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Geoffrey Irwin has written a splendid book on what is surely the longest-standing and perhaps the most contentious issue in Pacific anthropology: the question of how and why the far-flung Oceanic islands were discovered and settled by the ancestors of the indigenous peoples who now occupy them. Generations of explorers, missionaries, and scholars have formed strongly held opinions on these questions. Cook, Banks, the Forsters, and other late eighteenth-century explorers encountered Oceanic peoples who practiced inter-island and interarchipelago voyaging in impressive watercraft, and these explorers formed the opinion that Pacific colonization had been from the west through a long series of purposive voyages. But indigenous watercraft technology soon succumbed to the impact of Western intrusion, and esoteric navigational lore was also rapidly lost. Nineteenth-century missionaries began to question whether the settlement of the Pacific had not

been accomplished merely by chance voyages of drift. At the end of the nineteenth and in the early twentieth centuries the pendulum swung back, with scholars such as Fornander, Smith, Best, and Buck who drew upon (and sometimes elaborated) Polynesian oral traditions invoking images of impressive migratory feats: the 'Vikings of the sunrise.' After World War II, Heyerdahl and Sharp challenged anew the purposive voyaging models with new theories of drift voyaging, from east and west origins.

In the past quarter-century, research on the problem of Oceanic voyaging, discovery, and colonization has been invigorated by new approaches. Modern, stratigraphic archaeology has finally come into its own in the Pacific, providing "hard" settlement dates and direct evidence from initial or early settlement sites in several archipelagoes. High-speed computers have allowed for extensive simulations of voyaging and drift probabilities (Irwin himself extends this approach). Surviving indigenous navigational traditions, in Micronesia and the Polynesian Outliers for example, have been ethnographically documented. Most dramatically, replicated voyaging canoes such as *Hokule'a* and *Hawai'iloa* have made long-distance voyages navigated without instruments over thousands of kilometers, carefully assessing and documenting the conditions, possibilities, and limitations of this mode of transport.

Irwin's book brings together all of these new approaches for a fresh assessment of this long-standing debate. Irwin combines the perspectives of a seasoned archaeologist and of a deep-water sailor who has experienced, from the perspective of the washstrakes what it is like to cross the long seaways from Fiji to Vanuatu, or to navigate the reefs and cays of the Massim. His basic argument is succinct and elegant: in the Pleistocene "voyaging nursery" extending from Wallacea to the Bismarck-Solomons chain, early Pacific peoples gradually accumulated considerable technological and navigational skills. When they finally "burst out" into Remote Oceania about 3000 B.P. (with the Lapita expansion), they had developed a "survival sailing strategy" of upwind searches for new land, assured of a high probability of safe return on the prevailing winds and currents. Only relatively late in the sequence of island colonization did they deviate from this strategy, to attempt the "less safe" across-the-wind voyages that would lead to Hawai'i, for example, or the outright dangerous downwind voyages out of the tropics necessary to find New Zealand and the Chathams. Thus Irwin adds *method* to *intentionality* in modeling ancient voyaging practice. Irwin's book elaborates and explores this elegant model in considerable detail, and I find his arguments generally compelling. As a prehistorian, however, it is to some of the implications for *archaeology* raised by his model that I shall devote the thrust of my remarks.

