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# Models for the Lapita Cultural Complex: An Evaluation of Some Current Proposals

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## ABSTRACT

Some proposals by Clark and Terrell for multiple working models that may apply to the Lapita cultural complex are examined in the light of existing evidence and their defects set out in relation to the presumed state of each of the ten variables used to construct the models. Another model in which the state for each of eleven variables seems plausible given current evidence is then proposed. It is concluded that additional and probably more complex models are required to further an understanding of this cultural complex but their formulation and testing require data not available at present.

*Keywords:* LAPITA, CULTURAL COMPLEX, MULTIPLE MODELS, VARIABLES, EVALUATION.

## INTRODUCTION

In a recent paper, Clark and Terrell (1978) have stressed the need for explicit construction and evaluation of multiple working models in Oceanic archaeology, to replace what they view as less scientific methods of presentation such as culture-historical narratives. While they allude to the possibility of building other models for the Lapita cultural complex, to illustrate their argument they develop four simple yet contrasting models for the Lapita complex which they believe may enhance an understanding of what is happening and why (Clark and Terrell 1978:308-313). Their evaluation of these few models in the light of the archaeological evidence, however, is brief and inconclusive. In fact, they say that "while some who have written on Lapita seem to be of the opinion that the evidence as known is substantial", they believe it is not yet sufficient to answer the critical questions they pose (1978:313).

In contrast to Clark and Terrell, I believe in the validity of other approaches to Oceanic archaeology, including culture-historical narratives. I also believe that sufficient evidence already exists to evaluate simple models about the Lapita cultural complex and thereby assess our current understanding of it. This paper, therefore, considers the models proposed by Clark and Terrell in the light of this evidence and recommends another possible model which may assist in comprehending a complex but fascinating problem in Oceanic prehistory. In reading this evaluation, however, it should be kept in mind that there is a very clear conflict between Clark and Terrell and myself in matters of theoretical orientation, methodology, type of presentation, and the sufficiency of the empirical evidence.

The characteristics selected by Clark and Terrell for the construction of a set of Lapita models uses ten variables, each with two contrasting states. From the very numerous permutations and combinations possible among the ten variables and the two states for each, four Lapita models are proposed, constructed and named: Strandlooper, Supertramp, Trader and Population Growth. Also, as is evident in Table 1, these four models differ only in the states of *some* of the ten variables. For other variables a single state is common to each of the four models. It is thus efficient to evaluate the four different models by successively assessing the plausibility of the two possible states for each of the ten variables. Moreover, if one

**TABLE 1**  
**CHARACTERISTICS OF FOUR LAPITA MODELS**  
 (after Clark and Terrell 1978)

MODEL VARIABLE	STRANDLOOPER	TRADER	SUPERTRAMP	POPULATION GROWTH
<b>Descriptive variables:</b>				
Distribution				
Geographic range	widespread	widespread	widespread	ultimately widespread
Duration in generations	brief	long	brief	long
Variability	homogeneous	homogeneous	heterogeneous	heterogeneous
<b>Rate variables</b>				
Rate of dispersal	rapid	rapid	rapid	slow
Rate of extinction	rapid	slow	rapid	varies in time and place
Rate of interaction	infrequent	frequent	infrequent	not crucial
<b>Causal variables:</b>				
Subsistence strategy	specialised	generalised	generalised	generalised with local specialisation
Reproductive strategy	rapid population growth	rapid population growth	rapid population growth	regulated according to local circumstance
Dispersal strategy	skilful	skilful	skilful	not deliberate
Colonisation strategy	ineffectual	effective	ineffectual	effective

of the states for a variable can be shown to have little plausibility on the basis of existing evidence for these models, then any of the many other models which could be proposed using that state will probably also prove defective. In short, if some of the possible states of the ten variables appear unlikely to apply to the Lapita evidence, then the number of useful non-falsifiable models it is possible to construct from the variables becomes quite a bit smaller and the focus on the really crucial elements is more clearly defined. For these reasons this approach is followed in the paper, rather than evaluating sequentially each of the four formally proposed models. In doing so I have accepted that the variable conditions given by Clark and Terrell for each model are appropriate to it. In some instances this could be argued and alternative but closely related models to these four constructed and evaluated. That possibility is little explored in this paper, but it could be done with profit in the future.

