Respect versus contempt for evidence: Reply to Hunt and Lipo

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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 (Martinson-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 KA02 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 KA02 core in several places, using carefully selected macro-remains of Scirpus californicus (totora reed), and on earlier bulk sediment dates. 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, 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 Métaux 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
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 KA01. 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 de-
cline 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 re-
corded as having its fruit eaten by rats. The palm, Paschalo-
cocos dispersa, is closely related to Jubaea chilensis. 
Jubaea chilensis is the longest lived palm in the world, possi-
ble 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 basi-
cally valid, BUT when used as a secondary argument, the
Rapa Nui language cannot simply be brushed aside. Ac-
cording 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 Man-
garevans chose to communicate in Tahitian instead, which
worked well for the basics. And since mutual comprehen-
sion had been lost by 1926 (not because of external con-
tamination 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 Zea-
land 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 cer-
tainly enough vocabulary to assure some kind of basic mu-
tual 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 Sher-
lock 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 chrono-
metric hygiene – that the island was probably occupied by
the late 1st millennium AD, possibly ca. AD 800 (and it is
also worth noting that they (ibid.:401) estimate a popula-
tion growth of 0.8% per year for 800 years, leading to
15,000 people by the 17th century AD).

‘Chronometric hygiene’ is all very well, but it’s al-
ways 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 Flennley 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 extir-
pated (Steadman 2006).
7.3 Shell middens suggest that shellfish were being col-
lected 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 fo-
cused 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
‘Anakea 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
(Flennley 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 inter-
tribal 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; “tradition are filled with account of
sanguinary conflicts...continued through generations,
until one party or the other were entirely extermi-
nated” (Thomson 1891:476).

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


