

# Colonizing Continents: Some Problems from Australia and the Americas

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## INTRODUCTION

The end of the Pleistocene saw two great thrusts of colonizing *Homo sapiens*, radiations that affected the peopling of the new worlds: the Americas and greater Australia (Sahul). Excepting the more recent exploration and settlement of the Pacific Basin, these two emigrations from Asia completed the discovery, reconnaissance, and colonization of the Earth's landscapes. This was done by human populations of foragers with Paleolithic implements, hunter-gatherers who abandoned East Asia and became more or less isolated experiments in human evolution. Hosts of questions regarding the how and when of these great episodes in prehistoric migration have exercised the minds of archaeologists and paleoanthropologists in the respective hemispheres equally, if somewhat differently. It has been recognized (Shutler 1983) that some comparison of the two phenomena would be enlightening. Lynch (1974) was enthusiastic about what the then emergent picture of Australian prehistory might mean for Americanists, and Hallam (1977) saw value in pursuing comparisons of the technology, biogeography, and colonizing demography of the regions. Most recently Kelly and Todd (1988:240) have called for renewed consideration of these same important features of the process of colonization. They have suggested a number of useful and testable archaeological implications bearing primarily on Paleoindian subsistence and settlement dynamics, which, as they indicate, may be enriched by comparison to Australia.

Any comparison of the colonization of the two new worlds must, at least initially, be inspired by some seductively simple and obvious (and possibly misleading) parallels between the known prehistories of America and Australia. For instance, both new worlds were apparently colonized at



about the same time (very late to terminal Pleistocene times). Both colonizing populations derived from approximately the same source, East Asia. Both populations carried a relatively simple Paleolithic technology. Both immigrating populations met a naive and sometimes wholly unfamiliar fauna, and both would write new chapters in their ethnobotanies. Once landed, both populations appear to have left clear archaeological signatures (with impressive alacrity) in the far-flung corners of their new domains. If these parallels seem overly simple, then so too will seem the apparent lines of divergence in their biological and cultural histories. One population, the Australians, crossed a barrier (Wallacea), while the Americans made use of a land bridge (Beringea). The American prehistoric record is replete with highly distinctive regional cultural experimentation, while the equally successful strategy for survival in Australia appears to have been a much more conservative one, at least in the limited sphere of technocomplexes and material culture. In the end, the Americas supported a comparatively high population density and Australia mustered an arguably low one. Even put crudely and in an oversimplified manner, as I have purposefully done in this introduction, the similarities and contrasts between Australia and the Americas serve a purpose; they draw our attention to the general issues surrounding the process of continental colonization. These issues, such as foraging behavior, settlement practices, material culture assemblage composition, and so forth, I would argue, all need to be investigated not only as cooccurring phenomena, but as variable expressions of how living and growing populations became dominant elements of continental faunas.

In an incisive chapter entitled "On Theories and Models", Richard Levins (1968) reminded us that generality, along with precision and realism, were our three goals in conducting effective investigation. He also cautioned us that it is not possible to maximize for all three of these goals concurrently. Considering the history of research in Australia and the Americas, it might fairly be said that archaeologists in the two regions have sought a prime measure of precision, through cultural historical description, with somewhat less attention paid to the realism of biocultural dynamics or the construction of theory-based general principles. Precision, as I have suggested, would appear to be the front-runner and in no immediate danger of being overtaken by our other goals. Given our current understanding of the archaeological record of the Americas and of Australia, contributions that seek to link, in the manner of middle range theory (*sensu* Raab and Goodyear 1984), what is generally identifiable and somewhat precisely known will add a needed measure of realism to our reconstructions of prehistory. In the remainder of this chapter, I attempt to identify the important elements of the process of continental colonization, I critically review key elements contributing to fundamental differences and similarities in the two great colonizing episodes, and I suggest a perspective that may offer some preliminary help in generating a more realistic model of continental colonization. Finally, I conclude that Australian and American archaeologists do indeed have valuable messages to communicate to each other.

## Origins of the Australians and of the Americans

Recent volumes (Kirk and Thorne 1976; Kirk and Szathmary 1985) summarizing the genetic, linguistic, morphological, and archaeological evidence for the sourcing of immigrant population to the Americas and Australia indicate a northeastern and a southeastern origin, respectively. In both cases, complications arising from uncertainties in founder population size, numbers of subsequent colonial episodes, and limited samples of early material make exacting reconstructions difficult. For Australia, Birdsell (1957) saw multiple colonizations in blood group and morphometric studies. Thorne (1971) proposed a counterintuitive early-gracile late-robust complex in skeletal morphology, while Brown (1981) has argued convincingly for unity in the Australian population with artificial skull deformation practices masquerading as expressed genotypic difference.



In the Americas, pulses or episodes of major gene flow have marked at least three population-level introductions of people. Still, in America and in Australia there are clear indications of Asian origins, elements of which, such as certain dental morphologies, are shared. Importantly, both founding populations were modern *H. sapiens* and presumably had similar foraging, colonizing, and other abilities. Thus, we have reasonable grounds for expecting comparability in many details of their early colonial history, such as time of arrival, postarrival distributional history and strategy, relations with foraged resources, and perhaps even population growth characteristics.

## DATING COLONIZATION OF THE AMERICAS AND OF AUSTRALIA

The question here is one of the dating of the first successful colonization of the continents. We can conceive of at least two types of unsuccessful "colonizations"; one, which we might term a *landfall*, would occur when an immigrant population arrived on a continent and went extinct, and the other, which we might simply term a *visit*, would be the case where a population arrived on a new continent and returned to its homeland. Truly failed colonizations, such as landfall events, while an interesting and important aspect of any colonization theory, play no further part in this discussion, except in the limited sense that they may have left archaeological markers and thus have become confusing elements in the early chronology. Visits, similarly, may have introduced markers of human presence to a continent, but may or may not have lead to a subsequent true colonizing event. What is at issue is the evidence relating to the first significant and persistent human presence. This may have come, as Meltzer (1989) points out, as comparatively dramatic events involving once-, twice-, or thrice-off pulses of migrants, or as a relatively continuous "dribble" of colonizers. Archaeological signatures for any or any combination of the above would be equivocal in many respects, but some clearer resolution may be gained if we specify what form(s) of settlement/subsistence behavior colonists might subscribe to before we read too imaginatively between the lines of the archaeological record. It is not possible in this space to review each important site/claim for the two regions, although reference is made to a few key and/or typical sites. Reviews and discussions of details of the American evidence may be found in Erickson et al. (1982), Bryan (1986), Owen (1984), Mead and Meltzer (1985), and Waters (1985). The Australian evidence is best summarized in White and O'Connell (1982).

