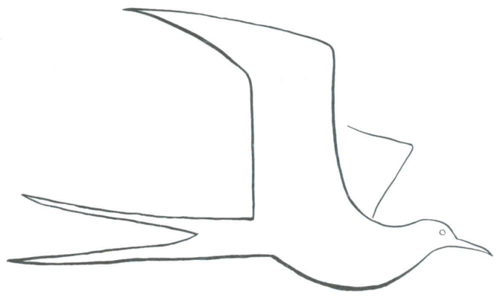

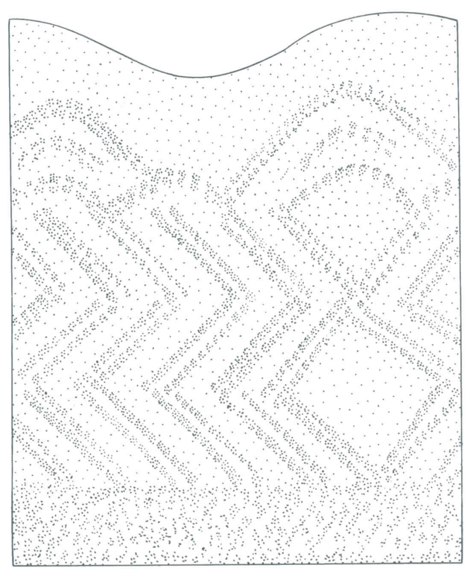

REPORT OF THE LAPITA HOMELAND PROJECT

edited by Jim Allen & Chris Gosden

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Edited by Jim Allen and Chris Gosden

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Department of Prehistory, Research School of Pacific Studies
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For Doug Yen

mate and mentor

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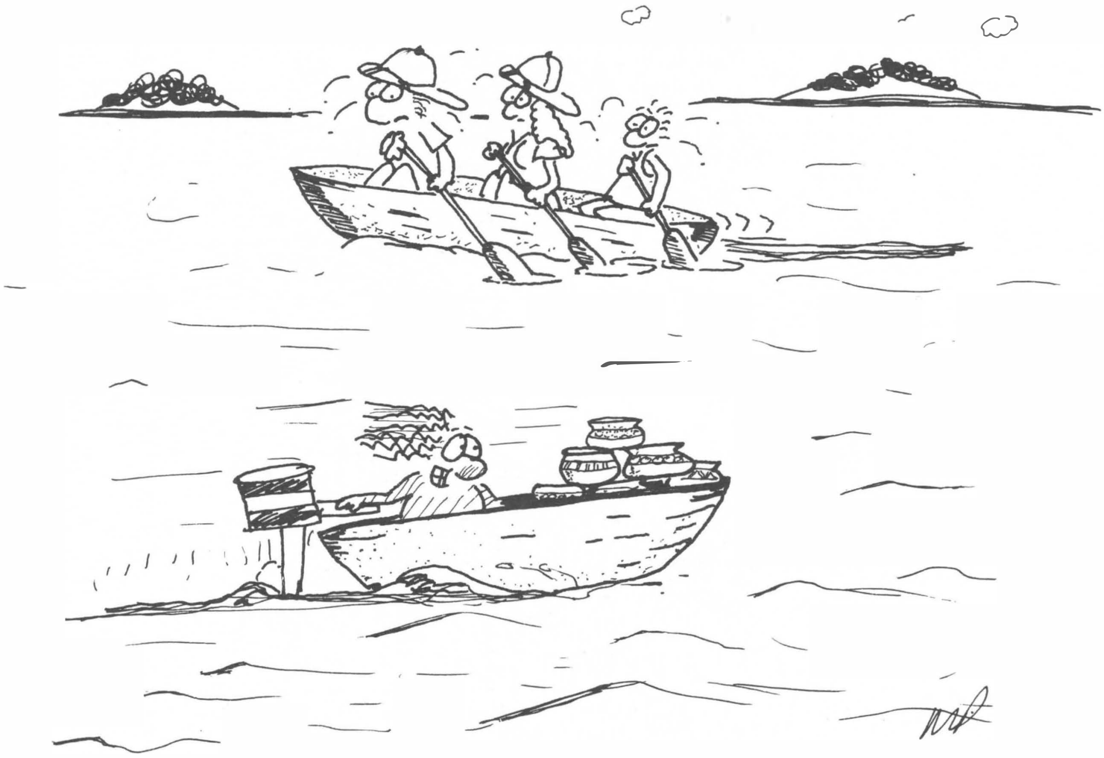

IN SEARCH OF THE LAPITA HOMELAND.

ACKNOWLEDGEMENTS

In carrying out this project, the team members met many people both in Papua New Guinea and elsewhere who provided much help and assistance. People who aided particular projects are thanked by the individual team members in each of their contributions. Here our thanks are confined to those individuals, institutions and organisations whose contributions were to the project as a whole.

We begin with Papua New Guinea. In the national capital, Port Moresby, and in each of the Island Provinces in which we worked, we encountered nothing but courtesy, co-operation and goodwill. We trust that this present contribution to the nation's history will be seen as a thank-you for this help.

We would like to thank the Prime Minister, the Right Honourable Rabbie Namaliu for his personal encouragement and for the specific assistance he provided as Foreign Minister in 1985. The officers of the Department of Immigration co-ordinated research and entry permits with efficiency and good humour. Members of the project were all affiliated with the National Museum and Art Gallery; there, in particular, we thank Pamela Swadling for her help and organisation. Both the Institute for Papua New Guinea Studies and the University of Papua New Guinea provided additional institutional support in Port Moresby; among the many individuals in these organisations who offered help and advice, we would like particularly to thank Lance Hill for help in New Ireland. Our archaeological colleagues in Port Moresby, Pamela Swadling, Les and Rosemary Groube, Jean Kennedy, Joe Mangi, John Muke, John Saulo, Meredith Sassoon, Marjorie Sullivan, Philip Hughes and John Burton variously sheltered and entertained us. The staff of the Angau Lodge provided us with accommodation and transport.

We thank each of the Island Provinces of Manus, New Ireland, East New Britain, West New Britain and the Northern Solomons, their Governments and the officers of those administrations. Many of these people are thanked by the individual authors, but here in particular we would like to mention Simeon Malai and the late Wes Rooney and Nahau Rooney in Lorengau; Esekia Tomon, Roger and Emelia Dixon, and Drew Wright in Kavieng; Ellison Kaivovo in Rabaul; Andrew Marenge, John Normu, John Namuno and Cecilie and Max Benjamin in Kimbe; Isidore Teli in Kandrian; and Bart Philemon in Lae.

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We wish to thank the Oceanic Research Foundation Ltd, Sydney, from whom we hired the research vessel, the *Dick Smith Explorer,* and in particular Robin Miller and Don Richards, who liaised with us on behalf of the Foundation and hired for us such a capable crew. As captain, Taffy Rowlands did an exceptional job and contributed enormously to the success of the project. In these nautical endeavours he was ably supported by the 'permanent' crew members, John Gordon and Michael Staples. On the outward voyage from Sydney they were supplemented by Sally Brockwell and Jill Allen.

In Papua New Guinea Sally Brockwell was joined by Prue Gaffey and they remained in the field until they returned with the research vessel to Sydney. They were variously archaeologists, sailors, book-keepers and general organisers. We thank them for their good humour and their major contribution to the project.

In Sydney, Tim and Susan Murray consented to having their home turned into a warehouse and dormitory before the expedition left Australia. We thank them for their contribution.

Finally Ann Andrews, Jennifer Elliott, Jeanine Mummery, Stella Bromilow and Ros Allen contributed their word processing skills and general help as the drafts of this report developed. Wal Ambrose designed the cover. Ian Faulkner did the final amendments to the art work. Mike Pickering drew the cartoon. Matthew Spriggs kindly read the final drafts. We thank them all.

INTRODUCTION

Jim Allen

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BACKGROUND

The idea for the Lapita Homeland Project arose out of a conversation with Jim Specht at the 52nd ANZAAS Congress held at Macquarie University in Sydney in 1982. It was a time of increasing interest in Melanesian archaeology; Green's decade of research into Lapita sites in the Reefs-Santa Cruz group of the eastern Solomons had culminated in his influential synthesis (Green 1979) and some associated disputes with colleagues (e.g. Clark and Terrell 1978; Green 1982). Specht himself was continuing to research and publish on the archaeology of West New Britain (Specht 1974, 1981; Specht and Koettig 1981; Specht and Hollis 1982; Specht, et al. 1981a; Specht et al. 1981b). At the Australian National University, Jean Kennedy and I had initially combined with Wallace Ambrose to expand his long term investigations into the Admiralty Islands, and Kennedy was extending this interest (Kennedy 1979, 1981a, 1981b, 1982, 1983).

One particular point had struck both Specht and me. As Green (1979) and a myriad of other publications had made clear by that time, sites with Lapita pottery (i.e. 'Lapita sites') were not questioned as the archaeological manifestations of the earliest colonisers of Remote Oceania - that part of the southwest Pacific beyond the main Solomons chain where open sea voyages must have increased dramatically in length. Almost as widely accepted was the derivation of these colonisers more or less directly from Southeast Asia (e.g. Jennings 1979; Bellwood 1983). The questions of cultural status being debated in Lapita research in the middle and late 1970s were not questions of origin, but rather questions of subsistence patterns and trading modes. It seemed to both Specht and me that these debates missed an important issue. They were being argued from data derived from the central and eastern portions of the known distribution of Lapita sites. If the Southeast Asian derivation of Lapita was indeed correct, these colonists had reached the Bismarck Archipelago before the Solomons or New Caledonia or Fiji, Tonga and Samoa. Thus, on first principles, these arguments all lacked vital data from the region where logically these colonists had first encountered the characteristics of the island world of Melanesia, smaller landmasses with significantly reduced varieties and numbers of natural food plants and land animals. While we knew of Lapita sites in the Bismarck Archipelago at that time - Eloaua, Watom and Ambitle, as well as a handful of findspots - none of these were without chronological or other problems, and were not contributing significantly to these debates.

In considering the need to re-establish the importance of the Bismarck Archipelago in these Lapita discussions, Specht and I explored the notion of a short term, but large scale archaeological investigation of the Bismarcks as a whole, taking as an initial model the Southeast Solomon Islands Cultural History project successfully run by Roger Green and Douglas Yen a decade earlier (Green and Cresswell 1976). Our own experiences in the region led us towards the necessity of such an integrated work; the Bismarck Archipelago covers some 400,000 km², and while 80% of this is sea, it still leaves an enormous amount of land to be considered. While the piecemeal work of a series of archaeologists in the Bismarcks had given us some insights into behavioural unities across this region in the past, especially through obsidian and pottery studies, none of us individually were in a position to perceive the dynamics of the total human use of this region.

Such a project needed an intellectual focus and one obvious one was at hand. A number of us working at the New Guinea end of Melanesia were less than convinced by the arguments that saw waves of colonists, the products of some poorly defined Southeast Asian neolithic revolution, streaming eastwards and bearing their superior technology, social organisation and subsistence modes towards a Polynesia-to-be, essentially by-passing the inhabited islands of Melanesia and creating two distinct populations in the Southwest Pacific. There seemed to me, a priori, to be sufficient archaeological evidence in the Bismarcks and the nearby New Guinea mainland to challenge an entirely intrusive cultural content required by this model of migration, although the model was not ever only, nor even mainly, proposed on archaeological grounds; it has instead depended heavily upon linguistic and physical anthropological reconstructions for its substance.

Thus, in the initial development of the project it was necessary to focus attention on a significant archaeological problem for the Bismarcks in the wider Pacific context, and in such a way that the archaeology was the central issue, rather than a less focussed multi-disciplinary project. Green (1979:45) had indeed already signalled this focus by referring to the Bismarck Archipelago as a Lapita homeland in respect of it being a likely region where the bearers of the Lapita cultural complex learned the crucial adaptations necessary to colonise the island world of Oceania. Green thus neatly dodged the question of ultimate Polynesian origins and did not elaborate on possible relationships between the creators of the Lapita archaeology and other contemporaneous groups in the Bismarck Archipelago. By buying directly into these issues, we were able to provide a focus from which to launch the Lapita Homeland Project.

The arguments to support a model of indigenous development in the Bismarck Archipelago for the Lapita cultural complex as an alternative to the Southeast Asian origin model have been published elsewhere (Allen 1984) and require no repetition here, beyond making the point that while they challenged what Spriggs (1984:202) called the 'orthodox' view, such a challenge was not new (see, for example, Groube 1971). Indeed, a certain general unease with the simplicity of this migration model, rather than any clear persuasiveness of the indigenous alternative was, in my view, one strong reason why the suggestion for a Lapita Homeland Project was so well received when it was first publicly discussed at the 15th Pacific Science Congress held in Dunedin, New Zealand, in February 1983. As a direct outcome of that meeting I began the arrangements for the project.

Later in 1983, Jack Golson and I spent a week in Port Moresby in preliminary discussions about the project, mainly at the University of Papua New Guinea and the Papua New Guinea National Museum and Art Gallery. It is fair to say that there was a certain amount of apprehension about the viability of a project of this size and I was unable to get any firm commitment for the direct involvement in the project of any of the staff or students from either institution.

RECONNAISSANCE 1984

In May and June 1984, I returned to Papua New Guinea, with Jim Specht, Wallace Ambrose and Douglas Yen. During this time we visited Port Moresby and the four Island Provinces of Manus, New Ireland, East New Britain and West New Britain. Our trip had two purposes. The first was to hold a series of discussions with the various political agencies, provincial governments, village groups and individuals throughout the proposed research area, to explain the nature, timing and purposes of the proposed research and to seek initial reactions to it. The second was to locate sites and regions to form the bases of individual projects during the 1985 season.

This trip was fully reported (Allen et al. nd (1984)) and will only be briefly summarised here. We were able to delineate some 15 potentially valuable, localised research tasks which would contribute to the overall aims of the project and which we would subsequently offer to members of the project, as described below. We were able to make first hand assessments of the logistical difficulties we would need to overcome and to discuss accommodation problems in villages where researchers would stay in the following year. Most importantly, it was only by being able to conduct direct negotiations with the various government and village authorities that we were subsequently able to obtain the various permissions to carry out our project. In Port Moresby we obtained the general support of the Institute of Papua New Guinea Studies, the University of Papua New Guinea and the Papua New Guinea National Museum and Art Gallery; it was to this latter organisation that our researchers were affiliated while in Papua New Guinea. As well, we received the individual support of several National Government members.

At the provincial level we arranged an official meeting in each capital and described in detail the project's overall plans and how these might impinge on any particular province. We stressed that we were merely foreshadowing subsequent written applications for permission to do research and answered questions. At the local meetings, held with various degrees of formality at district offices, councillors' villages or in private houses, we always discussed the whole project, although invariably the discussion quickly turned to the local prehistoric resources, the possibility of our subsequent return and local involvement. At all levels we received nothing but courtesy and help.

The remainder of 1984 and early 1985 was a busy time of organisation - refining the research objectives, developing the research design, writing research proposals and grant applications, matching our researchers with the topics we had identified and developing these sub-projects. My task was made easier by the co-option onto the project of Chris Gosden, who had arrived in the Department of Prehistory in the Australian National University at the beginning of 1984 on a Leverhulme postdoctoral fellowship. As this volume makes clear, Chris has continued to play an organisational role equal to my own (in addition to his substantial academic input) and I wish here to thank him for his support.

RESEARCH QUESTIONS

Stemming from all of the initial discussions and the results of the 1984 field trip, we isolated six research questions we wished to pursue. These appeared in our initial grant proposals in more extended form.

1. What was the nature of late Pleistocene/early Holocene human occupation in the Bismarck Archipelago? Here we were concerned with the geographical and chronological extent of such occupation. What use was made of offshore islands or specific ecological zones of larger islands? What was the nature of subsistence strategies and patterns of raw material exploitation? Could we say anything about population densities?
2. Was horticulture part of the subsistence strategy throughout the Holocene in the Bismarck Archipelago or was it a later introduction? What variations in horticultural technology - plant and animal husbandry, crop registers or techniques - might be identified or dated?
3. What was the nature of ceramic development or introduction and its subsequent evolution in the region? Were there antecedents to Lapita style pottery? To what extent do post-Lapita ceramics in the area reflect a continuing evolution of ceramic style as well as use?
4. To what degree is the distribution of Lapita sites in the region a reflection of cultural preferences, or a reflection of subsequent human and/or natural alterations to the landscape?
5. How far might studies of contemporary trading systems in the region elucidate the nature of past long distance and local exchange patterns?
6. What was the technological range of obsidian exploitation, and what measures of specialisation and production can be determined from these data through time?

These questions reflected the overall project design, which was not merely to pursue the study of Lapita sites but also to put these sites into regional and chronological contexts which acknowledged prior and subsequent, as well as contemporaneous, non-Lapita groups of people living in the Bismarck Archipelago. While we recognised that the strategy we had chosen, of casting our resources over a wide region rather than concentrating them in a small one, could lead us to expect only tentative answers to these questions, we now feel that the papers contained here have made substantial inroads into at least some of these issues. Question 5, concerning trade, fell out of consideration as the project developed, and Question 2, pursued only directly by Yen, who is now incorporating it into his wider current project on the history of Oceanic agriculture, is only peripherally addressed here, although evidence for it appears in a number of the contributions.

RESEARCH LOGISTICS

The fieldwork projects were all designed to be undertaken between May and September 1985. During this period 24 qualified archaeologists were involved in 19 separate projects, some surveying and excavating multiple sites, and others carrying out single excavations. Most of these were direct members of the project, but several were associates: at this time Geoffrey Irwin was conducting fieldwork in the Massim, using his yacht, the *Rhumbline*, and making some of those first hand observations which have led to the opening contribution to this volume. Holly McEldowney had begun her doctoral research in the Admiralties at this time and we welcome her and Chris Ballard's contribution here.

In addition to the archaeological contingent, we were accompanied for part of the trip by anthropologist Mimi George and historian and sailor, David Lewis. Their contribution (George and Lewis 1985) on sailing and trading in the Bismarck Archipelago appeared in the 1985 report and is not repeated here. Wally Johnson, Bureau of Mineral Resources, Canberra, contributed to the work done at Boduna Island, near Talasea and to the survey of the Witu Islands with geomorphological and geochemical advice, in return for being able to undertake soundings of the collapsed Ritter Volcano from the expedition's vessel, the *Dick Smith Explorer*.

The decision to hire our own research vessel was made in the interests of efficiency, economy and safety. After extensive discussions with the Oceanic Research Foundation Ltd, based in Sydney, we entered into the hire of the Foundation's 65 foot yacht, the *Dick Smith Explorer*. This yacht, captained by the Len Evans of Sydney Harbour, Taffy Rowlands, carried three other permanent crew members and left Sydney in April, returning in late September. It transported some people, food and gear to Papua New Guinea and returned people, gear and finds. While there, it delivered people to various destinations and enabled incidental work such as marine contours to be plotted. On one occasion it travelled to South Bat Island in the Purdy group to collect guano samples for Harold Brookfield, Professor of Geography at the Australian National University. Mostly it allowed us to survey with efficiency in isolated places like the Arawe Islands, Unea Island in the Witu group and Djaul Island, south of New Ireland. In retrospect, we could not have accomplished what we did without the support of the *Dick Smith Explorer*.

In particular the vessel provided a continuous project link for a succession of researchers who arrived and departed at different times in different parts of the research area. This enabled gear to be recycled and provided the necessary flexibility needed to accommodate the schedules of a number of researchers with other responsibilities.

RESEARCH DESIGN

As stated, some 15 sub-projects were formulated towards the end of 1984 and I entered into negotiations with the principal researchers to arrange the details. While each arrangement was different, they all followed a straightforward and simple pattern. Firstly I identified a site or area which we wanted investigated and in the most general terms outlined those aspects which I considered important. These might be absolutely general, as in the case of the Arawe Islands: given that these islands were unknown archaeologically, were there any Lapita sites there? The import of this for the project was whether or not Lapita sites existed in the Bismarck Archipelago away from the direct Southeast Asia-Polynesia track apparently indicated by the known Lapita sites of Eloaua, Ambitle and Watom. In other cases our requirements might be quite specific. For example, in 1985 the claimed date of 3900±260 BP for Eloaua (Bafmatuk et al. 1980) was the oldest Lapita date in the Pacific. Could it be duplicated? If it could, the import for the project might be a reasonable time gap between Lapita in the Bismarcks and Lapita further east.

Beyond these very general requirements, researchers were free to expand their investigation as much or as little as they pleased and in directions indicated by their own interests and experience. This, of course, was an advantage of assembling professional archaeologists - they required no close supervision. Another was that each became directly responsible for the analysis and production of their own research. This report constitutes the results of these endeavours. In retrospect I consider that there are both advantages and disadvantages in this approach. One disadvantage is that the project possessed no central direction or theoretical focus during the field research or subsequent analyses beyond the general issue of Lapita origins. This has made it more difficult to produce summary interpretations of what all these new data mean, although many of the present authors consider this to be one of the Lapita Homeland Project's strengths. To me, the major advantage of this approach was that it was the only way this expert team could have been tempted into the field at all in a concerted project. In this instance at least, I have been persuaded that the end justifies the means.

By the time fieldwork ended in 1985 the Lapita Homeland Project was represented by two projects in Manus Province, to which the Mouk project of McEldowney and Ballard was subsequently added; eight projects in New Ireland Province, of which one, Peter White's excavation at Balof 2, had been added to the project when his proposed ARGS funded work in West Irian could not be undertaken; two projects in East New Britain Province; three projects in West New Britain Province; and one project in the Northern Solomons Province. This latter project, Spriggs' Nissan Island surveys and excavations, had been developed and negotiated by Spriggs after the 1984 field trip, and brought to five the number of provinces covered in our field area. The final two projects, those of Yen and Irwin, were not province-specific.

In the absence of an overall framework, the specific research designs of each sub-project are discussed by individual authors.

OUTCOMES

In order to provide additional integration within the project once the general results began to appear, two post-excavation workshops were held, the first under the auspices of the Australian Museum in Sydney in 1986 and the second a year later in La Trobe University, Melbourne. Both of these were well attended and productive and helped generate the data which comprise this report.

This report contains 16 detailed statements on work undertaken under the general aegis of the Lapita Homeland Project. Of the three missing here, one has previously appeared (George and Lewis 1985) and, as discussed, Yen's general investigation of the botanical prehistory of the Bismarck Archipelago is currently being incorporated into a wider account of agricultural development in the Pacific. The analysis of materials from the final sub-project, the excavation of a limestone cave in central New Ireland, called Buang Merabak, is still in progress. Currently little is known of this site, save that it contains obsidian, pottery sherds in its upper levels, marine shellfish, and a well preserved fauna which includes phalanger, bat, rodent, fish and reptile. As the reports here of the other New Ireland cave excavations, Panakiwuk, Balof 2 and Matenkupkum, make clear, details of the Buang Merabak sequence are of great importance. Of equal importance is the age of the basal deposits in this site, where a radiocarbon age determination of 31,990±830 BP (ANU-6614) suggests that it was settled at the same time as Matenkupkum cave (Balean 1989:7; Gosden and Robertson this volume).

By 1990 some 24 journal articles directly related to the project had appeared, including two long syntheses in late 1989 (Allen et al. 1989; Gosden et al. 1989); directly and indirectly the project has also contributed to three collected volumes (*Records of the Australian Museum* 41(3) 1989; Kirch and Hunt 1988; Spriggs 1990). Unpublished reports followed each of the field seasons, and various sub-projects have so far furnished the data for 11 fourth year honours and postgraduate theses.

One of the most pleasing measures of the success of the Lapita Homeland Project has been the continuing and expanding development of archaeological field research in the Bismarcks since 1985. Kirch, together with graduate students, has returned to Mussau for two additional large field seasons. Peter White has also returned to Balof 2 for two major periods of further excavation. Gosden and I returned to Matenkupkum for further work there in 1988 and excavated an important second site, Matenbek, which we first recorded in 1984. Spriggs has returned twice to Nissan and in addition has teamed with Ambrose to investigate the early settlement history of Manus Island; they have completed two seasons of this work. Lilley has carried out new surveys on the north coast of New Britain. Gosden is currently preparing for a fifth season of work on the startlingly rich pre-Lapita, Lapita and post-Lapita sites which the project's surveys encountered in the Arawe Islands. This last project is being carried out in conjunction with a large scale investigation of obsidian sites in the Talasea area by Specht, Torrence and Fullagar. Three seasons of this latter work have been completed, and further work is planned.

In addition to direct academic returns to Papua New Guinea, Patrick Kirch produced a teaching kit of replicas of pottery and other Lapita artefacts which were given to schools in the province in which he worked. At La Trobe University, Stephanie Moser, a graduate student in archaeology, took on the unpaid responsibility of writing and producing a 40 page booklet, with illustrations, for distribution to schools and community centres in the Island Provinces in which we worked. Based on publications and prepublication reports arising from the project, the text discusses what archaeologists do, and the results of the Lapita Homeland Project in plain language text. All the sites reported here are discussed. Chris Gosden, who worked with Stephanie on this project, received a grant from the Fred Arthur Trust to enable the production and distribution of several thousand copies of this booklet.

THE LAPITA HOMELAND (REPRISE)

The full implications of the vast amount of new data generated on the prehistory of the Bismarck Archipelago over the last five years (and still arriving daily) cannot yet be properly assimilated. Thus this report attempts no concluding interpretation, although my co-editor, braver than me, ponders the future directions of Bismarcks research in a concluding chapter. Moves towards interpretations, such as they are, appeared in Allen et al. (1989) and Gosden et al. (1989) and here I merely note some of the more remarkable discoveries.

While the antiquity of humans in the Pacific east of the New Guinea mainland has been quadrupled, it is not so much the great age that is important but the information contained in these long sequences. From well before the Pleistocene ended we have clear evidence of deliberate sea movements which transferred obsidian from New Britain to New Ireland and probably transferred animals as well. We cannot avoid considering these long term adjustments to this tropical island world as a first step which 30 millennia later would bring people to New Zealand, the last permanently settled large landmass on earth.

In the Admiralty Islands, notwithstanding the strictures of McEldowney and Ballard (this volume) we still have no dramatic Lapita presence, despite the fact that surveys equally as poor or poorer than that which they castigate in the Admiralties, located some 17 new Lapita sites in other parts of the Archipelago. While on the one hand the project found no indications anywhere of a ceramic predecessor to Lapita, the geographic boundary, real or imagined, which contains Lapita as a ceramic style or a cultural complex inside Melanesia still holds. On the other hand, the recovery from the Baun site on Lou Island of a piece of prehistoric bronze (Ambrose 1988) underscores the fact that this boundary was not uncrossable, nor indeed, given Bellwood's and Koon's (1989:620) demonstration of Melanesian obsidian in Sabah, that such crossings went only one way.

The dramatic finds made by Kirch and his colleagues at the waterlogged Lapita site of Talepakemalai on Eloaua in 1985-6 have given us a more intensive view of how different Lapita sites are from other Melanesian sites in appearance, and have strengthened Kirch's view (and not only Kirch's) that the intrusive model of Lapita is, after all, correct (Kirch 1988). Equally, the distribution of the Bismarcks Lapita sites and their chronologies reflecting centuries of Lapita occupation now demonstrate that notions of a fast and direct migration of people who had little or no interaction with existing island Melanesian groups cannot be sustained. Also, excavated subsequent to the project, Gosden's Lapita site (FOJ) on Kumbun Island is also yielding waterlogged food plants and other organic remains. This work is now showing that sites with the full suite of Lapita material remains occur on smaller islands close to large landmasses in the Bismarck Archipelago, as well as distant and more isolated ones (cf. Turner 1989:296, and my further comment below).

Elsewhere, on Watom, Green and Anson recovered invaluable skeletal remains in association with Lapita pottery (Green et al. 1989) and also have been able to elaborate a developmental ceramic sequence over a relatively long-lived occupation which includes pottery in that style throughout. What has been particularly pleasing in regard to this and other ceramic sequences (see especially papers in this volume by Specht, Golson and Lilley) is that the project has produced good data with which we may evaluate the relationship between Lapita and the appliqué and incised styles which previously appeared to be separate and later in time. In the Bismarcks at least, there is a general intermingling of these styles, on individual sherds as well as in the same deposits.

Ultimately the question remains, have we discovered the Lapita homeland? The authors in this report remain divided. Unfortunately we have no Agatha Christie to produce the imperative deductive argument from the data that we have gathered and must be content with a poor imitation of T.H. White.

I have always considered the notion of a Bismarck Lapita homeland a useful heuristic device but one with insufficient explanatory power to elucidate the complexities of the vast and diverse Melanesian archaeological record. At the same time, it seems to me, that the Lapita Homeland Project has produced no new evidence to shore up an equally implausible migration theory. Indeed, problems for this view in the new Bismarcks data are immediately being disposed of by the use of standard particularising arguments: Turner (1989:296) concludes that the two available Lapita human dentitions from Watom are more like Melanesians than recent Thai or late prehistoric Hawaiians and thus, if I understand his argument, Watom is a Melanesian (as opposed to ancestral Polynesian) site whose inhabitants traded in at least some of the Lapita pottery there. The logic of this argument is based on the premises that true Lapita sites (like the Mussau ones) show no developmental ceramic sequences preceding classic dentate-stamped Lapita pottery and therefore 'must have had (their) origin in South-east Asia', and that these same true Lapita sites represent the ancestral Polynesians moving through Melanesia. Add to this the notion that Polynesian teeth are more like Southeast Asian ones, and it follows that sites with Lapita pottery but Melanesian teeth cannot be real Lapita sites. Bad luck Watom, despite the fact that it lacks a pre-Lapita developmental ceramic sequence in its earliest layers and thus is similar to the Mussau sites in this respect.

In this view, archaeology remains subservient to the physical anthropology. To me the real puzzle is that if we accept this reasoning, how many other sites do we now need to disqualify from being Lapita (without even considering the problem of how we detect them!)?

My own feeling is that the new data from the Bismarcks help confirm what we might have previously guessed. The cultural transformations which took place in the Bismarck Archipelago c.3500 BP were more complicated than we have so far acknowledged and they require more robust explanatory models than we are currently using. We now have an enormously long period for the development of adaptive strategies to accommodate the problems of surviving in the island world of the Bismarcks, but more importantly, and beyond this, there are sufficient indications in the archaeological record to consider as very likely that adaptations in this region were sufficiently successful to bring these Melanesians into increasing contact with different groups further west. Such contacts would have facilitated the flow of materials, technologies and people in both directions, as the Lou Island bronze and the Sabah obsidian evidence indicate. The ramifications of such a model can clearly be elaborated into future testable propositions.

The project failed in two areas. The first failure was our inability to find sites which immediately predate the appearance of Lapita in the Bismarcks, since as will be already apparent, this was a crucial period in our study. The second was our failure to attract any Papua New Guinea archaeologists to take reponisibility for any of the sub-projects. However John Saulo, from the National Museum, spent some time with the Kirch team on Eloaua, and Jean Kennedy, from the University of Papua New Guinea, visited Golson's team in the field.

This publication marks the completion of the Lapita Homeland Project as such. I am still surprised but gratified that it ran so smoothly and produced so many data. I would like to thank all my colleagues whose works appear here for their involvement, co-operation and prompt delivery, and for their tolerance of my own delays. In particular, while I was still at the Australian National University and in the early stages of this project, Douglas Yen offered endless advice and encouragement. I would like to thank both Doug and Jack Golson and the Department of Prehistory at the Australian National University for support and shelter and the opportunity to launch what was, in 1984, a particularly ambitious piece of research. It is right and proper that this report should emerge under the impress of the department from whence it all began. Finally, the dedication of this book to Doug in his retirement is from all the contributors and I refuse to take the blame.

ACKNOWLEDGEMENTS

Roger Green and Clive Gamble kindly pointed out the larger inaccuracies in a draft of this introduction. Those that remain are my responsibility.

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PLEISTOCENE VOYAGING AND THE SETTLEMENT OF GREATER AUSTRALIA AND ITS NEAR OCEANIC NEIGHBOURS

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When the sea level was lower, *Homo erectus* walked into the extended Pleistocene landmass of mainland Southeast Asia. At no stage of the Pleistocene was this connected to the former continent of Greater Australia and there is no evidence yet for a water crossing beyond Wallace's Line, although the continuing mystery of *Stegodons* (elephantids) in Sulawesi, the Philippines, Timor and Flores suggests these islands were once more accessible than now. *Homo sapiens* entered the island chains of Wallacea to reach Greater Australia, and beyond to the Bismarck Archipelago and the Solomon Islands. When and how this happened, and what routes it took, has been a matter of investigation, speculation and even simulation and this paper reviews the main arguments.

The suggestion has long been made that the penetration of the tropical belt of this new region would have been assisted by continuity of marine and plant environments (Golson 1971; White and O'Connell 1982:51) although by New Guinea there was a change to a mainly Australian fauna which, from a hunter's point of view, became sharply attenuated in the archipelagos further east (Thorne 1963; Green in press).

The archaeological evidence for the date of initial settlement now approaches 40,000 BP. One site of that age on what is now the Huon Peninsula of the north coast of New Guinea, had access to local lagoons and fringing reefs and a forested hinterland. There were flaked, possibly hafted, axes which have been interpreted as tools for forest edge manipulation (Groube et al. 1986) on this island, which by the early Holocene had evidence for probably indigenous plant domestication (Golson 1977).

By 30,000 BP there were continental adaptations as diverse as to the periglacial interior of Tasmania (Cosgrove 1989) and possibly even of arid parts of Australia (Allen 1989). Two New Ireland cave sites, Matenkupkum and Buang Merabak indicate use of coastal marine and lowland tropical forest resources c.32,000 BP (Allen et al. 1989; Allen this volume; Gosden and Robertson this volume) and possibly reflect initial New Ireland settlement, at least in the immediate area of these sites.

The pattern of dates does not show much delay before a successful crossing was made to the Solomon Islands further east. A cave on Buka which dates from c.28,000 BP contained mammal, lizard and fish bone, marine shell and flaked stone throughout its sequence (Wickler and Spriggs 1988). This island was then joined to a string of others stretching further south to Nggela. On Guadalcanal, which was separated by a very narrow strait, there have been surface finds of waisted axes similar to those dated on the Huon Peninsula which suggest a considerable antiquity in the south of the Solomons too.

DISTANCE AND ANGLE OF ISLAND TARGET

Birdsell (1977) examined in some detail the possible routes to Greater Australia. He noted that the sea between the Sunda and Sahul shelves was studded with islands, the majority of which were high, although a few chains of low ones existed as well. He identified two sizeable chains of large islands which extend between the two emergent shelves as the basis for his major Routes 1 and 2, the former in the north and trending towards the island of New Guinea and the other in the south and directed closer to the northwest coast of Australia. Birdsell specified five probable sub-routes - three in the north, two in the south - which contained between 8 and 17 stages (Fig. 1). At times of glacial maximum and, thus, lowest sea levels, no stage was greater than about 100 km and for the rest of the Pleistocene, when the sea generally stood about 40-70 m below current levels, the water gaps between high islands remained much the same although there were some appreciable changes of distance on the emergent shelves (Birdsell 1977:128).

Figure 2 plots inter-island distances and angles of island target as taken from British

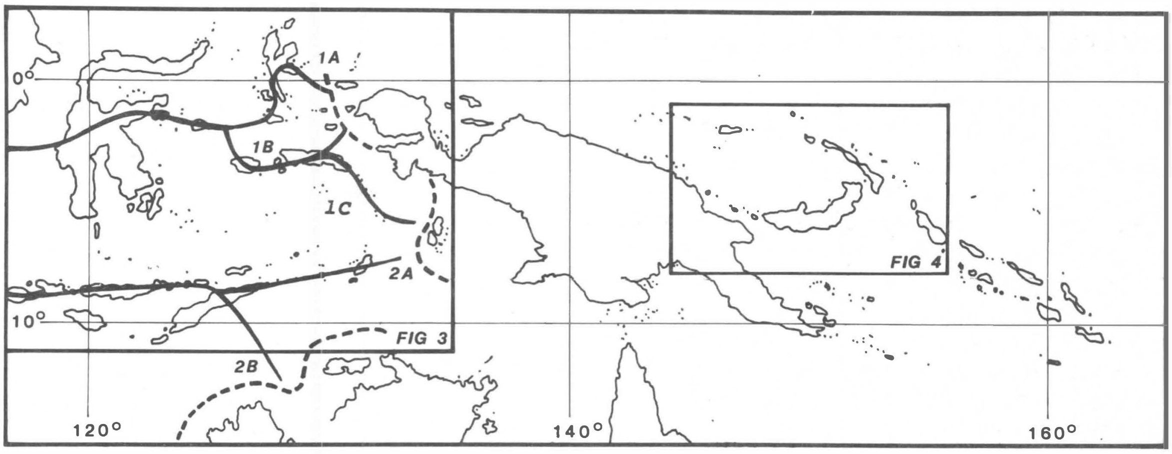

Figure 1 *A voyaging corridor of large, often intervisible islands joins Southeast Asia to Melanesia. Conditions allowed Pleistocene settlement as far east as the Solomon Islands. Possible routes shown through Wallacea are those suggested by Birdsell (1977).*

Admiralty Charts Nos. 941B, 942A and 942B, between the islands on the northern Route 1, which is shown here because it was intervisible all the way. Distances are calculated for the -50 m shoreline and angles of island target are similar to those shown by Birdsell (1977:124-5) although here they are directed only generally towards visible land ahead and do not necessarily distinguish every stepping-stone in between. These target angles do not allow for any expanded radii for it is not assumed here that colonists were able to detect the presence of land from beyond the range of sight offshore, using, for example, cloud formations, drifting smoke from fires, flotsam patterns, or seabird behaviour at this early time - although they had become part of the repertoire of purposeful navigation in the remote Pacific by the time of Western contact.

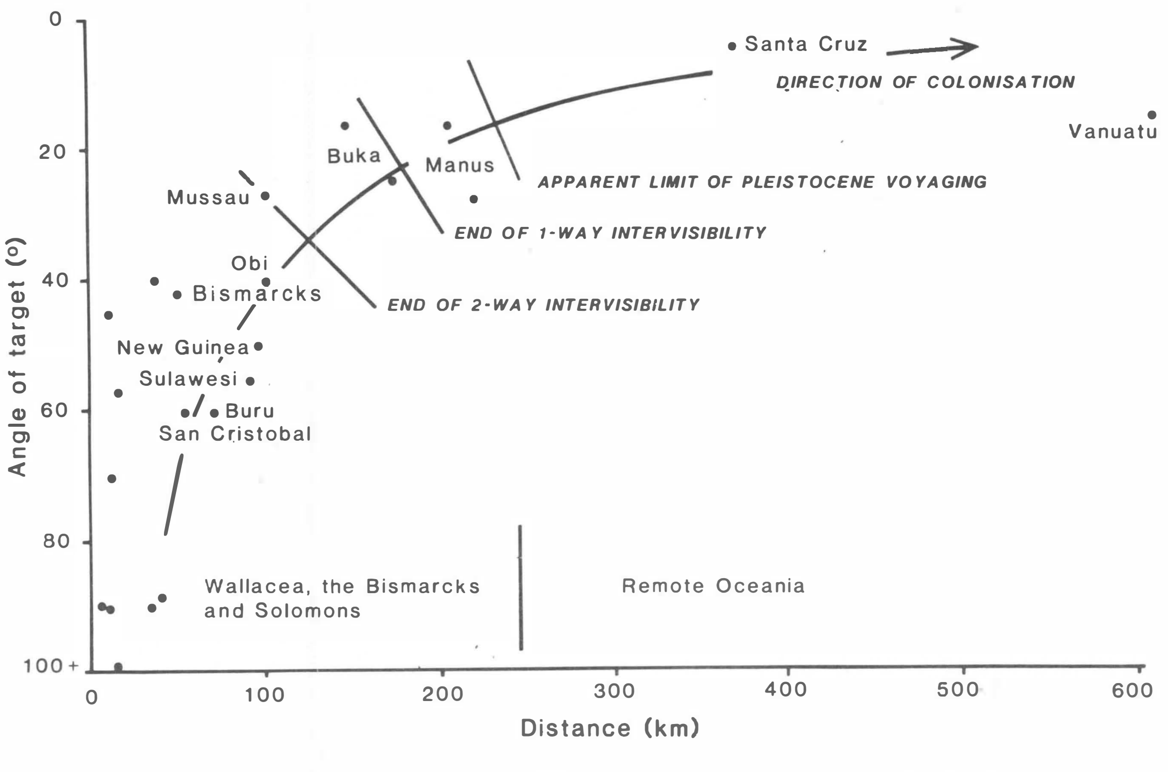

Figure 2 *A graph showing the relative accessibility of islands in the voyaging corridor by distance from their neighbours (calculated for a -50m shoreline) and by angle of target. In general, island accessibility and the nature of intervisibility relate systematically to the archaeological age of island settlement.*

It is interesting to note that the first voyage between the shelves, to Sulawesi, was much the same distance as the last, to New Guinea (Fig. 2) and that voyages further east to reach the Bismarck Archipelago and islands within the main Solomons chain were no greater. However, beyond the larger islands of the Bismarcks, circumstances gradually began to change. The island of Mussau lies c.98 km north of New Hanover at a distance no greater than others crossed already, however, the angle of target (23°) is rather less. No Pleistocene dates are reported yet from Mussau in spite of field work efforts (Kirch 1987; Kirch et al. this volume) but the island appears to be within range of the voyaging technology. Whether such a relatively small island was able to sustain early settlement, is another matter.

The distance from the Bismarcks to Buka, the closest of the Solomon Islands, was effectively greater than those before. Figure 2 shows a maximum crossing distance of approximately 140 km via Feni and of 175 km direct from New Ireland. Because Feni offers the smaller target there may not be much to choose between two routes. The Green Islands offer a possible but rather unlikely stepping stone, because they are small and low and could not have been detected except from close by. At all events, the greater distance to the Solomons was not enough to delay settlement appreciably, according to the current dates.

The island of Manus is still further. The then larger Pleistocene outlier was at similar distances from Mussau (200 km) and New Hanover (230 km) in the Bismarck Archipelago and also from mainland New Guinea (220 km from Karkar Island). The approximate target arcs from Mussau (17°) and New Hanover (15°) were less than from New Guinea (28°). Whether the greater distance to Manus delayed its successful settlement is still to be resolved. Distance and target angle are factors which are mediated by others including intervisibility, the pattern of winds and currents and island size but, certainly, Pleistocene settlement of Manus can be regarded as most likely (Irwin 1989:168-9 and Fig. 1) because the first significant navigational threshold occurs further east between the main Solomons chain and Santa Cruz (Fig. 2). This break has long been recognised as a biogeographic divide (Thorne 1963), and Pawley and Green (1973) have stressed the influence on human colonisation of diminishing resources. The marked difference in accessibility as measured by angle and distance of island target, helps explain why this navigational divide was concomitantly more difficult for humans to cross, just as it has been for other animals and plants.

INTERVISIBILITY

While Birdsell (1977) noted modern island heights, he did not translate this into specific distances from which they could be seen ahead, but he did correctly identify intervisibility as an important variable, as did Pawley and Green (1973). In fact it is (and probably was) possible to 'see', in the sense of island intervisibility, from mainland Asia to the end of the Bismarck Archipelago (Fig. 3). One of Birdsell's (1977) five hypothetical sub-routes (1A) provides intervisibility along an unbroken chain of islands. His other northern sub-routes (1B and 1C) are at the margins of intervisibility but the two southern sub-routes become blind. Fig. 3 shows ranges of visibility of a passive kind which refer to high land which can be seen ahead from sea level and which does not require a person to climb with the intention of seeing further. (Even if one stood on the highest point of Timor, it would not have been possible to discern the shore of Australia, when the sea was at its lowest stand in the Pleistocene.) The apex of each sighting arc in Figure 3 is positioned on a high point which can be seen from anywhere within the arc. It is interesting that the difference between lower or higher Pleistocene sea levels does not affect the drawing because the difference in sighting distance is accommodated by the thickness of the line representing the arc. Times of low sea level were no better than any others in this respect.

In terms of travelling in the reverse direction from New Guinea, Birdsell's Route 1A is now just beyond the limits of intervisibility whereas his Route 1B becomes clearly intervisible, which it was not from the other direction. However, it was not possible to see from Sulawesi back across the Strait of Malacca to what was then mainland Southeast Asia unless one travelled south from Sulawesi to islands from which Flores, in Route 2, is visible today. Whether it was in the late Pleistocene is a matter of conjecture.

In ideal conditions one can see as far as the curvature of the earth will allow, allowing for refraction of light by the atmosphere. If one is living on an island, at times conditions will be good enough to see what other land there is to be seen and people could acquire an habitual knowledge of neighbouring islands within sight. Visibility at sea is a very different case because conditions are variable and cloud, haze, etc.,

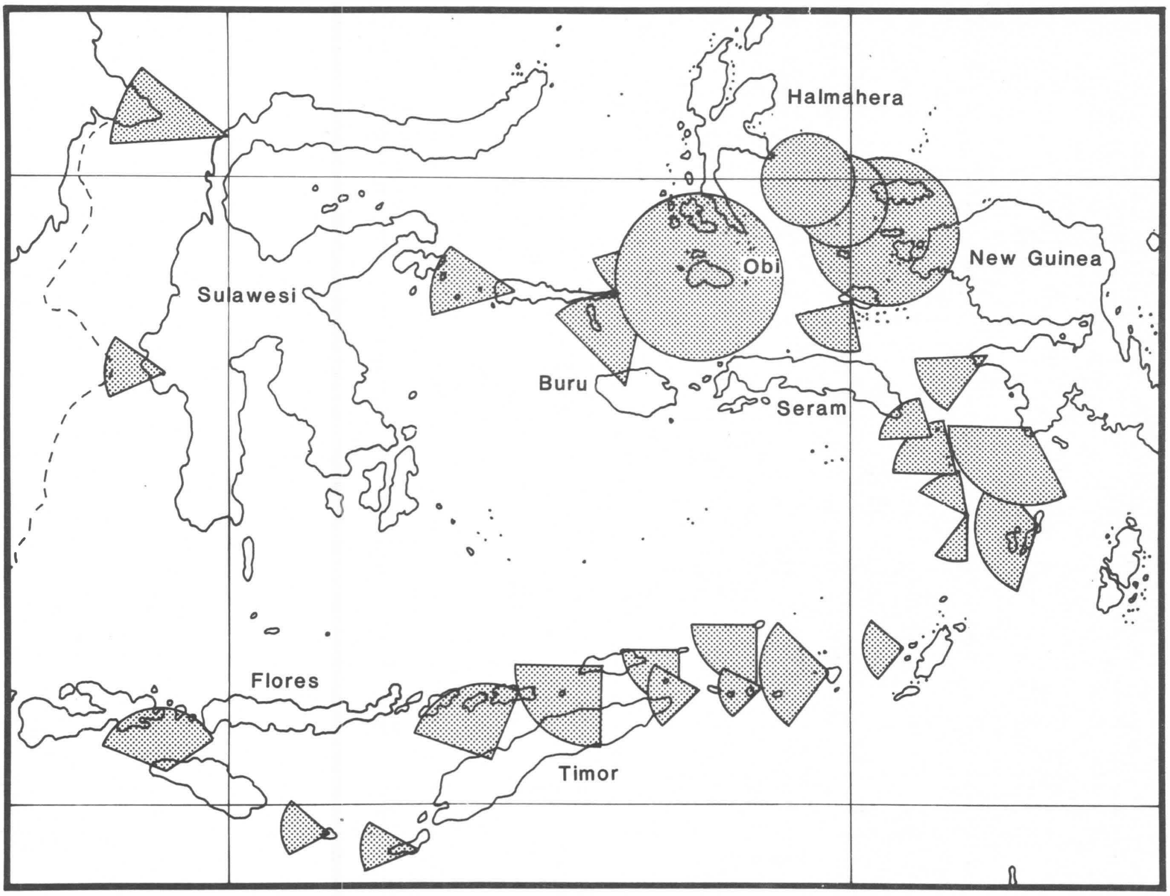

Figure 3 *Details of intervisibility eastward along Birdsell's (1977) postulated routes to Sahul. Sighting arcs show the ranges of visibility of high land standing at the apex of each arc. In the north, Birdsell's Route 1A is intervisible all the way from mainland Asia, while his Route 1B is intervisible on a westward journey. In the south, Birdsell's routes run blind. Fluctuations in Pleistocene sea level did not materially affect sighting ranges.*

severely restrict it. Sometimes one can see high land from a long way away, at sea, but such visibility cannot be depended upon.

Two-way intervisibility by land extends into the Bismarcks and Mussau lies approximately at the margins of it (Fig. 4). Crossing to the Solomon Islands involved a change to one-way intervisibility by sea - in which land appeared ahead before it was lost from sight behind. Feni can be seen today from New Ireland but the Solomons do not come into view until one is at least 40 km south of Feni. The Solomons can be seen some time after 55 km on a voyage from New Ireland. However, on voyages from both Feni and New Ireland the mountains of New Ireland can be seen behind, in ideal conditions, almost all the way across to Buka and, in most conditions, until the Solomons are in clear view ahead. Reaching the Solomons did not require losing sight of land but it is clear that Buka was not found because people already knew it was there before they left. However, the pattern of radiocarbon dates does not show that these conditions constituted a new navigational threshold here. Indeed, the same conditions could have been met already in Wallacea.

Manus is further offshore and lower in elevation than the northern Solomons. This necessitated a blind crossing and voyagers would have been out of sight of land for a minimum distance of 60-90 km, depending on the line of approach taken (Fig. 4), and on most occasions longer. Given that the Bismarcks were settled at least before 30,000 BP and the northern Solomons shortly afterwards, it is navigationally plausible that Mussau was reached at much the same time even if it was not settled then. However, Manus can only have been discovered by people already out of sight of known land which could have led to delayed settlement, but the length of any such delay remains to be seen. The recent terminal Pleistocene dates obtained for Manus by Ambrose and Spriggs (pers. comm. 1989) are in keeping with the aceramic but younger evidence

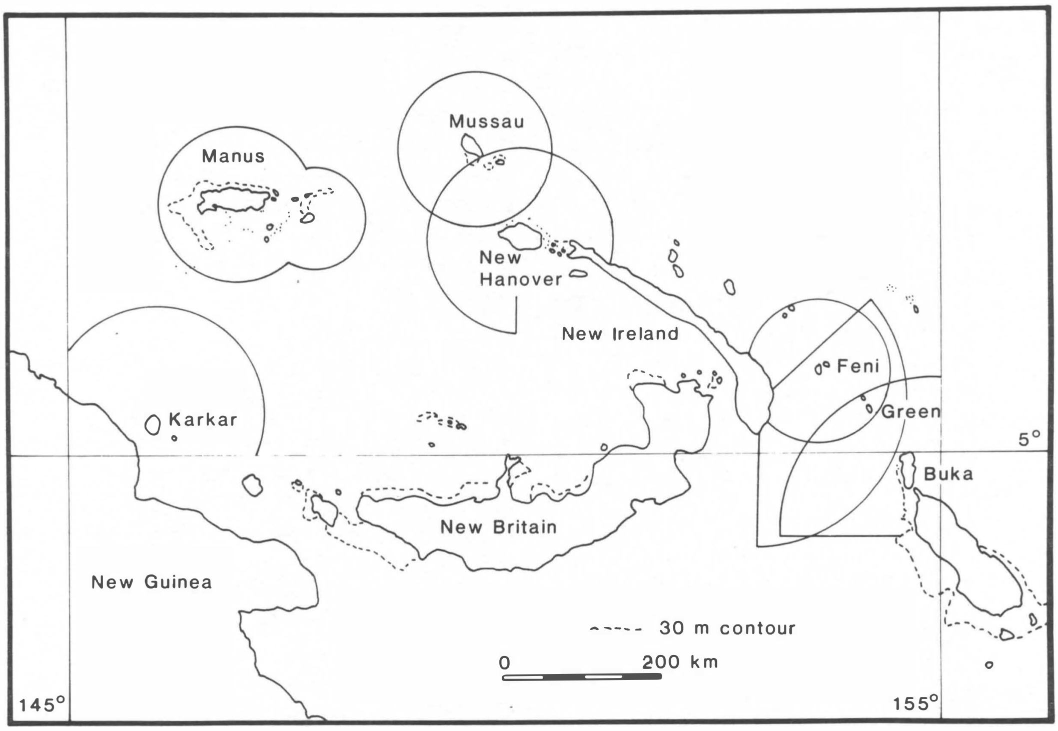

Figure 4 *Land is visible in both directions from New Guinea to New Britain and New Ireland. On a crossing to the northern Solomons, land cannot be seen ahead until New Ireland is at least 55 km behind. However, New Ireland remains within sighting range for most of the journey across. Manus requires a passage with a blind portion of at least 60-90 km. It can be expected that such differences affected the age of first settlement.*

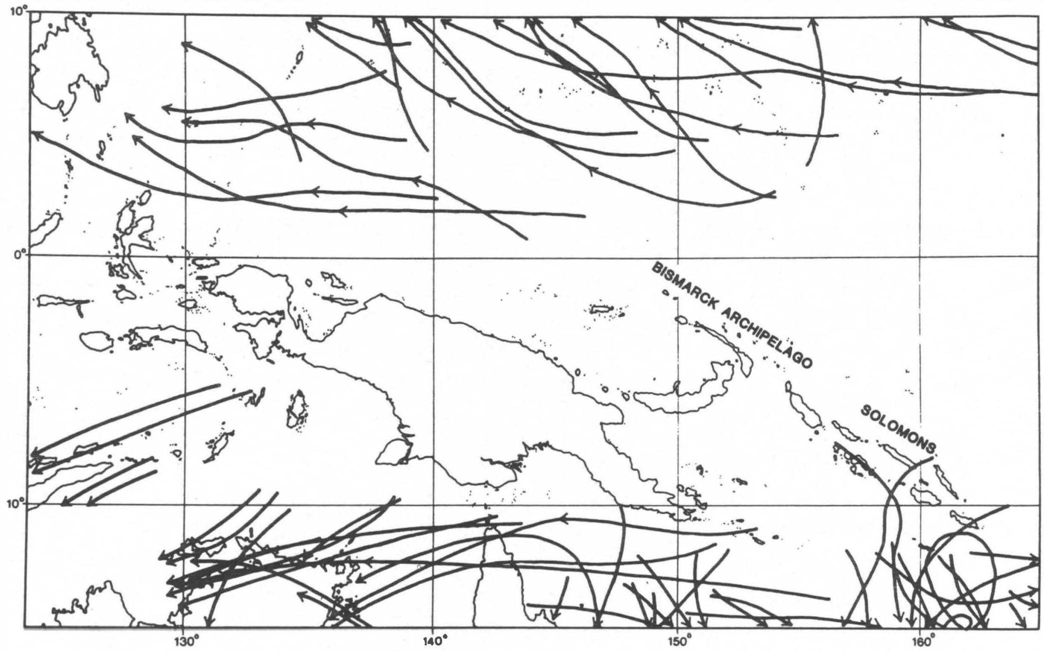

Figure 5 *The voyaging corridor lies in a band of shelter between northern and southern tropical cyclone belts. In terms of colonisation, it represented (1) a region of easy Pleistocene island-hopping, (2) a voyaging 'nursery' in which maritime technology was able to develop over a period of approximately 50,000 years, and (3) a large safety net to which the first tentative voyages of offshore exploration into the deep Pacific could return.*

previously published by Kennedy (1983) and with the proposed navigational context for Manus (Irwin 1989:168-9).

All calculations of sighting distance depend, of course, on substantial geological stability for the last 30,000 years or more in a region of known tectonic and volcanic activity (Birdsell 1977:133). Geological information suggests that the high points along Birdsell's Route 1 to New Guinea were likely to have been in existence during the time in question (R.W. Johnston pers. comm. 1988), while most uncertainty relates to the east of New Guinea. Southern New Ireland was probably already high, but both Feni and the northern Solomons are volcanic and could have been still rising and, if so, intervisibility would have been less than now.

CLIMATE AND WEATHER

Assessment of the settlement of this region is similarly affected by changes in climate, however, the assertion has been made that equatorial climates changed relatively little (Wild 1985:66-7) and there can be expected to be several points of similarity between the late Pleistocene and now. The temperature then was some five or more degrees cooler and sea levels were lower and shorelines were different. However, because the area is equatorial there would have been no substantial latitudinal displacement of weather systems. With respect to seasonality, the Intertropical Convergence Zone (ITCZ) would still have moved north and south with the sun. Today the ITCZ is the breeding ground of cyclones and those that blew in the Pleistocene probably left a band of shelter between the northern and southern cyclone belts not unlike that shown in Figure 5. Warmed air still rose over the climatic equator creating a wind flow north and south towards it while the earth's rotation and the Coriolus Force gave it the same easterly slant as today's trade winds. The monsoon of the southern summer is presumed to have persisted in some form. Thus we can allow for many differences but some continuity in general climatic pattern. It is also worth remembering the essential point that day-to-day weather is changeable and small boats, then as now, only require suitable conditions some of the time.

The general pattern of contemporary winds and currents is shown in Figures 6-9 (Indonesia Pilot Vol. III; Pacific Islands Pilot Vol. I). The southern winter is dominated by the southeasterly trade winds (Fig. 6) which the currents generally follow (Fig. 7). In the southern summer both the winds and currents around New Guinea and the archipelagos both west and east are influenced by the monsoon (Figs 8 and 9) while the northern hemisphere tropics are dominated by the north-easterly trades (Fig. 8). The general situation in this 'voyaging corridor' is of predictable seasonal reversals of wind and current, a sheltered equatorial position between bands of cyclones and a large measure of intervisibility which accommodates all islands but the Pleistocene outliers (Irwin 1989).

PLEISTOCENE BOATS AND NAVIGATION

In 1977 Birdsell wrote 'archaeology provides no information as to the types of watercraft which might have been used' (1977:134) and the situation has not changed since. However, he mentioned various possibilities, including logs, bundles of bark and reeds, dugouts, bark canoes and rafts of mangrove and bamboo - the last of which he preferred. He also noted that bamboo occurs naturally in the region of his Route 1 and as far along Route 2 as Java. Doran (1981) concluded, too, from a study of Pacific boat types that the raft would have been used first even though distributional evidence might suggest the bark canoe could be earlier. Lewis (1977:4-8) also noted that rafts were widespread, easy to build and very functional. Bamboo is ideally suited to rafts as it is strong and buoyant, does not rot and can easily be lashed together, so the case these commentators jointly make is very plausible. However, some other discussion in the literature concerning finer details such as vessel shape amounts to little more than unrealistic conjecture.

More than one kind of craft could have served because the conditions in the region were not sufficient to exclude all others. Horridge (1987) makes the suggestion that early boats had sails but distances were not so great that boats without them could not have made the same crossings. They could also have been easily driven by wind on the way. It is inconceivable, too, that in 25,000 years of Pleistocene water crossings, boats should not have varied and sometimes been improved. Further, arguments based on the modern distributions of boat types as an indication of former distributions ignore the obvious fact that boats are a mode of transport in themselves and not just passively moved like other cultural traits, nor is there any chronological control of when ideas about them may have spread.

One detail on which we may be more secure concerns boat size and White and O'Connell

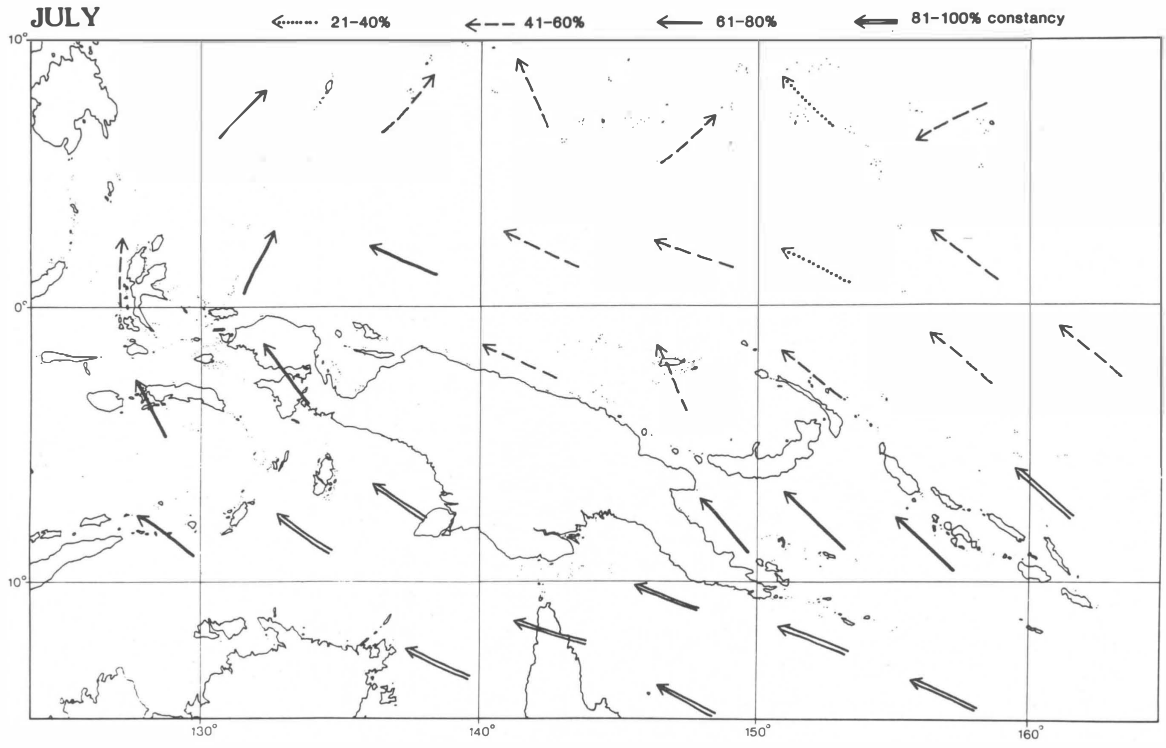

Figure 6 *Wind patterns of the tropical trade wind season in the southern winter. Seasonal changes in wind direction commonly allowed simple return voyaging between islands of this region.*

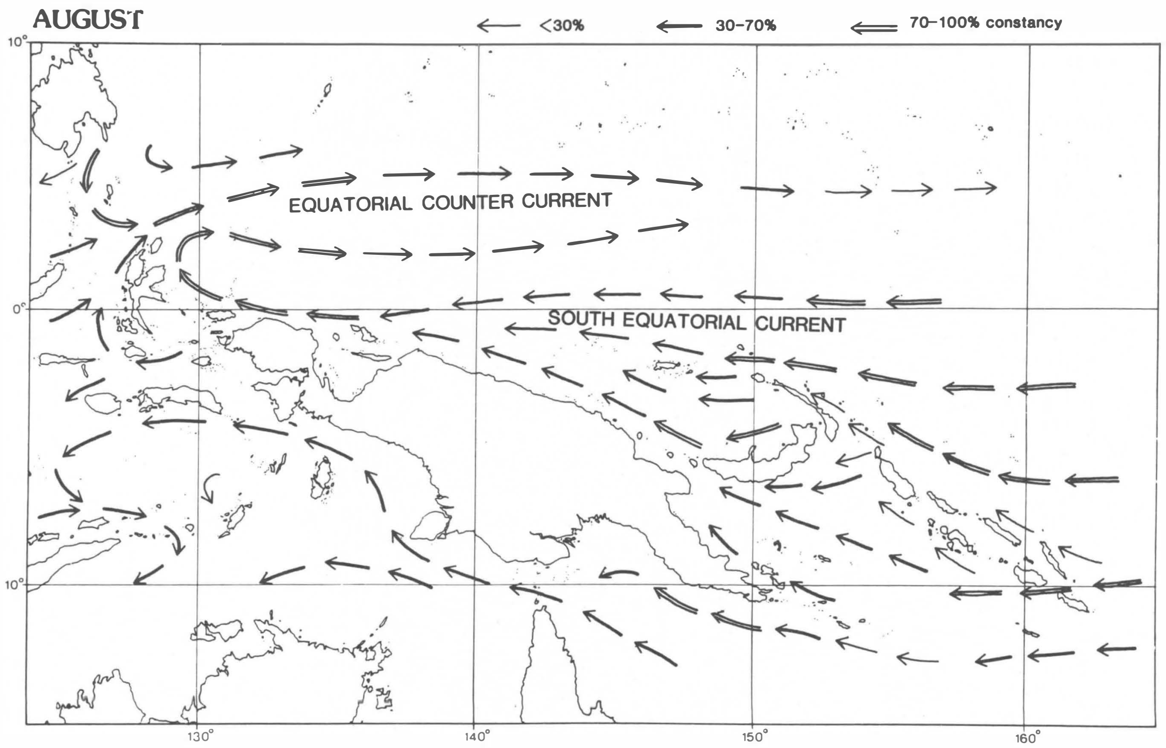

Figure 7 *Sea currents of the southern winter.*

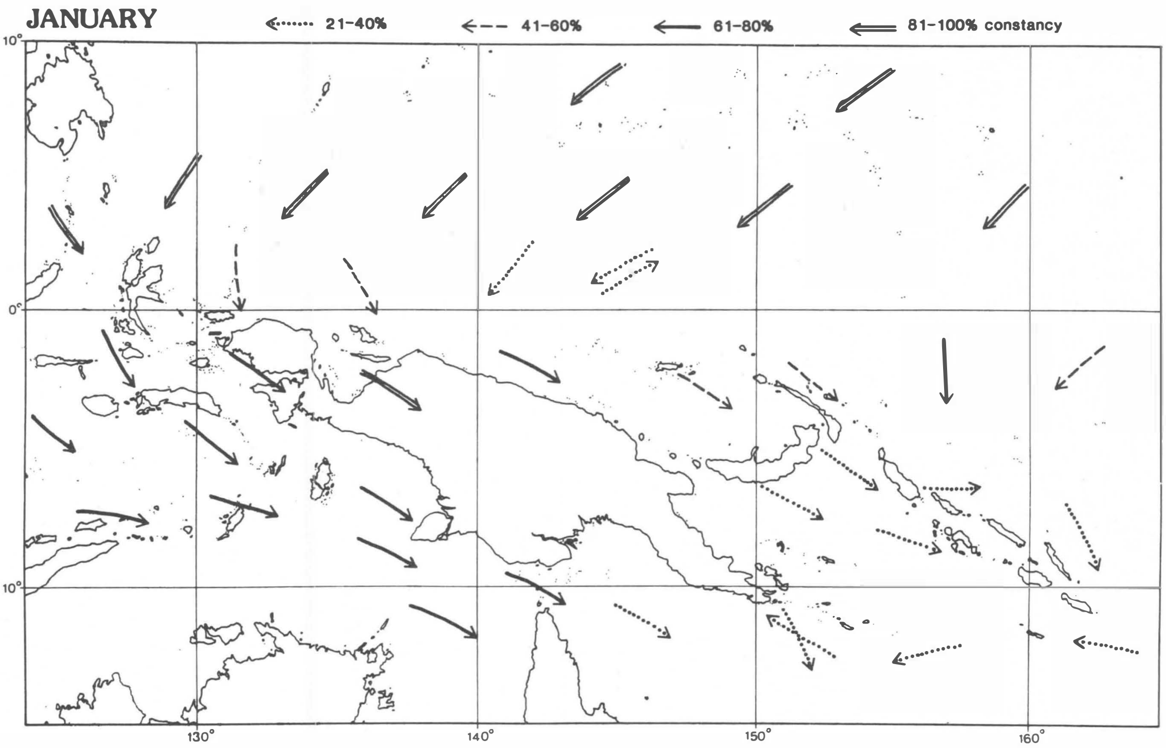

Figure 8 *Wind patterns of the tropical monsoon season of the southern summer.*

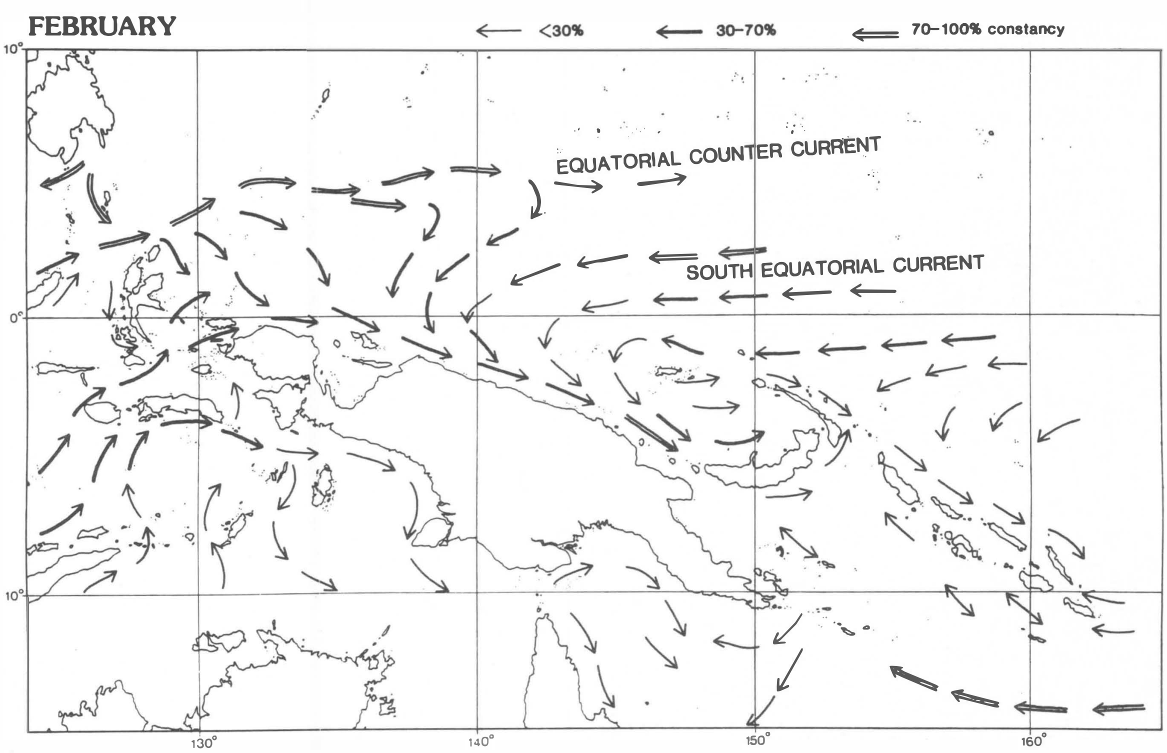

Figure 9 *Sea currents of the southern summer.*

pointed to the necessity of getting a founder population to a new island all at once.

> The often-promulgated idea that the offspring of a single pregnant woman clinging to a log could ultimately populate a continent ... can be relegated to the realm of theoretical biology (White and O'Connell 1979:24).

In fact, computer simulations in demography have suggested that founder groups could have scarcely contained less than four or five people to make up a viable unit (McArthur et al. 1976), and groups must have been sufficient to reproduce socially as well, which is a more difficult factor to put a number on. Therefore, whatever the boats were, they must have been reasonably substantial even if still unsophisticated.

PLEISTOCENE PASSAGES IN THE VOYAGING CORRIDOR

Some prehistorians have favoured migration routes which followed shorter water gaps (e.g. Birdsell 1977) but to make fine distinctions between the different distances involved is to miss the point that they were probably all short enough for the risks to remain much the same. Similarly, the suggestion that times of very low sea level may have been more opportune for island hopping fails to take account of the practicalities involved. A boat which is seaworthy enough to cross 10 km can probably cross 100 km or more provided it is not of materials which become waterlogged and provided the weather remains constant. Factors other than distance are more telling and, in particular, winds and currents greatly affect the speed and direction of simple craft. Favourable winds and currents can make some relatively long voyages simple while short ones against winds and currents may be impossible.

Single passages of just a few days would have been enough to settle Sahul and would have been within the endurance of those who made them. Inspection of the patterns of winds and currents in Figures 6-9 allows one to assess the feasibility of particular links although one could not automatically say the same was necessarily true in the Pleistocene. From Island Southeast Asia to New Guinea one would assume that easterly movement is easiest in the summer monsoon, while the reverse is true in the trade wind season. Contact between Timor and northern Australia has been the subject of a computer simulation by Wild (1985) which assumed similar conditions of climate and it was found that most canoes drifting off the south coast of Timor in midsummer (monsoon) reached Australia but, interestingly, the reverse voyage was difficult at any time of year and rafts from Greater Australia were blown back again to the Australian coast or into the Indian Ocean (Wild 1985:69). The pattern of currents shown in Figures 7 and 9 suggests that the settlement of both the Bismarck and Solomon archipelagos would be probably in the southern summer, but that Manus was most easily reached from New Ireland in winter when conditions in the area remain mild.

VOYAGING INTENT AND FREQUENCY

Keegan and Diamond (1987:66) suggest that Westerners assume they are the only ones capable of purposeful exploration and that others did it by accident while exploiting coastal areas in simple watercraft. In the case of Sahul, White and O'Connell (1982:46) have suggested that the long sea trips involved 'implies that the settlement was both accidental and unlikely to have been much supplemented by later voyages.' However, Birdsell (1977:123) took the opposing view that

> it is highly probable that there was a constant if somewhat straggling trickle of small groups of human beings over all or most of the routes. The size of the watercraft likely to have been used suggests that the groups consisted of small biological families.

As an alternative to this, one could postulate two main migrations (Bellwood 1986) to match the suggestion that there were two corresponding biological populations in Australia, possibly represented by gracile human remains typified by Lake Mungo and more robust individuals from Kow Swamp. However, Allen (1989) comments that we are not in a good position to draw implications for colonisation while there is still a dispute as to the origins and amount of biological diversity in Australia, and one might include New Guinea in this respect, too.

Lewis (1977:6) thought Sahul could have been reached accidently more than once, but that the intervisibility of islands would also have allowed deliberate settlement. Certainly, it is no surprise that, given suitable boats, people should have reached known land nearby and in some areas, with the change in season, crossed back just as easily. Indeed, it is unlikely that if Sahul was reached at all it should happen only once, and also most unlikely that any single voyage penetrated the whole distance from mainland Asia to Sahul. Not all agree and Thiel (1987) points out that the greatest possible distance from Sundaland to Greater Australia is about 1000 km, a distance which could be travelled

> in about 14 days or, with some knowledge of sea-faring, even less. There is no need to suppose generations of island-hopping and perfecting of seafaring ability. The entire journey could have been accomplished in two weeks (Thiel 1987:239).

One must concede the possibility as a remote one but, on balance, it seems likely that there were many inter-island crossings and, with growing experience, the number of intentional crossings increased and covered an expanding field, but how and when this happened, in pre-Lapita times, is not known.

Intentional movement from New Britain to New Ireland may be inferred from the presence of Talasea obsidian in the site of Matenbek some 20,000 BP (Allen 1989), but the strait between these islands is narrow and has the Duke of York group as a stepping stone (Fig. 4). It may be a measure of voyaging that no early obsidian has been reported yet from the Solomon Islands, or from across any other substantial water gap, although the archaeological samples may be too limited to tell.

To attribute the settlement of Sahul to the presence of *Homo sapiens* is only to beg the question of how their capacity to cross water was greater than earlier humans although, apparently, this was so. Another suggestion of interest is that environmental changes in the Pleistocene provided an inducement for this. The essence of an argument by Thiel (1987) is that during periods of rising sea level, land area would have reduced, possibly causing increased population density especially on islands, and this might have led to people searching for new land. Conversely, when the sea level dropped, land area increased, together with resources, and people had no incentive to leave. This argument is offered in opposition to scholars who have suggested low sea levels as the best time to cross - which has been rejected here already for different reasons. However, with regard to the issue of sea levels and resources, Bellwood (1986) effectively makes the opposite case and reports that the sea level rise at the end of the Pleistocene actually resulted in the approximate doubling of the coastline length of Sundaland (Dunn and Dunn 1977), suggesting such events were actually favourable for the mainly coastal dwelling population (Bellwood 1986).

Knowledge of the extent to which Pleistocene voyaging was purposeful and deliberate is not accessible archaeologically and to dwell on it might be to overlook the very clear evidence that the human colonisation of Wallacea, Greater Australia, the Bismarcks and Solomons, was undoubtedly systematic. The available archaeological evidence for the order, elapsed time and nature of early settlement of the islands and groups in the region fits neatly with the factors described above, including angle and distance of island target (Fig. 2), the patterns of intervisibility (Figs 2-4) and island size. Further, it is not inconsistent with other factors which are currently less well understood including geological history and the patterns of Pleistocene winds and currents (Figs 5-9). Finally, the coherence of the evidence, although far from complete, now allows us to make general predictions for islands which still lack archaeological control.

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MODELS FOR MATENKUPKUM: INTERPRETING A LATE PLEISTOCENE SITE FROM SOUTHERN NEW IRELAND, PAPUA NEW GUINEA

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This paper has two purposes: firstly, to present the data from the site of Matenkupkum in more detail than has been done so far (see Allen et al. 1988; Allen et al. 1989), although not in its final form; and secondly, to explore means of modelling a data set with some unusual features to it when compared either with other aspects of the Pacific archaeological record or with ideas current about the Palaeolithic in other parts of the world. As the site's data will only make sense when set in some framework, possible models are discussed in the first part of the paper, and the data is in the second part. Before considering either of these aspects, however, some discussion is necessary as to why the Matenkupkum data are unusual and challenging.

In a recent world survey of island colonisation Keegan and Diamond (1987) show that, with the exception of Southeast Asian islands and the continent of Australia, the first known human uses of islands occurred during the Holocene. Thus the first moves onto the Aleutian chain and the Mediterranean islands occurred in the terminal Pleistocene, whereas the Caribbean islands were not settled until the early Holocene (Keegan and Diamond 1987:56). Against this background the colonisation of islands in the Bismarck Archipelago and the Solomon chain (Wickler and Spriggs 1988) some 33,000 BP represents a remarkable phenomenon. The site of Matenkupkum in the southern part of New Ireland (Fig. 1) was first occupied c.33,000 BP providing the earliest evidence we have for movements off the continent of Greater Australia. Such a movement not only implies that seafaring technology was available during this period, but that people were able to live in relatively pauperate land environments, as will be discussed below. As well as exploiting existing terrestrial and marine resources (thus providing some of the earliest evidence of fishing in the world), during or after the glacial maximum the inhabitants of southern New Ireland were altering their local resource base to increase their sources of food. Some 30 millennia later when people moved into Polynesia they faced the same problems of travel by sea, plus lack of food on the land and solved this latter problem by taking with them most of the food, animals and crops they needed. The early evidence from Matenkupkum and the other New Ireland sites therefore fits within the overall aims of the Lapita Homeland Project in providing insights into the origins of the sets of skills necessary to colonise Oceania.

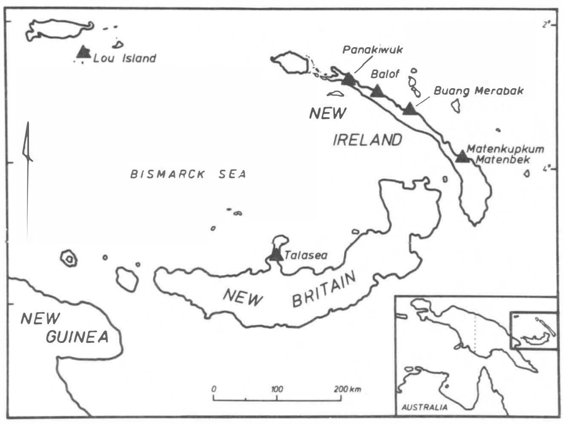

Figure 1 *The Bismarck Archipelago, showing the location of Matenkupkum and other places mentioned in the text.*

The New Ireland sites provide a new dimension to Pacific prehistory by showing that aspects of life evident from the late Holocene have an immensely long history. When viewed against a background of world palaeolithic studies the sites have different points of interest. The sites lie today within areas of lowland rainforest and although we have no data on past vegetation from the region it is likely that rainforest would have been present in lowland areas throughout the last glaciation. Very little is known about tropical hunter-gatherer groups, either past or present (the literature from Australia and southeast Asia is reviewed below); however, there is reason to believe that rainforest groups operate very differently from people in more seasonal environments where many of the world's palaeolithic studies are carried out. Put briefly, most world models are concerned with time scheduling in a seasonal round and with settlement patterns which are structured to take advantage of seasonally available resources. The humid tropics are non-seasonal (although plant resources do become available at different times of the year in a manner which is ill-understood) and therefore sites occupied at specific seasons are unlikely to occur. In the rainforest, problems of access to resources occur not due to time, but to space. Rainforest resources are widely dispersed and often in small units. Consequently, regional archaeological records in rainforests are likely to be different from those elsewhere and the site types defined for other parts of the palaeolithic world will not fit New Ireland. Unfortunately then it is impossible to borrow modes of interpretation used elsewhere in the world. Instead, part of the exercise of making sense of our new data is to develop new models to frame these data. Such a task cannot be based on the results from one site or accomplished within the bounds of a single paper. The treatment of the data from Matenkupkum will not provide interpretations, but rather represent an attempt to use the data we have to frame modes of explanation which can be tested against the results of future work.

DEVELOPING MODELS

It is a truism of recent archaeology that because people of all periods spread their activities through regions, rather than living on single sites, it is impossible to understand patterns of action from the past on the basis of excavations in sites which have no broader regional context. In the case of Matenkupkum we only have evidence from one site (a second site, Matenbek, some 70 m distant was excavated in 1988, however analysis of the material is still in progress and only slight reference will be made to it here - see Allen et al. 1989 for preliminary details). It is therefore necessary as an initial analytical step to situate the site within possible regional archaeological structures. To discover how this site fitted into possible regional patterns of action we need to do two things: firstly, to lay out models of the archaeological record on a regional scale and secondly, to attempt to understand the structure of the evidence within the sites. Both these sets of structures, regional and site specific, need to be linked to patterns of past human action. As the regional pattern of life is seen to underlie the role of particular sites, regional questions will be covered first.

When dealing with a new set of archaeological data a common response is to search for ethnographic parallels in the hope that these will provide some models. In this case, however, there are compelling reasons to believe that present day tropical hunters and gatherers are in a particular historical situation which had no parallels in the past. The major set of examples of hunters and gatherers within the tropical rainforest from our general region are those of Southeast Asia. During the nineteenth century many Europeans viewed hunter-gatherer groups in places like Malaya, Borneo and the Philippines as relict populations displaying ways of life which had vanished elsewhere due to the incursions of farming groups. Modern ethnographers tend to take a rather different view. Rather than seeing forest dwelling groups as being living fossils keeping palaeolithic traditions alive into the modern era, these groups are seen as having a particular position within a 'world system' that came into being some 1500 BP. Hoffman (1984) provides the most explicit presentation of this view and argues that the Punan of Borneo were once farmers who now live in the upper forested reaches of river valleys in a close symbiotic relationship with their farming Dayak neighbours who live at the coastal ends of the valleys. The Punan supply forest products (e.g. resin, birds' nests, camphor, rattan, rhinoceros horns) to the Dayaks and more especially into trading networks which transport these items to China. The Punan have positioned themselves in the rainforest for historically specific reasons, Hoffman argues, responding to Chinese demands for forest products by specialising in their supply, a form of trade which may go back to the middle of the first millennium AD when the first trading empires were set up. Hoffman (1984:144-5) feels that many, if not all, the nomadic groups of Southeast Asia may be designated as secondary hunters and gatherers, people who gave up farming to exploit a new set of social and economic opportunities. Similar points are made by Sandbukt (1987) and Ellen (1987), whilst Griffin (1984:115-8) surmises that a previously flexible strategy of hunting, gathering and swiddening carried out by the Agta of northern Luzon dropped the swidden component as population grew and non-Agta farming groups moved into the area.

The other set of examples of potential utility from this part of the world come from tropical Australia. However, examples of possible interest seen in studies such as those by Chase and Sutton (1987), who discuss relatively sedentary groups in the tropical environments of Cape

York, highlight the seasonal availability of resources and the complex set of social connections which existed in all directions. Neither the environmental nor the social situation of the Queensland groups fit those of Pleistocene New Ireland and the use of the former to understand the archaeological record of the latter is dubious.

Although consideration of present day models brings us to a negative conclusion as to their utility, a more positive aspect can be gained from the comparison through thinking about why recent models do not fit. The answer to this lack of fit lies in history: the groups of the present are situated within a set of historical relationships which influence their activities in the rainforests. A means of conceiving of these relationships on a broad scale is through the term 'world system', which has been used above without explanation. The term originally derives from the work of Wallerstein (1974), who used the idea to understand the development of the modern capitalist world from the sixteenth century onwards. The term has been removed from its original context and used in a broader sense within archaeology (Rowlands et al. 1987). Wallerstein's main point was that it is impossible to understand the development of capitalism by focussing just on Europe, as many historians had done, and ignoring the rest of the world which contributed raw materials, labour and precious metals to the industrial process. In his view Europe and the rest of the world were involved in one set of relations which could not be analysed piecemeal, but had to be seen as a global whole, with Europe forming the centre of the world economy and the rest of the world a periphery. Through the 1970s archaeologists have struggled to reconcile an interest in local development, which had arisen to replace diffusionism with an interest in trade and exchange as both an indicator and cause of particular social structures. Wallerstein's ideas are attractive in that they allow one to set local sequences of change in a broader social perspective, which includes links between regions through the movement of materials and people.

The basic metaphor of centre and periphery is used here as a device for understanding how local relations within Pleistocene New Ireland were situated within broader connections with other parts of Papua New Guinea. Even at this early stage in the occupation of Greater Australia there is evidence that ways of life were not the same in all parts of the continent (Allen 1989) and it is only by understanding the place of any one region within the continental mosaic that we can chart changes within that region. It is through the process of colonisation that this mosaic of different ways of life was extended and each new movement of settlement altered relations with existing areas of settlement. Colonisation is a double-edged process affecting both the regions already inhabited (a fact often overlooked) and the newly created parts of the world system. Seen from this perspective, colonisation represents a shifting centre of social gravity, as old peripheral areas become new cores with each extension of the periphery. This change from periphery to core seen in the movements into New Ireland has no precedent within historical studies of social expansion and is thus especially interesting. The parallels to such a process known historically, such as the western European colonial expansions or the movement of Russian speakers into the Asiatic republics of what is today the Soviet Union, were, firstly, moving into already settled regions and secondly, had such different social and economic structures to those of the Pleistocene as to provide no parallel at all.

The process of colonisation will set up social relations at the broadest level which are unknown to us today and these broad social relations will change as the process of colonisation takes place, whether or not there are more local causes of change. A corollary of this previous statement is that to understand patterns of life in a local region (e.g. the area around Matenkupkum) we must take these broader social changes into account. Analyses of local areas cannot start and finish with a consideration of local conditions but must take areal conditions into account. Thus although the main focus of this paper is the evidence from Matenkupkum we have felt it necessary to include discussion of broader changes within the region.

The model of regional change proposed here is as follows: for a long period of around 10,000 years after initial colonisation, the Bismarck Archipelago (and probably the Solomon chain) remained a periphery to the mainland Papua New Guinea core. During this period there were relatively low populations moving widely between the dispersed resources of the region. A crucial factor here is the notion of mobility. In the biogeographical models used to understand the human colonisation of islands, the sea is often seen to be the same sort of barrier to human movement as it is for other land animals. Obviously, given the appropriate sailing technology the sea can be seen as facilitating movement and communication, rather than hindering it. There is no reason to believe that people at the period of initial colonisation of New Ireland would not have been adept sailors and therefore one idea put forward is that groups may have been extremely mobile, due to their use of boats. The term 'strandloopers' springs to mind here and indeed a situation reminiscent of Groube's original idea (Groube 1971) might be envisaged, although transported 30 millennia into the past. This being the case the same tests apply to the present model as were applied to the original and it is necessary to consider the opposite hypothesis that people were in fact land-based and had more constrained patterns of mobility (see below).

During the glacial maximum, relations within the Bismarck Archipelago appear to undergo a change. Resources, such as obsidian and phalanger, appear in the sites for the first time. Also, the number of known sites increases, with Balof and Panakiwuk both in use for the first time around 15,000 BP. Such changes may indicate a new mode of life, where, instead of moving people to dispersed resources, the resources themselves are being introduced to increase the local resource base. These changes may also reflect a shift in the regional position of the Bismarck Archipelago from being on the periphery of the Papua New Guinean system to that of core in its own right, the change being caused by new extensions of settlement into the Pacific. In this regard the controversial New Caledonian tumuli, with their earliest dates at the end of the glacial maximum, are of especial interest, recent doubts about their human origin not withstanding (Green 1988). The regional position of the islands of the Bismarcks can only be clarified through further work, charting the process of expansion eastwards through Melanesia.

MODELS OF LAND USE AND ARCHAEOLOGICAL STRUCTURE

The above discussion has provided us with broad notions of the regional structure of society in the Pleistocene, which are amenable to further test. Such testing can only take place initially at the local scale. The following section explores possible models for local site type and local regional archaeological structures. In constructing such models the discussion will make the following temporal divisions of 30,000 BP to 20,000 BP and 20,000 BP to 10,000 BP, found in the above discussion. The earlier period is considered first.

For this period, the notion of seaborne mobility, utilised to move people between dispersed resources, was proposed. This idea can be tested by looking at the implications of a contrasting, landbased pattern of mobility. Following the seafaring notion first of all, we would expect to find sites (and perhaps off-site scatters) where people assembled to gain specific resources (stone for flaking, reef shells and plant foods such as sago). These concentrations are most likely to be preserved in caves and shelters, given the active geomorphological processes on New Ireland. Thus the earliest evidence will derive from cave sites near desirable resources.

The alternative landbased model can be divided into two variants, depending on the balance between the importance of land versus sea resources. A number of recent discussions of maritime environments, such as those by Palsson (1987) and Yesner (1980), have emphasised the possibilities that coastal economies offer to develop sedentary forms of life. Such ideas are obviously relevant to New Ireland, which, by virtue of its elongated shape, has an unusually high ratio of coast to inland, particularly in the north. Thus one might expect a concentration on sea resources to lead to relatively sedentary settlement, especially if initial population levels were relatively low. In contrast, the greater the use of the inland rainforest resources the more mobility we would expect in residential sites and daily moves. Estimates for the range of such movements are difficult to obtain. Kelly (1983: Table 1) gives mobility data for hunter-gatherers in different parts of the world (it should be noted that his tropical rainforest examples include the Punan and Semang who, as was mentioned above, have specific historical reasons for movements through the rainforest in a search for items of trade). In any case, the figures he gives vary widely for rainforest groups, from 40.5 km^2 to 2475 km^2. The upper figure encompasses about 25% of New Ireland, which has an area of 9800 km^2. Thus from Kelly's present day data New Ireland could either have accommodated 240 groups or four.

Returning to our main theme, if we assume that groups heavily utilised marine resources and were sedentary as a result, we would expect higher levels of archaeological material than with the strandlooper model, but that this would again be mainly concentrated along the coast. Again the archaeological record would only be preserved in some areas, so that geomorphological factors would have to be taken into account. But in this model sites would have to be near to the coast to have early deposits. As groups concentrated most of their activities on the coast, a rapid fall-off in archaeological evidence as one moves inland would be predicted. Following the second variant of the land based model, where there is a more balanced use of land and sea resources the expectation would be that of a more even spread of materials between the coast and the inland. Only in this variant would one expect to find inland sites from the earliest period of colonisation. The seashore would still represent a favoured zone for settlement, as it was only here that people could combine land and sea resources relatively easily. We would predict, following Binford's idea of foragers (Binford 1980) a site structure with base camps found near the sea and locations for specific activities inland.

The world systems model laid out above predicts a change in local landuse resulting from changes in the broader social system from around the onset of the glacial maximum. Here an earlier system of sea-based mobility is seen to give way to a more land oriented way of life. If this is true, then in the glacial maximum period we should see a change from an archaeological record occurring mainly on the coast to the sort of regional structure outlined at the end of the last paragraph, with a coastal base camp-inland location set up. If, of course, the broader model of changes proves to be wrong, then the local changes will not be of the form predicted and it could then be that coastal base camps and inland locations will be found dating from the period of the earliest occupation of New Ireland.

PATTERNS OF SITE USE

Having considered both the changing world system in which New Ireland may have been located and the possible consequences of these changes for regional archaeology on New Ireland it is now possible to get down to the real subject of the paper, the site of Matenkupkum. The link between the broader levels of discussion above and internal site structure is provided by the idea that the nature of site use will reflect the patterns of land use in a region. However, in order to make such predictions we need to be able to specify links between use of a region and site use. Binford's discussion of hunter-gatherers at a global scale would lead us to believe that rainforest peoples will be foragers, creating only two site types: the base camp and location (Binford 1980). Base camps will have evidence of a relatively wide range of activities carried out by the group as a whole, whereas locations will contain indications of very specific sets of hunting or gathering activities. Only some variants of the models presented above lead us to expect a base camp-location dichotomy. Therefore, following the predictions of the strandlooper model we would expect a site structure outside the range of variation known from examples cited in the ethnoarchaeological literature. Exactly how sites would diverge from this variation is unknown, leaving us only with a negative expectation that the early New Ireland sites will be different from any known today.

This unsatisfactory situation is exacerbated by recent comments such as those by Brooks and Yellen (1987) who have offered some sensible suggestions as to the factors operating to create or destroy evidence of the structure of activities at a site, based on ethnoarchaeological work in the Kalahari. They say that the more continuously a site is used, the more frequently its inhabitants will be forced to clean up the site and thus less of the structure of the activities on the site will be preserved. Sites which were frequently or continuously used will have much of the debris generated on them dumped elsewhere, leaving relatively unstructured evidence on the site itself. Sites only used once or occasionally may preserve a very good record of the activities carried out in them, as long as other taphonomic factors do not intervene. The nature of cleaning up will be all the more pronounced in constrained spaces such as caves, where people's room for manoeuvre is limited.

The above discussion does not provide us with any readymade guide to site types on New Ireland, as these may be different from any found ethnographically. It does, on the other hand, allow us to pose a series of questions which need to be tackled in order to establish individual site usage. These questions pertain to four areas: the intensity of site use, the range of activities carried out at a site, the nature of cleaning up on the site and the time resolution possible from the dates at the site (this last factor is in many ways crucial as it influences our ability to answer all the other questions). Each of these questions is addressed in the discussion below.

The problems discussed above are not quite as daunting as they might be because we can compare and contrast two sets of information. We can look both at the position and site types found in different parts of New Ireland in comparison with the internal structure of the materials at different sites. With Matenkupkum we are dealing with one site only. However, the models advanced above will provide the means for discussing the possible position of this site in the landscape, allow us to speculate about how the site fits into the pattern of evidence from the island as a whole and to put forward models for testing in the future. Before considering the data from the site, some general background is needed on the site, its local region and the process of its excavation.

GENERAL BACKGROUND

Matenkupkum is a large limestone cave located on a terrace above the sea, close to the village of Hilalon, on the east coast of the southern part of New Ireland (Fig. 1). The deposit in the cave dates to between c.33,000 BP for the lowest layers and c.10,000 BP for the upper layers, currently making it the oldest *habitation* site known in Papua New Guinea and with the Buang Merabak site to the north (Allen this volume) by far the oldest two sites in the Pacific. Matenkupkum has excellent preservation, containing a series of midden layers composed of shell, bone and stone as well as a number of hearths. The layers provide a detailed record of activities in the cave, allowing us insights into patterns of discard, subsistence strategies, environmental and faunal changes and outside contacts. The cave was originally visited in 1984 by Allen, Specht, Ambrose and Yen (Allen et al. nd (1984)) during the preliminary visit to New Ireland to set up the Lapita Homeland Project. It was thought to have excellent archaeological potential and this proved to be the case.

ENVIRONMENTAL BACKGROUND

The site of Matenkupkum is located on a strip of quaternary carbonates and alluvium lining the east coast of New Ireland. New Ireland as a whole tilts downwards to the northeast, with uplift in the south. Matenkupkum sits on one of a number of raised coral terraces caused by this uplift. The cave is about 15 m above the sea at present, looking out over a narrow beach and extensive reefs. Immediately behind the site is several km^2 of *kunai* grassland, covering raised coral terraces. The Rossel range comprising both karst limestone country with volcanic tuffs and ash form a narrow spine in the centre of the island about 15 km distance from the site. These hills are covered in rainforest. Not far distant in the south part of the island are the volcanic Hans Meyer and Vernon Ranges. These form the highest hills in New Ireland and offer a range of environments and possibly stone resources not available near Matenkupkum. Matenkupkum lies in an area of rendzina soil which is thought to be more productive than other soil types found on New Ireland, in terms of natural rainforest and tree crops and gardens. Rainfall is plentiful in southern New Ireland due to the relatively high mountain ranges.

In summary, Matenkupkum is located close to a range of marine and inland sources of food and raw materials, as these are distributed today. Changes in the environment and location of resources over the past 35,000 years are at present unknown. The main possible alterations are due to the effects of fluctuating temperatures and sea level changes. These can only be estimated, based on information from the mainland of Papua New Guinea. In the Highlands the climate at 40,000 BP seems to have been colder and drier than today. Temperatures rose around 26,000 BP, falling again during the glacial maximum from 20,000 BP to 15,000 BP. After 15,000 BP the temperature rose, reaching present levels at about 10,000 BP (Bowler et al. 1976:362, 388-90; Hope et al. 1983:39-43).

Such temperature fluctuations would have had effects on the vegetation of New Ireland, although these effects may have been moderated by maritime influences. However, the highest peaks of the southern mountain ranges may have been covered by grassland, rather than rainforest, during the colder, drier periods. It is unlikely that the vegetation in the immediate vicinity of Matenkupkum would have been affected greatly.

Fluctuating sea levels must have had some effects on the formation of the reef and its shell beds. However, the nature and timing of these changes are difficult to judge in the absence of information of how steeply the continental shelf drops off adjacent to Matenkupkum, the rate at which the land is being uplifted and our lack of data on how long it takes coral reef and shells to recolonise an area after the sea level has stabilised. It is worth noting that the continental shelf does apparently drop quite sharply and that the site was never far from the sea.

Suffice then to say at present, that there is evidence of relatively high water levels (c.50 m below the present) around 30,000 BP when the site was first occupied. Sea level reached at least 130 m below present levels around 20,000 BP to 18,000 BP, whilst present day levels were reached around 6000 BP (Chappell and Thom 1977:277; Chappell and Shackleton 1986). These changes may have had major implications for deposition at a site, from which one of the main classes of data is reef shells. Such implications are pursued later in this report.

EXCAVATION

The site of Matenkupkum is a dry limestone cave 18 m long and 10 m wide with a calcite pillar in the southwest corner. The cave mouth faces southeast, looking out to sea. The floor of the cave before excavation was littered with shell and recent debris and the surface at the rear of the cave seemed to have been disturbed by pigs.

A trench 10 m long and 1 m wide was excavated from near the middle of the site to the dripline. The 1 m squares were labelled E to O, E being in the centre of the cave, O by the dripline (Fig. 2). It is estimated that the trench removed about 5.5% of the total deposit in the cave. The first aim of the excavation was to obtain material from datable layers. As this is the only site which has been excavated in the southern half of New Ireland such material provides a vital fixed point from which to start building up a broader chronology. The second aim of the excavation was to identify any possible spatial variation within the deposit which might indicate how people had used the site at different periods. This aim necessitated sampling different areas within the cave. A trench was chosen as it provided long, continuous sections through the deposit which might show up spatial variation in a way that isolated test pits would not.

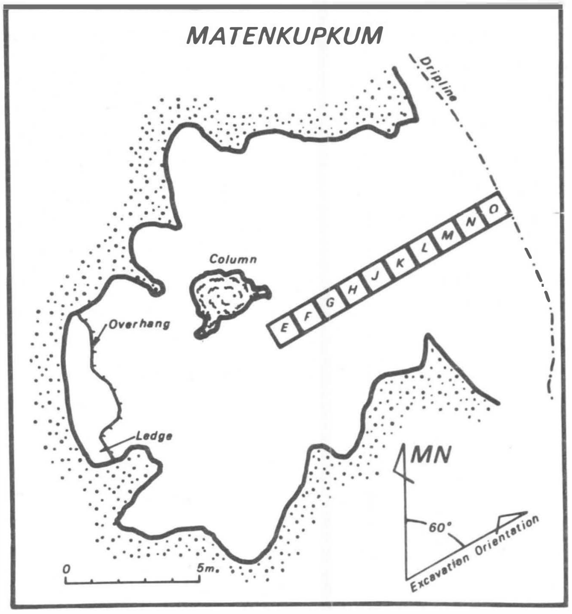

Figure 2 *Plan of Matenkupkum Cave with the excavation trench marked.*

The trench was aligned from the dripline to the centre of the cave because it was felt that a major constraint on how people used the cave would have been daylight. Due to the high, open entrance to the cave, light is normally excellent in the first 7 m back from the dripline (i.e. back to excavated Square H). In this area it would have been possible to carry out a range of tasks requiring good light. Beyond 7 m the quality of light declines and this would have limited the tasks which could have been carried out in the back of the cave. A measure of the quality of light was provided by the excavation. As far back as Square H, changes in the colour of the deposit were easy to see, further back fine changes were more difficult to detect, even using reflective foil. Excavation with any degree of care is impossible further back than Square E without an artificial light source. The importance of light as a determinant of how the cave was used was reflected in the nature of discard, as will become apparent in the analysis of material presented below.

The excavation took place between June 5th and July 10th 1985. We started by excavating Squares G and H to gain some idea of the stratigraphy in the centre of the trench. All finds were retained from these two squares: shell was bulk bagged for detailed analysis in the laboratory; bone, stone and other material was bagged separately. Sieves with a 3 mm mesh were used throughout to sift the entire excavated deposit. We then moved to Squares F and J on either side of the first two squares. From these, and from all subsequent squares, shell was weighed and counted for each species recognised and then discarded; all other finds were kept. Soil samples were taken from each of the archaeological layers distinguished. Square E and Squares K, L, M, N and O were excavated next. The excavation of Squares E to H took far longer than those towards the front of the cave due to the greater amounts of material in the rear squares and the fact that much of the original deposit in Squares M, N and O had been removed when the Japanese had dug a trench across the front of the cave in 1943.

All squares, except for E, were dug in spits approximately 10 cm in depth. This was initially because we had no idea of the stratigraphy. Spits were used later because some of the archaeological layers were difficult to distinguish clearly and also to provide information on variations in the amounts of material within archaeological layers. Square E was the last to be excavated. By then we had a reasonable idea of the stratigraphy on this part of the site and attempted to dig to the layers. However, the top part of the deposit was disturbed, making layering impossible to distinguish, so this was removed in spits. The lower part of the square was dug in layers.

Further excavations were carried out in 1988 by Allen and Gosden in Matenkupkum and the neighbouring site of Matenbek. The finds from these excavations are still being processed and only passing mention will be made of them here.

STRATIGRAPHY

Seven layers were distinguished during excavation. These were defined partly on the basis of the colour, compactedness and texture of the sediments and partly on the nature and amount of materials within them. As the stratigraphy was partially distinguished by the finds, further comments on the nature of the layers will be made in the section on the analysis of materials.

The stratigraphy is problematical because although the various layers were easy to recognise when we were in the middle of them, they tended to grade into each other making their boundaries indistinct. The lack of distinctly bounded layers means that it is difficult to relate some spits dug in transitional deposits to one layer or another. Table 1 provides a description of the layers and how the spits are related to them. Spit numbers followed by question marks show these spits which cannot be clearly assigned to a layer.

The layers run more or less continuously along the trench, as can be seen from Figure 3 (this section gives our view of the stratigraphy derived from the 1985 excavations, a slightly different version, revised after the 1988 fieldwork, will subsequently appear). The differences in the layers are probably due more to the nature of human activities in the cave than to natural geomorphological events. Thus the distinction between Layers 3 and 4 may be largely due to the deposition of ash, settlement debris, bone and shell. Layer 4 is greyer in character than Layer 3, almost certainly because of the greater amount of grey ash incorporated into it (probably deriving from the hearth pits in the centre of the site). Layer 7 also graded into the sterile natural sands at the base of the deposit, becoming less sandy as more material was laid down on top of the basal sands. We are probably dealing with changes in deposition due to gradual alterations in the way in which people used the cave, rather than sudden changes.

The exception to this pattern of gradual changes in deposition is the break between Layer 4 and Layer 5. Layer 5 is comprised of a hard packed brown soil, which in places looked like a trampled surface. The compactedness and colour of Layer 5 is easy to distinguish from the grey, loosely packed Layer 4.

Consequently we may be dealing with two sets of intergrading deposits, Layers 5 to 7 forming one unit, Layers 2 to 4 the other. A rider must be added to this statement that it may eventually prove possible to distinguish Layer 5 as a separate unit. This point is amplified below in the discussion of the chronology. At present, however, it makes sense to talk of the lower half of the site and the upper half of the site at times when it is difficult to distinguish differences within these units.

FEATURES

Hearths

Layer 3 in Squares H to L contained a number of hearths. These were made up of large limestone blocks (particularly in Squares K and L), which probably formed a flat base on which fires were placed, together with concentrations of ash and burnt limestone. As these oven pits were cut from Layer 3 into ashy Layer 4, it was often difficult to distinguish their lower boundaries. They were not therefore excavated as separate units. The hearths were all sealed by compacted Layer 2. Their position is shown on Figure 3.

Japanese Trench

Matenkupkum was occupied by the Japanese during World War II as one of a series of camps

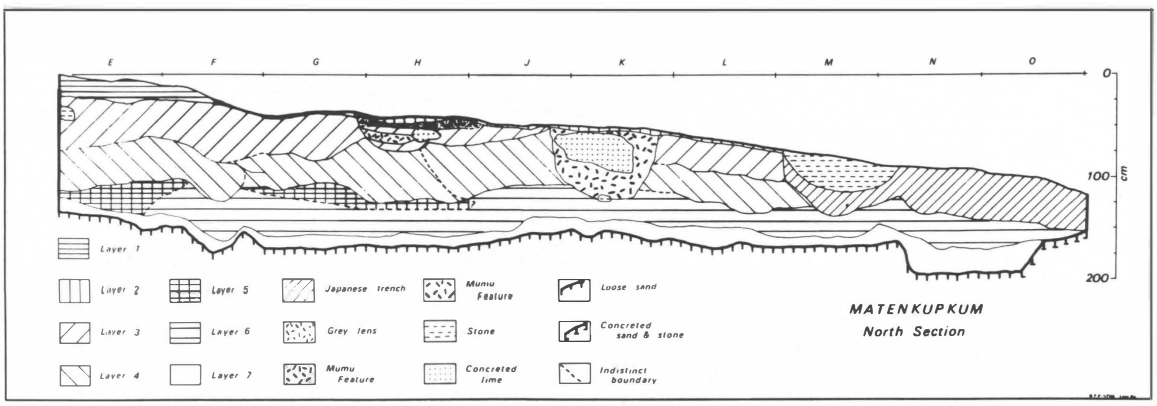

Figure 3 *Stratigraphic section of the northern face of the excavation trench in Matenkupkum.*

Table 1 *Stratigraphic layers and spits in Matenkupkum.*

Stratigraphic Layer and Description	Square E Spit	Square F Spit	Square G Spit	Square H Spit	Square J Spit	Square K Spit	Square L Spit	Square M Spit	Square N Spit	Square O Spit
1 Loose surface material contains recent finds: glass metal etc.	?	1	1	1	1					
2 Hard compacted layer	1	2	2	2	2	1	1	Japanese Trench	Japanese Trench	Japanese Trench
3 Loose brown silty soil becoming greyer and increasingly shelly with depth. Hearths in H,J,K,L	2,3	3,4,5,(?)	3,4	3,4,5	3,4,5	2,3,4	2,3,4	Japanese Trench	Japanese Trench	Japanese Trench
4 Grey-brown soil, much ash and shell. End of hearths	4,5,6,7	5(?),6, 7,8	5,6,7	6,7	6,7,8	5,6,7	5,6,7	Japanese Trench	Japanese Trench	Japanese Trench
5 Hard packed brown soil, few finds Difficult to distinguish boundary with Layer 6	8	9,10, 11,12	8,9	8,9,10	9,10	8,9,10	8 (roof fall)	4	Japanese Trench	?
6 Grey-brown silty soil, much shell	8(?)	?	10,11,12	11,12	11	?	?		4	4
7 Sandy brown soil, increasingly sandy with depth	9,10	13	13,14	13,14	12	11	9	5,6	4	4
[Natural-sands - consolidated at rear of cave, loose beach sand at front]										

Table 2 *Matenkupkum radiocarbon dates. All shell dates derive from the single species,* Turbo argyrostoma, *and are not corrected for marine reservoir effect.*

Layer	Square	Material	Date BP	Laboratory number
1	J	charcoal	Modern	ANU-5066
1	J	charcoal	Modern	ANU-5067
2	K	shell	10,890±90	ANU-5467
3	K	charcoal	Modern	ANU-5068
4	M	charcoal	Modern	ANU-5069
4	G	shell	11,940±130	ANU-5468
5	G	shell	12,940±160	ANU-5951
5	G	shell	14,250±240	ANU-5952
6	G	shell	21,280±280	ANU-5953
7	G	shell	31,350±550	ANU-5469
7	G	shell	32,500±800	ANU-5065
7	M	shell (degraded)	33,300±950	ANU-5070
7	M	shell (undegraded)	32,700±1530	ANU-5070

whose function was to prevent the sabotage of a telephone line running from their local centre of operations at Namatanai to the airbase at Nokon. They dug a trench across the front of the cave to protect themselves from attack and subsequently it was filled in with their rubbish. The trench was clearly delineated and excavated as a separate unit (see Fig. 3). It is assumed that the disturbed prehistoric deposit was thrown forward onto the talus in front of the cave.

CHRONOLOGY

The excavated part of the site dates entirely to the Pleistocene period; the uppermost prehistoric layer (Layer 2) dates to c.10,000 BP and the lowest levels go back as far as 33,000 BP. Thirteen dates have been obtained from the site so far: these are shown on Table 2 and in diagrammatic form in Figure 4. The dates obtained from shell samples all show complete consistency and made perfect sense stratigraphically.

On the basis of the shell dates, the site can be divided into two units comprising the pre- and post-glacial maximum periods, plus the recent surface material. It is possible that a third unit, represented by part of Layer 5, may be added to these two in future. Layers 6 and 7 form the first unit (refer to Tables 1 and 2), which predates the glacial maximum running from 33,300±950 BP (ANU-5070) to 21,280±280 BP (ANU-5953). Layer 7 is securely dated by three samples, one of which (ANU-5070) was divided into two fractions of a shell from Square M. The date of 33,300±950 BP came from a degraded (possibly burnt) portion, while the undegraded portion dated to 32,700±1,550 BP. The other two samples from Layer 7 came from Square G, a further 6 m towards the back of the cave and gave very similar results. The internal consistency of the Layer 7 dates and the fact that they came from 6 m apart allow great confidence in their accuracy. Two samples (ANU-5952, ANU-5953) were taken from Square G Spits 10 and 11. Spit 11 was fairly confidently assigned to Layer 6, while it was uncertain whether Spit 10 belonged to Layer 5 or 6. The sample from Spit 11 (ANU-5953) gave a date of 21,280±280 BP, predating the glacial maximum, while the sample from Spit 10 was dated to 14,250±240 BP (ANU-5952). On the basis of these dates it is possible to assign Spit 10 to Layer 5 and Spit 11 to Layer 6, at least in a preliminary fashion.

Layer 5 is securely dated by a sample from Spit 8 in Square G. This sample (ANU-5951) was dated to 12,940±160 BP. The top of Layer 5 was easily distinguished from Layer 4 above it,

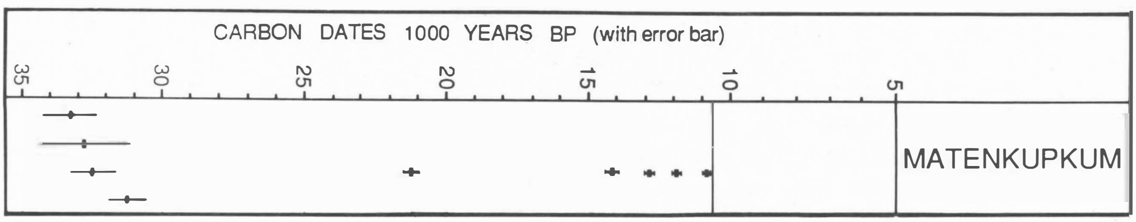

Figure 4 *Matenkupkum radiocarbon dates shown with one standard deviation.*

whilst the base of Layer 5 graded into Layer 6, making the two difficult to distinguish. If we accept that ANU-5952 does derive from Layer 5, then this layer dates to between 14,250±240 BP and 12,940±160 BP. The uncertainty with Layer 5 is whether it runs back towards the glacial maximum, forming some sort of continuous sequence with Layer 6. There is no discernible hiatus in the stratigraphy at any point within Layer 5 or between Layer 5 and 6. At the time of writing, Allen and Gosden have received a grant for extra dating samples from this portion of the site, obtained from the 1988 excavations. It is hoped that these samples will clear up this chronological uncertainty.

Layer 4 is dated by one shell sample (ANU-5468) to 11,940±130 BP. A charcoal sample from this Layer (ANU-5069) gave a modern date, as did the other three charcoal samples taken from the site. The Australian National University's Radiocarbon Dating Laboratory suggests that the results from the charcoal samples may be due to small sample size and/or the possible replacement in them of carbon by calcite. They are confident that all the shell determinations are accurate. This confidence is fully justified by the internal consistency of the nine shell dates obtained and the fact that these dates make perfect stratigraphic sense.

Layer 3 is at present only directly dated by one charcoal sample which gave a modern reading (ANU-5068) and which, as we have just mentioned, was highly suspect. On the other hand, Layer 2, the uppermost prehistoric level, is dated to 10,890±90 BP (ANU-5467). Layer 3 therefore dates to somewhere between 11,940±130 BP (the date for Layer 4) and 10,890±90 BP, making it a short lived event in terms of the site as a whole.

Two charcoal samples (ANU-5066, ANU-5067) from the surface (Layer 1) gave modern dates which may or may not be correct. In this case, however, the layer was also dated by the presence of glass, metal and other recent debris and its date certainly coincides with the charcoal determinations.

Time resolution

In summary, the site can be divided into two units (Figs 3 and 4). The lower unit comprises Layers 6 and 7 which span some 12,000 years and represent a relatively gradual build-up of material in the site. There is then a break in the dates during the glacial maximum, which may not necessarily indicate the complete abandonment of the cave by people, but is certainly indicative of slower rates of deposition and thus use. Layer 5 may represent a slow build-up of material possibly during, and certainly after, the glacial maximum. Layers 4, 3 and 2 represent the greatest concentration of material on the site and these deposits built up relatively rapidly over little more than 2000 years. This phase of deposition ceases just before the end of the Pleistocene on present evidence. However, it was noted during the 1988 excavations that remnants of deposit covered with calcium carbonate were adhering to the walls of the cave in a number of places. This may represent the remains of more recent material that has been removed from most of the cave either by natural or human action. A single pottery sherd found on the surface in 1988 indicates at least some late Holocene use of the cave.

The discussion section at the beginning of this paper picked out problems of time resolution as being a crucial limiting factor on the sorts of things that can be said about the site. From this point of view the two units defined within the site each provide different possibilities in interpretation. The lower unit (comprising Layers 7, 6 and probably 5) spans some 19,000 years, a vast period of time which must make us cautious about the sorts of inferences we can derive from these levels of the cave. The upper unit (Layers 4, 3 and 2) was laid down over a much briefer span of time, perhaps making interpretation easier. Certainly, most of the features, such as hearths, found in the cave were from this upper unit, allowing us to recognise relatively discrete results of human action in a way that is not possible with the lower layers.

Conjoin analysis on the stone material may give us insights into the numbers of flaking events which may have taken place in the cave and the subsequent movement of material within the deposits. Similarly, data on the matrix of the deposit by thin-sectioning and grain size analysis will tell us something of the sources of the sediments in the cave and the rates of build-up. Both these analyses are underway at present. It is only when we have more information on the rates of sediment accumulation and artefact deposition that we will be able to make more definite statements about our ability to distinguish patterns of action in temporal terms.

