

**RAIDERS OF THE LOST AXE: ON MACROPODS, PHALANGERS, WHERE AND WHY -  
comments on Sutton's comment on Morwood & Trezise (and Pleistocene Axes)**

**BRUNO DAVID**

*Anthropology & Sociology  
University of Queensland*

Although I agree with Sutton that we should take care to avoid presenting our data as 'fact' until suitably rigid procedures have been employed in the collection of data and in the testing of ideas, I think that Morwood and Trezise (1989) should be congratulated for presenting information on a poorly executed excavation undertaken over 20 years ago, the results of which may never have seen light of day were it not for their recent paper. Morwood and Trezise make it clear to the reader that uncertainties remain concerning the provenance of the lost axe and their broader discussion of the axe's implications should surely be read in the context of 'if' the axe indeed is of Pleistocene origin. It is only by discerned reading and testing of ideas that the line can be drawn between 'fact' and 'false fact' (although I think the line between them is finer than Sutton implies). The establishment of a 'fact' has to be argued at every level of information presentation from the statement that the axe once lay in situ in the gravelly deposits near bedrock at Sandy Creek 1, to the view that the item was in 'fact' an axe, to the dating of that level to over 30,000 years BP, and so on. For many of these levels of data presentation, it is often assumed that the reader is able to assess for him/herself whether or not the purported 'fact' contains enough information to withstand discerned testing (e.g. I accept that the item is indeed an axe/hatchet as defined by convention). In other cases, lack of sufficient data (other 'facts') precludes us from accepting other presented information (e.g. lack of adequate stratigraphic control in the original excavations have created a significant amount of doubt over the authenticity of a Pleistocene context for the axe). Morwood and Trezise's paper, it is felt, is no different from many other archaeological papers in that data is presented and theories are formulated. It is up to the reader to determine whether or not the data is of sufficient rigidity to warrant acceptance of ensuing theories (as is the case with other archaeological publications).

The aim of this brief comment is two-fold. First, I wish to briefly comment on Sutton's use of stratigraphic information in the construction of temporal sequence (with broader archaeological implications). Second, I comment on the types of questions asked by archaeologists - the where, when, and why in prehistory. Both of these issues are important components of Sutton's paper.

## 1. Stratigraphic data

Sutton (1989:100) notes that McBryde (1977) suggested some correlation between an increase in the number of axes at Graman and a similar (though relative) increase in the number of Phalangeridae in the sequence. Specifically, MNI for families were calculated in four levels, including the surface of the deposit, spanning a period of around 4,000 years. McBryde's presentation of the data (1977:237) suggests, on the basis of the relative percentages of Macropodids versus Phalangerids and other taxa which show no significant changes, that a significant increase in Phalangerids took place through time. McBryde notes the importance of these observations while cautioning, "the validity of these correlations must be rigourously tested" (1977:237). Morwood and Trezise (1989:85) and others (e.g. McNiven 1988:153) have used McBryde's inferences as fact but have not carried out her request for rigourous testing of the correlation.

I make two comments. First, I doubt that these authors actually considered McBryde's inferences as fact; rather they offered them as a possible way of interpreting their data (an interpretation which deserves more attention). Second, (and perhaps more seriously) Sutton argues that a more adequate method of determining change in faunal exploitation is to present the faunal MNIs in terms of stratigraphic units. Clearly, the presentation of absolute numbers versus proportions will reveal different types of information, with percentages offering insights into the proportional use of particular resources. For instance, the cessation of fish-eating in Tasmania at 3000-3500 BP is better understood by considering its occurrence in relation to an increase in the exploitation of terrestrial mammals (wallabies). But a problem inherent in Sutton's quantification is the presentation of raw numbers per excavation or stratigraphic unit; that is, it is a presentation of raw numbers per depth of deposit. This is not a problem per se, but becomes one when researchers claim that they are in 'fact' comparing quantities of materials through time. In Sutton's case, because the analytical units he contrasts are of unequal time depth, it is not appropriate to say that "a marked decrease through time" in the MNIs retrieved from the excavations after 2250 BP equals a decrease in deposition rates (Sutton 1990:100). Sutton claims to be analysing the frequency of Phalangerid exploitation at different points in time, an exercise which by definition involves comparing MNIs per equal units of time through the duration of occupation at the site. For illustrative purposes Table 1 presents a conversion of McBryde's Macropod and Phalangerid data from Graman and compares MNI frequencies with calculated meat weights (Kg) per 100 years.

Table 1. Comparison of MNI and meat weight per 100yrs at the Graman site (calculated from McBryde 1977).

Age span	Macropodidae		Phalangeridae	
	MNI/100y	Meat Kg/100y	MNI/100y	Meat Kg/100y
0-1750 BP	0.0	0.0	0.2	5.1
1750-2050 BP	2.0	100.0	13.0	6.7
2250-3250 BP	3.4	170.0	6.0	180.0
3250-c3750 BP?	1.8	90.0	0.6	1.8

Noteworthy is the fact that the temporal patterning as calculated from the meat weights is different to that obtained from MNI. In the MNI case, there appears to have been a significant increase in Phalangerid exploitation approximately 3200 years ago, with a further increase at c. 2000 BP, an increase especially evident when compared to a contemporaneous decrease in Macropodid exploitation. It is difficult to determine the significance of the subsequent decreases for they include surface materials deposited after the abandonment of the site in post-contact times.

The point made here is that comparisons of absolute numbers (e.g. MNI) between excavation units mean very little when the latter are used as units of time. Investigations of temporal trends should be undertaken on data converted to a common denominator based on units of time.

## 2. Where, When, How and Why?

The second point I wish to address concerns Sutton's proposition that questions of 'why', 'rather than where' and when, deserve our archaeological attention (Sutton 1990:103). He argues that the important questions in the intensification debate are questions asking why it took place, rather than where.

If changes in the archaeological record can be attributed to particular behavioural changes, why did these changes take place? Moving the location of the origin will not solve this kind of issue and would not seem to offer any hope of aiding the progression of the discipline. (Sutton 1990:103)

It seems rather odd that Sutton adopts this line of logic given his previous statements on the need for care in the presentation of 'facts'. Before any bigger question can be asked, we need to inquire into what happened, when and where. No scientific inquiry into the whys can proceed until the basic 'facts' (the rhetoric) have been forwarded (unless one proceeds at an existential level). Sutton would have us take 'intensification' for granted and proceed in asking why it took place. But first we must ask 'what is it'. Where and when did what take place? Once these questions are answered (and let us assume that agreements are reached, leading to the establishment of a 'fact'), we can only begin to understand the whys by investigating the historical process - how did the observed historical transformations take place? Such questions are central to archaeological investigation and need to take historical trajectories into account. Such trajectories are case-specific and may only be appropriately addressed by reference to the conditions of existence at different points in time. It is these conditions of existence which define the context of stability and change in social systems. They define the distribution of power in society, the contexts under which it operates and the nature of human (social and individual) reactions to that power. In short, without understanding the **when** and **where**, we cannot define **what** happened, let alone **why** it happened. In effect, the asking of why in archaeology will always involve a leap beyond science, beyond the establishment of 'fact', into the realms of art and poetry where the imagination ventures beyond the bounded domain of the observable and testable.

## References Cited

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