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Colonization is a hot topic in the prehistory of the Pacific and elsewhere. The five reviewers above have made a valuable contribution to the discussion. They enjoy a range of academic backgrounds and have often approached similar issues from different perspectives. This reply tries to draw many of their comments together into a number of themes. These include the need for navigational theory, the tempo of change, the identity of colonists in Remote Oceania, the question of continuity of colonization of the Pacific Plate, "mystery" islands, computer simulations, and future research.

But first let me say that I feel lucky to have had the opportunity to draw together the separate strands of this book. These include a background in

Pacific Islands archaeology that brought the chance to see and ride in traditional canoes large and small. A long interest in sailing came from growing up on the very shore of Auckland Harbour and later sailing in various boats in different places. A particular interest in colonization arose from an obvious need to add some voyaging realities to the subject. In 1985, at the same time as the small chartered sailing ship *Dick Smith Explorer* was supporting the Lapita Homeland Project organized by Jim Allen in the Bismarck Archipelago, a group of us took my own yacht *Rhumbline*, an 11.2-meter retired ocean-racing sloop built of New Zealand kauri wood, to survey the Louisiade Archipelago. Eventually, this became a round trip that spread over two years while I was a research fellow at the Australian National University. Writing this book afterwards left the *Rhumbline* tied up and growing weeds for most of the next three years.

### **The Need for Navigational Theory**

In the centuries-long debate about Pacific colonization, the reputation of traditional navigators reached a low point in about the mid-twentieth century. This reputation was progressively restored beginning about the 1960s and there is now, at the end of the century, a major cultural revival of voyaging. Many scholars and sailors contributed to this development with pieces of fine ethnographic and historical research, which was supported by marvelous experimental voyages in various boats, rafts, replica canoes, and even in computers. All this is well known. But much of this work was concerned with fairly recent voyaging between known islands in an ocean already mapped in the minds of indigenous navigators and the charts of Western ones. Not enough attention had been paid to early exploration, to fixing the position of newly discovered islands without instruments, and to the process of initial colonization. There was a range of views about unknowns such as the rate of loss of life and whether voyages were generally one-way or return (i.e., there and back). More fundamentally, it was unclear whether there were purposeful strategies of exploration or to what extent canoes were transported across the sea by mechanical agency. It was unclear how navigational skills developed as colonization spread and ocean geography changed.

Ben Finney describes in his review how the good news about traditional navigation was rather slow to reach prehistorians. Little notice was taken of the spaces between the islands. For example, William Keegan comments that the early theory of island biogeography did not consider the motives of colonists or the means by which they reached islands. Generally, the sea was regarded as such a barrier that the theme of island isolation has been exaggerated until now. Keegan also discusses how the study of island laboratories

as if they were closed led to underestimating the effects of external interaction on internal change.

A lack of concern for the methods of navigation explains another persistent theme of Pacific prehistory: that water crossing *happened* to people rather than was something that they *chose* to do. In his review Clive Gamble draws out a number of such issues with admirable clarity: namely, that humans are not random colonizers but have intention and rationality; that they were influenced but not dominated by the environment; and that exploration and colonization were processes rather than events. His book *Timewalkers* applies such themes to world colonization (Gamble 1993).

So with those few words as context, my main conclusions are that the exploration of the remote islands of the Pacific was purposeful and rapid but followed the order of safety rather than speed. But once islands were known, interisland voyaging was able to follow the patterns of accessibility. These propositions allow many predictions that can be tested against evidence--as it exists and as it comes to hand. Computer simulation provides another kind of investigation.

### **Abrupt or Gradualistic Change?**

Roger Green suggests that one theoretical framework for colonization might conform to a step or pause model as set out in his Table 1. This can be likened by analogy to biological evolution by punctuated equilibrium. But he also notes that many other aspects of cultural change with respect to voyaging may fit within a gradualistic framework. The world's first water crossings were made from mainland Asia to what Green terms Ancient Near Oceania (1991b) (or to a voyaging corridor, as I put it). In terms of evolutionary biology the initial settlement of this area is just one region implicated in the continuing global debate on the origins and spread of modern humans. Alternatives include both abrupt replacement or long biological continuity in the region. The accepted age of these early crossings is after 40,000 B.P. by radiocarbon and perhaps before 50,000 B.P. by thermoluminescence, but whether these dating systems are complementary, contradictory, or both, is still at issue (Allen and Holdaway 1995; Chappell, Head, and Magee 1995). A very recent suggestion of an antiquity for Australia beyond 116,000 B.P. (Fullager, Price, and Head 1996) might have implied the possibility of archaic *Homo*, but these early dates have not been accepted.

