

# Settling Madagascar: When Did People First Colonize the World's Largest Island?

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## ABSTRACT

Madagascar poses a significant challenge for understanding how people colonized islands. While its inhabitants also share an African ancestry, language, genetics, and culture all point to the arrival on the island of Austronesian-speaking settlers from the far side of the Indian Ocean. Recent decades have seen increasing acceptance of a late first-millennium BC date for Madagascar's initial settlement, based principally on arguments relating to the purported antiquity and presence of cut-marked animal bones and the pollen of humanly introduced *Cannabis* plants. More recently, these claims have been pushed much further back in time by the discovery of stone tools at Lakaton'i Anja and cut-marked bones at Christmas River and Lamboharana. Such arguments must be based on firm foundations if they are to be accepted. This paper evaluates them against criteria developed for assessing the timing and credibility of claims of pre-Clovis settlement in the Americas and early Polynesian presence in Remote Oceania. It concludes that they do not meet them and that for now there is thus no convincing evidence that Madagascar was settled before the mid-first millennium AD. Colonization around that time fits much better with broader patterns of contact, trade, and settlement in the wider Indian Ocean world, including other islands off Africa's eastern coast.

**Keywords** Madagascar; chronology; initial settlement; assessment criteria

## INTRODUCTION

Humans evolved in Africa, but the continent's largest offshore island — Madagascar<sup>1</sup> — was only settled during the Holocene. Madagascar's settlement also poses a further anomaly since the majority of its present inhabitants find their closest linguistic and cultural affiliations over 7000 km away among other Austronesian speakers in Island Southeast Asia (Dahl 1977), a match strongly supported by genetic evidence (Pierron et al. 2017), even though all three datasets, including genetics (e.g., Hurles et al. 2005; Regueiro et al. 2008), also document the arrival of other settlers and cultural

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<sup>1</sup> Greenland, New Guinea, and Borneo are all larger than Madagascar. However, Greenland is joined to North America by sea ice during winter, while during the Pleistocene New Guinea repeatedly merged with Australia and Tasmania to form the super-continent of Sahul, just as Borneo did with Java, Sumatra, and the Southeast Asian mainland. Separated from continental Africa by the 3292-m-deep Mozambique Channel and covering some 587,000 km<sup>2</sup>, there is thus ample justification for deeming Madagascar to be the world's largest (persistent) island.

influences from the African mainland. In conjunction with dated archaeological sites from the island itself, this strong Austronesian signal has historically been understood to indicate that Madagascar was first colonized around the mid-first millennium AD (Dewar and Wright 1993), possibly by voyaging directly from Southeast Asia, possibly by more indirect routes involving the Maldives Islands or Sri Lanka (Fitzpatrick and Callaghan 2008). However, over the past three decades several claims have been made that human presence extends back into at least the late first millennium BC, and this date has won increasing acceptance in overviews of Madagascar's prehistory (e.g., Adelaar 2016; Blench 2010; Burney et al. 2004; Campbell 2016:15–16; Crowley 2010; Douglass and Zinke 2015; Ekblom et al. 2016; Radimilahy 2013; Radimilahy and Crossland 2015). More recently, these claims have been dramatically extended, first by the discovery at the Lakaton'i Anja rockshelter of quartz artefacts in levels dated as far back as  $4380 \pm 400$  BP (Dewar et al. 2013) and second by the publication of apparently cut-marked bones of extinct giant avifauna directly dated to the mid- and early Holocene (Hansford et al. 2018).

If Madagascar was settled long before the arrival of Austronesian colonists this has several important implications. Methodologically, it raises the question of why evidence of human occupation before the mid-first millennium AD is so scanty since surely successful colonization would, over millennia, have produced a much more plentiful material record than that currently known. Related to this is the extent to which human hunting or modification of Madagascar's landscape was implicated in the extinction of the island's well-known megafauna: if people were present for much longer than previously thought what impact did they have on the island's ecological dynamics? And if extinction did not closely follow human settlement, why did Madagascar differ in this respect from other island environments? Finally, if people reached Madagascar from Africa at some time in the pre-Christian era how did they do so and what does this mean for understandings of maritime movement along the East African coast, including the colonization of other offshore islands?

The timing of Madagascar's initial settlement and the consequences this entailed are thus topics of wide concern within island and coastal archaeology as a whole. However, Madagascar's ability to contribute to these debates is only as good as the quality of its data and its dates. In this it is no different from other parts of the world. All existing claims for settlement before the mid-first millennium AD must therefore be critically assessed using the criteria already found useful in other regions where determining early human presence has proven contentious. Only if they pass such tests should we accept that people reached Madagascar much earlier in the Holocene than has conventionally been believed.