### EXAMINATION OF THE VARIABLES

We begin with the descriptive variables of distribution, the first of these being *geographic range*. Not surprisingly, none of the four models calls for the geographic range of the Lapita cultural complex to be "localised" or limited. As I have argued elsewhere "in the context of at least 30,000 years of Oceanic prehistory, or even of 4000 to 5000 years of Austronesian presence in the area, the Lapita cultural complex dating between 1600 B.C. and 600 B.C. is the only identifiable cultural entity which forms such a widespread and distinctive cultural horizon" (Green 1978:4). There appears to be no reason to revise that evaluation. In fact, with the discovery of a few Lapita sherds in an appropriate time context in the Admiralty Islands (Kennedy 1981) from whence some of the Lou obsidian used by Lapita communities came, and with the possibility of Lapita on the north coast of New Guinea hinted at by a sherd from Aitape (Specht 1974:304), it is likely that the geographic range of known Lapita sites may increase even further than at present.

The next descriptive variable concerns *duration in generations*. Here two models, Strandlooper and Supertramp, are said to require brief duration, whereas the other two require long duration. In the most western Lapita region, the presumed homeland of the complex, there was until recently a lack of radiocarbon dates as early as those from sites further east. However, this is no longer the case. The Lapita sherds from the Admiralty Island site of Kohin Cave (Kennedy 1981) are in a context bracketed between two dated layers, a basal one with an uncalibrated  $^{14}\text{C}$  age of  $3450 \pm 100$  B.P. and another some distance above with an age of  $2310 \pm 120$  B.P. The sherds are from a layer immediately above the basal midden, suggesting a calibrated age for them in the 15th to 16th centuries B.C. The Eloaue Island site in the St Matthias group now has two uncalibrated dates for one oven: one for the upper half of  $3030 \pm 180$  B.P. and one for the lower half of  $3900 \pm 260$  B.P. (Bafmatuk, Egloff and Kaiku 1980:80). Although the authors explain the younger date as too young because of possible contamination by mixing with recent garden charcoal, the two dates, which overlap at two standard deviations, are not significantly different from each other statistically and could be pooled to form one estimate for the age of the oven. Whether one accepts the older date, as they do, or a pooled date as I would prefer, a calibrated age greater, and perhaps much greater, than the 13th century B.C. is indicated. In cluster analysis of decoration characteristics, the Eloaue, Ambitle and Talasea sites exhibit close similarities and differ significantly from Watom, dated to the 6th century B.C. or before (Anson pers. comm.). For the first time we have an indication that the Lapita cultural complex was present in the western region for a number of centuries and that some sites there may prove to be

the earliest of any known. In the Reef/Santa Cruz area a 700 year sequence of Lapita sites has been documented, followed by a plain ware phase (Green 1976; McCoy and Cleghorn n.d.), while in the New Hebrides an early site of 12th to 14th century B.C. age and a late one of 6th century B.C. age are known (Green 1979: Table 2.1). Recently the New Caledonian sites have been shown to belong to a sequence dating to between 1600 B.C. and 400 B.C. (Green and Mitchell n.d.). Finally, the sites on Viti Levu and Lakeba in Fiji, Tongatapu and Niuatoputapu in Tonga, and those of Samoa, Futuna and Uvea can be arranged in a sequence of Early Eastern Lapita, Late Eastern Lapita, and Polynesian Plain Ware, with the Lapita phases dating to between 1600 B.C. and about 600 B.C. (numerous authors as reviewed by Green 1981). Any description of the Lapita cultural complex itself as brief in its duration (even without taking into account its development into later complexes) is incorrect. We are talking about 700 to 1000 years of prehistory and 28 to 40 generations at a very minimum. Therefore the Strandlooper, Supertramp or any other models which seek to characterise the duration of Lapita as brief are likely to prove false.