With regard to the second step, there was a long period of occupation of the voyaging corridor prior to the first moves beyond. Evidently maritime technology developed until, as Patrick Kirch puts it, people finally "burst out" into Remote Oceania after 3500 B.P. By this time deep-sea explorers

had learned how to search offshore and survive. But this was after some tens of thousands of years, which also saw the transition from Pleistocene to Holocene conditions and such changes as the emergence of a center of indigenous plant domestication in New Guinea, perhaps by 9000 B.P. (Golson 1991). This may have followed a long period of management of wild plant foods dating back to the Pleistocene settlement of the Bismarcks and northern Solomons (Groube et al. 1986; Loy, Spriggs, and Wickler 1992).

It may be difficult to distinguish between punctuated and continuous models of colonization after colonization began in Remote Oceania. We are dealing with a very different scale of time--now short enough to be relatively more affected by methodological problems including archaeological sampling and the error associated with conventional dating techniques. But certainly the question of whether Remote Oceania was settled continuously or in a series of steps merits close attention, below.

### Identifying the Colonists

Messing about in boats may be one thing, but who were the people on board the canoes during significant episodes of colonization? With the passage of time the archaeological evidence fills out and such questions now come within range of what both Kirch and Green refer to as a holistic approach. Interdisciplinary studies allow similar problems to be approached through independent data and analysis.

The last two decades have seen spirited debate on the origins of Lapita. In his review Green refers to the different views held by archaeologists about the contribution to "things Oceanic" by way of Southeast Asian Austronesian input to the Bismarck Archipelago or from local cultural developments in the region (see also Green 1991a). In the matter of Lapita origins, some prehistorians have recently reemphasized a strong association of Austronesian language and aspects of material culture, economy, and settlement as intrusive to the Bismarck Archipelago (Bellwood 1996; Kirch 1997; Spriggs 1996). One aspect especially relevant to colonization is the linguistic reconstruction for Proto-Oceanic (Pawley and Pawley 1994) of what Green calls the Austronesian watercraft complex; Kirch also refers to the introduction to the voyaging corridor of the Austronesian outrigger canoe. But Finney is careful to raise another possible input not to be overlooked: "the local knowledge of indigenous sailors and farmers descended from the Pleistocene seafarers who first colonized this region." It remains to be seen what linguistic indications of this remain in other languages.

Discussions of introductions often proceed geographically from west to east, but while navigation is our main theme, arguing from the other direc-

tion may be more pragmatic. The reason for this reversal is that it may be simpler at this stage to establish an identity for the first people into Remote Oceania than to distinguish them in Near Oceania, which has a long and complex history. First, I am still sure that there was no settlement of Melanesian Remote Oceania significantly before Lapita. In my book I argue there is such a navigational threshold (pp. 20-21; see also Pawley and Green 1973) that a much earlier crossing would have been largely accidental and the possibility of it remains to be demonstrated by archaeology. In navigational terms Lapita was a single integrated expansion in Remote Oceania that followed a general strategy of upwind exploration by return voyaging. It left no evidence for crosswind or downwind voyaging. In this region Lapita represents an archaeological culture, and I have said it probably approaches an ethnic reality as well. However, insofar as Remote Oceania was the extension of a wider communication field that stretched back into Near Oceania, then more diverse elements could have passed through it over time. Certainly information about Lapita pottery decoration was flowing (Summerhayes 1997). Moreover, Lapita colonization may not have been confined to Remote Oceania. By their similar nature, some sites in Near Oceania could be seen as "colonizer" rather than as "homelands" settlements.

Lapita people were not the only ones around. It was to be expected that seagoing skills developed over a wide field, and we note that the first deep-sea settlers of West Micronesia evidently came out of the voyaging corridor west and north of New Guinea. Again, these settlers appear to have been the first people in Micronesia and similar navigational skills and strategies are implied. A similar set of arguments that they were Austronesian speaking can be made, as for Lapita. Dates back to 3500 B.P. are reported for the Marianas (Butler 1994), much the same as for the beginning of Lapita in the Bismarcks (Kirch 1997). And the intervening Micronesian island of Yap is now finally beginning to show evidence for comparably early settlement (Dodson 1995), as expected. Early migrants in the Marianas show technological similarity with Lapita. For example, the early pottery was red-slipped and decorated with stamping and fine-line incision. However, the precise archaeological signature is different.