### The Americas

An uncommitted observer of the disputation concerning "early man" in the Americas is struck by a number of difficulties when attempting to ascertain what would be a fair assessment of a mixed and complex archaeological issue. That observer might begin by saying that American archaeologists have created an odd sort of "Three Age System" (cf. Irving 1985; Waters 1985), where chronology is argued to be of relatively long, medium, or short length. There is what might be called an Early Man school, which argues for a very long chronology, extending upwards of a few hundred thousand years. Next there is a pre-Clovis school, which stands firmly behind the medium length chronology of 30,000, 40,000, or 50,000 years for the first colonization. Finally, a Clovis-and-Later school is satisfied with the presence of humans in the Americas from about 12,000, or 13,000, or even 15,000 years B.P.



The Early Man school, those believing in a long chronology, having humans in the Americas in Sangamon (Irving et al. 1986) and earlier times, form a staunch minority and inspire an equally staunch but more numerous opposition. Their evidence comes largely from the dating of geologic features that are said to be associated with flaked stone tools. Identified with such sites as Calico (Simpson 1982), certainly the premier *bete noire* of American archaeological sites, the Early Man school draws a share of disbelief (Meltzer 1989) and rigorous challenges to the credibility of their evidence (Payen 1982). The idea of truly Early Man in the Americas is an exciting one, as it would imply immigration of pre-sapiens *Homo* and the very strong likelihood of failed colonization and the first evidence for the extinction of *Homo* on a continental-sized land mass. For the uncommitted observer, however, as interesting as such biogeographic phenomena might be, they do not in themselves make a case for Early Man in America, and the presentation of evidence in support of Early Man is itself not convincing.

Announcements such as that of Beltrao (1989), who claims occupation of the cave site La Toca da Esperanca in the Bahia of Brazil dated to between 204,000 and 295,000 B.P. by uranium-thorium method, where fossil animal bone, hearths, and quartzite tools are said to be undoubtedly "associated correctly", are certain to draw extreme, and I think fair, skepticism. Claims presented in this manner, i.e., without significant documentation and demonstration of claimed relationships, serve only to confuse the debate surrounding colonization.

The pre-Clovis school, those who favor the colonization by 30,000–50,000 years would appear to be more numerous than true "Early Man" hunters, but would still appear to be a distinct minority of American archaeologists. The pre-Clovis school, like the Early Man school, relies to a degree on associations between geologic markers and claimed stone tools, but there are also claimed subfossil human skeletal finds, claimed megafaunal prey associations, and numerous radiometric and other absolute dating determinations. Sites such as the rockshelters at Pedra Furada in eastern Brazil (Guidon and Delibrias 1986) and rancho La Amapola (Lorenzo and Mirambell 1986) in Mexico are but two recent examples of unqualified claims for more than 30,000 years of human presence in the Americas. There are numerous other such claims (see, for instance, Bryan 1986), and controversy surrounds each. In every case problems occur either in dating methodology, demonstration of association, or credibility of finds.

If the critics of the Early Man school are staunch, the critics of the pre-Clovis school are severe, probably because the likelihood of pre-Clovis occupation is not at all unimaginable and strong attention is brought to bear on each pre-Clovis claim. An important element of the criticisms of pre-Clovis claims is the demand that all such claims conform to strict canons of evidential credibility (e.g., Dincause 1984). Proponents of pre-Clovis react by arguing that their opponents are overly conservative (Bryan 1986), are unable to recognize pre-Clovis artifacts (Irving 1985:535), or are even in need of a change of "mind set" (Alsozatai-Petheo 1986:23). Arguments for pre-Clovis hinge, in part, for some (e.g., Bryan 1986) on the "accelerating accumulation of new evidence" (Bryan 1986:2), which in fact amounts to more claims of the sort consistently criticized as lacking in sufficient credibility. The uncommitted observer is here reminded of the case of the famous gangster who with 230 arrests, but no convictions, was still considered guilty by many. In the like manner, for some American archaeologists, apparently, the weight of claims is equal to the weight of evidence, regardless of the quality of each individual claim. Certainly no one doubts the possibility of pre-Clovis occupation of the Americas, but few would want to go to court with the evidence as it is presently constituted.

The "Conservative School" accepting only Clovis and younger occupations, bases its position on the repeated discoveries of unimpeachable archaeological associations having acceptable dating determinations. While Clovis, in the strict sense, refers to sites clearly marked by the fluted bifacial blades dating to approximately 11,500–11,000 B.P., it is probably fair to say that most "conservatives" would allow that there may be 1000 or 2000 years of earlier occupation by Clovis-bearing



people that are either not yet discovered, or are discovered, although for sampling reasons are not marked by the Clovis point. Although the ca. 13,000 B.P. occupations of Meadowcroft (Adovasio et al. 1978) and Monte Verde (Dillehay 1986) could be claimed by the pre-Clovis advocates, their likely associations are with people of the terminal Pleistocene, not some period ten or more thousand years earlier.

## Australia

Unlike American archaeologists, Australianists are not polarized on the issue of early dates. There are few sites in the Australian archaeological record that are controversial in so far as their artifacts are of questionable origin or the association of artifacts to datable material is suspect. Where this does occur, as at sites such as Keilor (Bowler 1976) in Victoria or Devils Lair (Dortch 1979) in southwestern Western Australia, Australianists adopt a wait-and-see approach, expecting resolution at a future date. The apparent patience of Australianists can be accounted for, in part at least, by the fact that since the time of the first demonstration of humans in the Pleistocene in Australia (Mulvaney and Joyce 1965), no claims have been made that would increase the total chronology by an order of magnitude, or even double it.

To the contrary, there has been an "accelerating accumulation of evidence", which, unlike that referred to by Bryan for the Americas, is firmly based in well-dated chronostratigraphic studies and points strongly to an approximate 40,000 B.P. date (Groube and et al. 1986) for the earliest period of occupation. Recent research, which is widely separated geographically, finds people in the north of Sahul on New Ireland in the Bismarck Archipelago at more than 30,000 years (Allen et al. 1988) and at the same time in Tasmania, at about 40° south latitude (Cosgrove 1989). Stone tools dating to ca. 38,000 B.P. (Pierce and Barbetti 1981) in the far west of Western Australia complete a triangle bounded by similarly old dates, and one that contains numerous dates on the order of 18,000–30,000 years B.P.