Following in the wake of Parker Pearson et al. (2010) and Anderson et al. (2018a, 2018b), I first discuss the criteria found helpful in assessing claims for early human presence in situations comparable to Madagascar elsewhere in the world — sites of pre-Clovis age in the Americas and the timing of the Polynesian settlement of Remote Oceania. I then address claims for early settlement in Madagascar against these criteria, beginning with Lakaton'i Anja and moving on to consider those based on the presence of cut-marked bones, modified shell, and potential palaeobotanical indicators of human settlement (specifically *Cannabis* pollen). I refrain from discussing arguments based on ethnographic, genetic, or linguistic evidence since,

while suggestive, these invariably have less tightly constrained chronologies than those available, in principle, from excavated data.

## **CRITERIA FOR ASSESSING EARLY HUMAN PRESENCE IN NEW ENVIRONMENTS**

One of archaeology's big questions concerns the settlement of new, hitherto unpopulated landmasses (Gamble 2014). Experience gained in evaluating claims for human settlement of the New World before the well-established and readily recognisable Clovis phenomenon (Meltzer 2009), in establishing when people first reached Sahul (Clarkson et al. 2017), or in determining when they arrived on the islands and archipelagos of East Polynesia, including New Zealand (Anderson 1991; Wilmshurst et al. 2008, 2011), is crucial in providing a sound answer to when this took place. Most recently, it has been deployed in assessing claims that hominins were present in California 130,000 years ago (Braje et al. 2017; Holen et al. 2017).

Such studies show that just three criteria are required to establish the presence of humans, but that all must be present if this goal is to be achieved. These criteria are:

- 1 – the recovery of undeniable traces of ancient human activity in the form of artefacts (which may include cut-marked bone or otherwise modified natural materials);
- 2 – the presence of such traces in undisturbed geological deposits in proper stratigraphic position, i.e. in primary context;
- 3 – their unambiguous association with indisputable (generally radiometric) ages (after Meltzer 2009:97; Holen et al. 2017).

Addressing the third of these criteria in greater detail, Anderson (1991) and Wilmshurst et al. (2011) draw attention to the importance of appropriate “chronometric hygiene” protocols that both privilege dates that are clearly and directly linked to cultural activity and favour those with the fewest intrinsic sources of potential error. For radiocarbon, which underpins most of Madagascar's chronology, this means seeking dates that are least likely to be problematic by reason of in-built age, dietary influences, or post-depositional contamination, and that can offer a calibrated result close to the true age of the human activity they seek to date. Long-lived plants and charcoals of unidentified or bulk origin are particularly problematic since both “can introduce substantial error through in-built age” (Wilmshurst et al. 2011:1816). Additionally, without good, local controls for marine reservoir effects marine shell dates must be treated with caution, along with any bone dates for which a marine dietary component is likely (Wilmshurst et al. 2011:1819). For Polynesia, short-lived, preferably identified, plant remains (twig charcoal, bark, seeds, leaves) are likely to give the most reliable results, along with terrestrial eggshell and the bones of short-lived, human-introduced rodents (Wilmshurst et al. 2008, 2011). More generally, concerns have also been expressed over the dating of collagen in tropical environments (van Klinken 1999), while improvements in pretreatment protocols (Wood 2015) mean that caution should be applied to all radiocarbon dates obtained before the 1990s. With these points and the three criteria set out above in mind, I now turn to the evidence from Madagascar (Figure 1).

## EARLY STONE ARTEFACTS AT LAKATON'I ANJA

Lakaton'i Anja is a large rockshelter ~2 km from the coast near the city of Antsiranana close to Madagascar's northern tip. Its excavation in 1986 produced what was long taken to be the oldest, unequivocal evidence for human presence on the island in the form of undiagnostic pottery and a mix of terrestrial and marine fauna. Though the charcoal analyzed was not identified, two associated radiocarbon dates come respectively from a "cluster of small charcoal fragments" ( $1680 \pm 65$  BP, Beta-29946) and "a single piece of charcoal" ( $1300 \pm 80$  BP, Beta-18424), minimizing the likelihood that they derive from multiple sources (Dewar and Wright 1993:429). Once calibrated, these dates point to people using the site, perhaps as a short-term foraging base, in the mid/late first millennium AD (Table 1). Re-excavation in 2011 led to identification of a small microlithic flaked stone tool assemblage on non-local chert and quartz, comparable to that from a second shelter, Ambohiposa in northeastern Madagascar, where these artefacts occur in an indisputably late first/early second-millennium AD context. Initial observations suggested that this was also the likely age of those at Lakaton'i Anja given their association with *sgraffiato* sherds and Song Dynasty (AD 960–1279) porcelain. However, further excavation uncovered additional examples below this in Layers 4 and 5 of Unit J (finds from the nearby Unit K and results of fieldwork in 2012 remain unpublished). Four OSL dates in correct stratigraphic sequence place Layers 4 and 5 between  $2210 \pm 190$  and  $4380 \pm 400$  BP (Dewar et al. 2013:Table S5).