In Island Southeast Asia to the west of New Guinea is another group of sites that are broadly comparable in technology, economy, and settlement to the ones mentioned above. The site of Uattamdi on Koyoa Island on the western coast of Halmahera dates from a time that falls between the range of early dates reported for Lapita settlement in Mussau and the Arawes (Bellwood et al. 1996; Kirch 1997; Summerhayes 1997). Uattamdi has plain red-slipped pottery with similarities to early Lapita, but some differences (Irwin et al. n.d.). Closer affinities may be with contemporary assemblages

at Bukit Tengkorak and Madai Cave in Sabah and Leang Tuwo Mane'a in the Talaud Islands (Bellwood 1976, 1988; Bellwood et al. 1996). Other associations at Uattamdi are of a kind seen by many prehistorians as part of a package of introductions related to the dispersal of Austronesian languages.

The question of precisely how similar these Southeast Asian assemblages are to one another bears on the question of how their occupants may be related historically. The same general issue applies to their similarities with Lapita assemblages in Melanesia and other assemblages from West Micronesia. There is an archaeological heterogeneity that bears on questions of identity and relationship. The questions apply within Lapita at a finer grain and Kirch accommodates this by referring to the Lapita "peoples" (1997:18).

Keegan draws a parallel between the Pacific and the Caribbean that may be relevant here. He says that in spite of the relative ease with which (it is now understood) people could have moved around the West Indies, the trend had been to assume such movement was difficult. Conventional wisdom has been that a single culture from the Orinoco River drainage called "Saladoid" peoples expanded through the area by a gradual advance from the south (Rouse 1992). However, Caribbean archaeologists are now moving away from isolationist models to ones that recognize the presence of multiple ethnic groups and cultural mosaics or matrices.

The relevant question for Near Oceania might be: does our current evidence for the period from 4000 - 3000 B.P. capture a suitable scale of heterogeneity among contemporary Austronesian and non-Austronesian groups together with the nature of interactions among them? It was not my purpose to try to answer this question in the colonization book. I am content to take the interim position that a Lapita people sailed into the Melanesian islands of Remote Oceania and, at about the same time, another people sailed into West Micronesia. Communities of the same or similar people may have also settled in islands of Near Oceania. The identity of the groups offshore is indicated by coherent episodes of colonization, and if each was the foundation culture, then there is no one else to confuse them with. Biological evidence may one day indicate more about the genetic composition of these populations. On the other hand, I am not so confident that we have the evidence to interpret a mosaic of identities and interactions that may have remained in the voyaging corridor. There is still no known archaeological site that shows how the elements of Lapita came together, even though the general whereabouts of a "homeland" may be predictable.

### **Pauses in Remote Oceania**

My position is that voyaging in the remote Pacific was an unbroken tradition, that the impetus to colonize was not interrupted, and that any apparent

pauses in settlement should be explained by changes in environmental circumstances, in the first instance, before we seek cultural ones. A related point is that there are cases where intervals of time separate discovery and settlement. Underscoring this latter point, and as Green notes, is the present debate whether the Pacific rat (*Rattus exulans*) may have been brought to New Zealand a thousand years before permanent human settlement (Anderson 1996b; Holdaway 1996).

Finney takes up the issue of whether colonization accelerated as it went. Taking a piece of string to represent 2,100 nautical miles, he measures whether colonization traveled as fast over a second length as in the first. My preference would be to begin with a shorter piece to represent the first voyages and lengthen the string with progressive offshore experience (and wider water gaps). However, the key question is whether colonization rate should be measured by linear distance to islands discovered or by area of ocean to explore. A look at the map will show that Lapita sailed within quite a limited range as compared with what was to follow. One thing the computer simulation showed was that there was more land to be found had they sailed towards it--Australia for a start. It seems the Lapita seafarers directed their attention upwind and, not knowing what lay ahead, they knew the direction from which they could most easily return. Compared with their descendants in East Polynesia and East Micronesia, one might even suggest the range of Lapita colonization was modest. I think this indicates that navigational knowledge was not as advanced, at that time, as it was to become. Nor did it need to be. Lapita culture did not leave its mark beyond the large archipelagoes of Island Melanesia and West Polynesia.