The next descriptive variable is one which divides *cultural variability* between homogeneous and heterogeneous states. In the Supertramp Model, heterogeneous is characterised by Clark and Terrell as "rapid cultural adjustment to new situations and resources" (1978:311), while in the Population Growth Model it is said to be "gradual change over time" (1978:313). Most archaeologists, including myself, however, appear to regard the Lapita cultural complex as surprisingly homogeneous given its extent over time and space. For example, I noted "that the only comparable ethnographic situation is that of Polynesian Outlier communities, and the cultural uniformity they have been able to maintain over the last 1000 years is not as close in either ethnographic or archaeological terms" (Green 1978:4). The best direct evidence is that reviewed under the Lapita Ceramic series (Green 1978:7-14). It shows some distance decay in decoration from West to East, but early Lapita sites from one end of the range to the other are surprisingly alike ceramically. The pottery exhibits slightly different types of change in each island group sequence, yet there is a remarkable conformity in the changes from early to late throughout the Eastern Lapita region. Also, later western sites like FAC in Watom and RF-2 or RF-6 in the Reef/Santa Cruz group, though 1300 km apart, are very similar in detailed aspects (allomorphs) of the decorative motifs they employ, suggesting continued contact between widely separated localities. Granted, there is some variation in the rest of the cultural inventory, as one might expect from adaptation to a range of widespread habitats, but in general the Lapita cultural complex is fairly uniform in the types of materials represented. In the raised atoll environment of the Reef Islands during the Lapita period, for example, people sought to maintain their previous adaptation to a high island situation by importing a range of materials not available locally (Green 1976). Thus the response in this case seems to have been just the opposite of the "rapid cultural adjustment to new situations and resources" required by the Supertramp Model. If Lapita is considered culturally heterogeneous at all, it is as a result of gradual change over time in the different regions as in the Population Growth Model. However, on the existing evidence, I think the Lapita cultural complex should be placed at the homogeneous end of the range in cultural variability. The Supertramp Model in this respect is false and the Strandlooper Model only marginally possible.

Most writers on the Lapita cultural complex regard its *rate of dispersal* as rapid. Irwin (n.d.) put the situation well: "the spread of Lapita communities in Oceania, while not necessarily simply an orderly passage eastward, occurred rapidly. In so far

as these communities had to reproduce biologically and materially, it was a gradual process that took some time. Yet in so far as archaeological sampling and dating are imprecise, the Lapita expansion appears practically instantaneous" (this is further documented by Irwin 1980:325). Arrival in Fiji and West Polynesia is dated to about the 16th century B.C. If the dispersal began from the Bismarck archipelago as I have argued (Green 1979:45) then on current dating evidence from that region (see above), no more than two or three centuries need be required. In fact, it would be hard to account for the similarity of the earliest Lapita ceramics over large distances if very much time was involved. This would require a quite extraordinary degree of down-the-line cultural conservatism in successively later colonies.

I mention this because in the Population Growth Model, widespread distribution of Lapita sites is assumed to have "come about only slowly following gradual population growth, gradual settlement expansion, and perhaps even extinction in some older areas of Lapita settlement as the crest of Lapita settlement slowly advanced eastward across the Pacific from the west" (Clark and Terrell 1978:311). Slow dispersal does not seem to conform with the existing empirical evidence. It implies some sort of A to B to C step-by-step scheme which patently does not apply to the settlement of Fiji and West Polynesia (Green 1981). Neither does it apply to New Caledonia, the New Hebrides, and the Reef/Santa Cruz area where Lapita sites appeared about the same time as those in Fiji/West Polynesia and persisted for a comparable period. Moreover, in the Reef/Santa Cruz case there is evidence that importing of obsidian continued over some 700 years from Talasea and to a small degree from the Lou source — in what I interpret as some kind of down-the-line exchange from Lapita communities further to the west. This suggests that the Lapita communities to the west were in large part contemporary with those of the Reef/Santa Cruz area. So far the data on dispersal heavily favour the rapid end of the variable range as in the Strandlooper, Trader and Supertramp Models, and render suspect any serious acceptance of the Population Growth Model.

