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REFLECTIONS ON REFUTATION

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It is an episode of some consequence that a discussion on epistemology has erupted in the pages of Australian Archaeology. The debate has covered many of the thornier issues of philosophy in archaeology, ranging from the sociology of the discipline to the vagaries of methodology. This was quite an unintended consequence of an article which was originally aimed at elucidating only a small aspect of this spectrum. The larger picture has since been shifted into focus in the debate with Murray, most recently with greater clarity in *AA* 31. It is clear that both of us previously had little understanding of each other's views. For example, I attacked Murray's argument on plausibility when he was talking about tradition, and Murray has chosen to discuss tradition when the focus of the original essay was on which of two advertised testing systems might be most appropriate; tradition and plausibility are relevant to this discussion, but all topics can be discussed independently. Murray is correct in noting that I focused overmuch on the context of justification without including the role of tradition in the context of discovery. After all that, however, it is fascinating to see that Murray (*AA* 31:99) now agrees that 'confirmation, because it tends to be conservative of existing approaches, is less useful than refutation'. As this was actually the point of my original essay, it is unfortunate that we have had to discuss other issues in such a foolhardy manner when simple correspondence might have saved a lot of trees.

As far as I can gather, Murray is making the major point that there are deep-rooted conceptual structures present in archaeology. These structures underlie our methodology; our ideas of what is plausible, which may condition the test implications we choose; the hypotheses we elect to test; and our acceptance of various propositions. These structures interfere with our use of testing procedures such as refutation, so that the methods need not in any way lead to theory change. Rather, theories will primarily tend to change because the advocates of the established tradition accept such a change. This argument differs from that presented by Murray and Walker (1988), and indeed it seems that Murray's views are at odds with Walker's (Walker pers. comm. 1991). Walker (1990) has stressed that his major argument is that neither refutation nor confirmation need succeed without framing propositions in a 'scientific' manner as biconditional propositions. In this case he is probably somewhat more akin to my position, though I would argue that an emphasis on refutation as being superior to confirmation is still necessary.

Murray's argument is reminiscent of 'dark-Foucaltism' and its claim that traditions negate epistemology. It is clear that the argument is partly applicable to the recent history of archaeology in America and England. In America, as Meltzer (1979) has pointed out, the New Archaeology did not constitute a paradigm shift, but may be seen as a vigorous takeover of the traditional powerbase by younger archaeologists (Courbin 1988). This would seem to underline the point that traditions will only accept change if these changes are compatible with tradition. In England, where a theory (Diffusionism) was abandoned after the onset of C14 dating, one might argue that a tradition was still not overthrown. Rather, traditional archaeology could drop Diffusionism and accept theories such as Renfrew's trade models as these were, like Diffusionism, premised on comparative culture history and the explanation of the past in social terms. One might also (cynically) add that Renfrew was in the forefront of the establishment. Consequently, Murray's arguments do concur with recent theoretical history.

I would agree with Murray that tradition does exert a profound influence in archaeology. We appear to disagree, however, essentially over the relations of tradition to hypothesis-testing. In part this would appear to arise from somewhat different understandings of the contexts of discovery and justification. Murray seems to treat the two as direct opposites; they may well be opposites logically, but in practice they seem to function as parts of one structure. Archaeological traditions would seem to reflect a morass of biases, judgements, propositions and traditions (the context of Discovery); built into this context, however, is a clause that our arguments gain validity through testing. As a consequence, the Context of Justification is a subset of the Context of Discovery. The Context of Discovery is essentially our traditions; part of our academic tradition is the inclusion of a validation component.

This results in a case where it is accepted that all hypothesis-testing is theory-dependent, and subject to biases, prejudices or traditional discomfort over innovative research. The crucial point is that hypothesis-testing is built into tradition as a component to ensure innovation, whereas tradition is built in to ensure conservatism.

The major flaw in Murray's argument is the absence of an explanation for the acceptance of innovations. Presumably he is not arguing that tradition **should** predominate; if not, however, he then does not explain how theories may change. His recent acknowledgement that refutation has a role to play in theory-building seems to indicate some ambivalence on his part, which it would be nice to see clarified. At first sight, especially after his first instalment in this serial, one might see him as arguing that change is only possible when tradition permits it. Here one is tempted to categorise him with the anarchism of Feyerabend (1975), and to note the connection of disciplinary solipsism to the sort of academic terrorism promoted by Shanks and Tilley (1987). Such a view would correspond with modern worries that archaeology constitutes little more than statements that fit our preconceptions and bear no relation to reality (Hodder 1983, 1984). It would also reduce us to an 'anything goes' position in which theories are only judged better than others because of their ability to attract more adherents; put another way, because they are compatible with some defined tradition. One might well end up having to affirm here, for example, that Binford's ideas are only more meaningful than Kossina's because of traditional anti-Nazi sentiment within archaeology – which defines them as more 'plausible'. In such a system it is hard to imagine anything other than a Planckian metaphysic for change, in which new ideas only become popular when the old brigade are all dead.