THE MATERIAL

Methods of analysis. During excavation the stone, bone and other miscellaneous finds from Squares E, F and J to O were bagged for each spit

Table 3 *Correction factors for spit and layer volumes. The upper table represents spit volumes per m³, calculated by multiplying the average spit depth by the length and breadth of each square. The lower table shows layer volumes, calculated by adding the volumes for each of the spits contained in the layers for each square (see Table 1). Artefact numbers and weights in these units, divided by these factors provide numbers and weights per m³.*

Table 3a

	Square									
Spit	E	F	G	H	J	K	L	M	N	O
1	.37	.08	.09	.02	.02	.10	.07			.19
2	.14	.13	.10	.10	.11	.12	.11			.12
3	.18	.11	.07	.10	.10	.13	.14			.13
4	.09	.10	.10	.10	.13	.12	.12	.01	.28	
5	.10	.11	.10	.10	.10	.09	.10	.03		
6	.08	.17	.11	.10	.10	.11	.11	.03		
7	.12	.11	.10	.11	.10	.11	.11			
8	.29	.11	.10	.10	.12	.10	.14			
9	.04	.11	.10	.05	.10	.10	.10			
10	.09	.09	.11	.11	.10	.08				
11			.06	.11	.11	.10	.05			
12			.06	.07	.10	.05				
13			.20	.11	.11					
14			.08	.07						

Table 3b

	Square						
Layer	E	F	G	H	J	K	L
1		.08	.09	.02	.02		
2	.37	.13	.10	.10	.11	.10	.07
3	.32	.32	.17	.30	.33	.37	.37
4a	.38	.39	.21	.10	.20	.20	.32
4b			.10	.11	.12	.11	
5	.29	.32	.20	.26	.20	.28	.14
6			.29	.21	.10		
7	.12	.20	.19	.18	.05	.05	.10

and returned to the laboratory for analysis. The shell from these squares, as stated, was weighed and counted by species and discarded in the field. The shell from Squares G and H was bulk bagged and returned to the laboratory for more detailed analysis.

In this report all the basic data gathered from the site so far is presented. However, the discussion of patterns of deposition and use of the site are made in terms of material corrected for density. Density corrections have only been made for Squares E to L. Squares M, N and O have not been analysed in this way because the top layers (1 to 5) in them were removed by the Japanese trench and Layers 6 and 7 were only tentatively distinguished at the front of the cave. This report presents the basic data from Square M, with parts of N and O. The lowest spits in N and O (probably belonging to Layers 6 and 7) are missing. The main discussion below will consequently focus on Squares E to L.

Corrections for density were arrived at in the following way. Firstly, the volume of each spit was calculated by taking the depth at each corner, adding these together and dividing by four to obtain an average. This was multiplied by the length and width of the square (100 cm by 100 cm) to obtain the average volume. The spit volumes represented in m³ are contained in Table 3 (corrected to two decimal places). In order to correct number or weight for volume, the number (or weight in grams) was divided by the volume to give number or weight per m³. To obtain the volume of the layers, the volumes of each spit in the layers were added together. The numbers and weights of material in each spit within a layer were added up and divided by the total volume of the layer (Table 3). The numbers and weights corrected for density within each layer form the basis of the analysis presented below. As we do not know the rates of sediment accumulation for various periods of the cave's occupation such corrections are provisional, allowing us to overcome some of the grosser distortions brought about by the varying depths of the layers encountered. As is obvious, there are no assumptions about the varying lengths of time over which artefacts may have been deposited inherent in these corrections of number and weight.

Shell

Shell formed the most numerous class of find in the site. The analysis of the shell can therefore give us considerable insight into the nature of use of the site, the changes in use through time and the patterns of discard, as well as the use of local resources.

During 1986 Robertson examined the shell from Square H in detail and in the process of

Table 4 *Shell categories used in analysis of Matenkupkum shells.*

Category	Species included in category
Nerites	*Nerita* sp., *Nerita polita, Neritodryas cornea*
Turbo	*Turbo sparverius, Turbo argyrostoma*
Tectarius	*Tectarius tectumpersica*
Chitons	*Acanthopleura gemmata*
Limpet	*Patellacea* sp.
Echinoid spines	*Heterocentrotus mammilatus*
Opercula	*Turbo sp.*
Other	*Trochus maculatus, Purpura persica, Oliva* sp.

washing and cleaning the shell recovered many small pieces of stone, bone, obsidian and charcoal fragments. The finds from Square H are consequently known in far greater detail than the same material from any other square. The bulk bagged shell from Square G has not been sorted as yet and has been weighed as a whole for each spit. Less is known about the shell from this square than any other.

The shell was analysed in two parts. For Squares E, F and J to O, the shell was weighed and counted on the site and then discarded. The shell was broken down into eight categories during this analysis (see Table 4), which are made up of groups of species, together with echinoid spines and the *Turbo* opercula (the opercula were considered separately as they are more robust than the rest of the shell and may be affected differently by taphonomic processes). Modern examples from each of the main species from the site were collected from the local area, as well as their names in the Sursurunga language. These samples were identified to Linnean categories by Sue Boyd of the Victorian Museum and to local names by people working on the site.

The shell from Square H was sorted in detail in the laboratory into 99 different species. However, the lack of published information on shells from New Ireland meant that the majority of the species could not be identified, much less assigned to a particular habitat. The analysis was consequently carried out in terms of the categories presented in Table 4. Individual whole shells were weighed in order to determine whether any changes in size occurred within each category through time. All the shell from Square H was sieved in 2 mm sieves and all the pieces retained in the sieve were kept for analysis. The more detailed recovery techniques possible in the laboratory, as opposed to the field, mean that the information from Square H may represent the patterns in that area of the site more closely than the data obtained from the other squares. Put another way, the numbers and weights of shell obtained during excavation may be under-represented when compared with Square H. This bias in recovery must be borne in mind when evaluating the results presented below.

Three main sets of data are of interest: the distribution of the shell through the excavation; the range of species found in each unit and the size of the shells. The results for the shell are presented in two parts: firstly, the patterns from the site as a whole (Squares E to L) are discussed, and secondly, the more detailed information derived from Square H is examined.

The distribution of shell through the site

The basic data from the whole site (Squares E to L) are presented in Table 5. Results corrected for density (Table 6) form the basis of the following discussion.

Changes in shell deposition through time

Two areas of change were focussed upon: variability in the deposition of shell through time and across the layers. Different factors are

Table 5 *Matenkupkum shell weights (g) by square and layer.*

Layer	Square E	Square F	Square G	Square H	Square J	Square K	Square L	Total
1		230	1707	360	640			2937
2	545	340	2670	1674	245	220	150	5844
3	2415	28270	8861	5034	2875	1815	775	50045
4	29715	18035	27829	6087	4080	1790	1475	89011
5	3415	3140	2833	12500	2400	2380	595	27263
6	100		13758	11741	590			26089
7		500	9788	8548	1270	280	1950	22437
Total	36190	50515	67446	45944	12100	6485	4945	223626

Table 6 *Matenkupkum density corrected shell weights (g per m³) by square and layer.*

Layer	Square E	Square F	Square G	Square H	Square J	Square K	Square L
1		2875	1896	18000	32000		
2	1473	2615	26700	16740	2227	2200	2143
3	7547	88344	52124	16780	8712	4905	2095
4	78197	46244	138475	59664	23383	12120	4609
5	11776	9816	14165	48077	12000	8500	4250
6			47441	55910	5900		
7	833	2500	51516	47489	25400	5600	19500

thought to have affected the structure of the shell assemblage in each case. Variation through time was likely to be affected by the original composition of the shell beds on the reef, the nature of collection strategies, the varying distance between the cave and the reef, and the manner in which shell was dumped in the cave. In investigating temporal change we looked first at the overall patterns of shell deposition through time, before going on to look at the internal structure of the shell assemblages.

Taking Squares E to L (Figs 5 to 11) as a whole there appear to have been two main phases of shell deposition: the earlier found in Layers 7 and 6, with a later phase most clearly manifest in Layer 4 and to a lesser extent in Layer 3. By contrast Layer 5 shows far less shell being deposited.

The basic reason for this pattern can at least be partly understood by looking at the dates for the site (Table 2). Layers 7 and 6 span a long period pre-dating the start of the glacial maximum (c.33,000 BP to c.21,000 BP) during which period the sea was at a lower level than today, but higher than the extremely low levels encountered during the glacial maximum between c.20,000 BP and c.15,000 BP (Chappell and Shackleton 1986). It was previously thought that this area of New Ireland may have been uplifted during the period of human occupation, but it now appears that the land may have been relatively stable over the past 30,000 years (J. Webb, pers. comm.), thus reducing the complexity of estimating relative land and sea levels. The approximate 12,000 year span covered by Layers 6 and 7 did not see a stable situation, but one of fluctuating sea levels. However, within this long period there must have been at least one point (if not more) during which the sea levels were stable enough for shell rich reefs to form at a point convenient to Matenkupkum. Shells collected on nearby reefs were then processed in the cave and deposited in Layers 6 and 7.

The upper layers, Layers 4 and 3, which have considerable densities of shell, post-date the glacial maximum. Layer 4 is dated to 11,940±130 BP and Layer 3 remains undated (apart from a modern charcoal determination), but lies somewhere between the date for Layer 4 and the date of 10,890±90 BP obtained for Layer 2. These two layers both have a greater real weight of shell and a greater density of shell than any other layers on the site. They are also the two layers laid down over the shortest period of time (with the possible exception of Layer 2). Layers 4 and 3 represent a relatively short-lived and intensive period of shell deposition by the standards of the site.

By contrast Layer 5 represents a period when less shell was being laid down in the cave. The problem here is that the dates for this layer (12,940±160 BP and 14,250±240 BP) are quite

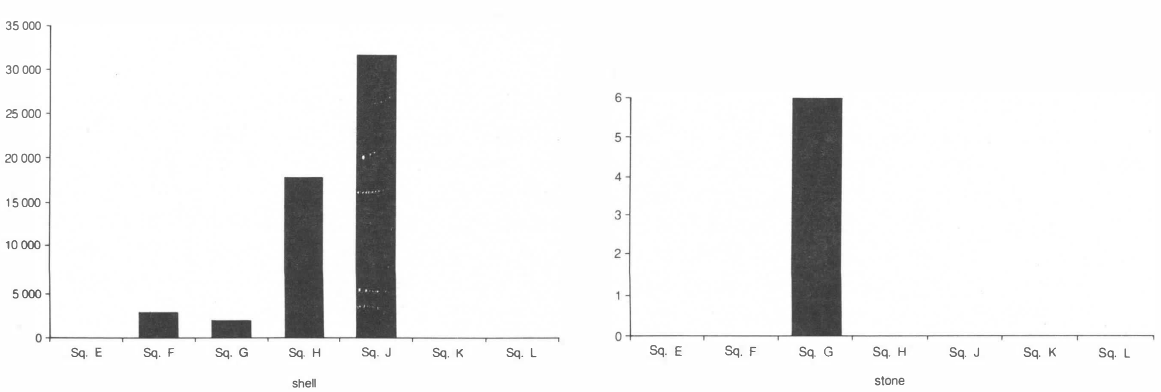

Figure 5 *Corrected weights (g) of shell and stone from Layer 1.*

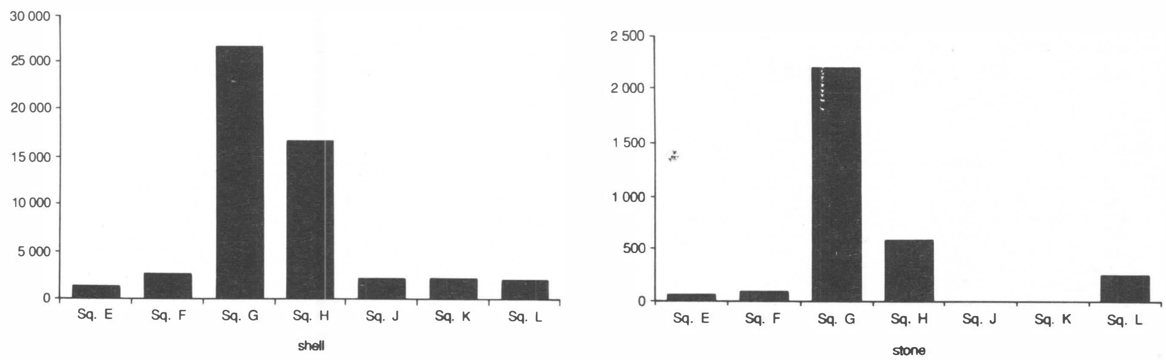

Figure 6 *Corrected weights (g) of shell and stone from Layer 2.*

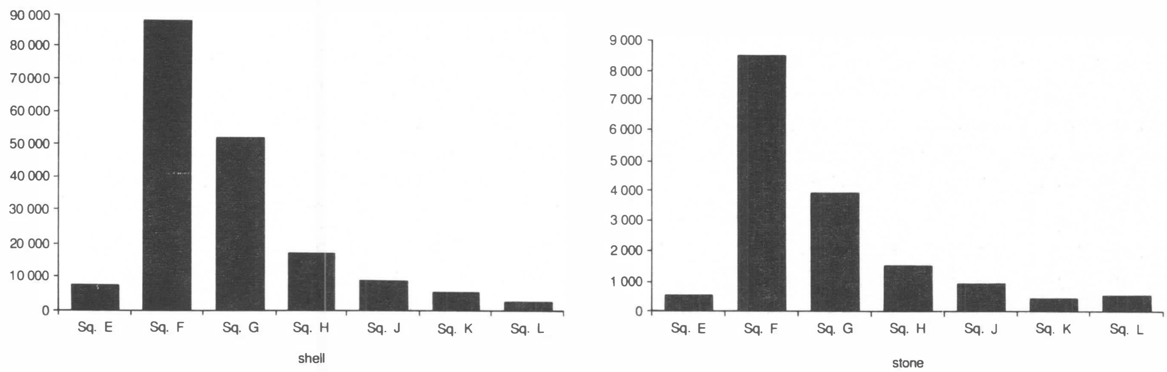

Figure 7 *Corrected weights (g) of shell and stone from Layer 3.*

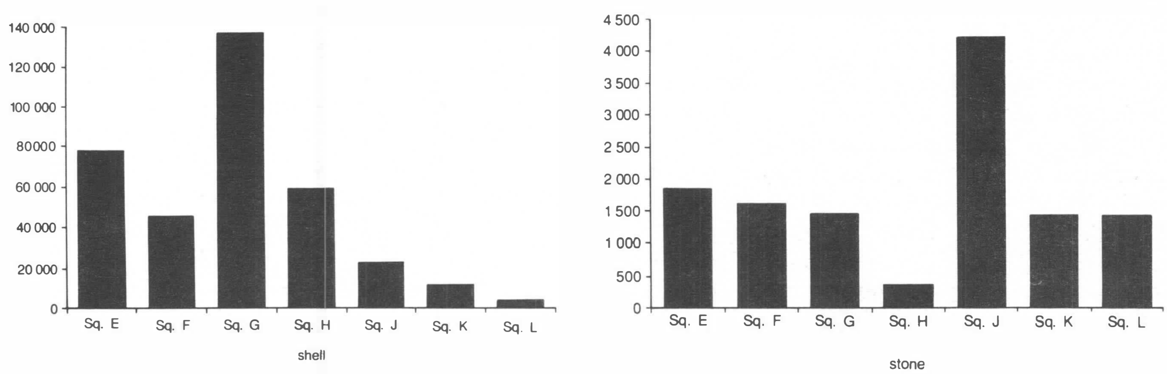

Figure 8 *Corrected weights (g) of shell and stone from Layer 4.*

close to those for Layer 4, but some 7000 years younger than Layer 6. It is difficult to know when Layer 5 started and how long its deposition continued. All that can be said at present is that there were no sterile layers recognised in the excavations in 1985 and 1988 and therefore no definite hiatus in site use and deposition has or can be established. At present Layer 5 may be seen as a period of less intensive deposition in the cave. The obvious explanation for this is that Layer 5 spans the period when sea levels were lowest during the glacial maximum, and that a relatively greater distance between shell beds and the cave meant perhaps that less shell was being laid down per unit of time (however this is measured). However, at no time did the deposition of shell cease in the cave and this may indicate that shell beds were never too far from the cave for shell to be brought back there. The notion of abandonment or reduced useage is at least partially supported by the Matenbek site; situated only 70 m away its earliest occu-

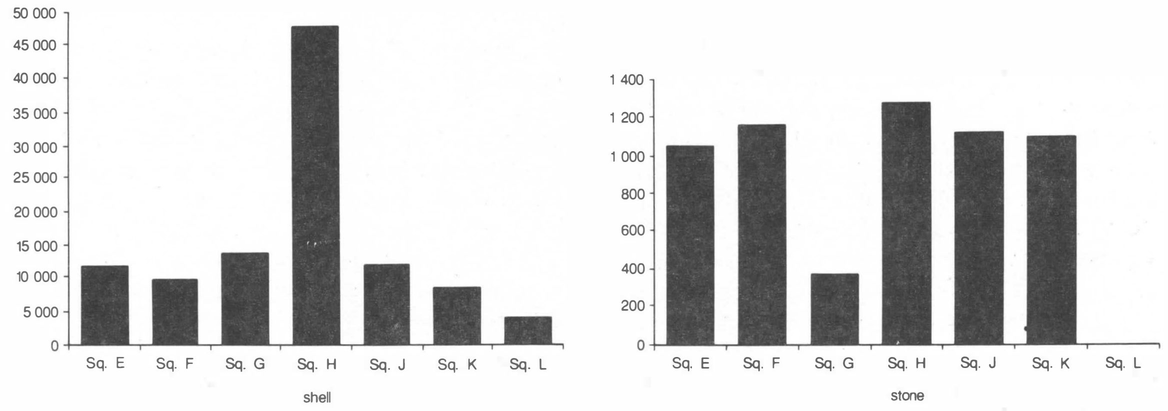

Figure 9 *Corrected weights (g) of shell and stone from Layer 5.*

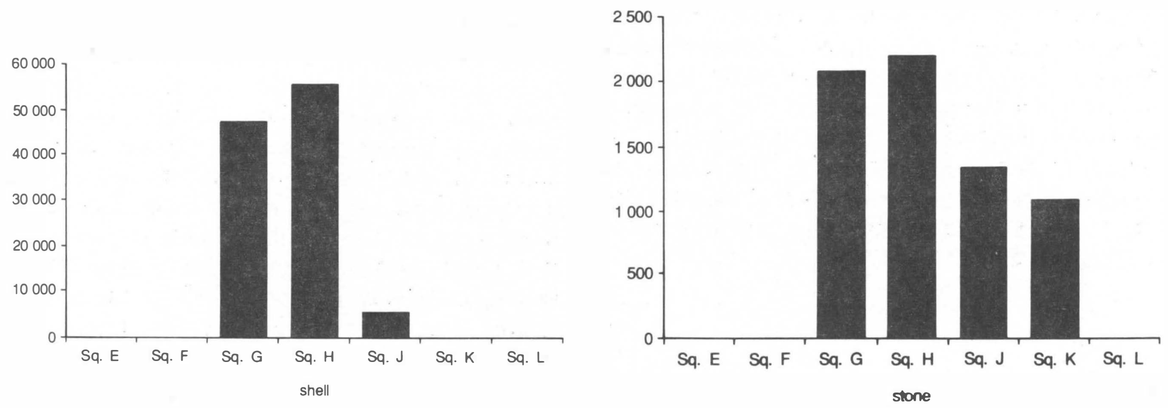

Figure 10 *Corrected weights (g) of shell and stone from Layer 6.*

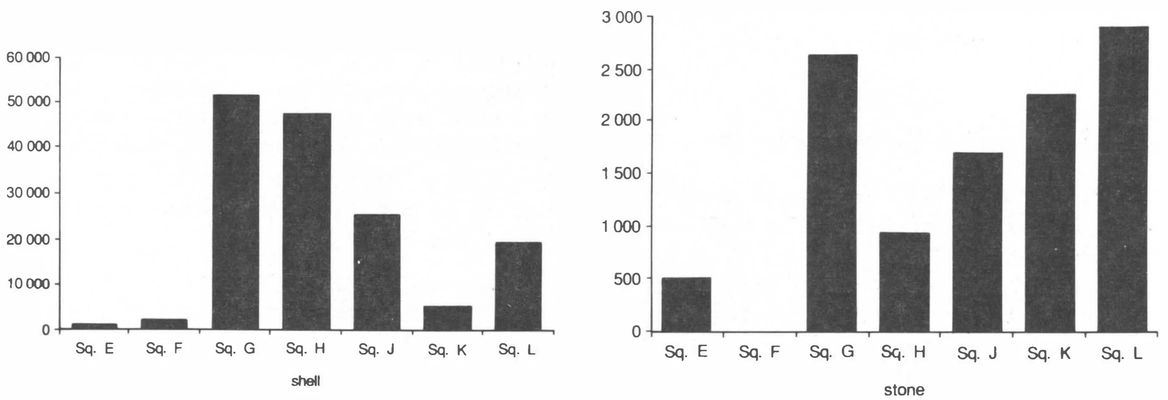

Figure 11 *Corrected weights (g) of shell and stone from Layer 7.*

pation layer has yielded three dates between c.18,000 BP and c.20,000 BP but then there is a clear stratigraphic and chronological break to deposits c.8000 BP (Allen et al. 1989:551).

Spatial patterns of shell deposition

The other direction of variability in shell deposition was spatial, along the trench. In investigating spatial variability the following working assumptions were made: preferential dumping of shell in certain areas and not others would show up as a non-normal distribution of weights along the layers. Skewing towards high values would occur in areas with much shell, low weights would be found in the areas kept clean. Sporadic dumping along the length of the trench, which was thus not directed towards particular areas, or cleaning up activities removing shell from the cave would result in random variation in shell weights around a mean, resulting in either a tightly clustered distribution of weights along the trench or a normal distribution of weights, depending upon the degree of variation around the mean. Therefore, two simple sets of statistical analyses were carried out, the first of which was to look at the nature of the distribution of weights within layers. Weight rather than number was used in order to overcome the problem of fragmentation. The assumption here, that different species would be unlikely to be dumped differentially due to their robustness might be questioned if there was a direct correlation between size and robustness. However, since many of the smaller shells are equally as robust as the larger ones, we accepted this assumption as being reasonable. These distributions are presented as box plots (Fig. 12). The second was to look at the spatial distributions of weight along the layers, showing where high and low values occur (Figs 5 to 11).

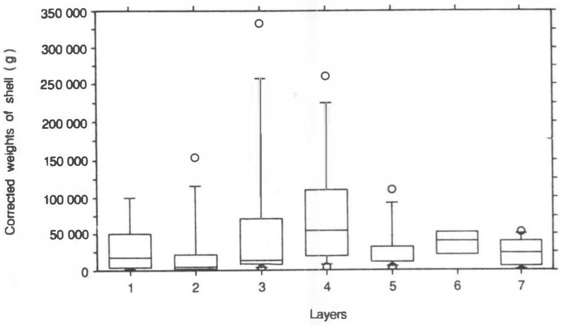

Figure 12 *Box and whisker plots of shell by layer.*

The box plots need to be considered first, as these give the overall distribution of the weights. Described briefly (see Shennan 1988:44-7 for a fuller description of box plots), the outside ends of the box enclose the middle fifty percent of the distribution, whilst the central line represents the median. The projecting lines and dots show outlying values. When a distribution is normal the median line sits in the middle of the box and the lines project from the box an equal distance in each direction (from Fig. 12 it can be seen that the weights of shell in Layer 7 are closest to being normally distributed). Inspection of Figure 12 shows that Layers 3 and 4 have the greatest range of values and depart from normality to the largest extent. This is especially noticeable in the case of Layer 3 where the distribution is skewed by a number of high values away from the median. By contrast, Layers 5 and 7 have respectively tightly clustered values and a distribution of weights close to the normal. From these distributions we would initially pick out Layers 3 and 4 as most likely to exhibit dumping, whereas the lower Layers 5 and 7 display less evidence of unequal spatial distributions. Layers 1, 2 and 6 do not have enough values within them to allow us to make any statements about the distribution of weights.

The above conclusions can be amplified when looking at the histograms showing the distributions of weight by square (Figs 5 to 11). Layer 1 is ignored in this discussion, as it represents loose surface material some at least of which is of recent origin. Layers 2 to 4 show a clear pattern. In these layers there are hearths found in Squares J, K, L and part of H. The squares where the hearths are, not surprisingly, have low densities of shell. In the squares immediately behind the hearths, Squares E, F, G, and H, there are relatively high densities of shell, resulting from the discard of shells behind the hearths out of the way of the main activity areas. It is worth noting that the pattern of stone discard is almost identical to that of shell for these layers (Layer 4 is an exception), a fact returned to below.

There are two main determinants of this pattern of discard. The first is the need to keep the hearth areas clear, as they would have been a major focus of activity. The second is the need to keep the best lit area at the front of the cave clean. It is useful here to distinguish between site maintenance, involving the small scale removal of rubbish from working areas such as hearths, and large scale cleaning operations designed to prevent the cave becoming full of debris. It is these large scale movements of material that destroy evidence for the structure of activities taking place on sites.

Following this distinction, it appears that we are witnessing evidence of site maintenance in the pattern of deposition of the shell in the upper layers, rather than wholesale cleaning. The overall determinant of the structure of the use of the cave seems certainly to be the light. Thus, the rubbish was regularly moved or directly discarded in the middle of the cave where it was quite dark, the hearths were positioned in front of this in an area of superior visibility, leaving the front of the cave (an area we can only speculate about because the Japanese trench removed the upper layers) relatively free for a range of tasks which most needed light.

The main area of deposition shifts slightly through time. In Layer 4, Squares E and G have the greatest densities of shell, whilst in the upper Layers 2 and 3 the greatest density shifts towards the hearths slightly, with the main concentrations being found in Squares F and G in Layer 3, and Squares G and H in Layer 2. Even allowing for these slight shifts in the concentration of shells, the most startling implication of this pattern is that human use of the cave remained much the same over a very long period of time. Layers 2 to 4 cover a short time span in terms of the site as a whole, but a very long time in human terms: a maximum of 2000 years, or some 80 generations. During this period people were renewing the hearth pits in the same spots and discarding their rubbish in a similar way. It might be argued that the stability of this pattern indicates a small number of short term occupations, during which intensive deposition occurred, rather than a repeated and regular pattern of use over 2000 years. However, as discussed below, the nature of the shell data indicate a repeated exploitation of the reef, such that it never fully regenerated.

In Layer 5 the hearths end. The densities of shell become much more even through the trench, indicating less patterning in the use of the cave. This may result from episodic usage of the cave over a long period of time. Low levels of use made it unnecessary to keep particular areas clean. Deposition became haphazard, spreading material through the cave in a fairly even density. An alternative, but less likely possibility is that there was such intensive use of the cave at this period that large scale cleaning up was needed, which has destroyed evidence of the structure of activities on the site.

Due to the discontinuous nature of Layer 6 it is impossible to perceive any trends in deposition. Layer 7, however, seems to echo the upper layers with greater densities of shell in Squares G and H and the front of the cave kept relatively clear. If this pattern has some validity, the main constraint on discard would be the light at the front of the cave as there is no evidence of hearths. The patterned nature of discard may indicate fairly short term occupations of the cave in Layer 7.

In order to understand the reasons for particular structures of evidence within the cave we must not only consider the evidence from within the site itself, but attempt to link internal evidence to the use of the region within which the site sits. For instance, from the model of various forms of cleaning activities presented earlier, we could say that an even spread of material, such as is found in Layer 5, might either indicate very intensive use of the cave, leading to large scale cleaning up, or to low levels of use over a long period, resulting in a lack of structure. In order to choose between these possibilities we have to think of the regional position of the cave. In the case of Layer 5, both the radiocarbon dates and the unknown greater distance between the cave and the sea during the glacial maximum would incline us to the idea of sporadic, rather than intensive use being the cause of lack of spatial patterning. The relationship between the material dumped within the cave and activities carried on outside the cave can be evaluated through looking at the internal structure of the shell assemblage.

Can we link changing shell densities to reef exploitation?

Having identified both temporal and spatial discontinuities in the deposition of shell, it is next necessary to ask what light the shells in the cave can throw on the exploitation of the reef, the use of Matenkupkum and broader patterns of human behaviour in the area?

In order to investigate these questions, one of us (Robertson 1986) used a model presented by Anderson (1981) for rocky shore environments near Black Rocks, Palliser Bay, New Zealand. Anderson predicted that the continued predation of shellfish populations resulted in a decreased mean size of shellfish collected. Anderson assumed that those shellfish with the greatest meat weight (which is directly related to shell weight) were most desirable. Following this argument, the largest individuals in a shell bed would have been selected regardless of which species they were. A range of species would be selected because large individuals of some smaller species would have more meat than small examples of some larger species. At first larger species would have been sought. However, if predation affected the recovery of the shell beds, the larger species would become scarce, so that smaller species would be more commonly selected. Over time predation should lead to a decline in the numbers of the larger species collected and an increase in the range of species.

Anderson's study was based on temperate conditions in New Zealand, which may be rather different to tropical reefs. However, Swadling (1976) has shown that continued predation caused mean shell size to decrease in shell beds in Papua New Guinea. Similar findings have also been made by Yesner (1980) in his survey of coastal exploitation on a world scale.

In order to test this model the shell from Square H was sorted and weighed in the laboratory. Fragment weight showed a continuing decline from the bottom of the square to the surface. The largest fragments were found in Spits 13 and 14 (Layer 7), Spit 12 (Layer 6) and Spit 8 (Layer 5). The weight of fragments in the upper spits and layers was considerably less than any of these (Fig. 13). As was discussed above, it is important to note that the role of Square H as a recipient of dumping may have changed over time; however, the changes in the amount of shell were consistently high through time and varied less than any of the surrounding squares.

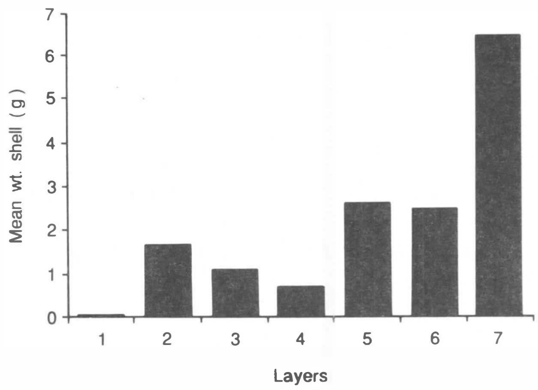

Figure 13 *Mean weight (g) of shell fragments in Square H.*

The pattern of fragment weight was then compared with the mean weight of whole shells to determine how far the patterns of fragmentation were responsible for the weight of fragments and how far this was affected by the original weight of the shells coming on to the site (Table 7). The mean weight of whole shells shows a very similar pattern to the mean weight of shell fragments (Fig. 14), with Layers 5 and 7 having the largest shells. This indicates that the original size of shells coming to the site was probably the main influence on fragment size.

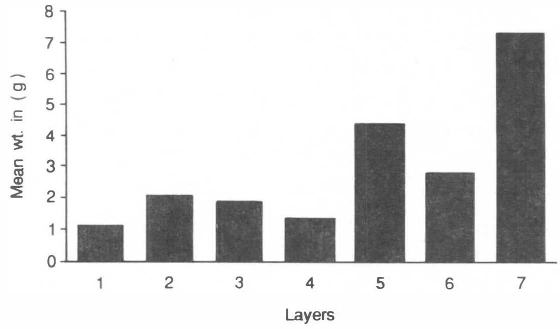

Figure 14 *Mean weight (g) of whole shells in Square H.*

Following Anderson's model, when levels of predation are relatively low, the shell beds will be able to regenerate and large species and large individuals of all species will be available for collection. Because at Matenkupkum Layers 5 and 7 contained the largest shells and the largest fragments, this argument might suggest that there were periods of relatively low levels of predation. If this were true we would also expect that the largest species would be the most common, whilst the range of species was smallest, thus reflecting the fact that people had the luxury of concentrating on a few large species.

Table 8 shows the five main groups of species present at Matenkupkum, with their mean sizes and percentage representations in layers and spits. From the mean weights, *Turbo* and *Tectarius* can be classed as large species, nerites as medium sized, while chitons and limpets are small. The percentage of each group of species in each spit is shown on Figure 15. The mean weight of whole shells declines from Layer 4 upwards for all species. The decline in weight corresponds with a decrease in the percentage by weight of large species, *Turbo* and *Tectarius*. In the lower half of the square the patterns are more

Table 7 *Matenkupkum Square H: whole shell numbers, weights and mean weights by spit and layer.*

Spit	Weight (g)	Number	Mean Weight	Layer	Weight (g)	Number	Mean Weight
1	362.8	302	1.2	1	362.8	302	1.2
2	599.6	289	2.1	2	599.6	289	2.1
3	656.6	304	2.2				
4	685.2	336	2.0				
5	631.2	361	1.7	3	658.1	333	2.0
6	612.8	533	1.1				
7	237.5	80	3.0	4	409.7	288	1.4
8	242.0	51	4.7				
9	1303.4	278	4.7				
10	767.0	197	3.9	5	766.1	176	4.4
11	1129.4	551	2.0				
12	1170.2	213	5.5	6	1148.6	392	2.9
13	1140.5	202	5.6				
14	1854.6	175	10.6	7	1413.5	192	7.4
Total	11392.8	3872	2.9		5358.3	1972	2.7

Table 8 *Matenkupkum Square H: mean weight of whole shells (g) and percentages of total weight of shell by category and layer. Percentage totals = 155.4% because nerites and turbos have been counted twice. Missing values indicate absence of whole shells.*

Layer	Spit	Nerites MnWt	Nerites %	Turbo MnWt	Turbo %	Tectarius MnWt	Tectarius %	Chitons MnWt	Chitons %	Limpets MnWt	Limpets %	Opercula MnWt	Opercula %	Unsorted %	Other %	Nerites + Turbo %
1	1	1.9	36.2		8.3			0.4	3.2	0.7	8.0	3.8	12.5	7.8	24.0	44.5
2	2	2.5	22.0	3.2	8.2	2.3	1.2	0.6	2.7	1.0	5.3	3.6	18.0	4.4	38.2	30.2
	3	2.8	32.7		7.1		1.0	0.7	1.6	1.0	9.7	4.2	17.0	1.7	29.2	39.8
	4	2.8	30.7	3.2	6.6	6.5	1.4	0.8	1.7	1.0	12.0	4.2	13.7	9.1	24.8	37.3
3	5	2.4	40.7	11.0	6.2		2.1	0.5	4.4	0.9	5.1	4.8	13.7	6.5	21.3	46.9
	6	2.2	40.1	1.5	7.8		4.3	0.6	7.3	0.7	2.1	3.8	8.3	11.1	19.0	47.9
4	7	6.3	31.0		21.5		4.1	0.9	3.5	1.2	0.2	5.4	13.5	8.1	18.1	52.5
	8	5.1	17.9		30.6	3.0	19.3	1.5	3.0	1.0	0.5	6.4	14.5	2.6	11.6	48.5
	9	6.0	43.9	32.2	11.6	16.8	22.6	1.7	4.8	2.0	0.5	5.9	7.7	4.4	4.5	55.5
5	10	5.9	55.4		3.4	1.9	20.6	1.6	3.0	1.7	0.2	6.0	6.0	4.9	6.5	58.8
	11	5.2	46.1		10.8	12.5	22.8	0.5	4.8	2.2	0.2	5.3	6.2	3.1	6.0	56.9
6	12	7.7	19.3	34.0	39.0		4.1	2.4	9.2		0.0	6.2	15.0	3.5	9.9	58.3
	13	6.8	7.1	24.7	66.5	18.4	2.6	2.6	3.1		0.0	6.5	11.5	3.2	6.0	73.6
7	14	6.6	20.2	44.2	34.0	3.0	2.0	2.2	4.8		0.0	5.8	23.5	5.9	9.6	54.2
Total			32.9		22.5		10.6		4.5		1.7		10.7	4.8	12.3	155.4

complicated. Spits 12, 13 and 14 show a predominance of *Turbo*, while Spits 9, 10 and 11 see an increase in smaller species, particularly nerites, with *Turbo* increasing again in Spit 8. Comparing these patterns with Anderson's model we might suggest that Spits 12, 13 and 14 indicate low levels of predation with large individuals from large species predominating, whereas Spits 2 to 7 show more intensive predation with a decline in the mean weight of whole shells and an increase in the percentage of small species. A further test of the predation model can be made by looking at the range of species in spits and layers.

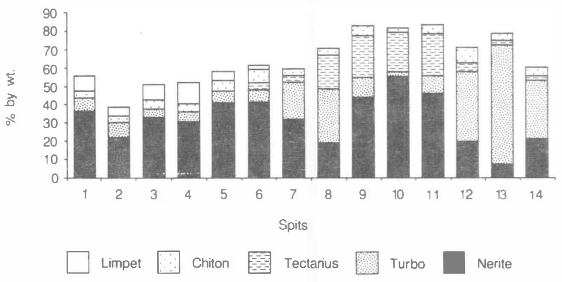

Figure 15 *Percentage weight of shell species in Square H.*

Table 9 shows that in general the range of species reaching the site increased through time, the most marked increase being from Spit 7 (Layer 4) onwards. On the face of it, such an increase fits in perfectly with the expectations generated from Anderson's model. In the uppermost unit of the site, where the mean size of shells was smallest and the representation of large species lowest, the widest range of species is found. These three strands of evidence together suggest, for the later period, a wide-ranging collection strategy necessitated by the fact that heavy predation had caused a decrease in large shells and large species.

Problems with extrapolating from shell data to predation strategies

The first unknown encountered in the interpretation of predation is that we are not dealing with the shell as it was collected, but rather with the shell that reached the cave. Not all the shell collected need have been taken back to the cave. A major factor here might have been the distance between the cave and the sea. Thirty thousand years ago when the sea level was relatively high, Matenkupkum may have been as near the sea as it is today (it is worth bearing in mind that there is unconsolidated beach sand in basal levels at the front of the cave). At this time it might have been easy to bring all the shells into the cave, to be eaten and discarded there. At later periods, the shells of larger species may not have been brought back to the cave in any quantity, because it was too far to carry them.

Further problems have to do with natural changes in the shells and the shell beds. As we are dealing with a time span of more than 20,000 years it is possible that the size of shells declined naturally, irrespective of human predation, perhaps in response to fluctuating sea levels. Also it is known that many land animals decreased in size during the late Pleistocene and early Holocene, a change which was often mistaken for human domestication (Dennell 1983). A similar problem derives from the overall structure of the assemblages. As Peter White (pers. comm.) has pointed out, underlying much of the above discussion is the assumption of a 'climax' population of shells on the reef resulting in the view that after each fluctuation in sea level the reefs will regenerate towards a fixed pattern in terms of species diversity, the range of sizes found in individual species, the relative numbers of each species and so on. Many biologists are moving away from the notion of 'climax' in relation to either flora or fauna and it may be that at least some of the variation observed within Matenkupkum is due to differences in the regeneration of shell beds over time.

Of more immediate concern than these two rather imponderable problems, is the length of time over which the site was occupied. Anderson attempted to model shell bed predation at Black Rocks over a 600 year period. This is less than 3% of the time period covered by Matenkupkum. Furthermore, Anderson's 600 year span did not encompass a period of such marked environmental change as that found before, during and after the glacial maximum. Given the length of occupation at Matenkupkum and the environmental changes, have we any hope at all of discerning predation strategies from the shell data at the site? The answer to this question,

Table 9 *Matenkupkum Square H: numbers of shell species present by spit.*

Spit	1	2	3	4	5	6	7	8	9	10	11	12	13	14
No. of species:	30	41	37	32	36	44	29	20	22	22	31	24	25	21

rather surprisingly, is 'yes'. Such a positive response must immediately be qualified by saying that predation strategies can be discerned most easily in the upper unit of the site, especially in Layers 4 and 3, but with increasing difficulty and lack of confidence in the lower unit.

Layers 4 to 2 cover a relatively short period of time, spanning a probable maximum of 2000 years and a minimum of 1500 years. These layers contain the densest concentration of shell on the site, which exhibits the characteristics of regular predation predicted by Anderson's model: a wide range of species including both small and larger species, together with relatively small individuals from the main species gathered. At this point in the site's history shell is the major element of deposition and the site can justifiably be termed a midden. As will be discussed in the following section, clear patterns of discard can be discerned in this phase, increasing confidence in the integrity of the site.

Layers 5, 6 and 7 cover between them an enormous length of time, approaching 20,000 years. In this part of the site there can be less certainty that changes in the size and range of shells and the density of deposition reflect human predation in any straightforward way. Having said that, it is interesting to note the numbers of large shells (mainly *Turbo*) in Layers 7 and 6, which may indicate low levels of predation and selection for species with greatest meat weight. Deposition resulting in Layer 5 may well have derived from episodic occupation and it is difficult to discern any clear pattern in the size and range of shells. Layer 5 may also span the glacial maximum, the period when changing sea levels would have had greatest effect on shell beds. During this period it would be very difficult to disentangle human influence from natural causes of variation in the nature of the shell data.

In summary, we can say that it is possible to draw inferences about the nature of predation from the shell in Layers 4 to 2 with a surprising degree of confidence, given the age of the deposit. There are also indications of low rates of predation in the early levels, when sporadic gathering may have allowed shell beds to regenerate. More data, either from this site or others of comparable age, are needed before we can be at all sure about the nature of predation. Layer 5 reflects a period of such considerable environmental change that it would be foolish to make any statements about the nature of shell use from the data available. However, when we consider the evidence for spatial variation, which shows greatest deliberate dumping in periods thought to have the highest rates of predation, the evidence for internal structure of the deposits and predation can be used to test and, in this case, support each other.

Stone

The figures for the stone presented here are only provisional; a much more detailed analysis of the stone has been concluded by a research student at La Trobe (Freslov 1989). The aims of the present analyses were to detect changes in the distribution of the stone through the site to be compared with that of the shell.

Changes in the spatial distribution of stone

Figures 5 to 11 show the weights of stone in each layer corrected for volume. For Layers 2 and 3 the corrected weights of stone in each square show very similar distributions to those of shell. It seems likely that discard of stone and shell was taking place in a similar manner; that is, the area around the hearths was kept clean by throwing things into the darker area in the middle of the cave. This idea is reinforced by the fact that larger pieces of stone are found in Squares E, F and G behind the hearths. The larger the stone, the more desirable it would be to remove it from an activity area. A rather different pattern occurs in Layer 4, where the spread of number and weight is more equal across the squares. The exception here is Square H where there are relatively few pieces of small size. In this layer slightly different patterns of discard and cleaning may be taking place, or it may be that the original patterning is more difficult to discern.

Layer 5 again shows a fairly even spread of material, as it did with shell. It is also possible that the average size of pieces is somewhat larger in this layer than others, perhaps showing that larger pieces were being deposited across the site, maintaining a higher general average, rather than the larger pieces clustering at the back.

Layer 6 contains too little stone to indicate a pattern. However, Layer 7 shows some interesting deviations from the other layers. The greatest weight of stone is at the front of the trench, whilst the largest numbers are in the middle. The possible reasons for this pattern are unknown. However, if people were leaving stone at the front of the cave, but dumping shell in the middle, it might indicate that there were different activity areas at this period, whereas in the later phases most tasks and discard were focussed around the hearths.

The stone from Matenkupkum provides a complicated data set. We are only just beginning to recognise and measure the nature of the variation within it; there is still some way to go before we understand the reasons for this variation. However, it can be said that in our present state of understanding there are no obvious changes in the reduction sequences employed through time, a remarkable continuity of technology over 20,000 years. The main changes are in the discard of stone within the site and this may be due to the changing regional position of the site, or the greater focus in site use provided by the hearths in the later period.

Obsidian

The obsidian is considered here separately from the rest of the stone as it is the only material known to have an exotic origin.

Obsidian was not found throughout the site, but only occurs in Layer 4 and above. During the 1988 excavations an area 50 cm x 25 cm was taken out of Square D. This was excavated in two sections; on the south side units 25 cm x 25 cm x 5 cm deep were bulk bagged for flotation tests in Melbourne in order to attempt to recover plant remains. The corresponding 25 cm x 25 cm column was wet sieved and finds retained. The excavation produced two interesting results: obsidian and cuscus bones continued almost to the bottom of the deposits in this part of the site, indicating that Layers 6 and 7 are here confined to the bottom few centimetres and confirm that early inhabitants used mainly the front of the cave. Secondly, 60 obsidian flakes were recovered from the small excavated sample compared with the 106 flakes found in the entire 10 m x 1 m trench dug in 1985. This adds to the notion of differential dumping of materials in various parts of the cave. As well, the excavations in Matenbek give an earlier date for the introduction of obsidian than that presently available from Matenkupkum (Allen et al. 1989). This difference may be resolved by the detailed analysis and dating of the 1988 season's finds.

Layer 4 in Matenkupkum dates to at least 12,000 BP. A total of 106 pieces were recovered (now complemented by an extra 60 from Square D) and this sample is too small to make any comment on the distribution of obsidian through the site. More obsidian is found in Layer 3 than Layer 4, although this may be because more stone material is found altogether in Layer 3, rather than the fact that supplies of obsidian increased.

All the obsidian from the 1985 excavations was sourced, first using specific density measurements (which fell within the range of Talasea and West Fergusson) and then by PIXE analysis at the Lucas Heights Atomic Energy Commission. The results from PIXE showed that all the obsidian came from New Britain, with the majority originating in Talasea and a few pieces coming from Mopir, near Cape Hoskins.

All the obsidian pieces were flakes. No cores were present. Some of the pieces showed evidence of bipolar working and some had been retouched. Some 36 pieces were examined for residues by Richard Fullagar. Four pieces showed evidence of residues suggesting use on plant materials.

Seeds and charcoal

Seeds, fragments of wood and charcoal samples collected during the 1985 season were sent to Douglas Yen for identification.

A number of seeds of *Celtis* species were identified. This is a native elm indigenous to Papua New Guinea and was probably part of the native lowland forest in southern New Ireland. From Square H, Layer 5, a seed, possibly a form of *Xylocarpus* was found and from the same square in Layer 2 came the carbonised midsection of a leaf belonging to the *Pteris* family. A number of other seeds from Layer 7 and fragments of wood from Layer 4 were recovered. These have yet to be identified.

It is noteworthy that all plants connected with arboriculture and horticulture were absent.

Bone

A detailed report on the bone will be made elsewhere (Marshall and Gosden, in prep.). Suffice it here to repeat the general statements made by Allen et al. (1989). The earliest layers contain a narrow range of species, including lizards, snakes and rats. After the glacial maximum new species are present in the cave, most notably *Phalanger orientalis* which is found in considerable numbers in the back of the trench (but occurs in glacial maximum levels in Matenbek). This species and others found later in the other New Ireland sites are interpreted by us as being human introductions.

DISCUSSION

Having presented the evidence, it is now possible to evaluate the models discussed above, not in the hope of coming to any definite decision as to which, if any, of the models is most appropriate, but rather so as to use the data gathered so far to define priorities for future research. The evidence will be considered from the two main periods of the site's occupation, before and after the glacial maximum.

The early occupation – 33,000 BP-21,000 BP

The internal site evidence shows hints of a structure to the distribution of material within the cave in this period, with shell concentrated in the middle and stone at the front of the cave. This lack of clear differentiation in deposition may indicate that activities were less focussed than in the later period of cave use. In order to further investigate the activities carried out in the cave, the detailed analyses of reduction sequences at present underway need to be completed. These should also be complemented with use-wear studies.

During this period of the cave's use, the build-up of deposits was slow; a maximum of 50-60 cm building up over 10,000 years. This might indicate some removal of debris from the cave through cleaning. However, the evidence of a degree of spatial structure in the material runs counter to this idea. It seems more reasonable to suppose that the cave was only used sporadically during this period.

Evidence for the use of the area outside the cave derives from the shell material. Here the large size of individual shells and the relatively narrow range of species may indicate low rates of predation on the nearby shell beds. Low levels of human activity in the region fit in well with the internal evidence from within the cave. The other set of indications of regional patterns of land use derive from the bone evidence. This is more equivocal. There is a small range of species found in these lowest levels, but it is difficult at present to know whether this was due to the impoverished fauna on the island as a whole, or whether people were only taking animals from the immediate vicinity of the cave.

There are no real indications of the type of world system within which the earliest inhabitants of Matenkupkum operated. The only possible indicator of outside contacts, amongst the evidence presently available, is stone sourcing, which might indicate the movement of stone through transport or trade.

Taking the evidence overall from this period, we feel the strandlooper hypothesis to be most likely (or at least a model based around low population density). The slow build-up of deposits indicates no great activity within the cave and the existence of some internal structure indicates that material may not have been moved out of the cave to any great extent. Also, the shell data may point towards low levels of predation on the shell beds. Slight support for the strandlooper idea comes from the fact that both of the other two sites known to date to this period in the Bismarck Archipelago are also found on the coast. These are Kilu (Wickler and Spriggs 1988) and Buang Merabak (Allen this volume).

The later occupation – 14,000 BP-10,000 BP

These upper levels provide much greater evidence for internal site structure, with a series of hearths in the middle of the site, dumping of large materials behind the hearths and the front relatively clear (where not removed by the Japanese trench). The evidence indicates some internal site maintenance intended to keep the hearth areas clean, but no mass removal of rubbish. This conclusion is supported by the fact that the deposit built up far more quickly in the top part of the site than at the bottom. The rate of build-up, but lack of large scale rubbish clearance, indicates that, although the site was used regularly over these 4000 years, site usage was never so intensive as to necessitate removing large volumes of rubbish from the cave.

Patterns of use of the area around Matenkupkum also demonstrate changes from the earlier period. The bones of a wider range of animal species are being deposited, with large numbers of *Phalanger orientalis* present. The explanation we have been developing for the first occurrence of certain species in the New Ireland cave sites has revolved around introduction by humans. However, the model being developed here of strandloopers using only the coast in the early period might be supplemented by a movement inland after the glacial maximum (the northern and somewhat more inland sites of Balof and Panakiwuk were first occupied at this time). If inland areas were only regularly being visited later in the history of occupation of New Ireland, certain species might only be deposited in sites later on, due to changes in land use and site function, rather than because the animals were not present on the islands when first colonised. Additionally, the shells being smaller in average size and encompassing a greater range of species, might indicate more intensive use of the shell beds. Matenbek also appears to be occupied within the glacial maximum for the first time. Thus in this later period we may see evidence of more extensive patterns of land use, with people using inland areas regularly for the first time, and a more intensive use of the local area's resources.

In the later period we gain the first evidence of the world system in which Matenkupkum was situated. Obsidian from Talasea and Mopir was moving in a straight line distance of some 350 km. The possibility of the movement of other types of stone in both periods still needs investigation. Similarly, the introduction of animals from outside the island is a strong possibility, which, however, needs consideration against alternative hypotheses.

In this later period the strandlooper idea no longer holds and there is definite evidence for the first time that both inland and coastal zones are being used. The internal structure of Matenkupkum changes, presumably in response to its new regional role. It is tempting to see in the exchange of obsidian more formalised links coming into existence between newly stabilised groups.

The Pleistocene archaeological record from New Ireland provides a rich and unusual data set and we are only just becoming aware of its potential. In this paper we hope to have shown the necessity for viewing sites in a regional context and that some of the ideas put forward here will prove amenable to test through future regional studies.

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THE BALOF SHELTERS, NEW IRELAND

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The excavations reported here were designed to contribute towards the overall aims of the Lapita Homeland Project, and particularly towards three broad objectives:

1. The nature of pre-Lapita human occupation in New Ireland, especially in relation to economies and exchange networks.
2. The impact of human settlement on the New Ireland environment. The severity of human impact on the flora and fauna of small Pacific islands is well known (e.g. Kirch 1983; Lewin 1986; Spriggs 1986), but is a subject of contention on the larger islands, especially Australia (White with O'Connell 1982:88-95). Could the situation on a smaller 'continental' island be determined?
3. The relationship between the development of Lapita pottery and its associated material culture, found almost entirely on smaller islands, and archaeological changes on associated larger landmasses.

Within the framework of these broader aims, more site-specific ones were also addressed:

1. The establishment of a reliable archaeological record of human history at the Balof shelters.
2. Determining the degree of variation between two closely adjacent sites that were apparently occupied over the same period.
3. Determining the extent of natural and anthropogenic changes in the faunal and vegetational records of these sites.

The scale and completeness of possible answers to these questions are being re-assessed as data are acquired and analysed. Some analyses of previous work are still incomplete, and further excavations at the site during 1990 have recently concluded. This report is therefore an interim one. It discusses primarily material excavated in 1985, focussing on two major analyses, the mammal fauna and some of the stone artefacts.

THE AREA

Balof 1 and Balof 2 (Papua New Guinea site register EAB) are overhangs that face each other across 20 m of collapsed doline at Madina, 90 km south of Kavieng (Figs 1 and 2). The sites are in secondary forest, at the inland edge of present gardens, c.80 m above sea level and 2.7 km south of the coast. This area is owned by Sanaile Talevat who lives nearby.

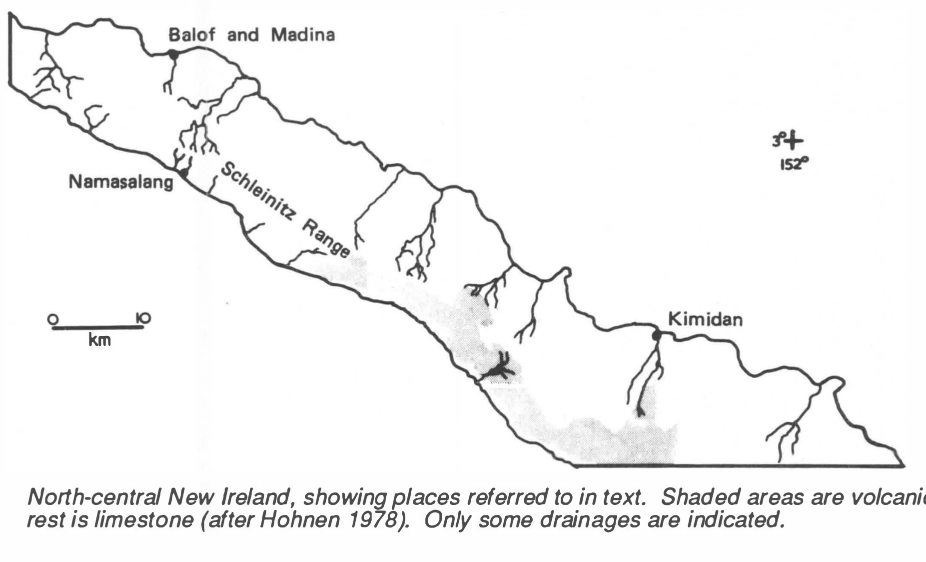

Figure 1 *North-central New Ireland, showing places referred to in text. Shaded areas are volcanic rocks, the rest is limestone (after Hohnen 1978). Only some drainages are indicated.*

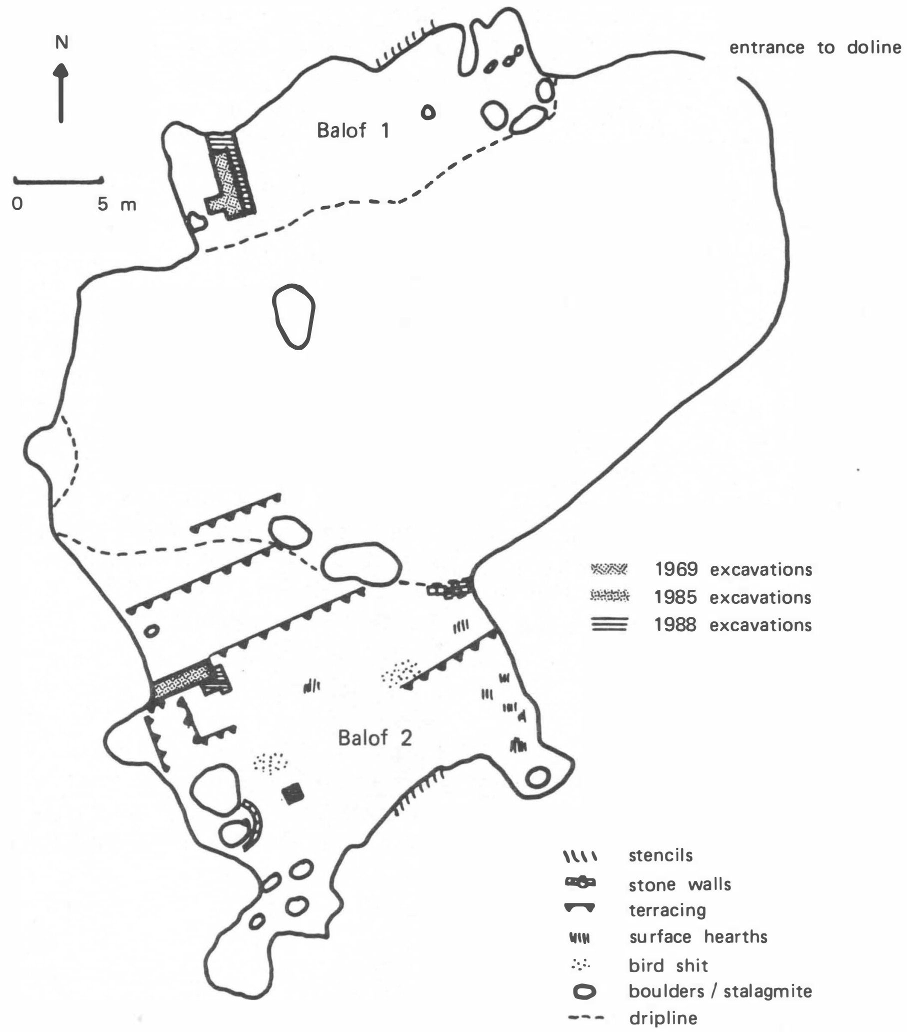

Figure 2 *Balof doline, with shelters, excavations and surface features.*

The local environment is based on limestone, a series of raised, folded and eroded reef terraces rising to c.750 m within 9 km of the coast (Hohnen 1978). At present people live entirely along the coastal plain, but oral history, colonial records and the presence inland of stone household perimeter walls, as well as stone terraces and garden borders, show there was fomerly more settlement inland. Offshore, the lagoon is only 150 m wide and the topography is steep outside the reef, so that available marine and littoral biomass is very low. Oral histories suggest the lagoon is shallower now than 80 years ago, but whether the floor is tectonically rising or the lagoon infilling is unknown.

SITES

Balof 1, c.9 m x 4 m, is on the northeastern side of the doline. It has a level floor, protected from direct rain but subject tc many drips – and some stalagmite formation – during extended wet periods. Balof 2 is much larger, c.17 m x 14 m, with a higher roof that is part of the original dissolution cavity. It is drier than Balof 1, but the floor slopes downward from the dripline towards the back of the site. Puddles occur along the back wall. The deposits in both sites are too damp to be dry sieved through a mesh of less than 5 mm. Both sites are used occasionally at present, as indicated by small hearths on the surfaces. Other surface features in Balof 2 include terracing to level the floor and a possible grave behind a stone wall in the southwestern corner. The terracing appears to have cut away no more than about 30 cm of floor, and that much only at the outer edge. This spoil was almost certainly thrown inwards, to make the shelter floor more level. The site was used as a refuge by many Madina people during the Pacific war and has been camped in by timber cutters since then.

In 1969, 6 m² were excavated from Balof 1 (Fig. 2). The 80 cm of deposit was built up over the last 7000 years or more and documented a two stage sequence (Downie and White 1978). The lower levels, older than c.2500 BP, included bone bi-points and flaked stone artefacts made in a variety of raw materials, including obsidian from Talasea. The upper levels contained small amounts of pottery and pig bones, as well as adding obsidian from Lou Island. The bulk of the remains in both levels, however, consisted of land mammals and reptiles along with fish and shellfish from the lagoon.

In 1985, 4 m² were opened in Balof 2, the material being excavated in 1 m² x 10 cm units and dry sieved through 5 mm mesh. This excavation was extended in 1988 by a further 1.25 m², and 1 m² was also excavated at the rear of the site. A further 3 m² were also excavated from Balof 1 adjacent to the 1969 trench. All of this material was carried to water and screened through a 2 mm mesh. Further excavations were undertaken at Balof 2 in 1990.

Stratigraphy

The stratigraphy within the 1985 excavation was clear, if gross (Fig. 3). An upper level (0-120 cm) of very fine grey roof debris and culturally derived deposit marked relatively intensive human use of the site, with constant, small scale reworking of the surface levels. From 120-160 cm the deposit was reddish-brown, with several intermittent levels of light, creamy coloured soil. Lesser amounts of cultural material suggest that more of this deposit was of natural origin, but artefacts and pieces of wood carbon were found throughout all levels. From 160-210 cm a fine red clay contained a range of small, unburnt, mostly unbroken bones of rats and bats (Table 1). The condition of the bones and the absence of any cultural material shows this clay is clearly non-human in origin and pre-dates human occupation of the site. Whether it pre-dates occupation of the island is another question, at present unanswerable since the bone is too leached to be dated even by the AMS technique. The radiocarbon dates obtained for the cultural deposits (Table 2) suggest that occupation has probably been intermittent, with minor breaks before and after c.3000 BP.

The other important aspect to note is that at some time within the last 3000 years the whole deposit slumped away from the western cave wall, opening a 10 cm wide crack well down into the basal clay. More recent material fell into this crack, but this was not detected during exca-

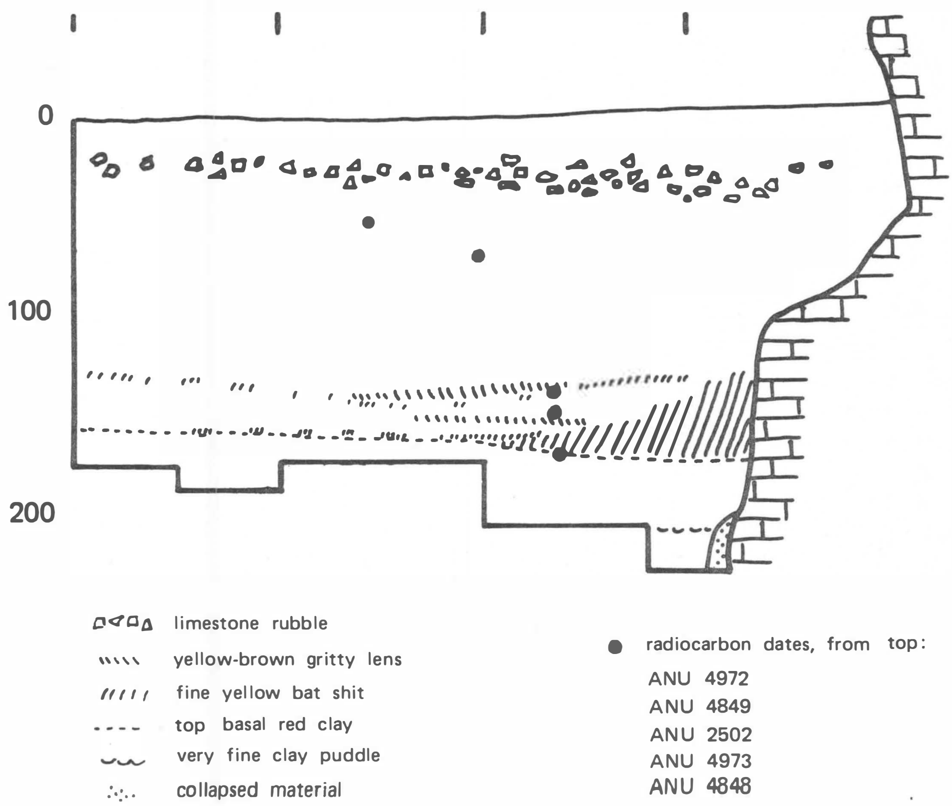

Figure 3 *Balof 2, 1985 excavation, southeastern section, with gross stratigraphy and approximate location of radiocarbon dates. Depth in cm.*

vation until the creamy coloured levels were reached. The material from the 1 m² containing this crack (K1) has not been used in this analysis.

Table 1 *Balof 2: fauna from the basal (pre-human) clay, with body weights.*

Animals	Wt. Range (g)
Bats	
Pteropus neohibernicus	1000-1500
Pteropus temmincki [endemic subspecies, not previously described from New Ireland]	200-400
Nyctimene albiventer	30-50
Dobsonia moluccensis	200-300
Dobsonia predatrix [presence questionable]	100-200
Emballonura ?furax	5-8
Hipposideros calcaratus	c.20
H. maggietaylori	c.30
H. diadema	c.50
Pipistrellus ?angulatus	c.5
Miniopterus schreibersi	c.10
Rats	
Melomys rufescens	70-100
Rattus mordax [endemic subspecies, not previously described from New Ireland, now extinct]	500-1000

Material discussed in the remainder of this report comes from 3 m² (5.5 m³) of the 1985 excavations in Balof 2 (Squares K2, K3, K4). These were excavated variously in 5 cm and 10 cm spits, which have been grouped into eight horizons in this analysis. The horizons bring together material from a similar depth below surface, in samples of a size that hopefully overrides possible small scale disturbances. Certainly information at this scale is gross, but finer resolution does not seem warranted until further studies on site formation processes and taphonomic changes are made. Horizons I-V (abbr. HI-HV) encompass the upper grey levels, HVI-VII span the red-brown levels, and HVIII is the red clay. Horizons and their volumes are also given in Table 2.

Table 2 *Horizons, volumes and C¹⁴ dates. All dates are on charcoal.*

Horizon	Vol. m³	Laboratory Number	C¹⁴ dates (years BP)	Horizon Location
I	1.0			
II	0.8	ANU-4972	3120±190	upper
III	0.65	ANU-4849	7680±510	
IV	0.7			
V	0.45	SUA-2502	10560±230	base
VI	0.4	ANU-4973	9970±390	upper
VII	0.45	ANU-4848	14240±400	top
VIII	0.6			

CULTURAL MATERIAL (BALOF 2:K2-K4): AN OVERVIEW

By far the largest cultural component consists of animal remains, mainly bones from land animals along with fish and marine shellfish. The animal bones, with a few insignificant exceptions, are clearly human food remains. Alternative explanations can be rejected. Owl pellets, recognisable by their unbroken bone, are not apparent, nor are the animals primarily represented (bats, reptiles, marsupials) likely to have been owl prey. There are no substantial native predators (other than a varanid) or scavengers on the island. The bone is broken up to some extent, but no dog gnawing is apparent and other experienced analysts agree that the small quantity of gnawed bone probably derives from human and not animal activity (B. Marshall (La Trobe University), B. Barker (University of Queensland), pers. comms 1989). Only three examples of rodent gnawing have been seen so far. Some bone (c.20%) is burnt and analyses to determine whether there is differential burning of species or body parts are being undertaken by A. Powell (University of Sydney).

Throughout the deposit there is about as much shellfish as there is animal bone. Basic sorting shows that a few small-bodied species dominate throughout.

Artefacts are much less common. The majority are of stone (n=324) and are made mostly on raw materials not available in the immediate vicinity. One flake from the side of a ground stone axe was found in HV. No other pieces of stone formally shaped by retouch have been found and the whole collection consists of cores, chunks, flakes, chips and cobbles. Microwear and residue analyses have not yet been undertaken.

There are also 10 tiny potsherds (HI:7; HII:3), none of which have clear decorative or formal morphological attributes. Shell and volcanic tempers are both present, as they are in pottery from coastal village sites (see for example White and Downie 1980). No bone points have been recognised, unlike in the first excavation at Balof 1. The edges of a number of large clam shells may have been used as knives, as they are today, but this requires confirmation. European period material (metal, glass) is found only in HI.

The following sections review some of the data in more detail.

Landbased Fauna

The animals which have been identified in the archaeological deposit, with an indication of their adult body weights, are listed in Table 3.

Table 3 *Balof 2: land animals identified from squares K2-K4, with body weights.*

Common Name	Species	Average Wt. (g)
Phalanger	*Phalanger orientalis*	c.2000
Wallaby	*Thylogale brunii*	5000-10000
Pig	*Sus scrofa*	30000-80000
Human	*Homo sapiens*	40000-70000
Large bats	*Pteropus neohibernicus*	1000-1500
	Dobsonia moluccensis	200-300
	? D. predatrix	100-200
Small bats	*Nyctimene albiventer*	30-50
	Rousettus amplexicaudatus	50-70
	Melonycteris melanops	c.40
	Hipposideros diadema	c.50
	H. maggietaylori	c.30
Large rats	*Rattus mordax*	500-1000
	R. praetor	200-400
Small rats	*Melomys rufescens*	70-100
	Rattus exulans	50-100
Lizard	*Varanus indicus*	2000-4000
Skink		c.1000
Python		1000-2000

Identification of the archaeological fauna has been done mostly by White and Flannery, using comparative collections made by the latter in New Ireland and other Australian Museum specimens.

In the identification of archaeofaunas a range of precision is possible, depending on expertise, the time one is prepared to spend and the nature and condition of the bone being studied. Mandibles and maxillae, especially those containing teeth, are relatively easy to identify to species level, and have been the basis of the identifications listed in Table 3. Precision declines when one tries to incorporate other bones. This relates partly to sheer size – larger bones are often easier to identify than smaller – but perhaps more to bone shape. It is, for instance, relatively easier to identify the distinctive mid-shaft process on a phalanger humerus than it is to be certain about a femur shaft; at another level, all bat bones are distinguishable by their thin walls, whereas sorting marsupial from reptile is not so clear cut. In New Ireland, the situation is made somewhat easier by the very limited range of large animals present. Working within gross and not totally consistent categories, it has been possible to identify nearly all of the jaw material and about 80% by weight of the post-cranial bones (Table 4). In using Table 4 to consider relative abundances, body size must, of course, be considered.

Table 4 *Balof 2: post-cranial bone from Squares K2, K3 and K4.*

Animal	Weight (g)	%
Thylogale	344.8	3.2
Phalanger	5030.2	47.4
Murid	138.2	1.3
Bat	1142.8	10.8
Reptile	1486.2	14.0
Pig	35.3	0.3
Human	54.2	0.5
Fish	69.1	0.7
? Bird	39.8	0.4
Unidentified	2279.6	21.5
Total	10620.2	100.1

We note that problems have been encountered in distinguishing the two *Dobsonia* species. Although readily identifiable as living animals, examination of a large sample of cranial remains has shown that most apparent differences between skeletons of the species are not reliable indicators. Since only *D. moluccensis* roosts in caves, it seems likely that this animal is more heavily represented than *D. predatrix*. If this is confirmed, some comment on bat hunting patterns will be possible. Many of the small bats roost in Balof or nearby dolines nowadays. It is striking that *Rousettus amplexicaudatus* is among the most common of these but is found only in the most recent levels of the site.

Occasional bones of very small skinks and frogs have also been recovered. These have not been identified further or counted, since they seem not to represent a major component of the diet.

Table 5 sets out the numbers of specimens of animals identified, by mandibles, maxillae and teeth. This table is strictly a measure of specimen numbers (NISP) so that variations in breakage or the extent of post-mortem tooth evulsion (e.g. more common in bats than phalangers) will influence the count. The influence of body size is expressed in Table 6, which sets out the weight of post-cranial bone by gross category. This table over-represents body size and weight, so that the bats, especially, appear to be less important, phalangers and reptiles much more so.

It must be stressed that neither of these tables give a 'real' account of the fauna of the site, or

Table 5 *Number of mandibles, maxillae and teeth by horizon.* (Dobsonia *includes* D. moluccensis *and* D. predatrix*).*

Common Name and Species	Horizon I	II	III	IV	V	VI	VII	VIII	Total	%
Bat										
Dobsonia sp.	53	168	90	182	208	321	44	7	1073	36.4
Nyctimene albiventer						4			4	0.1
Rousettus amplexicaudatus	2								2	
Melonycteris melanops						1			1	
Hipposideros diadema	4	11	2	7	12	12			48	1.6
H. cf. maggietaylori	2	3							5	0.2
Subtotal Bats									1133	38.3
Murid										
Melomys rufescens	8	2	3	2	4	69	24		112	3.8
Rattus mordax		7	9	4	18	50	1		89	3.0
R. praetor	14	4							18	0.6
R. exulans	6	1							7	0.2
Rattus sp.	9	8	2	5	5	33	18		80	2.7
Subtotal Murids									306	10.3
Phalanger										
Phalanger orientalis	57	299	314	316	286	22			1294	43.9
Wallaby										
Thylogale brunii	7	11	1						19	0.6
Reptile										
Varanus indicus	5	16	9	14	9	25	3		81	2.7
Skink		6	1	17	19	18	5		66	2.2
Snake	1	23	16	21	5	18	3		64	2.2
Subtotal Reptiles									211	7.1
GRAND TOTAL									2945	100.2
NISP animals represented by post-cranial bones only										
Pteropus neohibernicus	1	3	2	3	6	3	3			
Sus scrofa	27									

even the excavation, but between them give some guide to the relative proportions. A guide to the actual concentration of animal bone within the excavated area is given in Table 7, where the weight of bone per m³ is calculated. Phalangers (*P. orientalis*) are clearly the main animal represented in all tables, and some further investigation of the way they are represented has been made.

Phalanger remains occur only from the top of HVI to the present, that is they appear first in the site c.10,000 BP, bracketed by SUA-2502 and ANU-4973. They appear quite suddenly, in large numbers, and it seems likely that they were introduced to the island by humans.

Detailed identification of phalanger bones has been restricted to mandibles, maxillae and teeth, the six main long bones (femora, tibiae, fibulae, humeri, radii and ulnae) and innominates (c.Table 8, column 2), though other bones such as tympanic bullae, clavicles, scapulae and phalanges have been recorded. The basic recording system for dentitions follows Klein and Cruz-Uribe's (1984) DENTCODE, using the dental formulae: Maxilla: I1, I2, I3, C, P1, P2, P3, M2, M3, M4, M5; Mandible: I1, I2, I3, P1,

Table 6 *Weight (g) of postcranial bone by category and horizon.*

Category	Horizon I	II	III	IV	V	VI	VII	VIII	Total	%
Bat	75.8	204.8	102.2	211.2	173.9	291.6	69.2	14.1	1142.8	14.1
Murid	14.4	6.7	3.7	3.6	13.1	78.9	17.2	0.6	138.2	1.7
Phalanger	129.2	962.7	1213.1	1032.2	1620.5	72.5			5030.2	62.0
Thylogale	70.4	265.9	7.5						343.8	4.2
Reptile	60.7	266.0	171.4	138.1	367.5	417.7	31.8	0.6	1453.8	17.9
Total	350.5	1706.1	1497.9	1385.1	2175.0	860.7	118.2	15.3	8108.8	99.9

Table 7 *Relative concentration of animal bone, expressed as g/m³ of deposit, by category and horizon.*

Category	Horizon I	II	III	IV	V	VI	VII	VIII
Bat	75.8	256.0	157.2	301.7	386.4	729.0	153.8	23.5
Murid	14.4	8.4	5.7	5.1	29.1	197.3	38.2	1.0
Phalanger	129.2	1203.4	1866.3	1474.6	3601.1	181.3		
Thylogale	70.4	332.4	11.5					
Reptile	60.7	332.5	263.7	197.3	816.7	1044.3	70.7	1.0
Total	350.5	2132.7	2304.4	1978.7	4833.3	2151.9	262.7	25.5

P2, P3, M2, M3, M4, M5. M1 upper and lower are deciduous teeth replaced by P3. Any individual animal may have some, all or none of I2, I3, P2 and P3.

The main problems investigated so far in the analysis are whether whole animals were being butchered and eaten at the site, how many were recovered by the excavation and whether there has been any size change in the species over time, as it adapted to New Ireland.

It would be interesting also to look at the sex ratios of animals eaten, as an indication of possible conservation practices, but although this species is sexually dimorphic there is a considerable overlap in size and thus little chance of exploring the question. Aging the sample through tooth eruption and wear could also give valuable data on, for example, population structure, but there have been few studies on this even within the family Phalangeridae (cf. e.g. Kingsmill 1962; Thomson and Owen 1964) so that reference samples are a major problem. We return to simpler problems.

1. With an animal of phalanger size, butchering at hunting locations seems unlikely, as indeed does any butchering apart from skinning prior to cooking and eating. If this is so, then assuming animals were cooked and eaten at Balof 2 (as the burnt bone suggests), the numbers of bones for which recovery and identification probabilities are about the same should be roughly equal. Table 8 shows that this is so, within a variation which must be allowed for the small area excavated. This table shows a) generally smaller numbers of those limb bones which are smaller and more fragile, and b) larger numbers of highly recognisable bones – humeri, femora, ulnae and innominates. We note also that the percentage of each limb bone recognised per horizon remains relatively constant, implying constant breakage and recognition patterns.

2. Estimations of the number of animals recovered – usually MNI – are difficult in view of the many assumptions required about units of analysis, recovery and recognition probabilities, the chances of breakage and other pre- and post-depositional effects. Grayson (1979, 1984) has discussed these at length. Despite these cautions, the Balof 2 sample seems one for which MNI might usefully be calculated. Ed Roper (Department of Anthropology, University of Sydney) converted Klein and Cruz-Uribe's (1984) MNI programme to accommodate phalanger rather than ungulate dentitions and Table 9 sets out the results. Two things are immediately noticeable. Firstly, the number of mandibular specimens greatly exceeds the number of maxillary ones. Secondly, while the MNI increases as the size of the analytical unit decreases, as might be expected, there is only a small difference between results calculated on the basis of the site as a whole or separately on each of the smallest possible units. The difference between mandibles and maxillae is probably a function of

Table 8 Phalanger orientalis *from Balof 2. Number and percentages of particular bones, by horizon.*

Bones	Horizon I No.	%	II No.	%	III No.	%	IV No.	%	V No.	%	VI No.	%	Total No.	%
Radii	16	10	83	9	79	9	90	13	53	7	6	10	327	9
Ulnae	29	18	166	17	133	15	81	11	81	10	14	23	504	14
Humeri	39	24	247	26	195	22	144	20	144	18	10	17	779	22
Fibulae	19	12	89	9	67	7	76	11	98	12	10	17	359	10
Tibiae	10	6	80	8	99	11	62	9	114	14	3	5	368	10
Femora	24	15	152	16	146	16	120	17	153	19	8	13	603	17
Innominates	23	14	149	15	185	21	139	20	174	21	9	15	679	19
Total	160	99	966	100	904	101	712	101	817	101	60	100	3619	101

Table 9 Phalanger orientalis *from Balof 2. NISP and MNI for dentitions, for the excavation as a whole, by horizon and by excavation unit.*

Horizon	Maxillae NISP	Maxillae MNI	Mandibles NISP	Mandibles MNI	Total NISP	Total MNI	Percentage of total NISP	NISP m³	MNI as % of NISP
I	21	4	36	12	57	12	4.4	42	21.0
II	100	22	199	51	299	51	23.0	403	17.1
III	114	26	200	56	314	56	24.2	849	17.8
IV	117	36	199	66	316	66	24.4	518	20.8
V	129	46	157	65	286	65	22.1	386	22.7
VI	9	3	13	5	22	5	1.7	27	22.7
Totals by horizon	490	137	804	255	1294	267			20.6
Totals by excavation unit [n = 47]	490	173	804	302	1294	309			23.9
Totals for excavation as a whole	490	129	804	244	1294	244		251	18.9

robustness and thus breakage – only the rear part of the maxilla is robust. The low variation in MNI derived through the analysis of different units strengthens the idea that the site has undergone little post-depositional disturbance. To what extent the MNI of the samples (250-300) represents the 'real' number of animals present has not yet been tested. The procedure advocated by Allen and Guy (1984) could be usefully applied to this question.

Klein and Cruz-Uribe (1984) have also published a programme for determining MNI from post-cranial remains, but this cannot be so easily modified since its structure relies on the identification of a proximal or distal epiphysis on each specimen. (Such an emphasis is common in Old World ungulate analyses.) Some phalanger bones such as humeri have highly distinctive and robust shafts, and there are more recognisable shafts than epiphyseal ends in many excavation units. This can also be true of femora and tibiae.

Bereft of mechanical advantages, White manually calculated MNI for long bones within each excavation unit, taking the largest MNI in each case, no matter what bone it derived from (NISP=4346). The resultant total is 299, almost exactly the same as the number calculated on the same basis for mandibles and teeth. This would seem to confirm that the likely number of phalangers represented in the 5 m³ excavated is about 300. It also strengthens the probability that whole animals were being brought to the site.

3. To assess possible changes in size, P3-M5 lengths for mandibles and maxillae have been measured on the 168 specimens in which both these teeth survive. Table 10 shows that size does not vary over time, nor is there any measurable difference between *P. orientalis* in New

Table 10 *P3-M5 lengths for* P. orientalis *mandibles (MD) and maxillae (MX). Measurements are in mm. Modern New Ireland (NI) and Papua New Guinea (PNG) mainland figures are included for comparison.*

Horizon	MD/MX	Mean	Std. dev.	Std. error of mean	Range	No.
I	MD	24.2				1
II	MD	23.7	0.76	0.23	22.8-25.0	11
	MX	23.2	0.89	0.44	22.0-24.0	4
III	MD	24.2	0.90	0.22	22.4-26.1	17
	MX	22.5	1.30	0.51	19.8-23.9	7
IV	MD	24.0	0.66	0.13	22.7-25.0	27
	MX	23.2	0.70	0.16	21.9-24.4	19
V	MD	24.0	0.99	0.14	18.8-25.6	50
	MX	23.4	0.77	0.14	21.3-24.8	31
VI	MD	24.2				1
Modern NI	MX	22.2			22.0-22.4	2
Modern PNG	MX	23.3	1.07		21.2-24.8	9

Ireland and mainland New Guinea (W. Sepik) specimens measured in the Australian Museum.

Marine Fauna

Fish remains are found throughout the site. Most are small vertebrae, which are difficult to identify beyond 'teleost' (i.e. bony fish). Janet Davidson (National Museum, Wellington) has identified small numbers of bones from five families, all coastal dwellers around reefs: Acanthuridae (surgeon fish), Carangidae (trevally type), Balistidae (leatherjackets, trigger fish), Scaridae (parrot fish) and Pomacanthidae (angel fish). Although Carangidae are usually put into a pelagic category they, like the others, do come into lagoons and can be netted, speared or trapped. Netting and trapping have both been used at Madina in recent times.

Twenty elasmobranch vertebrae and 13 shark teeth occur in HI-HIV. The vertebrae may come from sharks, rays or skates, but it seems most likely that they come from sharks. The shark teeth have been identified to 3 species of Carcharinids by M. McGrouther (Australian Museum, Sydney), basing his identifications on Garrick (1982): *Carcharinus longimanus* (Poey 1861), *C. albimarginatus* (Ruppell 1837) and *C. falciformis* (Bibron in Muller and Henle 1841). All three grow to a maximum length of about 3 m and inhabit coastal waters in the tropics – i.e. they do enter lagoons, but are more likely to be found in the open sea nearby. It seems to us possible that these remains document a very long term history for some form of human-shark interaction. Whether this was in the form of a shark cult, similar to that documented in the 19th and early 20th centuries in northern New Ireland (Parkinson 1907:258-9; Powdermaker 1933:172-7), and whether it involved shark calling and catching using lassoes as Abel Tasman documented in the first European account of New Ireland in 1643, cannot be demonstrated from these finds, but the continuity is suggestive.

Shellfish occur throughout the archaeological deposit. So far, only five species comprising the bulk of the collection have been identified: *Nerita plicata, Nerita polita* Linne, *Cellana* sp., *Patella flexuosa flexuosa, Echininus cumingii.* All these species come from rocks and corals well within the reef and even well above high tide level; i.e. they are primarily collectible from very shallow or no water. There are very few large shellfish in any level; analysis is incomplete, but the range of species seems to be greater in HI-HII than it is below. We hope that P. Colman (Australian Museum) will study local shell fauna, including the range of environments in which various species are found and thus those which prehistoric people exploited.

Pollen and Sediments

In 1986 G. Hope and P. Fox (Department of Geography, Australian National University) examined ten soil samples and found more than 100 grains of pollen in six of them. In 1988 Hope collected a complete stratified sample from Balof 2, along with vegetation samples from the site area. In Balof 2, unusually for limestone shelters, pollen is quite well preserved, and Hope also reports that the range of pollen includes insect pollinated plants. It seems possible that this pollen comes from faeces deposited by bats or birds which predate the insects which feed on the flowers and pollen of local vegetation. The analysis of the pollen samples should be completed in 1990 and we hope to detect the impacts of introduced animals and human cultivation practices.

Stone Artefacts

Table 11 sets out the number and weight of the major rock groups, by horizon, as identified in hand specimen. As has already been intimated, while many of these pieces have been flaked, there are no formally retouched tools. Usewear and residue analyses have not yet been conducted. Most of our effort has been directed towards characterising the materials of manufacture with the idea that this may help us identify sources or, at least, associate specimens possibly from the same source area.

Firstly, we can be reasonably certain that the flaked limestone and calcite derives from the site walls, or nearby, especially since two wall blocks in the site have flakes removed from them. Secondly, the obsidian has been examined by W. Ambrose (Department of Prehistory, Australian National University) and his associates who, on the basis of its specific gravity, source most specimens to Talasea. Only two, both from HI, come from Lou Island.

The coarse grained volcanic rocks have not been sourced. According to Hohnen (1978), the nearest potential sources are the Jaulu volcanics, one outcrop of which lies along the western side of the Schleinitz Range, c.12 km south of Madina (Fig. 1). Cobbles from this outcrop occur in several streams around Namasalang on the west coast. On the east coast the closest streams which may contain similar cobbles are 45 km south of the site, although the main modern collecting location for these is around Kimidan,

Table 11 *Stone artefacts by raw material categories: number (n) and weight (g). A further 17 pieces of fine grain material from HVI are at University of Toronto and weights are not available.*

Raw Material		Horizon I	II	III	IV	V	VI	VII	VIII	Total	Mean Weight
Limestone	n	2	7	10	2	3	10	2	-	36	-
	g	22.43	397.04	155.27	2.88	22.72	104.37	538.08	-	1242.79	34.5
Dark, coarse	n	1	7	28	12	34	43	12	-	137	-
grain volcanic	g	34.18	98.38	541.61	106.35	103.84	101.86	13.79	-	1000.01	7.3
Other volcanic	n	2	3	3	5	2	8	4	-	27	-
and various	g	11.48	1.55	7.01	26.06	5.93	64.96	21.53	-	138.52	5.1
Fine grain	n	-	-	1	-	19	40	30	-	90	-
	g	-	-	0.43	-	18.95	77.78	72.67	-	169.83	1.9
Obsidian	n	6	10	1	-	-	-	-	-	17	0.3
	g	1.0	4.4	0.1	-	-	-	-	-	5.5	-
Total	n	11	27	43	19	58	101	48	-	307	-
	g	69.09	501.37	704.42	135.29	151.44	348.97	646.07	-	2556.65	-

20 km beyond this. Cobbles are now collected for *mumu* (pit-cooking) stones, but the prehistoric examples are flaked rather than fire cracked, while three cooking pits excavated in Balof 1 contained only limestone chunks and cobbles, none of them water rounded.

The fine grained rocks are even more of a problem. In the first report on Balof 1 (Downie and White 1978) 'coloured and black cherts and chalcedonies' were identified by Hohnen and associates as unlikely to be found in New Ireland, while grey and white cherts and chalcedonies, along with agates and jaspers, were said to occur on the island but were not referable to any particular source. In this situation elemental analyses offered by Instrumental Neutron Activation Analysis, carried out at the SLOWPOKE Reactor Facility, University of Toronto, seemed useful. Were the geology of New Ireland well researched, these analyses could be used to indicate probable sources of raw material. Since it is not, they can at least be used to group together rocks with similar mineral constituents which are potentially of similar origin and perhaps from the same source.

At the time of writing, analyses have been carried out on 102 samples, comprising nearly all the fine grained material. Hancock and Pavlish initially determined the occurrence of 12 elements (U, Dy, Ba, Ti, Mg, Si, Na, V, Al, Mn, Cl, Ca) in 17 specimens from K3/12 (HVI), and subsequently the occurrence of these and a further four elements (Sr, Br, I, K) in 85 specimens from HV-HVII. O'Brien standardised the data and carried out a principal components analysis on the 12 elements determined for all specimens. This analysis separated 13 pieces of stone which are highly distinctive in their quantities of Na, V, Al and Mn (Table 12). Hancock and Pavlish had already characterised these as calcium-aluminium silicate, which has a volcanic origin. All these stones come from HVII (eight) or the lowest spit of HVI (five) and we believe they were obtained from a single, currently unknown, source.

Table 12 *Elemental analysis of stone artefacts – PCA variable loadings.*

Variables	1	2	3	4
Eigenvalues	5.86	1.27	1.13	1.02
Percentage	48.84	10.54	9.41	8.48
Cumulative percentage	48.84	59.38	68.80	77.28
PCA variable (element) loadings				
U	0.22	0.75	0.06	0.10
Dy	0.90	0.08	-0.02	-0.07
Ba	0.53	-0.47	-0.14	0.56
Ti	0.77	-0.05	-0.10	-0.04
Mg	0.27	-0.29	0.63	-0.59
Si	-0.59	-0.35	0.20	0.25
Na	0.89	-0.11	0.05	0.20
V	0.96	-0.02	0.05	-0.06
Al	0.97	-0.11	0.11	0.06
Mn	0.96	0.04	0.09	-0.03
Cl	0.39	0.17	-0.58	-0.28
Ca	0.10	0.46	0.54	0.39

The other materials are primarily cherts and quartzites of types which form in marine environments. No such material has been seen to date either in the limestone itself or in weathering locations near the site or in local stream beds.

It is notable that all fine grained specimens are found in levels of the site that were deposited when the sea level was lower than it is today, so that sources that were drowned by rising sea levels are a possibility. It may also be the case, however, that such raw materials occur only in certain restricted parts of the limestones and volcanics of northern New Ireland, and these have not yet been located. Since available stone is likely to occur as small cobbles or pebbles, it would not be surprising if it was geologically 'invisible'. Finally, of course, some of these pieces may in fact have been imported from considerable distances, but this suggestion must, for now, be taken as not proven.

Nonetheless, there is another indication that a relatively distant source is involved. Table 11 shows that, where the source can be indicated, the mean weights of the different types of stone are inversely proportional to their distances from the site. Coarse grained volcanics (7.3 g) which come from some distance are an average of one-fifth the weight of limestone (34.5 g) which can plausibly be derived from the site walls. These volcanics are, by contrast, 25 times as heavy as the average obsidian flake (0.3 g). Using these indications, we can predict that the fine grained rocks (mean 1.9 g) are not derived from sources near the site, although they are likely to have come from somewhere on New Ireland. A complicating factor may, of course, be the size of available raw material, but this remains to be demonstrated.

Finally, Table 11 also indicates that there is a clear stratigraphic separation between fine grained rocks and obsidian, presumably indicating that the latter replaced the former. This occurred during the early Holocene, and is paralleled in other New Ireland sites. The reasons for this might be any combination of environmental (loss of old sources), functional (obsidian was a 'better' raw material), social (obsidian must have been part of an exchange network) or aesthetic ones. Research on the use of artefacts and the timing of the change to obsidian is proceeding.

DISCUSSION

The excavation discussed here is one part of a larger project and this report presents only some data and an indication of current research directions.

It is clear from the radiocarbon dates, the stratigraphic positions of European material, Lou obsidian and pottery, and the horizontal stratigraphy of the bottom part of the site, that there has been no gross disturbance to this area of the site. Below 120 cm (HVI-VII) it seems probable that human use was not regular or intensive enough to interfere with relatively ordered processes of accumulation. From HV up, however, the absence of any visible layering or clearly humanly made features indicates fairly continuous superficial disturbance. This would blur the division between any rapid cultural changes and might render less precise the dating of particular events. There is also the possibility of gaps (now also blurred) in the record between ANU-4849 and ANU-4972 (Table 2), where there is apparently relatively little accumulation, as well as above ANU-4972 from which period the material at the back of the cave seems to date.

With these considerations in mind, we turn to a broad overview of the evidence.

The most notable features of this record are, at once, the appearance and disappearance of certain animals and artefacts, along with continuity in other features. Overall, the site primarily gives us debris from hunted and collected animals drawn from both main local environmental zones. Land animals presumably were hunted in the site itself or adjacent caves (bats) or in the bush nearby. Shellfish, fish and sharks, however, had to be brought from the coast 2.7 km away – and further before 6000 BP. It is this aspect which suggests that Balof 2 was more than a transient stop for hunters throughout its 14,000 years of use. But the absolute quantity of material found suggests it was no more than an irregular or short term camp site. The most visible change in this record occurs around the junction of HVI-HV, about 10,000 BP, where the nature of the deposit changes, phalangers appear in quantity and endemic murids decline in numbers. Not long after, fine grained raw materials cease to be flaked or used. It seems unlikely that these changes all relate to something local and simple such as a change in the physical accessibility of the shelter to humans, and the readiest assumption would be that they are all related. But they may not be.

We are reasonably certain that phalanger was introduced to New Ireland and, given the lack of morphological differentiation from other populations of the species, this occurred relatively recently. Possible origins are currently being researched using electrophoretic studies of modern populations. There is absolutely no indication that phalanger was in the area for any considerable time before becoming human prey, so that before its arrival reptiles, large bats and murids were the main terrestrial meat protein sources.

Linking the changes in animals to changes in stone is more problematic. We might argue that, in fact, the two are only indirectly linked. The disappearance of fine grained rocks may signal a localisation of people's horizons, perhaps consequent on the development of more intensive plant curation techniques. The overall increase in animal remains could then be related to increasing use of the site by a somewhat more sedentary local population. Such changes might be indicated also in the pollen record.

Increasing sedentism, however, might be linked to prey changes. Within a particular area, Flannery has noted that phalangers seem to favour certain locations as resting places, so that hunters can be reasonably assured of a continuous 'take' over the medium term. We might recognise also that these animals seem to be much closer to human commensals than any of the animals previously constituting prey, so that their availability in a somewhat more regularly populated area may have been greater.

A later introduction is that of *Thylogale brunii*, definitely from about 3000 BP but perhaps earlier (the evidence of Balof 1 may also support this). These animals never seem to be as common a component of the fauna as phalanger, and this may suggest both their relative proportions in the region and something about the style of hunting in prehistoric New Ireland. Combining our data with the absence of dogs in this or any other site on the island (if this is real), we may think of hunting as more of a stealthy stalk than a dramatic chase.

Other changes occur among the murids. *Rattus exulans*, which is very much a human commensal, appears only at the base of HII, as does *R. praetor*, which is less well known in this role. With the arrival of *R. praetor*, the long-term island endemic, *R. mordax*, apparently becomes extinct. Our evidence suggests *R. mordax* is absolutely more common in Pleistocene levels of the site than it is later.

We note here that the apparent dates of animal introductions at Balof 2 are not always consistent with the evidence from other New Ireland sites (Marshall and Allen this volume; Gosden and Robertson this volume). Research to resolve these inconsistencies is in progress, focussing first on the dating of the earliest specimens. Samples of phalanger bone from Balof 2 and Matenkupkum have been submitted for AMS dating. If these dates continue to differ greatly, explanations concerning rates of animal colonisation of new habitats and human use of all available protein resources in an area will need to be considered.

Finally, of the domestic animals known today on New Ireland, pig alone occurs in the site, in HI only. This dates it to considerably less than 3000 BP (ANU-4972), consistent with its location in the 1969 excavations at Balof 1. We hope that the material currently being analysed from Balof 1 will allow for more precise dating of the arrival of pigs, perhaps by the AMS technique.

The other matter of note is that neither the Holocene sea level rise nor the presumed development of horticulture during the Holocene are clearly recorded in the data so far analysed. The latter change, in particular, has sometimes been seen as occurring suddenly in the area, concomitant with the arrival of Lapita pottery c.3500 BP. At Balof, two murids appear and one endemic one becomes extinct at this time, but the relationship between all these events needs further exploration. This will be carried out in the coming years.

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EXCAVATIONS AT PANAKIWUK CAVE, NEW IRELAND

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As part of the literature search connected with the 1984 reconnaissance trip of the Lapita Homeland Project (Allen et al. nd (1984)) it was noted that Peterson and Billings (1965) had excavated a two feet by two feet by three feet deep (60 cm x 60 cm x 90 cm) test pit in a cave called Panakiwa, inland from Mangai Village on the east coast of New Ireland, and c.50 km southeast of Kavieng. Although their report is brief, Peterson and Billings indicated that they had not reached sterile deposits, and reported no visible stratigraphy in the site. However their work suggested that deposits in the shelter, which contained animal bones, marine shells and stone artefacts, might be more than 1 m deep and thus worthy of further investigation. Their original designation of the site as Panakiwa is in error and should be Panakiwuk.

After two decades and the destruction of traditional forest paths by logging, the relocation of Panakiwuk (designated EAS in the Papua New Guinea site register) proved difficult. However, through the good graces of Apelis (Kas) Kasino MBE, a bigman in Mangai and the traditional owner of Panakiwuk, a new track was cut from the road and we were eventually able to visit the site. It was decided to excavate there and in July 1985 four members of the Project team returned to carry out the work which continued until mid-August.

THE SITE

Panakiwuk (*lit.* 'under the Kiwuk bird') is a small limestone cave or rockshelter situated in an eroded double doline c.150 m x 30 m in size. The steeply uplifting Lelet limestone which runs for much of the length of New Ireland reaches its northwestern limits immediately inland from Mangai. Here New Ireland is only 8-9 km wide, and thus while Panakiwuk is only c.3.5 km inland from Mangai, it is also close to the centre of the island (Fig. 1). Some oral traditions suggest that the site might have been used by travellers passing between Mangai and Lavalai on the opposite coast, but this is uncertain. The site seems unlikely to have been occupied extensively during World War II by either the Japanese or local people, since only one piece of bottle glass (probably from a Japanese bottle) was recovered. A few more recent artefacts were in the cave, but in general it seems to have been little frequented this century.

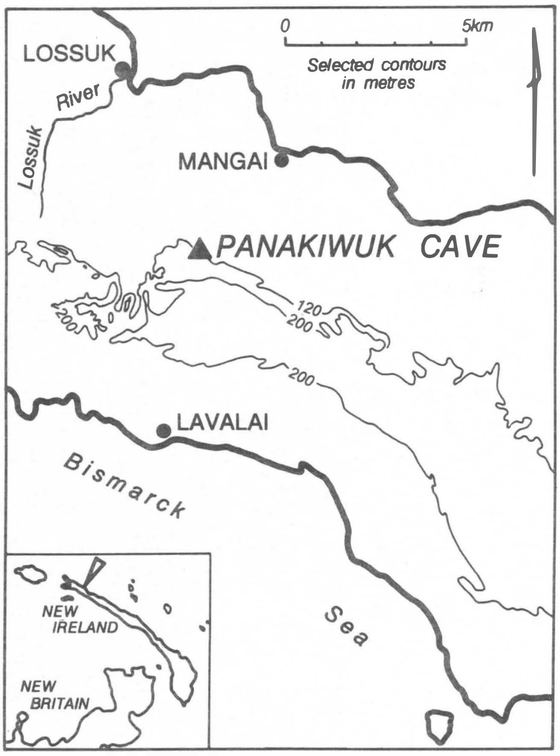

Figure 1 *Location map of Panakiwuk Cave.*

The site is currently in rainforest at an estimated altitude of c.150 m above present sea level. Allowing a maximum lowering of the sea at the height of the last glacial maximum of -130 m (Chappell and Shackleton 1986), soundings taken in Mangai Bay by Taffy Rowlands on the *Dick Smith Explorer* in 1985 indicate that the ocean floor drops steeply away at right-angles to the coast and that the -130 m mark is only 750 m from shore. It seems reasonable to accept that

Plate 1 *Panakiwuk Cave viewed from the east. Note that the floor of the cave is some metres above the doline floor.*

throughout the period from when Panakiwuk was initially occupied to the present, the shoreline has never been more than 600-700 m further away than it is today.

The site itself is unusual; rather than being at the base of the doline, the cave floor is c.7 m above it. The doline itself is at the end of a limestone spur which curves around in such a way that the ground slopes steeply away on all sides outside the doline (Fig. 2). This means that there is no major source of sediments which can have been transported into the site by water or other physical action, apart from airborne deposits or the weathering of the doline itself. The cave is not large, having a floor area of only 8 m x 14 m. The area of excavatable deposits is less than this, only 6 m x 8 m. The cave has a high (15-20 m) and wide entrance, which makes it more like a rockshelter than a cave (Plate 1). It is thus light and airy. Having a southerly outlook, the sun did not come into the cave at all during the July-August period of our excavation; as well, during the frequent rainstorms we experienced, the cave remained almost totally dry. The nearest water that we know about is 15 minutes walk, but closer sources may exist. As caves go, Panakiwuk appears an attractive location to our European eyes.

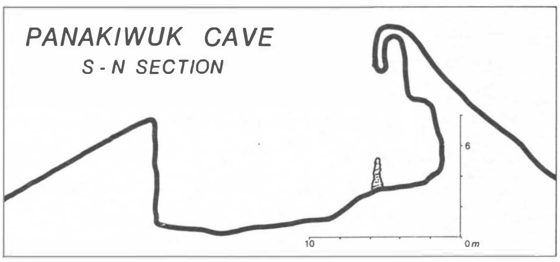

Figure 2 *North-south cross-section of the Panakiwuk doline.*

Surface indications of past human occupation are few. Several piles of oven stones, comprising mainly fist-sized pieces of limestone, were recorded (Fig. 3) and we noted one small undecorated potsherd and one or two stone flakes in dripholes in the surface. Three areas of Panakiwuk were tested by excavation. A 1 m² pit was sunk into the mouth of the shelter, just inside the dripline, and a second one excavated at the western end of the dirt floor against a protruding shelf of stone. The major effort, however, went into excavating a 2 m x 1.5 m pit in the centre of the site (Fig. 3). While the former two pits produced artefactual evidence, their main value was in helping explicate the sedimentary history of the site. Thus, the artefactual analyses reported here concentrate on the main excavation, largely because of the difficulty of relating the two outlying pits to the centre one in any satisfactory stratigraphic manner. This will become clearer below.

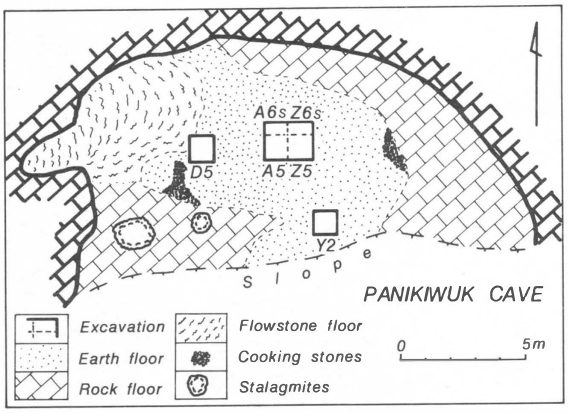

Figure 3 *Ground plan of Panakiwuk cave showing location of excavation squares and other surface features.*

EXCAVATION AND STRATIGRAPHY

All the excavated materials from Panakiwuk were dry sieved through 5 mm and 3 mm mesh. Initial plans to wet sieve the materials were scrapped, partly because of the difficulty of moving the excavated deposits to water, and partly because of the efficiency of our New Guinean sorters in recovering small bones and stone flakes from the sieves. Following normal practice, excavation units were determined according to colour and texture changes in the stratigraphy; where such changes could not be determined, arbitrary units ranging around 10 cm in depth were employed.

The central excavation comprised Square A5, Square Z5 and the southern halves of Squares A6 and Z6, designated A6S and Z6S (Fig. 3). As stated, this produced a rectangular excavation measuring 2 m east-west and 1.5 m north-south. Unlike the peripheral Squares Y2 and D5, in which artefactual layers ceased below c.60-80 cm depth, the central excavation continued down c.2 m, with bedrock being reached by augering down a further 1 m through sterile clays. A few blackened animal bones were still appearing at 2 m, but stone artefacts were recovered only to a maximum depth of 1.51 m and were seen to cease between 1.4 m and 1.5 m in these squares.

Excavation in this area proceeded slowly, partly because of the double sieving, but mainly because of the complicated stratigraphy which

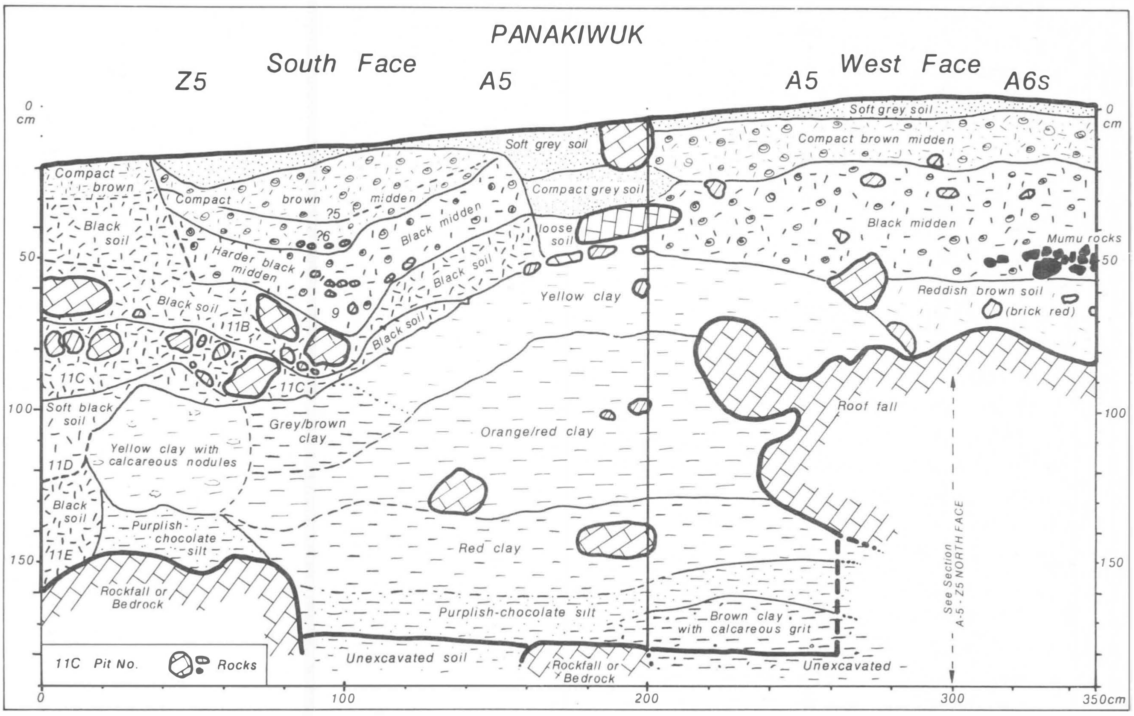

Figure 4 *Stratigraphic section showing the the south face of Squares Z5 and A5 and the west face of Squares A5 and A6S.*

will now be summarised. In Squares A5 and Z5, for example, no stratigraphic layer extended across the full area of these 1 m^2 units, with the exception of only the top surface dust and the lowest cultural unit. These disconformities, through nearly 1.5 m of deposit, included both human and natural agencies in their formation.

Given that Panakiwuk is a water-eroded cave and given that soil and clay deposits in the central excavation are much deeper than at the peripheries of the cave, at least in Squares Y2 and D5, it seems possible that the underlying bedrock forms a natural sink or well in the centre of the cave. Roof fall in the mouth of the cave may have added to the sediment trapping potential of such a formation. However, as stated, the sources of the lower natural sediments have not been clearly determined. A small chamber on the western side of the cave, now capped by a calcium carbonate layer, could once have been a source of sediment introduction from the western inner slope of the doline, but it is not now, and may never have been; direct entry of this western slope material into the present cave mouth is barred by a line of boulders and stalagmites on the western side of the entrance. Airborne tephras also cannot be discounted, although no nearby volcanic source is known. The most probable source of the clays is the breakdown of the cave walls and roof; today the walls are dry and stable, but one of the upper clay layers, discussed below, leads us to assume that this has not always been the case.

Thus the deposits in the areas tested consist initially of a series of sterile clays, the earliest perhaps laid down when ponded water was in the cave. These were followed by the deposition of concreted deposits containing only a few highly stained rat bones. Above this a distinctive purplish to chocolate, fine-particled silty soil developed. This contains animal bones throughout and small stone flakes in its upper 10-15 cm.

At this point a major depositional event took place in the cave which caused enormous problems during both excavation and subsequent analysis. A comparison of Figures 4 and 5 reveal totally dissimilar stratigraphic sections for the southern and northern faces of Squares A5 and Z5 even though they are only 1 m apart. In the southern section a huge wedge of clay layers, changing in colour from red at the bottom to orange/red in the middle and yellow at the top is a dominant feature in the section, being over 1 m deep in the southwest corner of A5.

However, these clay layers are absent in the northern section of the same squares. Here, instead, a largely homogeneous clayey soil, brown at the bottom and grading to a reddish brown at the top, has developed. The division between the clay wedge and these reddish soils was distinct during excavation and almost vertical.

The reddish soils have the normal characteristics of gradually accumulated cultural sediments in archaeological sites, in particular the incorporation of a range of human artefacts and elements of approximately horizontal stratigraphy. The wedge layers differ. They slope, talus-like, from the west to the east, thinning and indeed terminating before reaching the east wall of Square Z5 (Fig. 4) although they may be cut through at that point by pits which disappear into the unexcavated Square Y5. In addition the lowest red stratum in the wedge is devoid of artefacts although they occur in the underlying purplish-chocolate silt and in the upper strata of the clay wedge and above.

The clay wedge presented three stratigraphic problems: its origin, its depositional rate of build-up, and, most importantly, its temporal relationship to the adjacent reddish soils to the north. Only two of the possible explanations advanced for the presence and disposition of the clay wedge seemed at all credible, although we thank the 1987 3HN students for their imaginative suggestions.

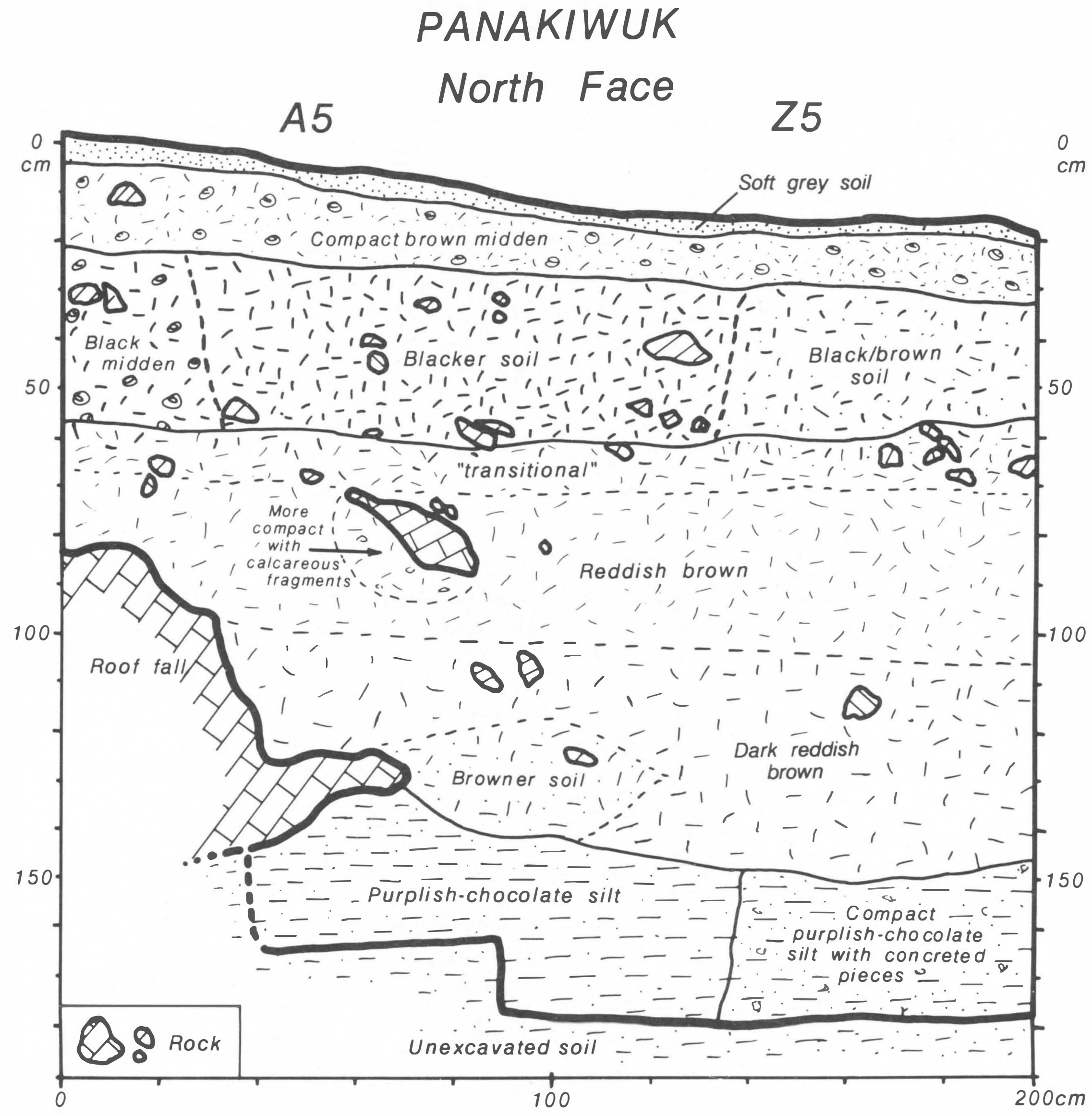

Figure 5 *Stratigraphic section showing the north face of Squares A5 and Z5.*

Origins

The less likely of the two suggestions is that the clay wedge layers once spread across the entire cave but that the back section was scoured out by water action and later infilled with cultural sediments after the water flow had ceased. This explanation fails to take into account the presence of the purplish-chocolate layer which extends under both the wedge and the reddish soils; why was this not scoured out as well on the northern side? In addition, a date of c.15,000 BP from the top of this layer, discussed in more detail below, offers little time for the clay wedge layers to accumulate across the cave in any gradual fashion. Finally, this explanation does not account for the talus shape of this material.

We favour a second explanation as more likely, although certainly not proven. The collapse of the cave roof, itself honeycombed by previous water action and comprising as much clay material as rock, may have dropped a mound of material onto the existing cave floor. In this explanation, the centre of this fallen material would likely be a few metres west of Square A5 and subsequently this material was spread eastwards into the talus seen in Square Z5. For reasons discussed below we assume that this material was spread by human trampling. We adduce the following points in support of this explanation:

1. As Figure 4 demonstrates, the purplish-chocolate silt sitting on top of the basal rock in Square Z5 is c.20 cm higher than the same material under the clay wedge. An examination of Figure 5 shows that the same is true of the purplish-chocolate silt under the reddish soils: it is c.20 cm higher than the same material under the clay wedge. This is consistent with this latter material falling from above and compressing the silt layer.

2. As Figure 4 indicates, the wedge strata are in close association with a large rock, seen in the west face section of Squares A5 and A6S. When first encountered during excavation this rock was thought to be bedrock, partly because of its size and partly because of its surface, which is smooth and rounded by water action. However, soil deposits continue underneath it and it appears to be roof fall. In the context of the present explanation it is possible that this water action took place before the roof collapsed; this is consistent with the idea of the roof being honeycombed by previous water action. The wedge layers also contain nodules and pebbles of limestone consistent with this interpretation.

3. As previously observed, the lowest stratum of the clay wedge is devoid of stone flakes, animal bones, or other artefacts. In the south-western corner of A5 where the overall depth of the wedge strata is greatest, the absence of artefactual material continues for 35 cm through the lowest red stratum and the lower part of the middle orange stratum (Fig. 4). Above this, in Square A5, the orange and yellow strata of the wedge contain artefacts in low densities. While these could conceivably derive from the more recent organic sediments above, they seem more likely to be in situ, since these upper wedge strata are not discoloured by the more recent black deposits.

4. In contrast, the 'talus' part of the wedge seen in Square Z5 (Fig. 4) is distinctly discoloured. We again doubt that this discolouration derives from above. Rather it appears to reflect the human contribution to its formation by trampling and use. This part of the wedge contains reasonable numbers of artefacts and a very clearly defined hearth containing burnt stone tools and bones which yielded a C^{14} date of c.13,000 BP.

Depositional rate

If the roof fall interpretation holds, then it implies that deposition may have been quite rapid. Much or all of the material may have come down as a single event. If there was only a single event then the upper strata of the wedge which have cultural materials incorporated in them must have moved eastwards as a separate and later event, presumably under human impetus. Alternatively, after an initial major fall, represented in Square A5 as the sterile red core of the wedge, lesser amounts of material kept falling over tens or hundreds of years, burying artefacts discarded on its accumulating surface. On the basis of the two C^{14} dates already mentioned, and discussed in more detail below, the initial event took place between 15,000 BP and 13,000 BP.