Dewar et al. (2013) rightly observed the potential of their results to transform our understanding of Madagascar's prehistory and this is how they have been generally accepted. Douglass and Zinke's (2015) review of the island's archaeology and reconstructions of its palaeoclimate, for example, is predicated on their acceptance and draws attention to the radical revision of seafaring in the Western Indian Ocean and the interactions between people and Madagascar's wildlife and ecology that they demand. Ekblom et al. (2016:203–204, 219) likewise accept the contemporaneity of Lakaton'i Anja's OSL dates and stone tools, supporting their assessment by reference to "findings of possible cut marks" on bone at multiple locations that I discuss below.

However, Lakaton'i Anja's sediments are not undisturbed. Dewar et al. (2013:SOM 5, 7) explicitly noted the presence of termite burrows in Layers 4 and 5, acknowledged that this had resulted in "some mixing" of sediments, and raised the possibility that "other burrows now completely consolidated" went undetected during excavation. They also observed that the charcoal AMS dates obtained from Layers 4 and 5 are wholly inconsistent with their OSL results, as well as being stratigraphically inconsistent among themselves, though fully compatible with previously recovered evidence of later first/early second-millennium AD occupation at the site (Table 1). Discussing this, Anderson et al. (2018a:39) are surely right to observe that if charcoal could move downward into Layers 4 and 5 then it is "very likely" that small chert and quartz flakes could do the same, something that the single glass bead recovered from Layer 5 or Layer 4B (Dewar et al. 2013:12587, Table S7) certainly did. Dewar et al. (2013) do not define what they mean by 'large' or 'small' flakes, but the information they provide suggests that all those recovered are quite small (Dewar et al. 2013:Table S2). In keeping with this, nine of ten artefacts illustrated are <2cm in maximum dimension and have a mass of  $\leq 2.0$  g (Dewar et al. 2013:Figure 4). No

refitting studies appear to have been undertaken (or perhaps were possible), but vertical displacement of artefacts and other small finds within rockshelter stratigraphies is far from unattested (e.g., Hunt et al. 2015; Richardson 1992; Sealy and Yates 1994; Staurset and Coulson 2014). That the percentage of ‘small’ coarse chert flakes relative to ‘large’ ones increases from 40% in Layer 1 to 55% in Layer 2 and then to 61% in Layer 4 and as much as 73% in Layer 5 (Dewar et al. 2013) itself suggests that artefacts have been displaced downward at Lakaton’i Anja, with smaller, lighter artefacts travelling further than heavier, larger ones. Thus, while the OSL dates may well be correct (though Anderson et al. 2018a:39 also dispute this because of possible sediment mixing), there can be no certainty that they are unambiguously associated with the very small number of microlithic artefacts recovered from Layers 4 (N=136) and 5 (N=151; Dewar et al. 2013:Table S2). For the moment, Lakaton’i Anja therefore fails to satisfy either Criterion 2 or Criterion 3 as set out above. A more parsimonious interpretation of this important site is that all its stone tools, like those from Ambohiposa, date to the late first/early second millennia AD and therefore do not unequivocally document early human presence on Madagascar.

