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## ON A FAST-TRACK: HUMAN DISCOVERY, EXPLORATION AND SETTLEMENT OF SAHUL

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Recent decades have seen the emergence of new scientific paradigms for the origins of anatomically modern humans (AMH), a series of relatively rapid migrations that took these people from Africa to the far corners of the world, and the role of coastlines, boats and fishing in facilitating such dispersals. These paradigm shifts all meet in Sunda, Sahul and western Melanesia, where archaeology and anthropological genetics are illuminating some of the earliest evidence for seafaring and maritime migrations by AMH. The exact timing, routes and adaptive diversity involved in such migrations remain to be fully documented.

O'Connell and Allen argue that the peopling of Sahul was an 'archaeologically instantaneous' process occurring between ca 46,000 and 44,000 ya. They offer a 'speculative model' that deals parsimoniously with numerous complex issues. I have no qualms about rapid coastal migrations, as coastlines offer linear migration routes, entirely at sea-level and with few geographic barriers, often with very similar marine resources over vast areas. For the Pacific Rim, Erlandson *et al.* (2007) argued that maritime skills and similarities in the ecology of kelp forest and other coastal habitats may have facilitated a rapid coastal migration and colonisation from Asia to the Americas shortly after the end of the LGM. A similar model could apply to the margins of the Indian Ocean, where mangroves, coral reefs and other nearshore ecosystems support a diverse array of similar shellfish, fish, birds and marine mammal taxa from Africa to Asia and Australia.

I find nothing in O'Connell and Allen's 'fast track' migration that is not eminently reasonable. I agree that such a colonisation process almost certainly involved relatively sophisticated watercraft and seafaring capabilities. I also agree that maritime immigrants would have spread rapidly into resource-rich interior areas. Coastal habitats are partly terrestrial, after all, and coastal peoples worldwide are well-attuned to subsistence opportunities in the adjacent interior. As O'Connell and Allen note, moreover, not all coastlines are equally productive, so those offering fewer aquatic or terrestrial resources might have been skipped over to focus on coastal sweet spots described by Bulbeck (2007) and others. O'Connell and Allen's model may overestimate the vulnerability of estuarine ecosystems to sea-level fluctuations, as intertidal organisms and communities tend by nature to be highly resilient. During a critical period (ca 50,000-35,000 BP) for their model, moreover, Pope and Terrell (2008:8) suggested that estuarine habitats were relatively extensive in South and East Asia.

O'Connell and Allen may be correct that the colonisation of Sahul occurred very rapidly, but understanding a late Pleistocene human colonisation of Sahul – as well as other Pleistocene coastal migrations – is clouded by significant problems (Erlandson 2001, 2010). First, the colonisation process took place at, near, or just beyond the effective range of  $^{14}C$  dating,

which reduces the confidence we can have in  $^{14}\text{C}$  dates from archaeological sites, especially those run without the advanced extraction, purification and measurement protocols essential for dating samples of such age. Second, migrations from Sunda to Sahul – primarily coastal and maritime in nature – took place at a time when global sea-levels were ca 60-30 m below modern. With sea-levels now at a 120,000 year high, vast areas of Sahul's coastal lowlands have been submerged by rising seas, including most of the ancient coastlines early immigrants would have traversed. Third, thousands of islands in Southeast Asia have still seen relatively little research on Pleistocene archaeological sites: a region that stunned the world with a relatively recent discovery of *Homo floresiensis* may have more secrets to unveil. As a result, I continue to teach my students that Sahul was probably colonised ca 50,000±5000 years ago, an Aristotelean 'golden mean' that averages models by scholars who know much more than I about the archaeology of the region.

There are some interesting parallels in O'Connell and Allen's model with a Clovis-First model for colonising the Americas that is now in full retreat. Clovis-First advocates long argued for a terminal Pleistocene terrestrial migration that settled North and South America within roughly a millennium. They created demographic models that were hypothetically possible, but now are proven to have been wrong. Current archaeological and genetic data suggest that the settlement of the Americas, which may have involved a coastal migration and detours that saw humans follow productive river systems deep into continental interiors, took several millennia to accomplish. The American experience, where two large continents now appear to have been colonised, explored and settled within as little as three millennia, could support O'Connell and Allen's fast-track model for colonising Sahul. One lesson learned from the widespread distribution of Clovis sites in North America, however, is that it took 2000 to 3000 years for pre-Clovis populations to reach levels of archaeological visibility that also seem characteristic of the earliest sites throughout Sahul.

Given some of the challenges alluded to above (drowned shorelines, limited chronological resolution, lack of research in key areas etc.), I hope the excessive conservatism of Clovis-First models will not be repeated in Australia and Southeast Asia. Given the active research and lively debate that continues on the human colonisation of Sunda and Sahul, this seems unlikely. I congratulate O'Connell and Allen on a fast track model for the peopling of Sahul that is a reasonable hypothesis, one with profound implications and numerous corollaries that provide fertile ground for further research and testing.

### **SHOULD I STAY OR SHOULD I GO NOW? THE SPATIAL DYNAMICS OF FORAGING AND DIMINISHING RETURNS**

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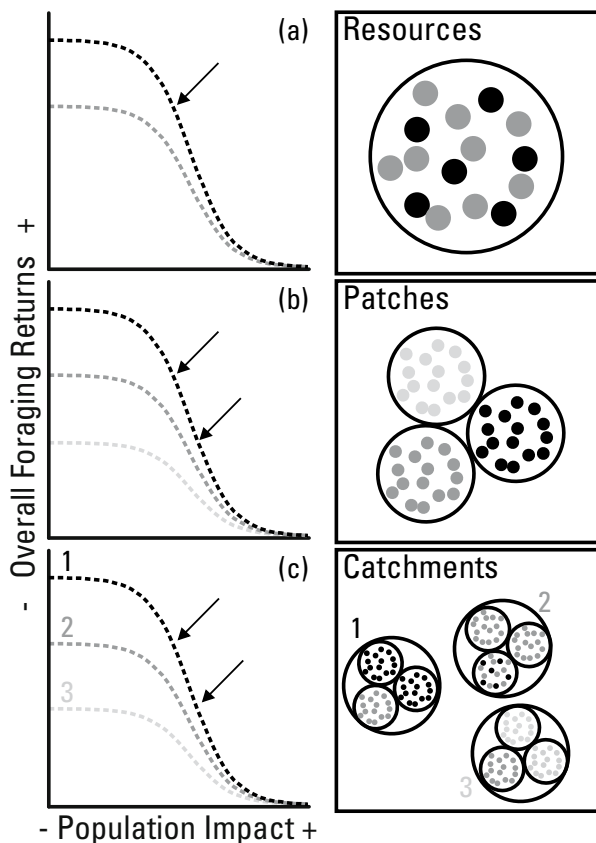
O'Connell and Allen should be praised for their concise and focused synthesis of the debates and issues surrounding the colonisation of Sahul. As they have done for the timing of this event (e.g. Allen and O'Connell 2003; O'Connell and Allen 2004),

they now reframe the scope and direction of research towards understanding how and why people rapidly entered the continent and where they went. We would like to use this opportunity to expand upon the theoretical framework presented in this and companion papers (Allen and O'Connell 2010; O'Connell *et al.* 2010), by examining foraging decisions across three explicit spatial scales and proposing some additional archaeological predictions within each.

At the heart of their model is the assumption that the rate at which foragers can capture food diminishes with foraging time (Charnov 1974): the more per capita time spent exploiting a particular resource, remaining within a particular patch or continuing to forage within a single catchment (sometimes termed habitat), the lower the expected gains will be (see Figure 1). According to O'Connell and Allen, this is the engine that drives people into and across Sahul: when resources are depleted, people are better off moving into adjacent, unoccupied areas than staying within depleted environments. However, the three different models from which these predictions derive (prey choice, patch choice and ideal free distribution) all operate at explicitly different scales represented as resources, patches and catchments, respectively. How these respond to population pressure at these different scales, and how people then respond to those changes will determine how, where and whether people are expected to move. The effects of foraging on the environment build cumulatively across these spatial scales, and devising testable predictions about mobility patterns depends on the scale at which foraging optimality is analysed.