Archaeologically, the onset of the second great episode in Pacific coloni-

zation--that which would lead to the settlement of virtually every habitable island--is closely associated with the Lapita cultural complex. Irwin calls Lapita an "uncertain archaeological category," although I would argue that recent advances in Lapita archaeology have made it quite firmly definable (Kirch 1997). One issue raised by Irwin is the dating of the first spread of Lapita sites, for this bears on the rate of human dispersal, and in turn on the nature of voyaging and discovery strategies. He suggests that the initial Lapita dispersal out of the Bismarcks as far south as New Caledonia and as far east as Tonga and Samoa might have taken 500 years, or some twenty human generations. Substantial numbers of new radiocarbon dates from Lapita sites as well as advances in calibration now allow a refinement of this chronology (Kirch 1997:57-63). While the earliest Lapita sites in the Bismarcks date to around 1550-1400 B.C. (calibrated), the expansion eastwards almost certainly did not get underway until at least 1200 B.C. Then, within two centuries at most (perhaps as few as eight to ten human generations), Lapita communities were established through the southeastern Solomons, Vanuatu, Loyalty Islands, New Caledonia, Fiji, Tonga, and Samoa. (See, for example, the clarification of the initial settlement date for the Koné Period in New Caledonia, provided by Sand [1995:73-76].) This is a truly remarkable expansion on the scale of world prehistory. As Irwin correctly observes (p. 39), this chronology cannot possibly be explained by "ecological or demographic pressures." A "pull" rather than "push" model of migration is required (Anthony 1990). If anything, this expansion must have required a very high birthrate or some means of recruitment of people (a distinct possibility within the long-occupied Bismarck-Solomons region), or both, simply to "fuel" such a rapid expansion. Most important for the theme of Irwin's book, the Lapita peoples clearly were in possession of both the technology for long-distance open-ocean travel, as well as a sailing strategy for safe and successful upwind exploration.

An issue that Irwin sidesteps to some extent is the cultural origins of that technology and sailing strategy. In the preceding chapter he discusses the importance of the Pleistocene "voyaging nursery" lying north of New Guinea and extending into the Solomons. However, the sudden appearance of the Lapita complex in the mid-second millennium B.C., and its extremely rapid and widespread expansion shortly thereafter, have convinced many archaeologists that it reflects an intrusion into Near Oceania of one or more groups of Austronesian-speaking peoples. In this regard, it is significant that historical linguists have reconstructed a large suite of Proto-Austronesian and Proto-Oceanic terms for a sophisticated outrigger and double-hulled sailing canoe complex (Pawley and Pawley 1994). Irwin's "voyaging nursery" is an important concept with regard to Pleistocene movements in Near

Oceania, but surely it was the introduction to this corridor of the Austronesian outrigger sailing canoe in the second millennium B.C. that rapidly allowed for the subsequent expansion of humans throughout Remote Oceania.

The western Polynesian archipelagoes of Tonga and Samoa, as well as the more isolated islands of Futuna, 'Uvea, and Niuaotupapu, all had Lapita communities established no later than the end of the second millennium B.C. Indeed, recent work by David Burley in the Ha'apai group of Tonga suggests a highly structured colonization pattern of establishing small (hamlet-sized) Lapita settlements on virtually every islet of habitable size (pers. com., 1996). And although classic dentate-stamped pottery has yet to be recovered there, the Manu'a group in eastern Samoa was also inhabited by 1000 B.C. Then, in spite of the fact that both the Northern and Southern Cook Islands to the east are closer to Samoa-Tonga than Fiji is to Vanuatu or the Solomons, the Lapita "bullet train" seems to have come to a screeching halt. Or did it?

Nothing has been more contentious in Pacific archaeology in recent years than the debate surrounding the chronology of settlement in central and eastern Polynesia. Archaeologists for some years have had to contend with the problem of a "long pause" between the Lapita colonization of western Polynesia and the expansion into central-eastern Polynesia. But with the application of "chronometric hygiene" to the corpus of available radiocarbon dates from eastern Polynesian habitation sites (Spriggs and Anderson 1993), this pause has been widened to a yawning gap of as much as 1,500 years! Others, including myself (Kirch and Ellison 1994), are extremely dubious that the interval between human settlement of Tonga-Samoa and the initial movement on into central Polynesia (e.g., the Cooks-Societies-Australis region) was much longer than about 500 years. One objection, on strictly theoretical demographic grounds, is that an ultra-long pause (or "short chronology" scenario) would require a huge population burst to fuel such a vast migration in a very short time period, a much greater demographic increase than that driving the documented initial Lapita expansion into Remote Oceania. In archaeological terms, the problems are largely methodological and concern such issues as sampling error, geomorphological constraints on site visibility (e.g., submerged coastlines and substantial valley in-fillings), acceptability and calibration of radiocarbon dates, and the validity of proxy measures of human presence on islands, particularly evidence for vegetation disturbance and anthropogenic fire regimes derived from sediment cores and pollen analysis (Kirch and Ellison 1994).

