

## Respect versus contempt for evidence: Reply to Hunt and Lipo

John Flenley and Kevin Butler  
Massey University, New Zealand  
and  
Paul G. Bahn

*It is of the highest importance in the art of detection to be able to recognize facts which are incidental and which vital...  
...I make a point of never having any prejudices and of following docilely wherever fact may lead me.*  
Sherlock Holmes, *The Reigate Squires*

### INTRODUCTION

Contrary to the claim by Hunt and Lipo (2007), our disagreements with them do not constitute a simplistic and patronising opposition of evidence and faith. This is a ridiculous claim. It's more a question of having faith in the available evidence, deciding what is vital and what incidental, and not being selective about it – such as ignoring the testimony of Forster, Geiseler and others (see Section 7). To a large extent, Hunt and Lipo seem to have set up some false oppositions to give themselves something to attack. And it is outrageous for them to claim that we do not have open minds, and that we are “unaware of both the historic impacts on Rapa Nui as well as the significant literature on the biological impacts Europeans wrought in the Americas and the Pacific”.

We would point out that both of our books (Bahn and Flenley 1992; Flenley and Bahn 2003) stressed the role of rats in the environmental decline (teeth marks on palm nuts, etc); it is just their degree of blame that is debatable – and yet Hunt and Lipo state that “it is incumbent upon Flenley and Bahn to demonstrate how rats had *no impact* on the forest of Rapa Nui”, as if we had ever propounded such rubbish. And since we assume that Hunt and Lipo accept that rats arrived with the colonisers, then humans are indeed primarily to blame for the environmental decline, whether through deliberate deforestation or damage by rats.

They are also economical with the truth concerning our claim for deforestation by AD 1000 – according to Hunt and Lipo, we argued for complete deforestation by AD 1000, whereas we specified that this was at Rano Kau; and it is simply mischievous to present our terms “anti-European bias” and “Europhobic model” as political in nature, when they simply describe – perfectly accurately and objectively – the attitude of those who have sought to lay all the island's ills at the Europeans' door, and thus absolve the islanders themselves of all blame. This does not make us anti-Polynesian in any way, and indeed it could be argued that it is the anti-Europeanists who insult the Polynesians by portraying them as simply hapless and passive people who had no impact on their own environment and who found themselves at the mercy of rats and Europeans.

Our refutations of the arguments put forward by Hunt and Lipo refer to a number of areas.

### 1. Stratigraphy

We hear a lot these days about chronometric hygiene, which is an admirable idea. We also, however, need stratigraphic hygiene if we are to interpret correctly the stratigraphic record. By that we mean that incomplete or duplicating stratigraphies must be eliminated. In particular, unconformities and other discontinuities in deposition are unacceptable. As Geikie (1903:821) puts it:

“But an unconformability leaves no room to doubt that it marks a decided break in the continuity of deposit. Hence no kind of geological structure is of more importance in the interpretation of the history of the stratified formations of a country.”

Hunt and Lipo (2006) describe an excavation which they made at ‘Anakena beach, in which they recorded a series of horizons of blown sand overlying a grey clay. The sands contained artefacts, bones, and rat-gnawed palm fruits. Radiocarbon dates for these items extended back to AD1200. Below the blown sand was a grey clay which was cored for 1 metre but contained none of the above items. It was therefore undated. In our opinion the boundary between the blown sand and the clay is a discontinuity in deposition. That is to say, there was a gap in time between the deposition of the clay and the start of deposition of the blown sand. Hunt and Lipo state that there is evidence of soil formation at the surface of the clay which supports this idea, since soil formation is a relatively slow process. The origin of the clay is obscure. It could be a marine deposit, or have a sub-aerial origin. There could even have been blown sand on top of it, which eroded before the present basal sand was laid down. The dates from the basal sand cannot therefore be more than *minimum* ages for the arrival of people in Rapa Nui.

It is noteworthy that in the adjacent excavation at ‘Anakena, Steadman *et al.* (1994) found very similar results to Hunt and Lipo, but these were not interpreted as necessarily an indication of earliest human presence. They also noted that the basal clay was a subsoil from which the topsoil had been eroded, again implying a hiatus in the depositional sequence.

