

Defending the Defensible or Offending the Sensible? A Response to Nunn & Hunter-Anderson

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‘Experiment adds to knowledge, credulity leads to error.’
(Arabic proverb)

I commend Nunn and colleagues for bringing to the forefront of archaeological discourse in the Pacific their intriguing ‘AD 1300 event’ model. But the response of Nunn & Hunter-Anderson (2010) to my original critique (Fitzpatrick 2010) does little to temper my position regarding Palau and the effects that climate change may have had on Pacific societies in general. Although I have limited space with which to respond to their rebuttal, I will do so by addressing several of their key points and highlighting a few others not mentioned.

To summarise, the model they propose is predicated on three major premises: 1) similar climate fluctuations during the interval between the proposed Medieval Warm Period (MWP) (c. AD 700–1250) and Little Ice Age (LIA) (c. AD 1350–1800) occurred simultaneously throughout the Pacific Basin; 2) these fluctuations caused societal responses which were catastrophic in nature; and 3) that these responses were similar and widespread across the Pacific Basin regardless of colonisation history, geography, geology, oceanographic conditions, and so on (e.g. Nunn 2000, 2007; Nunn *et al.* 2007). In essence, since all of these premises must be found in both palaeoclimatic and archaeological records to validate the model, the absence or inverse of these predictors thus invalidates it.

LAST MILLENNIUM CLIMATE CHANGE IN THE PACIFIC: CONTRARY EVIDENCE

Nunn & Hunter-Anderson (2010: 5) would like us to believe that there is ‘abundant climatic and societal data in favour of a warm MWP [dry] and cool [wet] LIA in the Pacific Basin’. It is true that some scholars support this general trend, but it is also important to note that there is substantial contrary evidence besides that described by Allen (2006) and me. As Hunt (2006: 677) notes ‘there is no universal agreement as regards the precise dates of these events, and they were not episodes of continuous above or below average temperatures. In fact, controversy

continues to exist over the reality of both the MWP (a.k.a. Medieval Climatic Anomaly [MCA]) and LIA. In an extensive simulation model meant to ‘identify sustained climatic fluctuations’, Hunt (2006: 691) did in fact find ‘climatic anomalies over the last millennium’ but that ‘synchronicity was not obtained on a global or even a hemispheric basis.’ He further concluded that ‘time averaged surface temperature anomalies of specified magnitudes produced the surprising outcome that such events were not generated over the majority of the globe, expressly the oceans and most of the southern hemispheric land areas’ (Hunt 2006: 691). Mann *et al.* (2009: 1258) found that the ‘MCA and LIA reconstructions are most reliable over the Northern Hemisphere and tropics, and least reliable in the Southern Hemisphere, particularly in the extratropics.’ It is interesting to note too that Nunn & Hunter-Anderson (2010: p. 5) cite Broecker (2001) as being in favour of a global extent of the MWP, but he is actually more cautious than this, noting that ‘the case for a global Medieval Warm Period admittedly remains inconclusive’ and requires additional data (Broecker 2001: 1499). This echoes the sentiments of Cook *et al.* (2002: 3) who documented tree-rings from New Zealand over the last 1100 years, finding that the ‘expression of the MWP is not homogenous in time. Rather, it is composed of two periods of generally above-average warmth, AD 1137–1177 and 1210–1260, that are punctuated by years of below-average temperatures and a middle period that is near average.’ Goosse *et al.* (2004) in their climate simulations suggest a delay in temperature evolution of 150 years between hemispheres. In a more recent publication, Esper & Frank (2009: 272) note that ‘we currently do not have sufficient widespread high-resolution proxy data to soundly conclude on the spatial extent of warmth during MWP.’

Although Nunn & Hunter-Anderson are critical of earlier references such as Cobb *et al.* (2003), Jones *et al.* (1998) and Jones & Mann (2004), the list of more recent publications referenced by Nunn & Hunter-Anderson (2010) supporting evidence for a MWP and LIA in the Pacific fails to cite Sachs *et al.* (2009: 523) who specifically investigated Pacific Islands and found ‘dry climates on Washington Island and in Palau and a wet climate in the Galapagos between about AD 1420–1560/1640’, i.e. Palau was not necessarily wetter during the LIA as stated by Nunn & Hunter-Anderson (2010: figure 2) and the

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Galapagos do not follow the same trend, perhaps because the intertropical convergence zone (ITCZ) was ‘south of its modern position for most of the past millennium’ (Sachs *et al.* 2009).