### CUT-MARKED BONE AND MODIFIED SHELL

Apparently cut-marked bones have long been part of claims for early human presence on Madagascar. Almost three decades ago, MacPhee and Burney (1991) identified such marks on four hippopotamus (cf. *Hippopotamus lemerlei*) femora from Ambolisatra and Lamboharana in southwestern Madagascar, arguing that they were made when the bone was still fresh, i.e. around the time of death. Subsequently, Pérez et al. (2005) identified a range of butchery markers (cut and chop marks near joints, oblique cuts along shafts, spiral fractures, percussion striae) on bones of two extinct (*Pachylemur insignis*; *Paleopropithecus ingens*) and one extant (*Propithecus verreauxi*) lemur taxa from two more southwestern sites, Taolambiby and Tsirana. Just over one-third (35.7%,) of the bones of the extinct species and 45.6% of those of Verreaux’s sifaka show such marks, which seem to have been made by sharp metal tools on fresh bone. A few of these bones have been radiocarbon-dated (Table 2). Two hippopotamus femora (one from Ambolisatra, the other from Lamboharana) only marginally predate the mid-first-millennium contexts at Lakaton’i Anja. On the other hand, wildly divergent dates of  $3495 \pm 55$  (AA-2894) and  $2020 \pm 300$  BP (TO-1438) for another hippopotamus femur from Lamboharana (MacPhee and Burney 1991) do not inspire confidence that either option is correct, while a date of  $2325 \pm 43$  BP (AA-45960) on a *P. ingens* radius bearing one apparent cut mark from Taolambiby but now in Oxford University’s Museum of Natural History (Pérez et al. 2003) is also suspect because “the specimens of *Palaeopropithecus* in the Methuen collection had been impregnated with wax” (Pérez et al. 2005:727) and the mark in question has not been described in detail (Dewar 2014). Neither consideration, it should be noted, has prevented this specimen from being cited as evidence of early human presence in some recent surveys of Madagascar’s prehistory (e.g., Ekblom et al. 2016:198, 200; Hansford et al. 2018:1; Radimilahy 2013:945).

Two further problems exist. First, all these specimens come from museum collections of early/mid-twentieth-century date and lack stratigraphic provenance, a situation that Pérez et al. (2005:722) themselves describe as “hardly the most ideal samples for analysis of this kind.” Second, it is not clear that the damage identified is, in fact, of ancient human origin. Writing of the specimens discussed by MacPhee and Burney

(1991), for example, Goodman and Jungers (2014:50) observe that most of the marks occur mid-shaft, not toward the ends of the bones as one might expect if they result from human butchery, a conclusion matching the comment of the original excavators that they occur “without method or discrimination on all parts of the bones” (Grandidier and Grandidier 1905:52). Likewise, a humanly modified *Aepyornis* tibiotarsus from Itampolo directly dated to  $1880 \pm 70$  BP (B-67659) that was reported by Burney (1999) only “indicate[s] postmortem utilization” (Pérez et al. (2005:723).

These issues have recently been explored in greater depth by Atholl Anderson and colleagues (2018a, 2018b). Their re-examination of megafaunal bones held in museum collections from Ambolisatra, Itampolo, and Taolambiby shows that much of the supposed evidence for cut marks likely derives from natural erosion, animal trampling, carnivore and crocodile puncture marks and breakage, and post-collection damage. Consistent with this, re-excavation of bone-bearing deposits at Itampolo and Ambolisatra yielded no instances of unambiguous cut marks while at Ambolisatra the occurrence of bones in anatomical association suggests that they represent a natural death assemblage (Anderson et al. 2018b). Only at Taolambiby were cut marks clearly apparent on newly excavated material. However, damage identical to cut-marked museum specimens was reproducible using modern excavation tools, a possibility previously raised by Parker Pearson et al. (2010:70) with regard to subfossil bones from Fanonta. It follows that even if damage is human in origin radiocarbon dating such specimens (Table 2) may not date that damage. Instead, “reliable dates [must be] restricted to bones recovered in controlled excavations, where the existence or absence of prior damage can be verified” (Anderson et al. 2018a:42). Further to this, Anderson et al.’s (2018b) own fieldwork at Taolambiby did recover a single, possibly chop-marked bone of an extinct hippopotamus and cut-marked bones of Verreaux’s sifaka and fossa (*Cryptoprocta ferox*) that all returned pre-modern ages (1260–1070 and 1150–950 BP respectively), but these dates clearly provide no evidence for the arrival of people prior to the mid-first millennium AD.

Similar difficulties surround supposed cut marks on bones from Anjohibe Cave in northwestern Madagascar where Gommery et al. (2011:272; Gommery and Ramanivosoa 2013) have argued that three hippopotamus (*H. lemerlei*) bones show “a significant quantity of marks due most likely to human activity” located in areas with muscle or tendon attachments. The same authors also identify several fractured femora, dating all these finds to the second/third millennia BC (Table 2). Several objections can be raised. First, since Gommery and colleagues were denied permission to date the supposedly modified bones the latter’s age is based on a presumed contemporaneity with others that were directly dated from the same fossiliferous layer. Second, lacking further dates we do not know when bones ceased to accumulate in this layer, opening up the possibility that those identified as bearing cut and fracture marks are, in fact, (much?) younger than those dated. Third, Goodman and Jungers (2014:50) note that “based on the illustrations provided” at least some of the cut marks appear similar to those made by gnawing mammals, do not show the parallel structure expected of genuine cut marks, and often concentrate in the middle of the bone, rather than toward its extremities. Hansford et al. (2018:102) concur that they “do not exhibit a pattern associated with butchery”, but in any event the entire hippopotamus assemblage from Anjohibe probably results from a catastrophic natural event (Burney et al. 1997; Goodman and Jungers 2014:148–149) in which people played no part (Dewar 2014). Natural death is also a likely

explanation for the fractured cranium of an *Archaeolemur* from Andrahomana Cave in southeast Madagascar where subsequent excavations failed to identify precolonial human activity and large animals like this instead most probably fell into the cave independently of human involvement (Burney et al. 2008: 15, 17).