Irwin's "continuous settlement model" for East Polynesia (see his fig. 30) is critical in this debate, and in my view reflects the closest approximation to what we may expect to find archaeologically. In his model, there was "no sys-

tematic delay" in voyaging eastwards out from the Samoa-Tonga region, and his prediction is that initial Polynesian discovery of the Southern Cook Islands was likely made by about 500 B.C. (see his fig. 24). Rather than a sudden "explosion" throughout central-east Polynesia (as required by the Spriggs-Anderson short chronology model), Irwin envisions a continuous but somewhat slower progression down the Austral, Society, and Tuamotu alignments, ultimately reaching as far as Mangareva, Pitcairn, and Easter. Only after this region had been thoroughly explored and settled would the more difficult voyages be made "across the wind," leading to the discovery of the Line Islands and beyond them the great Hawaiian archipelago, probably early in the first millennium A.D. The last of the Polynesian lands to be discovered, in Irwin's view, should have been New Zealand and the Chathams, given that they are both downwind and out of the tropics.

There is considerable new archaeological and paleoenvironmental evidence to support Irwin's continuous settlement model, although not all of my colleagues concur. Most notably, in the Southern Cooks our extensive coring on Mangaia demonstrates an unmistakable human presence on the island probably as early as 2400 B.P. and certainly no later than 1600 B.P. (Kirch 1997). Significantly, the Mangaia dates closely matched Irwin's predictions for the Southern Cooks (see his fig. 24). Mangaia also illustrates one of the problems of detecting early settlements, as the mid-Holocene paleosols of the valley floors (the most desirable locations for house sites) are now buried by up to six meters of clay in-filling. On Mo'orea in the Society Islands, Lepofsky, Kirch, and Lertzman (1996) also document massive in-filling and coastal progradation of precisely the sort pointed to by Kirch (1986) as a likely problem in locating early settlements in that archipelago. In the Mo'orea case, early domesticated coconuts and other signals of human presence date to A.D. 600, earlier than any as-yet-discovered actual habitation sites in the Society Islands.

A major problem with archaeologically assessing Irwin's model, however, is the almost complete lack of prehistoric sequences for the Austral, Tuamotu, and Gambier archipelagoes, one of the most likely corridors of upwind exploration. Weisler (1995) has firmly documented Polynesian occupation of remote and ecologically marginal Henderson Island by A.D. 900 (if not 100 to 200 years earlier), but we would hardly expect to see a permanent colony on Henderson until some centuries after the main Austral-Gambier chains had been permanently settled. The initial settlement date for Easter Island remains enigmatic, although the dates on a remarkable extinct avi-fauna recovered from Anakena by Steadman, Vargas, and Cristino make it clear this must have been sometime prior to A.D. 900 (1994). Flenley has recently published a new pollen core from Rano Kao on Easter Island (1996),

with a radiocarbon age suggesting anthropogenic disturbance beginning at 1630 ± 130 B.P. (A.D. 147- 676), which fits well with the Mangaian evidence. The fundamental point is that the current debate between "long" and "short" chronologies--and the testing of Irwin's continuous settlement model--will never be resolved on the evidence presently at hand. We urgently need much more archaeological exploration in the Australs, Tuamotus, Manga-*reva*, and even the Societies, and such work must be both geomorphologically sophisticated and go hand-in-hand with interdisciplinary paleoenvironmental research.