Temporal relationship of wedge strata to adjacent deposits

The roof fall explanation implies that the wedge strata predate the buildup of reddish soils between the wedge and the back of the cave to the north, and this is indeed borne out by the radiocarbon dates discussed in the next section. However, if the wedge talus in Square Z5 was being lived on 13,000 BP it seems improbable that no deposits began to accumulate on the purplish-chocolate silt layer immediately north of it until around 10,000 BP (see below, chronology). The nearness of the back wall, which is sloping sharply towards the front of the cave may mean that this area of the cave remained unused, even for dumping, and/or that the cave was never intensively used prior to c.10,000 BP. This latter interpretation is supported to some extent in the artefactual analyses. There is, finally, the possibility that the water scouring hypothesis is at least partly correct and that deposits laid down in the back of the cave between 15,000 BP and 10,000 BP have been lost. Without repeating the involved arguments already attempted, we abandon this problem by observing that if either explanation, or some combination of both, approaches the truth, then we can be satisfied that stratigraphically the wedge strata predate the adjacent reddish soil strata of equal depth, as the C^{14} dates clearly suggest.

The reddish soil layers are up to 90 cm deep and contain the richest artefactual levels in the site. They were the most difficult levels to differentiate while digging because we could not perceive textural changes in the soils, and although there is an overall colour difference (darker reds at the bottom, lighter higher up) the changes are gradual. There is a transitional zone at the top of this reddish soil, darkened to some extent by the organic rich sediments which overlie them. However, it is important to state quite clearly at this point that the archaeologists digging the site were all convinced of the stratigraphic integrity of this zone, and that the artefacts it contained were not derived from above; this is important because of significant artefactual distributions in this unit discussed later in this paper.

Both the reddish soils and the clay wedge are overlain by sediments of an entirely different character. From here to the surface are a series of organically rich, black-brown midden layers containing a number of nested and intercutting 'mumu' or cooking pits. The differences between these layers and the stratigraphically earlier ones are emphasised by the age differences apparent in the C^{14} dates, to which we now turn. Before doing this we note that Table 1 summarises much of this discussion. There the 15 stratigraphic units delineated while excavating are listed with their corresponding excavation squares and spits. This scheme will be refined after discussing the chronology of the site.

RADIOCARBON DATES AND SITE CHRONOLOGY

Fourteen C^{14} determinations are reported in Table 2. Starting at the bottom of the site, the two RIDDL (Radio-Isotope Direct Detection Laboratory, Simon Fraser University, Vancouver) dates are AMS dates on small charcoal samples. RIDDL-531 was a normal small sample, derived from small charcoal specks contained in a localised area in the upper part of the purplish-chocolate silt in the southwest corner of Square Z5. This feature was readily identified as a concreted and fire-reddened clay patch containing six burnt flakes. This patch was excavated separately and mainly bulk bagged for laboratory sorting; the cleanup of this feature was sieved in the field and produced several more

Table 1 *Stratigraphic units, showing the squares and spits represented. NR = not represented; * = mixed spit (Stratigraphic Unit 6 on north side of square, Stratigraphic Unit 11 on south side of square).*

Stratigraphic Unit	Description	Square A5	Square Z5	Square A6S	Square Z6S
1	Soft grey soil	1,2	NR	1	1
2	Compact brown midden	3,4,4A,5	1,2,3,4	2,3,4	2,3,4
3	Black midden mumu pits	9B	9,11,11B,11C,11D	5,6,6A	NR
4	Brown black midden	6,7	5,6,7,8	7,8,9	5,6
5	Early black mumu pits	NR	NR	10	NR
6	Transitional red brown	8*,9A	10,12	11,12	7,8,9
7	Upper brown	NR	13,16	NR	NR
8	Red brown	10,13	14,15,17,18,19	13,14,15	10,11,12,13
9	Brown	16	11E,20,20A,21,28	16,17	14,15
10	Yellow clay (wedge)	11,12	22,23	NR	NR
11	Orange or grey clay (wedge)	14,15,17	24,25,26	NR	NR
12	Sandy grey clay (wedge)	18	27	NR	NR
13	Red clay	19,21,22	NR	NR	NR
14	Purple soft with burnt patch	20,23	29,29A,30,31,32,33	18,19	16
15	Purple cemented	24	34	20	17

Table 2 *Panakiwuk uncalibrated radiocarbon dates, showing relationships to stratigraphic units and analytical units.*

Stratigraphic		Square and Spit	Laboratory No.	Date BP	Material Unit	Analytical Unit
1	Soft grey	A5/1-2	ANU-5529	640±130	Loose Charcoal	
2	Brown Midden	A5/3-5	ANU-5530	1040±110	Loose Charcoal	A
?		Y2/2	ANU-5376	1170±70	Charcoal	
4	Brown Black Midden	Z5/5-7	ANU-5531	1630±130	Loose Charcoal	
6	Transitional Red Brown	A6S/11	ANU-5547	8910±690	Bone (Apatite Frac.)	
		A6S/12	ANU-5546	10160±390	Bone (Apatite Frac.)	
			ANU-5546	2950±850	Bone (Collagen Frac.)	
8	Red Brown	A6S/15	ANU-5544	10300±310	Bone (Apatite Frac.)	B
		Z6S/13	ANU-5545	8480±500	Bone (Apatite Frac.)	
9	Brown	A6S/16	ANU-5541	8000±830	Bone (Apatite Frac.)	
		Z6S/14	ANU-5540	8530±520	Bone (Apatite Frac.)	
		Z6S/15	ANU-5542	6780±1220	Bone	
11	Orange or Grey Clay	Z5/24-26	RIDDL-316 (ANU-4978)	12930±210	Charcoal	C
13	Red Clay	Z5/30	RIDDL-531	15140±160	Charcoal	D

flakes and pieces of animal bones. Our original opinion, that this patch was itself a hearth, has been revised. It sits almost directly beneath the fire pit described in the next paragraph and is better explained as being discoloured and hardened by the heat from above. It is, however, stratigraphically discrete from that pit, a view confirmed by the C^{14} dates.

RIDDL-316 is the weighted average of two vials of CO_2 prepared by John Head in the Australian National University Radiocarbon Laboratory as ANU-4978. The results from each vial were concordant. The charcoal sample derived from a well-defined fire pit in the southwestern corner of Square Z5 which had also been recognised in the southeastern corner of Square A5 as a discoloured depression containing calcareous nodules and artefacts. The pit, as recognised in excavation, is approximately 40 cm long, 20 cm wide and 20 cm deep, although it may have been dug from a higher level than first recognised. As defined, it begins c.20 cm below the top of the clay wedge in the discoloured grey-brown zone in Figure 4, and appears to be totally contained within it.

The reddish soil layers to the north of the clay wedge contained no charcoal. In discussion with John Head we supplied him with bone from Stratigraphic Units 6, 8 and 9. The only available bone for this purpose was *Melomys rufescens* postcranial material, which in turn required that each sample comprised hundreds of loose bones. The probable mixing implied in this procedure may account, at least in part, for the minor inversions in this set of eight dates. In discussing these dates with John Head, he noted that in regard to the glaring discrepancy between the apatite and collagen fractions for ANU-5546, the collagen result indicates the presence of young organic material in this sample. However he said that the result on the apatite fraction should be regarded as a minimum age because of the possibility of younger carbonate being present in this fraction. The notion of younger carbonates possibly being present was also suggested by Head for ANU-5545, ANU-5541 and ANU-5542.

Collectively, these eight determinations suggest that only a general time range of between 10,000 BP and 8000 BP can be applied to these reddish soil units. Both the depth of these deposits, averaging c.70 cm, and the quantities of artefacts contained within them suggest that the tempo of accumulation was fastest during this phase of site deposition. It is uncertain, from the amount of the site dug, whether this indicates more intensive use of the site by humans during this period, or merely more intensive dumping of material in this local area, especially if this represented some sort of channel at the back of the site, as previously discussed. What is clear is that there is a 1500 year gap between the oldest date in this set (ANU-5544) and the upper date from the wedge strata (RIDDL-316) at two standard deviations.

The upper unit of organic shell midden soils and cooking pits provided good charcoal samples and the resulting dates are straightforward. ANU-5376 has been inserted in Table 2 according to its age determination since it lacks any stratigraphic control. However it came from close to the surface in Square Y2 and is consistent with the other upper dates from the central excavation. The very obvious and startling configuration is the time gap between the upper organic soils and the deposits which they immediately overlie. On this evidence Panakiwuk appears to have been abandoned c.8000 BP and not re-occupied until the post-Lapita period.

On the bases of the stratigraphic and chronological evidence we have isolated four blocks of excavated deposits which we have used as general analytical units. We have chosen to follow the broad stratigraphic units in creating these analytical units because they are supported by the chronological evidence as discussed. It is thus co-incidental, but still convenient, that they also form slices of time which are not themselves grossly dissimilar in length.

Unit A

This unit encompasses the organic rich soils at the top of the site. As stated, the site shows few indications of recent use, and excavation recovered only a single colonial period artefact, the possible Japanese bottle glass fragment. Thus the surface date of c.640 BP (ANU-5529) may be an accurate reflection of infrequent use over the last few centuries. Altogether this unit may represent only 1000-1200 years of episodic use, spanning Stratigraphic Units 1-5.

Unit B

This unit includes the reddish soils at the back of the site contained in Stratigraphic Units 6-9. While the internal dates are unclear, it would seem that these deposits span a period of c.2000 years or less, and that they are clearly differentiated in time from Unit A and Unit C.

Unit C

This unit represents the clay wedge layers, Stratigraphic Units 10-13. On the basis of the C^{14} dates it is predated by a date of c.15,000 BP and has a date of c.13,000 BP in Stratigraphic Unit 11. From its artefactual content we can suggest that it represents evidence of sparse human occupation over 2000 years or more, or marginally more intense occupation over a shorter period.

Unit D

This is represented by Stratigraphic Units 14 and 15 which contain the purplish-chocolate silts at the bottom of the artefactual layers. While it also contains the dated burnt clay patch on its surface, we have no real way of knowing what time period this unit may represent. As will be seen from the numbers of artefacts discussed below, this unit only fleetingly reflects human presence at Panakiwuk.

Although devoid of artefactual stone, the clayey deposit beneath the purplish-chocolate silts in the central excavation squares yielded a small quantity (60 pieces) of bone. This sample pre-dates human occupation of the site and is labelled 'Stratigraphic Unit 16' in the faunal data. It is not included in Analytical Unit D.

DATA ANALYSES

In the course of the analyses, which differed between and amongst the data classes, some were undertaken on the basis of the four analytical units just defined, and some were undertaken on the stratigraphic units delineated in Table 1. As well, because these units were sometimes wildly variable in volume, density correction factors were calculated for certain analyses when they were considered appropriate. Where these distinctions occur in the following descriptions, they are clearly identified.

Stone

Flaked stone artefacts were recovered from all four analytical units and all stratigraphic units with the exception of Stratigraphic Unit 12 in the wedge strata. While consistently present in the site, stone artefacts are never abundant. The industry can be generalised as comprising a high proportion of unmodified flakes and flaked pieces, and as far as present descriptions allows comparison, the Panakiwuk stone industry sounds very similar in character and frequency to that from Balof 2 (White et al. this volume). An initial, simple morphological analysis was carried out on 613 stone artefacts which comprised all those recovered from Squares Z5, A6S and Z6S. These exclude obsidian, which is treated separately below. To pre-empt the results reported here, this analysis indicated few trends of consequence in the site and this analysis of the remaining stone tools has been suspended until continuing work on the other data classes and other aspects of the stone analysis is completed. Distributions between the three squares analysed showed close similarities and the data are presented here in combined form.

Each piece was inspected under a low-powered microscope and allotted to one of the following categories: 1) *flake*, where a bulb of percussion is discernible but no other modification is present; 2) *modified flake*, where a bulb is present and where one or more of the following subcategories – usewear, retouch or core rejuvenation flake – is discernible; 3) *piece*, a waste chip or flake where no bulb or platform is evident; 4) *core*, a stone without a bulb of

Table 3 *Panakiwuk stone artefact counts for Squares Z5, A6S and Z6S combined, by stratigraphic unit and analytical unit. The bracketed figures indicate volumetrically corrected figures (per m³).*

Stratigraphic Unit	Flakes	Modified Flakes	Pieces	Cores	Core Tools	Hammer Stones	Split River Pebbles	Total No.	Total Wt (g)
1	1	2	6					9	16.5
2	15		23					38	39.4
3	13	2	14					29	26.3
4	6	1	8	1		4		20	137.0
5			2		1	1		4	45.1
Analytical Unit A	35 (35)	5 (5)	53 (54)	1 (1)	1 (1)	5 (5)		100 (101)	264.3 (267.5)
6	21	3	12		1	3	2	42	229.9
7	5	2	5					12	13.8
8	90	16	77	1	3		1	188	476.3
9	83	23	65	2	3			176	265.2
Analytical Unit B	199 (200)	44 (44)	159 (160)	3 (3)	7 (7)	3 (3)	3 (3)	418 (420)	985.2 (989.1)
10	4	9	6					19	25.1
11	8	2	11					21	12.9
12									
13	4	1	3					8	9.7
Analytical Unit C	16 (116)	12 (87)	20 (145)					48 (348)	47.7 (344.6)
14	24	4	17					45	83.3
15	1		1					2	0.2
Analytical Unit D	25 (70)	4 (11)	18 (50)					47 (133)	83.5 (232.9)
Site Total	275	65	250	4	8	8	3	613	1380.7

percussion, from which other flakes have been detached, exhibiting at least one platform and 2+ negative flake scars; 5) *core tool*, a core which exhibits usewear; 6) *hammerstone*, usually a pebble with battering along one or more margins; and 7) *split river pebble*. Split river pebbles form a significant aspect of the Matenkupkum assemblage (Gosden and Robertson this volume) where they derive from the local rivers. None of the northern New Ireland streams on the east coast contain similar pebbles (to our knowledge) so they likely derive from either the west coast or from further south in New Ireland. The first one we encountered in the laboratory was very similar to those from Matenkupkum so they were recorded separately.

For each of these categories various measurements were taken where applicable: maximum length, breadth, thickness; oriented length, breadth, thickness; number of platforms; number and length of retouched zones; weight. These data for each piece were recorded and stored using the Foxbase computer program.

Manipulations of these data indicate that flakes, modified flakes and pieces make up 96% of the sample. They are small, squarish in plan (average length approximately equals average breadth throughout the entire sequence) and are normally thin. They give the impression of having been struck from steep-sided cores, but as there are only 12 cores in the sample these do not support the level of specialisation implied in such a statement.

Table 3 shows the distribution of stone across the sampled layers according to category and stratigraphic distribution. This shows very clearly that Stratigraphic Units 8 and 9 have by far the highest level of discard, but there are no significant differences here in terms of raw materials, reduction sequences, or use patterns which might explain why these units contain more than half the stone material – 402 of the 509 flakes, modified flakes and pieces, and 10 of the 12 cores. If volumetric corrections are applied to these two units then stone artefact density is higher in Stratigraphic Unit 9 (632 artefacts per m^3) than in Stratigraphic Unit 8 (425 per m^3). Stratigraphic Units 8 and 9 are seen to be generally similar in terms of stone artefact distribution, but a remarkable distinction can be made between them on the basis of shell distributions, discussed below.

This apparent concentration of material in Analytical Unit B is also seen in other distributions, particularly the faunal one, but in the case of stone this is modified if volumetric corrections are applied. In Table 3 the bracketed figures in the analytical unit totals indicate numbers per m^3 and show that the densities of stone artefacts in Units D and especially C are closer to those in Unit B than the raw scores suggest. Unit A appears to be the most dissimilar unit, with fewer overall artefacts and a lower percentage of retouched pieces and whole flakes.

Table 4 demonstrates that the average weight of flakes and pieces (excluding modified flakes) is consistently small throughout, averaging only 1.3 g. Table 5 indicates that modified flakes (as might be expected) are larger, although still small, at an average of 4.4 g. Retouched flakes represent only 1.3% and usewear occurs on only 7.5% of the total flakes and pieces.

Table 4 *Panakiwuk average weight (g) for stone flakes and pieces. Stratigraphic Unit 8 contains one piece weighing 93.8 g and another weighing 35.7 g. Without these the average for this unit reduces to 1.22 g.*

Stratigraphic Unit	Total No.	Weight (g)	Average Wt (g)
1	7	3.4	0.49
2	38	27.4	0.72
3	27	25.6	9.95
4	14	12.2	0.87
5	2	2.2	1.10
6	33	81.7	2.5
7	10	9.7	0.97
8	167	331.1	1.98
9	148	150.9	1.02
10	10	2.7	0.27
11	19	5.2	0.27
12			
13	7	8.9	1.27
14	41	27.1	0.66
15	2	0.2	0.10
Total	525	688.3	1.31

John Webb, Department of Geology, La Trobe University, kindly identified 269 stone artefacts from Squares A6S and Z6S in terms of their raw materials and these data are presented in Table 6. Since Unit D is only represented by four items and Unit C is not represented at all, it is impossible to make comparisons between the earlier and later parts of the site on these data. Lower unit materials from Square Z5 are still being identified. Few trends are apparent in Table 6, although chert may be marginally more plentiful in the earlier layers there.

More recently Peter Sheppard, Department of Anthropology, University of Auckland, has begun a more intensive identification program on the cherts from both Panakiwuk and Matenkupkum. For Panakiwuk, Sheppard reports that

Table 5 *Panakiwuk modified flakes by sub-category.*

Stratigraphic Unit	Rejuvenation Flakes No.	Rejuvenation Flakes Wt (g)	Usewear Flakes No.	Usewear Flakes Wt (g)	Retouched Flakes No.	Retouched Flakes Wt (g)
1	1	13.0	1	0.1		
2						
3			2	0.7		
4	1	1.3				
5						
6			2	6.6	1	15.2
7			2	4.1		
8	4	14.0	10	50.8	2	3.4
9	6	43.2	16	49.2	1	0.8
10			7	12.4	2	10.0
11			1	0.3	1	7.4
12						
13	1	0.9				
14			3	50.0	1	1.7
15						
Total	13	72.4	44	174.2	8	38.5
Range (g)	0.3 - 34.6		0.1 - 13.9		0.8 - 15.2	
Average Wt (g)	5.6		4.0		4.8	

its inhabitants were exploiting a variety of siliceous rocks, none of which need be derived from outside the island. He suggests that the Lossuk River beds, 4-5 km to the north of the site, contain tuffaceous siltstones and limestones and are a possible source, although the fossilised wood which is a common raw material in the site may have a more specific source. At least three other chert groups have been identified in thin section and further identification, involving Instrumental Neutron Activation Analysis, is commencing under Sheppard's guidance. This same technique is being employed to identify the Balof 2 stone artefacts (White et al. this volume) so a detailed comparison between these sites will be possible. To date there are, not surprisingly, no chert source overlaps between Panakiwuk and Matenkupkum.

Table 6 *Panakiwuk Squares A6S and Z6S. Stone distribution by raw material. Stratigraphic Units 7, 10, 11, 12 and 13 contained no stone in these squares.*

Stratigraphic Unit	Silicified Limestone No.	Silicified Limestone %	Limestone No.	Limestone %	Quartz No.	Quartz %	Silcrete No.	Silcrete %	Trachyte No.	Trachyte %
1	1	12.5	1	12.5			1	12.5		
2	1	3.4	4	13.8	3	10.3			1	3.4
3	3	42.9								
4	3	23.1	2	15.4	1	7.7	1	7.7	4	30.8
5	2	66.6								
6	1	5.3	2	10.5	2	10.5	2	10.5	2	10.5
8	22	19.0	8	6.9	14	12.1	6	5.2	4	3.4
9	16	20.8			21	27.3	1	1.3	6	7.8
14					2	50.0				
Total	49	17.7	17	6.1	43	15.5	11	4.0	17	6.1

Stratigraphic Unit	Rhyolite No.	Rhyolite %	Chert No.	Chert %	Siltstone No.	Siltstone %	Mudstone No.	Mudstone %	Greywacke No.	Greywacke %	Total
1	1	12.5	2	25.0	2	25.0					8
2	6	20.1	8	27.6	5	17.2					28
3	2	28.6	1	14.3	1	14.3					7
4					1	7.7			1	7.7	13
5											2
6	2	10.5	3	15.8	3	15.8	2				19
8	2	1.7	40	34.5	16	13.8		10.5			112
9			25	32.7	7	9.1					76
14			2	50.0				0.7			4
Total	13	4.7	81	29.2	35	12.6	2	0.7	1	0.4	269

Obsidian

Panakiwuk yielded 10 pieces of obsidian. From Table 7 it can be seen that all pieces are quite small and that bipolar flaking can be identified on two of them. More interesting are the sourcing studies and site distributional data. The sources listed are those provided by Roger Bird (Australian Atomic Energy Commission, Lucas Heights, Sydney) using the PIXE-PIGME technique (Bird and Russell 1976; Bird et al. 1981; Duerden et al. 1979). These correspond well with earlier density sourcing undertaken by Wallace Ambrose. As can be seen, nine pieces occur in Analytical Unit A and were thus all deposited in Panakiwuk less than 2000 BP. By source they are distributed as follows: five are from Lou Island, one possibly from Lou Island, one from Pam Lin Island (Admiralty Islands, near Lou Island) and two from Talasea. The tenth piece is of interest because it derives from the uppermost stratigraphic unit in Analytical Unit B and was thus ostensibly deposited there c.8000 BP. Prima facie it must be allowed that the piece may be intrusive, but if it is in situ it is not necessarily contentious when we consider that 1) it derives from Talasea, 2) it equates in time with the earliest introduction of obsidian to Balof 2 (White et al. this volume) and 3) Talasea obsidian had begun arriving in southern New Ireland at least 10 millennia earlier (Allen et al. 1989:555). Given that no clear evidence of later intrusions could be detected in this unit, as discussed above, this piece may equally corroborate the Balof 2 evidence.

Vertebrate fauna

The analysis of the vertebrate fauna was pursued over a period of twelve months during 1986/87. Prior to its commencement the notion that some non-domesticated mammals identified at Balof 2 and subsequently at Matenkupkum might have been humanly transported into New Ireland had been advanced in discussions with Peter White and Chris Gosden (see for similar ideas elsewhere in Melanesia, Flannery et al. 1988). For this reason one major aim was to establish whether the same animals were represented at Panakiwuk and the degree to which the respective faunal sequences matched. Towards this end, the mandibles, maxillae and teeth of all mammals were separated from the rest of the bone assemblage and identified by Tim Flannery, The Australian Museum, Sydney. With the resultant list of species in hand and some idea of their distribution and relative abundance through the deposit, the analysis proceeded. At this stage our primary aim was to quantify the apparent variability in bone distribution and species representation through time.

It is well known that caves act as natural traps for bone accumulation from both cultural and non-cultural sources. In terms of the latter, the two probable modes of accumulation primarily responsible at Panakiwuk are non-human predators depositing the bones of their prey, and the natural mortality of cave dwelling animals, particularly bats.

Laboratory processing (washing and boxing) confirmed the field observation that the assemblage was dominated by rodent bones. Because large quantities of rodent bones in cave deposits are usually interpreted as bird of prey or raptor pellet accumulations (see Marshall 1986) a further aim of the analysis was to investigate the probability that a component of the assemblage derived from this particular non-human agent. Furthermore, since the rodents were mostly represented by postcranial elements it was decided to develop a methodology suitable for incorporating these elements in estimates of the relative abundances of species in the site.

In light of the spatial patterning in bone distribution, the likely agents of bone accumulation, and inferred discard behaviours of the human occupants, a further aim was to develop a model of the formational history of the faunal deposits in order to discuss Panakiwuk's possible functions within a regional context.

Table 7 *Panakiwuk obsidian identification and distribution.*

Stratigraphic Unit	Square and Spit	Length (cm)	Width (cm)	Weight (g)	Source	Comment
1	A5/1	1.0	0.8	0.3	Pam Lin	Bipolar Flaking
2	A5/2	1.1	0.5	0.1	?Lou	
2	A5/5	1.4	1.1	0.3	Lou	
2	Z5/1	2.1	0.6	0.4	Lou	
?2	Y2/3	Not measured			Lou	
3	Z5/11	1.0	0.5	0.1	Lou	
4	A6S/8	1.4	0.6	0.2	Lou	
4	Z5/5	1.3	0.9	0.7	Talasea	Bipolar Flaking
4	Z5/5	0.9	0.7	0.1	Talasea	
6	A6S/11	1.5	1.2	0.6	Talasea	

Identification

This was generally hampered by the paucity of current data on New Ireland's extant animals and by a lack of suitable reference skeletons. Reference collections from various institutions were used but mainly from The Australian Museum, the Museum of Victoria and the La Trobe University Zoology and Archaeology Departments. Other than Tim Flannery, specialist advice and assistance was provided by Mark Hutchinson, Zoology Department, La Trobe University, on reptiles, and Bob Baird, Zoology Department, Monash University, on birds.

Methods

As well as the initial removal of diagnostic pieces, preliminary sorting separated the abundant and excellently preserved rodent bones from all other remains. For the rodents, species were identified on maxillae and mandibles only. At this stage it was apparent that the various species differed significantly in relative abundance and that their body size differences were usually great enough to allow confident identification of the postcranial skeletal elements. With this in mind the rodent postcranial bones were identified to skeletal element and *assigned* to a species according to their relative size, taking age into account. Where an overlap in the size of postcranial elements between members of the same genus was a probability (as indicated by body size and reference skeletons) no differentiation was attempted.

With the non-rodent remains, an attempt was made to identify all cranial and postcranial bone to the lowest possible taxonomic levels. In this analysis fragments which could not be identified to class were deemed unidentifiable.

Quantification

As well as taxonomic data, the number and weight to the nearest tenth of a gram of each bone or set thereof were recorded. In order to calculate the minimum number of individuals (MNI) the body symmetry of each element (left, right or medial) was also noted. Bone portion was not recorded for incomplete elements since these were relatively few in number and no attempt was made to pair elements on the basis of size or age estimates.

Burning

Burning as indicated by surface discolouration was also noted when present. Although we recognise that burning may result in a range of colour changes (see Shipman et al. 1984:312-4) this was mostly restricted to calcined bone, that is, bluish-white or grey in colour. With many blackened pieces of bone, it was often not possible to distinguish between charring and chemical, presumably magnesium oxide, staining.

Results of the faunal analyses

General Composition

Although most bones are relatively small in size they are well preserved and are present throughout the deposit. Table 8 shows that the assemblage totals 16,080 pieces but only weighs 1734.1 g – a mean average weight per fragment of 0.1 g. It can be seen that the vast majority of bones (96%) derive from Squares A5, Z5, A6S and Z6S and that over half are from the two squares closest to the rear wall, A6S and Z6S (also see Fig. 6). In contrast, there is very

Table 8 *Panakiwuk bone counts and weights by square.*

Square	Number	%	Weight (g)	%
A6S	5910	36.8	636.9	36.7
Z6S	4791	29.8	492.6	28.4
A5	2002	12.5	219.5	12.7
Z5	2864	17.8	319.7	18.4
Subtotal	15567	96.8	1668.7	96.2
Y2	465	2.9	51.8	3.0
D5	48	0.3	13.6	0.8
Subtotal	513	3.2	65.4	3.8
Total	16080	100.0	1734.1	100.0

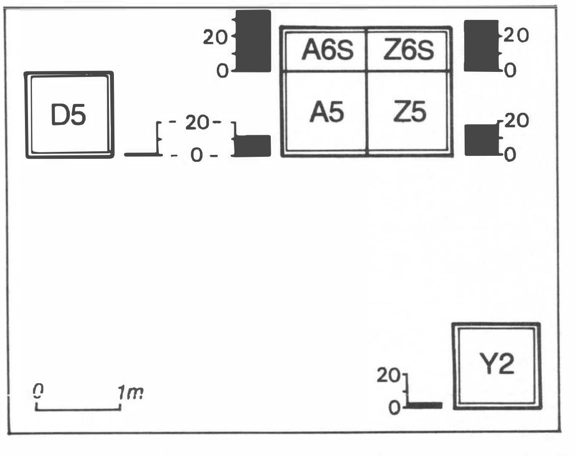

Figure 6 *Percentage frequencies of Panakiwuk bone counts by square.*

little bone in deposits sampled from the side (Square D5) and front (Square Y2) of the cave. Its distribution according to stratigraphic unit (Table 9 and Fig. 7) shows that most bone occurs in Stratigraphic Units 1-9, with 50% of the total in Stratigraphic Unit 8. Unlike the stone artefacts

Table 9 *Panakiwuk bone counts and weights by stratigraphic unit and analytical unit. Note the inclusion here of Stratigraphic Unit 16 below Analytical Unit D. As discussed in the text, this additional Stratigraphic Unit contains animal bones which have been separated because they are considered to pre-date human occupation in the site.*

Stratigraphic Unit	Number	%	Weight (g)	%
1	126	0.8	14.7	0.9
2	1148	7.4	134.9	8.1
3	696	4.5	104.0	6.2
4	914	5.9	126.2	7.6
5	182	1.2	22.5	1.3
Analytical Unit A	3066	19.8	402.3	24.1
6	1879	12.1	219.9	13.2
7	178	1.1	20.9	1.3
8	7933	51.0	786.8	47.2
9	1730	11.1	162.0	9.7
Analytical Unit B	11720	75.3	1189.6	71.4
10	40	0.3	5.3	0.3
11	386	2.5	37.2	2.2
12	33	0.2	2.0	0.1
13	29	0.2	0.5	0.1
Analytical Unit C	488	3.2	45.0	2.7
14	173	1.1	18.6	1.1
15	54	0.3	5.2	0.3
Analytical Unit D	227	1.4	23.8	1.4
16	66	0.4	8.0	0.5
Total	15567	100.1	1668.7	100.1

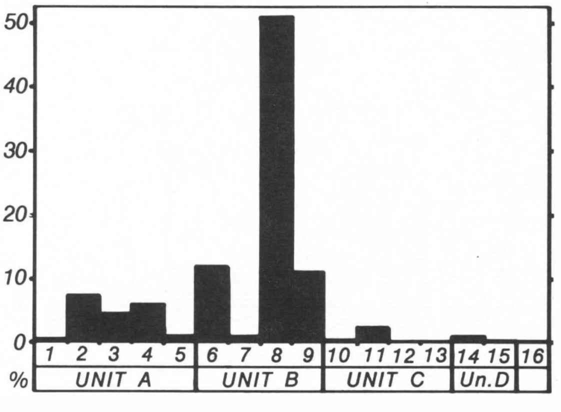

Figure 7 *Percentage frequencies of bone counts by stratigraphic unit and analytical unit.*

Table 10 *Panakiwuk bone counts and corrected density numbers by analytical unit.*

Analytical Unit	Number	Density per m^3
A	3066	1929
B	11720	9517
C	488	801
D	227	502
Total	15501	

from these units, applying volumetric corrections to Stratigraphic Units 8 and 9 indicates that the density of bones per m^3 is three times greater in Stratigraphic Unit 8, by both number and weight. Table 10 shows both raw counts and volumetrically adjusted densities for the central squares according to analytical unit. These show the same rank ordering and the adjusted figures confirm Unit B as the phase of greatest bone deposition.

Only 713 pieces of bone or 4.6% of the total displayed burning. Table 11 shows that burnt bone is present in small but remarkably consistent quantities throughout most of the deposit. It is absent in Stratigraphic Unit 13 in the clay wedge, and in the pre-occupation deposits of Stratigraphic Unit 16, confirming burning as an indicator of human activity at this site, and adding a further confirmation to the interpretation previously advanced for the formation of the clay wedge.

Table 11 *Panakiwuk burnt bones: counts and percentage frequencies by stratigraphic unit and analytical unit. Note the inclusion here of Stratigraphic Unit 16 below Analytical Unit D. As discussed in the text, this additional Stratigraphic Unit contains animal bones which have been separated because they are considered to pre-date human occupation in the site.*

Stratigraphic Unit	Number	Burnt (no.)	Burnt (%)
1	126	8	6.3
2	1148	53	4.6
3	696	28	4.0
4	914	40	4.4
5	182	4	2.2
Analytical Unit A	3066	133	4.3
6	1879	61	3.2
7	178	8	4.5
8	7933	393	5.0
9	1730	86	5.0
Analytical Unit B	11720	548	4.7
10	40	2	5.0
11	386	17	4.4
12	33	5	15.2
13	29	0	
Analytical Unit C	488	24	5.0
14	173	7	4.0
15	54	1	1.9
Analytical Unit D	227	8	3.5
16	66	0	
Total	15567	713	4.6

Table 12 *Vertebrate taxa present at Panakiwuk.*

Amphibians	Salentia
Birds	Rallidae Psittaciformes Passeriformes Columbidae Galliformes Non Passerine
Fish	Osteichthyes
Mammals	*Homo sapiens sapiens*
	Phalanger orientalis *Thylogale brunii*
	Chiroptera Megachiroptera *Pteropus neohibernicus* *Dobsonia moluccensis* *Nyctimene major* *Nyctimene* sp. *Melonycteris melanops* Microchiroptera *Hipposideros diadema* *Hipposideros maggietaylori*
	Melomys rufescens *Rattus mordax* *Rattus praetor* *Rattus* sp. *Rattus exulans*
Reptiles	Sauria Gekkonidae Scincidae Varanus ?Tiliqua Serpentes Boidae ?Elapidae ?*Crocodylus* ?*Chelonia*

Species Representation

Table 12 lists the identified fauna. It can be seen that all five classes of vertebrates are represented, although numerically mammals are dominant. Tables 13 and 14 show the raw weights and counts (NISP) of the taxa by stratigraphic unit. MNI calculations for each are shown in Table 15. These are based on the most abundant element within each stratigraphic unit, taking body side into consideration. Numbers of elements which could not be assigned a body side were simply divided by two and added to the number of the most abundant side. Since MNI exaggerates the relative abundance of rarer animals with decreasing sample size (see Grayson 1984; Allen and Guy 1984) these have been calculated according to stratigraphic unit rather than spit. Given the good correspondence between the calculated MNI and the NISP for the assemblage as a whole (Fig. 8), we have chosen to use the former as the relative abundance measure.

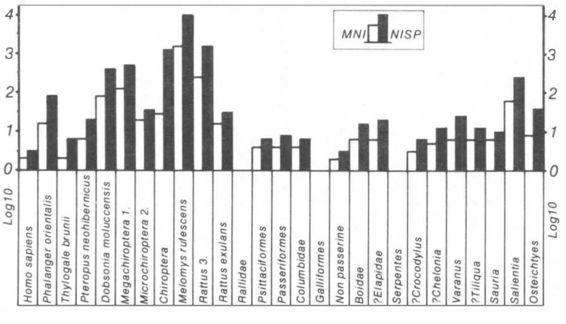

Figure 8 *Comparison of counts (NISP and MNI) for vertebrate taxa. Note that Megachiroptera includes the two species of* Nyctimene *and* M. melanops; *Microchiroptera combines* H. diadema *and* H. maggietaylori; *Rattus combines* R. praetor, R. mordax *and* Rattus sp.; *and Sauria includes the Gekkonidae and Scincidae.*

Marsupials

Of the seven terrestrial mammals present, excluding humans, *Thylogale brunii*, a small wallaby, is the largest by weight (5-10 kg) but also

Table 13 *Weights (g) of vertebrate taxa by stratigraphic unit and analytical unit. Note that Megachiroptera includes the two species of* Nyctimene *and* M. melanops*; Microchiroptera combines* H. diadema *and* H. maggietaylori*; Rattus combines* R. praetor, R. mordax *and* Rattus *sp.; and Sauria includes the Gekkonidae and Scincidae.*

Species	Stratigraphic Unit 1	2	3	4	5	**Anal. Unit A Total**	6	7	8	9	**Anal. Unit B Total**	10	11	12	13	**Anal. Unit C Total**	14	15	**Anal. Unit D Total**	16	Total
H. sapiens sapiens			4.2			**4.2**			1.2		**1.2**										5.4
P. orientalis	3.1	5.2	4.6	6.0	0.2	**19.1**	8.8	1.1	36.4	0.6	**46.9**		1.0			**1.0**					64.0
T. brunii		1.4	1.3		2.7															2.7	
P. neohibernicus	0.5	1.1	1.8	2.4		**5.8**	1.0		0.7	0.4	**2.1**										7.9
D. moluccensis	0.3	24.4	5.6	8.4	0.5	**39.2**	9.2	1.0	25.4	2.8	**38.4**	0.1	0.7			**0.8**	1.3	1.1	**2.4**	3.2	84.0
Megachiroptera	0.7	1.1	0.6	4.9	0.3	**7.6**	4.0	0.1	24.1	3.5	**31.7**	0.1	0.9			**1.0**	0.5	0.3	**0.8**	0.1	41.2
Microchiroptera	0.1	0.2	0.2	0.3		**0.8**	0.1		2.3	0.7	**3.1**						0.1	0.1	**0.2**		4.1
Chiroptera	0.3	13.9	11.3	6.6	0.8	**32.9**	16.1	1.6	42.1	9.3	**69.1**	0.3	0.9		0.2	**1.4**	0.6	0.5	**1.1**	4.1	108.6
M. rufescens	7.3	44.6	30.9	32.0	12.7	**127.5**	109.1	12.8	467.8	96.5	**686.2**	4.2	26.5	1.0	0.1	**31.8**	9.2	1.0	**10.2**	0.6	856.3
Rattus	2.5	16.5	20.4	28.8	6.5	**74.7**	40.6	1.2	124.9	27.2	**193.9**	0.1	3.7	0.1		**3.9**	3.8	0.2	**4.0**		276.5
R. exulans		0.8	0.3	1.2		**2.3**	0.1		0.1		**0.2**		0.1			**0.1**					2.6
Mammal (unidentified)	0.8	8.7	7.6	10.9	0.7	**28.7**	13.6	0.4	28.2	7.2	**49.4**	0.1	1.3	0.1	0.2	**1.7**	0.8	0.2	**1.0**		80.8
Rallidae							0.6				**0.6**										0.6
Psittaciformes		0.2				**0.2**	0.1		0.4	0.1	**0.6**										0.8
Passeriformes		0.1				**0.1**			0.8	0.1	**0.9**										1.0
Columbidae					0.2	**0.2**			0.9	0.1	**1.0**	0.2				**0.2**					1.4
Galliformes									0.7		**0.7**										0.7
Non-passerine									0.2		**0.2**			0.4		**0.4**					0.6
Aves (unidentified)		0.3	0.6	1.0	0.2	**2.1**	0.9	0.3	2.2	0.8	**4.2**		0.2			**0.2**					6.5
Boidae	1.3	1.4	4.5	0.9		**8.1**	0.4		0.1		**0.5**										8.6
? *Elapidae*			1.4	0.5		**1.9**		0.6	0.7	0.4	**1.7**							0.2	**0.2**		3.8
Serpentes									0.1		**0.1**										0.1
? *Crocodylus*				0.8		**0.8**			1.1	1.2	**2.3**										3.1
? *Chelonia*		0.5		4.7		**5.2**			2.3	1.5	**3.8**						0.4		**0.4**		9.4
Varanus	0.1	0.2	0.5	2.4		**3.2**	1.0		0.7	1.7	**3.4**										6.6
? *Tiliqua*		0.1	0.2			**0.3**			1.1	0.2	**1.3**	0.1	0.5			**0.6**					2.2
Sauria							0.2		0.3	0.3	**0.8**		0.1			**0.1**	0.1	0.1	**0.2**		1.1
Reptile unident.	0.1	2.9	2.7	1.0	0.1	**6.8**	0.4		1.4	0.9	**2.7**		0.4			**0.4**	0.6	0.1	**0.7**		10.6
Salientia	0.1	1.2	0.3	0.6	0.2	**2.4**	3.2	0.5	8.0	3.1	**14.8**		0.2			**0.2**					17.4
Osteichthyes	0.2	0.6		2.1		**2.9**	1.0		0.9	0.6	**2.5**	0.1				**0.1**	0.1		**0.1**		5.6
Unidentified	0.3	10.9	4.9	9.4	0.1	**25.6**	9.5	1.3	11.7	2.8	**25.3**		0.7	0.4		**1.1**	1.1	1.4	**2.5**		54.5
Total	14.7	134.9	104.0	126.2	22.5	**402.3**	219.9	20.9	786.8	162.0	**1189.6**	5.3	37.2	2.0	0.5	**45.0**	18.6	5.2	**23.8**	8.0	1668.7

Table 14 *Counts (NISP) of vertebrate taxa by stratigraphic unit and analytical unit. Note that Megachiroptera includes the two species of* Nyctimene *and* M. melanops; *Microchiroptera combines* H. diadema *and* H. maggietaylori; *Rattus combines* R. praetor, R. mordax *and* Rattus *sp.; and Sauria includes the Gekkonidae and Scincidae.*

Species	Stratigraphic Unit 1	2	3	4	5	**Anal. Unit A Total**	6	7	8	9	**Anal. Unit B Total**	10	11	12	13	**Anal. Unit C Total**	14	15	**Anal. Unit D Total**	16	Total
H. sapiens sapiens		1			1				2		**2**										3
P. orientalis	1	11	10	7	1	**30**	15	2	32	3	**52**		1			**1**					83
T. brunii			1	5		**6**															6
P. neohibernicus	2	3	2	4		**11**	4		2	2	**8**										19
D. moluccensis	3	119	25	42	4	**193**	37	2	149	21	**209**	1	4			**5**	7	8	**15**	16	438
Megachiroptera	7	10	6	28	3	**54**	44	1	300	44	**389**	1	10			**11**	4	3	**7**	1	462
Microchiroptera	1	2	2	3		**8**	2		20	8	**30**						1	1	**2**		40
Chiroptera		175	62	67	14	**321**	156	20	605	119	**900**	3	16		4	**23**	10	11	**21**	44	1309
M. rufescens	80	483	334	359	117	**1373**	1164	119	5044	1056	**7383**	30	269	14	1	**314**	95	11	**106**	5	9181
Rattus	13	102	105	162	24	**406**	202	10	779	195	**1186**	1	23	1		**25**	27	2	**29**		1646
R. exulans		9	4	14		**27**	1		1		**2**		1			**1**					30
Mammal unident.	4	100	63	100	13	**280**	121	7	621	111	**860**	1	39	12	24	**76**	15	6	**21**		1237
Rallidae								1				1									1
Psittaciformes		2				**2**	1		3	1	**5**										7
Passeriformes		1				**1**			6	1	**7**										8
Columbidae					1	**1**			4	1	**5**	1				**1**					7
Galliformes									1		**1**										1
Non-passerine									2		**2**			1		**1**					3
Aves unident.		3	5	5	1	**14**	4	1	20	5	**30**		2			**2**					46
Boidae	4	3	6	1		**14**	1		1		**2**										16
? *Elapidae*			6	4		**10**		2	3	2	**7**							2	**2**		19
Serpentes									1		**1**										1
? *Crocodylus*				2		**2**			1	4	**5**										7
? *Chelonia*		2		4		**6**			4	2	**6**						1		**1**		13
Varanus	1	2	2	6		**11**	6		4	5	**15**										26
? *Tiliqua*		1	1			**2**			6	1	**7**	1	3			**4**					13
Sauria							2		3	3	**8**		1			**1**	1	1	**2**		11
Reptile unident.	1	17	11	11	1	**41**	4		8	8	**20**		4			**4**	4	1	**5**		70
Salientia	1	15	3	7	2	**28**	37	5	109	47	**198**		2			**2**					228
Osteichthyes	2	4		12		**18**	5		9	3	**17**	1				**1**	1		**1**		37
Unidentified	3	84	47	71	1	**206**	72	9	193	88	**362**		11	5		**16**	7	8	**15**		599
Total	126	1148	696	914	182	**3066**	1879	178	7933	1730	**11720**	40	386	33	29	**488**	173	54	**227**	66	15567

Table 15 *Minimum number of individuals (MNI) for vertebrate fauna by stratigraphic unit. Note that Megachiroptera includes the two species of* Nyctimene *and* M. melanops; *Microchiroptera combines* H. diadema *and* H. macggietaylori; *Rattus combines* R. praetor, R. mordax *and* Rattus *sp.; and Sauria includes the Gekkonidae and Scincidae.*

Species	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
H. sapiens sapiens			1					1									2
P. orientalis	1	3	2	1	1	1	1	4	1		1						16
T. brunii			1	1													2
P. neohibernicus	1	1	1	1		1		1	1								7
D. moluccensis	2	17	5	6	1	7	2	18	4	1	1			2	4	4	74
Megachiroptera	3	3	1	6	1	10	1	72	8	1	3			1	2	1	113
Microchiroptera	1	1	1	3		2		5	3					1	1		18
Chiroptera	1	3	1	1	1	3	1	9	2	1	1		1	1	1	1	28
M. rufescens	15	85	63	65	24	203	25	798	170	7	38	5	1	14	3	1	1517
Rattus	2	12	19	17	4	27	2	76	77	1	3	1		3	1		245
R. exulans		5	2	6		1		1			1						16
Rallidae						1											1
Psittaciformes		1				1		1	1								4
Passeriformes		1						2	1								4
Columbidae					1			1	1	1							4
Galliformes								1									1
Non-passerine								1				1					2
Boidae	1	1	1	1		1		1									6
? *Elapidae*			1	1			1	1	1						1		6
Serpentes								1									1
? *Crocodylus*				1				1	1								3
? *Chelonia*		1		1				1	1					1			5
Varanus	1	1	1	1		1		1	1								7
? *Tiliqua*		1	1					1	1	1	1						6
Sauria						2		1	1		1			1	1		7
Salientia	1	5	2	2	1	6	1	25	14		1						58
Osteichthyes	1	1		1		1		1	1	1				1			8

the least abundant (MNI=2). Its remains, restricted to Stratigraphic Units 3 and 4, were deposited during the last 1000 years of occupation. A cuscus, *Phalanger orientalis*, is the second largest land mammal by weight (c.2 kg) but is also a minor element in the assemblage (MNI=14). A single bone at 13,000 BP (Stratigraphic Unit 11) marks its first appearance in the site, and from Stratigraphic Unit 9 onwards it is present in all levels.

Bats

Bats are represented by five genera and seven species. The largest fruit bat by weight, the forest dwelling *Pteropus neohibernicus*, (1-2 kg) occurs in small numbers in Stratigraphic Units 1-9. The other large identified Megachiroptera, *Dobsonia moluccensis*, which weighs up to 500 g, is commonly found roosting in large colonies in caves, the twilight zone of the vestibule being the favoured area (Smith and Hood 1981:91). It is common in New Ireland and is extremely abundant in this deposit, being represented in most stratigraphic units.

The postcranial bones of the small fruit bats – *Nyctimene major, Nyctimene* sp. and *Melonycteris melanops* – could not be distinguished. *M. melanops* appears to be restricted to the Bismarck Archipelago and is locally abundant on New Ireland (Smith and Hood 1981:112). All three are most common in Stratigraphic Units 1-9 and none are reported to roost in caves. The two Microchiroptera, *Hipposideros diadema* and *H. maggietaylori*, are both small cave roosting bats, the latter preferring to roost in deeper portions of caves on dome shaped ceilings (Smith and Hood 1981:106). These are also most abundant in Stratigraphic Units 1-9, but are relatively minor taxa in this assemblage.

Rodents

The small rodent *Melomys rufescens*, (90 g) dominates the assemblage, accounting for some 9000 bones or 56% of the total by number (MNI=1518). Its remains are found throughout the deposit in variable quantities but like many other taxa it is most abundant in Stratigraphic Unit 8. The genus *Rattus* is represented by at least three species. Their distributions according to mandibles and maxillae are shown in Table 16. The two largest, *R. praetor* and *R. mordax* (200-500 g) are both spinous lowland rats which have both mainland Papua New Guinean and island Melanesian representatives (see Taylor et al. 1982). They are closely related and were distinguished on mandibular and maxillary teeth and alveolar measurements (Tim Flannery pers. comm., and see Taylor et al. 1982). The few mandibles which did not fall into the range of either species were designated *Rattus* sp.

Table 16 *Distribution of* Rattus *by stratigraphic unit.*

Stratigraphic Unit	*R. praetor*	*R. mordax*	*R. exulans*	*Rattus* sp.
1	x			
2	x		x	
3	x		x	x
4	x	x	x	
5		x		
6	x	x	x	
7				
8	x	x	x	
9		x		
10				
11	x		x	
12				
13				
14		x		
15				
16				

R. praetor is a human commensal and inhabits forest, grasslands and gardens. It first appears in Panakiwuk in Stratigraphic Unit 11 (c.13,000 BP) from which time on it is common in the site. *R. mordax* first appears in Stratigraphic Unit 14 (>c.15,000 BP) and continues until Stratigraphic Unit 4 (c.1600 BP). It has a similar habitat range to *R. praetor* but since it is not known to occur on New Ireland now, it is believed to be extinct there. The postcranial bones of these two large rodents were too similar in overall size to be confidently assigned to either species and so were grouped. In combination these two large rodents are represented by at least 246 individuals making them the second most abundant taxon.

The third species of *Rattus* present in the site, *R. exulans*, is the smallest and least abundant. It is also a human commensal and is found in archaeological contexts throughout the Pacific region. It is commonly assumed to be a late Holocene human introduction into the Pacific, so that the one bone of this species in Analytical Unit C and the two further bones in Analytical Unit B are most parsimoniously considered to be derived from more recent levels. If this is so, this is one of the very few instances of vertical displacement that we can confidently identify in the site. If they are not displaced they are the earliest examples of *R. exulans* in Melanesia.

Birds

Avifaunal remains are uncommon. They have been identified only to a general level, representing members of the rail, parrot, pigeon and fowl families, and other perching and non-perching birds. Their known habitats vary from forest to marshlands or swamp. In Panakiwuk, bird bones occurred most frequently in Stratigraphic Units 8 and 9.

Reptiles

A wide range of reptile bones are present, all in small quantities. Both families of snake may be present, but the Elapidae is a tentative identification based on vertebral measurements. Other than two pieces from Unit 15, these are wholly restricted to Stratigraphic Units 1-9. Lizards occur as varanids, large and small skinks and a small gekko. The varanids and the large skinks occur as both cranial and postcranial bones. The varanids have the highest bone count of the identified reptiles.

A marine component is represented by tentatively identified crocodile and sea turtle. Like all reptile remains, these are most common in Stratigraphic Units 1-9.

Frogs and fish

As with the avifauna these are identified only to a general level. Frogs are very abundant in the upper levels, and almost 50% of individuals derive from Stratigraphic Unit 8. They are represented by cranial and post-cranial bones. The fish are all bony types and are uncommon throughout the deposit.

Skeletal element frequencies

Although skeletal element counts were tallied for all taxa, only those for the three most abundant, *M. rufescens* (Table 17 and Fig. 9), the large *Rattus* (Table 18 and Fig. 10) and *D. moluccensis* (Table 19 and Fig. 11) are presented here, and only those stratigraphic units which contained more than 20% of the total range of elements are included. This excludes only the smaller samples: for *M. rufescens*, Stratigraphic Unit 13 only; for large *Rattus*, Stratigraphic Units 10, 12, 13, 15 and 16; and for *D. moluccensis*, Stratigraphic Units 1, 5, 7, 10-13 and 15.

M. rufescens shows a remarkable internal consistency in its patterns of element frequencies throughout the deposit, in spite of variable sample sizes. In all assemblages pelves, humeri and the two large hind limb bones, tibiae and femora, are most abundant. Notable by their low frequencies are cranial remains and vertebrae, and especially mandibles and maxillae. Since these are two of the most robust skeletal elements their low quantities are hard to explain. Overall, however, the element frequencies demonstrate the deposition of whole or near whole animals.

In comparison, the frequencies for large *Rattus* show a fuller representation of elements which are more variable through the deposit. Vertebral and cranial bones occur in relatively low quantities, and hind limbs dominate. Again, mandibles and especially maxillae are relatively rare. Tarsals however are surprisingly abundant. Like *M. rufescens*, the deposition of whole animals is inferred from this pattern.

Unlike the rodents, *D. moluccensis* body parts show a bias towards the fore limbs, especially the humeri and radii. It should be noted however, that these long bones are vulnerable to breakage, thus this bias may be due to fragmentation. Like the rodents, there is a paucity of vertebrae and cranial bones, although again the overall pattern supports the deposition of whole animals.

Interpreting the faunal remains

Bone Distribution

Bone deposition is greatest towards the rear of the shelter, especially in Analytical Unit B. One explanation for such a distribution pattern is site maintenance activities, whereby animal remains

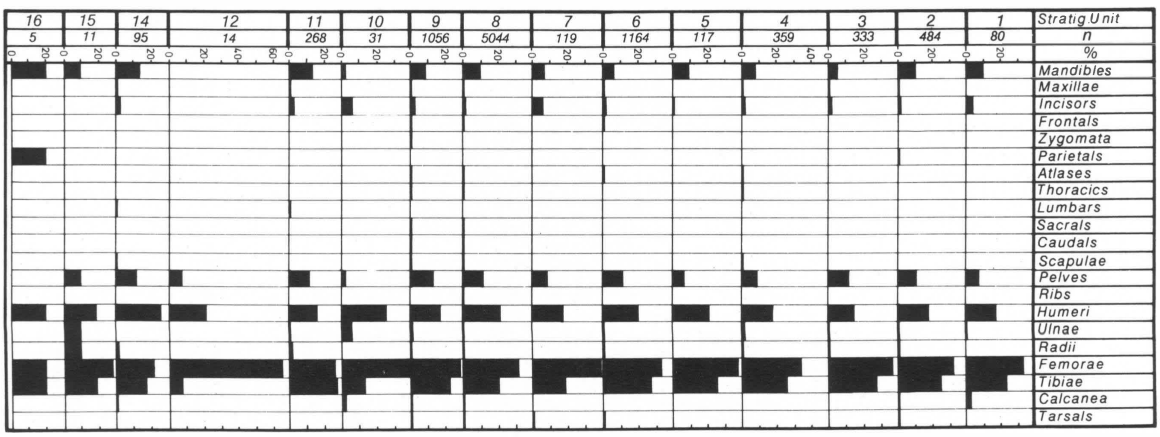

Figure 9 *Percentage frequencies of* Melomys rufescens *skeletal element counts.*

Table 17 Melomys rufescens *skeletal element counts by stratigraphic unit.*

Element	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
Mandibles	8	47	17	29	11	68	8	519	91	1	36			12	1	1	849
Maxillae		2	1	1		4		11	8		2			1			30
Incisors	3	8	5	6	1	18	7	99	32	2	8			3			192
Frontals						1		2	2								5
Zygomata									2								2
Parietals		2														1	3
Atlases				1		5		5	1								12
Thoracics				1				2	1								4
Lumbars											1			1			2
Sacrals								5	3								8
Caudals								2	1								3
Scapulae				1				7	1					1			10
Pelves	6	54	40	32	8	125	11	588	136	1	30	1		10	1		1043
Ribs								1									1
Humeri	14	85	53	66	24	231	22	1055	195	8	42	3	1	25	2	1	1827
Ulnae	1	5	1	6		7		56	21	2	1				1		101
Radii		2	2	1		7		19	9		6			2	1		49
Femorae	26	156	122	123	44	384	48	1590	308	12	69	9		22	3	1	2917
Tibiae	19	121	92	92	29	312	22	1077	242	4	73	1		17	2	1	2104
Calcanea	3	1						3	2	1				1			11
Tarsals		1				2	1	3	1								8
Total	80	484	333	359	117	1164	119	5044	1056	31	268	14	1	95	11	5	9181

Table 18 *Large* Rattus *skeletal element counts by stratigraphic unit.*

Element	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
Mandibles	2	7	5	15		7		27	9		3						75
Maxillae		1	2	2	1	1		20	3					2			32
Premaxillae			1					5	5								11
Incisors		5	5	11		3	1	48	12					1			86
Nasals									2								2
Frontals		1				1		12	8								22
Zygomata						1		7			1				1		10
Parietals								6			1			1			8
Occipitals								1									1
Bullae								1	1								2
Atlases						1			1					1			3
Cervicals								1	1								2
Thoracics	1					3	1	2			1						8
Lumbars		2	2			2		9	2								17
Sacrals	1		1	4	2	2		11	5					1			27

Caudals		2				9		15									26
Clavicles						1		6	1		1						9
Scapulae	2	3	2			5		13	2								27
Pelves		14	14	25	5	13		54	9		1			4	1		140
Ribs		1		1				2	3								7
Humeri	1	13	16	28		32	1	100	23		2			3			219
Ulnae	1	12	9	8	5	7	2	53	13		2	1		3			116
Radii		1	2	3	2	6		46	14	1				1			76
Femorae	4	24	27	25	5	49	1	121	15		4			3			278
Tibiae	1	8	11	28	4	27	1	143	14		3			4			244
Fibulae								2	1								3
Calcanea		3	1	4		7	1	19	10					1			46
Tarsals		5	7	8		25	2	55	41		4			2			149
Total	13	102	105	162	24	202	10	779	195	1	23	1	0	27	2	0	1646

Table 19 Dobsonia moluccensis *skeletal element counts by stratigraphic unit. Note that the only crania included here are complete ones.*

Element	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
Crania		2						1									3
Mandibles		14	1	9		6		15	1		1			1		2	50
Maxillae	2	3						2			1						8
Incisors						1											1
Canines		1															1
Molars						1											1
Zygomata								1									1
Atlases		2						1									3
Cervicals		3															3
Thoracics								1									1
Lumbars				1													1
Sacrals		1						1									2
Clavicles		9	3	6		2		13	2	1	1				1	4	42
Sternae		5				1		10	1					1			18
Scapulae		8		2				7	2								19
Pelves		1						3									4
Ribs								1									1
Humeri	1	21	9	10	1	13		31	5					2	5	5	103
Ulnae		1						3						1			5
Radii		24	6	8	2	5	2	21	4		1			1	1	1	76
Femorae		6	2		1	5		14	1								29
Tibiae		2							1							2	5
Metatarsals		16	4	5		3		24	4					1	1	2	60
Claws				1													1
Total	3	119	25	42	4	37	2	149	21	1	4	0	0	7	8	16	438

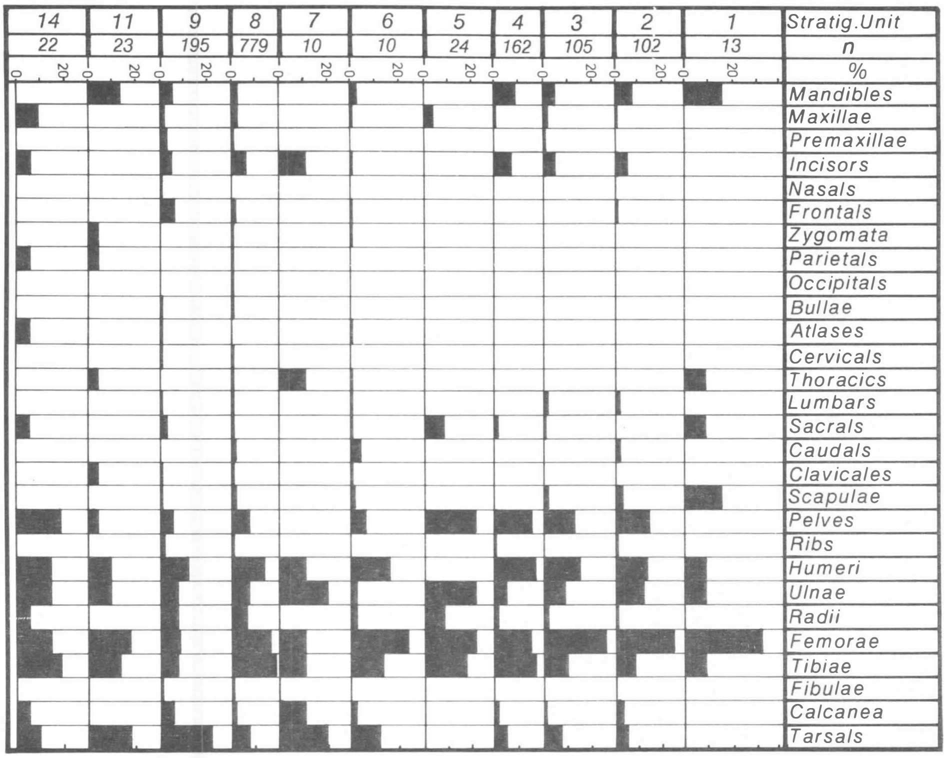

Figure 10 *Percentage frequencies of large* Rattus *skeletal element counts.*

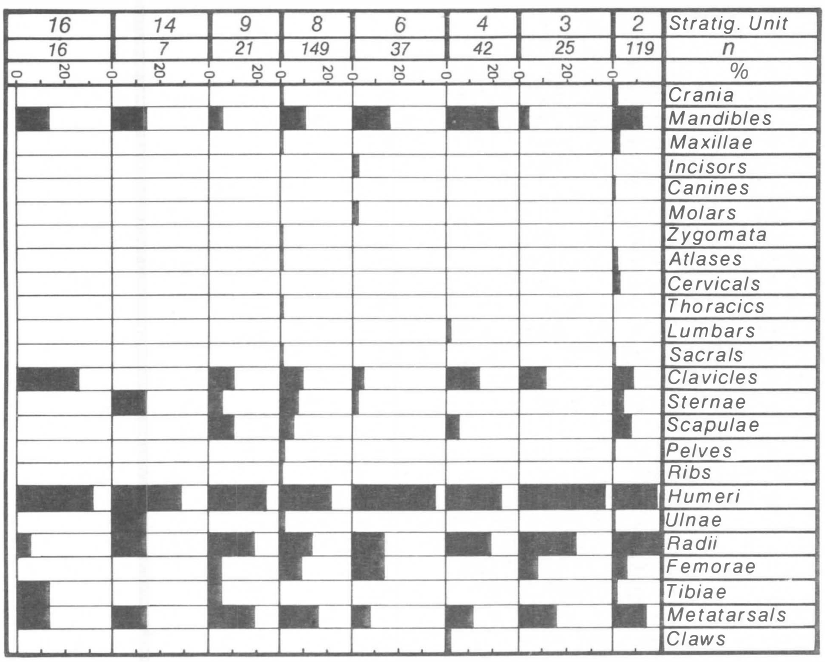

Figure 11 *Percentage frequencies of* Dobsonia moluccensis *skeletal element counts.*

and other debris were dumped or swept away from the middle of the floor and towards the back wall of the cave. The amount of living space available to the occupants of Panakiwuk would have been small throughout the accumulation of the deposit, a condition which would have encouraged such clearing.

Related to this is the paucity of wallaby and phalanger bones, the two largest animals. The phalanger in particular is common in Balof 2 (White et al. this volume), Matenkupkum (Gosden and Robertson this volume) and in Matenbek (Allen et al. 1989:557). This relative absence is intriguing in Panakiwuk since it cannot simply be due to differential post-depositional preservation. A parsimonious explanation is that the animals were simply not being hunted, either because they were absent in the area or relatively few in number. For *T. brunii* this may be the case during the earlier phase of occupation. Its presence in New Ireland is probably attributed to human introduction, and according to the Balof 2 sequence this did not occur until c.6000 BP. However, this does not account for the lack of wallaby remains in the most recent levels, nor the low frequencies of phalanger remains throughout. In the case of *P. orientalis* it appears that this animal was at least available throughout all but the earliest pre-13,000 BP period of Panakiwuk's occupation. Again, this pattern may simply reflect site maintenance activities. A testable hypothesis is that the doline at the front of the cave, which would fall within the easy toss zone of the site, was being used as a convenient rubbish dump for the bones of these larger species. However, while we do not doubt that material was thrown into the doline, we remain uncomfortable with this explanation for the absence of these bones; they are not so significantly larger than other classes of debris in the site to warrant such obsessive disposal.

Agents of accumulation

Rodents

The predominance of rodents at Panakiwuk argues that these are at least partly due to raptor pellet accumulation. This notion finds support in the excellent preservation of the bone and, as demonstrated by the skeletal element frequencies, the deposition of whole animals. Most specifically it was indicated during excavation by the recovery of two cemented pellets of bone from the site.

Evidence for a human derivation for the rodent bone is its consistent association and general similarity in frequency distribution with most classes of artefactual material. However, these may be merely depositional associations as opposed to human ones which see small rodents as a frequently hunted prey species. Allowing some human input, it is not unreasonable to expect some mixing of bone from cultural and non-cultural sources, especially if human occupation has been sporadic. While there is evidence for some movement of rodent bone between layers, one would have to invoke both an extreme and peculiar mixing of materials from separate episodes of accumulation to explain the strong association of rodent bone and artefactual remains. Even if raptors and humans were most active during the same time periods, we would expect at least some alternation in assemblage composition unless again there has been mixing of bone (such as might result from its redeposition via the cleaning of living space).

A second line of evidence for a cultural origin for the rodent bone is the element frequency patterns. The known patterns of element frequencies from raptor pellets according to experimental studies and observations on natural accumulations in caves show the presence of large quantities of mandibles, maxillae and vertebrae. This is in stark contrast to the patterns observed in the present analysis which do not match the expected frequencies from any such studies.

On the other hand, the use of rodent element frequencies for distinguishing between humans and raptors as sources of bone is methodologically weak. There is ethnographic evidence for human consumption of small mammals in the New Guinea Highlands, where it has been noted that 'all the bones, and the skull but not the mandibles are usually eaten' (Dwyer 1980:110). Although such observations may provide circumstantial explanations for the relatively high count of rodent mandibles at Panakiwuk, the rodent element frequencies in archaeological contexts which may result from human consumption remain unknown and even if known for specific cases, could not reasonably be generalised across sites. Besides, such observations do not account for the overall element frequency patterns recorded at this site.

The common practice of arbitrarily separating the prey of raptors from the remains of human meals according to animal size is especially problematic in this context since most of the mammal fauna at this site and on New Ireland generally is relatively small and mainly falls within the size range of prey taken by both raptors and humans. Regardless, the edible-waste ratios for small mammals such as rodents are high and, as noted by Stahl (1982), this 'in combination with their abundance, particularly in those areas associated with human activity, suggests that many small mammals... do represent a potentially rich food source' (Stahl 1982:826; also see Dwyer 1980:109)

Although some of the rodent and other small animal bone is charred, we do not cite this as evidence for a cultural origin since it may be incidental burning. While burnt bone in this site is certainly evidence of human activity, it need not imply human processing or consumption of particular animals.

Of the four rodents at Panakiwuk, we consider that *M. rufescens* is the species most likely to have been accumulated by raptors because of its relatively small size and superabundance. Furthermore, its preferred habitats of forest edges and clearings and forest outliers (Menzies and Dennis 1979:38) are those in which birds of prey generally hunt. This species also appears to be less commensal with human occupation than the other rodents. In comparison, *R. praetor* is clearly commensal with human occupation, *R. mordax* less so. The larger size of both of these species, and their greater availability within or adjacent to human habitation sites argues that of the rodents these are the most likely to be human prey. Like *M. rufescens*, *R. exulans* is a small rodent but also is essentially an inhabitant of humanly modified enviroments (see Dwyer 1978; Taylor et al. 1982 and refs). It is not common in the site, and while not necessarily the remains of human meals, its presence is ultimately due to human activity. At this stage we consider that natural bone accumulation probably occurred throughout the deposition of the cultural bone at Panakiwuk and that the rodent fauna in particular represents a variable combination of both raptorial and cultural agents.

Bats

As noted above, the most commonly identified bat is the cave roosting *D. moluccensis* and its element frequencies demonstrate the deposition of whole animals. While the simplest explanation for these patterns is natural death of roosting animals, they could equally reflect human predation, especially if Panakiwuk is considered a source of these animals (for an example see Menzies 1977:335).

Towards a formational history

Figure 12 presents a formational history of the faunal deposits at Panakiwuk according to their human and probable non-human sources for each analytical unit. The reconstruction assumes that post-depositional preservation has been similar through time. This finds support in the presence of small and relatively fragile bones throughout the sequence.

The first or pre-15,000 BP phase, Phase D (to which we here add the sample from Stratigraphic Unit 16) sees little bone deposition generally and apparently very little at all from human activity, although the minor presence of fish bone in Stratigraphic Unit 14 is apparently reflecting humans. Natural mortality of resident animals, particularly roosting bats and small lizards, is a major source of bone during this phase as are the pellets of raptors for the bones of rodents and other small animals.

Between 15,000 BP and 13,000 BP (Analytical Unit C) natural bone deposition continues but the human consumption of animals at the site and subsequent discard of bone increases. Human hunting activity is mostly restricted to forest prey which now includes birds, large skink lizards and possibly the large rats. A single *Phalanger* bone occurs in the upper stratigraphic unit in this zone. All of these animals were probably available within the immediate vicinity of the cave. Limited use of marine resources continues.

Analytical Unit B, dating from 10,000 BP to 8000 BP, is the period of greatest bone deposition and faunal diversity. Added to the faunal suite of the previous phase are several large prey items, such as the *Pteropus*, boid snakes and varanids and a wider range of birds including fowls and rails. With this diversification of human terrestrial prey we see a co-occurrence of crocodile, turtle and fish which together with the shellfish remains point to a more intensive utilisation of marine and coastal resources. Because of the association of rodent remains with these animals, it is in this analytical unit that at least part of the rodent fauna, especially *R. praetor*, and *R. mordax* and the cave roosting *Dobsonia*, are most likely human prey as well as the prey of raptors or the result of natural mortality. Whatever the source of such bone, its spatial distribution suggests its concentration, due to dumping or sweeping, towards the rear of the shelter.

The latest phase, Analytical Unit A (1600 BP to 640 BP), sees a slight decrease in the range of forest species, especially birds, but the appearance of the largest mammal in this site, the *Thylogale*. The deposition of bone from natural sources continues although small resident lizards appear to be absent.

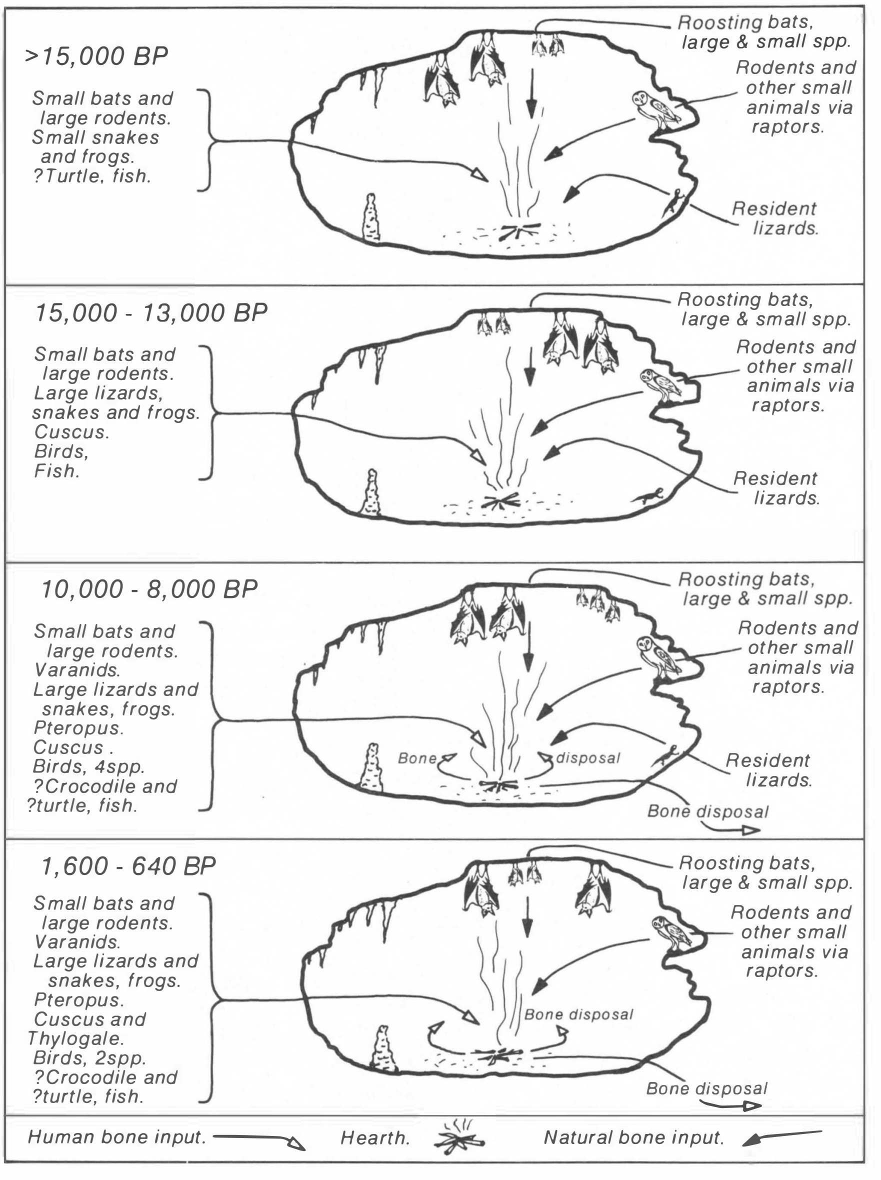

Figure 12 *A pictorial representation of the formational history of faunal deposition at Panakiwuk, indicating human and probable non-human sources for each analytical unit. See text for a fuller discussion.*

Shellfish and Crustacea

Shellfish remains from all the central squares were sorted according to the taxonomic listings provided for samples identified by Phil Colman, The Australian Museum, Sydney. Attempts were made to consider habitats, to see, for example, whether we could differentiate species brought to the site from the west or east coasts of New Ireland, but this proved impossible. All of the marine shells probably come from the intertidal areas of the reef and the microenvironments they contain.

Identifications vary between family, genus and species levels. Some 18 families of marine shells including both bivalves and gastropods have been identified, one genus of freshwater gastropods (*Neritina*) and four families of landsnails. Table 20 lists the identified shells. Analysis consisted of counting and weighing according to category (to species if possible, otherwise to a higher classification). All identifiable fragments were counted, and no attempt was made to calculate minimum numbers. While this fails to account for variable fragility, we do not believe that this bias was particularly great, except with the landsnail shells. Weights of shells by category were also recorded in order to overcome the biasses of differential fragmentation of different species. Whole shell numbers by category were also recorded.

Table 20 *Panakiwuk shells, identified to family, genus and species levels where possible. The bottom four families are landsnails. In addition to this list, shells of the Order Chitonida were also present.*

Family	Genus	Species
Angariidae	*Angaria*	*delphina*
Arcidae	*Barbatia*	*granosa*
	Anadara	
Buccinidae	*Cantharis*	*fumosus*
Bursidae		
Cerithiidae		
Corbiculidae	*Polymesoda*	*coaxans*
Cypraeidae	*Cypraea*	*annulus*
		moneta
Ellobiidae	*Pythia*	
Mesodesmatidae	*Atactodea*	*striata*
Mytilidae	*Modiolus*	*auticulatus*
Nassariidae	*Nassarius*	*concinnus*
		delicatus
Neritidae	*Nerita*	*plicata*
	Neritina	
Pteriidae		
Strombidae	*Strombus*	*mutabilis*
	Lambis	*lambis*
Thiaridae	*Melanoides*	
	Thiara	
Tridacnidae		
Trochidae	*Trochus*	*maculatus*
		incrassatus
Turbinidae	*Turbo*	*marmoratus*
		argyrostoma
Camaenidae	*Chloritis*	
	Papuina	
Cyclophoridae		
Helicanonidae	*Nesonanina*	
Trochomorphidae	*Coxia*	

Among the marine shells the consistently most common species is the bivalve *Polymesoda coaxans*. Both the fracture patterns of this species and indications of use along the edges of a number of its representatives in the site suggest that this shell was commonly used as a tool.

Other worked shell items in the site include pieces of a nacrous shell, possibly Pteriidae, ground on both surfaces; pieces of flaked *Tridacna*; the cut-out base sections or mouths of small cowries, almost certainly all from either *Cypraea moneta* or *C. annulus* (the only identified cowries in the site); and a single shell bead. All worked items derive from Analytical Unit A.

For Panakiwuk we concluded that it was a sufficient analytical device to divide the shell material into four categories, marine, fresh water, landsnail and crustacea (crab, salt water crayfish and echinoid, the latter being entirely represented by spines). Raw numbers and weights are given by stratigraphic units according to these divisions in Table 21. The startling distribution is that apart from a single piece of marine shell in Stratigraphic Unit 13 (the surface of Analytical Unit D, c.15,000 BP) which must be thought likely to have fallen in from higher up the section, all classes of shell material continue into Stratigraphic Unit 8 but none are found in Stratigraphic Unit 9 or lower.

The first point to make about this distribution is that it definitely crosses the chronological hiatus between the organic-rich layers of Analytical Unit A and the reddish soils of Analytical Unit B. In terms of the stratigraphic units this hiatus is between Stratigraphic Units 5 and 6 and shell materials continue down to the middle of Stratigraphic Unit 8 in quantity. We believe the shells are in situ in these Analytical Unit B layers, and not trodden in from the overlying organic-rich deposits, partly because of the quantities involved and particularly because these same organic soils lie directly over the clay wedge layers which contain in them not a single shell.

The second consideration is whether the absence of shell remains from the lower part of Stratigraphic Unit 8 and below is a genuine absence or merely the reflection of differential preservation. We can observe that the change does not occur between layers of distinctly different character, nor between areas of different soil acidity (pH levels are 8-9 throughout). As already discussed, hundreds of well-preserved small rat and other bones continue in the lower part of Stratigraphic Unit B and down to the bottom of the cultural layers. It can also be seen that when the totals of counts and weights are corrected for volumetric variations between units (Table 22) then the data present themselves in the

Table 22 *Volumetrically corrected shell numbers and weights (per m³) for Squares A6S and Z6S.*

Stratigraphic Unit	Marine		Freshwater		Landsnail		Crustacea		Total	
	No.	Wt (g)	No.	Wt (g)	No.	Wt (g)	No.	Wt (g)	No.	Wt (g)
1	2603	652	286	100	1030	169	57	14	3975	935
2	3370	1568	365	204	1339	322	101	24	5174	2117
3	3619	3877	611	357	2915	456	455	180	6260	4871
4	4845	3290	765	713	1155	230	300	87	7065	4319
5	2243	650	460	288	1035	305	58	12	3795	1254
6	1520	1382	365	239	380	111	91	17	2356	1748
7	1174	1093	206	70	1264	279			2645	1441
8	132	58	28	16	1096	188	24	7	1280	268

sorts of distributions which fit an explanation of introduction and human use patterns rather than differential preservation: in both Analytical Units B and A, separated by some six millennia, the earliest layers have the lowest scores and then follow regular distribution patterns (increase in Analytical Unit B, and increase followed by decrease in Analytical Unit A). On the other hand it seems more than coincidental that marine, fresh water and landsnail shells are all introduced into the site at the same time.

Shell remains, and particularly marine shell remains, are much more numerous in Analytical Unit A than Analytical Unit B. Beyond this, species analyses indicate few exploitation or preference changes between the two Holocene periods represented at Panakiwuk; both indicate generalised reef collection of marine species and the collection of *Neritina* from the nearby river. The landsnails present a number of unanswered questions. Do they simply appear in the local environment as a reflection of subtle vegetation changes at the end of the Pleistocene? Are they eaten? Are they inadvertently carried into the site on vegetable materials? Each suggestion raises other considerations in the data. If they arrive on vegetation, was this not previously brought into the site? If they get into the site during periods of human disuse, as previously suggested for Balof 2 (Peter White pers. comm.) why do we find a positive correlation between densities of landsnails and periods of intense human usage in the site?

Pottery

Including the surface piece noticed in 1984, we recovered only four small pieces of pottery in Panakiwuk. All were low fired and shell tempered body sherds and measured only c.2 cm in maximum dimension. Three were plain sherds and one was decorated with fingernail impressions. All three excavated pieces derived from Analytical Unit A.

Plant Remains

Douglas Yen identified 42 fragments of plant remains (Table 23). Additional pieces of bark and wood could not be identified further. The collection consists of stems, shells and nuts representing *Canarium, Pangium, Celtis, Cocos,* and one piece likely to be either sugarcane or bamboo. A number of the pieces of *Canarium* are likely to be *C. indicum.* Distributions are largely, but not totally confined to Analytical Unit A. While differential preservation seems likely, we do not totally reject the notion that it might also be reflecting different site uses.

Table 21 *Panakiwuk shell distribution by number of pieces and weight. Whole shell counts are in brackets and are included in the total count.*

Stratigraphic Unit	Marine No.	Marine Wt (g)	Fresh Water No.	Fresh Water Wt (g)	Land Snail No.	Land Snail Wt (g)	Crustacea No.	Crustacea Wt (g)	Unidentified No.	Unidentified Wt (g)	Total No.	Total Wt (g)
1	91(3)	22.8	10(6)	3.5	36(0)	5.9	2(0)	0.5	33(0)	6.5	172(9)	39.2
2	858(21)	405.0	102(68)	58.0	357(10)	74.9	29(0)	6.4			1346(99)	544.3
3	757(58)	575.9	110(61)	66.1	203(5)	53.2	66(0)	23.8	2(0)	0.2	1138(124)	719.2
4	667(48)	487.3	130(82)	104.6	179(12)	35.7	40(0)	10.1	5(0)	1.2	1021(142)	638.9
5	7(2)	6.2	5(4)	2.5	9(2)	2.6	1(0)	0.2			22(8)	11.5
6	342(23)	252.9	77(51)	29.9	220(5)	47.1	19(0)	4.6	9(0)	9.5	667(79)	344.0
7	40(1)	13.4	7(5)	3.0	15(2)	5.6	1(0)	0.2			63(8)	22.2
8	39(4)	18.9	8(6)	4.1	290(8)	41.7	7(0)	2.3	2(0)	0.2	346(18)	67.2
9												
10												
11												
12												
13	1(0)	1.5									1(0)	1.5
14												
15												
Total	2802(160)	1783.9	449(283)	271.7	1309(44)	266.7	165(0)	48.1	51(0)	17.6	4775(487)	2388.0

Table 23 *Panakiwuk plant remains, identified by D.E. Yen from carbonised samples. Confidence levels move from high (5) to low (1).*

Square	Spit	Stratigraphic Unit	Part and Species	Number of Fragments	Confidence Scale
A5	1	1	Stem/Gramineae (sugar/bamboo)	1	2
A5	2	1	Shell/*Canarium* (*indicum?*)	2	3
	2	1	Stem/*Bambusa*	1	4
	2	1	Shell/*Cocos*	2	2
A5	4	2	Shell/*Cocos*	3	5
A5	6	4	Nut/*Celtis* sp.	1	5
A5	8	6	Shell/*Cocos*	1	3
	8	6	Nut/*Celtis*	1	5
Z5	1	1	Nut/*Celtis* sp.	1	5
Z5	5	4	Shell/*Canarium*	3	5,4
Z5	6	4	Shell/*Cocos*	3	4
Z5	11	3	Shell/*Cocos*	1	4
Z5	12	6	Shell/*Canarium* (wild form?)	1	2
A6S	4	2	Shell/*Canarium*	2	4
Z6S	8	6	Shell/*Canarium indicum*	3	4
Z6S	12	8	Nut/*Celtis* sp.	1	5
Y2	2	?	Nut/*Pandanus*	1	2
Y2	3	?	Shell/*Pangium*	1	4
Y2	4	?	Nut/*Celtis* sp.	8	5
	4	?	Shell/*Canarium*	4	5,3
Y2	6	?	Shell/*Canarium*	1	3

A fragment of the nut *Celtis sp.* occurs in Stratigraphic Unit 8 (as well as higher in the site) and this nut is also commonly represented in the Pleistocene deposits at Matenkupkum (Gosden and Robertson this volume). One piece of *Canarium* comes from the topmost layer of Analytical Unit B, that is, c.8000 BP; without knowing its stratigraphic position Yen nominated it as the only piece of *Canarium* which might be a wild form.

Pollen

Leslie Head, Geography Department, University of Wollongong, examined samples from all parts of the sequence for pollen remains and reported negative results, with the exception of an unidentified spore which occurs in varying quantities throughout.

CONCLUSIONS

Fauna

Because a major aspect of the Panakiwuk analysis has involved the vertebrate fauna we begin with a resumé of this class of data. We realise that the data presented have not yet been subjected to any statistical manipulation despite their suitability for such analysis. In lieu of this, the data do provide a sound basis for detailed inter-site comparisons which we consider to be a next step after the full reporting of faunas from the other excavated New Ireland sites.

On present zoogeographical evidence (see Flannery and White 1991 for a summary), the Panakiwuk faunal sequence records the deliberate or accidental human translocation into New Ireland of at least two rodents, *R. praetor* and *R. exulans* and two marsupials, *P. orientalis* and *T. brunii*. It also documents the extinction of a third rodent, *R. mordax*. This aspect of the fauna, together with the variability in the appearance and extinction dates for these animals among the various New Ireland sequences have been discussed elsewhere and do not warrant further comment here (see Allen et al. 1989). In summary however, we note that the Panakiwuk fauna provides evidence for humanly initiated changes which enhanced New Ireland's terrestrial fauna as a resource base and which need be considered in interpretations of site function and the island's settlement history.

As an example, we consider that the availability of *P. orientalis* by at least 10,000 BP 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.

At this stage we consider that the formational history of the faunal deposits generally supports the notion of sparse use of Panakiwuk as a residential site, as indicated by other artefact classes. It appears that the most abundant animals, especially *M. rufescens*, are at least in part the result of natural agents of bone accumulation and that these have been contemporaneous with the formation of the archaeological deposits. In many ways this is not unexpected since Panakiwuk, like other limestone caves, presents a depositional environment conducive to the accumulation and preservation of bone from a variety of sources.

General summary

The purpose of this paper has been to report the excavation and subsequent analyses of the artefacts from Panakiwuk. We have deliberately sought to keep intra-site comparisons to a minimum, partly because one attempt at this (Allen et al. 1989) has recently appeared and partly because a more detailed attempt will require the addition of further information from the New Ireland sites at our disposal, including the as yet unpublished Buang Merabak excavation and the more recent seasons of excavations carried out at Balof 2, Matenkupkum and Matenbek. Thus we wish to conclude this paper only by summarising the principal patterns of past human behaviour reflected at Panakiwuk itself. Having said this, however, we note that Panakiwuk's archaeology contributes directly and in a supportive fashion to the model put forward by Gosden and Robertson (this volume) and to wider notions of developing patterns of interaction during the late Pleistocene and Holocene in the Bismarck Archipelago which are being developed by Gosden and Allen in respect of their more recent work in Southern New Ireland.

In terms of the stratigraphy, while we wish to add nothing to our previous in text discussions, we stress that the depositional history of Panakiwuk and the formational history of its artefactual sequences might in the future undergo modifications in the light of more extensive excavations in the site. On present evidence, however, Panakiwuk was first occupied by humans not long before c.15,000 BP. This date is similar to the initial occupation of Balof 2, but is only half the antiquity of human occupation in Matenkupkum and Buang Marabak. In the light of current discussions elsewhere in the world on whether tropical rainforests were extensively exploited before more intensive forms of plant manipulation were developed (e.g. Hoffman 1984; Ellen 1987) these later initial occupation dates of sites further from the coast may well be reflecting an earlier long period of human occupation in New Ireland which emphasised marine and coastal exploitation and which only rarely or never moved any distance inland from it.

As far as Panakiwuk is concerned occupation seems to have been sparse (and perhaps episodically non-existent) down until c.10,000 BP. It is clear that in the lower half of Analytical Unit B (i.e. Stratigraphic Units 8 and 9) we find the densest concentrations of all classes of artefactual remains. This does not simply appear to be a reflection of a slower accumulation of depositional matrix compared, especially, with the wedge layers in Analytical Unit C. Rather, we interpret this as a more constant use of the site over a shorter period of time than that represented in Analytical Unit C. By 'constant' we imply no choice between more people occupying the site, or the site being occupied more frequently; we have, however, for the purposes of this discussion, disregarded the possibility that this is simply a case of the excavated area of the site being used differently at different periods – a good enough idea, but one which we cannot test in the data available to us.

While we might not be able to convincingly attribute this apparent behaviour change to the appearance of phalangers in the region, particularly given the small numbers of phalanger bones recovered from the site, it is nonetheless true that this animal is represented in every stratigraphic unit from Stratigraphic Unit 9 to the top of the site. Within the limits of the data, as discussed, the evidence implies important changes in the uses of this site at this time and, in turn, wider changes to the patterns of human behaviour in the area.

For example, in terms of the stone tools, not only do the greatest densities occur at this time, supporting the notion of more constant use, but also we see the greatest diversity of industrial components (cores, flakes, hammerstones, retouching, etc.), although it may simply be that density and diversity are closely linked in this instance. We see in these data no obvious alternative explanations, such as that increased flake numbers result from changed core reduction strategies. Overall, here and elsewhere in the site, the preponderance of small and possibly negligibly used flakes and the dearth of cores might suggest that ready supplies of usable stone for artefacts might not be locally available in quantity and that larger flakes and cores were carried from the site.

Most apparent archaeologically at this time, is the direct incorporation of Panakiwuk into an integrated system of coast and inland activities. While Panakiwuk is, and was, no great distance from the coast, from Stratigraphic Unit 8 onwards coastal and marine resources were deliberately carried to the site in a sufficiently regular way as to generate a non-haphazard archaeological record. To this quite dramatic evidence we can add the less impelling, but equally tantalising, single flake of Talasea obsidian and the single piece of shell from a possibly wild form of *Canarium* in Analytical Unit B.

At a time not clear in the data available, but apparently c.8000 BP, this seemingly more systematic use of the cave ceases completely. The next human deposits laid down in the site are dated to only the last 1600 years. The character of this part of the record in Panakiwuk is very different from earlier levels. Organic remains are well preserved and we can identify a number of edible plants, especially *C. indicum* and coconut. The marine component in the subsistence remains is still high and an animal not previously seen, the wallaby *T. brunii* is present, although certainly not common. A few sherds of pottery are present, and presumably more distant links are reflected in the presence of Lou Island obsidian.

The non-use or sparse use of Panakiwuk between c.8000 BP and 1600 BP suggests a realignment of behaviour in the region which may have been as minor as merely omitting this site from an otherwise continuing hunting and foraging strategy, or as fundamentally significant as the introduction of gardening to the area. Unfortunately, unoccupied sites do not tell tales. It is equally unclear why, six millennia later, a further re-alignment of behaviour re-incorporated Panakiwuk into the human history of New Ireland.

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THE MOUK ISLAND SITE: MANUS AS PARADOX OR PARABLE IN RECONSTRUCTIONS OF THE LAPITA CULTURAL COMPLEX?

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Over the past ten years, the Admiralty Islands (Fig. 1) have been cast as something of a paradox in reviews of the Lapita cultural complex. This paradox derives from an apparent contradiction emerging from the results of survey and initial excavation both in Manus and elsewhere in the Bismarck Archipelago. Given the wide presence of obsidian from a Manus source in Lapita assemblages beyond Manus, implying, at the very least, contact between Manus and other communities in the Bismarck Archipelago by at least 3550 BP (Ambrose 1976), the failure of surveys in Manus to identify substantial deposits of Lapita pottery has come as 'a most surprising discovery' (White et al. 1988:414). While this paper does not address the sociological issues such as exchange that must ultimately frame any explanation of the role of Manus in the Lapita cultural complex, the point that we wish to make is simple: that the extent of our current knowledge of Manus prehistory, particularly for the periods prior to and coterminous with the existence of the Lapita cultural complex, is barely sufficient for conjecture, let alone pronouncement, on the presence or absence of either Lapita assemblages or producers in Manus. This conclusion has been prompted by the discovery of a new Lapita site at the cemetery on Mouk Island in Manus Province, Papua New Guinea. We describe a 2 m² excavation at the Mouk cemetery site and discuss the finds retrieved from the excavation and from a brief surface survey.

SITE LOCATION AND DESCRIPTION

The Mouk cemetery site, identified by the code GLT in the Papua New Guinea site register, is located in a well defined swale on the small volcanic island of Mouk (Fig. 2). A former eruptive centre, Mouk is a steep-sided cone reaching 72 m at its maximum height and measuring 300 m by 600 m (Johnson and Davies 1972:

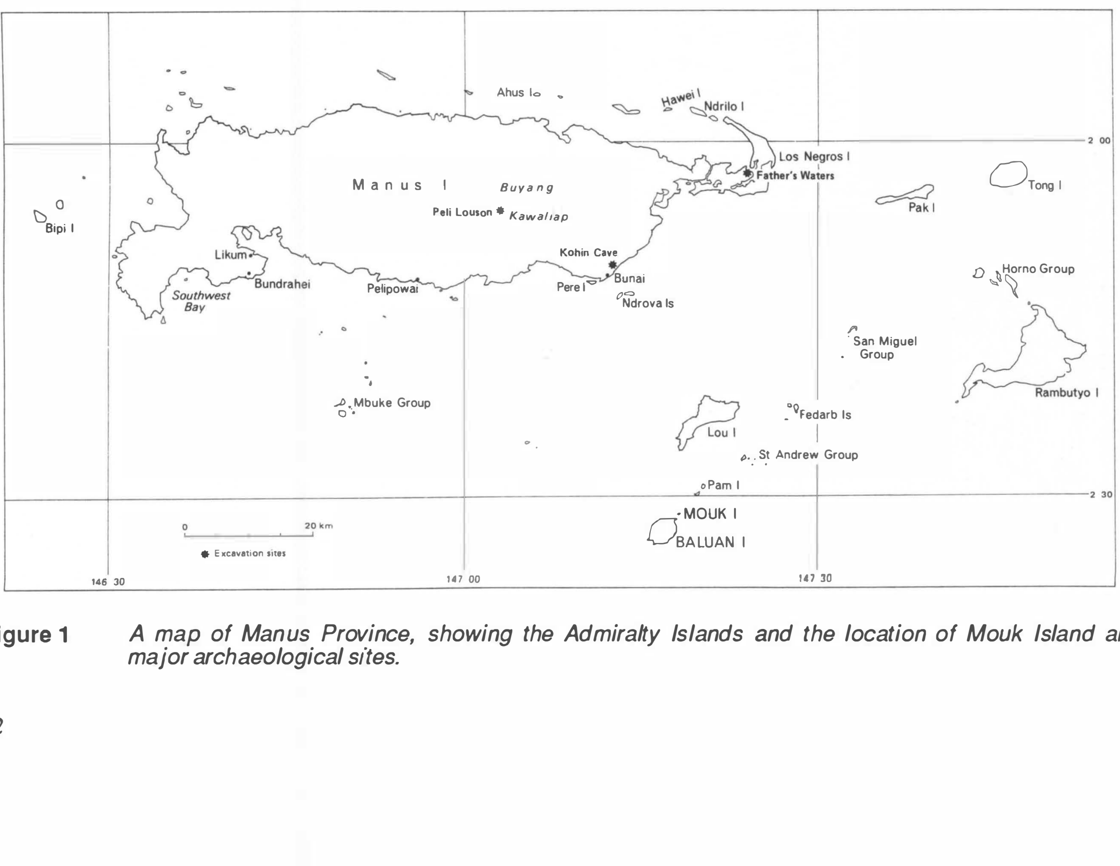

Figure 1 *A map of Manus Province, showing the Admiralty Islands and the location of Mouk Island and major archaeological sites.*

18-9). Sheer cliffs line the northern and western sides, limiting access to the island to the steep slopes of the southern and eastern sides. Thick vegetation covers the island, including extensive plantings of coconuts and other trees of economic value. Both Mouk and the neighbouring Baluan Island, located 700 m to the southwest, lie at the southeastern corner of Manus Province, 38 km south of the main island of Manus (Fig. 1).

Titan community of Peri, on the southern coast of mainland Manus, probably during the second half of the 19th century. Even then, their residence on Mouk was neither continuous nor constant in size. Shortly after the turn of the century, the island was periodically observed to be uninhabited, due in part to punitive raids following the murder of a trader (Cohn 1913; Nevermann 1934:53; Bühler 1935:31). Finally, in 1948,

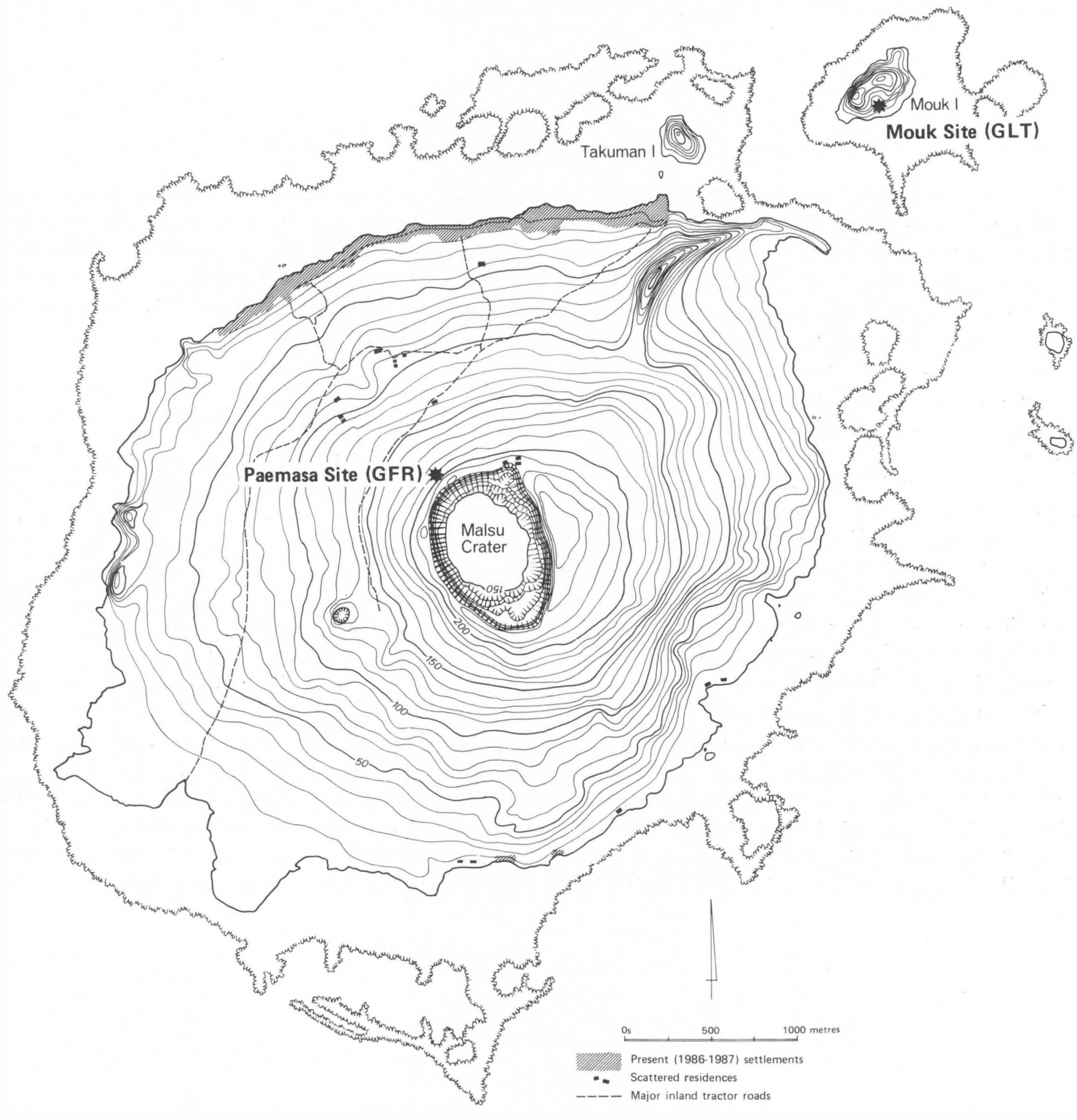

Figure 2 *A map of Baluan and Mouk Islands showing the location of the Paemasa and Mouk sites.*

The Titan speaking community currently using the cemetery site previously occupied a stilt house settlement on the fringing reef off the southern side of Mouk. Their oral traditions suggest that they first migrated to Mouk from the

the Mouk community relocated to a land-based settlement on Baluan following the dramatic political and social changes associated with the Paliau Movement (Schwartz 1962). Despite this history of residential instability, the Titan on

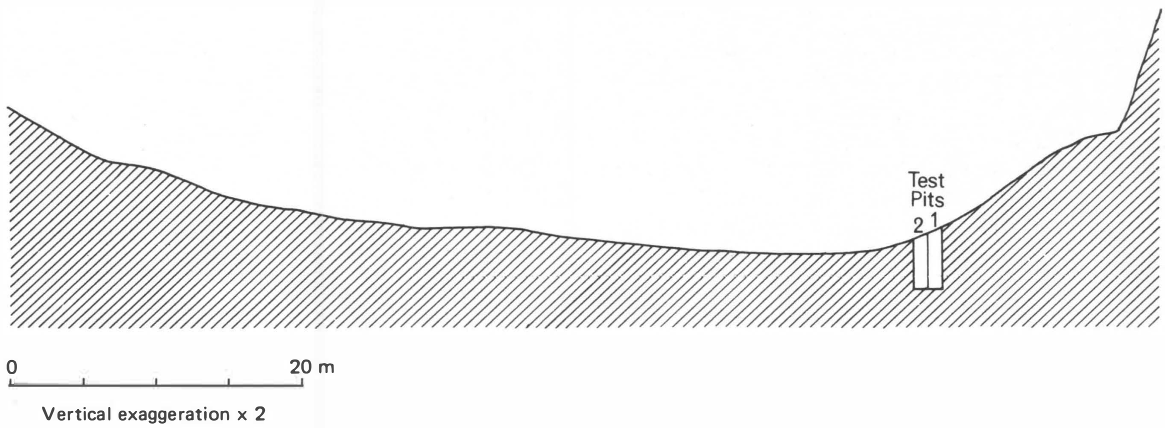

Figure 3 *A section of the swale at the Mouk site, showing the location of Test Pits 1 and 2.*

Baluan still maintain their role as a specialised fishing community and their rights to use Mouk as a cemetery and as a source of coconuts.

The swale which encloses the cemetery consists of a level, oval shaped clearing, measuring 50 m x 40 m, at a height of between 20 m and 30 m above sea level. With only two breaches in the slope surrounding the swale and with steep approaches from the rocky shore, the cemetery occupies a site with striking natural defences. The most recent burials, either capped by concrete slabs or marked with stone lines or ornamental shrubs, are all located at the perimeter of the swale. Titan informants maintain that the swale is entirely filled with earlier burials, and that those within the northern half of the swale, for which no surface evidence remains, belonged to an earlier, non-Titan community.

The archaeological potential of this site was first recognised when sherds and obsidian, exposed in the spoil from the most recent graves, caught the attention of Ballard during a survey of rock art sites on the Mouk cliffs. During a subsequent visit to Mouk by McEldowney, then studying the prehistory of subsistence on Baluan, a single decorated Lapita sherd (GLT/1) was recovered from the spoil heap of one of these graves. We then decided to excavate a small 2 m x 1 m test pit to determine the depth of the deposit and to explore the possible presence of further Lapita material.

EXCAVATION

Selection of a location for the test pit was limited to a narrow strip of cleared ground along the outer edge of the circle of recent graves, on the periphery of the swale, where our informants could be sure that no burials would be disturbed. Over eight days, two adjacent squares, each 1 m², and situated on a slight slope (Fig. 3), were excavated to a maximum depth of 160 cm. Deposits could only be sieved through a 1 cm mesh because of the compacted and sticky clay-like nature of the matrix. After removing a uniform 5 cm cover of turf, Test Pit (TP) 1 was excavated in 14 10 cm spits to a depth of 145 cm below the surface, where the deposit appeared devoid of cultural material (Fig. 4). TP2 was excavated initially in five 10 cm spits to a depth of 55 cm.

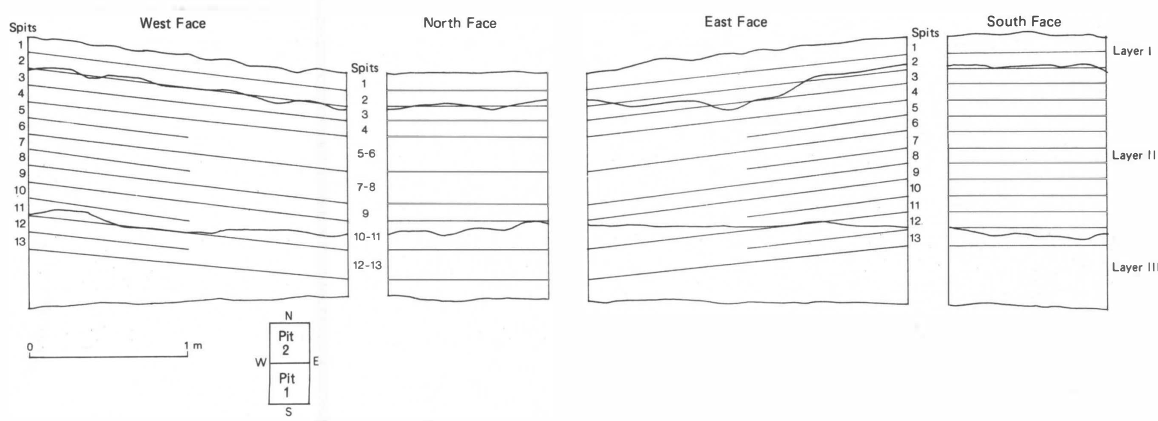

Figure 4 *Section drawings of the walls of Test Pits 1 and 2 at the Mouk site.*

Then, because of time constraints, Spits 5/6, 7/8, 10/11 and 12/13 were excavated as 20 cm units. Spit 9 was excavated as a 10 cm spit to allow comparison with Spit 9 of TP1, from which the first excavated Lapita sherd had been recovered. The base of the cultural deposit in TP2 was again reached at a depth of 145 cm. Both squares were then taken to a depth of 160 cm to confirm that the basal deposit was sterile.

The soil matrix consisted of weathered pyroclastics in which three layers with indistinct boundaries could be distinguished (Fig. 4). Layer 1 was a dark brown (10YR-2/2) clay-like compacted and sticky soil, which rendered excavation and sieving difficult. The general darkness of the soil probably reflects organic staining as it was most pronounced in the upper 5 cm and reduced in intensity with depth. Layer 2 tended to become increasingly more yellow to orange in colour (7.5YR-3/4), but retained the general textural characteristics of Layer 1. It was less homogeneous than Layer 1, with a higher frequency of pyroclastic debris and loose cinder patches mixed with a blocky clay. In Layer 3, the colour of the soil was lighter and more orange (7.5YR-4/6), but the layer was distinguished generally by its greater homogeneity and lack of compaction. The looseness of the pyroclastic matrix and the disappearance of any apparent organic staining at 160 cm was taken to represent the base of the cultural deposit.

ANALYSIS

Despite an almost neutral pH value of 6 to 6.5 throughout the depth of the site, the only cultural materials recovered, either in excavation or through the sieves, were pottery and obsidian. The only feature identified in the excavation was a roughly cylindrical lens of grayish soil very lightly flecked with charcoal, extending from 28 cm to 44 cm below the surface (Spit 3, TP1). From the approximately 2.9 m^3 of cultural deposit excavated from TP1 and TP2, 611 pieces of obsidian and 1193 pottery sherds were recovered. With the exception of seven decorated sherds (separated because the decoration was tentatively identified as Lapita), all of the artefacts and soil samples from the Mouk site, including the sherds collected from the surface of the site, were lost, together with other of McEldowney's research finds, during shipment to Australia from Manus. Analysis thus rests on the decorated Lapita sherds and on artefact counts and weights taken in the field, after preliminary cleaning, by McEldowney.

Obsidian

Following the loss of the obsidian artefacts, we can only presume that the obsidian was derived from the sources on the neighbouring Islands of Lou or Pam, 10 km and 4 km north of Mouk respectively. Although a higher number of obsidian fragments was recovered from TP2 (368 as against 244 in TP1), relative variation in the number and average weight of obsidian fragments remained similar in both squares through the full depth of the cultural deposit; Table 1 thus gives obsidian numbers and weights for both test pits together. Obsidian numbers were highest in the three uppermost spits, decreasing fairly evenly thereafter but, confirming a field observation of an increase in the 'chunkiness' of the obsidian with depth, average weights increased towards the base of the site, peaking in Spits 7/8 and 10/11 (Fig. 5). One exceptionally large obsidian flake was apparently recovered in the process of sieving the spoil from spit 14, but this is presumed to be the result of excavation error.

Table 1 *Raw scores for obsidian from the Mouk site GLT, Test Pits 1 and 2 combined.*

Spit	Number	Weight (g)	Mean Wt (g)
1	115	85	0.74
2	89	60	0.67
3	102	90	0.88
4	72	135	1.88
5/6	66	190	2.88
7/8	93	330	3.55
9	45	115	2.56
10/11	21	75	3.57
12/13	8	20	2.50
14	1	5	5.00
Total	612	1150	

Pottery

Matching the broad pattern of distribution of the obsidian, more pottery sherds were recovered from TP2 (798) than TP1 (401), and sherd densities generally decreased with increasing depth, the exception being Spit 4 of TP2 which yielded 249 sherds (Table 2 and Fig. 6). Unfortunately, our field records treated both decorated sherds and undecorated but diagnostic (rim and carinated) sherds as a single category. Table 2 and Figure 6 show the percentages of decorated and diagnostic sherds by excavation unit, and the locations of the decorated Lapita sherds illustrated in Figure 7. The peak ratios of decorated or diagnostic to non-diagnostic sherds in Spits 9, 10 and 11 match closely the distribution of the decorated Lapita sherds, with the notable exception of sherd GLT/2 which was retrieved

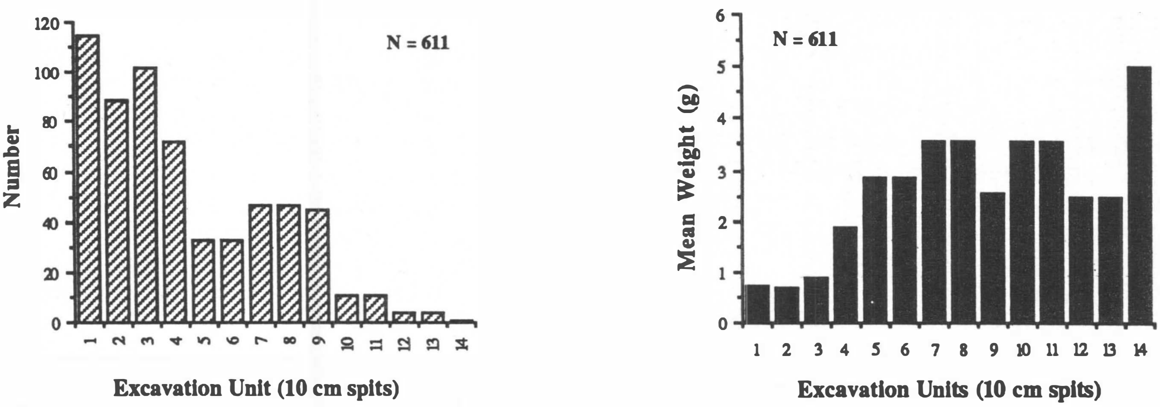

Figure 5 *Obsidian numbers and mean weights by spit, Test Pits 1 and 2 at the Mouk site.*

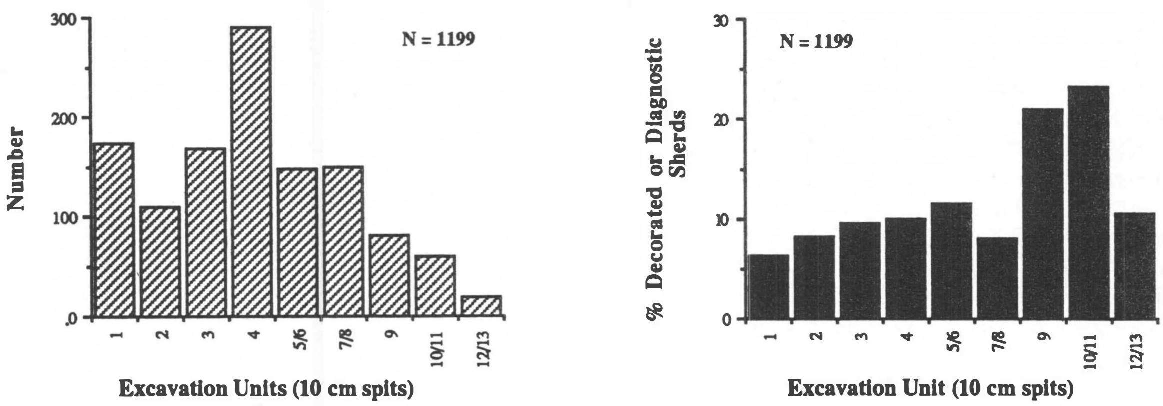

Figure 6 *Pottery numbers and percentages of decorated and diagnostic sherds by excavation unit, Test Pits 1 and 2 at the Mouk site.*

from the excavation unit comprising Spits 5 and 6 in TP2.

There appears to be some variation within the small sample of seven decorated Lapita sherds from Mouk (including the single decorated Lapita sherd from Paemasa, a site discussed below). Table 3 describes a number of characteristics for the individual Lapita sherds. After preliminary cleaning in water in the field, an attempt was made in a laboratory context to remove the thick film of remaining clay from the sherds to expose the decorated surfaces. One sherd, GLT/3, dissolved immediately upon contact with water, leaving only the slipped surface, with the decoration indented upon it, intact. A sherd of superficially similar composition, GLT/7, was not submitted to this treatment. The scale of variation in the average thickness of different sherds is exceeded by the minimum and maximum measurements for individual sherds. However, a preliminary examination of Xeroradiographs of the sherds suggests that there may be differences in composition: sherds GLT/4 and GLT/2 appear fine-grained, with no visible inclusions, in contrast to sherds GLT/5, GLT/6 and GLT/7, which are more coarsely grained and display inclusions of up to 2 mm diameter. Sherd GLT/1, with no visible inclusions but a grain more similar to that of the second group, falls between these two categories.

Variation in formal decoration within the decorated Lapita sample is also evident, though the small size of the sample and the fragmentary condition of the sherds obviously preclude any detailed external comparison. With the exception of GLT/2, all of the sherds show dentate-stamping. There is no visible evidence for infilling of the decoration on any sherd. GLT/1, GLT/3 and GLT/7 are finely stamped with a range of tools, and are of a similar colour and surface texture. GLT/4, GLT/5 and GLT/6 are more crudely stamped; GLT/6 and GLT/4 are also shell impressed, the latter further notable for the presence of decoration on both surfaces. GLT/2, recovered from the excavation unit com-

Table 2 *Distribution of pottery for the Mouk site, GLT, Test Pits 1 and 2 combined.*

Spit	No.	Wt.(g)	Mean Wt. (g)	Number of Decorated or Diagnostic Sherds: Sherds	% of Total	Number of Retrieved Sherds
Surface						1
1	173	300	1.7	11	6.4	
2	109	290	2.7	9	8.3	
3	169	470	2.8	16	9.5	
4	291	920	3.2	29	10.0	
5/6	148	700	4.7	17	11.5	1
7/8	149	605	4.1	12	8.1	
9	81	440	5.4	17	21.0	2
10/11	60	261	4.3	14	23.3	2
12/13	19	46	2.4	2	10.5	1
Total	1199	4032		127	9.4	7

Table 3 *Description of the decorated Lapita sherds from Sites GFR and GLT. Notes: a. Weight taken after dissolution of sherd. b. Sherd types: 1 = body sherd, 2 = carinated body sherd, 3 = rim. c. X-ray groups: 1 = very fine-grained, no visible inclusions. 2 = grainy texture, no visible inclusions, 3 = grainy texture, some inclusions.*

Registration number	Source (square/spit)	Weight (g)	Minimum thickness	Maximum thickness (mm)	Sherd type [b] (mm)	X-ray group [c]
GLT/1	surface	5.7	55	64	1	2
GLT/2	2/5-6	15.5	48	61	1	1
GLT/3	1/9	3.4 [a]			1	
GLT/4	2/9	6.4	65	72	1	1
GLT/5	1/10	6.2	48	65	2	3
GLT/6	1/10	9.4	74	85	1	3
GLT/7	2/12-13	1.4	60	65	1	3
GFR/1	surface	6.5	65	92	3	

posed of Spits 5 and 6 in TP2, is incised, possibly with shell, and whilst the decoration is not classic Lapita, the design may derive historically from the Lapita corpus. The omission of this sherd would limit the distribution of decorated Lapita material to Spits 9-13, and might be taken as evidence for some degree of stratigraphic integrity for the Lapita layer or layers of the Mouk cemetery site. The impression gained in the field of the non-dentate decorated sherds, which were subsequently lost in shipment, was of a wide range of decorative styles.