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Our reservations are not unusual in archaeology. When Kirch and Steadman were excavating in a rock shelter on Mangaia, Cook Islands, they excavated to the base of the soft sediment, below which was coral limestone. Their basal date was ca. 1000 BP (Kirch *et al.* 1995). They found artefacts to the base of the soft sediment, but they did not conclude that this was the date of arrival of people. They instead preferred a date of 1600 BP, for forest pollen decline from a nearby pollen core (Kirch *et al.* 1991; 1992), for an estimate of human arrival. Later pollen work by Ellison (1994) established that an earlier forest pollen decline at ca. 2300 BP was probably the indication of earliest human presence (Kirch and Ellison 1994).

## 2. Multiple Locations

The quotation of similar dates from three locations as evidence for the earliest presence of people is spurious. In fact the evidence that people were present at three separate locations actually argues against this being the earliest date. This is especially so as one of the dates (Martinsson-Wallin and Crockford 2002) refers to an 'agricultural structure' which seems an unlikely find to indicate earliest human presence. Elaboration of agriculture was, however, a feature of later development on Rapa Nui, when population pressure demanded it. This is not to say that early settlement may not have been inland. The paramount need for fresh water must surely have made Rano Raraku and Rano Kau of early importance. The latter also has a most favourable microclimate for the cultivation of tropical crops.

## 3. Palynology

Palynology of lake sediments has given remarkably clear indications and dates of human activity on several Pacific Islands, especially Mo'orea (Parkes 1997), Mangaia (Kirch *et al.* 1992) and Tonga (Flenley *et al.* 1999). In Tonga the correlation with the Lapita culture was particularly good.

Hunt and Lipo have criticized the work on core KAO2 from Rano Kau, Rapa Nui, in various ways. They point especially to the anomalous dates obtained from pollen extracts (Butler *et al.* 2004). These were indeed inexplicable until we discovered that the preparation technique for the samples included the strong possibility of contamination with old carbon (Prior *et al.* in prep.). These results are therefore now irrelevant. Hunt and Lipo (2007) also criticized the bulk sediment dates from the same core, on the grounds that these could be contaminated by in-washed or in-blown ancient carbon, or by down-growth of young roots from above. This criticism was partly justifiable. We have therefore recently re-dated the KAO2 core in several places, using carefully selected macro-remains of *Scirpus californicus*, the main plant in the Rano Kau swamp.

This species has been checked as reliable for radiocarbon dating (Smith 1961; Prior *et al.* in prep.). The pieces used by us were fruits and/or culms, i.e. the leafless green stems which compose the bulk of the aerial part of the plant. The core being considered here is the lake sediment section of core KAO2, taken near the centre of the lake.

This core has the best chance of showing early human activity, since it shows uninterrupted sedimentation for most of the last 10,000 years. The results of this dating are shown on Figures 1 and 2. In Figure 1 we can see that the sedimentation rate has varied considerably in the past. In the very early Holocene (10,000 to 9000 BP calib., ca. 20 m to 18 m depth) the sedimentation rate was about 1 m in 500 years. In Figure 2 we can see that this coincides with a small peak of herbs (mainly grasses) and some shrubs. This possibly indicates a drier and cooler phase (see Flenley *et al.* 1991). There then follows a long phase of dominance by forest,