New Zealand provides an especially intriguing case. Irwin argues that reaching Aotearoa "involved a more complex voyage across the trade winds, through a belt of variables to the latitude of prevailing westerlies" (p. 105). In his overall model of sailing strategies, it should have been the last sector of Polynesia to be colonized, and indeed, the orthodox archaeological view is consistent with this, initial settlement generally being put at A.D. 1000 or after (Spriggs and Anderson 1993). Yet in the chapter of his book on computer simulations (certainly one of his most innovative contributions), Irwin also observes that New Zealand could readily have been reached from islands other than the Southern Cooks, the point of departure presumed in most scenarios. Indeed, New Caledonia is its closest large neighbor, and ten simulated canoe voyages originating from La Grande Terre all made it safely through cyberspace to successful landings on the North Island. Irwin is characteristically cautious about the implications of his analysis, saying only that "voyaging considerations suggest a wider range of possibilities" than an exclusively eastern Polynesian origin. In this regard, the recent publication of AMS radiocarbon ages on *Rattus exulans* bones from New Zealand (Holdaway 1996), dating to ca. 2000 B.P., must be noted. *R. exulans* accompanied Lapita voyagers and later Polynesians throughout the Pacific, and can hardly have swum to New Zealand, although Anderson would have them arriving in drifting canoes whose crews had already perished (1996). While I would agree that the settlement chronology for temperate South Island is unlikely ever to be pushed back beyond A.D. 1000, I am still not so certain about the subtropical parts of the North Island. Sutton's (1987) provocative claims for a longer period of occupation there than the current orthodoxy allows may require a rehearing.

An issue that Irwin alludes to in passing but does not take up in any detail is the relationship between archaeological and linguistic models for Polynesian settlement. However, his voyaging models give cause to rethink some of the correlations between archaeological and linguistic scenarios. In the 1960s, the discovery of early sites in the Marquesas Islands was combined with a linguistic subgrouping model that derived the modern East Polynesian lan-

languages from two main stocks (Tahitic and Marquesic) to suggest that the Marquesas were a “primary dispersal center” for eastern Polynesian settlement. But the linguistic evidence in and of itself never provided an intrinsic reason to think that the Marquesas archipelago was the geographic location of Proto-Marquesic! It could just as well have been in Mangareva or in one of the other Austral Islands, most of whose languages or dialects have gone extinct without ever being adequately recorded. (In this regard it is noteworthy that Irwin’s voyaging simulations indicate the likelihood of the Marquesas being directly settled from western Polynesia as “a long shot” [p. 151].) Recent work by Marck now suggests that the Polynesian subgrouping model is itself in need of revision (1996; and pers. com., 1996), with Proto-East Polynesian being derived from Proto-Nuclear Polynesian via a Proto-Ellicean interstage that includes Tokelau and certain Outlier languages as witnesses. The Ellicean-Tokelau-Outlier linkage might lead one to favor a northern (atoll) route into eastern Polynesia. I think we must be cautious, however, of immediately assuming that an exclusively Northern Cooks-Societies route into East Polynesia is required by the linguistic phylogeny. It is entirely conceivable, as Irwin’s “close proximity analysis of mutual accessibility” (see his fig. 50) demonstrates, that the exploration and colonization routes into East Polynesia were multiple. From the linguistic viewpoint, I can only urge that the as yet mostly undocumented but clearly distinct language of Mangaia be studied by some enterprising linguist before the opportunity is lost. It is nonsense to continue to take Rarotongan as the exclusive witness for what was clearly a highly diverse linguistic situation in the Southern Cooks.

One of the tantalizing issues in Pacific voyaging also taken up by Irwin is the South American connection. Heyerdahl drew world attention to the matter with the *Kon Tiki* voyage, but Irwin rightly turns the question around to ask what was the likely success rate for voyages originating in East Polynesia to arrive on the South American coast, and to return again to their home islands. It turns out that success rates between 50 and 70 percent are achievable, and “controlled Strategy 4 returns to Easter Island and Sala y Gomez brought survival up to 95%” (p. 163). Given the radiocarbon-dated presence of the South American sweet potato (*Ipomoea batatas*) in Mangaia at 900 B.P. (Hather and Kirch 1991), it is no longer an issue whether contact between East Polynesia and South America was made. Irwin’s analyses, however, greatly strengthen the probability that it was Polynesians and not South American Indians who were the transferrers of this important crop plant, as Douglas Yen indeed averred (1974:264-267).