Within a patch, foragers should pursue the highest ranking resource to the exclusion of all others until the point when continuing to search for that resource is less profitable than taking the next highest ranking resource on encounter. As encounter rates with higher ranked resources decline through foraging-related resource depression, this results in an ever widening 'diet breadth', leading to ever increasing handling costs, but likely reduced search costs (Figure 1a). The prey choice model predicts that people should respond to reductions in high ranked prey with a reduction in mobility and a focus on technological enhancements to reduce the costs of handling lower ranked resources in patch, the standard model of resource intensification. As O'Connell and Allen point out, there is little archaeological evidence for 'broad-spectrum' intensification in Sahul until the late Holocene, suggesting that foragers did not tolerate in-patch declines for long. If, as they suggest, high ranked but slow to reproduce rocky shore resources were driving the decline, we should not necessarily see changes in species composition, but are likely to see reductions in the size of staple shellfish species over time.

If foragers decide not to remain and intensify their foraging effort, they may abandon their patch and move on to exploit other patches. These decisions are crucial if resources are distributed heterogeneously across different patches. In these circumstances, the critical foraging decisions involve which patch-type to exploit and how long to stay within a particular patch. With increasing consumption pressure, high ranking patches would decline in productivity, leading foragers to enter lower ranked patches (see Figure 1b). At the patch scale, foragers expand their range within a catchment or habitat to encompass patches of lower quality resources as high quality patches decline.



**Figure 1** Predicted foraging dynamics across each spatial scale: (a) in patch; (b) between patches; and, (c) across catchments. The arrows indicate the point at which foragers should switch to alternative resources, patches or catchments.

Expansions in foraging radii are likely accompanied by increases in logistic mobility, with people moving further to access more remote patches, identifiable by the deposition of exotic resources or raw materials acquired from patches located at a distance from the central place.

As with resources and patches, increased human consumption pressure should decrease overall foraging efficiency within catchments (Fretwell and Lucas 1969), eventually causing people to resettle in new areas (Figure 1c). Being the highest spatial scale, these movements are significantly greater than movements within or across patches. Given the marginal costs of movement into adjacent territories that remain unoccupied, this would likely result in the rapid dispersal of people as discussed by O'Connell and Allen. Foraging decisions at this scale form the backbone of O'Connell and Allen's argument. As a next step, we encourage the development of a spatially explicit ideal free distribution model (e.g. Winterhalder *et al.* 2010), which would rank catchments based on the aggregate returns and diminishing gains within geographically specific patches according to the expected resources within those patches. While such a model has not yet been developed for any continent in any context, it would provide detailed predictions about where the earliest settlements are likely to occur and where we might expect greater populations throughout time (Birdsell 1953).

Depletions at different spatial scales also likely occur over differing temporal scales. However, in circumstances where there are few opportunity costs to movement – as in scenarios where people have watercraft – individuals will likely relocate across

vast distances in order to occupy highly ranked catchments. This may appear as pulses and waves of settlement, with the first settlers inhabiting higher ranking catchments, exploiting the highest ranking patches within those catchments, and pursuing the highest ranking resources within those patches. Determining how these patterns encourage movement requires building models incorporating individual decision-making to test predictions at the largest spatial scales. O'Connell and Allen have made great progress with this and companion publications, and based on the available data, they are certainly correct. Next steps will include further refinement of the model particulars and tests of its predictions with new archaeological data.

#### FOUR QUESTIONS ABOUT FORAGING MODELS AND THE PROCESS OF COLONISATION

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The target paper takes the debate about the narrative of Australian archaeology a significant step forward, and sets up some new research problems to be tackled.

O'Connell's research with the Alyawara (Iliaura) (O'Connell and Hawkes 1981) demonstrated that, despite their access to purchased flour, modern fisher-gatherer-hunters will collect seeds and grind them, provided there is an anthropologist who can use a vehicle to drive them to the grasses. While this sounds dismissive, it is actually very important for two reasons: (1) the gatherers needed to know where and when the grasses were suitable for harvest; (2) the anthropologist's vehicle reduced the cost of travel and search effectively to zero, altering the values in the patch choice model. A distinguished ethnographer of fisher-gatherer-hunters protested angrily about this work: 'My people do not forage optimally.' I wondered, silently, whether they were somehow more virtuous because they had not reached optimality or perhaps they were somehow better than optimal. This questioning also has implications: (3) are there behaviours which do reduce the 'optimality' of foraging; and, (4) on what time scales do the considerations of behavioural ecology have to operate?

##### 1. The Importance of Information

All HBE/OFT models assume that the foragers have information required to make the appropriate choices, and ethnography demonstrates prodigious knowledge of plant and animal species (e.g. O'Connell *et al.* 1983), as well as key indicators of seasonality. Golson (1971) showed that there were some similarities between the vegetation communities on either side of the Wallace Line, but only small numbers of species familiar to initial colonists of restricted distribution within Australia. New colonists needed to acquire information about the nature of suitable resources, especially as some contain toxins, whatever their means of dealing with them (e.g. Beck 1992).

As a result of this uncertainty about information, O'Connell and Allen are right in the way that Bowdler (1977) was right: the easiest option was to focus on familiar resources that were present on both coasts. As coastal populations expanded, so there

was pressure to move inland; water and its variable availability was the chief limiting factor in many regions (cf. White and O'Connell 1982:51).

The first colonists of any region needed information on plant species, including variation in their availability and food yield. New colonists needed to discover what the resources were, as well as their relative value. Both availability and ranks of food resources could change stochastically from season to season, year to year and decade to decade, as well as biozone to biozone. In the fullness of time, such information could be obtained, but, as oral histories seem to suggest, passing on information about variation was difficult when the time scales of variation were longer than individual experience (Davidson 1992).

## 2. *The Costs of Foraging*

There are also hidden costs of foraging, such as the embedded cost of O'Connell's vehicle. These feature in the discussion of risk reduction in Australian archaeology deriving from the work of Oswalt (1976), who characterised technologies in terms of the number of technounits. Oswalt's fallacy is exposed by his equating of a digging stick and a chimpanzee's termiting wand because they are both single sticks (McGrew 1987). The digging stick, unlike the termiting wand, requires stone tools to make it and can be used many times. So, one of the costs of human foraging is not only about food sources, but also acquiring information about, and obtaining, stone for tools. It is noteworthy that at Sandy Creek 1 in Cape York, the earliest assemblages (ca 37 cal ka) were made on quartz crystal (Morwood *et al.* 1995), as if the first people had not yet found the raw material sources they later relied on.

## 3. *Are there Adaptive Behaviours that Reduce the Optimality of Foraging?*

Historically, symbolically supported sets of relationships and long distance interactions were part of the mechanism by which such uncertainties were dealt with, a process that began early (Veth *et al.* 2011). This could have been a disadvantage, particularly by defining ritual relationships with place that may have endured beyond the time that such places were sweet spots. Given that rock art of all ages, and much of the symbolic expression of relationships in the historical period, involved relationships with animals (and much less with plants), such expressions of value might affect assessments of resource ranking. In addition, early beads suggest symbolic marking of interpersonal relations that could promote the adaptive success of populations that differentiated in such ways (Boyd and Richerson 1987).

## 4. *On what Time Scales does Behavioural Ecology Operate?*

At some level, the sorts of decisions O'Connell and Allen address are almost encapsulated by the choice of restaurant alluded to in their title, but human behaviour was subject to many more selective pressures than those individual choices. As people exhausted resources or populations became too big for the resources of the region, they could have moved on or targeted lower ranked resources, perhaps becoming strategic generalists. Symbolic attachment to places probably tethered some groups who adjusted to the stress of variations in resources and gave up the option of seeking higher ranked resources in the restaurant

over the next sand hill. Sometimes they failed, as the patterning of radiocarbon dates suggests, but in the end they found ways to cope with the scale of variation that the environment threw at them (Davidson in press).