The next descriptive variable concerns *rate of extinction* and examines the frequency with which "old settlements were abandoned, died off, or were 'absorbed' into some other cultural tradition" (Clark and Terrell 1978:310). In the Strandlooper Model extinction was seen as rapid as settlements were frequently relocated. In the Supertramp Model extinction is also classed as rapid, while in the Population Growth Model it is suggested that it may have varied from place to place and from time to time. The main contrast, then, is between the Trader Model in which extinction is said to be slow, and the rapid extinction of the Strandlooper and Supertramp Models. Evidence bearing on this point is largely impressionistic. Certainly from the size of Lapita sites and the amount of cultural debris in them I tend to regard most of the Lapita settlements as reflecting some type of permanent settlement rather than transitory occupation. In the small Lapita site of RF-2 there is some control over relevant data. Here there is good evidence for the effect of human predation on some of the main shellfish species used for food. Swadling (n.d.) concludes that this resulted from continuous occupation over some period of time. The site has at least two activity areas which differ in the decoration and shapes of pots found in them (Parker 1981). There are also concentrations of other materials in the two areas and these are associated with differences in the distribution of features. Thus, while I do not conceive of most known Lapita settlements as yielding evidence of hundreds of years of occupation, neither does the evidence really fit with very short term settlements replacing one another in rapid succession as would be the case in the Strandlooper and Supertramp Models. Rather, it suggests a succession of settlements at something like century intervals over a fairly long time span without



the occupants being absorbed into some other cultural tradition. Far more Lapita sites than are known from most island groups would be expected, were their replacement really an extremely rapid affair. However, as this variable cannot easily be assessed on the available evidence, none of the four models should stand or fall on that basis alone.

The last descriptive variable concerns the *rate of interaction* and indicates the frequency of contact such as trade, travel and intermarriage between communities. Its assessment is not crucial for the Population Growth Model, but it is contrastive between the Strandlooper and Supertramp Models on the one hand, where interaction "between each small littoral community was infrequent or insignificant", and the Trader Model on the other hand, in which "frequent interaction among communities within ca 600 km of each other was crucial to the survival of the Lapita culture as such" (Clark and Terrell 1978:310-311). As is set out in more detail below, the case of the Reef/Santa Cruz Lapita sites appears to falsify an infrequent and insignificant state for this variable in the Strandlooper and Supertramp Models.

Specific evidence for importing of a range of products over time in the Reef/Santa Cruz area is reported in a number of sources (Green 1974, 1976, 1978, 1979; Dickinson, Moore and Green 1978) and more is forthcoming on the obsidian. This, and the less cohesive information for other regions is the basis for advancing an interpretation that the available evidence can, at best, "be viewed as indicating no more than a network of reciprocal exchanges between related communities that maintained frequent contact" (Green 1979:38). Nowhere is the position adopted that the Lapita were involved in "specialist trading" or that it was everywhere "crucial" as Clark and Terrell (1978:311) assert. Rather, I have been careful to emphasise that in Island Melanesia the "special wealth of Lapita communities lay in their skill at long-distance ocean voyaging and the attendant abilities in constructing suitable types of vessels, such as double canoes, necessary in establishing a series of communities over a vast area and carrying on regular exchange among some of them" (Green 1976:264). Also noted is the lack of evidence for exchange across the long water gap between the New Hebrides and Fiji (Green 1976, 1979) and the much lesser quality of the evidence for importing in the Fiji-West Polynesian region (Green 1979). Nonetheless, the evidence is substantial that continuing voyaging and communication took place in the Fiji-West Polynesian region from the time of first settlement throughout the Lapita period and beyond right up to the time of European contact (Green 1975; Davidson 1977, 1978; Kaeppler 1978; Green 1981). In sum, a characterisation of the interaction variable as infrequent and insignificant appears untenable for the Lapita cultural complex, making the Strandlooper and Supertramp Models deficient in this respect. On the information available, the frequent end of this variable range is more likely. However, one should be wary of overplaying it as Clark and Terrell (1978:311) have done in attributing to me the view "that Lapita potters were specialized traders". The published evidence, I think, is clear that this is the exact opposite of my stated position.

