
BOOK REVIEW FORUM

Geoffrey Irwin, *The Prehistoric Exploration and Colonisation of the Pacific*.
Cambridge: Cambridge University Press, 1992. Pp. 240, 83 figs. and
maps, photos, references, index. US\$54.95 hardcover; \$18.95 paperback.

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I FAVORABLY REVIEWED the 1992 hardback edition of this important book within an eight-hundred-word limit in the *American Anthropologist* (Green 1993). It is indeed pleasing to now have it issued in a much less expensive form, which means it will reach an expanded audience. It is also useful to have a forum here for more extended discussion of some of the more controversial issues it raises.

My basic views of the book have altered little. Previous discussions of ancient voyaging in the Pacific have covered a whole range of related topics in this general field (kinds of Oceanic watercraft, their sailing abilities and navigation; experimental and modern revival voyages; computer-based simulated map journeys; and often heroic travels that lived in memory or are recorded in historical sources). But until recently only a few works on these topics have been well informed by archaeological knowledge. That is in large part because much of this knowledge has been recovered in the last two to three decades.

Archaeology provides the context and chronological framework that shapes Irwin's book and allows the whole subject to be tackled anew, in a compact, readable, and instructively illustrated book-length form. What is brought together within these pages is firm control over current information on Pacific

prehistory on the one hand and an adept, critical, and innovative use of two centuries of books, articles, and debate on the subject of its pre-European exploration and colonization on the other. It also draws on the author's and many others' sailing experience within that ocean world and an array of computer-simulated journeys under a variety of conditions frequently encountered there.

In a recent book, coming at things from the opposite perspective but with many of the same themes, Finney writes of "putting voyaging back into Polynesian prehistory" (1994:255-306; see also Finney 1996). In his book Irwin achieves an opposite task over a broader canvas by seeing that theories of Pacific voyaging are firmly grounded within our present archaeological understanding. Thus from the earlier and rather naive theories of deliberate voyaging hither and yon wherever the prevailing hypotheses of settlement and inter-island interaction of that time seemed to require, to the spare and dismissive views of mainly unintentional and accidental voyaging proposed by Andrew Sharp in reaction, the pendulum has finally come to rest on what seems a far more robust and believable version of how the islands of the Pacific were sequentially colonized, and then its ocean utilized thereafter as an interaction pathway.

Gone are the rather romantic accounts evoking ancestral Polynesian "Vikings of the sunrise"; in their place is an explicit model of continuing indigenous exploration, colonization, and subsequent voyaging patterns involving many ethnicities over the millennia from 50,000 years ago to the time of European explorers of the sixteenth through eighteenth centuries. Moreover, the theory of navigation proposed is a developmental one, in which improvements in the strategies practiced by the ancient mariners of the Pacific occur as the watercraft technology improves, the experience and knowledge of their island world increases, and conditions for sailing change as human settlement moves from west to east and thence north and south. The story is complex, subtle, and stretches over centuries for which we still have only the sketchiest details.

Timing and tempo, often partial or missing from most previous studies of Pacific voyaging, structure Irwin's discussion. The first section deals mainly with the theoretical issues of colonization and voyaging, covering the Pleistocene period of Ancient Near Oceania beginning 50,000 years ago, the Lapita horizon of 3,500 to 2,000 years ago, and an outline of the basic strategy and general model for systematic exploration of Remote Oceania within these last few millennia. The book next looks at the archaeological evidence bearing on the settlement within that period of, in turn, eastern Melanesia, West Polynesia, and central East Polynesia, and then of Hawai'i and New Zealand. Micronesia, often ignored as too difficult, gets its own separate treat-

ment. Although a reasonable fit between theory and apparent practice as revealed by archaeology is found, this is further subjected to voyaging by computer simulation, not only to test many of the propositions arrived at in the first section, but also to explore further predictions about what seems to have happened and, importantly, not happened.

While this is what the book does, there are, as is inevitable, possibilities for disputes of interpretation over a number of issues, some of which I wish to raise here. Because they have been the subject of numerous informal discussions between Irwin and myself, both over the years of his books gestation and subsequently, and as colleagues in the same department co-teaching a course on Oceanic prehistory, these differences may prove of wider interest to others wishing to join in our dialogue.