But compare the subsequent range of colonization in Polynesia and Micronesia. The ocean expands hugely to capture Hawai'i, Easter Island, New Zealand, and Micronesia (not to mention South America). Unless, by a miracle, canoes tracked directly to these very distant and isolated islands, then a lot of empty ocean was traversed. The point is that expanding space absorbs more time. But was this continuous or discontinuous time?

Finney asks a very significant question that pertains to the discussion above. Why did colonization apparently stop in West Micronesia when voyagers could have been encouraged onward into the Carolines by the emerging reefs and shoals that would later be occupied as atolls? A delay in the radiocarbon dates occurs in both East Polynesia and East Micronesia. The former is the so-called Polynesian long pause. The issue is also raised by Green, and Kirch suggests that "nothing has been more contentious in Pacific archaeology" than the debate about the chronology of settlement in central and eastern Polynesia. He says that chronometric hygiene as applied to the corpus of C14 dates (Spriggs and Anderson 1993) has opened up the pause into a "yawning gap."

In fact the pause applies to the wider Pacific Plate and must be seen from that perspective. After 3500 B.P. deep-sea colonists spread into Remote Oceania to settle the remaining continental islands of the western Pacific. By 3000 B.P. a string of settlements with ceramics stretched 6,000 kilometers along the edge of the Pacific Plate from the Marianas to Tonga. Yet almost nowhere is there archaeological evidence to demonstrate that canoes crossed this geological divide that was hidden beneath the sea. The one exception concerns 'Uvea and Samoa where their close proximity to neighboring islands of Fiji and West Polynesia led to the rapid discovery and settlement of all. The speed of settlement among the continental islands has made the subsequent delay more puzzling. However, although there is a geological threshold here, there is no navigational one. The closer islands of East Polynesia, such as the Southern Cook Islands, present feasible targets compared with previous ones. Kirch attributes part of the anomaly to methodological problems that have been raised in the past including archaeological visibility, various sampling issues (Irwin 1981; Kirch 1986), and the acceptability of radiocarbon dates.

Green and Kirch may take rather different views on the length of the pause but we all agree more archaeological exploration is required to resolve it. In my view the case made by Kirch and Ellison (1994) for c. 500 B.C. human settlement of Mangaia in the Southern Cooks is in line with the wider context. They describe evidence of declining values for forest pollen and the appearance of charcoal and erosional sediments of similar age. Not all scholars agree with the dates or the anthropogenic cause (e.g., Anderson 1994). However, it seems fair to say that similar evidence located in a less-contentious time and place has been more readily accepted. The archaeological evidence from Mangaia, which includes bird extinctions, currently dates from only c. A.D. 1000, but such evidence of human impact on fauna does not necessarily contradict the evidence for earlier impact on vegetation and soils. Indeed, this may prove to be informative about the scale and nature of subsistence and settlement.

Chronological predictions follow from theories of exploration strategy, although such predictions are more closely concerned with the relative order of island settlement than with absolute dates. Graves and Addison maintain that Hawai'i had an archaeologically established population by around A.D. 600 (1994). If it was settled successively later in time than the Tuamotus/Marquesas and the Societies, then there is a clear need for earlier settlement in the Southern Cooks.

But even a shorter pause of half a millennium requires explanation. Archaeologists are aware that settlement of the volcanic islands of the Pacific Plate may have required various new adaptations. However, while changing



ecological circumstances might have slowed settlement, they would not have prevented discoveries. But we may have underestimated the issue of social adaptation that follows from diminished land area and increasing isolation. How much ocean area progressively expands with distance east on the Pacific Plate is hard to exaggerate. For example, within an area of some 2 million square kilometers surrounding the hundreds of islands of Fiji and West Polynesia is a total land area approaching 24,000 square kilometers. Within the same-sized area of ocean surrounding the Southern Cook Islands are less than a dozen habitable islands, with a total land area of approximately 240 square kilometers. The ratio for the change is in the order of 1:100. Both East Polynesia and East Micronesia were colonized by people living initially in greater isolation. If this represents a settlement threshold, then Lapita never crossed it (Irwin 1998). The circumstances for settlement of the Pacific Plate were expanding ocean area, increasing navigational experience and skill, more elapsed time during colonization, and greater isolation of settlement. The time interval between discovery and effective colonization becomes an issue.