It is apparent from Murray's second instalment that he is not entirely satisfied with this result. Murray does accept that refutation may have a role in theory change. This is actually something of an overt change on his part since his last comment, and his claims sit uncomfortably with the rest of his theory. How can refutation have a role if tradition is so omnipotent? What does it do, and why? How does it contribute to theory change, if it is so easily negated or ignored? Murray provides no answers. Indeed, it may well be that we have here an inconsistency in his position. The role of refutation in theory change must clearly be defined.

Murray appears (as is evident from his discussion of the C14 Revolution) to confine his analysis to high level theory. My essay was concerned, however, with low level applications. It is apparent that refutation does work at this level, despite the presence of established traditions. Archaeologists do refute statements that are of long standing and that are compatible with prevalent theories (such as humans have only lived in Australia for 6000 years) with test implications (dated sites) that relate to hypotheses (such as people may have lived in Australia since the Pleistocene). There is no guaranteed truth to any of these statements; rather, the theories and the hypotheses seem to be explicable as operating within an established tradition in which different views are to be evaluated by evidence, or by our interpretations of that evidence.

Tradition may obstruct change at this level – as the C14 example shows – but old statements do often seem to get thrown out. The C14 Revolution did inspire archaeological rejection of traditional European chronologies, and instigated thorough re-assessments of explanations of cultural relations, particularly between the Near East and Europe. As another example, despite the values that may lead to academic fraud, such acts may be detected, and often by empirical means. An example might be the dating of the Piltdown finds. One of the characteristics of frauds is that they tend to be successful, and to be in the forefront of their discipline as a result of their research (Broad and Wade 1982). This does not, however, stop their exposure despite the power of traditions to which they may belong. Consequently, tradition need not always counteract refutation at this level.

Why does refutation seem to function at this level? It may be that low level propositions about styles or chronology relate to some of the few functioning theories in archaeology, such as comparative culture history. The corollary to this is that theories with weak or ambiguous test implications, such as those about human mobility patterns or intensities of site usage, may not be easily refuted. It may also be the case that, should an hypothesis or theory be rejected, alternatives are present. It would seem unlikely that refutations would be accepted without alternatives being available.

This leads us to the workings of refutation. Lakatos (1970) stressed that many things needed to be taken into account before accepting refutations. Archaeological parallels may be drawn. For example, Hodder (1982) argued that stylistic and cultural differentiation need not be equatable. His claim has not, however, led to a decline in comparative stylistic or cultural studies. This may partly reflect a traditional bias toward culture history. It may also reflect the fact that Hodder did not provide any data with which to evaluate his claims; one would scarcely be justified in abandoning a theory without having the data to ascertain that it was improbable. Finally, some archaeologists such as Collett (1987) have tried to modify our understanding of stylistic and cultural relations, and to define the limits of the theory. This is precisely the role of refutation, and demonstrates the link between refutation and innovation. It is clear here that refutation as such operates rather like normal archaeological hypothesis evaluation; the difference is only that attention is paid to disconfirming data.

When assessing different theories refutation seems to be less useful. There are examples of the rejection of theories on empirical grounds (such as the refutation of the Theory of the Noble Savage in the early nineteenth century), but it would seem that theories may only be abandoned if alternatives are available. It is more likely that the theory will be modified, as the theories of Monogenism and Polygenism have been to form our current theories of cultural evolution.

It may be that Murray's arguments have some force here. His characterisation of the C14 Revolution is perhaps not apt, as the onset of radiometric dating may not have changed the conceptual structures of European prehistory (which I did not argue it did) but did force the re-assessment of theories about European prehistory such as Diffusionism (which I did argue). Murray's notion that C14 dates did not create a move towards Materialism is also somewhat crude; the evidence need not create a move towards anything. One could have looked for diffusion from Mars as equally as one could have turned to Materialism. The point is that the new dates did significantly weaken Diffusionism so that a new theory seemed necessary. This may well be a case of refutation working at a higher level; by showing Diffusionism to be invalid it may have inspired the proliferation of newer theories such as those based on trade and exchange. In this case refutation does seem to contribute to the deployment of innovative theories.

The deep-rooted structures that operate within academic traditions would seem to form a third level. It is clear that at this level refutation is inapplicable. Concepts such as the Idea of Progress remain in vogue in modern theories of cultural evolution, or in modern theories about hunter-gatherers changing from 'simple' to 'complex'. This is despite past refutations of Progressivist assumptions and the presence of fields of research into cultural variability that are specifically non-Progressivist, such as Structuralism. The deep-rooted structures would seem, then, to be impervious to evaluation, and may well condition theorists into looking at archaeology in a restricted way. It is doubtful whether, even if we are aware of these concepts, we 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 scientistic 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 Foucaultans 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