## COMMENT

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O'Connell and Allen have provided an articulate, multidimensional model of a subject made complex by its interdisciplinary nature, its long history of debate and the continuing shortage of hard archaeological evidence for many regions. I would like to expand briefly on some of the more general archaeological and anthropological aspects of the model, which are necessarily dealt with rather cursorily in the review.

O'Connell and Allen discuss the implications of an archaeological record defined primarily by infrequent, short-term occupation sites produced by small, highly mobile groups. They also infer that human populations were generally 'small and patchily distributed across Sahul'. Not discussed by the authors are the implications of this record for the absence, or minimal presence, of more complex hunter-gatherers at the end of the Pleistocene period. In other continents, the relationship between late Pleistocene plant communities and humans, for instance, often laid the developmental foundations for long distance social networks, reduced territoriality if not semi-sedentism, and eventually plant domestication. In the northern Peruvian desert and surrounding environs during the late Pleistocene, foragers were experimenting with new food procurement strategies, occasionally rejecting some or all of them, and then returning to prior ones (Dillehay 2011). Although the archaeological record of this region indicates short-term site occupation similar to that described for much of Sahul, it also reflects continuous use of the same habitats over several millennia, as evidenced by repeated yet ephemeral occupation of the same site locales. Some foragers even managed plant and animal resources by fostering environmental conditions (e.g. perhaps intentional burning or predator overkill) that promoted preferred foods. This certainly was the case for palm nuts in the Amazon rainforest and for shellfish along the Pacific coast of South America. Is there any evidence of incipient foraging complexity or early resource management in Sahul at the end of the Pleistocene, and, if so, how might they have impacted patch choice, marginal value and ideal free distribution?

Although the O'Connell and Allen model is focused on various concepts developed within optimal foraging strategy, I wonder why the social aspects of late Pleistocene humans are not discussed as possible factors influencing resource exploitation, diet choice, site patterning and so forth. For parts of South America, we can now infer that social fissioning was probably less common than we had thought and that it occurred when the local carrying capacity had been exceeded or social tensions were high. When it did occur, which probably resulted in smaller social units moving around the landscape, we suspect that splinter groups maintained close ties with previous groups for economic support and social interactions. We now understand that some

early foraging societies institutionalised practices that served additional (and diverse) functions for their individual members and which were organised as relatively specific entities in different places (e.g. Rowley-Conwy 2001). These practices were ritual orders, social networks, technological traditions, perhaps gender-based units, and others that were performed in specific private or public places, such as individual huts or rock art sites. Given the rich rock art of many areas of Sahul, can anything be inferred from it regarding early social and behavioral patterns?

A point I find intriguing relates to diet breadth and the complexity of technology. The review notes that a bifacial stone tool technology (and other technologies?) may have been lost en-route from Africa and South Asia to Sahul 'due to a series of demographic "bottlenecks" that ultimately reduced the cultural transmission and thus the technological repertoire of early colonisers.' Rather than imposing cultural transmission as a causal factor here, O'Connell and Allen suggest that diet breadth and technological complexity may correlate to explain the lack of diversity/complexity in late Pleistocene stone tool industries. They argue that a wider diversity of exploited resources increases 'handling time' and procurement techniques, which leads to greater technological complexity. In South America, the reverse seems to be the case: simple and less complex unifacial industries are generally correlated with broad-spectrum diets derived from large and small game hunting and the gathering of a wide variety of plant species. These diets required more handling and allocation time and procurement strategies across many different environments. The primary adaptive technology for these diets was a unifacial industry, which provided tools used for a wide variety of handling and other functions. More complex and diverse technologies, on the other hand, are associated with specialised game hunting. Further, the general pattern in Peru was ongoing frequency shifts in the use of alternative adaptive strategies that incorporated changes in the use of different technologies and tool types (stemmed and fluted point and unifacial industries) in different time periods and in different ecological settings.

Along similar lines, it seems that bifacial technologies were occasionally 'lost', or at least less diversified, as late Pleistocene people spread through Central America and into the wide range of environments in South America. But there are archaeological records across the continent suggesting that these technologies were both lost and recovered from time-to-time. We do not know if the lost, narrowed or recovered technologies relate to cultural transmission, but, if so, we need to examine early social networks and how information was passed along them, and whether 'technological diversity is likely more a function of ecological context and foragers' intellectual solutions.' This topic requires more inter-continental comparative research.

## COMMENT

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There is much to like in this paper, such as putting together all the most recent research and applying it to the age old problem of trying to define the timing and nature of the first colonisation of Australia. When Birdsell, and even when Bowdler and I,

considered the subject in necessarily broad terms, there was far less data than there is now.

I am also delighted to see recognition of the importance of climate change, and the dismissal of firestick farming and megafaunal extinctions nonsense. It is long past time Australian archaeology left these ancient shibboleths behind. They have been a drag on the discipline, preventing the analysis of new data in ideologically uncluttered ways. Worse, they have led to the great increase in prescribed burning, and to an inability to use past climate change as an analogue, and a warning, for what the country faces as the planet warms.

But, and there must be one, I do have a general problem with the paper. Many years ago Mortimer Wheeler famously said 'as for archaeology, I do not know whether it is to be considered an art or a science' (1954:50). The problem here, I think, is too much science and not enough art. Humans are not rabbits, or fruit fly, or yeast.

Humans use the most productive patches as a high priority then move to the next most productive and so on? Well, they might, I suppose, with perfect knowledge and a rather mechanical view of 'diet'. Or more likely they won't, depending on actual group composition, religious considerations, food preferences, seasonal variations, local water resources, shelter availability, relationships with neighbouring groups, topography, cultural matters, storage technology, gathering and hunting technology and so on. I don't think 'patches' are as important as the overall human habitat available, which is why I thought, and still think, that the slopes and plains of northern and eastern Australia are a likely initial pathway. The beaches look good to us, but for a hunter-gatherer without watercraft I think probably not so much. Sure you can strip all the shellfish off rocks and dig for them in sand, but then what? Where is the handy freshwater and shelter that doesn't face towards the storms, and topography that allows easy movement inland? Conversely, these days it is easy to underestimate the biodiversity and favourable environment of the slopes and plains, seeing it, as we do now, in its relatively sterile form. From 1813 on, with the Blue Mountains crossed, these slopes and plains, so favourable for grazing and later other farming practices, were subject to rapid, and far greater, change than elsewhere in the country. Trees were cleared enthusiastically to expand grazing grass, the hooves of sheep and cattle trampled the soil, weeds took hold, pastures were fertilised and improved, fences erected, native animals hunted, creeks dammed, rivers silted up, and foxes, rabbits, sparrows and honey bees arrived. The largely bare open hills I see from my window are as much an artefact of British farming as are the similar hills in England and as unappealing for Aboriginal occupation now as they would have been appealing 200 years ago. I think both beach and semi-arid areas, as permanent settlement areas, were only occupied as population built up in the woodlands of slopes and plains and the excess had to spread out further.

This brings me to population. As Allen and O'Connell point out, there is no evidence for the Birdsell-type calculation that sees population growing very rapidly from Adam and Eve to a million in no time at all as it grows exponentially through the generations: One, two, many ... like yeast in a test tube. But it is human beings involved here, living in groups, with social, cultural, spiritual, historic, psychological, ecological, physiological and

structural limitations and advantages, not to mention the effects of natural disasters. I don't think any human groups are likely to increase at the maximum possible rate, nor do I think Aboriginal groups in different parts of Australia are all going to increase at the same rate.

Finally, another problem the authors are well aware of. Projecting our current image of what the present-day Australian environment is like back into even the recent past, let alone the distant past, is very misleading. Applying the ethnography of a handful of well-studied Aboriginal groups from this very atypical period since 1788 even to other contemporary Aboriginal groups is fraught with danger. Applying it to, say, European Mesolithic hunters is even more inappropriate. And it is equally true that applying what we learnt about Yolgnu, Warlpiri and other groups in the 1960s and 1970s to the ancestral 'Aborigines' of 40,000 and more years ago simply assumes what needs to be proved. This is a self-fulfilling prophecy as damaging as that of fire-stick farming.