I do not mean to say that people had to learn to live in greater isolation. Rather, they needed to learn to mediate or manage it. This may have involved a greater frequency of long-distance voyaging until a sufficient population density was established. Kirch made a perceptive comment along these lines about the role of exchange in early Lapita settlement. He saw the formal exchange of noncritical resources as a kind of reinforcing lifeline back to parent communities (Kirch 1988).

Gamble refers to "a human prehistory where choice and contingency played a dominant part in exploration and colonization although tempered by the forces of the environment." This may apply to the "long pause," such as it was.

### **Ocean Landscapes and "Mystery" Islands**

It is a moot point whether Pacific island communities could ever live in isolation. I believe island societies were generally wider than their islands, and where they were not the island population was at risk (depending, of course, on island or archipelago size). Archaeologists have recently begun to appreciate the scale of postsettlement voyaging, and a first phase of investigation is finding increasing evidence for inter-island trade. I think this is only the start and a next phase may reveal that some internal social transformations occurred in the context of external contacts and influences. There are many interesting similarities and differences between island prehistories to consider. Oceans, like continents, are continuous fields. Gamble says, "The

Pacific becomes a surface inscribed with the tracks of voyages. These paths are themselves features that incorporate the ocean landscape into human action rather than leave it as a separate, foreign environment to be simply conquered or traversed."

To digress briefly back to the "long pause," acceptance that cultural diversification can continue in the context of continuing voyaging after colonization reduces a perceived need for cultural pauses during colonization in Remote Oceania. In other words, there was less obligation for people to stop and undergo a cultural change before they could proceed.

It was the decreasing frequency of interisland voyaging that stressed small communities in particular during the second millennium A.D. Kirch finds increasing isolation an insufficient explanation for the abandonment of the "mystery islands" and he may well be right. However, the "extinction line" defined by accessibility to outside contact (p. 175, fig. 67) unambiguously identifies the empty islands that, in many cases, had prehistories quite independent of one another. And that line was moving with changes in voyaging frequency. So, even if isolation was not the *cause* of their abandonment, I would still say that adequate communication was a necessary *condition* of living on islands. Measurements of island or archipelago size and accessibility point to the islands that will suffer other problems.

### **Computer Simulation--Fact or Fiction?**

Finney makes a number of pertinent comments about the nuts and bolts of the computer simulation of voyaging (see also pp. 133-173; Irwin, Bickler, and Quirke 1990). As Keegan comments, all models simplify reality in order to represent it. What gets included and what is left out usually depends on the aims of the experiment. One shortcut we took was to simulate only January and July to represent the seasonal weather patterns for summer and winter. Although we actually had twelve months' weather data, we found we could make our points without having to use them all. Including other months would have generated a very large number of intermediate solutions. However, one arbitrary consequence was that some canoes went sailing for a fifty-day month!

The simulation also selected winds on a daily basis, when in fact weather is continuous. Weather patterns such as low-pressure systems with associated fronts build and move and change over a period of days and their progress can be observed and predicted. One supposes that real navigators would have chosen a weather system to set off on and dealt with others as they came. But ours had no memory of yesterday's wind and no forecast for tomorrow. (Actually it is still hard to get weather forecasts at sea in the trop-

ical Pacific.) So some artificial changes of weather occurred in the computer as one day ticked over into the next. But this approach would not have affected the overall outcome of our experiments, because daily winds were selected according to probabilities based on records by month. Even so, new ventures in voyaging simulation will probably use more-realistic data as there are now computer models of global weather containing immense data (although these usually have inbuilt aspects of simulation themselves). Modeling continuous weather data will allow additional sailing strategies to be investigated. I would expect that if simulated sailors are given more weather intelligence they would be even more successful, not less.

Another pertinent question is whether it was unrealistic to sail virtual canoes over a currentless sea? Yes, it probably was. And one way to measure the effect would be to rerun some simulation experiments that include current data. This would not be hard to do, although conventional current data are generally less fine-grained than those for wind. Our reason for omitting current was that wind is by far the major variable in boat speed. On the other hand, current is an important consideration when navigating to a particular island target. Like leeway, it can offset a boat from its course. Leeway is a property of the boat as it moves; it varies with boat direction relative to the wind and it can be observed. Current relates to place, although its velocity and direction may vary. Finney, elsewhere discussing Andrew Sharp's view that current could not be judged accurately except over short journeys (Sharp 1963:35-37), noted that the "equatorial current that sweeps past most Polynesian islands is a fact of life, knowledge of which was essential to coastal fishermen as well as to inter-island voyagers" (Finney 1979:333). Certainly, current had to be taken into account in navigation. But our simulation was mainly about getting to previously unknown islands for the first time. Precise position fixing applied when getting back to them for a second time.