Newton *et al.* (2006) in their analyses of the Indo-Pacific Warm Pool (IPWP) from the Makassar Strait (Indonesia) find increased precipitation during the LIA that ‘may represent a continuation of the Holocene southward migration of the ITCZ into the last millennium’, as found by Sachs *et al.* (2009) in the Galapagos. ‘Specifically, the development of more arid conditions in the northern tropics/subtropics and wetter conditions south of the equator during the LIA is indicative of a pronounced and rapid southward displacement of the ITCZ and associated band of high precipitation during this time period’ (Newton *et al.* 2006: 3). Thus, there are hemispherical differences expressed in the Pacific and globally during the proposed LIA. Although Nunn & Hunter-Anderson’s figure 1 attempts to make a correlation between their date range interpretation of the MWP and LIA in the Pacific Basin with palaeoclimatic data from numerous locations around the world, it is telling that of the 50 events shown, 36 (72%), and possibly 40 (80%) if we include Kenya on the equator, are from the northern hemisphere with the remainder being from southern Africa/Antarctica, which would seem to follow predictions from other researchers (Hunt 2006; Mann *et al.* 2009).

My general perception of the literature on the global presence of the MWP/LIA, and on its local effects, is that it is *not* wholly accepted by scholars. In places where it seems to be sufficiently demonstrated (including parts of the Pacific), the time interval (decadal or sometimes centennial) during which it occurred is unclear, as is the degree of change to local temperatures and rainfall. What must also be considered is the varied geological history of each island/archipelago where subsidence and/or uplift would have affected the influence of any change in hydro-isostatic sea level (see Dickinson, 2000; Dickinson & Athens, 2007). While Nunn (1998) has proposed and continues to argue for a lower sea level in the Pacific during the LIA,

... the search for such fluctuations appears to be led by the *a priori* assumption that sea level during the LIA must have reached a lowstand. Inspection of the vertical errors and age uncertainties of the sea-level reconstructions demonstrate that such a lowstand cannot be proven. Although sea-level fluctuations during the MCO and LIA cannot be ruled out, the regional expressions of the MCO and LIA make it plausible that steric change was responsible for significant regional sea-level variability (Gehrels 2010: 30; see also Gehrels 2001 critique of Nunn 1998).

Indeed, we should be wary of Nunn and colleagues bearing gifts ‘which find unequivocal evidence for both

the MWP and LIA’ across the Pacific Basin (p. 5) that exclude these variable influences.

EVIDENCE OF SETTLEMENT CHANGES AND CONFLICT?

In regards to Palau, Nunn & Hunter-Anderson suggest I am misguided in trying to persuade readers that the AD 1300 event is wrong because the Rock Islands were settled before and after this date. But I simply pointed out that one of the key components of the model – the movement of peoples from coastal to inland (and defensible) locations – did not occur here in this way. Nunn & Hunter-Anderson *then* argue that even if people actually moved to locations with better coastal access instead of inland, this still would not refute ‘the suggestion that this change was ultimately a response to the independently verifiable climate change that took place at this time’ (p. 8). Given the small size of most Rock Islands and the lack of living space, inhabitants would be inherently tied to the coast anyway. This type of logic is endemic to their argument – in essence, it is a solution in search of a problem because the data can always ‘fit the model’.