This paper is a fine contribution to the 'Origin of the Australians' genre and I congratulate the authors. It is another step along a way which really relies much more on small steps than big leaps – much like the colonisation of Australia I suppose.

### NOW BRING ME THAT HORIZON

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Archaeologists seem to face far more complications in making the crossing to Sahul than the people who accomplished this feat about 50 kya. Radiocarbon dates are now so numerous that the presence of humans in Sahul at this early date cannot be ignored. The question is how and why this passage was accomplished. In answer, O'Connell and Allen present a very logical argument.

Reconstructing the crossing to Sahul focuses initially on the availability and type of watercraft. I do not see this as a major problem. Most large rivers dispatch substantial quantities of floating debris, including large trees (Keegan and Mitchell 1986). It is hard to imagine that modern humans, even 50 kya, did not understand buoyancy. It is relatively easy to construct a raft that can navigate calm waters (and often are flexible in rough seas<sup>1</sup>). Sahul may have been discovered by accident, but accident had nothing to do with its colonisation.

We face similar issues in the Caribbean (Keegan in press; see Figure 2). The first evidence for humans in the insular Caribbean comes from the Greater Antilles, with the earliest dates clustered around 7 kya. The initial colonists were mobile foragers who exploited chert sources in the islands and may have been responsible for the extirpation of sloths and manatees. Unfortunately, all of the sites from this time are lithic scatters with no faunal remains. This example is relevant to the colonisation of Sahul because in the Caribbean people with similar practices did not cross the shortest water gaps. Based solely on geographical distance we would expect the first colonists to have crossed from Florida, the Yucatan or Trinidad. Yet the material evidence indicates that the first colonists, and the second wave, came from

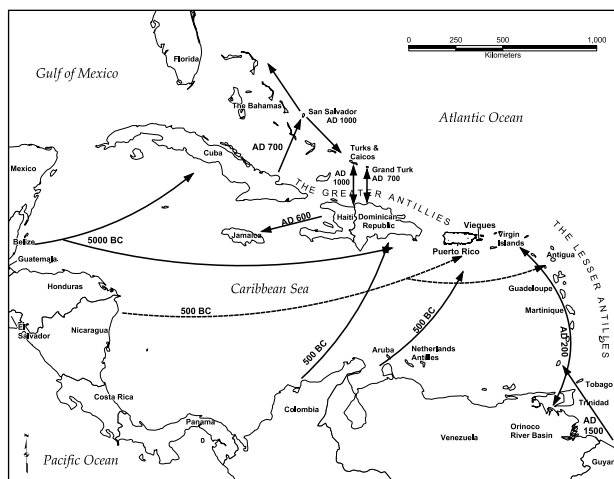


Figure 2 The timing and location of human migrations in the insular Caribbean (Keegan in press).

the 'Intermediate' or Isthmo-Colombian area (Keegan in press). Expeditions to colonise the islands involved crossing an expanse of the Caribbean Sea by peoples we assume to have limited maritime technology (Figure 3).

Efforts to explain the development of watercraft that were substantial enough to make the crossing to Sahul have emphasised the exploitation of marine resources. O'Connor *et al.* (2011) recently used faunal evidence from the Jerimalai site in East Timor to highlight the exploitation of coastal and pelagic resources ca 42 kya. I am not convinced that watercraft were necessary. Intertidal molluscs are on dry land for ca 12 hours each day, and many reef species can be collected by walking along the reef at low tide. Second, Scombridae (tunas), which they designated as pelagic, actually frequent shallow coastal waters as juveniles. Given the climatic variations described by O'Connell and Allen, these Scombridae may not be pelagic. Generalised marine habitat denominations for taxa do not capture the behavioral ranges of fishes (Keegan 2009). Nevertheless, this line of reasoning does suggest a motive for the production of watercraft.

O'Connell and Allen adopt a similar, but more operationalised, perspective in their application of optimal foraging theory (OFT). I agree with their reasoning, which enhances our understanding of why particular patches were targeted. The issue I have is that OFT focuses on marginal and average return rates, and ignores total return rates (Keegan 1986). The marginal return rate is optimised to achieve a goal, which is defined as enough food to meet quotidian requirements (total returns or demand). If the pursuit, capture and handling of additional prey will not provide a consumable yield, then there is no need to invest additional time in searching for prey.

Furthermore, if mobile foragers were the original affluent society, then they had an abundance of time. After foraging goals were met, foragers had to decide what to do in their 'free time'. The possible options are too numerous to mention, but one option was the collection and consumption of non-optimal comestibles. Most molluscs have an incredibly low marginal return rate, yet they are found in coastal and inland sites throughout the world. In my opinion, the consumption of small molluscs is an expression of wealth. People eat small molluscs because they have the luxury, and not the *need*, to do so.

<sup>1</sup> see <http://balseros.miami.edu/introduction.htm>



**Figure 3** Modern Haitian 'pi pris' used for coastal fishing (relevant to OFT its name can be translated as 'small price'). It is similar to rafts that made the 200 km crossing from Cuba to Florida (from the north coast), and the 400 km crossing to the Cayman Islands (from the south) in 1994 (photograph courtesy of Lisabeth Carlson).

OFT provides a formal economic structure to the most basic daily decisions in people's lives. Foragers should expand diet breadth during periods of abundance (high total returns from the highest ranked foods) by experimenting with non-optimal foods. Such experimentation can be achieved at relatively low opportunity costs. In contrast, diet breadth should contract during periods of stress (e.g. the colonisation of new territories) because the search for, and pursuit of, the highest ranked foods in the optimal set offers the most reliable diet (measured in terms of marginal return rates). Thus, the earliest sites should reflect a focus on the highest ranked items, such as at the Coralie site (Keegan 2007, 2010).

The distribution of patches and prey types promotes dispersion. This centrifugal force is countered by the distribution of people; a centripetal force that promotes mobility for the purposes of assembly. O'Connell and Allen are right in highlighting demographic parameters, and I agree that the colonisation of Sahul was accomplished by 'several separate landing parties' that maintained contact, that at the very least, allowed for the exchange of mates.

The missing element in their argument is social organisation. A small group colonising Sahul may have been demographically viable, but was it socially viable? All societies have marriage conventions. The rapid movement into Sahul, followed by an abrupt stop, probably reflects the accumulation of a social population sufficient to allow the socially constructed exchange of spouses without requiring return voyages to Sunda. Social relations established through the exchange of spouses require reciprocity (e.g. Jordan *et al.* 2009; Keegan 2010). Colonisation involves the movement of a collectivity of individuals who are members of multiple social groups and who require intercourse with their parent groups. Island colonisation is not unidirectional.

O'Connell and Allen present a strong case for the biological imperatives that structured the colonisation of Sahul. What is missing is a consideration of the social imperatives. Seaworthy vessels facilitated the exploitation of new areas and the colonisation of Sahul, but their invention was predicated on the need for social interactions. Humans developed watercraft to facilitate contact with other humans, and not solely to permit the exploitation of marine resources.

## COMMENT

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O'Connell and Allen propose one of the more structured archaeological colonisation models of the recent past. They present a theory of colonisation based on the available palaeoecological and archaeological data from the earliest part of the Sahul record. They suggest that their model can be tested and refuted. If so, it is a key element in any judgement of the predictive power of a model. It is an advance on earlier attempts, such as Birdsell, Bowdler and Horton, to explain the settlement process of Sahul. At least O'Connell and Allen cartographically recognise that New Guinea and Tasmania were in fact connected to Australia at that time. Further, some earlier models were largely immunised against testing. For example, Bowdler (1977) in her coastal colonisation model observed that many, if not all, of the earliest sites were now under the ocean. Fortunately, later discoveries of sites like Bobongara, Matenkupkum and Matembek, that continued to be tectonically uplifted over the last 40,000 years, beyond the highest sea-levels, preserve a record of human behaviour that remain unaffected by submergence. Indeed, late Pleistocene sites discovered before 1977 like King's Table, Kisope, Kenniff Cave and Koonalda Cave, suggested an altogether different adaptation process to the one offered up by Bowdler (1977). What is clear, however, from the contemporary Australian archaeological data is that its range and variety have become more complex, both in time and space, so the developments of explanatory models require a more sophisticated approach. This paper tackles the newer evidence, although the usefulness of their model is constrained by some inappropriate scalar comparisons.