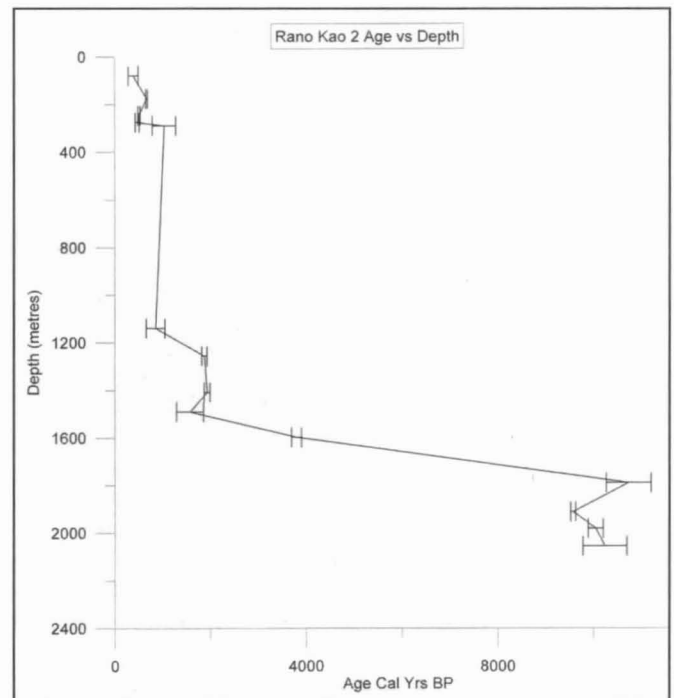


Figure 1. Graph to show accumulation rate of sediment in Core KAO2, near the centre of Rano Kau. This is based on calibrated AMS Radiocarbon dates of fruits and aerial culms of *Scirpus californicus* (*totor* reed), and on earlier bulk sediment dates.

from ca. 9000 BP to ca. 1900 BP calib. (ca. 18 m to 14 m depth), with a sedimentation rate that was rather slow, ca. 1 m in 1500 years. This apparently represents the warm, moist Holocene climate.

From about 14 m depth (ca. 1900 BP calib.) there is a great increase in herbs (grasses), accompanied by charcoal. There is also a large decline of trees, with an increase of shrubs, which may well include *Broussonetia papyrifera* (paper mulberry, formerly cultivated according to Métraux 1940). These changes coincide with a massive increase in sedimentation rate to ca. 1 m in 170 years (Figure 1), which probably represents an increase in productivity of the lake as a result of eutrophication caused by the blowing in of wood ash from forest fires. It is quite difficult to explain these changes in any other way than by human activity, possibly accompanied by the activities of the introduced rats. It is, however, just possible that these changes resulted, at

