CHAPTER 1

THE MUSSAU PROJECT IN THE CONTEXT OF LAPITA ARCHAEOLOGY

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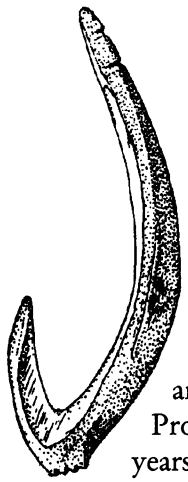

From its inception as a part of the 1985 Lapita Homeland Project, through two additional field seasons in 1986 and 1988, followed by a lengthy period of laboratory analysis and report preparation, the Mussau Project has now spanned more than 15 years, involving collaboration with 20 colleagues and students. How does one begin to summarize the complex history of such a research project, retaining at least something of the flavor of ideas, hypotheses, strategies, hunches, setbacks, unexpected discoveries, and a great deal of simple drudgery in both field and lab that lie behind the results presented in this monograph? Archaeological knowledge and understanding of the Lapita cultural complex has advanced tremendously over this time period, in no small part due to the results of the Lapita Homeland Project (LHP) and Mussau Project in particular (Kirch 1997). Yet it seems necessary to provide the reader with an account of the research issues and problems *as these were conceived in the mid-1980s when our work began*, for only in that context can one understand fully the various decisions regarding which sites we dug, and how we excavated them and analyzed their contents. Such is my aim here, to review the core research problems around which the Mussau Project was framed, in the context of what was known then about Lapita. I also review the history of the Project, discussing the work of each field season and showing how our research strategies evolved in response to the results we were obtaining, how we modified both the questions we sought to ask of the archaeological record, and the methods we applied. First, however, some background remarks on the Lapita cultural complex and the history of its definition are relevant.

THE LAPITA CULTURAL COMPLEX: HISTORY AND BACKGROUND

As related in greater detail elsewhere (Kirch 1988d, 1997:6-11), Lapita pottery was brought to scholarly notice by the German priest Otto Meyer (1909, 1910), who found decorated potsherds at his mission station on Watom Island, near New Britain. Archaeologist W. C. McKern (1929) excavated Lapita pottery in Tonga in

1920, mistakenly thinking that it was a late prehistoric variant of Fijian ceramics. Avias (1950) linked the Watom sherds with finds from New Caledonia, but it was finally Prof. E. W. Gifford of Berkeley who, after excavating at the "Lapita" type site (Site 13) on the Foué Peninsula of New Caledonia in 1952, recognized the significance of this distinctive ceramic horizon which spanned the ethnographic abyss separating Melanesia and Polynesia (Gifford and Shutler 1956). Further fieldwork at Watom, the Île des Pins, Fiji, Tongatapu, and Samoa during the 1950s and 60s produced an accumulation of data, permitting Golson (1971) to distinguish a Lapita *ceramic series*, the key aspect of an otherwise ill-defined cultural complex. Moreover, Golson argued that the great similarities in Lapita designs bespoke a "community of culture" that had once linked the early populations of the southwestern Pacific, again shaking long-held ethnographic conceptions about the separateness of Melanesian and Polynesian prehistory.[1]

A major advance in Lapita archaeology came during the early 1970s, with the Southeast Solomon Islands Culture History Programme organized by Roger Green and Douglas Yen (Green and Cresswell, eds., 1976). This project reoriented the geographical focus of Pacific archaeology out of Polynesia into Melanesia, and Green's own fieldwork in the Reef/Santa Cruz Islands applied sophisticated sampling and areal excavation methods to several Lapita sites for the first time (Green 1974a, 1976, 1978). Green's goal was to achieve a broader definition of the Lapita *cultural complex* beyond that of a ceramic series, including settlement patterns, subsistence economy, non-ceramic aspects of material culture, and so forth (Green 1979a). Lapita studies had now reached a level of sophistication in which nuanced debate was possible regarding contrasting models of colonization, trade, exchange, dispersal, and the like (Groube 1971; Clark and Terrell 1978; Green 1982, 1985; Spriggs 1984). However, there was by no means uniformity of opinion on what Lapita "meant" for Pacific prehistory. In particular, such issues as whether Lapita represented a fairly rapid intrusion and expansion of Austronesian-speaking peoples into Oceania began to be hotly debated, as were interpretations of Lapita economy, these ranging from an oceanic "standlooping" mode of existence (Groube 1971) to a fully horticultural economy (Green 1979a).

By the early 1980s, however, most archaeologists at work in the southwestern Pacific would have agreed with the following statements regarding Lapita. First, it was a well-marked ceramic "horizon" (albeit characterized by local variants and by temporal changes in particular areas, thus properly a ceramic *series*), with a distinctive design corpus and stylistic "grammar" (see Mead et al. 1975; Green 1978). Less certain were the relationships of some contemporary plainware assemblages (e.g., that of the early Anutan An-6 Site; Kirch and Rosendahl 1976) to the typical dentate-stamped Lapita ceramics. Second, the geographical range of Lapita (Fig. 1.1) was known to extend from Mussau, Watom, and Ambitle in the Bismarck Archipelago, to Vanuatu and New Caledonia, Fiji, Tonga, and Samoa (with a somewhat enigmatic gap in the main Solomon Islands). Third, most sites of the Lapita complex dated between the later second millennium BC until about the mid-first millennium BC, although claims had also been made for much later persistence of Lapita pottery in some sites (e.g., Hedrick 1971; Poulsen 1968). Fourth, there seemed to be a proclivity for siting Lapita communities either on small offshore islets or on the coastal terraces of larger islands, which combined with evidence for extensive shellfish gathering and fishing, suggested a strong maritime orientation to some investigators. Fifth, communication and material exchange between Lapita communities was increasingly evident in the archaeological record (e.g., Ambrose and Green 1972), prompting a consideration of models of Lapita people as itinerant "traders" (Clark and Terrell 1978). Sixth, continuous ceramic sequences in Western Polynesia demonstrated a founding role for Lapita in Polynesian origins, thus directly linking Lapita with one major "segment of culture history" in the Pacific (to use Romney's term [1957]). Far more contentious was the relationship between Lapita and other cultural complexes which appeared to follow it in Melanesia, such as the Mangaasi ceramic complex of Vanuatu (Garanger 1972). Some held that Mangaasi had developed directly out of Lapita (e.g., Spriggs 1984), while others viewed the former as a cultural replacement. Such then was the "state of play" among those concerned with Lapita archaeology and its significance for Pacific prehistory, immediately prior to the Lapita Homeland Project in 1984-85.

THE LAPITA HOMELAND PROJECT

At the 15th Pacific Science Congress held in Dunedin during February 1983, Jim Allen (then of the Australian

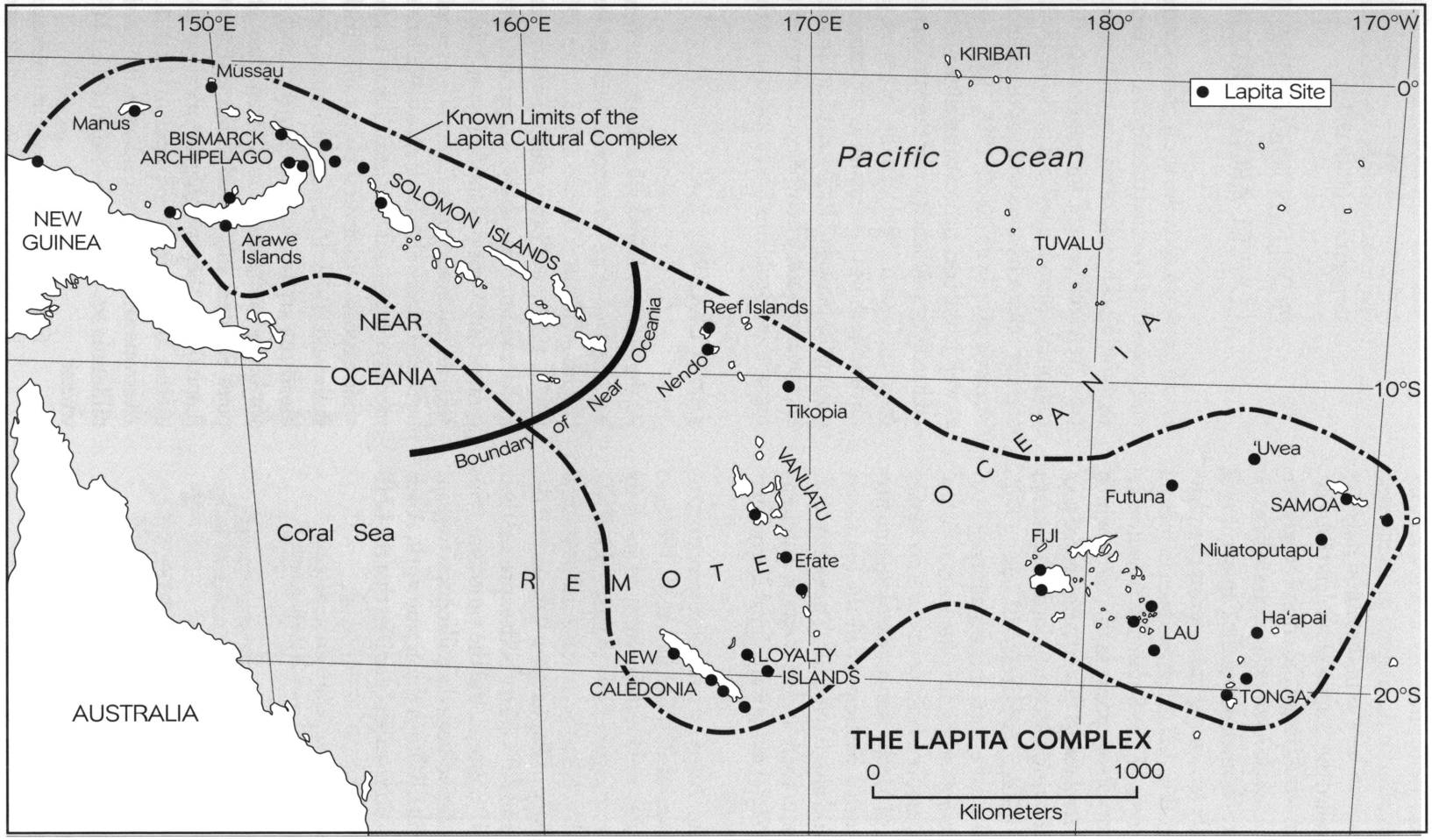

FIGURE 1.1 The geographic distribution of the Lapita cultural complex, showing the locations of major sites.

National University) approached me, along with various others, with an offer to participate in an international research program he was in the initial stages of developing. To be called the "Lapita Homeland Project" (LHP), this program would target the critical geographical region of the Bismarck Archipelago, thought to be the likely immediate "homeland" of the Lapita cultural complex,[2] but largely *terra incognita* in terms of archaeology and prehistory (Fig. 1.2). Allen had been impressed with the great success of the Southeast Solomon Islands Culture History Programme in the 1970s, which had placed several field teams in a circumscribed geographic region, following a loosely coordinated set of research strategies (Green and Cresswell, eds., 1976). He argued that a similar effort, this time focused farther W on the islands surrounding the Bismarck Sea, could lead to comparable advances in our knowledge and understanding of Melanesian archaeology and prehistory. Having been a participant in the Southeast Solomons work, and also having some prior involvement with Lapita sites there and in Western Polynesia (e.g., Kirch 1978, 1981), I tentatively signaled my interest in the LHP concept.

During 1984, Jim Allen led a reconnaissance trip to the Bismarcks region of PNG (Allen et al. 1984), to locate promising sites and localities for field work by the intended LHP participants, and to liaise with PNG government officials at both the national and provincial levels. The University of Papua New Guinea and the National Museum and Art Gallery both expressed their support for the LHP concept, and the various provincial administrators also indicated a willingness to grant the necessary permits. Within this framework, Allen (1991) laid out six main research questions that the LHP was intended to address:

1. What was the nature of late Pleistocene/early Holocene human occupation in the Bismarck Archipelago?
2. Was horticulture part of the subsistence strategy throughout the Holocene in the Bismarck Archipelago or was it a later introduction?
3. What was the nature of ceramic development or introduction and its subsequent evolution in the region?
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 specialization and production can be determined from these data through time? (Allen 1991:3).