Chronology, for Australian archaeologists, is one of the few areas where contentiousness does not play an important role in the literature, and with the exception of the Kartan industry (Lampert 1981) — a very heavy core, flake, and waisted blade industry — there are no suspiciously old assemblages that are undated and might be thought to lie outside the range of radiocarbon dating. Consensus on the earliest occupation of Australia, then, is got much in the manner that the "conservative" school in America agrees (among its members) is the best way to proceed in that arena — by careful dating.

## Thoughts on the Dating of Early Americans and Early Australians

Both the American "conservative" school and the Australianists appear to chart a reasonable course, guided by hard-earned evidence. Complaints, such as that of Irving (1985:535), which suggest that "conservative" Americanists have poorly developed analytical skills for recognizing nonbifacial stone industries, cannot be supported. Few, if any, American archaeologists would not recognize an industry such as the Southeast Asian Pacitanian (Bellwood 1985) or the Australian Core Tool and Scraper Tradition (Bowler et al. 1970). Similarly, Bryan's (1986:7) claim that Australianists, had they been restrained by the strict criteria of having to find diagnostic artifacts ... "would not have searched for and thereby recovered evidence for Pleistocene man on that continent" is difficult to accept. In fact, Australian archaeologists have never truly "searched" for such evidence



... it most often came to hand via the usual sources — geologists, farmers, amateur collectors, and others — who, along with Australian archaeologists, have no trouble in identifying the unifacial industry. Furthermore, Australianists have indeed been restrained by the canons of good evidence and repeatable dating procedures. To them it might rather appear that it is the Americanist “pre-Clovis school” that is bemused by their own data and evidence. Australianists, like the “conservative” Americanists, would be unaccepting of the pre-Clovis, and for the same reasons, had such evidence been presented in Australia.

In the Australian literature, however, particularly where major synthetic works (White and O’Connell 1982) or generalizing articles (Jones 1979; White and O’Connell 1979) are found, one notes references to the possibility (or even probability) of dates in excess of 50,000 years. The reasoning here would seem to be that if a 40,000-year date is assured, then there must be a somewhat older date lurking out there, waiting to be found. This is seen in references such as White and O’Connell’s (1982:42) “guesstimate” that the antiquity of first peopling of Sahul will not go beyond 50,000–70,000 years ago. This is a somewhat conservative approach, in its own way, which, rather than claiming a greater antiquity, as if it were some form of trophy, simply allows for the incompleteness of the known archaeological record.

However, with each new Pleistocene site that bottoms out before 40,000 B.P., and in the absence of undated yet much older-appearing tool forms, the Australianists may be coming rapidly closer to clearly identifying the first effective colonization of Sahul. Ultimately, it must be considered by both Australianists and Americanists that the well-dated earliest occupations (40,000 and 13,000, respectively) may indeed be very close to the real time of first colonization. It seems particularly unlikely that late Pleistocene foragers living in sufficient numbers to avoid extinction could remain archaeologically “invisible” over entire continents for 20,000, 10,000, or perhaps even 5000 years.

Finally, it is ironic that the American “pre-Clovis school” makes reference to the Australian prehistoric record to bolster its own case. The dozens of well-dated Pleistocene sites in Australia stand in stark contrast to the few claimants for similar antiquity in the two continents of the Americas. The uncommitted observer must ask, “How could it be that so much has been found in Australia, and so little (if anything) in the Americas?” It is sobering and instructive to consider that the history of archaeological activity in these regions has produced precisely the opposite picture from what it should have, if such a picture was there to be demonstrated. As one might expect, the state of archaeological enterprise in the United States alone dwarfs that of Australia. The hundreds of academic and CRM projects producing <sup>14</sup>C dates each year in America annually outnumber the total number of excavations ever done in Australia. On these grounds alone, Americanists should have annually discovered (and clearly dated) at least several sites from the period 13,000–40,000 B.P. during routine research or CRM activities, if such sites existed. Add to this the fact that in America a number of dedicated scholars have devoted much of their time precisely to finding and describing pre-Clovis occupation and have yet to convince their colleagues of their success, and the probability that the lack of recognized late Pleistocene remains in America can be accounted for by sampling error, would seem to be remote beyond further consideration. Moreover, since the very great majority (if not all) of early Australian sites would pass the expectation tests of the American Conservative school, it is unlikely that a change in “mind set” among these scholars is called for. There would seem to be little latitude left for comparing the two great colonizing events and maintaining that the American one has more than a third the time depth of the other.

Archaeological evidence relating the first peopling of the continents, of course, goes beyond the dating of the simple presence of people. Thin, but present, evidence for early population distribution, foraging practices and strategy, and technological apparatus all have relevance for understanding and guiding research into the process of colonization. In the following sections, I argue that features of these activities are closely linked in the Americas and in Australia, and point to some complementary conclusions about colonizing new worlds.



## TRANSIENT EXPLORERS AND ESTATE SETTLERS: IS THERE A METHOD FOR COLONIZING CONTINENTS?

Is it fair to say that we can perceive a colonizing strategy in Australia, or in the Americas, and if so can we point to elements of that strategy that have direct archaeological consequences? To approach this question we might look briefly at a few elements that are concerns of archaeologists in both regions, and that we can reasonably suspect to be linked in important ways:

1. Palaeodemographic features, particularly overall population growth rates, and fissioning pattern(s)
2. Foraging practices
3. Colonizing logic

### Palaeodemographic Features

We may take it that the colonists who gave rise to the modern aboriginal populations of Australia and the Americas were numerous enough to avoid extinction. It is also safe to say that those populations grew at some rate sufficient to "fill" the continents, either in the carrying capacity sense or in the sense of estate tenure, where no significant blocks of unoccupied land remained unclaimed and unused. It is also very likely that high mobility (Kelley and Todd 1988) was a feature of the immigrant life. The questions become, how mobile were these people and in what size groups did they live, and when might the continents actually have become "filled", in any sense?

It must certainly be the case that a crucial element of the early spread of immigrant populations is their method of group fissioning, where some socio/economic/genetic unit, possibly organized like some band-scale society (Williams 1974) gives off "buds", daughter colonies, which are themselves reproducing units. The threshold of parent population density, at which "buds" will tend to form and relocate, the distances that those new buds will be prepared to remain from the parent population (their mothers, fathers, siblings, etc.), and the ecological nature of the new estate or range in which the newly budded community will become established are of very great importance in the peopling process.

For instance, we can consider a type of colonizing strategist, who we might term the transient explorers, where budding off at minimum numbers (new bud equals one young adult man and one young adult woman), long distance relocation, and willingness/ability to relocate to different ecological circumstances (e.g., from parents' prairie homeland to virgin coastal, or montane situations) all favor rapid continental spread, much like the bow-wave models of Birdsell and Mosimann and Martin. Such a strategy also implies very low social connectivity, very high inbreeding coefficients, low fecundity, and relatively high likelihood of the extinction of new buds due to stochastic events, as is posted for island-colonizing populations (Keegan and Diamond 1987).

