater time depth. Much more archaeological work is needed before the archaeological story can be integrated with the traditional one. The traditional story is more complex than has been assumed.

In the last statement lay the root of the problem that has tended to beset attempts to integrate archaeology and Maori tradition: the assumption that there is one traditional story or indeed one archaeological story. Golson was well aware that the 'traditional' story that provided the framework for the models used by both Buck (1949) and Duff to discuss New Zealand prehistory was largely the work of one non-Maori scholar, S. Percy Smith (Golson 1959b:2; cf. Simmons 1969b). Although he did not say so specifically, Golson clearly held the view that genuine Maori tradition is tribal, not pan-New Zealand in its emphasis, and that the proper place for a comparison of archaeological and traditional evidence is at the level of the region or the individual site. Thus in his selection and initial interpretation of the Kauri Point site (Golson et al. 1961), he paid attention to Maori tradition in a
way that he would not do in his considerations of New Zealand prehistory as a whole.

Golson's strong reaction to Adkin's paper undoubtedly influenced some New Zealand archaeologists to avoid traditional history as far as possible. This reluctance to consider traditional evidence has not helped the relationship between archaeologists and Maori people, long alienated by the fossicking and grave robbing tradition of earlier generations of artefact collectors and further put off by the growing body of archaeological data which seemed irrelevant to their own concerns.

Not all students of New Zealand prehistory were frightened away from traditions, however. Because pa often loom large in specific traditions, some of those most concerned with pa studies were among those most committed to the view that New Zealand archaeology must embrace tradition in its interpretations. Groube and Simmons, weaving together distributions of pa and adzes and aspects of regional traditions, produced a hypothesis of Classic Maori development and spread from Northland, which unfortunately is largely untestable. It was sufficiently compelling, however, to influence Bellwood (1978a:400) who had worked with Groube on the excavation of the traditionally well known pa of Otakanini near Helensville (Bellwood 1971, 1972, 1973). This exercise had produced major problems in reconciling the radiocarbon dates with the genealogies pertaining to the site; since the charcoal of the samples was not identified, it now seems very likely that in this case the radiocarbon dates were too old, rather than that the genealogical dates were too recent. Despite the dating problem, the work at Otakanini was an example of a project in which archaeological and traditional evidence could be brought together. Such projects are still too rare, although more recent examples, such as Pouerua (Sutton 1990) and Waiwhau (Phillips and Green 1991) are only now being published.

At another level altogether, old frameworks derived from traditional history sometimes provide a conceptual straitjacket that is very difficult to break out of. An example is the long held conviction that Classic Maori artefact styles (and agriculture and warfare) were introduced to the South Island by migrating northern tribes. The first appearances of agriculture and pa in the South Island are not well enough dated to draw a useful conclusion on these aspects. It is interesting, however, to trace the changing attitudes of archaeologists to the interpretation of material culture.

In his earlier writings, Duff was cautious about the extent to which the culture of the invading tribes replaced, rather than merged with, the earlier Moa-hunter culture.

... while in all parts of the South Island it [the Moa-hunter culture] probably modified the intrusive culture of the politically dominant North Island invaders. The Moa-hunter adze form almost certainly survived south of Banks Peninsula and reacted in turn upon the form of the adzes of the greenstone age; the widespread substitution of 'k' for 'ng' in the South Island dialects probably derives from Moa-hunter times. (Duff 1956:20)

In his excavations at what he took to be the traditional Ngati Mamoe site of Pari Whakatau, however, Duff found 'artefacts typical of the South Island intrusion of the Post-Fleet ... or Classic ... Maori Culture.' (Duff 1961:286). He was then able to assert that 'the Classic Maori culture appears in sites associated with the sudden incursion of the Ngati Mamoe in the mid-sixteenth century and the Ngai Tahu in the mid-seventeenth', and list a number of specific artefacts as intrusive (Duff 1963:33).

Golson did not commit himself on this point. 'The vehicle for the spread of Classic Maori to Murihiku may well have been the Ngai-Tahu' (1959a:60), although he revealed his opinion more clearly with the remark that in the South Island 'Classic Maori is obviously intrusive' (1959a:67).

This view was to prevail for more than 20 years. Hjamo (1967) and Simmons (1973) both saw evidence of abrupt change they attributed to intrusion, although Foss Leach (1969) found some evidence of continuity in stone flake assemblages. At a conference in 1977, I raised the possibility that some typical Classic Maori artefacts may have originated in the South Island (Sutton 1977:145). However, Helen Leach (1978:107), after a careful review of the evidence concluded 'on the grounds of material culture alone, the thesis of a substantial migration southwards would appear to have been proved', although she pointed to problems posed by linguistic and traditional evidence.

Excavations at Long Beach provided some evidence of continuity.

There can be no doubt that some of the Classic Maori artefact styles are intrusive (e.g. the kinked pendant, the curved cloak pin, the desire to decorate with serrations), but we have also demonstrated that in those aspects of material culture that are closely geared to the successful exploitation of the environment, such as the design of fishing gear and stone tools, there is convincing evidence of continuity (Leach and Hamel 1981:139)
In his review of North and Central Otago, Anderson expressed the opinion that the general thesis that Classic material culture arrived with the immigrant groups, which included the Ngai Tahu, appears to remain sound. Yet it is worth noting that the new material culture is only partly represented by new types ... It is equally a case of new ideas being expressed upon older types (Anderson 1982a:123), thus returning almost to Duff's original view.

In the succeeding 25 years, relatively little progress has been made in this area. Golson had set a standard in area excavation seldom matched since. The excavations at the Sarah's Gully settlement exposed approximately 770 m², excluding the midden areas. The Kauri Point excavations of about 400 m² were matched by other pa excavations during the 1960s, but not greatly exceeded until a mechanical excavator was used to expose about 2000 m² at Ruahihiri in 1978 (McFadgen and Sheppard 1984).

Area excavation in itself does not necessarily produce results in the form of social interpretation, and for a long time there was little or no advance on the pioneering work of Groube (1964, 1965) on settlement patterns. One important development was Prickett's (1974, 1979, 1982) work on the Maori house and its symbolism. The entire Palliser Bay research programme placed major emphasis on the identification and study of prehistoric communities (B.F. Leach 1976; Leach and Leach [eds] 1979:251-72).

In his writings on New Zealand, Golson paid relatively little attention to social issues. Some comments in his 1965 critique, however, reflect views about such matters.

The postulation of a free-wandering stage in New Zealand prehistory is open to criticism on theoretical grounds: it would seem to require the reinvention of Polynesian forms of organisation by subsequent generations ... It would need an extremely depleted colonising group indeed for the residential practices of a prior existence to be completely abandoned. (Golson 1965:87)

In the same paper, Golson pointed to the need for studies of settlement type and concluded

The investigation of structures, to which increasing attention is now being paid, is vital in this regard. Valid interpretation of settlement sites, however, depends on extensive and costly area excavation, which cannot be often or lightly undertaken. Herein, however, lies the key to much that is at present obscure or in dispute ...

(Golson 1965:91).

And he proceeded to speculate about possible social reasons for the efflorescence of some artefact forms in the southern Archaic.
principles which underlay past societies (Sutton 1990). He has endeavoured to apply these principles to the study of structures, from houses to pa, and to the investigation of the origins of what he calls the northern Maori chiefdom. These developments may herald a return of conceptual direction in the 1990s from ecological to cultural (cf. Golson 1986:10), but to a cultural direction very different from that practised by Golson in the 1950s.

The differential distribution of the Maori population in the late prehistoric period has long been appreciated, and successive archaeologists have used or referred to the threefold division of New Zealand proposed by Cumberland and Lewthwaite. Yet discussions of the Maori population growth (Houghton 1980;108-9; Law 1977:104; Brewis et al. 1990) have tended to model growth as if it was uniform throughout the country, as if fertility, mortality, and life expectancy were the same in Northland and Murihiku. This could only be the case if there was regular and substantial out-migration from areas like Murihiku. A better understanding of demographic trends and constraints in different regions might help to illuminate processes of change.

This issue as it applies to southern New Zealand was posed by Hamel (1982:139):

given a choice between the hard work of growing crops and raising many babies or the lesser work of not growing crops and raising fewer babies, what did people with a stone technology do? ... We can assume that the first occupants and successive invaders of Murihiku brought with them such characteristics of agriculturalists as a stratified tribal society ruled by chiefs, a belief in the efficiency of the defended pa as a means of resolving conflict and a range of conscious and unconscious means of population control, quite different from those of hunter-gatherers.

The demographic problem of southern New Zealand is matched by the social problem. Despite their low population density and non-agricultural economy, the proto-historic Ngai Tahu were members of a ranked society — in fact of a chiefdom (Anderson 1980:17). One of the challenges facing New Zealand prehistorians in the 1990s is the investigation of demographic and social variation in a prehistoric culture and society that extended from true agriculturalists in the north to true hunter-gatherers in the south, yet linked them through a multitude of channels of communication and exchange.

CONCLUSION

In some of the areas reviewed above, New Zealand archaeology has made notable progress since 1954. These include the study of agriculture and pa origins, and the development of regional sequences. Radiocarbon dating, despite its difficulties, has proved useful in all these areas. If little progress has been made in typological studies of material culture or in the identification of types diagnostic of particular phases, this has been partly compensated for by new directions in the study of adzes, for instance, which have brought better understanding of how these tools were made and used. Faunal analysis, the sourcing of stone and the study of exchange systems, not reviewed here, have flourished. Yet little progress has been made in developing theoretical frameworks to guide the study of New Zealand prehistory to the end of the century and beyond. Pointers to the future are perhaps to be found in the demands of Sutton and Anderson for more attention to social interpretation, and in the latent but ever present desire of New Zealand archaeologists to reach a satisfactory accommodation with Maori tradition. What are needed, however, are new theoretical models of change in New Zealand prehistory, based on the abundant data accumulated in the last 35 years, and the development of research strategies to investigate them.

