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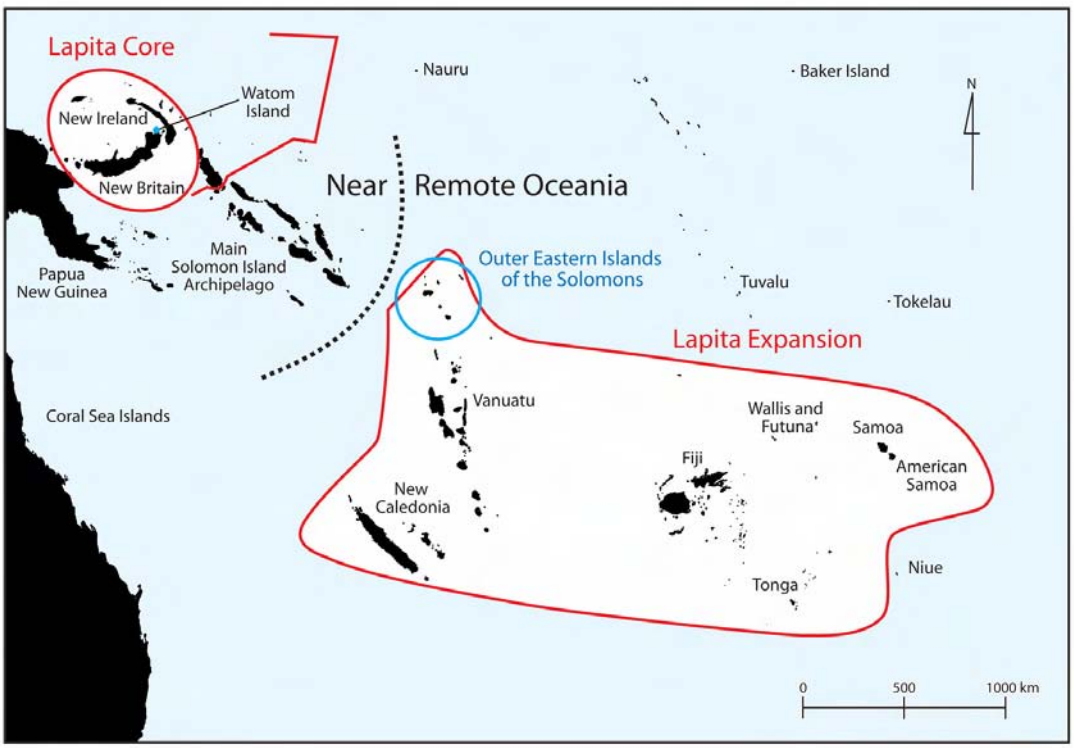
Background To This Essay

Over the past twelve years or so a great number of my contributions to various topics in Oceanic prehistory have attracted negative critiques. These commentaries usually set out to demonstrate that the views expressed are defective and call for their replacement by other understandings. Thus, some of the ideas explored in them have been dismissed, principally for a lack of support from the kind of hard physical evidence recovered by archaeology. Fair enough, that is one expected outcome in the nature of informed debate in cultural history, also called an anthropology of history, of a kind I favour and practice (Green 2000; Kirch and Green 2001).

The phenomenon of Lapita in general along with the distribution of those sites seen to be included within it (Figure 1), can be broken down into a number of more specific analytical categories. Many have found them useful for particular kinds of investigation (see Appendix I for their definitions). This monograph-length essay uses those canvassed in a recent review of contemporary positions in Lapita archaeology (Green 2003), which covered a range of contentious issues with as much care and precision as one could manage within a long article. It was the outcome of a presentation at the 2002 Koné-Noumea Lapita 50th Anniversary Conference and appeared in a volume of similar Lapita related articles, all edited by Sand (2003). Discussion in my published paper addressed issues raised by Terrell in multiple earlier publications, those of Anderson on subsistence and mobility (2003) and those found in Best (2002) concerned with the interpretation of three sites with potsherd assemblages decorated in the Lapita art style recovered in the Reef/Santa Cruz group. These last are under close scrutiny here, although it is also necessary to comment at the start on the Lapita status of the potsherd assemblages in another Lapita site on the Island of Watom that Best (2002:86-89) treated in a similar fashion.

Figure 1 The distribution within Oceania of sites containing Lapita-style pottery, highlighting localities on the island of Watom and that for three sites in the Reef/Santa Cruz Island group discussed in this essay.



Of various data issues in contention, the views presented by Best (2002) in his interpretation of the Lapita phenomenon were based very largely on its ceramic content. They attracted a fair measure of support, as for example in Felgate's (2003) PhD thesis on later Lapita-related ceramics from the Solomons. They were also given prominence in the writings of Spriggs (2001, 2002, 2003) on the subject of Lapita. These critiques should be read as background to this essay. However, the focus here dwells on the re-analysis of Lapita style motifs on three ceramic collections from the Reef/Santa Cruz island group. The basic concern is with their initial characterisation, their representative quality as valid samples of those parts of a site from which they were recovered, and their adequacy as useful data when dealing with matters of chronology in a comparative context.

Parallel issues about a late Lapita site on the island of Watom in need of clarification and additional investigation: The major problems requiring attention in respect to the Reber-Rakival Lapita site were matters raised by Spriggs and Best related to its dating and the integrity of the archaeological sequence proposed for that long known Lapita site situated on the island of Watom, near the town of Rabaul, a part of the East New Britain province of the Papua New Guinea (Figure 1). Each topic in contention was re-examined in a series of articles intended to treat one aspect of those issues the authors identified as in need of resolution. The first was (i) a paper on the ΔR marine offset value for Watom Island 14C age determinations using midden shellfish as radiocarbon samples from the surrounding sea (Petchey *et al*. 2005), (ii) the second was a demonstration that present day techniques of 14C age determinations, when done in the context of isotope analysis, allowed for the direct dating of human bone samples removed from some of the Reber-Rakival Lapita burials (Petchey and Green 2005; Beavan *et al*. 2008) and (iii) an interpretation of the site's sequence by Event Phases (Anson, Walter and Green 2005) that constructively countered the adverse claims of Best (2002) and Spriggs (2001, 2002, 2003) in respect to the cultural sequence found at this site and the respective age of each event within that sequence.

The resulting publication necessitated input from two colleagues, Dimitri Anson and Richard Walter, who along with myself, sought to explicate in painstaking detail the make-up of the Reber-Rakival Lapita site's lengthy cultural sequence and its transition into something else. That publication therefore is a rather unconventional compilation of detailed information that few editors in charge of current journals or monographs featuring Pacific archaeology would countenance. Publishing a manuscript of this length and complexity in journals of archaeology or anthropology was quite simply 'out of the question'. Their formats preclude such extended presentation of data, interpretation and discussion, and entirely rule out multiple supplementary appendices containing still further discussion of even finer points the authors of the manuscript felt needed exploration. Some other option was called for to address the issues in contention properly.

Thanks to the field photography of Dimitri Anson, and a monograph format that accommodated it, a unique set of colour plates could be published for those who may wish to closely examine the visual evidence. For instance, in the photographs a reader can see the actual upper surface of Layer C2 at SAC before its excavation: the photographs may then be compared with the few well marked disturbances – illustrated in the b/w line figure of that surface (Green and Anson 2002:41, & Fig. 7) – that can be seen to penetrate into layer C2 itself and cause a low level of expected disturbance, as in many such sites. The colour images reveal the limited extent of mixing between the two layers (see also Green 2006). In addition, a reader can view colour plates of the burials under excavation that show intact stratigraphy

behind each skeleton and make up their own mind about the stratigraphic context from which each pit containing human bones had been cut.

Is it really true, following Best and Spriggs, that the burial pits were cut from some layer above Layer C2 and extended down into Layer C1, and therefore have no connection with the C2 assemblages which those authors would accept as characteristic of late Lapita ceramics? Or, was the C2 stratigraphic position of the burials just as their excavators reported? The claimants for alternative interpretations provide no explanation of just how the rather meagre quantities of Lapita style decorated potsherds and typical obsidian assemblages recovered from Layer C2 seemingly expanded into the rather greater amounts encountered in Layer C1. In the case of both potsherds and obsidian, this proposition would require the C2 assemblage to produce far larger quantities of these items to be moved by an unspecified method, labelled 'disturbance' by these authors. In short, they prefer "a highly selective kind of disturbance" that happened, under non-routine conditions (beyond those usually encountered by archaeologists), as the most likely answer for the association of decorated Lapita-style potsherds with these burials. The point here is that to counter such claims indeed requires uncommon kinds of exposition beyond those normally accommodated by most forms of mainstream publication.

It is to be regretted that a last minute addition to Fig. 7 in the first appendix of the Watom monograph, made after an instruction to insert some internal block-like guidelines to make it easier to understand, resulted in the occurrence of an error that was shortly thereafter pointed out to me by Janet M. Davidson. Unfortunately, the internal separations between sets of dates are not co-incident with the numbered Event Phases from which the entire sequence for the Reber-Rakival site is constructed. As a result, some readers may inadvertently or mistakenly adopt that overly simplistic framework, rather than the one provided by the analytical use of numbered Event Phases found within the main text, along with their 14C determinations. A better rendition of a similar figure, with further and more highly refined calibration of all the dates, including new ones, is now readily available (Beaven *et al.* 2008: Fig. 3). Jim Specht, who has an excellent field understanding of the locality from conducting his excavations on Watom in the mid-1960s, and has been the sole archaeologist to work with local informants who had once assisted Father Meyer, remains dubious as to the placement of Father Meyer's long series of investigations at the Reber-Rakival site by Green (2005: Appendix 4, Fig. 11E). To resolve this issue has required yet further field investigations which Anson and his team undertook early in 2008. Another season of investigations will be completed 2009 that should bring this issue to a conclusion.

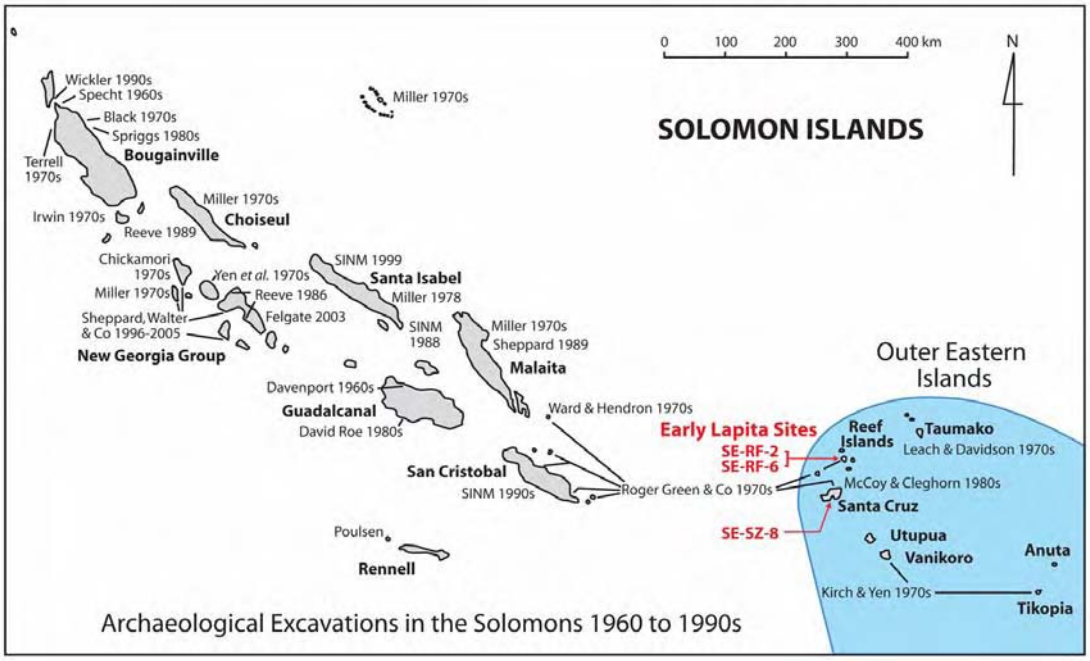
Despite minor criticisms, the overall response of colleagues to the choice of adopting an unconventional form of publication in order to present in full the evidence recovered from the Lapita site on the island of Watom has in large part been positive. Some readers have even volunteered the view that the Reber-Rakival site complex on Watom is the one Lapita site in Near Oceania published in sufficient detail, along with exploration of alternative proposals, to let all interested scholars make up their own minds on most of the issues surrounding the investigations of this site since Father Meyer first reported his own discoveries there in 1908-1910. Moreover, the outcome seems a highly satisfactory means of providing the necessary data in sufficient detail so that all researchers can evaluate the various contentions made about interpreting the Reber-Rakival Lapita site on Watom Island in a cogent manner.

Was Jim Allen, in a moment of irony, right or not? Is this site really so "stuffed" it is illegitimate to associate any information obtained from it with what some call Lapita – hence Jim's comment "so bad luck Watom". That opinion, now of decades standing, is still shared by a number of archaeologists. They wish to write off the entire range of evidence –

especially for pig husbandry, a major horticultural component, a well understood diet for its people, landscape change in the interior induced by clearance and cycles of gardening and fallow, along with resulting erosion – as coming from insecure contexts lacking integrity. Thus investigators should forgo using any of the above information from this site in their interpretation of issues in Lapita archaeology. This "wholesale rejection approach" I view as something which Pacific archaeology can ill afford. Given the suite of publications bearing on it, one wonders how the Reber-Rakival Lapita site, its contents and their interpretation, will be treated in the future – with the respect it is due or with continuing dismissal. Hopefully, the atypical degree to which detailed publications on the results obtained from the Reber-Rakival Lapita site have appeared in print will weigh heavily in determining a more favourable outcome despite those formats in which they occur.

Issues associated with the interpretation of early decorated pottery in Lapita sites of the Reef and Santa Cruz Islands: The concerns canvassed in the substantive part of this essay serve to re-evaluate the ceramic data from three Reef/Santa Cruz decorated Lapita sites excavated in the Outer Eastern Islands of the Solomons (Figure 2). They constitute a second focus of Best's (2002) overall critique of the existing ceramic analyses and dating of Lapita sites. In most respects, the data fail to conform to an Eastern Lapita-based model he thought should also apply to all other regional divisions designated a part of a culturally fairly uniform Lapita horizon. For Best, to do the comparative interpretive task properly requires alternative solutions and frequent challenges to the adequacy of the data itself when excavated by others.

Figure 2 The Outer Eastern Island (OEI) region of the Southeastern Solomons and the placement of the Lapita sites of SE-SZ-8 on Nendö and SE-RF-2 and SE-RF-6 on two islands of the Main Reef group.



Most of the basic research, and analyses of the field material on which Best and Felgate rely, was carried out in the early 1970s and published from 1973 onwards by myself and numerous other colleagues. Not surprisingly, just as with those dismissive of the utility of the Lapita site on Watom, countering the Reef/Santa Cruz site critiques has also required sustained

efforts. One learns soon enough that it is of little worth to aver, for instance, that Best is in error for readily identifiable general reasons (Green 2003:104-108). Such summaries simply do not carry sufficient weight to convince. Instead the debate degenerates into a typical kind of argument: "I am right and you are wrong," or vice-versa. Consequently, serious scholars find themselves unable to assess the merits of a case without access to the full range of facts and interpretations, while a large number of other readers (far too busy with other concerns) continue to hold to their established "citation circle allegiances", or simply maintain their previously adopted stances regarding the issues in contention.

I hold that efforts to avoid extensions of this "you say, I say" kind of exchange necessitate quite another strategy. In the instance under discussion here, this meant processing and publishing a whole series of 14C dates, the commissioning of entirely new illustrations of plan maps and cross-sections for some of the sites involved, and the writing of texts further detailing those explanations that clarify the radiocarbon-based chronological relationships that existed between the three Lapita sites. These efforts, of course, involved networking with a set of willing colleagues – M. Jones, F. Petchey, P. Sheppard – to whom I am extremely grateful. For a number of reasons, some of them medical, it has proven a prolonged and highly frustrating business, though at last all those articles initiated several years ago are now published.

This essay, however, a draft of which was written and circulated some years ago, was not placed in any mainstream journal publication 'queue' for good reasons. Firstly, overseas and Pacific archaeological journals cannot really afford to allocate extended space to a 30 year old data base and a detailed discussion of its content, interpretations and implications. Secondly, discursive essays of this sort are not sought nor acceptable to the largely theory-driven presentations found in most journals, where authors may only include a highly restricted amount of information necessary to support a given conclusion. In such tightly edited scholarly outlets space is at a premium and extended discussion is held to a minimum (see the Publication section below).

Two controversies form the main thrust of this essay. One is whether the previous 14C dates for a presumed age order of the three decorated Lapita sites from the Reef/Santa Cruz were an accurate or an inaccurate reflection of their relative antiquity. The second is whether certain components of the portable artefact content from sites occur in sufficient numbers that their analyses provide sound support for the previously published chronological interpretations. Since 2002 several helpful outcomes have appeared in print bearing on these issues.

First, Specht (2004) has dealt adroitly with Best's adoption of an earlier analysis by Wickler, re-employed by Best to support his own views on the proper age order sequence that should apply to these sites. In his article, Specht explores a similar theme to that in the essay here, namely that in sampling procedures the duration of each sample must be of comparable length when doing seriation analysis along the lines conducted by Wickler. The use of the data by Wickler and Best transgresses this requirement. Next, Peter Sheppard did a very insightful review of the present essay in first draft, resulting in many useful improvements to its presentation. More importantly the arguments therein stimulated him to undertake more sophisticated modelling procedures that basically employ the same data set. These statistically advanced procedures allowed him to directly address still other problems raised by Best (2002). At issue was the quality of assemblage samples so far recovered by Lapita archaeologists everywhere, not just those under review here. At present only a few Lapita sites are sufficiently fully published to make these kinds of judgements possible. This situation presents a major problem for any kind of intersite comparative study. Some have also asked, as did Felgate (2003), are the excavated sub-sample assemblages of most Lapita

sites with decorated pottery representative enough for any kind of task of comparative analysis when employed by others after analysing their own more recently recovered data sets? Felgate's opinion was that the response would in most cases be: 'not really'.

Still, a consequence of circulating an earlier draft of this particular essay was the publication of a mainstream academic article which addressed the questions Sheppard had initially raised in respect to the three decorated Lapita sites from the Reef/Santa Cruz Island group having read a draft of this essay (Sheppard and Green 2007). While it drew on the database presented in detail in this essay, it was not possible to include such dense information in that kind of publication. Articles within an edited monograph are simply unable to cater for the dissection and discussion of such data sets in detail just as is the case for articles in most academic journals.

It also seems necessary to highlight a final point that should be transparent, namely that Felgate (2003) has in my view adopted an overly negative stance, persuaded by Best's statements about these Reef/Santa Cruz decorated Lapita sites. However, Felgate's independent choice of that data set as a focus for a case study has in fact allowed further analyses yielding profitable new insights. In short, the commentaries of Best and Felgate have rather different characters, with each requiring separate treatment when setting out responses seeking to counter the various claims that stem from their analytical endeavours and interpretations.

Publication: Initially I made no plans for the formal publication of the draft essay. However, I have come to regard its appearance in an electronic website, e-monograph or e-journal outlet (the numbers of which are growing steadily) as providing a satisfactory vehicle. My inbox, sundry newsletter literature, and reports by journal editors to their organisations contain fairly frequent appeals of various sorts for the submission of items to fulfil the aims of the mainstream Pacific periodicals (e.g. AINZ; NZJA; JPS; AP). They constitute a commonplace in the background considerations of many potential contributors to typical academic productions. But this essay most certainly was *not* of that character, and those kinds of outlets remained unsuitable as venues.

On my own behalf, it therefore had always seemed far more feasible to circulate electronic or hard copies of detailed and data rich essays (often without any attempt to bring them beyond the date of the time they were written) to those few who might find them of use in their own research (Green, 1985 Ms.). However, other investigators who come across references to such productions find that practice more than a little frustrating when conducting their own research and find they are not included in the collegial Pacific 'kula' exchange network. Yet, I have often been told by hardworking editors that such outdated and detailed texts – unless re-cast in a succinct, highly constrained form deemed quite relevant to currently topical issues – are judged not suitable for the audiences served by contemporary regional journals of Pacific archaeology. With this judgement I tend to agree [see the opening comments in Green and Weisler (2000:5) or the Watom Island volume on Reber-Rakival Lapita sites described above (Anson, Walter and Green 2005)]. True, it has sometimes proven possible to slip an article of the 1970s era into today's literature (e.g. Green (2007) on Samoan Prehistoric Population), by placing it within a contextual framework more in keeping with a topic undergoing renewed debate, in this case studies of the paleodemography of certain Pacific Island societies. Still such opportunities are rare, whereas online e-journals or e-monographs with fewer limitations on space and more flexible formats do offer a suitable alternative. For

those concerned to fill the detailed data and extended discussion gap not otherwise catered for, this option should in my view be exploited to the maximum possible.

One drawback to this option within most academic accountability schema is that 'on-line' endeavours are often perceived to have no impact factor. This concept (in association with major English language journals in the field of archaeology) and the conjoint notion of an item's shelf life, has recently been addressed by L. Nagaoka (2006). Applying these concepts to publications from the Pacific indicates that such minor publications normally will have limited impact and by intention have shelf-lives of no more than five to ten years. An example of my own is a monograph in a no impact series focussed on research done 25 years ago in Mangareva (Green and Weisler 2000), which in fact accomplished several useful tasks. One was to provide a sound basic document of reference that covered the finer details of context in individual sites, which could then be cited in a series of subsequent mainstream articles on a particular topic or aspect of Mangarevan archaeology. Another was to precipitate renewed archaeological investigations within that island group, which it did (Conte and Kirch 2004). Interestingly, as predicted for such publications, this short monograph is now out of print. Similarly the stock of the Watom Island monograph in the same series has also been quickly depleted, but in this case another printing in which Fig.7 is corrected is currently again in print due to the additional investigations now underway. The contemporary technology offering enduring online access in an e-monograph format for such data-rich and often highly illustrated essays in an economical electronic form solves the problem for essay-like monographs of the kind I believe should be produced. In this instance this essay in an e-format allows corrections to be placed on record to troubling misrepresentations of field work efforts I have published in insufficient detail.

Finally, there is a positive feature associated with the format option with which I have considerable sympathy. This is highlighted by a commentator who wrote a truly apt "send up" of the current fashion for publishing only in "*the* prestige journals". Moreover, he published his commentary in a journal of this very type – namely *Nature* (Lawrence 2003). He had a serious point to make, and did it well, addressing his remarks in particular to senior scholars in any field. Publication in 'prestige' journals certainly enhances an author's reputation as well as opportunities for promotion or employment in other institutions. Yet such articles often fail to achieve those information-sharing tasks vital to building any discipline within a community of its most serious scholars.

That, in my view, has become a troublesome problem for the continuation of in-depth research into all aspects of the topic covered by the Lapita phenomenon. However, it continues to be a major problem that too few scholars in this field seem willing to address. I concur that there are few rewards in doing so as yet, and few suitable outlets of quality that offer support for the issuance of monograph length essays on Pacific archaeology on a sustained basis. Hence, the choice of placing this essay on the motifs on the potsherds of three Lapita sites in a newly instituted format designed to overcome some of the problems of full discussion and illustration to a degree one deems necessary resolves the concerns I had of just how to circulate this essay. It now can be easily accessed without cost by all interested scholars who are involved in Pacific archaeology, not just those participating in a personal exchange network. Other Pacific research institutions with a long standing publication series too, have adopted this new option. For an example go to:

www.australianmuseum.net.au/pdf/publications/1472_complete.pdf

to access another recent online outlet having the same intention and Lapita site content.

