Hiscock, P. 1986. Technological change in the Hunter River valley and the interpretation of late Holocene change in Australia. *Archaeology in Oceania* 21, 40-50.
Hiscock, P. 1988. A cache of tulas from the Boulia district, western Queensland. *Archaeology in Oceania* 23, 60-70.
Morwood, M.J. 1979. Art and Stone: towards a prehistory of central western Queensland. Unpublished PhD thesis, Australian National University.
Morwood, M.J. 1981. Archaeology of the Central Queensland Highlands. *Archaeology in Oceania* 16, 1-52.
Schrire, C. 1982. The Alligator Rivers: prehistory and ecology in western Arnhem Land. *Terra Australis* 7.
Smith, M.A. 1986. The antiquity of seedgrinding in central Australia. *Archaeology in Oceania* 21, 29-39.

The concept of cache: a reply to Morwood

The importance of definitions in archaeological interpretations is never more clearly illustrated than when an object or site is examined by archaeologists with dissimilar perspectives. Many of the issues Morwood has raised about my cache paper reflect our different use of the same term. For example, I defined 'cache' as the underground and concealed storage of objects, but specifically rejected the potential of those objects for re-use as a relevant attribute (Hiscock 1988:67). Consequently, I accepted previous claims for prehistoric caches of stone artefacts only when there was some stratigraphic evidence for underground concealment. In contrast, Morwood uses 'cache' in a more general sense, largely as a synonym for storage. As a result, he accepts claims for caches when it is likely that the objects remain in a useable condition, even though there was no stratigraphic evidence for concealment. These differences in the definition of a cache underlie our different interpretations of sites such as Native Well 1, Puntutjarpa and Nawamoyn. All of these sites provide evidence of storage but not of underground concealment. Thus, employing Morwood's definition they can be accepted as caches, but using my definition the claims for caching must be rejected.

This point can be reinforced by more specific replies to the issues raised in Morwood's comment. Let me deal first with the sites of Puntutjarpa and Nawamoyn, which I did not mention in my paper and which Morwood uses as examples of caches. In his description of the Puntutjarpa excavation Gould (1977:73) asserts that the vertical grindstones he recovered were probably part buried in the sandy floor to make them stand upright in clear view of the occupants. Hence, according to the definition of caching I employed, this cannot be considered a cache. Similarly, Schrire (1982:137) reported that at Nawamoyn axes were more commonly found along the shelter wall than away from it, and inferred caching. This distribution of axes certainly indicates that discard patterns varied spatially within the site, and, like Morwood, I am inclined to interpret the phenomenon in terms of storage. Since Schrire did not report any stratigraphic evidence of the axes having been completely buried and/or hidden from view, it is not clear that they were caches.

Most of Morwood's comments deal with the Native Well 1 site. In the paper on the Boulia cache I argued that there was no evidence to indicate that objects in the Native Well 1 deposit were deliberately buried. In response he has advanced five reasons why the objects in question should be regarded as caches:

1. Items near the rear wall are still useable. This reveals that useable items are discarded in different locations to exhausted ones, and may imply storage. As discussed above, however, the future potential for use does not, in my view, necessarily signify caching.

2. Archaeological recovery of useable items suggests concealment. If this logic is employed, all artefacts which are not broken or exhausted (such as intact backed blades) must be regarded as having been cached. Clearly this does not follow, because there are many other mechanisms by which useable items are lost from a cultural system and become incorporated in an archaeological deposit; for example, the burial by natural sedimentation of objects left on the ground surface between occupational events.

3. The objects may have been buried in shallowly-scooped pits which leave no stratigraphic indications. Even if this were so it is hard to see how the absence of such evidence provides support for an argument that the items were cached. Morwood's comments that sandstone shelters may not preserve stratigraphic evidence for small pits is well taken, although it implies that they may therefore be inappropriate venues for addressing questions about caching behaviour.

4. Reliability of grindstone identification. Morwood asserts that his recognition of grindstones is correct, and provides two arguments in support of his position: similar counts by Mike Smith, and the presence of non-local sandstone fragments. Neither of these arguments demonstrate that Morwood's identifications are better or worse than mine, although they do show that the definition of a grindstone varies markedly between archaeologists. Morwood is clearly prepared to classify non-artefactual sandstone manuports as grindstones, whereas I strictly limit the label 'grindstone' to fragments of rock which have been abraded. Differences in our artefact counts are a product of these differences in our approaches and definitions, and have nothing to do with competence (in the sense of the ability to define classificatory criteria and consistently employ them). Readers may choose whichever definition is most applicable to their questions and employ the relevant grindstone counts, or, alternatively, develop a third criterion and re-examine the specimens themselves. The sandstone fragments are housed at the Queensland Museum in Brisbane, and are available for inspection.