On the other hand, we might imagine an opposing strategy of "estate settlers", where budding off occurs when groups of many individuals depart the parent estate but relocate only at a minimum distance for the establishment of a new estate, and try to remain in circumstances of ecological familiarity. These strategists would enjoy high social connectivity, relatively numerous potential breeding partners, relatively high fecundity, and a lower random extinction probability than transient explorers. These two different colonizing strategies would have a number of different behavioral and activity correlates, which would generate quite different archaeological markers (Table 8-1).

At the heart of this matter, and indeed of many other related issues, is the knotty problem of population growth. All must acknowledge that such occurred, but the form of continental-scale

Transient  
explorers  
↳  
rapid & extensive

estate  
explorers  
↳  
banks' & extensive



**TABLE 8-1.**  
**Some Possible Correlates of Two Colonizing Strategies,**  
**Transient Explorers and Estate Settlers**

	Transient Explorers	Estate Settlers
Demography		
Budding threshold	Low	High
Group composition	Stable	Slightly fluid
Inbreeding	High	Low
Fecundity	Low	High
Extinction probability	High	Low
Economy		
Different ecological zone tolerance	High	High
Estate	Unconstrained	Bounded
Archaeology		
Site forms	Very similar	Varied
Tool inventory	Generalized conservative	Specialized inventive
Range of activity/site Strategy	Repetitive Forager/pursuer	Varied Searcher/collector
Colonizing Logic		
Diet breadth	Narrow	Wide
Geometry	Lineal	Bow-wave/radial
Ecology	Patch-similar	Cross-patch

growth is unknown either in Australia or the Americas, and is seldom addressed. Both Birdsell (1957) and Mosimann and Martin (1975) considered the rapid growth of new population on Pitcairn Island to be illustrative of what happens when *Homo sapiens* arrive on virgin soil. For Birdsell and for Mosimann and Martin, high growth rates from 1 to 3% (and higher) per annum implied the filling (to carrying capacity) or the saturation (to a sort of cadastral completeness) of the continents of Australia and the Americas in a very few thousand years following initial colonization. Did this happen?

For Australia, judging by the occurrence of early  $^{14}\text{C}$  dates, Jones (1979:455) has pointed out that by at least 20,000–25,000 years ago, people were present in all major ecological zones, with the exception of the Dead Heart, or central desert, of that continent (includes New Guinea and Tasmania). In 1987, on the basis of Smith's (1987) excavations into 22,000 B.P. levels of Puritjarra Rockshelter, Jones (1987:666) would add the Dead Heart to the list and effectively complete the picture.

Similarly, an inspection of the distribution of early and middle Paleoindian sites in the Americas (Waters 1985:Figure 1.) would suggest that there too the major ecological zones were occupied in early times, perhaps the result of rapid population growth and spread. Although the  $^{14}\text{C}$  dates show the presence of people in these areas, it might be unrealistic to consider such presence as the mark of residence, or true settlement. It might equally be the case that highly mobile foragers (transient explorers) simply explored certain ecological zones without establishing the kind of settlement system we might deem an occupation. For instance, at Puritjarra, Smith (1987) describes a chronostratigraphic situation where in the lower half of the deposit, from approximately 22,000–12,000 years, an average of six artifacts were deposited per unit volume per thousand in his excavated sample. This contrasts with 210 and 656 artifacts deposited per thousand years in upper levels of similar (but actually somewhat smaller volumes) during the past 5000 years at that site.

One might fairly hypothesize that the simple differences in artifact deposition rates reflect quite different settlement relations between the earlier people who visited rarely, those who visited more