The next variables are all causal ones, the most important of which are those assessing the subsistence and colonisation strategies. Three models, Strandlooper, Trader and Supertramp, all portray a third causal variable, *dispersal strategy*, or "wanting to establish new settlements" (Clark and Terrell 1978:310) as skilful, and only in the case of the Population Growth Model is it judged that "there may have been no deliberate emphasis on dispersal" (Clark and Terrell 1978:313). I can say little about this variable, although I tend to favour a judgement at the skilful end of the range. The one case where I argued that dispersal was deliberate was in crossing the water gap between the New Hebrides and Fiji (Green 1978:5-6). Certainly the

evidence points to the skill of those associated with the Lapita complex in long distance voyaging, although how much of their dispersal was intentional and how much was accidental and at the mercy of winds and currents, it is difficult to say.

*Reproductive strategy* is the fourth causal variable. Rapid population growth is used for three of the models, Strandlooper, Trader and Supertramp, while in the fourth model regulation of population growth rates is thought maybe to "have varied according to local circumstance" (Clark and Terrell 1978:313). The models, therefore, are not strongly differentiated on this basis. Real data on Lapita population growth rates is entirely lacking. However, as Clark and Terrell (1978:313) indicate, the models may also be tested by computer simulation. Something like this has been done by Black (n.d.), although he was more concerned with the settlement of Polynesia than with the Lapita cultural complex. Nevertheless, he concludes "This quick dispersal is reminiscent of the Strandlooper Model for Proto-Polynesians. Rapid dispersal and settlement is possible given the results from model 3. Under the range of assumptions and parameters used in model 3, it is possible to settle all of Polynesia in a few hundred years beginning with a small and highly mobile population" (Black n.d.:3). The rapid dispersal variable examined above for Lapita is not impossible given a reasonable reproductive strategy involving small numbers of people. At this early stage in the settlement of most of the island groups, population growth was probably not regulated, nor would its careful regulation seem to have had any real advantage.

Returning to the two causal variables which allow some assessment of the differential validity of the four models, let us begin with *subsistence strategy*. Clark and Terrell have briefly examined the information relating to this variable and noted my arguments that "evidence now available from excavations in Santa Cruz Islands implies that the economic base of Lapita communities did not rest on intensive exploitation of seafoods; contrary to Groube's Strandlooper hypothesis, both agriculture and domestic animals played an important part in their subsistence economy" (1978:313). They conclude "and even if Groube's hypothesis does not fit the archaeological data on subsistence found at all Lapita sites, evidence for a well established generalized economy does not help us to choose between the three other models, including Green's trader hypothesis" (Clark and Terrell 1978:313). In my view, the case for a specialised Lapita subsistence base has little merit whether it is applied to Tonga and Fiji (Green 1972), the Reef/Santa Cruz area (Green 1976, Swadling n.d.) or all Lapita sites (Green 1979).