## RANO KAO, Easter Island

Borehole Two  
Relative Pollen Diagram  
Analyst: K.R. Butler

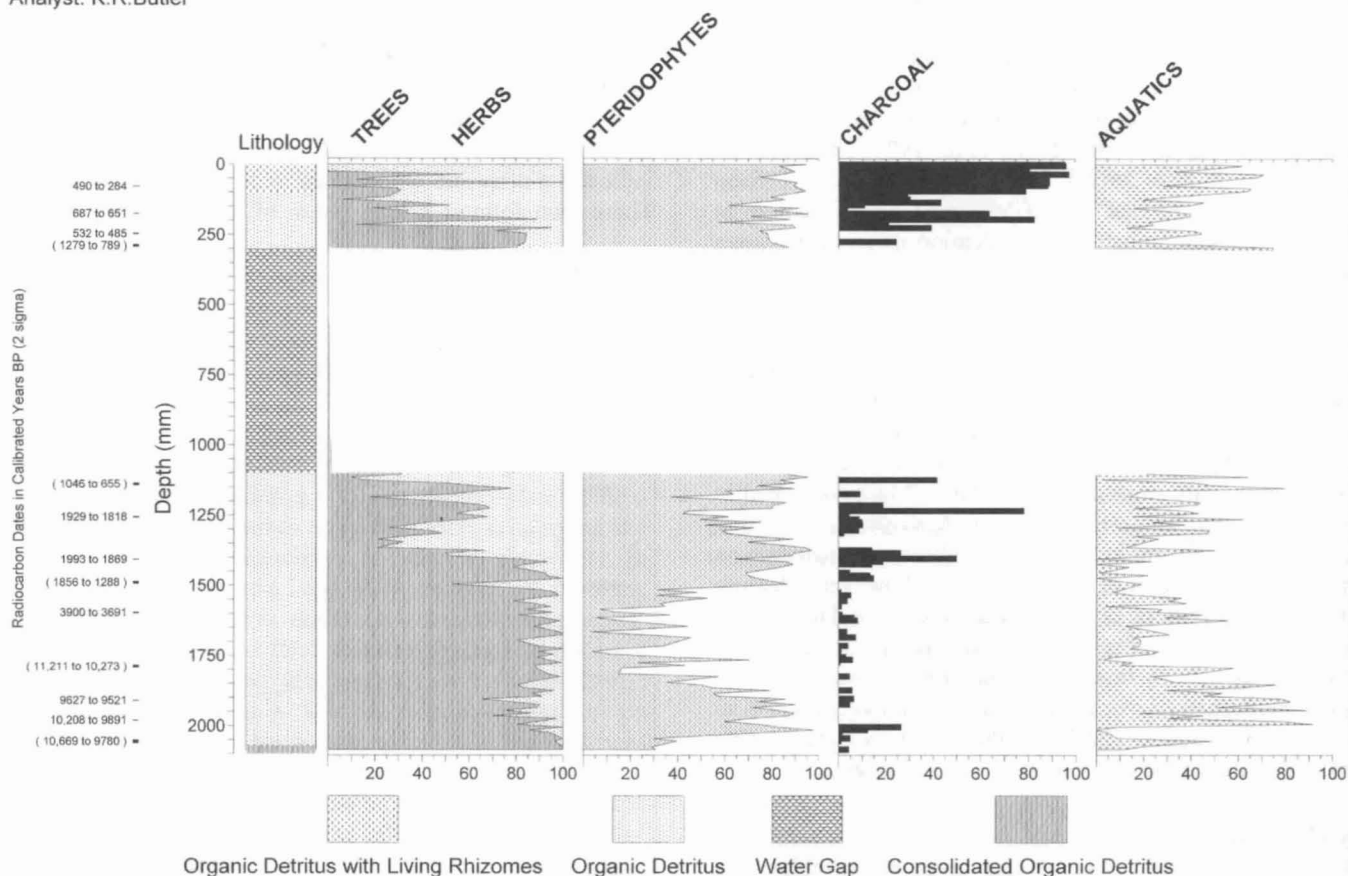


Figure 2. Summary pollen diagram from Core KAO2, near the centre of Rano Kau. Partly after Butler and Flenley (2001) but with AMS radiocarbon dates as shown on Figure 1. Previous bulk sediment dates are shown in brackets. Values are percentages of the total dry land pollen.

least in part, from climatic change, leading to major droughts and natural fires. We therefore regard the date of 1900 BP calib. as a maximum age for the presence of people on the island.

The new dates from the floating mat still show one inversion, so we conclude that the floating mat deposits could have been disturbed. A possible cause of this would have been their use for cultivation of taro. This usage of swamps, which is well known in Melanesia (Serpenti 1965; Golson 1977) and in Polynesia (Spriggs 2002), would be a further example of the intensification of agriculture on the island (see Section 7), and the possibility is currently under investigation. It is also possible that the disturbance could have been caused during the harvesting of the totora (*Scirpus*) reeds which were much used in thatching, and as mats and floats (Métraux 1940).

Another criticism by Hunt and Lipo (2007) relates to core KAO1. This core was taken near the edge of Rano Kau and therefore reflects the history of the nearby inner wall of the caldera (Jacobson and Bradshaw 1981; Turner 1965).

Not surprisingly, since that would have been the most favourable place on the entire island for growing tropical crops, forest clearance was complete rather early there – starting around 1300 BP uncalib. and being almost completed around 950 BP uncalib. (Flenley *et al.* 1991). We apologise for incorrect dates for these events given in Flenley and Bahn (2007). Chemical analysis of the core showed that in-wash of old soil carbon at the level dated was likely to be of minor importance (Flenley *et al.* 1991). It seems clear that deforestation on Rapa Nui was a time-transgressive phenomenon, as one might expect. On the other high islands of the Pacific (e.g. Tahiti, Mo'orea and Rarotonga) deforestation is not complete to this day.