PROBLEMS AND ISSUES IN LAPITA ARCHAEOLOGY

I turn now to a consideration of the central problems and issues that oriented the Mussau Project throughout its duration. The following five problems provided the core "research design" for our Project. Building upon the initial research questions of the LHP, these Mussau-specific research issues were outlined in my two NSF research proposals (Kirch 1986, 1987b), and discussed as well in the conclusion to an edited review of Lapita archaeology published while the Mussau fieldwork was ongoing (Kirch 1988e). In the following paragraphs I outline each of these issues as we conceived of them during the period of the Mussau fieldwork from 1985-88, but also note, where relevant, how our understanding of these problems has changed since then.

LAPITA ORIGINS

At the time our Mussau fieldwork commenced in 1985, nothing was more contentious than the issue of Lapita origins. Gifford (Gifford and Shutler 1956), Golson (1971, 1972), and other early investigators of Lapita had drawn attention to stylistic parallels between Lapita ceramics and certain island Southeast Asian assemblages, with the implication that Lapita origins would ultimately be traced westwards back across Wallace's Line. This model of an island Southeast Asian origin was given wide exposure in several books and articles by Peter Bellwood (1979, 1980). In particular, Bellwood combined the archaeological record of early ceramic assemblages extending from Taiwan, through the Philippines, Sulawesi, and Talaud Islands, and on into Melanesia (Lapita) with the emerging historical linguistic interpretation of a rapid expansion and dispersal of Austronesian language-speakers in the third and second millennia BC. Thus at the time of the LHP, Bellwood wrote:

> In the Western Pacific . . . Austronesian colonists between 1500 and 1000 BC left an extremely clear-cut trail of pioneer archaeological sites across about 6500 km of ocean and islands (many previously uninhabited), from the Admiralty Islands (north of New Guinea) to

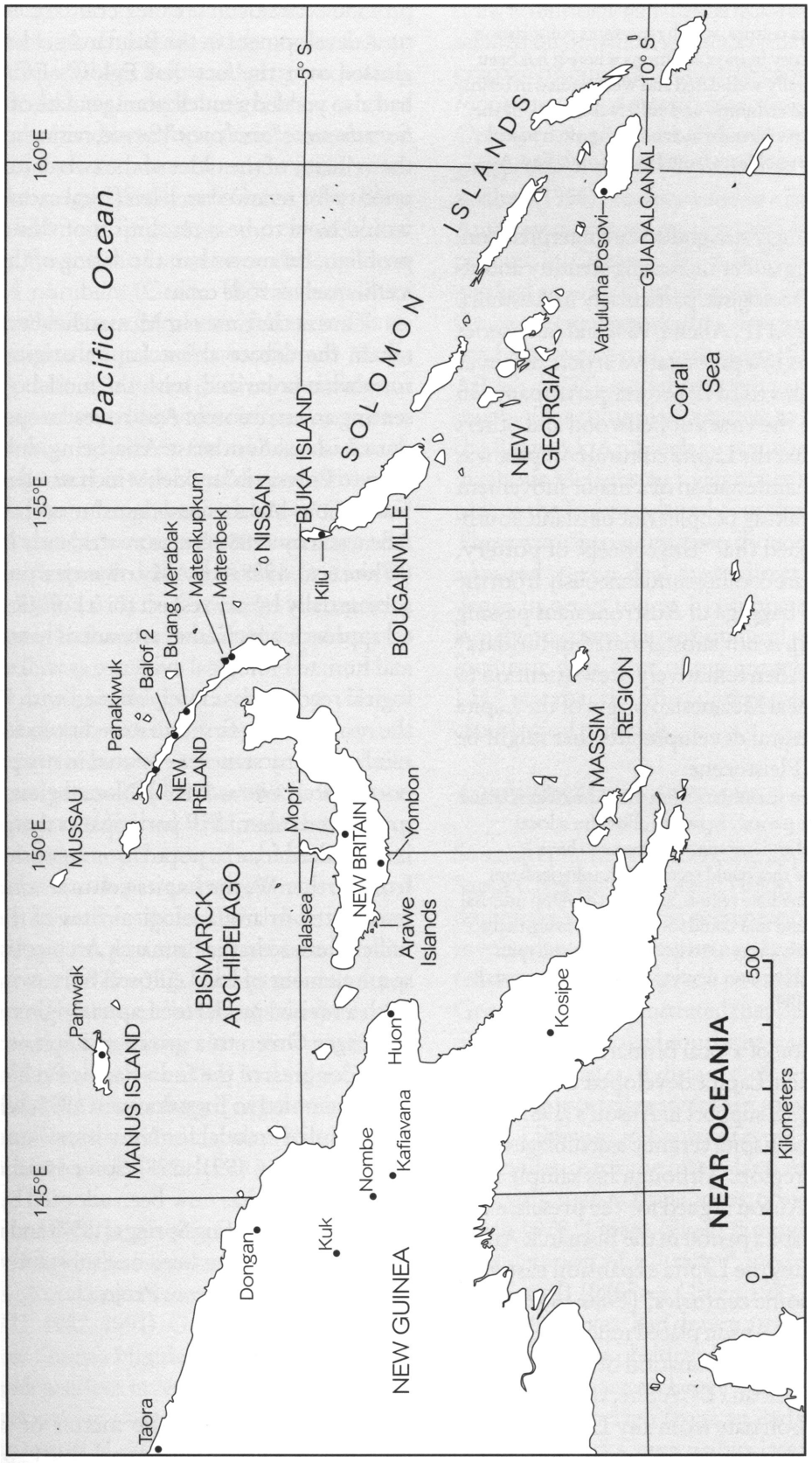

FIGURE 1.2 Near Oceania, showing the central position of the Bismarck Archipelago.

> as far east as Samoa, in western Polynesia.... The resulting Lapita culture, which represents colonisation of virgin territory in most locations where it has been found, is generally well-dated and well-studied in terms of artefacts and economy and suffers from few of the chronological problems which beset the often mixed and undated assemblages from Island South-East Asia (1985:252).

Bellwood's seemingly straightforward interpretation, however, was coming under increasing scrutiny and attack from some archaeologists, particularly in Australia. In anticipation of the LHP, Allen (1984) laid out a series of research questions in a provocative article meant as an intellectual stimulus to LHP project participants. In direct opposition to the views of Bellwood and others (e.g., Spriggs 1984) that the Lapita cultural complex was the archaeological manifestation of a major movement of Austronesian-speaking peoples out of island Southeast Asia, Allen argued that "the concept of pottery, canoes and horticulture coming simultaneously from the west, as the cultural baggage of Austronesians passing through [Melanesia], is not substantiated in the data" (1984:194). Rather, Allen tentatively drew attention to the possibility of a local Melanesian origin of the Lapita complex, out of cultural developments that might be traced back into the Pleistocene:

> What is evident in the data from the Bismarcks is that a sufficient time period elapsed to allow for a local cohesive social and economic universe to have developed, one that could receive technologies from outside its immediate region, as well as develop internal technologies; and that could subsequently bring them together in such a way as to lead to both the Lapita expression and the later development of the Bismarck region (1984:194).

Allen's contention of a local Bismarck Archipelago "homeland" in which Lapita developed out of local antecedents found some support in Anson's (1983, 1986) analysis of the limited Lapita ceramic assemblages then available from this region. Although his sample sizes were quite limited, Anson argued for the presence of a distinctive "earlier Lapita period in the Bismarck Archipelago which predates the Lapita expansion eastward into the Pacific by some centuries" (1986:164).[3] In advancing this argument, Anson placed much weight on a single radiocarbon age determination of 3900 ± 260 BP (GX-5499) from the Eloaua ECA Site, then the oldest acceptable radiocarbon date from any Lapita site, and predating Lapita sites in the SE Solomons by as much as seven or eight centuries. If correct, this would indeed provide a considerable time period for local Lapita cultural development in the Bismarcks. However, Anson glossed over the fact that Egloff's ECA excavations had also yielded a much younger date of 3300 ± 180 BP *from the same "coral oven" feature*, raising suspicions about the veracity of the older of the two dates. Clearly, one priority for renewed archaeological excavations at ECA would have to be a resolution of this chronological problem, for more than the dating of the ECA deposits themselves rode on it.

Thus at the time our Mussau fieldwork was undertaken, the debate about Lapita origins had become somewhat polarized, with the model of Lapita representing an intrusion of Austronesian-speaking peoples out of island Southeast Asia being dubbed the "fast train to Polynesia" model, which stood opposed to the "indigenous Melanesian origins" model championed by Allen, and by 1988 even more stridently by Peter White (White et al. 1988:416).[4] My own viewpoint, influenced substantially by my respect for a holistic anthropological approach which takes account of historical linguistic and human biological evidence as well as the archaeological record, was closely aligned with Bellwood, and the results of our first two expeditions to Mussau only reinforced my stance, as I stated in my preliminary reports (Kirch 1987a, 1988b). Nonetheless, it was clear to myself and other LHP participants that, even if Lapita had resulted from a population intrusion originating from farther W, the Lapita cultural complex as it was manifested in archaeological sites of the mid-second millennium BC in the Bismarck Archipelago did involve some element of local cultural borrowing and fusion. Such a revised model for Lapita origins was presented by Roger Green to a group of Lapita scholars at the 14th Congress of the Indo-Pacific Prehistory Association assembled in Jogyakarta in 1990, which he called the "Triple-I" model for "intrusion/innovation/integration" (Green 1991b:298). Some variation of Green's Triple-I model has now been adopted by most Lapita researchers, including Spriggs (1997) and myself (Kirch 1997:46-47). This has been one substantive outcome of the LHP and the Mussau Project.

LAPITA ECONOMY

Also contentious was the matter of what kind of economy had fueled the remarkable Lapita expansion from the Bismarcks eastwards into the previously unin-

habited archipelagoes of Vanuatu, New Caledonia, Fiji, Tonga, and Samoa. If Lapita truly reflected an Austronesian dispersal—as Bellwood proposed—then its economy should have included a significant array of tropical root, tuber, and tree crops, since reconstructed terms for these plants were known for Proto-Austronesian language (Pawley and Green 1984; Blust 1985). Bones of domestic pigs, dogs, and chickens had been recovered from some Lapita sites, although usually in limited numbers (Green 1979a), but direct archaeobotanical evidence for plant cultivation was restricted to carbonized remains of coconut shell. Moreover, most Lapita sites consisted of dense shellfish and fishbone dumps, indisputable evidence for intensive exploitation of marine resources. An evaluation of such sites in Tongatapu led Les Groube to hypothesize that the Lapita adaptation had been that of "Oceanic strandloopers" with a "restricted maritime/lagoonal economy" who "expanded ahead of colonization by agriculturalists" (1971:312). In Groube's view, agriculture (including pig husbandry) was introduced later (at least in Tonga).

In his 1979 synthesis of Lapita archaeology, Green had to adduce indirect evidence in support of his contention that the colonization of truly oceanic islands (lacking food plants in general) had to have included a horticultural component. He cited settlement size and duration of occupation, the presence of subterranean pits probably used for starch paste storage, and a range of material culture typically associated with horticultural societies (cooking ovens, pottery, vegetable scrapers and peelers; see Green [1979a:37]). The presence of pig bones (lacking in Groube's earliest Tonga sites) was also critical evidence in Green's favor. In my own work on the Lapita-to-Polynesian transition as evidenced on Niuatoputapu Island in the northern Tongan archipelago, I had also argued that the founding adaptation had included a dual horticultural-maritime economic base (Kirch 1978).