monstruosa es  
zonas ecolog.  
diferencia  
entre las  
de las  
de las

diferencia  
de  
densidad  
de  
estratigrafía  
de  
a punta

diferencia  
de  
estratigrafía

densidad de artefactos  
inducción de ocupación



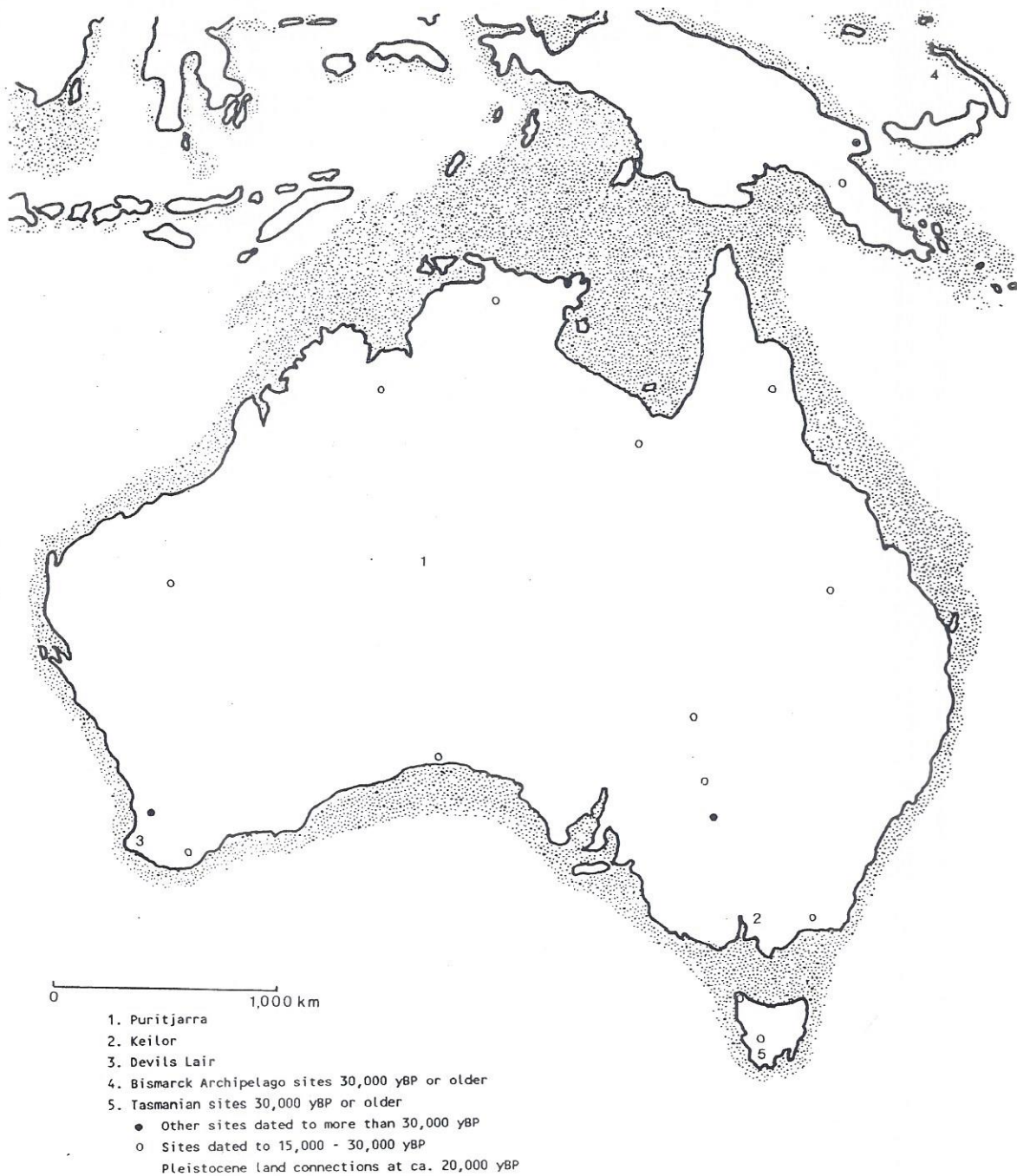


Figure 8-1. Some late-Pleistocene archaeological sites in Australia and Papua New Guinea.

often, and later peoples using Puritjarra Rockshelter. Certainly, shifts in patterns of mobility are possible, but is unlikely to be the case of all sides in all regions continued to show this pattern. No one would argue that Puritjarra is the “type site” for Australia, but it does illustrate a possibility that, if pursued and tested, would add a measure of realism to the reconstruction of early occupation in Australia and perhaps elsewhere.

For instance, assume for the moment that artifact deposition rates on a continental scale relatively and reasonably reflect the numbers of artifact makers and site inhabitants who lived at various time periods. We might then ask if the numbers of artifacts deposited per unit time (or new sites generated per unit time) are roughly equal for all time periods. Do the deposition rates of



10,000–12,000 look much like those of 30,000–40,000, or 5000–3000 B.P.? If that were to prove to be the case and deposition rates were generally similar through time, then strong support would be given to the models of Birdsell and Mosimann/Martin, i.e., saturation of people on the landscape should give some rough but equally saturated evidence in the form of archaeological sites and artifacts.

Certainly it could be argued that destructive processes would invalidate any such estimation, and that early sites are bound to be under-represented because they have been subject to destructive processes for greater lengths of time. Both chemical and physical weathering of organic and inorganic remains occur at various rates, but other things being equal, the results of these destructive processes are likely to be more profound with increasing time. Add to this the physical transport of artifacts and the sometimes dramatic evolution of local landscapes, and we have real cause to expect that the archaeological visibility of early sites and artifacts is diminished relative to later archaeological markers.

This argument holds best for organic remains and open sites on active landscapes. It does not hold so well for rockshelters, where much of the story of early people in both the Americas and in Australia is to be found, and it says nothing important about those crucial nonperishables, stone tools. In any case, the problem must be addressed at some level if relationships between colonizing peoples and landscapes are to become clearer.

It would seem that to begin in crude terms, and from first principles, might be a useful place to start. It would seem to be of lesser value to dismiss the problem by arguing that archaeological invisibility is primarily due to site destruction processes. An arguable extension of that reasoning would have it that people evolved separately in all continents, and that we simply, and sadly, have been denied the data via long-term site destruction processes. I doubt the Africanists, or most other Old World archaeologists, would be in much sympathy with that position. Where the weathering and/or transport of archaeological materials is thought to have occurred to a significant degree, we ought to be able to point to some local landscape feature(s), such as glaciation, solifluction, coastline migration, stream meander, tropical surface weathering, etc., to account for it. In the absence of such indicators, we might equally (at least) assume the absence of archaeological evidence to be due to the absence of past populations.

I have suggested elsewhere (Beaton 1983, in press) that the body of Australian evidence does not fit a rapid growth model, and that the relatively large number of mid- to late Holocene sites and the density of their deposits indicates quite the opposite, late population growth following a long period (ca. 25,000–30,000 years) of a very slow, very near-stationary growth rate. Assuming, for the sake of argument, that this was the case, we then would want to investigate such a model and see if any of its immediate implications can be seen (e.g., increasing number of site and increasing density of artifacts over time), given our understanding of known archaeological phenomena, both in Australia and in the Americas.

If we had confidence of population increasing in some form, then we would automatically expect that their increasing number of mouths to feed would have direct implications for diet/foraging strategies, as well as for archaeology markers landscape of occupation histories, assuming that these are inextricably linked.

## **Foraging Practices**

In America the kill site came first and the overkill hypothesis was generated subsequently; in Australia, we have an overkill hypothesis, but we are still waiting for the kill site. (Bowdler 1977:232.)



Little is known of early foraging strategies in Australia or the Americas, although in the latter case it is considered by some (Meltzer and Smith 1986) that a presumed focus on big-game hunting is unsupported and that a broader general foraging strategy is more likely from theoretical expectations, and more concordant with archaeological data. Even if highly suspect, the relationship of early foragers to now-extinct fauna is an example of strong asymmetry in the compared archaeological records of the two regions

There is evidence in the Americas for the taking of now extinct fauna, especially the genus *Mammuthus*, but including other genera such as *Equus* and *Tapirus* (Grayson 1984), and possibly *Camelops* (Frison et al. 1978). Said by Mosimann and Martin (1975) to be the victims of a blitzkrieg attack by early humans, the large-bodied, naive, and slowly reproducing "megafauna" did not survive the end of the Pleistocene. Extinct species not found in archaeological associations, such as the Shasta ground sloth (*Northrotheriops shastensis*) would be good tests of the inapplicability of the blitzkrieg model if they could be found to have been sympatric and coexistent with early hunters for a couple of thousand years or more (Martin et al. 1985).

The parallel situation in Australia is a curious one. While Australia had its own brands of "megafauna", i.e., large and now extinct marsupials, such as the diprotodontids and the large kangaroos, *Procoptodon* spp. and *Protemnodon* sp., there is no good evidence (in spite of the number of Pleistocene archaeological sites) that people bothered to hunt these creatures. While it may seem counterintuitive to some that such large, and again naive, meat resources were not hunted, there is good evidence for the foraging of freshwater shellfish, small marsupials, and reptiles, apparently while megafauna roamed unhunted. What possible justification could there be for astute foragers' avoidance of large meat packages and their apparent "preference" for the mundane shellfish and reptile?