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REFERENCES


CULTURAL RESOURCE MANAGEMENT IN AUSTRALIA: THE LAST THREE DECADES

Josephine Flood
Aboriginal Environment Section, Australian Heritage Commission, Canberra, ACT 2601, Australia

My first memory of cultural resource management issues in Australia was a visit to the Aboriginal campsite at Pialligo outside Canberra with Jack Golson and other members of the newly formed Canberra Archaeological Society. The year, I think, was 1964. Jack was the Society's president from 1964 to 1968.

There was then no legislation in place to protect Aboriginal sites in New South Wales or the Australian Capital Territory. Legislation was enacted in New South Wales in 1970 (in the form of amendments to the already existing National Parks and Wildlife Act 1967), and still has not come into being in the Australian Capital Territory, although this is likely to happen in mid 1991.

The only legislation in place to protect Aboriginal sites and artefacts in the 1960s was the Northern Territory’s Native and Historical Objects and Areas Preservation Ordinance 1955-1961 (Edwards 1975:214-7). (Ironically, this pioneering piece of legislation has never been revised, and now seems extremely dated, containing phrases such as 'a fine not exceeding one hundred pounds'.) However, the Australian Institute of Aboriginal Studies was founded in 1961, and instituted a valuable site-recording programme, which led to the employment of a number of archaeologists in State site authorities.

The Pialligo site was the major campsite of the Ngunawal people who traditionally occupied the Southern Tablelands. It was on a gentle slope beside a quiet stretch of the Molonglo River. Numerous middens of freshwater mussel shells, the remains of past meals, were scattered over the sandhills, together with many hundreds of stone artefacts. Notable were many finely made, long slender backed blades, made of siliceous materials from the nearby Fyshwick gravel beds (Bindon 1973:4-11). Matilda House, one of the surviving Ngunawal people, tells me how she remembers being taken out there by her grandfather and playing amongst the shells on the sandhills.

Unfortunately, part of the site lay at the end of the main runway of Canberra airport, part in an area which was selected for the Government Nursery, and the sandhills area was chosen for Canberra's main rubbish dump. When we went out to the site in 1964 we went on a salvage mission; I don't think it occurred to anyone there that we should battle to save the remnants of the site from further destruction. And yet there were plenty of alternative places for a rubbish dump.

In the 1960s people were thinking in terms of mitigation of the effects of development rather than cancellation of such projects. It is easy to be wise after the event, but in the 1960s there was virtually no legislation to protect cultural or natural sites, nor any cultural resource management or managers. The salvage archaeology done at Pialligo was therefore enlightened by the standards of the 1960s, but sadly lacking by those of today. The idea that a development could be postponed or cancelled in order to save an important site from destruction came later, as did that of consulting with Aboriginal people about the future of their sites.

It is when one looks back over the last three decades that we see just how much progress has been made. There are still plenty of problems, but there is now, I trust, no way that a site like Pialligo could be known and yet be destroyed.

Much of the impetus to provide better protection for Aboriginal sites came from archaeologists. Their thrust for legislation to protect Aboriginal sites and artefacts and to regulate excavation bore fruit, and meant that by the end of the 1970s there was 'blanket' protection in all States for Aboriginal 'relics' (i.e. sites did not have to be already recorded or registered to be protected). The legislation was of varying quality and scope, but at least it helped to curtail illegal collection or excavation of artefacts or destruction of sites by developers.

It is no exaggeration that much of the Aboriginal heritage of Australia owes its survival to the timely and tireless pioneering efforts over
the last three decades of academic archaeologists such as Jack Golson, John Mulvaney, Rhys Jones and Isobel McByde. This period likewise saw the beginning of 'public archaeology', its leading proponent being Sharon Sullivan, then head of the Sites Section of the New South Wales National Parks and Wildlife Service and now Director of the Australian Heritage Commission. Professor Golson was a member of the pioneering 'Relics Committee' of the New South Wales National Parks and Wildlife Service, and played an important part in development of consultation and participation of Aboriginal people in cultural resource management (Sullivan 1983).

Archaeologists were far more active over the last three decades in the cause of cultural conservation than anthropologists (in the narrow sense). There was little or no protection for sacred sites (also known as mythological, Dreaming, story or totemic sites). The existence of sites which might contain no visible material traces of Aboriginal activity but nevertheless be of great significance to Aboriginal people was not in the 1960s or 1970s even recognised in Victoria, Tasmania, South Australia or Queensland. Although there has been enormous progress during the 1980s, the legislation in Queensland and Tasmania remains far from satisfactory on this point.

The historic heritage has also had its champions, and the first heritage site to be declared by law was Captain Cook's landing place at Kurnell south of Sydney, in 1899. The National Trust came into being in Australia in 1945, as a voluntary non-government body primarily concerned with preservation of historic buildings. However, legislation to protect the built environment has been slow, and by 1991 is operational in only four States: New South Wales, Victoria, South Australia and Western Australia (See Australian Heritage Commission 1989 for a description of legislation relating to the national estate in Australia).

Among historians there has, until recently, been a remarkable lack of interest in heritage preservation, with a few notable exceptions. The Royal Australian Historical Society was founded in 1901 and there are now hundreds of historical societies around the continent, but they have maintained a consistently low profile regarding the numerous heritage issues which have come to public attention over the years. A few years ago the Australian Heritage Commission wrote to every historical society in Australia requesting nominations of significant historic places to the Register of the National Estate, and received not a single reply! (Yes: the letters were posted...)

One unique feature of the development of Cultural Resource Management in Australia has been the involvement of the Union movement. Green Bans were imposed in the 1970s by the Builders Labourers Federation, who refused to work on a site where to do so would lead to the destruction of a heritage place (Mundy 1981). This was an effective mechanism for both preserving Australia's heritage and bringing heritage issues to the attention of the media and thus the general public.

I use the term 'heritage' in its true, broadest sense, to encompass the Aboriginal, historic and natural heritage. There has been an unfortunate tendency in recent times to use the term 'heritage' to mean only the built environment. Such a narrow view is repudiated by the founding parents of the Australian Heritage Commission, the Commission of Inquiry into the National Estate chaired by Justice Hope (1974). In their report the terms 'heritage' and 'national estate' are used as synonyms, and as a result of the report the Australian Heritage Commission was set up in 1975 to compile a Register of the National Estate.

The national estate was defined as those places, being components of the natural environment of Australia, or the cultural environment of Australia, that have aesthetic, historic, scientific or social significance or other special value for future generations as well as the present community (Australian Heritage Commission Act 1975).

A development of the second half of the 1980s has been a growing awareness of the national estate and the need to preserve it. The term 'national estate' now appears in newspaper reports without continual explanations of what it means. This has happened particularly in the course of conflicts between the conservation movement and the forestry industry about the destruction of old growth national estate forests. The term has well and truly entered the national vocabulary now, although not yet the Macquarie Dictionary. It is used primarily to refer to 'green' issues, but of course applies equally to cultural places.

The Commission sees its goal as the identification and conservation of the national estate. This mission may not seem particularly controversial, but throughout its existence the tiny Commission has had to battle against the Goliaths of the Development lobby. For only eighteen months in fifteen years has the Commission not been under review. The first attack related to Aboriginal land rights, and in 1976 amendments were made to the Australian
Heritage Commission Act of the previous year. A new section (23[5]) was added to make it difficult for Aboriginal sites to be put in the Register of the National Estate unless they were already 'specially protected' under State or Federal legislation. Section 23(5) was finally deleted because it was clearly discriminatory, when the AHC Act was amended again in December 1990.

In order to try to resolve some of the thornier conservation issues, such as the question of mining at Coronation Hill in the Kakadu region, the Federal Government set up the Resources Assessment Commission (RAC) in 1989. Coronation Hill is a registered sacred site; it also contains one of the richest gold deposits in the world. In this situation the evidence of anthropologists and archaeologists is vital, and once again staff of the Department of Prehistory at The Australian National University are at the forefront in research on the archaeological significance of the area.

A new facet of the RAC inquiries into forests and into the Coronation Hill issue is the use of contingency valuation surveys. These in essence try to answer the question, 'What price heritage?' 'How many dollars are people prepared to pay to save Coronation Hill?' There are many problems with this type of survey, one being the hip pocket response. Thus people in Sydney and Melbourne are prepared to pay (theoretically) far more for the preservation of this heritage site than those in the Northern Territory, who might suffer economic loss, or at least the lack of economic gain, if the project were not to go ahead.

In addition to the RAC inquiries, new Resource Security legislation has been brought in in 1991. This may well (among other things) lessen the power of Section 30 of the Australian Heritage Commission Act in protecting natural and cultural resources in national estate forests. Since the Commission has already been categorised as a toothless tiger, this fresh assault smacks of overkill. Perhaps the real problem for developers is public opinion, which has become increasingly sympathetic to heritage conservation over the last three decades. Whilst in times of recession, the pendulum may swing back a little, there is a growing public awareness and sympathy for conservation of our heritage, environment and planet Earth.