A HISTORY OF ARCHAEOLOGICAL SURVEY IN MANUS

Obviously, our report can only hint at the potential of the Mouk cemetery site, and what little has been recovered hardly warrants extended speculation on the significance of Lapita material assemblages in Manus. What is suggested here is that the circumstances of this discovery and the nature of the site's archaeological context provide grounds for questioning the validity of using the existing data base, drawn from the results of a decade of site surveys, as evidence for the 'continued nonappearance' of Lapita sites in Manus (Allen and White 1989:135). A brief review of the history of archaeological survey in Manus provides a means of tracking the sequence of statements that have engendered claims of an 'almost total absence of Lapita' (White et al. 1988:414) in the province.

Mouk is now the third site in Manus Province to have yielded decorated Lapita pottery. Kennedy (1981) reported the presence of four decorated Lapita sherds from Layers 7, 8 and 9 in her 1979 excavation at Kohin Cave (Fig. 1), bracketed by uncalibrated dates of 3900±100 BP (ANU-2248) from Layer 10 and 2450±120 BP (ANU-2212) from Layer 6 (Ambrose this volume). While conducting a site survey along the rim of the Baluan crater in 1981, Kennedy and Ambrose (Kennedy 1982) recovered a single decorated Lapita sherd from the surface of an area cleared for gardening named Paemasa

(GFR) (see Ambrose this volume). As this last sherd has not previously been illustrated, it is included in Figure 7.

These discoveries were all made, however, after Kennedy (1979:72) had published the results of the first major season of site surveys on

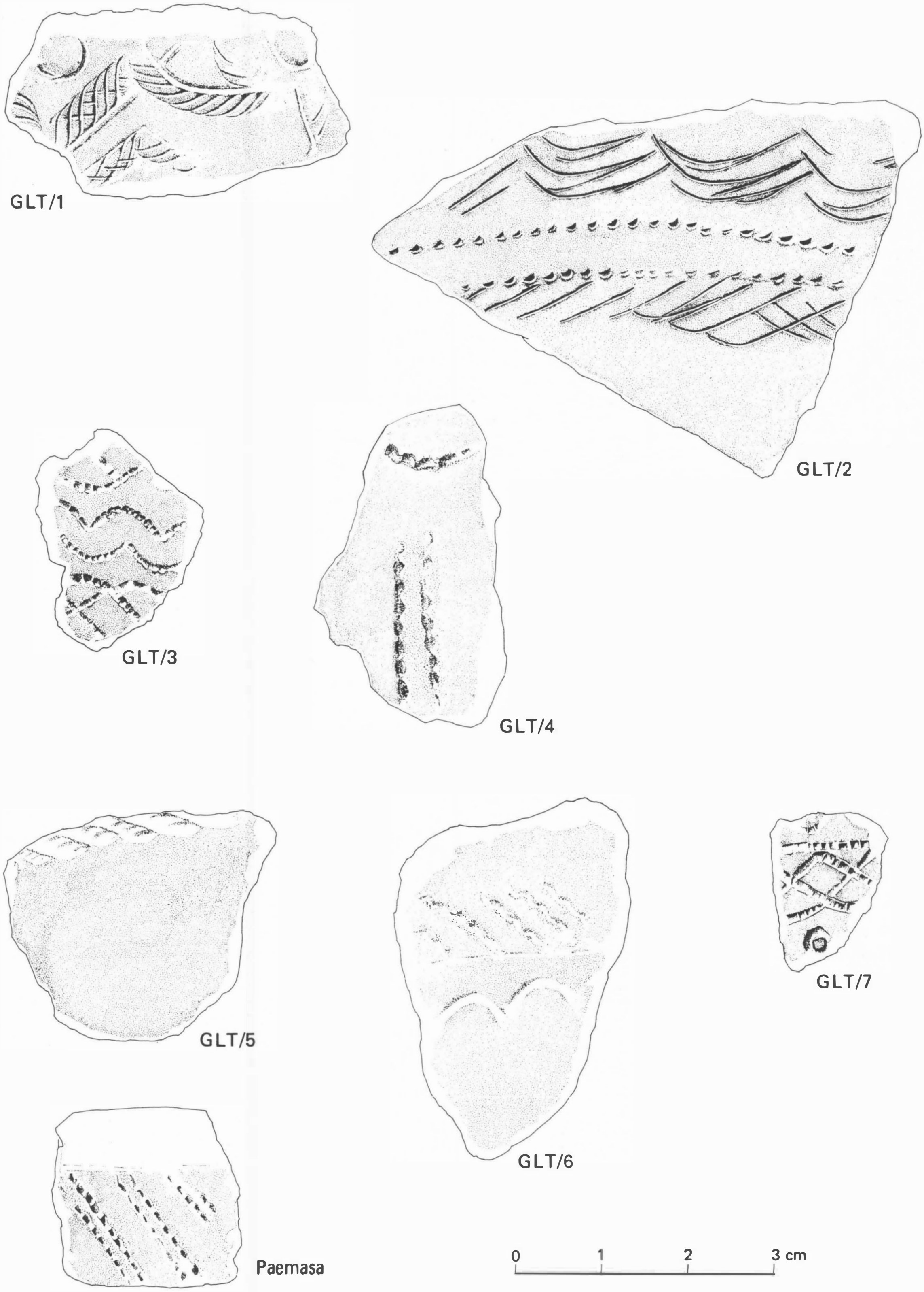

Figure 7 *Decorated sherds retrieved from the Mouk and Paemasa sites.*

Manus; these were conducted from September to October of 1977 by Ambrose, Allen, Harris and Kennedy. Kennedy reported the discovery of 'about 80 sites' and, though the majority of these sites were surface collections, she hazarded a general comment on the pottery component: 'Most striking is the absence so far of decoration in the Lapita style'. White and Allen took this observation a step further in their 1980 summary, probably submitted before the Kohin Cave excavations were complete, noting that 'surveys [in Manus] have failed to reveal any Lapita pottery in the 80 sites now located *in a wide range of likely environments*' (1980:733, our emphasis).

Subsequently, Kennedy was able to report the discovery of four decorated Lapita sherds during her second major season of excavations at Kohin Cave. The presence of these sherds at what was then the earliest dated site on Manus prompted her to propose a Lapita colonisation of the Admiralty Islands, though she did not consider Kohin Cave itself as 'a typical Lapita site' (1981:213). In 1982, Allen took issue with Kennedy's theory, stating that

> prima facie the evidence presently seems to me most suggestive of the Admiralties having been settled not by Lapita colonists but by contemporaneous people who put the valuable resource, obsidian, into the wider Lapita network to the east (1982:200).

The evidence he cited in support of this contention was that,

> so far no Lapita sites have been located on Lou containing Lapita pottery, and elsewhere in the Admiralties only four distinctively Lapita sherds have been recovered (1980:200).

By 1984, the uncalibrated date on shell of 4160±90 BP (ANU-3142) from the base of Kennedy's excavation at the Peli Louson site had demonstrated a pre-Lapita presence on Manus (Kennedy 1983:116), a situation now known to be common to most of the other islands of the Bismarck Archipelago. In a review immediately preceding work on the Lapita Homeland Project, Allen described the early site surveys as having covered 'all the major islands in the group' although he now acknowledged that volcanic activity could explain the lack of Lapita period deposits on Lou, stating that 'volcanic deposition on Lou has reduced visibility, and to date no sites older than 2,800 years have been located' (1984:199). This last date has since been corrected and the Sasi soil from the Baun village site, the earliest surface known from Lou, is now in fact dated to 2095±55 BP (Stuiver and Becker weighted average) (Ambrose 1988:489).

Recent reviews, following the Lapita Homeland Project, have continued to reinforce the impression of Lapita absence in Manus, despite acknowledgement of the site reported in this paper. Through the concretising effect of repetition, or in Strathern's happy phrase 'the illocutionary force of incantation' (1988:188), the current verdict is of an 'almost total absence of Lapita from the entire island group' (White et al. 1988:414). Despite the fact that none of the major excavations undertaken by the Lapita Homeland Project in Manus Province were in locales where Lapita sites might be expected to occur, a Project summary stresses as 'important ... the continued nonappearance of Lapita pottery in previously expected locations' (Allen and White 1989:135). That soils of the Lapita period have not yet been firmly identified on Lou Island (Ambrose pers. comm.) suggests that the description of the pottery from the post-Lapita Baun site as 'certainly not recognisably Lapita' (Allen and White 1989:140) is of questionable significance.

Manus as paradox or parable?

Prior to the Lapita Homeland Project, the discovery of most Lapita sites in the Bismarck Archipelago had been essentially a history of fortuitous finds, a pattern to which Manus has conformed despite the relatively long history of research in the province. The critique that follows is not intended to detract from the methods, goals or results of the reconnaissance surveys in Manus Province, but rather to question conclusions drawn from those results, which fail to take into account the limitations of such surveys and ignore the fact that they were originally directed towards much broader and more complex sequences of local prehistory. Three characteristics of these early surveys are considered: firstly, the extent to which 'the entire island group' has been surveyed; secondly, whether 'a wide range of likely environments' has in fact been covered; and thirdly, the specific nature of the observations made during the surveys. To address these questions we reviewed records listed on the Papua New Guinea site register for a total of 113 sites recorded in Manus Province between 1977 and 1985. Details were taken of location, topography, finds and the nature and date of the surveys when each site was recorded.

On the basis of this documentary evidence, it is readily apparent that large areas of the Admiralty Islands have not in fact been surveyed and that most of the areas surveyed have been covered only at a reconnaissance level. As Kennedy stated in her original report (1979) on the 1977 survey by Ambrose, Allen, Harris and Kennedy (still the most extensive coverage by an individual survey in the Province), 'efforts ... concentrated on two areas': Lou Island and the St Andrews group, and the Bunai/Pere area on the southeast coast of mainland Manus. Over a period of five weeks in 1978, this intensive but localised 1977 coverage was supplemented by brief visits by Ambrose and Kennedy to the islands of Little Ndrova, Pak, Tong, and Rambutyo, the island groups of Horne, M'buke and Pam, and Hawei, Ahus and Ndrilo islands off the northeast coast of the mainland. These visits yielded a total of 74 sites (Fig. 1). Surveys by Ambrose and Kennedy in subsequent seasons returned to Lou, but also extended the coverage to Baluan Island and on Manus itself, to Bundrahi, Likum and Southwest Bay in the southwest, to Pelipowai on the central south coast and to the Buyang/Kawaliap area in the interior. This scarcely constitutes coverage of the entire archipelago. Neither the large stretch of northern and western coastline between Ahus off the north coast and Southwest Bay on the south, nor the island of Nauna, nor the interior of western Manus had been surveyed in any way when the initial claims were made.

The contention that a 'wide range of likely environments' in Manus Province have been surveyed also requires possible qualification. Leaving to one side the question of identifying the topographic qualities characteristic of Lapita sites (Frimigacci 1980), we focus instead on the identification of appropriate geomorphic environments, those geomorphological units likely to date from the Lapita period. The role of geomorphic change in determining site visibility is well attested to on Pacific islands (Jennings 1974; Green 1979; Spriggs 1984), most particularly in the case of early occupation sites (Spriggs 1985). Allen and White (1989:136) have recently acknowledged the 'negative' effects of this 'destruction factor' in the identification of Lapita settlements in the Bismarck Archipelago, but perhaps Specht (this volume) has taken a more positive step in suggesting that

> reconstruction of past topographies is unlikely to be a simple operation, but appears to be essential *before* any understanding of the Lapita sites of the area can be achieved (our emphasis).

In order to arrive at a rough assessment of the role that geomorphological factors might play in the detection of Lapita sites in Manus, we correlated the locations of the recorded sites with generalised geomorphological units. This approach suffered from two major limitations. Firstly, the Holocene geomorphology of Manus is poorly understood. The most comprehensive treatment available (Jaques 1980) groups the Pleistocene and Holocene formations as a single class, a time span that obviously dwarfs the Lapita and post-Lapita periods. Secondly, these generalised units cannot fully account for a variety of localised circumstances or post-depositional processes that might affect site visibility or integrity. Of the 108 sites for which gross geomorphological conditions could be determined, 43 (40%) are located on landforms classed as being older than the Pleistocene, which includes those located in the interior of Manus or on inland hills and limestone formations nearer the coast. The 65 (60%) sites on Pleistocene/ Holocene formations can be divided into two further categories: 39 (36%) sites which are in landscapes likely to have developed or undergone severe transformation within the relatively recent Holocene and 26 (25%) site locations which were probably older and well formed by the Lapita period. The newer formations include the volcanic deposits on Lou (which are known to be younger than 2100 BP), sand cays, active colluvial slumps, alluvial deposits and the littoral deposits. The presumably older Pleistocene/ Holocene geologic units include raised coral and volcanic islands. Taken at face value and excluding the multitude of variables that diminish the probability of finding Lapita deposits in such a broad geologic and prehistoric time frame, only 25% of the site locations may even be eligible. Our review does suggest that a wide range of current environments was visited during the surveys. Whether or not these were likely environments for sites of the Lapita period is far from clear.

The third consideration, the nature of the survey observations, refers to the overwhelming dominance of surface, rather than sub-surface or excavation observations. Of a total of 113 sites, 84 (74%) were identified on the basis of surface observation alone. A further 15 sites contained exposures that permitted sub-surface observation, but nine of these sites were on Lou Island and thus, on the basis of our current understanding of the geomorphic history of Lou, almost certainly post-date the Lapita period. Only 14 sites (12%) were examined by excavation and only eight of these excavated sites could be considered substantial. Of these eight substantial sites (discussed by Ambrose this volume), five are understood to post-date the Lapita period, and a sixth, Kohin Cave, contains Lapita pottery. Thus there are only two sites, Peli Louson and Father's Water, that might straddle the Lapita period but contain no recognisable Lapita pottery; it should be noted however that neither of these two sites has been extensively investigated, with two 1 m^2 test pits at Father's Water and a single test pit at Peli Louson, which was only 0.25 m^2 in area and 0.75 m deep (Kennedy 1983). The addition of the Mouk cemetery site to the equation raises the number of dated sites in Manus that fall within the Lapita period to four. Two of these do contain Lapita pottery; both were identified only through excavation (whether by archaeologists or grave diggers).

CONCLUSIONS

On the basis of the minimal evidence available for presentation, the Mouk cemetery site adds little to our understanding of the role of Manus communities in the Lapita cultural complex. However the inferred size of the site (approximately 1750 m^2 of dry land alone), and its location on a small, offshore island, suggest that it enjoys a closer correspondence than the Kohin Cave or Paemasa locations to established expectations of a 'typical' Lapita site. How we might account for this stronger presence of Lapita material in Manus is not at issue here. Instead, the force of our argument is directed at the potential for prematurely narrowing the debate on the possibility of locating Lapita sites in Manus. Our entirely fortuitous discovery of the Mouk site and subsequent review of the initial survey coverage lead us to suggest that any attempt to consider the possible presence of Lapita sites in the Admiralty Islands will require a strategy founded upon predictive modelling of the Lapita period landscape as its point of departure. The recent assessment of an 'almost total absence' of Lapita sites in the Admiralty Islands must be considered far from proven; 'barely tested' might be a more appropriate verdict. In this, Manus may prove more of a parable than a paradox for reconstructions of the Lapita cultural complex in other areas.

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LAPITA OR NOT LAPITA: THE CASE OF THE MANUS POTS

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Radiocarbon dates from Manus Island indicate that settlement took place thousands of years before the earliest known Lapita sites in the Bismarck Archipelago. The typical Lapita style pottery is poorly represented and so far has only been found as four sherds in Kohin Cave (GDN in the Papua New Guinea site register) (Kennedy 1981a), one sherd from Paemasa (GFR) on Baluan Island and seven sherds from Mouk Island near Baluan (McEldowney and Ballard this volume). Coincidentally all these sites have a view of Lou and Pam Islands which are the main sources of obsidian in the Admiralty Islands (Fig. 1). The area is also the one where most archaeological work has been carried out. Nevertheless in the Manus Island sites of Peli Louson (GFJ) and Papitalai (GAC) (Kennedy 1983), which go back to at least 4500 BP, no Lapita sherds have been reported although other later pottery occurs and obsidian is present throughout. The reason for the poor representation of Lapita style pottery at the three locations where it has been found in the Manus archipelago, is probably related to the general observation that major Lapita sites occur as newly established outposts, away from areas of pre-existing settlement, and usually on small islands acquiring some defensive quality from their isolation.

Obsidian from the nearby Lou Island sources was being transported to the mainland Manus sites of Peli Louson and Papitalai by at least 4500 BP, but whether this implies permanent occupation of Lou Island, or whether the obsidian was recovered by day-trippers from the mainland is equally unknown. The 25 km water crossing from the mainland to Lou was no real obstacle to the traffic in obsidian for millennia before or after the presence of Lapita in the district. In

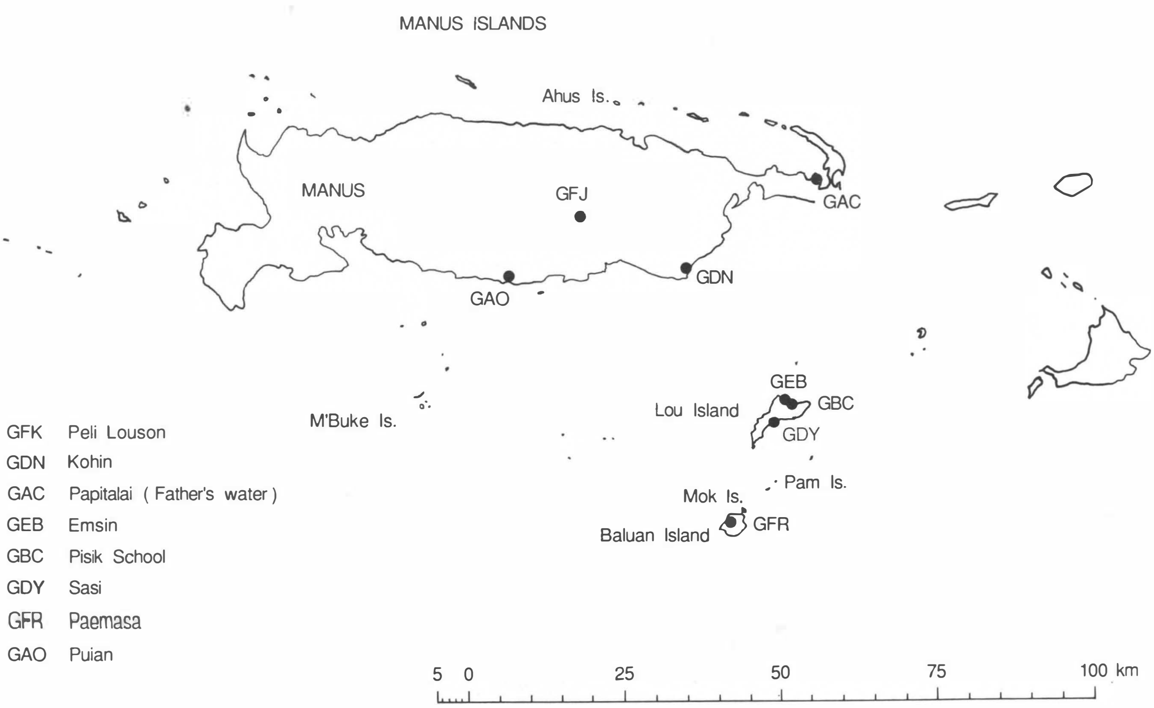


Figure 1 *Map of the Admiralty Islands showing locations mentioned in the text.*

July 1985 two canoes manned by children paddled the distance from Lou island to the mainland, overnight, across the often rough and fast currents of the intervening strait, for a social occasion from which they felt they had been excluded. The two canoes were small fishing outrigger types only 3-4 m long and with no additional freeboard. In the same year a villager from Lou similarly made the crossing in an outrigger paddling canoe to attend to a personal matter. The ease with which these journeys were undertaken emphasises the potential degree of communication within the Admiralty Islands, without the aid of large sailing canoes.

Thus, the presence of only a few sherds of Lapita pottery at Kohin Cave, Mouk and Baluan is not necessarily associated with any novel maritime technology at a local level. However, it does imply that long distance sailing technology was present in the region. If the Lapita pots were booty or trade items brought back by local groups from elsewhere, then long distance sailing was within the ability of local Manus groups; alternatively the Lapita sherds might indicate transient visits to local groups by whoever the carriers were, or lastly, the beginning of a new occupation not yet recognised in the archaeological record. So far there is no evidence to show that the islands of Mouk and Baluan, where Lapita sherds are found, shared the same long history of continuous settlement as Manus Island, but they were certainly accessible and probably exploited long before the pottery appeared, in the same manner as Lou Island was exploited for its obsidian.

One focus for persistent attention within the Admiralty Islands was the obsidian, already exploited locally for a long period before it became associated with distant Lapita sites at Mussau (Kirch and Hunt 1988) and elsewhere as far away as Vanuatu (Ambrose and Duerden 1982:84). Regionally, the arrival of settlers across a 200 km water gap and locally the carriage of obsidian across the 25 km gap to the Manus mainland before 5000 BP, implies a long term familiarity with sea crossings as part of a marine based technology leading to the development of the large ocean going sailing canoes first reported by the Spanish Conquistadors while on their way to the Moluccas in July 1528, and commended by many other visitors ever since.

MANUS POTS

Whether or not pottery was first introduced to the Admiralty Islands at the same time as the appearance of the cargo cult-like Lapita outposts in other islands is open to question, but it is clear that there was a long pre-pottery period of settlement and a local potential for the exploitation of the whole of the Manus archipelago. With the benefit of computer based radiocarbon calibration curves (Stuiver and Reimer 1986) it is possible to modify some of the earlier reviews of the chronology of Manus prehistory as they relate to pottery and obsidian use.

Kennedy (1983:120) suggested that pottery may have arrived in the Admiralty Islands as 'culturally unaccompanied baggage' and that there was a continuity of production from the first appearance of Lapita ware for around 3500 years. The fact that the first decorated pottery was the richly ornamented Lapita ware, and that it accompanied the earliest known transfer of obsidian for thousands of kilometres beyond its previous local Admiralty Islands distribution, argues against the hypothesis that there was little cultural influence. How pottery, and the manufacturing technology and specialisation it required, and the changes in food preparation and storage methods it engendered, could be introduced without other attached 'cultural baggage' has not been fully argued. Indeed the whole question of what effects the introduction of pottery and the new cooking and storage procedures might have had on prehistoric Manus society requires a clearer exposition. To be viable, the introduction of pottery requires more than importation of trade wares, and implies that the potters and their technology need to be an integrated part of the local economy. A system of already settled communities may or may not incorporate a pottery production technology, so that the 'settled' nature of any community need not necessarily imply the introduction of pottery. The innovation of pottery could conceivably be swiftly grafted on to Manus society by the acquisition of potters, perhaps from the New Guinea mainland, and possibly by the widespread traditional practice of abduction, which no doubt has a venerable past.

In the case of Manus there is also the tantalising prospect that traditional watertight coconut oil containers, made from the macerated putty nut (Parinari) plastered on to light wicker frames, could have been a valuable precursor to the ready recognition and adoption of the special properties of clay pottery. Parinari nut paste is widely used in Manus; as an essential caulking compound for large and small traditional canoes, a moulding compound for assembling small composite articles such as ladles and hair combs, and for general repairs to wooden artefacts. Parinari can probably claim a long history of use in Manus.

The main argument for a slow change and continuity in the Manus ceramic tradition rests on excavations at the (GDN) Kohin cave sequence (Kennedy 1981a), where no discontinuities in changing pottery styles were identified. More recent evidence, mainly gained during the Lapita Homeland Project, indicates that the suggestion of slow uninterrupted change needs some modification. Kennedy's original report (1981a: 758) suggests that the lowest four layers (7-10) at Kohin Cave contain material which belongs together; these layers contain the four Lapita dentate-stamped sherds, two shell impressed sherds and two incised sherds. This lower set is dated by a shell sample from the top of Layer 10, at 3900±100 BP (ANU-2248) which becomes 3860 BP when corrected by Stuiver's calibration curve (Stuiver et al. 1986) and makes it the oldest reported date for a Lapita site. Above these lower deposits are shell impressed and incised sherds dated to Layer 5 at 2450±120 BP (ANU-2212) and Layer 4 at between 2070±120 BP (ANU-2089) and 1910±90 BP (ANU-2215). (These are Stuiver-corrected by the intercept method to give mean values at one sigma of 2549±200 BP, 2050±135 BP and 1868±110 BP respectively). There are undecorated sherds throughout the deposit. The main point made of this collection was the juxtaposition of Lapita, shell impressed, incised and plain wares in the bottom Layers 7 to 9 and the continuance of shell impressed, incised and plain ware through to Layer 4 above. In contrast, work at Lou Island at three sites which contain unitary deposits sealed Pompeii-like beneath volcanic tephras, demonstrates a clearly separated set of pottery styles: that is, two distinctive ornamented wares sepa-

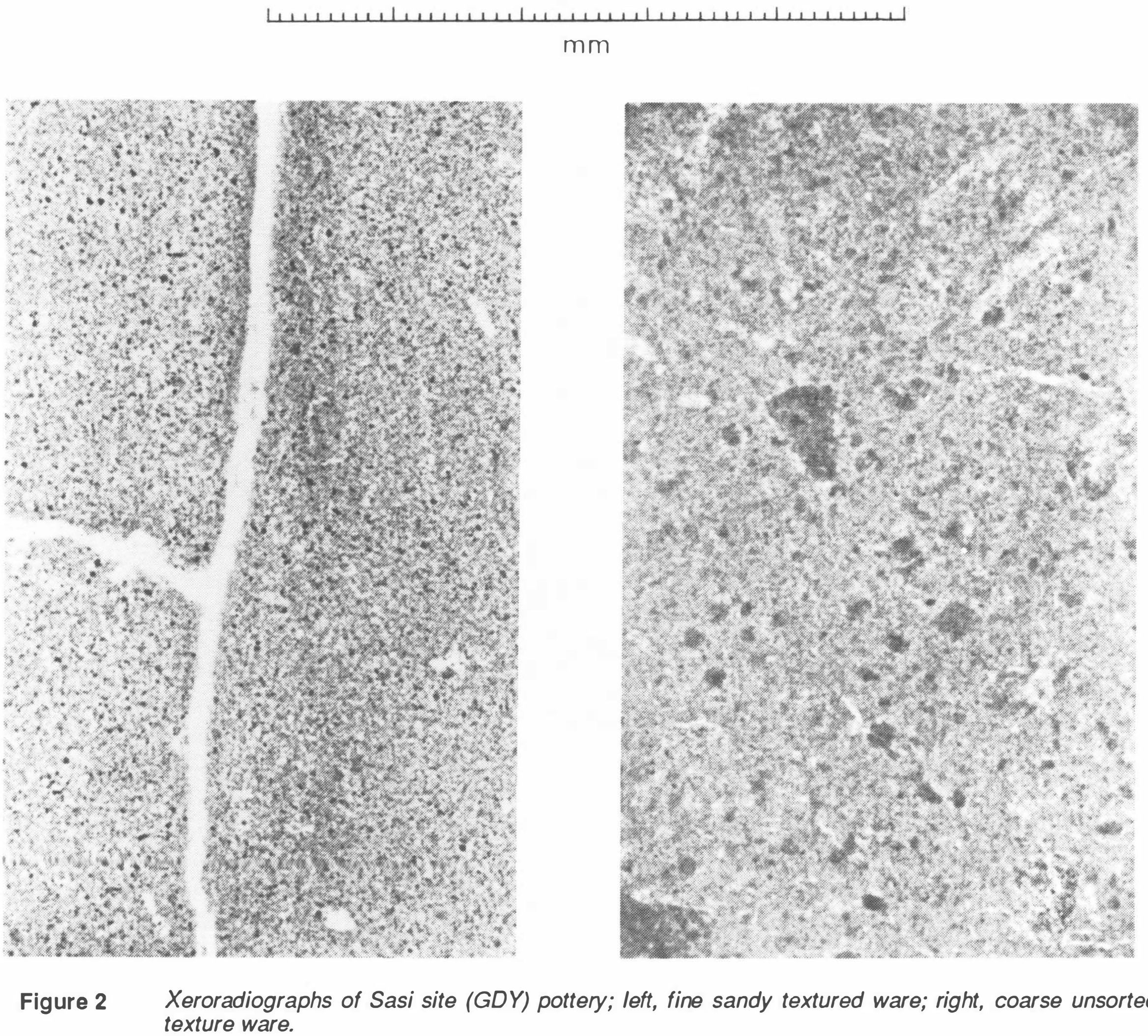


Figure 2 *Xeroradiographs of Sasi site (GDY) pottery; left, fine sandy textured ware; right, coarse unsorted texture ware.*

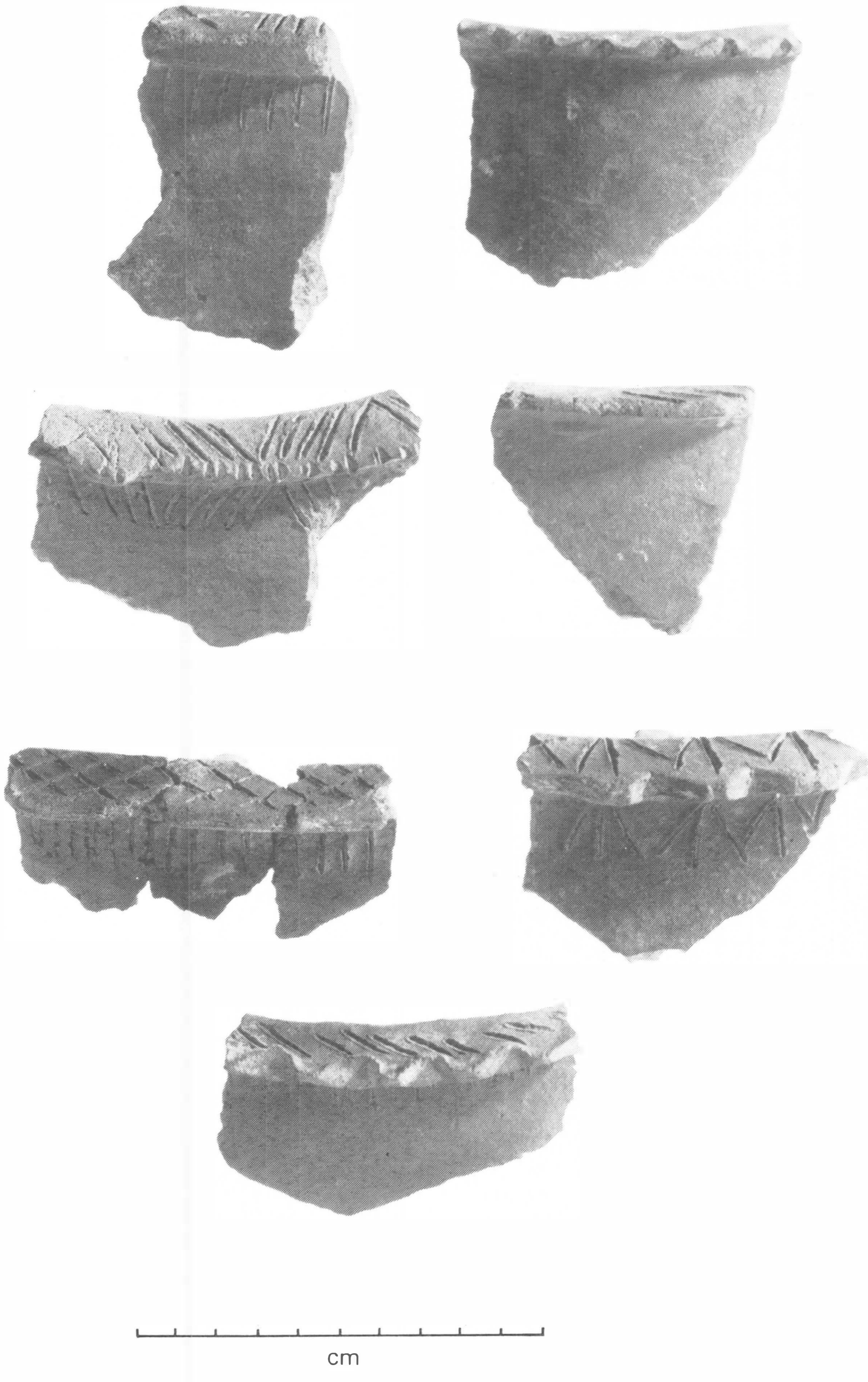
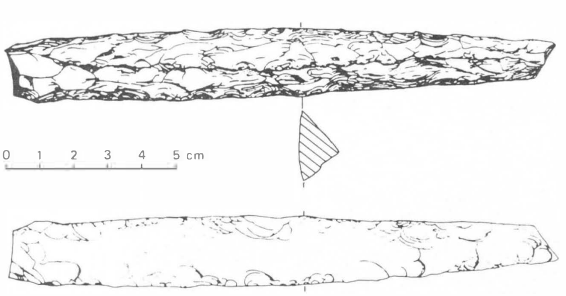


Plate 1 *Sasi site (GDY) rim sherds, typically incised and with flat horizontal rims.*

rated in time and a plain ware associated with both in all sites. The plain ware fabric is distinct from the fabric used to make the decorated wares.

The Baun village Sasi site (GDY) buried beneath 5 m of tephra contains a range of organic materials including bones of turtle, pig, human, and fish as well as common tropical shellfish. It is also a workshop area where hundreds of obsidian blades and points and thousands of flakes occur. There are also indications of former structures preserved as post holes. The pottery appears in two fabrics (Fig. 2), a light coloured one with a sandy textured temper and a generally clean undecorated surface, and a darker coloured ware with a coarse unsorted temper and a generally burnt or discoloured surface, with charcoal residues on some sherds. A small piece of bronze is part of the collection which is dated on three charcoal samples with a corrected pooled mean value of 2090±60 BP (Ambrose 1988).

The Emsin site (GEB), on the north side of Lou Island, is on a promontory above Rei village near the present shoreline. The site is distinguished by a concentration of obsidian points (Fig. 3), mainly plain pottery with a sandy temper, and a lack of organic remains, probably as a result of weathering beneath a porous 3 m deposit of Rei tephra. Rei ash is extensive over the eastern two-thirds of Lou Island (Pain 1981) and has been dated to around 1650 BP. The broken points and half-made points along with a wide size range of flakes marks the site as a workshop (Antcliff 1988).



GEB (43)

Figure 3 *Triangular section spear point from the Emsin site (GEB). This is one of the largest and most complete points in the collection.*

At the Pisik School site (GBC) on an elevated flattened ridge about 200 m from the shore, and about 500 m from the Emsin workshop site, three nested cauldron sized, open mouthed pots were uncovered after the base of the largest vessel was found protruding from the washed out storm-water drainage gully of the girls' dormitory. Excavations showed that the deposit containing the pots was the Rei tephra. The flattened pre-eruption ground surface had been a living area with the pots resting mouth down at the time of the eruption; several postholes were recorded, one with charcoal still in place. The main pottery from this site is decorated, coarse tempered, cooking ware. Unlike the Emsin site where points predominate, the obsidian tools at the Pisik School site are predominantly small flakes most suited to cutting purposes. Another two very similar large, intact, wide mouthed pots were pulled by villagers from an eroding cliff near Baun village on the south side of the island, again from beneath the extensive Rei ash deposits of 1650 BP.

Both coarse and sandy tempered wares are present at the Sasi site (2100 BP) and the Emsin-Pisik School sites (1650 BP), but their relative abundance differs between the Emsin site with mainly sandy plain ware, and the Pisik School site with mainly coarse decorated ware; the earlier Sasi site shares both plain and decorated wares. The decoration on the coarse wares differs in easily recognisable ways. The earlier coarse decorated Sasi ware (Plate 1) is predominantly incised in straight line designs on flat platform rims and around the neck. There is rim notching and some rim sherds have applied nubbins. In contrast the Pisik School coarse ware (Plate 2) has rolled rims and elaborate shell impressed designs on the rim and inner and outer surfaces of the neck area. Incised line, and applied strips are also common while a few sherds have fingernail crescents, and rim notching. Apart from the exuberant use of shell impression, the differences in rim profile are quite marked; the predominantly rounded and rolled rims of the Pisik School site (Plate 2) contrast with the distinctive horizontally flattened rims of the Sasi site.

The sandy tempered wares at Sasi are simple everted straight rim forms some with constricted necks giving the appearance of narrow necked flasks. At Emsin site the same everted rim forms occur while several double spouted vessels of very similar design have been found intact nearby, most probably from the same surface beneath Rei ash which preserved the large cauldrons. Kennedy (1982:26) discussed the distribution of double spouted pots and pointed to close parallels with others from Borneo. The tradition of double spouted water containers continued until recent times at Ahus Island off the north side of Manus Island. Parkinson (1907) extolled the value of these vessels as water con-

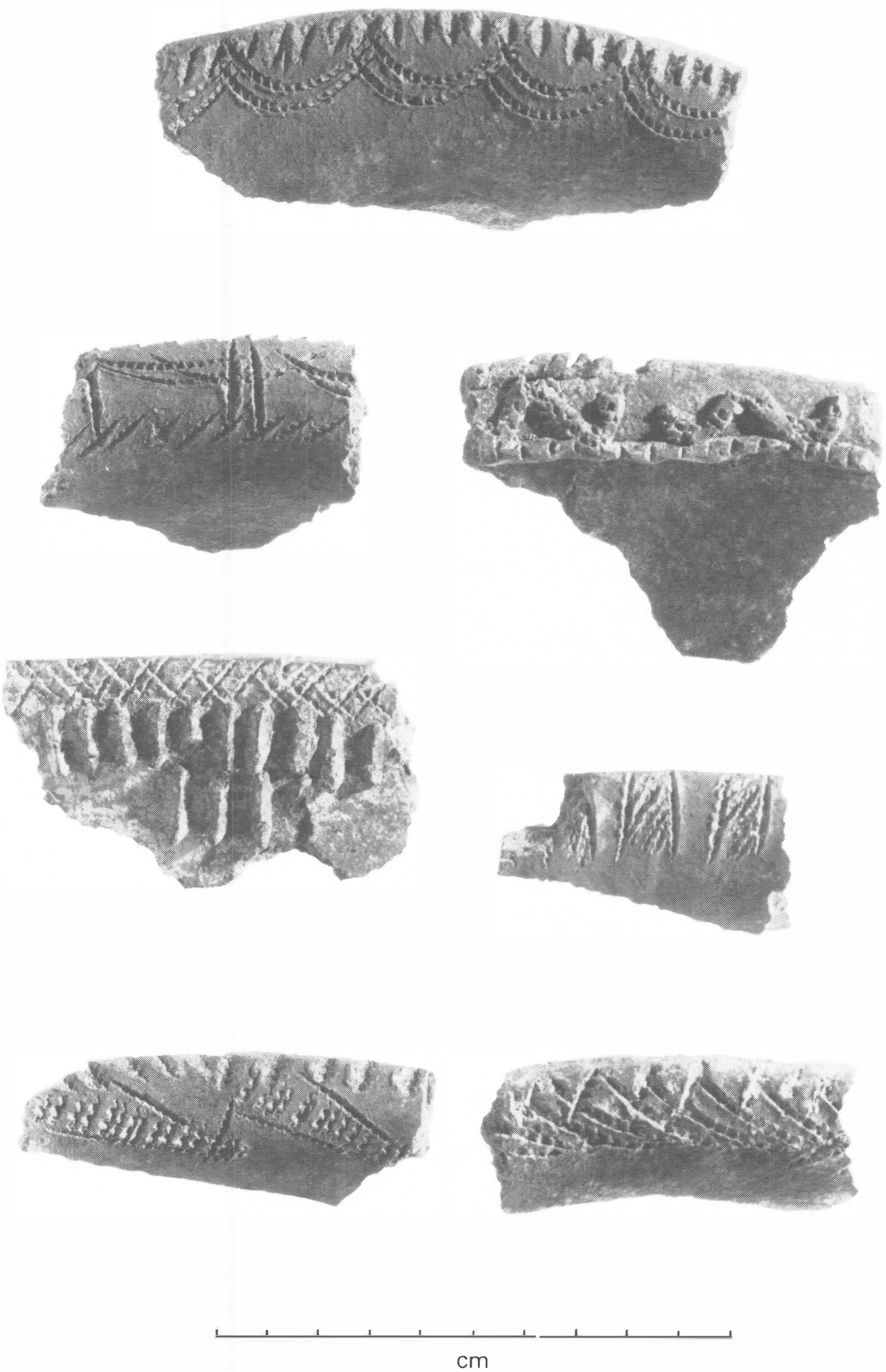


Plate 2 *Pisik School site (GBC) with typical 'Puian' ware rolled rims ornamented with shell impressed designs.*

tainers since, being porous, evaporation ensured the contents were cool and refreshing.

At a general level several activities seem to have been carried out at Sasi, from food preparation and consumption to obsidian point manufacture, whereas these appear to be separated between the contemporaneous Emsin and Pisik School sites. The difference in age between the Sasi and Pisik School sites is about 400 years, yet while the style of the coarse decorated ware differs, as would be expected for such a time span, the finer sandy tempered vessels seem fairly similar, having plain surfaces and modest decoration, mainly rim notching.

It is difficult to relate the dated Lou Island sites, with their very well preserved pottery, to the chronology of the Kohin sequence. Apart from the lack of any typical Lapita style decoration, in the coarse Lou wares the shell impressed rolled rims are clearly distinct and later in time than the Sasi vessels with their flattened and incised rims. At Kohin the combination of shell impressed motifs with rolled rims is also later, appearing in the upper layers, although shell impressed and line incised decoration appears generally on sherds throughout the Kohin sequence. It must be said that the Kohin sherds form a very difficult set to work with because of their very eroded surfaces and small size (Kennedy 1981a:757) nevertheless the shell impressed sherds closely match those of the Pisik School site. In particular, one example (Kennedy 1981a: Fig. 1e) is indistinguishable from the Pisik School rims, but the Kohin example is c.500 years earlier, according to the unpublished date of 2450±120 BP (ANU-2212) (J. Kennedy, pers. comm.). The question arises whether the Kohin site has its collection of later rolled rim shell impressed sherds somehow combined with earlier deposits.

The unitary nature of the Pisik School style pottery with its distinctive rolled rims and shell impressed designs, is exemplified in a large collection of broken pottery from near the Puian River mouth on the south coast of the main Manus Island, east of Pelipowai village. The site (GAO) was collected from over a wide area by villagers and comprises a narrow range of rolled rim shell impressed wares. No Sasi or Lapita style sherds are present in the collection. In view of the extensive nature of the site and its narrow range of pottery it is appropriate to name this distinctive ware, including the Pisik School collection, after the Puian River mouth location. Therefore the term 'Puian ware' will be adopted to nominate this pottery style.

A re-evaluation of all this would suggest that the Puian style shell impressed rolled rim wares at Kohin cave are not contemporaneous with the Lapita sherds. The Kohin site also lacks material similar to the Sasi forms and indicates a gap in the Kohin record. In fact the Sasi ware can be more appropriately compared, on grounds of rim form and incised line decoration, with the Erueti ware of Vanuatu (Garanger 1972: Figs 20 and 21), rather than the later Puian ware Pisik School pots. Unfortunately there are no sites so far from the Admiralty Islands with pottery unequivocally dated between 2100 BP and 3000 BP when a possible articulation of Lapita tradition and Sasi style wares could be investigated. It should be noted however that no such transitional connection need necessarily occur if the Manus wares are derived from a different pottery tradition. If indeed the Lapita wares are the precursors of the Manus pottery industry, then the use of unsorted, or self tempered clay in the Sasi ware is a change in practice at some stage from the use of better sorted sand tempered clays common to most Lapita wares. There may however be some Lapita connection through the plain or relatively undecorated sandy tempered wares of all these sites, which continues through to the present time in the pottery of Ahus Island off the north coast of Manus. On the evidence of the decorated sherds available for study, there appears to be a clear discontinuity between Lapita and later wares on Manus rather than the gradual change which has been suggested on the basis of the Kohin site. The published preliminary dates of between 3500 BP and 2450 BP (Kennedy 1983:115) imply a maximum possible gap in the record at the Kohin site of around 1800 years; from 3500 BP until 1650 BP when the ornate shell impressed rolled rim wares of the Puian style appear.

Another indication of change resides in the obsidian spear point workshops at both the Sasi and Emsin sites. Compared with collections made at earlier sites on the Manus mainland, where cutting flakes and blocks are reported from Peli Louson and Papitalai (Kennedy 1983), the Lou Island obsidian works appear ready made for hostile attack with a point industry that continued to this century. It could be argued that the spear points were for exchange but they could also be used to repel those who sought to exploit the obsidian resource. No such use of obsidian for projectiles has been reported from Lapita sites, and the only published record of what is undoubtedly a Lou Island obsidian spear point fragment from outside Manus is from New Ireland, at Lossu, at a period around 1700 BP

(White and Downie 1980: Fig. 7). The question arises whether the development of the obsidian projectiles, as seen at the 2100 BP Sasi site, was in some degree a response to other changes which included the arrival of pottery production over the previous 1500 years, and a wider regional connection of the Admiralty Islands within the Bismarck Archipelago.

While there appears to be a hiatus in the Manus ceramic record between Lapita and the later Sasi wares, there are sets of pottery sherds which are possible chronological gap fillers in surface collections from M'buke Island off the south coast of Manus. The present incumbents on M'buke are recent arrivals who displaced earlier groups (Crocombe 1965:44). There is therefore no support for propositions about the prehistory of the recent potters from M'buke, based on surface collections of an earlier people, belonging to a different language group from the present day Titan people (Kennedy 1981b). What is notable is that the modern Titan wares which have been described from M'buke (May and Tuckson 1982:336) have no resemblance in decoration to the surface collections of earlier pottery although the fabric of both has a fine grained texture.

On Ahus Island, an important pottery production centre existed up until the mid-70s, based on exchanges with nearby coastal communities. The sparsely decorated sandy tempered vessels of Ahus were made from clay collected on the mainland at Kali Bay. A notable vessel type is the double mouthed water jar which resembles the double spouted jugs from Lou Island, mentioned previously, both having a sandy textured fabric suited to vessels designed to contain drinking water.

There are therefore differences in Manus pottery based on the tempering treatment of the clay and the decorative elaboration of the vessel's surface which may reflect both a north coast versus south coast dichotomy, and specialised functions between water storage and cooking. In the light of the Lapita discussion, where it has been suggested that the decorated-undecorated distinction may parallel a non-secular or secular function (Kaeppler 1973), it may be instructive to examine the Manus case. The simplest explanation is that the sandy temper wares, some with constricted openings, and some double spouted, are water jars, while the heavier wares made from self tempered or unsorted clays are cooking vessels which happen to have more elaborate decoration. We look forward to the time when an analysis of residues, in the well preserved sherds from beneath the volcanic ashes of Lou Island, can be undertaken to give a direct assessment of the question of vessel function. The further question of whether the source of clays in the two wares is from the north or the south of the main island can be examined by determining the chemical nature of the clays in the pottery, and sourcing these to a likely provenance. To this end a limited trial of chemical analyses of clays has been carried out, with some encouraging results.

The conventional methods for pottery analysis include petrological thin sections, where tempering minerals or microscopic inclusions in the clay are identified mineralogically; or by using a microprobe to select various areas on a polished section for chemical analysis; or grinding the sherd to a homogeneous powder for examination by various spectrographic methods. These approaches are better suited to identifying the mineral filler and tempering fragments, rather than analysing the clay; or else they analyse an homogenised sample and fail to differentiate between clay and filler. In either case the technologically more important clay fraction is relatively neglected.

In the present study clays and fine silt were separated from pottery sherds by crushing and ultrasonic disintegration of 2-5 g sherds followed by elutriation of the suspended clay-fine silt fraction in distilled water. The dried clay-fine silt fractions were then compacted into 1 mm diameter cavities in a carrier plate and prepared for X-ray microprobe analysis to determine the major chemical clay components Si, Al, Fe, Mg, Mn, Ca, Na, K. The percentage composition of these elements was used to determine different clay groups with a clustering package from the CLUSTAN library of statistical programs (Wishart 1975). The procedure used was based on relocation of each clay analysis to a stable set of six groups whose centres were determined by a squared euclidian distance measure of the multivariate data.

Eighty-five clay-fine silt separations were analysed and the results are set out in Table 1.

Interestingly the Sasi coarse ware clay (Group 1) appears to be distinct from the M'buke clay source (Group 5). A major recently used source of clays from Timoenai on the Manus south coast is available for analysis but has yet to be prepared for analysis; this is a possible raw material for the prehistoric Sasi ware. The sandy temper light ware from Sasi is located with the Kali Bay clay as is a part of the Ahus collection.

It was expected that all of the Ahus sherds would sort to Kali Bay but, as shown, a large proportion became a group separate from all others (Group 4). The reason for this may lie in the amount of fine silt size particles of coral carried through with the clay in the separation procedure. This could be overcome by elutriating for only the clay size fraction and chemical removal of carbonate. This course is to be followed in future analyses. The Bunai sherds are similar to those from the M'buke surface collection but are from a limestone shelter area on the coast of the Manus mainland 30 km away; it is encouraging to see that they sort into a like group despite their location in a different environment to the basalt islands of M'buke.

Table 1 *Analysis of the chemical constituents of sherd clay-fine silt fractions and their allocation to one of six clusters.*

Clay Group Cluster	1	2	3	4	5	6
Sasi coarse ware	15		5			
Pisik School (Puian)	4		2		6	
Kali Bay clay		2				
Sasi fine ware	1	5				
M'buke surface	1	2			9	1
Ahus	1	6		17	1	
Bunai				2		
M'buke clay				5		
Totals	22	15	7	17	23	1

A current programme to include a wider coverage of clay source material, and more numerous analyses on pottery from dated sites, will no doubt modify the sketchy picture outlined here, while more refined techniques for separating clays from coarser fillers should help to sharpen the distinction between different sources, both known and unknown. These alternative tests on clays, the most important element in pottery, should allow a different range of propositions to be made about the locus of production, the transfer range, and the source of innovations in the changing wares of Manus, or elsewhere.

WIDER CONNECTIONS

Many of the decorative features present in the Puian-Pisik School wares parallel those from sherds in the New Ireland Lossu collections and appear to be of the same age at around 1700 BP-1650 BP (White and Downie 1980:206, Fig. 7). The triangular sectioned obsidian artefact from Lossu is indistinguishable from some of the broken sections of points from Lou Island. The rim forms and decorations of the Sasi wares at around 2100 BP are very similar to illustrated sherds from Erueti in Vanuatu dated to around 2400 BP (Garanger 1972: Fig. 21). Instances of the transfer of obsidian from Lou and Pam islands to distant depots from 3500 BP has been reiterated to a tedious degree. Therefore, it cannot be doubted that the Admiralty Islands have been an influential base for a wider area than their immediate surroundings. Manus connections with the Sepik Coast, and the far west through the Western Islands to the edge of the present Indonesian archipelago, should draw our attention to these New Guinea north coast areas as important places in the development of Manus and wider parts of Island Melanesia for thousands of years before the development of pottery.

CONCLUSIONS

An important aspect of Manus pottery to emerge from the Lapita Homeland Project is evidence for the advent of quite new styles of pottery making and decoration some time before 2100 BP. This change appears to be reflected in other sites in the Bismarck Archipelago and as far away as Vanuatu. The change outside of Manus is also accompanied by the reduction or absence of Talasea obsidian from site collections but the continuance of supply from Lou and Pam. In fact, the Manus obsidian supply appears to have been more or less continuous for over 3500 years with its range to New Ireland and the New Guinea north coast diminishing only at the beginning of this century. The obsidian industry itself widened from simple flake cutting tools to include formalised projectiles at some time before 2100 BP, and this continued with variations until the present.

The superficially minor Lapita ceramic event in Manus was followed hundreds of years later by different pottery technology and styles, for which at the moment we have no transitional evidence, so that the question of whether there was slow or abrupt change is unresolved. Further work is required to span the apparent gap in the archaeological record of the first millennium BC. Perhaps we should be looking to the New Guinea mainland as the source of inspiration for the development of the ornamented coarse wares which are clearly different from earlier Lapita wares. On the other hand there is a continuation in Manus of the light coloured sandy textured wares up to the present. This may be one class of ceramic which is a direct lineal descendant from a Lapita model, but it is not yet clear what the relationship of the coarse self tempered ware is, if any, to the earlier decorated Lapita pottery.

Whatever detailed response was triggered in Manus 3500 BP by the advent of Lapita in the region, there is no doubt on the basis of the continuous export of obsidian from that time onwards, that the Manus' view of the world was greatly extended to all points of the compass and that is a major change. But whether the Lapita event was simply a trigger for local change, an incidental marker for changes already underway, or marked the presence of sustained influential factors from abroad, is at present not clear in the ceramic archaeological record from Manus.

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OBSIDIAN EXPLOITATION AT UMLEANG, LOU ISLAND

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Obsidian artefacts are so common on Lapita sites west of Fiji that it has often been assumed that obsidian was distributed as part of a systematic system of exchange (e.g. Green 1979, 1987; Kirch 1987, 1988a, 1988b). Kirch (1988a) has gone even further to suggest that obsidian functioned as a 'primitive valuable' within a social network. Such inferences about obsidian exchange during the time of Lapita pottery are questionable because they are based largely on analogies with recent systems of exchange, rather than on a methodology which explicitly links the properties of the artefacts, their archaeological contexts, or their spatial distributions to particular systems of exchange. Ambrose (1978) has already pointed out one critical flaw in the use of analogy with historic systems. Since obsidian was moved over far greater distances in prehistory than in historic times, Ambrose has argued that it is unlikely that traders similar to historic specialist traders in Melanesia were operating during the time of Lapita pottery. Clearly, an alternative approach to direct analogy is needed in order to reconstruct the mechanisms responsible for the prehistoric distribution of obsidian. The most appropriate solution would be to devise methods which depend on archaeological data. With this aim in mind, fieldwork was undertaken at the quarry and flaking floors at the site of Umleang (PNG site code GBJ) on Lou Island in Manus Province. The ultimate goal of the research was to describe the archaeological remains resulting from the acquisition of raw material and the production of goods associated with one type of exchange system.

Umleang is located on a ridge about 1 km from the modern coastal village of Rei. A series of at least 24 deep mine shafts are scattered over an area of about 7 hectares. Allen, Ambrose and Kennedy excavated a test pit near Shaft 9 (Area A, Fig. 1) which yielded a stratified deposit of obsidian waste. A radiocarbon date of 220±80 BP (ANU-2019) was obtained from Layer 4 (Ambrose et al. 1981), suggesting that although quarrying at this locality is a relatively recent phenomenon, it probably began before regular European contact.

In 1985 Ambrose and Fullagar spent about four weeks at Umleang as part of the Lapita Homeland Project. The specific objectives of their work at Umleang were to produce a map of the site and to collect samples of archaeological material. Although mine shafts have also been reported on the nearby ridge at Umrei, these were not included as part of this project. They also filmed the manufacture of an obsidian tipped spear (Plate 1) by Kavon Kekes, a Lou Islander. In addition, Fullagar conducted a number of experiments using obsidian tools and collected both modern and prehistoric samples for usewear and residue studies (Fullagar 1986, 1989, 1991; Fullagar and Torrence 1987). Samples of waste by-products collected by Fullagar were brought back to Australia and analysed in collaboration with Torrence. This report provides a very brief summary of observations made during the surface reconnaissance at Umleang and the results of a technological study of the sampled debitage.

Research in several parts of the world has shown that the nature of raw material extraction and of the manufacture of artefacts are linked to the kinds of economic systems in which stone is circulated and consumed (Torrence 1986). Consequently, archaeological analyses of quarry/workshop sites should provide a sound basis for examining whether a resource was exchanged and if so, to what extent the economic system was commercial and market-oriented (e.g. Ericson and Purdy 1984; Torrence 1986; Baker 1987; Edmonds 1989). Umleang provides an excellent test case of this hypothesis because there are historical and ethnographic accounts describing the system in which obsidian quarried at Umleang was distributed. The archaeological signature at an obsidian source of recent exchange in the Admiralty Islands should also provide an invaluable model for the archaeological remains that would be expected at prehistoric quarries if obsidian had been exchanged systematically.

In the report that follows we examine both how obsidian was selected and quarried, and how

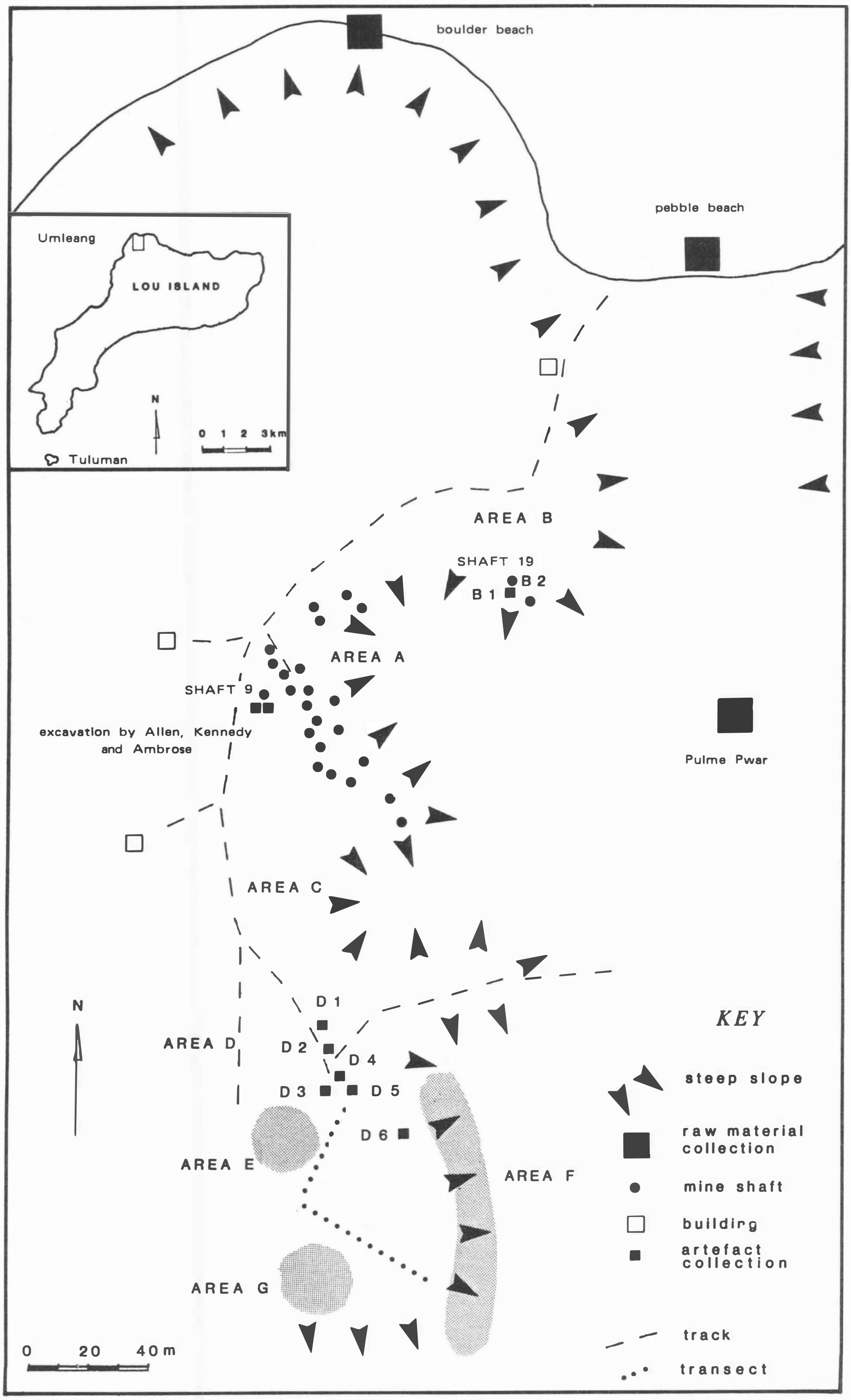

Figure 1 *Obsidian sources and archaeological material at Umleang, Lou Island.*

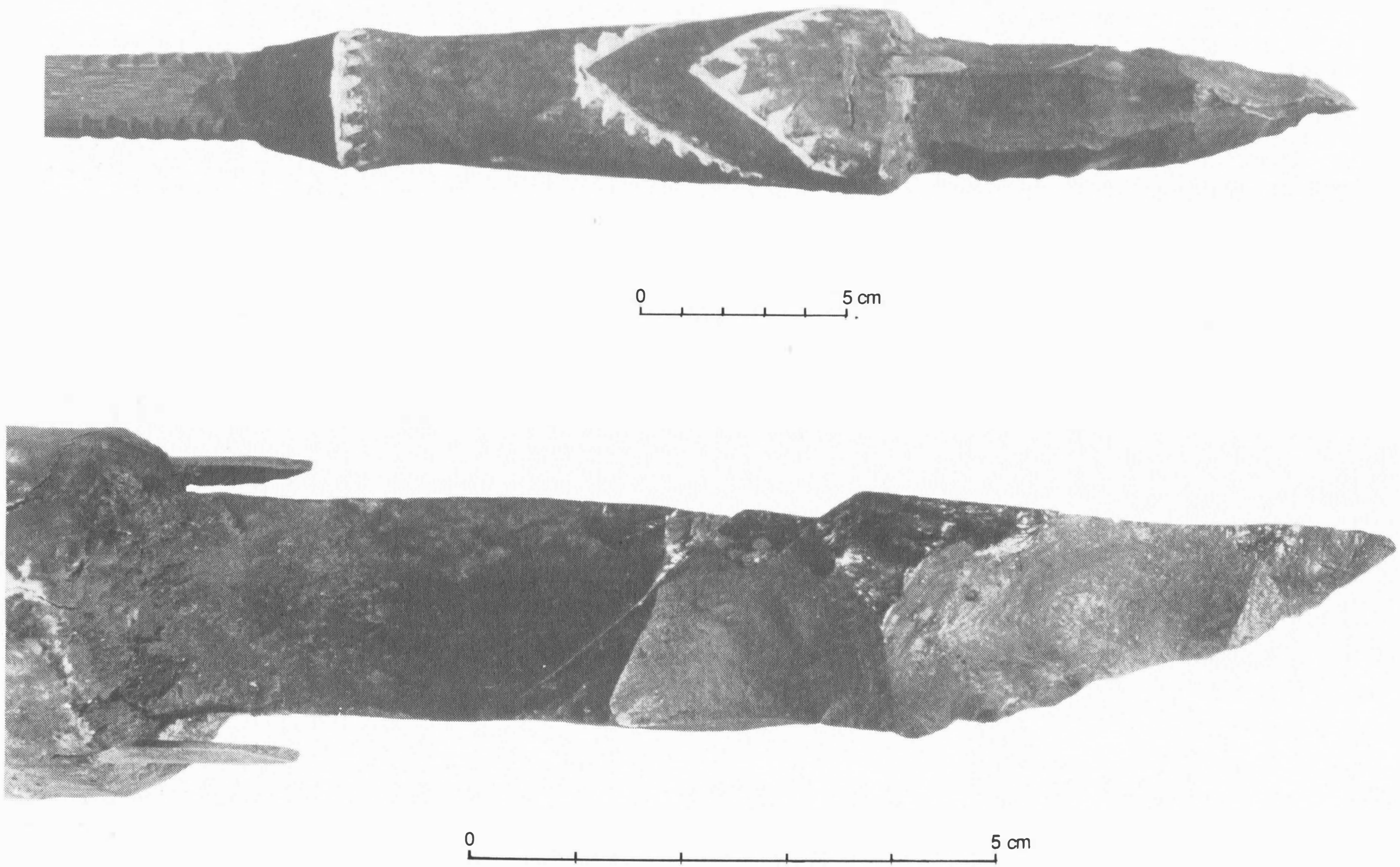

Plate 1 *Obsidian tipped spear made by Kavon Keves in 1985. Closeup of the ventral side shows the rough, irregular retouch typical of artefacts made for sale to tourists.*

artefacts were produced. Although the major goal of the study has been to describe these forms of behaviour in as much detail as the data will allow, we have also attempted to address the programmatic statements put forward by Torrence (1986) concerning the relationship between efficiency of behaviour and degrees of competition for material profits. After summarising the ethnographic data on obsidian exchange and the nature of specialist production at Umleang, we evaluate the choice of obsidian sources used in the past and consider reasons for the use of shafts. Following this, we reconstruct the manufacturing sequence for blades, analyse the behaviour reflected in the waste by-products in terms of efficiency, and, finally, explore the nature of spatial patterning at the site. The picture of obsidian procurement and production that emerges from these analyses raises a number of important issues for future research.

ETHNOGRAPHIC ACCOUNTS

Early travellers to the Admiralty Islands often noted the presence of large obsidian tipped spears, used primarily as weapons, but also for hunting wild pigs. Daggers and a few miscellaneous artefact types are also mentioned (e.g. Mosely 1877; Parkinson 1907; Nevermann 1934). The obsidian was said to have been obtained by trade from people living near the sources located on Lou, Pam Lin and Pam Mandian Islands. Unfortunately, the earliest account of how the trade actually took place dates to the period after World War I, at a time when local warfare had been stopped and obsidian was no longer mined. According to Mead (1930; cf. Schwartz 1963) the entire area of the Admiralty Islands was bound up in a network of exchange. Middlemen, who were members of one language group calling themselves Titan, were primarily responsible for transporting craft items and agricultural products throughout the region to villages belonging to the other two groups. Each area specialised in the production of at least one type of good. For example, Lou Island was said to export

> obsidian glass for knives, carving tools, and spear heads, painted gum oil containers, carved dancing poles, decorated lime gourds, more wood work, and most of the fish nets used by the Manus people (Mead 1930:119).

In the past, exchange was said to have been carried out by means of silent trade, but during Mead's fieldwork in the 1920s barter was conducted in small periodic markets or between trade partners using a system of balanced exchange. No medium of exchange was used; valuables such as shell money or dogs' teeth were reserved for social transactions.

Although there are no first hand accounts of how obsidian was quarried, Parkinson's (1905, 1907:373) account of his visit to Lou Island in 1904 is very useful. He noted the presence of deep shafts from which blocks of the best obsidian (*bailo*) had been excavated (1907:373). These were then traded widely. It is not clear, however, whether Parkinson was referring to the shafts at Umleang or to another location. Based on information from Thilenius, Nevermann (1934) states that the obsidian mines were owned and access to them was tightly controlled. He also adds that fire was used to break blocks and that men stayed in the mines from two to three days.

The art of manufacturing spear blades also appears to have been kept secret and known to only a few highly skilled knappers, although these specialists may not have been restricted to the obsidian producing islands (Nevermann 1934:234). Parkinson (1905, 1907:373) noted that he could only find one man on the Pam Islands who could make blades and on Lou he was only told about a craftsman who lived in another village. In contrast, Mikluho-Maclay's account indicates that many people could produce flakes (Nevermann 1934:234). Both observations may be accurate. It seems likely that only blade production was restricted to specialists, whereas casual flaking was more widely practised. Certainly, Nevermann's (1934:234) summary of Mikluho-Maclay's account indicates that very little skill was utilised since the knapper applied several blows before flakes were removed. Flakes with various shapes were obtained, nevertheless, by applying different types of force to several areas of the core. A *Cyprea* shell was said to have been used as the hammerstone.

Early descriptions of blade manufacture for spears are obviously very important for interpreting the archaeological remains at Umleang. Although there are no reports of spear production from Lou Island, we do have two descriptions by Parkinson from his visit to the Pam Islands.

> Although the manufacture appears to be very easy, it nevertheless requires extraordinary skill to judge the direction in which splinters will break off from the block; the only tool used is a stone with which quick, light blows are struck against the block. The splinters then chip off on the opposite side, which is firmly clasped by the hand (Parkinson 1905; trans. Alun Edwards).
>
> On Poam the blademaker gave us a demonstration of his skill. After selecting a small obsidian block, he held it tightly in the left hand

> and, using a hammerstone of about one-half pound in weight, struck with the right hand small chips from one side of the block. Then he held the block tightly with his hand so that the side rested on the flat of his hand whilst his fingers gripped both ends and gave a quite sharp blow on the outer face of the block, immediately producing from the opposite facing side, that side gripped against his hand, a long flake which was then finally formed into a spear blade through light blows. The obsidian appeared to have a distinct, definite flaking property which the manufacturer understood and knew how to put to use (Parkinson 1907:373; trans. Andrew Myers).

Nevermann (1934:234) also summarises a report from H. Klinck who noted that a shell or sharp stone percussor could be used. After creating a triangular side on the block, the core was rotated and a straight blow was applied. The core was then heated, thrown into water and after cooling, a blade was struck off.

By the time Mitton (1979:48-50, 68) visited Lou Island in 1975, the primary production of obsidian spears had ceased. His informants reported that during the German occupation just before World War I, officers had forced people to hand over all their obsidian weapons and cores. These were then dumped in the sea and further mining of obsidian was forbidden. Although obsidian blocks were no longer dug up, the spoil heaps at Umleang were scavenged for discarded flakes which were hafted into spears and daggers for sale in the tourist trade (Plate 1; cf. Ambrose 1975:Plate 1; Swadling 1981:61). The change from primary production of spear blades to the re-use of waste by-products is also evident in changes in the blades hafted into spears which were acquired by traders and collectors after about 1910 (Torrence 1989).

In 1985 Ambrose and Fullagar collected additional information on mining and knapping. Korup, the traditional owner of Umleang, explained that obsidian originally came out of the sea and went into the ground. It could be found by drinking kava and chewing betel and then spitting onto a leaf. The direction of the red spittle along the veins of the leaf would indicate where the mines were to be dug. He also stated that obsidian was mined by people called *kupkup*. There were two specialist miners in each of the villages at Umrei and Umleang. Obsidian blocks were lifted from the shafts using rope and baskets. The obsidian was then taken to a specialist knapper who made the spears. In addition, Kavon Kekes described how people who wanted a tool selected a piece of obsidian from the basket, washed it, and gave it to a specialist knapper (called *sike*, the name of the original flaking specialists) who knapped cores (*pulil*) to make the blades for spear heads (*peilo*) and other unretouched flakes (*parial*), some of which were used for shaving. According to this informant, obsidian for making spears must come from the mine shafts. The range of tools described by Kavon Kekes also included knives (*samel*) and daggers (*kosum*). Small unretouched blades (*tak*) were used to set in the ground along walking tracks as booby traps during enemy raids. The knapping implements included an anvil stone shaped like a pyramid (*roen pulpul*) and a hammerstone (*roen rhit rhit*), comprised of a small water-worn pebble.

In recent years Kavon Kekes has produced many obsidian tipped daggers for the tourist trade using material scavenged from local surface scatters. In 1985 he placed an old trapezoidal blade on an anvil and shaped it with direct, unifacial retouch (Plate 1). Since the use of an anvil is not mentioned in the early accounts, it may be a recent invention designed specifically for reshaping old waste by-products. Alternatively, Lou Islanders may have used a different method of retouch from the areas where we have direct accounts.

In summary, then, during the past 120 years at least, the obsidian industry on Lou Island has certainly undergone a number of changes as a result of interaction with Europeans and the incorporation of the local economy into the modern world system. When Europeans first arrived, obsidian appears to have been widely available to communities throughout the Admiralty Islands, although we do not know how many sources were exploited nor what proportion of the obsidian in use had been mined at Umleang (cf. Ambrose et al. 1981). At this early date, both access to raw material and to the skills necessary for making spear blades were strictly controlled and were limited to a relatively few specialists. The specialists working at the obsidian sources extracted unmodified blocks of obsidian and produced spear blades (and possibly other artefact types as well), all of which were exchanged to other communities. Whether middlemen from the Titan language group played as large a role in the exchange system before pacification as they did in the 1920s when Mead lived with them is questionable, but the issue cannot be resolved with current data.

Around 1910, however, the industry changed markedly. Mining and primary production of spears ceased and raw material was procured from earlier spoil heaps. From the early part of this century until quite recently, spears and daggers have been produced as much, if not more so, for commercial sale to tourists and collectors as for local consumption (Torrence 1989). Economic changes must have had profound effects on the archaeological remains at Umleang. Although our study of surface collections at Umleang lacks good chronological control, the effects of modern scavenging can be accounted for in the analyses. In addition, the deficiencies of the archaeological research are offset by an historical study of obsidian-tipped spears and daggers which were made during the past 150 years and are now incorporated in museum collections (Torrence 1987 in prep.).

OBSIDIAN SOURCES

It has been proposed that the choice of raw materials provides a good measure for inferring the economic setting in which production took place (Torrence 1986; Edmonds 1989). For this reason it is important to investigate the relationship between the type of balanced reciprocity which operated in the Admiralties and the value which people placed on their labour. This can be accomplished by evaluating the degree to which people chose to exploit outcrops of obsidian which produced the highest returns of useable obsidian for the least investment of labour. Since exchange in the Admiralties was conducted in a non-commercial setting, one can assume that labour costs did not greatly influence the choice of outcrops.

Currently, obsidian suitable for knapping can be obtained in four discrete areas illustrated in Figure 1: the pebble beach, the boulder beach, the Pulme Pwar gully and at the base of mine shafts on the ridge (Areas A and B). Firstly, obsidian pebbles bearing a smooth, water-worn surface are common in the pebble beach area. As shown in Table 1, the generally small size of the pieces would have limited the types of artefacts that could have been produced from this source; spear production would certainly have been precluded. Although some pebbles possess potential striking platforms, many are so small and rounded that bipolar flaking would be required and it would therefore be extremely difficult to make the large blades required for spears. On the other hand, the quality of the obsidian, in terms of its ability to produce regular and predictable fractures, is very good. This has been determined by flaking experiments carried out by Fullagar. The obsidian is usually very dark and homogeneous, although occasionally layers of air vesicles and crystallised rhyolite are present on the less rounded pieces and would have hindered flaking. The results of a study by Mark Domanski of the mechanical properties of the beach pebbles are reported in Table 2 (based on Appendix C in Fullagar 1986). Since this is the first analysis of its kind of Melanesian obsidians, comparisons can only be made to other types of raw materials (Fullagar 1986;Appendix C).

Table 1 *Mean size and modal shape of blocks at Umleang obsidian sources. For roundedness, higher values are more angular; for sphericity, higher values are more spherical and less irregular.*

Characteristic	Pebble Beach	Boulder Beach	Pulme Pwar
Weight (kg)	1.5	1.6	3.8
Length (cm)	8	6	20
Width (cm)	6	12	14
Thickness (cm)	3	8	12
Shape:			
Roundedness	R1	R2	R3
Sphericity	S2/S3	S2	S2

Table 2 *Mechanical properties of Umleang beach obsidian. Data from Domanski in Fullagar (1986:Appendix C).*

Property	Sample Size	Mean
Elasticity (N/mm)	11	50,661
Tensile strength	16	27.74
Compressive strength (N/mm x mm)	4	421.10

Secondly, obsidian appears to be eroding out of the base of the cliffs, about 100 m along the coast to the west of the main Umleang beach. The flakeability of obsidian from the boulder beach is much more variable than the pebble beach source. Some pieces are of superior quality and when flaked exhibited few rhyolitic inclusions, although many others contained bands of rhyolite. On the whole, raw material from the boulder beach area is larger and more angular than from the pebble beach (Table 1) and the nodules do not bear the characteristic water-worn cortex found so commonly on pieces from the pebble beach.

The third source is located in the Pulme Pwar gully, which slopes to the south of the pebble beach. Obsidian can be found both in the stream bed and on the gully slopes. Nodules have either been dumped over the edge of the gully or, more likely, are derived from deposits eroding out of the sides of the slope. Obsidian at this source is generally less abundant than at the two beach locations, but the blocks are larger (Table 1). One nodule which was flaked was found to be

Plate 2 *Large angular block from the pile of waste near Shaft I9. The core was rejected because it contains many flaws. Scale in cm.*

riddled with crystallised inclusions and air vesicles, but many others were quite solid. Nearly all the pieces sampled had several potential striking platforms with edge angles of less than 80°.

Finally, obsidian was quarried in the past from beneath ash levels. The obsidian was reached by excavating deep pits down at least 10 m through the Rei ash and then working along short horizontal shafts (Fig. 1, Areas A and B) (see illustrations in Swadling 1981:61). The shafts are c.1-1.5 m in diameter (cf. Ambrose 1975:359; Mitton 1979:50). For a description of how the same technique was used in British flint mining and an analysis of the engineering principles employed, see Felder (1979). Obsidian is said to occur as nodules within an ashy layer. Although the bases of several shafts have been examined, loose fill has covered the strata containing obsidian and so nodules have not been observed in situ. We are therefore unable to fully evaluate the quality of the ash levels as a source. On the basis of the waste by-products lying near the shafts, it appears that the blocks from this area are larger than elsewhere. In addition, the obsidian appears to have fewer inclusions and flaws than the two beach sources. The large angular block collected from a spoil heap near Shaft 19 (Plate 2) provides an indication of the very large size available from these outcrops. This particular example was rejected early in the process of production, presumably because of the flaws in the raw material visible on one side. In future work, stratigraphic excavation of a mine shaft (which would require extensive shoring of the sides) would be quite valuable for evaluating the nature of this source and for determining the quantity and quality of the labour needed to mine the obsidian in this way.

Given the paucity of waste adjacent to the beach deposits, it seems likely that these sources were not used extensively in the past. The decision to concentrate on other sources appears not to have been made in order to economise on energy expenditure, but because the rolled beach pebbles were not suitable for most purposes. Although the nodules are extremely easy to collect and can be flaked, their small size, and the rarity of appropriate striking platforms means that much of the material is sub-standard for spear production. These sources, however, were not avoided entirely. A small number of flakes bearing the distinctive sugary texture and rounded outer surface of the beach cobbles were recovered both on the beach and from samples collected near the mine shafts. Rather than being the waste from the manufacture of artefacts, it seems more likely that these flakes represent wear and tear on obsidian hammerstones. Furthermore, a beach pebble bearing the characteristic small pits created by battering was collected from Area D4 (Fig. 1).

Evaluating the choices made concerning the inland sources is much more difficult. Given the high rate of erosion, the quantity and density of waste material in the gully at present is an inadequate measure of how much exploitation actually took place. The major question that arises when trying to evaluate raw material selection at Umleang is why people chose to excavate the deep shafts when it seems likely that they could have dug out all the nodules they needed from the gully slopes. For example, recent exploitation of obsidian in the Talasea area of West New Britain was largely carried out by shallow excavations of material deposited in gully bottoms or eroding out of gully slopes (Specht 1981; Torrence et al. in press). Not only would the shafts have required a significant investment of energy, but also working within them would have been quite a risky venture, given the soft, crumbly nature of the walls. It seems plausible that the gully slopes could have provided just as much high quality raw material with much less effort or risk.

Although a definitive answer to the question of why shafts were dug at Umleang demands a more detailed comparative analysis of the distribution, abundance, and quality of the various obsidian sources, it seems likely that the reasons for choosing shafts were not solely based on material variables such as raw material attributes or desire to conserve the expenditure of energy. The need to maintain ownership and control over a resource which enabled participation in the inter-island exchange network is a more plausible explanation of why exploitation was focussed on shafts.

The reciprocal exchange of goods in the Admiralty Islands system, as described by Mead (1930) and Schwartz (1963), was based on the differential distribution of resources or products. Desire for a good was created simply by whether one possessed it or not. Some areas had access to food items which were unavailable to others, but in other cases the particular good offered for exchange was a craft product which was not manufactured elsewhere. The monopolies which underlay the operation of the exchange system, therefore, were partly due to real differences in access to goods and partly due to artificially imposed rules of ownership over goods or the knowledge of production. These rules allowed everyone to have a 'speciality' that would allow them to participate.

By defining useable obsidian as coming from one small area at Umleang, access to this resource could have been monopolised by a relatively few 'owners'. It is worth noting that the adjacent ridge at Umrei also had shafts, but these are said by informants at the modern village at Rei to have belonged to a different group to the one who 'owned' Umleang. It is possible that obsidian was not quarried from the gully slopes because the area between the two controlled areas was agreed to be off-limits to both groups and so it served as a boundary between them.

Monopolies can also be created if only those people who possess specialised knowledge are allowed to extract and/or process a resource. For example, in a survey of ethnographic cases Torrence (1986:84-5) found that restrictions on knowledge about flaking stone were common where raw materials were not limited to small, isolated outcrops. The presence of shafts at Umleang, therefore, may be the archaeological consequence of using limited access to *skills* in order to create a monopoly which was necessary to allow the local residents to participate in the regional exchange network. In other words, the choice of where and how to extract obsidian at Umleang was not determined solely by the nature of the resource itself or by a desire to conserve labour or maximise outputs. Instead, the use of deep shafts was probably determined by the particular nature of the non-commercial, reciprocal exchange network in which obsidian circulated.

BLADE MANUFACTURE

The analysis of production at Umleang is based on a study of debitage from samples collected by Fullagar in 1985. Table 3 summarises the sampling procedures and the composition of the samples. Although all the material has been inspected, only the samples from Areas D1, D2, and Shaft 19 in Area B were subjected to systematic classification and measurement. The remainder were either grab samples designed to recover unusual artefacts or were explicitly designed for usewear and residue analyses (see Fullagar and Torrence 1987; Fullagar 1991).

In most cases archaeological reconstructions of how tools were made depend to a large extent on an analysis of partially worked, unfinished, or broken artefacts and on cores which were rejected at various stages of reduction. For analysing the sequence of events at Umleang, the study of cores and blades (e.g. Plates 3, 4 and 5; Figs 3 and 4) has provided an invaluable picture of the way the product was transformed as it moved through the production sequence, but the rarity of these types of artefacts in the samples recovered from Umleang has meant that our analyses of the nature of production at Umleang have had to rely much more heavily on other types of debitage. In summarising our reconstruction of the methods used to produce blades at Umleang, we will focus on three interrelated approaches and sets of data. Firstly, a model was created using the presence and absence of key technological types. Next, the general reduction sequence was contrasted with the individual histories which could be gleaned from the small number of blades and especially of blade cores which were collected. Finally, these two qualitative approaches were evaluated by examining the metrical attributes of the waste by-products.

Since stone working is a subtractive process, it is possible to use the abundant waste by-

Table 3 *Summary descriptions of sampling at Umleang. See Figure 1 for locations of samples.*

Sample	Type	Size (m^2)	Comments
B1	systematic, surface; SW quadrant to a depth of 0.25 m	1	all material from SW quadrant measured; remainder sorted by type
B2	material left in place	1	blades only
D1	systematic, excavation to a depth of 0.25 m	0.25	all artefacts measured
D2	systematic, surface	1	all artefacts measured
D3	grab	c.0.5	2 cores (Plate 5) 1 flake core
D4	grab	c.0.5	1 blade 1 hammerstone
D5	grab		1 core (Fig. 3 and Plate 4)
D6	grab, all large pieces	c.2.0	for usewear/residue analyses

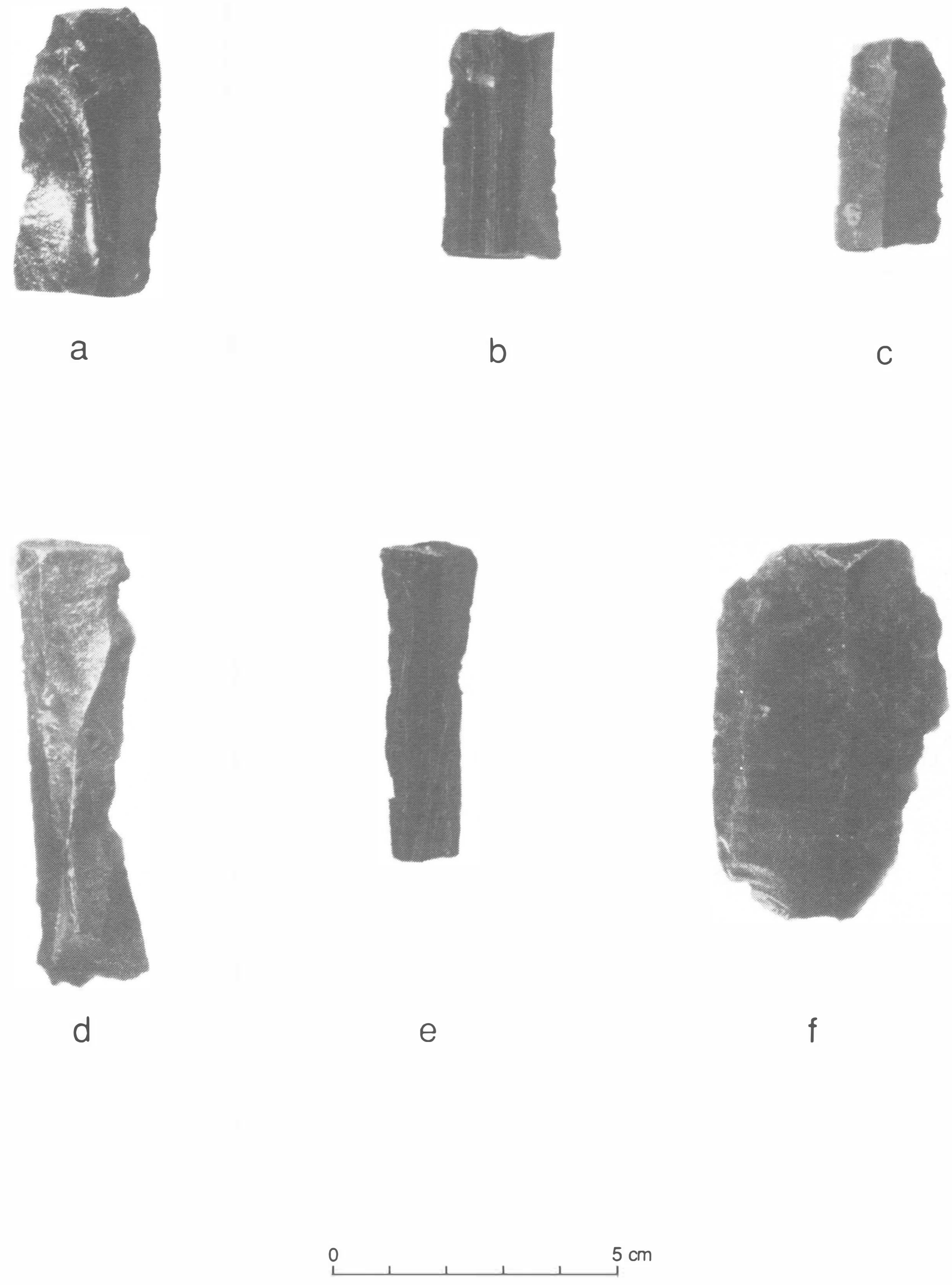

Plate 3 *Blade fragments from D2.*

Plate 4 *Blade core from D5 showing the crest. Two layers of working are visible: long narrow scars on the intended blade surface, followed by short shallow scars to shape the crest. Scale in cm.*

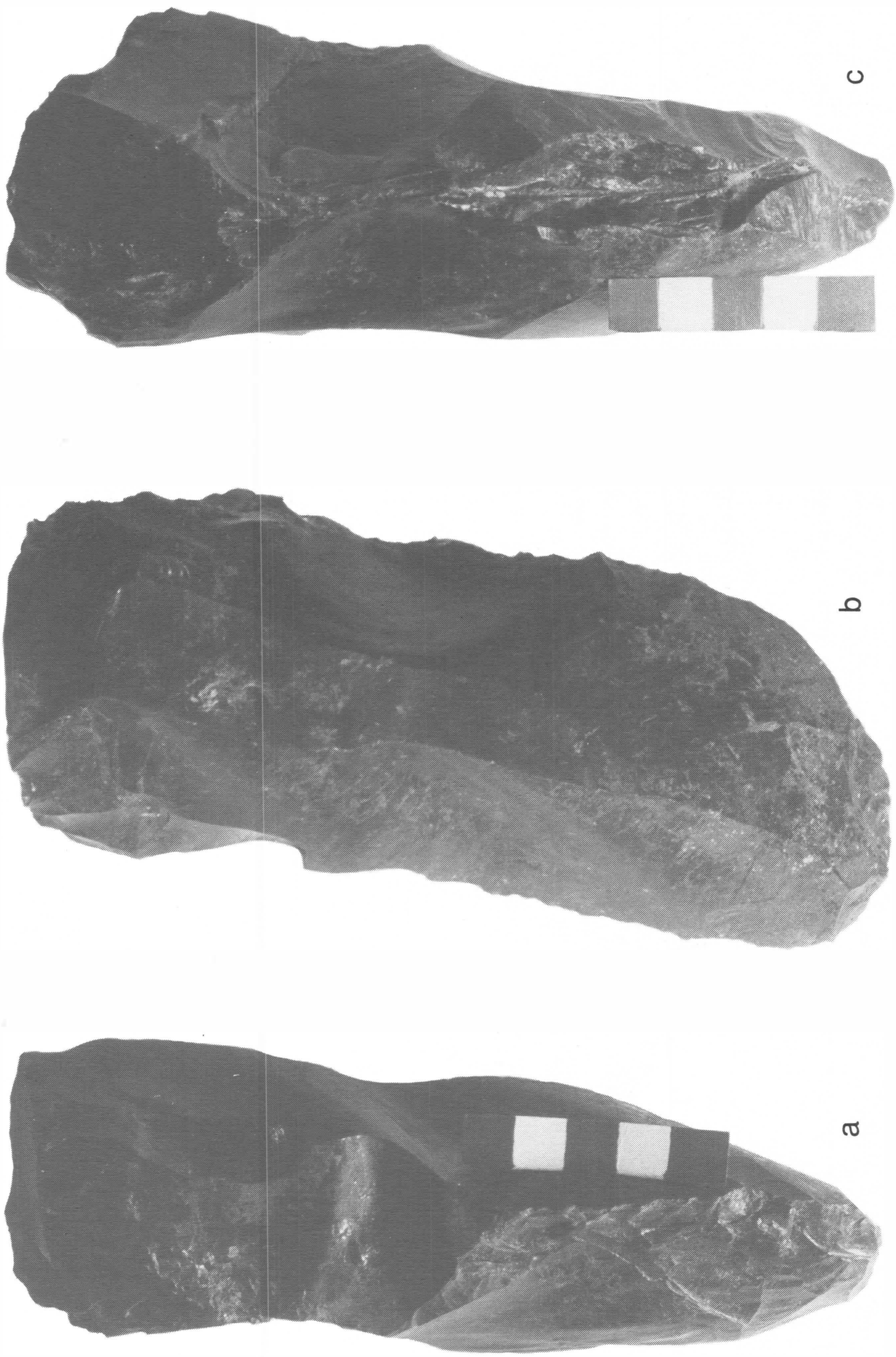

Plate 5 *Three views of blade core D3(ii). Scale in cm.*

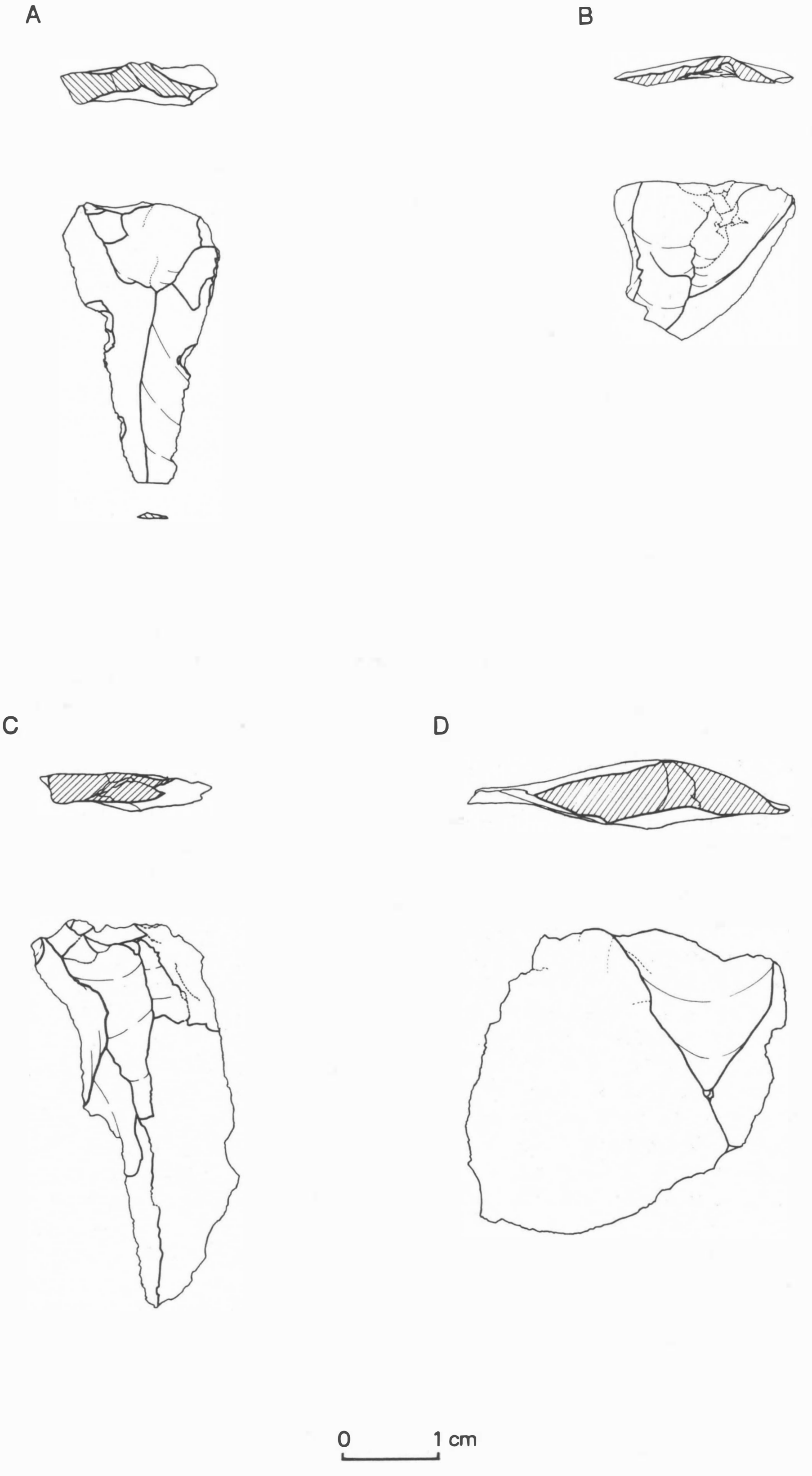

Figure 2 *Examples of the typical flakes created during the manufacture of crests: a, b, d, flakes with deep 'pocket' scars on the dorsal side; c, flake with trimming at the proximal end of the dorsal side (Area B1, Quadrant 3, Layer 2).*

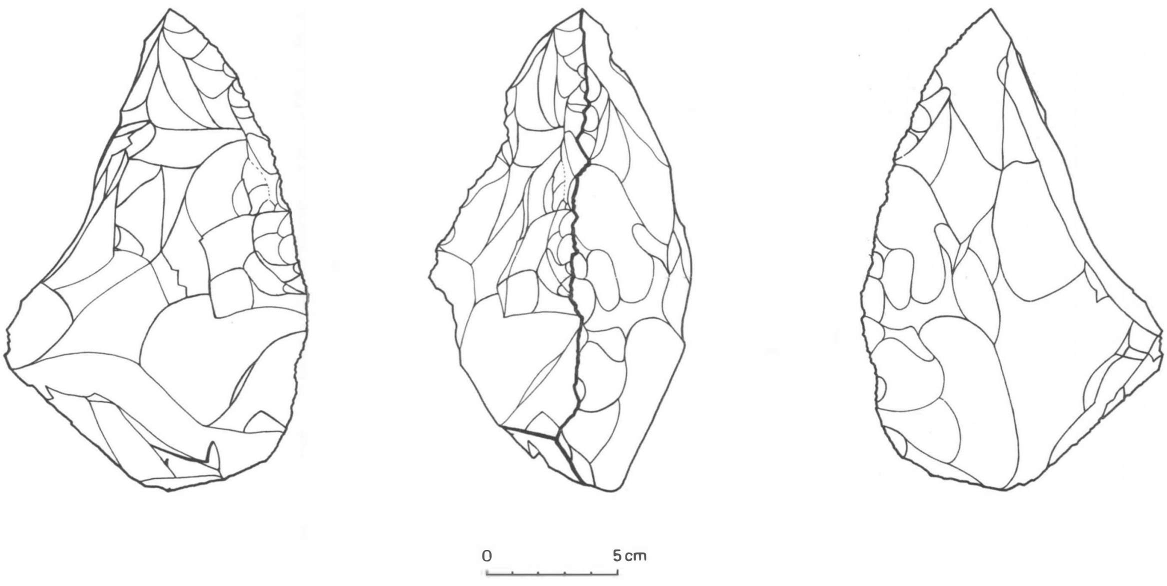

Figure 3 *Three views of cresting on blade core from D5 shown in Plate 4.*

products left behind to reconstruct how artefacts were made. In our case this rather inductive procedure was carried out by moving backwards and forwards among models, experiments and archaeological material. We began by sorting the collection into different technological types, defined initially in terms of the debitage produced by strategies for blade production which were used at quarries in other parts of the world (e.g. Holmes 1919; Torrence 1979, 1984, 1986; Clark 1977; Leach 1984). The presence or absence of particular types of debitage is a good indicator of which procedures were used to prepare cores and to detach blades. In addition, the proportion of different types is useful for evaluating the degree to which particular techniques were favoured over others. Classification of the waste from Umleang also revealed several types of flakes which resulted from the particular methods of core preparation used at Umleang (e.g. Fig. 2). We did not attempt to refit material in a systematic way, although we did find a small number of joins.

The first activity that many Umleang knappers engaged in was to test the quality of the raw material which had been brought up from the shaft. Since the exterior surface of the obsidian nodules quarried at Umleang appears to have been relatively smooth and regular, there was little need to systematically decorticate nodules. On the other hand, the raw material is not completely homogeneous and can contain hidden flow structures which would cause difficulties during core preparation. Consequently, people often knocked off large flakes or split the cobbles open in order to test for flaws and to examine their potential for further work. The core in Plate 2, for example, was rejected because it failed to meet the desired criteria. In other cases a more pragmatic approach was taken: one simply began preparing the core. If the material responded well, then it passed the test and could be moved to another area for further work. Given the debitage collected at the site, it appears that very little raw material was rejected in this early stage of working.

Having selected an appropriate nodule, the next stage was to prepare the periphery of the core. In order to remove blades from a surface, two basic requirements must be met. Firstly, the surface must be curved rather than flat. Secondly, the area for removing blades must be relatively smooth and level and therefore free of irregular cortex, inclusions and deep flake scars. Getting the correct curvature on the periphery of the core is particularly important if an overlapping series of blades is to be removed. As in many other parts of the world, the knappers at Umleang placed one or more crests at appropriate points around the circumference of the core to achieve the desired curved surface (cf. Pélègrin 1984; Torrence 1984). Pélègrin (1984) has demonstrated that the placement and number of crests required depend on the initial shape of the nodule. In the majority of cases two crests will be placed at opposite ends of the intended surface. The opposing slopes running down from the peak of the crests create the desired rounded shape.

Figure 4 *Blade core fragment from D2.*

At Umleang crests were manufactured by striking blows in alternate directions along a line running perpendicular to the intended platform, as illustrated by the cores in Plates 4 and 5 and Figure 3. Although there is some overlap with the trimming flakes used to prepare the core platform, many of the flakes which resulted from fabricating the crests are very distinctive. In order to make the negative scars which define the crest as deep as possible, blows were often struck repeatedly in one place, one on top of the other. The resulting flakes generally have crescent-shaped platforms and characteristic deep, 'pocket-shaped' scars on their dorsal surfaces, as illustrated by the examples in Figures 2a, 2b and 2d. Another more careful approach was also employed to yield deep scars. In this case the intended platform on the crest was trimmed back very carefully. A blow could then be placed accurately enough so that only one flake would be needed to achieve the desired shape for the scar. Consequently, some flakes bear a series of small, overlapping negative scars at the proximal end of the dorsal side, as in Figure 2c. Not only do the distinctive platforms, dorsal scar patterns, and dorsal trimming make these two types of flake easy to recognise in the assemblages, but both forms also have very bulging bulbs of force.

In addition to forming a projection, many of the flakes involved in the cresting process were also struck in such a way that they would not leave irregular bumps and hollows on the surface adjacent to the crest because it was intended for blade manufacture. As a result, the flakes, which are very thick at the bulbar end, taper off considerably along their length, and become very thin at the distal end. The resulting longitudinal section of the flakes is highly distinctive. As revealed from the cores themselves, however, some of the flakes struck from the crest were used intentionally to clear off irregularities on the intended core surface. In general, these flakes are long and thin. Since their shape is not consistent, they cannot be differentiated from flakes derived from a wide range of activities.

In many blade technologies (e.g. Clark 1977; Leach 1984; Pélègrin 1984; Torrence 1979, 1984), crests formed on the edge of blade cores are deliberately removed in the first stage of blade production. The triangular cross-section of the ridge means that it is relatively easy to remove a blade with very straight, parallel edges. The scar left behind can then be used to direct the force in such a way as to yield further blades. This practice may have been used at Umleang on some occasions, as, for example, in the case in Plate 5, but it does not appear that crests were consistently removed for this purpose. Crested blades (as opposed to irregularly shaped flakes) bearing bulbar scars on both sides of a ridge are extremely rare in the samples of debitage collected at the site (Tables 4a and 4b). Furthermore, since they are absent from the museum collections of spears studied by Torrence (in prep.), fully crested blades were not used to make spears either during the period when mining took place nor in recent times when waste scavenged from the site was reused for making tourist items. In some cases, however, only the dorsal ends of the

Table 4a *Assemblage composition of samples from village areas, D1 and D2.*

Technological Class	D1 n	D1 %	D1 % cortical	D2 n	D2 %	D2 % cortical
Unmodified nodule	2	0.6	100.0	1	0.9	100.0
Flake core	1	0.3	100.0	0		
Flake	191	60.6	34.0	74	65.5	23.0
Chip	42	13.3	38.1	26	23.0	19.2
Blade	13	4.2	7.7	2	1.8	0.0
Crested blade	4	1.3	25.0	1	0.9	0.0
Crested flake	11	3.5	45.5	2	1.8	50.0
Preparation flake	34	10.8	20.6	7	6.2	14.3
Trimming blade	17	5.4	29.4	0		
Total	315		33.0	113		22.1

Table 4b *Assemblage composition of samples from mineshaft Area B1. Note that for Quadrants 2 and 4, crested and trimming blades were recorded as blades.*

Technological Class	Quadrant 3 n	Quadrant 3 %	Quadrant 3 % cortical	Quadrant 2 n	Quadrant 2 %	Quadrant 2 % cortical	Quadrant 4 n	Quadrant 4 %	Quadrant 4 % cortical
Unmodified nodule	4	0.3	100.0	0			0		
Flake core	7	0.6	100.0	0			3	1.7	100.0
Flake	668	53.7	44.3	125	66.8	45.6	100	55.2	69.0
Chip	248	19.9	57.7	26	13.9	61.5	39	21.5	35.9
Blade	58	4.7	41.4	8	4.3	37.5	6	3.3	33.3
Crested blade	8	0.6	50.0						
Crested flake	25	2.0	44.0	3	1.6	33.3	4	2.2	100.0
Preparation flake	219	17.6	29.7	25	13.4	32.0	29	16.0	58.6
Trimming blade	8	0.6	12.5						
Total	1245		44.1	187		45.5	181		60.2

scars from one side of the crest are present on the spear blade, suggesting that the blade had been removed from the edge of the surface directly adjacent to the crest. In addition, the crests represented on the cores at the site as well as on the dorsal surfaces of some flakes are much too irregular in both plan and section to have been removed as blades with parallel sides. Instead, the major function of cresting at Umleang appears to have been to form the necessary rounded surface on the periphery of the core.

Having created the appropriately shaped and smoothed surface, the next task is to prepare a platform for the core. Most of our information about platform preparation comes from the small numbers of blades collected at Umleang (Plate 3) and from observations made on museum collections of the small number of spear blades which have become detached from their hafts. The majority of those which were made prior to World War II and before intensive scavenging began were struck from prepared cores (Torrence in prep.).

Two major methods were used to create and maintain platforms. To begin with, a number of short flakes were struck from the top of the core down the face. This procedure was designed to strengthen the edge and later in the process to remove the protrusions resulting from the negative bulbar scars of previous blades. The results of trimming the platform are visible at the proximal end of the broken spear blade and on the dorsal surfaces of some of the blades at the site, but the technique was clearly not necessary in every case, as illustrated by the fragments in Plate 3. As well, a parallel line of small flakes were struck from the edge of the platform across it. These would have further strengthened the edge, but were also intended to create the desired angle between the platform and the core face. Facetted platforms similar to the one on the broken spear blade are common on the prismatic blades in museum collections.

Both triangular and trapezoidal forms are present in museum collections of obsidian spear blades (Torrence in prep.). Although prismatic blades were not well represented in the systematic surface collections, a concentration noted in Area B2 appears to have been overlooked by modern scavengers. It is interesting that these blades fall within the size range of those observed in museum collections. Some have even been retouched. Observations made in the field are reported in Table 5; at the time the possible

Table 5 *Blades from Umleang, Area B2.*

Length (cm)	Width (cm)	Cross-section	Retouch
12.0	1.6	triangular	unifacial
11.5	5.2	triangular	unifacial
7.3	4.1	triangular	unifacial
13.0	2.7	lenticular	absent
15.4	6.2	triangular	absent
11.1	3.5	trapezoidal	absent
11.6	3.0	trapezoidal	absent
11.1	3.4	triangular	absent
11.2	4.7	trapezoidal	absent
10.4	3.4	trapezoidal	unifacial
14.4	6.3	trapezoidal	absent
11.4	4.4	trapezoidal	absent
12.6	2.6	trapezoidal	absent
8.0	2.2	trapezoidal	absent
Mean: 11.5	3.8		

reasons for why these should have been rejected at the quarry were not noted. Although trapezoidal blades were most common in this grab sample, triangular blades made up 63% of the sample from Quadrant 3 in sample B1 and 50% of sample D1. These proportions may not be representative of original production, however, since blades have been scavenged from the site recently for re-use as tourist items. Nevertheless the presence of trapezoidal blades both at Umleang and in museum collections demonstrates that a series of blades were often struck from the cores, again highlighting the importance of cresting. Furthermore, in order to remove more than one layer of blades from a core, it is highly likely that both the core platform and its periphery would have needed to be rejuvenated from time to time. The presence of a small number of flakes and irregular blades bearing remnants of crests in the systematic samples (Tables 4a and 4b) demonstrates that cores at Umleang were modified occasionally in order to prolong their useful life. In addition, since many trapezoidal blades observed in museum collections do not bear negative bulbar scars left from previous blade removals, the core platform must also have been rejuvenated before the second set of blades was detached. Surprisingly, core tablets and other smaller flakes used to remodel the angle of the platform to the core face have not been recognised in the samples of debitage collected at Umleang.

Percussion flaking at Umleang was carried out with hammerstones, some of which were discarded at the site. Six small battered and pitted cobbles of water-worn, fine grained volcanic rock were noted on the surface of the pile of waste near Shaft 19 in Area B. Their dimensions are given in Table 6. All would have been suitable for preparing cores or for removing blades. As noted previously, small beach pebbles of obsidian were also used as hammerstones.

BLADE CORES

The general blade reduction sequence as described above can be further substantiated by examining the individual histories of cores which were rejected at the site. Surprisingly, blade cores appear to be extremely rare at Umleang. Only four examples were collected from the village area, including three as grab samples (Table 7) and one fragment from the systematic surface collection (Fig. 4). Fortunately, this very small sample nevertheless includes cores from several stages of manufacture. Because the sample is so small, the information derived from each core is quite important for assessing the general reduction sequence and for establishing a basis for further research. Detailed descriptions of three of the cores therefore follow.

The core from grab sample D5 was nearly ready for blade manufacture when it was rejected (Plate 4; Fig. 3). The maker had run into a number of problems caused mainly by faults in the obsidian, for example large inclusions of rhyolite, a fracture running through the raw material, and natural cleavages which had created deep parallel steps in several places. The core is quadrilateral in cross-section and pyramidal in longitudinal section. Crests were flaked on two opposing edges; two surfaces had also been partially clear-

Table 6 *Hammerstones from Umleang.*

Location	Material	Length (cm)	Width (cm)	Thickness (cm)
Spoil heap near Mine shaft 19	fine-grained volcanic	10	6	4
	"	8	4	3
	"	10	8	6
	"	9	6	4
	"	9	6	3
	"	6	5	3
SW Quadrant, B1	"	16	15	broken
	"	6	4	
	"	7	5	broken
D4	obsidian	6	3	2

ed off in preparation for blade manufacture. On both surfaces there were two phases of flaking which derived from the crests. Firstly, long shallow flakes were struck across the entire face. Secondly, short, deep flakes were terminated quite close to the edge of the crest.

Table 7 *Blade cores from Umleang.*

Attributes	D3(i)	D3(ii) (Plate 5)	D5 (Fig.3, Plate 4)
Length (cm)	16.2	14.5	19.8
Width (cm)	6.7	6.4	12.7
Thickness (cm)	5.6	3.6	9.0
Weight (g)	788.9	382.4	1871.2
Platform length (cm)	4.7	5.6	7.3
Platform thickness (cm)	1.8	5.6	10.7
Platform preparation	none	multiple scars	none
Number of Crests	1	2	2
Stage of reduction	late	late	early
Rejuvenation	yes	yes	no

One surface was probably quickly abandoned because attempts had failed to clear the abundant fracture planes existing in the raw material. The other surface has no such faults, but the angle of the surface to the potential platform is extremely acute. No attempt had been made to prepare the platform. The difficulty of removing a large inclusion and a natural fracture on the current platform may have also deterred further efforts at shaping the core. Furthermore, if the angle of the platform to the core face had been corrected, then the overall length of the core would have had to be reduced considerably. Blades struck from the shortened core would probably have been too small for spears.

The second core, D3(i), is extremely interesting because the crest has been created on the side of a thick flake, rather than on a nodule. The flake may have been struck from a blade core as there is one nearly complete irregular blade scar (10.1 mm in length; maximum width 2.8 mm) on its dorsal surface and possibly the distal end of a second blade scar adjacent to it (maximum width 2.1 mm). These two scars were struck from opposite ends of the original core. Both were too narrow to have been used as spear blades. After the flake was detached, a crest was created down one margin. The majority of the flaking is confined to the dorsal surface, with very few flake scars on the already flat ventral surface of the flake. The platform of the flake was totally corticated and had not been modified before the core was abandoned. The reasons for selecting the flake as a core in the first place are as puzzling as why it was finally rejected. Certainly the bulb of percussion on the flake and the already small platform would have been foreseen as obstacles in successfully removing blades. The re-use and/or complete reduction of cores, as illustrated by this case, however, may help explain why there are so few blade cores at the site.

The third core, D3(ii), seems to have proceeded farthest along the manufacturing sequence before it was rejected (Plate 5). Like the previous example, it also represents an attempt to rejuvenate an exhausted blade core. The present core represents the distal fragment of a larger core which had been split by a blow to its side. Prior to this, blades had been removed on the curved surface between the two crests (Plate 5, b), but not on the opposite side, which is much too irregular. The sequence of flaking along the crest is identical to the core in Plate 4 and Figure 3, i.e. long shallow flakes succeeded by short deep ones. One crest is aligned along a cleavage plane present in the raw material (Plate 5, c). The flaking merely served to emphasise this flaw and ended up creating a huge cavern down the side of the core. An irregular blade struck from the platform of the original, larger core had removed all but the distal end of the other crest (Plate 5, a). It seems unlikely that this blow was used to initiate a series of blade removals, since a larger blade had already been struck from the core prior to the removal of the crest. More likely, the knapper was simply trying to squeeze as many blades out of the core as possible.

The blow which truncated the parent core created an angle of approximately 90° between the top of the new core and its sides. Along one edge of the new platform, however, a rhyolitic inclusion had formed a bump, which could not be levelled off, although many blows were directed at it. At the opposite side of the platform, a row of four short flakes were struck down the side of the core from the core platform but one of them terminated in a deep hinge, thereby ruining the chances of successful blade manufacture from that area without a great deal of effort to clear it off. By this stage the core was already too small to have produced blades large enough to have been hafted as spears. Perhaps the rejuvenation was intended to produce other blade tools, such as the *tak*, which were supposedly placed in pits to deter intruders from ambushing the settlement. This core is really the only example from the site of an effort to economise on the use of raw material by maximising the number of blades per core.

Table 8 *Descriptive statistics for waste by-products from Areas B1 and D1. Only complete artefacts measured. Measurements are: axial length, axial width, maximum thickness, platform length, and platform width. All measurements are in mm.*

Technological Class	Number	Length		Width		Thickness		Number	Platform Length		Platform Thickness	
		Mean	SD	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Prismatic Blade, Triangular												
B1	5	91.2	33.0	38.0	11.4	12.6	4.4	5	17.6	12.0	5.0	2.6
D1	3	51.7	16.6	20.0	1.0	7.7	2.9	2				
Prismatic Blade, Trapezoidal												
B1	5	102.2	43.4	35.4	8.9	11.8	2.3	5	24.0	6.8	8.8	1.3
D1	0							0				
Blade, Other												
B1	18	69.9	41.5	32.0	28.2	11.2	10.3	16	16.0	11.9	8.2	7.1
D1	2							2				
Crested Blade												
B1	6	87.0	44.5	41.3	13.1	24.5	6.4	6	17.3	6.8	8.8	5.7
D1	3							3				
Flake												
B1	245	51.5	29.6	51.2	30.5	13.8	10.0	221	27.2	24.0	10.2	7.4
D1	79	49.0	41.8	40.4	31.2	10.3	8.9	62	21.7	15.4	22.6	123.9
Crested Flake												
B1	16	83.7	27.9	68.1	21.0	22.6	12.5	16	28.1	17.7	13.6	8.7
D1	5	74.4	57.4	60.8	26.9	14.2	6.1	5	18.4	11.6	8.0	5.0
Preparation Flake												
B1	133	38.7	19.4	35.9	19.2	22.6	12.5	110	18.2	12.6	5.1	3.8
D1	21	31.2	16.0	33.4	14.0	14.2	6.1	18	18.4	11.6	4.5	2.5
Trimming Blade												
B1	5	54.8	24.3	26.0	6.9	5.6	1.9	4	9.5	5.3	3.0	0.8
D1	2							2				

A QUANTITATIVE APPROACH TO REDUCTION SEQUENCES

A final method of describing the reduction sequence used at Umleang is to identify whether it was characterised by discrete stages. Rather than focussing on the presence or absence of diagnostic artefact types, as was done in the previous descriptions of production, one can also explore patterning in the size and shape of the waste by-products (summarised in Table 8). The data are plotted on a 'core reduction chart' (Witter et al. in press) in which the maximum thickness is represented by the vertical axis and the horizontal axis portrays the surface area of the artefact, measured here by the axial length times the axial width; the square root of the value is used to give a better visual presentation. Note that in this analysis axial dimensions are used because block sizes as defined by Witter et al. were not measured. What the scatter plot does is to provide a graphical portrayal of the different forms of cross-section represented in the sample of artefacts. The reduction charts for Areas B1 and Dl are given in Figures 5a and 5b. When illustrated in this way, no specific stages can be detected since variation in cross-section is nearly continuous.

One reason why stages are not represented by these reduction charts may be that the waste in these samples was generated by a combination of different activites, of which blade production was only one. To get around this problem, therefore, we prepared a reduction chart in which only complete blades are represented (Figs 6a and 6b). For the mine shaft sample B1 the result is striking (Fig. 6a). At least three and perhaps up to five clusters of blades are segregated.