The notion of increased conflict in Palau around AD 1300 is also tenuous. Nunn & Hunter-Anderson (2010: p. 9) are all too happy to report warfare starting in AD 1250 (figure 2), but claim use of the earlier earthwork sites (*ca.* 600 BC) might be a reflection of competitive territoriality, as a way of ‘detering land-taking’ without violence. However Liston (2009) has outlined other probable uses including defence both during earlier phases and after stonework village construction begins *ca.* AD 1000–1200 (see also Liston & Tuggle 2006). A defensive purpose is supported by the presence of associated features such as embankments and ring-ditch palisades. Regarding traditional stonework villages, Nunn (2007:156) implies that these sites were constructed in the Rock Islands during the LIA because they were more ‘readily defensible’. But as Masse *et al.* (2006:112) note, ‘[t]he dearth of archeological remains on Babeldaob dating to the turn of the second millennium AD is countered by only a slight rise in the frequency of Rock Island cultural deposits, but not enough to account for a wholesale migration of the Babeldaob population to the smaller islands.’ The sheer number of stonework villages on Babeldaob alone ($n = \sim 175^2$ or one village per $\sim 2 \text{ km}^2$), not to mention numerous others found in Koror, Peleliu, Angaur, and the Rock Islands, implies that population growth was increasing along with the establishment of firmer territorial divisions post-AD 1200. That Palauans seem to have emphasised the defence of individual villages after this time could easily be a natural outgrowth of population expansion and more rigid provincial development, rather than conflict deriving from food shortages which would have prohibited population growth.

2 Data from the Palau Bureau of Arts & Culture GIS database.

Nunn & Hunter-Anderson (2010: p. 8–9) correctly note that there is ‘no archaeological evidence of violence, no weapons and no traumatic injuries among the few known burials excavated from Earthwork-Era sites’ (i.e. it *only* happened after this period ended). In fact, this same comment could apply to burials found on Palau over the last three millennia (Fitzpatrick & Nelson 2008)! Any suggestion that warfare was endemic at certain times is based on how one interprets the building and location of monumental architecture and how far one is willing to project what is seen ethnohistorically and ethnographically into the past. If earthwork terraces represent a defensive feature we could easily attribute these to endemic warfare as well because such complexes number in the dozens and cover an estimated 20% of Babeldaob’s land area (Liston 2009).

I refer again to another casuistic point championed in the AD 1300 event model and that is the suggestion that cannibalism became widespread as a result of climatic changes (Nunn 2000, 2007: 157–160; Nunn *et al.* 2007). Although cannibalism was noted ethnographically in Palau by Parmentier (1987), the fact that Nunn & Hunter-Anderson (2010) do not respond to my assertion that this is virtually undocumented archaeologically in the Pacific (despite Nunn’s repeated references to this in the literature), demands either proof or retraction.

RESOURCE DEPLETION AND SUBSISTENCE CHANGES

Nunn (2000: 726) suggests that after AD 1300 there was widespread decline in the food resource base in the Pacific Islands due to environmental changes which ‘led to increased competition among human groups for what remained, and this, in turn, caused conflict.’ Nunn & Hunter-Anderson continue to cite Masse *et al.* (2006) as evidence for this in Palau, but they ignore several important facts and references. These include Masse *et al.*’s (2006) use of ¼ inch (6.35 mm) mesh for recovery of faunal remains which is known to severely underestimate smaller individuals and taxa (and consequently, influence interpretation of particular fishing strategies). Fish remains recovered using 1/8 inch (3.2mm) mesh at the Chelechol ra Orrak site in the northern part of the Rock Islands (Fitzpatrick & Kataoka 2005) contrast with those reported by Masse *et al.* (2006). A statistically robust sample from the same site (Fitzpatrick *et al.* 2011) suggests among other things that: 1) fishing gradually declined here over the last 1500 years; 2) taxonomic richness did not increase over time; and 3) the number of fish families remained fairly consistent. This is slightly different at Ngelong on the southernmost island of Angaur where the numbers of fish and molluscs seem to increase over time (Clark & Wright 2005; also Fitzpatrick & Donaldson 2007: 919). Clark’s (2005) work on Ulong Island indicates that fish generally increase in abundance (MNI and NISP) over a 3000 year period while molluscs