The 1980s have seen the coming of age of the conservation movement in Australia. It has been a decade of battles, some won and some lost, between conservationists and developers. The biggest battle was that for the Franklin River in southwest Tasmania, to prevent the region being drowned by a hydro-electric scheme. The final victory was due to two principal factors, the inspiring leadership of Dr Bob Brown and the archaeological evidence which was of major significance in winning the High Court Case in 1983.

Surveys for cultural sites were omitted from the Tasmanian Hydro-Electric Commission's Environmental Impact Studies, and it was only through the initiative of Drs Rhys Jones and Jim Allen in Jack Golson's Department at The Australian National University and Don Ranson, archaeologist in the then Tasmanian Department of National Parks and Wildlife, that an archaeological survey of the Franklin was carried out, and the archaeological potential of Fraser (later Kutikina) Cave was discovered. The rest is history, and has been told elsewhere (Green 1981; Flood 1987), but one fact that perhaps is not well known is the soliciting by some of Australia's leading archaeologists of letters of support from leading overseas archaeologists. These proved highly effective, which is a sad indication that the cultural cringe is not yet over.

The increasing involvement of 'academic research archaeologists' in the public arena is a reflection of both a growing conservation ethic and the development of contract consultant archaeology as part of a university department's activities.

A 'conservation ethic' has been widely accepted among Australian archaeologists. The main precepts are that the archaeological 'resource' is non-renewable, so archaeologists should minimise any adverse impacts of their own research on this resource, and also campaign vigorously in the public and government spheres for its conservation.

A code of conservation philosophy and practice is enshrined in the Burra Charter, developed by the Australian National Committee of ICOMOS (the International Council on Monuments and Sites) (Australia ICOMOS 1988 quoted in Flood 1989b). The Burra Charter is an Australian development of the International Charter for the Conservation and Restoration of Monuments and Sites, formulated at the Venice meeting of ICOMOS (part of UNESCO) in 1966. It was adopted by Australia ICOMOS at Burra Burra on 19th August 1979 (at about 2 a.m. as I recollect!) The Charter and accompanying guidelines cover the conservation of all places of cultural significance, the assessment of cultural significance and regulation of archaeological work.

Australia ICOMOS came into being in 1976, and has been a fundamental player in raising professional standards in the field of heritage
conservation practice. The success of the Burra Charter came partly from its early appearance on the scene; the first version was adopted in 1979 after some memorable nights thrashing out the wording during that historic meeting at Burra Burra. A further important development was the adoption by the Australian Heritage Commission of adherence to the Burra Charter as a condition of National Estate Grants. Now in 1991 an illustrated Burra Charter is being produced in a more user-friendly format.

Cultural resource management is of necessity multi-disciplinary, and the professional membership of ICOMOS includes archaeologists, architects, planners, historians, heritage managers, museum curators and many other disciplines. However, the focus has tended to be rather narrowly confined to the conservation of structures and sites. There is a need to conserve other types of evidence as well as the physical fabric; historic documents, photographs, oral traditions, artefacts and records may be of equal importance.

Artefacts have become the poor relation in cultural resource management, largely perhaps because the recommendations of the Hope inquiry vis-a-vis artefacts have never been implemented. In spite of the Hope committee's recommendations, moveable artefacts were specifically excluded from protection under the Australian Heritage Commission Act, and no parallel body has been set up to fulfil this role.

There are therefore still some gaps in legal protection for artefacts, particularly historic ones. However, Australia finally in 1985 became a party to the 1970 UNESCO Convention on the Means of Prohibiting and Preventing the Illicit Import, Export and Transfer of Ownership of Cultural Property, by the passing of The Protection of Movable Cultural Heritage Act. There is also the Aboriginal and Torres Strait Islander Heritage Protection Act of 1986, administered by the Aboriginal and Torres Strait Islander Commission (ATSIC). This provides a means for Aboriginal people to apply to the Minister for Aboriginal Affairs to protect their sites or artefact collections when under threat.

Archaeological heritage management, or public archaeology, has developed as a profession in its own right and now attracts far more funding and employs far more archaeology graduates than do academic institutions. Indeed, many of Jack's ex-students now occupy high level positions in this field. Environmental protection legislation and the need for environmental impact studies provide much work for consultant archaeologists. The Australian Association of Consulting Archaeologists (AACA) has done valuable work in developing a code of ethics for client relations, producing bibliographies of contract reports, and generally raising the professional standards of its members and others.

Archaeological practices have changed for the better over the years. Consultation with the relevant Aboriginal traditional owners or custodians before any archaeological work is undertaken is now virtually mandatory. Artefacts on open campsites are often now studied in situ rather than being collected, and, if excavation occurs, only a small percentage of the site is affected.

Great progress has been made in the 1980s in aspects of cultural resource management (CRM) such as the assessment of significance of sites (Sullivan and Bowdler 1984), and site survey, recording and registration techniques (Flood et al. 1989; Connah 1983). Management of sites has also received considerable attention. Professor Fay Gale and Jane Jacobs have pioneered methods of protecting rock art sites from visitors, published in their seminal book Tourists and the National Estate: Procedures to Protect Australia's Heritage (1987). This has been followed by a useful volume on practical conservation measures for site managers by David Lambert (1989).

Another valuable work resulted from a workshop held in Kakadu National Park in 1983 on the question of Visitors to Aboriginal Sites: Access, Control and Management (Sullivan 1984). This canvassed issues such as conflicts between the needs of Aborigines living in out-stations in national parks and normal park management practices such as the banning of dogs and four-wheel-drives from ecologically fragile areas. There have been many other works on cultural resource management in recent times (Flood 1989b), and the Australian Heritage Commission published a bibliography on CRM in Australia in 1990.

Recently the focus has shifted to the development of cultural tourism – benefits and problems. There is an increasing public interest, among both Aborigines and non-Aborigines, in Aboriginal cultural heritage. This has arisen from the renaissance in Aboriginal culture and the efforts of educationists to put Aboriginal studies into school and tertiary curricula. The result has been the production of numerous school kits on Aboriginal history and culture, the establishment of Aboriginal cultural centres, an increasing literature in this field (e.g. Flood 1989a, 1990; Stanbury and Clegg 1990), and a growing Aboriginal role in tourism.
There have been some notable successes, such as the Brambuk Centre in Halls Gap in Victoria and the Dreamtime Centre in Rockhampton which came into being in 1988 through the vision of one outstanding Aboriginal woman, Nola James. Totally Aboriginal run and staffed, the centre not only gives the visitor a positive view of Aboriginal society but is also almost paying its own way.

The growth of cultural tourism has also caused some problems. Introduction to Aboriginal sites by Aboriginal tour guides provides a memorable experience for the tourist, but can also lead to a loss of control by custodians over visitation to those sites. For example, Bill Harney’s Jankangyina tours to the Land of the Lightning Brothers in the Northern Territory take tourists to visit a number of photogenic rock art sites in the Katherine region. The tourists take photographs, and some, such as local hoteliers, later introduce other tourists to the sites. The whole exercise snowballs, and soon pictures of the rock art appear in commercial advertisements or are reproduced as jewellery designs. In other words, the development of cultural tourism can lead to traditional owners’ loss of control over their heritage.

Consultation, custodianship and control of the Aboriginal heritage have been the issues which have dominated the Australian archaeological world through the 1980s into the 1990s. Aboriginal people have become active and vocal in demanding not only a say, but control, over their own heritage. Much progress has been made, particularly in requiring researchers to consult the people whose heritage they wish to study. A requirement that consultation take place has been built into new Aboriginal heritage legislation, such as that now in place in South Australia and Victoria.

Virtually every State sites authority employs Aboriginal sites officers, and more and more Aborigines are doing tertiary courses in Aboriginal studies or heritage management. The Australian Heritage Commission has employed the first Aborigine or Torres Strait Islander to work in northern Australia in the dry season in April 1991 that the University of Edinburg, one of the major overseas holders of Aboriginal skeletal remains, has agreed to return the bones to Australia. The collection comprises four skeletons and about 295 skulls and cranial fragments.

Although some remains have been given back for re-burial by both Australian and overseas institutions, there remain many concerns. The issues were thoroughly canvassed at the 1989 Conference of Museum Anthropologists (Galla 1990), but there is no consensus view either among Aboriginal or non-Aboriginal people about the fate of prehistoric material from sites
such as Kow Swamp and Mungo (Mulvaney 1991). This whole issue has been the source of much bitter conflict, not helped by often ill-informed and sensation-seeking media coverage.

A similarly bitter debate occurred in the 1980s over the issue of the repainting of rock art sites in the Kimberley under the government-funded Community Employment Program (CEP). At the Australian Rock Art Research Association (AURA) conference in Darwin in 1988 both Aboriginal and non-Aboriginal people were deeply divided on the issue. It is hoped that the forthcoming publication of the proceedings of that conference will reflect the widely differing views and complexity of this difficult question.

In conclusion, the last three decades have witnessed enormous progress in the care of Australia’s heritage. Appropriate legislation has been widely developed and Australia’s cultural resources are now generally well-managed. Attention has gradually shifted from the development of management and conservation techniques and their implementation to other questions such as the involvement of Aboriginal traditional owners and custodians in heritage management, the promotion of Aboriginal studies and the restitution of Aboriginal cultural property such as human remains.