The core of Irwin’s book deals with Polynesia, but he does not entirely neglect the western Pacific. A chapter devoted to Micronesia begins by pre-

senting the results of computer voyaging simulations, from which it is clear that relatively high successful rates could be had for reaching the Marshalls-Kiribati chains from an origin point in the southeastern Solomons, such as the Reef Islands. Voyages originating in the central Solomons (e.g., Malaita) could readily reach Kosrae and other central Caroline islands. At the time Irwin was writing, archaeologically based chronologies for Micronesia were either contradictory (as in the case of Palau and the Marianas) or very preliminary (as for Chuuk, Pohnpei, and Kosrae). The last few years have seen an outpouring of new archaeological data for this region, however, and we can now be a bit less tentative and cautious than Irwin was inclined. For the central Caroline high islands, in particular, initial colonization around 2000 B.P. is increasingly well established, and the highly probable point of origin is one or more late Lapita communities in the Solomons region (Kirch 1997: 74-77). The earliest ceramics in Pohnpei and Kosrae consist of sand-tempered plainwares that are almost indistinguishable from late Lapita plainwares found at sites ranging from Mussau to Tikopia. Here is a case where the linguistic, archaeological, and voyaging models all converge robustly on a scenario of central-eastern Micronesian settlement out of island Melanesia some two millennia ago. The situation in western Micronesia is more complex, and the probability of multiple origins out of the Philippines or even Taiwan remains high.

Irwin also applies voyaging models to the period following initial colonization, for example to the Polynesian Outliers and the so-called mystery islands of Polynesia. While I concur with his analysis of the Outlier situation, I am more skeptical of his conclusions regarding the abandonment of the roughly twenty small islands that are sometimes labeled "mysteries." Irwin's argument is that they were abandoned primarily because they are "less accessible," and he adduces a graph of distance and target angle (his fig. 67) as supporting evidence. But I fear that Irwin may be mistaking a correlation for causation, for it remains the case (as he rightly notes) that these islands are also characterized by small size and by ecological marginality (limited water, poor soils, or other resource limitations). Irwin argues that when long-distance voyaging began to decline throughout Polynesia, these islands were the first to be affected. Weisler's research on Henderson Island (1995), however, has revealed a continuous human population on that tenuous habitat between A.D. 900 and A.D. 1650. Weisler believes that eventual abandonment was linked to severe ecological and sociopolitical transformations in Mangareva, to which Henderson was vitally linked by a complex, long-distance interaction network. I suspect that a similar scenario might hold for Nihoa and Necker in the Hawaiian chain; but more work there will be required to test this model. In short, while Irwin's accessibility model adds

to our understanding of the “mystery islands,” I find it an insufficient argument.

Finally, Irwin’s book also draws attention to the matter of continued interisland and interarchipelago voyages long after initial colonization and therefore to the question of the influence such contacts may have had on culture change and differentiation among the Polynesian societies. Again, this is a complex issue, at the heart of current intellectual debates within anthropology regarding phylogenetic versus reticulate models of cultural change (Bellwood 1996). Of course, such models need not--indeed, should not--be mutually exclusive, and Kirch and Green explicitly included contact between islands as a “mechanism of divergence” among what are nonetheless genetically (that is, phylogenetically) related groups (1987). In providing what he calls an “independent navigational theory of Pacific colonisation and change,” Irwin now augments the tools available for investigating the extent to which contact may have acted either to encourage, or in some cases to minimize, divergence.

Geoffrey Irwin’s book demonstrates how much more sophisticated and nuanced anthropological discussions of Pacific origins and voyaging have become. In an age of increased subdisciplinary specialization, he also reminds us of the great strength that a truly holistic anthropology commands: an anthropology willing to bring to bear methods, evidence, and theories from archaeology, ethnography, biological anthropology, linguistics, and even from related disciplines. It is in the continued application of such an approach that Irwin’s models will be tested, and our ultimate understanding of Pacific history thereby advanced.

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