Let me pose the first as a question about the tempo of long-term evolutionary change using the Pacific example. Are the broad outlines of human expansion within the Pacific--a last chapter in world prehistory--able to provide empirical support for either of the two main alternatives in cultural evolution when they are expressed as follows? "Does cultural transformation occur primarily by the accumulation of innumerable slight variations within long established systems analogous to predominant gradualism of Darwinian theory? Or is the principal pattern one of abrupt and episodic change, followed by relatively long periods of stasis, analogous to the theory of 'punctuated equilibrium' in evolutionary biology?" (Durham 1990: 195). In a recent paper on the topic Larson, Johnson, and Michaelson state that "opinions by social scientists vary widely on this matter; however it is commonly agreed that empirical research derived from systematical longitudinal studies that focus on the process of cultural change are essential to the evaluation of these important research questions" (1994:285). Their study of historical change among the Californian Chumash during the missionization period focuses on a rather short-term temporal scale and assumes that in this case the tempo of cultural change is most frequently a mixture of the two alternatives (*ibid.*).

My own focus in two recent essays has been to show that one framework describing human expansion in the Pacific conforms to a step or pause model (Green 1991, 1994), without denying that many other aspects of change within that process, such as the continuous development of voyaging technology and navigational skills, outlined by Irwin (pp. 134, 209), may fit better within the gradualist framework. Thus, a one-page summary of my views on exploration and colonization of the Pacific looks something like that outlined in Table 1. Causation at each step but one seems to have a lot to do with a set of innovative developments in the water-transport mechanisms and sailing techniques and strategies that became available to those

TABLE 1. Human Colonization of the Pacific: Time and Steps

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1. Pacific parts of **Sundaland** first settled 800,000-900,000 and maybe several million years ago. 850,000-year temporal delay before **first** step into:
 2. **Ancient Near Oceania** first settled 40,000 and perhaps 50,000 years ago, or even perhaps 116,000 years ago (Fullagar, Price, and Head 1996).
 3. Development of **Modern Near Oceania**, Island South East Asia, and Australia with additional external cultural inputs in each region 6,000-3,500 years ago. 37,000-year temporal delay before **second** step into:
 4. **Remote Oceania** first settled 3,300-3,200 years ago. Either a 500-800-year (early settlement) or 1,300-1,500-year (late settlement) temporal delay before **third** step into:
 5. **Central East Polynesia** and **East Micronesia** first settled:
 - a. early view 2,200-2,500 years ago
 - b. late view 2,000-1,500 years ago
 700-800-year temporal delay before **fourth** step into:
 6. **New Zealand (Aotearoa)** first settled:
 - a. early view 1,400 years ago
 - b. late view 700-800 years ago
 7. **Antarctic** first explored 19th century. Settled 20th century.
 8. **Space** first explored late 20th century. Settled 21st century?
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peoples who, in ever shorter but successive bursts, settled some of the last remaining land spaces in this world before yet others occupied Antarctica and then moved out into space. This view contrasts somewhat with Irwin's claim that "colonisation was a continuous process without systematic cultural pauses. There may have been geographic reasons for the elapsed time of island settlement, but cultural ones have to be confirmed" (p. 134).

Causation at the initial step of movement out into the western Pacific (Ancient Near Oceania) has been explored by a number of writers (Birdsell 1977; White and O'Connell 1982:46; Thiel 1987; Jones 1989; Green 1994: 22-25). But Clark's 1991 discussion of the subject coupled with Irwin's work on Pleistocene developments within the voyaging "nursery" or corridor (1991; pp. 18- 30) seem to me to constitute the most satisfactory accounts presently available. They have to do with the invention of water transport, ecological circumstances, and people's motivation to explore lands they could see on the horizon.