Summing up, none of these studies satisfy the assessment criteria set out above. The material discussed by Pérez et al. (2005) lacks a secure stratigraphic provenance (Criterion 2) and reliable dating (Criterion 3). The same difficulties arise with MacPhee and Burney's (1991) study, while the Itampolo *Aepyornis* bone (Burney 1999) fails to meet Criteria 1 and 2. Re-excavation of all these sites also queries whether the previously reported cut marks are genuinely ancient (Anderson et al. 2018b), while the hippopotamus bones from Anjohibe (Gommery et al. 2011) meet none of the criteria. Contrary to the conclusion drawn by Ekblom et al. (2016:200), none of this material makes "a convincing argument for an early human presence in Madagascar."

In my view, the same holds for the cut-marked elephant bird bones recently reported by Hansford et al. (2018) from Christmas River (Ilakakabe) in south-central Madagascar, even though these authors are at pains to relate their findings to established experimental frameworks (Galán and Domínguez-Rodrigo 2013) and conservative recognition criteria (Godfrey et al. 2016; Pérez et al. 2005). Hansford et al. (2018) discuss two articulating skeletal elements of an *Aepyornis maximus* individual bearing cut and chop marks consistent with those made by stone tools. One bone (a tibiotarsus) also shows two possible impact fractures and produced two statistically overlapping early Holocene radiocarbon dates (Table 2). Subsequent exploration of museum collections yielded a cut-marked tibiotarsus of the smaller, extinct ratite *Mullerornis* sp. from Lamboharana, which returned a mid-Holocene radiocarbon date (Table 2).

If genuinely modified by people, these three bones alter Madagascar's prehistory even more dramatically than the claims advanced for Lakaton'i Anja, since they would, however episodically, extend human presence back almost to the start of the Holocene. Careful reading of Hansford et al.'s (2018:3) paper nevertheless suggests that only one of nine modifications (TT3, a cut mark on the directly dated tibiotarsus) is explicitly related to peri-mortem damage, while the possibility that the marks observed could have been made by metal tools is not discussed. This is relevant since none of the specimens were recovered in controlled circumstances. The Lamboharana *Mullerornis* bone is from a historical collection and lacks a precise stratigraphic provenance, while the Christmas River bones present difficulties of their own: said to have been collected as an articulated pair by the late palaeontologist Elwyn Simons, they lack a published stratigraphic section or other information detailing their exact origin within the locality's bone bed (Hansford et al. 2018). An accompanying comment implies that they were among a group of "dinosaur bones" found by a local sapphire miner prior to 2008 and then stored by one of the paper's co-authors in a field research centre until examined by Hansford in 2016 (Lawler 2018); the sole additional reference notes that fossil bones were "collected", but does not furnish more than the most minimal further details (Muldoon et al. 2012:25).<sup>2</sup> We can draw

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<sup>2</sup> Muldoon et al. (2012:25) note that, unusually for an open-air fossil-producing site in Madagascar, Christmas River "has the potential to yield a stratigraphic profile in the

but one conclusion: intriguing as they are, the Christmas River finds do not satisfy Criterion 2 for demonstrating early human presence.

For now, then, Madagascar has precisely no instances where unambiguously human-modified animal bones have been recovered under properly controlled conditions from undisturbed primary sedimentary contexts and reliably dated. The same sobering assessment pertains to the one other potential faunal specimen that might signal early human settlement of the island. This is a drilled *Nerita textilis* shell from Level 3 of the NSS2 rockshelter at Velondriake in southwestern Madagascar, a site that also produced ratite eggshell, marine shell, and some charcoal, but no further artefacts (Douglass 2016, 2017). AMS pretreatment took the possibility of secondary calcite deposition into account and the shell did not exhibit weathering of its exterior surface, suggesting that it was quickly buried. Nevertheless, the single date obtained ( $3086 \pm 32$  BP, D-AMS-012442) “is not enough to offer a satisfying chronology” (Douglass 2016:153), even though Hansford et al. (2018:1) uncritically blown this up into “support for regional human presence from ~3000 years B.P.” in western Madagascar “through evidence of protracted subsistence on endemic coastal and marine fauna”! The need for caution is all the greater since — unlike the well-known Middle Stone Age *Nassarius kraussianus* shell beads from Blombos Cave, South Africa (d’Errico et al. 2005) — the operation of non-human agents capable of producing the drilling observed has not been ruled out by detailed taphonomic and experimental studies. The NSS2 drilled shell thus fails to satisfy Criterion 1 of the benchmarks set out above.