Models are abstractions of reality that should specify the expected range of attributes used at various scales that can identify the archaeological correlates of human behaviours. O'Connell and Allen could have better dealt with this issue, explaining the links between the various scales of the observed phenomenon and archaeological processes as identified in the data sets. The use of ethnographic analogy is not a problem in itself and they admit that these are only guiding principles. Their assumptions are explicit, such as certain behaviours like ranking food, obtaining mates (and keeping them), and social interaction, which almost certainly took place in the past. However, there is a lack of sufficiently strong analogy to link the different scales of observation together.

For example, their model is based on small scale ethnographic observations that assume that some common hunter-gatherer behaviours are likely to be embedded in the archaeology. However, for their model to be useful, predictions must be made at commensurate scales between the observed and predicted archaeological, ecological, climatic and environmental phenomena. Identifying these is difficult, since a much higher resolution chronology from all early sites is needed for comparisons, not just climatic data. Their model does not sufficiently discuss its limitations, especially the fact that not all early sites can be commensurately compared given

the variable structural properties of each record. It is noteworthy that a majority – 63% – of their archaeological data come from rockshelters of various geologies.

This is crucial if, for example, the ‘narrow diet’ hypothesis of early colonists is to be supported. How ‘narrow’ is ‘narrow’? Detailing preservation conditions and site similarity are as important as identifying specific taphonomic differences that structure the archaeological evidence. Their discussion of ‘subsistence and diet breadth’ importantly includes plants, but, because evidence of plant use is rarely found, predictions about narrow diet breadths are difficult to make. This limitation compounds the difficulty of testing their model in any meaningful way so that the essence of a good model is lost.

Some of the conditional elements in the argument, such as colonisation and climate change, are discussed at both small (human response time) and large (Marine Isotope Stage) time scales. The ‘Archaeological Assessment’ section is problematic, as only the earliest dates are described. Significantly, other chronological markers of change and associated material culture need to describe the *process* by which settlement took place and can be understood. In other words, what are the range of archaeological *attributes* needed to identify the behavioural responses and how are these to be tested given the scalar problems of the data?

In their ‘simple scenario’, O’Connell and Allen hypothesise that human groups were small, highly mobile and focused on ‘sweet spots’. These foraging locations, once depleted of high ranked resources, were abandoned, with human groups moving to the next location rather than to the next lowest ranked resource in that location. Identifying such processes is again a scalar problem in the model, since the archaeological predications must be made at levels of human responses, not large scale climate cycles.

Further, sweet spots are where it is argued people pulse out in the good time and retreat in the bad. These sweet spots, however, will be equally affected by climatic downturns and become rather sour spots to live, with populations taking a hit. As is known, it’s the bad times that control population levels. The point here is that there is a wide range of geographic variability across the 11.5 million square kilometre Sahul continent. The variable effect of modern drought over regional areas of Australia is something that could be expected in the late Pleistocene but perhaps of higher frequency and magnitude. O’Connell and Allen seem to fall into the trap of assuming similarity across this vast Sahul continent in terms of climatic impact, although archaeologically it is quite variable during the LGM. However, as they recognise, it’s the embedded small scale, ENSO-like perturbations that dictate population levels. Thus, momentum for post-drought mobility will be linked to recovery rates of both population and the palaeoecology. Modelling the predicted archaeological patterns in both space *and time* is dictated by whether group relocation or accessing a lower ranked food can be identified archaeologically elsewhere. At this point we are back to the problems of scale and the resolving powers of the different records.

Overall it’s a positive attempt at model building. At least it has a greater level of detail, specificity and predictive power than other pan-continental model building attempts of the past.

## THE SHIPPING NEWS

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Joe Birdsell was a clever chap. He has had some bad press over the years, but I don’t think it is really deserved. This paper gives us another reason to dust off Birdsell’s reputation, as O’Connell and Allen return to a proposition he raised 35 years ago, namely that colonisation of the continent was effected with boats that were more sophisticated than those recorded ethnographically. In a related paper they published with Hawkes in 2010, O’Connell and Allen drew attention to Birdsell’s deduction, but not here, even though for other reasons they cite the chapter in which Birdsell made the point. It may seem inconsequential in the greater scheme of things, but we in the Birdsell Liberation Front are always watching ...

On a less flippant note, this paper (and indeed Birdsell’s original chapter and the writings of others on the topic of early Australian watercraft) raises a persistent question for us: the extent to which ethnographic analogy helps or hinders archaeological analysis and interpretation. The discipline goes back and forth on this issue without resolution, and as much as I agree with the thrust of O’Connell and Allen’s present paper, it seems to me that they want to have it both ways when it comes to the use of ethnographic examples, without any explanation of why they think it’s justified to do so.

The accumulating empirical evidence for the use of capable ocean-going watercraft in and around Sahul 40,000–50,000 years ago may confront our crustier colleagues, but it seems incontrovertible. This in turn has major implications for the initial movement of modern people out of Africa around the Southern Dispersal Arc, and all of it shines a very strong light on orthodoxies concerning human evolution and progress. This is truly exciting stuff! Yet if we are turning things on their heads in this way and, like O’Connell and Allen, explicitly ‘reject inferences ... that rely on assumptions about marine technology constrained by the Australian ethnographic record’, we have to ask why we still rely on ethnographic analogy as heavily as they do in other parts of this paper. If the boats were different back then, why assume that what was true of ethnographic subsistence and settlement patterns ‘was also true for early Sahul colonisers’? Why assume ‘foraging options ... were similar to those reported historically’? And so on, whether regarding women’s roles, population growth and density, or diet breadth.

I know we have to start somewhere, analytically-speaking, and O’Connell and Allen have certainly been clear about the assumptions they are making in this regard. As a discipline we’ve been insisting for a while now, though, that the past is a foreign country where they did things differently. It seems inconsistent to be so forceful about boats in this regard, but to make no comment about how appropriate it might be to use analogies in the other instances. This is especially true of matters concerning subsistence and settlement if, as O’Connell and Allen contend, ‘human populations ... remained much smaller than sometimes imagined’, and thus presumably also much smaller than the ethnographically-recorded (or at least guesstimated) populations whose foraging and habitation patterns underpin



the authors' optimal foraging approach. The authors themselves seem to confirm this point when they note that 'Historically known patterns of economy, technology and demography emerged only in the Holocene'.

Where to from here? I understand, and am philosophically inclined towards, the critique of inductive reasoning O'Connell and Allen made with Hawkes in 2010, which is behind their willingness to use ethnographic analogy in deductive theoretical models. As their own work demonstrates in this paper, though, it is induction from facts emerging from the ground in places such as Timor and the Bismarck Archipelago that is forcing the issue with boats. This is not the place for extended methodological discussion, but clearly we need to move beyond what are now very stale arguments about deductive vs inductive reasoning. Quality scholarship usually entails a mix of both, in proportion to the nature and quality of one's empirical data, the internal logical consistency of one's deductive theoretical models, and the fit between one's data and one's models. Despite their seemingly hard-nosed rhetoric, this is pretty much what O'Connell and Allen do in this paper. Deductive models can be based on almost any sort of information, including potentially quite misleading ethnographic analogy. One needs to make one's sources of such information clear. This the authors do. One should also make explicit the limitations of that information, in this case ethnographic analogy, which they don't (unless it's about boats). I may have become a bit obsessive about it, but in the end the omission really detracted from my reading of the paper.

### **DANCING ON PINS: TENSION BETWEEN CLEVER THEORY AND MATERIAL RECORDS IN AUSTRALIAN ARCHAEOLOGY**

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Thomas Aquinas (ca 1225-1274 AD), like many medieval philosophers, explored the nature of his world through detailed theorising – building models through logic and dialectic. This model building was focused on the consequences of phenomena believed to exist, with an emphasis on the exercise of reason but not of empiricism. Consequently, although Aquinas was a crucial contributor to modern thinking, his theorising involved questions as abstruse as whether there was defecation in Paradise or whether angels were material entities (often misquoted as how many angels could dance on the point of a pin). Those medieval debates displayed impressive intellectual dexterity, but in retrospect we can view the sophisticated thought as squandered on illusory concerns and fantastical visions of the world.