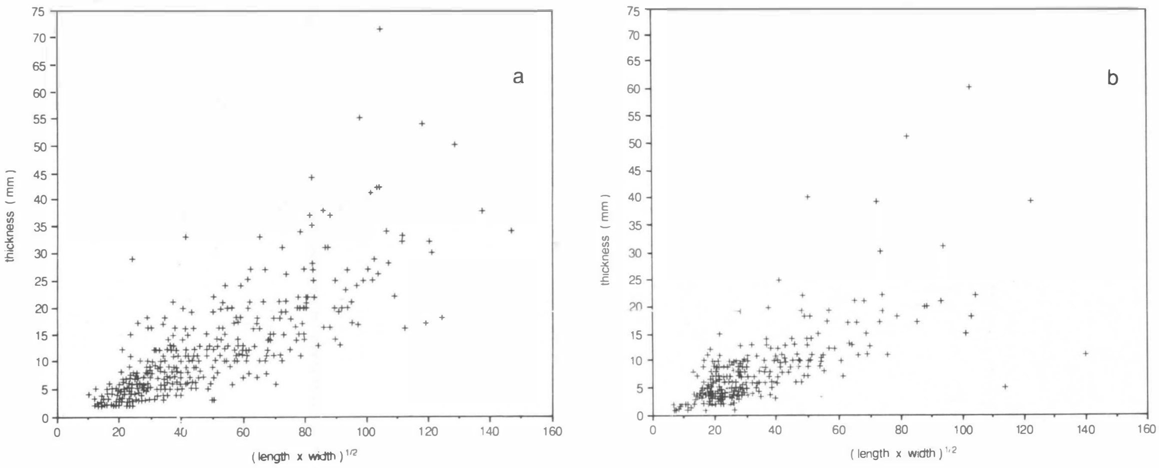

Figure 5 *Comparison of the degree of core reduction (a) at the mine shafts (B1) and (b) near the village (D1). As the core is reduced flake shape will change so that the earliest stages of reduction are represented at the upper right hand part of the graph, while the small thin flakes near the origin are most common in the latest stages.*

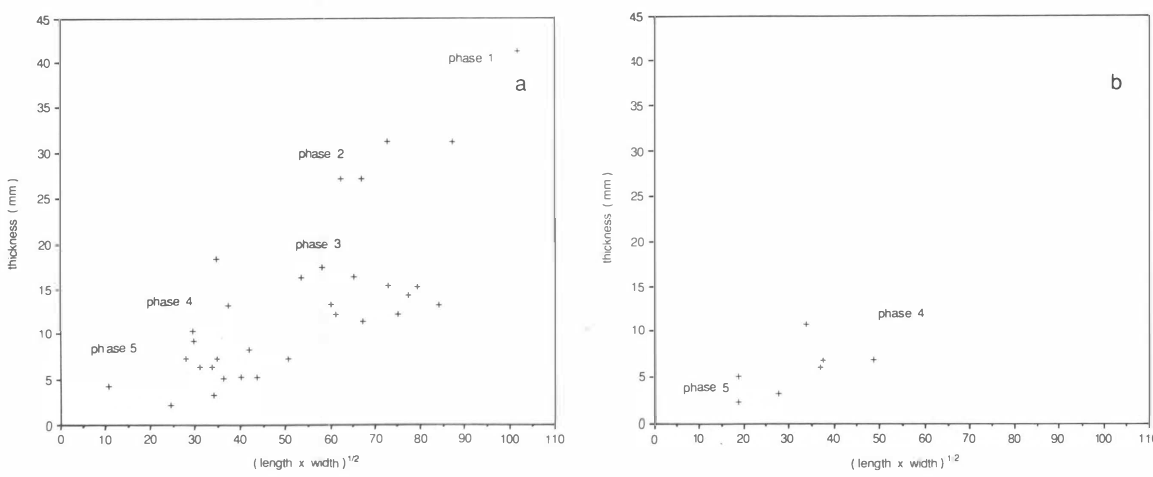

Figure 6 *Stages of core reduction as represented by complete blades: (a) mine shaft (B1), (b) village (D1).*

Each group may represent a different stage in the reduction sequence. The single blade in the top right of the diagram is much too large and thick to have been useful in reducing a blade core; it is probably the waste from initial testing of raw material. The second group, labelled Phase 2, would have been removed early in the process of blade production as the shape of the core was being established. In support of this interpretation, it is important to note that all the blades in these two early phases bear cortex from the original outer surface of an obsidian nodule. Phases 3 and 4 are interpreted as the primary production of useable blades. The Phase 5 blade is so small that it would not have been acceptable for a spear or dagger blade and is better explained as the result of reusing a blade core to make other types of artefacts, such as the small *tak* blades previously described. It is worth noting that the complete reduction of cores, as represented by Phase 5, did not take place very often.

The absence of Phase 3 blades at the village (Fig. 6b) could be interpreted to mean that blades with this shape and size were originally produced for export or have been scavenged more recently for the tourist trade. Their presence at the mineshaft and not at the village can be explained if scavenging has been heavier nearer modern settlement and has therefore been less intense at the mineshaft. Furthermore, the blades in Area B1 could have been rejected not because they were the wrong shape (as seems to have been the case with the examples collected in Area B2), but for some other reason. Many of them bear cortex, which could have been considered undesirable by consumers. Another possibility is that the gap between what we have called Phases 3 and 4 represents the items which have been selected from the total range of blades (including both Phases 3 and 4) and have been removed from the site. In future work it will be interesting to compare the blades from Phases 3 and 4 with the finished products as represented by hafted spear and dagger blades in museum collections.

EFFICIENCY OF PRODUCTION

One of the goals of the study of waste by-products from Umleang was to acquire information concerning the levels of efficiency in the industry. Torrence (1986) has argued that relatively high levels of efficiency are associated with production aimed at achieving material gains. Since the exchange system among villages in the Admiralty Islands was based on balanced reciprocity and the achievement of social status rather than material profits, then one would predict that the minimisation of time, energy and raw material inputs would not have been an important factor in the way blades were manufactured at Umleang until at least the turn of the century, when tourist production began to have an effect on production (Torrence in prep.).

The data collected in 1985 certainly does not contradict this hypothesis, although there is an interesting difference between the extraction of obsidian and the production of spear blades. The miners may have made efforts to conserve time and energy. It is quite striking that there are almost no unmodified nodules of obsidian among the surface deposits in the vicinity of Shaft 19 and very few blocks appear to have been rejected in the early stages of flaking. The absence of rejected raw material argues either that everything mined was used up regardless of its quality or that raw material was carefully evaluated at the bottom of the shaft before it was lifted to the surface. Another possibility is that miners attempted to protect their monopoly over ownership of obsidian by not leaving any useable material on the surface for others to steal. In contrast to the mining operation, the people flaking the obsidian show very little concern for conserving raw material. As already noted, cores do not appear to have been rejuvenated or reused except in unusual circumstances.

As well as conserving raw material, efficiency can also be achieved by adopting simplified and standardised methods of working as well as using special purpose tools and skilled, specialist personnel (Torrence 1986:42-6). Although specialists are said to have carried out the knapping, they do not appear to have been highly skilled workers. In general, the flaking has a very rough and ready appearance. Hinge fractures are common both as terminations (29% in B1, Quadrant 3; 36% in sample D1) and on the dorsal surfaces of flakes. An overlapping series of large scars with step terminations is present on a sizeable proportion of the proximal ends of the dorsal surfaces of flakes from the mine shafts, demonstrating that knappers struck cores not only with considerable force, but also repeatedly in one place before the flake was detached. As noted above, in making crests, people frequently removed an overlapping series of flakes rather than carefully preparing a platform and striking off one ideally-shaped flake. As well as requiring less skill, this form of cresting is wasteful in terms of raw material and possibly in terms of time and energy as well.

Precision of flaking can be evaluated by measuring the tightness of the relationship be-

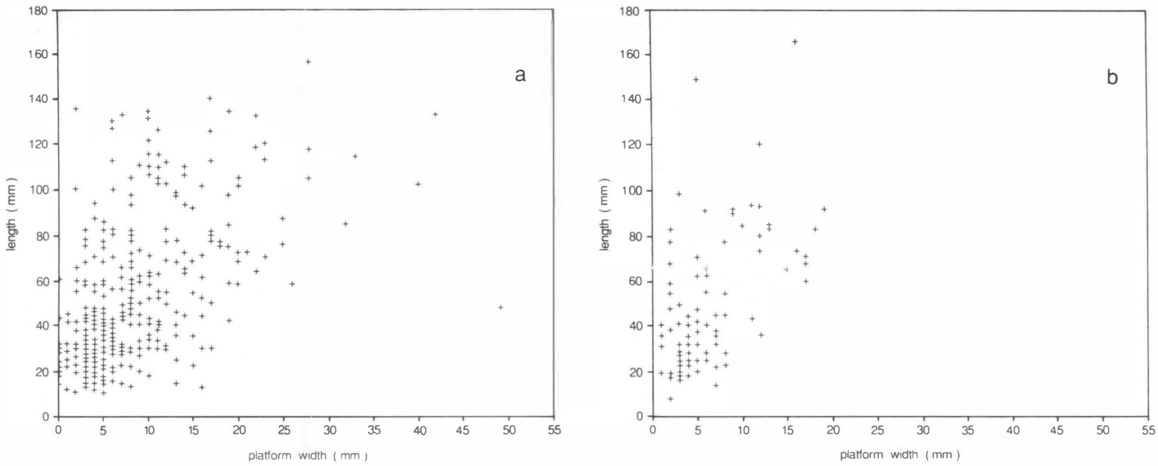

Figure 7 *Precision of flaking at Umleang expressed graphically as the relationship between flake length and platform width: (a) mine shaft (B1), (b) village (D1).*

tween flake length and platform width (cf. Witter et al. in press for an explanation of this technique). As shown in Figures 7a and 7b, the flaking as represented in samples B1 and D1 is highly variable and imprecise. One could argue that the apparent lack of consistency in precision in the data at Umleang is the product of lumping together the products of many knappers operating over a long time period, but each one was quite consistent. If skilled specialists had produced these by-products, however, one would still expect to find clustering in the data and not such a continuous distribution of values. In contrast, the data are most consistent with the hypothesis that the knappers at the site did not maintain high levels of precision because they were not attempting to be efficient and therefore were not trying to obtain standardised results.

As noted above, the metrical data depicted in the reduction charts for Areas B1 and D1 are also not standardised (Figs 5a and 5b). Instead, the cross-sectional characteristics of the waste by-products form a continuous distribution with no clusters and no real breaks. Clearly, there was no attempt to remove flakes having well defined sizes and shapes. Although several types of manufacture may be represented in the samples, therefore blurring any patterning that existed in waste by-products, one would still not expect such high levels of variation. The lack of standardisation is also well summarised by the enormously high values for the coefficients of variation of complete flakes and blades as shown in Table 9. On average, the values well exceed those obtained from the Melos obsidian quarries, where it has already been argued that production was competent but neither tightly organised nor efficient (Torrence 1986:198-200).

Specialist production is certainly not negated by the extremely low levels of efficiency and standardisation present at Umleang, although the lack of skill is quite puzzling. As clearly shown by these data (cf. Torrence 1986:45-6), not all craft specialists produce in economic settings in

Table 9 *Coefficients of variation for waste by-products. Only complete artefacts are included. Sample sizes for prismatic blade, crested blade and trimming blade in Area D1 were too small for meaningful results.*

Area B1-Quadrant 3	Number	Length	Width	Thickness	Number	Platform Length	Platform Width
Flake	245	57.6	59.5	72.1	221	88.3	72.4
Prismatic blade, triangular	5	36.2	30.0	34.9	5	68.2	52.0
Prismatic blade, trapezoidal	5	42.5	25.1	19.5	5	28.3	14.8
Blade	18	59.3	88.0	92.0	5	74.3	87.0
Crested blade	6	51.2	31.6	26.3	6	39.2	64.6
Crested flake	16	33.3	30.7	35.4	16	63.0	64.0
Preparation flake	133	50.1	53.4	63.3	110	68.9	73.5
Trimming blade	5	44.4	26.5	34.8	4	56.0	27.2
Area D1							
Flake	79	85.2	77.1	86.1	62	71.0	547.5
Crested flake	5	77.1	44.1	43.2	5	62.9	62.5
Preparation flake	21	51.5	42.0	75.4	18	52.5	55.1

which they need to be cost effective. The data from Umleang demonstrate that production by specialists for goods which are exchanged within a system of balanced reciprocity can be quite unorganised and wasteful of resources.

SPATIAL PATTERNING

Since the historical accounts claim that specialist craftsmen were responsible for mining obsidian and for the manufacture of tools, we might expect to find that different stages in each task were differentiated in space (cf. Torrence 1986). Furthermore, if each of these activities was carried out by different groups of specialists, as suggested by recent informants, then the production of artefacts can be expected to have taken place away from the mines. It would therefore be very interesting to know whether any of the stages in the preparation of cores or the manufacture of specific tools had taken place near the shafts. Similarly, one might expect to find that artefacts made by the specialist craftsmen had been produced in special areas reserved for that activity. So, for example, the fabrication of spear and dagger tips might be restricted to just a few localities, whereas the production of unretouched flakes and small blades might be more widespread.

Although obsidian and pottery is scattered widely over the site as a whole, it was possible to discern several very dense clusters of waste material (Fig. 1). In line with our predictions, these concentrations could be hypothesised as either places where particular activities took place, or more likely, given the nature of obsidian debitage, purposeful deposits of waste by-products from these events. The extremely sharp edges of flaked obsidian create a serious hazard for people who do not wear thick-soled shoes. Consequently, in many parts of the world the waste from tool production is carefully collected up and discarded in special locations (e.g. Torrence 1986:65-6). Although the concentrations at Umleang are probably purposeful dumps and not the flaking floors themselves, one might still expect the contents of the waste heaps to reflect differences in the use of space at the site (cf. Schiffer 1976).

To begin with, there is a very heavy scatter of material in the vicinity of the mine shafts (Areas A and B in Fig. 1). Some of the small piles of waste located adjacent to a particular shaft are quite discrete. If tasks had been differentiated, then these might represent places where nodules were evaluated for size, shape, and quality and where the early stages of core reduction took place. If not, then all phases of manufacture might have been carried out directly adjacent to the source. A sample of waste was collected by hand from 1 m^2 in the middle of a pile associated with Shaft 19 (B1). The square was divided into four equal quadrants. Obsidian lying directly on the surface was picked up for all the quadrants, but in the southwestern corner (Quadrant 3) collection continued until all visible obsidian was removed. The Quadrant 3 sample therefore comprises all material down to a depth of c.25 cm below the surface of the pile of waste.

Secondly, local informants were able to identify the site of the old village, which is located on the edge of Pulme Pwar gully c.100 m to the south of the area of the mine shafts (bounded by Areas D, G and F on Fig. 1). An elderly informant was still able to recall when people lived there and to both identify the location of previous houses and to remember who had occupied them. Several modern residences are still located nearby. Interestingly, although the area of the mine shafts does not appear to have been defended, the village is said to have been heavily palisaded and booby traps containing obsidian flakes set on end were supposedly hidden along paths leading up to it. Two large mounds of obsidian waste are located on the periphery of the settlement at Areas E and G. It seems likely that these represent specialised disposal areas, but these were not sampled.

Two transects with shallow shovel pits (c.5-10 cm. deep) spaced at roughly 5 m intervals were made across the centre of the settlement, where the houses were located, in order to determine the density of obsidian (Fig. 1). The results are quite interesting because they support the notion that this area was kept clean and that obsidian was very carefully disposed elsewhere. In the first transect only 42 flakes were collected in seven pits over a distance of c.35 m (on average one per metre), but nearly all the sample comes from two pits located on the periphery of the village near the scree slope. In the second transect only one flake was recovered, again on the edge of the village and in the direction of the mine shafts. Since there was no point in sampling the village itself, two systematic samples were taken from Area D just to the north. These could be used to test the hypothesis that the quality of flaking and/or the stages of manufacture differed from those which took place in the mining area. At D1 an area of 0.25 cm^2 was excavated in three arbitrary levels (0-10 cm, 10-20 cm, 20-25 cm). The sample from D2 is composed of the surface material from 1 m^2.

The third major concentration of obsidian, at Area F in Figure 1, is represented by vast dumps of obsidian waste spilling down the ridge slopes into the Pulme Pwar gully. Because of their enormous size and density, it seems likely that they represent a mixture of general purpose household discard possibly also mixed with by-products from specialist production. These dumps were not sampled.

During surface reconnaissance the composition of surface scatters of obsidian debitage appeared to be reasonably similar. In contrast to the ethnographic accounts and local memory, the archaeological data from Umleang show that mining and blade production were not always segregated in space. At Area B, for example, some blades were manufactured directly adjacent to Shaft 19. Not only were retouched blades present in the nearby pile of waste (Area B2; Table 5), but also in the samples from Area B1 blades make up as significant a proportion of the total assemblage as they do in the village area at D1 (Tables 4a and 4b). The reduction chart for complete blades (Figs 6a and 6b) also demonstrates that the entire sequence of blade production took place near Shaft 19. The presence of crested blades and flakes as well as core preparation flakes and blades further demonstrates that blade cores were definitely prepared here. We cannot determine from the data whether core preparation and reduction were carried out by the same people who mined the obsidian or by separate specialist knappers: the close proximity of the two activities, however, does suggest that the two tasks were not always carefully differentiated.

Although blades were produced at the mine shaft, in many cases partially prepared cores were transported to the village. A comparison of the composition of the debitage from Areas B and D shows that near the mining area core reduction was more likely to be terminated at an earlier stage than in the village. For example, when compared to the village, a higher percentage of the artefacts at the shaft area bear cortex from the original outer surface of an unworked nodule (Tables 4a and 4b). Cortex is predominant on the undifferentiated flakes and chips in sample B1, artefact types which were largely produced in the early stages of testing the raw material and when levelling off the periphery of the core. Interestingly, very high proportions of blades and crested blades at the mine shaft also have cortex remaining on them and consequently must have been removed at a very early stage of core reduction.

The metrical characteristics of the waste by-products also illustrate spatial patterning. When reducing a core, it can be expected that large, thick flakes will be removed first and these will be followed by progressively smaller and thinner flakes (cf. Witter et al. in press). The scatter plot of the dimensions of complete flakes from B1 and D2 can therefore be used to identfy which stages of core reduction were carried out at each locality. As discussed above, both the graphs in Figures 5a and 5b exhibit a continuous distribution of values, suggesting no breaks in behaviour at each area. Furthermore, since a very wide range of flake sizes and shapes are present in both areas, one can infer that all stages of flaking are present in both areas of the site.

Despite these similarities, however, the differences between the reduction charts are also instructive. The plot of sample D1 has a tighter concentration of cases located near the origin, representing the smaller, thinner flakes. In contrast, the data from B1 are slightly more spread out, with a higher proportion of flakes belonging to the larger, thicker variety. Again one can conclude that the earlier stages of core preparation were more likely to be carried out at the mine shafts, whereas the village area witnessed slightly more of the later phases of manufacture.

A close inspection of diagnostic traits also reveals the subtle differences between the two areas. Although the proportions of waste comprising core preparation debris (i.e. the crested blades and flakes, preparation flakes and trimming blades in Tables 4a and 4b) are almost identical in samples D1 (21.0%) and B1 Quadrant 3 (20.8%), blades make up a greater percentage of these types in the village sample (6.7% to 1.2%). The different degrees of bladedness are also expressed in the types of scars on the dorsal surface of artefacts (Table 10). The percentage of by-products bearing blade scars is much greater in the sample from Area D. In addition, at the mine shaft one or two blade scars may be present on the dorsal surface, but at the village the tendency is for debitage to have two or more blade scars.

Bladedness can also be expressed in terms of how elongated the artefacts are. One can predict that until a blade core is fully set up with long, straight lines to guide the force down the entire length of the core, the earliest set of blades struck off will be shorter than the main group. The plots of 'elongation' (cf. Witter et al. in press) shown in Figures 8a and 8b again reveal both a significant overlap between the samples from the two areas of the site and some interesting differences. Although the bulk of artefacts from each sample lie in the same region, blades (where the ratio of length to width is greater than 2) from the village

Table 10 *Characteristics of dorsal scar patterns on samples from Areas B1 and D1.*

Attributes	Mine shaft Area B1 No.	%	Village Area D1 No.	%
Flake scars:				
0	39	4.0	30	11.2
1-3	713	74.0	177	66.3
>3	212	22.0	60	22.5
Total flakes/blades	964	100.0	267	100.0
Blade scars:				
0	916	95.0	225	84.6
1	23	2.4	5	1.9
2	19	2.0	19	7.1
>2	6	0.6	17	6.4
Total flakes/blades	964	100.0	266	100.0
Numbers of directions of dorsal flake removals:				
1	686	73.4	176	66.4
2	192	20.5	57	21.5
3	54	5.8	30	11.3
4	3	0.3	2	0.8
Total flakes/blades	935	100.0	265	100.0
Trimming on proximal end:				
Absent	498	87.1	204	83.6
Systematic small scars	41	7.2	20	8.2
Multiple large scars	5	0.9	14	5.7
Crushing	7	1.2	1	0.4
One large scar	6	1.0	5	2.1
Large scars with step terminations	15	2.6	0	0
Total flakes/blades	572	100.0	244	100.0

Table 11 *Incidence of platform types in Areas B1 and D1.*

Platform Type	Mine shaft Area B1 Number	%	Village Area D1 Number	%
Corticated	118	21.0	27	17.9
One flat scar	199	35.5	44	29.1
Multiple flat scars	42	7.5	34	22.5
One convex scar	25	4.5	7	4.6
Two facets	26	4.6	5	3.3
Multiple facets	30	5.3	13	8.6
Crushed	116	20.7	9	6.0
Single point	5	0.9	12	8.0
Total	561	100.0	151	100.0

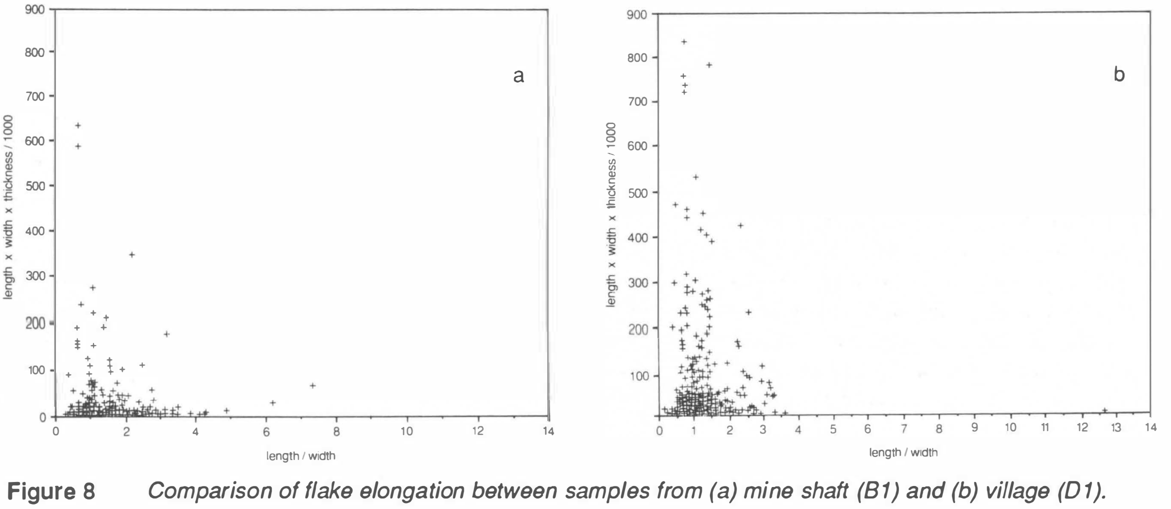

Figure 8 *Comparison of flake elongation between samples from (a) mine shaft (B1) and (b) village (D1).*

tend to be more elongated than those at the mine shaft, suggesting that in the latter area blade production was often terminated in the early stages.

Further support for most of the later stages of flaking taking place away from the mine shaft comes from the distribution of platform types as given in Table 11. It is notable that platforms which are crushed, completely corticated or prepared with a single flat scar dominate near the mine shaft. These are most likely to occur in the early phases of core reduction, when less care is taken to remove a flake with exact specifications, in contrast to later stages when more precision is required. The more frequent occurrence of complex types of platforms, those with multiple flat or facetted scars, at the village, therefore supports the hypothesis that a far greater proportion of blade production took place there, rather than near the source area.

As well as some differentiation in the activities represented, the character of the flaking is also dissimilar in the two areas sampled. To begin with, the pattern of flaking at the proximal end of the dorsal surface varies slightly (Table 10). The difference between the first two types of core preparation may be a product of how the data were recorded (such that 'small' and 'large' were not carefully distinguished) and so the two categories should be taken together. When this is done, it can be seen that core preparation was carried out nearly twice as often in the village. Whether this difference is a product of more careful and possibly more skilled flaking in this area as compared to the mine shaft, or whether this type of core preparation is more associated with the later stages of flaking cannot be determined at present. Possibly both factors were at work simultaneously. The same problem arises with interpreting the much higher incidence of crushing and large step fractures on the dorsal surfaces of artefacts in Area B. Both would be expected if the flaking was less controlled, but this behaviour is also likely to be associated with the earliest stages involving testing of nodules and the removal of large irregularities on potential cores.

In summary, then, there is good evidence for differences in the nature of activities that took place at the two areas sampled at Umleang. Although all stages of core reduction had been carried out both adjacent to Shaft 19 and at the village, in the majority of cases nodules were only partially prepared before they were moved away from the shaft to the village. The majority of blade production took place there. The absence of a strict segregation of activities in space does raise a number of issues about craft specialisation. Does the prediction that activity differentiation will be associated with specialist production need to be modified slightly to take account of the results from Umleang, or are the data demonstrating that tasks were not as specialised as the ethnography would lead one to believe? Were the tasks of miner and spear maker not clearly demarcated? How restricted was access to the knowledge for making blades? Did non-specialists participate at least to some degree? In other words, the presence of some but not completely clear cut spatial patterning may indicate that specialisation in obsidian production was also not so hard and fast as the recent ethnography would lead one to believe. If this was the case, then we might need to reassess the role of craft specialisation in the type of exchange system in which obsidian was circulating.

COMPARISONS WITH ETHNOGRAPHIC DATA

Given the ethnographic and historic accounts, in many ways the archaeological reconstruction of behaviour at Umleang presents no surprises. In other respects, the data highlight a number of interesting issues both for the prehistory of the Admiralty Islands and for archaeological methodology. In the following comparison of the two types of data, we will highlight the differences in an attempt to suggest where future research is needed. The discussion will be focussed on four specific kinds of behaviour: techniques of blade manufacture, form of exports, craft specialisation, and monopoly of raw material.

To begin with, the method for manufacturing blades as reconstructed from the collections at Umleang can be favourably compared to Parkinson's (1905, 1907) descriptions of blade production quoted previously. Clearly, the 'small chips' which he describes were removed from 'one side of the block' in order to fabricate a crest. Further support for our model is provided by the pattern of flaking preserved on an obsidian core which Parkinson sold to the Field Museum of Chicago in 1908 together with three blades and a distal blade fragment (catalogue number 106921). All the blades are triangular in cross-section. One can be refitted on to the core. Although no provenience has been recorded, it seems likely that Parkinson obtained these artefacts when he visited the Admiralties; the core and blades may even be from the same incident which he described.

As it is only 24 cm long, the core donated by Parkinson was probably near the end of its useful life for producing blades suitable for spears.

Unfortunately Parkinson does not appear to have collected any of the small chips used to shape the crest which runs along one side of the core parallel to the line of the blade scars. One side of the crest has been removed by a previous blade, but the other side has been rejuvenated at least once as there are two layers of flakes along it. The three blade scars preserved on the core are only roughly parallel with each other and are not especially regular in shape. The blades were all removed from the same platform which had been previously rejuvenated by a single flat scar which was struck transversely across the top of the core. Before each blade was removed, a row of small flakes was formed along the edge of the platform in order to strengthen the edge and remove overhangs. The two complete blades in the collection bear small, facetted platforms.

In another important respect, however, the archaeological data at Umleang do not substantiate the ethnographic accounts and the evidence from the spears themselves. The final stage of production, the shaping of the pointed tips by direct retouch, is absent at the site. Although the nature of the retouch on the spears has changed remarkably over the past 120 years (Torrence 1989), one would nevertheless expect waste by-products from retouch to be present in the samples collected at Umleang. Given the steep angle of retouch applied at all times, characteristic circular flakes should be relatively abundant. Surprisingly, these are conspicuous by their absence; retouch on any form of artefact is also extremely rare at the site.

One possibility for the absence of retouch by-products is that the flakes are too small to have been recovered adequately by surface collection. Alternatively, Parkinson's description of spear manufacture on Pam could be misleading when applied to Umleang. Retouch on Lou Island may have taken place at a different place from where blades were fabricated so that this separate activity area was not recognised and sampled in 1985. More recently, since the abandonment of Umleang village, retouching spears is very likely to have been carried out in the vicinity of the modern settlement of Rei and therefore outside the boundary of the study area.

The absence of data to suggest that blades were retouched at Umleang combined with the rarity of blade cores there raises a number of questions concerning the nature of production at the site. How important was spear production? What was the most common form in which obsidian was exported from the site? The presence of other diagnostic types of debitage make it clear that at least some blades were manufactured at Umleang: core preparation flakes and blades form a very significant proportion of the total waste, especially in the vicinity of the mine shafts (Tables 4a and 4b). The surprisingly small quantities of the irregular and broken blades that one would expect with a blade industry can easily be explained by the scavenging which has taken place in recent years. It seems less likely that blade cores were also collected and re-used since oral history states that the knowledge of making blades was lost when the mining stopped. Another possibility is that the knappers maximised the number of blades per core and were therefore completely exploiting the cores. On the other hand, if cresting were the main form of core preparation, then the very small number of crested blades and flakes in the samples (Tables 4a and 4b) can be interpreted to mean that very few cores were reduced to such a degree that they needed to be rejuvenated. Even if cores had been recycled and used for another purpose, one would expect to find abundant core fragments and crested debitage, or a large proportion of flakes with blade scars on their dorsal surfaces, but these are all conspicuous by their absence in the samples collected (Tables 4a, 4b and 10). Clearly the two rejuvenated cores described previously do not represent the typical pattern of cores at the site.

A more plausible explanation for the abundance of core preparation waste by-products together with a scarcity of blade cores and core reduction debitage is that Umleang obsidian was mainly exported as preformed cores and not as blades. Cores may have been exchanged directly to consumers within the regional exchange network or perhaps were simply passed on to specialist knappers residing elsewhere, who in turn made spear blades for exchange.

The ethnography, however, may suggest yet another possible type of obsidian export. Parkinson (1907:373) states that blocks of obsidian were commonly exported from Pam Island. Mead (1930:119) also refers to obsidian glass and not to the products themselves. It seems likely that the same was true for Lou. Since Parkinson does not mention that the blocks had been partially prepared, we can assume that he is referring to unworked nodules of obsidian, although the former is still a possibility. The export of unmodified nodules does seem extremely plausible when other evidence is taken into account. For instance, the obsidian flaking which Mikluho-Maclay (described in Nevermann 1934:234) observed makes better sense if raw nodules of obsidian were accessible to consumers as well as spear blades. Furthermore, it is important to note that the islands which exported obsidian also exchanged a number of other products and therefore were not dependent on obsidian alone for their participation in the inter-island network (e.g. Mead 1930:119). In fact, if the exchange rates which Parkinson (1907; data from unpublished translation by N.C. Barry p.327) collected from Sori Island (located north of Manus Island) are correct, obsidian was not a particularly valuable commodity. For instance, a large container of coconut oil was worth thirty and one half fathoms of shell money; a small packet of manganese earth was equal to three fathoms; and ten finished obsidian spears were said to be equivalent to three fathoms. What is even more interesting is that there is virtually no difference in the value of spear blades and unmodified blocks, since one block (which is said to make twenty spears) was worth five fathoms. The extremely low value of completed spears in relation to obsidian raw material implies that the time, effort, and skill needed to make a spear blade had an extremely low value. Another possibility is that obsidian blocks were valued highly in relation to spears because the raw material was used for many other purposes.

If unmodified nodules and unfinished or even partly finished artefacts were the major form of export from Umleang, then it would not be surprising if specialist production had not been a major factor at that site. Presumably the blades that were produced at Umleang were made only for local consumption. The same may also have been true for spear blades in other areas: raw material was imported and the points were made locally, probably by a knowledgeable flaker, but not necessarily one whose speciality was deliberately limited to a few knappers. In this light the weak spatial patterning of activities and lack of skills in the flaking at Umleang also make better sense. If the majority of the spear blades, used in great numbers throughout the Admiralties, were made in various locations outside of the obsidian producing areas, it would also help explain why Parkinson (1905, 1907) had difficulty in finding people on Lou Island to demonstrate blade manufacture. It is hard to believe that the absence of knowledge at that date can be fully explained by the impact of the European presence, since in other areas a large numbers of spears were still available for sale. Furthermore, it is interesting that spears collected during the past 120 years were hafted onto shafts according to two completely different methods, suggesting at least two different centres of manufacture (Torrence 1989 in prep.). Although one method is identical to the one used currently on Lou Island for hafting tourist artefacts (e.g. Plate 1; Ambrose 1975:Plate 1), the other is radically different; instead, it is characteristic of items which were collected from various places around the north coast of Manus Island (Torrence in prep.).

Since raw material was the only obsidian commodity, the people who controlled access to it were the miners. At Umleang there may have been some specialist production of spear blades, but the spatial differentiation of activities could also be explained if the miners preformed the cores at the shafts and consumers produced the blades themselves back at the village. The focus on raw material as the primary object of exchange also helps explain the sophistication of obsidian extraction as exemplified by the presence of shafts. Maintaining a strict monopoly over access to the raw material would have been impossible, given the widespread distribution of useable obsidian near Umleang. Instead, only certain obsidian, that obtained by mining from shafts, was culturally prescribed as suitable for use in spears. Only the people who were given access to the requisite knowledge of how to quarry the obsidian (not a trivial matter; cf. Felder 1979) could obtain the potential exchange item. Note that Parkinson (1907:373) reports the word *bailo* as the best obsidian for spears, whereas recent informants call the spears themselves by basically the same term (recorded by Mitton as *peilo* in the register at the National Museum and Art Gallery, Port Moresby of artefacts collected at Rei in 1975 and also repeated in 1985). Unfortunately, neither the ethnography nor the archaeology can inform us how the system originated: i.e. did the act of mining actually create an exchange good because the shafts defined the resource as owned and controlled, or did the shafts come later as a way to restrict access to obsidian, which was already part of an exchange system?

FUTURE PROSPECTS

Clearly, there is a great deal to be learned concerning the relationships among type of export (spear blade, core, raw nodule), specialist production, and the nature of the exchange that took place in the Admiralty Islands. Further research on each aspect of the system is needed before the interrelationships can be better understood. Equally, there is much more to be learned from studying the site of Umleang itself. Our work is based on a very short season and on quite small samples. A great deal more could be learned from detailed, systematic sampling of a wider range of spatial contexts. Excavation could provide a chronological framework, especially if hydration dating of buried deposits proves feasible. It would be extremely interesting to date the transition from the earlier type of highly retouched triangular point (e.g. Antcliff 1988; Swadling 1981:67) to modern blade production and to trace changes in the industry based on core preforming and manufacture of spear blades. In addition, an attempt to estimate the amount of obsidian that was derived from the shafts as well as the number of cores and blades that were produced from this locality (cf. Torrence 1979, 1986:203-6) could be made given better field data combined with a program of experimental replication.

Our work has focussed on production at the obsidian source. Future research would benefit from broadening the data base to include both the wider regional scale in which obsidian circulated as well as an analysis of consumption. An analysis of assemblages from a number of sites throughout Manus Province would be extremely useful. For example, it would be valuable to find out the relative quantity of the various possible forms of export, to see if spear blades were made everywhere or just at a few sites, to explore the role of specialist production outside Lou Island, and to compare and contrast the role of Lou Island obsidian with that from other sources. The value of obsidian to consumers at various points in the exchange system could also be assessed by a study of the rates of consumption and of the nature of production (e.g. Torrence 1986:10-38 for a summary of methods).

A third source of data which merits further research is the large quantity of spear blades which have ended up in museum collections throughout the world. Although these cannot be considered to be a completely representative sample of the products that circulated in the past, they should provide some clues into the nature of production and consumption during the past 120 years. Torrence's (in prep.) study of a sample of spear and dagger blades has already aided us in our interpretation of flaking methods at Umleang, but there are many more potential avenues of research using this form of data.

Eventually, with better data at hand, it would be extremely productive to compare and contrast the role of Lou Island obsidian in the wider exchange network with another durable item such as pottery, which was produced in particular centres such as Ahus Island. Ideally, the knowledge gained from a thorough assessment of how a specialised exchange works and what the archaeological signature generated by it looks like would be invaluable for evaluating the meaning of the widespread distribution of obsidian during the time of Lapita pottery. We still have a long way to go to apply the results of our study of Umleang directly and unambiguously to the analysis of Lapita sites. Contrary to our initial aim, this study of Umleang cannot be used as a definitive test of archaeological methods for inferring nature of exchange on the basis of production. Nevertheless, the results are highly encouraging. Unfortunately, the ethnographic data are simply not detailed enough to permit a thorough assessment of the questions raised by the archaeology concerning monopolisation of resources and/or production by specialists. As in many places of the world where memory about the past is now hazy, the only way to retrieve knowledge about obsidian exchange and social process in the Admiralty Islands is through archaeology. We hope that our initial attempts will encourage others to find the better methods and to collect the additional data that are required to answer the questions posed by this pilot study.

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MUSSAU ISLANDS PREHISTORY: RESULTS OF THE 1985-86 EXCAVATIONS

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Following up on initial discoveries by Egloff (1975; Bafmatuk et al. 1980) of Lapita sites in the Mussau (St. Matthias) Islands, the senior author in 1985 carried out renewed archaeological investigations in Mussau as one component of the Lapita Homeland Project (Allen 1984, 1985). The significant results from this 1985 season (Kirch 1987) inspired plans for an extended Mussau Project, focussed not only on Lapita, but on the longer term of human occupation in these islands. A second three month field season was supported in 1986 by the National Geographic Society, and a subsequent phase of intensive laboratory analysis, funded by the National Science Foundation, has enabled detailed study of ceramics, obsidian, and faunal materials. In this paper we summarise the key field results of the 1985-86 seasons, and formulate a tentative outline or framework for a Mussau cultural sequence. In our view, understanding both the origins and subsequent transformations of the Lapita Cultural Complex – as well as the ultimate contribution of Lapita to cultural diversity in Melanesia – requires the development of such regional sequences.

Lying astride the northern arc of the Bismarck Archipelago, the Mussau Islands occupy a critical position in the topology of western Melanesia. Access to the Manus group (and its important obsidian sources) from New Ireland or elsewhere in the Bismarcks may be best effected via Mussau. Graph theoretic models of Bismarcks geography indicate that Mussau would have commanded a high degree of centrality in any Lapita network (Hunt 1988), as indeed in later prehistoric exchange systems. Mussau also lies closest to the Micronesian islands of the central Carolines, and ethnographic as well as linguistic data hint at possible prehistoric connections between these areas.

Linguistically, the inhabitants of Mussau (including Emira Island and Tenis or Tench Island) speak a single Oceanic language with substantial dialect variation (Blust 1984). The phyletic position of Mussau within Oceanic remains unclear; indeed, there is no firmly accepted subgrouping of the Oceanic languages of western Melanesia (Lynch 1981; Pawley 1972; Pawley and Green 1985; Tryon 1984; Ross 1987, 1988). According to Ross (1987:12; 1988:315), Mussau is not a Meso-Melanesian language (the cluster including New Hanover and New Ireland languages), but does share some features with the Manus cluster. Whether these shared features reflect descent from a common Manus-Mussau proto-language, or have resulted from a long history of contact between these two populations, remains unresolved. Should Mussau be found not to group phyletically with Manus, the Mussau language would have to be considered a unique, first-order subgroup of Oceanic (Ross 1988:315).

The 1985 field season, summarised by Kirch (1987), focussed largely on the ECA Lapita site, with some testing of sites ECB and EHB (designations of the Papua New Guinea site register). The 1986 season was organised around several main field objectives: (1) to complete an intensive survey of the cluster of 11 islands situated southwest of Mussau Island (Fig. 1), and to extend reconnaissance survey to selected portions of the main island; (2) to expand the areal excavations at Area B of Site ECA, and to work out in detail the micro-geomorphology of site formation in this locality; (3) to extend the transect excavations at Lapita sites ECB and EHB initially tested in 1985; (4) to test one or more aceramic middens in order to extend the Mussau cultural sequence into the post-Lapita phase; and (5) to locate and test one or more rockshelters that might yield evidence of pre-

Lapita occupation. The survey and ECA site excavations were directed by Kirch, assisted by Hunt and Weisler. Hunt was responsible for the ECB and EHB excavations, and Weisler for the aceramic midden and rockshelter testing program, and reconnaissance survey of the northwestern part of Mussau Island.

SETTING

The Mussau group is composed of a large high island (Mussau) of 348 km^2, and 11 smaller coral islands (Fig. 1). Mussau lies about 150 km northwest of New Hanover, which reportedly can be sighted from higher elevations on Mussau during clear weather. The larger of the coral islands, Eloaua and Emananus, surround an extensive lagoon, and provided the settings for three open Lapita sites (Egloff 1975; Kirch 1987). Southeast of Mussau is Emira, a large island of uplifted limestone (*makatea*) terrain which remains archaeologically unexplored.

No published information exists on the geo-

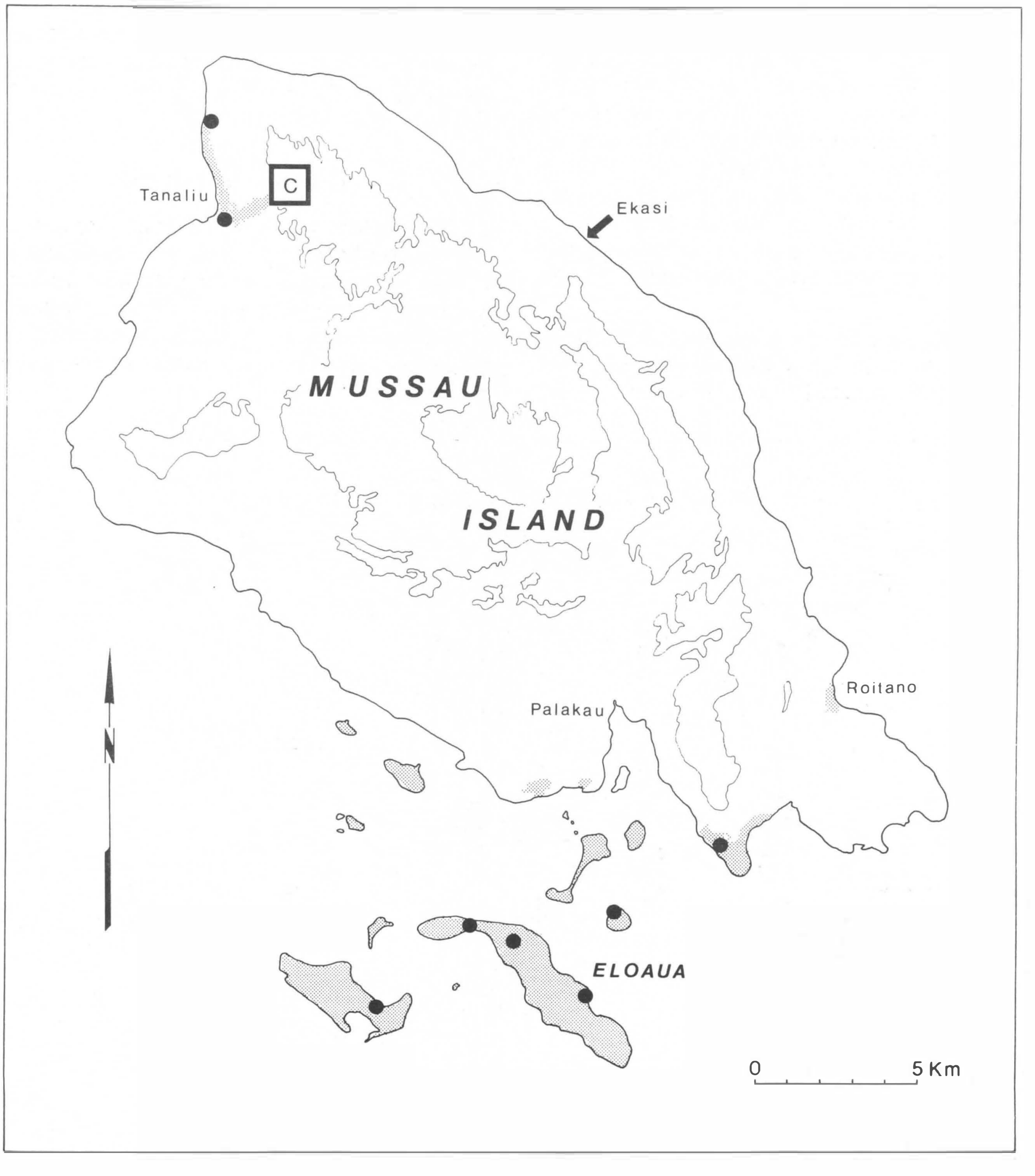

Figure 1 *The Mussau Islands. Stippled areas have been intensively surveyed for archaeological sites. Black dots indicate excavation localities. The boxed C indicates the location of the only known clay source on Mussau.*

logy of the Mussau Islands, but our own explorations augmented by discussions with economic geologists who have recently reconnoitred the group for its mining potential, indicate that the surficial geology is dominated by an ancient series of tectonically upraised reef terraces. The large island of Mussau has a volcanic core, exposed only in limited areas on the north side. In our reconnaissance survey, volcanic rocks were found in streams at Ekasi and at several locales 2 km east of the Tanaliu area (Fig. 1). In all cases, these rocks are very similar in hand specimen, and consist of highly porphyritic plutonics (gabbro). To our knowledge, these intrusives are the only volcanic rocks exposed within the Mussau group. Several examples of these local intrusives have been excavated from the Lapita sites on Eloaua and Emananus, and were evidently used as oven stones. The apparent absence of andesites, basalts, or other island-arc volcanics in Mussau is significant, since such materials when they appear in archaeological contexts can be identified as exotic imports.

The upraised limestone terrain that dominates the Mussau landscape is capped with only a thin organic soil, generally unsuitable for use as a potting clay. However, informants led us to a clay deposit in the northern interior of Mussau Island (Fig. 1, Area C), coinciding with the area in which underlying volcanics are exposed. Ceramics are unknown in Mussau ethnographically, but the clay was quarried to be eaten in certain ceremonies only vaguely remembered by our informants. Samples of this clay have been compared for elemental composition with the clay matrix in sherds from Mussau Lapita sites. As discussed further below, some of the Mussau Lapita pottery derives from this locally available clay.

Other aspects of the Mussau environment, including available biotic resources, have been discussed elsewhere (Kirch 1987, 1988a), and will not be repeated here.

SURVEY RESULTS

As indicated in Figure 2, archaeological survey has now been carried out on all of the smaller islands southwest of Mussau, and in the south and northwest parts of Mussau Island A total of 35 archaeological sites has been recorded (see Appendix). Four primary classes of sites can be defined.

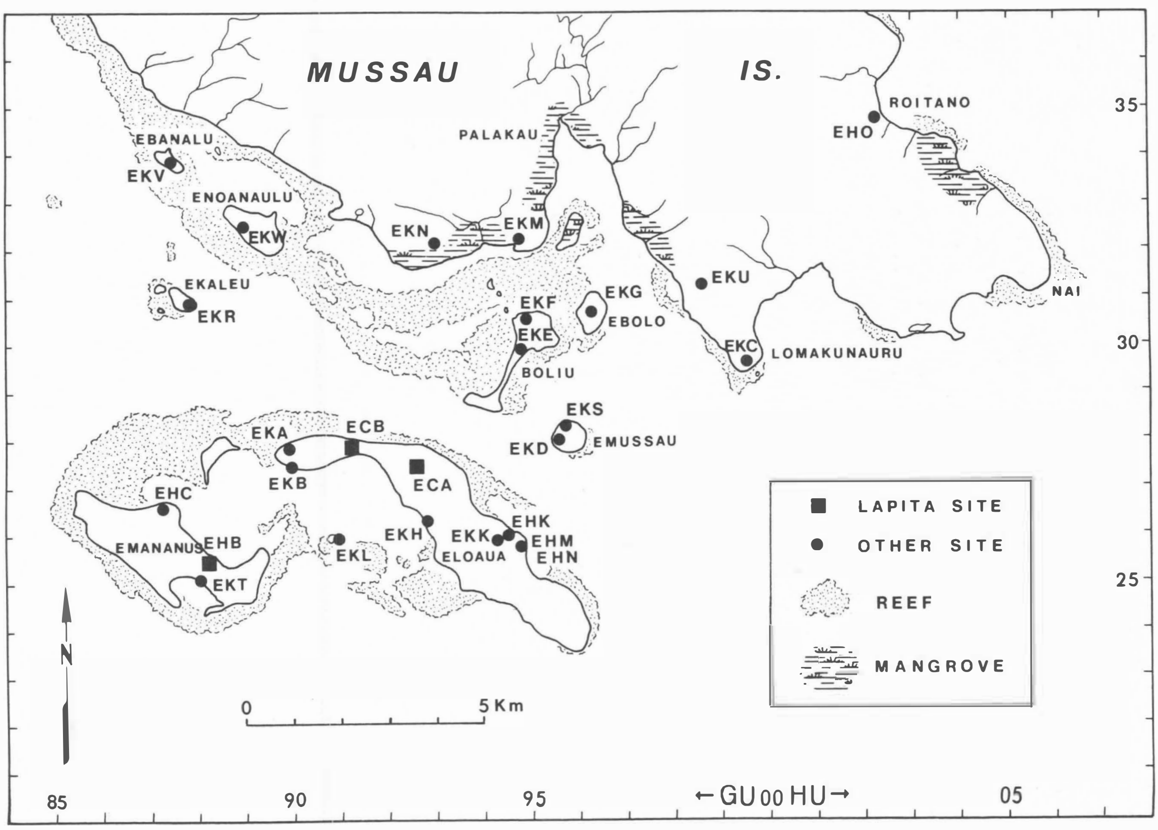

Figure 2 *The southern portion of Mussau Island and nearby coral islands, showing the locations of archaeological sites.*

Lapita Open Sites

Three open sites contain classic, dentate-stamped Lapita ceramics: two on Eloaua Island (ECA, ECB) and one on Emananus Island (EHB). These are all situated on former beach ridges, and in the case of ECA the Lapita occupation extended out over a subtidal reef flat now buried under 1-1.5 m of sand (Kirch 1987, 1988a). In all three sites, the beach ridge occupation areas have been extensively disturbed by gardening and by land crab activity. Site ECA, however, contains an undisturbed, waterlogged occupation component.

Non-Lapita, Ceramic Bearing Open Sites

Two open sites, both in the southern part of Mussau Island (EKU and EHO), contain pottery but lack ceramics with dentate-stamped Lapita decoration. The EHO site at Roitano Village was heavily disturbed by construction of a church. The EKU site at Sinakasae was test excavated, and is described further below.

Aceramic Midden Sites

The dominant site class on the southern islands of the Mussau group consists of extensive shell middens that on surface inspection lack ceramics. These middens are found both on low-lying beach terraces and on upraised limestone terrain, and 20 such sites have been recorded. In area, these sites range from less than 100 m^2 to more than 5000 m^2. A common feature is the presence of mounds, ranging from 5-15 m in diameter and from 1-2 m in height, that were presumably midden dumps. Surface collections on these sites also produce a consistent artefact assemblage of *Terebra* shell adzes, dorsal margin type *Tridacna* shell adzes, *Trochus* shell armbands, and low frequencies of small obsidian flakes. These artefact classes are documented ethnographically (Nevermann 1933), and the sites are believed to post-date Lapita, and in many cases may only be a few hundred years old. One of these sites, EKS on Emussau Island, was tested in 1986 and is described further below.

Rockshelters

The reefal limestone topography of Mussau produces numerous overhang rockshelters and solution caves at the bases of uplift escarpments, and nine shelters or caves with evidence of human occupation have now been recorded. Aware that recent excavations in New Britain and New Ireland (Specht et al. 1981; Allen et al. 1988) had demonstrated that such sites frequently have lengthy Pleistocene occupation sequences, we focussed some effort in 1986 on the testing of rockshelters. In the four shelters and one cave tested, however, there was no evidence of a pre-Lapita occupation phase. Excavations at these sites are described below.

RESULTS OF THE 1985-86 EXCAVATIONS

Excavations have been carried out at five open sites and in four rockshelters and one cave, on the islands of Eloaua, Emananus, Emussau, and Mussau (Table 1). The total excavated sample is 107.5 m^2.

Site ECA, Talepakemalai

Approximately half of the Mussau excavation sample is concentrated on the large Talepakemalai Lapita site (ECA) on Eloaua Island. This very extensive site spans an area in excess of 72,500 m^2, and includes a waterlogged deposit that preserves the wooden post bases of stilt structures. Results of the 1985 ECA site excavations were summarised in Kirch (1987), and the 1986 ECA results in Kirch (1988a). The ECA excavations will not be further described here, although analytical results from ECA are incorporated in later sections of this paper.

Table 1 *Mussau archaeological sites excavated 1985-86.*

Site	Island	Site Type	Period	Site Area (m^2)	Area Excavated (m^2)
ECA	Eloaua	Midden	Lapita	72,500	57
ECB	Eloaua	Midden	Lapita	3,000	19
EHB	Emananus	Midden	Lapita	1,150	9
EHM	Eloaua	Solution Cave	Lapita	152	3
EHN	Eloaua	Rockshelter	Lapita		1
EKO	Eloaua	Rockshelter	Lapita	25	2.5
EKP	Mussau	Rockshelter	Post-Lapita	66	5
EKQ	Mussau	Rockshelter	Lapita	88	2
EKS	Emussau	Midden	Post-Lapita	74,100	4
EKU	Mussau	Midden	Post-Lapita	21,000	5

Site ECB, Etakosarai

Site ECB occupies a Holocene calcareous dune ridge and its adjoining terrace near the northwest end of Eloaua Island, immediately west of a low-lying narrow 'neck' of unconsolidated sands. This neck was formerly an open reef flat extending eastwards to site ECA, so that in the second millennium BC the two Lapita settlements faced each other across a wide reef flat. ECB was first noted by Egloff (1975:15) who described it as an 'irregular midden approximately 20 metres in diameter'. Our investigations revealed that the site covers an area of about 3000 m². Much of the site surface was under cultivation in 1985-86 by villagers from Lomakunauru Village, and gardening activity had exposed quantities of shell midden, sherds, and obsidian flakes.

In 1985, Kirch excavated six 1 m² units along a transect across the site, oriented perpendicularly to the late Holocene +1.5 m shoreline defining the eastern site perimeter. This transect sampling strategy was extended by Hunt in 1986 with the excavation of an additional 13 m². A primary north-south and three east-west transects provide a clear picture of site geomorphology, and also permit definition of internal distribution and patterning of cultural materials. Although no intact features were discovered during excavation, the densities of shell midden, burnt coral oven stones, ceramics, and obsidian vary significantly in different parts of the site, suggesting discrete activity areas.

The Etakosarai stratigraphy consists of four main layers, all situated within the depositional context of a low energy beach ridge. The uppermost stratum is a dark organically enriched sandy loam reworked by gardening. This grades into a lighter coloured medium grained sand with a low density of cultural materials. The third stratum, found over much of the site, is a light grey medium to fine grained sand with varying degrees of $CaCO_3$ concretion. This layer contains the greatest quantity of cultural materials, including large dentate-stamped Lapita sherds, and represents the primary occupation of the site. The basal deposit consists of coarse, white sand with shell detritus and no cultural materials. Surf clams (Veneridae, Tellinidae) in death position in this deposit indicate a lagoonal context pre-dating beach ridge formation and site occupation.

The ECB ceramic assemblage includes sherds decorated with dentate-stamping, incising, and red slip, and rims with notched lips (from otherwise plain ware jars). Decorated pottery amounts to 9.9% of the 5973 sherds recovered. Shell artefacts include a *Tridacna* adze, three *Trochus* shell fishhooks, scrapers of *Cypraea*, *Cassis rufa*, and *Anadara*, and *Conus* shell rings. Fractured, chipped, and ground shell of several species indicates that the manufacture of shell artefacts took place on site. Obsidian and exotic volcanic stones (especially andesite) were recovered in abundance. Exotic stone artefacts include a basalt adze and hammerstone.

Site EHB, Etapakengaroasa

Site EHB is situated on the northeastern, lagoon shore of Emananus Island, on a former beach ridge (90-100 cm above sea level) now separated from the sea by a mangrove swamp. Shellfish, sherds, and obsidian flakes cover an area of about 1150 m² corresponding to the slightly higher ground of the former beach ridge. About 50-65 m inland of the present shoreline is a poorly drained low-lying area. Further inland (65-89 m) the topography rises with a gentle slope of decomposing, tectonically elevated reef limestone, presently under shifting cultivation.

Two test units were excavated in 1985 (Kirch 1987:166) and in 1986 seven more units were excavated along two transects, one parallelling the shoreline, the other bisecting the beach ridge perpendicular to the shoreline. The site contains three major strata. Layer I, from the surface to 25-40 cm, is a dark brown organically-enriched loam, extended in depth through cultivation. Most of the Lapita cultural material derives from Layer II, a medium, greyish brown calcareous sand with $CaCO_3$ concretions. Layer II ranges from 60-70 cm thick. Concretions occur primarily in the upper part of the layer, with the deeper portions continuously moistened by tidal fluctuation of the Ghyben-Herzberg aquifer. Layer III, continually wet, is a coarse to medium-grained white sand lacking cultural material, and containing bivalves in death position as well as branch coral fingers. Lapita occupation thus began on a low, sandy beach ridge prior to the development of the mangrove swamp that presently separates this area from the lagoon.

Site EHB artefacts include 4637 sherds, of which 5% are decorated with dentate-stamping, incising, red slip, and with notched rims. Shell artefacts include a ground *Conus* shell cap (an early stage in the manufacture of a ring or disk), fractured *Trochus*, chipped *Tridacna*, and an *Anadara* shell scraper. A hammerstone was also recovered.

Site EKU, Sinakasae

The contemporary hamlet of Sinakasae occupies a terrace of ancient upraised limestone about 200 m above sea level, on the southwestern end of Mussau Island. The area of site EKU largely corresponds to the present day hamlet, with obsidian, shellfish midden, *Tridacna* and *Terebra* shell adzes, and a low frequency of sherds littering an area of c. 21,000 m^2. The site was brought to our attention by Aimalo Lavatea who had collected 36 plain and 4 decorated sherds on the site surface.

Five 1 m^2 units were excavated along a north-south transect positioned through the centre of the site. Excavation revealed a shallow deposit of black (5 YR 2.5/1) loam with shell and bone midden and artefacts up to 30 cm deep, overlying a non-cultural mixture of yellowish brown (10 YR 5/4) sandy loam with angular cobbles and outcrops of decomposing limestone.

Site EKU is notable for its unique ceramics, all of which are characterised by a dark red paste with black volcanic sand (pyroxene) temper. Only 10 sherds were recovered from the excavations. The decorated sherds are quite unlike anything from the Lapita sites, consisting of parallel rows of simple tool-impressed rectangular punctations. No other style of decoration was noted at EKU, either from the surface collections or from excavation. The presence of *Terebra* shell adzes suggests that the site belonged to a post-Lapita period of Mussau prehistory. Shell midden was abundant, as was pig bone.

Site EKS, Emussau Island

Site EKS, an aceramic midden characterised by surface artefacts such as *Terebra* shell adzes, *Trochus* shell rings, and a few small obsidian flakes, was chosen for test excavation as a representative of the numerous aceramic midden sites located on the offshore islands of the Mussau group. The site occupies a sandy terrace 1-2 m above sea level on the northwest side of Emussau Island, about a 20 minute canoe ride from Eloaua Island. EKS covers an area of at least 74,100 m^2, and is marked by the presence of round to oval mounds, ranging in diameter from 3.8-14.3 m and in height from 0.2-1.3 m. These mounds, often surrounding level areas c. 20 m by 30 m, appeared to be refuse dumps.

Four 1 m^2 units were excavated, two in a large mound and two in the adjacent level area. In three units, wet sieving was used to facilitate the recovery of cultural materials. The test unit in the mound centre was excavated to the water table, 1.96 m below the top of the mound. Eleven distinct stratigraphic units can be grouped into two major depositional strata. The upper 1.2 m of the deposit consists of a black (7.5 YR 5/0), compact, greasy midden with dense concentrations of shellfish, fish, and pig bone. No discrete dumping features or fine lenses were noted, although the deposit was divided into four layers based on sediment colour, compaction, and density of shellfish. Below the cultural deposit was a 80 cm thick stratum representing a beach depositional environment.

In the level areas, the stratigraphy was quite different. Here the cultural deposit was about 60 cm thick, a dark grey (10 YR 4/1) sandy loam of loose consistency, with few shellfish.

A wide range of mostly shell artefacts was recovered from excavation and surface collection. Notable are rings and manufacture detritus of *Trochus niloticus* shell, a *Cassis* shell chisel, and various *Terebra* shell chisels or adzes in all stages of manufacture. The front section of a volcanic stone adze was also surface collected. Other artefacts included worked *Anadara* and *Tridacna* shells, pumice nodules, ground sea urchin and pearl shell, a coral file, and nine small obsidian flakes. No ceramics were recovered.

The mound yielded a very high density of shell midden (55.55 kg in 1 m^3), with gastropods and bivalves almost equally represented by weight. Gastropods consisted predominantly of *Strombus luhuanus*, *Lambis lambis* and *Trochidae*, while the dominant bivalves were *Anadara* spp., *Chama* spp., and *Tridacna maxima*. Also notable was the high frequency of pig bone.

Sites EHM, EHN, EKO, Eloaua Island

Along the reef-protected central coast of Eloaua Island are two rockshelters and a small cave situated at the base of an upraised limestone escarpment, all about 50 m inland from the shoreline, and separated from the present lagoon by a mangrove swamp. The sites are all within a few metres of each other.

Site EHM is a solution cave, 24.5 m deep and 10.4 m wide, with a constricted entrance (partly blocked by several courses of coral cobbles and boulders), and ceiling height of 0.4-1.4 m. The dirt floor covers an area of 152 m^2, which was tested with three 1 m^2 units. Only a single cultural stratum was present, with a total depth of about 50 cm. The deposit consists of dark reddish brown silt (5 YR 3/4) with water-rounded pebble-sized limestone, coral, and shell inclusions.

The deposit yielded sparse cultural materials, primarily in the lower half of the stratum. Artefacts included two undecorated shell-tempered sherds (from near the base of the excavation), three obsidian flakes, worked *Tridacna*, *Anadara*, and *Trochus* shell, and one *Tridacna* shell adze preform. The low midden and artefact densities, and absence of hearth features suggest that the site was used only infrequently.

Site EKO is a small overhang shelter about 11 m west of EHM; site area measures 10 m wide by 2.5 m deep. A 1 m by 2.5 m trench was excavated across the shelter floor perpendicular to the main axis.

EKO stratigraphy consisted of three cultural layers totalling 60 cm thick: Layer I is an ashy, silty dark grey (10 YR 4/1) sediment with ash pockets, charcoal chunks, and artefacts, shellfish, crustacea and coral oven stones. Layer II had less ash, a darker colour (10 YR 2/1), and more limestone pebbles and cobbles. Layer III was brown (10 YR 4/3), with much less ash in a sandy matrix. Large limestone boulders were encountered atop limestone bedrock at the base of the deposit.

Artefacts from the EKO excavation include 125 sherds, with a single dentate-stamped sherd, 37 obsidian flakes, and eight worked *Anadara* shells. Midden material included 11.5 kg of molluscs, primarily *Lambis lambis*, *Strombus luhuanus*, and *Trochus niloticus* among the gastropods, and *Hippopus hippopus*, *Tridacna* sp. and *Anadara* sp. among the bivalves. Little bone was present, although crustacean fragments were recovered throughout. The density of cultural material, oven stones, and the thick, dark ashy sediment suggest that the shelter was used repeatedly, possibly over a long period of time.

Site EHN is a small overhang rockshelter in the face of an upraised reef terrace, a short distance from sites EHM and EKO. The shelter floor is 6.6 m long and 2.2 m deep. A 1 m² test excavation was positioned in the centre of the floor just inside the dripline. The deposit, with a total depth of 85 cm, consisted of an upper layer of fine powdery soil overlying angular limestone cobbles (roof fall). The only artefacts recovered were 5 pieces of worked *Trochus* shell. About 4 kg of molluscan midden was also recovered, dominated by bivalve species (*Tridacna*, *Anadara*, and *Hippopus*).

Site EKP, Eatulawana Shelter

Site EKP, along with EKQ, is situated in the vicinity of Pomanai Village, in the northwestern part of Mussau Island. Excavations in these two large rockshelters were carried out with the aim of exposing well stratified sequences that might include pre-Lapita components.

The Eatulawana shelter is about 0.5 km inland, and consists of an overhang (floor area 11 m by 6 m) at the base of a high limestone escarpment which defines the edge of a swampy lowland. Five discontinuous 1 m² units were excavated in a transect perpendicular to the long axis of the shelter, and extending outside the dripline. Despite its initial promising appearance, the shelter proved to contain only a single cultural stratum, averaging 14 cm thick. This black (7.5 YR 2/0) silty, organic-rich cultural deposit overlays a stratum of very dark grayish brown (10 YR 3/2) silty-clay. With much difficulty the excavation was continued in one unit 60 cm below the cultural deposit through very compact, yellowish brown (10 YR 5/8) decomposing limestone.

The artefactual content of the cultural deposit was limited to 33 undecorated ceramic sherds with shell temper, eight obsidian flakes, and a few clay nodules. Faunal materials include medium mammal bone, one pig tooth, crustacea, and 3.2 kg of molluscs. The low density of artefacts and faunal materials, lack of hearths, and thin cultural deposit suggest that the site was not a primary habitation locale.

Site EKQ, Epakapaka Shelter

Two kilometres north of Pomanai Village and 200 m inland from the coast is EKQ or Epakapaka, a large overhang rockshelter (24 m by 7.4 m floor area under the dripline) at the base of a high limestone cliff. The interior floor area of about 88 m² is relatively level, and cultural deposits extend over at least 100 m² outside the dripline, indicated by potsherds and shell midden exposed by land-crab activity. The site was used as a refuge for local villagers during World War II.

Excavation was limited to two 1 m² tests near the centre of the shelter, one inside and one outside the dripline. Both units exposed a deep stratigraphic sequence to depths of 2.6 m. Four cultural strata overlying four basal non-cultural strata were defined. Layer I was a disturbed, very dark gray (10 YR 3/1) sandy loam with remnant ash pockets and dispersed charcoal chunks, heavily disturbed by crab burrowing. Layer II was a very dark grayish brown (10 YR 3/2), silty deposit that appeared to be less disturbed than Layer I. Charcoal and ash were mixed within the sediment matrix, but a few discrete hearths were identified. Layer III was a very dark gray (10 YR 3/1) silty sand, containing numerous scoop hearths. Layer IV was a black (10 YR 2/1) sandy matrix with charcoal throughout. Underlying these four cultural deposits were four more strata of beach sand alternating with gravel-andcobble deposits in coarse sand matrix, representing beach building episodes prior to human use of the shelter.

A total of 12,463 sherds were recovered from the two test units, providing the best stratified sequence of ceramic materials from any Mussau site. However, sherds are generally very small (average weight 1.54 g), making stylistic analysis difficult. Decorated sherds account for 3.9% (490 specimens) of the ceramic assemblage. Dentate-stamped pottery is found throughout the deposit, but in somewhat higher frequency at the base of the cultural sequence. Notched rim sherds are found throughout. A single incised sherd was found in Layer IV, while the bulk of the incised ceramics occurs in Layers II and III. A single red-slipped body sherd was present in Layer IV, and three knobbed sherds in Layer II.

A total of 745 obsidian flakes were recovered, 90% from Layers II and III. Other artefact classes represented include *Conus* shell rings, one-piece *Trochus* shell fishhooks, *Trochus* shell trolling lures, worked *Trochus* shell, ground pearl shell, and a *Spondylus* shell bead. Faunal material included 15 kg of molluscs with 51 taxa represented. Vertebrate material includes human, pig, rodent, phalanger, pteropodid, medium charadriid, lizard, turtle, and abundant fish bone. Echinoderm, crustacean, and chiton were also identified. The density and range of cultural materials present, hearth features, and one disturbed burial of a child (see Kirch et al. 1989) all indicate that the Epakapaka rockshelter was a primary occupation locale utilised over a lengthy period. The site is thus one of a very few such rockshelters identified for the Lapita Cultural Complex, in which open middens account for more than 80% of known sites (Kirch and Hunt 1988b, Table 2.2).

RADIOMETRIC CHRONOLOGY

Twenty-seven radiocarbon age determinations have been obtained on samples from sites ECA, ECB, EHB, EKQ, EKO, EKU, and EKS. Some of these dates were reported by Kirch and Hunt (1988a), and the full suite is listed in Table 2, with conventional C^{14} age BP (i.e. C^{13} corrected) and calibrations following the CALIB programs of Stuiver and Reimer (1986). Marine samples have been calibrated using a Delta-R value of 100±24 (Kirch and Hunt 1988a:162, 1988b:23).

All but three of these C^{14} dates are on samples directly associated with Lapita ceramics and other cultural materials, and thus provide a firm basis for establishing the temporal framework of Lapita occupation in Mussau. In brief, initial Lapita colonisation occurred about 1500 BC with settlements at ECA and ECB well established by 1300 BC. The earlier Lapita occupation phase, characterised by elaborately decorated vessels, extends until perhaps 1000 BC. A subsequent phase from about 1000 BC to perhaps as late as 500-300 BC is marked by increasingly coarse dentate-stamped ceramics, giving way to a dominance of incised decoration. This later phase is particularly well evidenced in the EKQ rockshelter sequence. In short, the Lapita period in Mussau spans approximately one millennium, during which time a continuous sequence of ceramic change can be traced.

The post-Lapita period in the Mussau cultural sequence is as yet only minimally attested to in our excavations. Site EKU, dating to the thirteenth century AD, contains exotic ceramics at least some of which, on the basis of sourcing studies discussed below, were imported from the Manus group. Site EKS, an aceramic midden, dates to the very late prehistoric period. Layer I of rockshelter site EKQ also dates to this late prehistoric era.

TOWARDS A PREHISTORY OF THE MUSSAU ISLANDS

Having reviewed the 1985-86 Mussau excavations, we turn to the evidence upon which a prehistoric sequence for the Mussau Islands may be constructed. Although our data are weighted toward the earlier time periods (the result of our emphasis upon Lapita sites), the limited excavations at EKS and EKU, the upper layer of EKQ, and the surface survey provide some points of reference for the post-Lapita time periods.

Ceramic sequence

The Site ECA ceramic sequence has been discussed by Kirch (1987, 1988a); it exhibits a progression from complex, intricate, dentate-stamping to more open, coarser dentate-stamping. This gives way in the upper deposits of Area B to a greatly increased emphasis on the use of incised designs (Fig. 3). Despite the shift from dominantly dentate-stamped to dominantly incised decorative techniques, there is continuity in particular motifs and in the formal structure of the design system. Thus the incised pottery which prevails in the later deposits does not represent a wholly new or intrusive style, but derives from a continuous process of stylistic change.

The deep and well stratified sequence from Site EKQ closely matches the upper portions of

Table 2 *Mussau radiocarbon age determinations (1985-86). See text for discussion.*

Laboratory Number	Grid/Level	Material	Conventional C^{14} Age BP	Calibrated Age
Site ECA-Area B				
ANU-5075	W200N149/7	Charcoal	2370±120	761-364 BC
ANU-5076	W200N151/8	Charcoal	2430±230	820-212 BC
ANU-5078	W201N151/9	Charcoal	2450±160	800-390 BC
ANU-5079	W200N150/18	Charcoal	2600±160	967-437 BC
ANU-5081	W200N151/11	Shell	3010±80	803-658 BC
ANU-5082	W201N149/12	Shell	2950±80	772-514 BC
ANU-5083	W200N149/3	Shell	2810±80	548-364 BC
ANU-5790	-	Post 1	2950±80	1368-1021 BC
ANU-5791	-	Post 2	2930±80	1308-1012 BC
Beta-20452	W198N148/C	Post 30	3050±70	1429-1135 BC
Site ECA-Area A				
ANU-5084	W228N1023	Shell	3190±80	1001-809 BC
ANU-5085	W229N100/9	Shell	3130±80	919-779 BC
Site ECA-Test Units				
ANU-5080	TP9/6	Charcoal	3260±90	1687-1441 BC
Beta-20451	TP18/9	Coconut	2950±70	1315-1051 BC
Site ECB				
ANU-5086	Unit 1/1	Shell	3120±80	909-774 BC
ANU-5087	Unit 1/2	Shell	3150±80	949-790 BC
Site EHB				
ANU-5088	Unit 1/9	Shell	3470±90	1397-1161 BC
ANU-5089	Unit 2/6	Shell	3380±90	1292-1016 BC
Site EKQ				
Beta-20454	Unit 1/11	Shell	3280±70	1121-911 BC
Beta-21789	Unit 2/17	Shell	3030±80	813-719 BC
Beta-25670	Unit 2/9	Shell	3270±80	1120-895 BC
Beta-25671	Unit 2/13	Shell	3190±90	1010-804 BC
Beta-25036	Unit 2/3	Shell	740±70	AD1631-1719
Site EKU				
Beta-25930	TP2-5/1	Bone	740±70	AD1222-1284
Site EKO				
Beta-25669	Unit 1/4	Shell	3200±70	1001-823 BC
Site EKS				
Beta-20455	TP2/5	Charcoal	350±60	AD1445-1640

the ECA sequence. Thus the EKQ ceramics are dominantly incised, with a variety of thin walled vessels displaying everted rims with notched, finger pinched, or crennate lips (Fig. 4). The senior author, while examing the collection of ceramics excavated by Golson at Lasigi in New Ireland in 1985, was struck by the close parallels in vessel and rim form, and in lip decoration, between the EKQ and Lasigi assemblages.

Subsequent to the abandonment of ECA and EKQ (i.e. after 400-300 BC), the frequency of pottery use in Mussau declined dramatically (we say pottery 'use' rather than 'production' since there is evidence that much of the pottery was imported to Mussau; see below). The small EKU assemblage, dating to the thirteenth century AD, contains small quantities of primarily plain ware (but with a few sherds decorated with punctate designs). Some of this pottery can be linked compositionally with a Manus Islands clay source (see below). The unexcavated EHO site at Roitano Village may also date to this period, although this remains to be determined.

The late prehistoric occupation at EKS, and Layer I in EKQ, contain no ceramics, corresponding with the ethnographic descriptions of the early twentieth century (Nevermann 1933).

Changes in the portable artefact record

The Lapita site excavations yielded a wealth of portable artefacts including obsidian flakes, stone and *Tridacna* shell adzes (made from the thick, hinge portion of the clam shell), hammerstones, abraders of various types, *Trochus* shell fishhooks, pearl shell peeling knives, *Cypraea* shell peelers/scrapers, and a wide variety of rings, disks, beads, and pendants made from several genera of molluscs. These artefacts

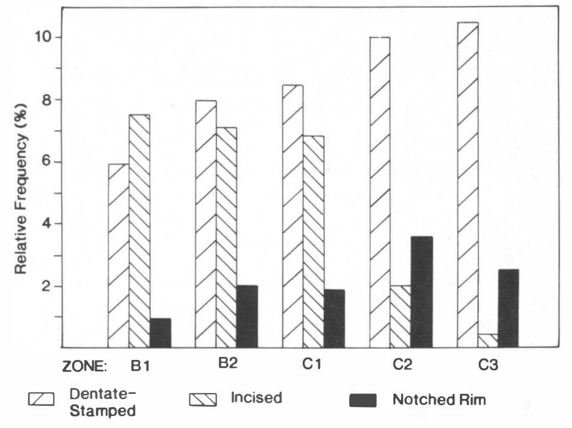

Figure 3 *Relative frequencies of major categories of decoration on sherds from the Area B excavations at site ECA. Distribution is plotted by analytical zones within the Area B deposit; Zone C3 is the stratigraphically oldest, Zone B1 the youngest. Note the significant increase in relative frequency of incised decoration beginning in Zone C1, and the general decrease in the use of dentate-stamping over time.*

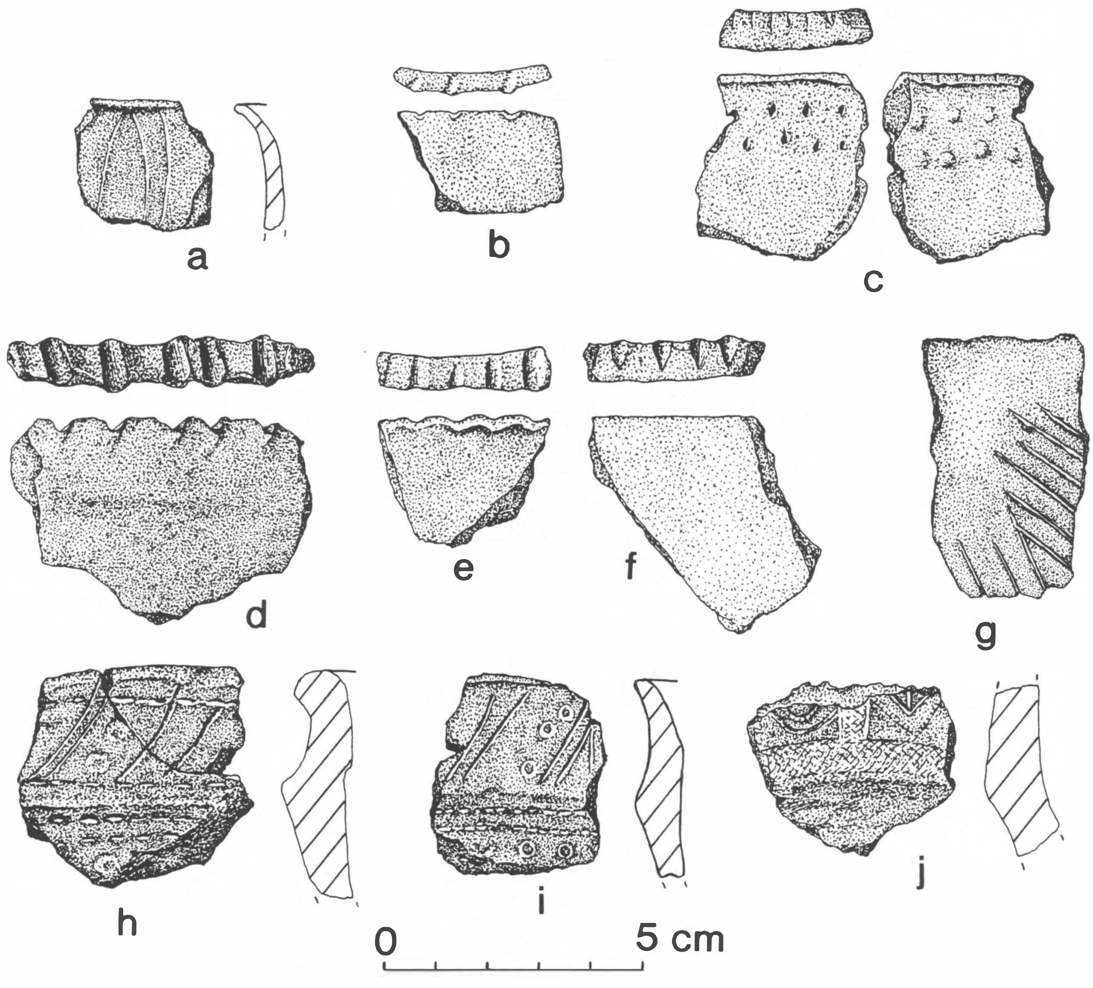

Figure 4 *Selected sherds from the EKQ rockshelter site: a) rim sherd with incised decoration; b) notched rim sherd, c) notched rim sherd with punctations; d-f) notched rim sherds; g) incised body sherd; h and i) rim sherds with coarse dentate-stamped decoration; j) carinated sherd with dentate-stamping.*

have been briefly described, and examples illustrated, in other publications (Kirch 1987, 1988b, 1989).

This consistent Lapita artefact assemblage contrasts markedly with the excavated sample from site EKS, and with surface collections from non-Lapita open midden sites (e.g. sites EHC, EHK, EKC, EKE, EKN, and EKV). These post-Lapita artefact assemblages are characterised by the presence of adzes/chisels of *Terebra* shell with a curved bevel ground from the body whorl of the shell; by *Tridacna* shell adzes worked from the dorsal margin; and by large *Trochus* shell armrings (Fig. 5). These sites also yield low frequencies of small obsidian flakes, almost invariably of Manus (Lou) source.

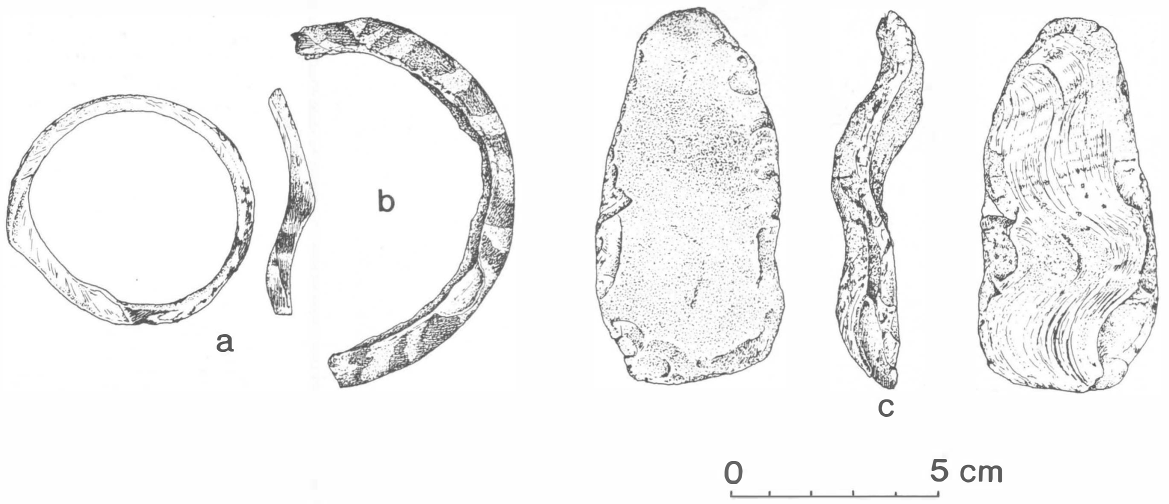

Figure 5 *Selected artefacts from the EKS aceramic site: a) small ring of* Trochus *shell; b) fragment of unfinished* Trochus *shell armband; c) dorsal-margin type* Tridacna *shell adze.*

At some time between the late Lapita period (c. 500 BC), and the mid-second millennium AD, the material culture inventory of Mussau underwent substantial change, including the deletion of various Lapita artefact classes, and the addition of new forms such as the *Terebra* shell adzes and *Trochus* shell armrings. Until more sites dating to the post-Lapita period have been excavated, the chronology of these changes cannot be further specified. However, it may be significant that the late prehistoric appearance of *Terebra* shell adzes, and of certain forms of dorsal margin *Tridacna* shell adze, have now been documented from several Melanesian sequences. In Santa Cruz, *Terebra* adzes appear in abundance during the last 600 years of the sequence (McCoy and Cleghorn 1988:114). In Vanikoro, they appear virtually at the end of the prehistoric sequence (Kirch 1983). Garanger (1972:132) has documented the appearance both of *Terebra* adzes and of distinctive dorsal margin *Tridacna* adzes beginning about 800-1100 AD in central Vanuatu.

Subsistence economy

All excavated sites produced substantial vertebrate faunal assemblages, the bulk of which consist of fish bone, upon which our analysis has concentrated. In contrast with sites in the eastern Pacific, bird bones are relatively rare, and there is no evidence for the massive extinction of species that has been reported for many central Pacific Islands (e.g. Steadman 1989). The early Lapita sites show a heavy exploitation of marine turtles, although turtles continue to be represented in later assemblages. Other faunal categories represented in low frequencies throughout the sequence are cetacean, fruit bat, cuscus, the domestic fowl, and dog.

A major contrast between Lapita and later prehistoric vertebrate faunal assemblages is the frequency of pig (*Sus scrofa*) bones. Pig is present in the Lapita assemblages (46 NISP in the ECA site), but is a relatively minor component. In contrast, pig bones are a major component of the later prehistoric faunal assemblages at sites EKU and EKS. This corresponds with ethnographic information that pig husbandry was a key aspect of indigenous Mussau society (pigs were eliminated after conversion to Seventh Day Adventism in 1930). Exactly when pig husbandry began to assume importance in the local production system is not yet clear, although a date sometime in the mid-first millennium AD seems likely. Significantly, the emphasis on pigs occurs after a decline in long distance importing of pottery and obsidian. Given that pigs in contact period Mussau society were a major item in inter-lineage exchange, it may be that the appearance in the prehistoric record of abundant pig bones marks a shift from long distance exchange, to an internally focussed system based on competitive production and exchange of pigs.

Since the majority of vertebrate faunal remains from the Mussau sites consists of fish bones, emphasis has been placed on the identification and analysis of these materials. From the 1985-86 seasons, 21,726 fish bones were recovered, of which 3563 (16%) could be identified to skeletal element and, in most cases, to family. Subfamily taxonomic assignments are in progress; the present summary is based on family or ordinal level information. Six sites produced identifiable fish remains, and the frequency of families represented in these site assemblages (Table 3) is remarkably similar. Two families, Lethrinidae and Scaridae, dominate at all sites, representing between 38-57% of the collections. The same eight families (Serranidae, Lutjanidae, Lethrinidae, Labridae, Scaridae, Acanthuridae, Balistidae, and Diodontidae) are prominent at all sites, comprising between 71-90% of the assemblages.

The general comparability of fish faunal frequency is remarkable, suggesting little change in the indigenous Mussau fishery over nearly four millennia. For instance, the fish faunal assemblage from EKS, a late aceramic midden, is virtually indistinguishable from those of Lapita sites ECA, ECB, and EHB. Interestingly, the EKQ site assemblage is the most distinctive, in having a relatively high proportion of holocentrids, mullids, and scombrids. This difference might reflect the respective marine environments near the sites, with EKQ on a fringing reef coast lacking lagoon habitats.

Comparisons of the Mussau Lapita fish bone assemblages with those from other Lapita sites show strong similarities. Butler (1988, Table 4) notes that eight families account for more than 85% of the fish remains in five well documented Lapita assemblages: Serranidae, Lutjanidae, Lethrinidae, Labridae, Scaridae, Acanthuridae, Balistidae, and Diodontidae. These same families dominate Mussau assemblages (Table 3). In addition, two families, Scaridae and Lethrinidae, are the most abundant taxa in three of the other Lapita assemblages (Butler 1988) and these two families dominate all the Mussau collections as well. Contrasts between Mussau assemblages and the other Lapita collections are also apparent. Of particular note is the relative scarcity of Diodontidae at Mussau Lapita sites, where the family represents an average of 5.3% (range 0.3-15.5%) of the site assemblages, in contrast to other Lapita sites where Diodontidae account for an average of 21% (range 6.5-41.5%).

Our results support the conclusions of Green (1986) that Lapita fisheries were focussed on inshore reef environments. We have also been able to further document the utilisation of scombrids, the remains of which are rare or nonexistent in

Table 3 *Frequency of identified fish bones (NISP) by family from Mussau sites.*

Family	ECA	ECB	EHB	EKQ	EKU	EKS
Carcharhinidae	1			3		
Lamniformes	2			1		
Lamnidae	1					
Myliobatididae	2		1			
Anguilliformes	16	1	1	4	1	1
Belonidae	43					
Holocentridae	10			21		
Serranidae	111	7	12	24		27
Carangidae	43	1		2	2	7
Lutjanidae	62	6	2	20		16
Haemulidae	4					1
Lethrinidae	686	32	25	57	2	62
Mullidae	4		1	8		1
Mugilidae	3			2		1
Sphyraenidae	5					1
Labridae	114	7	7	6	5	11
Scaridae	620	26	38	59	10	89
Acanthuridae	80	4		29		10
Scombridae	7			9		
Balistidae	169	3	12	20	2	14
Ostraciidae	12			2		
Tetradontidae	19	3	3	1		3
Diodontidae	43	18	4	1	3	5
Serranidae/ Lutjanidae	7			4		
Scombridae/ Sphyraenidae	2					
Non-Scaridae/ Non-Lethrinidae	124	6	3	26		13
Unknown	62	2	2	7		5
Total NISP	2250	116	110	305	25	267

most Lapita assemblages (Green 1986). Finally, the abundance of fish remains representing taxa not normally taken by angling (e.g. scarids, acanthurids, balistids, diodontids) suggests Lapita fish procurement did not focus heavily on hook and line techniques.

All excavated sites produced abundant quantities of marine molluscs, both gastropods and bivalves, from a wide variety of habitats (mangrove, sandy bottom, reef edge, and surge zone species). While there are some differences between site assemblages, no major temporal trends have as yet been noted. Shellfish gathering appears to have been a consistently practised subsistence strategy throughout the entire span of Mussau prehistory.

An extremely important result of the 1986 excavations at ECA was the recovery from the waterlogged deposits of more than 5000 anaerobically-preserved plant remains, primarily seeds, seed cases, endocarps, and similar materials representing at least 19 taxa. These materials have been enumerated and described elsewhere (Kirch 1988a:Table 3, 1989) The taxa represented include most of the tree cultigens dominant in indigenous Bismarck Archipelago arboriculture (Yen in Allen 1985). These include *Cocos nucifera, Inocarpus fagiferus, Canarium indicum, Corynocarpus cribbeanus, Dracontomelon dao, Spondias dulcis, Pometia pinnata, Pangium edule, Terminalia catappa* and *Burckella obovata.* These materials provide direct botanical evidence that complex arboriculture was a component of Lapita subsistence, and that the tree cropping so important on the Mussau Islands today has an antiquity extending back at least 3500 years.

Settlement patterns

Our survey and excavations suggest relatively little change in settlement patterns over the course of the Mussau sequence. The primary pattern, from Lapita through the late prehistoric period, has been one of reasonably large villages situated on coastal beach ridges or terraces, both on the offshore islands, and along the coast of the main island. At ECA, we have evidence of both beach ridge occupation and stilt houses over the reef flat; there is no evidence that stilt houses persisted into the later prehistoric period. Rockshelters were used throughout the sequence. There was some tendency in the late prehistoric period, however, toward dispersed inland settlement of small hamlets. This is clear on Eloaua Island from the evidence of very small midden sites dispersed over the interior upraised coral plateau. On Mussau Island, we also observed small inland sites on interior ridges. These dispersed inland settlements, however, did not replace the pattern of coastal villages, such as EKS, which continued to be occupied into the historic period.

Importing and exchange

Although inter-island exchange has figured prominently in discussions of Lapita (e.g. Green 1976, 1978, 1982; Clark and Terrell 1978; Kirch 1988b), direct evidence for the movement of materials between Lapita communities has been restricted primarily to obsidian and other rocks of restricted natural distribution (Allen and Bell 1988). Indeed, although substantial work has been done on the sourcing of Melanesian obsidians using PIXE-PIGME and other techniques (Ambrose and Green 1972; Bird and Russell 1976; Duerden et al. 1979), archaeological data for the quantities of obsidian moved, the proportions of different sources represented, and temporal trends in these variables, were available only for a single case, that of the Reef-Santa Cruz Islands (Green 1979, 1987). In the Mussau Project, a major objective has been to extend our knowledge of Lapita (and post-Lapita) exchange through the quantitative analysis not only of obsidian, but of ceramics, and of other exotic materials such as oven stone. Here we summarise the preliminary results of our investigations of obsidian and ceramics.

Obsidian. We have followed the method developed by Green (1987) of sorting obsidian specimens into specific gravity density ranges using a nontoxic heavy liquid (sodium metatungstate). This allows the entire archaeological collection to be stratified, with subsequent PIXE-PIGME sampling (101 Mussau specimens were submitted to Roger Bird at the Australian Atomic Energy Commission laboratories at Lucas Heights in Sydney). To date, 2981 obsidian artefacts from 10 Mussau sites have been sorted with the sodium-metatungstate method. Cutting points at 2.356 and 2.387 specific gravity were based on prior work by Green (1987), following Ambrose's (ms.) determination of the density ranges for Admiralty (Manus) and Willaumez (West New Britain) sources. Subsequent PIXE-PIGME analysis of 101 selected Mussau specimens confirms that all artefacts with densities equal to or greater than 2.387 derive from the Admiralties (Lou), as did all of those in the 2.387-2.356 'overlap' category. However, 35% of those specimens with densities of less than 2.356 are also chemically classified with Admiralties sources, with 65% from Willaumez.

Some of the Mussau Admiralities obsidian is evidently from subsources which have densities of less than 2.356, therein extending the previously identified density range of this source.

The major temporal trends revealed by our analysis of obsidian are diagrammed in Figures 6 and 7. At site ECA, 852 specimens from six analytic zones have been sorted. Obsidian use peaks in Zone C3, corresponding with an increase in incised ceramics. Thereafter, there is a clear trend of decreasing obsidian as measured by both counts and weights. Furthermore, in the earlier zones, Willaumez and Admiralty sources are about equally represented, whereas later zones show a dominance of Admiralty material. The Lapita sites ECB and EHB also show a mix of Willaumez and Admiralties sources, although with the latter dominating; disturbed stratigraphy in these sites does not make temporal analysis possible.

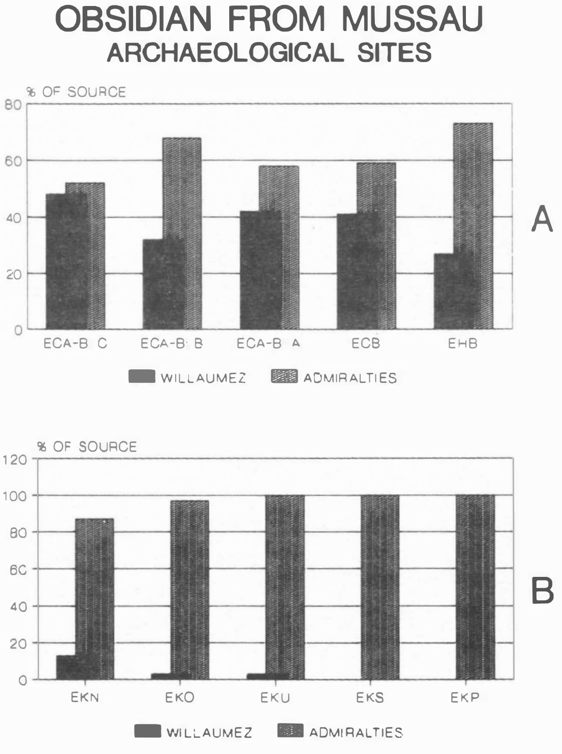

Figure 6 *Relative frequencies of Talasea and Admiralty Islands obsidian in Mussau archaeological assemblages: A) relative frequencies in Lapita sites, ECA Area B (Zones C, B, and A), ECB, and EHB; B) relative frequencies in non-Lapita sites EKN, EKO, EKU, EKS, and EKP.*

At site EKQ, 734 specimens from four strata have been sorted. In the basal Layer IV, obsidian use is relatively low, and the two sources are equally represented (in Fig. 7, the correlations of spits and strata are: I=1-4, II=5-10, III=11-16 and IV=17-20). As in the ECA site sequence, the Admiralties source becomes increasingly dominant in higher levels of the site. Obsidian usage peaks in Layer II, with Willaumez representing only 11% of the assemblage.

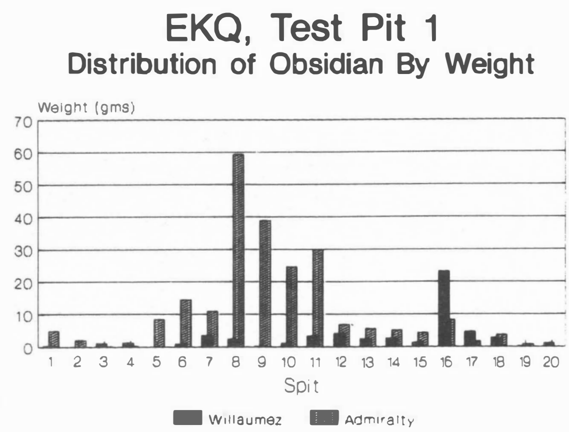

Figure 7 *Distribution of obsidian (by weight) from the Talasea and Admiralty Islands sources in TP-1 of the EKQ rockshelter. Excavation spits are labelled from top to bottom. Note the major increase in Admiralty Islands source material in Spit 11.*

Taking the Mussau collection as a whole, the dominant trends are: (1) a decrease in the use of obsidian from the Willaumez source; and, (2) an overall decrease in obsidian importing and utilisation. At the thirteenth century AD site of EKU, small quantities of Willaumez obsidian are present, but at late prehistoric sites EKS, EHM, and EKP, only Admiralties materials occur. Considering the pene-contemporaneous Lapita sites (ECA, ECB, EHB), the obsidian assemblage of Site ECA-Area B is distinctive in the high frequency of Willaumez materials represented, approximately 50% in basal Zone C. There are also indications from the technological analysis of this assemblage of a more wasteful reduction strategy at Area B.

In sum, an overall trend in the Mussau sequence of obsidian importing and use can now be identified: from an early phase of importing large quantities of obsidian from both New Britain and Manus, to a later phase in which the total flow of obsidian decreased, and in which the Manus source dominated almost exclusively. Obsidian importing evidently continued into late prehistory, but only from Manus. These trends contrast with those from the Reef/Santa Cruz sites, in which Willaumez materials predominate throughout the 700 year sequence, with Admiralty and

Vanuatu sources representing only about 1% of the collections (Green 1987). For the Ambitle site, in contrast, the primary source of obsidian was evidently the Admiralties (Ambrose and Duerden 1982; Bird et al. 1981; Ambrose 1978). It is likely that the Admiralties obsidian reaching Ambitle passed through Mussau (see proximal point analyses of Hunt 1988), an interesting possibility in light of the close similarities between the Lapita ceramic assemblages of these sites (Anson 1986).

Ceramics. Determination of ceramic provenance has been based on investigation of both temper and clay constituents. Relatively large samples of sherds from ECA, ECB, EHB, EKQ, and EKU were examined for temper composition as a first stage in analysis. Ceramics with temper mixtures predominantly non-calcareous (e.g. volcaniclastic) in composition were regarded as necessarily being imports to Mussau (assuming that pots, and not temper, were transported between Lapita communities). This conclusion is based on our knowledge of Mussau geology, which cannot account for concentrated mineral sand deposits relatively free of calcareous or other carbonate detritus. Temper mixtures in pottery predominantly of calcareous sand are consistent with sediments naturally occurring in the Mussau group, and in terms of temper alone, these classes are regarded as 'local' in origin. However, the likelihood that calcareous sand tempered pottery was imported to Mussau could not be rejected a priori, and required that we undertake compositional analysis of the clay matrix of these ceramics.

Elemental analyses of ceramics from the five sites listed above were conducted using a scanning electron microscope (SEM) in conjunction with an x-ray analyser. All analyses were conducted on a JEOL model JSM-840A SEM fitted with a Tracor Northern energy-dispersive x-ray detector (lithium-drifted silicon crystal) housed at the University of Washington (Seattle). Quantitative analysis of elemental proportions was achieved using the ZAPF correction method which compensates for factors affecting x-ray counts detected and consequently quantified (Goldstein et al. 1981:275-392). Resulting values represent concentrations of elements by elemental weight percentages. Goodness of fit between those elements quantified and standard (theoretical) intensities for particular operating conditions was evaluated by chi-square test (Goldstein et al. 1981:411-2). Further details of methodology are presented in Hunt (1989).

A total of 172 sherds, representing all major ceramic classes defined by dimensions of decoration and of temper, together with three clay source tiles (from local Mussau clay, and from two Manus sources) were analysed for elemental composition. These 175 ceramic specimens together comprise the data matrix used in quantitative analyses. Statistical analysis used two agglomerative hierarchical clustering procedures: complete (furthest neighbour) linkage and average (within groups) linkage (SPSS-X Version 3.0). Use of two methods allowed an objective evaluation of cluster solutions, and only those dendrogram solutions that arose independently were considered valid, or accurately descriptive (cf. Dunnell 1983:146; Sokal and Sneath 1963: 166). Specimens not occurring in identical clusters across both dendograms were treated as unassigned (i.e. of unknown cluster membership).

These analyses revealed 16 consistent cluster groups including 150 ceramic specimens; an additional 25 sherds (14.3%) were placed differently in the two cluster solutions and were thus considered as unknowns. Those sherds which clustered with the known Mussau clay control tile are of posited local Mussau origin. Similarly, sherds that occur together in clusters with the control tiles from the contemporary Ahus and M'buke potting industries in Manus are regarded as deriving from those sources. The remaining clusters represent clay compositional groups as yet unmatched to specific clay source samples, but believed to represent imports to Mussau. Table 4 summarises the results of these compositional analyses.

Examination of the occurrence of clusters across sites of different age in Mussau reveals that ceramic exchange from a number of production locales (as evidenced by compositional distinctions comprising 13 clusters, including the local Mussau clay) has been important throughout much, if not all, of the prehistoric sequence. The sites with the earliest components (ECB, ECA Area B, and EHB) also have the greatest variety of compositional groups (12, 11, and 9 groups respectively). The post-Lapita site of EKU has only three compositional groups. Moreover, the simple volume of pottery represented in the earliest sites, together with the evidence of compositional heterogeneity, suggest that ceramic importing decreased in scale both in terms of numbers of pots transferred and in the number of production locales involved.

Sherds from ECB, ECA Area A, and EKQ included those which clustered with paste collected by May and Tuckson (1982) from the

Table 4 *Results of compositional analysis of Mussau ceramics: [a] includes Mussau clay source; [b] includes M'buke clay source; [c] includes Ahus Island clay source.*

	ECB Area B	ECA	EHB	ECA Area A	EKQ	EKU
Size of assemblage	933	1133	1340	348	6663	43
Size of analysed sample	33	56	29	18	32	4
Sample as a percentage of assemblage	3.5	4.9	2.1	5.1	0.4	9.3
Number of compositional groups	12[a]	11	9	6[b]	8[a,b]	3[c]
Estimated percentage of local ceramics	11.7	0	0	0	9.3	0
Estimated percentage of exotic ceramics	88.3	100.0	100.0	100.0	90.7	100.0
Approximate age range of assemblage	1450-800 BC	1350-400 BC	1300-1150 BC	900-800 BC	1000-400 BC	AD 1250-?

contemporary potters of M'buke Island, Manus group. Only the later site of EKU yielded sherds which clustered with the paste collected from contemporary potters of Ahus Island off the north coast of Manus Island (May and Tuckson 1982; cf. Kennedy 1981). The sherds which form clusters apart from the known clays of Manus and Mussau derive from as yet unknown sources somewhere in the Bismarcks or beyond.

The results of the ceramic compositional analyses indicate that all classes of pottery (dentate-stamped, incised, and plain ware) analysed from sites ECA Area B, ECA Area A, EHB, and EKU were imported to Mussau. No sherds from these assemblages clustered with the local Mussau clay sample. A small proportion of sherds from Sites ECB and EKQ clustered with the local Mussau clay and significantly, these contained only calcareous sand temper – consistent with local ceramic production somewhere in the Mussau group. Otherwise, sites ECB and EKQ have ceramic assemblages with an estimated 88% and 91% exotic provenance.

The results of these ceramic compositional analyses provide evidence for the extensive and long term inter-island transfer of large volumes of pottery, both during the Lapita period and in later prehistory. Further understanding of this complex pattern of ceramic exchange now requires the extension of these, or similar, analytical techniques to other excavated assemblages throughout the Bismarck Archipelago.

CONCLUSIONS

The substantive results of our efforts to define a prehistoric sequence for the Mussau Islands – in terms not only of ceramics and portable artefacts, but also of subsistence strategies, settlement patterns, and exchange systems – have been summarised above. We conclude by suggesting four major implications of the Mussau sequence for Western Melanesian prehistory.

1. Despite a concerted effort to seek pre-Lapita assemblages (particularly through the testing of several rockshelter sites both on Eloaua and on the main island of Mussau), we have no evidence of any occupation of the Mussau group prior to Lapita, beginning at c.1600 BC. Given the presence of pre-Lapita sites in such islands as Manus and Nissan (Kennedy 1981; Spriggs, this volume; Ambrose, this volume), the possibility that similar sites remain to be discovered in Mussau must be left open. Nonetheless, on the present evidence, a true Lapita colonisation of Mussau (in the fullest sense of that term) may be indicated. The earliest Lapita materials in Mussau include the most elaborate ceramics, both in terms of the range of vessel forms and in decoration. We have no evidence here of a local development of the Lapita complex. We believe this finding bears strongly on the question of Lapita origins, and does not lend support to the extreme view of an indigenous Bismarck Archi-

pelago homeland for Lapita recently advocated by White et al. (1988).

2. The areally differentiated assemblages from the large ECA site, in conjunction with the well stratified EKQ deposits, and the smaller ECB and EHB assemblages, provide a temporally controlled sequence of changes within the period of Lapita occupation in Mussau (i.e. from c.1600-500 BC). The Lapita ceramic sequence is particularly interesting, with a series of changes in vessel form and decorative technique. Most significant is the fairly rapid shift from a dominance of dentate-stamping to incising in decorative technique. These changes in the Mussau Lapita sequence appear to be parallelled in other locales throughout the Bismarcks, such as Watom and in the Arawes (Green and Anson, this volume; Gosden, this volume).
3. Since Lapita has always been defined primarily on the presence of its characteristic dentate-stamped ceramics, the absence of such pottery after about 500 BC has raised the question of whether Lapita communities were replaced by other cultural groups after this time. However, the Mussau ceramic sequence (which, as noted, appears to be parallelled elsewhere) clearly indicates that the loss of dentate-stamped pottery simply reflects a stylistic change in the ceramic complex, which continued to be manufactured by the same groups of people. In other words, there is direct continuity between Lapita and succeeding time periods.
4. While the last 2000 years of Mussau prehistory are at present less well attested archaeologically than the Lapita period, the appearance of several new artefact types (*Trochus* shell armrings, *Terebra* shell adzes, and dorsal margin *Tridacna* shell adzes in particular) is noteworthy. Again, the appearance (simultaneously?) of these forms in other parts of Melanesia may be significant. The *Terebra* shell adze, which appears to have an older prehistory in Micronesia, hints at possible contacts between Mussau and Micronesian islands to the north. The possibility of contacts between Mussau and Micronesia is also suggested by ethnographic evidence, such as the presence of the backstrap loom (Nevermann 1933), and by possible linguistic borrowings (Ross 1988). Continued contacts between Mussau and Manus are also indicated in the archaeological record, by both Lou Island obsidian and small quantities of Manus ceramics in Mussau sites of the last 2000 years.

POSTSCRIPT: THE 1988 SEASON

This article was essentially completed prior to undertaking a third field season in Mussau, from August-December 1988, and it has not been possible to incorporate the results of this latest field season into the present discussion. Nonetheless, some brief remarks on the 1988 field season may help to amplify certain points raised above.

In 1988 additional excavations at ECA concentrated on a new transect sample, extending along the W250 line from W250N70 to W250N200. These transect units revealed a geomorphological sequence in most respects identical to that exposed in 1986, reinforcing our interpretation of the site's depositional history. Further evidence was obtained to support the interpretation that at the time of initial Lapita occupation, sea level was approximately 1 m higher than at present, and that a relative drop in sea level was probably responsible for the progradation of the Eloaua shoreline and burial of the stilt house zone under calcareous sands.

The 1988 ECA transect excavations also confirmed that the zone of stilt houses – marked by anaerobically-preserved post bases – extends over a considerable area to the north of the Eloaua airfield, covering in excess of 10,000 m^2. At the north end of the transect, a concentration of such posts was exposed in an 8 m^2 areal excavation, designated Area C. Two successive occupation zones in Area C were characterised by an almost exclusive presence of incised ceramics, similar to those from Layers II and III of the EKQ rockshelter.

Extensive excavations were also carried out on Boliu Island, immediately north of Eloaua, at Site EKE, under the direction of Jason Tyler. The EKE site was chosen for investigation as an example of an extensive, largely aceramic midden, with such surface artefact types as *Terebra* shell adzes and *Trochus* shell armbands. A total of 19 m^2 was excavated, along one primary transect, with a second perpendicular transect and several test pits. The large site has a complex stratigraphy, which includes discontinuous basal beach deposits containing dentate-stamped Lapita pottery, intermediate deposits with incised ceramics, and later deposits with low frequencies of what we have tentatively identified as imported Manus ceramics. Some portions of the site are also wholly aceramic. We anticipate that the EKE site, after detailed analysis of the excavated assemblage, will greatly amplify the last 2000 years of the Mussau sequence.

More limited excavations were also carried out at EHK, an aceramic midden along the eastern coast of Eloaua Islands, under the direction of Nick Araho of the PNG National Museum. This site is also expected to yield important information on the later prehistory of Mussau.

A single test unit was excavated in a small, eroding shell midden (EKL) on Enusagila Island, a small coral islet on the southern side of the Eloaua-Emananus atoll. The islet is a frigate-bird rookery, and is not presently inhabited.

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Appendix 1 *List of archaeological sites in the Mussau Islands.*

Site	Island	Type Reference*	Grid
ECA	Eloaua	Lapita open site	GU925274
ECB	Eloaua	Lapita open site	GU913278
EHB	Emananus	Lapita open site	GU883254
EHC	Emananus	Open midden site	GU873267
EHK	Eloaua	Open midden, aceramic	GU945261
EHM	Eloaua	Cave, ceramic	GU948257
EHN	Eloaua	Rockshelter	GU948257
EHO	Mussau	Open midden, ceramic	HU023348
EKA	Eloaua	Open midden site	GU898279
EKB	Eloaua	Eroded coastal midden	GU899276
EKC	Mussau	Open midden site	GU995294
EKD	Emussau	Open midden, aceramic	GU954281
EKE	Boliu	Open midden site	GU947298
EKF	Boliu	Open midden site	GU949306
EKG	Ebolo	Open midden site	GU964305
EKH	Eloaua	Midden scatter	GU928264
EKI	Eloaua	Burial cave	GU927264
EKJ	Eloaua	Midden scatter	GU942262
EKK	Eloaua	Open midden site	GU945260
EKL	Enusagila	Open midden, aceramic	GU909259
EKM	Mussau	Open midden site	GU947322
EKN	Mussau	Open midden site	GU928322
EKO	Eloaua	Rockshelter	GU948257
EKP	Mussau	Rockshelter, aceramic	GU836494
EKQ	Mussau	Rockshelter, Lapita	GU833523
EKR	Ekaleu	Open midden site	GU879308
EKS	Emussau	Open midden, aceramic	GU956284
EKT	Emananus	Rockshelter complex	GU880251
EKU	Mussau	Open site, ceramic	GU984315
EKV	Ebangalu	Open midden site	GU875339
EKW	Enoanaulu	Open midden site	GU888327
EKX	Mussau	Open midden site	GU832538
EKY	Mussau	Rockshelter	GU832531
EKZ	Mussau	Rockshelter	GU857491
ELA	Mussau	Rockshelter	GU835544
ELB	Mussau	Rockshelter	GU832542
EKJ	Eloaua	Burial Cave	GU927264

* Coordinates refer to Universal Transverse Mercator Grid, Zone 55; Papua New Guinea 1:100,000 topographic survey, Sheet 8894 (Edition 1), Series T601, 'Mussau' (1980).

LAPITA SITES IN THE DUKE OF YORK ISLANDS

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The Duke of York Islands are in St George's Channel between New Britain and New Ireland (Fig. 1). The largest, Duke of York, is about 50 km^2 in area, while the other 11 islands range in size from 1 ha to about 7 km^2. Makada Island rises to a maximum of 120 m and consists of a volcanic plug and raised coral, with an extensive sand platform to the east. All the others are raised coral reefs, and apart from Duke of York, which reaches heights of 80 m in places, all are less than 40 m high.

Although patches of secondary rainforest occur throughout the group, coconut and cocoa plantations or food gardens dominate all except the two small islands off the northwest tip of Duke of York. Some coastal fringes, particularly in the south of Duke of York and on Makada, have extensive mangroves and swamps. In low areas, soils are poor and sometimes clayey coral sands, while in elevated places, soils are clayey and sometimes semi-cemented coral sands.

The Duke of Yorks group was one of the first places in Papua New Guinea to be permanently settled by Europeans and, amongst other interesting characters, saw the flamboyant 'Queen' Emma Kolbe as an early colonial resident (Firth 1977:5-6; Sack and Clark 1980:7-9). More recently, cultural anthropologists such as Errington (e.g. 1974) as well as Summer Institute of Linguistics (SIL) researchers have worked in different parts of the islands. However, despite the known importance of nearby Watom Island, the Duke of Yorks remained archaeologically unexplored until the present project was initiated.

The Lapita Homeland Project study of the Duke of Yorks aimed to assess the nature and integrity of the local archaeological record. Owing to time restrictions, only Duke of York, Makada and Mioko Islands were investigated. The survey of Duke of York entailed close inspection of all coastal margins and areas with caves and less intensive examination of swamps and raised inland areas. Test excavations were made in a number of localities thought to be good potential sites on the basis of surface finds or geomorphological context. Generally, soundings were dug along one or more traverses across the prospective areas. A circumnavigation of Makada found that all areas except the central south coast, where test excavations were undertaken, were either too swampy or too precipitous to warrant further attention. Mioko was examined in least detail because time was pressing and we had unusual difficulty in conveying the nature of our interests to local residents, who persisted in thinking we were concerned with traditional ceremonial sites until just before our departure, when we found a prehistoric site in Palpal hamlet. No excavations were undertaken anywhere on the island.

RESULTS

The survey and excavation programme found six Lapita sites, five on Duke of York and one on Mioko, as well as a variable background scatter of obsidian and a number of much more recent aceramic sites, most of which are dispersed on raised parts of Duke of York. All of the sites, and especially those containing Lapita, have been very severely disturbed by human activity. The most common form of damage is that inflicted by continual house building and gardening, but most sites have also been adversely affected by mission, commercial and World War II military activity. None of the several caves examined on the north and south coasts of Duke of York contained prehistoric remains, possibly because all but one (which is only accessible through its roof) were extensively modified by the Japanese during the war.

The Lapita site on Mioko (Papua New Guinea site register SDQ) is on the southern edge of Palpal hamlet on sheltered southwest coast of the island (Fig. 1). It comprises a substantial amount of badly degraded Lapita pottery and obsidian on the surface and very occasional sherds eroding from the upper 10 cm of a cutting for a beachside road. A grab sample of surface material was dominated by plain body sherds (Table 1).