An evaluation through deconstruction of 1970s motif database

Simon Best (2002:9) raised issues that I feel need to be further addressed with respect to the initial databases for motifs from Solomon Island Lapita sites SE-SZ-8, SE-RF-2 and SE-RF-6 (Figure 2). The issues relate to both presence/absence and to frequency counts used in various analyses. Essentially, his claims reduce to two concerns:

(a) The published results describing the number of motifs on the decorated sherds recovered from the investigation of these three early Lapita sites are a dubious representation of their inhabitant's human behaviour, given the enormous disparity (in percentage terms) of the limited area of each site subjected to excavation. Instead, the numbers of listed motifs are for the most part affected by the small areas sampled and the great variation exhibited by the number of sherds recovered from any one site.

(b) Therefore, the number of motifs recorded for each site during their analyses in 1973 is deemed to be directly proportional to the *actual* areas excavated at that time, and that factor is the one which in large part controls their frequency. As a consequence, changes in motif frequencies between sites do not reflect chronological differences in the age of the site's contents, whether of potsherds or of other items, along the lines claimed by different authors using that data. In short any employment of the findings of previous investigators is wholly unjustified.

The conclusion Best draws from the above contentions is that it is simply never going to be possible to demonstrate any kind of chronological order for sites SE-SZ-8, SE-RF-2 and SE-RF-6 on the basis of their published motif or other contents. In his opinion, all attempts to do so – whether based on presence/absence as with Green (1978) or on frequency as with Anson (1986, 1987) – are flawed, and these or other analyses that employ this data for temporal or similarity purposes are rendered suspect. In sum, both the ordering of sites and the analyses will continue to have minimal potential in supporting outcomes of significance by those carrying out comparative analyses should they employ these data sets.

As a prelude to this exercise in evaluating such devastating claims, it is useful to reproduce the Table 18 published by Green (1976) with the amendments; it appears here as Table 1. The contents of the table indicate excavation procedures employed in the recovery of the kinds of artefactual assemblages whose chronological ordering my colleagues or I set out to determine (a) by 14C dating and (b) through supporting evidence from the portable artefactual assemblages recovered from each site.

Table 1 Comparisons of survey and excavation strategies on three Lapita sites in 1971.

A. *Survey and Excavation Details*

Site number	SE-RF-6	SE-RF-2	SE-SZ-8
Nearest modern village	Ngamanie	Nenumbo	Nanggu
Approx. dimensions of site (m)	40 × ?	25 × 55	100 × 250
Total area (m²)	c.2400+[1]	1105	c.14,000
Sampling area (m²)	180	676	459
Excavated area (m²)	20	72	51

1 In 1976 its length on surface evidence increased the estimated size by 8,400m².

In SE-RF-2 another 81.5m^2 was excavated in 1976, taking the total excavated area within the site to 153.5m^2. Although both sets of data are employed in some places in this essay, that for 1971 of 72m^2 is the sole one used by Best in his 2002 critique. Thus it serves as the basis for most computations.

B. *Sampling Methods*

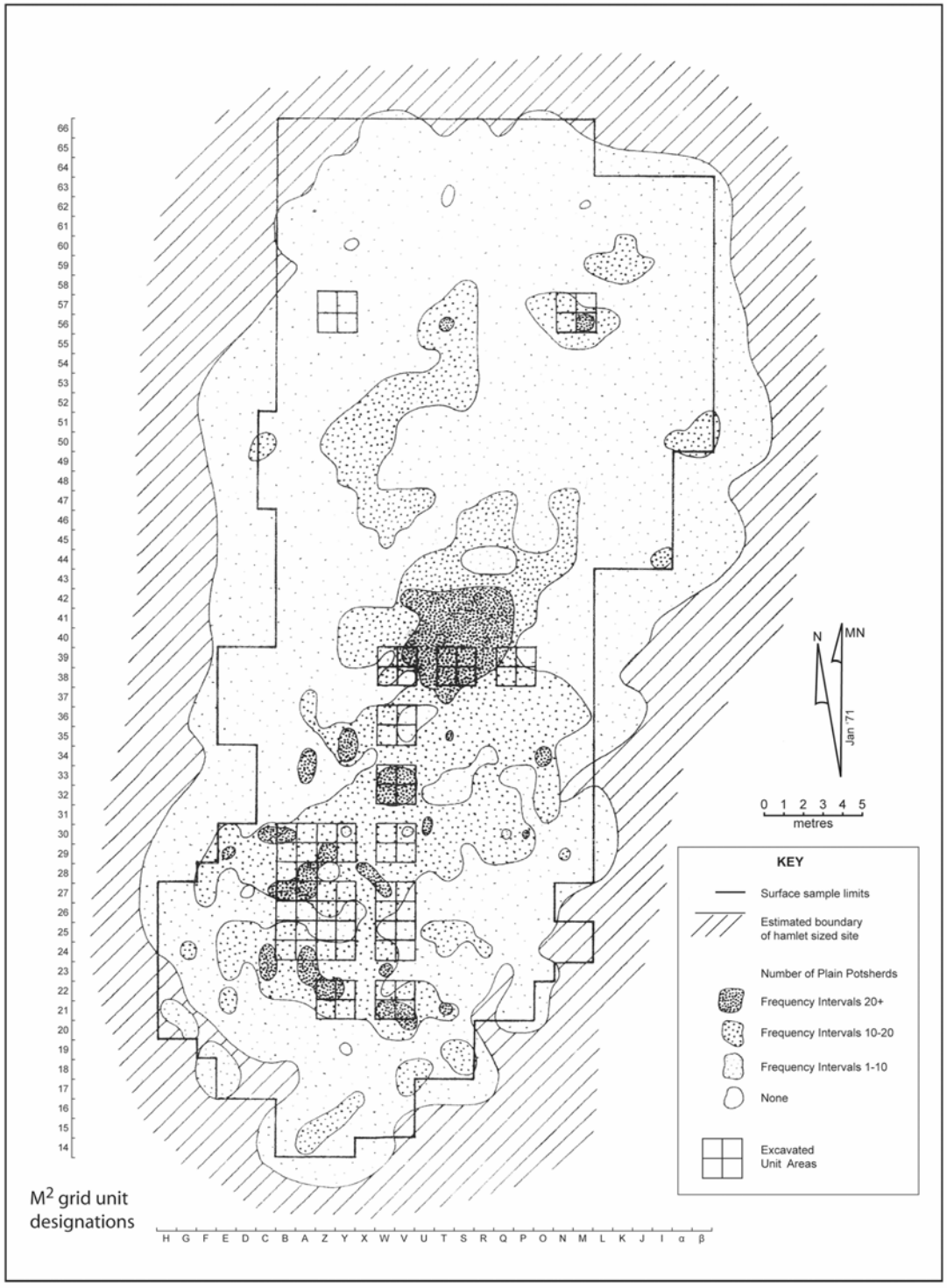
Site Number	*Sample Type*	*Method*	*Shape of Sample Area Excavated*
SE-RF-2	Near total, or systematic aligned	Screening and analysis of content of top 3cm followed by excavation	Rectangular area of 64m² and a few on distribution of sherds (see Figure 3)
SE-RF-6	Stratified, systematic unaligned	Sampling of a target area 1m in 9 by excavated squares	Cross section (9m × 24m)
SE-SZ-8	Stratified, systematic unaligned	Sampling of 1m in 9 by excavated squares	T-shape intersecting main axis of site

Drawing on Table 1, outlining the kinds of samples involved and additional details for each, this exercise in evaluation adopts a simple and reasonably uniform protocol to arrive at its computational conclusions. Ultimately, these conclusions rest on the ability to scale all three samples to a common base of 100m^2 when carrying out between-site comparisons. Scaling involves proportional manipulation of raw data numbers for an area of 72m^2 up to 100m^2 in the case of SE-RF-2, doubling the raw data figures for an area of 51m^2 for SE-SZ-8 to achieve approximately the same kind of sample for 100m^2 from that site, and multiplying those for SE-RF-6 by 5 to permit the calculation of comparable figures for it. The resulting figures allow readers to focus on the first major aspect of a counter argument that deserves deconstruction and amendment – namely a need to work with some form of normalized numbers, not just cite the raw data counts. Adopting this stance calls for a substantial section within this essay to open the discussion that seeks to evaluate the claims by both Best and Felgate as to whether the sample collections from the three Lapita sites continue to stand as representative sub-samples from these sites. Are they still useable in analyses, or are one or more of them so biased that they are not to be trusted as representative sub-samples of the contents to be expected from that site or a part thereof? All these discussions and the accompanying text involve Tables 1 to 6.

The next major section in the paper then sets out two individual motif databases: firstly as percentages based on frequency of all motifs found in any one site (Table 7), and secondly as percentages based on the total number of decorated sherds from that site (Table 8). In this last case three options are possible. The first is that on some sherds no motifs were identified, just the fact that the sherd was decorated by one of the known Lapita decorative techniques. Those categories cover a range starting with dentate-stamping and linear incising, and continue with the less prominent techniques of applied relief, shell impression, punctuation, and cutting through or cutting away the surfaces between the various decorative motifs. The second and most common option is that a sherd was complete enough that the decorative motif inscribed on it was sufficient to assign it an alloform number. This meant it could also

be listed under the primary overall inter-regional motif category that has up to now commonly been employed in comparative exercises, though alloforms conforming to each general motif category may exhibit minor differences. The third option is that two (or in some cases even more) motifs were identified on a single sherd.

Figure 3 Interpolated isopach zones based on frequency distributions for plain potsherds recovered from the uppermost 3cm of circa 50% of site SE-RF-2 in relation to those grid square units excavated in 1971.



A fourth option existed, although this was not well explored during these early analyses of Lapita decoration. The design field, usually seen only in part on any one sherd or across several sherds when joined, turned out to be one example of a repeating set of very complex central panels that formed the principal decorative band around certain kinds of vessel shapes. Recently, nine kinds of these repeating panels, each with a distinctive geometric motif theme, have been identified; they are based on decorations seen on the more complete vessels, particularly those large pots from New Caledonia. These panels occur together with other widely distributed anthropomorphic or 'face' images that have recently attracted a far greater amount of attention (Chiu and Sand 2005:142-147). Moreover, this latter category of panel has been further decomposed into five major subdivisions that comprise a complex set of "face designs", or in the view of some researchers a group of anthropomorphic and abstract representations of an ancestral icon. Donovan (1973), in her study of the Reef/Santa Cruz assemblages, frequently sketched these panel-like designs in outline (especially in her appendix to the analytical part of the essay). We have since learned, as a result of the re-analysis of these collections by Chiu (2005, 2007), that there were a fair number more of yet further panels represented in the three sites than Donovan had initially figured. In particular, there were more images of faces than previously recognized featuring in the subdivisions of the category comprising anthropomorphic or face design panels.

Outcomes from both kinds of database calculations in Tables 7 and 8 are used in the following interpretative parts of the second section of this essay, one as a check on the other, in order to assist in countering claims that a single method of calculation for the percentage values would perhaps yield the kinds of biased result that another method might not. Of course, other kinds of manipulations are also possible and one such has been published by Sheppard and Green (2007).

Normalised numbers for the three Reef/SantaCruz Islands decorated Lapita sites

Best sets out the basis for his alternative interpretation in the following long quote (with Table 2):

> "Differing proportions of motifs between the sites are also held to indicate that SZ-8 is the oldest and RF-6 the youngest, with
>
> an impoverishment of the design system of RF-6 with respect to SZ-8 and RF-2 through loss of motifs both restricted to the area and among those which are wider spread. This is consistent with the fact that of the 23 motifs unique to individual sites, SZ-8 has 6, RF-2 has 14, and RF-6 has 3 (Green 1978:13).
>
> However the number of different motifs recorded for each site in 1973 is directly proportional to the areas excavated at that time; RF-2: 72m² and 242 motifs, SZ-8: 51m² and 165 motifs, RF-6: 20m² and 82 motifs, with 3-4 new motifs added for every m² dug at each site. Although I suspect these are mainly alloforms of motifs already present, never-the-less the posited impoverishment of the design system of RF-6, and also the chronological ordering of the three sites on their motif component (Green 1978:12, 13, 1979:43), would appear to be a product of sample size differences.

The data on site sizes and areas sampled is summarised in Table 5 [now Table 2 below]. It is possible, when sampling the results of human behaviour, that despite the use of strategies to counter the problem, the enormous disparity between the percentages of the sites' areas actually excavated (a factor of 36 between RF-6 and RF-2) has resulted in the recovery of non-representative samples.

Table 2 Reef/Santa Cruz Lapita site area data (percentages for RF-6 and SZ-8 are rounded to the nearest 0.1%).

Site	*Site size (m²)*	*Area sampled (m²)*	*Area excavated (m²)*	*Site sampled (%)*	*Site excavated (%)*
RF-2	1100	153.5 (72*)	135.5 (72*)	13.9 (6.5*)	13.9 (6.5*)
RF-6	10,800	180	20	1.7	0.2
SZ-8	14,000	459	51	3.3	0.4

* Refers to ceramic analysis only of the 1971 excavations (see the explanation in Table 1 and Part A).

The possibility of sampling problems has been mentioned by both Parker and Sheppard. The former commented that "if a much larger area of these last two sites [RF-6 and SZ-8] was sampled, then it is likely that a much wider range of vessel form and decoration would result" (Parker 1981:118). Sheppard's analysis of the sites' lithics (only c.4% of which are in RF-6) also notes the intra-site differences, and warns against generalising from RF-2 to the other two sites (Sheppard 1993:123, 131)" (Best 2002: 91).

The various criticisms Best (2002) raises in the quote above about the adequacy of the Reef/Santa Cruz motif database can be joined with yet other arguments that Felgate (2003) has developed querying those samples as of insufficient size. They furnish further support for some of Best's concerns. Felgate's discussion, however, ranges far more widely, covers all aspects of decorative analysis undertaken on Lapita pottery to date (across a range of sites), and embeds it within more general discussions of method and theory on this topic. In this respect it is well to note that Felgate is also critical of some of the points raised by Best, and puts forward his own views of the Best (1984) analysis and seriation of the decorated pottery from Lakeba.

To illustrate his more general concerns, Felgate (2003:79-85), has singled out the three Reef/Santa Cruz decorated Lapita sites of SE-SZ-8, SE-RF-2 and SE-RF-6 as a critical case study of methods applied to date in that particular region. He concludes that a number of the points he comes to in this case also hold more widely and involve a range of other sites containing decorated sherds with motifs in the Lapita ceramic style. Felgate held no realistic expectation that Best's counter-proposals, which used yet other methods, will prove "to support a reversal of site chronology" in the Reef/Santa Cruz case (Felgate 2003:84). However, the numerical counts in Tables 2, 3 and 4 of his PhD thesis do indeed appear to provide challenges to the adequacy of the available data in relation to the purposes to which it has been put in the Reef/Santa Cruz analyses. This leads him to the conclusion that the methods in use for the study of Lapita decoration are defective. Moreover, the flaws that they exhibit are too seldom a target for the kind of insightful commentary he provides. Useful comment – yes, I would certainly agree. Still, does it really lead to the wholesale dismissal of nearly all existing comparative endeavours? I think not.

Within each of these critiques problems arise that neither of those authors take sufficiently into account in their discussions of the decorated potsherd sub-samples from the three Reef/Santa Cruz Lapita sites. Significantly, these problems relate to what kind of sample was collected for each site and what the data recovered represents in relation (i) to the area systematically sampled and then (ii) to the entire site. Instead, both authors deal with the raw figures that derive from the excavated squares, ignoring the different sampling strategies involved in their recovery. The discussion which follows reduces their figures to ones in which the sample excavated from each site is presented as that for $100m^2$ along the lines set out above and in previous publications.

The position adopted is that in each case the smaller size of the actual area excavated in the three sites investigated has to date proven adequate to represent each of the larger sampling portions of the sites subjected to intensive investigation in 1971. This assumption continues to hold in the evaluation that follows, given the various sampling strategies employed at each site along the lines set out in Table 1. I will also assess Felgate's Table 3 (my Table 5), focused on the rims. The rims were not treated separately in any of the 1973 laboratory analyses or in those of Anson employing the numerical counts for motif frequency (in which only those motifs that had been recorded for the body sherds were employed). This essay's Table 5, based on Felgate (2003: Table 3), although it uses the excavation data for the whole $153.5m^2$ serves to demonstrate the kinds of bias introduced when the additional excavations of 1976 were targeted on just those squares bearing the highest frequencies of plain (and thus, by implication, also decorated) sherds. That, of course, led to the much higher recovery of rims concentrated in a particular portion of the site. Felgate's Table 4 (my Table 6) reverts to the 1971 data only and helps to demonstrate the consistency and coherence of the sub-samples from each site, despite their origins in different stratigraphic and level contexts, and their "brokenness" (a term deployed by Felgate, Bickler and Murrell, ms) found in layers above the primary occupation into which the less broken sherds were initially discarded by the site's inhabitants. In summary, in the layers above the habitation event, taphonomic and natural processes took control of the increasingly broken potsherd numbers and their movements during the next 2500 to 3000 years.

In this essay's Table 3 the basic results of 1971 for three items of most interest from the three sites begins with the number of items found in a given area of a site. They appear in Section A. They are then normalised for comparative purposes in Section B of Table 3 by recalculation of numerical counts for a $100m^2$ area, and in Section C by calculating percentage figures as a suitable basis on which to conduct standardized comparisons.

Table 3 (from Green 1991:205) – Excavated specimens of decorated body sherds, obsidian pieces and chert items in three Lapita sites in the Reef/Santa Cruz Island group. Note that for SE-RF-2 the data for the pieces of obsidian and chert recovered relate to an area of 153.5m^2 that require adjustments which are discussed in the text.

A. *Number of items in relation to actual excavation area.*

	Sites:	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
Decorated body sherds (per excavated area)		2664 (51m²)	3715 (72m²)	1124 (20m²)
Obsidian pieces (per excavated area)		296 (51m²)	646 (153m²)	30 (20m²)
Chert pieces (per excavated area)		46 (51m²)	433 (153m²)	33 (20m²)

B. *Estimated number of items recovered per 100m².*

	Sites:	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
Decorated body sherds		5224	5160	5620
Obsidian pieces		580	422	150
Chert pieces		90	283	165
	Totals:	5894	5865	5935

C. *Estimates by percentage among three items recovered per 100m².*

	Sites:	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
Decorated body sherds		88.6	88.0	94.7
Obsidian pieces		9.8	7.2	2.5
Chert pieces		1.5	4.8	2.8
	Totals:	99.9	100.0	100.0

The percentages of decorated body sherds occurring in each site as calculated within Section C of Table 3 are remarkably similar. The numerical counts and percentages calculated from them for obsidian pieces and chert items of the kind taken from SE-RF-2 are not quite so representative of a typical 100m^2 random sample, however, and must be adjusted downwards. This is because they include the actual number of those two items excavated within an additional area of 81.5m^2 that represented the ceramically very rich central portion of that site. Still, a reasonably uniform distribution of obsidian obtained over the whole 153.5m^2 zone (Sheppard and Green 1991: Figs 8 & 9) and that means the percentage values are not overly high when used comparatively to those for general samples for other sites. In contrast the number of chert pieces encountered in that zone has clearly been lifted in percentage terms above what it would be if only a 72m^2 sub-sample of the original 1971 excavation had been used when calculating the numerical counts displayed in Section B of Table 3 (see also Sheppard and Green 1991: Figs 6 & 7). Any use of the percentage values for chert in Section C of the table in a comparative context would require a significant adjustment downward from 4.8%. An outcome of 2 to 2.5% would appear to constitute the kind of value for the overall amount of chert to be expected in percentage form from a rather more typical portion of this site that fell in the 100m² sub-sample range for SE-RF-2, rather than actual numbers

for the whole 153.5m^2 because that would include the part of the site known to be richest in chert pieces. Its use would introduce bias when used in any comparative study that did not take their unusually concentrated context into account.

Next, I evaluate a summation by Best of what is approximately the same data as that which appears in Table 1. First, one needs to correct a typological inversion in the Best (2002:91) entry for SE-RF-2 under area in m² sampled, by changing 135.5 to 153.5. In the lengthy quote cited earlier, Best dwells on the "enormous disparity between the areas actually excavated", a factor he says of 36 between SE-RF-6 and SE-RF-2. Actually the degree of disparity on this basis is 32.5 (i.e. 0.2% x 32.5 = 6.5%) while that for SE-RF-2 to SE-SZ-8 is half as much, or 16.3 times larger (i.e. 0.4% x 16.3 = 6.5%). In his documentation of the data set, it follows from this that a non-representative sample has been recovered for each site. One must add, however, that Best really means that the samples may not be representative of the (very large) total areas of sites SE-RF-6 and SE-SZ-8. Surely 6.5%, which in 1971 was the basis for the motif analysis of decorated body sherds from SE-RF-2, is by no means an overly poor sample. The sample was certainly further enhanced through subsequently targeting the area dominated by decorated sherds through additional excavations of another 81.5m^2 square in 1976. This has not confounded the initial results employed here, only improved on them in ways not discussed in this essay. Certainly their comparative use in some exercises would have its own bias due to its now very much larger size in respect to the other two site sub-samples. Therefore any use of the entire highly targeted SE-RF-2 sub-sample for 153.5m^2 needs adjusting to ensure comparability, and such problems are being addressed by Chiu (2007) in her more recent studies of these site collections.

How bad, however, are the samples from the partial area investigated of each site that was the target for a highly sophisticated procedure of sampling? For the body sherd motif analysis the figures are 6.5% for SE-RF-2, 1.7% for SE-RF-6 and 3.3% for SE-SZ-8 (as in Table 2 under % of site sampled). The factors of 36 etc. stated in the critique by Best (2002:91& his Table 2 included in this paper) prove misleading if not wholly spurious. For the whole area covered by each site, the 72m² from SE-RF-2 excavated in 1971 is twice as representative as that of SE-SZ-8 (6.5% versus 3.3%), and four times as representative as that for SE-RF-6, for which 1.7% constitutes an appropriate value. The sampling interval was 1m² in 9m², or 11% of the total area within a site that was targeted for sampling (Table 1) in the cases of SE-RF-6 and SE-SZ-8, a fair basis on which to scale up the raw numerical counts to a 100m² area for each of these sites when making statistically sound comparisons. As indicated above, this has meant doubling the number for the 51m² of SE-SZ-8 and multiplying that for SE-RF-6 by 5. For the initial 72m² excavated at SE-RF-2, a different assessment is necessary requiring reference to Figure 3 and attention to the three frequencies categories employed in displaying the distribution of undecorated sherds: 1-10, 10[11]-20 and 20[21]+.