decrease. Overall, at Ulong there is ‘no discernible trend of any vertebrate species decreasing in numbers through time; in fact the opposite appears to be true’ (Fitzpatrick & Donaldson 2008: 921). It is also important to note that while Masse *et al.* (2006: 125) suggested that the decreasing size of the mollusc *Strombus gibberulus* on Ngemelis was related to overharvesting possibly related to the onset of the LIA, a more recent study of the same species at Chelechol ra Orrak, 44km away (but with a significantly larger sample spanning three millennia), suggests the opposite – *S. gibberulus* actually *increased* in size (Giovas *et al.* 2010), including during the interval generally coinciding with the proposed MWP/LIA transition. In terms of pig, although Nunn & Hunter-Anderson prominently list these as having been extirpated at around AD 1450 (figure 2; citing Masse *et al.* 2006), again assigning this to the onset of the LIA, Masse *et al.* rightly take a more circumspect approach given the paucity of pig remains found in Palau and issues with dating, noting that ‘any attempt to assign a likely role for the LIA in postulated Micronesian pig extirpation, including Palau, is premature’ (2006: 121). Giovas (2006: 87) finds a ‘correlation between island size and the likelihood of extirpation for the Polynesian pig’, and this is something to which the Rock Islands of Palau would also be susceptible.

My point here is that temporally broader samples from sites within the same archipelago show dramatically different (and statistically significant) results. That Nunn & Hunter-Anderson attribute changes in fish taxa reported by Masse *et al.* (2006) to a ‘trend away from pursuit of marine resources as protein supplements toward pursuit of species that yield maximum calories per unit effort’ resulting in a decline of ‘land-based food resources’ is simply unsupported based on other lines of empirical zooarchaeological evidence. The fact remains that more archaeofaunal data recovered with smaller mesh screens (1/8 inch [3.2mm] and 1/16 inch [1.59mm]) are sorely needed across well stratified sites in the Pacific, including Palau, to justify any interpretation related to climate or social change around AD 1300.

AN END TO OCEAN VOYAGING?

In my paper (Fitzpatrick 2010) I referred to the quarrying of Yapese stone money in Palau (Fitzpatrick 2008) and the *sawei* exchange system between central and western Micronesian atolls with Yap as evidence that inter-island voyages persisted post-AD 1300. Given that these activities were taking place in the mid-1800s (with Europeans seeing and later becoming involved in stone money production; Fitzpatrick 2008), there is no reason to believe that sailing between the two groups had ceased for the interval c. AD 1300/1500–1800s. In the case of the *sawei*, I find it curious that Nunn & Hunter-Anderson (2010: p. 10) have failed to cite Descantes (1998) – the only authoritative work on archaeological manifestations of this exchange

system – who suggested, based on radiocarbon dates from Mog Mog (Ulithi), that it may have began as early as perhaps AD 600–800. They also downplay these particular seafaring connections as being only ‘instances of localised inter-island voyaging’ of ‘relatively short sailing distances’ (100–250 nautical miles), inferring that the model only pertains to longer-distance voyaging (p. 10). Why is it then that Nunn (2000: 733–734) himself argues that the model also applies to cases of ‘within-archipelago voyaging’ and a reduction in the ‘comparative frequency of routine ocean travel, both within and between island groups’ (Nunn *et al.* 2007: 397)? In any case, the Chathams, used as an example by Nunn (2000), are only 350 nm from New Zealand – is the additional 100 nm the boundary to which voyaging is a more formidable barrier? Regardless, distance is not always the best indicator of effort required to sail from one place to another – the nature of oceanographic conditions (winds, currents) were far more important, and so a longer voyage may actually take less time and require fewer provisions than a shorter one – the difficult currents surrounding Palau is a good case in point (see Callaghan & Fitzpatrick 2007, 2008).

CONCLUSIONS

Nunn & Hunter-Anderson (2010: p.1) encouraged me to present a ‘great deal of archaeological and palaeoenvironmental evidence that suggest the inverse of the proposed model.’ Given space limitations, I can only respond to some of the Pacific palaeoclimate and Palau archaeological data, although the list of quandaries surrounding the

model is much longer and will require a lengthier piece to describe fully. I would like to stress again that I am not advocating an approach that dismisses the role that environment may have played in structuring the behaviour of ancient cultures, nor do I reject the possible occurrence of major climatic anomalies such as the MWP and LIA in the Pacific (that is for palaeoclimatologists to decide). We should not, however, consider the Pacific Basin as a virtual *tabula rasa* on which can be superimposed a blanket model for societal change based on chronologically ambiguous and poorly defined (or perceived) climatic anomalies sprinkled with occasionally dubious and selectively favourable archaeological data. As my Figure 1 illustrates, many of the so-called ‘events’ in Palauan prehistory can be extended before and/or after the ‘AD 1300’ window and one gains a much different perspective when these data are included.