By the early 1980s, it was also evident that simple *diffusionist* models for the origins of Oceanic horticultural and agricultural systems were no longer tenable (Yen 1971, 1973, 1982, 1991). Golson's excavations at Kuk in the New Guinea Highlands had raised the possibility of an independent invention of agriculture, involving water control in swampy valley floors, as early as 9000 BP (Golson 1977, 1990). Yen, building on earlier studies by Barrau (1965) and other ethnobotanists, drew attention to a wide range of cultivated plants that seemed on botanical grounds to have a Melanesian geographic origin (e.g., such tree crops as *Canarium* and *Inocarpus*, the *Australimusa* section bananas, or the *Cyrtosperma* giant taro). Moreover, there was the nagging problem of rice (*Oryza sativa*) which linguists such as Blust (1996) reconstructed as a key part of the Proto-Austronesian economy, but which had never been important in Oceania. If, as Bellwood averred, Lapita marked an early Austronesian intrusion into the New Guinea region, why had such an important crop as rice not been transferred with them? On the other hand, if Allen's (1984) model of Lapita as emerging out of a much older cultural adaptation to the environment of the Bismarck Archipelago was correct, perhaps an indigenous Melanesian form of horticulture had been a part of the Lapita cultural complex from its inception. These were intriguing, even provocative, ideas but they required a great deal more direct archaeological evidence in order to test hypotheses which were based largely on comparative botanical, ethnobotanical, and linguistic data. One of my great hopes in joining the LHP was that the Mussau sites might provide such archaeological materials.

LONG-DISTANCE EXCHANGE

Soon after he began excavating Lapita sites in the Reef/Santa Cruz Islands, Green (1974) realized that he was dealing with the residue of communities which had been involved in quite complex and in some cases areally extensive networks of trade or exchange. Ambrose and Green (1972) demonstrated that Bismarck Archipelago obsidian had moved southeastwards out to the Reef/Santa Cruz sites, a distance of ~2000 km, part of a suite of imported materials in these sites (Green 1974, 1976). Likewise, in Lapita sites of Fiji and Tonga, there was evidence for the interisland movement of certain materials, such as stone adzes, obsidian, and chert (e.g., Kirch 1978, 1988a). These discoveries had prompted consideration of a "trader" model for Lapita (Clark and Terrell 1978; see Green 1982 in response). Allen (1984), however, had drawn attention to the presence of New Britain obsidian in pre-Lapita sites to suggest that long-distance exchange was in operation in the Bismarck Archipelago from an early date, and that the Lapita networks were only a later manifestation of a long-standing tradition.

Obtaining new empirical data that could bear on the interpretation of Lapita long-distance exchange was thus a key aspect of our Mussau Project, throughout its three seasons. Among the specific questions that concerned us were: Was complex exchange present from the beginning of Lapita occupation in Mussau, or did it develop over time? Were there changes in the qualitative types of exotic materials imported, as well as in their quantitative rates of flow (as reflected in rates of deposition)? Was there any evidence, at any of the Mussau Lapita sites, for specialized production of materials used for exchange? Were exotic materials differentially distributed within the Mussau Lapita sites, in such a manner that might shed light on the social mechanisms or correlates of material exchange?

Clearly a full analysis of Lapita exchange, even within the more restricted ambit of the Bismarck Archipelago, would require carefully controlled data sets from well-dated sites throughout this region, a task well beyond the resources of the Mussau Project, or even the LHP itself. Thus my approach to this problem has been to focus on Mussau as just a single node or locale within a larger set of such nodes, whose linkages and inter-connections have changed over time, changes which can only be incompletely tracked from the Mussau perspective. That is, from the archaeological record of Mussau we can trace the changes which occurred at a single node—albeit one that was likely quite central—within the larger Bismarcks sphere. A great deal of our work has been directed toward this end, through careful analyses of obsidian, ceramics, chert, other exotic stone manuports, and evidence for specialized shell object production at Site ECA. This has allowed us to construct a dynamic model of Lapita exchange, as reflected in this single node, utilizing such analytical variables as content, magnitude, diversity, network size, directionality, centralization, specialization, and complexity (see Plog 1977). What must be left to the future is the combining of other such single node-based analyses, requiring similar studies of other sites and site-complexes, into a broad areal model of Lapita exchange in Near Oceania.

LAPITA SOCIETY

Issues of cultural origins, economic adaptations, or long-distance exchange of goods are all matters potentially resolvable from direct material evidence, to a greater or lesser extent depending on the particular circumstances of the archaeological record in any locality.[5] Inferring the nature of prehistoric social formations is far more problematic, often requiring extensive spatial data and complex chains of indirect inference, argument by ethnographic analogy, and so on. Yet to the extent anthropological archaeologists aspire to be social scientists—or even social historians—understanding the social organization and structure of past societies must be a key objective. Certainly for a phenomenon so central to Oceanic prehistory as Lapita, we should like to be able to lay some claim to knowledge of Lapita *societies* as well as their material base.

The very fact that the Lapita cultural complex exhibits complete disregard for the ethnographic distinction drawn for so long between Melanesia and Polynesia by cultural anthropologists (Thomas 1989) makes the use of ethnographic models much more difficult in this case. This is because ethnographers have typically seen fundamental differences between the social systems of these two regions, Melanesia the home of "big man" societies while Polynesia is the classic locality for the "chiefdom" (Sahlins 1963). How then, could Lapita represent a "foundation" culture from which this diversity of social forms simultaneously arose? How, indeed, except perhaps for the possibility that the ethnographers have been misled to overemphasize difference, and to ignore the "devolution" of many island Melanesian social formations in the face of colonial encounters? Reconsideration of the Melanesian ethnographic record suggests that the "big man" model might more properly be restricted to New Guinea itself, where it is associated mostly with Non-Austronesian speaking peoples (Scaglion 1996).

Prior to the LHP, Brian Hayden (1983) had put forward a rather speculative model for Lapita society as consisting of an early form of chiefship. In some ways more useful was Friedman's (1981, 1982) model of transformation in early Oceanic societies, which although it did not specifically refer to Lapita, clearly applied to the problem of Lapita social formations in its implications. Drawing upon both comparative ethnographic and historical linguistic evidence, Friedman proposed that early Oceanic societies were organized as "prestige-good systems" characterized by the following: "(a) generalized exchange; (b) monopoly over prestige-good imports that are necessary for marriage and other crucial pay-

ments, i.e., for the social reproduction of kin groups; (c) bilineal tendency in the kinship structure (asymmetrical); and (d) tendency to asymmetrical political dualism: religious-political chiefs, original people-newcomers, etc." (1982:184). The significance accorded to prestige-good exchange by Friedman is noteworthy to the archaeologist, for it opens up one possible *material* line of investigation into Lapita social structures. During the Mussau fieldwork, we attempted to pursue this avenue through investigation of *internal spatial differentiation* in Lapita sites, especially the large ECA Site.[6]

Subsequent to the Mussau fieldwork itself, another kind of model for Oceanic societies has energized comparative ethnographic analyses, and also has considerable promise for archaeology and prehistory. This is the concept of the "house society," deriving from Claude Lévi-Strauss' original proposal of the *société à maison* (1982), and now being applied by a number of ethnographers to the analysis of Austronesian societies (e.g., Carsten and Hugh-Jones 1995; Fox 1993; Fox and Sather 1996; Kirch 1996; McKinnon 1991; Waterson 1990, 1995). In such "house societies," the fundamental social unit is typically a group of people (not all of whom are necessarily consanguines or affines) who affiliate to a "house" which endures through time, carries a proper name, is associated with an estate of land, has its own prerogatives and rituals, and so forth. As Fox explains:

> Throughout the Austronesian-speaking world, houses are given great prominence .... Although a house has a physical referent, the category of "house" may be used abstractly to distinguish, not just households, but social groups of varying sizes. The "house" in this sense is a cultural category of fundamental importance. It defines a social group, which is not necessarily the same as the house's residential group (1993:1).

Application of the "house society" concept in Oceanic archaeology is still embryonic, although Green and Pawley (1998) have productively used Proto-Oceanic historical linguistic reconstructions and archaeological data from Lapita sites to argue for important transformations in Lapita houses, with social implications. I have likewise proposed that the Lapita peoples ordered their social world around "houses" in which ancestors played a central role, and that the anthropomorphic representations so commonly displayed on early Lapita pottery were linked to a cult of ancestors (Kirch 1997:132-44, 188-91).

WHAT HAPPENED TO LAPITA AFTER 500 BC *?*

A final issue that concerned us was in some ways as perplexing as that of origins: what had become of Lapita after about 500 BC, when throughout the southwestern Pacific the distinctive ceramic series seemed to disappear, often quite abruptly? For Western Polynesia (Samoa, Tongatapu, Niuatoputapu, Futuna, and 'Uvea) this was not a problematic issue, for a continuous ceramic sequence beginning with an Early Eastern Lapita phase, and ending with Polynesian Plainware, had been demonstrated at many sites (Green 1974b; Davidson 1979; Kirch 1988a). The clear implication was that Lapita had not ceased to exist; rather, it was transformed into something new, which we called "Ancestral Polynesian" culture (Kirch 1984; Kirch and Green 2001). There was direct cultural continuity, and the Eastern Lapita culture was directly ancestral to the later ethnographically attested cultures of Polynesia.

Outside of Western Polynesia, however, continuity between Lapita and post-Lapita phases was less clear-cut, or in some cases ambiguous (Spriggs 1984). In Fiji, the break between Lapita (Sigatoka Phase) and Navatu Phase (paddle-impressed) ceramics was seen by some as evidence for cultural intrusion and possibly replacement (e.g., Frost 1979), although others were inclined to see continuous development. In Vanuatu, Garanger (1972) had excavated sites with distinctive incised, relief, and appliqué decorated ceramics whose relationship—if any—to Lapita was quite uncertain. Chronological issues regarding Mangaasi were also unclear, so that whether Mangaasi followed Lapita temporally or had overlapped with the latter, was unresolved (Spriggs 1984; Green 1985). In New Caledonia, the work of Frimigacci (1975), building upon the earlier excavations of Gifford and Shutler (1956), revealed incised and appliqué ceramics with apparent Mangaasi affinities that likewise followed after Lapita. However, in New Caledonia issues were further complicated by the possibility that a plainware with paddle-impressing (termed Podtanéan by Green and Mitchell 1983) had been contemporaneous with Lapita, giving rise to the possibility that Podtanéan, and not Lapita, had been the direct antecedent to the later incised wares.

In the Bismarck Archipelago itself the transition from Lapita to whatever came after was wholly enig-

matic. Much of the region appeared to have been aceramic during the past two thousand years, although pottery production was ethnographically documented for certain locales, such as Manus (May and Tuckson 1982). Specht (1968, 1972) had defined a two-thousand year ceramic sequence for Buka in the nearby NW Solomons, but whether this represented a continuity from Lapita was uncertain. Likewise, Vanderwal (1978) and others had proposed that the earliest ceramics along the Papuan coast had a probable origin in Lapita, but this putative link was not empirically established. In sum, for most of Near Oceania, it was not possible to archaeologically test the competing hypotheses that later, ethnographically attested cultures had their origins in Lapita (as was the case in Western Polynesia) or, that there had been significant cultural replacements after about 500 BC.

This issue could only be resolved through the time-consuming work of local sequence definition in many different localities throughout the Bismarcks and adjacent parts of Near Oceania. Well-stratified and accurately dated ceramic as well as non-ceramic assemblages were required to test the possibilities of continuity or of more abrupt change that would imply cultural replacement. Our own contribution to this effort would therefore have to be the definition of such a sequence for Mussau, based on the excavation of sites that carried the local cultural record forward in time into the last 2,500 years. We hoped that it might indeed be possible to find and excavate a series of sites that would yield a continuous sequence bridging the Mussau "ethnographic present" with Lapita, through a series of intermediate phases. This would permit the application of the "direct historical approach" to Mussau prehistory, a much more powerful method than mere ethnographic analogy (Trigger 1989). Thus while our greatest emphasis was always on Lapita sites per se, we did devote considerable energy to the excavation of non-Lapita sites, especially during the 1986 and 1988 field seasons.