Comparable situations occur repeatedly in science, because diagnosing what happens in the world can be extremely difficult. Perhaps, as we archaeologists always claim, it is more difficult to interpret the human past from material residues than to do many other kinds of science. In any case, tensions between explanatory theory and the inferred pasts that are the subject of those explanations have occurred throughout the history

of archaeology. The issue is not that explanatory models are eventually refuted but that, at the time they were proposed, they sought to explain impressions of the past which were empirically dubious or even demonstrably incorrect. A well-known instance of this was Bowdler's (1977) coastal colonisation model, which at the time it was formulated was already contradicted by inland sites such as Kenniff Cave. Currently, the same tensions are displayed in the extended debates over whether the temporal coincidence between the arrival of humans and the extinction of megafauna is evidence of human culpability, despite the uncertainty about the timing of either arrivals or extinctions and the existing inability to adequately test human contributions (see Field and Wroe 2012). Of course these tensions can be extraordinarily productive, driving scholars to collect further data and to reconceptualise their analytical framework, but creative outcomes are most likely when scholars acknowledge and engage with the uncertainty and interpretative disagreements.

In this interesting paper O'Connell and Allen offer explanations that are both elaborate and novel for past cultural events and processes. The use of principles from behavioural ecology, and specifically OFT, is intriguing and clearly worthwhile. These approaches to understanding behavioural decisions are known to be powerful and will have intuitive appeal to researchers in the field. It is therefore of considerable value that they have chosen to explore the colonisation of Sahul in this way. My concern here is not with the application of ecological perspectives to the range-expansion of humans, since that seems both appropriate and inevitable, but to the question of what is being explained. In presenting their analysis they discuss archaeological data that supports a particular image of the colonisation process, specifically a late, rapid dispersion of *Homo sapiens* from Africa to Australia, scattering in low population densities, using only simple technologies across the continent and being subsequently largely isolated. Such an image is, and should be, contested. Certainly O'Connell and Allen (2004) have given their reasons for advocating a late date for colonisation, but the reconstructions of the colonising event by other researchers do not accord with theirs, and the evidence is clearly arguable. Certainly many of the components of the colonisation story told by O'Connell and Allen should be considered carefully. I alert readers to some of the obvious claims they may wish to consider.

#### *The Colonisation of Sahul Occurred 44-46 kya?*

This assertion ignores sites that have older dates claimed to be in association with cultural material, and fails to account for the sampling issues in investigating the archaeological record. Furthermore, it does not reflect the dating uncertainties of sites such as Parkupirti which may be older than 40 kya, but how much older is currently very poorly defined. Additionally, I and others have repeatedly made the case that the oldest sites found, especially in the south and inland of the palaeocoast, are not likely to date the arrival of humans but more likely reveal the time when population and landscape use had risen to the point of being archaeologically visible. So even if claims for earlier sites are eventually rejected and O'Connell and Allen are supported in their assertion that uncontested dates for *Homo sapiens* fall in a relatively narrow time range (44-46 ky BP), this must be considered a time when humans have established themselves across Australia, not necessarily

the time at which colonisation occurred. Consequently, assertions about the timing and rapidity of movement and the relationship of that movement to sea-level movement are speculative at best.

### *Occupation in the Early Millennia Involved Infrequent, Short-Term Occupation by Small, Highly Mobile Human Groups?*

This oft-cited claim is usually based, as it is in O'Connell and Allen's paper, on the discovery of sites that are individually small in area and/or low in perceived artefact discard rates. While this might be a consequence of small groups staying only for short times this literal interpretation is not without its uncertainties. Critical questions remain largely unanswered, such as what proportion of early occupation took place inside rockshelter sites, what proportion of early occupation sites are preserved, and what selection of archaeological sites have been recovered (see Langley *et al.* 2011). At any time period the interpretation of site numbers and sizes in terms of population and residential group size and mobility is tricky (see Attenbrow 2004), and for the earliest period of human occupation I suspect more discussion will be required to resolve what interpretations are sustainable.

### *Early Sahul Technology was Simple and Displayed Limited Variation through Time and Space?*

It is unfortunate that the authors do not elaborate and substantiate this statement, for it is contrary to my impressions of the evidence (see arguments in Hiscock 2008). Not only are the technologies revealed in early assemblages of flaked stone artefacts quite diverse across the continent, we now have evidence of bone and shell working creating tools/ornaments and of the production of edge-ground axes at or before about 40 kya. Using the late colonisation chronology advocated by O'Connell and Allen these data indicate a geographical and temporal diversity of technologies immediately after the dispersal of humans across the continent. Additionally the notion of simplicity is undemonstrated, and relies on out-dated depictions of stone assemblages that have long been challenged. O'Connell and Allen conclude their paper with an admission that the pelagic fishing, and the elaborate gear that probably supported it, is hard to explain in their model. Indeed that does merit further investigation, because it is one possible pointer to a colonisation-era cultural system in which population might have been greater and empty territory often less available than their model implies.

These are just a few of the inferences about the past that O'Connell and Allen present and attempt to explain. Each is debatable and warrants investigation, because, while the ecological approach applied in the paper is intriguing, if it is applied to visions of the past that are more fantastical than evident, it too may be squandered.

## **BOATS, STONES AND RISKS**

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This interesting model combines evolutionary ecology with the prime mover of population pressure invoked to kick off the

human colonisation of Sahul. I question whether population pressure was necessary to 'push' groups only targeting resources with high returns. More likely, once they left Africa, colonising populations would have been 'pulled' by the high ranked resources in adjacent unoccupied land and seascapes. Given the nature of tropical environments, especially in Sahul, such food sources tend to be relatively sedentary (e.g. plants, molluscs, varanids and arboreal mammals). Their rapid depletion would necessitate frequent moves, which may help explain the use of boats, especially because these could reduce the stress of constant mobility on women and children. Marine technology would be better incorporated into the model by acknowledging its initial role as transport required by a subsistence focus on high return resources, rather than for fishing, which is perhaps better conceived as a by-product.

Continuing the theme of technology, the authors rightly attack northern latitude scholars, such as Mellars, who continue to make simplistic, direct links between the evolution of human intelligence and the nature of stone tool assemblages, despite the contradictory archaeological record of *Homo sapiens* in Sahul which is dominated by a simple flake-core technology (cf. Brumm and Moore 2005). Instead, O'Connell and Allen try to show that a better way to explain human behavior is to study the relationships between 'the ecological context and foragers' intellectual solutions to the problems of making a living'. Whereas I agree that the particular configuration of stone technology is the consequence of people finding optimal solutions to problems posed by both their physical and social environments (Torrence 1989), I run into difficulties with the specific hypotheses they put forward, although I appreciate these are preliminary attempts to exemplify their proposed approach.

Accurate definitions underpin all good modelling. Tools and technology refer to very different entities. Technology encompasses 'physical setting, social context, actors, knowledge, energy sources, raw materials, tools, actions and outcomes' (Torrence 2001:74; cf. Bleed 1997). Boats are not necessarily 'complex' tools, but the knowledge and skills required to navigate across the ocean may be very sophisticated. Does this mean that pelagic fishing is necessarily more complex than the behaviour and knowledge associated with successfully hunting kangaroos? When O'Connell and Allen ask 'why is Pleistocene Sahul technology simple in some settings but complex in others', they are actually mixing up stone or shell *tools* with fishing *technology*.