Things do not seem so clear-cut with respect to the next pause and then step into western Remote Oceania. Here matters turn on how one views the issue of additional cultural inputs between 6,000 and 3,500 years ago within what I have called Modern Near Oceania (Green 1994: fig. 1.2). Despite the views of those who steadfastly hold to the notion of almost complete local development of most things Oceanic within New Guinea and adjacent islands

of the Bismarck Archipelago in the mid-Holocene, there was, in my view, a significant Southeast Asian Austronesian cultural input in the Bismarcks at 3,000 to 4,000 years ago that cannot be denied (cf. Bellwood 1992; Spriggs 1995, 1996a, 1996b; Kirch 1995, 1997). One example of this is in the form of improved water-transport mechanisms. On current information this is best associated with the Lapita horizon, where we first get the very long distance component of Lapita systems of trade and exchange, and the incorporation of Lou Island (and Fergusson) obsidian into those networks. It also forms the basis for and the means underpinning the rapid expansion of highly decorated ceramics in the Lapita style associated with a whole complex of non-ceramic items and features, which are the foundation cultural assemblages within western Remote Oceania (that is, from the Reef/Santa Cruz group through Vanuatu to New Caledonia and the Loyalties on the one hand, and Fiji-West Polynesia on the other).

The improved water-transport mechanisms introduced to Near Oceania at this time comprise the Austronesian watercraft complex as it is reconstructed linguistically for Proto-Oceanic (Pawley and Pawley 1994; see also Horridge 1987:153-163; Green 1991:498). A seemingly slightly later innovation at this stage was the double canoe (Pawley and Pawley 1994:339-340), restricted to the Eastern Oceanic languages, in large part consistent with its later ethnographic distribution and probable time of first employment (Doran 1974:134-135). These additions provided the power (sail types), large stable platform with sufficient carrying capacity (ocean-going double canoe), and development of out-of-sight-of-land navigational skills and strategies (pp. 42-63) needed in the rapid settlement of Remote Oceania.

The next proposed pause and step is that into Micronesia on the one hand, and central East Polynesia on the other. The proposal here for a shorter but still significant pause has caused continuous controversy (Terrell 1986: 81-87). First there is the matter of its timing, where Irwin seems inclined to the early view of not just exploration, but established settlement, in the zone from the Southern Cook Islands to Tahiti and the Marquesas 2,000 to 2,500 years ago (pp. 80-82, 215, and figs. 24 and 83). A considerable literature has since grown up over this matter of timing in respect of the first established and continuing habitation with obvious impact on the landscape in the Southern Cooks, the Society Islands, and the Marquesas (Spriggs and Anderson 1993; Kirch and Ellison 1994; Anderson 1994, 1995, 1996a; Anderson et al. 1994; Peters 1994; Lepofsky, Kirch, and Lertzman 1996; Kirch 1996).

Certainly on the Tahitian evidence it seems that a period before A.D. 600 (1,600 years or more) is reasonably well attested to by means of a lake-based pollen record, presence of directly dated semidomesticated coconuts, and the initiation of a well-dated sequence of human-induced landscape change

(Lepofsky, Kirch, and Lertzman 1996), together with a long and continuous archaeological sequence for human habitation (Green 1996). In the Cook Islands and the Marquesas, one or another of those components are not entirely synchronous or simply not yet adequately documented, so disputes have arisen over how to interpret each of the different lines of evidence (Rolett 1996:535-538). The maximal date range for any line of evidence, however, lies in Irwin's estimate of 2,500 to 2,000 years ago.

All of the evidence adds up to indications of a considerable time interval, if not pause, before human settlement was established in central East Polynesia. Linguistically in Polynesia I can see no way out of the need for a culturally based, as well as perhaps a geographically induced, pause of many centuries in length (Pawley 1996; Marck n.d.). Thus, even allowing 200 to 300 years for the initial Lapita expansion into Remote Oceania of 3,200 to 3,300 years ago, followed by another 400 years of continuing voyaging to explore and find landfalls in central East Polynesia, does not take us much beyond 2800-2600 B.P. This leaves some 700 to 800 years at a minimum to be accounted for before a colonization event in the form of established settlement may be reasonably postulated at present for central East Polynesia, even on the most liberal interpretation of the available archaeological and environmental evidence. The conservative view requires an even longer interval (Spriggs and Anderson 1993). For these reasons I too strongly support Pawley's view that the linguistic evidence does indeed indicate "a long delay after Western Polynesia was settled, before the effective colonisation of East Polynesia" (1996:404).