The five Lapita sites on Duke of York are grouped in two places (Fig. 1). The first is an

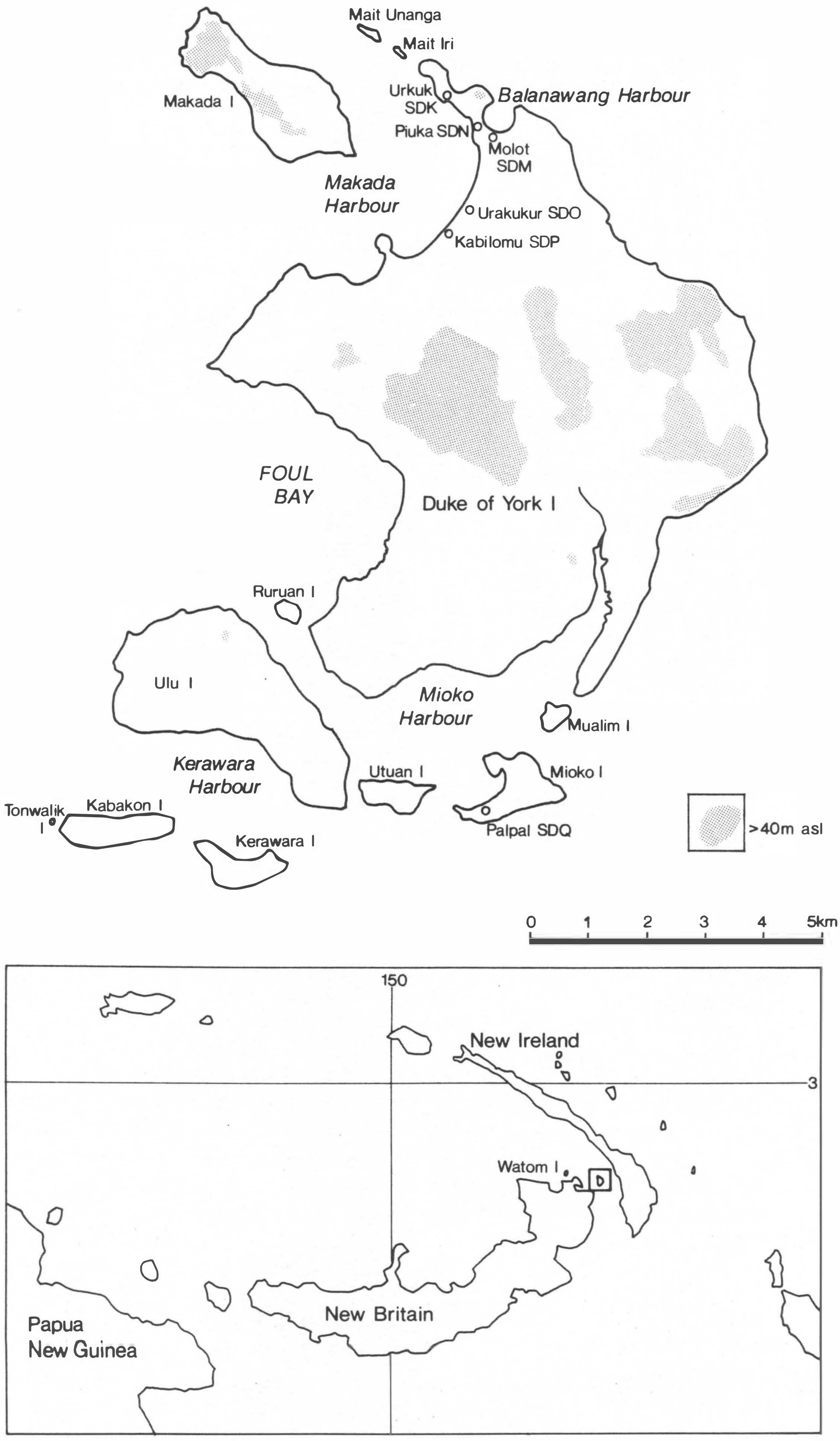

Figure 1 *Duke of York Islands showing Lapita site locations and other places mentioned in the text.*

Table 1 *Lapita pottery from the Duke of York Islands, divided into rims and body sherds by site. In 'other', [1] indicates* appliqué; *and [2] indicates fingernail impression.*

Site	Body sherds total	plain	dentate	incised	other	Rims total	plain	lip-notch	dentate	incised
SDM	242	242	-	-	-	7	6	1	-	-
SDN	228	215	3	9	1[1]	5	2	1	1	1
SDK	176	172	1	-	3[2]	5	-	4	1	-
SDO	53	53	-	-	-	1	-	1	-	-
SDP	820	818	-	-	2[2]	18	8	10	-	-
SDQ	119	117	-	2		3	2	1	-	-

isthmus, comprising raised reefs joined by sandy flats, which forms the northwestern extremity of the island (sites SDM, SDN, SDK). The second is the sheltered, low-lying northwest coast (sites SDO, SDP).

The isthmus sites SDM, SDN, SDK are in almost identical settings, namely sandy flats between raised reefs, and exhibit very similar stratigraphy and cultural contents. In all cases, clayey sand of variable depth overlies clean beach sand of undetermined depth. The clayey sand contains Lapita, obsidian, oven stones and occasional marine mollusc shells mixed with abundant post-contact material, while the beach sand is culturally sterile. SDM extends between Makada and Balanawang Harbours at the base of the isthmus in the area of Molot hamlet; SDN is in Piuka hamlet about 100 m northwest of SDM; and SDK is on the southwestern edge of Urkuk hamlet about 500 m northwest of SDN. Some details of the Lapita pottery excavated from these and the other Duke of Yorks sites are provided in Table 1 and Figure 2. Sites SDO and SDP are also in very similar settings immediately inland from the beach on the northwest coast. SDO is in Urakukur hamlet, while SDP is in Kabilomu, about 500 m to the south. The stratigraphy and meagre cultural contents of SDO mirror those of the isthmus sites, but while the abundant cultural remains from SDP are similar to those from the other sites, the stratigraphy above beach sand is considerably more complex. Incorporating sand, pumice, volcanic ash and organically rich soil, the present configuration of these upper sediments almost certainly results from continual human disturbance.

DISCUSSION AND CONCLUSION

The Lapita sites found by this study of the Duke of Yorks are all too severely disturbed to warrant radiocarbon dating or detailed analysis of the cultural material they contain. There are, however, three sets of general points which can be raised. The first is that while Lapita sherds are found in a number of western Melanesian assemblages dominated by fingernail impressed and appliqué pottery (e.g. Spriggs 1984), the reverse situation of relatively small numbers of fingernail impressed and appliqué sherds in otherwise 'classic' Lapita assemblages has so far been encountered only on Watom, Duke of York, the Kreslo surface collection and in the Arawe and Siassi Islands, and then at varying dates (Gosden et al. 1989:569-70; Lilley 1986:169; Gosden this volume; Green this volume, Specht this volume). While the status of the impressed and applied material in Lapita sites is yet to be ascertained, its distribution in time and space suggests three things.

1. That Lapita pottery assemblages in the New Britain region shared certain distinctive characteristics over a reasonable period.
2. That there may have been some sort of interaction sphere encompassing the whole New Britain region during this period, unlike in ethnographic times, when overlapping but more-or-less separate spheres covered its eastern and western halves (e.g. Harding 1967:9-14; Chowning 1978).
3. That the pottery and any interaction it may reflect are connected in some way with post-Lapita developments in the west New Britain-northeast New Guinea region (cf. Lilley 1988) as well as the eastern Bismarck Archipelago (e.g. Green and Anson this volume).

These possibilities are of interest in view of recent discussions concerning local and regional facies in Lapita ceramics and their possible relationship(s) with more recent cultural traditions in Melanesia (e.g. Gosden et al. 1989: 576-8; Spriggs 1984).

The second set of points concerns site locations. All the Lapita sites are on the sheltered western side of the islands on which they occur. Further, all are on low-lying sandy beaches facing usually calm harbours fringed by relatively narrow reefs, rather than in elevated or swampy

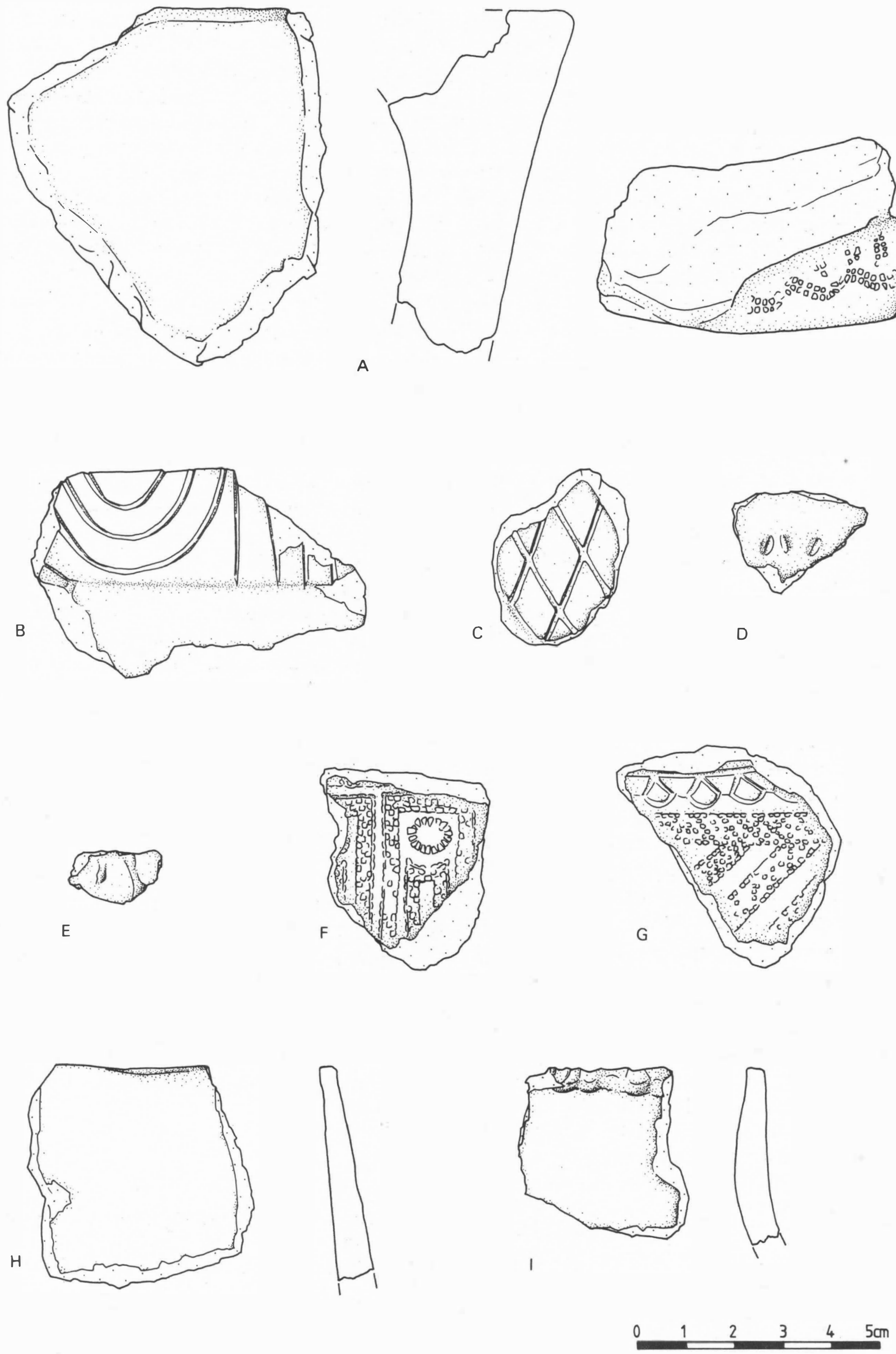

Figure 2 *Pottery from the Duke of York Islands. A: Lapita rim with dentate-stamped lip (SDN); B: incised and carinated Lapita body sherd (SDN); C: incised Lapita body sherd (SDN); D: fingernail impressed body sherd (SDK); E: appliqué decorated body sherd (SDN); F and G: dentate-stamped Lapita body sherds (SDN); H: plain Lapita rim (SDP); I: lip notched Lapita rim (SDO). Note: exterior of vessel to the right in cross-sections.*

areas or where there is substantial fringing reef. Near SDO and SDP, for example, the fringing reef is about 300 m wide, which, while not particularly narrow, stands in contrast to Foul Bay, where the reef is 1-2 km wide. Finally, the Duke of York sites occur in clusters. These general locational characteristics suggest that Lapita-using settlers sought sheltered, dry and relatively insect free village localities with reasonable canoe access and within easy reach of all marine and terrestrial resources (including arable land) available in and around the Duke of York group. The clustering of the Duke of York sites and the absence of Lapita in elevated areas could imply that in contrast to more recent times, social relations were relatively harmonious and village defensibility was not a major concern (cf. Gosden 1989). The point about arable land is an important one, because despite the disturbance of the sites, Lapita materials were found only in clayey sand, which could indicate that Lapita settlers gardened more elevated areas from the time of first occupation. Just such a pattern has been inferred for the Arawe Islands (Gosden 1989), but differs from that in the Siassi Islands (Lilley 1986:122-6, 1988), where clays from ground disturbance appear only in post-Lapita deposits.

The third set of points which can be made about the Duke of Yorks sites concerns spatial variation in the distribution of decorated Lapita sherds. In SDN and SDK, about 7% and 5% respectively of all sherds are decorated. Moreover, dentate-stamped sherds were only found in these sites, where they comprise 1-2% of the excavated assemblages. In the other sites, only 0.5-2.5% of sherds are decorated.

It may be that these differences result solely from site disturbance or sampling error. It can be speculated, however, that the variation may be of chronological or functional significance. On Watom, Green and Anson (this volume) found that dentate-stamping and linear incision diminished through time. In the earliest Watom assemblage, dated to c.2800 BP, 28% of sherds are decorated, half with dentate-stamping. Between 2400 BP and 1900 BP, these proportions drop from 9% and 7% respectively to as little as 3.8% and 0.4% in some localities. They continue to diminish until in the most recent assemblage, dated to c.1700 BP, they represent only 1.6% of decorated sherds, with less than 0.5% dentate-stamped. If this pattern holds for the Duke of Yorks, the spatial variation in sherd decoration could imply that SDN and SDK date to c.2400 BP-1900 BP and were settled and perhaps abandoned before the other Duke of Yorks Lapita sites were occupied.

Why any such shift in settlement pattern may have occurred cannot be ascertained. It can be speculated, though, that the move(s) accompanied changes in the nature of human activity in the islands which, amongst other things, reduced the use of decorated pottery in general and dentate-stamped pottery in particular. If, as has been proposed before (e.g. Kirch 1988), dentate-stamped pottery was a high status or ritual good, it may be that some sort of sociopolitical change occurred.

If the spatial variation in decoration is real but has no temporal implications, and all the Lapita sites in the Duke of Yorks are roughly contemporaneous, the foregoing discussion suggests that there may have been functional differences between the dentate-stamped material from SDN and SDK on the one hand and the remainder of the pottery on the other. A similar situation occurs on Watom between the SAC and SDI localities (Green and Anson this volume; cf. Gosden et al. 1989:572-3). In both instances, such variation may reflect differences between the sociopolitical standing of at least some of the inhabitants of the dentate-stamped pottery sites and that of the rest of the local Lapita population. While testing such conjecture is beyond the existing Duke of Yorks data, further work in the group and elsewhere in northwest Melanesia may cast further light on such matters.

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THE REBER-RAKIVAL LAPITA SITE ON WATOM. IMPLICATIONS OF THE 1985 EXCAVATIONS AT THE SAC AND SDI LOCALITIES

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More than 79 and perhaps as many as 100 Lapita sites are known today (Kirch and Hunt 1988:17). The first of these to be investigated and published was in the area of the Reber mission station and Rakival village on the small island of Watom at the northern end of New Britain (Fig. 1). Here pottery sherds, some with unusual decoration, came to light as a result of stream erosion following a storm during the wet season of 1908-9 (Meyer 1909, 1910). Three localities were subsequently investigated by Meyer: Maravot on the southern side of the mission station near the stream; Kainapirina to the north of this, where the church and cemetery are located adjacent to the village, and Vunaburigai at the northern end of Rakival village itself. While Meyer later found more pottery on the mission station and the village proper, especially in the 1920s, the significance of his results was not really appreciated until the 1950s and 1960s with the recognition that others had reported or were now finding similar pottery from sites in Vanuatu, New Caledonia, Fiji and Tonga. For these ceramically similar sites the name Lapita from a locality with this pottery on the Foué Peninsula in New Caledonia came into prominence (Golson 1971).

One aspect of the renewed interest in Lapita sites were the further investigations of a number of them. In the Reber-Rakival area of Watom modern systematic excavations were made in 1965-66 by Specht (1968, nd). This time extensive work was done in the Maravot locality, or Site 6, which yielded some 3873 sherds in excavations covering 100 m^2. Specht also made two test squares covering 16 m^2 in the Kainapirina locality, or Site 8, which produced only 155 sherds. His testing in Vunaburigai, or Site 7, encountered sherd bearing deposits disturbed by more recent burials, unsuitable for further work.

These investigations by Specht and the associated stratigraphic details and radiocarbon dates answered many questions about Meyer's initial claims. They also laid the basis for a further more focussed enquiry in 1985 as part of the Lapita Homeland Project (Allen 1984). One conclusion was that additional work at Maravot, Site 6, or the SAD (Papua New Guinea site register designation) locality, would not prove particularly profitable. In the Lapita period the locality was a low-lying rocky area, subject to waterlogging from the stream and/or an associated swampy embayment, so that while basal deposits there containing Lapita age materials underlay a thick volcanic ash from the c.7th to 8th century AD Rabaul eruption, they did not provide an entirely satisfactory stratigraphic context for them, nor one easily excavated. In contrast, what seemed to be ceramically less productive basal deposits at Kainapirina, also known as Site 8, or locality SAC, did exhibit a secure and undisturbed stratigraphic context underlying the volcanic ash. Moreover the locality was one associated with otherwise elusive evidence on Lapita age burials (Green et al. 1989). Equally important was an unpublished subsurface soil contour map of this general area done by C.A. Key for Specht based on his excavation data plus Key's more extensive augering at intervals along a grid. It suggested that here the Lapita occupation was on a well drained sandy coralline beach flat with low-lying dunes, a surface supporting intact Lapita deposits reasonably suited to areal excavation. Consequently the 1985 excavations focussed on reopening the 10 m^2 of Specht's Trench I at Site 8, completing the excavation of its basal levels, and then extending out to encompass a contiguous zone 38 m^2, which together with Specht's Trench II gave a total sample spread over some 42 m^2 of surface in the SAC locality (Fig. 1).

A resolution to the problem of where to excavate in Rakival village itself, in order to confirm and complement the results emerging from the excavations at SAC, was guided by

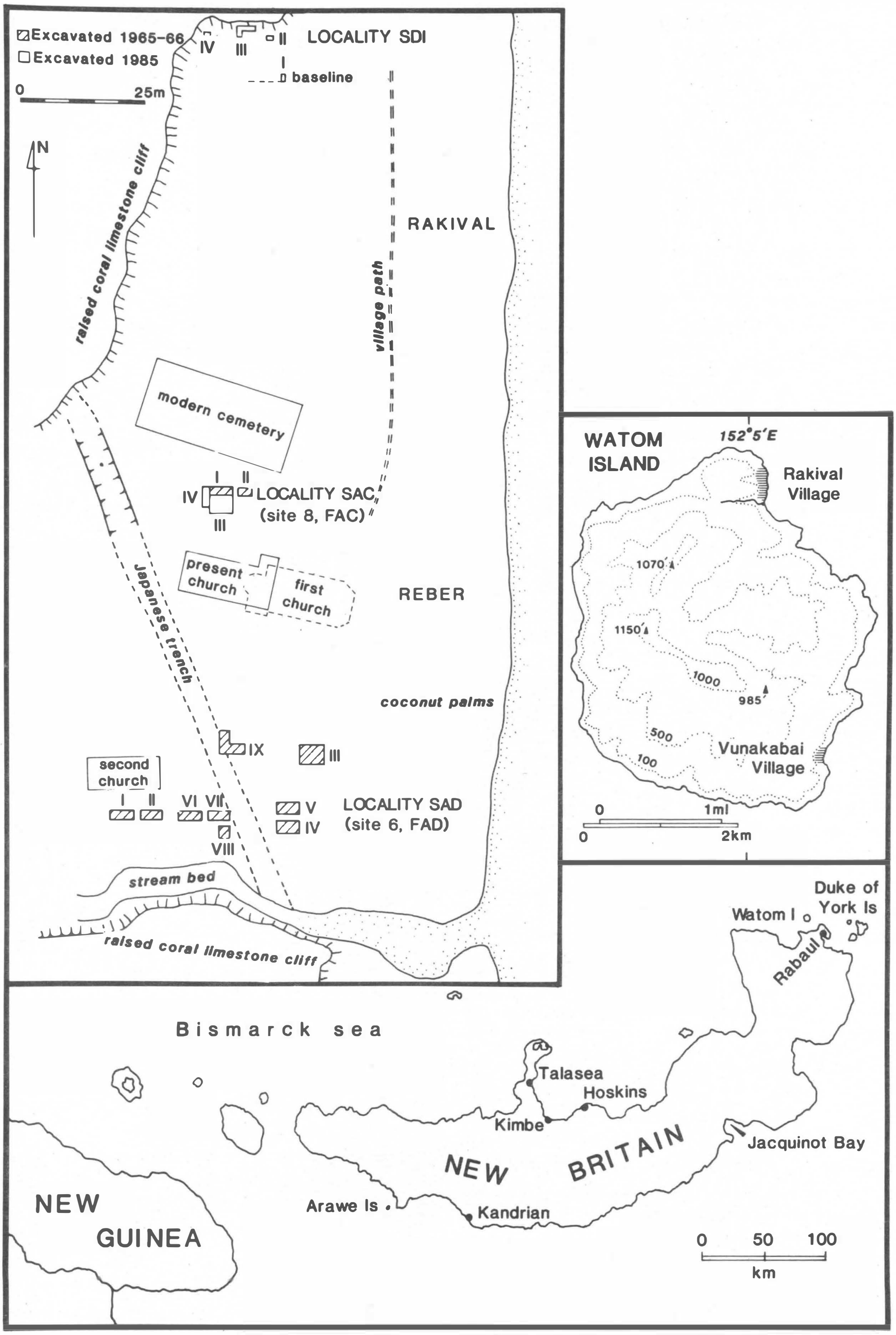

Figure 1 *Maps of part of the Bismarck Archipelago, Watom Island, and the excavation localities in the Reber-Rakival Lapita site.*

items retained and given to us on arrival by the local storekeeper and village headman. These he had recovered when digging two garbage pits inland of his residence and the main village path on land called Vunavaung. This locality, with its baseline 82.5 m northeast of site SAC, was coded as SDI. Initially, tests near the garbage pits turned up the expected pottery but in deposits which were either partially or totally disturbed by later activity. Only through opening test trenches upslope and further inland, close to the coral limestone cliffs along the back of the village, were undisturbed Lapita deposits sealed by volcanic ash finally encountered. Here they were capped by some 1.8 m or more of deposit starting with topsoil and then proceeding through layers of redeposited and finally in situ volcanic ash, all lying on top of four separate occupation layers that were in total some 1.5 m deep.

The outcome now is that there have been three sets of investigations in the course of some 77 years in at least four separate localities in the Reber-Rakival area. From these we currently have useful if sometimes equivocal results for the Maravot (SAD) locality, excellent and highly informative data from the Kainapirina (SAC) locality, limited but important supporting information from the Vunavaung (SDI) locality, and indicative evidence only of a Lapita presence at Vunaburigai (SAB). Still, it was not until after the excavations of 1985 and analysis of the data recovered from them that an informed assessment of aspects of Lapita occupation for localities within the Reber-Rakival area became possible. They at last allow the first Lapita site discovered and reported in the literature nearly 80 years ago to be properly integrated among the others assigned to that cultural complex. Watom now becomes much more than a series of find-spots of Lapita pottery with sometimes insecure stratigraphic provenances of rather uncertain age that seldom seemed to offer much potential for revealing reliable associations between the different categories of portable artefacts, midden, burials, and structural features encountered below the volcanic ash of the Rabaul Eruption. Instead, reasonably well supported statements about the Lapita deposits in the area can be made in respect of the sequence and ceramic contents of occupations in the various localities examined. In addition, for the two occupation layers at the SAC locality there are more detailed data on aspects of the faunal contents of each, information reflecting temporal change in their pottery and obsidian assemblages, and preliminary results on the exotic sources for some of these items. Finally there is, for the first time, important information on the structural features from the lower occupation at SAC associated with securely dated skeletal material providing data on both biological features and burial position. Brief comments on each of these topics will indicate how the Lapita assemblages from Watom serve to enhance the rapidly accumulating knowledge of this cultural complex in the Bismarck Archipelago.

SEQUENCE OF OCCUPATIONS AND THEIR AGES

A date of c.1400 years ago has been assigned to the latest large magnitude eruption of the Rabaul volcano usually referred to as the Rabaul Eruption (Walker et al. 1981:181). The characteristic primary ash falls from this eruption reached Watom, where up to 40 cm of mostly fine airfall ashes have been observed above the basal Plinian pumice (Walker et al. 1981:186). Subsequent erosion and redeposition then resulted in secondary deposits of ash of various thicknesses. In the Reber-Rakival area excavation has revealed such primary and secondary ash deposits with the following depths: Meyer recorded an overall depth of 70 cm for both (Specht 1968:70); Specht recorded some 40 cm depth for his Zone 3 redeposited volcanic ashes at SAD (Specht nd, Specht 1968:122), we recorded up to 1 m depth for both at SAC, and up to 1.4 m depth for both at SDI. Modern soils and other deposits above these ashes range from 40 cm up to 1.5 m in depth. Everywhere volcanic ashes in the area seal underlying deposits associated with Lapita pottery, although extensive later disturbance, where it has penetrated the ash, destroys this relationship and pottery can be found in more recent stratigraphic contexts or on the surface, in the stream, or on the current beach. From a lens of charred vegetation directly under the primary ash at SDI, a radiocarbon determination of 1290±90 BP (ANU-5338) fixes the primary ashfall in this area of Watom as occurring with a probability in the one standard deviation range between 650 AD and 850 AD, including three intercept values between 686 AD and 767 AD (Stuiver and Becker 1986; Stuiver and Reimer 1986). This is slightly later than but not inconsistent with the previous 1400 BP general estimate for the Rabaul Eruption.

Specht (1968:123-4) was unable to radiocarbon date his sherd-bearing Zones 4a and 4b at Maravot (SAD). Stratigraphically, neither zone is a direct equivalent of the pre-eruption occupation layers at other localities in the Reber-

Rakival area. General correspondences, however, may be suggested. Zone 4a seems to reflect deposition in a watery environment of the initial phases of primary volcanic ashfall interspersed with washed-in lenses of silt containing sherds from the immediate vicinity. The underlying Zone 4b, on the other hand, is a thick grey sandy clay giving way to an intermittent black clayey mud at its base which contains many sherds plus bones and a number of stone artefacts. Presumably the deposit is indicative of a waterlogged environment into which sand from the beach flat, just to the north, was washed and one where a certain amount of debris from Lapita occupation close by was also actively dumped. In many trenches in this locality these deposits have unfortunately been subsequently reworked by crab-burrowing and stream or sea activity (Specht 1985:13-5). Thus the SAD assemblage can only be relatively dated and associated with the assemblages from the other localities by ceramic comparisons (see section on pottery below). The cultural material recovered probably spans much of the time of the late Lapita occupation in the Reber-Rakival area. What is important is that within an age corrected one sigma range of 394 AD (429 AD) 539 AD (ANU-73:1595±60 BP), there was a fairly extensive swamp vegetation over this locality and it was probably unsuitable for human occupation (Specht 1968:125). Not long after it was covered by secondary ashfall deposits.

The SAC locality at Kainapirina exhibits two distinct occupation layers under the volcanic ash and overlying the coralline beach sand surface, which is here only just above current high tide level, although it is some 50 m inland from the present beach (Fig. 1). Specht (1968:123) referred to these two occupation layers as zones C1 and C2. We called C1 the black loam layer. It was largely devoid of coral or shell fragments and exhibited only a few shallow remnants of now truncated features at its base. Underlying that is Zone C2, a grey sand 'midden' layer with coral, whole shells and shell fragments and numerous features definable towards its base. We believe the black loam layer is a palaeosol, probably well turned over by gardening activity that destroyed most structural features associated with the more recent Lapita occupation. Only a few partially dissolved *Tridacna* shells were recovered from its base, one of which was radiocarbon dated to 2390±80 BP (ANU-5330). When corrected for the currently used Delta R ocean reservoir effect of O (Gosden et al. 1989: Table 1; Spriggs 1990) and then calibrated to a calendrical time scale, there is a one sigma probability the age lies within the 160 BC (96 BC) 38 AD range.

The C2 zone produced no charcoal samples suitable for dating. Previously a sample consisting of a selection of human bone from the burials recovered by Specht (1968:124) yielded a result of 2420±110 BP (ANU-37b). However, interpreting the true age of this bone sample is notoriously difficult due to a variety of factors including the effect of human diet on age determinations by the C^{14} method. On a normal terrestrial standard any two sigma age from 810 BC to 211 BC could be possible; on a marine standard using a Delta R of O any two sigma age from 370 BC to 150 AD is likely. Both are compatible with the stratigraphic position of the burials as well as the other C^{14} determinations for Zones C1 and C2. Two laboratories obtained determinations on a *Tridacna* shell from the base of Zone C2 of 2530±90 BP (ANU-5336) and 2470±75 BP (Beta-16835). When the two very similar results are combined by the appropriate Case I formula (Polach 1969; Ward and Wilson 1978), a pooled result of 2495±58 BP can be calculated. Using a Delta R ocean reservoir correction of O would suggest a two sigma calendrical age somewhere between 350 BC and 50 BC with an intercept value of 184 BC. This result, together with that for the human bone, and that for Zone C1, all suggest the two occupation layers at SAC would seem to span the age range between circa 400 BC and 100 AD.

A *Turbo* shell from within the base of a feature at SAC produced a radiocarbon result of 3490±80 BP (ANU-5339). This is perceived as being too old to date the C2 zone occupation, and is thought to relate either to the age of the coralline beach or just possibly to some food shell from initial Lapita occupation of the Reber-Rakival area in the one sigma 1509 BC (1415 BC) 1350 BC age range.

More certain indications of earlier Lapita occupation in the Reber-Rakival area come from the Vunavaung (SDI) locality. Here the basal occupation of Layer 4, exhibiting the highest percentage of Lapita decorated pottery so far recovered from anywhere in the area, has a *Tridacna* shell radiocarbon determination of 3020±90 BP (Beta-16836). A Delta R ocean reservoir correction of O and a one sigma calendrical calibration suggests a probable age in the range 914 BC (817 BC) 770 BC. Two occupations with decreasing amounts of decorated pottery follow. A *Trochus niloticus* shell from Layer 3, yielded a C^{14} age of 2630±80 BP (ANU-6475). Its calibration provides a one sigma result of 405 BC (365 BC) 264 BC. From the next occupation layer a *Hippopus* shell sample was submitted which furnished a radiocarbon result of 2190±80 BP (ANU-5329). Again, using a Delta R value of O indicates a likely one sigma true age somewhere in the 84 AD (163 AD) 261 AD time span. This and the small undated sample of sherds from Layer 1 suggest a final occupation in this locality during the first few centuries AD.

In sum, Lapita occupation in the Reber-Rakival area is indicated at several localities by the 4th to 3rd centuries BC, and may well have begun in some localities such as SDI by the 8th century BC. However, most of the seven stratigraphically defined pottery assemblages recovered so far probably date from the first three or four centuries BC, to the first few centuries AD (Table 1). Subsequently, a soil horizon began to form in the locality of SAC, at times seemingly accompanied by gardening, while swamp vegetation established itself in the Maravot locality of SAD. All these Lapita bearing deposits were then precipitately and deeply buried by the Rabaul Eruption volcanic ashfall, quickly followed by redeposited ash transported from inland onto the coastal flat. Those events interrupted occupation in the locality for circa 600 years (Specht 1968:124 and ANU-72:720±57 BP). Later occupants of the area no longer used ceramics, and their relation to the former Lapita inhabitants remains obscure.

FAUNAL CONTENT AT SAC

Bone but not shell was preserved in the Zone C1 occupation layer at SAC; both bone and shell were recovered from the Zone C2 occupation layer there. This has allowed the identification of fish bone and other bone for both layers plus the shellfish for the Zone C2 layer. Aside from fish bone, pig, other mammal not assignable to species, bird, turtle, reptile, bandicoot, and crab were all identified as present in both layers by Ian Smith (Department of Anthropology, University of Otago). Minimum numbers could be established only for the pig, seven in Zone C1 and five in Zone C2, with an age typically in the range 12 to 24 months. This is consistent with Specht's (1968:125-6) reports of pig at SAD and SAC. His early bird bone from SAD was identified as that of domestic fowl. Pig and chicken are both well known from Lapita sites (Green 1979:37; Gosden et al. 1989:575; Kirch 1988:336-7).

Seven families of fish, including three identified at genus level were recognised from both Zones C1 and C2 by Michiko Intoh (Department of International Cultural Relations, Hokkaido Tokai University). Scaridae were the most numerous (55%) with Sparidae, (mostly *Monotaxis*), at 17% the next most common family. The remaining bones were distributed among the Labridae, Lutjanidae, and Serranidae families, plus an unidentified species of shark and a skate. These families indicate an emphasis on inshore fishing with a minor benthic element. That and the dominance of scarids is consistent with other Lapita sites, especially with the assemblages from the SE-RF-2 site in the Main Reef Islands, where a much larger and more diverse fish bone sample was recovered (Green 1986), and from the Eloaua ECA site (Kirch 1988:336).

Identifiable shells and shell fragments recovered from Zone C2 were sampled. Difficulties were experienced with separating food or midden shell from beach shell. Thus in large part only the more entire non water-rolled shells and larger shell fragments were considered. A detailed analysis of these by Elizabeth Hudson (Department of Anthropology, University of Auckland) is now available, and it compares favourably with the results from the Reef/Santa Cruz Lapita sites analysed by Swadling (1986) and those under study from the Eloaua ECA site (Kirch 1988:336). Intertidal bivalves make up

Table 1 *Order of Reber-Rakival sherd assemblages using percentage of decoration in each. Note that while the decrease in decoration is supported by radiocarbon dates and stratigraphic order of layers within localities, the trend is more variable between localities.*

Locality and layer	Assemblage sample size	Percentage of decorated sherds	Percentage of dentate-stamped sherds	Calibrated C^{14} date (1.sd)
SDI, Layer 1	182	1.6	0.5	
SDI, Layer 2	460	1.9	0.2	AD 84 - AD 261
SAD, Zone 4a & 4b	3873	2.2	0.7	
SDI, Layer 3	258	3.8	0.4	405 BC - 264 BC
SAC, Zone C1	487	6.0	3.4	160 BC - AD 38
SAC, Zone C2	129	9.0	7.0	291 BC - 113 BC
SDI, Layer 4	64	28.0	14.0	914 BC - 770 BC

20% of the sample in Zone C2 Spit 1 and 25% in Zone C2 Spit 2. Largely browsing molluscs, along with a few sand or mud species, make up the majority of shellfish gathered, 68% for Spit 1 and 63% for Spit 2. Nearly half in Spit 1 and more than half in Spit 2 of these browsing molluscs are either *Turbo* or *Trochus* shells. *Tridacna* are also well represented in Zone C2 and are the only shellfish known from Zone C1. Intertidal predatory and scavenging molluscs make up the remaining 12% for each Zone C2 Spit. Clearly a beach and reef zone much like that in front of Rakival today was the source for the shellfish.

Although this faunal information allows one to move from the rather general situation noted by Specht (1968) to a more detailed level of analysis, it is entirely consistent with what both he and Meyer reported previously. However, it is now possible to use these data with confidence in comparisons between Watom and the few other Lapita sites from which similarly detailed economic information is available (Gosden et al. 1989:573).

TEMPORAL CHANGE IN POTTERY AT SAC, SDI AND SAD

The ceramic assemblages from the SAC and SDI site are presently being studied by Anson and full details will be presented elsewhere. However, from the preliminary analysis (Table 1) we know the basal layer at SDI, which is the earliest dated assemblage, exhibits the highest frequency of decoration (28%) for any secure assemblage from the area. Fourteen percent of it is dentate-stamped. It is also known that the assemblages from the later layers at both SAC and SDI plus those excavated by Specht at SAD have much lower amounts of decoration.

In the SAD locality, decoration among the large sample of excavated sherds was a little more than 2% of which less than 1% used a dentate stamp. This contrasts with SAC Zone C2, where decoration is present on 9% of the sherds of which 7% is dentate-stamped. While the SAD collection is more like that of SAC Zone C1, where decoration is on 6% of the sherds, of which only 3.4% are dentate-stamped, it is most similar in its decorative content to the sherd assemblage from Layer 3 of SDI. Here decoration, 500 years after the initial occupation, had fallen to 3.8% and very few sherds (0.4%) were dentate-stamped.

It is now known that the amount of decoration in a Lapita site varies not only through time, but also from one locality in a site to another, presumably as a result of different activities or social arrangements (Gosden et al. 1989:572). They may in this case also be affected by sample size. Thus the 5-6% difference between the near contemporary SAC, Zone C2 and SDI, Layer 3 in amount of decoration and occurrence of dentate-stamped sherds may have more to do with location in the overall site and associated activities, plus sample size, than simply with their precise chronological placement. Therefore, insofar as the amount and types of decoration may be used as a rough guide to the chronological age of an assemblage, the large collection of sherds from the SAD locality can only be placed as discards in a wet area of the site, probably spanning the entire late end of the Reber-Rakival sequence.

We also know that the SDI site, despite the very small area (4.5 m^2) opened, yielded more than 960 sherds whereas at SAC the total yield was c.770 sherds for 42 m^2 of surface area of occupation deposit excavated. At the SDI locality this amounts to 6.75 m^3 of deposits, or 143 sherds per m^3. Given that the total Zone C occupation deposit at SAC was a more or less uniform 0.5-0.6 m thick, from the estimated 21-25 m^3 of occupation deposit excavated by us and by Specht, sherd recovery was c.31-37 per m^3 and only a couple of sherds in each m^3 were decorated. Often this meant only 3-5 sherds were recovered per 10 cm spit in each 1 m^2 within a layer, although the range was from none or one to 17. More sherds by nearly a factor of four occurred in Zone C1 than in Zone C2. Compared to many Lapita sites elsewhere, and the SDI locality at this site, the occupation deposits in locality SAC lie at the low frequency end in their ceramic content. However, the low yields are in fact quite comparable to the 3873 sherds Specht recovered at locality SAD. Here, he excavated 100 m^2 of SAD, often to depths of more than 1 m of sherd-bearing deposits. Again the effects of locality within a site are indicated.

These data also raise the question of a possible heirloom or curation effect in respect of dentate-stamped vessels. It may well be that their actual production ceased around the beginning of the first century AD. Yet broken examples of valued pots continued to be discarded for some time thereafter with sherds bearing new styles of decoration. More data are required to assess this possibility, but it has merit as there are few other regions in Oceania, except perhaps New Caledonia, where dentate-stamping appears to last into the first millennium AD (Spriggs 1990).

What attracted attention during excavation of the two occupation layers at SAC in the 28 m^2 of new area opened, which was not observed by Specht for the 155 sherds recovered in the 16 m² which he excavated, was a change in temper and other characteristics between sherds in Zones C1 and C2. We observed in our field report that

> the black loam layer pottery is thinner walled, and has a blackish dark mineral temper whereas the grey sand layer pottery is often thicker and most often has calcareous or shell temper in it.

Anson, in subsequent analysis in the laboratory, was able to show that some of these observations were in error, others were supported, and additional ones were possible. There is in fact no observable change in sherd thickness, and the dark minerals and rock fragments occur in sherds from both layers. Rather, detailed analysis revealed a gradual change in the frequency of white inclusions observable in the sherds from the two layers, and also a correlated (but more difficult to document) change in the colour of the clay fabric. This has been underpinned by mineralogical point counting in thin-sections of a selected sample of the sherds from SAC. It shows that Zone C2 at SAC is dominated by a carbonaceous modal temper type, while Zone C1 is dominated by a plagioclase feldspathic modal temper type. Moreover, sherds with the carbonaceous modal type are characterised by a matrix volume significantly lower than that of the feldspathic group. Thus the amount of clay in the sherds and their colour, as well as the temper itself, change between the lower grey sand 'midden' layer of Zone C2 and the upper black loam layer of Zone C1.

Anson has since re-examined some of Specht's material from Trench I. This was excavated by Specht in six 10 cm spits in Zone C. The selection of sherds was based on a sample chosen in the 1970s for elemental analysis using optical emission spectroscopy. Further analysis of those unpublished data showed that sherds in Spits 1, 3 and 4 (no sherds from Spit 2 had been included in the sample) are able to be statistically separated from those in the lower two spits, primarily on a complete lack of strontium in all but one of the sherds from the upper levels versus its presence in the lower ones. Point-counting of the same set of sherds indicates a similar pattern to that suggested by the distribution of strontium. In this instance the carbonaceous modal temper type is predominant in the sherds from the lower three spits, and the feldspathic modal type in the sherds from two spits of the upper set, no sample being present from Spit 2. It would therefore seem that while not perfect, the change in inclusions in the sherds from the two layers at SAC documented by us is also observable in the more limited sample recovered by Specht once one is looking for it.

There is also a difference in the mean size of the sherds in the two layers, probably because those in the upper gardened palaeosol are more comminuted by that activity than those in the lower layer. The same phenomenon was observed in respect of the reduced size of the mammal bone fragments.

In addition to the two localised pottery types, there are some sherds among these two modal temper types which possess quartz and potassium feldspars that possibly signal an exotic source in one of the island-arc plutonic intrusive rock complexes of nearby New Britain, probably the nearest one of the North Baining (Whalen 1985:604). The bulk of the pottery, however, based on the temper, is of local origin (Dickinson and Shutler 1979:1647), a conclusion similar to that reached by Anson (1983:166) on the basis of a microprobe analysis of a local Watom clay compared to Lapita sherds from there.

It was thought in the field that the gradual change observed in temper and colour of sherds at the SAC locality would also prove to be in evidence at SDI. The possibility was also considered that a change in temper type might assist in sorting and dating of the sherd collections from SAD. These hopes have not been realised. Among the grab sample of 28 sherds from the SAD locality so far examined in thin-section, 24 possess an indigenous Watom calcareous sand temper with a consistent plagioclase feldspar component (Dickinson and Shutler 1979:1647; E. Lohu, Appendix II in Anson 1983:289-91). These can be grouped together with two closely related sherds of a less calcareous Watom placer type. It seems evident that the tempers in these 26 sherds correspond pretty closely to the SAC calcareous modal temper type. Dickinson also established two other temper types for SAD based on a single sherd each, one of which, the Watom feldspathic, corresponds to our feldspathic modal temper type. The other, a sherd rich in hornblende, not found in any other sherds by either him or us, is an apparent exotic. Anson (1983:148) too, on elemental analysis, identified three apparently exotic sherds from SAD. As a consequence the SAD collection, which on its percentage of decoration, could be placed as spanning the late end of sequence of assemblages from the Reber-Rakival area, does not exhibit the expected numbers of sherds possessing a feldspathic temper, at least on the limited sample looked at up to now. Rather the SAD collection is of the SAC, Zone C2 type in respect to the temper used in those sherds.

This outcome, however, causes little concern when the SDI assemblages are examined more closely for evidence of change in temper type. Contrary to what might have been anticipated, no change could be found in controlled low-powered microscope counts of the frequency of white inclusions observable in plain sherds throughout the four layer SDI sequence. Rather, these results suggest a mixture of both calcareous and feldspathic modal temper types at all stages. This was also indicated by a limited sample of 10 thin sectioned and point-counted dentate-stamped sherds from this locality. A calcareous modal temper type was present in half of the sherds, and a feldspathic modal temper type in the other half. One is therefore left with the view that either the sequence went from a dominance of the Watom calcareous temper type, to the feldspathic type and then back to the calcareous type over the course of 800 plus years, or more likely, that the gradual change observable in temper type dominance in the sherd assemblages at SAC is something that is again locality specific and perhaps based on where the pottery was produced and/or its function. Temper changes need not be some general phenomenon applying in an overall fashion to the Reber-Rakival area. In sum, the evidence suggests that two temper types are present throughout the entire sequence, with dominance of one type at least sometimes affected by locality and/or time, and with some exotic sherds normally present in the various assemblages so far analysed.

Specht (1968:127-9) indicated that the ceramic collection from the SAD locality included three other decorative techniques besides dentate-stamping: these were linear incision, nail impression, and applied relief. Linear incision in the Lapita style is now widely recognised as part of the Lapita ceramic series and design system (Anson 1983:36; Donovan 1973, Frimigacci 1974). In the Reber-Rakival area it is present in all assemblages except for the upper three layers at SDI. However, its general occurrence is always in very low frequencies (i.e. less than 1%).

Anson (1983:48, 142) makes a strong case that nail-impressed and applied relief sherd types often occur together in the same deposits, and when sherds decorated in those styles are examined by elemental analysis of their clay fraction, they often form a discrete cluster from associated sherds decorated with dentate-stamping. Taken then as a single category, one, or sometimes both of the techniques, are present on individual sherds from all layers in SDI, and on some examples from Zone C1 of SAC, and from SAD. Given a generally low frequency of occurrence, sampling error could well explain their absence in Zone C2 of SAC, among the 129 sherds available from that assemblage. Moreover, because only one sherd with possible nail impressing occurred in the very small assemblage (64 items) of SDI Layer 4, it is not at all certain yet that this category is securely established as part of the earliest assemblages in the Reber-Rakival area. On the other hand the association of these distinctive techniques of pottery decoration with the later Lapita assemblages of Watom (SAC, Zone C1; SAD; SDI, Layers 1-2) is now established beyond reasonable doubt, even though they are only occasionally found in Lapita assemblages elsewhere. As a consequence, it is no longer necessary to interpret them as an entirely exotic element in the Reber-Rakival or other Lapita assemblages. This point is made elsewhere (papers by Lilley and Specht this volume). Specht reports from the Kreslo site a single sherd decorated with both dentate-stamping and nail impressions.

TEMPORAL CHANGE IN OBSIDIAN SOURCES AT SAC

Parallelling the change in pottery between Zones C1 and C2, is a change in the obsidian assemblages from the two occupation layers. Again, like the pottery, the amounts of obsidian in the two layers differ significantly, with something like 11 times more in the upper occupation layer. Detailed study of this material is proceeding. However, preliminary results from a sourcing study by Green, in conjunction with Roger Bird (Atomic Energy Commission, Lucas Heights, Sydney), are indicative of more than a change in frequencies of occurrence. For the 289 pieces from the lower layer, three source regions are indicated, Talasea on the Willaumez Peninsula, Mopir on nearby Cape Hoskins (Fig. 1), and Lou in the more distant Admiralty Islands (W. Ambrose and R. Bird pers. comm.). Results to date from elemental and density measurements on obsidian pieces in Zone C2 suggest nearly equal amounts from the Talasea and Lou sources, and a significantly lower percentage from Mopir. However, the quantity of obsidian from the Talasea sources rises significantly in Zone C1, while that from Mopir remain the same, and Lou falls to just slightly more than the Mopir level. Thus the nearest source in distance, Mopir, is never the dominant one, while the second nearest (and ethnographically only traditional source of obsidian – Specht 1981:350), Talasea, is not initially of very much more importance than the more distant one of Lou which required a long open sea voyage. This same phenomenon of factors other than simple distance to source dictating the degree of source utilisation was also observed in the Lapita sites of the Reef/Santa Cruz Island group (Green 1987:245). There the failure of distance from source to determine intensity of use was put down to various other social and ideological factors. In that article it was also observed that the complete dominance of the Talasea source in all three Reef/Santa Cruz Lapita sites would probably 'prove to be more like that in sites much closer to the Talasea source area (like Watom) or in the Talasea area itself' (Green 1987:245). It turns out this holds only for the later assemblage at SAC, while the slightly earlier assemblage at Watom, with nearly equal amounts of Lou obsidian, is more like the other early Lapita sites of Eloaua and Nissan (Gosden et al. 1989:575). The Watom trend to increasing amounts of Talasea obsidian, however, is opposite to one towards more Lou obsidian in later levels of Eloaua, Nissan and other sites in northern Bismarck and Buka region.

OTHER ARTEFACTS

Other artefacts do not occur in sufficient numbers to allow their quantitative assessment or extended consideration. The most important find from SDI was the *Tridacna* dorsal region shell adze from the uppermost level under the volcanic ash in Trench IV. To it can be added a piece of worked turtle shell from Trench III. The unstratified artefacts (apart from sherds) from SDI consist of two adzes and two modified pebbles, perhaps hammerstones. One adze has a vaguely sub-triangular cross-section, apex to the front, and the other is the cutting edge portion of the blade of an adze with a rectangular cross-section.

Another very small adze with a planilateral cross-section was recovered from SAC, as was one blade-like flake adze, in which the front slopes in to a ridge formed by previous flake scars. The poll of an adze of planilateral section, and a whole chisel were also found in this locality. They form part of the Lapita assemblage from there together with the shell shank leg of a one-piece fishhook, and two *Conus* shell fragments from arm ring ornaments, one of them grooved. These are all consistent with items found in association with the Lapita cultural complex (Green 1979), but do not add greatly to its definition in the Reber-Rakival area.

STRUCTURAL FEATURES AND ACTIVITY SEQUENCE AT SAC

Due to the waterlogged environmental situation at SAD, plus stream erosion and crab disturbance, structural features of cultural origin were not in evidence. In contrast, on the low sandy beach flat at SAC, they were common, particularly toward the base of Zone C2 (Fig. 2). While no record exists of features for the Zone C1 occupation in Trenches I and II at SAC, there is now one for a small number of shallow features penetrating into Zone C2, filled with the black loam layer. This record covers 28 m² and consists of seemingly truncated pits, stake and post holes and depressions. It seems likely that gardening activity to a depth of c.30 cm, turned over and incorporated into Zone C1 a small amount of material from the top of Zone C2. This is consistent with our finding of occasional disturbance of the upper part of Zone C2 burials, and the occurrence in the lower spits of Zone C1 of fragments of human bone. Presumably the more intensive occupation of Zone C1 was in the upper 20 cm of that layer, as evidenced by the higher numbers of pottery and obsidian pieces recovered from those spits. Thus despite being an instantaneously sealed deposit, the integrity of Zone C1 as an intact occupation at the time of the eruption had been compromised by gardening activity in the interval between the last occupation and the eruption. This interval may have been of the order of some 500 years, sufficient for the well developed palaeosol encountered to have formed with leaching from the soil itself of shell and coralline fragments.

The structural features associated with the Zone C2 layer are more diverse, more numerous, and more interesting. Stratigraphic relationships and superposition suggest that initially the SAC locality was occupied as one of the sections in the Reber-Rakival area used for habitation. This is attested to by an oven, numerous and sometimes intercutting small pits, and a number of post holes, some of fair size and depth. The function of this locality then seems to have changed to a cemetery area. At this time a low north-south trending stone alignment, largely of basalt boulders, but with some coral stones as well, was constructed along the eastern seaward side of the 38 m² excavation area, while inland of that seven oval to round burial pits were found, as well as an extended burial laid out in an east-west direction for which no pit was defined. Altogether the bones from eight adults were recovered between the two excavations.

The layout of the SAC burial ground, the types of inhumations represented and body positions in evidence are all fully discussed in Green et al. (1989). In that same volume Houghton (1989) and Pietrusewsky (1989) describe the human remains in detail and compare them to other Oceanic populations, and to three other individual burials from the Eastern Lapita region. In this respect the Reber-Rakival SAC locality represents the only recorded example of a burial ground among the many excavations in Lapita sites. The skeletal materials exhibit some Polynesian affinities and also possess features that one associates with some populations in Island Melanesia.

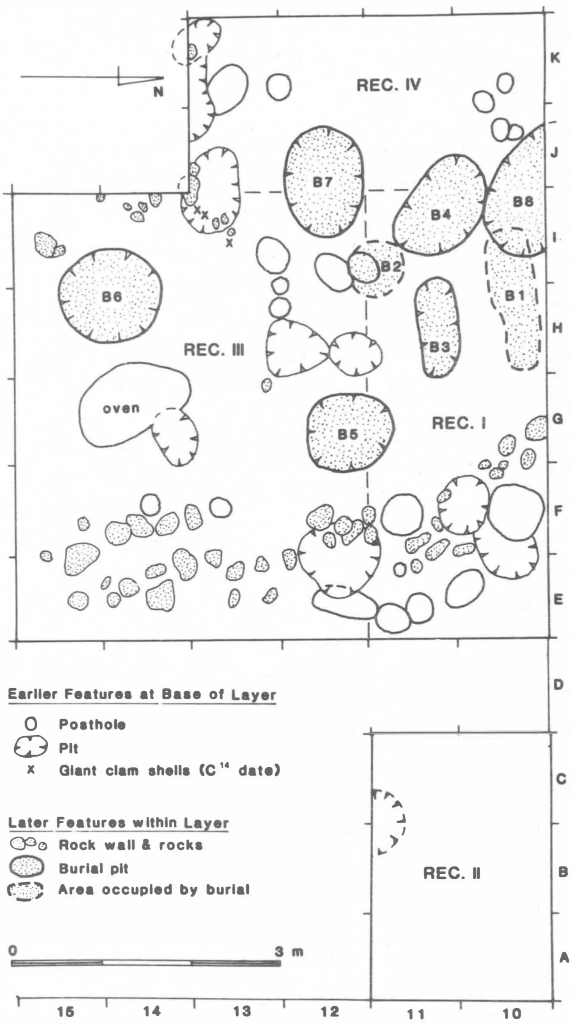

Figure 2 *Plan of features at the base and within Zone C2 at the SAC locality of the Reber-Rakival Lapita site on Watom Island.*

From the above evidence it appears probable that after a short initial domestic occupation of the SAC locality represented by the Zone C2 layer, it became for a time a burial ground, before again being used for residence during the first part of the interval represented by the Zone C1 layer. This would explain why the quantity of pottery and obsidian is so much less in the Zone C2 layer, than in Zone C1. Thus a minimal interpretation of the activity sequence at the SAC locality over a 1000 year span is: unoccupied beach flat, residential, burial complex, residential, gardening, and abandonment, all before the Rabaul Eruption of 650 AD or after.

CONCLUSION

The pre-Rabaul Eruption assemblages from the Reber-Rakival area of Watom are now seen to be largely of middle to late age among the 1500 year sequence of Lapita sites emerging from the Bismarck Archipelago as a result of the Lapita Homeland Project (Gosden et al. 1989:571). The post 500 BC segment of that sequence is not well represented in most other excavations to date. There are, however, related sites with ceramics of a similar type in the nearby Duke of York Islands that are disturbed and undated (Lilley this volume). These form an important link between the earlier 'classic' Lapita assemblages and the contemporary to later Lossu-Lasigi pottery assemblages of New Ireland (White and Downie 1980; Golson this volume), the Buka and Sohano phase ceramics that are part of a northern Solomon's sequence (Specht 1972), or the relevant post-Lapita portion of the Nissan sequence (Spriggs this volume).

Anson (1983:8-9, 1986:162) has set out this argument most fully and we will try to develop it further here. The important category in decorative technique to be considered is the Watom one of nail impressed/applied relief, common to the late assemblages there and present at least in respect of elaborate applied relief decoration at Lossu and applied strips of clay at Lasigi (Golson this volume) and Pinikindu (Clay 1974). The difficulty comes over the incised versus nail impressed techniques as categories. At Watom there are some sherds in the Lapita style of linear incision and other sherds with linear incision not forming a pattern or in motifs typical of the Lapita style (Anson 1983:51). In addition there are two sherds from Watom which Anson (1983:147) sometimes referred to as punctate/incised, that on analysis proved to be exotic. Finally there are those sherds with incisions that are the result of nail impressions. The correspondences here between Watom sherds and those in other sites noted above are largely either with sherds carrying linear incision that is not the typical Lapita style or with the rare exotic punctate/incised sherds. The parallels among some Watom 'incised' wares would then appear to apply only to Lossu and perhaps Sohano, but not to Lasigi or Pinikindu. The other parallels with these assemblages would seem to be in the several varieties of notched (crenellated) rim forms which occur in association with plain Lapita at Watom, as well as on sherds carrying applied relief and linear incision. Therefore, if one accepts that 'classic' Lapita style decoration in dentate-stamping and linear incision was on its way out by the late end of the Watom sequence, this leaves a pottery assemblage there not far removed from some of those which were either contemporary or follow in New Ireland and perhaps even in the northern Solomons. No major break in ceramic continuity is apparent; only slow change with time.

The recovery of suitable in situ data from the SAC and SDI localities in the Reber-Rakival area during the field portion of the Lapita Homeland Project in 1985 and their subsequent analysis have allowed us to achieve several objectives. The first is to provide a reasonably coherent and adequately dated chronological framework for a set of assemblages from the area. This allows us to evaluate and better interpret previous work in various localities there. In addition it adds significant new data from secure stratigraphic contexts on the associated faunal content, ceramic and obsidian assemblages, and a set of structural features including for the first time aspects of a late Lapita burial component. It also provides us with important biological data on people associated with this pottery. Some of these results support what was known previously about the Lapita cultural complex; others add materially to our knowledge of it. The status of Watom as an important middle to late Lapita site in the Bismarck Archipelago, suspected by Anson (1986:162), is now fully demonstrated.

ACKNOWLEDGEMENTS

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INVESTIGATIONS ON BODUNA ISLAND

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Boduna is a small low coral island (less than 2 m above sea level) on the east side of the Willaumez Peninsula some 4 km from Talasea Mission (Fig. 1). The island is roughly 100 m long east-west and a maximum of 60 m wide north-south. This area is tectonically unstable and during the Lapita period the island may have been larger, having subsequently sunk. The island is covered with Lapita pottery and obsidian. Around the edge of the island sand has indurated into a beach rock, with many finds cemented into it. The site was seen to be one of the few Lapita sites in the region of Talasea which might not be severely disturbed by human or geological action and was thus considered worth investigating because of its proximity to the Talasea obsidian sources. Excavations were carried out to determine whether any undisturbed Lapita deposits existed and if the site would warrant further excavation.

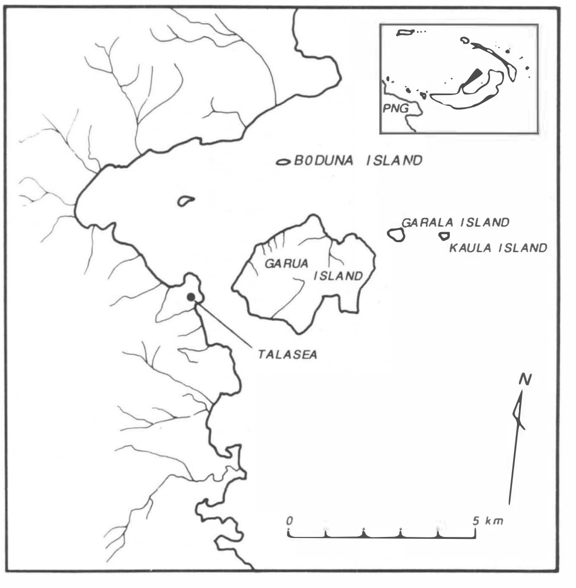

Figure 1 *Location map showing Boduna Island, Talasea on the Willaumez Peninsula and nearby islands.*

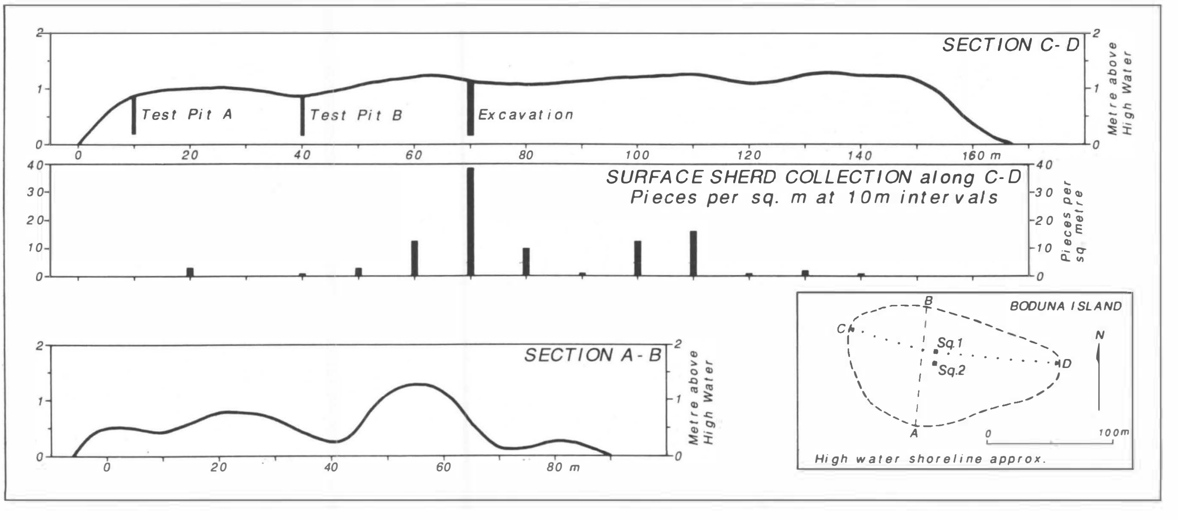

Figure 2 *Boduna Island showing cross-sections and surface sherd densities along the C-D transect.*

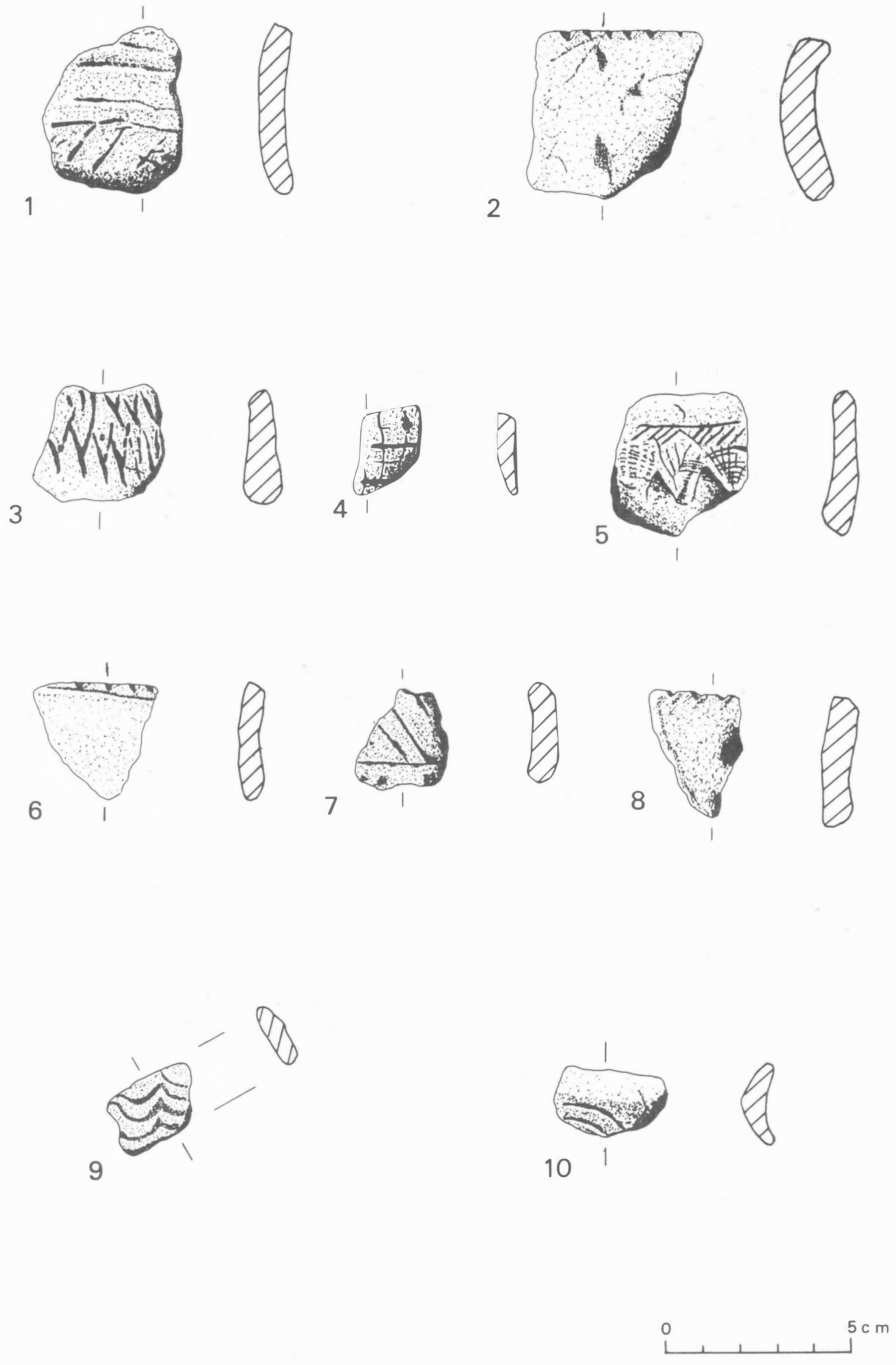

Figure 3 *Boduna pottery: numbers 1, 2, 7 and 9 from Square II Spit 4; numbers 3, 4, 5, 6, 8 and 10 from Square II Spit 7.*

Figure 4 *Boduna pottery: numbers 1, 4, 7 and 10 from Square I Spit 2; numbers 2, 8 and 9 from Square I Spit 1; numbers 3 and 6 from Square II Spit 7; and number 5 from Square II Spit 6.*

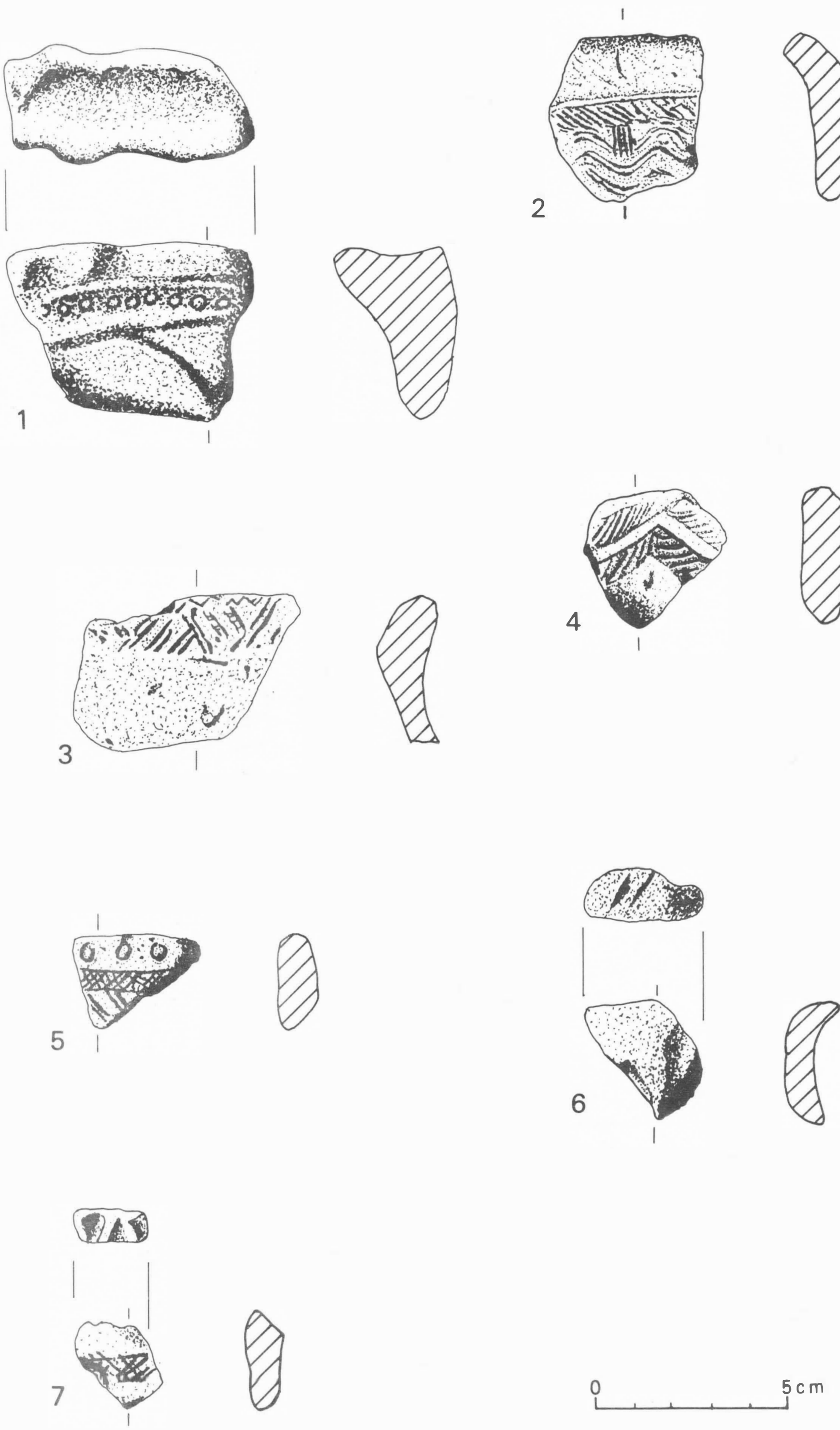

Figure 5 *Boduna pottery: numbers 1, 2, 3, 5 and 9 from Square II Spit 7; numbers 4, 7 and 8 from Square II Spit 4; number 6 from Square II Spit 1.*

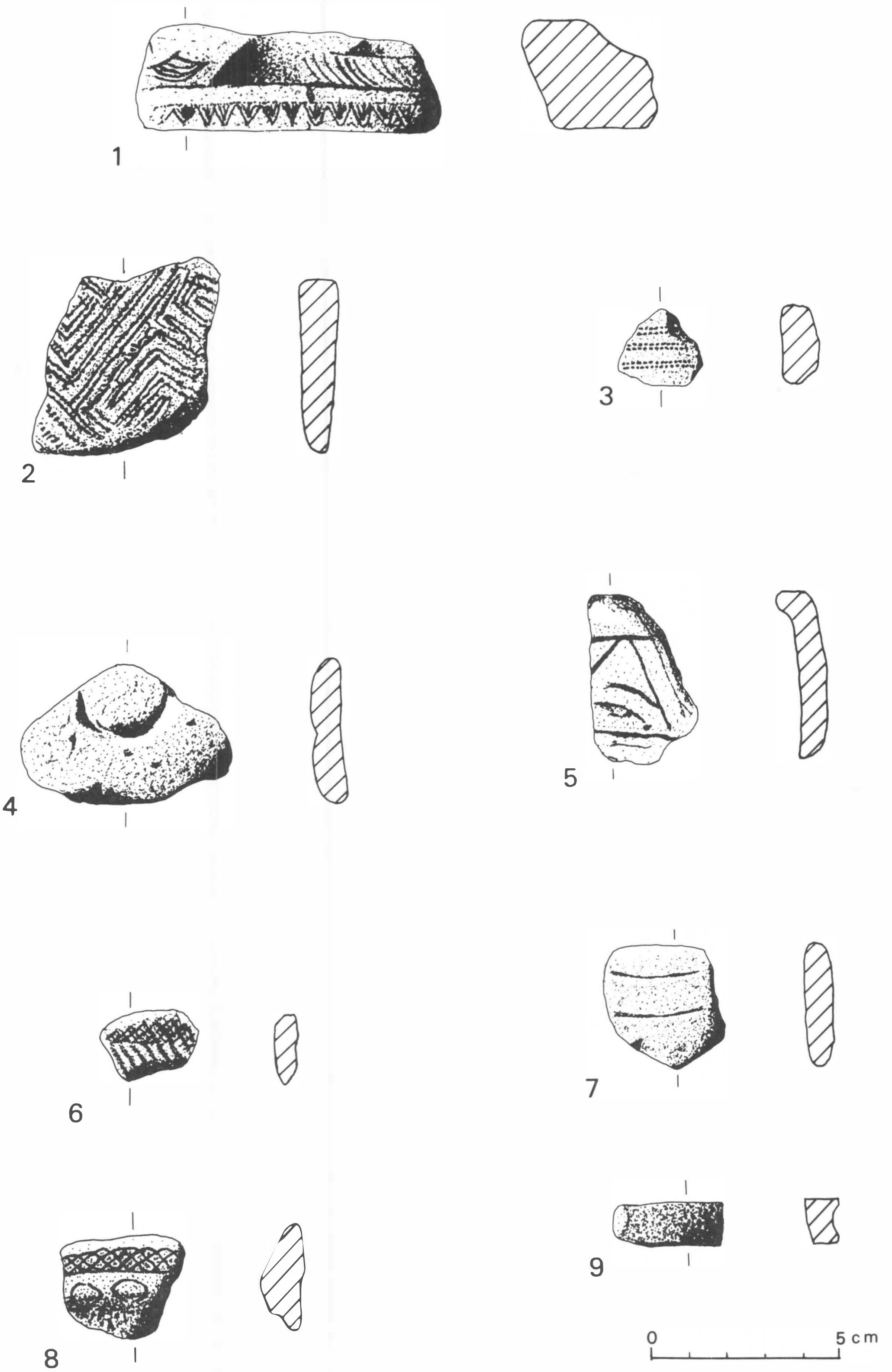

Figure 6 *Boduna pottery: all surface finds.*

INVESTIGATION

The island was surveyed and a contour plan was made across transects running roughly north-south and east-west across the island (Fig. 2). We then collected obsidian and pottery from a 1 m^2 area at 10 m intervals along the east-west transect in order to discover where the densest concentrations of finds were. The two cross-sections we made of the island are shown on Figure 2, together with the numbers of pottery sherds found. Surprisingly, very little obsidian was found on the surface – three pieces at the 70 m point, one piece at 100 m. Shovel holes were dug at A and B (see Fig. 2). At the 10 m point alternating ash, pumice and sandy layers were noted to a depth of 60 cm, beneath which a buried soil displayed some humic development and was somewhat cemented. At the base of the hole geothermal heat was noticeable, measuring some 50°C at 70 cm. At 40 m there was a dark root zone on the surface, below which were reworked volcanic ashes containing very rounded pottery. Below 50 cm there were increasing numbers of shell and coral fingers, less rounded pottery and some evidence of a buried soil. This buried soil seems to continue out to the present edge of the island where it is found underlying the present indurated beach rock. We benefited greatly in these assessments from the expert advice of Wally Johnson (Bureau of Mineral Resources, Canberra) who was with us on Boduna.

Two 1 m^2 squares were then excavated within the area with the densest amounts of finds (see Fig. 2). The two squares were placed roughly 10 m apart on the highest part of the island just west of its centre. These squares revealed a similar series of deposits to those found in the shovel holes, with reworked ashes down to c.70 cm containing heavily abraded pottery. Beneath this layer were deposits with less heavily rolled finds in beach sands.

The pottery found throughout the deposits and from surface collections is of 'Western Lapita' type (Anson 1983), but displays a range of forms and decoration (see Figs 3-6). In the upper level of Square II a tanged obsidian object was found which had been bifacially flaked (Fig. 7). While this was initially thought to be some form of point, Specht, Fullagar and Torrence (Australian Museum, Sydney) have recovered similar pieces from nearby Garua Island and Bitokara Mission, Talasea and identify these as part of an industry composed of tanged artefacts.

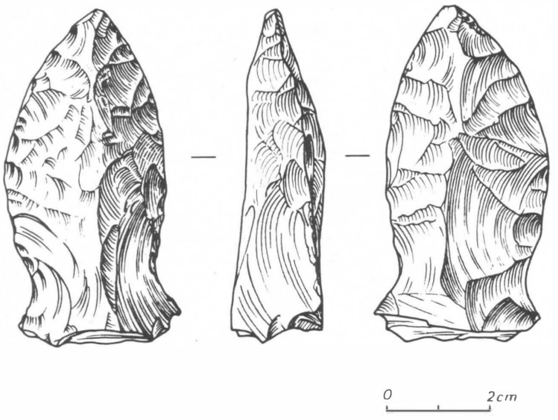

Figure 7 *Bifacially flaked obsidian piece from Boduna Square II Spit 1.*

Three dates were obtained, all from Square I and these are given in Table 1. The discrepancies in these dates suggest that although they are all of the Lapita period, the area excavated may indeed have been subject to disturbance.

Based on these limited investigations our conclusions are that the island is comprised of a coral core, overlain by sand which is now hardened in places. This natural substrate underlies a Lapita period soil. This soil is relatively undisturbed at its base, as shown by the condition of the artefacts, but has been progressively turned over and mixed with a series of volcanic ashes to form an undifferentiated profile. These ashes are similar to those found at the north end of Garua Island and the connection between the two requires further investigation. Given the richness of the finds and the lack of any material obviously post-dating Lapita, the island definitely merits further investigation, despite the disturbance evident in the deposit.

Table 1 *Radiocarbon dates from Boduna. Calibrations are according to Stuiver and Reimer (1986). The marine shell calibrations assume Delta R = 0.*

Square	Location	Material	Laboratory number	C^{14} date years BP	Calibrated date BP (1 sd)
I	60-70cm	shell	ANU-5071	2050±90	1713(1602)1509
I	60-70cm	shell	ANU-5072	3090±80	2946(2847)2760
I	50-60cm	shell	ANU-5073	3130±90	3015(2891)2782

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KRESLO: A LAPITA POTTERY SITE IN SOUTHWEST NEW BRITAIN, PAPUA NEW GUINEA

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In 1985 I recorded a site with Lapita and other pottery at a locality named Kreslo on the south coast of West New Britain Province, Papua New Guinea. The finds were unstratified, but the pottery forms and decoration extend our knowledge of the nature and external relationships of the Lapita ceramic series in the Bismarck Archipelago. The site context, furthermore, raises questions of interpretation that bear on the exchange of views between Spriggs (1984) and Green (1985) concerning the location and visibility of Lapita sites in general. Ten Lapita sites are now known on the south coast of New Britain: three around Kandrian, one at Kreslo, and at least six in the Arawe Islands (Gosden 1989; Gosden et al. 1989). A brief consideration of their locations and of some of the other Bismarck sites suggests that general statements about site location must be made with due caution (cf. Lepofsky 1988) and that local geomorphic and tectonic conditions must not be ignored.

SITE DISCOVERY

At the start of the Lapita Homeland Project, I travelled from Kandrian to the Arawe Islands by speedboat (Fig. 1). I called at Wasum village to see John Kamusio, the district representative on the West New Britain (WNB) Provincial Cultural Council, and made observations of the coastline around Wasum. My general impression was that the area did not promise well for finding Lapita sites. The village extends around and westwards from the mouth of the Anu River along an exposed beach flat without a fringing or barrier reef. During the southeast season (the wet season on the south coast of New Britain), heavy swells roll in across the Solomon Sea and break as surf on the exposed beach. Eastwards from the mouth of the Anu, vertical cliffs of uplifted coral limestone rise about 20-30 m above a coral platform for about 1 km. Rounding a point the cliffs retreat inland behind a small embayment with low-

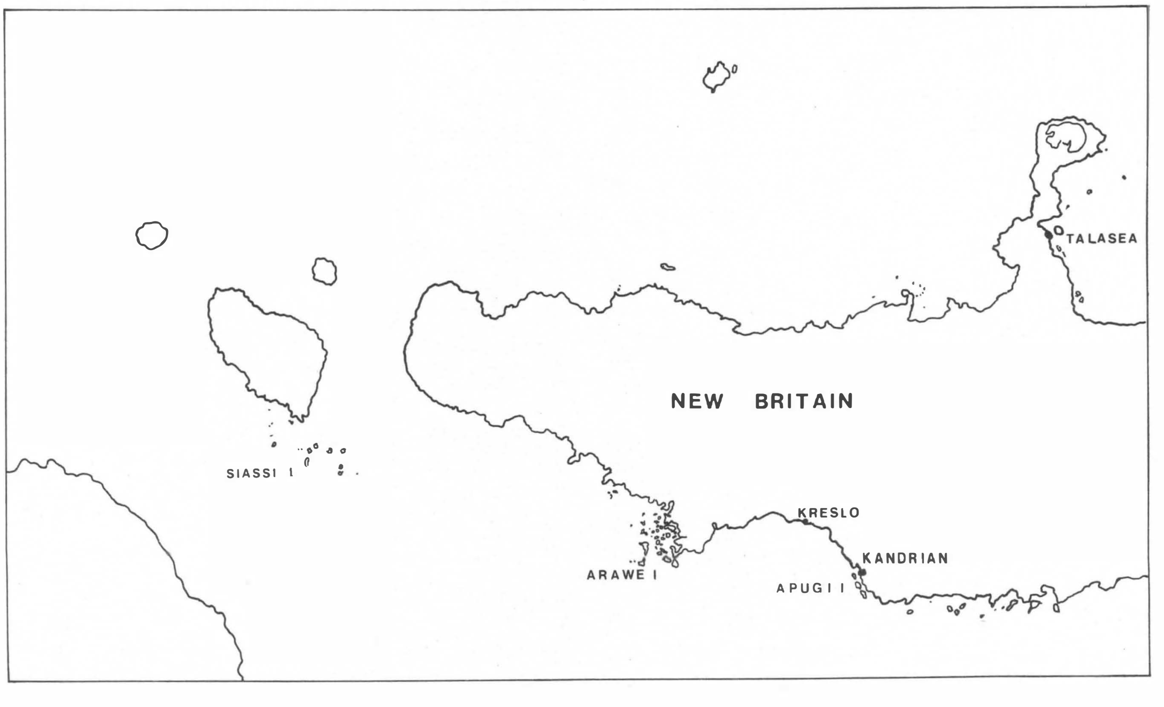

Figure 1 *Location of Kreslo (Site FNT) on the south coast of New Britain, Papua New Guinea.*

lying swampy shores. On the east side of this embayment the cliffs return to the sea, rising above a narrow beach with a fringing reef. There are few, if any, points along this beach suitable for a settlement and the area looks an unlikely context for a Lapita site.

Several weeks later I was working at a Lapita site (Papua New Guinea site register designation FFT) at Iangpun village on Apugi Island, near Kandrian, with John Normu, curator of the museum component of the WNB Provincial Cultural Centre. Makis Agrinyo, a senior man of Iangpun, told us he had found pieces of pottery with Lapita-like designs in the sea just east of Wasum, near Aukur Point, where he was developing a coconut plantation. He claimed that many pieces were larger than those we were finding at Iangpun, and included handled forms. He had searched the beach and cliff behind it, but had not found any pottery on land. John and I agreed to visit the site with some reluctance since Makis had not retained any sherds and with the start of the wet season we had limited time for our own work on Apugi. Makis is familiar with pottery, since his mother used pots imported from the mainland of New Guinea, and he was adamant that the pieces he had found in the sea were different. Furthermore, his description of the area was far more detailed than my casual observations from a speedboat, as would be expected of a man accustomed to using the resources of the area.

SITE DESCRIPTION

We visited the site after first calling at Wasum to see Robert Lawrence, the local member of the Provincial Assembly and now Premier of West New Britain. Makis and Lawrence are kinsmen and claim through their fathers' lines the right to use the waters and reef platform of the site and the land behind it.

The site is about 2 km east of the mouth of the Anu River in an area known as Kreslo (also as Areslo, Akreslo), and is given the code FNT in the Papua New Guinea site register. The cliffs here rise 20-30 m above the sea with short narrow beaches too small to accommodate houses. The only place suitable for a settlement, and then only a small one, is on the eastern side of the embayment, between Kreslo and the mouth of the Anu. In 1985 that area had two houses only. Fringing reefs extend along the base of the cliffs on both sides of the embayment and there are several areas of reef development across its entrance. Between the embayment and Aukur Point, including the Kreslo area, the reef bifurcates parallel to the shore for about 400 m. The outer arm thus creates a lagoon-like passage with its entrance at the mouth of the embayment, and protects the Kreslo shore from the heavy swell of the Solomon Sea.

Site FNT starts about 250 m east of the embayment. The name refers to the waters and reef from high tide to the outer edge of the bifurcated reef. The inner part of the reef along the shore is a platform covered with fine sand and seaweed and is partially exposed at low tide. The site itself appears to be wholly covered by the sea except at times of extremely low water, and is confined to the sand covered platform as far as Makis could determine. Its area is difficult to estimate since the artefact scatter is partially covered by the sand and many items have calcareous encrustations which make it difficult to recognise them under water. As defined in 1985, the site begins about 15 m seawards from the high tide level and extends about 25 m out towards the passage in the reef. Its overall length appears to be about 250 m, extending just beyond the end of the passage in the reef, though artefacts are not evenly distributed over this area.

No pottery or exotic stone was found on the narrow beaches at the cliff base. The beaches are up to 15 m long and 10 m wide, sloping steeply to about 2 m above high tide level at the cliff base. At Kreslo itself the beach is only 4 m wide, with a small sea cave in the cliff base. This cave is about 6 m deep, 3 m wide and up to 2 m high, with two small chambers at its rear. It appears to have been scoured by the sea and the only find on its limestone floor was part of a human long bone. Part of a human vertebra was found in loose sand on the beach below.

About 7 m east of the cave and at 3-4 m above high tide level is a small rockshelter large enough for two or three people and with two recent fireplaces. Small hand holes dug in this shelter revealed 25-35 cm of dry ashy soil with several marine molluscs over limestone bedrock. Another hand hole dug just outside the shelter had damp clayey soil with marine and terrestrial molluscs over limestone. No pottery or exotic stone was found in the shelter, on the beach below, on the irregular cliff face or on the clifftop above the site.

THE FINDS

Pottery

We collected about 500 sherds and sundry items of exotic stone from the reef flat. The sherds were examined on the beach and only those considered likely to yield information about decoration or vessel form were retained (total 184). The remainder were replaced on the reef flat. Many sherds were so weathered that their original surfaces have been worn away, often making it difficult to determine whether or not they were once decorated. A few are less weathered, as though they have been exposed to the weathering processes for a shorter time. Even the Lapita sherds show this variation.

The sherds were brought to Sydney, where they were washed in distilled water to remove sea salts. Calcareous encrustations were removed with dilute hydrochloric acid, and then the sherds were washed again before being consolidated with a solution of Paraloid B70. Several dried out before they could be treated in Sydney and suffered splitting and degeneration, but most remain as complete as when they were found.

The 184 sherds were examined under a 10x hand lens to try to group them according to similarity of clay and non-plastic inclusions. This was of limited success. Between three and seven groups could be proposed. The sherds have volcaniclastic sand fillers that can be variously grouped in hand specimens. At the invitation of W.R. Ambrose, all but the most degenerated sherds were subjected to Xeroradiography in the Department of Prehistory, Research School of Pacific Studies, Australian National University, using a Rank Xerox System 125-6 machine for Xeroradiographic imaging. This system produces positive x-ray images with better definition than conventional x-ray film, and is a useful tool for first-level sorting of pottery sherds. The sherds were processed on twelve sheets of film at an exposure value of 60Kv for 2.5 seconds. The resulting images allow gross composition of the sherds to be seen, facilitating the sorting procedure. Ideally, groups thus defined should be characterised more precisely by elemental or petrological analysis. Because of the taphonomic conditions and post-recovery treatments experienced by the sherds, elemental analyses cannot be applied to them. To date, petrological analysis has not been carried out, so the following groupings must be regarded as tentative.

The sherds were grouped according to the size, shape, distribution and density of inclusions, irrespective of whether they were part of the original clay body or deliberately added as a filler. With only a few exceptions, the inclusions are from volcaniclastic sands; the exceptions are rock fragments probably deriving from various geological contexts and possibly part of the original clay body. Sherd colour is not used as a sorting criterion because of the high degree of variability even within individual sherds. Four sherds were not grouped.

The initial sorting resulted in nine groups, ranging from very fine grained, uniformly textured sherds to coarse grained, irregularly textured sherds with inclusions up to 6 mm long. The groups between these extremes do not form an obvious continuum though some may be variants of others. The sherds may in fact represent only two or three major groups with internal variability. The results are presented here in terms of four paste groups.

Group 1 (75 sherds)
These are closely related, finely and uniformly textured sherds with no inclusions over 1 mm long and mostly less than 0.5 mm long. Weathered surfaces have a fine sandy texture. The fine inclusions are mostly rounded.

Group 2 (7 sherds)
Superficially, this is very similar to Group 1, but with a much denser radiograph image. The inclusions are very fine (under 1 mm long), densely packed and mostly rounded and black.

Group 3 (95 sherds)
This is a highly variable group with unevenly distributed inclusions in matrices of varying densities. The inclusions are both angular and rounded, ranging in size from very fine (under 0.5 mm) to coarse (6 mm long).

Group 4 (3 sherds)
This group could in fact be a variant of Group 3. It has angular to rounded inclusions unevenly distributed and reaching up to 3.5 mm in length.

Seven decorative techniques and three combinations of techniques are recognised on the 84 decorated sherds (Table 1).

a) Relief: one sherd (FNT/7) has two applied knobs (Fig. 2a). Another (FNT/11) has pointed projections formed by cutting away clay (Fig. 2b).
b) Grooved decoration: six sherds have wide deep lines gouged or excised out to produce a bold 'incised' effect. One sherd (FNT/100) is a rim with two perforations and notching on both the inner and outer edges of the flat lip (Fig. 2c). Two other sherds have one or more shallow grooves (FNT/124, FNT/128) and one (FNT/5) is an incurved rim (Figs 2d, 2e, 2f).
c) Incised decoration: 39 sherds have rectilinear incised designs, often as groups of diagonals with horizontal boundary markers. One sherd (FNT/81) is made from an unusual grey-fired clay and has four parallel horizontal lines (Fig. 2g). The other sherds have designs,

Table 1 *Distribution of paste groups by decorative technique and among plain pot stands.*

Decorative Technique	Group 1	Group 2	Group 3	Group 4	Ungrouped	Total
Relief	2					2
Grooved	2		4			6
Incised (includes lip notching)	9	1	25	1	3	39
Nail impressed	1	1	3			5
Incised and nail impressed	2					2
Dentate-stamped	8	1	9		1	19
Dentate and plain tool	1		5			6
Dentate and nail impressed			1			1
Plain tool impressed		1				1
Notched lip only	1		1		1	3
Pot stands	10	1	12			23
Total	36	5	60	1	5	107

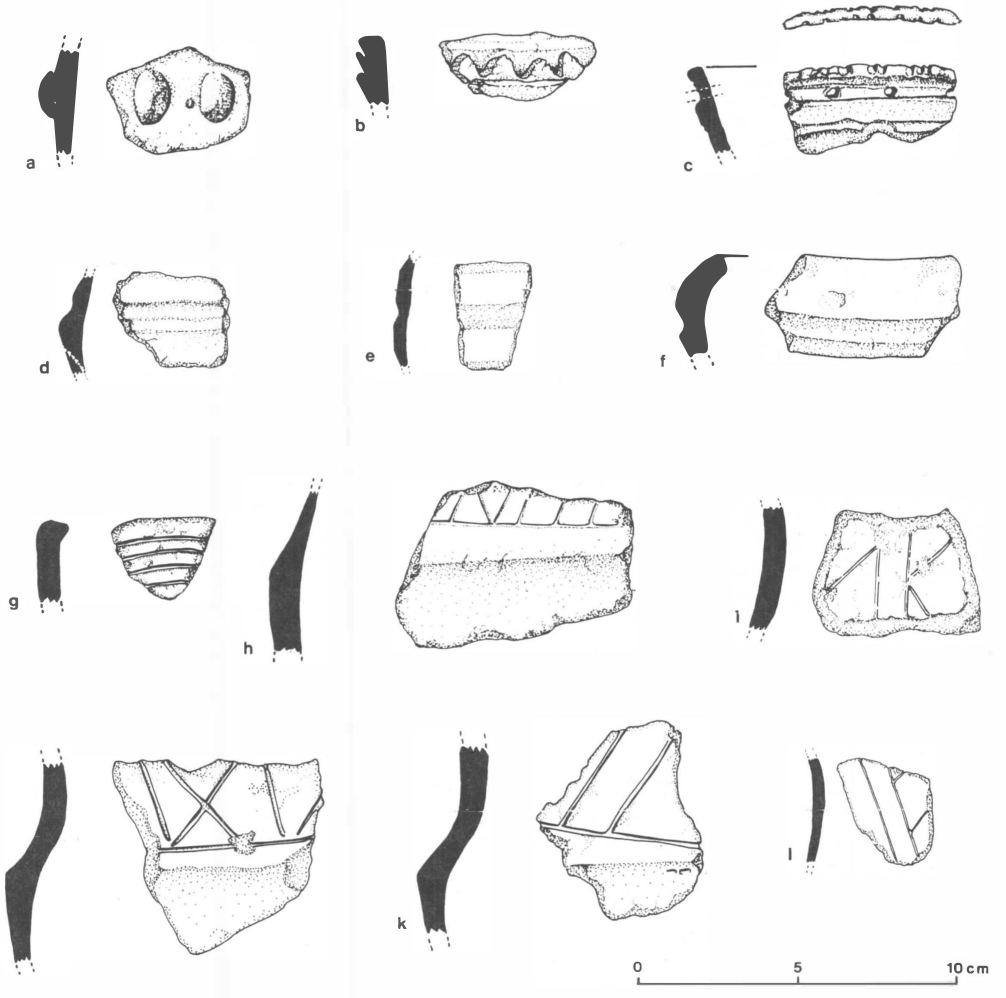

Figure 2 *Relief, grooved and incised sherds from Kreslo.*

where they are sufficiently well preserved to identify them, comparable with incised pottery from Watom Island (cf. Garanger 1971; Golson 1971) and in some Talasea surface collections (Figs 2h to 2l, 3a). Nine range from everted to nearly vertical rims with lip notching (Figs 3b to 3h). One (FNT/75) is an unusual double rim, having an extra piece of clay added to the original rim (Fig. 3i). The junction was poorly bonded and part of the second rim has broken away. Several sherds are from carinated shoulders similar to those found on some dentate-stamped sherds. One thick, inward curving rim (FNT/15) is unlike any of the Talasea or Watom incised sherds (Fig. 4b). Another sherd (FNT/174) has a

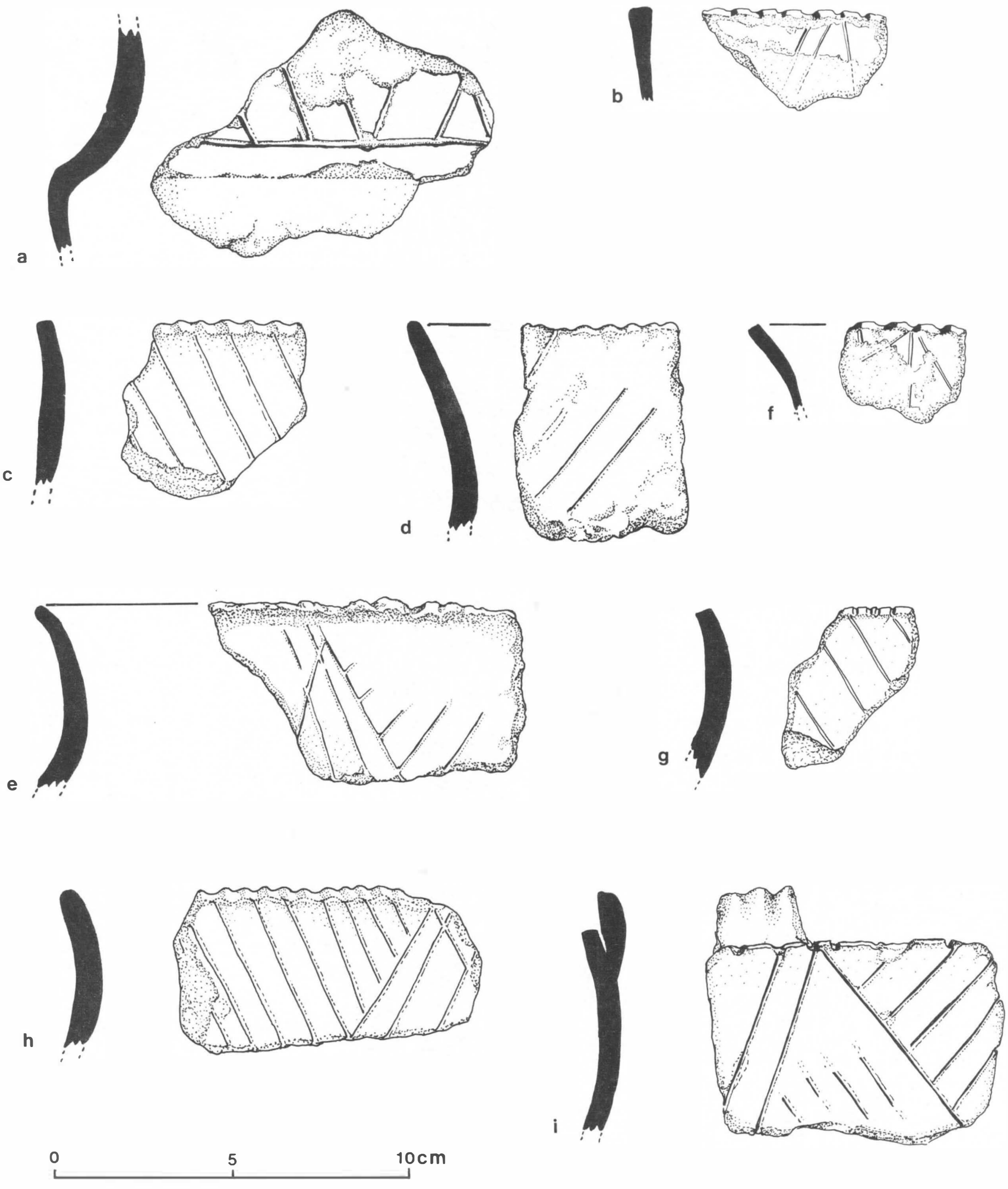

Figure 3 *Incised shoulder and rim sherds from Kreslo.*

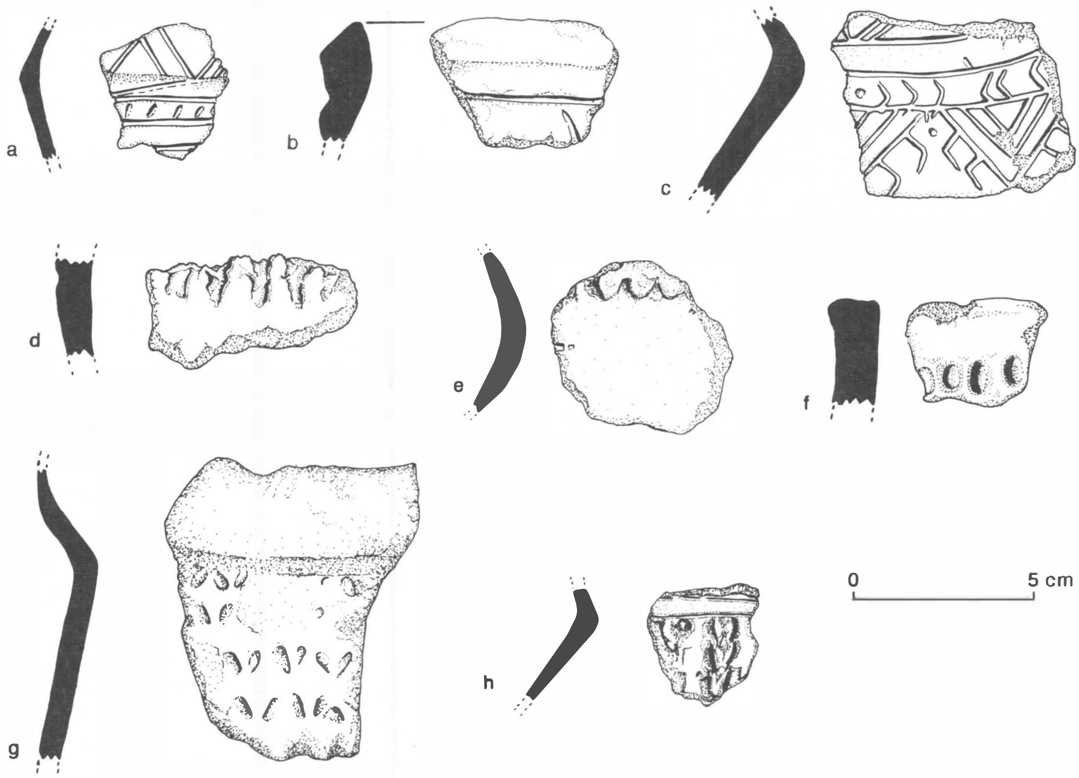

Figure 4 *Incised and fingernail impressed sherds from Kreslo.*

design similar to some found on dentate-stamped sherds (Fig. 4c). This and several other designs are similar to some on Anson's (1983:Table XII) motif list for the Bismarck Archipelago, but cannot be assigned a specific code.

d) Fingernail impressed decoration: five sherds have designs formed by impressing fingernails or a similarly shaped tool, sometimes (FNT/98, FNT/140) pinching the clay so that it appears to be in relief (Figs 4d, 4e). One rim sherd (FNT/29) has a flattened lip and a single row of impressions (Fig. 4f). On one sherd (FNT/16) the impressions are in a panel of horizontal rows (Fig. 4g).

e) Incised and fingernail impressed decoration: two sherds (FNT/142) have a panel of fingernail impressions with two incised lines as a boundary marker (Fig. 4h).

f) Dentate-stamped decoration: 19 sherds have the typical Lapita dentate-stamped designs. They show a range of angled shoulders, some (e.g. FNT/102, FNT/112) with a concave upper body (Figs 5a, 5b). Other angled shoulders (FNT/168, FNT/149, FNT/120) are too small to determine upper body form (Figs 5d, 5e, 5f), though several concave decorated body sherds may be from concave upper bodies (Figs 5g to 5j). The few identifiable rims (FNT/110, FNT/111, FNT/179) have rounded to slightly flattened, sometimes expanded, profiles (Figs 6a, 6b, 6c). One sherd (FNT/105) has a square flange on the shoulder angle (Fig. 6d). Some designs are too weathered to identify their forms, but at least five, possibly eight, of Anson's motifs (Anson 1983:Table XII) are represented. The designs are mostly arranged in bands around the vessel, but some curvilinear forms appear to extend over the vessel body (e.g. Fig. 5i).

g) Dentate-stamped and plain tool impressed decoration: six sherds have plain tool impressions combined with dentate-stamping. One (FNT/106) has pairs of plain arcs between a dentate-stamped line and diagonal pairs of dentate-stamped lines (Fig. 6e). Another (FNT/107) has overlapping plain arc impressions (Anson's M35) with vertical groups of three dentate-stamped arcs (Fig. 6f). Two of the other four sherds (FNT/1, FNT/

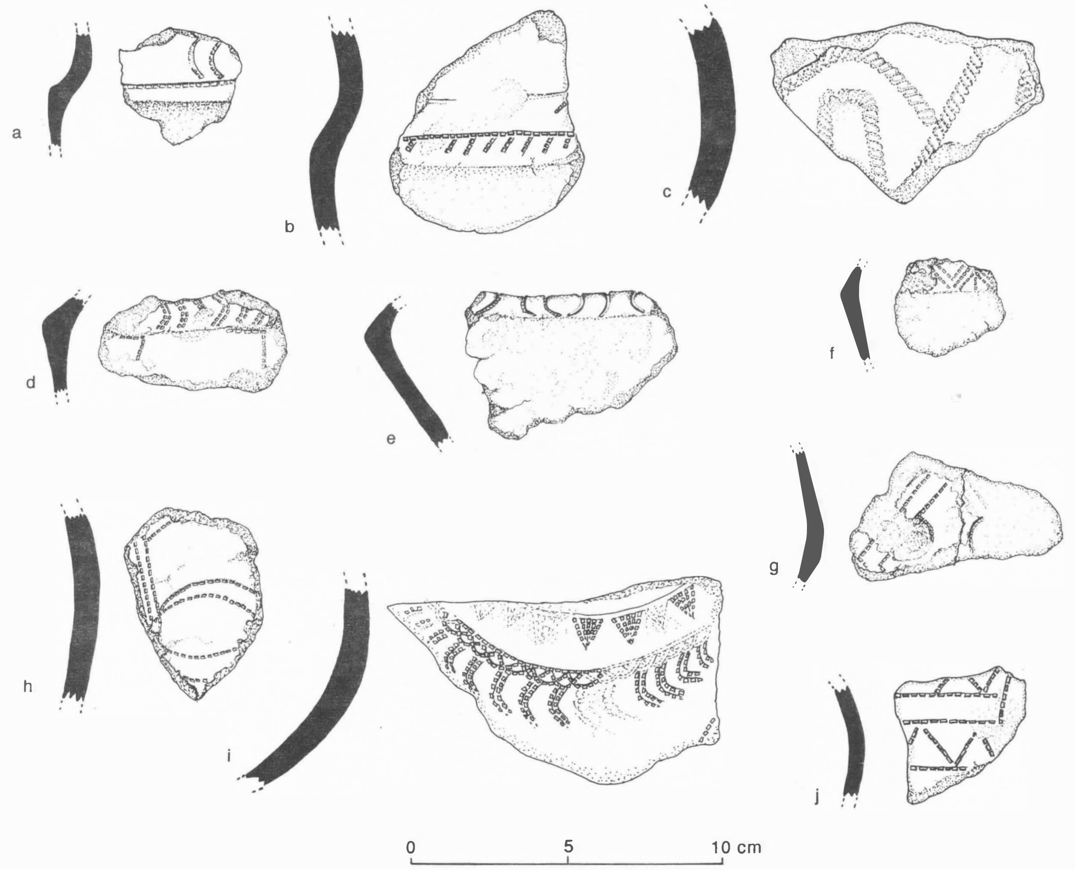

Figure 5 *Dentate-stamped sherds from Kreslo.*

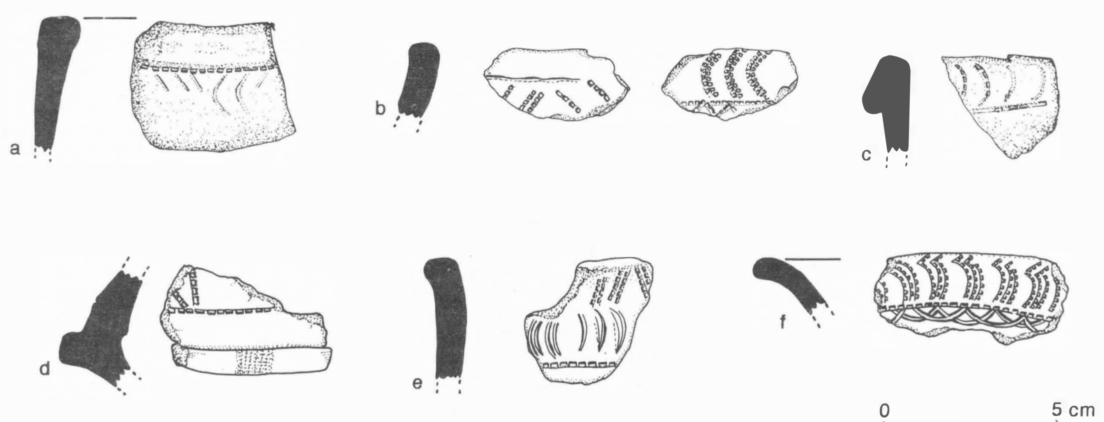

Figure 6 *Dentate-stamped (a-d, f), and dentate-stamped and plain tool impressed (e) sherds from Kreslo.*

2+4) combine dentate-stamped forms with plain circle impressions (Figs 7a, 7b). These have curvilinear designs extending over the upper body and seem to come from very large vessels. FNT/1 cannot be measured with accuracy, but appears to have had an orifice diameter of about 35-45 cm; from the curvature of the upper body, an overall vessel height greater than 60 cm seems to be indicated.

h) Dentate-stamped and fingernail impressed decoration: one sherd (FNT/154) combines dentate-stamped arcs above a panel of fingernail impressions (Fig. 7c). As far as I am aware, this is the first definite association of the two decorative techniques (but see also Green and Anson this volume; Lilley this volume).

i) Lip notching: this creates a crenellated effect and occurs on both plain and incised rims, usually as direct applications on a flat lip (FNT/137, FNT/75) or on rounded to irregular forms (FNT/79) (Figs 7d, 3i, 3h). Only three sherds have lip notching and no other surface modification.

j) Plain tool impressions only: one sherd (FNT/50) seems to have only plain tool im-

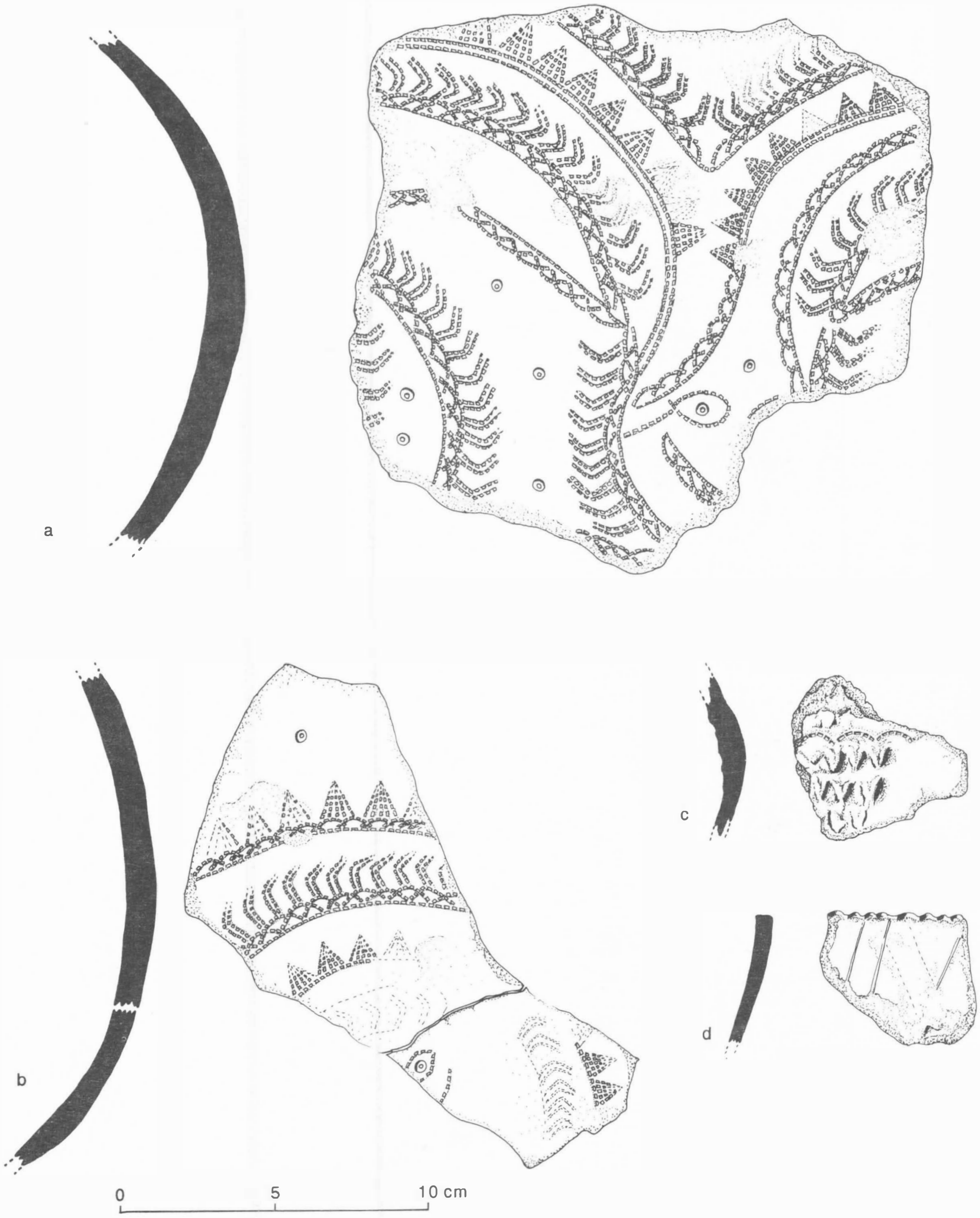

Figure 7 *Dentate-stamped and fingernail impressed (c), and dentate-stamped and plain tool impressed (a and b) sherds, and incised rim with lip notching (d) from Kreslo.*

pressions, but they may be very weathered dentate-stamped impressions.

A range of vessel forms is indicated, although most sherds seem to come from globular vessels with constricted necks and everted rims. The angled shoulders are common, with varying degrees of angularity. The rim sherd with dentate-stamping and plain arc impressions (FNT/106) has a very small diameter (not measurable) suggestive of a spout rather than a vessel orifice (Fig. 6e).

There are at least 23 sherds that stand apart. They are generally thick (between 8-10 mm and 20-21 mm), often with crudely made rims and usually with a coarsely finished inner surface. These I interpret as pot stands or pedestal feet for bowls (Fig. 8). Such pot stands have been reported from the Bismarck Archipelago at the ECA site on Eloaua Island (Egloff 1975), but appear absent from Lapita collections from Kandrian, Talasea and Watom. They may occur in the Arawe Islands to the west of Kreslo (Gosden pers. comm.). Five of the Kreslo specimens carry definite or possible decoration:

FNT/28: incised and fingernail impressed
FNT/29: fingernail impressed (Fig. 4f)
FNT/39: single dentate-stamped line
FNT/41: panel of four dentate-stamped lines
FNT/45: possible dentate-stamped line.

The remaining sherds are plain, indicating aspects of form only. One angled sherd (FNT/10) appears to be from a flat-bottomed bowl (Fig. 8l), a form present in other Bismarck Lapita sites (cf. Anson 1986). One rim sherd has two bands of ribbing at its neck (FNT/101), a feature characteristic of recent Madang pottery on the mainland of New Guinea, an industry that also has angled shoulders (May and Tuckson 1982).

The distribution of the decorative techniques according to paste group (Table 1) shows few immediately obvious differences in the distribution of techniques between the pastes. At this stage, the best that can be said is that the assemblage indicates that the features considered to be Lapita occur in two or more paste groups, possibly reflecting the importation of Lapita pottery from more than one production centre.

External Comparisons of the Pottery

Some comparisons made above clearly indicate the affinity between many Kreslo sherds and Lapita collections from the Bismarck Archipelago. A few, however, currently have no known Lapita relationships. The grooved decoration and the deeply incised lines fall into this group and at this stage I can offer no external comparisons.

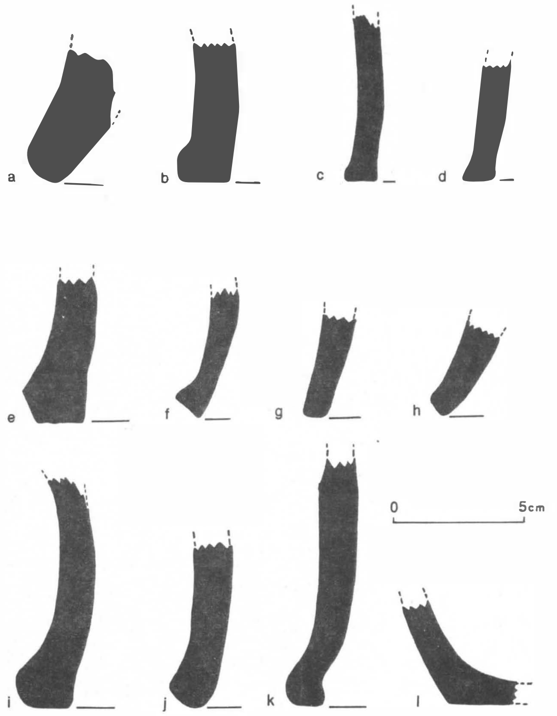

Figure 8 *Rims/bases of potstands from Kreslo.*

The rim form of FNT/15 (Fig. 4b) likewise seems to fall outside the known Lapita range.

The weathered and fragmentary nature of most sherds hinders recognition of the original motifs. Hence, while general comparisons can be made with Anson's motif list, only five designs can be assigned a definite Anson code. In three instances several Anson motifs could be indicated, while others seem to be unrepresented on his list. I have not allocated new number codes to these, to extend Anson's list, since the designs are too imperfect. Some general comparisons can be made, however, with other Lapita sites using Anson's scheme.

The motifs that definitely occur on Anson's list (M35, M133, M207, M435, M496) include two not on his Watom list (M435, M496), but both of these occur in New Caledonia; one is found in Vanuatu and Fiji, and the other in the Santa Cruz Islands (Table 2). One motif is shared with Talasea, in a collection from the FEA site on Boduna Island not included by Anson, and one with Ambitle. Although the sample is small and poorly preserved, in Anson's terms (1983, 1986), the Kreslo sherds would fall into the 'Western Lapita' designation rather than the 'Far Western' one.

Table 2 *Correlations between decorative motifs at Kreslo and other known Lapita localities, following Anson (1983:Table XII).*

Anson Motif Number	M35	M133	M207	M435	M496	M494/497
Watom	x	x	x			x
Ambitle			x			
Santa Cruz		x	x	x		x
Vanuatu	x		x		x	
New Caledonia	x		x	x	x	x
Fiji	x		x		x	

The prevalence of fingernail impressed and incised designs at Kreslo further strengthens this tentative allocation to 'Western Lapita', since both techniques form a secure part of the Watom Lapita assemblages (Specht 1968; Green and Anson 1987), in a late context (cf. Gosden et al. 1989).

As noted above, pot stands are known from Eloaua, and possibly the Arawe Islands, in the Bismarck Archipelago, but are perhaps best known from the Sigatoka area of Fiji (Birks 1973). At this stage, they appear to be absent from other Bismarck Lapita sites.