## CANNABIS

I turn now from animals to plants and the most unusual line of evidence advanced to support early human presence on Madagascar, the supposed occurrence of *Cannabis* pollen in lake sediments from the island’s central highlands. This has been widely cited (e.g., Crowley 2010; Ekblom et al. 2016; Pérez et al. 2005; Radimilahy 2013; Radimilahy and Crossland 2015), but without critically examining the evidence on which it is based. The claim derives principally from a study of the Triterivakely crater lake (Burney 1987a), although *Cannabis* pollen has also been identified at a date of ~1500 BP in a core from Lake Kavitaha (Burney 1987b). Subsequently, Gasse and Van Campo (1998:307) cored Lake Triterivakely to a much greater depth, obtaining 14 AMS determinations to complement Burney’s (1987a) five conventional dates and noted that “*Cannabis/Humulus* pollen occur as traces since 2200 cal. yr” BP. The argument built from this is that since *Cannabis* is exotic to Madagascar people must have introduced it and that its age in the Lake Triterivakely core provides a *terminus ante quem* for human presence on the island (Radimilahy and Crossland 2015:501).

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sense of vertical, chronological relations of sedimentary units”, but do not then provide such a section drawing. Their description of the site’s stratigraphy is confined to the observation that “the deepest and most prolific layer reached...is a grey-green clay” containing very early Holocene remains of *Aepyornis*, crocodiles, tortoises, and dwarf hippopotami and that “isolated remains” were found above this layer. Elsewhere, they note that the bone bed is “10–15 m deep” and that — while the sapphire miners who found the site “focused on the collection of large macrofossils, and the younger layers of the pit have not yet been fully explored” — sediment samples from the subsurface also confirmed the presence of small vertebrate and botanical remains (Muldoon et al. 2012:24).



Several caveats can be expressed regarding this conclusion. First, the date cited in the literature is interpolated from two of Burney's (1987a) radiocarbon dates:  $1240 \pm 100$  BP (Beta-15584) at 50 cm depth and  $3810 \pm 70$  BP (Beta-15585) at 150 cm. Such interpolations assume constant sedimentation, but Gasse and Van Campo (1998:301) report a "significant increase" in magnetic susceptibility evidence for increased detrital input into the lake between 100 and 200 cm, an observation that should call into question the age attributed to the pollen traces under consideration. Second, *Cannabis* spreads rapidly in mesic tropical environments even without human assistance and produces large amounts of pollen that is dispersed over long distances by wind (Burney 1997:441; cf. Cabezudo et al. 1997; Small and Antle 2003). Birds also disperse its seeds (Small 2015). In other words, we cannot be certain that *Cannabis* was introduced to Madagascar by human action (Burney 1988:71).

A third caution must also be considered. The material identified at Lakes Tritrivakely and Kavitaha is assigned to the related genera *Cannabis*/*Humulus* as their pollen cannot be readily distinguished. Both belong to the family Cannabaceae, to which trees of the genus *Celtis* also belong. This is significant because *Celtis* is well represented at Lake Tritrivakely, where it "comprises up to 40% of terrestrial plants in the upper part of the diagram" published by Gasse and Van Campo (1998:302–303). Moreover, while pollen is said to be "well preserved" here (Gasse and Van Campo 1998:302) that of *Cannabis* and *Celtis* is known to be "similar when degraded" (Reinhard et al. 2008:245). Apart from the other reservations I have noted, can we therefore be absolutely sure that it is *Cannabis* pollen that has been identified at Tritrivakely? Precisely the same question arises from the apparent presence of *Cannabis*/*Humulus* pollen in speleothem core DC87-2 at Drotsky's Cave, Botswana. Associated with a uranium-series date of  $4440 \pm 350$  BP (Burney et al. 1994), this predates by over 2000 years *any* evidence of cultivation in Africa south of the Equator.