For the sake of having something concrete to observe, archaeologists focus on the tools themselves rather than the technology. Considering pursuit tools, the opposite of what O'Connell and Allen predict is well-known. For recent hunter-gatherer tool-kits, there is a strong negative correlation between diet breadth and the complexity of both individual tools and assemblages (Bamforth and Bleed 1997; Torrence 1989). The reason for this pattern is that a wide range of complex and reliable tools are required in situations where there is a high probability of failing to acquire adequate resources. High risk is often, but not always, correlated with specialised diets: environmental parameters, such as seasonality, are also important. Where a broad range of resources are targeted, risk of failure is generally low and less time and energy are invested in tools. In these situations generalised tools further reduce costs. I suspect the same is true for processing and/or handling tools. Where the

risk of failing to acquire sufficient resources is high, such as with specialised diets, complex tools are required.

A further problem with the modelling in this article is that the majority of tools used directly in subsistence tasks during the late Pleistocene were made from plant materials rather than stone. The primary role of flaked stone was for manufacturing these tools rather than in pursuit or handling of resources (stone points are only common later in time). Since manufacture can easily be embedded in other activities, the risk of failing to have the necessary tools prepared in the required timeframe would have been low. Consequently, the stone tools for producing other tools were fairly simple, although we need better use-wear studies to investigate if and how they were hafted.

Given the scarcity of evidence, I question O'Connell and Allen's emphasis on pelagic fishing, especially as a planned subsistence activity, rather than as an adjunct to travel. If boat technology was created and maintained primarily for voyaging, then dropping a hook and line over the side would not have been a costly or risky undertaking and so the unpredictable returns would have been inconsequential. Given the paucity in late Pleistocene contexts of fish remains and associated tools, it is difficult to be certain about the relative contribution of pelagic fishing to the diet, but I suspect that boats were primarily designed for transport and exploration, and not for direct capture of resources.

This brings up a major question raised but not answered in the paper: despite the lure of high ranked resources lurking just over the horizon, why did people risk deep-sea journeys? Modelling the costs of search and pursuit takes us a long way toward predicting human subsistence and consequences for colonisation, but a more complete picture requires understanding the factors behind taking and managing risk.

## COLONISING SAHUL

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A vague notion of 'wanderlust' seems to be the driving force in many narratives about hominin migration (e.g. Dennell and Roebroeks 2005), but, true to the zeitgeist, O'Connell and Allen have shown us that wanderlust is all about food. The strength of behavioural ecology is the explicit nature of the underlying assumptions and the clear connection between forager theory, predictive statements and archaeological evidence. Summarising several optimal foraging models, O'Connell and Allen conclude that optimising hominins are pulled from patch to patch by the serial depletion of highest ranked resources. The logic of their scenario is straightforward: the archaeological record shows that humans colonised Wallacea and Sahul, and the theoretical model stipulates that forager movement is linked to exploitation of highest ranked prey, therefore colonisation was driven by the pursuit of highest ranked prey. One might question whether certain assumptions of optimal foraging models – for example, that foragers have perfect resource knowledge and the perfect ability to exploit it – would apply to the first wave of colonists to cross the Wallace Line, but the successful colonisation itself

might be *de facto* evidence that the costs of imperfect knowledge were not prohibitively high. O'Connell and Allen posit that, after colonisation, movement between patches in pursuit of highest ranked prey became the norm as foragers made nearly-continuous readjustment to unstable climatic conditions.

Issues of colonisation rates and routes aside, the model succeeds or fails by the prediction that the Pleistocene menu was dominated by highest ranked prey. The data reviewed by O'Connell and Allen are sketchy and, in most cases, the regional and local subsistence resources surrounding early archaeological sites are not sufficiently understood to warrant informed scenarios of resource ranking (for instance, where are the Australian megafauna?). Perhaps in light of this limitation, O'Connell and Allen nod to the established tradition of continent-wide generalisation and essentially treat all of Sahul as if it were one patch. The continental zones with the greatest density of highest ranked resources are termed 'sweet spots' and, consistent with the model, the empirical evidence shows that most early archaeological sites in Sahul are comfortably nestled within them. However, it is not clear that the empirical pattern identified by O'Connell and Allen is distinct from patterns that might be predicted from the unspecified prior narratives they term 'minimalist'. The issue here is scale, and while the data marshalled by O'Connell and Allen seem to support their scenario, the true test will turn on detailed regional and site specific reconstructions of palaeoclimate and subsistence.

O'Connell and Allen conclude their discussion with the generalisation that simple technologies should correlate with diets focused on highly ranked prey because greater investments in finding food outweigh technological improvements in obtaining it. This is a surprising observation, as many complex forager technologies – particularly in North America and Eurasia – are associated with the pursuit of highly ranked prey species. Since a technological approach is an unarguable aspect of hominin adaptation through the 'extended phenotype' (after Boone and Smith 1998), a key challenge is to differentiate the attributes of a technology that directly improve reproductive fitness from those that are, at best, only indirectly related to adaptation – what Bettinger (1991) has characterised as 'adaptive' and 'adjustive' traits, respectively.

For example, evolutionary models of flaked stone technology implicitly assume that the tool as a whole was adaptive and that inadequate tools directly compromised fitness (Surovell 2009; Torrence 1986). But one might infer from O'Connell and Allen that the only truly adaptive part of a stone tool was the sharp edge, and the shape of the stone bearing that edge – and thus the nature of the reduction sequence that created the shape – was unimportant for reproductive fitness, at least in the pursuit of highly ranked prey. Most evolutionary narratives suggest that our early hominin ancestors improved their fitness by creating sharp edges for food procurement through least-effort flaking (Ambrose 2001). This approach was exclusive among pre-modern hominins for at least 1 my. The concern with tool shapes that subsequently emerged among some (but not all) modern human foragers was perhaps only an 'adjustment' within the extended phenotype and was only indirectly related, or perhaps unrelated, to hominin fitness; direct fitness requirements continued to be met through simple, least-

effort edge production similar to that seen among the modern human colonisers of Sahul. Indeed, most complex stone technologies worldwide, associated with a variety of foraging strategies, included a least-effort reduction trajectory (often under-analysed by archaeologists and sometimes ignored altogether) alongside more complex ‘add-on’ approaches to modifying stone shapes (e.g. Moore *et al.* 2009). Indeed, once variables such as manufacturing skill, time investments and ancillary costs of hafting are considered, it is rarely self-evident that tools made by complex approaches significantly improved the handling efficiency of the tools they augmented (e.g. backed microliths vs flakes; tula adzes vs retouched flakes, stone-tipped spears vs single-piece wooden ones) (cf. White 1977). Perhaps the development of flaked stone tool complexity beyond least-effort stone flaking had negligible effects on the choices directly modelled by cultural ecologists, and instead reflected adjustments to other parts of the foragers’ extended phenotype.

## AND THE ANSWER IS 42 ...

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The final line in the *Restaurant at the End of the Universe* quote is a brilliant beginning to this interesting and ambitious paper by O’Connell and Allen and speaks of the likely explanations for the initial forays into, and expansion across, Sahul. That ‘42’ may be the answer to the meaning-of-life question, also from this series, and perhaps the answer to the question of human arrivals in Sahul – tells us something we already know – that the fun is in the journey. I suspect all aspects of this model will be debated and tested for a long time to come, but the beauty of it lies in its breadth and the challenges that will ‘feed’ a whole generation of archaeologists to come.

Having been engaged for some time in the ‘seemingly eternal’ and faith-based debate about megafauna and its relevance in the archaeological sphere, there are some issues I would like to expand on that may contribute to the discussion. O’Connell and Allen argue that one of the drivers for rapid expansion was ‘serial depletion of high ranked prey’, which on face value would support (and possibly revive) Flannery and colleagues’ approach to the extinction process. The Steve Wroe-coined phrase comes to mind – ‘spear-wielding hoards hacking their way through startled megafauna’ – but this time let’s throw in some of those extant species. If people arrived on the northwest coast of the continent, the pickings may have been sparse indeed and this alone would have accelerated movement through the region. Entering via the Birds Head of New Guinea may have been a different proposition altogether. While the suite of animals and plants that were encountered were entirely new, the ‘package’ of knowledge brought with them may have enabled small populations to quickly adapt to the new landscapes they encountered (as argued by Denham *et al.* 2009). Certainly the discovery of *Pandanus* and yam in the early occupation horizons from the Ivane Valley in the eastern highlands of New Guinea points to this capacity

(Summerhayes *et al.* 2010). The observations by Geoff Hope on *Cyathea* above the tree-line in this region also speaks of important carbohydrates being readily accessible to, if not exploited by, people in the recent past.