Selection of food resources is likely to result from complex and dynamic interactions between imperfect foragers and uncooperative prey, the resultant diet being determined by the outcome of their thrusts and parries. The diet breadth model posits that the "desirability" of a resource is determined by the forager's ability to locate and to secure the resource, and to convert its substance to the forager's own upkeep. This sort of microeconomic reasoning implies that a resource is only as valuable as its place (rank) among alternatives, with rank being determined by forager/predator ability relative to that particular prey species in relation to other species in the habitat. Ultimate inclusion of any species in the diet would therefore be determined by the predator ability factor (pursuit time) and resource distribution (search-time).

This simple expression of optimal foraging strategy, derived from ecology, has gained a degree of respectability and sophistication (see Thomas 1989) among archaeologists since its early and enthusiastic, but very crude, initial applications in archaeological thinking (e.g., Beaton 1972). Speculating from optimal foraging theory, one might suggest that in Australia the megafauna, relative to other prey options, were either very rare during the time of humans, or very difficult to pursue, or both, and therefore excluded from the diet. But difficulty of pursuit, particularly for creatures such as the diprotodontids, is most unlikely. In turn then, the diprotodontids ought to have been very rare. Certainly they were not so rare as to be absent altogether, as they are known to have occupied parts of the well-watered eastern seaboard up until about 8000 B.P. (Gillespie et al. 1978).

Thus, in terms of the optimization model, the situation seems to be an unaccountable one, with competent Pleistocene hunters and extant megafauna in sympatry, but not in a predator/prey relationship. Could it simply be that the pursuit/search factor for megafauna was high, and that smaller fauna was easier to find and to secure, and were thus higher ranked; or, could there be some other ecological relationships that we have failed to consider?

Recent thinking in hunter-gatherer predator/prey systems (e.g., Winterhalder et al. 1988) has added sophistication to the theoretical realism of optimization models by trying to allow for the dynamism of human population, as well as prey population variation. Along these lines, it is



tempting to hypothesize that if the early Australians subsisted as transient explorers, then perhaps it was their own low population density (not that of the diprotodontids) that would relegate large packages of meat to low rank-order if more abundant and equally easily pursued small marsupials (or other taxa) were available.

If low human population densities and more acceptable alternative food sources discouraged people from exploiting megafauna in Australia, should not a similar situation hold for the Americas, particularly if the early colonists there were behaving in the manner of transient explorers?

## COLONIZING LOGIC

It is unlikely that the actual colonizing methodology of early immigrants to new continents was random. For some (e.g., Martin, Birdsell), founder human populations spread rapidly across continents in a manner largely independent of regional environmental variation. The key assumptions here would appear to be that hunter-gatherers of the terminal Pleistocene were immediately successful in their new lands, with their generalized tool kits, lack of competition, and mobile foraging strategies combining to complement the windfall profits to be taken of the naive fauna. These fortunate immigrants would then reproduce rapidly and radiate across the continents in the fashion of a "bow-wave". But what if the early immigrant populations were not such raging successes, and in spite of abundant resources, naive prey, and generalized tools and unrestrained mobility, they spread throughout the continents in some rational and even cautious pattern, minimizing risks while maximizing survival probability? If early populations acted in this manner, they might be seen to have directed their efforts toward certain types of habitats rather than others, choosing their "turf" with some care.

## MEGAPATCHES

One of the simplifying assumptions of the diet breadth model is that the forager searches and pursues over a fine-grained environment, where encounter rates for all prey are identical. This is of course quite unrealistic, as the real world is, for the most part, a coarse-grained environment, with species associating in different compositions and abundances in a mosaic of "patches". The "patch choice" model (Pyke et al. 1977) seeks to complement the diet-breadth model (and add a dose of realism) by predicting that movements of predators/foragers will be, at least in part, a product of their expectations for improved results if they shift operations to different "patches". O'Connell and Hawkes (1981) have shown the utility of patch choice foraging among the Alyawarra, and this inspires a question that must be posed at a much lower level of resolution, but one that might fit the study of continental colonizers.

For the purpose of thinking about colonization, would it be useful to view the virgin continents at a sufficiently low level of resolution that we perceive them to be made up of such coarse patches as coasts, mountains, plains, forests, deserts, riverine courses, etc.? Clearly this oversimplification overlooks significant differences between various types of coasts and subtypes of all the other coarse "megapatches" we might want to identify. But, if it is important for the scale of investigative resolution to fit the scale of problem, then the problem of continental colonization will, perhaps, require that we first look for patterns of the grossest scale.



It is possible then, that early colonizers might be so closely associated with gross habitat types that we might see them as "megapatch-choice" strategists? In this way they would be expected to conform to certain rules of "megapatch selection", whatever we determine these rules to be, and we might look for aspects of these rules expressed in the archaeological record.

If, for instance, early colonists demonstrated clear habitat preferences, then these would be reflected more in site location similarities within megapatches than in material culture within megapatches, although differences in assemblage composition could occur between megapatches, even among broad-scale foragers. Riverine/lakeshore dwellers should approach their resources in very similar ways, and ways not subtly different from how plains or coastal dwellers marshal their resources and sort themselves out over the landscape. The question would then become ... "does this sorting out in the colonization process indicate a trend toward the selection of one gross category (e.g., lakeshores, plains, coasts, etc.) over others?". In other words, have colonists ranked habitats in the way that they appear to have ranked resources within habitats?

### Colonization by Megapatch

Notions of early colonists selecting "megapatches" should not appear to be a truly new idea to Americanists or to Australianists. For the Americas, Fladmark (1978, 1979) has argued that, given the alternatives, migrants across Beringea would find the coast a hospitable route (and megapatch) for entry to the Americas, that is, a route that would offer abundant food and opportunities to make use of existing cultural adaptations. Following on from this, Gruen (1988) suggests that such a biogeographically based hypothesis could be tested using linguistic data.

Similarly, Bowdler (1977) has argued for a coastal colonization by early migrants to Australia. She saw the crossing of Wallacea as a feat accomplished by boat-building peoples who lived on the coasts of Sundaland, a coastal people who had a coastal economy, and who pursued this strategy in their successful colonization of Greater Australia (Sahuland).

Subsequently, for Bowdler, once the coasts had been occupied the population made its way up the major river systems, as this would have required a minimal change in their economic strategy. Residential incursions into significantly different ecological zones, such as the central desert or the eastern highlands, which would have required significant adaptation, would only have occurred following occupation of more "preferred" environments; first the coasts, then the rivers. In his own model of Australian colonization, Horton (1981) considered the early colonial thrust to have been one that targeted riverine and lacustrine systems with associated woodlands and their fauna as the best and most likely option for immigrant populations who were intent on surviving on a new continent.