The 72m² excavated in SE-RF-2 in 1971 in fact sub-sampled 21m² where the frequency of sherds without decoration was greater than 20, 18m² where the frequency was less than 10 surface sherds in a square metre, and 33m² where the widely common frequency lay between 10 and 20 sherds within any square metre. The southern half of the site had been very well sampled through excavation, but the northern half not so: 68m² in the southern portion to 8m² in the northern part. Two interpretations have been suggested for the differences in the two halves of the site. Felgate's suggestion, following observations made by Green (1976:255), is that the duration of the occupation of the northern portion of site SE-RF-2 was shorter. This led him to raise the question of whether 14C dates from the northern sector, had they been available, would have been of similar age to the later 14C dates from SE-RF-6. Drawing on four additional 14C dates for SE-RF-2 (beyond the two available in 1971) there is now a total

of six for the southern part of the site, that exhibit a tight chronological clustering over a wider area of the site (Jones *et al.* 2007). Those and the generally simple stratigraphy lend support to this view that is further strengthened by the coherence of the features throughout the southern portion of the site (Sheppard and Green 1991: Fig. 3). The evidence for an occupation of short duration finds more backing in the relationship of the total surface survey of sherds to some of the underlying structures (Sheppard and Green 1991:Fig. 2), in the predictive indications of the surface pottery to those amounts found *in situ* at depth (Jones *et al.* 2007; Specht 2002:45; Sheppard and Green 1991), and finally in the very small size of the site itself. Elsewhere, I have discussed functional differences in the kinds of activities conducted in various parts of the SE-RF-2 site, and especially that its the northern end, as offering another more appropriate religious-based explanation for the observed differences in the two sectors (Green and Pawley 1999: 77-79). Together they furnish a sounder basis for a functional interpretation rather than the suggestion that there were significant temporal differences in the timing of the activities taking place in just one sector of this relatively small hamlet-sized Lapita site.

Next it is necessary to discuss the question of whether the area of each site that was sampled in 1971 may be representative or not of the whole site. The answer of 6.5% of SE-RF-2 is set out in Table 2 and this has only been improved by the additional excavations of 1976 which take that figure to 13.9%. Even critics like Felgate (2003:85) are inclined to write that the SE-RF-2 sample "must rate as the most comprehensive and detailed surface collection and area excavation of a Lapita site undertaken to date." SE-SZ-8 is also reasonably well sampled, given that only parts of it were available for excavation (Green 1976: Fig. 76). Some sampling closer to the centre of the site 35m or so to the north might have improved analyses, but I am now convinced that the duration of this site must be reduced from the earlier unsupportable estimation of 300 years (Green 1991:203) to no more than 150 years and probably closer to the 100 years at most assigned to SE-RF-6. Thus the 3.3% sample for the area investigated at SE-SZ-8 would be only half as representative as that for SE-RF-2, but given the sizeable 459m^2 from which the excavated sub-sample derives, it constitutes a fairly representative large area within a Lapita site that was subjected to sensibly controlled sampling procedures that could stand for the whole site (Green *et al.* 2008).

This leaves only the much smaller though still adequate sample of the 180m² area investigated at site SE-RF-6 open to the charge that is in fact not sufficiently representative of the entire site in percentage terms, just as its critics have deduced. Interestingly reasons in this instance may well follow along the not very convincing lines of a chronological difference suggested by Felgate to explain to the northern half of SE-RF-2, reasons which may far more aptly apply to SE-RF-6. When it was excavated, I thought the site to be no more than 60m long. However, during the next phase of field investigations beginning in 1976, I came to realise that its length was far greater and extended not 60m but 170m in parallel to the tidal shoreline of that coastal inlet (Green 1979:51). This finding opened up a real possibility that the 180m^2 area I sampled from which the excavated sub-sample was drawn lay across one of the late ends of a large and elongated Lapita site whose occupation could well have begun somewhat earlier towards its central portion. However, if that is the case, it does not preclude the specific well-dated assemblage now available being a reasonable representative sample of a later occupation stage among the Reef/Santa Cruz decorated Lapita site occupations (Green and Jones 2008). On that view it remains the kind of later occupation sub-sample useful in documenting changes that have taken place over time. The sub-sample of 20m^2 remains a well constructed sample representative of 180m^2 at one end of SE-RF-6. It is simply not the same kind of sub-sample as a 1m wide trench 20 meters long, or a 2m wide transect 10m long, much less a 4m by 5m test square plunked into some open part of a very large site. Best and

Felgate's use of the raw data from these excavated squares unfortunately ignores the matter of the way this information was gathered. Further evaluation, therefore, of these samples requires using normalised data, not raw data for each, if comparisons are to be made and conclusions drawn from them are to have substance of the sort Felgate demands for all Lapita sites, especially if one is to compare them on the basis of their ceramic content.

The focus now falls on Felgate's (2003) Tables 2, 3 and 4. In Table 4A (Felgate's Table 2) he notes that "Green had applied a correction for sample-size-related richness using Donovan's unpublished motif frequencies to assess whether a motif was absent as a result of sampling error or absent-not-present." This statement is subjected to the challenges that the "allowance for the effect of sample-size differences was probably insufficient". In Table 4A, I provide Felgate's use of motif counts employed by Anson (1983), and then transform them in Table 4B to a common comparative base for a sample representative of $100m^2$.

Table 4A Reef/Santa Cruz motif counts for body sherds as given in Anson 1983.

Site	*Total Motif Count*	*Motif Richness*
RF-2	841 examples	178 motifs
RF-6	252 examples	79 motifs
SZ-8	627 examples	133 motifs

Table 4B Reef/Santa Cruz motif counts – same data normalised to a base of $100m^2$.

RF-2	1168 examples	239 motifs
RF-6	1260 examples	395 motifs
SZ-8	1254 examples	266 motifs

The outcomes in Table 4B will not please proponents of either the theory that the diversity of motifs in these sites in sample-area related (Best 2002:80) or the theory that it is related to the size of the samples (Felgate 2003:80). The surprise is that even small area sizes and small sample sizes prove to be reasonably rich sources of motifs, and especially those for SE-RF-6. It seems the claims of Best and Felgate are greatly overstated, a finding not unlike that also reached with respect to Best's interpretation of Table 2 already discussed above. One strategy – Table 4A – uses the actual numerical samples, without allowing the use of techniques to make them comparable; the other – Table 4B – sets out to do so. They offer two choices: either dismissal as irrelevant, or as in the reply conducted here namely the view that it is essential the sampling procedures be taken into account in full.

This is not to say that the samples are perfect, or that they could not have been more robust under other more ideal circumstances. Rather it is to claim that these sub-samples, with all their defects, may not be quite as misleading or inadequate as these authors and those others inclined to agree with them may think, provided proper care is taken with their employment. They should be treated as sub-samples of a sampling universe within each site, or in the case of SE-RF-6 for one end part of a larger site than was expected when it was excavated.

In Felgate's (2003) Table 3 in his PhD thesis he employs data from a later analysis done by Parker (1981) after the excavations of another $81.5m^2$. Drawing on both data sets makes it possible for him to quantify rim data not considered by Donovan (1973:5). He correctly sees

the rims as a "useful independent quantification of relative sample sizes" (2003:81) and concludes that by using sherd counts, SE-RF-2 has 12 times as many rims as SE-RF-6, 23 times as many when using MNI, and 21 times as many when counting only sherds large enough to estimate the vessel mouth diameter. He concludes from this data that it is highly likely that motif presence/absence [from the body sherds] is strongly affected by sample size (Felgate 2003:83). Consider Table 5B, my transformation of his Table 3 data (Table 5A here) to a normalised base of 100m². Results using this "independent" measure for RF-6 and SZ-8 appear remarkably similar, especially if one discounts, as Felgate (2003:81) suggests, the counts for plain rims versus decorated, which may fluctuate over time and are sensitive to "brokenness". In contrast, drawing on Table 5A the number of rims from SE-RF-2 is not 12, 23 or 21, but *three* times as large. Why? Because the sample of rims employed in fact includes the additional potsherd rich 81.5m^2 portion of the SE-RF-2 site also considered by Parker. By excavating the part of that site containing the highest proportion of decorated sherds (see various plans with the pottery distributions presented in Sheppard and Green 1991), the rim sherd counts used by Parker quickly rise to numbers three times greater than a less targeted excavation sub-sample would have obtained. The bias here is real, known, and needs to be accommodated when the overall results for a sub-sample representing 153.5m^2 are evaluated against the sub-samples drawn from the other two sites.

Table 5A Rim sherd counts and MNI assembled from various tables in Parker (1981).

Site	*Dentate rims*	*Incised rims*	*Impressed rims*	*Plain rims*	*Total rim sherd count/MNI*
RF-6	43	1	15	2	61/55
SZ-8	111	8	10	14	143/?
RF-2	848	50	425	100	1423/666

Table 5B Same data normalised to a base of 100m² (no MNI).

RF-6	215	5	45	10	275
SZ-8	222	16	20	28	286
RF-2	552	32	277	65	926

On Page 2 of an unpublished manuscript by Donovan that contains cross-section drawings of the rims and their decoration recovered from SE-RF-2, some 417 kinds of rims are identified as present (note that like Parker, Donovan tends toward being a 'splitter', so that every little difference marks a separate type). These do *not* represent *all* the 'kinds' of rim sherds Donovan had encountered, a point on which she is very clear in her opening commentary. However, it is unlikely that the difference was of the order of 417 kinds subtracted from 1423 rim sherds, or more than 1005 other rims of one kind or another. If one assumes she did not illustrate say 33 additional kinds of rim (7.9%), then something of the order of 450 kinds of rim under Donovan's 'splitter mode' of sorting, as an estimate, were recovered during just the 1971 excavations. Parker's use of the larger rim sherd data simply compounds the problem by splitting them into four general categories, before she too turned these categories into multiple rim types.

Coming at this puzzle from an opposing perspective, that the 1423 rims documented by Parker are 3 plus times those in the other two sites, would suggest 474 kinds of the Donovan sort of rim as a normal expectation for the 72m² excavated in 1971. Scaling that up to 100m² (and so incorporating 28m² of the additional 81.5m² excavated) yields 658 rim kinds per 100m², predicted by the actual raw counts of 1971. This represents an addition beyond a predictable expectation of 268 rim sherds due to the more targeted sub-sample region of the additional 81.5m^2. I conclude therefore that, in rim sherds, the richness of SE-RF-2 is in fact 2.35 times that of the other two sites.

Although one might deduce from this data that the motif richness on the body sherds should also be more than twice, this does not appear to occur (Table 4B). One possible explanation is that a very much higher number count of plain rims in SE-RF-2 is a factor. Another possibility is that the correlation between decoration on the rims and on the bodies of pots is at least in part an independent variable, and not a correlated or predictive one as Felgate has assumed. An observation from my own experience is that when rims on Lapita pots are decorated, their upper body surfaces where most designs occurred are not always decorated to the same degree. The third possibility is that to some degree the targeted sample, controlled in its placement by surface indications of subsurface pottery distribution, has proved a better sub-sample for both the area excavated in 1971, and most certainly for the 153.5m^2 excavated by 1976. However, Felgate's argument that sample-size is the controlling variable by a factor of 12 to 23 times must be deemed overstatement. It certainly does not hold up to the informed evaluation provided here of the kind of bias these figures reveal, when they have been standardised.

Turning now to Table 4 in Felgate's PhD, reproduced here as Table 6, I found the content highly informative and in further confirmation of deductions made previously from other data. Discussed in several previous publications, this perspective has been reiterated yet again recently in Jones *et al.* (2007). The first site listed in Table 6 is SE-RF-2. Here a breakage in size occurs as these sherds move up from their primary layer of discard (B) to that of Layer A. First the top 10cm or less, of a once thicker Layer B in Table 6, is incorporated into Layer A due to subsequent use of the site locality as a garden. Initially Layer A was formed by an ash fall of 15-20 cm depth from Tinakula volcano that covered both this site and site RF-6. In both sites, on the basis of stratigraphic lithology they became a zone labelled Layer A. In my view, Felgate's recalculations (excluding plain sherds that happened to have been retained in the decorated bags during the separation of the two categories) have to do with "brokenness" between the contexts rather than with differences in potting behaviour in the past (Felgate, Bickler and Murrell, ms). These authors define the concept of brokenness using data from a submerged site in the Roviana Lagoon in the Solomons.

In Table 6 a normalisation procedure using percentage has been employed by Felgate for the decorated body sherds of RF-2, RF-6 and SZ-8 from which the overwhelming majority of decorated sherds derive. They include virtually all the sherds presenting motifs that could be coded. As Felgate (2003:81-82) observes, data do not support any change in *relative* (i.e. percentage based) frequency of dentate-stamping to incised decoration if evaluated using the context from which they were recovered in any of the three sites. Even more significant, he finds that any differences in these two kinds of decorative techniques between the three sites are slight. That conclusion fits well the published statements about the temporal duration of these sites, which began with their description by Green (1976:263). In brief, the occupation layers at each of these sites are temporal slices of fairly short duration and each contains portable artefacts of all the classes recovered from any one of the occupations. Overall, the ceramic assemblages reflect short intervals of time within a decorated Lapita pottery phase –

spanning perhaps 500 or 600 years before the start of a following plain ware Lapita phase – (or that of Doherty's (2007) Period II). This plain ware phase is in fact a continuation of this region's local Lapita ceramic tradition, though with only minimal decoration, found throughout the Outer Eastern Islands, and plain ware pottery Lapita sites spanning another 500 to 600 years that follow those with highly decorated ceramic assemblages. After this, cultural assemblages associated with locally made pottery vessels disappear from the regional record.

Table 6 Relative proportions of dentate and incised body sherds, recalculated using data presented by Donovan (1973).

Dentate count	*Dentate %*	*Incised count*	*Incised %*	*Total count (ds + inc)*	*Context*
40	74	14	26	54	RF-2 Surface
1647	64	**926**	36	**2573**	RF-2 LayerA
758	66	384	34	1142	RF-2 LayerB
586	75	**199**	25	**785**	RF-6 LayerA
240	79	65	21	305	RF-6 LayerB
416	74	143	26	559	SZ-8 Surface
789	70	**344**	30	**1133**	SZ-8 LevelA
643	66	**328**	34	**971**	SZ-8 LevelB
206	67	99	33	305	SZ-8 LevelC
147	67	73	33	220	SZ-8 LevelD
25	71	10	29	35	SZ-8 LevelE

* Bold indicates redeposited sherds in an intensively gardened soil, resulting in highly broken sherdage.

Strong continuity is well displayed in the decorated component of the three Lapita site assemblages documented in Table 6. To convince yourself, first examine the data for SE-RF-2 where the biggest sherds often have several motifs on their body surfaces. Inevitably, when they are matched to other sherds and joined, even greater numbers of motifs are evident on a single entity. Most of these come from the Layer B formed of charcoal discoloured beach sand retaining cultural debris from a primary occupation on a former back beach composed of white coral sand. Items from this midden deposit including the sherds have become incorporated into a much later tephra-derived ash fall, with the smallest of them of them lying on the site's surface at the time of its investigation and their systematic recovery. The sherds within Layer A in particular have subsequently been broken into twice as many pieces, although some still can be joined to those in Layer B (Sheppard and Green 1991; Jones *et al*. 2007). Thus, the two decorative techniques have remained constant, but the number of motifs that can be coded from them is much smaller in Layer A. During the investigation of the heavily gardened sherds (through screening the first 3cm of the site's surface), the size of the sherds recovered was very tiny indeed, in most cases sufficient only to determine which decorative technique had been employed. Thus, even though potsherds were collected from more than 50% of the surface of the site, these highly broken sherds yielded only a few fragments for which a decorative motif could be recorded, even after their re-examination in the laboratory. The same pattern can be observed for SE-RF-6, where more than twice the

breakage occurred among the dentate and incised sherds that had also migrated, due to post occupation activities, into the ash fall of Layer A. For this site no comparable intensive surface survey was attempted; surface sherds were few indeed and in this case not particularly indicative of what lay below the surface, especially where the depth of the primary deposit markedly increased towards the shoreline of the marine water channel between the two islands.

On the other hand, an intensive surface collection was undertaken for the 459m² of the sampled area within SE-SZ-8, before it was investigated by excavating a sub-sample of 51m^2 employing the strategy of stratified, systemic unaligned selection of squares, which was the same as that used in the excavation of SE-RF-6 (Table 1). Levels C and D in SE-SZ-8 reflect the primary deposits forming the stained white sand occupation layer, with Level E (the lower-most portion of that occupation layer), present in only some squares, hence the low numerical counts from it. Levels A and B reflect alluvial and colluvial soil formed in the last 1000 years, with elements of Tinakula ashfall incorporated into this sealing deposit, whose age was determined by the dating of an earth oven from near the base of Layer B (Green *et al.* 2008). Thus the overlying soil of Layer A exhibits only a "dusting" from the same Tinakula tephra as an incorporated element, making it an andeptic soil type (Wall and Hansell 1976:37). This suggests its presence within the gardened zone has a secondary origin because the covering layer derives from in wash of soil from the volcanic interior of the island. In short, the age of primary tephra, which did not reach this part of eastern Nendö Island as an ash fall (occurring only as a dusting), would have to have been greater than the 1000 years indicated by the antiquity of the oven. Yet, the age of the major Tinakula eruption is not as great as 1800 years ago on current evidence from elsewhere on both Nendö and the Main Reef Islands (Doherty 2007).

The interest here is that the same pattern of "brokenness" prevails. The 560 sherds in the primary deposits have yielded the larger portion of the motifs coded, while the 559 sherds screened from the surface provided little more information than the type of decoration that was once employed during their manufacture. The 2104 sherds in the overlying soil have migrated into that context (Levels A and B) due to extensive gardening and some temporary daytime sheltering activity inland by the inhabitants from the current coastal villages or their immediate predecessors. This time the breakage or "brokenness" has been up to four times more severe in SE-SZ-8 than at SE-RF-2 and SE-RF-6. In this instance, Felgate's compilation of the decorated ceramic data has proven extremely helpful. Anyone wishing to utilise the ceramic collections from all three sites in a comparative exercise is better able to understand what has happened to them taphonomically after the period of their primary discard (i.e. during the short interval when the site was occupied by those manufacturing or importing the pottery vessels they used and in the course of those activities broke and then discarded the pieces).

Objectives and Predictions employed in the analytical section evaluating motif occurrence.

Following Dunnell (1970), Felgate presents a golden rule to be followed when conducting a seriation analysis – the aim of achieving as careful as is possible selection of those types of attributes that exhibit temporal changes through time. "Some sorts of attributes carry higher risk than others, and can usefully be omitted from analyses when the aim is chronological" (Felgate 2003:55). The aim in examining the two databases compiled in Tables 7 and 8 is to conduct the analyses with precisely those objectives in mind (i.e. keep some motifs and set others aside). In this case, the tables embody the use of percentage calculations of two kinds

to normalise the data. The first is a usual technique of the percentage in each motif category against the total number of all motifs recorded. This is displayed in Table 7. The second is calculated in part to compensate for "brokenness" as a biasing factor, despite the fact that the resulting percentage values become very small. This outcome is displayed in Table 8. The results from inspection of Tables 7 and 8 reveal that a whole series of motifs should be judged as unsuitable for chronological purposes, however useful they may be in documenting motif variation in the samples from the three sites examined in Part B of this section. These leave the investigation carried out in the final Part F of the section with four sub-sets of motifs judged to be the most informative if one is seeking to use the motif databases for the purpose of furnishing support to the chronological age order indicated by the radio carbon dates. Among the four sub-sets of Part F, one is counterintuitive to the proposed chronological order for the sites (based on independent 14C determinations). One larger set placed in Part D incorporates motifs whose occurrence varies within an expected range, indicating these motifs fail to provide evidence of chronological change or order. Finally, two of the four sub-sets in Part F contain those motifs exhibiting the most chronological promise in establishing the relative age order (Tables 17 and 18). In short, it proves possible to make selection of those motifs most useful in supporting the chronological order indicated by 14C dating when that constitutes a major objective. Yet it must be done in the context of all motifs and an *explanation* of why the others should be discarded, if documenting chronological change is the analytical purpose. However, one should not reject or dismiss this data if other concerns are also seen as relevant, especially if they answer other questions through the use of those motifs for quite different purposes.

The other assumptions are predictions that stem from an exchange between Kirch *et al*. (1987) and Anson (1987, 2000:120, 129-30, 132) about sample-size and its effect on the occurrence of motifs in any database used for comparison. In summary, Anson's first response to Kirch *et al*.'s criticisms was that the larger the sample involved, the greater the number of motifs recorded would be, but most of the motifs in that larger sample would occur in very low frequency, and might not register at all in smaller samples. His second response was that in samples with high motif counts for a particular motif, or a version of that motif in his system of coding, the numbers of such sherds would certainly increase, but the frequency in proportional percentage or other normalising procedures would alter rather little. Thus his comparative techniques were not as biased as some may have assumed, even where the numerical counts were rather less than one might wish. In sum, the outcome from using Anson's procedures was not necessarily as misleading as Kirch *et al*. (1987) imply, as long as the motifs employed appear in each site or site context above some minimal level. In the data sets of Tables 7 and 8, after the analyses set out in Part A, it proves possible to exclude those motifs where the sample numbers are so small in percentage terms that one cannot distinguish between fluctuations due to sampling error and those due to motifs that might prove useful in demonstrating chronological change. These motifs only serve to cloud any kind of successful data analysis.

Finally the exercises carried out in the following section and Parts A to F are *not* seen as definitive, and other statistically more powerful techniques could certainly be applied. An example is a bootstrap approach to estimating the effect of sample size on motif number (Sheppard and Green 2007). However, the fine grained analysis conducted here does indicate which are the motifs whose analyses are most likely to prove productive as chronological indicators in this region, and which could be omitted at present. This seems a far better outcome than the tendency to dismiss the whole enterprise. Although Best has done the latter, his prime objective was to replace that kind of analysis with yet another set of ceramic-based attributes and methodologies seen to accomplish the same kind of relative ceramic based

dating. It turns out that has proven even more misleading, because the application of his procedures results in a far more bizarre age order, one that all other evidence – including standard methods of independent dating by 14C age determinations – fail to support.

It is important, however, to emphasize that the assessments conducted here do provide answers to some of the issues Best raises: (i) in what ways has the size of the area excavated, and thus the number of sherds actually excavated, affected the results? – and (ii) should a common normalised comparative base be employed in their manipulation, analysis and interpretation? The latter procedure, of course, neither Best nor Felgate have adopted in their critiques. For that reason a strict proportionality to area size claim has been effectively shown to be statistically invalid when it only uses raw counts. Still that does not mean that in certain instances, the amount of the area excavated could not be part of the story, and for those motifs so affected, they only should be omitted from the database when the aim of an analysis is strictly chronological. In regard to the fine-grained evaluations presented in Parts A to F and their accompanying tables, the results should be of more than a little interest to all future users of the basic databases provided in Tables 7 and 8. Previously this information has not been available except to those few requesting it. Hence their publication here in the RAL-e series along with the illustration of the majority of those motifs seen as providing useful information should prove beneficial to future researchers.

Basic lists for the 100 coded lapita-style motifs providing number frequency and percentages used in all those computational analyses appearing in parts A to G.

The database of the motifs identified and described by Donovan (1973) in due course became the basic descriptive information on which nearly all subsequent analyses were conducted during the rest of the 1970s and 1980s. However, neither the text and tables of Donovan's research essay, nor her accompanying catalogue of illustrations were ever edited or prepared for publication. Nor did various emendations or expansions to it appear except in outline form (Green 1990: Appendices 1 to 5). Moreover, Anson (1983, 1990) converted a great number of the rule-related variations on a Donovan/Mead motif, designated as alloforms of the initial coded design, into a fine-grained corpus of dentate-stamped motif designs that consecutively numbered 516 in total. He subsequently, using both the older and more recently recovered dentate-decorated sherds from the Reber-Rakival Lapita site on Watom Island, expanded that total to 527 illustrated and numbered dentate-stamped motif designs (Anson 2000: Table 1). Tables 7 and 8 which follow, published in full for the first time, provide the basic numerical information on the frequency with which each of the 100 motifs occurs in the three sites of SE-SZ-8, SE-RF-2 and SE-RF-6. The percentages for their occurrences are then calculated, first to a base of the total number of motifs identified in any one of the three sites, and then to a base of the total number of decorated sherds from each site.

Databases for all motif counts

Table 7 Base for percentage calculation: total motifs identified and provided with some overall motif designation (i.e. all alloforms of a motif are bundled under the one general motif number).