However, as Finney notes in his review here, the case does become more relevant when we are dealing with a canoe trying to make progress into the east against both wind and current. As he explains, windward progress of a canoe sailing at four knots per hour into a half-knot current could be cut to half a knot, or twelve (sea) miles a day. Anderson makes a similar point (1996a). But we all know the wind does not blow from the east all the time. If it did Thor Heyerdahl would have to be right. There is the intermittent effect of the northwesterly monsoon beyond West Polynesia in the summer, and subtropical weather affects winds in the southern reaches of the tropics at whatever latitude they may be found in any season. Captain Cook found out about these "westerlies" at Tahiti on his first voyage (Beaglehole 1968: 137-139). John Williams, head of the London Missionary Society mission at Raiatea (Society Islands) from 1817 to 1839, gave an explicit account of their

Tahitian names, the months they blew, and their duration. He describes his own experiences, including one voyage from Rarotonga to Tahiti with a fair wind and fine weather all the way (Williams 1837:169). To turn to modern times, in July and August 1986, on her only passage from West to East Polynesia, the *Hokule'a* canoe took ten days from Samoa to the Southern Cook Islands and another ten on to Tahiti. Over the two legs the canoe averaged around sixty sea miles of eastward progress per day, although she by no means sailed a direct route (Finney, Rhodes, and Thompson 1989). This was in the trade-wind season. It was a brilliant example of strategic use of weather, the use of noninstrument navigation, and of dealing with the contingencies that arise at sea. The computer simulation seems to be in the same ballpark as a number of such real-life passages. For comparison with the voyaging simulation, in one experiment of 325 canoes that set off eastward from Western Samoa for eighteen days, 49 reached the Northern Cook Islands and 44 the Southern Cooks; a further 25 got directly to the Societies and 4 to the Tuamotus; none reached the Marquesas (p. 151). In addition, even without current the simulated voyages along the north and south tracks between Hawai'i and the Tuamotus/Societies are generally similar to the experiences of the *Hokule'a* and the remarkable fleet of canoes that traveled north over that route in 1995.

One further comment about sailing east when the wind is in the east: usually it is by no means due east but has a northerly or southerly slant. We could expect canoes to take the best tack, wind shift by wind shift, as real boats do between episodes of other weather.

To turn to more important things, there is little point in getting a bad back crouched over a computer playing with simulations that simply imitate life. We know about real things already. Where simulations help is in making explicit the variables relevant to real life. In this case the simulations did so, by suggesting the kinds of information needed to make the various passages actually made in prehistory and by showing the increasing levels of skill required as colonization extended in space and time. The simulations were also both illuminating and rather subtle in showing that certain voyages were possible that, according to archaeology, were not made. These results offer insights into exploration strategies or, as Gamble says, human choices.

### **Where to Next?**

Kirch comments on how sophisticated anthropological discussion of Pacific origins and voyaging has become. But where do we go next? More archaeology on more islands is needed to produce the data to resolve various issues. Changes to existing models are inevitable. This is probably the most impor-

tant aspect of future work but it will take time. I hope enough new scholars will do their pioneering in the field as well as at the keyboard.

The study of colonization can be expanded to include early subsistence strategies. But we still have to be careful to distinguish means from ends. For instance, I would prefer the motivation for oceanic colonization to lie more in the minds of explorers than in the stomachs.

More computer simulation would be useful and should use a new generation of computer-modeled climatic data. Simulation of water crossing in the voyaging corridor should employ corrections for Pleistocene climate and sea levels. As for colonization of the Remote Pacific, it would be useful to simulate weather patterns over a period of some years. Perturbations such as El Niño need more attention (see Finney 1985). With regard to the different voyaging strategies there could be development of Strategy 5, which involves voyages with indirect returns through intermediate islands rather than return trips from A to B and back. Also, easier targets could be used as waypoints to more distant and difficult targets. However, I doubt that using more-realistic data of wind and current would greatly alter the conclusions to date. But allowing navigators the kind of ability to forecast weather as described by Banks (1962) on Cook's first voyage to Tahiti might produce surprising results. It could make the navigation aspect of inter-island voyages in known ocean even more secure although the passages themselves would be by no means uneventful.