5. The absence of any need to discredit the Native Well 1 evidence. Morwood has stated that I could have accepted the Native Well caches and still argued that trade-related caching was associated with '...an intensification of trade and settlement restructuring in the late Holocene' (Hiscock 1988:69). This would have required me to adopt a less rigid definition of caching. Morwood may ultimately be correct in arguing that while trade-related caches became

common only in the recent past, non-mortuary caching behaviour has a long antiquity in Australia, rather than such caching only being a recent development as I suggested. If the evidence eventually supports his position, I will happily modify my hypothesis on the history of caching. As I described above, however, none of the available evidence convinces me that caches (as I define them) occurred prior to the late Holocene. For this reason I maintain the position advanced in my paper on the cache of tulas from the Boulia district.

Having dealt with these specific points, I turn now to the most important issue raised by Morwood, namely the nature and scientific relevance of re-interpreting archaeological data. Morwood believes that I have questioned his professional competence. On the contrary, no malice or defamation is implied by my re-interpretation of the Native Well material. Indeed, the fact that the material can be re-evaluated reflects well upon Morwood's excavation and analytical procedures. It is widely accepted that '...in a science, data is always open to re-interpretation and previously "confirmed" theories may be falsified' (Morwood 1975:114). Knowledge develops not only by the derivation and testing of new theories, but also by re-casting data as our view of the world and our interpretative principles alter. Re-analysis of existing archaeological collections should therefore be encouraged as a valuable contribution to theory testing. As evidenced by recent papers (e.g. Hiscock 1986, Smith 1986), Australian prehistory is being extensively modified by demonstrations that the evidence is not what was thought. The progress of our discipline may partly be measured by the ability of its members to re-interpret evidence without enmity or umbrage.

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References

Gould, R.A. 1977. Puntutjarpa rockshelter and the Australian Desert Culture. *Anthropological papers of the American Museum of Natural History* 54(1).

Hiscock, P. 1986. Technological change in the Hunter River valley and the interpretation of Late Holocene change in Australia. *Archaeology in Oceania* 21,40-50.

Hiscock, P. 1988. A cache of tulas from the Boulia district, western Queensland. *Archaeology in Oceania* 23. 60-70.

Morwood, M.J. 1975. Analogy and the acceptance of theory in archaeology. *American Antiquity* 40, 111-116.

Schrire, C. 1982. The Alligator Rivers: prehistory and ecology in western Arnham Land. *Terra Australis* 7.

Smith, M.A. 1986. The antiquity of seedgrinding in central Australia. *Archaeology in Oceania* 21, 29-39.

Radiocarbon dating very small samples

Although radiocarbon dating has been in use for about 40 years now, it is only in the last decade that the underlying technology has made a dramatic advance, increasing the sensitivity of the technique more than a thousandfold. The development that is transforming radiocarbon dating has become known as accelerator mass spectrometry (AMS), and is based on the special characteristics of certain types of nuclear particle accelerators that makes it possible to detect carbon-14 by mass spectrometry methods, instead of measuring the weak radioactivity of the object to be dated.

Conventional methods of measuring radiocarbon age assessments involve counting the number of carbon-14 decays that occur in a period that is typically 12 to 24 hours. One gram of 'modern' carbon contains 6×10^{10} atoms of carbon-14. A simple calculation shows that in a 24 hour period, for every 3 million carbon-14 atoms in the sample only one will be detected. Since it is necessary to count something like 20,000 decays to give adequate statistical precision on the age, several grams of carbon are required for a good measurement, and this in turn means that it may be necessary to consume up to several hundreds of grams of the original material in order to extract sufficient carbon.

In contrast, the AMS method works by actually counting the number of carbon-14 atoms present in a portion of the material. The essential difference is that we no longer have to wait for a radioactive decay to occur in order to count the carbon-14. Because of this, a measurement of the age of, say, a bone fragment, that may have required 100 grams of carbon to be counted for 12 hours using the old method, with AMS can be accomplished in 20 minutes and needs only 2 milligrams of carbon. This in turn means that rare or precious artefacts that would have been damaged or destroyed if dated by the radioactive counting method can be dated by AMS with almost no visible sign of interference.

Perhaps the most spectacular exploitation of the potential of AMS that has been in the news recently is the successful dating of the Shroud of Turin. Because of the very small sample sizes required for AMS it was possible to remove enough material from the Shroud to enable three different laboratories to make independent measurements. As is now known, there was excellent agreement between the three measurements, and the Shroud can confidently be regarded as having been fabricated about one thousand years ago.

Less publicised, but of greater archaeological significance, has been the use of AMS to demonstrate that *Homo sapiens* skeletal bones from sites in North America were of Holocene age, whereas dates based on amino acid racemization and uranium series analysis apparently indicated that human settlement in the New World existed from Pleistocene times. (Taylor *et al.* 1984).

The AMS installation at Lower Hutt, New Zealand, has been operating for almost two years, and has undertaken many radiocarbon measurements that would have been impossible, or at least impractical, to do with the radioactive decay counting method.