Both the Bowdler and Horton models are developed in the manner of the "megapatch choice" type, where some most suitable (i.e., preferred) environmental situation is selected and dominates as the social/economic homeland until conditions, presumably ecological change or human population growth, change sufficiently to allow, to encourage, or to demand fulltime occupation of previously less desirable "megapatches". The great value of these two models (and those of Birdsell and Martin), and of others that might take their form, is that they are testable with routinely collected data, namely, the gross distribution of sites in regions and the time of first occupation of that region.

In his formulation of the peopling problem of Australia, Birdsell (1957, 1977) did not concern himself with the ecological/economic hunter, but focused more on the palaeodemographic opportunities for new arrivals. He argued that it was not only feasible, but rather likely, that people spread throughout the continent in little more than 2000 years, and would have achieved population densities across the continent, much like those of 18th century ethnohistoric Australia. Although



Birdsell did not concern himself with the ecological/economic hunter-gatherer on a complex and patchy landscape, his demonstration (1953) of the relationship between environmental productivity (as measured by effective precipitation) and population densities among Australians could be seen as concordant with the notion of patch-wise colonization.

At this time it might be instructive to consider just how the differences in colonization strategy between the hypothetical colonist types, estate settlers and transient explorers, might play out. To do this I speculate about how two polar-extreme strategies might operate on virgin continents. The crucial factor is that in both of the following scenarios the people inhabit continents in the early stages of colonization, when these parts of the world were not in any sense "saturated" with people. In this regard, these people inhabit lands for which there are neither modern nor ethnohistoric analogs. That alone might be sufficient justification for the following speculation.

### **Colonization by Estate Settlers**

We might first imagine some estate settlers, perhaps a patrilineal, patrilocal, exogamous band who occupy their "country" with its named and familiar places. Here we might find a couple of old men who are brothers, their multiple wives, their married and unmarried children, and growing number of grandchildren. But it is not all kin in the camp. There is, for instance, a young couple who are "refugees" from another estate, their passion having overcome their discretion. They will not return to his estate until things cool down, if ever.

These 25–40 persons, for the most part, do not leave their estate, except on regular social "business" or under extreme economic conditions. Most of the social business has to do with visiting kin in neighboring estates. This has the effect of temporarily changing the group composition of the camps in both places. Minor and transitory changes in group composition is the norm, rather than the exception, in camps in the estate. While small parties may leave the camp for visits to satellite bases for economic or other purposes, their lives are centered on one or two main campsites from which most activities emanate.

Over the years, the population of this estate has grown, as both fecundity and infant survival are relatively high. Finally, two brothers set off for an unprecedented "visit" away from their estate. This visit takes them not to kin connections in another estate, but to a place on the frontier, beyond the claims of any estate, where no people live, but where they find some unusual and interesting opportunities.

The brothers return to their father's estate after a period of 10 days, and remain there for only a month before making their second excursion to the new place. On a subsequent trip they overnight in a camp in a neighboring estate, into which one of their sisters has married. Next day, they are joined on the excursion by that sister and her husband, who, after all, gets on better with her kin than he does with his own. After several such trips, over a period of a couple of years, the brothers, one of whom has recently married a woman from the same estate into which his sister had previously married, stay longer in the new place, and less long in their father's estate, returning mostly for important ceremonial occasions or social events, such as the birth of babies.

After a time, the sister and her unpopular husband come to the new place on their own extended visits, relieving social tensions at their old estate. In the new place they are welcome, and they have every reason to stay, particularly now that she, like her brother's wife, is now pregnant. With the birth of the new children the new parents find it more difficult to return to their fathers' estates, but they instead become hosts to frequent visitors from both kin groups who want to see the new babies and have a look at this new place. Jokingly, both kin groups tease the new babies (for the benefit of the parents) about being bona fide owners of the new estate. But it is no joke, and everyone knows it.



The new estate is not exactly what the brothers might have wished it to be, as it differs markedly in some respects from their father's estate, which they intimately knew and where they felt comfortable. But, all other environmental situations most like that of their father's estate already had claimants and established resident bands, so they had little choice but to strike out on somewhat new ground. Kin visitors, reporting to their respective estates, compared the new estate unfavorably to their own, but the new settlers were little concerned by what might be considered to be deficiencies in the new estate. Being few in number, their requirements were not great, and with a few adjustments in foraging strategy, they foresaw few real problems.

If over the years the move to this new estate was a successful one, then there would be a necessity to formalize the relationship between the new settlers and the landscape. Locations would be named, often in relation to spirit beings and their heroic deeds. Ultimately, the estate would come to be configured as a landscape of camps, foraging grounds, and other special places organized around the particular geographic characteristics of that estate. If for any reason the move to the new estate failed, the new settlers risked little; they had undeniable claims (and near ones) on at least two other estates, those of their fathers. Their lines of communication and supply, so to speak, were short ones.

In this manner, the continent became saturated by estate owners who only went as far from their fathers' estates as they had to. What possible advantage, they might have thought, was to be gained from extending themselves beyond communication with their families? These were, after all, very social beings. They risked little, but moved only slowly across the latitudes and longitudes, each daughter colony being spawned by the overflow of a saturated estate. The long-term occupation of estates allowed for considerable manipulation of their environment and noticeable exercise of their talents. By most measures, the estate settlers are successful, and this ought to be reflected in their widespread, dense, and variable material works and markers of strategic behaviors.

### **Colonization by Transient Explorers**

We might next imagine the other extreme form of colonist, the highly mobile transient explorers. Across the new continent the transient explorers arrived as the new predators, occupying some suitable patch of country only until the return on investment began to slow, and then a move was the obvious solution. We might pick them up, not long after first colonization, peregrinating from good patch to good patch, but always seeking something with a familiar theme: a coast, a river valley, a well-watered mountain chain. As an example, we might imagine one such group, one that found it could do very nicely along the banks of a river.

There were only 15 persons in the group that had been camping in the big river valley for the past 5 years. Oddly enough, with the birth of the second child in 2 years, the group diminished in size as two men and three women packed up their kits and together moved off to the south, destination unknown. They said that they had planned only to follow the river south, or whatever direction it took, and that they could be found along its track, probably. In any case, they might come back toward the big valley in a year or so for a visit, maybe. The big valley held no particular sentimental value for them, for they had only lived in it for 5 years. In that time they had burned it off, seen the large animals thinned out, and generally become bored with their surroundings.