It has been said that ‘the past is a foreign country’, but the future is also a foreign country. Nevertheless, it can be predicted with confidence that there will be ever-increasing Aboriginal involvement, participation, and eventually control of cultural resource management and the study of the Aboriginal past. It is to be hoped that this transition can be accomplished without undue conflict in a spirit of goodwill and cooperation with those whose past efforts have done so much to identify, conserve and promote the Aboriginal cultural heritage.

REFERENCES


Decolonisation around the world has frequently been accompanied by the assertion of identities distinct from those imposed by the colonial powers. The language and practise of these movements have been a feature in the Pacific region, where the first decolonisation – that of Western Samoa – came in 1962. Some parts of the Pacific still remain in a colonised state, while others fall into what has been termed the Fourth World, indigenous minorities in their own land. Despite this complex of conditions, there has been a common movement throughout the Pacific over the last 25 years towards the assertion of regional, national or group-specific cultural identities. In this paper I shall look at some aspects of this movement relating to museums, particularly in terms of historical elements in the cultural heritage field and practical problems in the achievement of objectives. A key theme will be that despite considerable expenditure of funds and effort, in many areas little progress has been made, not because of indigenous incompetence, but because of the inappropriateness of certain measures and certain problems endemic to the region. The convergence of these two lines currently leaves most parts of the Pacific in a relationship with the external world which in essence, differs little from that of the colonial era. There are some successes, but the re-evaluation of approaches and objectives as well as a new form of cooperative effort are essential if they are to become actual.

CUSTOM AND IDENTITY

These two terms have become rallying points for islanders seeking to raise awareness of a cultural 'otherness' distinct from the colonial or quasi-colonial condition. By appeals to tradition, an identity unique to a group can be asserted. This has occurred largely 'in a contrastive relationship with an opposing semantic category, the non-kastom (modern or foreign)' (Lindstrom 1982:317; cf. also 1980). The term 'kastom' is a symbol that 'can mean (almost) all things to all people' (Keesing 1982:297; cf. Tonkinson 1982:302). Its use in political rhetoric throughout the western Pacific has contributed greatly to a sense of regional and national unity and identity, even though the reality may be one of diversity and 'kastom' does not carry, but evokes, meaning (Keesing 1982:299).

At the regional level, political leaders have referred to the 'Pacific Way' (Crocombe 1976) and the 'Melanesian Way' (Narokobi 1980), invoking a regional relatedness that overrides imposed geopolitical boundaries. Such references recognise the essential unity of history of the Pacific Islands as revealed by anthropology, archaeology, human biology and linguistics. It is often the Western scholar who emphasises difference for purposes of academic study, and creates artificial divisions that are not wholly supported by archaeological research (Golson 1961:176; cf. Green 1991). At the same time, the invocation of such a unity has been counterbalanced by the assertion of separate cultural identities at a sub-national level, particularly in the west (e.g. Ravuvu 1977:17). For political purposes this emphasis on commonality provides a sense of 'otherness' essential for coping with the external world, while reference to difference supports the local populations in defining their positions within the new nations.

An almost inevitable result of this generalisation through rhetoric has been the creation of an idealised past upon which the present may be based and from which the present has declined. This is not to denigrate the reality of respect for the past, which for many Pacific Islanders is a deep-seated emotional issue, but reflects an uncertainty about the present. It represents what Lindstrom (1982:317) describes as 'an attempt to read the present in terms of the past by writing the past in terms of the present'. At the same time it provides a reference point for pride in the human heritage, even though aspects of that heritage may have become distorted through idealisation. In this sense, the situation differs little from the experience of Western Europe on the 'rediscovery' of the ancient worlds of Greece and Rome which led to a sense of pan-Europeanism: there was a common cultural heritage the roots of which lay in a classical
golden age when life was 'pure, simple, and moral' (Lowenthal 1988:730). At the same time, some Pacific Islanders have displayed a more practical, and pragmatic, approach to cultural needs of the present by recognising that there was 'no state of cultural priority (or perfect state of cultural goodness) from which there is decline: usage determines authenticity' (Wendt 1980a:25).

In such a context of competing and conflicting appeals to custom and unity in the Pacific, cultural heritage matters are placed in an invidious situation. Few Pacific nations have comprehensive cultural policies, let alone policies that fully explore the ramifications of this conflict (cf. Crocombe 1980). The Constitution of the Independent State of Papua New Guinea specifically identifies the debt that the present owes to the past, and pledges 'to guard and pass on to those who come after us, our noble traditions' (Crawford 1977:9). Yet there is no adequate document that states how this is to be achieved, particularly in the context of a society undergoing extensive and rapid change. In this Papua New Guinea is not alone, since few nations in the world have succeeded in producing such a document (cf. Battersby 1980). But in the Pacific context more progress might have been expected, given that culture and independence politics have long been closely interlocked. In Vanuatu, the Vanuaku Pati that took Vanuatu into independence began as the New Hebrides Culture Association (Kele-Kele 1977:24), and in Papua New Guinea the Pangu Pati has long championed cultural heritage matters.

**VALUE FOR MONEY**

Notwithstanding this deficiency of cultural policies, there have been large expenditures of effort and funds on heritage services both before and after independence in virtually all Pacific states. Have these states obtained value for their money? It is tempting to suggest the answer 'no', since the majority of problems and deficiencies existing at the time of independence remain a source of concern, and no state's cultural activities are conducted in a sustainable way.

Such a negative response requires clarification, and this must be sought in historical and other factors. Perhaps the most significant factor is that as far as cultural matters were concerned, the decolonisation process of the 20th century had no historical precedents to provide guidelines. There was no model derived from appropriate experiences elsewhere and from other times to provide adequate frameworks for developing and implementing cultural services, other than from the very same countries that were the former colonial rulers.

In the Pacific, emphasis was placed on three elements. Firstly, most island nations comprise many, often hundreds, of islands, often scattered over vast areas of ocean. This presents major logistic and communication difficulties. Secondly, in the case of the Western Pacific, the new nations were faced with considerable linguistic diversity. Thirdly, the historical orientation of western cultural heritage services was felt by many to be inadequately addressing the needs and potential of the new nations.

It was in this context that many Pacific Islanders questioned the relevance of Western-style museums. In Europe, museums had grown out of many interlocking strands, but particularly through the collecting activities of private individuals. For most, there was an element of curiosity associated with the exploration of the new and unknown, as well as the pleasure of possessing 'artificial curiosities' (Kaeppeler 1978). The late 18th century saw the development of many such collections into museums. William Bullock declared the objective of his museum to be 'for the advancement of the Science of Natural History', with the added bonus that:

> by displaying it advantageously for the Study of the Naturalist, the Instruction of the Curious, and the Amusement of those who are delighted in viewing the Beauties of Nature, or the Curiosities of Art, he has endeavoured to make it worthy of the British Metropolis (Bullock 1812:iii-iv).

The formation of these early museums - most of which ended up under the auctioneer's hammer - was followed by European colonisation and missionisation of the Pacific. Both brought new power structures and relationships to the islands, based upon external dominance (cf. Donaldson and Donaldson 1985:15). For the missions this included the power to compel or coerce islanders into destroying or surrendering the material expressions of their former belief systems (e.g. Crawford 1981; Weymouth 1988). Many of the artefacts ended up in mission museums or private missionary collections, where they could be seen as tangible proof of the need to Christianise the islands. For the colonial administrators, the collecting of artefacts often was an adjunct of the 'pacification' process, in which the removal of symbols of power and the means for aggression seems to have been regarded as essential for the imposition of colonial rule. For planters, traders and others, buying and selling of artefacts provided yet another way of earning a dollar, for there were ready markets of individuals and museums around the world. For the islanders
this often meant access to new goods in exchange or cash with which to buy them, and there was essentially mutual benefit (cf. McBryde 1988 on transactions carried out during the early days of the settlement at Port Jackson). Occasionally, however, transactions occurred in which no fair return was made by the buyer, and artefacts were effectively stolen, in some cases under threat of violence.

While not all artefacts were collected under some form of duress, for many islanders in the late- or post-colonial era the museum collections held overseas were symbolic of a cultural rape that their ancestors had been powerless to prevent. This is supported by the attitudes expressed by some museums in the colonial era which declared that their role was to present tangible evidences of imperial power or to preserve relics of peoples and lifeways that were likely to become extinct as a result of colonialism. The former reached its most explicit statement in 1910, when Charles Read wrote of the British Museum:

At no point in the world's history has any nation exercised control over so many primitive races as our own at the present time, and yet there is no institution in Great Britain where this fact is adequately brought before the public in a concrete form (Read 1910:vii).

In Australia an anonymous contributor to a Sydney magazine in 1828 urged the development of a museum for the 'public exhibition of the natural productions and curiosities of Australia'. In doing so, such a museum would 'raise her (i.e. Australia) in the estimation of the world at large' (cited in Strahan 1979:8; a similar view of improving national image and public taste was used to justify the British government's purchase of the Parthenon marbles: Lowenthal 1988:729; Greenfield 1989:67).

In the early days of the Australian Museum in Sydney, George Bennett, an English gentleman-traveller, advocated that the new museum should include in its collections both the artefacts and the physical remains of Aboriginal people as 'lasting memorials' to a people doomed to extinction (Specht 1980). In the Pacific, the collections made by Sir William MacGregor in Papua had a similar objective (MacGregor 1897:88-9; cf. Quinnell 1986:204). As late as 1902, the Curator of the Australian Museum, Robert Etheridge Jr, helped to set up the New South Wales Ethnological Committee to promote the development of Aboriginal collections in his museum. The concerns of that time reiterated the 'interests of science' reminiscent of Bullock's objectives, as well as the motive of preventing Aboriginal artefacts from going to overseas collections (Thorpe 1931:6-7).