Motif	*SE-SZ-8 (607)*	*SE-RF-2 (840)*	*SE-RF-6 (247)*
M1	19 (3.13%)	21 (2.50%)	31 (12.55%)
M2	18 (2.97%)	50 (5.95%)	18 (7.29%)
M3	2 (0.33%)	3 (0.36%)	-
M4	-	1 (0.12%)	-
M5	45 (7.41%)	72 (8.57%)	31 (12.55%)
M6	39 (6.43%)	33 (3.93%)	21 (8.50%)
M7	-	-	-
M8	4 (0.66%)	56 (6.67%)	10 (4.05%)
M9	1 (0.16%)	16 (1.90%)	-
M10	2 (0.33%)	12 (1.43%)	2 (0.81%)
M11	-	-	-
M12	-	-	-
M13	-	4 (0.48%)	-
M14	3 (0.49%)	29 (3.45%)	5 (2.02%)
M15	-	-	-
M16	5 (0.82%)	6 (0.71%)	-
M17	-	2 (0.24%)	-
M18	28 (4.61%)	53 (6.31%)	8 (3.24%)
M19	61 (10.05%)	46 (5.48%)	5 (2.02%)
M20	-	-	-
M21	-	-	-
M22	-	-	-
M23	-	-	-
M24	26 (4.28%)	21 (2.50%)	4 (1.62%)
M25	-	1 (0.12%)	-
M26	-	-	-
M27	-	5 (0.60%)	-
M28	9 (1.48%)	35 (4.17%)	4 (1.62%)
M29	11 (1.81%)	3 (0.36%)	3 (1.21%)
M30	1 (0.16%)	20 (2.38%)	1 (0.40%)
M31	-	-	-
M32	-	-	-
M33	-	-	-
M34	-	-	1 (0.40%)
M35	-	-	-
M36	-	-	-
M37	-	-	-
M38	-	-	-
M39	23 (3.79%)	8 (0.95%)	2 (0.81%)
M40	-	-	-
M41	-	-	-
M42	-	-	-

Table 8 Another base for the percentage calculation: total decorated sherds recovered from each site (whether or not a motif or motifs could be identified on the sherd).

SE-SZ-8 (3079)	*SE-RF-2 (3927)*	*SE-RF-6 (1230)*
19 (0.62%)	21 (0.53%)	31 (2.52%)
18 (0.58%)	50 (1.27%)	18 (1.46%)
2 (0.06%)	3 (0.08%)	-
-	1 (0.03%)	-
45 (1.46%)	72 (1.83%)	31 (2.52%)
39 (1.27%)	33 (0.84%)	21 (1.71%)
-	-	-
4 (0.13%)	56 (1.43%)	10 (0.81%)
1 (0.03%)	16 (0.41%)	-
2 (0.06%)	12 (0.31%)	2 (0.16%)
-	-	-
-	-	-
-	4 (0.10%)	-
3 (0.10%)	29 (0.74%)	5 (0.41%)
-	-	-
5 (0.16%)	6 (0.15%)	-
-	2 (0.05%)	-
28 (0.91%)	53 (1.35%)	8 (0.65%)
61 (1.98%)	46 (1.17%)	5 (0.41%)
-	-	-
-	-	-
-	-	-
-	-	-
26 (0.84%)	21 (0.53%)	4 (0.33%)
-	1 (0.03%)	-
-	-	-
-	5 (0.13%)	-
9 (0.29%)	35 (0.89%)	4 (0.33%)
11 (0.36%)	3 (0.08%)	3 (0.24%)
1 (0.03%)	20 (0.51%)	1 (0.08%)
-	-	-
-	-	-
-	-	-
-	-	1 (0.08%)
-	-	-
-	-	-
-	-	-
-	-	-
23 (0.75%)	8 (0.20%)	2 (0.16%)
-	-	-
-	-	-
-	-	-

M43	-	-	-	-	-	-
M44	-	-	-	-	-	-
M45	-	11 (1.31%)	-	-	11 (0.28%)	-
M46	-	-	-	-	-	-
M47	-	2 (0.24%)	-	-	2 (0.05%)	-
M48	7 (1.15%)	8 (0.95%)	1 (0.40%)	7 (0.23%)	8 (0.20%)	1 (0.08%)
M49	4 (0.66%)	14 (1.67%)	1 (0.40%)	4 (0.13%)	14 (0.36%)	1 (0.08%)
M50	-	1 (0.12%)	-	-	1 (0.03%)	-
M51	3 (0.49%)	-	-	3 (0.10%)	-	-
M52	-	2 (0.24%)	-	-	2 (0.05%)	-
M53	3 (0.49%)	3 (0.36%)	3 (1.21%)	3 (0.10%)	3 (0.08%)	3 (0.24%)
M54	-	1 (0.12%)	-	-	1 (0.03%)	-
M55	-	9 (1.07%)	-	-	9 (0.23%)	-
M56	11 (1.81%)	13 (1.55%)	-	11 (0.36%)	13 (0.33%)	-
M57	-	7 (0.83%)	-	-	7 (0.18%)	-
M58	-	2 (0.24%)	-	-	2 (0.05%)	-
M59	7 (1.15%)	4 (0.48%)	-	7 (0.23%)	4 (0.10%)	-
M60	1 (0.16%)	12 (1.43%)	-	1 (0.03%)	12 (0.31%)	-
M61	-	2 (0.24%)	-	-	2 (0.05%)	-
M62	4 (0.66%)	8 (0.95%)	2 (0.81%)	4 (0.13%)	8 (0.20%)	2 (0.16%)
M63	-	1 (0.12%)	-	-	1 (0.03%)	-
M64	4 (0.66%)	7 (0.83%)	-	4 (0.13%)	7 (0.18%)	-
M65	-	10 (1.19%)	-	-	10 (0.25%)	-
M66	-	5 (0.60%)	-	-	5 (0.13%)	-
M67	21 (3.46%)	50 (5.95%)	31 (12.55%)	21 (0.68%)	50 (1.27%)	31 (2.52%)
M68	-	4 (0.48%)	1 (0.40%)	-	4 (0.10%)	1 (0.08%)
M69	161 (26.52%)	56 (6.67%)	19 (7.69%)	161 (5.23%)	56 (1.43%)	19 (1.54%)
M70	1 (0.16%)	1 (0.12%)	1 (0.40%)	1 (0.03%)	1 (0.03%)	1 (0.08%)
M71	-	1 (0.12%)	-	-	1 (0.03%)	-
M72	-	3 (0.36%)	2 (0.81%)	-	3 (0.08%)	2 (0.16%)
M73	-	3 (0.36%)	-	-	3 (0.08%)	-
M74	-	6 (0.71%)	-	-	6 (0.15%)	-
M75	-	4 (0.48%)	-	-	4 (0.10%)	-
M76	3 (0.49%)	5 (0.60%)	-	3 (0.10%)	5 (0.13%)	-
M77	33 (5.44%)	29 (3.45%)	6 (2.43%)	33 (1.07%)	29 (0.74%)	6 (0.49%)
M78	3 (0.49%)	-	2 (0.81%)	3 (0.10%)	-	2 (0.16%)
M79	4 (0.66%)	35 (4.17%)	2 (0.81%)	4 (0.13%)	35 (0.89%)	2 (0.16%)
M80	4 (0.66%)	3 (0.36%)	2 (0.81%)	4 (0.13%)	3 (0.08%)	2 (0.16%)
M81	-	4 (0.48%)	-	-	4 (0.10%)	-
M82	-	3 (0.36%)	-	-	3 (0.08%)	-
M83	1 (0.16%)	2 (0.24%)	2 (0.81%)	1 (0.03%)	2 (0.05%)	2 (0.16%)
M84	-	4 (0.48%)	-	-	4 (0.10%)	-
M85	-	6 (0.71%)	1 (0.40%)	-	6 (0.15%)	1 (0.08%)
M86	-	-	2 (0.81%)	-	-	2 (0.16%)
M87	1 (0.16%)	-	1 (0.40%)	1(0.03%)	-	1 (0.08%)
M88	-	-	-	-	-	-
M89	3 (0.49%)	-	2 (0.81%)	3 (0.10%)	-	2 (0.16%)
M90	2 (0.33%)	-	1 (0.40%)	2 (0.06%	-	1 (0.08%)
M91	2 (0.33%)	-	-	2 (0.06%)	-	-
M92	1 (0.16%)	-	-	1 (0.03%)	-	-
M93	3 (0.49%)	-	-	3 (0.10%)	-	-
M94	2 (0.33%)	-	-	2 (0.06%)	-	-
M95	1 (0.16%)	-	-	1 (0.03%)	-	-

M96	1 (0.16%)	-	-	1 (0.03%)	-	-
M97	-	-	-	-	-	-
M98	-	1 (0.12%)	-	-	1 (0.03%)	-
M99	19 (3.13%)	1 (0.12%)	15 (6.07%)	19 (0.62%)	1 (0.03%)	15 (1.22%)
M100	-	-	4 (1.62%)	-	-	4 (0.33%)

PART A: Sherds bearing motifs whose presence and frequency is affected by area excavated

One predictable outcome of an effect of sample size is that a far higher than expected number of very low frequency or uncommon motifs would be expected in site SE-RF-2, where the excavated area was 72m². Fewer low frequency or uncommon motifs would be expected for SE-SZ-8 where the area was 51m². And very few such motifs would be expected in SE-RF-6, where the sample derived from just 20m². This prediction relies on the same density of items per m² in each site as set out earlier in this essay. It also relies on approximately the same thickness of the cultural deposit from which the sub-samples derive.

The evidence supporting these proposals is for sherds bearing motifs that are not encountered in any other site, and are also in very low numbers in the site in which they occur, especially when expressed in % terms. This can be seen in Table 9.

Table 9 Motifs uncommon in, and not in the other two sites, or seemingly common in (i.e. over 1%) though not appearing in the other two sites.

Note: Some of the seemingly more common motifs in fact register presences well below 1% when computed against a base of all decorated sherds. The square bracket outcomes in the table below are indicated only for those examples with a number resulting in an outcome greater than 1%.

SZ-8:
- <1%
  - M51, M92, M95, M96 – 1 example
  - M91, M94 – 2 examples
  - M93 – 3 examples

RF-2:
- <1%
  - M4, M25, M54, M61, M63, M71, M98 – 1 example
  - M17, M47, M52, M58 – 2 examples
  - M73, M82 – 3 examples
  - M13, M75, M81, M84 – 4 examples
  - M27, M66 – 5 examples
  - M74 – 6 examples
  - M57 – 7 examples
- >1%
  - M55 – 9 examples (1.07%) [9 (0.23%)]
  - M65 – 10 examples (1.19%) [10 (0.25%)]
  - M45 – 11 examples (1.31%) [11 (0.28%)]
  - M9 – 16 examples (1.90%) [16 (0.41%)]

RF-6:
- M34 – 1 example (0.40%)
- M86 – 2 examples (0.81%) [2 (0.16%)]
- M100 – 4 examples (1.62%) [4 (0.33%)]

These outcomes conform to the expected predictions discussed above, based on the experience of Anson in dealing with many other decorated pottery Lapita sites. As anticipated, the most numerous motifs restricted to a single site occur in the largest sample of sherds among the sites compared, in this instance SE-RF-2, and the fewest unique motifs derive from the smallest sized sample of sherds in SE-RF-6.

PART B: Motifs discarded due to infrequent occurrence

This part includes those motifs which occur on sherds in more than one site—usually all three—but which may not be useful in detecting changes through time, if the relative age order of sites is SE-SZ-8, SE-RF-2, SE-RF-6, i.e. from the earliest to the latest sub-sample. This is an order postulated principally on the basis of a suite of older and current 14C dates indicative of an age order where SE-RF-6 is clearly the youngest and SE-RF-2 is older, and SE-SZ-8 near contemporary but perhaps slightly older that SE-RF-2.

(a) In this analysis certain motifs are not recommended for further use in chronological analyses, either because (1) their presence in each site falls below 1.2% (Table 10), or (2) they were present in some sites and their failure to appear in others is probably due to sampling error (Table 11), or (3) their number in SE-SZ-8 and SE-RF-2 is too small to predict their expected frequency in SE-RF-6 (Table 12).

[NOTE: in this Part B—and in Parts C through F—square bracketed outcomes are those calculated from a total decorated sherd base set out in Table 8. In all cases the number of examples appears first, followed by the percentage in brackets.]

Table 10 Motifs present in each site, but at too low a frequency.

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M3	2 (0.33%) [2 (0.06%)]	3 (0.36%) [3 (0.08%)]	Error-*p.90 in Donovan indicates it as present
M70	1 (0.16%) [1 (0.03%)]	1 (0.12%) [1 (0.03%)]	1 (0.40%) [1 (0.08%)]
M83	1 (0.16%) [1 (0.03%)]	2 (0.24%) [2 (0.05%)]	2 (0.81%) [2 (0.16%)]

Table 11 Motifs present between 0.33 and 1.0% in SE-SZ-8 and SE-RF-6, but whether to be expected in SE-RF-2 is uncertain. (Their failure to appear is most probably due to sampling error, rather than a case of true absence, even with the far greater excavated area and sherd sample size from SE-RF-2).

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M78	3 (0.49%) [3 (0.10%)]	Expected, but none	2 (0.81%) [2 (0.16%)]
M87	1 (0.16%) [1 (0.03%)]	Expected, but none	1 (0.40%) [1 (0.08%)]
M89	3 (0.49%) [3 (0.10%)]	Expected, but none	2 (0.81%) [2 (0.16%)]
M90	2 (0.33%) [2 (0.06%)]	Expected, but none	1 (0.40%) [1 (0.08%)]

Table 12 Motifs present in SE-RF-2 and SE-RF-6, but whether or not in SE-SZ-8 is uncertain. (The presence of three motifs, M68, M72 and M85 are all at levels of 1% or less where present, so that only 0.6 of a sherd would be expected in SZ-8 on sample size alone).

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M68	None really expected	4 (0.48%) [4 (0.10%)]	1 (0.40%) [1 (0.08%)]
M72	None really expected	3 (0.36%) [3 (0.08%)]	2 (0.81%) [2 (0.16%)]
M85	None really expected	6 (0.71%) [6 (0.15%)]	1 (0.40%) [1 (0.08%)]

PART C: Motifs over represented in site SE-RF-2

The set of motifs listed in Table 13, is clearly over represented in SE-RF-2 (column 2 of Table 13) on the basis of the evidence of their frequency in raw numbers and in percentage form. This outcome holds most clearly when using the proposed order in which sites are thought to occur, i.e. the frequency of the motif in percentage form in SE-RF-2 is many times the very low frequency by percentage in either SE-SZ-8, the earlier site, or SE-RF-6, the later site of columns 1 and 3).

These constitute a limited set of just seven motifs, among the 100 recorded, which certainly could be interpreted as complying with Best's expectations of influence due to the much greater area sampled at SE-RF-2. Their numbers are not, however, unduly many, nor is the effect of that factor a sole or major cause for the differences apparent in the motif frequencies.

Table 13 Motifs over represented in site SE-RF-2 due to the far greater area excavated.

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M8	4 (0.66%) [4 (0.13%)]	56 (6.67%) [56 (1.43%)]	10 (4.05%) [10 (0.81%)]
M79	4 (0.66%) [4 (0.13%)]	35 (4.17%) [35 (0.89%)]	2 (0.81%) [2 (0.16%)]
M28	9 (1.48%) [9 (0.29%)]	35 (4.17%) [35 (0.89%)]	4 (1.62%) [4 (0.33%)]
M14	3 (0.49%) [3 (0.10%)]	29 (3.45%) [29 (0.74%)]	5 (2.02%) [5 (0.41%)]
M30	1 (0.16%) [1 (0.03%)]	20 (2.38%) [20 (0.51%)]	1 (0.40%) [1 (0.08%)]
M9	1 (0.16%) [1 (0.03%)]	16 (1.90%) [16 0.41%)]	None
M49	4 (0.66%) [4 (0.13%)]	14 (1.67%) [14 (0.36%)]	1 (0.40%) [1 (0.08%)]
M10	2 (0.33%) [2 (0.06%)]	12 (1.43%) [12 (0.31%)]	2 (0.33%) [2 (0.16%)]

[Note the ordering of the motif numbers in Table 13 is not consecutive, but by their descending frequency in SE-RF-2].

PART D: Motifs exhibiting no trend between the three sites

A number of motifs are found at raw number frequencies, which might be thought to indicate some trend over time. But when converted to percentages, they fit a neutral pattern of relatively constant occurrence from early to late. This means the observed outcomes do not really deviate significantly from those to be expected, but can be explained most simply as normal variations due to chance in the sampling process.

For purposes of caution, these can be separated into two clusters, each of which is set out separately in Table 14. The first cluster of motifs includes those where occurrences in any of the three sites are at 1.2% or less, and it would be unwise to place much confidence in the outcome. The other cluster, in contrast, is one where there are significant numbers of sherds, and therefore reasonably informative percentages, allowing one to discount sampling error with more assurance. The outcome from this assessment is that M6, M18 (and perhaps M29) all qualify as motifs which prove not to possess strong indications of change over the period of time represented by these three sites; they therefore should not be employed in any analytical attempts to determine their chronological order. This may also be true of some motifs in the first cluster, had we larger samples on which to pass judgment.

Table 14 Motifs exhibiting fairly constant occurrence over 500 years time.

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
1.2% or less, and thus marginally significant percentages			
M48	7 (1.15%) [7 (0.23%)]	8 (0.95%) [8 (0.20%)]	1 (0.40%) [1 (0.08%)]
M53	3 (0.49%) [3 (0.10%)]	3 (0.36%) [3 (0.08%)]	3 (1.21%) [3 (0.24%)]
M62	4 (0.66%) [4 (0.13%)]	8 (0.95%) [8 (0.20%)]	2 (0.81%) [2 (0.16%)]
M80	4 (0.66%) [4 (0.13%)]	3 (0.36%) [3 (0.08%)]	2 (0.81%) [2 (0.16%)]
Significant percentages			
M6	39 (6.43%) [39 (1.27%)]	33 (3.93%) [33 (0.84%)]	21 (8.50%) [21 (1.71%)]
M18	28 (4.61%) [28 (0.91%)]	53 (6.31%) [53 (1.35%)]	8 (3.24%) [8 (0.65%)]
M29	11 (1.81%) [11 (0.36%)]	3 (0.36%) [3 (0.08%)]	3 (1.21%) [3 (0.24%)]

Figure 4 Motifs M48, M53, M62 and M80 along with their alloforms (Figure 4a) and M6, M18, and M29 and their alloforms (Figure 4b) displaying fairly constant frequencies of occurrence over a temporal interval of circa 500 years. (*over page*)

Note: In order to create the following motif and alloform images in Figures 4 to 7, the original motifs from Donovan's 1973 work were consulted. The motifs were recreated in Adobe Photoshop using Donovan's original drawings as a guide. Zone markers or borders were considered in most instances to have been composed of overlapping dentate-stamped composition and the bulk of the motif designs within them were also composed in the dentate-stamped technique. In cases where a motif appeared in the original ceramic series in both a dentate-stamped and fine-line lineal incision form, motifs were illustrated separately in both techniques to ensure clarity of the graphic representations and their observed occurrences. Those motifs represented only in the form of incised lineal form on a potsherd are illustrated only in that technique.