The remaining dozen or so people might choose to do something similar, perhaps down a different river system, or they or some still smaller faction might remain in the big valley indefinitely. They were quite flexible on the matter. If they stayed in the big valley, they might encounter another small group of riverine types in a year or so, and this would be a good opportunity to change group composition if desired. At least some new genes might flow from one group to the other, or both. No one would be particularly conscious of marriage rules, as the need to find any mating



partner was a serious consideration when so few people were spread over such a wide area, in tiny little independent clusters. Such things as the incest taboo were luxuries they had not been able to afford in the past, and might not be able to subscribe to for some time to come.

In their own lifetimes, the daughter populations would move fast, unhampered initially by children, and slowing down only slightly when encumbered by the needs of demanding offspring. If they slowed down a bit more, perhaps pregnancy could occur at shorter intervals and infant survivorship would be higher. But this is not their intent. In the space of only a decade, they have moved from a land with long, dark, and bitter winters to something more agreeable. Food and drink are never a real problem, nor have they ever been, but things just seem easier in the river valleys to the south, where the snow can be seen only on distant mountains, and while the game animals are somewhat smaller, there is no shortage of them, and the tubers and other plant foods seem endless.

Life then, for these transient explorers, has many advantages, but there are the usual dangers, chiefly accident and age-related illness. For these reasons, they understand how it is that other small groups have been rumored to have gone off and possibly died. Or maybe they did not; communication among these highly mobile bands is so infrequent and indirect that no one is ever well informed of the status of other bands. At the first sign of every distant fire, speculation breaks out about its origin. May be people and may be not. If there are people ahead, or over there, perhaps they will see them and that would be good. If not, that's alright too, as the transient explorers are not the social sophisticates of ethnographic monographs. The job of these folk is to move and to survive. Experimentation (the mother of disaster) is kept to a minimum.

In their way, the transient explorers penetrate deeply and quickly (albeit thinly) across latitudes and longitudes, and can be found to have left their marks in opportune accommodations, such as a rockshelter from one side of the continent to the other, in the wink of a C14 eye. The risks they took were very high, or so they might seem to us, but with a generalized tool kit, a naive fauna, an unconstrained social/political environment, and a bit of luck, a lineage might see the Northern Lights, note the transit of the equatorial sun, and feel the chill winds of the southern oceans in the space of ten or fewer generations. This is its own form of success.

## CONCLUSIONS

The purpose of the preceding is to direct discussion toward hypothetical aspects of the colonizing process that may lead to more robust and realistic models of how late Pleistocene foragers might behave on virgin continents. While a useful theory of continental colonization may be yet a chimera, the archaeological evidence relating to the early settlement of the Americas and of Australia are suggestive of some intriguing similarities, and differences. By way of considering some of the issues and possibilities, I offer the following:

1. In generating a theory of continental colonization by foragers, it is important to consider the likelihood and causes of failed colonizations, as well as those successful ones that are thought to have given rise to the peopling of the continents. Failed colonizations could, for instance, be an alternative explanation to account for the otherwise inexplicably early dates for some sites that greatly predate the established presence of people on continents. In the Americas, sites such as Pedra Furada (or even Calico, other criticisms notwithstanding) could conceivably represent a failed colonization, and the temporal gap that follows would be a reasonable expectation.



2. Successful colonizations must turn, ultimately, on demographic success. Studies into the processes by which people come to incorporate either micro- or macrohabitats into their home range, territory, estate, etc. should have explicit notions of group size, group structure, and mobility at their core.
3. Archaeologists and others who are interested in human behavior in early periods of Australian and American prehistory might do well to consider if any of the hypothetical colonizing correlates of what I have called estate settlers and transient explorers can be found in the archaeological record of the continents. Strategists behaving like estate settlers will leave a very different archaeological signature from colonists behaving in the manner of transient explorers. In America some might see Clovis sites as something akin to estateless mobile foragers on the move. Others might interpret the houses at Monte Verde as strong evidence for the kind of settlement that goes along with estate sedentism. In Australia, the Pleistocene evidence from rockshelters, such as Puritjarra (and many others), might be read as resulting from highly transitory foragers, exploring. Others, like me, might view the emergence of estate-like behavior as coming only with the "saturating" populations of the middle or later Holocene.
4. If it can be considered that there are parallels in the colonization of Australia and of the Americas, then archaeologists in both places might want to ask some continental-scale questions of their opposite numbers. For instance, Americanists might want to know from Australianists just why there is no apparent predator/prey relationship between early (and later) aboriginal Australians and the megafauna. Is this simply a matter of sampling? Or might the large marsupials have been low-ranked food targets, and for what reasons? Or is there some other explanation? Americanists will also be struck by the comparatively extraordinary cultural conservatism of aboriginal Australia. Is this related to low population densities and/or ecological dimensions peculiar to the Australian continent, or other factors?

Australianists will be equally impressed by the lack of credible pre-13,000–15,000 B.P. sites in the Americas. But if claims for sites of such an age are to be preferred by the "Early Man" and "pre-Clovis" schools, then does this imply *Homo erectus* in the New World and extinctions of some early colonists? And how can it possibly be that so many of the claims for early dates come from sites in South America? Australianists might also be interested to hear from the American conservative school on the subject of how it is that in the space of only 13,000–15,000 years the Americas, at the continental scale, have witnessed such extraordinarily high degrees of cultural experimentation and differentiation. What factors might account for this? Population, environment, unrestrained social vitalism?

Continental-scale questions are seldom seized upon as appropriate research problems. There are many reasons for this, but one among them is that archaeology embraces the comparative methodology, and few places or problems have sufficiently clear common histories to invite intercontinental comparison. Australia and the Americas are an exception to this. These places were the recipients of the last continental colonizations. This occurred at roughly the same time in human history, and the source for the colonists was roughly the same. Yet, in the space of 40 (or so) thousand years, these continents became the stages for the playing out of very different cultural evolutionary trajectories. It is not a trivial question to ask how this came about.

In the Americas, archaeologists appear to take cultural differentiation at face value and do not question how it might be that in the space of a few hundred miles Great Basin hunters remained so apparently distant in cultural morphology from the fishers of the northwest coast. While the answer might seem terribly obvious that living in a high desert is quite a different matter from living on a very wet coast, Australianists could point to equally wet-dry environment zones, of equal nearness,



where no such cultural differentiation occurred. Australianists might view their prehistoric record as indicating distinctive regional cultural differences, but the scale of these differences is intracontinental and takes on a very different coloration when compared with complexities in cultural morphologies in the Americas.

One research problem that would appear to benefit greatly from an intercontinental perspective is that of colonization. Questions relating to foraging patterns, colonizing strategy, population history, and cultural evolution may be better phrased, and ultimately understood, for each continent with consideration of the other.

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