Other Finds

Fragments of various stone artefacts were found with the pottery at Kreslo, all made from rocks exotic to the area. The rock identifications given below were made by F.L. Sutherland (Australian Museum, Sydney).

a) Obsidian: eleven pieces were retained, ranging in weight from 1.7 g to 15 g. One flake has been bifacially flaked to form a discoid shape. The other pieces are irregular flakes and chunks.

b) Ground stone: three pieces of ground stone artefacts were found. One (FNT/187) is the

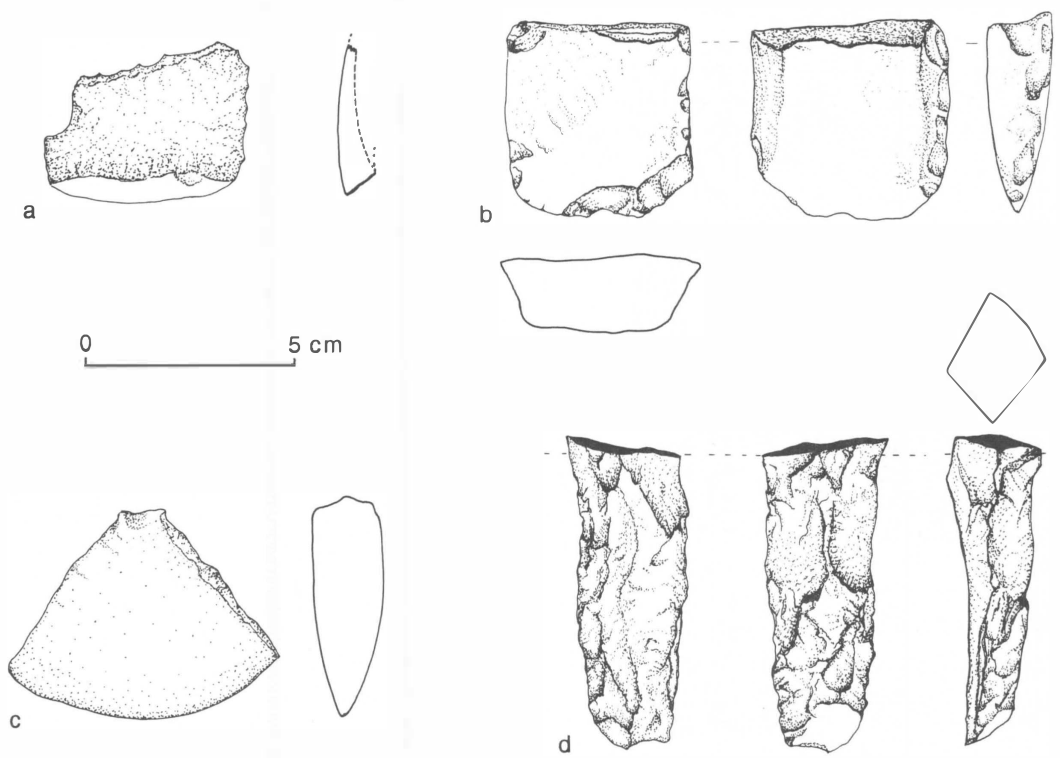

Figure 9 *Ground and flaked stone tools from Kreslo.*

cutting edge of an axe or adze blade made from a grey fine grained basalt or andesite (Fig. 9a). The second (FNT/193) is from an adze blade with trapezoidal cross-section and is also made from a fine grained dark grey basalt or andesite. This item was first flaked to form and then finished by grinding (Fig. 9b). The third item is part of a discoid object, possibly a club head, with a central hour glass perforation (FNT/189). It is made from microdiorite, formed by hammer dressing and grinding. Its original diameter was about 100 mm (Fig. 9c).

c) Flaked stone: one flaked item with a diamond cross-section may be the stem of a large tool (FNT/194), made from a fine grained basalt or andesite (Fig. 9d). Tools with similar stems are known from the FCR/FCS Lapita site at Talasea (Specht 1974a), and a complete specimen was collected in the Kandrian area in 1979 by O. Kaiku (Papua New Guinea National Museum and Art Gallery) and me. The latter item is said to have been found in a garden near Ais village to the east of Kandrian.

THE KRESLO SITE CONTEXT

The site context raises problems of interpretation of its history and use. While the pottery evidence is not conclusive, it does suggest that pottery from several production centres, and possibly of different time periods is represented. We are faced with the problem of explaining why they were found together under water on the reef flat. Was all the pottery originally deposited in the sea, or was there once a dry land site that has been washed away?

It seems improbable that settlements of different periods were located on the narrow, short beaches of Kreslo, depositing all of their rubbish 15 m out to sea. How then do we explain the present site context? We can consider three obvious possibilities:

1) The reef flat on which the cultural materials were found was above sea level at the time the cultural debris was deposited, but subsequent tectonic activity has lowered the land and most of the site matrix has been washed away.
2) The site was originally on land, but sea action such as tsunamis or rising sea level has destroyed the land.
3) The site represents one or more settlements on piles over the reef and was never on dry land.

Tectonic Activity

This proposition seems unlikely, since it is generally accepted that the south coast of New Britain is being uplifted, a process that extends back into the Pleistocene at least (Ryburn 1976). New Britain marks the interface of the Solomon and Bismarck Plates, forming a subduction zone in which the north coast is being thrust down by the clash of the plates, and the south side is being forced upwards. Some of the extensive uplifted Pleistocene reef limestones of the south coast are tilted, but sediments in Misisil Cave, inland from Kandrian, suggest vertical uplift since the late Pleistocene (Pain and Specht 1984/85). Recently reported data (Gosden 1989:54) from the Arawe Islands, just west of Kreslo, indicate that beaches have been forming along this coastline over the last 5000 years, thus supporting the view that parts of the south coast of the island are still in their formative stages. There are no reliable uplift rates for this coast, but the nett effect of uplift would have been to raise archaeological sites above sea level, rather than drown them. There is no evidence that conditions local to Kreslo favoured subsidence, whereas Lapita sites in the Arawe Islands and around Kandrian did not suffer the same fate.

A similar objection can be raised against the suggestion that rising sea level has washed away the site. There is no published evidence for rising sea levels along this coast, and I see no reason why rising sea level after the Lapita occupation and, presumably, after the time of the other pottery at the site, washed away the Kreslo site but left those in the Arawes and at Kandrian untouched.

Tsunamis

More likely is the effect of a massive tidal surge or tsunami. The impact of such natural events on archaeological sites has been little appreciated, though Spennemann's recent discussion (1986/87) of several Tongan sites illustrates the severe effect of Cyclone Isaac in 1982. The caldera collapse of the Ritter Island volcano in 1888 provides a relevant illustration of the effect of a tsunami (Cooke 1981).

Ritter lies at the northern end of the Vitiaz Strait between New Britain and New Guinea. Prior to 1888 Ritter was a massive volcanic cone, probably in eruption at the time that Dampier sailed through the strait in 1700 (Ball 1982). In March, 1888, this cone collapsed and the resulting explosion caused a massive tsunami that devastated coastlines of neighbouring islands up to several hundred kilometres distant from Ritter (von Schleinitz 1888). The tidal wave was about 10 m high when it hit the western end of New Britain (Parkinson 1907:30). This event seems to be preserved in the oral histories of the Arawe Islands and Kandrian.

On Pililo Island in 1985 I was told about a massive tidal wave that reached up to 15 m on the steep slopes of the island's raised limestone core behind the main settlement of Paligmete. At Kandrian in 1979 I heard a similar story at Ais village and on Apugi Island. These stories can be tentatively linked to the Ritter tsunami on genealogical evidence. Informant testimony is remarkably consistent. The event is well remembered at Kandrian for several reasons. Only one person was killed, when he tried to escape the tsunami by climbing a breadfruit tree instead of joining fellow villagers of Ais by moving to higher ground; the man's name is still remembered. Land around a promontory at the mouth of the Ais River was washed away, forming the small island that now stands just offshore from Ais village; and finally, on Apugi Island the tsunami washed away gardens on the northern tip of the island opposite Ganglo Island. This caused a severe food shortage and exposed an old reef flat which is visible today.

In the Siassi Islands the Ritter tsunami had a similar devastating effect (Cooke 1981; Lilley 1986:108, 1986/87:53). The Ritter tsunami similarly could have devastated the Kreslo coastline, and any pottery site that once stood there. Makis, however, could not recall any stories about a tsunami in this area, but this event must have had some impact on Kreslo. Either the Ritter tsunami or an earlier one could have washed away an archaeological site in the area. The small cave behind the site is so clean that it appears to have been scoured out, perhaps by a tsunami or tidal surge.

If one or more tsunamis did destroy a site at Kreslo, we could expect the same to have happened elsewhere along this coast. As far as I am aware, there is little evidence for this, either around Kandrian or in the Arawe Islands (Gosden this volume).

At Kandrian there are three Lapita find localities. That at Aringilo village consists of a single decorated sherd and several small plain ones. Test trenches in the area failed to reveal further sherds or even indicate the presence of a site. At the time of our examination of the area in 1979 we did not inspect the reef flat in front of Aringilo, so the possibility remains that material might be found on it.

The second find locality is on a peninsula running south from Iumiello village between Kandrian and Aringilo. This site (Ngaikwo – code FLX) faces west and is set back about 40 m from the current beach line, below a low saddle. Here Lapita sherds are small and weathered, probably reflecting the use of the area as a garden. Small test pits did not reveal an in situ deposit.

The third site (FFT) is the Rapie area of Iangpun village on Apugi Island. This site stands several metres above the high tide level, facing the mainland. The main deposit is a firm clay soil resting on an old coral and shell rubble beach. The situation of the site, together with the nature of the clay soil may have provided protection against the ravages of tsunamis. At the western end of the village is the present-day cemetery close to the area said to have been devastated by the Ritter tsunami. Several sherds, including a dentate-stamped piece, were found in the cemetery area, but not on the exposed reef flat. Today the cemetery area is low-lying and damp, with many crab burrows. We did not attempt to excavate in the area because of the cemetery. It is not clear whether this area should be regarded as part of the FFT site.

The Aringilo, Ngaikwo and Iangpun cemetery areas would all have been exposed to the Ritter tsunami, though there is not direct evidence to indicate what damage the tsunami caused. In each case, however, material remains on land and has not been totally removed by the wave action.

Pile Settlements

The third proposition is that the Kreslo settlement(s) was built on piles over the reef, a possibility worthy of consideration following the work of Kirch at the ECA site on Eloaua Island (Kirch 1987, 1988a; Kirch et al. this volume), Gosden's waterlogged Apalo site (FOJ) in the Arawe Islands (Gosden 1989; Gosden et al. 1989; Gosden this volume), and reports of reef edge Lapita sites in the North Solomons Province (Spriggs in Gosden et al. 1989; Spriggs this volume; Wickler 1990).

Settlements built out over the reef are widely known in Melanesia (e.g. Motu, Titan, Kove), but they have not been a feature of the south coast of New Britain in recent times. There, settlements seem more opportunistically located as beach flats and level ground permit. Even allowing for some settlement pattern changes ensuing from colonial administration, as has been recorded for other areas such as Buka (Blackwood 1935: 18-9), the pattern seems to predate colonial and mission intervention on the south coast (cf. Chinnery nd (a), nd (b)).

The present coastal settlements have much in common with each other (with the exception of those whose existence can be attributed to government or mission influences). Open sea aspects seem to be avoided, presumably to lessen the impact of heavy seas and storms of the southeast season. Fresh water access is generally widely available, either through beach seepages or from streams and pools at the base of the limestone cliffs. Some have passages through the fringing reefs for easy canoe access. All have access to reefs for collecting marine molluscs and other fauna. Direct access to garden land does not seem to be an immediate requirement, although in all cases suitable land is not far away. Also common is relatively easy access to swampy ground, another economically important environment, though some of this may be of recent origin in view of the uplifting coastline.

The success of recent populations in occupying coastal areas without resorting to pile settlements over the reef suggests that if the Kreslo remains are a result of pile built settlements, then the decision to so locate them may have been cultural rather than environmental. This would not explain why some Lapita sites (or at least their refuse) in this region were located on dry land, though we should remember that in recent times some communities that had pile houses over the reef also had land based settlements (e.g. the Motu). Particularistic explanations for a pile settlement at Kreslo, such as easier access to marine resources or the lack of suitable dry land, appear unconvincing to me.

This last point highlights an expectation that has yet to be justified, namely, that all sites with Lapita pottery should reflect the same or similar past behaviour patterns. One of the differences between the Lapita sites of the Bismarck Archipelago and those found further southeast is that the Bismarck sites are in an area with undeniably pre-Lapita populations. While much has been written about the transport and exchange of goods at the time of Lapita pottery, the discussions all appear to assume that the goods were moving between 'Lapita' communities: that is, between communities that had more in common with each other than simply exchange links. Yet some sites of the Bismarck Archipelago could be viewed as perhaps having been occupied by descendants of the preceding populations. This, of course, assumes that Lapita was introduced by a separate population, another point that has yet to be established. If Lapita was introduced as a result of population movement, we must add to our models possible interactions with preceding populations.

Much has been made over the last decade and more of the possible significance of long distance exchange networks during Lapita times (e.g. Kirch 1988b; Clark and Terrell 1978). Some discussions were premature and based on little firm evidence. We know that obsidian from the Talasea/Mopir source areas was being transported around the Bismarck Archipelago many millennia before the appearance of Lapita pottery (Allen et al. 1989; Gosden and Robertson, this volume). Any exchange patterns introduced or developed by the makers of Lapita pottery are best seen as building onto an existing situation (cf. White and Allen 1980). We have comparatively little firm information about the period preceding the appearance of Lapita pottery in the Bismarck Archipelago, and that mostly from caves and rockshelters; there is little from open sites other than Father's Water in the Manus Province (Kennedy 1983), the FGT site at Yambon inland from Kandrian (Specht et al. 1981) and possibly at Bitokara Mission at Talasea (Specht et al. 1988). We know virtually nothing about settlement patterns prior to Lapita, so I feel relatively free to indulge in some speculation.

The cultural diversity of Melanesia has long been acknowledged, though for some authors this has been equated with linguistic diversity. Schwartz (1963), however, has pointed out that in some areas emphasis on diversity has obscured underlying similarities, and has introduced the idea of 'areal culture' in his discussions about Manus Province populations, a view that I have also suggested could be applied to the Buka-North Bougainville area (Specht 1974b), as Blackwood (1935) herself noted many years earlier. The south coast of New Britain has a similar kind of cultural unity covered by the name 'Arawe'. This name applies to many groups on both the coast and in the hinterland who regard themselves as different from their neighbours at one level, but recognise a commonality of culture at another. In this sense the name does not apply just to the island group west of Kandrian (Berman (1983) discusses the origin and application of the name). Despite linguistic differences, these groups are united in many aspects of material and social culture, extending from just west of the Arawe Islands eastwards almost as far as the border with East New Britain Province (cf. Chinnery nd (a), nd (b)). Linguistic difference, then, does not automatically mean difference in all other aspects of culture. This has implications for the prehistory of the region, particularly for the situation in New Britain at the time Lapita pottery was introduced.

It is easy to fall into the trap of viewing Pacific prehistory in terms of a linguistic model, in which diversification from some kind of ancestral monoculture increased through time.

There is no evidence for that at this stage. In the Bismarck Archipelago it would be strange to assume that change and diversification occurred only after the Lapita period, that the human populations preceding Lapita did not diversify in any way during the 30,000 years or more available to them. We should expect some degree of cultural diversity prior to Lapita.

Since we know that the Talasea and Mopir obsidian sources were being exploited for at least 10-15,000 years before the appearance of Lapita pottery (Allen et al. 1989:554-5), we can postulate that there were probably non-Lapita settlements in those areas when the obsidian was being transported to Lapita sites (cf. White et al. 1978). Leaving aside for the moment other possible views, including a Lapita invasion-and-conquest model, we can expect that the archaeological expression of those settlements need not conform partially or entirely to what we have come to expect from Lapita pottery-producing sites (cf. Lepofsky 1988).

Most of the Talasea Lapita sites are today at or below high tide level, some in contexts reminiscent of the Kreslo situation (Specht 1974a; cf. Specht et al. 1988), though in two cases the archaeological scatters extend on to and above beach level, in two others the sites stand on low coral platforms above high tide level, and in two further cases the sites are on ridge tops. These sites, however, form a minority.

New Britain's position on the subduction zone formed by the collision of the Solomon and Bismarck Plates results in subsidence along the north coast of the island, as well as the high degree of volcanism in the area. There are no published estimates for the rate of subsidence, though it is possibly fast enough for sites with Lapita pottery that were once on dry land now to be at or below high tide level. If this is so, we can ask why only some of the coastal Talasea Lapita sites have suffered this fate.

The Talasea area has undoubtedly experienced considerable geomorphological changes over the last two millennia, resulting from tectonic movement and volcanic activity. Reconstruction of past topographies is unlikely to be a simple operation, but appears to be essential before any understanding of the Lapita sites of the area can be achieved. The possibility cannot be ruled out that, even though local subsidence is taking place, not all coastal Lapita sites were originally on dry land. Some may have been pile settlements built out over reefs or mudflats. Some sites with Lapita pottery may not have been occupied by Lapita producers, but by importers, as Hunt (1989) has recently argued for the Mussau Islands. We cannot dismiss, then, the possibility that the Kreslo site may represent a pile settlement over the reef, while the apparent presence of pottery from several Lapita production centres may indicate that the people using the site in Lapita times were not in fact part of the Lapita production scene. I hesitate to go further and argue that they belonged to a different population, though this possibility cannot be totally ruled out.

At this point we confront a potentially major problem in the discussion of the region's prehistory, namely, our conceptualisation of the nature and relationships of the groups represented by the archaeological evidence, and the manner in which Lapita pottery was introduced to the region.

CONCLUSIONS

The Kreslo site offers no possibilities for excavation or for exploring radiocarbon dating and stratigraphic relationships of the various kinds of pottery recovered from the reef. It does lead our attention, however, to issues about aspects of Lapita site locations and their significance. The papers by Spriggs (1984) and Green (1985) opened discussion on some of these issues, particularly the difficulties of locating sites on large islands of the western Pacific which have undergone extensive geomorphological changes since the Lapita period. New data from the Lapita Homeland Project require us to go beyond Spriggs and Green.

When discussing site location models we must distinguish clearly between models designed to assist us in finding sites from those which purport to describe locality selection by the former site inhabitants (e.g. Frimigacci 1980; Green 1979; Lepofsky 1988). The latter have some utility for predicting likely areas of Lapita site occurrence, but are not necessarily adequate in their present form for use as a field guide in a wide range of present-day geomorphological situations. Both Spriggs and Green recognise that localised geomorphological phenomena may confound a generalised regional model, and the Kreslo site adds a new dimension to their discussion. We need both a revision of our interpretation of the original nature of Lapita site locations and more localised predictive modelling that takes particular geomorphological factors into account. We should also, perhaps, pay more attention to the implications of these and other models about Lapita for discussing relationships between Lapita producers and the region's prior populations. Exchange of pottery between the two could lead to a confusing situation that can only be resolved by ensuring that we distinguish between producing/exporting sites and consuming/importing sites. How this will be achieved remains to be addressed (cf. Hunt 1989).

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TOWARDS AN UNDERSTANDING OF THE REGIONAL ARCHAEOLOGICAL RECORD FROM THE ARAWE ISLANDS, WEST NEW BRITAIN, PAPUA NEW GUINEA

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Recently, archaeologists have started to rethink the nature of a regional approach to their data. Previous attempts to tackle regions, particularly those pioneered in the USA since the 1950s, concentrated on sites as the basic unit of analysis and were not centrally concerned with the nature of formation processes operating on a regional scale. The last 10 years have seen a shift towards the analysis of regional artefact distributions as a whole, rather than a concentration solely on the archaeological 'hot spots' defined as sites (Foley 1981). Newer regional approaches have also been constructed around the processes of landscape change as a crucial influence of regional patterns of artefact distributions, with geomorphological evidence also providing insights into human land use patterns (Butzer 1982). These moves follow a recognition that a fuller understanding of the materials on single sites can only be gained within a *regional context*, which includes patterns of land use, the procurement of raw materials and contacts between groups. The idea of regional context closely parallels the notion of the 'space economy' developed recently by geographers to look at the changing configurations of production, exchange and consumption as these are laid out across the landscape, with a particular recent focus being the changes from feudalism to capitalism in Europe (Dunsford and Perrons 1983).

ASSEMBLAGES AND CULTURAL COMPLEXES

The main point made here is that we cannot understand an archaeological record unless we know the processes responsible for its formation. These formation processes operate over long time scales and form a more realistic starting point for interpretation than models currently in use within the Pacific. These models derive from a need to establish sequence and to interpret changes in archaeological evidence in human terms, these terms having either an ethnic connotation, as with the idea of Polynesians, or a sociological meaning, in the case of bigmen and chiefs.

The main point of this paper is that this inferential structure needs a drastic rethink, both in terms of the basic units into which we divide variation in the archaeological record and the meaning we assign to these units. It seems to me dangerous to assume that the archaeological record can be translated directly into categories derived from short time scale studies of society. A more realistic starting point is that the archaeological record is a different record of human action to those we recognise from our personal experience and from studies in other disciplines and that we should find ways of understanding this difference. Such an understanding can derive from theoretical and methodological areas of investigation. I will concentrate here on the necessity for developing methods for discerning the variability of artefacts on a regional scale and not so much on the meaning which can be attached to that variability.

The main point to be made on this level is that sites, and the spatially and temporally discrete assemblages they contain have been analysed as they best accord with our notions of the world derived from short time scale studies. Assemblages are seen as relatively discrete units and tend to be translated into terms such as 'village', 'activity area' etc; terms which we are familiar with from the present, but which do not adequately describe deposits building up over centuries or millennia. Similarly, the cultural complexes made up of numbers of assemblages, spanning varying periods and created by a variety of formation processes, may not be susceptible to division into discrete units of time and space (Green 1985; Spriggs 1984).

There are two reasons why purely site and assemblage based approaches are inadequate for the definition of variability of the type and number of artefacts and the understanding of that variability in human terms. The first reason has to do with the variation of activities across the landscape. Sites form only nodes in a complex series of activities distributed across the landscape. It is impossible to understand a site removed from this broader context of action, in just the same way as we cannot understand how a single household operates within a present city. Or, to be more precise, there are some activities within a single household which can be analysed in the absence of broader context, but these tend to be of such a detailed nature that to analyse them in isolation will only give us a partial picture of particular ways of life. The disadvantage, then, in analysing sites in isolation is that they only tell us about particular aspects and scales of life in the past, providing a series of details, which need a broader context to give them full significance. The second reason is that the formation processes creating archaeological sites are determined by the special conditions, human and natural, operating in particular parts of the landscape. If we grant for a moment that Lapita assemblages represent a suite of material culture peculiar to a Lapita 'people', these assemblages would still vary considerably in time and space because they are contained within a depositional landscape which formed due to a variety of human and natural processes.

This is stating the obvious; one of the strengths of Pacific archaeology has been the emphasis on geomorphological processes in the creation of the landscapes we see today. However, such a stress on landscape formation has often been divorced from the analysis of the structure of artefactual assemblages and the manner in which taphonomic processes have affected the numbers and types of artefacts found across the landscape. What is needed is a holistic framework encompassing both the artefacts within the landscape as a whole and the deposits containing them, which can be understood in archaeological terms as the record of past human action. The rest of this paper will lay out, in preliminary fashion, the methodology that is being developed within the Arawes project to further our understanding of the structure of the regional archaeological record.

THE ARAWES PROJECT

The Arawes were first surveyed in April and May 1985 by Gosden and Specht, as part of the Lapita Homeland Project, in order to determine whether the region had sufficient potential for further study. The sites that were found during that year did indeed indicate considerable archaeological potential and the bulk of the work has been carried out in two subsequent field seasons, 1986-7 and 1987-8. A total of five months were spent in the field during these visits. The theoretical background to the project has been laid out elsewhere (Gosden 1989) and the data presented briefly here will be published in fuller form shortly (Gosden and Webb in prep.). However, the methods being developed in order to understand the Arawes as a region have not been discussed in any detail so far.

Following this earlier work, wider systematic survey commenced in December 1989 - February 1990 and we are hoping eventually to cover three of the islands (Pililo, Kumbun and Adwe), on which we have excavated and which have reasonable surface visibility (i.e. where areas have been cleared for gardens). Subsequently we will compare these with a similar sized area on the south coast of West New Britain chosen for its surface visibility and variety of landscape forms (discussed below). The main focus of the survey will be on obsidian and the manner in which different elements of the reduction of the obsidian pieces are distributed in these areas; preliminary survey has shown that there is continuous distribution of obsidian of varying density across the landscape. In this overall pattern it will be possible to include information on other artefacts, such as pottery and non-obsidian stone, and these artefacts, although less plentiful, may act as time markers allowing some dating to be applied to the surface material. These patterns of artefact distribution on the present land surface will provide the broadest information on the distribution of past activities across the landscape and the operation of geomorphological processes on a regional scale.

PROBLEMS OF SAMPLING

It is possible to define two populations (using the word in its statistical sense) which are of interest: the total of depositional contexts in an area; and the sum total of artefacts existing in an area. These two populations need sampling in different ways in order to gain samples which inform us about the populations from which they are drawn. Although it would be possible to uncover the total range of depositional contexts through random sampling, it is more efficient to work deductively from an understanding of how the depositional contexts formed and thus to draw a judgemental sample. Judgements as to how the sample should be drawn are based on knowledge of the landscape today and the probable range of formation processes operating on it. The area will be divided initially into broad landscape zones in which different processes may be operating. Both the islands and the adjacent coastal strip of West New Britain appear to have been formed through one process of uplift, which perhaps took place in the late Pleistocene (we hope to obtain dates for this uplift from coral limestone soon). Excluding the beach area, which had its own processes of formation, discussed below, the region (both islands and 'mainland') can be divided into three zones, each with its own taphonomic effects on assemblage structure. The relatively flat hilltops and tops of the islands are subject to erosion in the present and this has probably taken place over the whole period during which they have been cleared. Here we would expect palimpsests of artefacts from different periods as the surface is gradually reduced downwards, the hilltops perhaps having the most artefacts; here we can expect little chronological control. The next unit, the hillslopes, have sediment washed down them continuously and there is little build up either of sediment or of artefacts. The flat areas at the base of the hills receive both sediments and artefacts continuously from higher up. Here there may be few artefacts on the surface, but continual burial may provide artefacts in situ and thus offer some chronological control.

Once the range of depositional contexts has been recognised they can then form the frame within which the population of artefacts can be sampled, with each depositional context (here hilltop, slope and slope bottoms, plus the beaches) forming a stratum from which random samples can be drawn through excavation and survey. Such a stratified random sample allows us to estimate the range of variation in the densities and types of artefacts in each depositional context and to draw estimates of statistical population characteristics on the basis of the samples. We can then compare like with like in different parts of the region (hilltops on the islands with hilltops on West New Britain, for instance). As both densities and types of artefacts on distinct parts of the landscape will vary because of differing taphonomic effects, the comparison between units formed by similar processes will allow us to hold taphonomy constant to some extent and to focus on the real object of interest: the spatial variation in human activities across the landscape in the past.

A further problem to be tackled in any regional analysis is how to define a region which is meaningful in terms of the prehistoric society under study. As Foley (1981:159) points out, ideally, the area investigated should be at least as large as the area within which defined sets of human action took place in the past. In the Arawes' case, as in many other regional approaches in prehistory, we have no means of knowing what size areas were regularly used by local groups, nor do we have major physiographic features, such as large river valleys, which can act as arbitrary units of survey. The means we do have of dividing up the area is to use the bounded nature of islands as units of space. We therefore plan to survey Pililo, Kumbun and Adwe Islands, together with a similarly sized area on the 'mainland'. This will allow us to compare patterns of land use on the smaller islands and the 'mainland' of West New Britain. Such surveys will not give us access to the total sets of activities practised by particular groups in the past; they will, however, allow us to set the sites we have excavated in a broader context and perhaps move towards a definition of meaningful geographical units of analysis at a later date.

Such a framework also allows us to assess the nature and significance of negative evidence. At present, throughout the Bismarcks (and many other parts of the western Pacific) we are lacking two crucial sets of evidence. The first concerns the period immediately prior to Lapita, from which there is only one open site: that of Yambon, inland from Kandrian (Specht et al. 1987), plus a small number of caves and shelters. The second has to do with the Lapita period: it is unknown at present how many areas have sites contemporary with the Lapita period, but which do not contain Lapita assemblages. Some areas, such as Manus and New Ireland clearly demonstrate contemporary non-Lapita settlement and other areas such as Mussau and New Britain so far lack such evidence. In the absence of evidence immediately pre-Lapita we know virtually nothing about the prior social landscape. The situation within the Lapita period is also in doubt, with the material culture found on sites varying within the Bismarck Archipelago. In both cases the question arises 'is absence of evidence, evidence of absence?'. To resolve questions of this sort we need to define and date the full range of depositional contexts in an area, as well as determining which contexts contain artefacts and what types and densities of materials occur in deposits of particular dates. This is obviously an unrealisable ideal, but must be a primary aim if we are ever to use negative evidence of the lack of artefacts in particular contexts in an informed manner. Only when we have some overall picture of the temporal spread of artefacts can we decide whether Lapita deposits are restricted to small islands and what pre-Lapita land use patterns look like.

THE STRUCTURE OF THE EVIDENCE RECOVERED SO FAR IN THE ARAWES

Systematic surface and sub-surface survey started in 1990 and the only comments that can be made on the structure of the evidence recovered so far is on the basis of samples from excavations.

The first comment that can be made on regional structure is that the Lapita sites of the Arawes appear to be more tightly grouped than those of later periods. As can be seen from Figure 1, six or seven sites of Lapita date have been found (it is uncertain at present whether the FNY and FNZ sites on Pililo form one or two sites). Five of the sites, those on Pililo, Adwe, Kumbun and Agussak are inter-visible, with the sites on the three last named islands being within shouting distance of each other. The site on Maklo forms something of an outlier to the distribution as known at present. It is highly likely that we have not yet found the full number of Lapita sites in the area and if and when more are discovered they will change the distribution pattern. Also, more dates need to be obtained before we can say that all the sites are directly contemporary with each other. Even if the pattern so far proves to be from settlements occupied serially, we can still say that people were gravitating towards the same area when they built each new settlement. Our present perception of that pattern throws it into contrast with the more even spread of settlement today or in the late prehistoric period. More recent settlements are not shown in detail on Figure 1 as further information needs to be gathered on their distribution and types. Nevertheless, it is possible to say that if the positioning of sites on the landscape reflects both the social structure of the communities living in those sites and their patterns of land use, then it is likely that both these aspects of life changed drastically between the Lapita and post-Lapita periods. In Lapita we may be studying ways of life, therefore, which are quite different from those of the more recent periods.

This tentative conclusion is reinforced by the evidence from the sites themselves. Four of the five sites excavated so far are in beach locations. These have been excavated with geomorphological considerations in mind and most have been sampled by way of transects running from the beach front to the base of the slope up to the higher part of the island. Thus FOJ was sampled through a series of nine test pits each 1 m^2 (seven of which are included in the statistics below) and a larger area, 2 m x 4 m (labelled Squares O1-04, Z1-Z4), an extension of Test Pit 2. At FNY five test pits were excavated, plus two larger excavations measuring 4 m^2 (Squares M and N) and 3 m x 4 m respectively (the latter has not been included in the analyses below). FNZ has had five test pits excavated in it so far; however, the statistics on these artefacts are not included below.

The sedimentary structures of three of the sites (FOJ, FNY and FNZ) were very similar. All were composed of beach sands near the beach front, with clays near the bases of the inland slopes. In the case of FOJ and FNY Lapita pottery and obsidian were found down to the base of the clays, indicating that the clays started to build up in the Lapita period. The clays are probably the result of clearance on the top of the island, indicating that the island landscapes may have changed with the advent of Lapita sites. On FNY the clays are overlain by midden deposits, probably deriving from late prehistoric settlements on the top of the island and which have basal dates of c.800 BP (see Table 1). The deposition of the clays thus ceased sometime after the Lapita period. Clay deposition may provide evidence of a pattern of land use of the islands starting within the Lapita period and ceasing around 1000 BP, indicating a different, although not necessarily more intensive, use of the islands. The inference of intensive land use based on the clays must be combined with the indications of clustered settlement during the Lapita period. Taken together these strands of evidence may indicate that it was not the landscape as a whole that was being intensively used, but only parts of the landscape. This would then contrast with the later period where patterns of exploitation were more extensive, but possibly not as productive overall.

More tantalising is the evidence from the beach fronts. The dates which follow have been calibrated using the CALIB computer program of Stuiver and Reimer (1986) and BP dates are given at one standard deviation with the intercept in brackets. From one site (FOJ) we have obtained a date from the basal coral of 4793 (4572) 4440 (Beta-29242), which overlaps at two standard deviations with a sample taken from an area of mangrove on West New Britain itself (see Table 1), thus dating the formation of the present beaches to somewhere between 4500 CAL BP and 5000 CAL BP. However, very little build up took place on the beach at FOJ until the Lapita period, with the earliest Lapita deposits sitting in top of basal coral which pre-dates them by 1000 years. There are a number of possible explanations for this phenomenon, but one possibility is

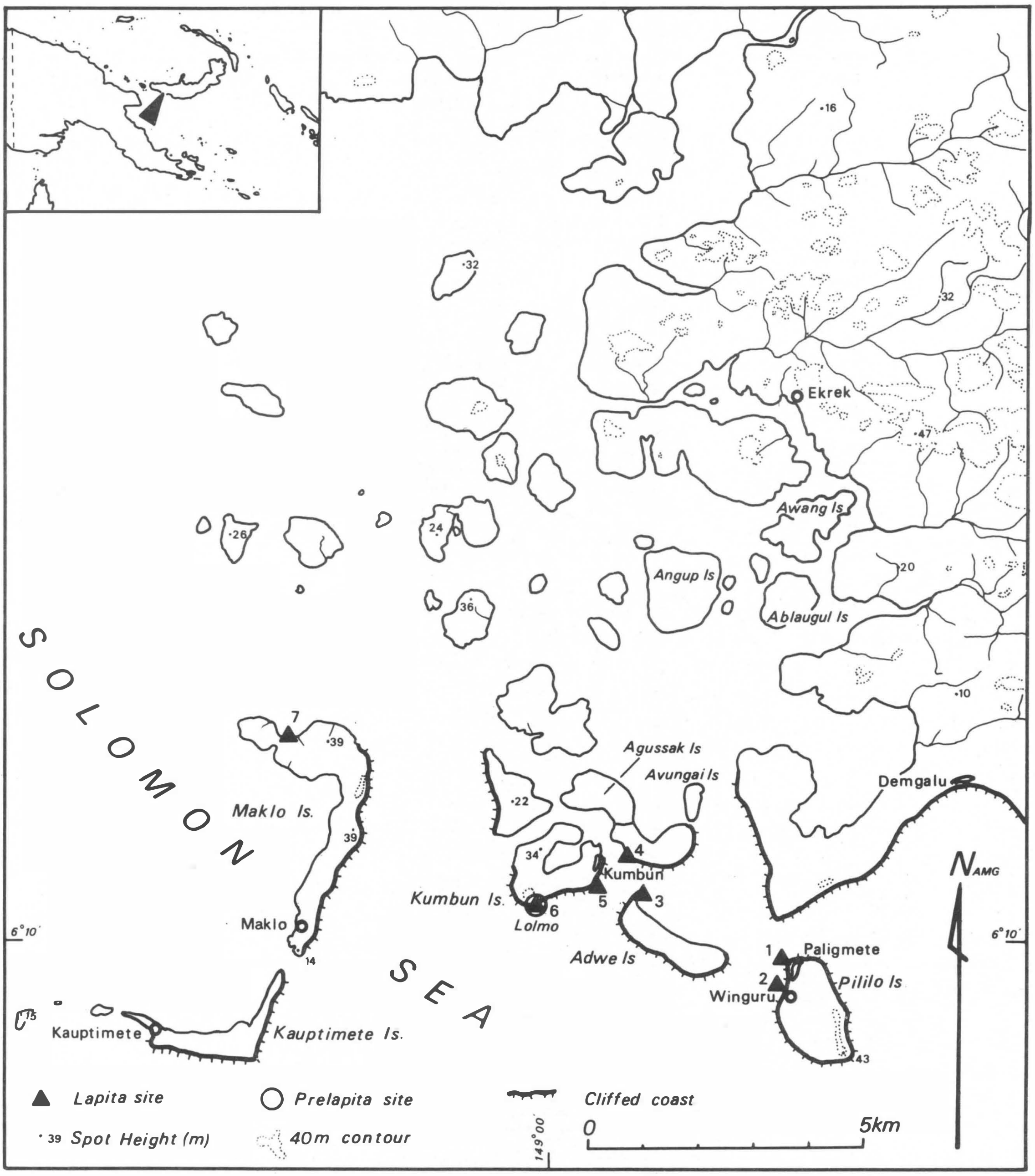

Figure 1 *Map of the Arawe area showing sites. Lapita sites are numbered as follows: 1) FNY - Paligmete, 2) FNZ - Winguru, 3) FOH - Magekur, 4) FOL - Agussak, 5) FOJ - Apalo, 6) FOF - Lolmo (Lapita and pre-Lapita), 7) FOR - Maklo.*

that this beach only started to build up because of human action in the Lapita period, with the result that, at FOJ at least, we are unlikely to find pre-Lapita evidence whether or not it originally existed. A corollary of this conclusion is that all the sediments on sites like FOJ, sands and clays, may be there because of human action and are as much evidence of prehistoric ways of life as the artefacts they contain. Measures such as density of artefacts per m^3 of deposit are thus influenced by human action on two counts: the rate of artefact deposition and the humanly influenced accumulation of sediments. It is worth noting in passing that the clay from the upper slopes of the island may well have washed out to sea before the beach started to build up and therefore *might* have started to erode before we have any evidence of its deposition.

Table 1 *Dates from the Arawe sites. Calibrations are according to Stuiver and Reimer (1986). The marine shell calibrations assume Delta R = 0.*

Site	Location	Material	Laboratory number	C^{14} dates years BP	Calibrated dates (BP - 1sd)
Paligmete	Above clay	Charcoal	ANU 4982	900±140	950(790)690
Paligmete	77 cm below surface	Charcoal	ANU-4985	560±170	680(549)480
Paligmete	Red clay layer	Charcoal	ANU-4989	1110±130	1050(1048)997
Paligmete	Red clay layer	Charcoal	ANU-4990	1150±200	1290(1061)920
Paligmete	1.3 m below surface	Oyster	Beta-27940	2870±70	2725(2688)2516
Paligmete	Base of beach	Tridacna	Beta-27941	3960±70	4060(3947)3848
Magekur	TP 2, Spit 7	Charcoal	Beta-27942	310±80	422(396)317
Magekur	TP 1, Spit 11	Oyster	Beta-27946	3200±70	3099(2983)2891
Apalo	Sq. O3, Spit 13	Tridacna	Beta-29244	2510±80	2292(2148)2055
Apalo	Sq.O3, Spit 17	Tridacna	Beta-29245	2770±50	2589(2467)2366
Apalo	Sq O3, basal coral	Coral	Beta-29242	4430±100	4793(4572)4440
Mangrove		Coral	Beta-29243	4860±120	5299(5210)4969
Lolmo	Sq E, Spit 5	Tridacna	Beta-26643	4320±80	4535(4430)4363
Lolmo	Sq E, Spit 13	Anadara	Beta-26644	3530±70	3473(3401)3343
Lolmo	Sq E, Spit 19	Tridacna	Beta-26645	4930±80	5318(5272)5179
Lolmo	Sq E, Spit 26	Muridae sp.	Beta-26646	4210±90	4414(4311)4166
Lolmo	Sq E, Spit 28	Tridacna	Beta-28223	5670±100	6189(6083)5941

The fifth site excavated so far is in a cave called Lolmo on the upper part of Kumbun Island at its southwestern end. Two areas were sampled through excavation: a 2 m x 1 m area within the cave itself and 1 m^2 outside an old entrance to the cave. Here two depositional phases were encountered. A lower unit composed of orange-brown compacted sediments makes up the bottom metre of deposit and was probably formed through weathering within the cave. This unit dates to the pre-Lapita period. The upper unit, the top 1.7 m, is comprised of a loose dark brown earth with high humic content which may have washed in from outside the cave. This upper unit may provide further evidence of a change in land use. Unfortunately its dating is uncertain at present. The lowest date we have from the upper unit so far comes from Spit 13 and dates to 3473 (3401) 3343 CAL BP (Beta-26644) fitting in nicely with dates from FNY and FOJ. However, from Spit 5 in the same unit comes a date of 4535 (4430) 4363 CAL BP (Beta-26643) prior to the Lapita period. The dates from these two units can be used to separate them into a pre-Lapita phase for the lower unit, and Lapita and after for the upper one. The lack of sequence in the dates may well be due to the nature of the formation processes, which were probably both episodic and disruptive to the previously laid down deposit. Especially in the uncompacted upper unit, clearing of rubbish and the digging of hearths (hearth stones are found throughout both levels) would have mixed the deposits. Because of the distinctive appearances of the upper and lower sediments, though, it is unlikely that much mixing of materials occurred between them.

EVIDENCE FROM ARTEFACTS

The other main strand of evidence to date is that of the types and densities of artefacts found in the excavated deposits. Here I limit comments on the data to densities alone, as information on the types of artefacts (the elements and types of reduction sequences of the obsidian, the fabrics, forms and decorations of the pots, etc.) is still being gathered.

The densities of artefacts from different sites are compared using simple descriptive statistics, which are based around the median, rather than the mean, following exploratory data analysis techniques utilising statistics which are resistant to extreme values (Hartwig and Dearing 1979; Tukey 1977). Density is not a straightforward measure, resulting, as we have discussed, from rates of discard and sediment build up, plus the formation processes which aggregate and disperse finds. Discard is also not open to commonsense interpretation, taking place sometimes at the point of production and use, at other times elsewhere. Much of the day-to-day discard taking place in the Arawes at present is into the sea and is thus not incorporated into archaeological deposits at all. The densities of artefacts in varying locations can then only really be understood when something is also known of the types of artefacts in the deposit. However, density is used here as preliminary means of gaining insight into the nature of variation within the excavated samples recovered from the Arawes, while more detailed data on the stone and pottery are still being gathered.

Table 2 *Descriptive statistics from the Arawes on which the box and whisker plots are based. The table shows weights in grams of pottery and obsidian corrected for density, identifying the total range (the highest and the lowest values) and the interquartile range encompassing the middle 50% of the distribution and thus excluding outlying values. The median and the quartiles are shown on the box and whisker plots (Figs 2-8).*

Site	Pottery					Obsidian				
	Range	Lower quartile	Median	Upper quartile	Inter-quartile range	Range	Lower quartile	Median	Upper quartile	Inter-quartile range
Adwe TP. 1	52-467	96	279	343	247	5-222	33	113	182	149
Adwe TP. 2	0-789	11	46	368	357	0-261	4	27	78	74
Apalo										
TP. 1	0-8700	355	560	4333	3978	0-1200	21	60	258	237
TP. 3	10-2754	133	500	1593	1460	16-300	20	40	147	127
TP. 4	0-9100	8	80	473	465	0-440	35	60	130	95
TP. 5	0-7820	919	1820	2778	1859	20-280	45	77	150	105
TP. 6	0-12633	18	50	2180	2162	0-200	35	65	85	50
TP. 7	80-25751	115	570	3355	3240	10-702	53	80	260	207
O1	0-20800	121	389	1991	1870	0-470	31	67	100	69
O2	0-21128	30	414	2942	2912	0-585	3	14	81	78
O3	0-10240	121	274	1573	1452	0-273	22	36	65	43
O4	0-19908	37	300	1418	1381	0-454	13	32	84	71
Z1	0-22072	64	976	1613	1549	0-445	8	39	75	67
Z2	0-12124	106	360	1432	1326	0-774	10	26	60	50
Z3	0-22674	106	703	1540	1434	1-456	10	53	98	88
Z4	0-14021	60	329	1048	988	0-236	13	44	88	75
Lolmo										
Sq. D	0-374	11	78	166	155	0-180	21	44	94	73
Sq. E	0-365	75	166	225	150	0-365	22	48	129	107
Lolmo 2	25-41		33			12-410	30	70	167	137
Pililo										
M4	0-3542	608	896	1587	979	45-195	81	187	193	112
N4	0-2755	303	1630	2141	1838	22-382	50	93	201	151
M5	13-2000	481	1475	1709	1228	23-259	80	108	123	43
N5	73-2666	462	1269	2080	1618	28-207	90	122	173	83

These data from the Arawes are presented in Table 2. The numbers are weights of pottery and obsidian corrected for density, a simple transformation of the data allowing for greater comparability within and between sites. Each row in the table refers to a 1 m^2 test pit, all of which were dug in 10 cm spits, except on rare occasions where detailed layering was discernible. The measures given refer to the amount of material in each Spit. The data are presented in the form of the statistics most favoured by exploratory data analysts for giving information on the dispersion and shape of distributions – the range, median, upper and lower quartiles (cutting off the bottom and the top 25% of the distribution respectively) and the inter-quartile range. The inter-quartile range is included, as it allows comparisons between the central 50% of the distributions. The extreme upper and lower values are excluded in order to distinguish between central and outlying values for each distribution. Further information on the shape and normality of the distributions is given in the box and whisker plots (Figs 2-8), which show the larger artefact samples in graphical form. These are produced with the Statview package on a Macintosh SE. The lower end of the box represents the lower quartile, the upper end of the box the upper quartile, with the line in the middle showing the position of the median. The box thus encompasses the inter-quartile range which is the middle 50% of the distribution. The dots represent outlying values. Box and whisker plots give an immediate visual impression of the shape of the distributions – with a normal distribution the median sits in the middle of the box, with the percentiles an equal distance either side of it; the outlying values are distributed symmetrically above and below the box.

From these figures it can be seen that most of the distributions are far from normal, being skewed considerably towards lower values. From this it can be concluded that there is a low level of deposition taking place in most deposits for much of the 6000 years covered by the Arawes data. This forms a background noise of deposition and re-incorporation of old material into new deposits. However, certain sites are subject to periods of intense deposition lasting for relatively short periods within the occupation of the area as a whole. This shows up particularly with the weights of pottery on Apalo, where the upper end of the range has very high values. Spits with large amounts of pottery are all found at the base of the site, where pottery and obsidian were laid down practically on the basement coral, perhaps an instance of deposition in a low energy environment, where much material was deposited and little removed by natural processes. That this material was deposited over a relatively short time is shown by the dates from Square O3, where Spit 17, near the base of the deposit, dates to 2589 (2467) 2366 CAL BP (Beta-29245) and Spit 13, above the main concentration of finds, has a date of 2292 (2148) 2055 CAL BP (Beta-29244).

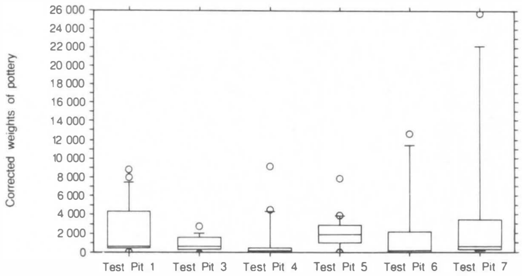

Figure 2 *Box and whisker plots for corrected weights of pottery from Apalo test pits. The outer edges of the box are the 25% and 75% quartiles, the area that encloses the middle 50% of the distribution. The line inside the box is the median. The dots represent outlying values. In this figure, Test Pit 5 is closest to a normal distribution with the median lying roughly in the middle of the box, with one outlying value at 7820. This shows that the deposition of pottery was fairly even over the time the deposits were building up in this part of the site. The rest of the distributions are far from normal with Test Pit 7, in particular, being skewed towards a small number of high values. These come from the waterlogged levels of the Lapita period, where large amounts of pottery were being deposited when compared with the history of the site as a whole. The figures used in these plots are contained in Table 2.*

The amounts of pottery from other excavated samples dating to the Lapita period, for instance those from Adwe and Pililo, do not give such high values at the top of the range. Possible implications are that similar parts of the site have not been sampled in all cases, or that there were different forms of sites and deposition taking place on various islands. It is worth noting that the two pits excavated into clay deposits on Apalo (Test Pits 3 and 5) have ranges more similar to the clay deposits on Pililo than to the rest of the Apalo deposits (Figs 2 and 4). If such variations are to be found in the densities of artefacts within sites, then such internal variation needs to be taken into account when comparing assemblages between sites. How far the densities and types of artefacts found in any area of a site

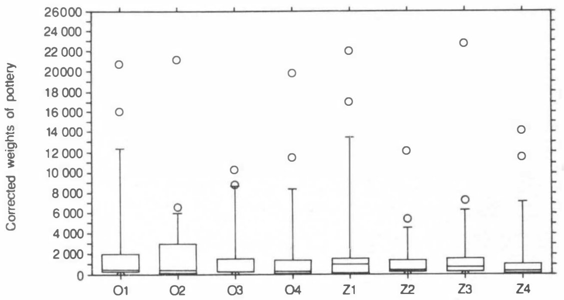

Figure 3 *Box and whisker plots for pottery from Squares O and Z, Apalo.*

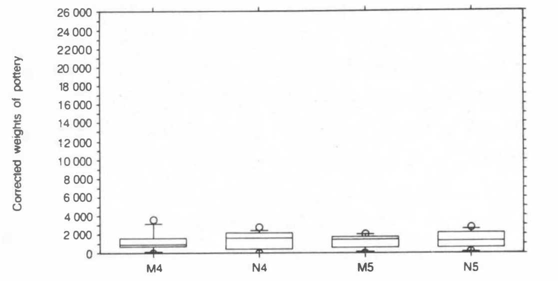

Figure 4 *Box and whisker plots for pottery from Squares M and N, Pililo.*

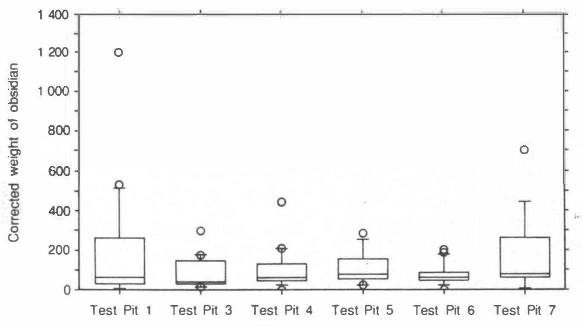

Figure 5 *Box and whisker plots for obsidian from Apalo test pits.*

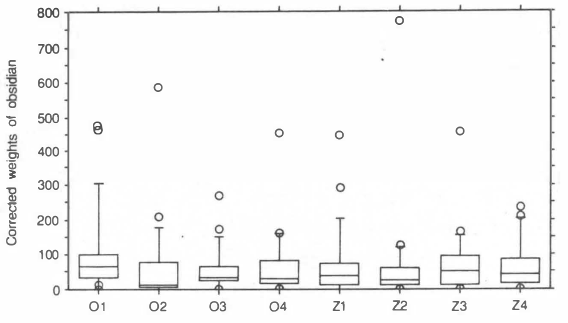

Figure 6 *Box and whisker plots for obsidian from Squares O and Z, Apalo.*

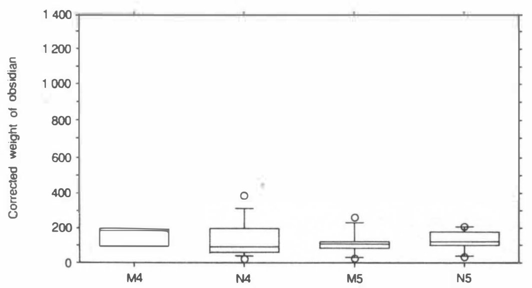

Figure 7 *Box and whisker plots of obsidian from Squares M and N, Pililo.*

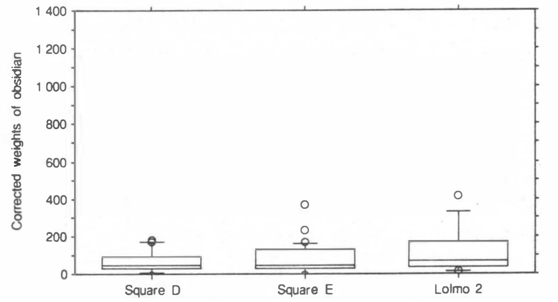

Figure 8 *Box and whisker plots of obsidian from Lolmo cave.*

are representative of the whole site needs to be discussed; all too often the richest areas of sites are taken as standards of comparison and not the range of variation within the site as a whole.

By contrast, the obsidian demonstrates less variability in density throughout the region, although this is due partly to its lower weight, which, of itself, creates a lesser dispersion of values. Again the highest ranges are found on Apalo (Figs 5 and 6), but in the case of obsidian they do not all stem from the Lapita period, with, for example, the extreme upper end of the range in Test Pit 1 deriving from the amount of obsidian found in Spit 1, the result of a relatively recent knapping episode. It must be noted that although both the recent and Lapita period deposits exhibit large amounts of obsidian at Apalo, the deposits were laid down under different conditions, with the Lapita period obsidian probably being discarded into shallow water and the recent material definitely being left in sand. In this instance, density alone is not a sufficient basis for judging similarity; the circumstances of discard also have to be taken into account.

Somewhat less obsidian was deposited in the Pililo (Fig. 7) and Adwe sites during the Lapita period, with the deposits in the cave at Lolmo (Fig. 8) accruing at a fairly high rate throughout its 6000 years of use. On the basis of analyses being run at present it seems likely that the reduction sequences employed at Lolmo were rather different to those employed at the contemporary open site at Apalo, reflecting different usage on these two parts of the landscape; this needs to be investigated further. It will be interesting to discover whether the sites with the widest range of densities also have the widest range of elements of the reduction sequence in them. This will provide an estimate of the usefulness of density alone as a measure of land use.

Comparisons between the densities of pottery and obsidian allow us insights into both changing internal relations within the Arawes and connections with outside regions. Differences in the densities of pottery and obsidian arise from a number of causes. Firstly, obsidian has been used throughout the period of human presence covered by the Arawes deposits, whereas pottery has not. Obsidian has always been imported, mainly or totally from the Talasea and Mopir sources, while the only time pottery is assumed to have been made within the Arawe region was during the Lapita period. In the last 1000 years pottery was imported from other regions, as is still happening today. This change in the location of production would, of itself, cause the decline in pottery weights in the post-Lapita deposits. The situation with obsidian is more complicated, as its deposition takes place under different circumstances. Obsidian is worked using a reductive technology and both the by-products of working and objects of use are deposited. Reduction can also be more or less complete, depending on the amount of obsidian entering the area, the technology employed and the spatial and temporal patterns of use and discard. Pottery generally enters the archaeological record when it is broken and this is most likely to take place at the points where pots are most used. The point of use of the highly decorated Lapita wares is a question of considerable interest. Whether or not this pottery was used within a domestic context or how we might establish what a domestic context would look like are still questions which require considerable thought. Obsidian can be reduced and used on different points of the landscape and its discard will be more closely related to both its production and use. The closer relation between production, use and discard and the fact that obsidian is more widely spread across the landscape than pottery make it more informative concerning patterns of social land use.

The two main sets of artefacts found on sites in the Arawes (and elsewhere in the Bismarcks), pottery and obsidian, had quite different life cycles of production/acquisition, use and discard. To treat them both merely as parts of homogeneous and temporally ordered assemblages, either in terms of their densities of occurrence or their structures, is to over-simplify the processes of their accumulation. As Carr (1987:249) has pointed out, we should see the material in sites (and by extension, across the landscape as a whole) as the result of multiple processes, which will operate in a variety of combinations in different circumstances and need to be teased apart using a variety of analytical and statistical methods. It is only through an understanding of the multiple causes of the archaeological record that we can come to an appreciation of the histories of human uses of regions and the long term continuities and changes in such patterns of human action.

It is worth making a few comments, also, on other types of material recovered from the sites excavated so far, as this is an additional area of variation. Lolmo cave has produced quite a large number of shell artefacts, considering the relatively low density of material as a whole, and some of these date to the pre-Lapita period. Smaller numbers of shell artefacts are found on Apalo, although these do include two half-finished armbands, indicating the production of shell artefacts was taking place on this site. Only one shell artefact of Lapita age has been found on Pililo, and none come from Magekur. The greatest range of non-obsidian stone has been found on Apalo, where quite a variety of grinding and pounding tools have been recovered. Finally, large chert artefacts with tangs and waisting have been recovered from surface collections, but have not so far turned up in stratified contexts, indicating that there are sites of particular types and/or periods we have not yet picked up or that there are activities taking place across the landscape in different periods which are not manifested in sites. No site has the full range of material recovered from the region and the site with the highest densities of both obsidian and pottery (Apalo) does not have the greatest numbers of other types of artefacts. To take any particular site assemblage as a central point of analysis, even with such simple measures as densities and ranges of material would therefore be misleading.

The variety which we have perceived in the types and densities of the material from the Arawes results from a number of causes. Firstly, the areas excavated played different roles on the landscape at different periods, roles which need to be elucidated further. Of particular interest here will be the distinction between cave sites, such as Lolmo and contemporary open sites. There is the possibility that the fairly large sample of cave sites now known from around the Bismarck Archipelago are linked by common processes of use and deposition, which have resulted in series of assemblages not found on open sites; this makes the two sets of sites not directly comparable. Secondly, the excavated samples are small compared with the size of the sites as a whole, most representing less than 1% of the total site area, where this can be estimated. Furthermore, the samples have not been taken randomly, but were part of a judgemental strategy designed to uncover the geomorphological structure of the sites. Only samples which are randomly drawn from a population can be viewed as representative of that population and can be used to derive reliable estimates of population parameters. With judgemental samples it is impossible to be certain what sorts of comparisons can be made between sites and we cannot put any real reliance on such comparisons. Problems of sampling are exacerbated when we take into account the fact that there is uncertainty as to what sort of unit of comparison sites represent. All the sites known from the Arawes so far have different histories, both geomorphological and social; they have been created over varying periods of time and under a number of different conditions. With all these factors in mind, it must be concluded that simple comparisons between site assemblages are not possible as yet, either within, or between, regions. More theoretical and methodological work is needed before such comparisons can be made in a controlled manner.

CONCLUSIONS

The information from the Arawes is being stored and manipulated on a Geographical Information System developed at Melbourne University for analysing information on the type, density and spatial position of material through one set of routines, into which data on archaeological material can be input through dBase. Such systems break down the division between mapping the spatial position of material and its amount and type, as the spatial co-ordinates can be digitised and treated in the same manner as the archaeological data.

However, the use of new technologies alone will not give us an understanding of the archaeological record. What is more pressing at the moment is the need to develop theoretical frameworks outlining the formation processes creating the archaeological record and the periodicities which exist within these. Unfortunately, it will not be possible to make any easy translation between these processes and ethnographic categories such as site types, ethnic groups, production and exchange. On the other hand, our data may contain a wealth of information on long term patterns of land use and the slower moving aspects of social formations which will show up differentially on various parts of the landscape. If we can start to appreciate the landscape as a whole in terms of the differentials in the densities and types of artefacts, plus the rates of build up of the sediments that contain them, then we will have a basis for understanding past social forms derived from the evidence in the archaeological record. It is a failure of nerve to give up the difficult business of archaeological analysis in archaeological terms before we start and to translate our data into terms such as villages, cultural complexes and peoples, which we borrow from other disciplines. In the process of such translation, use of the idea of the assemblage, as a means for parcelling up the data into site type and period on the local level and ethnic affiliations on a broader geographical scale, has played a crucial and constraining role. A move towards an archaeological appreciation of the data will necessitate abandoning the idea of assemblages as *discrete* units of comparison and replacing them with attempts to set the detailed information from sites within a broader regional context.

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A LAPITA SITE AT LAMAU, NEW IRELAND MAINLAND

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[3] The Radiocarbon Dating Laboratory, Research School of Pacific Studies, Australian National University, Canberra, Australia

During the initial Lapita Homeland Project survey of New Ireland, two small pottery sherds 'with possible dentate-stamp marks' were observed at Lamau, on the west coast of New Ireland (Fig. 1); it was also found that 'the site is notable for its lack of obsidian' (Allen et al. 1984:17). If these sherds were confirmed as Lapita, then the Lamau site would be highly significant since it would be the first site with Lapita pottery found on the New Ireland mainland.

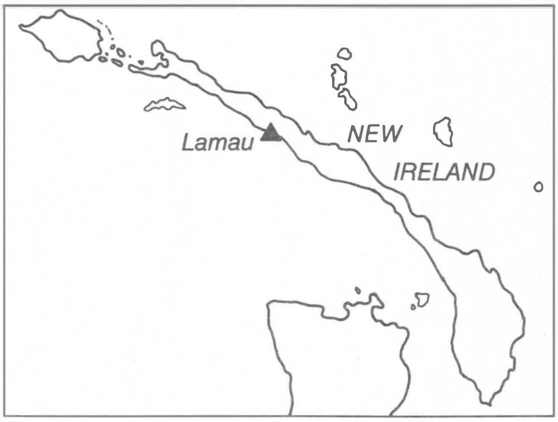

Figure 1 *Location map of New Ireland, showing the village of Lamau.*

The opportunity for Paul Gorecki to revisit Lamau arose in September 1985. This village is set on a small, secluded, deep water bay with easy access to the open sea by a large gap in the fringing reef. Present inhabitants originally lived in the karst country inland and settled the unoccupied bay early this century, possibly following German pacification in the region (Sack and Clark 1979:228, 278, 290). Small mounds (up to 1 m high) were found nearer to the beach while larger ones (up to 2 m high) were noted some 150 m further inland from it. The 1984 finds were made on a low mound located nearest to the beach and it was there that a trench 1 m x 2 m was excavated, with an additional 1 m² opened 30 m further inland from it. The location of these soundings was determined by the 1984 finds, as well as by geomorphological reasoning. Indeed, one hypothesis has it that because of local tectonic activity, the New Ireland east coast is uplifting at the expense of the west coast (including Lamau) which is gradually sinking (see also Löffler 1977:35, 59, 71, 122, 125). Therefore, the possible Lapita site at Lamau, if preserved, could be located very close to the present shoreline.

SOUNDINGS

A 1 m x 2 m trench (TT1) was excavated beside the mound where the 1984 finds were made. Basal coral beach was reached at a depth of 120 cm below surface. Only two stratigraphic units were noted. The upper 28 cm consisted of organically-rich top soil mixed with beach sand. Numerous modern artefacts (e.g. iron, beer bottle caps and nails) were found throughout this deposit. There were also 22 undecorated pottery sherds.

The transition between this layer and the next was very sharp. It was at this transition, but still within the topsoil, that the only obsidian flake was recovered. It was pale grey in colour, and its source is most likely Talasea, New Britain (ANU Cat. No. 2516; Density 2.3451; W. Ambrose pers. comm. 1986). This is the closest source for Lamau, from where the mountains of New Britain can be clearly seen some 130 km to the south. The flake had also evidence of residue derived from cutting the meat of a mammal (rather than its skin), possibly a membrane such as guts (T. Loy pers. comm. 1985).

Between 28 cm and 120 cm below the surface, where the basal consolidated beach was reached, the deposit consisted of pure wet yellow beach sand totally void of organic matter. The only cultural materials encountered were in two clusters. Firstly, at a depth of 50 cm below the surface, one small plain pottery sherd was found together with another which was decorated. The latter was clearly classic Lapita, with characteristic dentate-stamping (Plate 1). Three different stamps were used to produce the decoration.

The second cluster of finds was made at a depth of between 69 cm and 73 cm below the surface. It consisted of many fragments of a single clay pot, decorated with incisions reminiscent of the incised Lapita style (Fig. 2 and Plate 2). All the sherds were lying perfectly flat, next to each other, perhaps suggesting a relative lack of disturbance since they were originally deposited there. Again, no other cultural or organic remains were found associated with these pottery finds. The important exception was dark brown to black material forming a crust on the internal side of sherds that had formed the base of the pot.

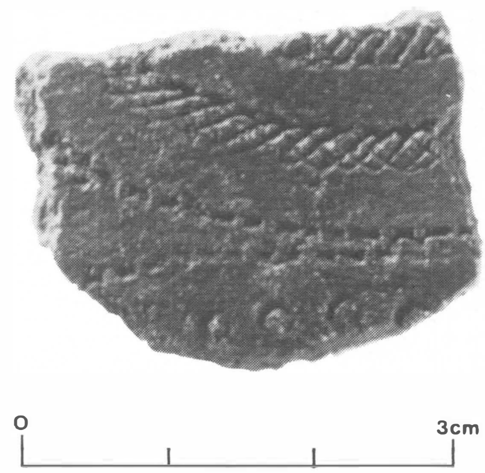

Plate 1 *The only dentate-stamped Lapita sherd found at Lamau. Three stamps were used to produce the decoration.*

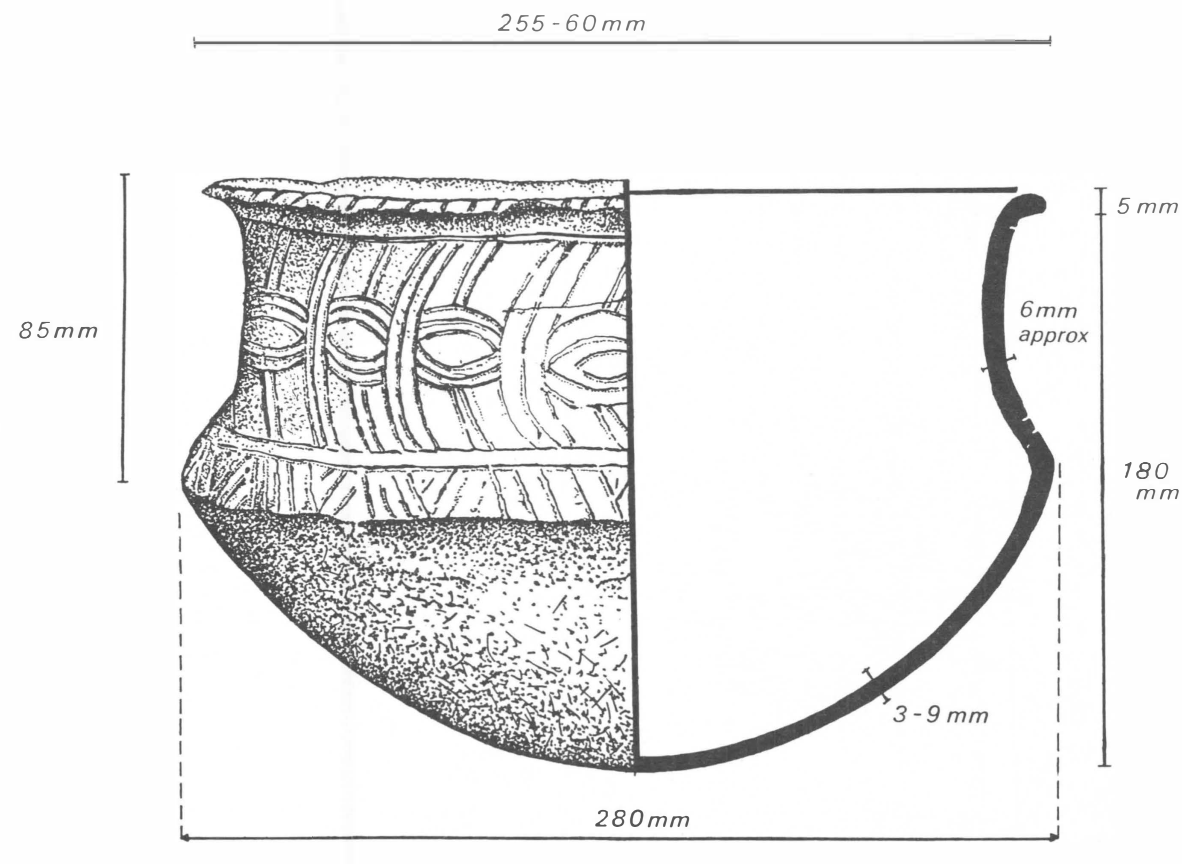

Figure 2 *Profile and dimensions of the Lamau incised Lapita pot.*

A second sounding (TT2; 1 m x 1 m) was excavated some 30 m inland from TT1. It confirmed the overall stratigraphy and paucity of cultural remains of the Lamau beach front. It had an upper mixed topsoil 23 cm thick containing modern artefacts and seven plain pottery sherds, underlaid by sterile yellow sand to a depth of 67 cm below surface where the basal coral beach was reached.

INTERPRETATION

The greatest surprise from these excavations was the apparent contradiction in our understanding of tectonic activities. While it was assumed that the Lamau coast was sinking, the yellow sand deposit, and particularly its pottery content in TT1 clearly indicated a reverse geomorphological process. The sand itself, and the position of the classic and incised Lapita sherds suggested beach formation, i.e. a landform related to coastline progradation possibly from tectonic uplift. Indeed, it had been anticipated that if Lamau was occupied during Lapita times, evidence for this would be closest to a 'drowning' beach. In contrast, it now appears that such Lapita evidence should be sought among the higher mounds some 150 m further inland.

Plate 2 *Reconstruction of the Lamau incised Lapita pot.*

If this interpretation is correct, then the incised pot recovered in TT1 probably broke and was discarded where found, possibly after it had cracked somewhere else while cooking something in it. This was at a time when the find spot was under calm, shallow water: it did not allow for vertical and horizontal displacement of sherds by wave or other action, yet sand did build up rapidly without accumulation of other cultural debris.

The still limited evidence from Lamau confirms the presence of Lapita on the west coast of New Ireland. Even so, the 'classic' phase is so far only represented by one sherd, although the 'incised' phase has one pot! It has been noted that the motif on this pot is very similar to motifs on two sherds from a collection made at the Lapita site of Vatcha in New Caledonia by J. Golson (J. Allen pers. comm. 1986).

The upper horizon, with much European material, suggests a very recent origin for the top. It may have been formed after Lamau was reoccupied by inland people following German pacification.

RADIOCARBON DATING

No sample from the organically rich upper deposit was submitted for radiocarbon dating, since there were so many modern artefacts found throughout it. The only datable material from the lower sandy deposit was the remains attached on some sherds from the incised Lapita pot. Indeed, the inner surface of these sherds was encrusted with powdery, black, mainly inorganic material, that contained a small amount of carbon. This material was carefully scraped from the sherds by John Head to be used for dating, and a portion of this material was set aside for further analysis.

Originally, the material was assumed to be food remains. After determination of the carbon content, it was decided that since the carbon content was so low (6.1%; for the material to be charred food remains a carbon content of about 70% would have been expected), it probably existed as an organo-metal complex. It would probably have been modified with time, and an alternative strategy involving both inorganic and organic analysis was proposed, but this work has not been carried out yet because of time constraints. If the pot was used for boiling up something to use as a pigment, or something similar, then it all makes sense.

The amount of material dated was approximately 2 g, and no pretreatment was carried out. From this material, only 126 mg of carbon could be obtained. Hence the carbon content of the material was approximately 6.1%. Benzene synthesis was carried out and the C^{14} determination was made using a Quantulus Liquid Scintillation Spectrometer. The conventional C^{14} age (Stuiver and Polach 1977) for this 126 mg of carbon is 1680±200 BP (ANU-5518).

POTTERY RESTORATION

The incised Lapita pottery sherds recovered in TT1 were so numerous and obviously from a single vessel, that an attempt to reconstruct it was made by Sue Bassett. Of the 120 sherds found, there was only one which was relatively large (125 mm x 102 mm), while the remainder were considerably smaller (less than 30-50 mm in length) or fragmentary (minimum length 5 mm).

The condition of the ceramic varied. Most areas appeared to be reasonably sound, but some of the small fragments were quite friable. Friability seemed to be inversely related to thickness and directly related to degree of blackness. The most friable and degraded pieces were in the thinnest and blackest areas, where blackening was the result of firing during use rather than production.

Three aims were followed in an attempt to reconstruct the vessel and in order to give an overall impression of completeness in the final reconstruction:

1. To leave all breaks visible.
2. To fill losses in areas which were predominantly original only where the fills would provide necessary structural support.
3. To reconstruct the remainder of the vessel using new materials.

It was felt that the incised decoration should be reproduced on the new areas to provide some visual continuity, but that inpainting of these areas should only blend with the original and not match it exactly. In this way, the areas of original and of reconstruction would remain sufficiently distinctive so as not to mislead the viewer.

It was decided that the only sherds requiring consolidation were several of the very small fragments, and that, if it was possible to locate their positions during restoration, they would probably receive sufficient support from adjoining pieces or fills to present no real problem. If, on the other hand, positions could not be found for them, it was considered preferable not to apply a consolidant and to leave them in their original state (they may therefore, possibly, be used as samples for any future ceramic analyses).

It was possible to join 91 of the 120 sherds into 2 major and 3 minor segments for use in the reconstruction. Although these segments constituted only a little less than half of the original vessel, they did provide a complete profile. Nine of the remaining 29 fragments were also joined (six into three pairs, and a further three together), but it was decided not to incorporate any of these fragments into the reconstructed vessel, as they were very small and predominantly undiagnostic. Their relative positions could not be established with any degree of certainty. Using a synthetic modelling material to form large areas and fill losses where necessary, the vessel was reconstructed, beginning with the rim and neck area and concluding with the base (the vessel remained inverted throughout the reconstruction). Had the pot not been hand built and had it been more regular and symmetrical in shape, reconstruction would have been a simple matter of reproducing the form of the existing pieces. However, the diameter and the profile varied. Some areas of the neck were concave while others were straighter, and the thickness varied considerably (between 9 mm and 3 mm). Therefore, the reconstructed areas had to vary accordingly to accommodate all of these pieces.

Once the reconstruction was completed, the earthenware vessel was found to have a rounded base (not flattened), a prominent shoulder and a tall and slightly concave neck which flared at the rim (Fig. 2 and Plate 2). The rim is rounded on top and has a slightly everted lip. The diameter at the mouth is 255-260 mm and the vessel stands 180 mm high. It is constructed of a coarsely levigated, gritty clay with many large grey and white quartz (to 4 mm) and small sandy inclusions. Sand with high mica content has been used as a temper. Such sand is naturally found at Lamau, which might suggest a local manufacture.

The pot appears to have been built in two pieces, joined at the shoulder. After the outer surface had been smoothed, a sharp instrument was used to incise the top of the shoulder, the neck and the top of the rim with elliptical, curvilinear and rectilinear decoration. The incisions are 0.5-1 mm deep, U-shaped in cross-section and often accompanied by burrs. The vessel was low-fired and has a dark grey core and varigated buff, brown, red and grey interior and exterior surfaces. A black deposit on the interior of the base and some black patches on the exterior surface were indicative of use.

The Lamau pot has been deposited in the National Museum, Port Moresby.

CONCLUSION

The major point emerging from this Lamau exercise is that we have been able to squeeze a lot of information out of very little evidence. We know now that there is a Lapita site at Lamau, that both dentate-stamped and incised pottery decorations are present, that something was boiled in one vessel some 1700 years ago. One obsidian blade from New Britain may have been used to butcher a mammal.

Yet we are unable to say if the site is an extensive one, only a find spot, or represents brief occupation. This is possibly because the location of the excavations was in the wrong place, i.e. too close to the shores of a prograding (uplifting) beach. A lot more data are required on the nature and extent of the Lamau Lapita deposits and on local tectonic processes. Lamau deserves such urgent attention before new models using it are carried too far. Indeed, a scatter of 120 Lapita sherds would undoubtedly lead an archaeologist to state that it is a Lapita site, but when these sherds are 'reduced' down to one vessel, i.e. one artefact, then to label such a location a Lapita site is questionable.

An illustration of the excessive use of this limited Lamau evidence is its use in the 'graph theoretic network models for Lapita exchange' proposed by Hunt (1988:135-55). In these models, Hunt tries to demonstrate that Lamau is a key Lapita site, and that it is a very important one in the prehistoric exchange network of the region. These propositions need to be demonstrated in the field.

Nevertheless, we believe that the Lamau finds are significant and further research there and at nearby bays will yield valuable data on the Lapita phenomenon. Lamau may have been a solid 'mainland' Lapita base, with plenty of spare room for temporal and spatial cultural and economic development, unlike the usual Lapita sites found on restrictively small off-shore islets. Lamau could contribute greatly in our attempts at explaining the cultural processes at work in the Bismarck Archipelago during Lapita times.

ACKNOWLEDGEMENTS

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NISSAN, THE ISLAND IN THE MIDDLE. SUMMARY REPORT ON EXCAVATIONS AT THE NORTH END OF THE SOLOMONS AND THE SOUTH END OF THE BISMARCKS

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Archaeological fieldwork was undertaken on Nissan and Pinipir (Pinipel), atolls of the North Solomons Province in July and August 1985 as part of the Lapita Homeland Project. Further fieldwork on Nissan in June and July 1986 was funded by the North Solomons Provincial Government and the Bougainville Copper Foundation, and in July 1987 by the Australian National University (ANU) Department of Prehistory. A total of just over 12 weeks fieldwork has been undertaken on Nissan, 77 sites have been recorded, and eight of these have been tested by excavation.

BACKGROUND AND INTRODUCTION

Nissan, the larger of the Green Islands (154°12' E., 40°30' S.), is 64 km north of Buka at the northern end of the Solomons chain, 57 km southeast of the Anir (Feni) Islands and 110 km east of New Ireland (Fig. 1). Buka and Ambitle (one of the Anir islands) can be seen from Nissan and the mountains of New Ireland are occasionally visible. As Nissan is a low island, however, it is not directly visible from any of these islands. Nissan is an elliptical raised atoll of probable Pleistocene age, 15 km in maximum length and seven km in maximum width. The land area is nowhere wider than two km. The island is broken by three reef passages on its northwest side. The highest point is over 40 m above sea level and cliffs are common on the ocean side. Total dry land area is 28.5 km^2. Pinipir atoll is of similar formation though much smaller, being 10 km in maximum length. Its dry land area is 4.3 km^2. There are no rivers on the islands and the only water sources are tanks or wells tapping the underlying freshwater lens. Rainfall is about 3000 mm per year. In 1971 there were 2551 people on Nissan, and 543 on Pinipir (Nachman 1978). The combined population now exceeds 4000.

The inhabitants are Melanesians, with physical types varying between Buka and New Ireland peoples. The people live by gardening (yams are the main crop), fishing and the produce of nut trees. The language (Nehan) is an Oceanic Austronesian one and has been assigned by Ross (1988) to his Meso-Melanesian Cluster, which includes languages of New Britain, New Ireland and the Solomons, as one of the North-West Solomonic grouping. Its closest relatives are the Austronesian languages of Buka and North Bougainville.

The islands were sighted by Schouten and Le Maire in 1616, Tasman in 1643, and Carteret in 1767 but no significant contact with Europeans occurred until the late 19th century when blackbirders and traders visited the area (Krauss 1973). As late as 1884 it was reported that nearly all the tools used were of stone and shell and the inhabitants were eager to acquire iron (Rannie 1912:45).

The first commercial coconut plantation was set up in 1885 by an agent of Queen Emma and there are early German ethnographic accounts from the 1890s and 1900s (see especially Krause 1907). Catholic missionary influence began in about 1926 and the island was said to have been fully converted by 1960.

In the late 19th and early 20th centuries Nissan was a key node in the traditional exchange system linking Buka and Bougainville with the Bismarck Archipelago. A range of goods was exchanged (see Table 1) with Nissan's main contribution being pigs, along with large coconut shell water containers, various food items, and one type of clamshell ring. All stone had to be imported to Nissan, which is of coralline formation, but an additional source of volcanic stone apart from exchange was from the roots of trees from the south washed up on the coast.

Apart from early German accounts, the only major ethnographic study of Nissan has been undertaken by Nachman (1978), who also col-

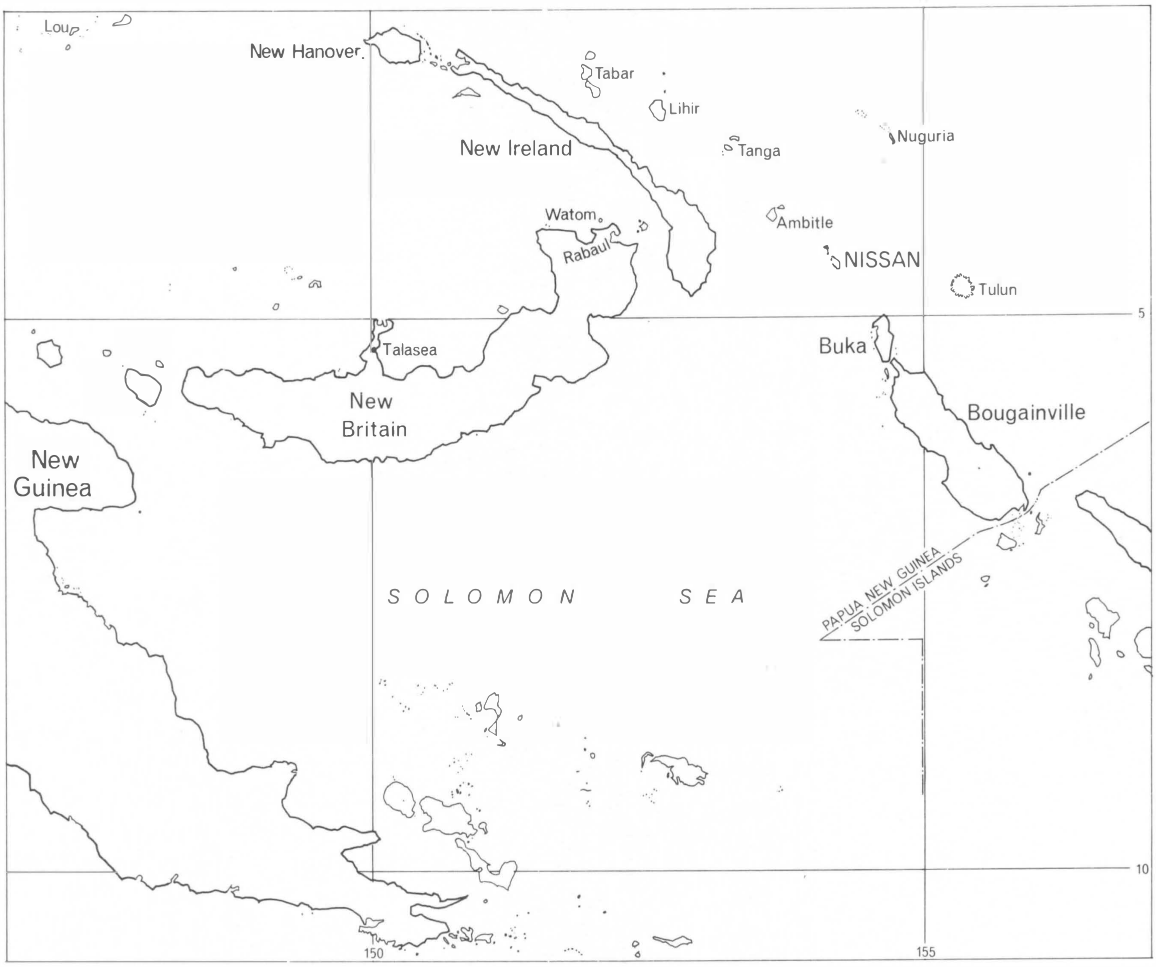

Figure 1 *Location of Nissan Island between the Solomon and Bismarck Archipelagos.*

lected pottery from former village sites in northwest Nissan and Sirot Island. This pottery was analysed by Kaplan (1976). All but one of the potsherds studied by Kaplan were sourced to Buka Island on stylistic grounds and could be fitted in to Specht's (1969) pottery chronology for that island. Nearly all the sherds belonged to the last three periods of Specht's chronology – the Recent (according to Specht's tentative chronology, 350-50 BP), Mararing (750-350 BP), and Malasang (1150-750 BP) periods. Two sherds were attributed to the earlier Hangan period (1350-1150 BP). No sherds of Specht's two earliest phases were found, the Sohano period (approx. 2150-1350 BP) and the Buka period (approx. 2500-2150 BP) which Specht claimed was derived from the Lapita tradition.

PRESENT RESEARCH

Nissan was chosen as one of the areas to be investigated by the Lapita Homeland Project because of its important position linking the Solomons chain with the Bismarck Archipelago and because of its proximity to the early Lapita site on Ambitle Island in the Anir or Feni group, which had been excavated by Ambrose in the early 1970s and subsequently analysed by Anson (1983, 1986). Anson identified sites at Talasea (West New Britain), Emananus and Eloaua Islands in the St Matthias Group, and Ambitle as forming an early 'Far-Western Lapita' style thought to pre-date the Lapita expansion into Island Melanesia and Western Polynesia represented by previously defined 'Western' (which included Watom) and 'Eastern' styles. Given Nissan's circumscribed area and simple geomorphology it was considered that early sites might be located more easily than on the more geologically complex islands to the south, but at the same time give clues as to their prehistory and cultural affinities.

SURVEY

Seventy-seven sites were recorded on Nissan (Fig. 2) and Pinipir. All seven sites recorded on a short visit to Pinipir were open village sites, as

Table 1 *Bismarcks to Buka exchange via Nissan. Compiled from ethnographic sources and archaeological evidence from Recent sites.*

To Tanga
and New Ireland

_«_________________**ANIR**_«_________________________________**NISSAN**_«____**BUKA**

Anir pigment Buka clay pipes Buka clay pots	pigs *Tridacna* rings *(kwamanaulek)* bows and arrows Buka clay pipes Buka clay pots	clay pots, clay pipes war bows and arrows, spears tobacco pigments (especially red ochre from central Bougainville) volcanic stone (stone axes, oven stones, pounders, nutcracking stones etc.) flying fox and porpoise teeth necklaces slit gongs

_________________»_**ANIR**_________________________________»_**NISSAN**____»_**BUKA**

canoes *Tridacna* rings shell disc necklaces obsidian	*Tridacna* rings *(palauer, palboroi)* Anir pigment tobacco volcanic stones *Trochus* armbands obsidian	pigs, fish large coconut water containers Nissan arrows *Tridacna* rings shell disc necklaces

Anir produces:
pigment, tobacco, volcanic stones, possibly *Trochus* armbands.

Nissan produces:
pigs, arrows, shell rings *(kwamanaulek)*, coconut shell water containers, fish, occasionally *Trochus* armbands

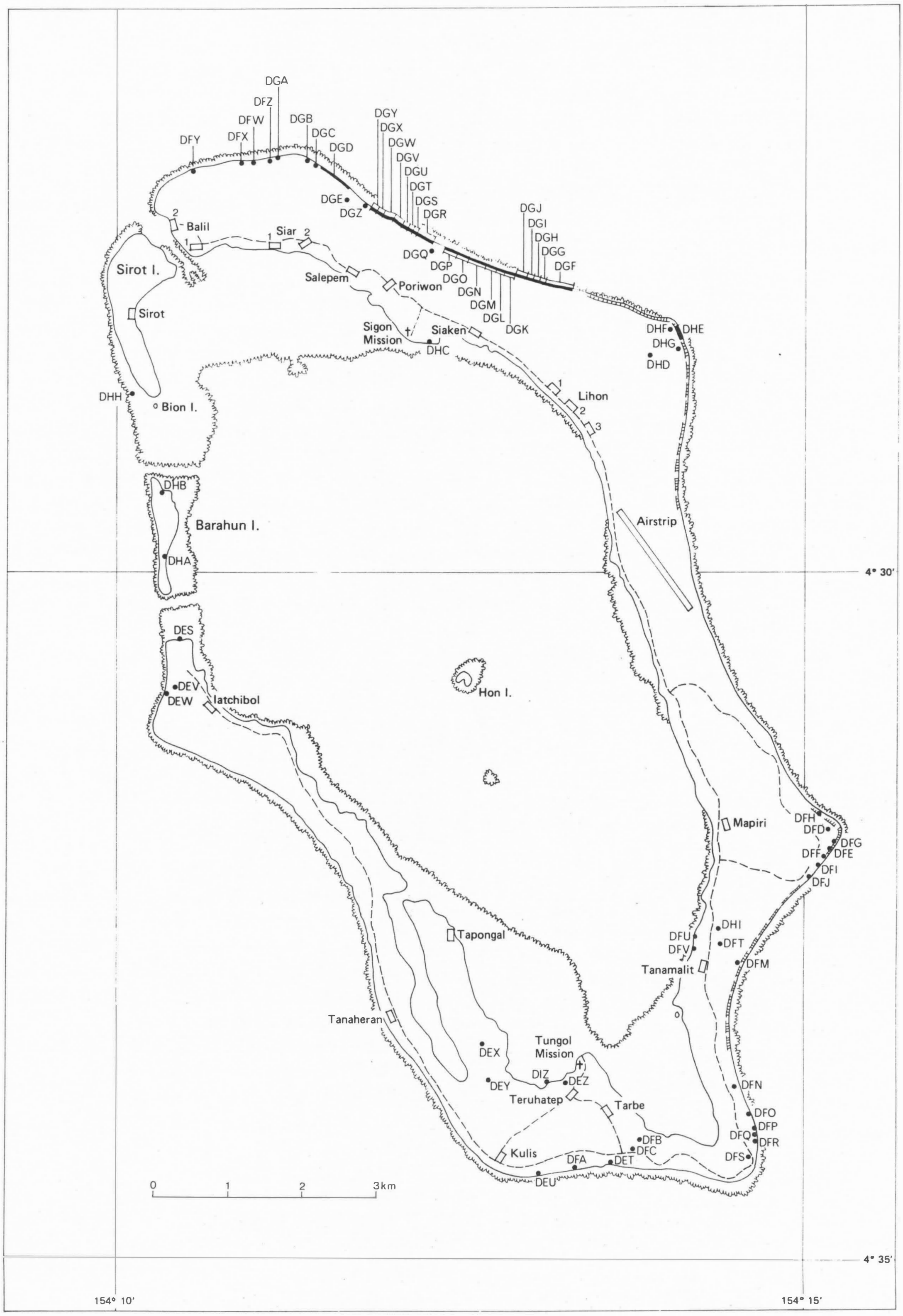

Figure 2 Nissan Island: sites and locations mentioned in the text. Dashed lines represent vehicular roads.

were two recorded on Barahun Island of Nissan atoll. One 'site' or find spot was recorded on Sirot Island, a block of obsidian embedded in the reef off the west coast of the Island (DHH) [1]. Of the remaining 67 sites on Nissan Island, 45 were rockshelter or cave complexes, 20 were open sites, generally former village areas, and the remaining two sites were artefactual scatters on reef flats (Sites DES and DIZ). The majority of caves and shelters on Nissan were visited except for those in the southwest portion of the Island. No shelter sites occur on Sirot and Barahun.

Systematic surveys of former village sites were not undertaken, although coverage was good for the south part of Nissan. Most surface sites from Tanamalit to Lihon were destroyed by the construction of a large American base in World War II, as were some areas of Barahun Island and Iatchibol. Nachman had collected pottery from 105 named localities in the vicinities of Balil, Siar, Salepen, and Poriwon villages and on Sirot Island, and so no resurvey of these areas for village sites was undertaken, although many such sites were noted in passing on Sirot and northwest Nissan. Pottery collected from old village sites confirmed Kaplan's analysis of Nachman's collection in that nearly all the pottery examined was of the Malasang, Mararing and Recent periods. Possible Hangan period sherds were recovered from two village sites and outside one of the shelters. All Pinipir sites contained Malasang to Recent pottery. One of the reef sites (DES) contained pottery styles other than those of Specht's sequence, and also produced a few sherds of Sohano style pottery. This site is further discussed below.

Carved volcanic stone pillars were observed at three sites, two on Nissan (DFB, DGQ,) and one on Pinipir (DHJ), all over 1 m in height and decorated with non-representational pecked and incised carving. These are comparable to similar stone pillars found on Buka (Blackwood 1935: 531-40), the designs on which are said to be similar to designs used for cicatrices. All three pillars are probably of Buka volcanic stone. The two northern pillars (DGQ, DHJ) have incised curvilinear designs, but the one at Halian (DFB) has triangular pecked designs exactly similar to those on the lower part of the stone pillar at Iltopan in the northern part of Buka illustrated by Blackwood. Halian village was traditionally inhabited by migrants from Buka who are said to have brought this carved pillar with them. Other features of former village sites included shell midden, stone ovens, occasional shell and stone axes and other stone tools, and at one site a large clamshell ring broken during manufacture.

Many cave and rock shelter sites had surface pottery and midden often extending outside the shelter. Some were burial caves with human bones, pottery, and in one case fragments of local and imported arm rings. Shell adzes and adze blanks were also common. Most of the caves and shelters occur at the base of the cliffs around the outside rim of the island, only three being noted on the inner lagoon side. Solution caves and sink holes are found on the interior plateau of the island. Most of the rockshelters are wave cut and relate to present sea level which would have been reached about 6000 years ago. Solution has also been a force in their creation and some extend considerable distances back into the cliff face. Most of the caves and shelters recorded have some potential for excavation.

EXCAVATION

Excavations were carried out at six coastal rockshelters, one solution cave near the lagoon edge (DFV), and immediately behind the beach fronting the DES reef site. Of the six coastal shelters, five were on the north coast near Balil (DFX, DGD/1, DGD/2, DGD/3 and DGW), and one on the southeast coast near Mapiri (DFF). Sites were dug in 10 cm or 20 cm levels, following natural stratigraphy where this was apparent. In 1985 materials were dry screened through 7 mm mesh, and in 1986 and 1987 wet screening in sea water was undertaken through 5 mm mesh. Screen residue was not generally retained, all midden and artefacts being hand-picked from the screens. Sixteen radiocarbon results are available so far from the three seasons (Table 2). These have allowed division of the Nissan sequence into seven phases: Takoroi (c.4900 BP), Halika (3650 BP-3200 BP), Lapita (3200 BP-2500 BP), Yomining (2500 BP-1150 BP), Late Hangan (750 BP), Malasang (700 BP-c.500 BP) and Mararing/Recent (c.500 BP-50 BP). It will be noted that the dates for the end of the Hangan phase and for the Malasang phase have revised Specht's original chronology. Analysis of materials is continuing and only preliminary and incomplete analyses are presented here. Further radiocarbon

[1] The site, called Tatarsikirion (297 049), meaning 'the house post of the island' is a block of Lou Island obsidian embedded in the reef flat about 10 m from the outer edge of the reef. The visible part of the block is 19 cm x 10 cm, with 6 cm protruding above the reef surface. It presumably represents the general size of obsidian block which would have been transported from the Admiralties as raw material. One can only assume that it 'fell off the back of a canoe' and subsequently became cemented to the reef at some time in prehistory.

Table 2 *Nissan radiocarbon dates. All calibrations according to the Calib program of Stuiver and Reimer (1986). Marine shell calibrations assume Delta R=0. Hemispheric reservoir differences have not been taken into account in calibration.*

Laboratory Number	Provenience Phase	Date BP (C^{13} adjusted)	Material	Calibration BP (1 sd)
Site DFV, Lebang Takoroi				
ANU-5220	Sq.2 50-67 cm Takoroi	4300±210	charcoal	5268(4864)4564
Site DFF, Lebang Halika				
Beta-14447	Sq.2 110-135 cm Halika	3740±80	marine shell	3770(3654)3563
ANU-5225	Sq.7 100-110 cm Halika	2910±150	charcoal	3331(3049)2859
ANU-5224	Sq.6 70-80 cm end of Halika	3140±120	charcoal	3469(3373)3218
ANU-5221	Sq.3 30-50 cm Lapita	2920±80	marine shell	2755(2715)2660
Site DGW, Lebang Tatale				
ANU-5222	Sq.3 290-310 cm Halika	3460±80	marine shell	3417(3343)3248
ANU-6138	Sq.3 210-220 cm Halika	3380±130	charcoal	3829(3634)3469
ANU-5223	Sq.3 170-180 cm Yomining	1860±150	charcoal	1959(1822)1612
ANU-5226	Sq.2 140-150 cm Yomining	1310±100	charcoal	1310(1273)1160
ANU-5142	Sq.3 120-130 cm Hangan	860±110	charcoal	920(768)680
Site DGD/2, Yomining				
ANU-5229	Sq.2 145-165 cm Lapita?	3110±80	marine shell	2969(2864)2775
ANU-5228	Sq.2 105-115 cm Lapita	3350±80	marine shell	3321(3212)3099
ANU-6137	Sq.4 90-100 cm Lapita	2480±120	charcoal	2749(2709,2633, 2605,2587,2541, 2535,2499)2349
ANU-6136	Sq.4 70-80 cm Yomining	2440±110	charcoal	2739(2472)2349
ANU-5227	Sq.2 55-65 cm Yomining	1590±70	marine shell	1222(1142)1053
Site DGD/3, Yomining				
ANU-5230	Sq.1 170-180 cm Hangan	850±80	charcoal	907(741)688

samples are currently being processed. Excavations and results will be briefly described below and summary details given in Tables 3-7.

DFX
Lebang Dedelam
(Nissan 1:50,000 map reference, 311080)

The site is located 55 m from the reef edge, and consists of a large sink hole and former sea arch. Two pits, each 1 m², were excavated in different parts of the cave in 1985. One showed extensive disturbance during World War II, while in the undisturbed test pit sterile sand was reached at 35 cm. All artefacts were of the Recent or Historic periods. It appears that within the last few hundred years this cave was open to the sea.

DGD/1
a shelter in the Yomining Complex (324076)

The shelter is situated 110 m from the reef edge, and is c.10 m x 5 m. One 1 m² pit was dug to 175 cm in 1985. Sediments consisted of a coral rubble floor over dark brown medium sand down to 70-80 cm, changing to grey sand down to 120 cm with sterile white sand below to at

Table 3 *Nissan sites: summary excavation details.*

Site	Site Size(m^2)	Excavated Area(m^2)	Percentage Excavated	Volume excavated by Phase (m^3) Takoroi	Halika	Lapita	Yomining	Hangan	Malasang	Mararing/ Recent
DFV	180.0	7	4	1.5						2.18
DFF	227.0	6	3		3.95	4.89				
DGW	7.5	3	40		2.1		1.0	1.2	0.9	2.16
DGD/2	34.5	6	17			3.0	1.6			2.3
DGD/3	26.0	1	4					0.2	1.3	0.3
Totals	475.0	23	5	1.5	6.05	7.89	2.6	1.4	2.2	6.94

Table 4 *Nissan sites: midden and artefact densities. [a] excludes unworked manuports of exotic stone (total 25 for Takoroi Phase, 22 for Mararing/Recent Phase). [b] excludes sherds from one level lost in transit. Brackets unite assemblages of the same phase in order to provide numbers and percentages per phase. At DGW, Hangan and Malasang assemblages are combined because of the short duration of Hangan occupation at this site.*

Site	Phase	Calibrated ages BP	Marine shell g/m^3	Marine shell g/m^3/ 100 yrs	Total artefacts	Artefacts/ m^3	Artefacts/ m^3/100 yrs	Total sherds	Sherds/ m^3	Sherds/ m^3/100 yrs	Sherds/ m^3/100 yrs/ phase
DFV	Takoroi	c.4850	3724	?	49[a]	32.7[a]	?	0	0	0	
DFF	Halika	3650-3200	7020	1560	30	7.6	1.7	0	0	0	0
DGW	Halika	3650-3200	4843	1076	3	1.4	<0.1	0	0	0	
DFF	Lapita	3200-2500	6021	860	206	42.1	6.0	144	29.4	4.2	4.2
DGD/2	Lapita	3200-2500	5423	775	106	35.3	5.0	86	28.7	4.1	
DGD/2	Yomining	2500-1150	2330	173	53	33.1	<0.1	31	19.4	<0.1	4.4
DGW	Yomining	c.1900-1250	3172	488	96	96.0	14.8	56	56.0	8.6	
DGD/3	Hangan	c.750	1295	?	12	60.0	?	12	60.0	?	
DGW	Hangan	c.750	6327	2746	104	86.7	71.8	89	74.2	67.3	45.0
DGW	Malasang	c.700-500	7405		245	272.2		236	262.2		
DGD/3	Malasang	c.700-500	1588	794	68	52.3	26.2	59	45.4	22.7	
DFV	Mararing/ Recent	500-50	4761	1058	70[a]	32.2[a]	7.2[a]	50	23.0	5.1	41.6
DGD/2	Mararing/ Recent	500-50	4394	976	307[b]	133.5[b]	29.7[b]	275[b]	119.6[b]	26.6[b]	
DGD/3	Mararing/ Recent	c.500-50	2160	480	99	330.0	73.3	97	323.3	71.8	
DGW	Mararing/ Recent	c.500-50	6090	1353	629	291.2	64.7	612	283.3	63.0	

Table 5 *Nissan obsidian sourcing results. [a] the total includes samples not yet analysed to source, those anomalous in compositional analysis and seven specimens lost in transit from the field. Brackets unite assemblages of the same phase in order to provide numbers and percentages per phase. At DGW, Hangan and Malasang assemblages are combined because of the short duration of Hangan occupation at this site. DFV Mararing/Recent figures are not included in phase totals and percentages because they probably reflect re-use or mixing of deposits (see text).*

Site	Phase	Total obsidian[a]	Talasea obsidian (%)		Admiralties obsidian (%)		Admiralties percentage by phase	Obsidian/ m^3	Obsidian/ m^3/100 yrs	Obsidian/m^3 100 yrs/phase
DFV	Takoroi	47	29	(100)	0	(0)	0	31.3	?	?
DFF	Halika	0								
DGW	Halika	2	2	(100)	0	(0)	0	1.0	0.2	0.2
DFF	Lapita	51	22	(45)	27	(55)	57	10.4	1.5	1.2
DGD/2	Lapita	19	4	(31)	9	(69)		6.3	0.9	
DES	Late Lapita	14	0	(0)	13	(100)	100			
DGD/2	Yomining	21	3	(25)	9	(75)	93	13.1	1.0	3.5
DGW	Yomining	39	0	(0)	29	(100)		39.0	6.0	
DGD/3	Hangan	0								
DGW	Hangan	6	0	(0)	5	(100)	100	5.0	1.88	
DGW	Malasang	4	1	(25)	3	(75)	86	4.4		1.7
DGD/3	Malasang	4	0	(0)	4	(100)		3.1	1.6	
DFV	Mararing/ Recent	15	8	(89)	1	(11)		6.9		
DGD/2	Mararing/ Recent	17	3	(21)	11	(79)	81	7.4	1.6	0.9
DGD/3	Mararing/ Recent	1	1	(100)	0	(0)		3.3	0.7	
DGW	Mararing/ Recent	5	1	(25)	3	(75)		2.3	0.5	
DIZ	Mararing/ Recent	17	2	(12)	15	(88)				
Total		262								

least 175 cm. The test pit cut through part of a burial from 40 cm to 130 cm, but the area opened was too small to determine the extent of the burial pit or the presence of earlier deposits. Burial remains were photographed and returned to the landowner. Pottery was found down to 70 cm, of Recent style, as well as a shell nose ornament of a type traditional on Buka.

DGD/2
a shelter in the Yomining Complex (325075)

This medium sized shelter is located 65 m to the southeast of DGD/1 and c.110 m from the reef edge. The main shelter is approximately 34.5 m^2, with a low ceilinged second chamber 8 m x 4.5 m. In 1985 two 1 m^2 were dug to 200 cm, and in 1987 a further 4 m^2 were excavated to sterile. The generalised section is as follows: 0-20 cm light brown coarse sand; 20-60 cm brown fine sand, with at c.40 cm a pavement of stone slabs over much of the excavation, but not covering an extended male burial in Square 4 and part of Square 3; 60-90 cm very dark grey-brown medium sand; 90-130 cm coral rubble, white medium sand and ash lensing on top of white sterile fine to medium sand which extends to at least 200 cm in Squares 1 and 2.

The pottery down to 60 cm is of Recent and Mararing styles. An apparent hiatus in occupation, not reflected in sterile deposits then occurs. From 60-80 cm incised and plain pottery of mineral and calcareous temper is associated with calibrated ages of 2450 BP-1150 BP, a phase labelled Yomining after this site. The sequence of Lapita below 80 cm is continuous, including calcareous tempered decorated pottery of probable 'Far Western Lapita' style in Anson's terminology, with a date near the base of the cultural deposit of 3200 BP. The small collection of pottery from the lower levels of the site yielded 3.6% decorated sherds in the Lapita phase: 1.2% dentate, 1.2% incised and 1.2% notched rims. There were no plain rims. Yomining phase deposits yielded 22.6% decorated sherds: 16.1% incised and 6.5% notched rims, again with no plain rims.

As shown by midden and artefact densities, the most intensive use was earlier in the sequence. Obsidian results reflect the general Nissan trend with an increasing percentage of Admiralty Islands obsidian over time. It is interesting to note that three pieces of obsidian have been sourced to Talasea in the top 30 cm of the site – a similar number to those sourced to Lou from these levels. These perhaps represent increasing use of Talasea obsidian in the latest prehistoric or early historic periods. Associated with the Recent-Mararing levels were shell adzes, *Trochus* armband fragments, a stone ball of unknown use and a metal washer. From Yomining levels came a possible volcanic rubbing stone, and a stone adze fragment came from the Lapita deposits. *Cocos nucifera* and *Canarium indicum* were identified with high confidence in all phases, and *Sterculia* in Mararing-Recent levels with *Pandanus* and *C. salomonense* identified with less confidence from Lapita levels. All plant macrofossils in this and other sites have been identified by Douglas Yen (see Table 6).

DGD/3
a shelter in the Yomining complex (325075)

This is 35 m to the southeast of DGD/2, about 110 m from the reef edge, and is approximately 26 m² in area. One 1 m² was dug to 200 cm in 1985. The sediment consists of dark brown sticky medium sand down to 180 cm, followed by grey sand grading to white sterile sand. A large boulder, or cave floor bedrock, was encountered at 200 cm. The top 20 cm consisted of disturbed deposits, including World War II bullets, Recent pottery, one obsidian flake and at 20 cm a Malasang to Mararing transitional style sherd. From 20-50 cm there were no diagnostic sherds, and one shell adze. At 50-160 cm Malasang pottery, a filed bone point, one bone bead, three flakes of obsidian, one shell adze, a *Trochus* armring fragment, and part of a *Tridacna* armring were recovered. Deposits between 160 cm and 180 cm contained Late Hangan pottery. Only four pieces of obsidian were recovered: the three from Malasang levels were sourced to Lou Island, and one from Recent/Mararing levels was from Talasea (cf. the DGD/2 obsidian results). *Canarium indicum* and *Cocos nucifera* were identified with varying levels of confidence in virtually every level from the bottom to the top. Two 'not confident' designations of *Canarium harveyi* from Malasang levels were made, along with 'not confident' designations of *Ricinus* or *Celtis* in the top 20 cm, and *Sterculia* in a Malasang level. The Late Hangan level at 160-180 cm was dated to 750 BP, several centuries later than Specht's

Table 6 *Plant identification by phase on Nissan. Note that only* Cocos nucifera *was identified from Takoroi phase deposits (c.4850 BP), confidence B. Key: A = confident; B = some doubt; C = questionable; D = questionable-not confident; E = not confident. Notes: [1] Could be* Areca. *[2] Near surface, could be intrusive. [3] Some specimens are possibly* C. salomonense. *[4] Some specimens are possibly* C. decumanum. *[5] Could be* C. indicum. *[6] Alternative identifications for the same specimen.*

Plant Species	Halika 3650-3200 BP	Lapita 3200-2500 BP	Yomining 2500-1150 BP	Late Hangan 750 BP	Malasang c.700-500 BP	Mararing/Recent 500-50 BP
Cocos nucifera	A	A	A	A	A	A
Canarium indicum	A	A	B[3]	A	A[4]	A
Canarium harveyi	B		C	E	B	B
Canarium decumanum	E	D[2]				
Canarium salomonense	C	E			E[5]	
Pangium edule	D					E
Sterculia sp.	D				D	B
Metroxlyon sp.	C					
Burckella obovata	E					E
Spondias dulcis	D					
Caryota sp.	D[1]					
Dracontomelon dao	E					
Thespesia sp.	E					
Areca sp.	D					
Dioscorea sp. or *Colocasia* sp.	E					
Ricinus communis		B[2]				E[6]
Pometia pinnata	D					
Aleurites moluccana			D			
Pandanus sp.		D				
Terminalia catappa	E					
Tree fern sp.	D					
Celtis sp.						E[6]
Total taxa identified	18	4-6	4	3	4-5	7

Table 7 *Presence (x) and absence of material culture and fauna by phase. n.a. = not yet analysed; ? = identification as an artefact is uncertain.*

Traits	Takoroi c.4850 BP	Halika 3650-3200 BP	Lapita 3200-2500 BP	Yomining 2500-1150 BP	Late Hangan c.750 BP	Malasang c.700-500 BP	Mararing/Recent 500-50 BP
Material culture							
pottery source			Ambitle? Buka?	Ambitle?	Buka	Buka	Buka
stone adze			x				x
stone knife		x	x				
stone pounder	x						x
stone ball							x
pumice/volcanic stone abrader				x			x
Tridacna adze		Hinge region, unique form	Hinge region		dorsal region	dorsal region	dorsal region
Tridacna nose plug					x		
Tridacna armring						x	x
Trochus fishhook		x					
Trochus armring			x		x	x	x
Trochus (?) nose ornament							x
pearl shell knife		x	x				x
small shell bead							x
small shell ring					x		
shell pendant					x		
shell abrader		?	?	?	x	?	?
human tooth pendant			x				
mammal bone point			x			x	
bone bead						x	
human burial in caves						x	x
'ochre' block							x
historic artefacts (glass, metal)							x
Fauna							
Phalanger orientalis	x	x	x	x	x	x	x
Pteropus sp.	x	x	x	x	x	x	x
Sus scrofa		x	x	x	x	x	x
Rattus exulans			x	x	x	x	x
fish							
: Muraenidae	n.a.	x	x	n.a.			
: Scaridae	n.a.	x	x	n.a.	x	x	x
: Balistidae	n.a.	x	x	n.a.	x	x	x
: Scombridae	n.a.			n.a.	x	x	x

(1969) estimate from Buka. It is this result and the similar date for a Late Hangan level at DGW which have led to the revision of Specht's chronology, mentioned earlier.

DGW
Lebang Tatale (332072)

This small shelter is 155 m from the reef edge, and yielded surprisingly deep deposits. In 1985 a 1 m^2 pit was dug but problems with minor collapsing of the side walls made results below 140 cm unreliable. In 1986 a further 2 m x 1 m area was opened up, 1 m of which was excavated to bedrock at a maximum of 330 cm below surface. On top of the bedrock was a rafted pumice beach deposit and occupation commenced on top of this. A generalised section is: 0-40/50 cm (below datum) dark brown medium sand, disturbed by burial pits; 50-100 cm white-flecked grey-brown medium sand; 100-120 cm sticky brown medium sand with cobbles; 120-135 cm layer of roof fall boulders and cobbles; 135-145 cm brown medium sand; 145-270 cm orange-brown medium sand; 270-300 cm medium sand grading to finer grey sand with cobbles; 300-310 cm pumice beach on top of bedrock.

The cultural sequence is as follows. Down to 70 cm Recent and Mararing pottery occurs. These levels include six inhumations (flexed and secondary) marked by layers of stones around and above the burials but with no directly associated grave goods. Artefacts included shell adzes, a pumice abrader, shell beads, *Trochus* armring fragments, a possible stone pounder, obsidian, a glass bead and a lead pellet. From 70 cm to 100 cm Malasang pottery occurs. The deposit included two burials. Artefacts included shell adzes, a *Trochus* armring fragment, and obsidian. From 100 cm to 130 cm Late Hangan pottery is associated with a radiocarbon date of 750 BP. Artefacts included a shell adze, a *Tridacna* nose plug, a shell pendant, a shell ring, a *Trochus* armring fragment and obsidian. Below this are roof fall boulders marking a hiatus in occupation until about 1250 BP. From 140 cm to 190 cm plain calcareous tempered pottery of Yomining style occurs. Apart from pottery the only other artefacts were obsidian flakes. Bone was not found below 170 cm. From 170-180 cm came a date of 1800 BP. Between 190 cm and 310 cm an aceramic Halika phase deposit occurs, with shell midden, charcoal, and (below 250 cm) bone, including fish and phalanger. Two obsidian flakes and a volcanic stone knife like those found at Lapita site DES were the only artefacts, but the knife and one of the flakes occur in the top 10 cm of the Halika levels and may in fact be associated with the Yomining phase.[2]

Two radiocarbon dates, though stratigraphically inverted, overlap at two standard deviations and suggest an aceramic phase from about 3650 BP. Obsidian results confirm general trends with the two Halika phase flakes coming from the Talasea source, all Yomining and Hangan phase obsidian which has been sourced coming from the Admiralties, only one out of four Malasang phase flakes coming from Talasea and only one out of four sourced pieces in Recent/Mararing levels coming from Talasea. Significantly this last flake came from the top 20 cm in the site.

Canarium indicum and *Cocos nucifera* were positively identified from all phases of occupation, with *Canarium harveyi* identified from all phases (except Yomining) with confidence varying from 'some doubt' to 'not confident', and *C. salomonense* 'questionable' in the Halika phase and 'not confident' in the Malasang phase. Other identifications at a 'questionable/not confident' level included *Pangium* (Halika, Recent/Mararing), *Sterculia* (Halika), *Metroxylon* (Halika), *Terminalia* (Halika), *Aleurites* (Yomining), and *Burckella* (Recent/Mararing).

Rapid deposition of sediment in the Halika phase may be a result of erosion from forest clearance for agriculture on the plateau above the shelter. Ephemeral use associated with nearby gardening activities and/or reef fishing is suggested for this shelter, with Yomining phase utilisation less intensive than that attested for the Halika phase on the basis of shell midden density and land snail accumulation. Although there is no obvious stratigraphic break, the shelter appears to have been abandoned during the Lapita phase. In Yomining phase deposits there were no decorated sherds.

DFV
Lebang Takoroi (373394)

This is a solution cave adjacent to the lagoon edge near Tanamalit village, with two entrances

[2] NOTE ADDED IN PROOF (September 1991). Subsequent dates from DGW confirm that the stone knife and one of the obsidian flakes are associated with Yomining phase deposits, and establish that the lowest date from the site (ANU-5222) is in error. The aceramic levels are now assigned to the Takoroi phase ending at 3650 BP (ANU-6138). This new information does not seriously affect interpretation of the Halika phase in general except that there is now no obsidian known to be in use in that phase. Exploitation of *Canarium* spp. and possibly *Pangium*, *Sterculia*, *Metroxylon*, and *Terminalia* can be added to the corpus of plants used in the Takoroi phase, and *Pangium*, *Metroxylon* and *Terminalia* deleted from the Halika identifications in Table 6.

(one partly blocked by World War II bulldozing), and several side chambers. The main chamber is c.15 m x 12 m, and the floor is c.4 m above high tide level. It is almost the only habitable cave on the lagoon side of the island. In 1985 two 1 m test squares were opened 6 m apart, and in 1987 a further 5 m² were excavated adjacent to Square 2 in the inner, lower part of the cave. Bedrock was reached at 63-74 cm below datum, and the sediment was a fairly uniform brown medium-coarse sand with occasional large roof fall boulders throughout the deposit.

In Squares 2-7 pottery of Mararing and Recent styles was found to between 35 cm (Squares 3 and 7) and 45 cm (Squares 4, 5, 6) below datum, following the slope of the cave floor. In ceramic levels were 15 obsidian flakes (nine so far sourced, eight Talasea, one Lou) and from aceramic levels 44 obsidian flakes (29 so far sourced, all from Talasea). It is likely that many of the pieces in the ceramic levels result from re-use of older material or disturbance of the earlier deposits. This is suggested because of the high percentage of Talasea obsidian, at odds with all other contemporary sites on the island. In two squares no obsidian at all was found in ceramic levels. In the ceramic deposits were a few possible pumice and volcanic rubbing stones, a block of 'ochre', and a *Tridacna* shell adze blank. A small volcanic stone pounder came from aceramic levels. A sharpened piece of worked shell was first thought to have been in situ in aceramic levels but the context has now been established as representing a localised disturbance. Volcanic stone manuports were found throughout the deposits. Charcoal from all squares has been analysed and *Cocos nucifera* has been positively identified from all levels, with no other species identified. A date was obtained from the base of Square 2 of 4850 BP. Nine human teeth were found in aceramic levels.

This is the earliest site found on the island and has given its name to the Takoroi phase. It is not clear if this represents a permanent occupation of the island during the fifth millennium BP, or merely periodic visits.