Some informed person might take a modern anthropological approach to voyaging traditions, which often, if not always, have had a hard time in the past. But tradition is a living thing with its own social dynamics. In a similar vein one might consider the structure of social categories, perhaps as reconstructed from proto-language, but I hope we can come up with something more enlightening than two millennia of migrations by ambitious younger brothers.

Motive is an ultimate question, but the extent to which it is an archaeological question is not clear. Just as prehistorians have distinguished ultimate from immediate origins, much the same may be said of motive. At a very basic level colonizing behavior seems to be part of being human. However, there is still a lot more to be said for the Pacific, even if the most useful suggestions made so far have been about what motives or "pressures" to eliminate. Another thing to remember is that voyaging was going on before the colonization of each part of the Pacific and it continued afterwards.

We are so removed from Pleistocene voyaging that its circumstances rather than its events have been perceptible so far. For voyaging in Remote Oceania the evidence is more recent and discernible. As I suggested before, voyaging was an unbroken tradition and colonization was a continuous pro-

cess "embracing a multitude of individuals, events and transformations played out on island after island" (p. 212). And subsequent cultural change should be seen in the context of continuing voyaging although that, too, changed with time.

## REFERENCES

- Allen, J., and S. Holdaway  
 1995 The Contamination of Radiocarbon Determinations in Australia. *Antiquity* 69: 101-112.
- Anderson, A.  
 1994 Palaeoenvironmental Evidence of Island Colonization: A Response. *Antiquity* 68:845-847.  
 1996a Adaptive Voyaging and Subsistence Strategies in the Early Settlement of East Polynesia. In *Prehistoric Mongoloid Dispersals*, ed. T. Akazawa and E. J. E. Szathmáry, 359-374. Oxford: Oxford University Press.  
 1996b Was *Rattus exulans* in New Zealand 2000 Years Ago? AMS Radiocarbon Ages from Shag River Mouth. *Archaeology in Oceania* 31:178-184.
- Banks, J.  
 1962 *The Endeavour Journal, Vols. I and II, 1768-1771*. Ed. J. C. Beaglehole. Sydney: Angus and Robertson.
- Beaglehole, J. C., ed.  
 1968 *The Journals of Captain James Cook on His Voyages of Discovery*. Vol. 1, *The Voyage of the Endeavour, 1768-1771*. Cambridge: Hakluyt Society.
- Bellwood, P. S.  
 1976 Archaeological Research in Minahasa and the Talaud Islands, Northeastern Indonesia. *Asian Perspectives* 19:240-288.  
 1988 *Archaeological Research in South-eastern Sabah, Kota Kinabalu*. Kota Kinabalu, Malaysia: Sabah Museum Monograph 2.  
 1996 Early Agriculture and the Dispersal of the Southern Mongoloids. In *Prehistoric Mongoloid Dispersals*, ed. T. Akazawa and E. J. E. Szathmáry 287-302. Oxford: Oxford University Press.
- Bellwood, P. S., G. Nitihaminoto, Gunadi, A. Waluyo, and G. Irwin  
 1996 The Northern Moluccas as a Crossroads between Indonesia and the Pacific. Paper presented at the International Conference on Linguistic and Culture Relations in East Indonesia, New Guinea, and Australia. Gadjah Mada University, Yogyakarta, June.
- Butler, B. M.  
 1994 Early Prehistoric Settlement in the Mariana Islands: New Evidence from Saipan. *Man and Culture in Oceania* 10:15-38.