As always it is the preservational limitations of Australian fossil sequences that inhibits a more fruitful exploration of the ideas concerning exploitation of many animal species in the Pleistocene record. What has become clear in the last few years is the lack of evidence for a human-megafauna association, despite the limited evidence for a temporal overlap. Our scrutiny of the fossil record places only a handful of megafauna still wandering around the landscape post-50 ka. The spatial distribution of these remnant megafauna is unclear and does not seem to be continuous across the landscape. Yet this has not inhibited the supporters of human-driven extinctions to push their particular barrow. The most recent case is the report from Lynch’s Crater (Rule *et al.* 2012) where they argue, on the basis of evidence from a sediment core (analysing *Sporormiella* spores, charcoal and summary diagrams of sclerophyllous vegetation), that the extinction of megafauna at 41 ka (where this date comes from in their paper is unclear), drove changes in vegetation composition because of ‘relaxed pressure’ on vegetation. *Sporormiella* spores are used as a proxy for megafauna (but see critique by Feranec *et al.* 2011). Despite these largely unproven assertions about the timing of megafaunal demise, and the dismissal by Rule *et al.* of climate change as an important factor, it now seems clear to many that megafaunal extinctions cannot be considered an archaeological question. For the most part, megafauna played no significant role in the success or otherwise of colonising populations in Sahul (Field and Wroe 2012).

I believe one of the important points that emerges from this paper is the suggestion that we should broaden our gaze when it comes to defining the particular conditions that allowed/facilitated the movement of people to Sahul for the first time. While to some extent we are still constrained by the current sequences and dates for the earliest sites, O’Connell and Allen challenge us to move away from the ‘lowest sea-level moment’ when looking at the first arrivals. We are in no doubt that the first Australians were behaviorally modern and, as such, modes and routes of travel may have been more complex than we can currently predict. One point that has received little attention is the importance of access to freshwater, a point raised by Horton (1984) some time ago as it related to the faunal extinction process. It would seem that the same principle may apply to colonising humans, especially on the Australian continent. It is a fact that MIS-3 was not the relatively stable climatic period as asserted by some authors (e.g. Miller *et al.* 2005 and others), and clearly evident from the Antarctic ice cores (EPICA Community Members 2006). This instability must have been a determining factor in the presence or absence of free water in the more arid regions and also in the timing of movements across the Australian continent. The unpredictability of sudden swings in climate may have meant that the earliest archaeological records reflected the necessarily ephemeral nature of occupation across most landscapes.

So much to think about – this paper has given us a compass: I look forward to seeing how it is used.

## RESPONSE

This paper developed as a Forum piece at the suggestion of previous editor Sean Ulm, with continued positive input from Lynley Wallis and Heather Burke. We thank them, and all the contributors who took the time to respond.

Codding and Bliege Bird add an important dimension to our argument by distinguishing the spatial (and by implication temporal) scales at which foraging decisions operate – among resources within patches, between patches within habitats, and between habitats on continental and sub-continental scales.

Davidson, Dillehay, Horton and Keegan note that our model makes no reference to considerations other than those related to subsistence that might constrain movement into and across Sahul. Our reductionist approach is intended to develop falsifiable propositions about human behaviour in the distant past – it is deliberately simplistic. It ignores social (except gender) and other potential complexities, not because they were unimportant, but because sidestepping them for the moment simplifies hypothesis testing. We do not expect subsistence-related decisions to predict or explain all aspects of past behaviour. Instead, we seek to determine how much of that behaviour can be anticipated in those terms, and to highlight what is not accountable in this way. The exercise is intended to be iterative – we simply picked the easiest, theoretically and empirically best-grounded place to start. We recognise and share Cosgrove's concerns about reconciling data on different scales. To maintain simplicity and conserve space this was not the place to pursue them. We use Oxygen Isotope Stages and the like as familiar conveniences, but, with Cosgrove, we believe that shorter-term climatic oscillations, like ENSO – recognisable in human life spans – are the significant modifiers of foraging behaviour.

Davidson and others note that the model assumes what ecologists refer to as 'complete' knowledge of foraging opportunities, and the costs and benefits of exploiting them. The forager's task of accumulating this knowledge is probably less daunting than they imagine, especially at the resource and patch scales on which short-term decision-making operates, and especially with humans who can exchange knowledge through language. Foragers' abilities to assess the costs and benefits of immediate subsistence-related opportunities and act on them optimally is clearly indicated by the past four decades of ethnographic research conducted within the framework of behavioural ecology in Africa, the Americas and Australia. That same research underlines foragers' capacity to adjust to changes in those constraints in real time. Davidson's reference to Alyawarra women's use of O'Connell's vehicle to reach and exploit high ranked resources (mainly geophytes, *never* low ranked seeds) in distant patches illustrates that ability. In archaeological perspective, such adjustments, including those involving previously unfamiliar resources, should be 'instantaneous'. All else being equal, we expect delays in the exploitation of new opportunities to reflect ongoing assessments of economic and ecological trade-offs, not slow learning. Tree and grass seeds enter Australian diets late not because of a long-term lack of relevant knowledge, but because the costs of exploiting them are high relative to those associated with the use of many other foods.

Lilley chides us for what he sees as an inconsistent appeal to ethnographic information. In our view, the question isn't

*whether* to appeal to it, but *how*. Direct-historical analogies of the kind once popular among Sahul archaeologists are now often seen to be inappropriate, especially in late Pleistocene settings. But rejecting *any* reference to human behaviour in the present makes the archaeological enterprise entirely descriptive and chronological. Simply accumulating more archaeological data and 'discussing' them, the gambit Hiscock advocates here and elsewhere, is a retreat to mindless empiricism and baseless speculation, a Luddite strategy that sharply restricts archaeology's potential to reconstruct and explain the human past. Our approach appeals to a uniformitarian theoretical framework, capable of generating testable predictions about many aspects of behaviour and shown empirically to be applicable across a wide range of organisms, humans and fruit flies alike (*pace* Horton). Among its most important virtues in this context is its ability to generate predictions about patterns in human behaviour unrepresented in ethnography. Codding and colleagues' (2011) recent work on the sexual division of labour, specifically on the circumstances under which men's prey choices might be expected to trump women's as a determinant of residential site location in middle and low latitude habitats, is a good example and one that we expect to build on in future work.

On the relationship between technological complexity and diet breadth: our sense of the global record, especially across the late Pleistocene and Holocene, differs from the views expressed by Torrence, Dillehay and, to some degree, Moore. Patterns in Australian lithics are clearly consistent with our position, as are those in, for example, western North American Paleoindian vs Archaic contexts. Broader review is beyond us here, but this difference of opinion may be at least in part a matter of definition. For reasons stated in our paper and developed at length in the sources we cite, we expect greater diet breadth to be associated with increased complexity in *handling* technology, defined as that connected with the post-encounter procurement of prey, including all tools used in the pursuit, capture and processing of those resources. We will explore this issue at greater length elsewhere.

Minor points: (1) Our previous arguments for developed watercraft did not rely heavily on the presence of pelagic fish bones or fishing tackle in pre-LGM sites, but rather on the need to maintain biologically viable colonising groups across large distances. Nevertheless, the availability of such craft would have facilitated pelagic fishing as a consequence; (2) Our model is not simply an appeal to population pressure as a catalyst for movement, but instead draws attention to the complex interplay between unevenly spread populations and resources, and the choices made about those resources in relation to such movement; and, (3) Despite frequent appeal to risk avoidance as a determinant of pre-European technological diversity across Australia, the link between the two remains poorly developed (White 2011). Recent research from the perspective of behavioural ecology clearly indicates that the significance of risk varies across habitats and economies and (importantly) by gender (e.g. Hawkes and Bliege Bird 2002). We look forward to further debate on this issue. For the moment we side with Field and think the answer to this and wider questions might still be '42'. We trust that this piece opens a few doors.

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