For many Pacific Islanders, then, museums in overseas countries could be, and were, viewed as part of the process of colonial control. This was reinforced by the fact that although museums had been established in the Pacific as early as 1904 in Fiji (but see Rose 1980 for an earlier date in Hawaii), all were the result of expatriate action and were under expatriate control. For many islanders, such museums were part of the colonial world, for the expatriates did little to make their museums relevant to the indigenous peoples. This was in part because museums such as those in Port Moresby (founded 1913: Australia 1914:129, 137) and Rabaul (founded some time prior to July 1923: Australia 1925:43), were poorly funded and lacked competent staff (in Rabaul the museum came under the Department of Agriculture, see Australia 1926:32). Indeed, so inadequate was government support for their maintenance that the entire contents of the Port Moresby museum were transferred to Sydney for safekeeping (Australia 1916:125), in much the same way that MacGregor's collection had been transferred to Brisbane (MacGregor 1897:89). In the Mandated Territory of New Guinea, the Australian Administration was similarly placed, and transferred items from the Rabaul Museum to Canberra in 1933 (Bolton 1980:86).

It is hardly surprising, then, that many Pacific Islanders felt alienated not only from museums in general, but also from even those set up in their own countries. For many islanders, museums were simply not relevant to their needs, and some even viewed them as holding 'the unimportant relics of almost forgotten cultures' (Eoe 1990:29). In the late- and post-colonial periods, therefore, comparatively few islanders were openly supportive of museums in their countries. The western-style museum was viewed as a dead and irrelevant institution redolent of the power of colonialism. Worse still, being located in the colonial capitals and lacking any kind of outreach programmes, meant that the museums were essentially inaccessible to the majority of people who lived in rural contexts. The museums were serving an urban-based elite.

This reaction led to the emergence of the 'cultural centre'. Roger Duff (1971:189) was apparently the first to put forward ideas along these lines in the Pacific, but since then the idea has been developed and championed by a large number of people (e.g. New Zealand National Commission for UNESCO 1975; Wendt 1980b; Crawford 1977; Danielsson 1980c). The cultural
centre is seen as a complex of activities, emphasising the contemporary as well as the historical; and embracing, in addition to a museum, library and archives, craft workshops and performance areas. The centre is a self-conscious break from the museum-as-everything view (cf. Myers 1980:63 for his description of the Tiwi keeping place as 'a statement of self-determination').

The actual introduction of cultural centres may have taken place first in Papua New Guinea, with the employment in 1973 of an expatriate to assist the Gogodala people of the Western Province in a cultural revitalisation programme (Crawford 1981:163-4). In 1974 the Australian Government was promoting cultural centres at the 14th Conference of the South Pacific Commission (Australia 1974). Since then cultural centres have been established in most provinces of Papua New Guinea, in Vanuatu and elsewhere. In form and activities they vary greatly, from the national centre in Port Vila to virtually village-based centres in the Solomon Islands (Huffman 1979; Akin 1982).

The enthusiasm with which cultural centres have been adopted has not always been matched by a comparable degree of success. The reasons for this are simple: first of all, although the concept has been developed within the Pacific Islands, it was based on western perceptions of need and performance. Secondly, implementation has all too often been by inexperienced, usually untrained, staff. Thirdly, funding has been totally inadequate. Finally, the most difficult aspect of developing cultural centres has been the fact that they are alien institutions to Pacific Islanders. Indeed, at a major cultural heritage workshop organised by the National Museum and Art Gallery of Papua New Guinea in 1989, many participants had great difficulty in distinguishing between a museum and a cultural centre (Bolton 1990). The centres are agglomerations of activities, with little sense of integration. This is unfortunate, since much money and effort has been devoted, particularly by UNESCO, to their development over the last 20 years.

The meeting organised in 1971 by the then Australian National Advisory Committee for UNESCO at which Duff made his original proposal did not have one substantive paper presented by a Pacific Islander. This meeting was designed to implement a proposal first put forward by Australia and New Zealand in 1966 for a UNESCO-sponsored Project for the Study of Oceanic Cultures and adopted by UNESCO in 1970 (Kono 1971:11). A follow-up meeting in Fiji in 1971 was still largely dominated by non-islanders (Wendt 1980b:106), and it was not until 1974 that islanders began to take a prominent part in the Project through its Advisory Committee. Even then, despite protestations to the contrary, the agenda was still largely dictated by UNESCO headquarters in Paris, since most of the Pacific countries in the Project were not members of UNESCO, either because of their colonial status or because they had simply not joined following independence.

This situation led to continued dominance by the Paris-based decision makers and money-controllers. The reality was that power remained outside the Pacific, and even after the Project was given a Pacific-based Bureau, it had only an advisory role.

It is interesting to review the agendas of the various meetings of the Bureau and the Advisory Committee. As late as the 1982 meeting of the Advisory Committee in the Cook Islands, the principal items remained those originally identified at the 1971 meeting, not so much because they were the correct ones, but because they were those espoused by Paris and other so-called experts. The Advisory Committee had the right, however, to reorder the priorities, though the result often showed little substance in terms of funding and promotion of the identified priorities. Several studies were carried out to investigate the setting-up of cultural centres (e.g. in Western Samoa), but little was achieved.

Perhaps the most difficult aspect of the promotion of cultural centres has been that as a concept it was new and untried. There was no past experience on which to draw, though for Papua New Guinea Crawford (1977) prepared a workbook on cultural centre operations as he perceived them. UNESCO did not produce a similar volume, even though Duff prepared proposals (unpublished) for the Cook Islands. By serving merely as a catalyst, and not as a provider of guidance and capital funding, UNESCO created unfulfilled hopes and incomplete projects. Local governments turned to the idea of a cultural centre in the expectation of finding a cheaper and more effective alternative to the western models of preserving and recording aspects of traditional cultures and for delivering cultural services. They discovered that the centres are just as expensive, if not more so. Cultural centres with museum components cannot avoid the high operating costs of a properly managed museum, particularly if the centre is the main or only national body involved with cultural heritage matters. Also, frequently staff with absolutely no experience in the management of cultural pro-
grammes were appointed and then left to struggle, trying in all good faith to do an impossible job. In most island states, the problem of delivering cultural services to dispersed island groups was not addressed in a coherent fashion. Vanuatu addressed this issue by bringing people together for cultural festivals on both the main island of Efate and other islands (K. Huffman pers. comm.). The Solomon Islands National Museum organised a travelling 'loan case' covering cultural topics to be shipped around the islands for use in schools (Rogers 1982).

The injection of funds from local governments and funds and expertise through UNESCO and the Australian Government's South Pacific Cultures Fund (Langdon and Specht 1974, 1977, 1983) ensured the success of some projects, as in Vanuatu. But many centre projects ended very quickly, often with the loss of substantial sums of money. In Papua New Guinea, the National Cultural Council (NCC) had responsibility for the provincial cultural centres (Crawford 1977; Wari 1980), but was unable to provide a regular and reliable advisory and support service, even though the NCC had provided substantial establishment funds. In at least two cases, money apparently found its way into the wrong pockets, and artefacts collected for the museum component of the centre were stolen or sold. In some situations a new centre flourished as long as there was an expatriate 'minder', but on his departure activity declined and eventually halted.

In Western Samoa, there were at least two proposals for the setting-up of a national cultural centre, and a national museum project was actually started. The simple reality of cost scuppered the plans. UNESCO agreed to assist in the raising of funds (over US$1 million), but gave no other assistance. In doing so, UNESCO followed its traditional role of being a catalyst and facilitator, rather than a major funding body. Yet at the same time UNESCO dollars were going to projects in other parts of the world, including the endless round of meetings held under UNESCO's auspices. While Western Samoa was trying to raise funds for its proposed centre, ICOM (with UNESCO and French funding) sponsored a Pacific-wide meeting in Papeete, French Polynesia, in 1980 to discuss ways and means of protecting the natural and cultural heritage in the Pacific (Specht 1981). This expensive exercise effectively duplicated much of the work of the Project for the Study of Oceanic Cultures, and at least one of its outcomes could have been predicted before the very first session (O'Keefe and Prot 1982). The funds spent on this meeting were sufficient to run a cultural centre for at least one year.

While UNESCO was busy organising meetings with minimal progress, the Australian Government implemented its own cultural assistance programme for the Pacific Islands (Australia 1974). This programme is officially known as the Fund for the Preservation and Development of Pacific Cultures, but is known widely as the South Pacific Cultures Fund (SPCF). Initially it was set up for only five years but is still running 17 years later. As its name indicates, the SPCF is concerned with both the present and the past; with oral traditions, contemporary art and archaeology; with publishing in social sciences as well as providing funds for cultural festivals.

The SPCF differs from the UNESCO Project in three main ways: firstly, it is as simple to operate as possible, within normal audit and diplomatic conditions; secondly, its direction and allocation of funds are as wide as possible and only to islanders unless an island-based government or organisation specifically requests otherwise; and thirdly, it permits small-scale capital works support. SPCF has been able to ensure funds are spent wisely and where needed by having an immediacy of contact between Islanders and the Fund (which can be approached through any Australian mission in the Pacific), by making funds available easily and promptly and by keeping the scheme as simple as possible.