THE MUSSAU PROJECT: SPECIFIC RESEARCH DESIGN AND STRATEGY, 1985-1988

While the five major problems discussed above oriented our research throughout its promulgation over several years, the specific objectives and research strategies we applied on site and in the lab evolved substantially over the course of the three field expeditions, and through our continued studies. As some problems and questions were resolved others would arise. To give some flavor of this continuing and constantly changing context within which our research was conducted, I describe below each of our field expeditions along with the specific objectives that we outlined for each season, and a review of the work actually accomplished. This account has been drawn primarily from our several grant proposals to the National Science Foundation, National Geographic Society, and Wenner-Gren Foundation for Anthropological Research (Kirch 1985, 1986, 1987b), from preliminary field reports submitted to these agencies and to PNG government officials, and from my field diaries.

THE 1985 MUSSAU EXPEDITION

By 1985, substantial funding for the LHP had been secured (from the National Geographic Society, Australian National University, and other sources), and some 19 separate field projects involving 24 qualified archaeologists were scheduled to be undertaken between May and September (Allen 1991:3). Transportation was largely provided by the LHP's chartered vessel, the *Dick Smith Explorer*, a 65-foot steel-hulled yacht which would permit field teams to travel to remote localities independent of the frequently tenuous local means of transport.[7] Likewise, most excavation and survey gear, and food, was to be centrally provisioned.[8] Among the archaeological innovations were the use of standardized, pre-printed field recording forms (a duplicate set of which were to be retained centrally at the Australian National University), coordinated radiocarbon dating through the ANU Radiocarbon Laboratory, and an agreement among project participants to submit samples of their obsidian specimens for centralized analysis at the Australian Atomic High-Energy Commission's Lucas Height's PIXE-PIGME laboratory (Bird et al. 1978; Duerden et al. 1980).

The Mussau sub-project of the LHP was under my direction, and scheduled for approximately two months from late July through late September, 1985. Mussau had been an obvious choice for LHP fieldwork because it was one of a few localities within the Bismarck Archipelago already known to have two Lapita

sites, based on limited reconnaissance survey and test excavations by Brian Egloff and other staff of the PNG National Museum in 1973 and 1978 (Egloff 1975; Bafmatuk et al. 1980). Jim Allen and Jim Specht had briefly visited Mussau in 1984 on their Bismarck reconnaissance, making contact with local officials and finding a third Lapita deposit (Allen et al. 1984:8-11).[9] Egloff had declined an offer to continue work on the Eloaua Lapita sites under LHP auspices, and Allen suggested that I might therefore begin where Egloff's earlier fieldwork had left off, a plan I concurred with. Specifically, I resolved to do several things: First, I wanted to fill in some critical information on the ECA and ECB Lapita sites, such as defining their exact areal size and extent, the nature of their stratigraphic sequences, and their chronologies.[10] Second, I wanted to test the newly discovered EHB Site on Emananus Island, to see if this contained similar ceramics and other cultural materials to ECA and ECB. Third, I proposed to carry out a more thorough survey of the Mussau group, concentrating especially on the cluster of smaller islands to the SW of the main island, these being the most likely location for Lapita settlements.[11]

On July 22, 1985, I flew from Seattle, Washington, (where I was then Director of the Burke Museum at the University of Washington) to PNG, spending several days in the capital of Port Moresby to meet with staff of the University of Papua New Guinea and of the National Museum and Art Gallery.[12] Plans were made to have the National Museum's Assistant Curator of Anthropology, John Saulo (a Mussau islander himself), join me later during the field season. I flew to Kavieng, the provincial capital of New Ireland Province on July 30, and spent a day visiting the in-progress excavations at Panakiwuk rockshelter some distance down the coast, under the direction of LHP members Jim Allen and Chris Gosden.[13] Awaiting me in Kavieng were Sally Brockwell and Pru Gaffey, two archaeology students from the Australian National University, who had signed on as assistants with the LHP and who would accompany me in that capacity to Mussau.[14] In the late afternoon of July 31, after a hectic day assembling additional gear and food supplies from the Chinese trade stores in Kavieng (and withdrawing in small bills our entire field budget for wages, there being no banking agency in Mussau), the *Dick Smith Explorer* weighed anchor and set her course for Mussau.

The passage was wet and squally as the ship plugged along at 6 knots under a combination of sail and motor power, but we avoided a heavy thunder and lightening storm centered over New Hanover Island to the SW. Just after dawn the 650-m high peak of Taleanuane on "Big Mussau"[15] was sighted ahead, and Captain Taffy Rowlands posted a lookout to the masthead, since prior experience had shown that the World War II vintage Admiralty Hydrographic charts of these seas were often highly inaccurate with regard to reefs. An hour later the *Dick Smith Explorer* was running up the Malle Channel, fairly close off the SE coast of Eloaua, and I could readily discern from the characteristic "stair-step" topography that the island consisted of several elevated limestone terraces. We dropped anchor outside the reef off of Eloaua Village, and soon five or six dugout canoes came out to greet us, one bearing Eric Kop, the local Council member for Eloaua and Emananus. I went ashore with Eric who introduced me to Ave Male, uncle of John Saulo and traditional leader of the Eanaiyu clan which claimed the ECA Site area (Fig. 1.3). Ave Male had hosted Egloff on his previous field trips, and readily agreed to our proposed work as well, generously offering to put one of his two houses at our disposal.[16] Delighted that local arrangements could be so quickly secured, we returned to the ship, offloaded our 14-foot aluminum launch (*Lapita*, as she was named, powered by a 25-hp Johnson outboard, gave us the ability to freely reconnoiter the various islands of the Mussau group) and our gear. The *Dick Smith Explorer* did not tarry, hoisting anchor at 11:30 am for a quick return to Kavieng.

Over the next several days, Sally, Pru and I reconnoitered Eloaua and Emananus Islands, locating the ECA, ECB, EHB sites and making arrangements with the local land-holders to excavate at each of these. It was clear to me that the ECA Site—by far the largest of the three Lapita sites we had visited—should be the first to demand our attention, and we commenced work at ECA on August 6, assisted by a crew of nine Eloaua villagers. As the details of our archaeological fieldwork at ECA are given in Chapter 4, they will not be repeated here. Suffice it to say that my initial plan was to use a series of widely spaced transect excavations to define the site's areal extent and to clarify stratigraphy. Not only was this field strategy successful for those objectives, it led us to discover that the area with Lapita-ceramic deposits was not confined to the approximately 2-m high former beach terrace as suggested by Egloff

FIGURE 1.3 Senior leaders of the local Eloaua community lent their support to the Mussau Project from the first field season. From right to left, Ave Male, traditional landholder of the Talepakemalai site; Ororea; Aimalu Lavatea, traditional landholder of the ECB site; and, Bauwa Sagila.

(1975), but that waterlogged deposits continued into a lower (1-m asl) sandy flat to the N of the airfield. Here, over the next few weeks, we gradually opened a 12-m^2 excavation—designated Area B—yielding a stunning array of classic decorated Lapita pottery (including many partially reconstructable vessels, some with anthropomorphic face designs), shell objects, obsidian, and other small finds (Fig. 1.4). Most remarkably, we uncovered the anaerobically-preserved bases of 16 wooden posts or stakes in the waterlogged layer, indicating that our Lapita settlement had originally consisted of stilt houses constructed out over a tidal reef flat. This was the first clear indication of such a settlement pattern for Lapita (although Chris Gosden would soon find similar preserved posts in his Arawe Islands sites off New Britain), and was tremendously exciting.

These unanticipated discoveries at ECA resulted in our concentrating primarily on that site during the course of our 51 days in Mussau (during that time we opened up 41 m^2 of test pits and areal excavations at the ECA Site). However, we also took time out to survey and test excavate the ECB and EHB sites, both of which proved to be quite small in extent and lacked the waterlogged deposits found at ECA. We also made reconnaissance surveys over much of Eloaua and Emananus islands, to Emussau, Ebolo, and Boliu islands, and to parts of the main Mussau Island. In mid September we heard via radio that the *Dick Smith Explorer* was experiencing engine trouble in Manus, and that we could not depend on her for transport back to Kavieng. We packed 17 boxes with pottery and other samples, leaving them in Ave Male's care until the ship could be repaired and pick them up on her return to New Ireland. After a farewell feast with the Eloaua villagers,[17] Sally,

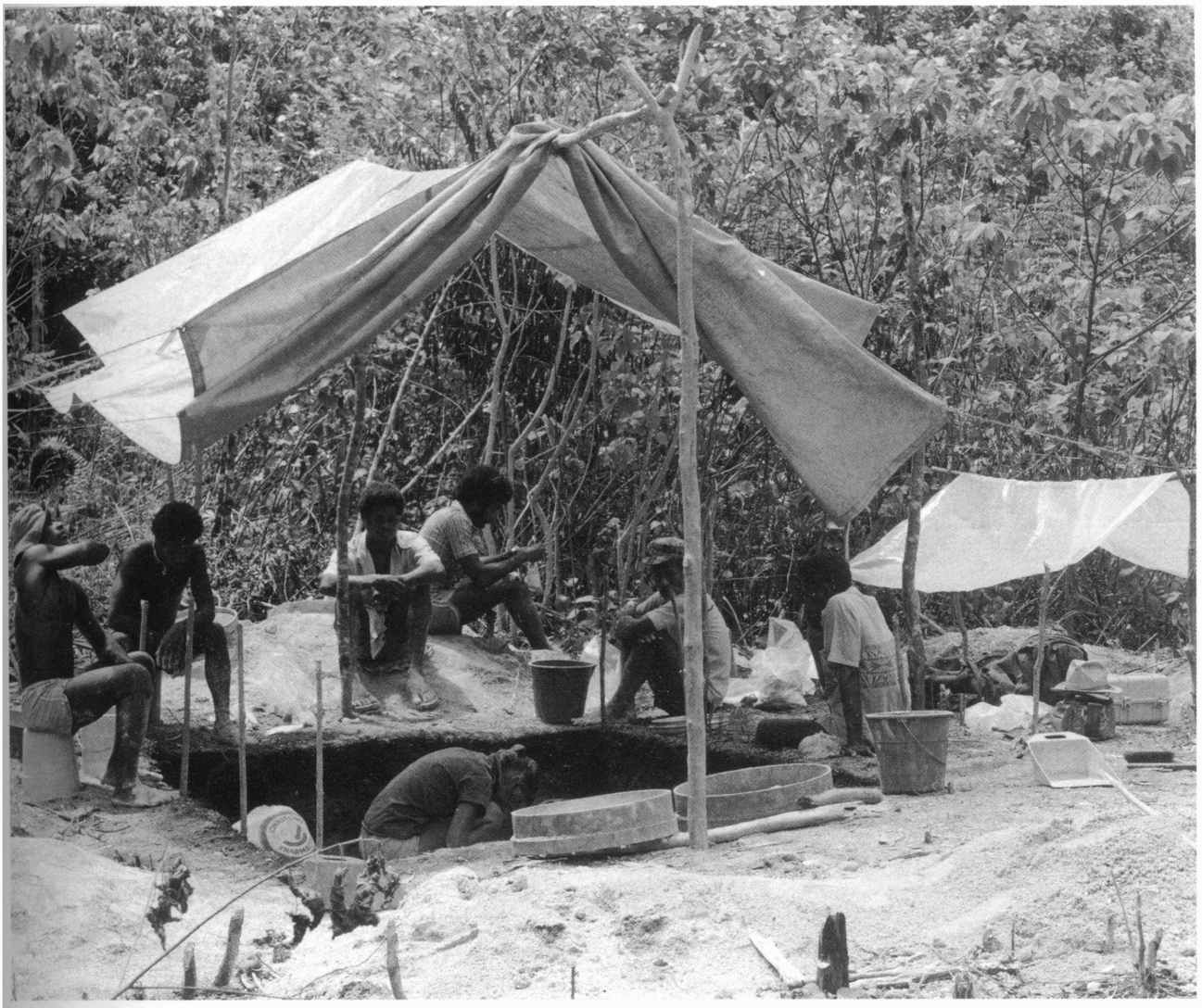

FIGURE 1.4 Excavations in progress at Area B of the Talepakemalai (ECA) Site, 1985. This initial 6 m^2 excavation revealed the first anaerobically preserved remains of wooden stilt houses of the Lapita period.