Figure 4a

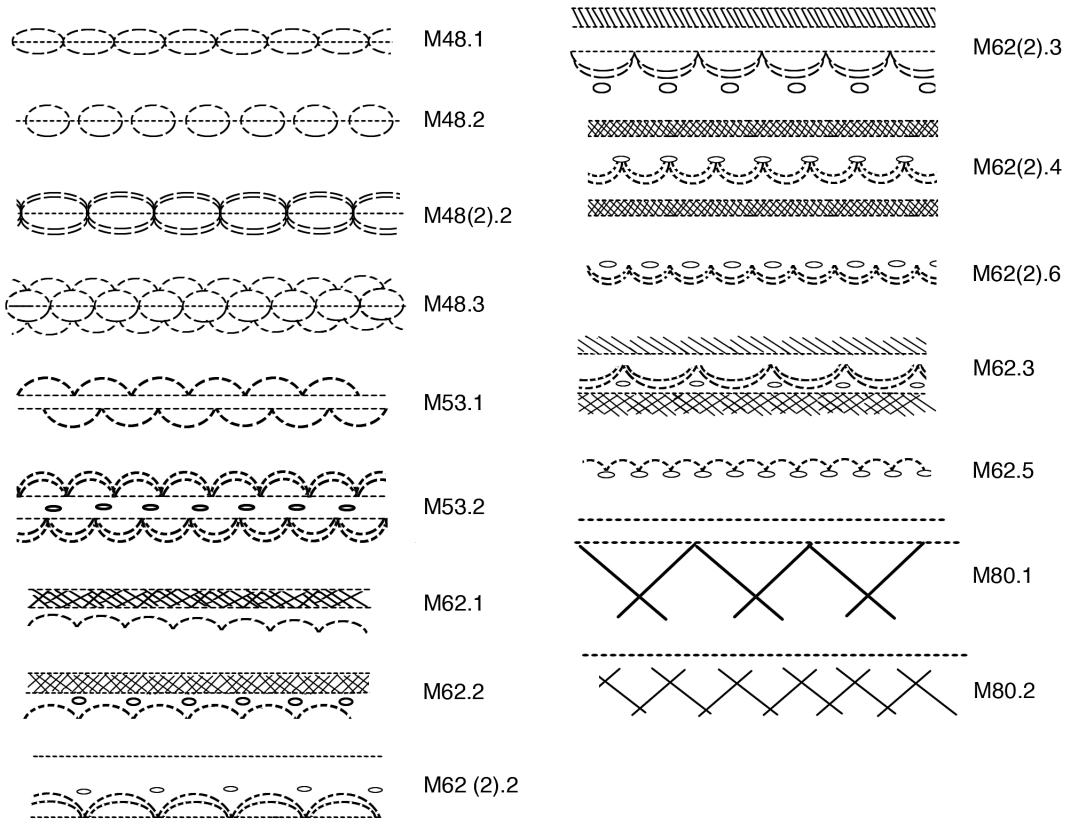


Figure 4b

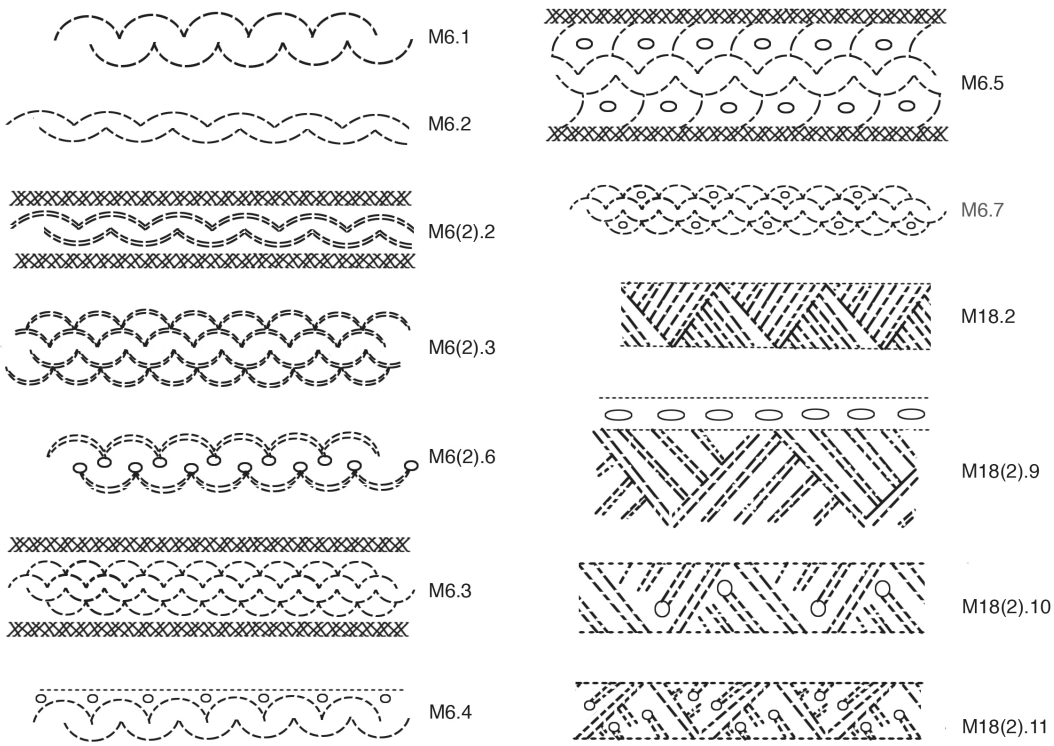
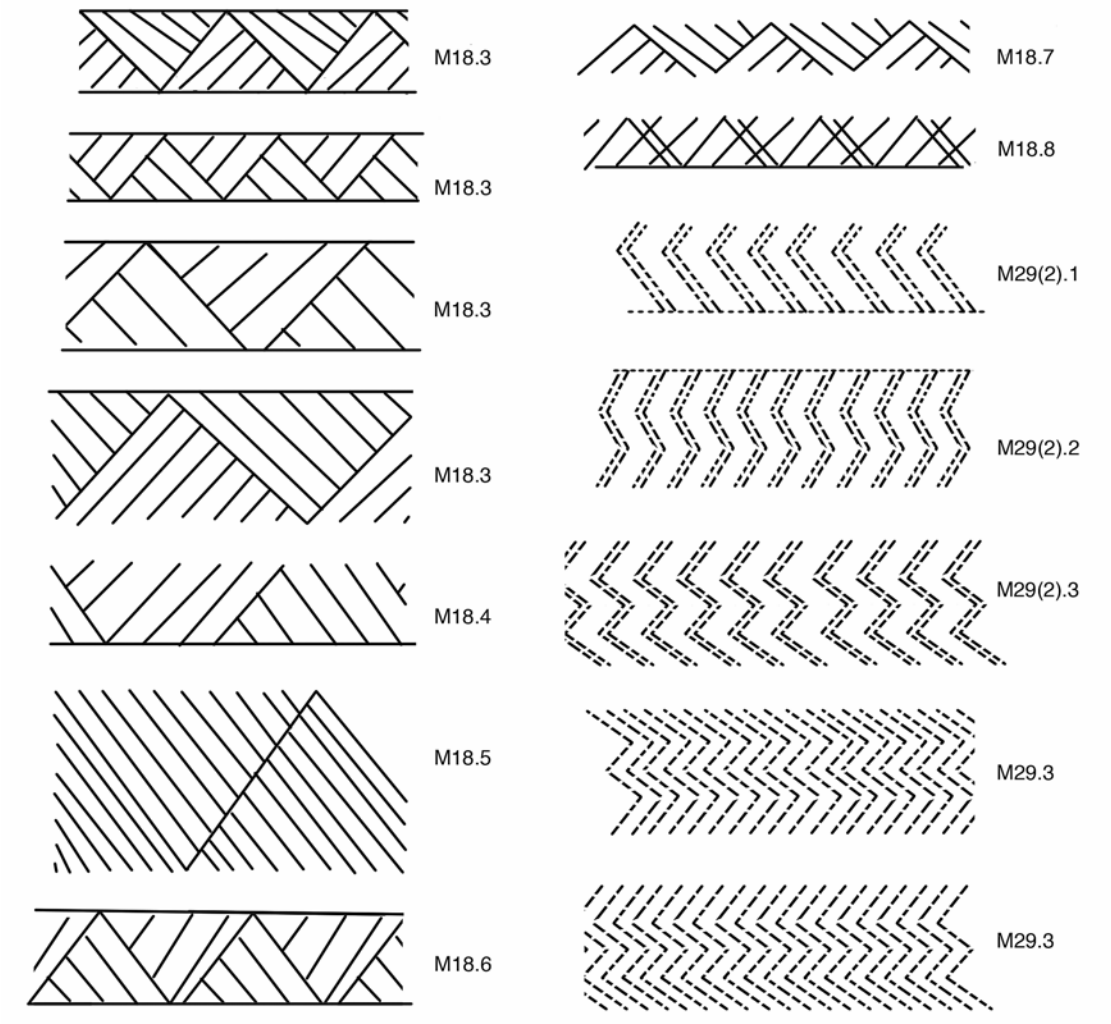


Figure 4b *continued*



PART E: Motif numbers not in conformity with the assumed temporal trend

Part E has been reserved for motifs whose percentages are contrary to all the current chronological or other expectations, i.e. the outcome for SE-RF-2 was 1 or zero, despite the large size of the sub-sample of sherds involved.

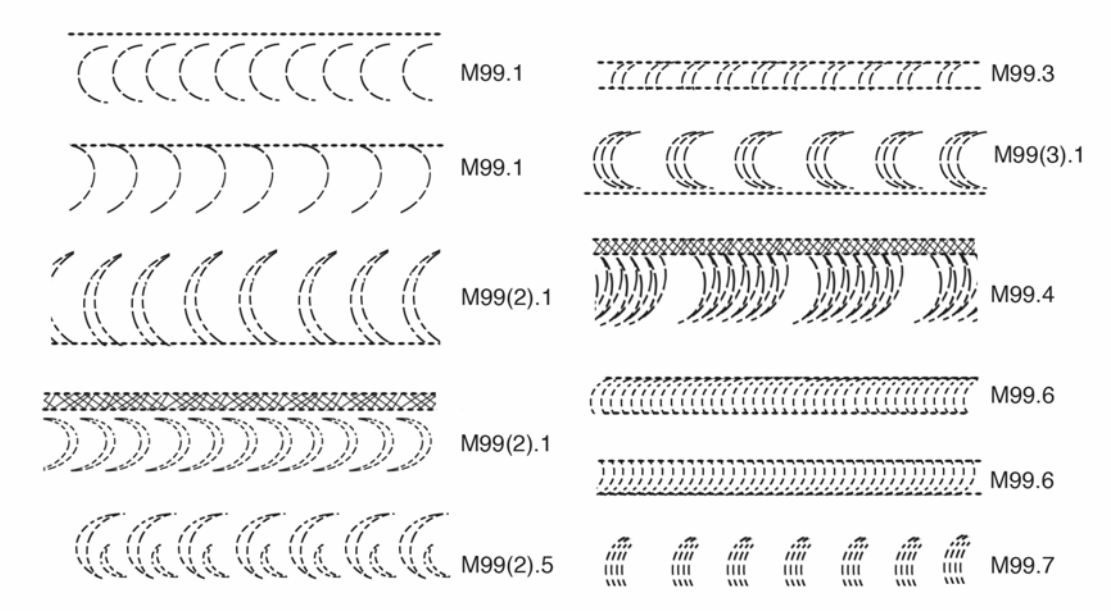
Case 1 in Table 15 involves motif M99, which was well represented in sites SE-SZ-8 and SE-RF-6, yet quite unexpectedly recorded as present only on one sherd in SE-RF-2. As it was also a common motif in Site 13 (the toponymic Lapita site in New Caledonia), its paucity in SE-RF-2 is not easily explained, given that on all other motif criteria, SE-RF-2 and Site 13 are deemed the most similar. This outcome may be a failure of recognition during the recording process, yet, if so, it stands as the only instance of this to be identified.

Table 15 An unexplained outcome of motif analysis – perhaps due to a recording failure.

Motif	*SE-Z-8*	*SE-RF-2*	*SE-RF-6*
M99	19 (3.13%) [19 (0.62%)]	1 (0.12%) [1 (0.03%)]	15 (6.07%) [15 (1.22%)]

Figure 5 Motif M99 along with its 7 alloforms (Figure 5a) and M78, M89 and its allomorph and M90 (Figure 5b) displaying the infrequent or nil occurrence of these motifs in SE-RF-2, a result not in keeping with their limited frequency of occurrence in SE-SZ-8 and SE-RF-6.

Figure 5a

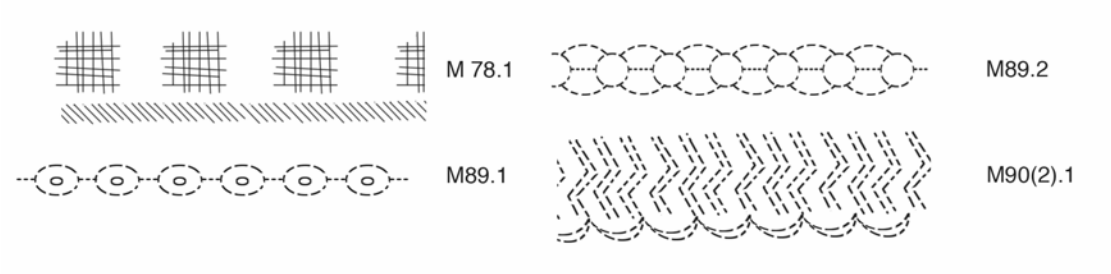


Case 2 in Table 16 involves the examples of motifs where the expected numbers in site SE-RF-2, based on either SE-SZ-8 or SE-RF-6, are so small that one questions the predicted numbers for SE-RF-2. Thus, one would have expected 2 or 3 sherds of motifs M78, M89 and M90 in SE-RF-2 using their percentage frequency in SE-SZ-8, but none occur. Alternatively, one could predict 3 sherds of M90 and 7 sherds of M78 and M89 based on the percentage frequency of those in SE-RF-6. Again, however, none occur. Table 15 and 16 clearly show that, contra Best, the size of area investigated alone does not necessarily yield a sample that is highly predictive. Moreover, it indicates that the kind of motif forming the sample constitutes yet another variable.

Table 16 Yet another unexplained outcome of motif analysis – perhaps due to low numbers in predictive base (their absence in SE-RF-2 therefore may be attributable to sampling error)

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M78	3 (0.49%) [3 (0.10%)]	-	2 (0.81%) [2 (0.16%)]
M89	3 (0.49%) [3 (0.10%)]	-	2 (0.81%) [2 (0.16%)]
M90	2 (0.33%) [2 (0.06%)]	-	1 (0.40%) [1 (0.08%)]

Figure 5b



PART F: Motifs exhibiting a trend between the three sites in support of a chronological order

The three sets of motifs in Part F occur in sufficient numbers and percentages to be forwarded in support of an age order of SE-SZ-8, SE-RF-2 and SE-RF-6. In the exercise conducted here, it has proven possible to refine the 100 motifs recorded – in the database that includes the three sites – down to a much smaller set that adequately serves as the most effective indicator of a chronological order. The first set of five motifs in Table 17 is a set in which the frequencies for each motif decline by a percentage difference of 3% or more between SE-SZ-8, SE-RF-2 and SE-RF-6, or SE-SZ-8 as opposed to SE-RF-2 and SE-RF-6, or in total between SE-SZ-8 and SE-RF-6.

Table 17 Highest frequency of motifs by percentage in Site SE-SZ-8, with declining frequency in SE-RF-2 and/or SE-RF-6

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M69	161(26.52%)[161(5.23%)]	56 (6.7%) [56 (1.43%)]	19 (7.69%) [19(1.54%)]
M19	61 (10.05%) [61 (1.98%)]	46 (5.48%) [46 (1.17%)]	5 (2.02%) [5 (0.41%)]
M77	33 (5.44%) [33 (1.07%)]	29 (3.45%) [29 (0.74%)]	6 (2.43%) [6 (0.49%)]
M24	26 (4.28%) [26 (0.84%)]	21 (2.50%) [21 (0.53%)]	4 (1.62%) [4 (0.33%)]
M39	23 (3.79%) [23 (0.75%)]	8 (0.95%) [8 (0.20%)]	2 (0.81%) [2 (0.16%)]

Figure 6 Motifs M69, M19, M77, M24 and M39 along with their alloforms (Figure 6a); motifs M5, M2, M67 and M1 and their alloforms (Figure 6b) and M56, M16, M64, M76 and M60 along with their alloforms (Figure 6c). Each instance exhibits either a significantly increasing or decreasing trend in occurrence between the three sites when the three sites are placed in their 14C determined chronological order. One increasing trend (Figure 6c) starts from a limited frequency of occurrence in the two early sites (SE-SZ-8 and SE-RF-2) and ends with a nil representation in the much later site of SE-RF-6.

Figure 6a

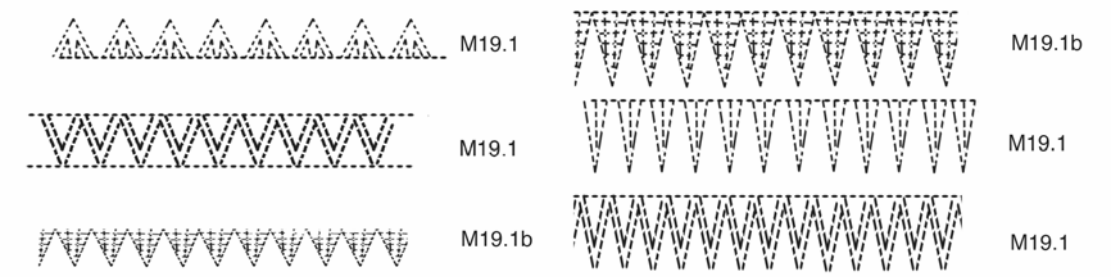


Figure 6a *continued*

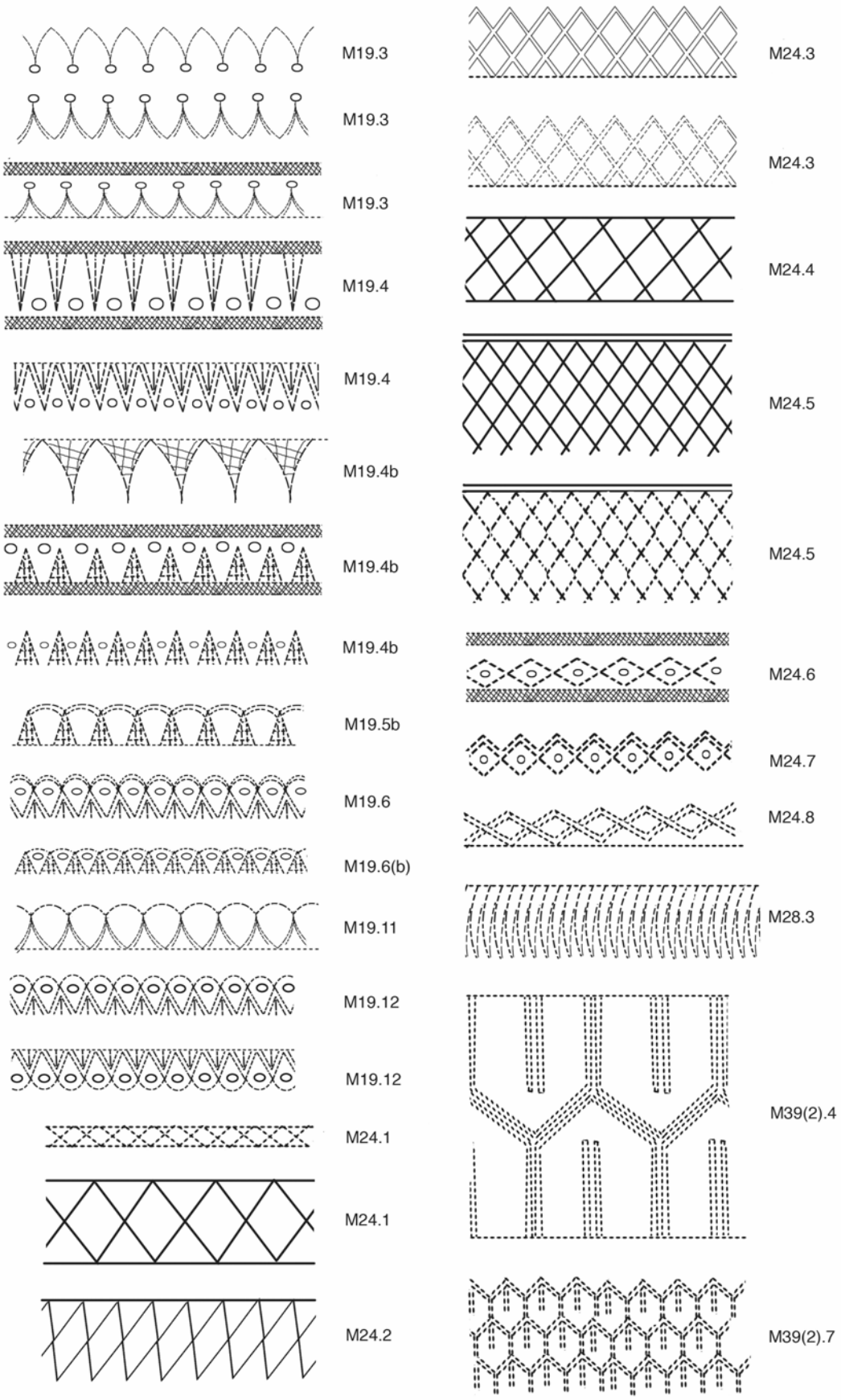


Figure 6a *continued*

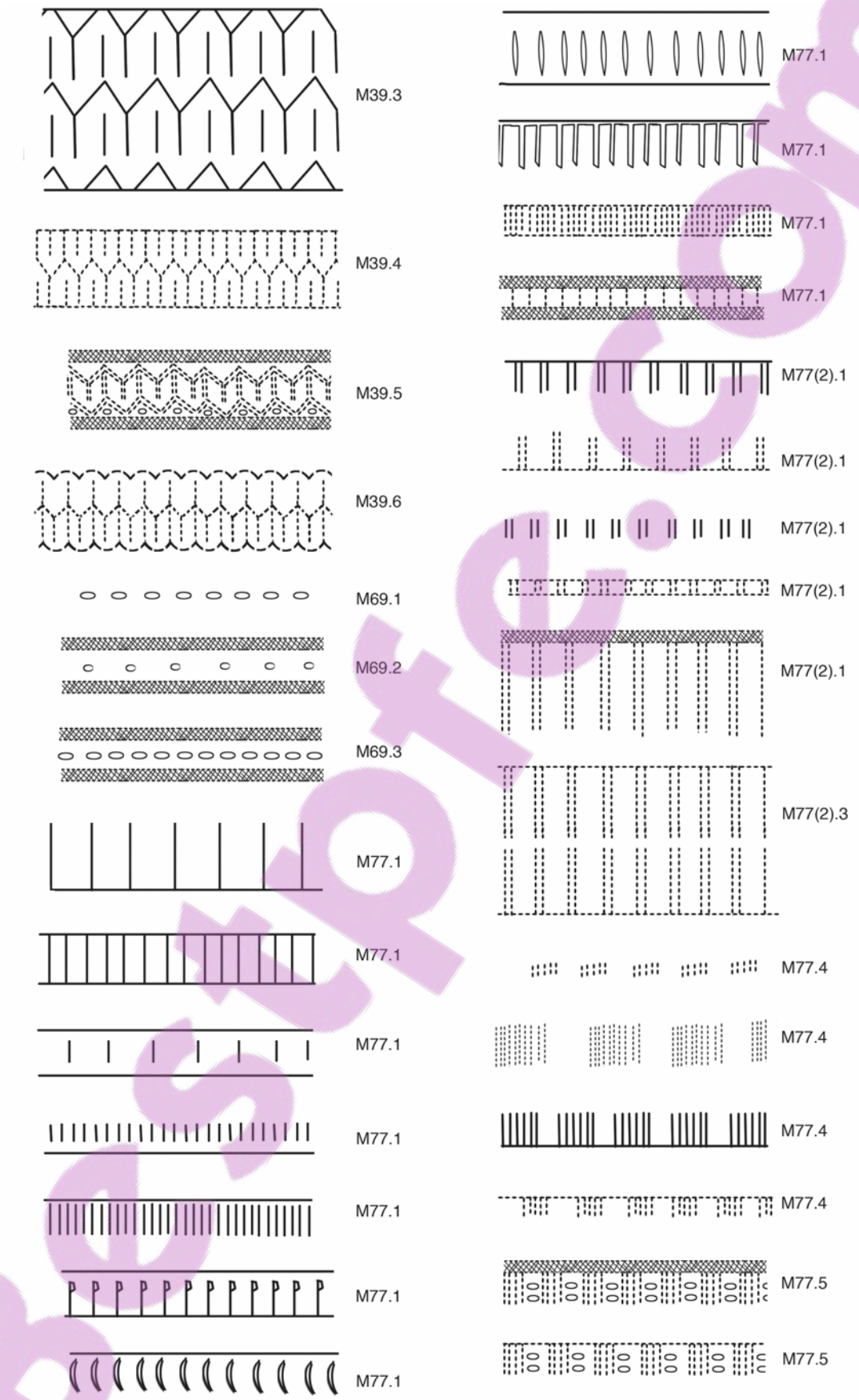
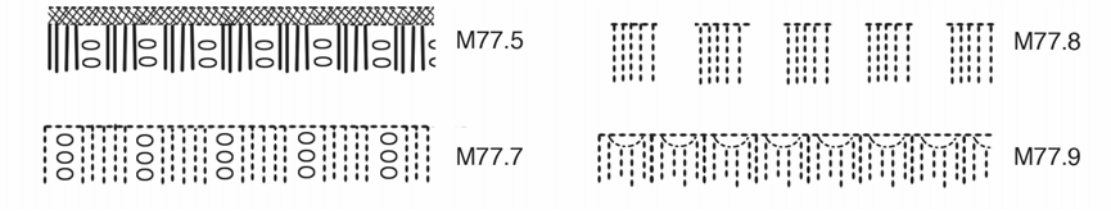


Figure 6a *continued*



The second set of motifs in Table 18 is where the differences are 4% or more, and the lowest percentage of occurrence occurs in SE-SZ-8 and rises in SE-RF-2, or SE-RF-2 and SE-RF-6, or in SE-RF-6 from SE-SZ-8 and SE-RF-2. It is most encouraging that this trend is the reverse of that revealed in Table 17. It establishes that the trend of change in motif occurrence need not be assumed to have been unidirectional among all such chronologically ordered motifs. In seriation, in respect to this technique of establishing relative age orders, this point was made from the beginning of its practice in archaeology.

Table 18 Lowest frequency of motifs by percentage in Site SE-SZ-8, with rising frequency in SE-RF- 2 and/or SE-RF-6.