In characterising the Strandlooper Model, Clark and Terrell (1978:310) write that "Groube once hypothesized that the Lapita potters originally practiced a restricted maritime/lagoonal economy which was not supplanted until the latter half of the first millennium B.C. when newcomers to Oceania introduced them to a more viable agricultural economy". Groube (1971:311-312) was in fact careful to state two possibilities: it could have been either by *development* "when the Lapita style had completely changed" or by *introduction* "when a new group of people had arrived" that the more viable horticultural economy emerged which "enabled them to expand and survive in Fiji and Tonga to eventually colonise the remainder of the Pacific". He believed that "careful analysis of the economic evidence from other Lapita sites throughout the Western Pacific should test the validity of this hypothesis" (Groube 1971:312). He rested part of the case on the presence or absence of the pig which he claimed was usually associated with horticulture and in Fiji was seen as sound evidence for the establishment of a viable horticulture during the later periods. The remainder of his case he rested on the cessation of shellfish dumping or concentrated midden accumulation in post-Lapita sites, especially in Tonga but also in Fiji.



Two points deserve comment. In the Eastern Lapita region where Groube developed his Strandlooper case, there is continuity with later cultural complexes rather than cultural replacement. Secondly, it has not been demonstrated that new arrivals, especially newcomers with a more viable horticultural economy, replaced a non-horticultural form of the Lapita complex anywhere in Island Melanesia. Most of the economic and technological evidence from Lapita sites differs little from that of later sites, including protohistoric sites, leading directly to fully horticultural societies of the ethnographic period. Tongan Lapita sites on Tongatapu have concentrated midden remains (as do later sites) because of local lagoon conditions (Green 1972), but elsewhere, in Samoa, Lakeba in the Lau group, Fiji itself, New Caledonia, and in the Reef/Santa Cruz group, there is no evidence that concentrated shell midden dumping in Lapita sites contrasts with non-concentrated midden remains in later periods. Nor does the range of fish or other bone differ markedly between Lapita sites and those of later periods. The only documented difference is that pelagic fish, present in a few later sites, are represented poorly or not at all in Lapita sites (Kirch and Dye 1979). As for pig bone, if Groube can use it as sound evidence for the establishment of viable horticulture in later periods, then its presence in numerous Lapita sites from Watom to Tonga and Fiji (Green 1979:37) must be taken as falsification of Groube's hypothesis with respect to Lapita sites. Evidence presented recently by Hunt (1980) for Fiji and by Kirch (1978) for Niuaotupapu reinforces the position that the Lapita subsistence strategy was generalised rather than specialised. In fact, Kirch (1978:12) claims "The Niuaotupapu excavations reinforce the interpretation of Lapitoid economy having been broad-spectrum, integrating both developed cultivation systems and marine exploitation technology", and cites a range of supporting evidence. The use of this state as a major causal variable in the Strandlooper Model has for some time been capable of falsification from archaeological evidence. Its continued consideration does not really improve our understanding of the Lapita cultural complex.

The other causal variable which allows some assessment of the validity of the four different models is that of *colonisation strategy*. In two models, Strandlooper and Supertramp, the state of this variable is characterised as ineffectual, while in the other two models, Trader and Population Growth, it is described as effective or indicating "probably skilful colonists" (Clark and Terrell 1978:313). The view that the Lapita people were ineffectual colonists warrants little discussion. It is hard to see how they not only managed to insert themselves as a viable long term cultural complex in the already settled area of western Island Melanesia, but also to pioneer and settle (probably for the first time) the new Fijian-West Polynesian region of the Eastern Lapita, if they had not "worked to maintain colonial settlements and economies" (Clark and Terrell 1978:310). That the Lapita cultural complex persisted everywhere for as long as it did (see above) also suggests they were effective colonists. Again the Strandlooper and Supertramp Models seem capable of falsification on this criterion.

#### EVALUATION OF THE FOUR MODELS

Putting all this together (Table 2) it is possible to see that two models, Strandlooper and Supertramp, fail on four counts to conform with the existing information on the Lapita cultural complex. Three of the four variables where the state selected fails to comply with the evidence are the same, namely Duration, Interaction and Colonisation. However, the most serious defect of the Strandlooper Model is its reliance on a specialised subsistence strategy, whereas the fourth defect of the Supertramp Model, its requirements of cultural heterogeneity, is probably no more serious than its three

other defects. The Population Growth Model is difficult to assess or neutral for four of the ten variables. Of the two variables in which it fails, the most serious is that which requires a slow rate of dispersal following some sort of A to B to C model in successively later steps, with the possible extinction of A before C is settled. The one model which does not appear capable of falsification on existing evidence is the Trader Model. However, I do not believe that this model provides the best possible fit with the data, at least in the way the model is set out by Clark and Terrell.