#### 4. Rats

The idea that rats were partly responsible for deforestation by eating the palm fruits was introduced by Flenley *et al.* (1991). Whether the rats could have been ALMOST totally responsible, as suggested by Hunt and Lipo (2006) is a different matter. On the island in Hawai'i where this idea was

first applied, there was a pollen diagram where forest decline was not accompanied by charcoal (Athens and Ward 1993; Athens, 1997). This is not the case on Rapa Nui, where deforestation is always accompanied by charcoal. Furthermore, only one woody species (the palm) is yet recorded as having its fruit eaten by rats. The palm, *Paschalecocos dispersa*, is closely related to *Jubaea chilensis*. *Jubaea chilensis* is the longest lived palm in the world, possibly surviving for 700+ years (Tomlinson 2006; Grau 2004). Thus if rats, preventing regeneration by eating the palms, were the major cause of deforestation starting at AD 1200, we might expect many palms to have survived into historic time. None did so, and carbonised palm stumps have been found (Mieth *et al.* 2002). Who lit the fires? Rats or people?

### 5. Language

What Hunt and Lipo say about glottochronology is basically valid, BUT when used as a secondary argument, the Rapa Nui language cannot simply be brushed aside. According to Steven Fischer (pers. comm.), when in 1926 the Rapanui visited Mangareva (which at present is generally reckoned to be their island of origin), they specifically stated that they could not understand the Mangarevans. Had they truly left Mangareva in AD 1200, as Hunt and Lipo claim, then in 1926 limited mutual intelligibility would just about be assured. As it was, both the Rapanui and the Mangarevans chose to communicate in Tahitian instead, which worked well for the basics. And since mutual comprehension had been lost by 1926 (not because of external contamination of either language but by natural attrition), then not 700 years but more like 1400 years of separation must be the explanation. As a valid analogy, today's New Zealand Maori still understand a good deal of Cook Islands Maori, from which they have been separated for some 700 years, with very similar dynamics going on as in the case of Rapa Nui and Mangareva. So how is it possible that the Rapanui could not understand the Mangarevans in 1926? These were not all a new generation of Rapanui; in the 1920s a great deal of the 'old language' remained, and certainly enough vocabulary to assure some kind of basic mutual intelligibility if separation had been a matter of only 700 years. Any linguist would see a more profound time depth here than AD 1200, and indeed almost a guarantee that the separation was closer to 1400 years.

### 6. Date of Arrival of People

We have no particular interest (or 'prejudices', to use Sherlock Holmes's term) concerning the specific date of the human arrival on Rapa Nui, but it is worth noting that the latest survey by Vargas *et al.* (2006:396/7) concludes – after a careful examination of all the evidence, including chronometric hygiene – that the island was probably occupied by the late 1<sup>st</sup> millennium AD, possibly ca. AD 800 (and it is also worth noting that they (*ibid.*:401) estimate a population growth of 0.8% per year for 800 years, leading to 15,000 people by the 17<sup>th</sup> century AD).

'Chronometric hygiene' is all very well, but it's always worth remembering that since there is only a 66% chance of a normal date being accurate; a third of all dates are probably wrong. Single dates are always risky, it's true, but they exist, and should not be tossed aside so casually, especially when they tie in with other kinds of evidence such as language.

### 7. Collapse

Whether the collapse of the Rapa Nui civilization was brought about by external forces (European contact; Peiser 2005) or by internal factors (Bahn and Flenley 1992), will probably be argued for some time. There is, however, good evidence that the ecology of the island was under strain from early times:

- 7.1 Several species of native birds had become extinct (Steadman *et al.* 1994; Steadman 2006).
- 7.2 Twenty four species of nesting seabirds had been extirpated (Steadman 2006).
- 7.3 Shell middens suggest that shellfish were being collected at a progressively smaller size: e.g. "It can be said...that the exploitation of coastal resources during the 1600s and 1700s contained a component that focused on the collection and probable consumption of very small items" (Stevenson *et al.* 2000:153).
- 7.4 Absence of dolphin bones in the later horizons at 'Anakena suggests reduced ability to catch large fish and dolphins from canoes (Steadman *et al.* 1994; Hunt and Lipo 2006).
- 7.5 Declining forest suggests inadequate timber for canoes (Flenley *et al.* 1991). Cook confirmed this (Cook 1777).
- 7.6 The analysis of 33,000 charcoal fragments from hearths (Orliac 2000) suggests that burning of firewood was largely replaced by burning of grass from AD 1640.
- 7.7 The use of stone mulching (Stevenson *et al.* 1999, 2002; Wozniak 1999; Gossen and Stevenson 2005) suggests a need for horticultural intensification. This is supported by Horrocks and Wozniak (2007).
- 7.8. The presence of damaged bones is suggestive of intertribal warfare (Owsley and Gill 1997). Owsley (cited in van Tilburg 1994:107) has found multiple injuries and wounds and "depression fractures from blunt force trauma are frequent...."
- 7.9. The abundance of obsidian spearheads supports this: e.g. Englert (1970: 139) specifies that "they seem to have come into use shortly before the arrival of the first Europeans".
- 7.10. Legends speak of warfare, famine and destruction of *ahu* in the period AD 1650-1680 ( see e.g. Englert 1948, 1970); and, "traditions are filled with accounts of sanguinary conflicts...continued through generations, until one party or the other were entirely exterminated" (Thomson 1891:476).

Any of these ten items of evidence taken alone would be suggestive. Together they form a massive body of evi-

dence which argues strongly that the civilization collapsed before the first European contact in AD 1722.

Attempts to refute this require either that the first European contact was earlier, or that the dates of ecological changes were later, or that the changes have been misinterpreted. It has been claimed, for instance, that the spearheads (7.9 above) were, in fact, horticultural implements. This seems unlikely because of their delicate construction and elaborate hafting design.

We certainly agree that *mata'a* come in many shapes and sizes, and there are so many that they were probably used for all kinds of things – like the all-encompassing term “handaxe” for what was almost certainly a multi-purpose tool in the Palaeolithic. Think of how many things a basic pocket knife or dagger would have been used for in prehistory or medieval times. So it is obvious that *mata'a* were probably multi-purpose implements. These were the Swiss Army Knives of ancient Rapa Nui.

Regarding use-wear analyses, it should be recalled that Church and Ellis themselves (1996: 84) stress that obsidian is a particularly difficult material from which to derive accurate use-wear data. But even accepting the validity of the few published analyses – and we are perfectly content to do so – it is noteworthy that in each case (Church and Rigney 1994; Church and Ellis 1996; Church 1998) only a very few *mata'a* were analysed, sometimes just fragments of them, and the vast majority of analyses were done on flakes.

It is true that these authors conclude that *mata'a* were not spear points but were used for cutting green plants; but this seems a somewhat sweeping statement after the analysis of a mere handful from a category of tools which, as Hunt and Lipo stress, have a very wide variety of forms, and which were made in their hundreds, if not thousands.

There is another problem with their view of these artifacts. If they were all simple kitchen utensils, used for cutting and processing plants, why did they appear in such large numbers relatively suddenly and so late in the island's occupation? And why do they occur in large caches? Obsidian flakes were perfectly adequate for plant-processing tasks, as shown by use-wear analyses, so those who dismiss all *mata'a* as nothing more than utensils are ignoring other awkward facts; we are stunned that Hunt and Lipo are prepared to simply ignore the 1774 testimony of Forster, (Flenley & Bahn 2003:153) one of the best scholars ever to visit Rapa Nui. We repeat his detailed observation that: “some ... had lances or spears made of thin ill-shaped sticks, and pointed with a sharp triangular piece of black glassy lava”. That seems pretty clear – or are they asserting that the islanders had suddenly decided to take a few kitchen utensils and attach them to the end of poles, in order to show them to the visitors?

It is clear that one of the primary uses of *mata'a* was as spearheads, inconvenient as this may be to Hunt and Lipo. Quite apart from Forster's crucial testimony, we also have that of Geiseler (1995:72): “the chief weapon was all along the spear.... The spear was a formidable weapon be-

cause of its sharp point. These spears caused deep, mostly fatal, and in other cases, hard to heal wounds.”