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M5	45 (7.41%) [45 (1.46%)]	72 (8.57%) [72 (1.83%)]	31(12.55%) [31(2.52%)]
M2	18 (2.97%) [18 (0.58%)]	50 (5.95%) [50 (1.27%)]	18 (7.29%) [18 (1.46%)]
M67	21 (3.46%) [21 (0.68%)]	50 (5.95%) [50 (1.27%)]	31(12.55%) [31(2.52%)]
M1	19 (3.13%) [19 (0.62%)]	21 (2.50%) [21 (0.53%)]	31(12.55%) [31(2.52%)]

Figure 6b

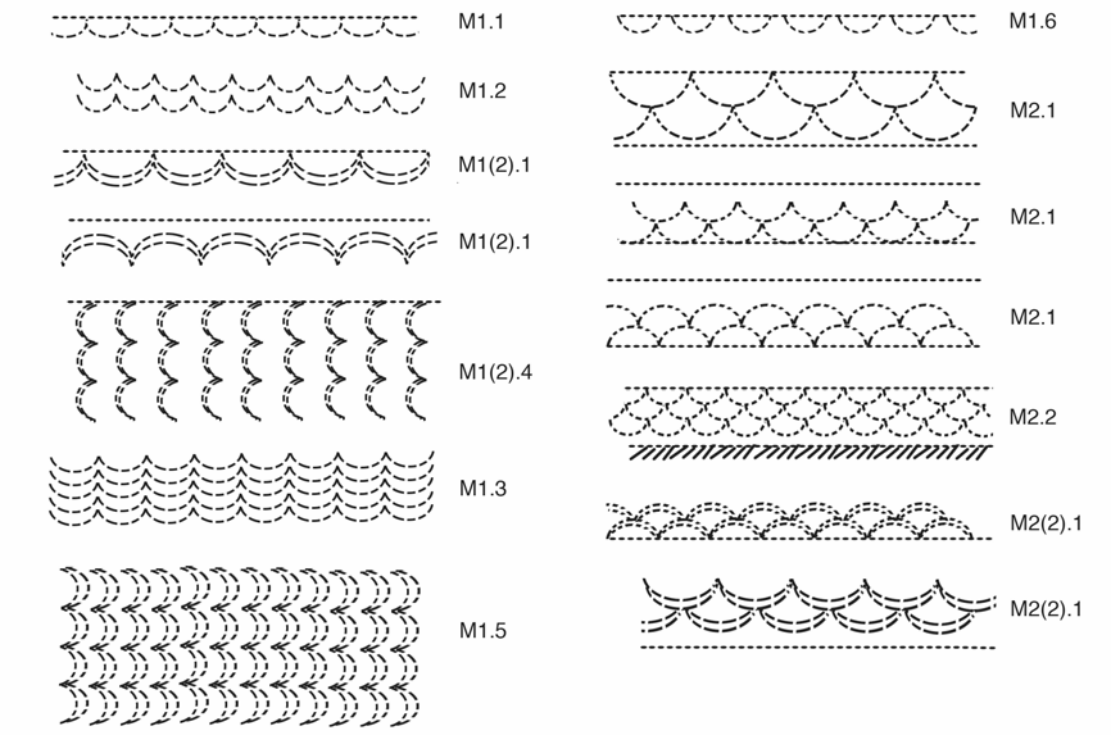


Figure 6b *continued*

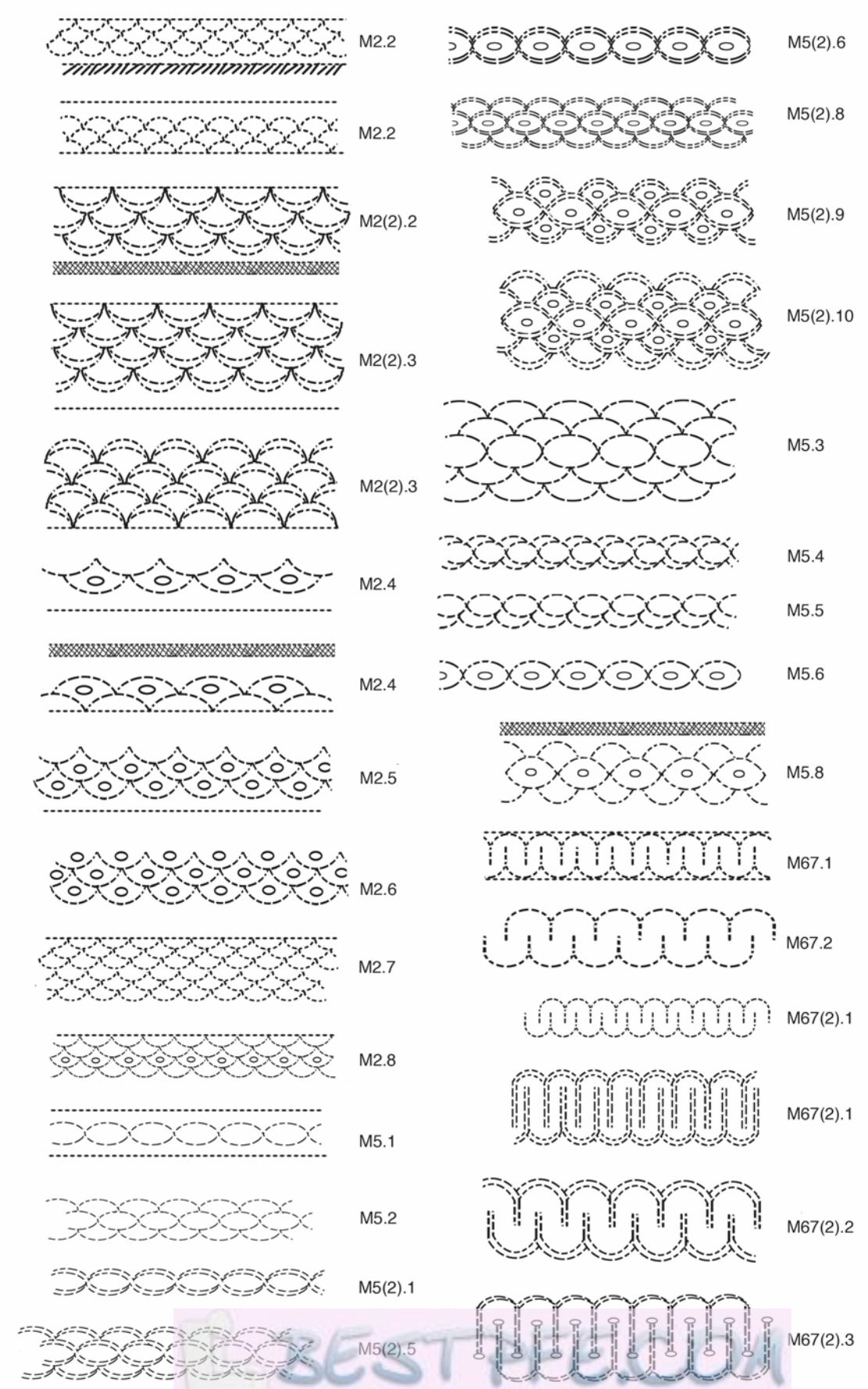
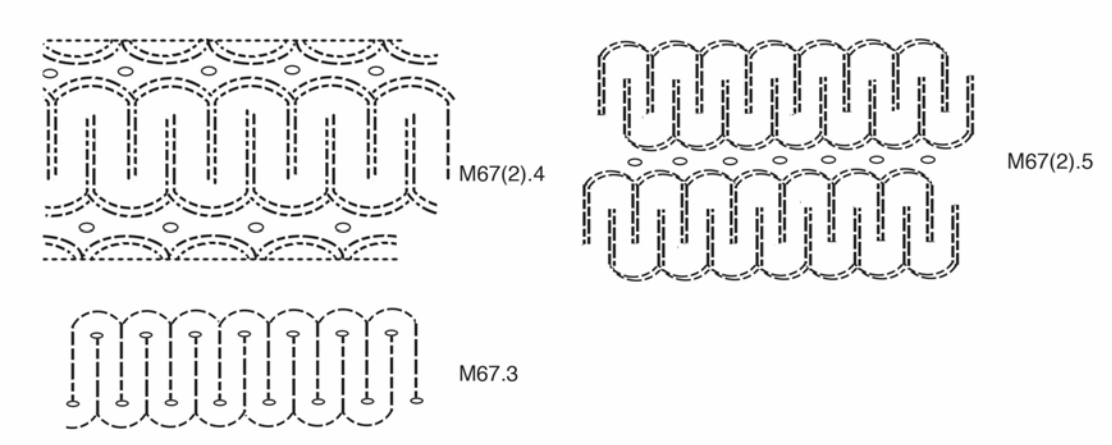


Figure 6b *continued*



The last set in Table 19 is the most complex, and looks at the *non-occurrence* of certain motifs in SE-RF-6, asking (based on the data from SE-SZ-8 and SE-RF-2), how many sherds might be expected in that sub-sample of a later date assigned to this site. This is to ascertain whether an inference of absence in the latter site is in all probability a reasonable deduction, versus whether the outcome might simply be one of absence due to sampling error.

Table 19 Motifs present in SE-SZ-8 and SE-RF-2, but not represented in SE-RF-6.

Motif	*SE-SZ-8*	*SE-RF-2*	*SE-RF-6*
M56	11 (1.81%) [11 (0.36%)]	13 (1.55%) [13 (0.33%)]	Expected – 4 sherds
M16	5 (0.82%) [5 (0.16%)]	6 (0.71%) [6 (0.15%)]	Expected – 1 to 2 sherds
M64	4 (0.66%) [4 (0.13%)]	7 (0.83%) [7 (0.18%)]	Expected – 1 to 2 sherds
M76	3 (0.49%) [3 (0.10%)]	5 (0.60%) [5 (0.13%)]	Expected – 1 sherd
M60	1 (0.16%) [1 (0.03%)]	12 (1.43%) [12 (0.31%)]	None expected or up to 2 sherds expected *

* Note: No potsherds bearing M60 would be expected if calculated solely on the basis of just a single occurrence of M60 in SE-SZ-8. Potsherds exhibiting M60 in SE-RF-6 could very well be expected, however, if the 12 in SE-RF-2 were the count basis for the prediction. Given that the 12 sherds bearing the M60 motif in this site are very likely an overestimation of their frequency, due to the targeted large sample origin, that raw number count could well be reduced to 5 or 6 M60 sherds for the purpose of estimating how many might be expected in SE-RF-6. Still, this reduction continues to suggest that at a minimum up to 2 sherds with M60 would be expected to occur in SE-RF-6, if not more. Because none do, its absence in SE-RF-6 is like that for the other motifs in Table 19, probably significant, and therefore they are not attributable to sampling error.

Figure 6c

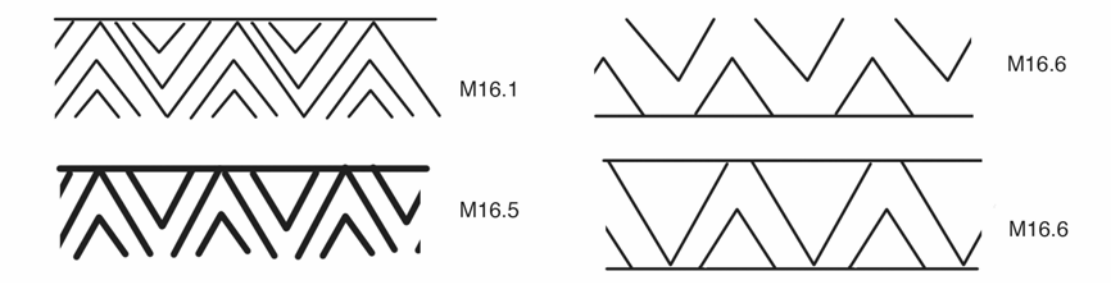


Figure 6c *continued*

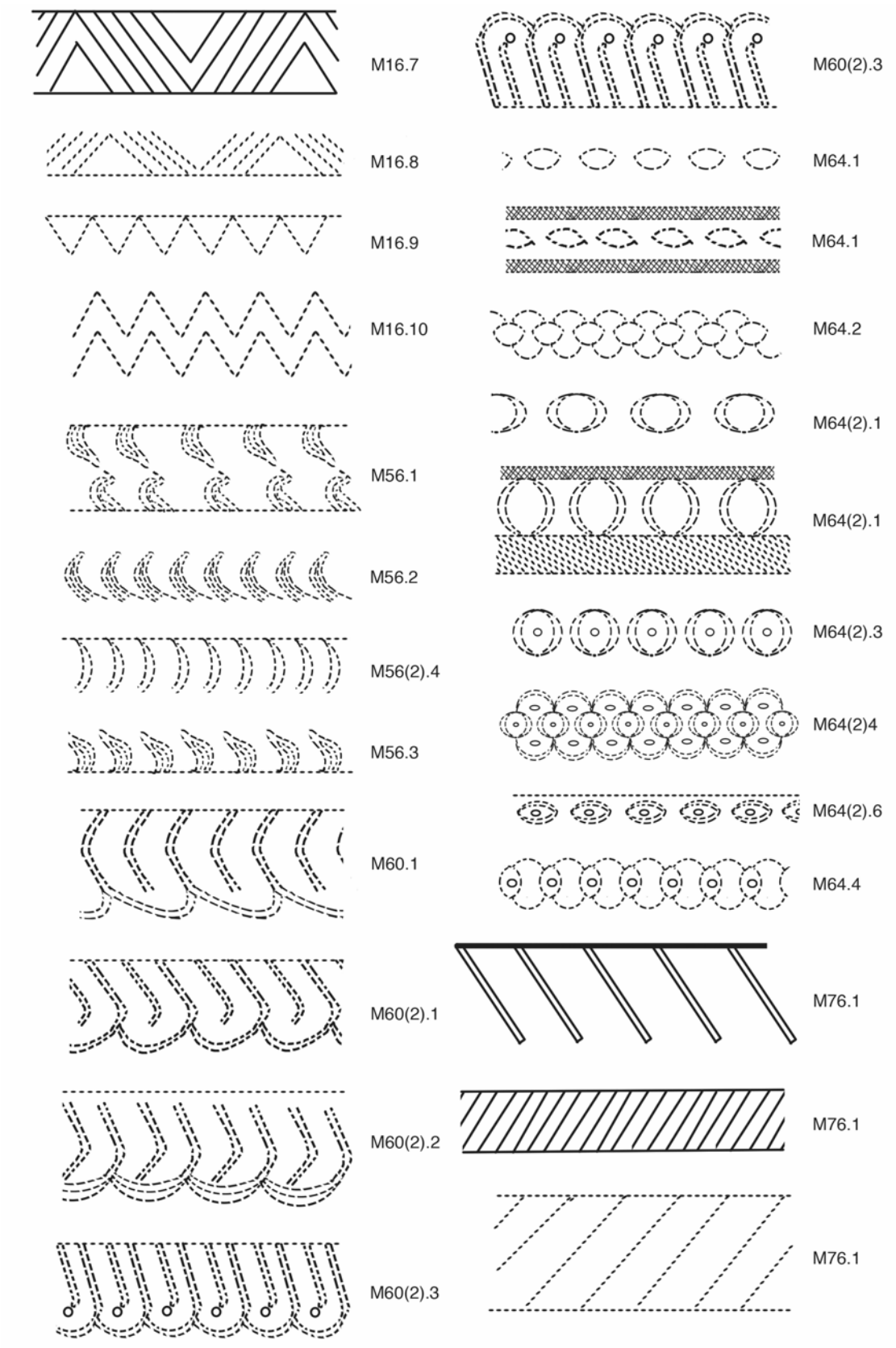
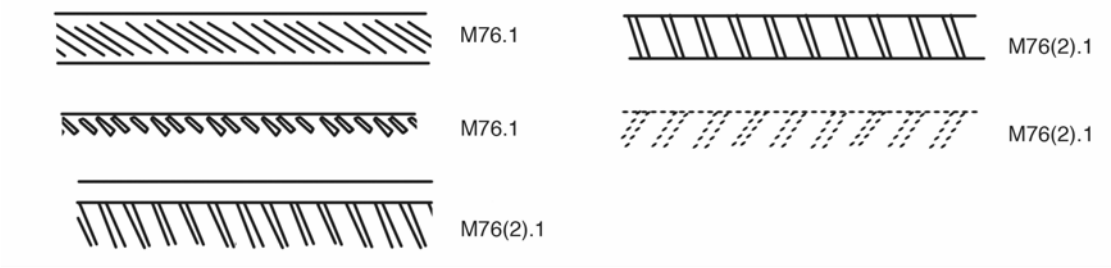


Figure 6c *continued*

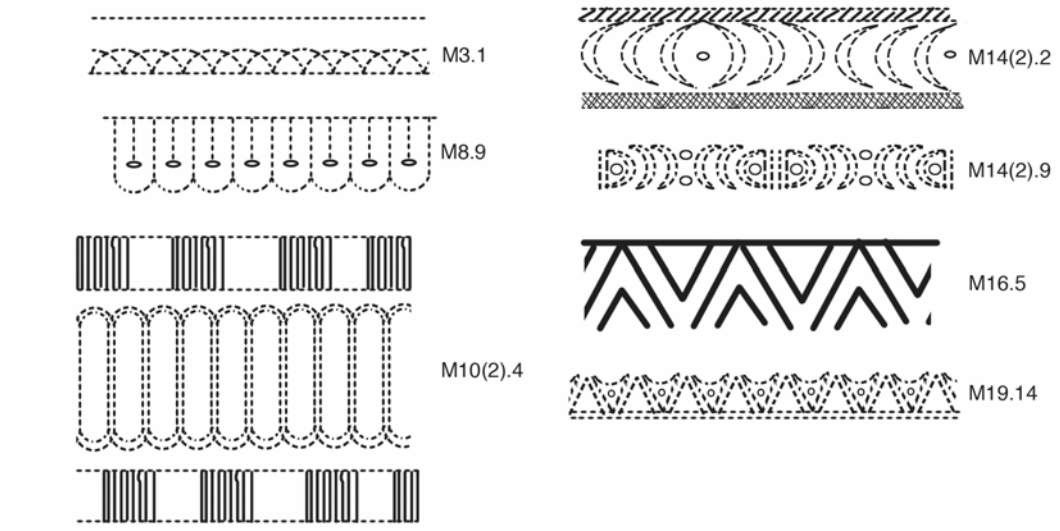


PART G: Motifs which have significance beyond that of establishing a serial order of change.

Some Lapita motifs do indeed provide information beyond that sought when undertaking a seriation analysis designed to reveal a possible age order. A fairly substantial number of these are found in the three Lapita sites under discussion and displayed in Figure 7. All have been previously illustrated (Green 1979: Fig.2.11). Their publication served to highlight which among the 100 motifs recorded from these sites (Table 7 & 8) appeared to be closely tied to those appearing in Lapita sites belonging to the Far Western or Central Western regional traditions of the Lapita Cultural Complex. These constitute certain motifs that in the three Reef/Santa Cruz sites are of low occurrence and exhibit little or no change over time within that region. Others display a dual function, in that they do indicate change over time, but also point, as do all of those motifs illustrated in Figure 7, to their probable historical origin in regions much farther west within the overall Lapita site distribution (Figure 2).

Figure 7 Motifs M1, M2, M3, M5, M6, M8, M10, M14, M16, M18, M19, M24 and M28 and some of their variant alloforms, all of them categorised as early widespread Lapita-style motifs (Figure 7a) and motifs M57, M58, M65, M73, M85 and M100 and their associated alloforms, all of them characterised as of Western Lapita origin (Figure 7b). Although most of these motifs appear only rarely in the Lapita-style decorated potsherd assemblages of the three Lapita sites in the Reef/Santa Cruz Island group, they are important indicators of a widely shared inheritance. M6, M8, M10, M14, M28, M30 in particular are demonstrative of a strong degree of consistent continuity obtaining over some 500 to 600 years within that conservative segment of the Lapita art style found on potsherds in the sites of the Outer Eastern Islands of the Solomons dating from circa 3150 BP to 2600 BP.

Figure 7a



Previous comparative studies of motifs across the known site assemblages associated with decorated potsherds in the Lapita style from West to East had long ago revealed at least 18 motifs judged to be early in age and widespread in their distribution. Of these 18, a sub-set of 9 motifs [M1, M2, M5, M16, M19, M24, M39, M67, & M77 – Figure 7a] listed in Part F above also register frequency changes through time in the Reef/Santa Cruz region indicating they are sensitive chronological indicators as well. However, they have added inter-regional significance in respect to their ties to the sites in the regions of Near Oceania to the west whose inhabitants first conceived the ideas those occupying the Reef/Santa Cruz region inherited, as well as ties to those early Lapita sites of Remote Oceania to the southeast into which some of the descendants of those populations next migrated.

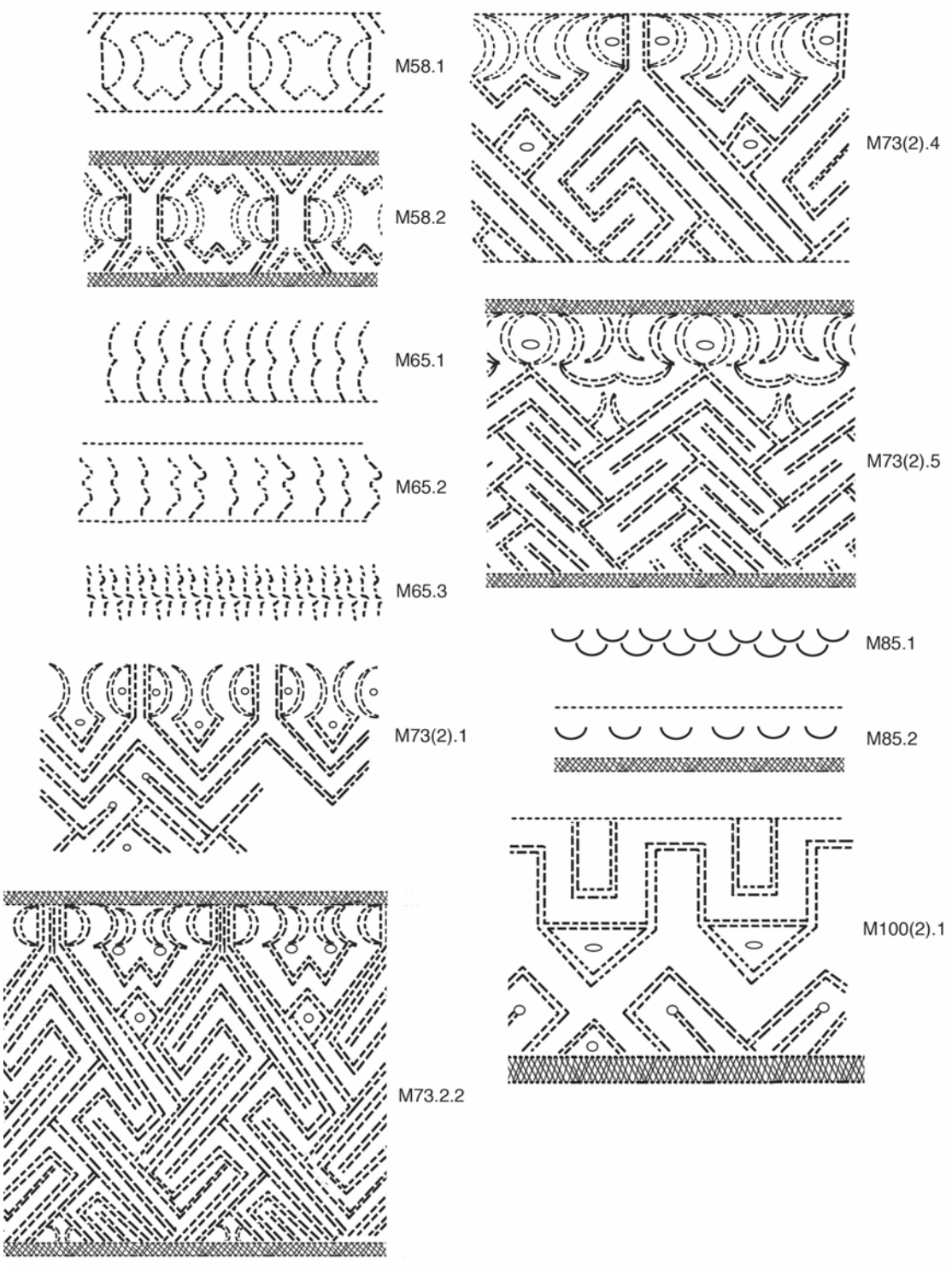
Seven of the 18 early, widespread motifs well known from early Lapita sites elsewhere [M3, M8, M10, M14, M28, M30 & M34] also occur all three of the analysed sites in the Reef/Santa Cruz area. M3 in Table 10 is a low incidence incised motif recorded for SE-SZ-8 and SE-RF-2, which if a Donovan (1973:90) drawing illustrating it is correct was also indicated as having been found in SE-RF-6. One potsherd bearing M34 (Table 9) was found only in SE-RF-6. The other five motifs [M8, M10, M14, M28 & M30] from Table 13 are those that are due to sample size overly represented in SE-RF-2. They are among the early widespread motifs that persisted without significant change to their frequency in this region until dentate stamped decoration finally ceased.