Notwithstanding the success of the SPCF, it has been unable to take effective action to improve museums and cultural centres in the Pacific unless specifically requested by governments and organisations. It has certainly sought value-for-money, but cannot force funds or action on another body. Some of the support provided by the SPCF and other non-UNESCO sources follows closely the priorities identified by the UNESCO Project (cf. Wendt 1980b:106ff). In one sense this has proved advantageous for the Pacific, since it has eased pressures on UNESCO to cover the full range of priority areas.

The outcome is that, with few exceptions cultural centres have not replaced the standard institutions of museums and libraries. These continue as before, though in the museum area there is evidence that at least one major Pacific museum is questioning its own performance and seeking appropriate new directions (Eoe 1990). In Vanuatu, the museum component of the National Cultural Centre continues its basic museum function separately from other activities, such as field worker bases and language courses, which have little bearing on the traditional museum role.
MUSEUMS, EXPORTS AND RETURNS

Despite much rhetoric and polemics against museums, they have retained both a presence and a purpose in the Pacific. No one has come up with a better way of protecting, preserving and presenting the cultural heritage. While there have been occasional calls to leave important heritage items in their village contexts, as happens in Papua New Guinea with many of the items that are declared National Cultural Property (Kaiku 1980), this exposes them to a range of dangers to their continued presence (Craig 1983) or existence. The haus tambaran in Kanganaman village, East Sepik Province, was severely damaged by an earthquake in 1980 (Holden 1975; Craig 1982; Swadling 1983:93). Although efforts were made to restore some parts of it, the cost of doing so and then maintaining it was apparently beyond the capacity of the villagers, who would have preferred to sell the most important carved posts to raise money to construct a new house. This was not possible, for the house is classified as National Cultural Property (B. Craig pers. comm.). Even though massive structures like the Kanganaman house cannot be transported to a protective area, most of the National Cultural Property items in Papua New Guinea could be housed in a museum-like environment, but at some cost.

Cost has been a major factor in the generally poor development of most museums throughout the world, and not just in the Pacific. In the Pacific, the new micro-states do not have the financial resources to cope with expensive heritage infrastructures. It can be argued that the decision makers have the power, and opportunity, to allocate adequate resources to museums and related cultural bodies, but the reality is that often such an allocation would leave other areas of society very badly depleted of funds. The size of the national economies of most Pacific nations is miniscule compared with even one of the constituent Australian states. As a result, many national museums and related facilities in the Pacific have relied heavily on external granting agencies (such as UNESCO, SPCF, the European Community and Japanese sources) for basic operating funds and equipment. Yet to make each museum fully relevant to its national context requires activities that would consume even more funds: a Catch 22 situation. In the Solomon Islands, one of the most original attempts to deliver cultural services to an island-based population spread over a large area of ocean (Rogers 1982) relied heavily on external funding. Yet the raising of such external funds becomes an impossible burden for most museum workers around the islands, where often the total staff of a national institution may be inadequate for even normal operations. There is, then, a never ending circle of problems that cannot be broken without massive, and long-term, external support.

This takes us back to the cultural centres. Many statements have been made about the need for cultural centres to become at least partially self-supporting through various fundraising activities. Unfortunately, many cultural centre projects were initiated as economic rationalism took hold of the capitalist world which supplied the island nations with their economic advisers. Museums and cultural centres were told to do more for themselves. In itself, such advice is not bad, but must be backed by sufficient support to make it viable. It is foolish to assume that a cultural centre can support itself by selling postcards and mementoes to tourists. The tourist dollar, so beloved by economic advisers, is a fickle currency, subject to worldwide trends of economic decline or improvement, fashions, and local conditions. Sponsorships for collection acquisitions, exhibitions or publications, are more satisfactory, since museums can approach potential donors with a clearly defined outcome of their support. Yet even this source of additional funding has its difficulties, for sponsors are reluctant to provide funds for essential operating costs; they prefer higher profile activities.

This is a very real problem in the Pacific, where each museum worker is required to wear several 'hats'. Because of the small number of people employed in senior positions, frequently the top person is absent on yet another overseas trip to attend a UNESCO, ICOM or other international meeting. The well-intentioned international gatherings – of which there is a steady flow each year – do not allow for such a low staffing situation. Instead of helping the Pacific nations who attend, they in fact create further difficulties.

A related problem arises in other aspects of heritage protection, especially in the policing of foreign researchers and export controls. Most Pacific nations now have some kind of research fee and/or bond for foreign researchers. This fee is supposed to be a cost-recovery exercise for handling the research application. Yet the level demanded often leaves little doubt that it is employed as an impost to help subsidise the managing body or some higher organisation in the host country. But given the totally inadequate funding of most organisations, foreign researchers should be willing to pay this impost in the hope that it will contribute to the continued
existence of the very bodies through which they work. This fee-for-service principle is compatible with practise in our western societies. In most areas of the Pacific where such fees are charged, researchers have accepted the principle, but there are many fields in which researchers do not have to pay. Some inequity operates here that should be corrected to the benefit of the struggling cultural institutions in the host countries.

The policing of export controls and other heritage legislation raises very serious problems. Most Pacific countries have some kind of legislative control over what may be excavated, purchased, sold and exported (O'Keefe and Prout 1982). Yet none of the relevant national institutions appears to have adequate resources to ensure the even and effective application of these provisions. The problem involves collectors and others in outside countries, for they are frequently the ones whose abuse of the provisions causes most difficulties. In the 1960s it was common to hear among the expatriate population that Papua New Guinea only introduced export controls over heritage items in 1965, or in later years that the lack of action by those responsible for the application of the provisions justified the illegal removal of heritage items. Such attitudes not only perpetuate the ruler/ruled mentality of the colonial era, but are in most cases patently wrong. Papua introduced export controls in 1913 (Australia 1914:112), New Guinea in 1922 (Australia 1923:41) (see O'Keefe and Prout 1982 for other Pacific colonies/nations). The problem was not one of the lack of regulatory controls, but of their consistent application.

As early as 1885-86 the Australian Museum was seeking permission to collect in the colony (Australian Museum Letterbook 1885-86:228), and although the special commissioner administering the protectorate refused his permission, he did so on the grounds of personal safety and not ethics. In 1921 Frank Hurley visited Papua to take photographs and film, but made the mistake of not obtaining permits to collect artefacts and natural history specimens for the Australian Museum, 'with the result that I was debarred from securing relics that would have been of great value and interest to the (Australian) Museum.' (Australian Museum Archives H42/22).

For his second visit in 1922/23 Hurley was more careful and obtained the required permits. Accompanied by biologist Allan McCulloch of the Australian Museum, Hurley made a large collection of artefacts and natural history, but when he applied for the export permit the Acting Administrator, Staniforth-Smith, refused to issue it. During their visit to the Purari Delta, Hurley and McCulloch were joined by Government Anthropologist F.E. Williams, who disapproved of their methods of obtaining photographs and artefacts. Williams later devoted a special issue of the Papua Anthropological Reports to the collection of artefacts, all but naming Hurley and McCulloch (Williams 1923). The matter was resolved many months later, after the return of the Lieutenant-Governor, Hubert Murray, but only after an acrimonious exchange of letters, some of which appeared in the local Sydney press (Specht and Fields 1984:6-7, 104). Some of the impounded artefacts were sent back to their villages of origin in the Purari delta, presumably on the evidence of Williams.

This action to prevent export from Papua was one of only a very few such steps taken before independence. In the late 1960s, after the enactment of the 1965 Ordinance controlling export, the then Public Museum and Art Gallery in Port Moresby successfully sought the confiscation of a collection of engraved human skulls from the Gulf of Papua made by a German collector (R.D. Mackay pers. comm.). But several very old malanggan carvings were exported from Tabar Island without a permit, and with no success in obtaining their return (D. Smidt pers. comm.).

As late as 1972 the Australian Museum was offered wooden implements excavated from the Manton Tea Plantation near Mount Hagen (Lampert 1970; Powell 1974), and the person making the offer openly admitted that he had ignored the regulations. These items were returned to Papua New Guinea, but only after the exporter was threatened with prosecution if he ever returned to the country (since his business interests were located there, this was a simple matter). His justification for his action was that the protective legislation had never been properly applied, and so was essentially a dead issue. Earlier in that same year several large consignments of heritage items were seized in Madang by the Papua New Guinea Customs, just prior to their export without a permit (Smidt n.d.). This latter action did not lead to prosecutions, but did result in a loud outcry by those who felt they had a right to act as they wished, even if contrary to local laws.

It is not widely appreciated that the earliest export controls were introduced in the Mandated Territory of New Guinea in 1922 (Australia 1923:41). The implementation of the New Guinea Antiquities Ordinance 1922 was the responsibility of the Director of Agriculture, acting on behalf of the Administrator (Australia
The Ordinance specifically required that 'No such article may be taken out of the Territory until it has first been offered for sale at a reasonable price to the Administrator' (Australia 1923:63). An amending Ordinance of 1923 extended its powers to protect archaeological sites, especially rock art sites, sites with 'ancient pottery or historical remains of any description' and burial and ceremonial grounds (Australia 1924:40). The discovery, or reputed existence, of such places and objects was to be reported immediately to the nearest District Officer and sites could not be disturbed without the written permission of the Administrator. Furthermore, the transfer of ownership of stone carvings could only take place with the written authority of the Administrator (or his delegated officer).

The wording of the original Ordinance and its Amendment was retained, almost verbatim, by the National Cultural Property (Preservation) Ordinance 1965 and its subsequent Amendments, and for the National Cultural Property (Preservation) Act 1976 (cf. S. Bulmer 1969; Papua New Guinea Museum 1974:2-3). The terms of export control and protection, therefore, have not changed for more than half a century. What have changed, however, are some of the administrative details.