TABLE 2  
EVALUATION OF FOUR LAPITA MODELS

MODEL VARIABLES	STRANDLOOPER	TRADER	SUPERTRAMP	POPULATION GROWTH
Geographic range	+	+	+	+
Duration	-	+	-	+
Variability	+	+	-	-
Dispersal	+	+	+	-
Extinction	o	o	o	o
Interaction	-	+	-	o
Subsistence	-	+	+	+
Reproduction	+	+	+	o
Dispersal	+	+	+	o
Colonisation	-	+	-	+
Negative totals	4	0	4	2

+ Positive, - Negative, o Neutral

### THE LAPITA EXCHANGE SYSTEM

Although the general evidence for Lapita exchange is summarised in recent articles, the one case it is possible to examine more fully, that for the Reef/Santa Cruz Island Lapita sites, has not been described in all the detail which is available. Such examination, however, suggests that what the Lapita exchange system consisted of, had we evidence of all its pieces, is a multi-mode, complex, generalised exchange system between related communities, some of which may have specialised in making and/or exporting a certain range of products — obsidian, cherts, pots, adzes, shell tools and ornaments — for which they had immediate access to the materials required. These possible exports about which little is known may be set against a better known range of imports. However, the social dimension was as important in keeping the system going as was any economic necessity of importing to survive.

The complex multi-mode range of imports may be illustrated by a brief description of the Reef/Santa Cruz example. *Direct access* (26 km) importing of many oven stones and of a little clay and temper for pots is attested from Santa Cruz Island and Tinakula into the Reefs. *Local reciprocity* (26 km) of already manufactured pots from Santa Cruz Island to the Reefs in quantity (0.4 tonne in a site with an area of about 1000m<sup>2</sup>) is also suggested by the evidence. *One stop reciprocity* (275-380 km) from the west is indicated for most adzes imported as entire nearly finished objects, for a few pots imported as finished products and for major amounts of chert imported as raw material; the same applies to minor amounts of an inferior grade of obsidian imported from the east. *Down-the-line exchange* (1500 to more than 2000 km) is implied by small amounts of muscovite-garnet-schist (glitter) and metamorphosed sandstone, by small amounts of obsidian from the Lou source, and by much larger amounts from the Talasea source, again all sources being to the west.

However, no instances of quantitatively really large transfers of items have yet been documented. Extensive trade does not seem a viable interpretation of the evidence. Also, if the exchange itself had been so important to maintaining the Lapita system, then it is unlikely the people would have moved so far out into the Pacific, certainly not to the Fiji-West Polynesian region, where the evidence for exchange is much less, or to the New Hebrides and New Caledonia, where it is also less. Neither specialist trading nor unspecialised exchange appears to be a major characteristic of the Lapita cultural complex. "Certainly it enabled Lapita people to maintain their culture while living in habitats with sometimes scarce resources, but it does not explain their expansion across the Pacific, from West to East" (Whitehouse n.d.:16). What both the exchange system and the huge distribution of the Lapita cultural complex do imply is a great deal of skill in local and long-distance voyaging.

### ANOTHER MODEL

It is possible to propose other possible models which may more realistically account for the evidence. Whitehouse (n.d.) has constructed one such model which she calls the *Coloniser Model*. Its characteristics are:

- Distribution — widespread, long duration, culturally homogeneous.
- Rates of change — rapid dispersal, resistance to extinction or cultural replacement, frequent interactions among communities close at hand and at distances up to 600 km.
- Causal variables — a generalised economy with both maritime and horticultural components, effective colonisers, skilful voyagers, rapid population growth, effective exchange network.