To sum up, the argument that *Cannabis*/*Humulus* pollen in the Lake Tritrivakely core documents early human presence on Madagascar is weak and fails to meet any of the three assessment criteria outlined above. It, too, should therefore be excluded from future discussions of when people first settled the island. Directly dated evidence of the early presence on Madagascar of commensal animals (such as rodents) that could serve as a clear proxy for human arrival or visits of the kind known from Remote Polynesia or Madeira (Rando et al. 2014; Wilmshurst et al. 2008) is also entirely lacking (*pace* Blench et al. 2010:240).<sup>3</sup>

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<sup>3</sup> Lack of space precludes discussion of other potential palaeoenvironmental proxies for human settlement. However, changes in the frequency of other pollen taxa, charcoal particles, and *Sporormiella* fungal spores that have been cited as evidence of human presence and landscape modification (e.g., Burney et al. 2003) present similar difficulties to those outlined here at Lake Tritrivakely: chronology depends upon interpolation from a small number of radiocarbon dates and the assumption that sedimentation rates have remained constant; and the changes observed are open to explanation by non-human agencies, including climate and taphonomy (cf., Perrotti and van Asperen 2019). They are not, in other words, based upon the presence of unimpeachable traces of human activity recovered in primary context and dated by unambiguously associated radiometric ages, the three criteria set out above.

## DISCUSSION

Archaeologists have devoted considerable effort to identifying criteria that can provide a secure basis for identifying human presence on newly settled landmasses. Applying these filters to the claims advanced for people having settled Madagascar in the pre-Christian era shows that none can be supported (Table 3). Arguments for human presence based on supposedly cut-marked bones from Ambolisatra, Anjohibe, Itampolo, Lamboharana, and Taolambiby, modified shell from the NSS2 rockshelter at Velondriake, and *Cannabis* pollen in the Lake Tritrivakely sediment core all fail to meet Criterion 1, the recovery of undeniable traces of ancient human activity. The same datasets, along with the cut-marked elephant bird bones recently reported from Christmas River and the stone artefacts from Layers 4 and 5 at Lakaton'i Anja, fail to satisfy Criterion 2, the presence of such behavioural traces in secure primary stratigraphic contexts. And Lakaton'i Anja Layers 4 and 5, all known examples of cut-marked bones except possibly those dated by Hansford et al. (2018), and the Lake Tritrivakely pollen core collectively fail to meet Criterion 3, the unambiguous association of human behavioural traces with indisputable radiometric ages.

In short there is no compelling evidence that people were present on Madagascar before the mid-first millennium AD. Even thereafter, however, chronological data are patchy and of variable quality (Table 4) for sites predating the large urban centre of Mahilaka in the island's far north, occupation of which began in the tenth century (Radimilahy 2013). Surface finds of Tana Tradition pottery at three locations near the mouth of the River Menarandra on Madagascar's southwest coast also fit this picture. Although the one radiocarbon date obtained ( $654 \pm 30$  BP, OxA-13584) is too late to be associated with these ceramics, the overall regional sequence strongly implies that these occurrences should predate the tenth-century beginnings of southern Madagascar's large, densely occupied enclosed *manda* settlements (Parker Pearson et al. 2010), a proposition that fits well with the seventh- to eleventh-century age of Tana pottery in East Africa (Fleisher and Wynne-Jones 2011).

An early or middle Holocene colonization of Madagascar of the kind envisaged by Dewar et al. (2013) or Hansford et al. (2018) presents a much greater challenge. The reason is quite simple: the earlier the date of human settlement the longer the time in which one might expect people to have left behind material traces of their presence. Yet outside Lakaton'i Anja and Ambohiposa (where they are definitely of late first/early second-millennium AD age), claims for stone artefacts (e.g., Bloch and Vérin 1966; Kellum-Ottino 1972) are widely considered to be "tenuous or [to] lack secure archaeological contexts" (Radimilahy 2013:945). Supposedly Indonesian-type stone adzes from subfossil bone-bearing deposits at Lamboharana (Battistini and Vérin 1972:322) are, for example, eighteenth- or nineteenth-century gunflints of European manufacture (Parker Pearson et al. 2010:72).

Of course, early settlers could have used a very informal lithic toolkit, the density of archaeological fieldwork is still low in relation to Madagascar's size (with vast expanses of its east and west coasts still not explored and potentially subject to site loss through coastal erosion), and archaeologists may not have sought or identified stone tools they were not expecting to find. Nevertheless, one must wonder if these factors can explain the complete absence of such finds except at Lakaton'i Anja and

Ambohiposa, not least in areas like southern Madagascar where coastal dunes *have* been intensively surveyed for stone tools (Parker Pearson et al. 2010:517). Their total lack at any of the locations where supposedly cut-marked bones or modified shell have been found is particularly striking, especially at Anjohibe, NSS2, Christmas River, and Lamboharana where the dates reported would necessitate the use of stone, rather than metal, tools. Nor is there any reason (*pace* Blench 2007) to think that the DNA or subsistence practices of Mikea hunter-gatherers in southwestern Madagascar identify them as residual survivors of earlier stone-using foragers: genetic studies provide no evidence of a unique ancestry (Pierron et al. 2013) and the anthropological consensus sees the Mikea abandoning agriculture relatively recently under pressure from the expanding precolonial Sakalava state and its French successor (Huff 2012; Poyer and Kelly 2000).