Pru, and I flew out to Kavieng on the Talair 6-seater Baron, on September 20. In my field journal, I was already making detailed logistical notes for a return visit.[18]

THE 1986 MUSSAU EXPEDITION AND ITS OBJECTIVES

After returning to Seattle, and as soon as my responsibilities as Burke Museum Director permitted, I prepared and submitted a research proposal to the Committee on Research and Exploration of the National Geographic Society (NGS). This proposal outlined plans for a second field season in 1986, with several objectives: (1) expansion of the Area B excavations at ECA, to expose more of the stilt-house structure and its associated materials; (2) further sampling of other portions of ECA, especially those dominated by plainware ceramics; (3) additional, systematic sampling of the ECB and EHB sites, combined with additional survey for Lapita sites; and (4) systematic sampling of one or more post-Lapita shell middens which had been discovered during the 1985 reconnaissance forays. The NGS awarded this grant, and with the budget of $22,500 assured, I was able to make plans for a return to Mussau

in late August of 1986.

During the interval between the 1985 and 1986 field seasons I also began laboratory analysis of the 1985 season collections, which had in due course been air freighted to Seattle after transport by the *Dick Smith Explorer* from Mussau to Sydney.[19] A Mussau Project laboratory was established in the Burke Museum, and cataloging of the large volume of ceramic sherds and other materials commenced, with financial support from the Wenner-Gren Foundation for Anthropological Research, which had awarded me a grant of $6,211 for this purpose. Meanwhile, 15 samples of wood, charcoal, and shell were submitted to the Radiocarbon Laboratory at the ANU for dating, another admirable aspect of the LHP's central coordination. Before departing Seattle for the 1986 field season, I completed a preliminary report on the 1985 results for submission to the *Journal of Field Archaeology* (Kirch 1987a). In addition, a grant proposal was developed and submitted to the National Science Foundation (NSF) to support a phase of intensive laboratory analysis of the combined 1985 and 1986 collections (subsequently funded in January 1987, for $59,920).

To assist me in the 1986 season, I invited the participation of two University of Washington Anthropology graduate students, both of whom had prior field experience in the Pacific: Terry L. Hunt and Marshall I. Weisler. My plan was that Hunt and Weisler would assist me in the expanded ECA Site excavations, then undertake additional work at ECB, EHB, and other sites under their own direction.[20] Based on my 1985 experience, I also made a number of critical logistical arrangements for the 1986 season. I purchased a 12-foot Metzler "Maya-S" inflatable dingy which we airfreighted to PNG, to assure our own local interislet transport,[21] and also procured three hand-operated marine bilge pumps to allow us to better excavate the waterlogged deposits at the ECA Site. Along with a large supply of waterproof labels, plastic bags, and a great deal of cotton wool for packing the fragile pottery, we were now far better equipped to handle the particular challenges of excavating at ECA.

Accompanied by Hunt and Weisler, I arrived in Port Moresby on August 24, 1986 and after visits to the University and National Museum, we flew to Kavieng on August 26. Three days were spent procuring a variety of supplies (including arranging for a large drum of gasoline to be shipped by small boat to Mussau[22]), and picking up a 4-hp Mariner outboard engine which I had ordered. I also renewed acquaintances with Mr. Drew Wright of PNG Fisheries Research, whom I had met in 1985 and who was interested in our evidence for prehistoric fishing as recovered from faunal remains from the Lapita sites. Wright agreed to send his assistant, John Aini, up to Mussau later on during our field season to help us collect contemporary fish specimens for reference materials.

On August 29, we flew to Eloaua on a chartered Talair "402" aircraft loaded to its maximum capacity of 650 kg with gear and supplies, including our inflatable boat. We were again warmly received by brothers Ave and John Male, and other members of the Eloaua community, and re-established our field quarters at Ave's house. After a few days of orienting Hunt and Weisler, including a trip to Lomakunauru on the main island, we recommenced excavations at ECA on September 1, which continued until September 25. Again, as details of the 1986 season at ECA are given in Chapter 4, here I need only comment that we were able to expand the Area B excavation—with the stilt-house remains—to 24 m^2, obtaining a greatly increased sample of decorated ceramics, shell objects, and other materials (Fig. 1.5). The marine bilge pumps worked admirably both to keep the excavation units free of standing water, and to provide a steady flow for wet screening (Fig. 1.6). In addition, we dug a number of additional transect units, greatly clarifying the geomorphology and micro-stratigraphy of the site. One of the remarkable results of the 1986 season was a vastly increased sample of anaerobically-preserved plant remains, including many species of domesticated nuts and seeds, particularly from certain organically-rich deposits just S of the Area B excavations. In analyzing these materials, I was greatly assisted by Ms. Holly McEldowney, an ANU doctoral candidate who was carrying out her own fieldwork in Manus, and who was able to join us in Mussau from 17 September until 3 October. McEldowney and I collected modern reference specimens of most of the plant taxa represented in the ECA deposits, and made preliminary identifications of these materials in the field. Mr. John Aini of Fisheries Research also joined us from 5-19 September, assisting us in preparing a large number of fish skeletal reference specimens which were to prove invaluable in the subsequent identification of our archaeological fish fauna by Virginia Butler (see Volume II).[23]

FIGURE 1.5
Excavations in progress at the expanded Area B locus, Talepakemalai (ECA) Site, 1986. Note the standing water which had to be pumped continuously to keep the excavation dry.

After closing down the ECA excavations on 25 September, I continued to work in our field lab (Fig. 1.7), processing shell midden and ceramics, while assisting Hunt and Weisler in beginning their own excavation projects (Figs. 1.8 and 1.9). Several small cave and rockshelter sites on Eloaua (EHM, EHN, EKO) were tested by them during the last week of September (see Chapter 5). Hunt began renewed transect excavations at the ECB Site on October 6, and later carried out expanded transect tests at EHB on Emananus Island (see Chapter 4). Meanwhile, Weisler excavated a late prehistoric midden site (EKS) on Emussau Island from 5-9 October (see Chapter 7).

On October 2, we organized a reconnaissance trip from Eloaua Island to Tanaliu on the far NW coast of Mussau, 30 km distant by sea. This was too far to travel with our little Metzler inflatable and its 4-hp motor, so the large dugout canoe *Two Mile* was engaged for the trip (Fig. 1.10). One objective of this trip was to locate a reputed source of clay inland of Tanaliu that might have been used by prehistoric potters on Mussau. Leaving Eloaua at dawn we made Tanaliu by 10:30 am, and were guided by several Tanaliu villagers to the clay source, some two hours walk inland at an elevation of ~250 m asl. A sample was procured for later XRF analy-

FIGURE 1.6 Naomi Kavi and Liah Aite operating one of the marine bilge pumps which we adapted for use in the Talepakemalai excavations.

FIGURE 1.7 The field laboratory was set up adjacent to Ave Male's house in Eloaua Village, which was also our home throughout the Mussau Project. Here shell midden is being sorted, counted, and weighed, while potsherds dry in the sun.

FIGURE 1.8 Marshall Weisler records stratigraphy in one of the ECA transect units.

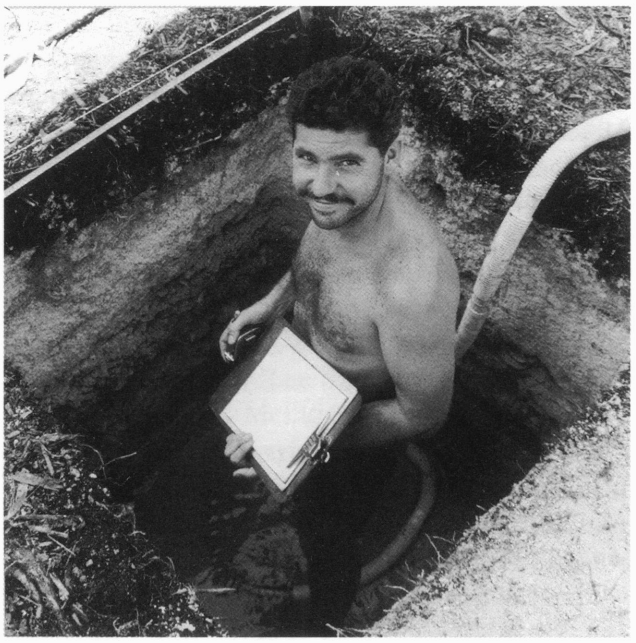

FIGURE 1.9 Terry Hunt excavating at the ECA Site (note the pump hose necessary to keep the pit from flooding).

FIGURE 1.10 Loading the dugout canoe *Two Mile* for a reconnaissance trip to the N end of Mussau Island. In the distance, the large island of Mussau displays its classic "stair step" topography of uplifted limestone terraces.

sis and comparison to our prehistoric ceramics. During the day, we also were shown two large rockshelters, one of which (Site EKQ) had calcareous sand-tempered sherds on its surface and looked especially promising for excavation. Arrangements were negotiated for Weisler to return to Tanaliu and spend several weeks excavating the EKP and EKQ rockshelters. We returned from our successful Tanaliu reconnaissance well after dark, having a slightly hair-raising experience of trying to find the narrow passage through the reef off Eloaua without the benefit of lights!

I flew out of Eloaua on October 10, 1986, leaving Hunt and Weisler with their respective field objectives of excavations at ECB and EHB, and at the newly discovered EKP and EKQ rockshelters; the two of them remained in the field carrying out this work until November 7. As previously, we were given permission by the PNG National Museum to export all collections to Seattle for intensive analysis, a phase of the Project which was to occupy the next two years.

LABORATORY INTERLUDE, 1986-87

By the close of the 1986 field season, we had obtained a significant sample of materials from ECA and eight other sites, including approximately 60,000 potsherds, nearly 3,000 obsidian flakes, about 800 non-ceramic portable artifacts, 21,726 vertebrate faunal remains, and more than 5,000 anaerobically-preserved plant remains. Fortunately, the NSF proposal which I had submitted prior to departing for the 1986 field season was awarded effective January 1, 1987, providing funds for three graduate research assistants at the University of Washington to help conduct specialized analyses of ceramics, obsidian, and fishbones, and for an undergraduate assistant to help with cataloging and database preparation. The grant also included funds for a zooarchaeological consultant (Dr. Alan Ziegler, a specialist in New Guinea fauna), and necessary laboratory supplies and equipment.

