

would significantly weaken them. Within the restricted range that these structures determine, however, theories may be assessed and hypotheses framed within theories evaluated. Consequently, at the low level and, in part, at the level of alternative theories refutation seems to be applicable.

This discussion may be summarised into a number of salient points. In the first place, refutation is not to be seen in Popperian terms as leading to truth, or in scientific terms as posing a methodological solution to solipsism. On the contrary, it is a procedure that permits hypothesis-evaluation within the confines of our traditions in the context of discovery. Low level applications of refutation seem most possible, followed perhaps by the level at which different theories are assessed; refutation does not, however, permit the rejection of deep-rooted structures. As the Foucaultians noted long ago, these seem to condition and therefore to negate conventional methodology.

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SOME SORT OF DATES AT MALAKUNANJA II: A REPLY TO ROBERTS ET AL.

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It is unfortunate when scholars feel the need to resort to illogical and personal vituperation, in lieu of being able to present a case which can stand on its merits. Roberts et al. (1990a) ask, what has caused me to change my mind? The answer is, nothing; I have not changed it, nor was there any need to do so. In 1989 it was my view that we need not, on present evidence, go beyond 40,000 years ago for the initial date of colonization of Australia. I also thought then, although I did not say so, that there would indeed not be anything especially surprising were earlier evidence to be demonstrated. That is still my view, on both counts.

I did (Bowdler 1989) mention two forthcoming papers in that context. *Search* made an editorial decision not to include the names or any other details of those papers. Obviously Roberts et al. can know absolutely nothing about them, **except** the fact that they contained a view similar to the one quoted above. Clearly, 'sadly out of date' is the new **bullyspeak** for 'not in agreement with me/us'.

Turning to the substance of their reply, we do not find a model of clarity which speaks for itself. We find the likes of 'If one wishes to estimate the total uncertainty at say

two standard deviations, the correct procedure is to deduct the systematic component from the total uncertainty, multiply the random uncertainty (which is characterised by the standard deviation) by two, and then add the systematic component' (Roberts et al. 1990a:94). Now if we are not completely dazzled by Science, we might be able to dimly discern what they could possibly be trying to say. I think this is to the effect that radiocarbon dates from within a single site are usually all quite independent dates from unconnected events. Thermoluminescence dates, on the other hand, from within a single sedimentary context are all connected with each other, due to the basic principle of the procedure, and each date in that sequence constrains each other one in terms of its uncertainty factor. It does still seem to me that the dates, if not singly, then as part of the series, must have some sort of error factor attached to them, and if it is not the one which is quoted in each case, what is it? Perhaps future replies might actually explain this. Figure 1 does not help much.

With respect to the argument about calendar years and dating methods, Roberts et al. (1990a:95) state firmly that geomagnetic evidence suggests that radiocarbon ages could be at most 5.5 kyr too young. 'These calibration corrections are insufficient to make a 40 kyr BP radiocarbon age equivalent to TL age of 50 kyr' (Roberts et al. 1990a:95). If that is the case, what is the explanation for the difference between the supposedly paired dates SUA-265 (18,040 ± 300 BP) and KTL 97 (24 ± 5 kyr BP) (especially if the latter does not have the error factor it appears to have)? Roberts et al. (1990a:95) go on to invoke contamination of radiocarbon dates as a possible source of underestimations. It actually seems to me that they are making the same point I was, but invoking a different reason: 'that Australian sites previously dated by radiocarbon are in fact considerably older in real time terms than they currently appear' (Bowdler 1990:93).

In my view however the real drawback to accepting the Malakunanja II TL dates is not to do with that dating technique, but lies in the archaeology. I accept the point (on face value) that the method dates the time that the sands themselves were last exposed, and thus the origin of the sediments is irrelevant. I do not think however that Roberts et al. (1990a, 1990b, 1990c) have convincingly demonstrated a clear association of dated sediments and the artefacts intended to be dated. I await with great interest, as I am sure Hiscock (1990)

does also, the 'detailed description of the archaeological data, which is under preparation' (Roberts et al. 1990a:95). Until that time, I remain sceptical about the stratigraphic association of the augur hole and the excavation; and the arguments against the working down of artefacts into the underlying sands I find sadly out of date (*sensu* Roberts et al. 1990a).

For such an important claim and given the absence of comparable-aged evidence elsewhere, we need unambiguous proof of the primary stratigraphic position of the artefacts in the dated sediments. Given the potential importance of the claim for human occupation in northern Australia at this time, the stratigraphic association of these artefacts with the deposit being dated has to be unimpeachable, and supported by direct excavation *in situ* (cf. Jones 1989:750, 762). I do agree that what is needed is replication of this dating at other sites and the establishment of a pattern of dates of this order of antiquity (Roberts et al. 1990a:96), but it is to be hoped that the archaeological associations involved will be better controlled.

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Editors note:

Roberts, Jones and Smith were invited to respond to Bowdler's remarks but felt that any comment was unnecessary.