As for the supposed lack of evidence of wounds, we repeat that wounds were observed and mentioned in some early visits; and some have been seen on skeletal material (see 7.8 above). Hunt and Lipo claim that the latter are few; but without quibbling about numbers, it is worth mentioning that obsidian is ideal for cutting and slashing, and so it is extremely likely that many wounds or deaths caused by such weapons would have left no trace on bones. We will never know, so trying to deny the presence of such weapons by that route is not particularly helpful or relevant. On the other hand, we have testimony from early visitors: “the scars were so conspicuous that they amazed the Spaniards under Gonzalez” (Métraux 1940:165); Geiseler (1995:72) said that “one skull showed that it was pierced through by a single thrust of the spear”; while Zumbohm (Métraux, *ibid.*) writes that wounds produced by *mata'a* were always fatal if they were deep. Métraux himself (*ibid.*) was told by the Rapa Nui people that the *mata'a* were used as weapons, and he even (*ibid.*:376) published the “legend about the origin of the Mata'a spears”! Thomson (1891: 475/6) also wrote a great deal about these spears and weapons. Ayres *et al.* (2000:175), in a paper about use-wear, say that the *mata'a* is “referred to traditionally by Easter Islanders as a weapon ... that is, a point for a spear (*kaukau*) or thrusting lance (*vero*), and hafted examples are known in ethnological collections”. Métraux (1940:165-68) presents these hafted examples. Why do Hunt and Lipo show such contempt for all this testimony and prefer their own subjective opinions on the likely use of these points?

It is pretty obvious that the islanders had no need to build defensive structures in view of the wealth of hidden caves at their disposal. We are at a loss to know how Hunt and Lipo feel able to assert that the use of caves as refuges suggests “conflicts on a relatively small-scale”!

As for cannibalism, contrary to Hunt and Lipo, we do not agree that White or Cochrane *et al.* made a convincing case for its presence in their respective regions, and archaeology still awaits reliable criteria for establishing the presence of the phenomenon through study of human bones.

It is difficult to argue that the many dates which contribute to the Orliacs' generalization regarding fuel (7.6 above) are incorrect. While firewood could give dates earlier than the date of felling, the dates on grass must surely be contemporary with its collection and use. To be reduced to burning grass on a cooking fire must represent a fairly desperate situation. Furthermore, the pollen evidence is entirely compatible with the charcoal evidence, deforestation being early in favored localities (e.g. Rano Kau) and probably later in others, especially at altitude and on cliffs.

The idea of European contact before 1722 is not impossible, but there is no convincing evidence for it at present. It is certainly correct that many of the *ahu* remained intact until the end of the eighteenth century or later, since they were described and even drawn by La Pérouse (1997). But this does not mean that the *moai* cult was still opera-

tive. La Pérouse reported that the people ignored the statues, and he even published drawings of the people apparently doing so. This is not surprising. For instance, in the Communist Soviet Union, atheism was the approved belief, yet Christian churches mostly remained, even if scarcely used and largely ignored.

It should not be imagined that Rapa Nui was unique in having an internal crisis without European contact. On many islands the latter was indeed disastrous (Moorehead 1966), but there are other cases where ecological crisis was overcome internally by various means. Several of these have been described by Kirch (1997). On Mangaia (Cook Islands), a population crisis was apparently averted by adopting, among other things, human sacrifice. On Tikopia, a Polynesian outlier near the Solomon Islands, a similar problem was solved by a combination of enforced out-migration, infanticide and elimination of pigs.

## CONCLUSION

We welcome the publication of the views of Hunt and Lipo, and of Peiser. It is good that accepted ideas are challenged and discussed critically. This is the way that science progresses. But there is very little that is original or new in this "late dates and rats" approach, despite its inevitable popularity with the media; and we feel that far more respect should be paid to the geological evidence and to non-archaeological evidence such as the ethnographic testimony of Forster, Geiseler, Thomson, Métraux and others, and the very important factor of language. We remain open-minded, and perfectly willing to modify our position, given solid reasons for doing so; but in the present case we have not yet found anything sufficiently convincing to cause us to change our previous views.

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