Only one other motif in this set of early widespread motifs, that of M18 discussed in Part D, stands out from the other 9 exhibiting frequency changes over time, because it instead reveals a persistent frequency without change during its occurrence in three sites spanning some 500 to 600 years. In that respect it belongs with the five in Table 13 just discussed.

Of considerable interest are those motifs of great complexity that were labelled in the 1979 illustration as Western. Today they would be called Central Western or Far Western, due to their restriction to the then known Lapita sites lying far to the west of those in the Reef/Santa Cruz Islands. This set consists of M47 and M58 (2 potsherds of each), M73 (3 potsherds) and M100 (4 potsherds) plus M65 which occurs on 10 potsherds (Figure 7b). As is fully documented in Part A all occur only in very low frequency except for M65, and *all* of them were recovered *solely* from SE-RF-2. As they do not occur at all in either SE-SZ-8 or SE-RF-6, their minimalist presence in SE-RF-2 can be confidently attributed to the far larger number of decorated sherds recovered during the excavations of that site. As a result a much wider range of motifs were recorded, described and illustrated from it than for the other two sites.

This illustrates the point that had the potsherd assemblages from the other two sites been a great deal larger numerically, comparable in the total number of decorated potsherds recovered to that from SE-RF-2, what could have been rare but informative data might have been forthcoming, providing otherwise rare data that decorated ceramic vessels of these Western types too were once present in these sites. In short, a few of these very rare motifs with strong connections to those from Lapita sites much farther to the west could easily have turned up on one or two sherds among much larger ceramic assemblages. This would especially hold for SE-SZ-8, the earliest of these three Lapita sites, in which a pot of an early Western type might well be anticipated, given such decorated vessels occur in SE-RF-2, a site with a similarly early age. Again their current absence is far more likely to be explained as due to the overall low frequency counts for decorated sherds that obtained in SE-SZ-8 and SE-RF-6, and should therefore at this juncture their absence should be attributed to sampling error, rather than one of true absence.

Figure 7b



In a contrary fashion, even small sample counts of decorated sherds, as in the site of SE-RF-6, do at times reveal significant data among their rarest motifs, when they occur in a site assemblage of later age, i.e. between 2800 and 2500 BP. Here Anson (2000:131 and Table 3) has identified two motifs in his code system – M271 and 448 – as occurring both in Watom and SE-RF-6. M271 occurs in the lowest layer 4 of the SDI locality dated to between 2800 and 2400 BP and M448 was among the sherds recovered by Father Meyer, believed to belong

to the early end of the Reber-Rakival sequence, and also in Layer C2 of the SAC locality now firmly dated to between 2800 and 2450 BP (Beavan *et al.* 2008: Fig 3). The dates from the earliest of the Reber-Rakival Lapita sites are three or more centuries later than those attributed to most Far Western assemblages, (Summerhayes 2004:149) and stem from a later phase of Lapita sites with a lower frequency of dentate-stamped potsherds. In overview commenting on the Reber Rakival Lapita site, Green and Anson (2000b:191) found this new evidence provides yet further support for continuing contacts between the Watom Island Duke of York region and that of the Outer Eastern Island region of the Solomons (Summerhayes 1996:257, 2000) centuries after the period when Remote Oceania was first colonised.

Further support for such an interpretation derives from the 4 potsherds bearing Motif M100. All derive from the much smaller number of sampling squares excavated at SE-RF-6. Thus their absence from SE-SZ-8 and particularly from SE-RF-2 may stand as an example of true absence as it is unlikely M100 was probably ever present in either of those sites, their true absence instead being due to their earlier age. Speaking chronologically, I would suggest this motif, given its presence in later Lapita sites to the west, yet only present in SE-RF-6, should be interpreted as a rare instance attesting to contact during a later time interval between the two regions. If so, this would constitute an example of much later design transfer, rather than of the more common ancestral inheritance proposition to explain how they come to have a later contact-based common origin.

CONCLUSION

Those wishing to employ ceramic information from the three sites with assemblages of decorated Lapita-style found in the Reef/Santa Cruz region, especially the data for the coded motifs found on them, have a responsibility to thoroughly understand the manner in which these data sets were assembled in the period from 1973 to 1983. This also applies to those who consider any use of this data as suspect for whatever reasons and therefore unreliable. Understanding requires treating each of the sub-samples which any researcher enters into their analyses in ways which accurately reflect the procedures used to collect the raw sample data. Only then will it become reasonably informative of the probable content, first of the limited sampling area of the site subjected to investigation, and second, to a lesser degree, the probable content of the entire site given the sub-sample positioning and extent of the actual excavations. In short these excavations were *not* the usual Flannery (1976) Mesoamerican intuitively-placed "telephone booth" type "grab samples." Neither were they plotted out as units within a site guided by unknown factors, nor were they determined solely by access factors such as where one was most able to dig a test pit, trench, or small grid-based unit of rectangular form. Rather they were systematically selected excavation units, randomly placed within a designated area within site SE-SZ-8 and SE-RF-6, and systematically placed units over an area of SE-RF-2 whose choice was controlled by the analytical distribution of the tiny sherds found on that site's surface. Because of these constraints each of the assemblages under analysis requires transformation when the raw data counts are employed in comparative exercises either between these three sites or between them and those from other regions. This is so whether the raw numerical counts are for the potsherds recovered, the coded Lapita-style motifs appearing on their surfaces, or are those for artefacts made in obsidian, chert or other materials and ecofacts.

The other considerations that need to be taken into account are those of taphonomy—that some call C and N transforms (Schiffer 1976, 1987). By employing two very recent commentaries on this data, both of which challenge the adequacy of the three Reef/Santa Cruz

excavation samples, it is possible to evaluate each of the sub-samples for each of the three sites. One of the challengers avers that the sub-samples from each of these sites are highly biased by the areas actually sampled; another avers the bias lies in the actual sample sizes themselves. The responses developed in this essay demonstrate the commentaries and criticisms of Best (2002) and Felgate (2003) are often wide of the mark and overstated. This is because they employ raw data scores without transformation when making their comparative evaluations. As a consequence, their analyses exclude the sampling procedures conducted in the assembly of the raw data. Thus they have failed to transform such data into a normalized form when they use that data to make their comparative statements.

Nevertheless, one of Felgate's manipulations of the data using normalizing methods contributes further to the coherence of the sample from each site, and the contexts in which it was recovered. These are issues of taphonomy, and Table 6 adroitly displays the breakage suffered by sherds as taphonomic processes alter the sherd counts, when the sherds move toward the surface (very largely through subsequent cultural practices) during the natural accumulation of additional overlying deposits. This, however, should warn those who use the raw sherd counts from the samples for each of these sites, and especially Best and Felgate or those who uncritically follow their example, not to attempt to draw unwarranted conclusions from some of that data, without also taking these natural post-occupation processes into account in their calculations.

That said, Best (2002) raises one important issue in asking how the areas actually excavated may have influenced the databases available for motif frequency used by most authors thereafter, claiming that it is a proportional function related to the area actually excavated. This has been done without reading the pertinent literature on this topic, curiously omitting even that specifically relating to the number of motifs found in other Lapita sites. Because answers to such queries have been inadequately addressed to date, one portion of this essay (covering Parts A to F) attempts to provide more informed explanations, if the question is, as it was, one of using motifs to determine chronological relationships. Some motifs do in fact provide better indications of temporal relationships than others, and a few are demonstrated to be influenced by size of area sampled. This outcome seems an important and rather better balanced assessment than a current situation of claim and counter-claim about whether the contents of the three sites exhibit a chronological order in their relative ages or not. Aspects of those contents do indeed reflect their chronological order as might well be expected, though 14C determinattions are necessary to date their age order more precisely.

Because the issue addressed was that of using motifs to determine chronology, other concerns are not investigated. For example, if variation in motifs is the concern, and the occurrence of rare motifs present in only very low frequency is an objective, the SE-RF-2 site alone, and then only an analysis based on the full 153.5m² sample, is likely to suffice. An informative case for this statement has been set out in Part G. However, if the aim is chronology, it is possible to identify the motifs present in very low frequencies in only one site, or those of similarly low frequencies in two or three, and then omit them from any chronological analysis, because one can not overcome the issues of sampling error raised by the low frequencies of occurrence.

What is proportional to the area investigated in these three sites is precisely what is predicted from the existing literature. There are number of unique motifs found in only one site, and the greatest number is in the site with the largest area actually sampled. On that point Best is correct, although it is this essay that makes this case in convincing detail. Second, he is correct that certain motifs of very low frequency in sites SE-SZ-8 and SE-RF-6, prove to be very much over-represented in SE-RF-2 (in frequency terms), if frequency counts rather than

simple presence/absence counts are used. Again the claim of over-representation does not apply to all motifs, and this essay clarifies that point by pinpointing the particular motifs involved. In the first instance of frequency values, the effect could be large, even after normalizing them to a common base so that all three sites are correctly compared. In the second instance of simple presence/absence, such an effect is likely to be less.

A far more important claim, that has yet again found support here, is that for continuity. There are indeed commonly occurring motifs that reinforce Donovan's (1973:36, 43) original observation of marked decorative similarities between the sites due to common origin within the same decorative style tradition, along with indications of strong regionalism. Moreover, both Green (1978) and Anson (1983) demonstrated similar outcomes to those of Donovan, although they employed additional numerical motif data and other methods. The observation of common origin within the same decorative style tradition has received renewed emphasis most recently by Felgate (2003:82). However, he counters with the reminder that the sites may be "similar because they were of similar or overlapping age and that the differences arose for reasons other than chronology." On the basis of this essay and its various analyses one can now point to a substantial set of motifs that do in fact differ for other reasons (Part G), and *also* to some motifs whose occurrence appears unaffected by chronological considerations. Moreover, within a given range there are certainly a few motifs whose variation can be detected, yet the variation turns out to be quite within the lines of what might be expected, if one properly uses the sampling protocols and frequency numbers involved.

Still, if the targets are differences in motif occurrence, reflecting significant changes of frequency over time, then the analyses undertaken do point to those motifs most likely to furnish this information. And, significantly, the trends they exhibit conform – not totally but in very large part – to expectations suggested by an independent means: 14C dating of these sites and their chronological order. This order Best (2002) has rejected for quite another solution, although his proposal has proven to one that encounters even greater difficulties of chronology and site order.

This essay ends with a few comments on the subject of chronology using 14C dates. There is a published article by Jones *et al.* (2007) which employs four charcoal and two shell 14C determinations to provide a secular age for the SE-RF-2 site. It takes the form of a Bayesian analysis, in which it is argued as a prior that the site's duration was no more than 50 years on archaeological evidence, and probably of only some 25 years in reality. Any time slice over 50 years between 2825-2983 and 2949-3145 BP would adequately cover the most likely possibilities within confidence intervals set at 68%, though at 95% 2724-3062 and 2878-3271 could also be argued, if one adopts an even more conservative position.

In contrast, 3 charcoal samples from the one end of site SE-RF-6 that was sampled through excavation places that end portion of the site in the interval 2579-2805 and 2345-2661 BP. This age range serves as a useful approximation at 68% of the interval within which that portion of this site's occupation might be placed, given that (on the archaeological evidence) the habitation activity at that site probably spanned no more than 100 years. With an even higher degree of confidence, the samples from SE-RF-6 date to some restricted interval toward the centroid of a 95% age range of between 2079-2744 and 2466-2912 BP. The article by Green and Jones (2008), setting this out in full with maps, stratigraphic cross-sections and excavation plans, is now available in printed form. However, the conclusion from the Bayesian analysis confirms what is evident from inspection, that the short duration archaeological record of SE-RF-2 can confidently, on a very sound statistical appraisal, be deemed to pre-date that of SE-RF-6, and to begin sometime in the interval of 2878-3271 BP.

It is then followed by a possibly rather longer occupation at SE-RF-6 beginning sometime in the interval 2466-2912, but after SE-RF-2.

Best (2002:81-86) used his techniques of ceramic analysis to place SE-RF-6 as the earliest site, a complete reversal of site order that even Felgate (2003:84) judged unlikely. And indeed, as Felgate anticipated, once these collections have undergone further assessment of the kind conducted here, it too reaches conclusions opposite to those Best (2002) advocated. Furthermore, Scarlett Chiu has now initiated a re-investigation employing a completely revised and newly informed method of decorative and vessel shape analysis. This full scale re-examination is, of course, taking place some 25 years after the initial pioneering period of such endeavours, and is overdue because our methods and protocols for analyzing Lapita decoration are radically changing (Chiu and Sand 2005). Our understanding of that symbolic system has vastly improved with more recent discoveries and the advent of new forms of analysis. That can only improve things in all comparative studies of decoration on Lapita pottery, and those efforts do indeed need to reach new levels of sophistication.

Lastly, there are now a total of four shell dates from SE-SZ-8, from shell carefully chosen for independence of type and position in the site. The article by Jones *et al*. (2007) computes a ΔR that may be applied to all dates in the 3000 years BP age range, calculated from the six dates of SE-RF-2 applicable to the Reef/Santa Cruz region. These have now been applied to the calibration of each of four shell CRA 14C determinations for SE-SZ-8, in an article by Green, Sheppard and Jones (2008). By a small and thus arguable margin, their calibration reveals it is probably the earliest in age of the three sites. A constraining prior to the age for that site is that no date in the calculated age range should extend much earlier than 3150 BP, given the reasonably well-attested view that Lapita decorative assemblages far to the west from which it derives appear in much of Near Oceania around 3200 BP. In an approximation using Bayesian methods, a calibrated date range lies between 3700 and 3000 BP, the probability graph of successive ages indicates a region of support that is normally distributed around 3200 BP and therefore it should be of a slightly younger age . In addition, reconsideration of the archaeological evidence by Green outlined in that article would now reduce an original 300 years duration for the SE-SZ-8 site by half or more, to a prior of 100 to 150 years of habitation activity at most. Although any kind of chronological age estimation overlaps with the SE-RF-2 date range described above, the probability of a slightly older age for SE-SZ-8 appears no longer out of the question. This independent age assessment is therefore entirely compatible with the current and previous analyses of both the motif ceramic data sets assembled in 1973 and those arrived at in this essay. All of these endeavours certainly improve on the age assessments found in Green (1991). It also demonstrates that these sites and their contents can continue to play their part in understanding the foundational portion of the dentate-decorated part of the Lapita horizon when it first appeared in Remote Oceania.

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APPENDIX I

Definitions of terminology referring to aspects of the Lapita Phenomenon in Near and Remote Oceania

The topic of Lapita has become increasingly alluring over the half century since connections among the examples of an art style were recognised on potsherds decorated in a distinctive technique now called 'dentate-stamped', and their potential interpretive promise articulated in the published literature concerned with new ways of interpreting Pacific archaeology. Yet definition of this collection of phenomena has also become ever more elusive as the information base from excavated sites of this period has expanded into an ever-growing corpus. Still, for a majority of Pacific cultural historians, a Lapita phenomenon of some distinction currently serves to attest a post 3500 BP colonisation of the Southwest Pacific by populations speaking Oceanic dialects of the Austronesian family of languages.

Pacific archaeologists, myself among them, have tried to practice a measure of transparency in the terminology they use to communicate aspects of this Lapita phenomenon (Green 1992). Other writers have simply opted for variable degrees of opacity, leaving the context in which the word Lapita occurs as noun or adjective to indicate a probable or intended meaning. In the course of their deliberations, most in the latter group express serious doubts as to whether any attempt at precise terminological transparency actually works. Does it really enhance the interpretations of the plentiful information and theories relating to this topic or the deductions derived from the content of sites displaying pots or potsherds reasonably designated as one or more types of pottery that constitute the Lapita ceramic series?

An anonymous referee for this paper identified the conflicts between the two opposing strategies:

> My concern is the use of 'Lapita" as an adjective and as a noun. I counted at least 13 different contexts in which the term appears: "Lapita", "Lapita sites", "Lapita archaeology", "Lapita phenomenon", "Lapita pottery", "Lapita decoration", "known Lapita techniques", "Lapita burials", "Lapita-style" , "Lapita potsherds", "associated with Lapita", and "Lapita-based model", and "Lapita archaeologists". Other authors have noted the confusing use of the term "Lapita" – and have added to this confusion themselves. The present paper needs a simple statement regarding what the author means by Lapita. This does not have to be an extended discussion, but a short statement.

I still hold the view that it is no more feasible now, than it ever has been, to make any kind of short and simple statement on what constitutes Lapita, at least not one that might prove acceptable to a majority of other scholars who conduct research on this subject. Yes, it would indeed be practicable to write an entire essay or even a whole book on what might be meant by Lapita in its various guises, drawing on my own publications and those of many others writing on the associated topics. However, long experience in publishing on the subjects and issues covered by the term Lapita, and offering various terminological distinctions one might follow to disentangle its components, has taught me that such attempts are often to little avail. Rather the chances of achieving even a modicum of consensus about any coherent conceptual framework within which to analyze the Lapita phenomenon has instead in recent years become vanishing small. The conflicts over what are appropriate terminological distinctions,

and the degree of confusion contributed by other authors, noted by the cited referee, are all too real. Thus what follows can be only a guide to the terms used in this essay for Lapita. These concepts are comparable to those employed elsewhere by myself and other scholars who have demonstrated the utility of such terms when writing about the Lapita phenomena. To assist the discussion, I offer appropriate references to the more general literature on various, sometimes confusing, definitions of Lapita where certain terminological distinctions are explored in far greater detail should a reader be interested in the ramifications of any specific concept.

The current baseline synthesis for most non-specialists delving into this topic is a book by Kirch, *The Lapita Peoples: Ancestors of the Oceanic World*, in which he deliberately down plays "the usual barriers erected by academic culture with its theoretical and paradigmatic posturing" (1997:xxiii). The book opens with the proposition that:

> "A majority of prehistorians and historical linguists have now come to regard Lapita as the archaeological manifestation of those peoples who spoke Proto Oceanic and its immediate daughter languages. Lapita – which spans a time period between about 3600 and 2000 years ago, and is distributed in space from the Bismarcks to New Caledonia and eastwards to Samoa and Tonga – has come to be recognized as the ancestral cultural stock from which the modern diversity of Oceanic-speaking peoples and cultures ultimately arose" (Kirch 1997:xx-xxi).

This is a general viewpoint with which I am quite comfortable. However, it seems to me any detailed academic research into more precise terminology to dissect even a very general concept such the **Lapita phenomenon** requires a formal terminological apparatus, conceptual framework and set of explicit definitions, if progress is to be made not only in assessing its internal components and their structure, but more importantly in their interpretation.

Terminology found unhelpful

Maintaining continuity with the older literature is a common use of a formulation often employed by Childe (1956:16,33-34) and other European prehistorians when writing about those "archaeological cultures" they identified as significant in the pre-Roman history of that part of the world. Early in Pacific archaeology this imported European nomenclature resulted in the coining of "**a Lapita culture"** (Bellwood 1978:244, 1987, 2005:137), a usage that has continues to appear in Bellwood's writings and those of many others given to that interpretation of the available information. There is also the (never widely adopted) use by Rouse (1986) of a similar concept called **"Lapitan culture"**, consistent with his attempt to formulate a suitable high level integrative device when faced with his need for term referring to the relationship of any set of discrete units evidencing some form of historical contact (Green 1992:10). A false claim by Terrell (1989:625) that in Melanesia "Lapita, to repeat, was a trade ware" comes to mind as an example espousing this form of integrative definition, when – for the most part – it was the ideas and not the pots that moved Summerhayes (2000). *It is in fact necessary to contrast, rather than align the pots and their decoration, to other associated non-ceramic hard goods found in a great many Lapita sites that firmly demonstrate items which continue to characterise aspects of the node and network exchange systems typically conducted by Lapita peoples.* Finally, for either the contemporaneous or more often derivative and closely related plain ware assemblages lacking dentate-stamped potsherds, though they otherwise display affinities with the kinds of pottery that make up the Lapita ceramic series, a few researchers at times have employed a general label –

"Lapitoid". Earlier, that term had been deemed to cover some of the non-standard kinds of variation in Lapita ceramics (Kirch 1988:161) and this usage became especially notable in writing about in the later ceramic site assemblages of Fiji, Tonga and Samoa. Initially, it had led Bellwood (1975:12-15) to assign those 'not quite Lapita' potsherd-bearing assemblages to what he called a **Lapitoid ceramic series** whose makers were ancestors of those who colonized all of Polynesia. Then, far to the west within the Bismarck Archipelago, the Area A – a dry beach ridge locality belonging to site ECA in the Mussau group - had its plainware assemblage initially interpreted as comprising simplified later **Lapitoid** ceramics dating to the first millennium BC (Kirch 1987:172). Subsequently, after sufficient 14C dating revealed they should be reinterpreted as part of one large village and contemporary with a zone of aligned stilt-house structures paralleling the beach terrace some 10 to 50m offshore, that term was abandoned. The latter assemblage was associated with plainware vessels but also, to a far greater degree, with many highly decorated dentate-stamped vessels of several shapes exhibiting classic designs belonging to an early period within the Lapita art style (Kirch 1997:172). It would seem any attempt to use the concept of **Lapitoid** ceramics analytically is bound to incur unresolved issues and therefore should be avoided.

For the most part I tend not to employ any of these blanket cover terms as analytic concepts. They seem far too fuzzy or problematic devices. Instead, claims for "**a Lapita culture"** at present possessing a reasonable degree of legitimacy is in my view best limited in application to the region of Remote Oceania and a short interval of several centuries between 3150 BP and 2800 BP at most (Green 2003:112-113). It was during this interval alone when the pioneer human colonizers of this previously uninhabited region very rapidly expanded throughout those island groups leaving their archaeological traces in site assemblages containing not only the full range of potsherds belonging to the Lapita ceramic series, but also a rich non-ceramic component of durable ecofacts and artefacts that greatly assist in defining its cultural content (Green 2003:109-111 & Tables 4, 5 & 6). In short, pioneers not only carry their culture with them (Golson 1971:75), they also conform to a particular physical makeup that during that time period constitutes a paleo-Oceanic population able to be associated with distinctive molecular and anthropometric biological evidence (Green 2003:111-112). The communities of pioneers also spoke a fairly homogeneous late stage of numerous Oceanic Austronesian dialects (Green 2003:97). These last are currently subsumed under two primary subgroups of Proto Oceanic – 'Proto Temotu', 'Proto Central Pacific', plus a cluster of languages that do not conform to the usual innovation-based definition of a subgroup, resulting in their designation under a 'Southern Oceanic Linkage' instead (Ross *et al.* 2008:7-10 & Fig.8).