The research design for this analytical phase of the

Mussau Project was oriented around five specific objectives, which I posed as a series of questions. These are given below exactly as they were specified in the NSF proposal (Kirch 1986), with comments on the laboratory methods we used to address them:

1. *In morphological (functional) and stylistic terms, what was the sequence of Lapita ceramic change in Mussau? Is a distinct, "formative" Lapita phase indeed recognizable on ceramic criteria? What are the formal relationships between the Mussau Lapita assemblages and those known from other sites in the Bismarcks and from sites farther to the east?* Answering these questions required a formal, descriptive analysis of the large sample of excavated ceramics, using the procedures and systems developed by Rye (1981), Mead et al. (1973), Donovan (1973), Anson (1983) and others (e.g., Kirch and Yen 1982) for Oceanic ceramic assemblages. Important data classes included vessel manufacture and forming processes, vessel form and details of rim and lip variation, and formal attributes of motifs and design fields. An attribute-based approach was chosen, and data were coded for entry into a computerized database, initially using the MINARK application (for subsequent changes in our database software as the Mussau Project evolved, see Chapter 3). Before we were able to undertake this analysis, however, it became evident that we would need to do emergency conservation treatment of the thousands of sherds from the waterlogged ECA deposits, as these were beginning to exude salts and in some instances were rapidly deteriorating. Consulting with Objects Conservator Laura Word of the Bishop Museum, we decided to hand treat every sherd by applying a consolidant (B-72 Acryloid) to all exterior surfaces. This was a major task, requiring hundreds of hours of painstaking work, all of which had to be done under a fume hood because the consolidant and the acetone in which it was suspended were both toxic.[24]

2. *What is the extent of heterogeneity in Mussau Lapita ceramic composition? To what extent were Mussau ceramics locally manufactured or imported? What were the changing configurations of ceramic importation over time?* The matter of exchange or trade of Lapita pottery had already emerged as a major topic of discussion and debate, in part from Green's earlier work in the Solomon Islands (1976), and from Anson's (1983, 1986) studies of "Far Western" Lapita ceramics. However, with the exception of Anson's pioneering application of X-Ray fluorescence (XRF) compositional analysis, virtually all work on the characterization or sourcing of Lapita ceramics had been confined to petrographic analysis of temper or nonplastic inclusions, through the examination of ceramic thin-sections under polarized light (Dickinson and Shutler 1979). For the Mussau ceramic assemblages, we proposed a staged sampling procedure, beginning with macroscopic sorting, moving to binocular examination of temper and paste characteristics, and finally to compositional analysis using energy-dispersive X-Ray fluorescence (ED-XRF) by means of a scanning electron microscope (SEM) microprobe. This technique was fortunately available to us at the University of Washington, and Terry Hunt was engaged to carry out this research, which became the basis of his doctoral dissertation (Hunt 1989).

3. *What range of exotic lithic materials were imported into the Mussau Lapita communities, and what temporal changes occurred in the diversity and frequency of such imports? To what extent did the importation of exotic lithics change after ~ 2500* BP *with the cessation of classic Lapita?* Allen (1984) had drawn attention to the movement of obsidian within the Bismarck Archipelago well before Lapita, a finding that was amplified by results of excavations in New Ireland rockshelter sites during the LHP in 1985. It was also well known that obsidian had been a significant component of Lapita long-distance exchange as far to the E as the Santa Cruz group (Ambrose and Green 1972; Green 1976, 1979a). But the changing configurations of obsidian (and other lithics, such as chert) movement between Lapita communities needed to be worked out in detail, and with extensive samples. While we took full advantage of the LHP program of PIXE-PIGME sourcing of obsidian from our Mussau assemblages, it was not possible to use this expensive technique on all of the approximately 3,000 obsidian specimens obtained. Thus I proposed to apply the method of rapid heavy liquid (sodium metatungstate) density sorting which had been developed by Roger Green (1987), taking advantage of the fact that the main Melanesian sources (Talasea and Lou) could be—at least partially—discriminated on this specific gravity basis. Melinda S. Allen, then a graduate student at the University of Washington, agreed to take on this project (see Volume III).

In addition to the obsidian, the ECA and other site excavations had yielded a collection of some 300 or so "manuports" (many of which were evidently oven stones), revealing a considerable diversity of lithologic types. A further objective was to at least characterize—

if not source—these using relatively standard petrographic methods. For this work Prof. John Sinton of the University of Hawaii Institute of Geophysics was engaged as our initial consultant, although the work would ultimately be completed by Prof. William Dickinson of the University of Arizona.

4. *What was the extent of marine exploitation in Mussau Lapita economy? What diversity of resources and biotopes were exploited, what was the intensity of exploitation, what specific strategies were practiced, and how did these change over time? Can the long-term effects of human predation of reef-lagoon resources be detected archaeologically?* This objective was seen as critical to several key problems in Lapita archaeology. It was, for example, relevant to the Lapita origins debate, since it had been suggested that sophisticated marine exploitation was an aspect of early Austronesian development in island Southeast Asia (Bellwood 1985). Second, the ability of the Lapita economy to support an unprecedented phase of dispersal and colonization into Remote Oceania must have required, in part, a successful adaptation to tropical reef-lagoon ecosystems. There was also the related question of human exploitation pressures on marine and littoral resources, and whether Lapita populations practiced a kind of "strandlooper" adaptation (e.g., Groube 1971).

During the 1985 and 1986 field seasons, we had obtained a large assemblage of faunal materials, the vertebrate faunal material all being returned to our Seattle laboratory, while close to a metric ton of molluskan remains had been identified and weighed in the field (with samples returned to confirm identifications). We had also spent time in 1986 collecting modern fish reference skeletons which, when combined with a reference set I had made on Tikopia in 1977-78 (deposited in the Bishop Museum, Honolulu), allowed us to identify a large number of the excavated fishbones.[25] The time-consuming task of sorting and identifying more than 15,000 fishbones was undertaken by Virginia Butler, then a University of Washington graduate student pursuing an interdisciplinary Ph.D. in zooarchaeology and ichthyology. Butler continued to work on the fish fauna from Mussau for the next three years, eventually preparing a remarkably thorough study and analysis (see Volume II).

5. *To what extent were horticultural production and animal husbandry components of the Mussau Lapita economy? Is there evidence for the development or intensification of terrestrial production over time?* During the 1970s and early 80s, there had been considerable debate as to whether Lapita communities depended entirely on marine exploitation for their subsistence, or whether they also possessed a horticultural complex (Groube 1971; Green 1979a; Kirch 1978, 1979, 1982). Our discovery that the waterlogged deposits at ECA contained large numbers of well-preserved plant remains, especially of various Oceanic tree crops such as *Canarium* almond, *Terminalia* almond, or Tahitian chestnut (*Inocarpus fagiferus*) had greatly strengthened the arguments in favor of a horticultural, and more specifically an arboricultural, basis for Lapita economy. Further laboratory study of the approximately 5,000 plant remains, along with secondary evidence such as shell peeling and scraping tools, and the bones of domestic pig and chicken, was thus seen as another important aspect of our Project. I took full charge of this work, having experience in Pacific ethnobotany gained in part through my long association with Douglas Yen. A preliminary analysis of the ECA plant remains was submitted for publication in 1988 (Kirch 1989).

While this intensive phase of laboratory analysis was in its early stages, in the spring of 1987, I organized a graduate seminar at the University of Washington on the topic of the Lapita cultural complex, in which all of the graduate research assistants were enrolled, along with several others with interests in Oceanic prehistory. My objective for the seminar participants was to critically read, review, and evaluate every available report, paper, or monograph dealing with Lapita—published or unpublished—to arrive at an assessment of the state of our knowledge. Just assembling the scattered literature was a formidable task. Seminar participants divided up this large domain topically, Virginia Butler for example taking on fish faunal analyses, Melinda Allen and Gwen Bell reviewing lithic assemblages, and Terry Hunt assessing what had been done with ceramic technological and compositional studies. The exercise proved to be rewarding beyond my expectations, with each participant producing a thoughtful, in-depth evaluation of our knowledge in some eight areas, including spatial and temporal boundaries of the cultural complex, environmental correlates of settlement patterns, ceramic and lithic sourcing and technological studies, a reconsideration of the Mead system for analyzing Lapita designs, and vertebrate and invertebrate faunal assemblages. Not only did this provide us with the necessary comparative base in which to place the new Mussau evidence, but collectively it amounted to a thorough, yet critical re-

view of the *Archaeology of the Lapita Cultural Complex*, and was published under that title by the Burke Museum (Kirch and Hunt, eds., 1988).

By the end of 1987, the laboratory team which I had assembled at the Burke Museum had made substantial progress in meeting the objectives listed above. We had prepared a preliminary synthesis of the results from both field seasons for publication in the final report of the LHP (Kirch et al. 1991), as well as several other papers reporting specific results (Kirch 1988b, 1988c, 1989; Kirch and Hunt 1988b; Kirch et al. 1987; Kirch, Swindler and Turner 1989). It had also become evident to me, however, that a third field season would be essential to fully resolve a number of the research questions which had emerged in the course of our work. Thus in December of 1987 I submitted a second proposal to NSF, requesting $94,663 to support a third expedition to Mussau, along with an additional phase of laboratory analysis. NSF awarded the grant on June 1, 1988, and plans were draw up for fieldwork from September through November.

THE 1988 MUSSAU EXPEDITION AND ITS OBJECTIVES

In my 1987 NSF proposal, I specified five new objectives for the third phase of the Mussau Project, which I again reproduce here verbatim. These provided the framework around which our 1988 field season was organized.

1. *What is the internal spatial structure of the ECA Site as evidenced architecturally, and by the distribution of cultural materials? To what extent does spatial differentiation indicate functional specialization?* In my proposal, I observed that "Lapita sites are notorious for their lack of meaningful stratigraphy, extensive post-depositional disturbances, and paucity of architectural features" (Kirch 1987b:9). Only Green's RL-2 Site in the Reef Islands (Green 1976, 1978), and two incompletely-reported sites in New Caledonia (Frimigacci 1980), had yielded significant architectural or spatial-distribution data on Lapita settlements. In this context, the ECA Site was seen to have particular significance, because: (1) at more than 70,000 m^2 area ECA is the largest Lapita site on record, with the greatest potential for internal structural differentiation; (2) it was the only Lapita site then known to have well-preserved evidence of wooden architecture; and (3) our prior excavations had demonstrated some degree of spatial differentiation in the distribution of ceramics and other artifact classes. We therefore proposed to continue excavations at ECA in 1988, with particular emphasis on defining the full extent of stilt-house occupation at the site, and carrying out further systematic transect excavations that would help to refine our knowledge of the site's internal structure.

2. *What were the technical processes and reduction sequences used to manufacture shell artifacts at ECA? To what extent was the production of shell 'ornaments,' fishhooks, or other artifacts functionally specialized within the site?* During the course of the 1986 field season, and more particularly as I analyzed the sample of excavated material culture in the laboratory, I had come to the conclusion that the ECA Site had been a specialized center for the manufacture of certain kinds of shell artifacts. Many such shell 'ornaments' had been found at other Lapita sites, but little attention had been paid to their manufacture, or to the possibility that rather than being merely objects of bodily adornment, these had functioned as "exchange valuables" in a manner directly analogous to the shell valuables known ethnographically from many parts of Melanesia. A comparative analysis of data from 10 excavated Lapita sites (Kirch 1988c) convinced me that while many sites yielded small quantities of such finished objects, only three sites (including ECA) showed evidence for their manufacture. This strongly suggested that certain Lapita communities, including those in Mussau, might have specialized in the production and exportation of shell exchange valuables, perhaps in opposition to the flow of imported ceramics, obsidian, or other exotic materials.

This hypothesis, however, needed to be tested against further data from the ECA Site, in particular by paying close attention to manufacture detritus from such mollusk species as *Conus litteratus*, *C. leopardus*, *Trochus niloticus*, *Spondylus* sp., and *Tridacna maxima*, from which the putative 'valuables' were made. In prior field seasons we had tended to treat such materials (unless they were obviously manufacture stages of shell artifacts) simply as components of the site's shell midden. In the 1988 season, my objective was to pay close attention to the quantities of incipiently worked or rejected material of these taxa, as well as to the more obviously recognizable stages of artifact reduction, broken specimens, and completed items. My goal was to be able to estimate the quantities of shell being worked at the site (and within particular areas of the site), as well as to

describe the full sequence of production of these important artifact classes. I hoped that in doing so we would open up the analysis of Lapita long-distance exchange to incorporate another important component, beyond pottery and obsidian which had received the majority of archeological attention.