The 1922 Ordinance in its original and modified forms gave authority for export to District Officers, persons reputedly familiar with and responsible for the application of many laws of New Guinea. Yet some seem to have been ignorant of this responsibility. One in particular apparently authorised the export of a stone figure from Unea Island, West New Britain Province, in 1967 (Riebe 1967; Muhvich 1978; Berger 1981). In 1978 this figure was offered for sale to the Australian Museum by Dr George Berger, a Sydney-based art critic and dealer. Investigation demonstrated that the figure appeared in Riebe's photographs, and the matter was reported to the National Museum and Art Gallery in Waigani. A Statutory Declaration made by the exporter, Gerhard Otto Porath, a copy of which was held by Berger, revealed that the export permission had been given by a District Commissioner of West New Britain. The Statutory Declaration also declared that the export occurred 'before legislation was passed prohibiting such exports'.

Not only is the final statement incorrect, but even under the 1922 Ordinance the item would have been a prohibited export. The National Museum and Art Gallery has no process through which to recover this figure; legal action would be a potentially long and costly exercise. Regrettably, such situations fall outside the scope of the recent Australian legislation enabling ratification of the 1970 UNESCO Convention on illegal export and import (cf. Prött and Specht 1989).

The power of District Officers/Commissioners to issue export permits was subsequently removed, for this was only one of a number of dubious cases (R. Bulmer 1969:1). This removal of the delegation has simply shifted the full burden of inspection and policing to the National Museum and Art Gallery, a body based in the Port Moresby area with few opportunities to oversee activities in the provinces. In the view of some people, effective policing is impossible, and some loss of heritage items is inevitable. Indeed, many go so far as to argue that only by allowing such items out of the country can a Pacific nation ensure their survival for the enjoyment of future generations. Spacious though this view may be, there is a very real problem of who will pay for the effective application of export controls.

All Pacific nations have suffered from the export of heritage items, many of them illegally. The export forms part of a worldwide 'art market', though it is questionable whether it is appropriate to group together works of art produced for sale in the open market, with items produced for significant cultural reasons. Many indigenous voices have raised this issue (cf. Prött and Specht 1989:105), a complex issue fraught with ethical and legal dilemmas. It is saddening, however, that the micro-states of the Pacific should have to devote their extremely scarce resources to protect themselves from the greed of foreign collectors, some museums among them. There is no published figure for the cost of the infamous case of the Taranaki panels – sometimes called the 'Ortiz case' – in which the Government of New Zealand sought the return of five carved panels excavated from a swamp in Taranaki that had been illegally exported (Cater 1982). One can only assume that the cost was high, for the case went through three appeal stages, including the Privy Council in Westminster, after the initial court case.

One of the major problems in this regard is the general lack of adequate national inventories of significant heritage items still within their territories. The UNESCO Convention of 1970 requires some kind of inventory as part of the ratification process for Member States, but the human and financial resources to achieve this are beyond most Pacific nations. Papua New Guinea has a list of National Cultural Property, but experience has shown that presence on this list is no protection for the items if they remain with their traditional owners (cf. Craig 1983). Regular field
inspections are essential, but can rarely be carried out.

In this context it might be argued that one problem that can be addressed by Pacific governments is to reduce the emphasis, and thereby the cost, of maintaining expensive centralised museums and other cultural bodies. In its own way, this is precisely what Vanuatu has done, by promoting rural-based activities requiring frequent contacts between the National Cultural Centre and its extensions on various islands. To some degree this has been at the expense of the development of a large national collection of artefacts, though this has also been hindered by inadequate resources in Port Vila. Plans to develop a new Cultural Centre on a more appropriate site are admirable, but will depend heavily on external funds for construction.

Given these almost overwhelming difficulties, it is not surprising that there have been few major cases of repatriation of heritage items to Pacific museums. In many instances, the museums are simply overwhelmed by the business of surviving. Repatriation has occurred, but in its own way has raised other problems of resource availability and allocation. When the first calls for the return of heritage items were made in the early 1970s, few people had a clear picture of how much or what was held by overseas museums and related institutions. The Australian and New Zealand sponsorship of the draft resolution to UNESCO to set up the Project for the Study of Oceanic Cultures envisaged the inventoring of overseas holdings of Pacific artefacts to facilitate requests for repatriation, and sought the support of the Meeting of Experts to be held in Fiji in late 1971 in ensuring the provision of adequate conditions in the events of repatriations (Australian National Advisory Committee for UNESCO 1971:244, 246). Of all the projects carried out under the auspices of the UNESCO Project, that addressing the inventories has probably had the greatest impact outside of the Pacific itself (Gathercole 1983, 1986; Specht 1979a).

To date, major surveys of museum holdings have been completed for Australia (Bolton 1980, 1983a, 1983b; Bolton and Specht 1984a, 1984b, 1985a, 1985b); New Zealand (Neich 1982), Canada and the United States of America (Kaeppler and Stillman 1985), and the United Kingdom and Eire (Gathercole and Clarke 1979). To these can be added the results of other surveys, not directly linked to the UNESCO exercise (e.g. Maughan n.d.; Schumann 1986), and numerous exhibition catalogues from museums around the world.

All of the UNESCO-sponsored inventories were financed by sources external to the Pacific Islands, particularly through UNESCO programme allocations. This has resulted in a veritable wealth of information on the holdings of heritage items outside the Pacific, but has the expenditure been worthwhile? One reviewer (Starzecka 1983) and one person directly involved in the inventories (Bolton 1984) each have queried aspects of the objectives and implementation.

Yet without this kind of baseline data, it is difficult to envisage any Pacific-based museum developing an effective programme to seek the repatriation of significant items. As Greenfield (1989) has so elegantly illustrated, repatriation is not a simple matter of 'ask and it shall be given'. Many museum workers still practice what Gathercole (1989) has termed the 'fetishism of artefacts', and feel threatened by requests for the return of heritage items. They are not the only ones who feel this way. David Wilson, then Director of the British Museum, argued forcefully against any returns on the basis that one return would lead to an overwhelming flood of requests (Wilson 1985:106). Our own experience in Sydney suggests otherwise. The British Museum position is excellently and expertly analysed by Greenfield (1989), and must be seen as one of the extreme cases.

The main calls for repatriations have come from African nations (cf. Commonwealth Arts Association and The African Centre 1981). The Pacific voice has been less strident, but has indeed been heard with sympathy. Returns in one form or another have been made to every major Pacific museum. These have been achieved by using inter-museum contacts rather than pursuing claims through quasi-legal or governmental structures (Specht 1979b; Newton 1983; cf. Anderson 1986, 1990 on the social relationships of artefact returns). Despite the vast volume of information made available through the UNESCO inventories, there has not been a flood of requests from the Pacific nations for returns. A common attitude expressed to me in private by several Pacific museum directors is that at this stage there is no need for them to do so. Most would have great difficulty accommodating large numbers of items; in the case of Papua New Guinea, the transfer of the MacGregor collection from Brisbane back to Port Moresby has been a major task (Quinell 1981).

The inventory programme has relieved Pacific museums of much of the burden of determining what heritage items are held outside the region. This was achieved in an atmosphere of cooperation and trust. Not so the sale of the George Brown collection in 1988. This sale, of a collection made by the Rev Dr George Brown, one
of the first Methodist missionaries in the Pacific islands, has raised a number of significant questions about procedure (Specht 1987). The collection was made by a man dedicated to replacing traditional belief systems with Christian- 
ity, who worked in New Zealand, Tonga, Samoa, Fiji and Papua New Guinea (Dark 1985; Anon. 1986). While destroying the traditional religions, he acquired over 3500 artefacts, most of which ended up in the Hancock Museum, Newcastle- 
on-Tyne, England. When the owner, the University of Newcastle, decided to cash-in on the collection's value, it gave Sotheby's (United Kingdom) the task of finding a purchaser. One was found in Japan, but no effort appears to have been made to determine whether the Pacific nations were interested in or could afford to acquire either the whole or its constituent parts. The sale was made on the basis of the collection being kept as a whole. At the price placed on the collection, reported to be about A$1.3 million, no Pacific nation could have afforded the entire collection, but may have been able to raise funds to acquire those items relevant to themselves. The process of sale, however, denied them even this opportunity, even though the Papua New Guinea government sought to have the sale suspended while it investigated such a possibility (S. Eoe pers. comm.). The outcome of the sale was that several items were retained in the United Kingdom under its export regulations, thus effectively destroying the condition of sale as a single collection.

This event raised a significant question that appears to have escaped the attention of most museum personnel in the United Kingdom when they supported the sale in terms of a single collection: no Pacific nation could afford to purchase it. Some curators later argued that they hoped that insistence on the 'whole collection or nothing' approach would prevent the export of the collection. This was a vain hope, indeed, and has led to a much worse situation than if the major parties to the sale had been properly advised of Pacific interest in the collection. As Gathercole has commented (1989:77), for western scholars it should not matter where the collection is eventually housed. Most have or can obtain the resources to travel for study. But for most Pacific Islanders overseas travel for such study is virtually non- existent, particularly those who do not have employment in high-paying positions. Gathercole further observes (1989:80) that the distribution of Pacific heritage items still reflects the history of imperialism, and not the current geopolitical and cultural realities.