The warrant for Lapita phenomenon and Lapita peoples as the general descriptors

Given that a broad cover term such as **"Lapita phenomenon"** is one way of expressing Golson's (1961:176) interpretation of Lapita as an early and widely spread "community of culture" in the Southwest Pacific, it would seem the 'phenomenon' designation may well serve that sense in a fairly neutral vein. Put another way, it is a useful way of referring to an umbrella category of reasonably cohesive and coherent information seeking additional and more discerning interpretation. Evidently the concept holds a presumptive degree of commonality in the minds of a diverse range of researchers concerned with this topic. Within that elastic category, it is then becomes possible to turn to the **"Cultural Complex"** for a more acceptable kind of terminological discrimination, one which allows investigators seeking additional precision to abstain from an often unwarranted notion of a "Lapita culture"

or the all embracing convention of “Lapita” on its own as a noun capable of definition (Terrell 1989).

Facing this kind of conundrum at the Neolithic level of culture in their general book on how to frame modern archaeological concepts, Renfrew and Bahn (1991:157) offered as one possible term – “a tribe” – because at one time in anthropology this implied a larger grouping of smaller units that carried with it the assumption that the communities involved shared a common ethic identity and a degree of self awareness. However, as they acknowledge, closer analyses of the tribal concept revealed that this ethnographic outcome was infrequently the case. Rather, while these relatively small and autonomous groups did sometimes join together with others, or maintain contact with others acknowledging the notion of shared ancestry to form a larger admissible ethnic unit or "tribe", in a greater number of instances this had not really taken place. Any adoption of the term into archaeology was therefore open to the same criticisms that had established it as flawed elsewhere in the anthropological literature.

While the general discussion by Renfrew and Bahn of the designation of a “tribe”, and its unsatisfactory nature is helpful, their suggestion of an alternative deemed a “segmentary society” appears hardly more cogent should one wish to attribute congruence with some particular social formation to the Lapita Cultural Complex. As a consequence, it was argued that observations other than those explored by Renfrew and Bahn seem necessary when describing the Lapita phenomenon of the Southwest Pacific (Green 2002:31). Neither 'tribe' nor 'segmentary society' appear compatible with what is presently known about or claimed for known forms of societal organisation that current Pacific researchers would identify as suitable socio-political descriptors for the Lapita Cultural Complex. Few would interpret that complex as representing either a tribe or involving some other unified large-scale ethnic unit rather than a number of them, hence the use of the term preferred by (Kirch 1997:18), **Lapita peoples**. Moreover, among the some time useful higher-level socio-political nomenclatures, none of the common terms of ‘ancient civilization, emergent nation, archaic state’ or others of this kind sound even remotely appropriate, as Terrell (1999:55) has rightly insisted. Nor, at a more specific level, does the Sapir definition of a cultural complex really prove acceptable as a plausible alternative (Best 2002:99; Green 2003:109), when its advocates contend that 'complex' represents no more than a major social practice such as a cult complex, dance complex or a complex involving social and ritual events of a ceremonial nature (Terrell 2003:76; see also Terrell and Welsch 1997).

The preference for a Lapita Cultural Complex and Lapita Ceramic Series

Instead, my preference has always been to view **a cultural complex** as the archaeologically demonstrable core of a culture, i.e. those surviving and recovered items usually considered diagnostic or the common denominator of all assemblages in a class (Rouse 1972:87; Green 1992:10, 2003:109). Rouse (1972:87) long ago saw style as one potential synonym, that component often being exemplified by a particular style of pottery or an identifiable architectural style that characterised the built environment. In the Lapita case this core initially clustered around the ceramics decorated in Lapita art style. With this defined core, a number of investigators, myself among them, then sought to further round out its definable cultural content to include other aspects of the material culture, through their repeated association with surviving ecofacts and artefacts of the same age. This process took some decades to achieve and depended on an increase in the number of excavations on Lapita sites in both Near and Remote Oceania. By the turn of this century it proved possible (Green 2003: Tables 4, 5, & 6) to present a robust listing of items forming a **Lapita Cultural Complex** that could be broken down into 3 categories – (a) integrated elements, (b) core components

demonstrated archaeologically, and (c) those very likely to be associated, but without definitive archaeological proof as yet. For the last category a triangulation approach was employed to demonstrate the probable presence of non-durable items that would also be associated with the hard physical evidence recovered though excavation. By 2002, these three categories formed the durable ceramic component *plus* the continually expanding non-ceramic core, divided into the hard core and inferred parts, of a very much-enlarged cultural complex.

A final step has proven more controversial, and only a few researchers from among the various sub-disciplines within Pacific anthropology have accepted and adopted it. The methodological strategy leads to the practice of a holistic anthropology of history. In my case, this methodology was in large part conceived and practiced in joint essays with Andrew Pawley (Green and Pawley 1999) and with Patrick Kirch as well as discussions with some biological anthropologists. To formalize its main methods Kirch and Green (2001:1-91), in Part I of their 'Essay in Historical Anthropology' when it was applied to Ancestral Polynesia, sought to attach to particular cultural complexes or cores, the relevant historical, linguistic and biological dimensions and entities which research into those domains revealed as fully applicable. Kirch (1997) had done this to the extent possible for *The Lapita Peoples* without foregrounding in that general book-length overview any of the more academic aspects of a theoretical and paradigmatic apparatus within which that summary account was conceived. Still, the overall aim in both cases is achievable only through a process of step-by-step construction, each attempting to flesh out something nearer to the full sense of an earlier culture, one no longer available to direct ethnographic observation. Slowly its application to the Lapita Cultural Complex continues to yield insightful results, some of which have opened up new issues and revealed yet other problems needing resolution.

Some diagnostic element or related elements usually lie at the core of those recoverable components that make up a cultural complex and are therefore normally placed to the fore in its definition (Rouse 1972). In archaeology these related elements often focus on a typologically defined category of pottery that plays this central role as noted above. Within the **Lapita Cultural Complex** it is the **'Lapita Ceramic Series'** that for decades has performed this important task; the utility of the general concept of a 'ceramic series' as it was defined by Rouse (1960) was first introduced by Golson (1971:75) as likely to be fruitful in the study of the wide range of decorated and undecorated potsherds found in what were judged to be Lapita sites at that time. In doing so, Golson drew on the successful use of these concepts by Rouse and Cruxent (1958:22-25) when dealing archaeologically with related pottery traditions within the Caribbean region of Latin America (Rouse and Cruxent 1963). Kirch provides an excellent summation of the theoretical origins of these two concepts with respect to their application to the **Lapita phenomenon** (Kirch 1988:4-7; 1997:119-124 & 294, fn. 9)**,** including a recent description of the known content of the **Lapita Ceramic Series,** so there is no need here to further rehearse the details. What needs emphasis is that, although many researchers use the concepts of a Lapita Cultural Complex and a Lapita Ceramic Series, various critics of these terms most certainly do not. For example, dismissal of the **Lapita Cultural Complex** as having become a scholar's white elephant is representative of a negative kind of response to the first term (Terrell 1990: 827; 1999:54; and see Terrell (2009) counting the number of times – 53 – that the term was used in a recent collection of essays on Lapita), while that of a **Lapita Ceramic Series** has recently been demoted to include only site assemblages with dentate-stamped ceramics employing the Lapita art style of decoration.

Not surprisingly, this emerging group of researchers may be accused of taking a narrow dentate-centric stance (Summerhayes 2001:54; Green 2003:104) when they restrict the term Lapita to dentate-stamped potsherds and the vessels from which they derive (cf. Anderson *et al*. 2001; Anderson 2001; Spriggs 2001:241; Best 2002, plus many other writers on this topic). The consequence is that only those site localities with ceramic assemblages containing one to many dentate-stamped potsherds are counted as "Lapita" in their lists of sites, frequently subjected to analysis by geographic region (Bedford and Sand 2007); all other sites with related kinds of pottery are viewed as something else, and typically referred to as constituting "post-Lapita" assemblages. In numerous instances these are given other phase names (Bedford 2003, 2006a: 157-192 & Fig.8.16; Bedford and Spriggs 2002; Green 2003:107 & Fig.4 for the sequences applying to the Central and Southern Vanuatu region and to those of New Caledonia and Loyalty Group). Each such regional sequence begins with a brief Lapita Phase, containing abundant dentate-stamped pottery, which is viewed as initiating the colonising process for that region. Thereafter other named phases derive from a Lapita phase at the base, but are not designated as representing Lapita *stricto sensu* because the manufacture of pottery vessels decorated with dentate-stamped designs that had initially declined in frequency, by then had ceased altogether within a given region. Thus a sole change in one kind of pottery decoration with the Lapita Ceramic Series signaled the need for another phase and different name for it, when continuity in nearly all other aspects of the recoverable material culture really did not warrant total removal from a broader conception of the **Lapita Cultural Complex**.

However, other kinds of ceramic vessels – with and without decoration – were also there from the beginning, and thus equally a part of the full **Lapita Ceramic Series.** Moreover, the rest of the series (whose manufacture was otherwise continuous) was ongoing even as the dominating motif designs executed in the dentate-stamped technique disappeared from certain pottery vessels. Explicit recognition of these two contentions are provided by the recent summations of Bedford and Sand (2007:8): "We are well aware that the Lapita ceramic series includes decorative techniques such as appliqué, incision, excising and shell impressing along with an often substantial component of plain pots"…. and "Moreover, all non-dentate forms of decorative techniques and the plainware vessels are also found after the dentate phase".

Lapita Sites and the Concept of a Lapita Ceramic Series

Given these two very different methods employed in identifying what is to be counted as a **Lapita site**, there are inevitably two quite separate meanings when different writers on the subject use the term**.** One is that just discussed, a site within the Southwest Pacific zone of Lapita distribution containing numerous potsherds, one or more of which are decorated with dentate-stamped designs in the Lapita art style. The other is that of any site within the Southwest Pacific zone of Lapita distribution, which on a variety of criteria one assigns to the **Lapita Cultural Complex** because it contains potsherds however decorated, or even plain wares, all of which are seen as forming one or more pottery types belonging to the **Lapita Ceramic Series.** This second option even allows odd examples from non-ceramic bearing sites to be designated as "Lapita sites without pots" (cf. Spriggs 1991:37-38), provided that they contain other suitable ecofacts and artefacts typically found in Lapita sites despite an *absence* of potsherds belonging to the Lapita ceramic series. The proviso is that the dates for the assemblage in question falls somewhere within the timeframe – 3500 to 2000 BP – covered by the Lapita Cultural Complex. In this essay it is this second meaning that I have chosen to employ, despite it recently having become a minority practice among many of those writing on the topic. In short, while those sites with decorated and plainware pots are, in

agreement with the alternative group of researchers, quite obviously **Lapita sites** (and often contemporaneous), so also are those sites associated with potsherd assemblages bearing minimal non-dentate stamped decoration or even just plainware vessels with little or no decoration, for example the Area A in the Mussau Lapita site of ECA discussed above. In this definition of a Lapita site, those with very limited non-dentate decoration on a few potsherds (and often only sections of vessel rims), most definitely would qualify as part of the **Lapita Ceramic Series**, just as plain ware pots, usually interpreted as utilitarian vessels, would form a part of that series. Consequently, some **Lapita sites** contain potsherd assemblages that may be almost or even entirely made up of plainware potsherds. Hence one commonly applied description of them is: 'Assemblages from Lapita sites representative of a later plainware stage or period within a regional tradition' often shortened to **Plainware phase [or stage] Lapita sites** as distinct from **Dentate phase Lapita sites.** In sequential chronological order within any given region these two phases constitute a **Regional Lapita Tradition,** one with a temporal age range often spanning a millennium or even longer. Another equally valid option then is to see some of these plainware phase sites as contemporary locations displaying the outcomes of involvement in quite different activities and social and economic arrangements.

The Lapita art style and design system

The **Lapita art style** is most frequently identified by two observations about any given ceramic assemblage from the Southwest Pacific: 1) the application on the potsherds of one or more of the **Lapita decorative techniques** and 2) the employment of a **Lapita design system** when potsherds carry decoration**.** The decorative techniques span a range that starts with dentate-stamping and very fine-line linear incising, and continues with the less prominent techniques of applied raised relief forms, shell impression, punctuation, and cutting through or cutting away the surfaces between the various decorative motifs (see also quote from Bedford and Sand 2007 above). The range is now also known to include surface painting, with the use of only particular colours as slips, much of which has been either deliberately removed in the course of the involvement of these ceramics in ritual practice, or subsequently washed away through taphonomic processes. For instance, when recovered today, an overall white wash is often preserved as a white lime infilling in the dentate-stamp impressions of those decorated potsherds, while one kind of red slip endures on a number of potsherds yet not on others from the same vessels. Still other colours remain only under certain rather rare and highly stable conditions, or as relatively minor traces on just a few sherds (Bedford 2006b).

During investigations it is important to record which among the motif designs occur only in a dentate-stamped technique, which in either a dentate or lineal incised form, and which are observed exclusively in a continuous fine-lined incised form. On the evidence available now, only restricted ranges of Lapita motifs recur among those created by means of continuous fine-line incision**.** The primary reason for recording these technique distinctions is their potential to offer original patterns for the elaborated set of dentate-stamped designs that became the dominant fashion for a short interval of three or so centuries. The repertoire of motifs has parallels in the lineal tattooing of human skin with similar patterns accomplished by the use of obsidian flakes and recorded ethnographically in Near Oceania; archaeological support is provided by the similar flakes and tiny graver-like projections (Green 2003: Item 6c in Table 5) that are among the hard core of non-ceramic items associated with the distinctive ceramic series at the centre of the Lapita Cultural Complex. Like the toothed turtle scute tools for dentate stamping (Ambrose 2007:220; Sand *et al*. 2007:276, Green 2003: Item 6b in Table 5), in numerous cases the obsidian may have functioned as an incising tool in the tattooing of

the human skin and in making the very fine-lined incisions on pot surfaces. As Ambrose (2007:216) points out, the turtle scute combs used to produce dentate stamped motifs do not have pin-prick points as is usual among Polynesian tattooing implements; instead their points are filed away and their edges flattened, and in that form they could not have served to also carry out those aspects of the tattooing process. The illustrations (Figures 4 to 7 of Parts A to G) in this essay display this asymmetric relationship between the two techniques, as did the studies of the early ceramic collections from New Caledonia; Frimigacci (1974:38-70 & Table 11-Category E, 1977) eventually called these less well known lineal incision techniques **Lapita géométrique.** In sites of that island group this technique appears on approximately one in five of its potsherds. A single incised set of three, only slightly different, panel designs typify the main forms employed in the central band, together with only two other rare categories of incised paneling (Sand 2007: 280). Given that dentate stamped designs frequently had their boundaries carefully outlined in linear fashion (through a serial application of straight dentate stamp tools to establish a draft line before the spaces between them were in-filled by a narrow range of curved stamps or other hollowed sectioned or pointed bone tools (Ambrose 2007: 215-216. Green 2003: Item 6a in Table 5), it would appear that the fine-line incision could also have served this same kind function. From this base, a unique Lapita innovation of the turtle scute dentate tool in both straight and bent forms made it feasible to develop the short-lived elaborations – evident in the more complex dentate designs on classic types of Lapita pottery – from the simpler incised designs. In short, as Ambrose (2007:216, 220) argues, it was the innovation of this particular Lapita form of dentate-stamping that laid the basis for its elaboration, and not that of rouletting as proposed by Best (2002:50). The latter technique can be dismissed as not evidenced when the designs and motifs are examined in sufficient detail to reveal their true technological origin (Ambrose 2007; Sand 2007:267-268).

Research into an already well-defined Lapita design system as described in some detail by Kirch (1997: 124-132) has further blossomed in the decade since that publication. Investigative developments can be followed in the publications of Chiu and Sand (2005), Chiu (2003, 2005, 2007) and Sand (2007). In the present essay, we now know from analyses and discussions that the motifs identified during the 1970s occur in a regularly repeated fashion and are largely confined to well-defined zone of motif marked bands or friezes that encircled various pottery vessels. Yet, a number of them also attest to the presence of increasingly important set of repeated panels that also encircled just a circumscribed set of pot types (Sand 2007:268-280). They are typically large carinated jars of a particular vessel size and form (Clark 2007:292-293, Figs.1 & 2). Thus the archaeological investigations of that **Lapita art style** and its **design system** are currently undergoing a conceptual transformation that will further heighten their importance within the definitions lying at the core of the **Lapita Cultural Complex**. A current synopsis put it thus: “Both the meticulous way in which the designs were applied and the formal complexity of some of the design fields are evidence of strict design rules” (Ambrose 2007: 220), precisely as Mead *et al*. (1975) long ago recognised when they began their systematic rule-based analyses of the motifs on the less complexly decorated ceramic vessels belonging to the early phase of Eastern Lapita Regional Tradition.

Furthermore, the Lapita style of artistic decoration need no longer be restricted to sole occurrence on ceramics; on occasion these motifs, even an anthropomorphic face, have been recorded on other artefacts recovered from Lapita sites that were made from shell and bone. Thus **Lapita decoration** is advantageously assigned to a pervasive category among the world’s art styles (DeBoer 1991: 147-148; Kirch and Green 2001: 185) due to its appearance on a wide range of media, just as argued by Mead (1973: 20), by Green (1979: 18-19), by

Kirch (1997:126,132); by Kirch and Green (2001:184-185) and by Marshall (2008). One has to allow an equal importance to a former presence of non-durable objects made from bark cloth, or the skin of the human body, that were decorated by the technique of tattooing (Green 2003:112; Gell 1993: 95-96, 190) – once part of this pervasive **Lapita art style** – along with some of those ethnographic designs formed by the techniques of sennit-lashing on many wooden objects recently made by men who subsequently carved decorations on some items e.g. clubs (Kaeppler 1989,1999:15-32). In addition, some designs were woven into the baskets, mats and other fibre items made by women (Taylor 1960; Conner 1982:162-64; Kaeppler 1999:33-45).

The Lapita Horizon, Regional Lapita Traditions and models tracking Lapita Origins

The final three items within the conceptual framework employed in this essay focused on aspects of the Lapita phenomenon are those of a **Lapita Horizon,** a related concept of **Regional Lapita Traditions,** and those involved in formulations concerned with models tracking **Lapita Origins.** From the time of the initial interpretations those sites containing potsherds decorated in the Lapita design style, Americanist concepts of horizon and tradition were deemed by some to be appropriate designations when investigating certain aspects of this phenomenon. Thus these two concepts were early used for this purpose by Green (1967: 234) and shortly thereafter by Golson (based on a symposium draft manuscript also dated to 1967, eventually published in 1972 and cited in Golson 1971:75). By the 1990s it was possible to state that only a few would dispute that aspects of Lapita could usefully function as a horizon, or operate as that kind of archaeological unit (Green 1992: 10). In the current 21st century explorations of the **Lapita Horizon**, Anderson (2001: Fig 1) and Green (2003: fig.1) have more than amply demonstrated this concept's integrative utility and its beneficial employment as a very widespread foundational base from which various **Regional Lapita Traditions** arose. This foundational base Bedford and Sand (2007:55) have recently termed a 'formative period' underpinning the earliest stages of Lapita pottery development in the Bismarck Archipelago, a period which "witnessed developments in the pottery and arguably other elements of material and social culture that laid the foundation for the later dispersal into Remote Oceania". Specht (2007:61) provides precisely the same term – a formative period of the Lapita cultural complex. He too places the justification for this term with developments in Lapita pottery, and other elements of its material and social culture, of same kind that took place contemporaneously within the Bismarck Archipelago. Thus, at present, the productive role of the term **Lapita Horizon** in signifying an integrative horizon of limited time-depth and wide geographic distribution seems assured.

In contrast the concept of 'a Lapita Tradition' has to date not achieved anything like the same well-considered status. In my view, the concept requires division into regionally based Lapita traditions to achieve pertinent analytic cogency; a strategy designed to accomplish this was constructed (Green 2003:103-109) and illustrated in summary form in Figs. 3, 4, and 5 of the article. Expressions of support for this kind of regional tradition concept have occurred in the literature, from time to time and for various regions, because it combines aspects of both time and space in a schema of local Lapita traditions each of which have their own geographical and temporal dimensions (Bedford and Sand 2007:55). The Kone tradition proposed by Galipaud (1998:148) for New Caledonia, and that of the Eastern Lapita tradition with its Early and Late Eastern phases outlined by Kirch (1997:157-159) stand as sound examples. My hope is that the concept of local long duration Lapita traditions will be more widely adopted as I have done in this essay and elsewhere when describing the situation exemplified by Lapita sites in the Outer Eastern Islands of the Solomons.

Among all the topics where writers attempt to restore order into discussions about aspects of the Lapita phenomenon in the Southwest Pacific, those concerned with its origins continue to be the most highly contested and fraught with controversy. One might go so far as to assert that there are more named models than there is strong evidence lending support to their conjectured designations, especially if one mentions the often archaeologically sin-binned word – 'migration(s)'.

That is why my 2002 overview, presented at the 50th anniversary Lapita conference, was entitled "The Lapita horizon and traditions – signature for one set of oceanic migrations". This title deliberately set the tone for the stance adopted (Green 2003). On the one hand, those who argue for some kind of a mass 'people movement' (Oppenheimer 2003:57) can be contrasted with the others who hold there was no migration of any consequence at all (White and Allen 1980; Allen 1984; Terrell 2004). Terrell (2009) currently avers that "even if the term "migration" is proper and fitting (which I don't think it is)", he and his co-authors assuredly take umbrage against just two large scale migration scenarios, one of initial colonisation and one much later involving Lapita (Terrell 1999: 53; Terrell *et al.* 2001 plus CA commentaries thereon by multiple scholars from several fields). In the review of all the named propositions I could assemble, they were collectively assessed on the degree of warranting evidence in support, the counter evidence serving to diminish the plausibility of the conjecture, or (in a few examples) the lamentable lack of cited quality information in support of a proposal. Most of individual entries gathered for the overview display catchy titles seeking to encapsulate the essence of their preferred model versus one or more selected alternatives deemed less apt. All those having some degree of merit were reduced to just five sets, each set sharing in common a number of elements stressed by their authors as of significant consequence. These models were neutrally labeled SETS A, B, C, D and E, rather than by one of the various names each model had been given, along with citations to the author when each proposed model was assigned to a given set (Green 2003: Table 3). To distinguish the sets they were given designations drawing on those common words that seemed best to characterise the content and thrust of the collective named models within it, and a short hand code was adopted to identify the set name whenever its repetitious citation in a text was required. The hope was that this strategy would help to channel the discussion to the basics with respect to each set, so that there would be clarity when and if new evidence required extended critique, detailed discussion and/or modification of any of the sets in the future. To date this device has proven to hold limited appeal – the chaos prevails, and features former allegiances to one or another catch-phrase titles for a particular models when discussions arise about Lapita's supposed origins. Currently, choices are made in support of one or another of the former propositions as to which is superior when the selection of propositions chosen for discussion are just a few among the many alternatives available.

Fair enough – my preference was evident as to which among the five choices discussed at some length (Green 2003: 99-103) seemed strongly warranted on the basis of existing evidence from all disciplines. For Near Oceania and the immediate origins of the Lapita Cultural Complex, it was the model listed under Set D with the title *Voyaging Corridor Triple I* coded as *VC Triple I*. For Remote Oceania it was Set E with the title *Mobile Founding Migrant Model* with the code *MFM*. No one model of Lapita origins is appropriate for the whole of Oceania; Near Oceania was occupied by human populations for over 37,000 years, thus millennia before Lapita as a phenomenon appeared within that region some 3500 years ago. In contrast, the southeastern region of Remote Oceania had never been settled until a process of rapid human colonisation took place – through serial landfall events to the east, to

the south, and then southeast or southwest down that island chain – all of them associated with human populations bearing what was by then for a time a very well-defined form of **Lapita culture**.

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