3. *What was the extent of marine invertebrate exploitation in Mussau Lapita economy? What diversity of resources and biotopes were exploited, what was the intensity of exploitation, what specific strategies were practiced, and how did these change over time? Can the long-term effects of human predation of reef-lagoon resources be detected archaeologically?* Although by 1987 our database for vertebrate and invertebrate marine fauna from ECA was already the largest for any Lapita site, our analyses had raised several questions that required further fieldwork to answer. Moreover, Nagaoka's (1988) review of shell midden analyses from Lapita sites throughout the SW Pacific revealed that despite sweeping generalizations regarding Lapita marine exploitation, a mere handful of sites had any quantitative data on invertebrate faunal materials, and these were uneven as to recovery methods and biases. What was now required, in my opinion, was to engage a qualified marine biologist to work with the archaeologists, *in the field*, to apply more sophisticated and biologically informed analyses to the problems of Lapita marine exploitation. In particular, we wished to resolve the thorny question of whether the level of Lapita predation on local reef-lagoon invertebrates had been sufficiently intensive or sustained as to result in measurable effects on the molluskan populations themselves. Elsewhere in the Pacific, Swadling (1976, 1977a, 1977b) and Anderson (1979, 1981) had demonstrated such effects, using metric and morphological measures.

Fortunately, two Australian marine biologists, Ian Poiner and Carla Catterall, had recently conducted studies of contemporary human predation effects on the ecology and population biology of *Strombus luhuanus*, one of the dominant gastropods in our ECA midden assemblage, and an important food resource in many parts of island Melanesia. Dr. Catterall agreed to join us in the field in 1988, to undertake biological control sampling of the contemporary Mussau molluskan populations, mapping of the microenvironmental zones on the Eloaua reef and lagoon areas, and to develop a joint protocol for metric and morphological analysis of the archaeological molluskan materials.[26]

4. *What was the extent and role of arboricultural production in the Mussau Lapita economy? Is there evidence for the development or intensification of terrestrial production over time?* Our 1985-86 excavations had yielded a large collection of anaerobically preserved plant remains, representing 24 taxa of domesticated or arboriculturally significant species (Kirch 1989). As this was the first substantial direct evidence for Lapita horticulture, we felt it would be worthwhile to expand our sample. Moreover, these finds raised questions of whether the characteristic forms of Melanesian arboriculture described by Yen (1974, 1991) had their origins in the Lapita cultural complex.

All of our plant remains from the first two excavations seasons had been recovered by standard sieving procedures, however, and several colleagues had posed the question of whether application of finer sieving (e.g., 1 mm mesh or smaller), combined with flotation, might yielded additional plant taxa. Although I was dubious (given the range of domesticated taxa known ethnobotanically from Near Oceania) that flotation would significantly increase our sample, it was essential that this be empirically tested. Thus in our 1988 fieldwork we resolved to apply extensively flotation and fine-screening in the ECA excavations. Also, since the modern Eloaua islanders continue to practice intensive arboriculture—cultivating much the same range of species as witnessed in our archaeobotanical samples—there was scope for an ethnoarchaeological study of Mussau tree cropping. The only prior ethnobotanical study of such Melanesian arboriculture had been Yen's pioneering work in the Santa Cruz Islands (Yen 1974), and additional research along these lines in Mussau would certainly add much of ethnobotanical interest. For both the flotation work and the ethnoarchaeological study, I engaged Dana Lepofsky, then a University of Washington graduate student who had prior training in paleoethnobotany. Her work with me on Eloaua in 1988 resulted in a superb ethnobotanical study of contemporary arboriculture (Lepofsky 1992), as well as a detailed metrical analysis of the archaeobotanical assemblages (Lepofsky et al. 1998).

5. *What was the chronology and nature of the transition from Lapita to post-Lapita phases? How is this transition reflected in ceramics, material culture, subsistence strategy, long-distance exchange, and settlement pattern?* Despite our testing of several post-Lapita sites during the 1986 field season (such as EKU and EKS), I was concerned that we had not yet adequately sampled the non-Lapita archeo-

logical record of Mussau, and resolved to see if we could not expand our definition of the local cultural sequence from Lapita through to the early contact period.[27] For this, it would be necessary to choose at least one additional site for extensive excavation, preferably one of the shell middens on Boliu or Ekaleu islands which we had discovered during reconnaissance, and which did not appear to contain Lapita pottery but rather were characterized by *Terebra*-shell adzes and *Trochus*-shell armrings. To carry out the excavation of such a site, I enlisted the assistance of Jason Tyler, a recent graduate of the University of Washington with prior excavation experience.

Accompanied by Lepofsky and Tyler, I flew to Port Moresby via Honolulu and Sydney on September 8, 1988, where we spent several days meeting with staff of the National Museum and University, and making the usual formal arrangements. We arrived in Kavieng on September 16, and after securing the necessary supplies and provisions, once again flew by chartered Talair aircraft to Eloaua on September 20. Two days were spent acquainting Lepofsky and Tyler with local field conditions and re-clearing the ECA Site.[28] On September 22, we commenced excavations at ECA, and as in 1986 Lepofsky and Tyler were to assist me in this work, taking on their own research projects later in the season (Figs. 1.11 and 1.12). The main emphasis at ECA in 1988 was on excavating a 130 m long transect along the W250 grid line (with pits every 10 m), but we also opened an additional 4 m^2 at Area B, and two 4 m^2 units at the N end of the W250 transect, designated as Area C. This work, combined with additional augering, gave us a much clearer understanding of spatial differentiation within this large and complex site.

Lepofsky initiated a program of flotation and fine water-sieving of 1 liter samples taken from a large number of stratigraphic contexts, to check that our field methods were not biasing the recovery of certain kinds of cultural materials, such as fine plant remains. Although the results of this work were largely negative, they were necessary to dispel any suggestions that we were recovering a sample biased to macroscopic remains. At the same time, we also undertook an intensive analysis of the abundant invertebrate faunal materials (primarily mollusk shells) being recovered from the W250 transect units. Prof. Catterall, accompanied by her assistant Mike Ritchie, had arrived on Eloaua on September 23, and with their aid we not only quantified all molluskan remains by counting and weighing, as in previous field seasons, but also measured certain metric variables on literally thousands of shellfish remains (Fig. 1.13). This was a daunting task, and required a huge effort on the part of all of us, since after an already exhausting day of digging in the equatorial sun, we would then measure shells with calipers by kerosene lantern light, often until midnight.[29] The database thus obtained, however, was essential for testing hypotheses regarding shellfish size changes in the ECA midden deposits.

While Lepofsky, Tyler, and I were excavating at ECA, Catterall and Ritchie spent their days undertaking a detailed ecological survey of the reefs and lagoons of the Eloaua, Emananus, and Boliu islands, from which they were able to produce a detailed environmental map.[30] In addition, they sampled the range of marine and littoral microenvironments for mollusks, and collected control samples that would prove essential for interpretation of the archaeological assemblages.

On October 6, Catterall and Ritchie departed via the weekly Talair flight, while Nick Araho (Assistant Curator of Prehistory, National Museum of PNG) arrived on the same plane. Araho joined in helping us complete the ECA transect and Area C excavations, which were finished on October 14. The next two weeks

FIGURE 1.11 Dana Lepofsky and Eric Kop sort anaerobically preserved plant remains from the Talepakemalai (ECA) Site in 1988.

FIGURE 1.12 Jason Tyler and John Male wet-screening along the W250 transect excavations at Talepakemai (ECA) in 1988.

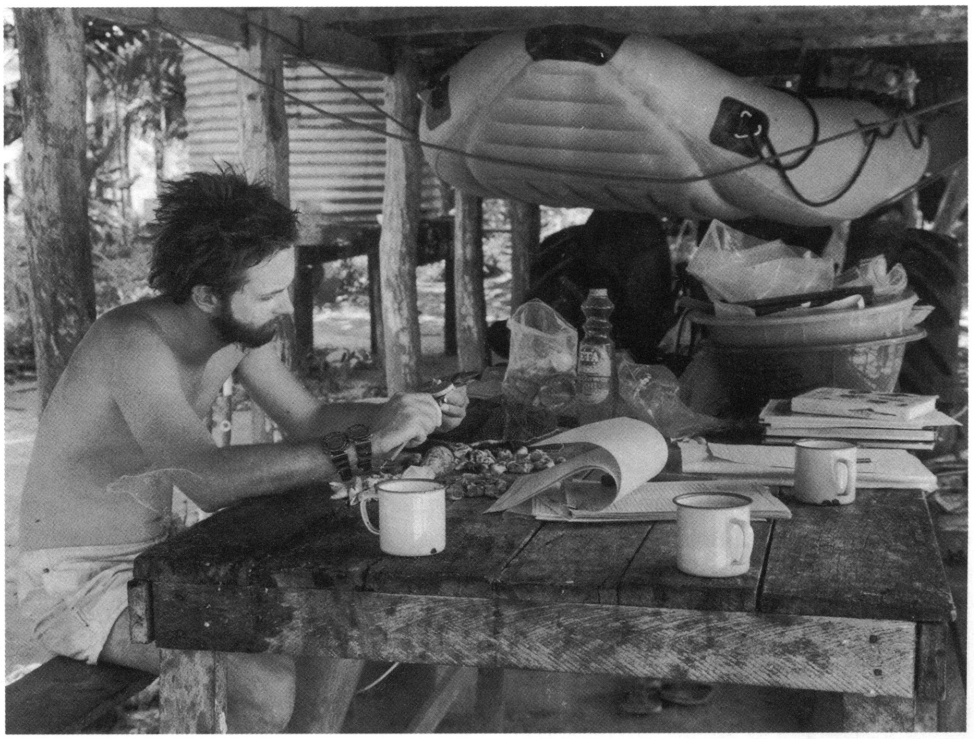

FIGURE 1.13 Mike Ritchie records metric data on control samples of mollusks collected from the reefs around Eloaua Island, during the 1988 field season. Our Metzler Maya-8 inflatable boat hangs under the floorboards in the background.

were spent in completing the analysis of shellfish samples, flotation analysis, and packing of the ECA collections for air freighting to Seattle. Meanwhile, Lepofsky started on her ethnoarchaeological study of contemporary Mussau arboriculture, while I assisted Tyler in starting an excavation program on Boliu Island at the EKE Site. I had decided that EKE held the greatest promise for providing a well-stratified record of the post-Lapita period in Mussau, and Tyler excavated there from October 16 through November 24, at times assisted by Nick Araho (Fig. 1.14). (Tyler also tested the EKL Site on Enusagila Island, from November 25 to December 1.) I also decided that it would be useful to at least test another post-Lapita midden deposit on Eloaua Island, and for this chose the EHK Site at Elunguai, and assigned Araho the task of supervising that excavation (see Chapter 8). With all of these projects safely underway, I departed Eloaua on October 29,[31] leaving Lepofsky, Tyler, and Araho to carry out their respective tasks. Tyler and Lepofsky remained in the field throughout November, returning to Seattle in early December.

FIGURE 1.14 Nick Araho (PNG National Museum) supervises test excavations at the Boliu Island Site (EKE) in 1988.

POST-1988 ANALYSES

At the end of the 1988 season, I reached the decision that sufficient fieldwork had now been undertaken, and that the Project should move into the final phase of laboratory analysis and publication.[32] Following my professional relocation to the University of California, Berkeley, in January 1989 (which required setting up a new Oceanic Archaeology Laboratory and moving all of the Mussau collections from Seattle, a time-consuming task), I organized a conference of all Project participants on 19-21 January 1990, at the Archaeological Research Facility, Berkeley. Following a memorable reunion dinner at the Berkeley Faculty Club, research presentations were given over the next two days by Allen, Bell, Butler, Catterall, Hunt, Kirch, Lepofsky, Tyler, and Weisler; Prof. Roger Green of the University of Auckland participated as discussant. Problems of stratigraphy and data correlation were ironed out, and plans drawn up for the publication of a final monograph. I hoped that this might be accomplished within a year or two, but a number of factors intervened to delay completion of the Mussau Project for a full decade. Chief among these was a disappointing failure of several participants to complete their manuscripts in a timely manner. Fortunately, most reports were eventually forthcoming.