- Chappell, J., J. Head, and J. Magee  
1995 Beyond the Radiocarbon Limit in Australian Archaeology and Quarternary Research. *Antiquity* 70:543-552.
- Dodson, J.  
1995 Palaeoenvironmental Investigations of Micronesia. In *The Acceptance of the Outside World on Fais Island in Micronesia*, ed. M. Intoh, 6-15. Yap: Preliminary report submitted to the Historic Preservation Office.
- Finney, B.  
1979 Voyaging. In *The Prehistory of Polynesia*, ed. J. D. Jennings, 323-351. Cambridge: Harvard University Press.  
1985 Anomalous Westerlies, El Niño, and the Colonization of Polynesia. *American Anthropologist* 87:9-26.
- Finney, B., P. Rhodes, and N. Thompson  
1989 Wait for the West Wind. *Journal of the Polynesian Society* 98:261-302.
- Fullager, R. L. K., D. M. Price, and L. M. Head  
1996 Early Human Occupation of Northern Australia: Archaeology and Luminescence Dating of Jinmium Rock-Shelter, Northern Territory. *Antiquity* 70:751-773.
- Gamble, C.  
1993 *Timewalkers: The Prehistory of Global Colonisation*. London: Penguin.
- Golson, J.  
1991 Bulmer Phase II: Early Agriculture in the New Guinea Highlands. In *Man and a Half Essays in Honour of Ralph Bulmer*, ed. A. K. Pawley, 484-491. Auckland: Polynesian Society.
- Graves, M. W., and D. J. Addison  
1994 The Polynesian Settlement of the Hawaiian Archipelago: Integrating Models and Methods in Archaeological Interpretation. *World Archaeology* 26:380-399.
- Green, R. C.  
1991a The Lapita Cultural Complex: Current Evidence and Proposed Models. *Bulletin of the Indo-Pacific Prehistory Association* 11:295-305.  
1991b Near and Remote Oceania--Disestablishing "Melanesia" in Culture History. In *Man and a Half Essays in Honour of Ralph Bulmer*, ed. A. K. Pawley, 491-502. Auckland: Polynesian Society.
- Groube, L., J. Chappell, J. Muke, and D. Price  
1986 A 40,000-Year-Old Human Occupation Site at Huon Peninsula, Papua New Guinea. *Nature* 324:453-455.
- Holdaway, R. N.  
1996 Arrival of Rats in New Zealand. *Nature* 384:225-226.

Irwin, G. J.

1981 How Lapita Lost Its Pots: A Question of Continuity in the Colonisation of Oceania. *Journal of the Polynesian Society* 90:481-494.

1998 The Colonisation of the Pacific Plate: Chronological, Navigational, and Social Issues. *Journal of the Polynesian Society* 107: in press.

Irwin, G. J., P. Bellwood, G. Nitihaminoto, D. Tanudirjo, and J. Siswanto

n.d. Prehistoric Relations between Island Southeast Asia and Oceania: Recent Archaeological Investigations in the Northern Moluccas. In *Papers Presented at the Vila Conference, Vanuatu, July 31-August 6, 1996*. Port Vila: in press.

Irwin, G. J., S. H. Bickler, and P. Quirke

1990 Voyaging by Canoe and Computer: Experiments in the Settlement of the Pacific. *Antiquity* 64:34-50.

Kirch, P. V.

1986 Rethinking East Polynesian Prehistory. *Journal of the Polynesian Society* 95: 9-40.

1988 Long-Distance Exchange and Island Colonization: The Lapita Case. *Norwegian Archaeological Review* 21:103-117.

1997 *The Lapita Peoples: Ancestors of the Oceanic World*. Oxford: Blackwell.

Kirch, P. V., and J. Ellison

1994 Palaeoenvironmental Evidence for Human Colonization of Remote Oceanic Islands. *Antiquity* 68:310-321.

Loy, T., M. Spriggs, and S. Wickler

1992 Direct Evidence for Human Use of Plants 28,000 Years Ago: Starch Residues on Stone Artifacts from the Northern Solomon Islands. *Antiquity* 66:898-912.

Pawley, A. K., and R. C. Green

1973 Dating the Dispersal of the Oceanic Languages. *Oceanic Linguistics* 12:1-67.

Pawley, A. K., and M. Pawley

1994 Early Austronesian Terms for Canoe Parts and Seafaring. In *Austronesian Terminologies: Continuity and Change*, ed. A. K. Pawley and M. D. Ross, 329-361. Pacific Linguistics C-127. Canberra: Australian National University.

Rouse, I.

1992 *The Tainos: Rise and Decline of the People Who Greeted Columbus*. New Haven: Yale University Press.

Sharp, A.

1963 *Ancient Voyagers in Polynesia*. Auckland: Pauls.

Spriggs, M.

1996 What Is Southeast Asian about Lapita? In *Prehistoric Mongoloid Dispersals*, ed. T. Akazawa and E. J. E. Szathmáry, 324-348. Oxford: Oxford University Press.



Spriggs, M., and A. J. Anderson

1993 Late Colonization of East Polynesia. *Antiquity* 67:200-217.

Summerhayes, G. R.

1997 Interaction in Pacific History: An Approach Based on the Production, Distribution, and Use of Pottery. Unpublished Ph.D. thesis, La Trobe University, Melbourne.

Williams, J.

1837 *A Narrative of Missionary Enterprises in the South Seas*. London: J. Snow.