On the evaluation above, this model too would score highly and it is much like Irwin's continuous settlement view of Lapita (1980, n.d.). I would change Whitehouse's last causal variable to an effective communication network, thus further lowering the emphasis on exchange of goods, and add communication strategy with an effective-ineffective dichotomised range to Clark and Terrell's set of causal variables. Rates of interaction may continue to be assessed as frequent or infrequent, but it also matters how important or effective that contact was to the communities involved. It was important, and the long water gap between Island Melanesia and Fiji-West Polynesia, which was difficult to cross regularly to the east against the wind, is significant to our understanding of what happened to the Lapita cultural complex in the two areas for just this reason. The two ends of the communication network were cut off from effective two-way contact with each other.

### CONCLUSION

At this point, it is over to the reader to decide how much this exercise has done to improve our comprehension of the Lapita cultural complex. There are certainly instances where the construction and evaluation of multiple working models is capable of providing fruitful new insights into aspects of Oceanic prehistory, but for me it remains merely one of the many possible approaches.

It appears that the attempt by Clark and Terrell to explain the Lapita cultural complex has proved disappointing because of the rather insubstantial empirical base on which three of their four models, Supertramp, Strandlooper, and Population Growth, were formulated. This, more than anything else, accounts for the poor showing of these three models when evaluated against known data. It also accounts for the more favourable assessment of the Trader Model. In considering either an

array of competing hypotheses or multiple working models for a body of data, one has always to consider the prior plausibility of the various choices that are available. As Smith (1977:604) says of this problem "It is at the same time clear, however, that scientists, including archaeologists, do not consider all logically possible hypotheses, but initially distinguish between those that are reasonable and those that are not". Clark and Terrell constructed a simple paradigm for building a large array of possible models, and then selected four of them for presentation with little or no assessment of why they chose these four or examination of their prior plausibility. Rather their assumption seems to have been that since, in their view, there was not enough data available, all possible models were reasonable and none were really capable of testing given existing knowledge.

As we have seen, for some of the variables selected by Clark and Terrell, the evidence strongly suggests that one of the two variable states is untenable, and therefore, that models using that state are not at present worth examining further because of their lower initial plausibility. Thus, one possible approach would be to stay with the same formulation for constructing the models, but to select ones with more prior plausibility from which alternative hypotheses can be generated and tested through focusing on what seem to be the most crucial variables and states. This is done here by proposing the Coloniser Model as opposed to the Trader one, both models being very similar in their other characteristics.

Another possible approach would be to complicate the models further, especially in the area of contact, its frequency, its effectiveness, and the type of communication network involved. If the Reef/Santa Cruz evidence is any guide, importing and exporting of materials and finished items are an obvious area where more archaeological information about the Lapita cultural complex is to be expected on which to build more complex models. However, at present we do not have such information, and the formulation of empirically reasonable yet more complex models is therefore difficult as there are no really comparable ethnographic examples on which to draw for those parts where data are lacking.

A third approach is to use the culture historical narrative form. This is the strategy used previously, which brought interpretation of Lapita from a vague continuum of culture between Melanesia and Polynesia through various chronological problems to interpretative statements based largely on the pottery, and then to the Lapita cultural complex as we understand it now. Although this developing framework furnishes the basis for one of the sounder models of the complex used by Clark and Terrell, it is an approach which they believe ought to be abandoned in favour of some putatively more scientific methods. Yet to abandon it entirely would seem to rule out the use of what has in this case proved a reasonably productive approach and one that has furnished much of the data on which their multiple modelling approach is based. If materials cast in a culture historical framework are capable of transformation by others into more formal models, and if the models can be evaluated through examination by a culture historian using such materials as here, then it may be that culture history is complementary to and supportive of the newer approaches in archaeology, just as many of the recent text books on the subject suggest.

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