DFF
Lebang Halika (390986/7)

This large rockshelter and solution cave below the cliff at Mapiri is about 110 m from the reef edge. Part of the shelter floor has been dug away for the bed of a copra dryer. In 1985 a 1 m² excavation was taken to bedrock in the base of the copra dryer pit and, because it revealed an aceramic deposit, a further 2 m x 1 m was excavated. In 1986 a further 4 m² were excavated. A generalised section is: 0-60/70 cm dark brown fine to medium sand with cobbles; 70-95 cm partly cemented coral rubble; 95-110 cm coral rubble with darker lenses, occasionally cemented; 110-142 cm maximum, lenses of brown and grey medium sand, interspersed with thin charcoal lensing, coral rubble and (Layer 4c) a greasy dark brown fine sand. The cultural sequence consists of a disturbed Lapita pottery deposit including 'Far Western Lapita' style sherds in the top 70 cm with occasional Mararing and/or Recent sherds in the top 20 cm and small metal fragments in crab disturbances to 70 cm. Below 70 cm is an aceramic Halika phase deposit on top of a bedrock base. A marine shell sample from basal grey sand gave a date of 3650 BP for the start of occupation. Charcoal at 70-80 cm at the top of the aceramic levels yielded an age of 3350 BP, and an age of 2700 BP was obtained from the 30-50 cm level of the Lapita deposits.

Artefacts associated with Lapita levels included polished *Tridacna* shell hinge region adze fragments, a bone point, a *Trochus* armring fragment, a human tooth pendant, a polished shell knife fragment, various pieces of worked shell and 51 obsidian flakes. Below 30 cm the ratio of Talasea to Lou obsidian is 50:50, but above that there are twice as many Lou pieces as Talasea, suggesting a trend of replacement of Talasea by the Lou source over time. The Lapita pottery includes both mineral and calcareous tempered sherds; 12.5% were decorated, with 5.6% dentate-stamped, 2.8% incised and 4.2% notched rims. In aceramic Halika levels no obsidian was found, but artefacts included *Tridacna* shell adzes, several polished shell knife fragments, cut shell and a *Trochus* shell fishhook. A significant feature of the Halika phase levels was the quantity of animal and fish bone. In Squares 4-7, 463 gms equalling nearly 83% of the total amount of bone from those squares, suggests greater reliance on hunting and fishing in the earlier phase occupation.

Confident identifications were made of *Canarium indicum* and *Cocos nucifera* in both Lapita and Halika phases, and slightly less confident identifications of *Canarium harveyi* in the aceramic, and *Ricinus communis* in the Lapita levels although the latter could be intrusive from the surface. For the Halika phase 'doubtful/not confident' identifications were made of *C. harveyi* again, *C. decumanum, C. salomonense, Spondias, Caryota* (could be *Areca*), *Sterculia, Dracontomelon, Thespesia, Burckella, Pometia, Areca*, and a tree fern, as well as a rind possibly of taro or yam. In the Lapita levels possible identifications were made of *Pandanus* and

Plate 1 *Halika Phase artefacts from Lebang Halika (DFF) and Lebang Tatale (DGW). Top row: pearl shell knife fragments (DFF); second row:* Trochus *fishhook fragment (DFF); third, and fourth rows: cut pearl shell (fishhook tabs?) (DFF); fifth row: (left) stone knife (DGW); (right)* Tridacna *adze (DFF).*

Canarium decumanum, but again these samples may be intrusive from the surface.

DES
Tarmon (303016)

This is an artefact scatter on the reef adjacent to the shore at the southernmost reef passage, probably representing settlement on a former sand spit. The main archaeological work carried out here was an intensive surface collection from the reef flat at low tide over an area of 73 m x 70 m directly off the beach. Two distinct periods of occupation are represented: decorated pottery of 'Western' Lapita type and Yomining styles was collected, with a few Sohano sherds (c.2200 BP-1500 BP) as well as later styles from the Late Hangan phase onwards. Materials of both periods of occupation are concentrated in a limited area and separation of material apart from pottery is therefore not possible. The presence of typical Lapita stone adzes and other tools, some of a similar material to that used for Reef-Santa Cruz Lapita adzes (Roger Green pers. comm.), as well as many kilograms of oven stones suggest initial occupation was a Lapita village settlement controlling the reef passage into the Nissan lagoon. All obsidian comes from the Lou Island sources.

The total percentage of decorated sherds has not yet been calculated, but is of the order of 16%. Of the earlier period rims, handles and decorated sherds, which total 381, there were 56.2% notched rims as opposed to 27% plain or worn rims and 3.1% handles. Of these 381 sherds, 13.4% were appliqué decorated, 7.3% were dentate-stamped, 6.8% were incised, 4.2% had perforations at or below the rim, 2.6% were handles decorated with notching and perforations, 1.8% were stick or fingernail impressed, 1.3% had notched appliqué bands or carinations which were not part of rims, and one flat base sherd (0.3%) had rectangular cut-outs as well as dentate-stamping. None of these decorative categories are mutually exclusive as more than one type of decoration may occur on one sherd. A 1 m² test pit was excavated immediately behind the beach to the water table but no intact cultural deposit was found. The beach seems to have shifted several times, having eroded back and then prograded and subsequently eroded again at least once since the original occupation. Older informants claim that 70 m width of land has been eroded during their lifetimes. This beach is a traditional location for canoes from Buka to come ashore for trade.

DIZ
Warom Reef (353957)

No excavation was carried out on what was an artefact scatter on the wide reef flat near Torohatep village, covering an area of 150 m by 75 m. The site is interesting, however, because, with one possible exception, all of the pottery was of Mararing and Recent styles, thus tying the site down to the period from about 500 BP-50 BP. In addition to pottery, 17 pieces of obsidian, two stone adzes and an adze chip were found. All but two of the obsidian pieces were from Admiralty Islands sources, consistent with the age of the site based on the pottery. One piece of obsidian appeared to be part of a triangular sectioned blade tool. The site may represent a stilt house settlement on the reef flat as there is no evidence of recent shoreline retreat in the area, or alternatively a refuse dump for a former village on the shore.

ANALYSIS

Several kinds of analysis of material are in progress or have been planned. Fish bone from the 1985 and 1986 seasons has been analysed by Dr Akira Goto, formerly of the University of Hawaii (Table 7). Few identifiable elements were recovered from DFV and from pre-Hangan levels of DGW, but the Lapita and Halika phase site DFF contained three common taxa: moray eels (Muraenidae), parrotfish (Scaridae) and triggerfish (Balistidae). All of these are reef/lagoon fish, as are the less well represented fish species in these sites (Elasmobranchs, Holocentridae, Serranidae, Lethrinidae, Lutjanidae, Kyphosidae, Carangidae, Labridae, Acanthuridae and Diodontidae). There is a marked difference in fish species between this site and the Hangan and later phases of DGW and DGD/3 dating to the last 750 years. Parrotfish and triggerfish continue to be exploited but moray eels completely disappear from the archaeological record, and for the first time pelagic fish (Scombridae) appear and are the commonest fish in both DGW and DGD/3. The concentration on reef fish in Lapita levels fits with the evidence from other Lapita sites (Green 1986; Kirch and Dye 1979), and there is a later development of the techniques ethnographically recorded for pelagic fishing involving trolling. The absence in later sites of moray eels perhaps represents a food taboo, although given the small numbers involved sampling error cannot be ruled out. Today they are only eaten in some villages on Nissan (cf. Kirch

Plate 2 *Lapita and Yomining Phase pottery from Yomining (DGD/2) and Lebang Halika (DFF). Top and second row from DGD/2, third to fifth rows from DFF. Top row: (left to right) 1. dentate-stamped, Sq.5, 100-110 cm. 2. incised, Sq.3, 60-70 cm. 3. notched and perforated rim, Sq.2, 55-65 cm. 4. incised, Sq.4, 70-80 cm; second row: 1. incised, Sq.2, 95-105 cm. 2. notched rim (inner side notching), Sq.4, 110-120 cm. 3. incised, Sq.4, 60-70 cm; third row: 1. incised carination, Sq.6, 30-40 cm. 2. notched rim, surface. 3. incised, Sq.5, near surface; fourth-row: 1. dentate-stamped, Sq.2, 10-30 cm (Anson motif 52?). 2. incised, Sq.4, 30-40 cm; fifth row: 1. dentate-stamped and fingernail impressed, Sq.4, 40-50 cm. 2. dentate-stamped, Sq.3, 10-20 cm (Anson motif 490). 3. dentate-stamped, Sq.4, 30-40 cm (not in Anson).*

and Yen 1982:292 for archaeological and ethnographic evidence for a taboo on moray eels in Tikopia within the last 300 years). Yomining phase samples have not yet been studied in detail.

Dr Tim Flannery has analysed mammal bone from the 1985 and 1986 seasons and summary results have been published (Flannery et al. 1988; cf. Table 7). A diminutive form of *Phalanger orientalis* is present from the beginning of the Nissan sequence and was presumably introduced by the first settlers. A species of *Pteropus* (fruit bat) is also present throughout the sequence, but other mammal species have only been identified in later phases, and no dog remains were found in any sites. Pig first occurs in Halika phase levels in DFF but in small quantities, primarily represented by isolated dentary fragments, and becomes more common in later levels. *Rattus exulans* is present from Lapita levels onwards in small quantities but *Rattus praetor* (not present on Nissan today) is known only from a single dentary fragment in a Malasang level at DGW and presumably represents an accidentally introduced animal rather than an established population.

Residue analysis of obsidian specimens was commenced by Richard Fullagar and is being continued by Tom Loy. Blood residues have been found on several obsidian artefacts, as have fish scale fragments and residues suggesting wood and plant working.

Pottery sourcing involving initial sorting by xeroradiography as well as electron microprobe and other analyses has been started in association with Wallace Ambrose. On X-ray, some Nissan 'Far Western' Lapita sherds have shown identical temper patterns to some Ambitle Lapita sherds and so the suggestion that this is the source of early Nissan Lapita pottery has not been disproved.

A brief comparison of Lapita design motifs has been made using Anson's (1983) catalogue. The four identifiable motifs from the DFF site (515, 306, 490, possibly 52) are uniquely shared with Ambitle, or with Ambitle and Eloaua, and along with the 'small and needle-like' dentate techniques (Anson 1986:161) place DFF and the lower levels of DGD/2 within the 'Far Western' style of Lapita, thus dating it at DFF to somewhere between 3350 BP and 2700 BP. In contrast, comparison of the eight identifiable motifs from the DES reef site (Anson motifs 394, 2, 230, 448, 260, 435, 1, possibly 35) and the 'broad, rather rectangular' dentate technique (Anson 1986:161) used there places this site firmly in the 'Western Lapita' style and closest to RL-2 and SZ-8 in the Reef-Santa Cruz area of the Solomons and New Caledonia Site 13. This suggests an age of somewhere between about 2900 BP and 2350 BP, probably towards the later end of that range (Spriggs 1990). The Buka Island Lapita sites are also most similar to the same three sites in motif analysis (Wickler 1990), and it can be suggested that the DES sherds are of Buka origin.

Analysis of human skeletal material is being carried out by Dr Michael Pietrusewsky of the University of Hawaii.

THE NISSAN SEQUENCE

A preliminary cultural sequence for Nissan island is presented below using, for convenience, the intercepts of the calibrated dates (Table 2). Further radiocarbon dates will refine the dating of parts of the sequence. Phase names have been assigned on the basis of sites or area names where their materials are best represented. For phases when Buka pottery of types recognised by Specht (1969) are being imported, Specht's style names are used. The Nissan evidence suggests however that revision of the absolute dates for his sequence from the Hangan phase onwards is necessary. Summaries of distribution of floral, faunal and artefactual remains by phase are given in Tables 6 and 7.

Takoroi Phase. c.4850 BP (2900 BC). Represented at site DFV. Aceramic. All obsidian from Talasea. Small volcanic stone pounder the only other clearly associated artefact, apart from manuports of exotic stone of unknown function. Exploitation of phalanger, fruit bat, fish, and shellfish. Coconut present but no evidence of other plant exploitation.

Halika Phase. c.3650 BP-c.3200 BP (c.1700 BC-c.1250 BC). Represented at DFF, DGW. Aceramic. Generally lacks obsidian, only two flakes found, both from Talasea. Several pearlshell knife fragments. *Trochus* shell fishhook, other cut shell, a *Tridacna* shell adze, and a volcanic stone knife (Plate 1). First evidence of pig. Fish all inshore reef species, no pelagic species. First evidence of a range of tree crops/domesticates including various *Canarium* spp. At DFF greater reliance on faunal exploitation than in the next phase. But for the lack of pottery and rarity of obsidian, the sites have a typically Lapita material culture. They are contemporary with Lapita sites elsewhere and essentially can be considered 'Lapita without pots'.

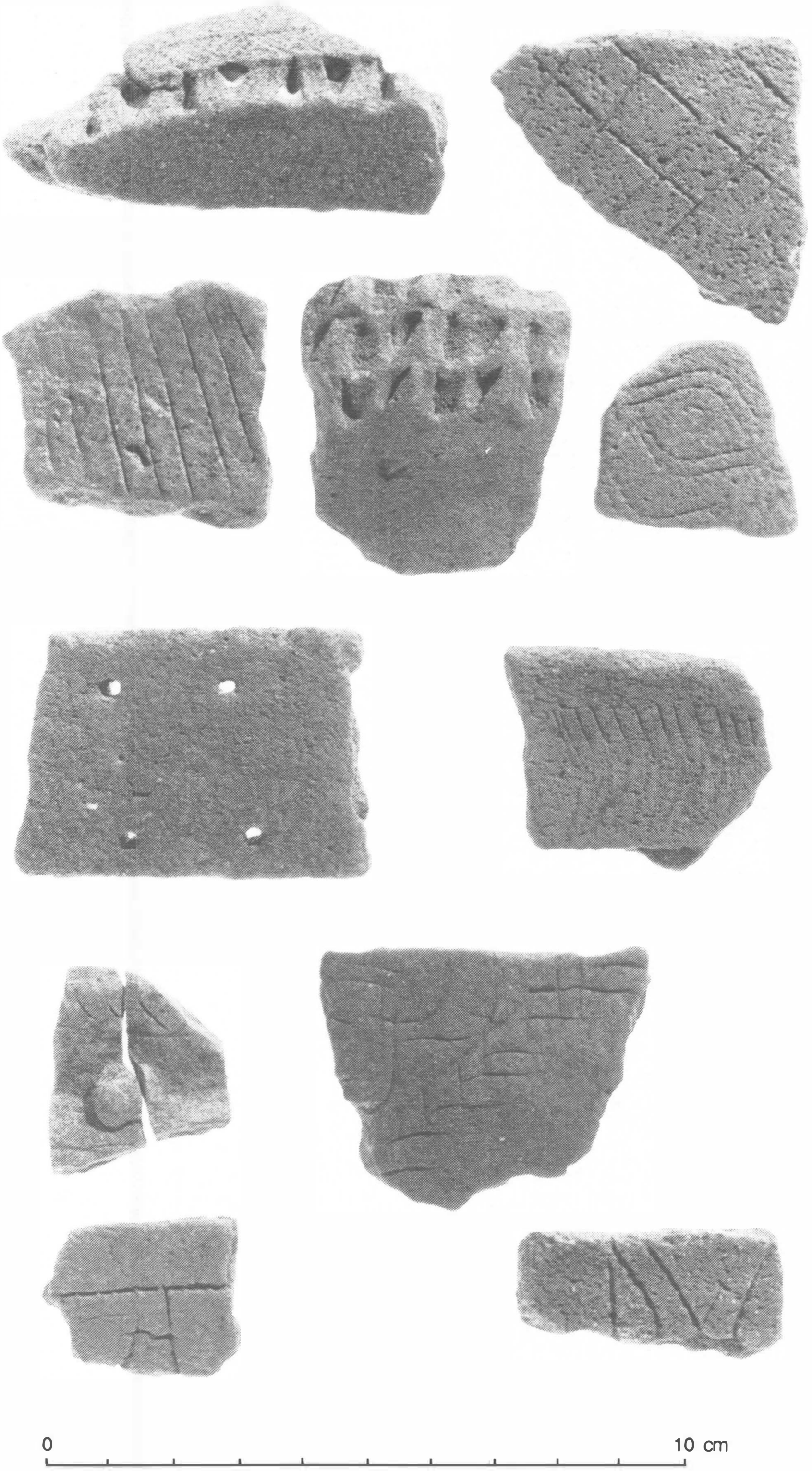

Plate 3 *Lapita pottery from Tarmon Reef (DES). Top row: (left to right) 1. flat bottomed bowl, dentate-stamped and cut outs. 2. incised (cf. Anson motif 230); second row: 1. incised (cf. Anson motif 435). 2. fingernail impressed. 3. dentate-stamped (Anson motif 394); third row: 1. handle, notched and perforated. 2. rim, stamped (Anson motif 260); fourth row: 1. rim, appliqué and stamped or incised (Anson motif 35?). 2. stamped or incised; fifth row: 1. dentate-stamped. 2. dentate-stamped.*

Lapita Phase. c.3200 BP-2700/2500 BP (c.1250 BC-750/550 BC). Represented at DFF, DGD/2 and DES. Lapita pottery (mineral and calcareous tempered) probably from Ambitle of 'Far Western' style at DFF and DGD/2 (see Plate 2). At DES undated Lapita of 'Western' style probably made on Buka, time range 2900 BP-2350 BP on stylistic comparison (Plate 3). Lou Island obsidian first occurs in sites and becomes the dominant source over time. *Tridacna* shell hinge region adzes (cf. Kirch and Yen 1982:213), a *Trochus* armband, a pearlshell knife fragment, other worked shell, a bone point, and a human tooth pendant. Less reliance on faunal exploitation but a probable increase in pig bone. First evidence of introduction of *Rattus exulans.* Some of the polished plano-convex stone adzes and other stone artefacts at DES probably from this phase.

Yomining Phase. 2500 BP-1150 BP (550 BC-800 AD). Represented at DGD/2, DGW. Generally plain calcareous tempered pottery with (towards the end of the phase) incised decoration and some mineral tempered sherds (Plate 2). It is assumed that the pottery is from Ambitle or New Ireland, because at 2200 BP-1500 BP the distinctive Sohano style is found all over Buka Island. On Buka, similar generally plain calcareous tempered pottery occurs from c.2500 BP-2200 BP. This is Specht's 'Buka style' (Specht 1969). Lou Island obsidian predominates. Artefacts include a stone adze and a *Trochus* armring. The pottery represents continuity from Lapita, no break being apparent at DGD/2. At DES a few sherds from Buka of Specht's Sohano style were found indicating some links to the south at about 2200 BP-1500 BP.

Late Hangan Phase. c.750 BP (c.1100 AD). Represented at DGD/3, DGW, and occasional surface finds. Pottery from Buka of Specht's Hangan style is found but dates from Nissan indicate his dates for this style (1500 BP-1300 BP) are incorrect. These suggest 1500 BP-700 BP for the Hangan phase. Lou Island obsidian continues to predominate as it does until the latest prehistoric or early historic period. Other artefacts are dorsal region *Tridacna* shell adzes (cf. Kirch and Yen 1982:213), a *Tridacna* nose plug, a shell ring, and a shell pendant. The range of inshore/reef fish continue except for the disappearance by this phase of moray eels, and the first appearance of pelagic fish (Scombridae) caught by trolling. As on Buka, in this phase elements of the ethnographically described culture become apparent. Only the very end of Specht's Hangan phase is represented on Nissan.

Malasang Phase. c.700 BP-c.500 BP (c.1050 AD-c.1350 AD). Represented at DGD/3, DGW, and by many surface finds. Pottery is from Buka of Specht's Malasang style, dated by him 1300 BP-800 BP. As well as obsidian flakes, there are dorsal region *Tridacna* shell adzes, a *Trochus* armring, a *Tridacna* armring, a mammal bone point, and a bone bead. The first evidence of cave burial occurs at DGW.

Mararing/Recent Phase. c.500 BP-50 BP (1350 AD-c.1900 AD). Represented at DGD/2, DGD/3, DGW, DFV, and many surface finds. Pottery is of Buka origin, in Specht's Mararing style (Specht 1969 chronology, 800 BP-300 BP) and Recent style (300 BP-100 BP). Nowhere in the excavations are Mararing and Recent sherds stratigraphically separated and it is suggested that the Mararing style was a very short lived phenomenon. Artefacts included dorsal region *Tridacna* adzes, *Tridacna* armrings from Tanga and Nissan, *Trochus* armrings, shell 'money' beads, stone adzes (generally waisted) of Buka origin, pearlshell knives, *Tridacna* nose plugs (ethnographically recorded, Krause 1907:85), pumice abraders, a stone pounder, a stone ball, and a shell nose ornament of a type known from Buka.

Historic trade items were also found such as glass beads and a lead pellet. In the top levels of some sites there appears to be a slight resurgence of the use of obsidian from the Talasea source, perhaps relating to increased contacts between New Britain and the Solomons in the German colonial period. Cave burial is common involving extended, flexed and secondary modes of interment.

One interesting aspect of the Nissan sequence is the way in which rockshelters go in and out of use. Thus DFV is the earliest utilised shelter and the only reasonably dry rockshelter on the lagoon side of the island. It was used for an unknown period, presumably ending prior to 3650 BP and then only utilised again within the last few hundred years. All shelters bottom out on either sterile beach sand or wave-scoured bedrock (except for DFV which appears to be either a solution cave or a sea cave feature of the original reef) and it appears that prior to human occupation starting 3650 BP-3200 BP in DGD/2, DGW and DFF these shelters were in reach of wave action and indeed are probably at least in part wave cut features created since present sea level was reached at c.6000 BP. When DFV was initially used at c.4850 BP or earlier it may have been the only habitable cave on the island. Only after about 3650 BP did other caves become usable, as reef growth and storm beach build up led to coastal progradation away from the base of the cliffs and the progressive creation of the narrow coastal plain which even now does not entirely circle the island on its seaward side. DFX for instance appears only to have become usable in the last few hundred years. Initially adjacent to the beach these newly created shelters would have been attractive locations for fishing camps, rest stops, etc. As coastal progradation continued, they were situated further from and no longer within sight of the beach, in dank and mosquito-ridden coastal forest, cut off from sunlight and coastal breezes. Their value thus declined and progressive abandonment occurred. Only in the latest prehistoric period did some of the caves come to be used again, this time primarily for burial purposes.

DFF was used from c.3650 BP-2700 BP and then used again very intermittently only in the last few hundred years. DFV's abandonment was even longer. In DGW there is a hiatus in use marked by roof fall accumulation between about 1250 BP and 750 BP, and at DGD/2 a hiatus between about 1150 BP and the last few centuries. It is unlikely that these rockshelters ever constituted the primary settlement focus on the island, but the high rainfall and constant turning over of the very thin soil by gardening activities and pigs means that surface indications of earlier phases are less likely to be preserved, except where the evidence is on the reef flat itself, such as at DES. Elsewhere there is evidence of village sites from the Hangan period on, but particularly for the Malasang to Recent phases. Despite the taphonomic problems already noted, the numbers of sites in these later phases do seem to indicate a rising population trend over the last 750 years at least.

There are some interesting trends revealed by the midden analysis and artefact density measures. Shellfish midden density decreases markedly following the Halika phase through to the Yomining phase, then rises markedly in the Hangan, Malasang and Mararing/Recent phases. This perhaps shows greater reliance on hunted and gathered food early on and then a switch to an increasing reliance on agricultural production in Lapita and Yomining phases. With the evidence of rising population from the Hangan phase onwards shellfish again became a commonly exploited food source. Artefact densities in general increase from the Hangan phase onwards, the one exception being imported obsidian. Obsidian flake numbers increase from the Halika phase to their highest level in the Yomining phase, and then decrease markedly. It would be interesting to know if such fluctuations in supply are evident in sites further north and closer to the obsidian sources.

CONCLUSIONS

When first excavated, the c.4850 BP date for site DFV represented the earliest clear evidence for human settlement in the Pacific south and east of New Ireland. In sight of Buka, the Nissan evidence strongly suggested that the Solomons chain was inhabited in pre-Lapita times, presumably by people speaking non-Austronesian languages ancestral to those spoken over much of Bougainville today. This suggestion was spectacularly confirmed by Wickler's excavation of a 29,000 year old human occupation in a rockshelter on Buka in 1987 (Wickler and Spriggs 1988). The initial links of Nissan are clearly to the north, however, given the Talasea obsidian. Just how long Nissan had been habitable is unknown. It is a Pleistocene age raised atoll but for much of the Pleistocene may have been a submerged reef. If so, it would not have provided a stopping point for travel to the Solomons and an open ocean crossing of perhaps 180 km without island inter-visibility would have been a greater challenge than the initial human voyages from Sunda to Sahul.

There is a question as to how hospitable for settlement Nissan would have been at 4850 BP or before. There is very little soil development today over the limestone bedrock and that largely as a result of decay of vegetative matter. How much less would there have been nearly 5000 years ago? The coastal plain on the seaward sides did not exist and waves broke against the base of the cliffs all round the outside of the island. It is not clear what natural vegetation the island would have supported at that time, nor whether the early inhabitants had an agricultural base. The only identified plant, the coconut, is certainly part of the natural strand vegetation of the region (cf. Spriggs 1984a). The only land mammals were bats and the phalanger, the latter almost certainly a human introduction. Whether the human presence on the island was a permanent one after 4850 BP is also open to question.

The second identifiable phase on Nissan, the Halika phase, starting around 3650 BP, is contemporary with the earliest Bismarcks Lapita sites, such as Kohin Cave on Manus dating to 3850 BP (Kennedy 1981) and ECA on Eloaua whose oldest dates calibrate to 3450 BP (Kirch 1987:168). I would identify this phase as a component of Lapita culture, but without pottery and with very little (Talasea) obsidian: perhaps a pioneer phase of Lapita settlement. The introduction of the pig, the range of domesticated nut trees and other introduced plants, the shell adze, the *Trochus* fishhook, and pearl shell knives (Kirch's (1987) 'scrapers') are all comparable to material found in Lapita sites and not generally found in earlier sites in the Island Pacific. Around 3200 BP pottery and obsidian exchanges were established with the north and the presence of probable Ambitle pottery on Nissan allows us to cross-date the Ambitle Lapita site to c.3200 BP-2700 BP. By this time the Lou Island obsidian sources were being exploited and over time became increasingly the dominant supplier of obsidian used on Nissan.

The Lapita pottery is of 'Far Western' style as defined by Anson (1983; 1986), but the DES reef site has presumably later 'Western' style Lapita which may be of Buka origin to the south. Nissan may have formed the non-pottery producing Lapita link between 'Far Western' Ambitle and 'Western' Buka which allowed these styles (the 'Western' originally derived from earlier 'Far Western' models) to drift apart stylistically. [3]

The general trend in Lapita sites is towards calcareous tempered plain ware, representing a breakdown of whatever symbolic network the decorated pottery helped to maintain. This is evident on Nissan where some time after 2700 BP nearly all pottery becomes plain, as it is on Buka from about the same time. It seems that this plain calcareous tempered pottery which was imported to Nissan until about 1150 BP must come from the north as it overlaps in time the Sohano and most of the Hangan style on Buka (Specht 1969). Only a few Sohano sherds are known from Nissan and significant contact with Buka is only evident again from about 750 BP at the end of the Hangan phase.

It may well be this re-establishment of ties to the south which is recorded in traditional Nissan stories:

> We also have a legend that explains how our people first found out about the existence of Buka. The Buka people, it says, used to keep tame doves, and they tied bits of red rattan cane round their legs to identify them. Now during a certain season the doves would fly to Nissan Island, because we had many galip trees. One day an old woman saw one of the doves sitting on the roof of her house and she observed the red rattan cane on its leg. Then the people caught the dove and they took off the rattan and they tied some leaves on in exchange. This was to let the unknown people in the strange land know of our existence. Then people started to build their long canoes for the first time, and they set out on the sea and followed the doves (Hannett 1971:19).

It is from the Hangan phase onwards that Nissan material culture appears similar to that recorded in the early years of European contact. Specht (1969) made the same point for the Hangan period of Buka. But what of earlier phases? I have elsewhere (Spriggs 1984b) argued for an essential continuity in Island Melanesia from Lapita onwards, based on a model of linguistic continuity and on admittedly imperfect samples of material culture inventories. Since then Malcolm Ross' (1988) re-examination of the Austronesian languages of the area has suggested a two phase migration of Austronesian speakers through the area from a homeland in New Britain. His Meso-Melanesian cluster which includes the ancestor of the current Nissan (Nehan) language is seen as representing a second migration which may have replaced Austronesian languages more closely related to the languages extant in the Southeast Solomons and the Central Pacific. Some Northwest Solomonic languages bear traces of contact with Oceanic languages of this earlier type.

While Ross (1988:Chapter 10) does not explicitly make the connection, the first migration agrees well with the evidence of the Lapita expansion south from the Bismarcks about 3200 BP-3000 BP. The spread of the Meso-Melanesian cluster, in particular the Northwest Solomonic group, could perhaps be marked by the spread of later pottery styles in the area such as Lossu/Lasigi ware (White and Downie 1980),

[3] Kirch (1987:176-7 cf. Kirch et al. 1987) has recently cast doubts on the reality of an earlier 'Far Western' Lapita, based on his Eloaua excavations. The existence of 'Western' traits such as flat bottomed bowls and broad and rectangular dentate techniques along with the finer dentate technique in a series of sites which span a millennium of Lapita development is, however, not surprising. Anson (1983) suggested change from 'Far Western' to 'Western' over time, and the ECA dates for Area B, Kirch's largest sample, range from 2952 BP-2352 BP, except for a wooden house post dating 3325 BP-3267 BP and perhaps representing use of old wood, and are thus scarcely relevant to questions of Lapita origins (dates from Kirch and Hunt 1988, recalibrated by Spriggs, cf. Spriggs 1990). There is obviously considerable horizontal stratigraphy within Kirch's various sites (note the much earlier date of 3472 BP for ECA, TP9) and some indication of when exactly 'Western' traits appear rather than reportage of their mere presence is necessary before 'these new results require that we rethink current notions of Lapita origins' (Kirch 1987:177). In fact, a more recent publication by Kirch (1988:334-5) provides information on the Eloaua ceramic sequence which would tend to confirm Anson's original hypotheses.

Sohano on Buka (Specht 1969), and as yet undated incised and relief styles from New Georgia, Choiseul and other Western Solomon Islands (Reeve 1989). The time frame for this would be a few centuries before 2000 BP, and it would not necessarily represent any major changes in material culture other than pottery because any putative new migrants would be of essentially the same culture and from the same area as the first migration of Austronesian speakers.[4] The unity of the symbolic system of this culture, as represented by classic Lapita decoration, had however broken down and increasing divergence was to become the mark of the cultural sequences of the Island Melanesian region to the ethnographic present.

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[4] The differences of course would be in terms of degree of physical admixture with pre-existing populations of the source area and degree of linguistic contact with non-Austronesian speaking populations. The initial Lapita spread out of the Bismarcks appears to have been too fast for much of either sort of contact to have occurred before Polynesia was reached.

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TWO SITES AT LASIGI, NEW IRELAND

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In the course of their travels in mid-1984 the members of the reconnaissance team of the Lapita Homeland Project stopped at the small village of Lasigi (3° 12'S, 151° 52'E) on the east coast of New Ireland, about 150 km southeast of Kavieng and 75 km northwest of Namatanai. They located a small number of mounded areas in the village, some of them associated with archaeological debris in the form of marine shells, small fragments of pottery and pieces of obsidian. The circumstances were similar to those already reported for two neighbouring localities: Lossu No. 1 village, some 35 km up the coast towards Kavieng, examined by Peter White in 1969 under the name of Lesu (White 1972; White and Downie 1980) and already visited by the reconnaissance team; and Pinikindu, about 20 km up the coast, investigated by Berle Clay in 1970-71 (Clay 1974).

None of the pottery from Lossu and Pinikindu was in the Lapita tradition, save for a single piece found on the surface at Lossu (White and Downie 1980:214), but White and Downie (1980:196, 197) had accepted as in likely primary association with the Lossu materials a radiocarbon date on charcoal (GaK-2441) of 2460±120 BP (uncalibrated, as all BP dates are in this paper), which made consideration of them relevant in the context of the Lapita project. Investigations at Lasigi were therefore put on its agenda and I was asked to undertake them.

THE SITES AND THEIR SETTING

Lasigi sits on both sides of the small Lasik River, with a northerly aspect to the sea across a wide reef platform exposed at low tide. The village consists of three wards, each with its own men's house. Lasigi proper is the traditional beach settlement, clustering around the river. Continuous with it on the west is Munuwai, also on the beach, and to the east is Wataton, situated on uplifted coral. This uplifted coral swings away from the coast behind the village and eventually encloses a broad coral sand plain, with Lasigi tucked into its northeastern corner.

Of the archaeological areas identified in the village by the reconnaissance team in 1984, two were investigated during June and early July of 1985, both of them immediately south of the Buluminski Highway, the main road linking Kavieng and Namatanai, which runs through the village about 100 m back from the beach.

1. *The Dori site* (ELS in the code of the Papua New Guinea site register) is a mounded area above the right bank of the Lasik River, in the angle formed by the river and the road. It is within the family compound of Klement Suabi and although much of it is under lawn, three buildings scattered on its margins and a trade store and adjacent cookhouse situated on one flank have disturbed it to the extent that its dimensions can only be approximately given: 10 m north-south and 20 m east-west. I have named it for a former bigman, prominent in the re-establishment of the village after its abandonment under the Japanese occupation of World War II. He is commemorated in a small concrete memorial erected by the Australian Administration by the roadside just east of Klement's compound.
2. *The Mission site* (ELT) is west of the river, just outside the western boundary of the Catholic Mission compound, on ground under coconuts and a cover of ferns. This boundary of the compound is a long linear mound resulting from regular sweeping of the sandy surface of the compound by village women, in the manner described by Clay (1974:4) at Pinikindu. It was along this mound that the reconnaissance team found pottery, obsidian and shell, but these are materials displaced by the sweeping from original positions in the Mission compound, where similar materials are visible over its swept surface and in a small garden behind. The Mission was established, as far as I could gather, after World War I. A German priest, whose name is still remembered, Father Leo, built a small

church destroyed during World War II, whose concrete footings are still preserved. Buildings of bush materials have replaced the original structure, but in another part of the compound.

The circumstances at the Mission compound showed that archaeological materials could be present where there was no surface mounding, and indeed White had reported the same thing at Lossu (White and Downie 1980:194). As a result work at Lasigi in 1985 began with a reconnaissance out from the Mission to both sides of the Buluminski highway. South of the highway my colleague, Andrée Rosenfeld, and I searched disturbed areas visible from pathways through fern and grass covered coconut plantations of the villagers, as far as the now abandoned airstrip of Kamerebak Plantation, immediately beyond which there is steeply rising coral limestone. At a dozen places we found shells, pottery and a little obsidian, in pig rootings and new coconut groves. North of the highway we inspected the cleared surface of the village in Lasigi and Munawai wards, as far as the beach, but found nothing. Indeed, the only place we found pottery north of the highway anywhere at Lasigi was close to the bridge opposite Klement's store and we were told that this resulted from bulldozing at the south side of the road to prepare the approach to the bridge. This suggested a period of coastal progradation during which pottery was not in use. There are no reports of pottery manufacture or use on New Ireland from the period of early European contact (beginning with Le Maire in 1616, but effectively from the late 19th century onwards).

THE SITES AND THEIR EXCAVATION

The Dori site was the scene of the major excavations, in the form of a 5 m x 1 m trench on the mound. At the shallower Mission site a 2 m x 1 m trench was dug for me towards the end of fieldwork by Andrée Rosenfeld, who had by then completed her own excavations at Buang Merabak, a cave in the coral limestone behind the village of Kanangusngus, a few kilometres east along the coast from Lasigi. About five times as much prehistoric deposit was excavated at the Dori as at the Mission site.

With exceptions noted later, all deposits were dug by trowel. At the Dori site there were visibly different layers in the build-up of the site and these were dug out separately, and normally in spits of up to 10 cm depth where they were thick. At the Mission site, where no such layering was present, the deposit was dug in spits and, because the site was shallower, these were 5 cm, more or less, in depth. At both sites the excavated ground was removed in plastic buckets, which were serially recorded and weighed before being passed through a 5 mm sieve. Sieves of finer mesh had gone astray during transport and could not be replaced either in Kavieng or Rabaul. We had some 1 mm fly-wire, which was used at the river for wet sieving at least one bucket of soil from each excavation level in each 1 m^2 of the excavation trenches. This was not a very efficient operation and there is no doubt that small pieces of bone and perhaps of charcoal and stone were lost.

Digging was done by the archaeologist in charge at each site and sieving and sorting by a team of four or five village men under the direction of one of their number, Mas Maratin. Excavation began with first light, about 6.30 am, and generally finished between 11 am and noon, as the hot New Ireland sun began to have its effect. In mid-afternoon, under the shade of the trees at the village plaza, Mas and one or two of the others would wash the morning's finds, dry them in the sun and divide them into categories of stone, bone, shell and the like for bagging. It was my job to prepare the plastic bags, recording on them the site name, the number of the square where the finds had been made, the number of the vertical spit and/or the name of the stratigraphic layer from which they had come and, as a cross-check, the serial number(s) of the bucket loads in which they had been recovered from the site.

The Dori site

The 5 m x 1 m trench was dug from the top of the mound south towards the low scarp of an old coral terrace. The trench began as a 1 m^2 test pit at the summit. This showed the mounded aspect of the site to be largely a post-European feature, formed by material from a prehistoric occupation layer containing small pieces of pottery and obsidian, as well as marine shells, mixed with glass and metal. This remodelling of the European period is Phase 5 in the history of the site and the deposits belonging to it over the 4 m of trench subsequently dug to the south were removed by spade.

Beneath this mounded upcast, 50 cm deep at the north end of the trench, the crest of the mound, and 20 cm at the south, near its margin, there are prehistoric deposits, resting on the clean coral sand of a coastal terrace, which slopes back very gently landwards. The matrix of this prehistoric site, 70 cm thick at the northern end of the trench and 50 cm at the south, is an accumulation of coral sand, building up as human activities proceeded on the spot. Two main units were identified, a lower one of varying thickness, 10-30 cm, called 'dirty coral sand' (archaeological Phase 2) and an upper 'dark-grey earth' (archaeological Phase 4), 30-50 cm thick on average. Both the 'dirty coral sand' and the 'dark-grey earth', but not the underlying coral sand or the overlying upcast, became brown in colour towards the southern end of the trench. This suggests the inwash of red-brown clay from the old coral terrace immediately south of the site, but laboratory analysis of samples brought back to Canberra suggests that if this were so, not much material was involved.

The history of that part of the site investigated by the trench can be divided into five phases.

Phase 1, as interpreted, is exclusively concerned with mortuary activities on and in the surface of the clean coral sand.

1. In the very northwest corner of the trench a grave was partially uncovered, most of it running into unexcavated ground where the superimposed deposits were thickest. The grave was filled with white coral sand, very difficult to distinguish from the sand into which it was dug, and only enough was removed to determine the disposition of the bones: the lower bones of the legs upright and the upper leg bones disappearing horizontally into the end wall of the trench, which makes it appear that the body had been buried in a seated or squatting position, back to the north. The lie of the sealing layer above the grave in the north wall of the trench raises the possibility that it was reopened to recover the skull or other parts of the upper skeleton. A few fragments of human bone, mainly from long bones, including some from the tibia, were found scattered in layers directly above the burial, particularly the first 30 cm, but not in the 1 m^2 immediately south. These are likely to derive from the burial and to witness some disturbance of it, whether from the re-opening of the grave suggested above or from the building of earth ovens during Phase 3. The stratigraphic complexities in this northern end of the trench could be resolved by further excavation, since most of the grave lies in unexcavated ground.

2. The recognition of the second instance of mortuary activity at the site was made not in the field, but in the workroom during study of the finds. What was noted at the time of excavation was the occurrence in one particular area towards the southern end of the trench of fragments of burnt bone in the bottom levels of the site, which in number and concentration contrasted with the situation everywhere else. Only back in the workroom, however, was it recognised that the burnt bone, where it could be identified, was human bone. My colleague, Dirk Spennemann, who made this discovery, found amongst the fragmented relics elements from all anatomical parts of the human body – head, trunk, upper and lower arms and legs, hands and feet – and the presence of at least two individuals. To him the evidence suggests cremation on the spot, with the remains, if not deliberately smashed, severely fragmented by subsequent activities at the site.

There was no concentration of charcoal in the area of the presumed cremations, such as might be expected. However, there was little charcoal in the site as a whole, which could be due to the type of fuel used. Moreover, there is the possibility of significant leaching in such porous sands (D. Gillieson, pers. comm.). On the other hand, there was an archaeological feature at the appropriate place and level, never satisfactorily understood, or even defined, during excavation, which may be associated with the crematory process. This is a depression, some of it still unexcavated beyond the west wall of the trench, which was infilled with a grey coral sand hard to distinguish from the clean coral sand surrounding it but quite distinct from the 'dirty coral sand' above, which in this part of the trench is brown in appearance. Under subsequent analysis, however, the grey sand gave no magnetic signal to suggest that it owed its colour to burning. The situation is further complicated by the fact that the depression and its grey sand infilling were cut through by a round vertical feature, 15 cm in diameter, looking just like a posthole. Well over half the fragments of burnt human bone were found in the dark grey infilling of this 'posthole' feature. The same is true of the burnt fragments of unidentifiable bone which are responsible for the high values shown in Figure 1 for Phase 1 land fauna at the Dori mound. These are without doubt human.

What the relationship is of the 'posthole' to the depression and of either to the crematory process is obscure, but could be resolved by renewed excavation. However, the depression, the 'posthole' and the cremations seem to represent activities earlier than and different from those associated with the layer of 'dirty coral sand' above. This is why they have been accorded independent status, with the burial at the other end of the trench, as Phase 1. It is true that there are items relating to more mundane activities, like oven stones and fragments of pottery, here assigned to Phase 1

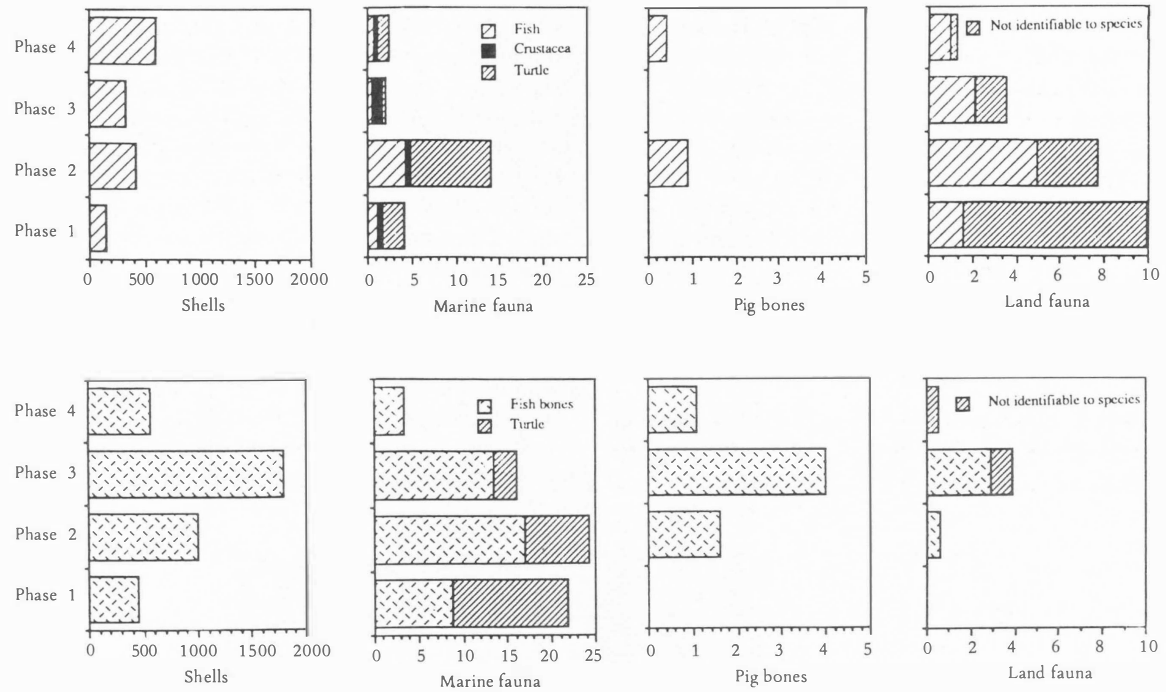

Figure 1 *Densities of shell and bone in g per 100 kg of excavated ground at Dori (upper) and Mission (lower).*

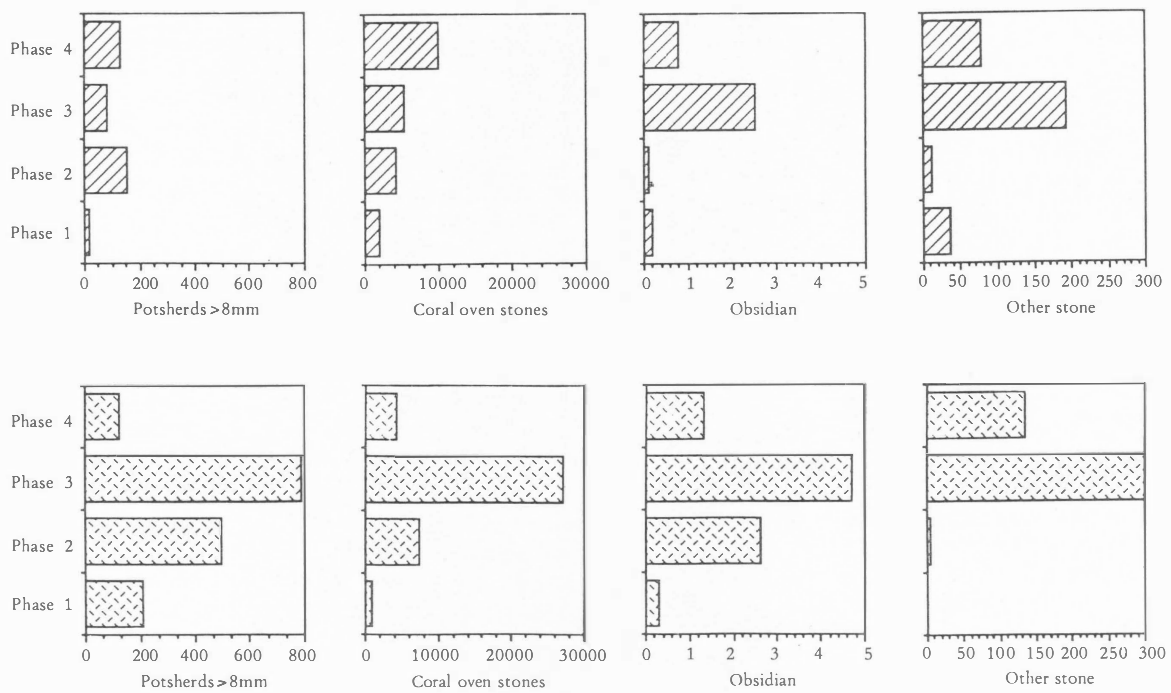

Figure 2 *Densities of pottery and stone in g per 100 kg of excavated ground at Dori (upper) and Mission (lower).*

(Figs 1 and 2). These are few in number and may be intrusive from the 'dirty coral sand' of Phase 2, where they are well represented.

Phase 2 is an occupation layer, by the character and quantity of its contents (Figs 1 and 2).

Too hasty excavation in the final week of the 1985 season of the 'dirty coral sand' which represents the phase, failed to record the distribution of the debris through the occupation layer, which was substantially thicker (up to 30 cm as against up to 15 cm) in the southern half of the trench than in the northern part where it was first investigated. As a result, the relationship between Phases 1 and 2 cannot be specified with certainty. I have already suggested that there may have been some intrusion of Phase 2 materials into Phase 1. The reverse process certainly took place, with the incorporation in 'dirty coral sand' of fragments of burnt human bone from the Phase 1 crematorium in the southern part of the trench and, as already mentioned, that of unburnt human bone from the Phase 1 burial at the northern end.

Phase 3 was a time when digging took place on the surface of the 'dirty coral sand', which penetrated through this into the underlying clean coral sand and disturbed and displaced some of the human remains of Phase 1 in the process. In the middle part of the trench against the west wall, and further south, half beneath the east wall, two large features were found, nearly 1 m deep, interpreted as postholes (Plate 1). The steep-walled lower shafts of both are oval in plan, along the same roughly northwest-southeast axis, and nearly 60 cm in length and 40 cm in width. The more southerly shaft has a double base, suggesting that it is a conflation of two holes of different age, though there was no firm evidence of this in the profile. If this were true of the other shaft as well, it would account for the large size and oval shape of both features. Some 90 cm apart, they straddled the area where the

Plate 1 *Dori site, Phase 3 structures at the surface of the 'dirty coral sand' of Phase 2. View from NW.*

cremated remains were concentrated and marginally penetrated it. They were linked by a shallow channel, which was presumably associated with the substantial structure indicated by the two postholes which it connected. There were other structural features at the same level in the same part of the trench, but too little of them was present within the confines of the excavation for their nature and connections to be clear (Plate 1). At the northern end of the trench oven pits were dug above and into the grave.

The question arises as to the relationship between the two large postholes and associated structures with the occupation of Phase 2. Stratigraphically the situation is quite clear: the structures were dug from the surface of 'dirty coral sand' and were empty, or at least by no means full up, when deposition of the 'dark-grey earth' of Phase 4 began. It is possible to argue, however, that the postholes are contemporary with at least the later part of the Phase 2 occupation. Digging them must have produced a great amount of spoil from the 'dirty coral sand' and particularly the clean coral sand underneath. This spoil presumably was disposed of in the vicinity, on top of and increasingly mixed with the 'dirty coral sand' of the Phase 2 occupation. Several things would be neatly accounted for, were this the case: the thicker representation of the 'dirty coral sand' horizon in the southern than in the northern part of the trench; the localised mounding of that horizon immediately south of the large posthole in the west wall; and the presence of at least some of the fragments of burnt human bone displaced from the Phase 1 cremation level.

The process of excavation itself should have provided better evidence to indicate whether the Phase 3 structures were chronologically and functionally associated with, or distinct from, the Phase 2 occupation. However, as I have admitted, the investigation of the 'dirty coral sand' in the southern half of the trench was rushed and the layer was taken out as a single stratigraphic unit, despite its appreciably thickened expression there. There is unexcavated ground, however, especially west of the trench, where more careful digging could address the question, now that it has been identified. As it is, there is some evidence from radiocarbon dating that bears on the matter.

I have mentioned that the structural features of Phase 3 were substantially unfilled when Phase 4 began at the site: the 'dark-grey earth'

Table 1 *Radiocarbon dates. Calendar years BP calculated using the Calib computer program, version 2.0, of Stuiver and Reimer (1986), with their generalised oceanic correction applied to the dates on marine shell, - R = 0. The first line gives the age range in years BP at one standard deviation and the intercept (or, in one Lossu case, the intercepts) with the calibration curve in brackets.*

Site and Phase	Laboratory Number	Material: Charcoal (M = modern)	Material: Shell	Calendar years BP (age range BC,AD)
Dori 4 upper				
	ANU-5522	99.8 ± 1.0%M		
	ANU-5524	114.0 ± 1.4%M		
Dori 4 lower				
	ANU-5525	109.5 ± 2.6%M		
	ANU-5851		2370±80	2081(1985)1886 (131 BC-AD 64)
Dori 3 posthole fill				
	ANU-5523	116.5 ± 1.8%M		
	ANU-5850		2870±80	2730(2688)2492 (780-542 BC)
Mission 3 top				
	ANU-5527	101.4 ± 2.0%M		
	ANU-5528	98.8 ± 1.7%M		
	ANU-5852		2370±80	2081(1985)1886 (131 BC-AD 64)
Lossu base at Mound V				
	GaK-2441		2460±120	2749(2704, 2647,2486)2349 (799-399 BC)
Lossu base at Mound VI				
	GaK-2439		1600±70	1560(1521)1406 (AD 390-544)

associated with that phase is continuous with and very similar to the material filling at least the upper part of the shafts of the large postholes, as well as the minor features on the top of 'dirty coral sand', including both the channel linking the two big postholes and the ovens at the north end of the trench. There is always the possibility with dug features which penetrate underlying layers that older material will find its way into the cavities they provide, either at the time of construction or after abandonment, and thus become incorporated in the infilling. By these means fragments of burnt human bone ultimately from the crematorium of Phase 1 found their ways into the fill of the two large postholes and of the channel which connects them. Similarly, I can only conclude that the marine shells which provided one of the two useful radiocarbon dates from the Dori site came into the fill of the more northerly of the two large postholes from an older source. This is because its radiocarbon age of 2870±80 BP (ANU-5850; see Table 1 for calibration into calendar years) is appreciably older than that of the other dated sample of marine shells, 2370±80 BP (ANU-5851; see Table 1), from the lower part of the Phase 4 deposit, which, as we have seen, forms an appreciable component of the posthole infilling. Whether the time gap represented here is between Phase 3 and Phase 2 or between Phase 3 and Phase 1 is uncertain, since we do not know the original provenance of the dated shells.

It is necessary to point out at this juncture that the archaeological finds entered for Phase 3 in Figures 1 and 2 in fact come from the infilling of the constructional features under discussion. In the light of what has been said above, they must be looked upon as being a mixture of materials from a number of phases, early Phase 4, Phase 2, Phase 1 and, no doubt, from the constructional Phase 3 itself.

Phase 4 constitutes the greatest volume of the prehistoric site. It is an internally undifferentiated formation of 'dark grey earth' compact though friable, and because of its compactness, distinctive in comparison with the looser sand of underlying formations, although it is basically sand. Figures 1 and 2 show that while the range of materials it contains is identical to that in the Phase 2 occupation horizon, it is distinguished by a greater proportion of coral cooking stones. In the light of this, and of the character of the matrix which contains them, it is possible that we are dealing with the rake-out of ovens. Charcoal is scarce, as everywhere on this site, and I can only again suggest that this may be due to the type of fuel used. Certainly, soil analysis gave clear evidence of burning in these levels.

The top of the deposits of Phase 4 formed the ground surface at the time of European arrival. All the indications are that it had been the ground surface for a considerable time before. The only date we have for Phase 4, 2370±80 BP (ANU-5851; see Table 1), is on marine shells from its lower levels. However, Phase 4 deposits are uniform throughout, in character and contents, including pottery, which, as mentioned before, was nowhere in manufacture or use on New Ireland at European arrival. In addition, there is a concentration of stone and charcoal/charred plant remains (though, interestingly, not of potsherds) in the top spit (up to 10 cm deep) of Phase 4, such as could be expected on a long-exposed surface: 59 of the 111 pieces of stone belonging to Phase 4 (46% by weight) occurred in this spit, and 68% of the 158.75 g of charcoal/charred plant remains found in the entire excavation, about which more will be said below.

Phase 5 is the episode of post-contact mounding, to which reference has been made. Material was heaped on top of the prehistoric site to a depth of 50 cm at the north end of the trench, the crest of the mound, and 20 cm at the south, near its margin. The spoil was derived from prehistoric deposits, perhaps north of the trench where there is a steep slope, mixed with rubbish containing items of European manufacture, including metal and glass. Like other mounded areas in the village, this site is associated with the residences of big-men of the European period. Between the two World Wars there was Lilikas and Dori himself, who was the son of Lilikas' sister. On the slope behind and to one side of the area archaeologically named for him, Dori built the rambling European-style house which is now lived in by Mentong, his clan brother, who took his place after his death. Mentong, however, and also my informant Malakot, the son of Lilikas' brother, Wasombo, who gave the land for the Catholic Mission, have both passed their authority to Paul Dori, who was our sponsor in the village.

The Mission Site

In the 2 m x 1 m trench dug roughly east-west immediately west of the Mission compound, the deposits were fairly unconsolidated coral sand, grading from topsoil through brownish sand to clean sand, which first appeared at about 35 cm depth. The maximum depth of excavation was about 45 cm, reached in a 50 cm wide sounding across the eastern end of the trench. Elsewhere digging ceased at about 35-40 cm.

In the absence of archaeological stratigraphy, internal division of the site for purposes of analysis had to rely on the vertical distribution of the archaeological materials. Fortunately this proved straightforward and the following four phases can be proposed, from latest to earliest: Phase 4, Spits 1-2; Phase 3, Spits 3-5; Phase 2, Spits 6-7; and Phase 1, Spits 8-9, only fully excavated in the eastern part of the trench. Phase 3, from 7.5-10 cm to 20-25 cm below the surface, is defined by the greatest density of finds (cf. Figs 1 and 2). To what extent the earliest and latest Phases, 1 and 4, are real or the product of displacement of materials from the main horizons is difficult to say.

Marine shells from Spit 3, the top spit of Phase 3, have returned a date of 2370±80 BP (ANU-5852; see Table 1). This determination is identical to ANU-5851 for the lower part of Phase 4 at the Dori site.

THE MATERIALS RECOVERED

By and large the same sorts of material were recovered at the two sites and in roughly the same proportions, but densities are much greater at the Mission site (Figs 1 and 2). Marine fauna (other than shells) is noticeably better represented in the lower levels of both sites. A major difference is the virtual absence of human bone at the Mission site, compared with the Dori site. Since at this site there was a burial and two cremations in the bottom level, from which the scattering of human bone in higher levels could all derive, the scarcity of human bone at the Mission site (one fragment from the lowest spit of Phase 3, two from the lower spit of Phase 2, all unburnt) means no more than that the small trench failed to hit a mortuary area, though the few bones present may indicate that there is one somewhere close by.

Lumps of coral are by far the commonest objects: they were counted and weighed in the field and not retained. They are presumed to be the remnants of oven stones, for which coral lumps are used today. By contrast, charcoal is rare, if indeed, as we shall see, originally present at all. Despite the small size of the Mission site excavation, there is a striking contrast between the occurrence of coral cooking stones in the two 1 m^2 squares making up the 2 m trench at the site. In the three Phases, 2-4, and in all the spits contained within them, there is a significantly greater weight of cooking stones in the western square. The meaning of this is not clear, except to suggest continuity in site use over the period of occupation.

After coral lumps, the most frequent items at both sites are marine food shells, which have yet to be studied in detail; there is a wide range of species, such as is expectable for a tropical coastal location. Bone from animals consumed by people is not plentiful; it is also highly fragmented and much of it unidentifiable to more than type. It is to be expected that small bones and bone fragments were lost because of the sieve size used (5 mm). The salt water component of the fauna comprises turtles, crustacea (but not at the Mission site) and fish, the latter belonging, where they can be identified, to inshore species. Among the mammal bones, there is pig and, at Dori, one fragment each of cuscus (*Phalanger orientalis*), fruit bat (*Pteropus* sp.) and rat. Tim Flannery identifies this last, on the preserved distal fragment of a left humerus, as a very large *Rattus praetor*, a commensal species (Flannery et al. 1988:93). Finally, there are five fragments of bird bone at Dori and one at the Mission site.

The remains of charred plants, as of charcoal, are concentrated in the top level of Phase 4 at Dori, where they have been interpreted, with other evidence, as an indication that this was the ground surface over a prolonged period. In the body of both sites these organic materials are meagre and minute, though Doug Yen recognises *Cocos nucifera* (coconut, at both sites) and *Canarium indicum* (at Dori) amongst them. However, there are problems about the in situ status of all such finds, both of charred plants and wood charcoal, in view of the modern C^{14} values returned by a number of samples from both sites (Table 1). Crab activity (Specht 1985) is a likely agency for the contamination of older levels with newer materials, such as the odd piece of glass or metal occasionally found in the bowels of the Dori deposit. Crabs were intermittently active on site during the excavations, leaving hollow shafts 5-7 cm in diameter deep into the deposit, and fossil holes were recognised in the site matrix by their softer and sometimes differently coloured fill.

Pottery (Plates 2 and 3) is a prominent item among the finds, but extremely fragmented and abraded. Its detailed analysis remains to be done. There are 126 rim sherds and 63 decorated body sherds at the Dori site, and 114 and 41 at the Mission site. It appears that the material can be adequately included, both as regards decoration and rim form, within the White/Downie (1980) categories from Lossu (Lesu), although the Lasigi range in both is narrower. Body decoration

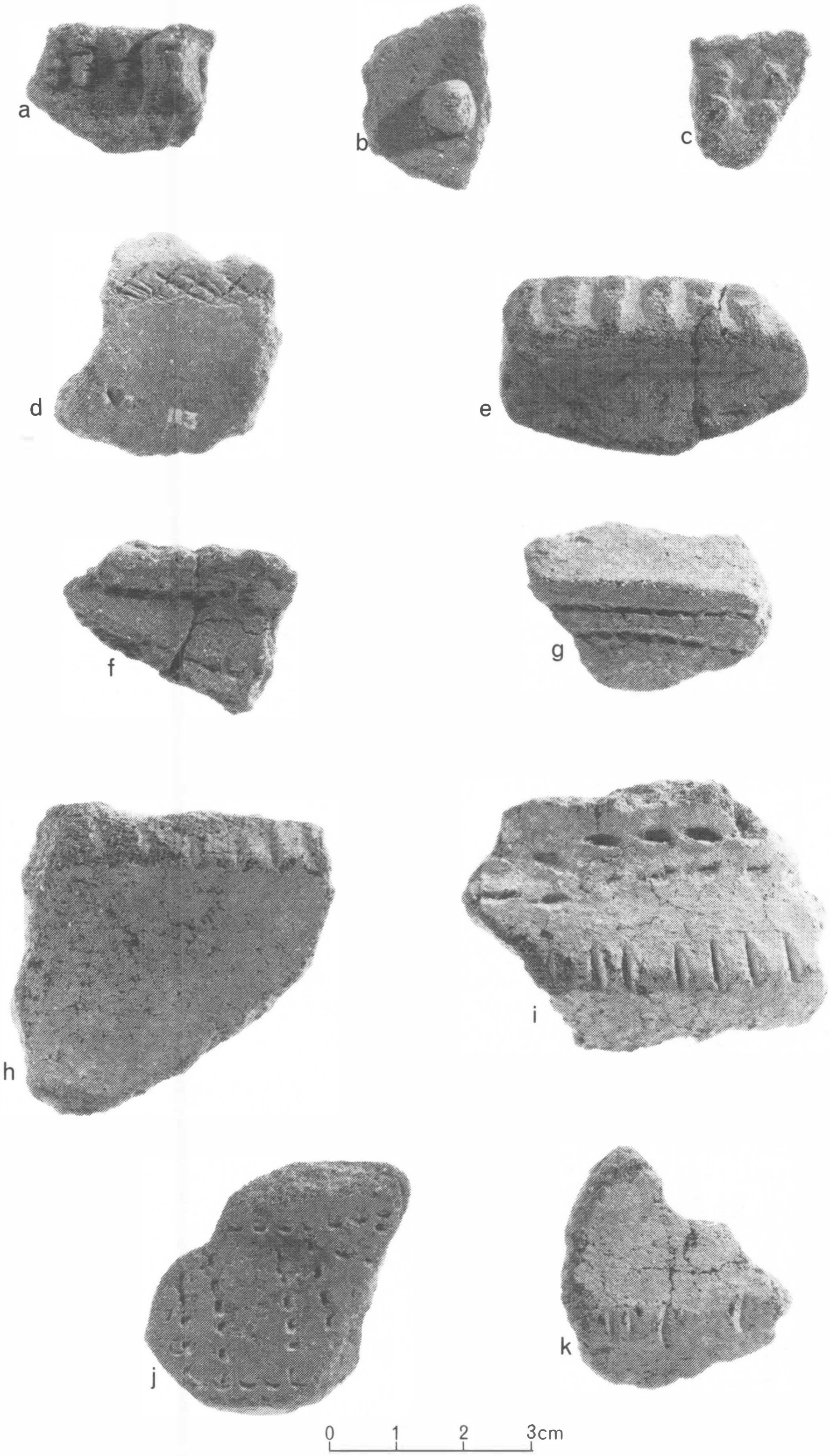

Plate 2 *Decorated sherds: a and b)* appliqué*; c) nubbins poked from inside of notched rim; d) fine incision inside strongly everted rim; e) stick impressed; f and g) dentate-stamping; h) parallel stick impressions; j) small tool impressions above carination; i and k) fingernail impressions on carination and (i) above. Provenance: a) Mission Phase 3; b) Lasigi surface; d and j) Dori Phase 2; rest) Dori Phase 4.*

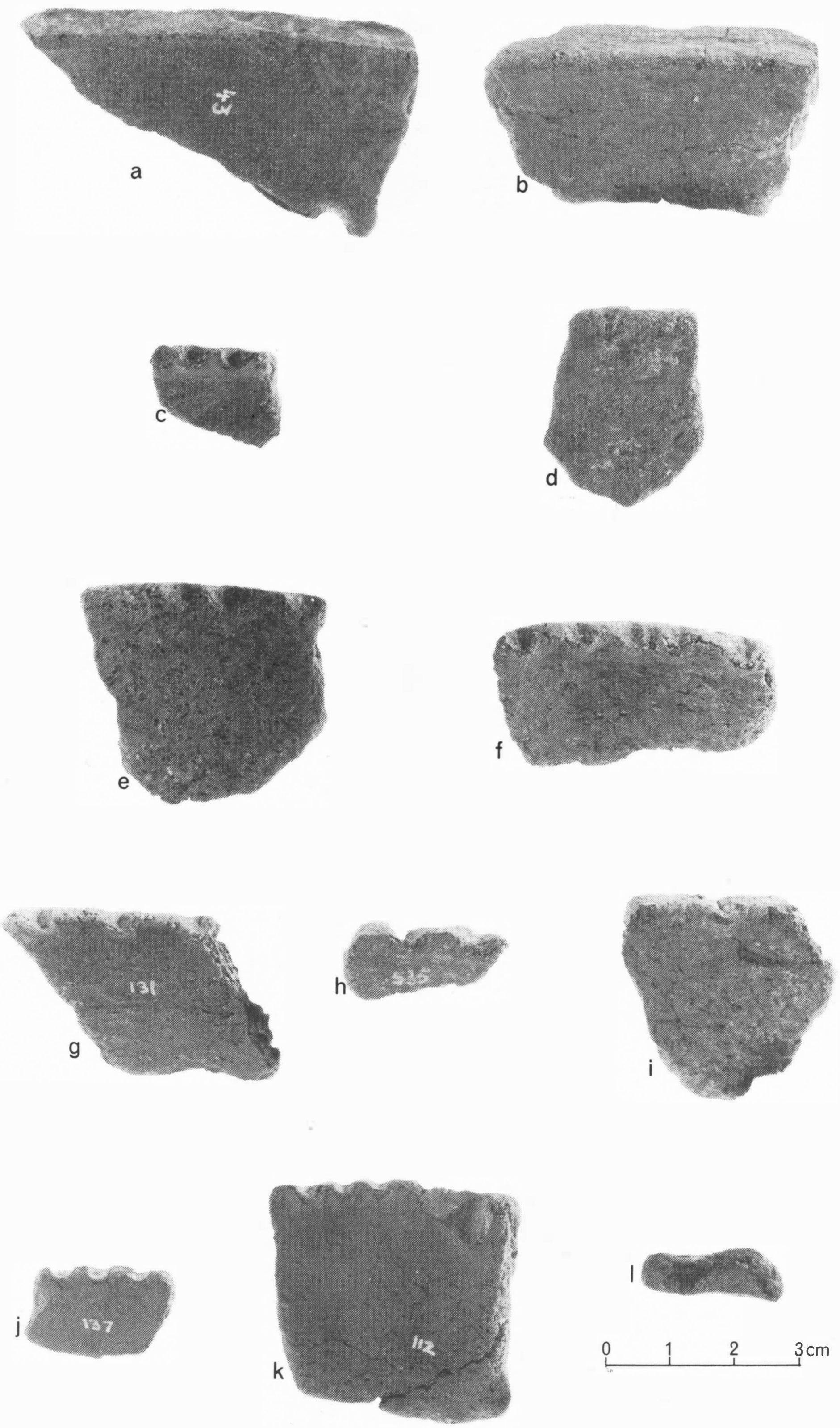

Plate 3 *Rim sherds: a and b) outer aspect of flat lipped, everted rims, plain; c-k) stick impressions on flat lips: c and d) outer edge of lip; e) inner edge of lip; f and g) inner edge, but extending across lip; h-k) inner aspects of rims impressed evenly across lip, i) having a single impression, k) a thumb impression at right; l) top view of rim thumb impressed to form a sinuous line. Provenance: l) Mission Phase 3; c, f and k), Dori Phase 2; a, h and i) Dori Phase 3; rest) Dori Phase 4.*

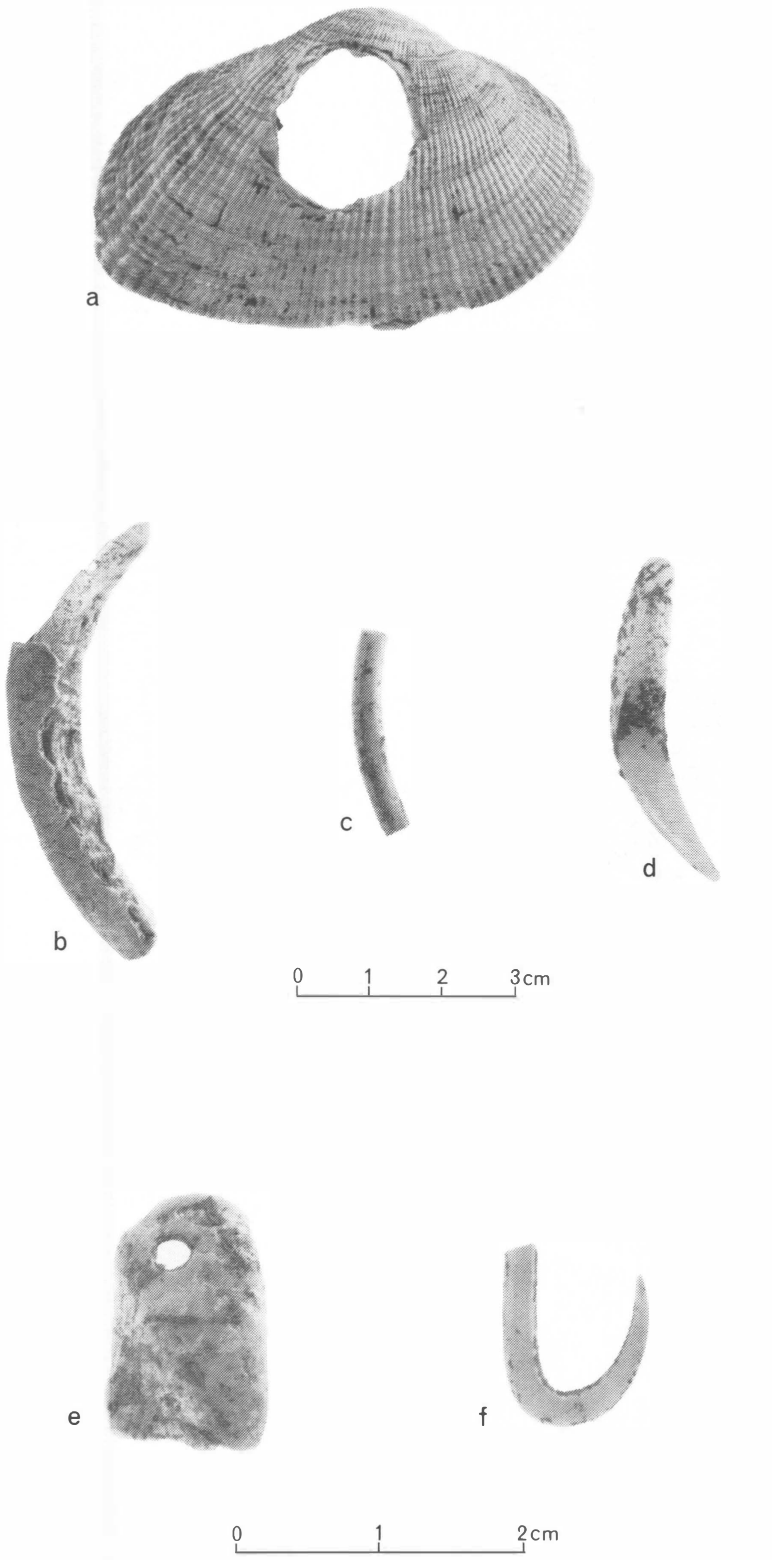

Plate 4 *Shell artefacts: a) perforated valve of* Asaphis violascens*; b) drilled segment,* Trochus niloticus, *probably early stage of armring manufacture; c) fragment of armring of* Trochus niloticus*; d) probably fragment of unfinished armring of* ?Trochus niloticus*; e) perforated fragment of ornament of* Modiolus *sp.; f) one-piece fishhook of unidentifiable shell. Provenance: a, c and d) Dori Phase 4; b) redeposited in Dori Phase 5; e and f) Mission Phase 2.*

comprises appliqué (strips, nubbins and pellets), fingernail impressions, a little incision and, on a few sherds, simple lines in dentate-stamping (Plate 2). The commonest decoration is on the rim, where wide (thumb?) and narrow (stick?) impressions applied to the top or inner or outer angles of the lip give differing effects of waviness, crenellation and notching (Plate 3c-i). These modifications compound the difficulty inherent in small sherd size of orienting rims to their position on the original pot, but the most prominent class appears to be a straight, everted rim with flat lip, of various thicknesses (Plate 3a-b). It will be difficult to get much idea about overall pot shapes, but there are no definite flat bases and few shoulders. There are a few fragments of handles and some indication of lugs. The technique of slab building and the use of paddle and anvil are in evidence. The pottery is heavily tempered.

Obsidian, present in low numbers and small pieces, comes from Admiralty Islands sources, with the exception of five Talasea (New Britain) pieces (out of 65) at the Dori site. Stone other than obsidian and coral is not common and its origin is almost exclusively from pebbles of volcanic rock, such as can be found in the bed of the river at Kimadan, a few kilometres up the coast.

A few flakes of ground stone axes were found. Plate 4 illustrates a number of shell artefacts, including pieces of armrings, mainly of *Trochus*, and one of two small fishhooks from the Mission site.

DISCUSSION

Table 1 sets out the radiocarbon dates from Lasigi and gives their conversion into calendar years. The two excavated sites represent occupation during the final millennium BC. The older date from the Dori site (ANU-5850, on marine shells) is likely to mark an early stage of settlement within the first half of the millennium. It is in good agreement with the date (on charcoal) for the lowest level of occupation at Mound V of the related site complex of Lossu (GaK-2441, White and Downie 1980:194, 196-7; see Table 1 here). Two other dates (on marine shells) show that the occupation represented by the Lasigi sites went on up to or slightly beyond the birth of Christ. They are ANU-5851, dating an early part of the latest prehistoric phase (Phase 4) at the Dori site, and ANU-5852, dating the top 5 cm spit of Phase 3 at the Mission, which begins 7.5-10 cm below present ground surface. They are likely to register a late stage in the particular episode of occupation investigated. There is a date (on charcoal) from Mound VI at Lossu which might indicate that prehistoric occupation with ceramics persisted as late as the middle of the first millennium AD (GaK-2439, see Table 1). However, by implication, White and Downie (1980:197) have doubts about its acceptability. Its provenance is given as basal in one account (White 1972:309) and 60-80 cm below the surface of a 1 m deep excavation in another (White and Downie 1980:194, 196), while the information supplied about the excavation of Mound VI (White and Downie 1980: 194) is not detailed enough to come to any decision about site integrity. However, there is some possibility, discussed below, that the date in question is acceptable on other grounds. We may also note that the four sherds of pottery found at the inland rockshelter of Panakiwuk were in deposits dated after 1600 BP (Allen et al. 1989:554; Marshall and Allen this volume). The relevant date would seem to be ANU-5531, 1630±130 BP (Allen et al. 1988:Table 1), which calibrates to 1700(1534)1390 BP.

The two settlements at Lasigi took place on coral sand at locations which at the time are likely to have been at the shore. I have mentioned before that a 100 m stretch of land now separates them from the coast and that no pottery was found in situ on it. This situation must result from coastal progradation during the post-ceramic period on New Ireland, possibly as a result of local uplift. The surface at the Mission site and the pre-European surface at Dori are just over 1 m higher than the ground surface in the village just back from the beach in Munuwai ward.

Because of the presence of pig, I presume the economy was horticultural, though there is no direct evidence. The orientation is coastal (reef molluscs, fish, turtles). For Lossu, White and Downie (1980:215) reconstruct a similar picture and contrast it with that presented by the inland shelter of Balof (now Balof 1) with its emphasis on hunting.

At this stage in the analysis and publication of the data made available as a result of the Lapita Homeland Project, it is difficult to fit the New Ireland coastal sites into the wider picture. On the dates available (Gosden et al. 1989:Tables 1 and 2), they collectively exhibit a sometimes appreciable overlap with 'classic' Lapita manifestations in the region – locality B of site ECA in Mussau (Gosden et al. 1989:579; Kirch 1987: 167-72; Kirch et al. this volume), Spriggs' Lapita

Phase at site DGD/2 on Nissan (Gosden et al. 1989:581; Spriggs this volume), sites FNZ and FOJ in the Arawes (Gosden et al. 1989:584) and sites SDI and SAC on Watom (Gosden et al. 1989:582; Green and Anson 1987:123-5; Green and Anson this volume) – and run parallel with later assemblages in these localities – locality C of site ECA and the EKQ rockshelter in Mussau (Gosden et al. 1989:571, 579; Kirch et al. this volume), Spriggs' Yomining Phase at sites DGD/2 and DGW on Nissan (Gosden et al. 1989:569, 581; Spriggs this volume), sites FNY and FNZ in the Arawes (Gosden et al. 1989: 583-4) and the two Watom sites (Gosden et al. 1989:582; Green and Anson 1987:123-5; Green and Anson this volume).

Some connections have been suggested between the ceramics of these later assemblages and those from Lossu and Lasigi (e.g. Gosden et al. 1989:571, 582; Green and Anson this volume; Kirch et al. this volume). However, there is need for detailed comparison of all of them with each other and also with Lapita, which is generally present in the same localities, and sometimes, as the listing in the previous paragraph makes clear, in the same stratigraphic sequences, as the later ceramics. Moreover, Lapita itself is emerging as a complex phenomenon. To the regional variation in Lapita decoration suggested by Anson (1986; cf. Kirch et al. 1987; Anson 1987), the size, internal differentiation and long life of Lapita settlements revealed by recent work bring the additional complications of spatial variability and chronological change in ceramic expression at the local level (Gosden et al. 1989: 571-2, 573). What Lapita sites and those with the later ceramics do have in common is a number of items – pigs, a shell industry including one-piece fishhooks and ornaments, obsidian from Admiralty Islands (in addition to New Britain) sources, possibly the Polynesian rat (*Rattus exulans*) – which on present evidence make their first appearance in the region with Lapita pottery (cf. Allen et al. 1989:554-5, 556 and Table 2; Gosden et al. 1989:569). There is one qualification to this statement. Pigs and shell artefacts, but not Admiralty Islands obsidian, appear without pottery at a Nissan Island site (DFF) excavated by Spriggs, in levels beneath Lapita pottery but contemporary with Lapita in Mussau (Gosden et al. 1989:568-9, 581; Spriggs this volume). Spriggs calls this Nissan manifestation the Halika Phase and describes it as Lapita without pots: lower levels of site DGW are also attributed to this phase (Gosden et al. 1989:568-9, 581; Spriggs this volume).

In contrast to the situation at Nissan, the Arawes, Watom and Mussau, there are no known Lapita sites on the New Ireland mainland with which to compare the younger ceramic finds from Lasigi, Lossu and Pinikindu. There is a single sherd of classic Lapita from the surface of Mound V at Lossu (White and Downie 1980: 214) and a part vessel with incised decoration of Lapita type from Lamau on the west coast (Gosden et al. 1989:571), for which Gorecki et al. (this volume) provide a full description and a date of 1680±200 BP (ANU-5518), which calibrates to 1830(1569)1350 BP. The excavations at Lasigi, however, have produced the pigs, shell industry and Admiralty Islands obsidian associated with the Lapita sites of the region. None of these things occurs in the sequences from New Ireland rockshelters before the appearance of pottery there, in the form of small numbers of undiagnostic sherds (Allen and White 1989: 139 for their undiagnostic character), at and after 3000 BP (Allen et al. 1989:554-5, 556 and Table 2 for pottery, Admiralty Islands obsidian and pigs; Downie and White 1978:774, 776, 779, 784 for distributions of pig, shell armrings, pottery and Admiralty Islands obsidian at Balof 1). The 3000 BP date for the appearance of pottery, Admiralty obsidian and pigs refers to Balof 2 and is presumably based on 3120±190 BP (ANU-4972) (Allen et al. 1988:Table 1), which calibrates to 3559(3364)3080 BP. Allen et al. (1989: 556 and Table 2) accept *Rattus exulans* as another innovation in the rockshelter sequence at this time. Its absence from faunal inventories at the coastal sites may well be due to the sieve size used at excavations there, 5 mm at Lasigi, quarter inch (6.35 mm) at Lossu (White and Downie 1980:194) (see Flannery et al. 1988:91 on this point).

At Balof 1 there were 12 pieces of obsidian from levels before the appearance of pottery, all from New Britain, and 33 pieces afterwards, 13 from New Britain, 20 from the Admiralty Islands (Downie and White 1978:Table 12; on p.784 the total is given as 46). At Balof 2 17 pieces of obsidian were recovered from the deposit as far back as the early mid-Holocene, of which the two from the Admiralties were associated with the pottery at the site, while nine out of the ten pieces of obsidian at Panakiwuk came from the ceramic layers, with seven of the nine being from the Admiralties (Marshall and Allen this volume). At the Lasigi sites, as we have seen, the obsidian is overwhelmingly from the Admiralty Islands and, with few exceptions, from Lou Island. Ambrose (1978:331) reports 17 out of 20 analysed specimens from Mound V at Lossu as coming from the same sources.

Of interest in this connection are two broken obsidian artefacts of triangular cross-section from Mound VI at Lossu (White and Downie 1980: Fig. 7). Antcliff (1988:38) identifies these as fragments of points of the type excavated by Ambrose at the Emsin and Pisik School sites (GEB and GBC) on the obsidian producing island of Lou and notes that the Lossu occurrences constitute the only report of similar points outside the Admiralty Group. Both Lou sites are buried by a volcanic ash known as Rei tephra, which is dated around 1650 BP (Ambrose 1988: 484 and Fig. 2; Gosden et al. 1989:578), while there is a direct radiocarbon date for the Pisik School site of 1720±100 BP (ANU-2018) (Gosden et al. 1989:566), which calibrates to 1740(1657)1530 BP. These dates are close to that for the lower part of Mound VI at Lossu, in the base of which one of the obsidian points (White and Downie 1980:Fig. 7a, Table 1) was found. It may be therefore that the date in question, 1600±70 BP (GaK-2439) (see Table 1 here for calibration), is more reliable than White and Downie (1980:197) allow (cf. Antcliff 1988: 38). If so, this means that only the upper levels of the mounded site are disturbed and a similar case is argued for Lossu Mound V below. We must note, however, that triangular sectioned points of obsidian are also known from an earlier site on Lou Island than GEB and GBC, the Sasi site (GDY) (Ambrose 1988:484). The date for the relevant occupation horizon here is argued by Ambrose (1988:489; Gosden et al. 1989:578) to be around 2100 BP (calibrated), which would allow for an earlier *terminus post quem* for the appearance of Admiralty Islands points at Lossu.

I catalogue these indications of connections between New Ireland and the Admiralties during the ceramic period represented at the Lasigi and Lossu sites, because a recent discussion of Lossu-type ceramics has made reference to Admiralty Islands pottery (Gosden et al. 1989:570, 571), in the context of an applied and incised decorative style with claimed parallels as far away as Vanuatu and New Caledonia (Gosden et al. 1989: 570, 577; cf. White and Downie 1980:209, 212, 214-5; Kennedy 1982:24, 26). This points to another possibility which the analysis of the Lasigi ceramics must take into consideration.

So far I have emphasised the 'new' elements represented by the Lasigi assemblages when compared to the long record of earlier occupation of New Ireland (Allen et al. 1988; Allen et al. 1989), particularly pottery but also the pigs, shell industry and Admiralty Islands obsidian associated with it. As indicated above, the appearance of these elements at the rockshelters roughly parallels the occupation of the coastal sites. This confronts us with a complex of problems: how the innovations are to be interpreted in the context of sequences of occupation at inland rockshelters where they appear as a late manifestation; what they mean in the context of sites newly founded on the beach; and what they imply about the relationship between the inland and the coastal sites. To say this, of course, is to do no more than specify for the New Ireland situation the issue which is fundamental to the Lapita Homeland Project as a whole: whether the innovations were acquired by and absorbed into an existing system or introduced as part of a new system by incoming foreigners (Allen and White 1989:142, with reference to pottery). On this matter the Lasigi excavations have, by themselves, no critical evidence to offer.

Nevertheless, the Lasigi sites have importance for the study of developments in the region after the Lapita phenomenon, whatever it proves to be, had become established and widespread. In addition, the Dori site has a number of uncommon features which promise interesting results from renewed excavation, in the light of which the excavated trench was specially protected during infilling. On the one hand there is the concentration of human remains, both inhumed and cremated, on the other there are the structural remains. The most conspicuous of these are the two large shafts, 60 cm x 40 cm in plan and nearly 1 m deep, which have been interpreted as postholes. Even if, as has been suggested, the dimensions are exaggerated by redigging, they indicate a massive structure, if they are indeed postholes and not pits of quite different function.

I conclude on quite a different topic, that of mounded sites on New Ireland. For the Pinikindu peninsula, Clay (1974:2-5) records prehistoric beach-side middens, up to 1 m in height, and larger and more definite interior midden mounds, up to 2 m in height and variable in form – linear, semi-circular, circular and doughnut shaped. These latter are associated with named hamlets and have been formed by the constant sweeping of living surfaces, as with the linear mound forming the western boundary of the Catholic Mission at Lasigi.

For Lossu, White and Downie (1980:193-4) note a number of linear and oval mounds rising above the generally flat ground, with Mound V the largest, rising in places 2 m above the surrounding level. It is in fact a most impressive structure, in size and form, with flat top and sloping sides. Excavations here (White and Downie 1980:194) found mixed European and prehistoric materials down to a depth of 2 m, approximately to the level of the surrounding ground, below which the prehistoric materials went down a further 2 m, or 4 m below the mound surface. The situation is very reminiscent of the Dori site at Lasigi, with spoil from a prehistoric occupation layer used during the colonial period to make a mound. As argued above, I suspect the same for Mound VI at Lossu.

The Dori mound is not as deliberately shaped as Mound V at Lossu, but there is another mounded site at Lasigi, between the Catholic Mission and the river, which appeared more formed under its rather heavy cover of scrub. As already noted, these are sites associated with bigmen of the village before and after World War I. It is possible that Mound V at Lossu is a similar phenomenon and that all of them exemplify externally inspired developments within New Ireland society under colonial rule.

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LEARNING ABOUT LAPITA IN THE BISMARCK ARCHIPELAGO

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The last six years of work in the Bismarck Archipelago have altered our knowledge of the data from that region out of all recognition. We now urgently need new frameworks of interpretation in order to allow us to take stock of the data we have and to collect more evidence in a concerted and planned fashion. This paper focuses on the Lapita period, after making a few brief comments on the Pleistocene/early Holocene, then looks at what insights we can glean from our new results and lays out a framework for further interpreting the Lapita period. Rather than diving straight into the intricacies of the data, I want to work from a more general and abstract starting point, looking at broad frameworks of interpretation. Then the problems of scale will be considered, before finally tackling some of the more detailed features of the data sets we have recovered.

OLD MELANESIA

We are just starting to come to terms with some unusual facts about the Pleistocene period in island Melanesia. Although it has long been apparent that the islands of southeast Asia were colonised well back in the Pleistocene, it came as something of a surprise that the same might also be true of islands further out into the Pacific. Considered as a whole, the islands from southeast Asia to the Solomon chain have a number of similarities and differences. They are all united by the fact that they are tropical, but are divided by differences of fauna and, to a lesser extent, flora. The overall trend, of course, is towards increased impoverishment in terms of plants and animals and greater isolation from large land masses as one moves from east to west.

Given the increasing evidence for the existence of maritime skills back at least 30,000 BP (Irwin this volume) there seem to be no physical barriers to movement throughout the series of archipelagos from Southeast Asia through to the Solomons. The hindrances to movement rather seem likely to be social and environmental; island groups could not be colonised at particular periods because people did not possess the set of social strategies necessary to overcome isolation and environmental constraints. The islands of the western Pacific thus allow us to study the emergence over the long term of social forms which can cope with increasingly isolated and impoverished islands. I have elsewhere laid out a preliminary framework for understanding the process of colonisation in the western Pacific over the long term (Gosden nd).

One implication of these data is that initially we should make no divisions between the island groups we are studying, either geographically or in human terms. To people with adequate maritime technology the sea would have been very much a bridge rather than a barrier. Thus the long term history of the islands of Asia and the Pacific is one of movements of people, language, genes and material culture over millennia; movements which must have taken different forms in various periods. The sets of connections which are beginning to emerge in the Lapita period linking southeast Asia and Melanesia are one particular (and rather late) aspect of these changing connections.

Lapita sites have recently furnished us with evidence of the subsistence base during that period and it appears that the major elements of recent horticultural forms were then in place. Much earlier evidence from the Pleistocene cave sites on New Ireland seems to be indicating the introduction of animals in order to boost the originally poor resource base. We urgently need further information on the structure of faunas within the Bismarck Archipelago and beyond, as well as evidence on the changing use of plants from the Pleistocene onwards. Here a combination of pollen work, phytolith analysis, both from sediments and from the edges of stone tools, plus the recovery of plant macrofossils will provide the necessary database.

Much needs still to be learnt about the basics of the Pleistocene record from this part of the world. Given the nature of discoveries over the past six years it seems certain that many more surprises await us within the Bismarck Archipelago. A comparison of this area with island southeast Asia will necessitate more work in the latter region in order to weigh up the nature of similarities and differences throughout these archipelagos. There is the possibility that there may have been island ways of life in existence which had some broad similarities over huge areas. As an initial strategy we might consider these archipelagos as some form of unit in human terms. Within this island world a similar family of techniques may have been in use. These techniques include seafaring, environmental manipulation and the movement of materials over long distances from a very early period. Exactly how these techniques developed and were deployed in various areas is thus of interest, as are variations in the relationship between islands and larger land masses. The long term history supplied by the Pleistocene evidence will ultimately provide a perspective on the relatively recent changes of the later Holocene. This period is the focus of the remainder of this paper.

LAPITA AS PEOPLE

In this section I want to consider briefly what may still be the dominant view of the Lapita phenomenon as being a form of cultural complex representing a distinct social unit, a people (Bellwood and Koon 1989; Kirch 1988; Spriggs 1989; see Terrell 1988, 1989 for a contrary view). I shall then contrast this with two views of my own which may give us a fuller basis for future systematic investigation.

The 'Lapita as people' model, like Janus, has two faces. The first points towards southeast Asia, the supposed origin point of groups of Austronesian speakers

> the strong argument that the Lapita people were speakers of some branch of Austronesian language – very likely Proto-Oceanic ... logically suggests a southeast Asian source for Lapita (Kirch 1988:157).

The second face points eastwards towards Polynesia, for the Lapita 'people' were destined to become Polynesians. Once Lapita populations reached the Bismarck Archipelago they spread rapidly out to Tonga and Samoa (Kirch 1988: 158). The fundamental role of the Lapita cultural complex in the narrative structures of Pacific prehistory is thus to connect southeast Asia and Polynesia in a neat and readily assimilated fashion. The Bismarck Archipelago can only play an intermediate role in such a structure, lying between the source of the Polynesians and their ultimate destination.

The Lapita 'people' are by definition some form of unit, sharing a genetic, linguistic and material cultural inheritance which is seen to mark them off from the other (previous) inhabitants of the western Pacific. Exactly how the boundaries between Lapita and the others is manifest and was maintained has never been adequately argued. Archaeologically, the marking traits of the incursion of the Austronesians are Lapita pots, shell technologies and a preference for beach locations. Whether these are an adequate and sustainable archaeological definition of the incursion of a social group is discussed below. At this point I am more concerned with the nature of the model itself.

My feeling is that to use an idea such as 'Lapita people' is to commit what philosophers would call a 'category mistake'; that is to apply a category which works well in one situation to a different and inappropriate context. The idea of a people derives from observations in the present when we can observe living, talking human beings with particular physical characteristics and sets of material culture. Language, physical make up and material culture can then be used to divide the world up into social units. Categorisation of this sort makes some sense of the range of social diversity in the present, although it should be noted in passing that many anthropologists are dubious about the utility of parcelling social identity up into discrete blocks. Whether we can define 'peoples' in the here and now is not the issue. My real point is that we cannot use categories of social identity originally developed to deal with the diversity of the contemporary world to investigate the past.

There are three reasons for this. The first has to do with timescales. Archaeological data are composed of a series of palimpsests representing a concatenation of human actions and even in the best dated sites we cannot define temporal units which span less than several generations. Given the speed with which social formations and their material manifestations may change, notions of social identity which can be usefully pursued in the present will look very different when pursued in the archaeological record. To put the same point in another way, to use a concept such as that of a 'people' we need to define what such a social unit and its changes would look like archaeologically. This brings us to the second point: the current archaeological markers of Lapita as a 'people' are fairly thin, boiling down to distinctively decorated pottery, shell technologies which are thought to be new, and a particular pattern of coastal settlement. Evidence of distinctive physical type is so far debatable (we have no human skeletal remains from 'non-Lapita' sites of the relevant period) and it is difficult to see how direct evidence of linguistic affiliation could ever be adduced.

The third point concerns notions of history. The idea of Lapita as 'people' is sustained partly by reference to the present, in that Lapita is seen as ancestral to various social groups which exist in the Pacific today, prime amongst these being the Polynesians. The features of Lapita society can then be retrodictively supplied from social groups in the present, often through the medium of historical linguistics (Kirch and Green 1987). Such a view assumes the continuity over 3500 years of essential features of social forms and that social categories, such as hierarchy, deriving from analyses in the present, existed in embryonic form in the Lapita period. Such a view makes assumptions about the nature of continuity in human society, the chief of which is that the features of social groups that anthropologists have been most interested in over the last few decades have existed in recognisable form throughout the late Holocene. How history operates on social forms within the Pacific should be debated (see Terrell 1988 for interesting discussions on this point), as should links between the present and the past, as Kirch has pointed out (Kirch 1988:164).

In a nutshell, my real objection to the view of Lapita as a 'people' is that it takes us too far, too fast, supplying an answer to the problems of the late Holocene archaeological record before we have even thought up satisfactory questions. In addition, although we are all guilty of the sin of presentism, that is of constructing our pictures of the past in a manner dominated by the categories of the present, the idea of a 'people' as creators of the Lapita archaeological record takes such construction to an extreme. If we are concerned with how the world as it exists in the present came to be and, if for instance, we want to know the origins of the social groups we gloss as the Polynesians, we may have to accept that the archaeological record will only allow us to formulate questions in certain ways. A starting point in our investigations therefore must be to ask how we can usefully formulate questions to be pursued through the archaeological evidence of the western Pacific; that is to question the nature of our questions. A second equally important line of thought has to do with the nature of continuity and change in human society and how far the features of life in the present can be seen to have a long history in the past. We need to ask in what terms can we understand long lasting social structures and the nature of the changes within them. These are deep and demanding questions for all archaeologists and not just those in the Pacific, but it may be that a combination of knowledge of past and present in the Pacific may eventually provide us answers more difficult to come by elsewhere. For the answers to do justice to the past the questions we ask must not be framed solely in terms derived from the present. How to ask meaningful questions of the past is thus the biggest question of all at this stage of our investigation.

ALTERNATIVE FRAMEWORKS

The initial question I would pose of the archaeological record of the Bismarck Archipelago is how may we understand social forms in the past on the basis of the temporally gross palimpsests of the archaeological record? The starting point from which I will pursue this question is the assumption that social relations have a spatial aspect and that the patterns of distribution of people across a portion of the earth's surface will help create the nature of social links between them. Conversely, social structures will alter and affect the spatial distribution of human communities. Space and society can thus be seen to be mutually constituting (see Gregory and Urry 1985 for two geographers' discussion of these points). This point is particularly valuable to bear in mind when we are dealing with periods when people were moving rapidly from one area to another, changing the nature of spatial and social structures in the process.

Over at least the past 30,000 years people have been progressively moving into the western Pacific. Each movement has set up new demographic structures, different flows of people and materials. The progressive filling up of empty island groups has lessened or altered the possibilities for new colonisation. Any new colonising movement, or any difference in the flows of people through already inhabited areas, changed the social conditions not only in areas to which people moved but also in the areas from which they originated. From the late Pleistocene when movements into the Pacific started, to the late Holocene when all the major island groups were peopled, we can expect a series of social forms unfolding and changing through the process of colonisation. The period from 3500 to c.2000 BP which we refer to as Lapita is thus one phase in a long series of colonising movements, which set up the first permanent settlement on Fiji, Tonga, Samoa and perhaps beyond, bringing new spatial and social circumstances into being. Following this argument the social forms of the present would not have come into being at least until the Pacific was full up (that is in the last millennium) and the current social geography was constructed. The particular set of social relations and practices we know as 'Polynesian' would not have taken on recognisable shape until this recent period and previous social forms, because of their different topology, may not be easily recognised as bearing ancestral Polynesian traits; they are simply different.

The Lapita period therefore had its own set of social structures, quite different from those of today, which had to do with movements of people towards the east and probable connections towards the west (Ambrose 1988; Bellwood and Koon 1989). As noted earlier, there is no reason to assume a barrier between the Bismarck Archipelago and areas to the east and the west at any period from the late Pleistocene onwards. The islands of the Bismarcks form part of a series of archipelagoes stretching from southeast Asia out into the western Pacific between which there are no major physical barriers and there is also no reason to assume that social barriers existed between areas which we conceive of as different, influenced as we are by present political and cultural boundaries. Hence the social geography of communities in the Bismarcks throughout prehistory would have been composed of links to the east and the west, as well as movements to and from both directions.

In describing social and spatial structures I will use two sets of terms which indicate different geographical scales. 'Social geography' will refer to broad areas, varying in size from the whole of the western Pacific to the Bismarck Archipelago, and designating the changing social and spatial relations in these broad areas. Obviously, looseness of definition is a problem here, but as I discuss below, it may be possible to define spatial units on the basis of archaeological evidence, such that the total distribution of Lapita pots forms the most extensive level of social geographical unit and divisions within the Lapita distribution (e.g. Far Western, Western) may define smaller social and spatial units. Far less extensive areas will be referred to by the term 'social landscape' (see Gosden 1989), which may have been occupied by smaller social groups (although we cannot assume that the areas in which we have set up our research (e.g. the Arawes, Mussau, Duke of Yorks) originally formed meaningful social units). Social landscapes are limited areas in which social forces meet local physical conditions and the manner in which people expend their social effort will have to do both with local circumstances as well as broader patterns of social geography. Each broad social geographical area was thus made up of a mosaic of social landscapes. How to define the divisions within the mosaic and the links between various areas is a problem for future research. Questions of scale and definition will be taken up again below.

LAPITA AS AN ARCHAEOLOGICAL CONSTRUCT

Having made the point that the process of colonisation of the Pacific has taken place from the late Pleistocene onwards, we have to beware that we are not over-emphasising the Lapita period as one of crucial change. There is the possibility that the visibility of Lapita sites and the existence of a framework of interpretation to link them to broader questions of Pacific colonisation (Lapita as Austronesian colonists and Polynesian ancestors) have led to the assumption that the Lapita period is something special within the overall process of colonisation.

As a result, we now know of more sites from the Lapita period in the western Pacific than from any phase before or afterwards. Although we must not be misled by this relative wealth of information into arguing that Lapita represents a crucial point of change, we can use this relatively rich knowledge to our advantage. An understanding of the overall process of colonisation of the Pacific must be our eventual goal. The special feature of the Lapita period is not then that it represents a more important or far reaching movement of people than any other, but simply that it is more visible (Allen and White 1989). The role of Lapita material within our interpretative strategies is that, because it is relatively well known, we can use it to understand periods which come both earlier and later. Thus Lapita will remain the centre of attention for the moment and allow us to devise plans of research to investigate what led up to Lapita and what social forms succeeded it. Indeed, this was essentially the strategy employed with the Lapita Homeland Project (see Allen 1984 and Allen this volume for general background and research plan). Once we have a better idea what occurred before and after Lapita, we will be able to place the Lapita period within Pacific prehistory as a whole and judge its importance as a period of change and colonisation.

The thread running through all of the above discussion is that the Lapita period represents a complex series of changes and realignments within the western Pacific which we cannot understand by recourse to concepts derived from present day social forms and analyses. The types of society existing in the past were different from those of the present and part of the nature of this difference lay in the spatial structure of settlement within the area as a whole. I have introduced the idea of social geography as a framework through which spatial and social structures can be investigated (the idea of social geography in this context is elaborated elsewhere Gosden in press). Given the fact that spatial and social structures have changed continuously over the last 30,000 years of colonisation there is no reason to believe that the Lapita period was more formative than any other of the social structures existing in the Pacific today. Thus Lapita social forms were not necessarily directly ancestral to either Melanesian or Polynesian societies in the present. Indeed it seems very likely, given the long sequences we now possess from the Bismarck Archipelago and the Solomon Islands, that the role of Lapita in Pacific prehistory has been over-emphasised and that we have mistaken a relative wealth of information for crucial social change. Lapita will remain important in the near future, however, as we have an outline of social geographies and social landscapes from this period which can act as a starting point when looking at other times, especially the long history of pre-Lapita change. Against the background of these discussions we can now move to consider the data from the Bismarck Archipelago in more detail.

LAPITA POSES PROBLEMS OF SCALE

Lapita pottery has attracted attention partly because it is so widely distributed. The breadth of this distribution, however, poses problems as to the varieties of scales of analysis at which the Lapita phenomenon can be approached. Using the framework outlined above we can identify three levels at which analysis can proceed. The first and most inclusive is that of social geography. Taken most broadly this can refer to the distribution of settlement throughout the western Pacific and the new patterns set up though the process of colonisation. These patterns are beyond the scope of this paper, given its Bismarcks focus. Considering the Bismarcks alone, the idea of social geography refers to patterns of settlement and interconnection within the islands. I feel that analysis of the linkages between sites within the Archipelago as carried out by Hunt (1988) is premature, as site patterning reflects areas of research concentration rather than the original distribution of population (see Gorecki et al. this volume). However, it may be possible to discern differences in the distribution of materials in various areas and link these to patterns of interaction and contact.

The first class of material which appears to have a differential distribution of types is pottery. Large or chronologically secure pottery assemblages have not been found in many parts of the Archipelago before 3000 BP, with only Mussau and the Arawes having early assemblages with large sample sizes. In Manus and Nissan early pottery assemblages come from rock shelters or caves. These are difficult depositional contexts to compare with the open sites, having small amounts of all types of material and problems of chronology and stratigraphy (see Ambrose this volume on problems of comparison between Kohin and the Lou sites). The early pottery assemblages which do exist are dominated by dentate-stamped and incised wares.

After 2500 BP pottery assemblages occur from many parts of the Archipelago, such as the Admiralties group, New Ireland and Watom, as well as Mussau and the Arawes. During this period regional differences in pottery types become apparent. As well as incision, rim notching and applied decoration is found with a variety of rim forms in the Admiralties, Mussau, Lossu and Lasigi on New Ireland and Nissan. Pots of these types are so far absent from contemporary sites on Watom, or in sites in the Duke of Yorks and the Talasea area which lack absolute dates but may fall within the period after 2500 BP. The only sites outside the Admiralties-New Ireland-Nissan axis which may have similar types are those of the Arawes, but further work is needed to confirm the date of these assemblages. The other set of pottery probably from this period, with a restricted distribution, is the notorious type 'X', which is found on the north coast of Papua New Guinea but is limited to West New Britain (Siassi, Eleonora Bay and the Arawes) within the Bismarck Archipelago. Dentate-stamped pottery may still exist in Watom after 2500 BP. Current wisdom seems to be that the diversification of pottery industries only started in the late Lapita period and contrasts with earlier homogeneous Lapita styles. Until more early assemblages become available from the period before 3000 BP we will not be able to make secure statements about the nature of diversity in pottery types throughout the Archipelago. Here the Manus case is especially interesting, displaying a variety of post-Lapita pottery styles and possibly being the source of Lapita pots on Mussau (Hunt 1989), but not having produced any large dentate-stamped or incised assemblages from early in the

Lapita period (but see McEldowney and Ballard this volume).

Whatever turns out to be the case with the early pottery industries, it is interesting to note that the restricted distribution of wares after 2500 BP is parallelled by differences in the distribution of obsidian. By this late Lapita period Lou island obsidian is the dominant component of assemblages throughout the Admiralties, Mussau, New Ireland and Nissan; a distribution which mirrors that of the incised and applied relief wares. Given Ambrose's demonstration that some of these wares were produced within the Admiralties group (Ambrose this volume) it would be interesting to determine whether Manus was acting as a centre of both pottery and obsidian production in this period. In the areas where such wares are not found, such as Watom, Lou island obsidian occurs, but is found in smaller amounts in the later phases at Watom than in the earlier periods. The New Britain obsidian assemblages are dominated by local sources, as far as we know. It may be, therefore, that we have different patterns of distribution of pottery and obsidian, with New Britain being different from the rest of the Archipelago and at its west end sharing similarities with the north coast of Papua New Guinea in the form of type 'X'. Such differences represent varying social geographies, a Manus-New Ireland network contrasting with New Britain. It is possible that the sets of connections between communities alter from the early to the late Lapita period. Motif analysis from the large dentate-stamped and incised assemblages currently available might throw light on patterns of interaction in the earlier Lapita period, especially if this was combined with sourcing studies to look at patterns of production and exchange.

The next more detailed level of scale is that of the social landscape, a term which I have used to refer to socially based patterns of use of a local area. Direct evidence of patterns of landscape use will come in the form of the depositional landscape, composed of the series of depositional contexts found within the area of study and the artefacts within them. The aim of a landscape based analysis would be to understand how people use an area as a whole, rather than concentrating on particular sites within the landscape (see Gosden this volume). The two best known landscapes from the region so far are those of Mussau and the Arawes. The structure of the evidence from the two regions is remarkably similar. Both areas have extremely rich beach sites from the Lapita period (ECA in Mussau, FOH and FOJ in the Arawes) with large amounts of material preserved within the Ghyben-Herzberg aquifer. These rich deposits are the result of the original position of the Lapita communities in stilt houses out over the reef, the sheltered situation of the islands such that material thrown into shallow water was not subsequently disturbed and the fact that crabs do not penetrate the brackish water of the Ghyben-Herzberg lens to move the artefacts. The range of artefacts found in these sites seems to be very similar (see Gosden 1990) and we must expect similar ranges of material to be found wherever the right conditions of preservation occur.

A combination of factors may help to overemphasise the Lapita period, as then settlements seem to have been in exactly the best spot on the landscape for preservation. In neither Mussau or the Arawes do people seem to have inhabited stilt houses before or after the Lapita periods and sites of such richness do not occur, making comparisons between Lapita and other periods difficult. It is also difficult to compare these sites with other spots on the landscape in which artefacts were deposited during the Lapita period. Sites away from the beach do not display the same range of materials and certainly not the same density of artefacts (site EKQ in Mussau may be something of an exception here). It is impossible to gauge whether the lack of material discovered away from beach locations is due to lack of original deposition or because it is not preserved. At the present it appears that Lapita sites are restricted to coastal locations, but it may prove true that rich, obvious Lapita sites are only found on beaches while areas further inland have not produced large Lapita assemblages due to factors of preservation above all. In order to pursue these questions we need to be able to make controlled comparisons between sites on different parts of the landscape. Here obsidian as a reductive technology may provide us with a lead, as we can investigate on which areas of the landscape particular aspects of the reduction sequence are found and this may reflect patterns of land use (see Fullagar and Torrence, this volume on the analysis of the spatial distribution of a reduction sequence).

At a local regional scale therefore we need to set up means of comparison between sites of the same period on different parts of the landscape, as well as between landscapes of different periods. The main clue to the fact that we are not picking up all sites equally well and therefore some of our comparisons are spurious is contained in our knowledge of the last 1000 years. Presumably, the Archipelago as a whole was well populated in this period and relatively large numbers of sites are known. However, none discovered so far have the richness of the big Lapita sites nor offer the same chance for detailed analysis. The relative paucity of site assemblages from the recent period should alert us to the possibility that the pre-Lapita period also lacks good archaeological visibility, not because people were only present in small numbers, but because they may have occupied spots on the landscape which did not have the preservative qualities of waterlogged beach deposits.

So far we have very few indications of the immediately pre-Lapita landscape and almost all of these derive from caves and shelters. Such deposits seem to have common characteristics: low levels of material, relatively high proportions of 'special' materials such as worked shell and problems of chronological sequence. The possibility exists that all over the Archipelago between 5000-3500 BP people were using shelters and caves in a similar manner, carrying out a limited range of tasks in them and not using them as their main focus of habitation. On a number of small islands the first evidence we have of occupation comes from caves and rockshelters. This evidence should be seen as partial and not reflecting all elements of human activity in the pre-Lapita period. Further indications that our knowledge of the pre-Lapita period is impoverished comes from Mussau, which appears to have had considerable links with Manus from the Lapita period to the present. Despite evidence from a number of sites of pre-Lapita occupation on Manus, we have no evidence before 3500 BP of people on Mussau. It is unlikely that absence of evidence in this case is evidence of absence and it seems more probable that people were moving around the northern reaches of the Bismarck group in the pre-Lapita period but no archaeological evidence of them so far has turned up on Mussau. Until we have fuller sets of evidence from the pre-Lapita period at both the local and wider levels many questions about the density and nature of Lapita versus the pre-Lapita period cannot be resolved.

Problems of scale and chronological comparison need to be distinguished carefully in our discussion of the Lapita phenomenon itself and its comparisons with other periods. We have to be careful not to slip from statements drawn from the evidence from single sites to generalisations about Lapita as a whole. We need a framework to fit individual sites into the context of a local landscapes and to look at how these local regions were knit together into larger groupings at various periods. We also need to be able to distinguish between different types of sites in terms of their formation processes and the sorts of evidence of human action they contain and here a distinction between caves, beach sites and surface scatters is vital. We also need concerted frameworks for analysing particular materials, primarily pottery, shell and stone, as the contributors to Kirch and Hunt (1988) have pointed out.

CUTTING EDGES AND CONTAINERS

One aspect of technology so far unremarked on is the change between the Pleistocene and early Holocene cave sites in New Ireland on the one hand and Lapita sites on the other in terms of the cutting edges they employed. Although we know that obsidian was moving around the Archipelago from at least 20,000 BP, with the exception of Matenbek it forms a minority component amongst the stone assemblages of those early sites. In the Lapita sites this situation is dramatically reversed, in that the overwhelming majority of stone found on all sites within the Archipelago is obsidian. Obsidian as the main cutting technology in stone is complemented by increasing use of shell for a whole range of artefacts. Shell artefacts are found in pre-Lapita contexts (Lolmo and Matenbek, and now on Manus, Matthew Spriggs pers. comm.) in small numbers and we must question whether shell technologies were only employed in small amounts or whether the pre-Lapita contexts we have excavated have only provided limited evidence of shell. Very little non-obsidian stone is found on sites of the Lapita period and later; that which occurs is commonly in the form of axes and axe fragments.

Obsidian forms a strand of continuity between pre-Lapita and Lapita and was exotic in all areas outside the few sources. Its movement therefore represents sets of social connections, which may or may not remain stable over long periods. Detailed analyses of the reduction and use of obsidian are now urgently needed to establish its technological and social place within the Archipelago from the Pleistocene onwards (Specht et al. 1988). Much of the stone used in the Pleistocene caves (at least in southern New Ireland) appears to derive from areas local to the sites, with obsidian coming in as an exotic raw material. There is the intriguing possibility that from the mid-Holocene onwards this local stone component may have been replaced by shell, with obsidian continuing in use for the same range of functions or perhaps expanding its range. If this were the case then overall the structure of Bismarcks technology might look similar to that in Polynesia, with shell providing the main cutting edge, with obsidian and other stone as an additive element not found in Polynesia. Use wear studies on stone, obsidian and shell on assemblages from the Pleistocene onwards are needed to choose between these possibilities.

Work on Lapita pottery throughout the western Pacific has so far concentrated on decoration, although this is now being added to by analysis of fabrics. Given the proliferation of decorative motifs on Lapita pots and the size of some of the assemblages now available, an initial sorting by fabric will provide some bounds and structure to classification. Also, as Ambrose has shown (this volume), the analysis of fabric may provide evidence of continuity between the Lapita and later periods.

What is necessary again is some framework for the analysis and interpretation of pottery. I propose a twofold division of pottery for analytical purposes. The first element of the division has to do with pottery in its context of excavation. Here we can distinguish the assemblage: a group of pots from a defined context in excavation. These derive from a particular point on the landscape and a defined time period. Assemblages thus form the minimum units of analysis. Assemblages from different sites in one area can be combined into a series, which encompasses all the spatial and temporal variation in pottery in one defined region. Lapita pottery from the western Pacific as a whole can be seen as a combination of such geographically defined series.

Cross-cutting this form of ordering are characteristics of ceramic production. Here fabric can form the initial basis for classification as this provides direct evidence of the nature and location of production. It may be useful to distinguish between individual workshops and overall industries. Workshops represent the smallest unit of production using one set of raw materials and producing pots which fall within a defined range of forms and decoration. In order to identify the products of one workshop, various petrographic and chemical analyses of the pots will be necessary in order to group and, where possible, source sets of fabrics. After this it will be possible to see how far fabric, form and decoration are correlated. In the case of dentate-stamped pottery it will be of great interest to see whether local production centres were producing pots with their own special set of designs or whether combinations of motifs cut across divisions of fabrics. Discovering how far the Lapita design system was locally based and how far it operated over broader regions will tell us much about its social role.

Industries can be seen to embrace a number of individual production centres. An immediate question which arises is how far dentate-stamped and incised wares can be seen as different industries. The thin walled, fine textured incised wares with their characteristic forms look immediately different to the eye from the dentate-stamped pots. We need to go beyond this immediate characterisation of difference into looking at whether the two sets of pottery were produced from different raw materials and at varying points on the landscape. The series of wares found after 2500 BP throughout the Bismarcks and elsewhere also need investigation, to discover how far they represent the use of new sets of raw materials, points of production and networks of movement. It may be that dentate-stamped, incised and applied relief wares will all represent different industries either in operation contemporaneously or gradually replacing each other through time. As a number of authors in this volume have remarked (Ambrose, Green and Anson, Kirch et al.), there is no evidence of sudden breaks in ceramic production within the Bismarcks. Rather, the story seems to be one of gradual change from one set of types to another. Exactly how this change takes place in terms of both production and exchange needs much further work to elucidate. Luckily we now have assemblages of the right size from a number of areas to start to address questions of this type.

CONCLUSIONS

After brief consideration of the Pleistocene background, the theme of this paper has been the construction of frameworks for posing questions of the Lapita period. I have used twin assumptions as a starting point for these frameworks: firstly, that social groups of the Lapita period were different in their essentials to any in the present and, secondly, that the archaeological record is hard to understand and that we need to develop methods which encompass a series of scales of analysis.

A starting point is that the structure of archaeological evidence in individual regions needs to be understood as a totality; we cannot read off the past from the remains of individual sites. This regional approach involves the analysis of geomorphological patterns and how far the distribution of artefacts is influenced by factors of preservation and destruction, as well as the original prehistoric patterns of artefact deposition. Such local landscapes can then be combined into broader social wholes, evidenced by differences in the distribution of artefacts throughout the Archipelago, such as the late Lapita pottery styles and obsidian distributions. At this scale of analysis a number of questions arise. We need to establish whether New Britain sat within a slightly different social network to Manus and New Ireland and how this affected relations of these two areas with the rest of the Pacific.

Ultimately, it is questions of a pan-Pacific nature which concern us. The Pacific represents the last portion of the earth's surface to be colonised and this process of colonisation took place over a relatively short time period compared with many parts of the world. So far the 30,000 year span of colonisation has been broken into discontinuous blocks by the Lapita period, as it has been assumed that Lapita represents the point of crucial change, different from anything which preceded it. This temporal fragmentation has been echoed by a tendency to see southeast Asia and the western Pacific as separate units in human terms, reflecting biogeographical divisions. A more open way of viewing these problems is to see the area from island southeast Asia through the western Pacific as a group of archipelagos with no obvious physical barriers, through which people started sailing unusually early in world terms (Irwin this volume). Over at least the last 30,000 years there has been a complex series of movements of people and materials through these islands setting up varying spatial and social forms in different periods. It is an immense task to understand the nature of long term continuity and change in this area, but the process of learning will tell us much about doing archaeology and, more importantly, about the human past.

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