The delay occasioned by recalcitrant collaborators, however, did permit us to extend certain kinds of analysis beyond what was originally planned. Under my supervision, several Berkeley students have studied parts of the Mussau collections, including: (1) a spatial analysis of ECA Area B by Steve Midgley; (2) a stylistic study of the EHB Site ceramics by Deborah Cembellin; (3) an analysis of the somewhat enigmatic EKE ceramics by Scarlett Chiu; and, (4) re-firing experiments with ECA ceramics to determine their original firing temperatures by Emily Dean. In addition, William Dickinson analyzed samples of both the ECA ceramics and the stone manuports using petrographic methods.

In his Preface to *Guilá Naquitz*, a monograph on a Mexican cave excavation that took some 20 years to complete, Kent Flannery wrote that he had become converted to "what might be called the 'Paul Masson' approach to archaeology: 'I will publish no site . . . before its time'" (1986:xvii).[33] I too, seem to have converted to this approach, although such was not my intention when I began fieldwork in the Mussau Islands in 1985. Having maintained (up to that time) a solid record for relatively rapid publication of primary site reports, I naively thought that the Mussau results would likewise be brought to publication within two or three years. Expansion of the Mussau Project into a major international collaborative effort involving more than 20 participants from seven different institutions, and two more field expeditions in 1986 and 1988, however, put the end to any such notions of prompt publication. Modern archaeology has increasingly become a complex science, involving a variety of field and laboratory specializations and often collaboration with natural scientists as well. While this team approach to research is essential, it unquestionably makes the preparation, coordination, and final publication of results a much more challenging task. Some Mussau Project team members met all deadlines and produced results on schedule; to them I will forever be grateful. Others have had to be cajoled and prodded, but eventually have come through. In the end only two participants did not produce reports, despite a decade of promises. Perhaps that is not too bad a record. In any event, directing the Mussau Project—my first experience in running a large international research effort—has taught me many invaluable lessons about the advantages as well as the pitfalls of contemporary archaeology.

NOTES TO CHAPTER 1

[1] On these ethnographic concepts and their history, such as the role played by the great Polynesian scholar Te Rangi Hiroa (Peter H. Buck) in maintaining that the Polynesian past was entirely divorced from that of Melanesia, see Kirch (2000).

[2] This suggestion was based primarily on Green's idea that "the original Lapita adaptation was to an area with a complex continental environment, which possessed a wide range of resources that related communities could assemble through exchange. This I place in the New Britain-New Ireland area..." (Green 1979a:45).

[3] The sample size issue was not trivial, as pointed out by Kirch et al. (1987). In the case of Mussau, Anson's sample of sherds from ECA (provided by Egloff's excavations) was restricted to a mere 16 sherds.

[4] Of course, it had never been the position of Bellwood (1979, 1985), Spriggs (1984), or others that Lapita represented a cultural group "passing through" Melanesia on its way to Polynesia. The term "fast train" was not used by them, although it was the title of a short commentary on LHP radiocarbon dates authored by biogeographer Jared Diamond (1988).

[5] This is a point not always fully appreciated either by the lay public, students, and even some professional archaeologists. What may be "reconstructed" for any given prehistoric culture or time period is very much influenced by the particular nature of the archeological record, including specific depositional and post-depositional circumstances. For example, the shallow stratigraphy of most Lapita sites and the fact that they have often been extensively reworked by post-depositional gardening, has greatly hampered certain kinds of archaeological investigation.

[6] One line of evidence which has been so productive for archaeologists concerned with prehistoric social organization in other parts of the world has been mortuary analysis. With the exception of Watom, however, Lapita sites are remarkable for their dearth of human skeletal remains. This then is one example of how the nature of the archaeological record controls what avenues one may pursue, specific objectives or goals notwithstanding.

[7] From my viewpoint this was a mixed success. Although some LHP teams did benefit from the use of the *Dick Smith Explorer*, it was not essential in the case of Mussau, which can be reached at lesser expense by chartered plane. Moreover, our field collections (the bulk of which consisted of fragile pottery) suffered greatly by being transported from Mussau to Sydney in the ship's hold, the constant motion of the vessel shattering and in some cases pulverizing sherds. In hindsight it would

have been better to air freight all collections directly out of Mussau, which is what we did in subsequent field seasons.

[8] I am forever indebted to Sally Brockwell and Pru Gaffey, who by the end of July had already had more than their fill of the tinned spaghetti which seemed to be the primary food stuff provided to expedition participants by the LHP organizers, and who hastily advised me that we should purchase our own supply of more appetizing foods in Kavieng before sailing to Mussau. A quick visit to the Chinese trade stores netted a somewhat bizarre array of canned meats and vegetables which, combined with fresh fish, lobster, and coconut crab obtained periodically in Mussau, meant that our tinned spaghetti could be conserved for the gastronomic pleasure of the *Dick Smith Explorer*'s crew on the long return voyage to Sydney!

[9] During their seven-day stay in Mussau, Allen and Specht recorded 16 new sites, although most of their site descriptions were minimal. They recognized that the ECA Site previously tested by Egloff was much larger than the latter had indicated, and suggested that "the site offers scope for further testing of areal differentiation" (Allen et al. 1984:9).

[10] In particular, there was the matter of the single radiocarbon date of 3900 BP for ECA, obtained by Egloff (Bafmatuk et al. 1980), and whether this accurately represented the age of the "early Far Western Lapita" style, as Anson (1983, 1986) averred.

[11] This view was based on the proclivity of Lapita sites to be concentrated on small, coral islands or islets, frequently offshore from a main high island, as in the Santa Cruz/Reef Islands (Green 1979a).

[12] At the National Museum, I was able to examine Egloff's collections from ECA and ECB, and determined that a final report of his 1978 excavations was unlikely to ever be completed, which made it more critical that I spend some time on the ECA Site to resolve the problems mentioned earlier.

[13] Elsewhere (Kirch 1997:23-25) I have related the excitement felt by all of us at the results emerging from Panakiwuk and other New Ireland sites, which were yielding the first evidence for a deep Pleistocene sequence in the Bismarck Archipelago.

[14] I would like to take this opportunity to thank Pru and Sally for their cheerful assistance during the exciting 1985 field season. I can only hope that the marvelous finds which we jointly uncovered can in some small measure make up for the hardships they put up with. For, as it was later related to me by a third party, when they signed on to my part of the LHP they thought they were "going to work in the Bismarck, not the Gulag, Archipelago."

[15] The main island of Mussau is locally referred to as "Big Mussau," in distinction to the small offshore islet of Emussau, from which the name of the group derived.

[16] During the course of our project, there were only two houses in the village constructed of Western materials; one belonged to Ave Male and one to Tamengei. These were actually adaptations of the traditional Eloaua house, being raised on wooden piles, but with rough sawn timber floors and walls, and roofs of corrugated iron. Ave's house, which we rented during all three expeditions, provided a very comfortable field base, although it had neither plumbing nor electricity. However, potable if slightly brackish water was available from a nearby well.

[17] To this feast I was able to contribute a basket containing seven Nicobar Pigeons (*Caloenas nicobarica*) which I had purchased a few days before from some Tench islanders. The Tench people evidently raise these pigeons in a semi-domesticated state, and bring them as exchange items when they occasionally travel to Mussau. Tench, which is highly isolated and no more than 1 km in area, has not to my knowledge been anthropologically documented since the German Südsee Expedition in 1908. I made several inquiries about arranging transport to the island in 1986 and 1988, in the hopes that we might be able to carry out archaeological tests, but this proved impossible.

[18] A preliminary report on the 1985 field season (Kirch 1986) was published in the *Report of the 1985 Field Season* volume edited by Allen (1986).

[19] I would like to acknowledge the generosity of the National Museum and Art Gallery of Papua New Guinea (and especially of the Director, Soroi Eoe and Curator of Prehistory, Pamela Swadling) for allowing us to remove all of the excavated collections for extended study in the United States. Without this privilege, it would not have been possible to carry out such detailed and exhaustive analyses of these materials as are reported in this monograph.

[20] As Director of the Burke Museum at this time, it was not possible for me to be absent from Seattle for more than about two months at a time. My plan was thus for the three of us to work intensively at ECA for 6-8 weeks, and once Hunt and Weisler had been thoroughly introduced to the field situation, leave them to continue the Project's work for an additional two months after I returned to Seattle. This plan worked quite well, and I used it again in 1988 with two other U.W. students, Dana Lepofsky and Jason Tyler.

[21] While it is possible to hire dugout canoes, the prices requested are often exorbitant; moreover, canoes are not always available when needed.

[22] It was impossible to purchase gasoline, which we needed for our outboard engine, in Mussau. Moreover, we could not take gasoline on the chartered aircraft which carried our other supplies, so shipping the gasoline by the small interisland boat *Tikana* was the only option. Fortunately, the *Tikana* was able to carry the 55-gal drum to Eloaua a few days later, where she dropped the drum outside the reef, we lashed a line to it, and towed it ashore!

[23] The work of preparing a Mussau fish reference collection was continued by me in 1988, and in the end we obtained some 70 specimens. These are curated in the Oceanic Archaeology Laboratory at UC Berkeley.

[24] Here I would like to acknowledge the assistance of Debra Connelly Prentice, who carried out much of this work.

[25] Loan of the Tikopian fish reference collection was kindly arranged by Toni Han of the Bishop Museum.

[26] Mussau seemed a particularly suitable locality to undertake such work, because the Eloaua islanders have been staunch Seventh Day Adventists since missionization in 1930. As such (following the dietary restrictions of *Deuteronomy*), they do not eat shellfish and thus there had been little or no human collection pressures on the Eloaua mollusks for nearly four decades.

[27] It may be worth noting that in Melanesia, as elsewhere in Oceania, the "direct historical approach" is an extremely powerful tool for historical analysis. Unfortunately, many archaeologists not trained in the North American anthropological tradition fail to recognize this, seeing the ethnographic record as relevant only for "analogic" interpretation of the archaeological record. In Mussau, there is every reason to think that the contemporary islanders have a direct historical link to the Lapita sites we were excavating, albeit a "link" that more properly should be conceived as a "chain" extending over more than three millennia of cultural change. The fundamental point is that there is, in all likelihood, direct continuity from past to present.

[28] As in prior seasons, Ave and John Male and other friends in Eloaua greeted us warmly. I was increasingly concerned during the 1988 season, however, to observe evident changes in the social fabric of this little community, especially among the teenage and young adult male population. Exposure to Western videos (including pornography), alcohol, and an evident breakdown in the authority of the local SDA church elders were having disturbing effects. Among the events which occurred on Eloaua during our stay were the nocturnal disturbance of a recent grave site (to obtain the cranium which several young men evidently thought would give them supernatural powers), several burglaries, an attempted rape, and the robbery and beating of a visiting Korean *Trochus*-shell buyer. These events became one desideratum in my decision not to continue fieldwork beyond the 1988 season.

[29] This work had to be carried out in Mussau itself since in 1988 there was no boat transport out of Mussau, and air freighting several hundred kilograms of molluskan shells back to an overseas laboratory would have been prohibitively expensive.

[30] Catterall and Ritchie had come equipped with diving gear, and our Metzler inflatable boat gave them the flexibility to cover the diversity of habitats necessary.

[31] On my return through Port Moresby I gave a seminar at the University of Papua New Guinea on the results of our Mussau work, in which I also proposed that the Lapita design system was fundamentally focused on representations of the human face, an interpretation independently derived by Spriggs (1990b).

[32] There was, of course, always the temptation to return to the field for yet another field season, which doubtless would have added important new data. However, this possibility had to be balanced against the consideration that we had already obtained a massive collection of materials and data, analysis and reporting of which would only be exacerbated and delayed through further fieldwork. There was also the consideration that Eloaua had become an increasingly dangerous locality (from a social perspective) in which to bring a foreign field team, including graduate students (see fn 28, above).

[33] The reference, for those unfamiliar with American wines, is to a marketing ploy once used by the Paul Masson winery (a purveyor of rather low cost "jug" wines), which claimed it would "sell no wine . . . before its time."