Using Nunn’s ‘AD 1300 event’ as a model, can we without a doubt say that: 1) similar climatic fluctuations occurred throughout the *whole* Pacific Basin around and after AD 1300?; 2) these fluctuations caused societal responses which were on the order of catastrophic?; and 3) these responses were similar and widespread across the region? Given the empirical data now available, the answer for Palau is an unequivocal ‘no’ to each. Overall, the ‘AD 1300 event’ operates as a temporally broad target that is moved to catch arrows tipped with favourable data and avoid those which are not. Such a method cannot be regarded as scientific.

It is clear from the literature to date that there is no overall consensus on whether the MWP and LIA was a

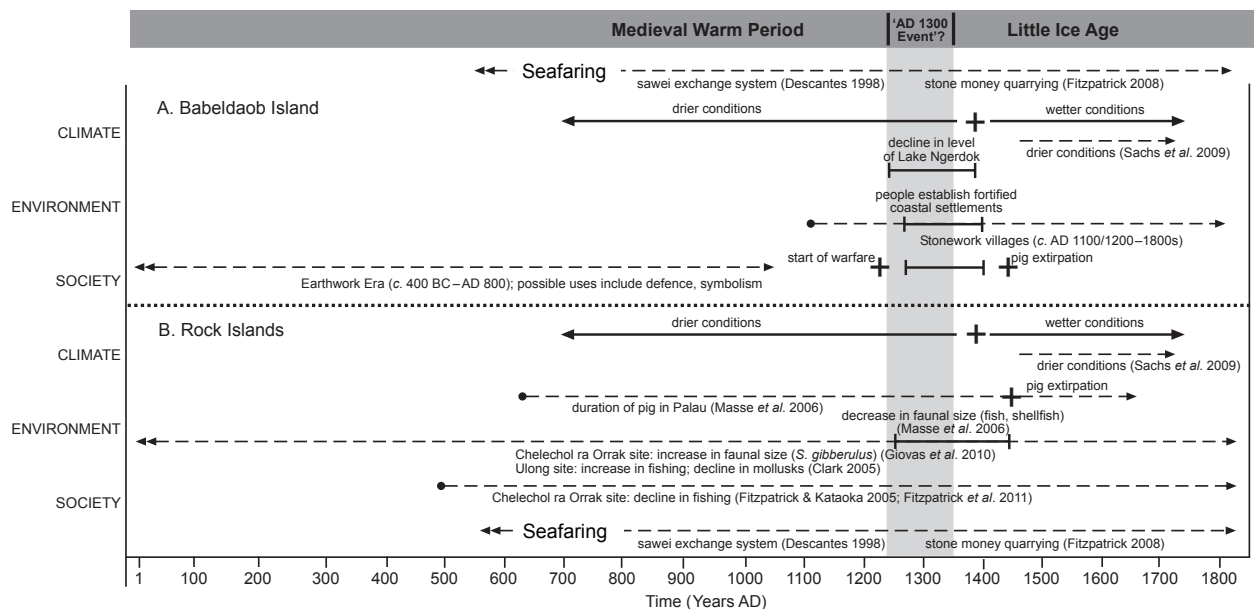


Figure 1. Adaptation of Nunn & Hunter-Anderson’s figure 2 which shows their original ‘events’ in Palauan prehistory (bold lines, text above) but with additional archaeological and paleoenvironmental data extending before and/or after the AD 1300 window (hatched lines, text below).

global phenomenon or not. When they do appear to be discrete events in palaeoclimate records they do not necessarily exhibit the same temporal range or temperature/precipitation/sea level fluctuations across hemispheres or even general regions, Palau being one major example. Though Nunn & Hunter-Anderson would like us to believe that the occurrence of the MWP and LIA and their effects is cut and dried, they are anything but. Future research will require more higher-resolution palaeoclimatic and archaeological data throughout the Pacific Basin, sophisticated analyses of specific phenomena (the occurrence of fortifications by Field & Lape 2010 is one good example), and an equally weighted view of all relevant empirical evidence. For now, acceptance of the 'AD 1300 event' requires scholarly blinkers and interpretive sleight of hand.

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