The same timing of circa 2,000 years ago is also suggested by the eastern Micronesia evidence. But here the archaeological evidence of its settlement from western Remote Oceania at that date is rather more convincing (Intoh 1996). It is therefore the parallel delay in the effective settlement of both areas that must be explained. Although Pawley says he is willing "to leave it to others to work out why there should have been such a lengthy pause . . . in West Polynesia" (1996:404), for me that situation will simply not do. Irwin and I have often discussed possible causal factors, being reasonably in agreement that these factors probably did not involve major changes to the then existing transport mechanisms or methods in searching for or exploiting new lands. Moreover, he has continued working on the problem since the publication of his book, and I know he may now be nearing one possible solution. That would be a most welcome development, even if we cannot as yet pin down the exact timing of the event itself, as I think we both find unconvincing proposals such as those of Anderson for an adaptive change in sailing and subsistence strategies (1996a).

Irwin's explanation of the need for a later development in Polynesia of a

further strategy of viable “across and down the wind” voyaging as the reason underpinning the delay in the settlement of Aotearoa (New Zealand) and Hawai‘i seems to me particularly well motivated. Exploration of Hawai‘i may extend back into the earlier part of the first millennium A.D., but ecologically and archaeologically well-established settlement does not seem to occur until after A.D. 600 to 700 (Graves and Addison 1995; Athens and Ward 1993:219 and n. 1). It is just possible that discovery and exploration of New Zealand (from New Caledonia/Fiji or West Polynesia) occurred at about the same time East Polynesia was also first visited (Holdaway 1996; Anderson 1996b), but it seems that truly effective settlement of those temperate, continental-type islands may have been only a few centuries older than the currently archaeologically well-attested dates in the twelfth to thirteenth centuries A.D. (Anderson 1991). Going on the pollen, charcoal, and sedimentary record, settlement may be as much as one or two centuries earlier in the warmer northern zone of the North Island (Elliot et al. 1995). The initial ecological adaptations required for such tropical East Polynesian societies to establish themselves in Aotearoa are well attested (Green 1975; Davidson 1984); what is not so often appreciated are the semantic linguistic innovations that also occurred at the time of effective initial settlement (cf. Biggs 1991). In my view this geographically imposed need to adapt also had a major impact on later voyaging technology and the maritime skills and strategies exhibited by the contact-period Maori. Irwin does not really discuss this in his otherwise fine chapter on “voyaging after colonisation and the study of culture change,” but such a sketch could have been included with profit (cf. Green 1975: 608-609).

In summary, then, the broad temporal outlines of Pacific settlement are in my view now reasonably established. This was not so a decade ago. Granted, at nearly every step some argument obtains (and probably will continue to do so) over the precise timing of that particular event, but the overall framework outlined in Table 1 now provides a reliable guide to the kind of tempo involved in the colonization process. It is my claim here that one pattern that can be detected within that process is more akin to the punctuated or ramp model of evolutionary development rather than the one of slow, steady change during a gradualist movement eastward from an Asian source. Moreover, the tempo of the overall pattern conforms to a log-linear model where the pauses grow significantly shorter as the distances covered grow longer. At the same time plausible cultural, as well as geographical, explanations for each pause and then further rapid expansion are now being developed and debated. Yet it would be wrong to overlook other aspects in the colonization process that may conform more closely to continuous or gradualist process. Both alternatives are in fact likely to be involved,

differentially displayed by the various domains that make up the full range of processes involved in establishing new migrants in both previously uninhabited as well as already inhabited landscapes.

In this respect, one of the most contentious issues raised by Irwin is that of Lapita (pp. 31-41), to which he devotes a separate short chapter. His view is that the term covers "an uncertain and variable archaeological category," which "does not begin to approach an ethnic category except in just a few archaeological sites where the data are under reasonable control" (p. 34). Such cases are stated to be rare and their connections undefined. Yet Irwin concedes that in western Remote Oceania "Lapita more plausibly approaches an ethnic category because it can be shown to be associated with a maritime tradition in a rapid and integrated burst of colonisation, which is likely to have been the first in the region. But there is still no telling how representative a Lapita 'culture' in the remote Pacific was of Lapita in the region it left behind, and it is too soon to say to what extent the deep-sea Lapita colonists were themselves a homogeneous human group" (p. 34).

In Irwin's view, therefore, the uncertain archaeological category of Lapita has "no precise biological or linguistic identification," though in Remote Oceania, as noted in the quote above, he has certainly long seen it as representing "a largely integrated episode of colonisation" (p. 38). For these reasons he seems to favor Terrell's suggestion that only east from Fiji might it be "safe and historically appropriate to speak, biologically and culturally, of a Lapita people who spoke an Austronesian language" (1989:625), while allowing for the possibility in the Remote Oceanic islands to the west of there.