**CONCLUDING OBSERVATIONS**

This selective excursion through various matters relating to the cultural heritage in the Pacific has been largely negative. It is not intended to be completely so, for much has been achieved in the last quarter of a century. Perhaps one of the major stumbling blocks to progress has been our own perceptions, which have led us to expect far more than could be achieved in such a short time. Western museums and managers of the cultural heritage are continually faced with dilemmas and problems similar to those that confront Pacific Island nations. The major difference is one of scale. It was naive of outsiders to believe that one or two key personnel from an island group could be trained to carry out, immediately after training (often at an inadequate level), all of the functions demanded of them by their responsibilities. It was equally naive of island governments to assume this.

Independence brought only a selection of freedoms to the Pacific nations. Some Pacific peoples remain in an actual or quasi-colonial position. In the area of cultural heritage preservation and protection, they are virtually all in the same situation. The cost of comprehensive and effective action is essentially beyond the capacity of the independent nations, unless their governments rethink their priorities. Cultural Centres may seem a highly desirable thing, but who can afford them in the way they are supposed to function? In preparing his original proposals Duff had in mind a single-language nation – the Cook Islands – and did not offer guidance on the delivery of cultural services to outlying islands. Cultural centres cannot achieve this without cost, though the Vanuatu Cultural Centre has probably been more successful than any other.

Were, then, the standards expected of Pacific nations too high or perhaps even inappropriate for achieving the desired ends? I believe the answer may be 'yes' to both of these questions. The Pacific nations must develop effective cultural heritage services that they can afford, otherwise they will continue to be the poor relations of larger countries, forever seeking external assistance. Given the non-urban populations of all of the Pacific nations, a centralised structure may be neither desirable nor feasible. On the other hand, a reallocation of resources by major bodies such as UNESCO could alleviate many of the smaller funding problems, and perhaps allow more cooperative ventures between Pacific museums and those of larger metropolitan countries. These will be essential, for few Pacific museums 'will ever have the resources to fully equip them-
selves with the paraphernalia now considered necessary to carry out professional museum duties.

This, of course, would have implications for the management of export controls and requests for repatriation. Knowledge that some museums may not have the resources to curate collections to a desired level would almost certainly strengthen the resolve of those not wishing to repatriate items. I suspect that pragmatism may prevail in this area, since the professional training of the Pacific museum staffs would have to be set against the ideological pressures for controlling their own cultural heritage items.

Similarly, the control of exports is likely to suffer. But here there is much that could be attempted, with relatively little cost and effort. Better public education about the problem is essential. Some steps have been taken in this regard. Papua New Guinea and the Solomon Islands have tried through the issuing of information sheets to visitors advising them of their export controls. This, however, does nothing to reduce the desire of the unscrupulous collectors and dealers, for whom the heritage items of any culture are fair game for marketplace speculation and profit. For auction houses, ever-increasing prices and new ways of encouraging purchases are all part of the game of increasing profits (cf. Hughes 1989b). The global value of the 'art market' (primarily dealing in western art) has been estimated at about US$50 billion per annum, a large market with enormous power (Hughes 1989a:63). Within this the power has shifted, as the USA continues to lose its dominance and is replaced by Japan. In such a situation, Hughes' (1989a:67) comment that 'it is fatuous to utter bromides about art's being the Common Property of Mankind' is most appropriate. Even more so is his observation that 'what strip mining is to nature, the art market has become to culture'. For the Pacific Islands' nations, this may seem a pessimistic view, but realistic. Unless steps can be taken to remove, or isolate, heritage items from the marketplace, a step that some economists would find unbelievable (e.g. Gramp 1989), there seems little hope to control the forces that threaten them. It is sobering to realise that the price of one or two major pieces from the Sepik River area of Papua New Guinea could exceed the total annual operating budget of that country's national museum. Yet the problem will not be solved by selling annually one such piece to boost revenue! It may be solved by seeking new approaches, setting new priorities (both by government and the institutions), and by a lesser dependency on externally-generated solutions that are geared to more substantial economies. There are signs of moves in these directions, with the development of plans for lower-cost facilities in Vanuatu. But how successful they will be without external moral and financial support remains to be seen. Central to all efforts will be concern for establishing and developing the idealised notions of cultural identities that Pacific leaders have promoted so vigorously through their espousal of concepts such as kastom, fa'a Samoa and la coutume (Bolton 1990).

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Specht


<table>
<thead>
<tr>
<th>Name</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>ACH 84/1, Tasmania</td>
<td>281</td>
</tr>
<tr>
<td>Acheron River</td>
<td>281</td>
</tr>
<tr>
<td>Adelaide</td>
<td>281</td>
</tr>
<tr>
<td>Admiralty Is</td>
<td>283</td>
</tr>
<tr>
<td>Ahus</td>
<td>283</td>
</tr>
<tr>
<td>Aitape</td>
<td>283</td>
</tr>
<tr>
<td>Aitutaki, S. Cook Is</td>
<td>282</td>
</tr>
<tr>
<td>Alofi</td>
<td>282</td>
</tr>
<tr>
<td>Alor</td>
<td>285</td>
</tr>
<tr>
<td>Amphlett Is</td>
<td>283</td>
</tr>
<tr>
<td>Andrew River</td>
<td>281</td>
</tr>
<tr>
<td>Anuta, Santa Cruz Is</td>
<td>282</td>
</tr>
<tr>
<td>Arawe Is</td>
<td>283</td>
</tr>
<tr>
<td>Armidale</td>
<td>281</td>
</tr>
<tr>
<td>Arnhem Land</td>
<td>281</td>
</tr>
<tr>
<td>Aru Is</td>
<td>285</td>
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<tr>
<td>Atauro</td>
<td>285</td>
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<tr>
<td>Auckland</td>
<td>284</td>
</tr>
<tr>
<td>Australia</td>
<td>282</td>
</tr>
<tr>
<td>Austral Is (Tubnai)</td>
<td>282</td>
</tr>
<tr>
<td>Avoca Point, Marlborough</td>
<td>284</td>
</tr>
<tr>
<td>Bali</td>
<td>285</td>
</tr>
<tr>
<td>Balof, New Britain</td>
<td>283</td>
</tr>
<tr>
<td>Baluan</td>
<td>283</td>
</tr>
<tr>
<td>Banks Is, Vanuatu</td>
<td>282</td>
</tr>
<tr>
<td>Banks Peninsula</td>
<td>284</td>
</tr>
<tr>
<td>Bass Point, New South Wales</td>
<td>281</td>
</tr>
<tr>
<td>Bass Strait</td>
<td>281</td>
</tr>
<tr>
<td>Bathurst Is</td>
<td>281</td>
</tr>
<tr>
<td>Bay of Islands</td>
<td>284</td>
</tr>
<tr>
<td>Bay of Plenty</td>
<td>284</td>
</tr>
<tr>
<td>Beginners Luck Cave, Tasmania</td>
<td>281</td>
</tr>
<tr>
<td>Bismarck Archipelago</td>
<td>283</td>
</tr>
<tr>
<td>Bluff</td>
<td>284</td>
</tr>
<tr>
<td>Boera, Port Moresby</td>
<td>283</td>
</tr>
<tr>
<td>Bone Cave, Tasmania</td>
<td>281</td>
</tr>
<tr>
<td>Bougainville</td>
<td>283</td>
</tr>
<tr>
<td>Bougainville Straits</td>
<td>283</td>
</tr>
<tr>
<td>Brisbane</td>
<td>281</td>
</tr>
<tr>
<td>Broome</td>
<td>281</td>
</tr>
<tr>
<td>Buang Marabak, New Ireland</td>
<td>283</td>
</tr>
<tr>
<td>Buccaneer Archipelago</td>
<td>281</td>
</tr>
<tr>
<td>Buin, Bougainville</td>
<td>283</td>
</tr>
<tr>
<td>Buka</td>
<td>283</td>
</tr>
<tr>
<td>Burra Burra, New South Wales</td>
<td>281</td>
</tr>
<tr>
<td>Burrill Lake, New South Wales</td>
<td>281</td>
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<td></td>
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<td>Canberra</td>
<td>281</td>
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<td>Canterbury</td>
<td>284</td>
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<tr>
<td>Cape Egmont</td>
<td>284</td>
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<td>Cape Reinga</td>
<td>284</td>
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<td>C (continued)</td>
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<tr>
<td>Cape York</td>
<td>281</td>
</tr>
<tr>
<td>Cape York Peninsula</td>
<td>281</td>
</tr>
<tr>
<td>Capertee, New South Wales</td>
<td>281</td>
</tr>
<tr>
<td>Carnarvon Range</td>
<td>281</td>
</tr>
<tr>
<td>Caroline Is</td>
<td>282</td>
</tr>
<tr>
<td>Cave Bay Cave, Tasmania</td>
<td>281</td>
</tr>
<tr>
<td>Cenderawasih Bay</td>
<td>285</td>
</tr>
<tr>
<td>Central Line Is</td>
<td>282</td>
</tr>
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<td>282</td>
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<tr>
<td>Sarah's Gulley, Coromandel</td>
<td>284</td>
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<td>283</td>
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<td>283</td>
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<td>284</td>
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<td>Sleisbek, Northern Territory</td>
<td>281</td>
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<tr>
<td>Society Is</td>
<td>282</td>
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<td>Solo, Java</td>
<td>285</td>
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<tr>
<td>Solomon Is</td>
<td>282</td>
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<tr>
<td>South Alligator River</td>
<td>281</td>
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<td>282</td>
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