A further obstacle to notions of early or mid-Holocene settlement of Madagascar from the African mainland relates to the distance that would need to be covered. The two are separated by the Mozambique Channel, which is over 400 km across at its narrowest point. Given the channel's bathymetry, any conceivable lowering of sea level during the Holocene would not have significantly reduced this distance, notwithstanding Ekblom et al.'s (2016:206) speculations, and even at the Last Glacial Maximum Mozambique and Madagascar were divided by at least 250 km of open sea. One might, of course, envisage people arriving via the Comoro Islands, which lie roughly midway between northwestern Madagascar and the northern coast of Mozambique. However, decades of archaeological research there have failed to identify any convincing signs of human presence before the eighth century AD (Wright 2018). Not only that, but people did not even reach the Mafia archipelago — which forms part of Africa's continental shelf — before the fourth/fifth centuries AD (Crowther et al. 2016a; Kwekason and Walz 2015). Unguja, the main island of Zanzibar, has produced evidence of earlier occupation, but only during the late Pleistocene when it was connected to the mainland. As it became an island, however, it seems to have lost its human population, remaining unoccupied until the mid-first millennium AD (Prendergast et al. 2016). The wholly offshore island of Pemba ~130 km to its north seems to have been first settled slightly later, around the seventh century (Fleisher and LaViolette 2013). Summing up, there is no evidence that East African hunter-gatherers “had boats [and] also were able to navigate currents and a complex and shifting shoreline” (Ekblom et al. 2016:206) even close to the mainland, let alone across the hundreds of kilometres of the Mozambique Channel.

## CONCLUSION

Debate about when, how, and from what direction people reached Madagascar will no doubt continue. However, the claims advanced in that debate, as in those regarding the colonization of the Americas (Meltzer 2009) and Remote Oceania (Anderson 1991; Wilmshurst et al. 2011), must adhere to well-established criteria for recognizing human presence. Thus far, arguments for identifying early human presence on the basis of apparent cut-marks on bone, the presence of stone artefacts, or the occurrence of *Cannabis* pollen fail to do so. Until they do, they must be placed to one side. Most crucially, one potentially suspect dataset should not be employed to lend credence to another as, for example, Ekblom et al. (2016:204) do when arguing that “the archaeological evidence from Lakaton'i Anja and Ambohiposa is supported by findings of possible cut marks that are very plausible...[such] that claims for an early

human presence are strongly supported”. Neither dataset is “very plausible”, while that from Ambohiposa is wholly irrelevant, since its stone artefacts are unquestionably of late first/early second-millennium AD date (Dewar et al. 2013). As such, they — like their equivalents from Lakaton’i Anja — form part of a wider regional pattern of expedient stone tool use also evident on Mafia and Unguja (Crowther et al. 2014:39; Shipton et al. 2016) and at Tana Tradition sites on the African mainland (Shipton et al. 2013; Walz 2010).

The sum of Madagascar’s existing radiometrically dated excavated archaeological record points to people arriving there no earlier than the middle of the first millennium, and perhaps not before the eighth century, AD (Dewar and Wright 1993; Anderson et al. 2018a). The upper levels of Lakaton’i Anja still offer the earliest evidence for this (Dewar et al. 2013). As Anderson et al. (2018b:11) have observed, it is of interest that recent assessments of the diversity of Malagasy dialects (Serva et al. 2011), the genetics of Madagascar’s inhabitants (Pierron et al. 2017), the introduction dates of non-endemic mammals (Boivin et al. 2013) and Asian crops (Crowther et al. 2016b), the timing of large-scale landscape transformation (Burns et al. 2016; Crowley et al. 2016a), and the antiquity of Sanskrit loanwords in Malagasy (Adelaar 2009) all point to a similar time frame. Situating Madagascar’s colonization — from both Southeast Asia and East Africa — within the broader context of the explosive growth of voyaging, island settlement, and the development of trade across the western Indian Ocean in the later first millennium AD and of the emergence of Srivijaya’s maritime hegemony in Island Southeast Asia (Beaujard 2012; Boivin et al. 2013) has more to commend it than paying heed to the siren voices of weak or ambiguous evidence calling for a much earlier arrival date.

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