These statements, of course, contrast rather markedly with the data-rich and carefully argued studies of Bellwood (1989a, 1989b) and of Pawley and Green (1973, 1984) for a linguistic and biological identification of different regional aspects of Lapita with various Oceanic Austronesian language subgroups as well as of Proto-Oceanic with Lapita in general, made well before the first publication of this book. The views also differ substantially from those of Green (1997), presented since Irwin's book was published, for both a fairly precise linguistic and biological affiliation of Western Lapita with an Eastern Oceanic linkage or cluster of languages and with a pre-Polynesian biological population in western Remote Oceania (that is, to the west of the Fiji-West Polynesia region, where such an equation is not currently in dispute). And the statements contrast as well with the most recent views of Pawley and Ross for a reasonable case of linguistic correlation of Lapita with Proto-Oceanic itself (1993, 1995), which would include the well-defined Lapita sites with rich assemblages in Near Oceania as part of the equation. Finally, they stand in almost direct opposition to the position forwarded in Kirch's new book on *The Lapita Peoples* (1997:79-117), where the linguistic

and biological cases are argued in some detail from a now-ample literature on these topics. Here a "peoples" concept is adopted for the whole region from Fiji-West Polynesia to the Bismarck Archipelago (cf. Kirch 1997:15-18 and n. 24).

As I currently view the matter, Pacific archaeologists are attracted toward one of two positions over the issue of Lapita and its interpretation. For one group the term has a poor to nil prospect of ever embodying varied ethnogenetic, biological, or linguistic characteristics. Those studies attempting to establish such identifications are deemed speculative or unprofitable, if not impossible. This is especially so when the Lapita constructs with which they are being equated are viewed as not constituting a culture, cultural complex, horizon, tradition, or any other similar archaeological unit in common use. On this point Irwin offers as an alternative the suggestion of Terrell (1989) and Hunt (1989) that Lapita "is made up of what could be seen as the elements of trade, at least in Near Oceania" (p. 34). Usually this group of Pacific archaeologists also holds that the history of a language family may actually tell us little or nothing about the history of the people speaking that set of languages, and neither archaeology (Smith 1995a, 1995b) nor material culture (Welsch, Terrell, and Nadolski 1992; Welsch and Terrell 1994; Welsch 1996) are able to be correlated with or used in testing these linguistics-based constructs. Analytical attempts along these lines are considered neither productive nor informative avenues of inquiry.

A second, fully interdisciplinary position sees the endeavor of establishing correlations as a necessary product of a holistic anthropological approach to history, one that is certainly fraught with difficulty, but one in all its complexity certainly well worth pursuing by employing an abundance of data not usually discussed in any detail by the other group. For Lapita, Kirch's 1997 study exemplifies this second position, but it is possible to find supporting arguments and analyses for correlations of archaeological information, material culture, linguistics, and biological data in the work of Bellwood (1989a, 1989b, 1995, 1996a, 1996b), Moore, Roberts, and Romney (Moore and Romney 1994, 1995, 1996; Roberts, Moore, and Romney 1996), Pawley and Ross (1993, 1995), Spriggs (1995), and Pawley and myself. Along with Sutton, this group takes a position in contrast to the other that it is possible and necessary to deal critically and in depth with the problems involved in employing these other means for making constructions of the past, so that "archaeology can actually test them and must" (Sutton 1996:382).

Irwin, in this excellent book, has dealt critically with the problems involved in making constructions about the history of Pacific voyaging and colonization from a point of view encompassing several decades of historical accounts, ethnographic studies, experimental voyages, and computer simula-

tions. And he has productively tested these constructions against the current archaeological evidence. Here he clearly adopts a strategy in line with the second group's position; on the topic of Lapita it is less certain that this is the case, as the issues involved are not in my view sufficiently explored by him for what we both agree is an important homogeneous voyaging and colonizing event in Pacific history. Where we differ is that I see Lapita as constituting a range of acceptable archaeological categories that function with greater degrees of ethnic, linguistic, and biological salience than he so far has been prepared to allow.

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