

Revisiting the 'Neolithic Problem' in Australia

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ABSTRACT – The more we learn about varieties of subsistence, the less clear definitions of 'agriculture' become, and the harder it is to see the Australian and New Guinean data as falling into separate classes. Some Australian data, if found in New Guinea, would label those societies as agricultural. I suggest two avenues, residue analysis and historical research, along which research in this matter might usefully continue.

KEYWORDS: agriculture, domestication, New Guinea

INTRODUCTION

More than 35 years ago, I began a paper on the 'Neolithic Problem' by quoting Capt. John Moresby's (1876: 18) contrast between Australia and New Guinea (White 1971). Prominent in Moresby's account is the difference between the Australians 'living precariously on wild fruits' and the 'Papuan' Torres Strait Islanders, who 'supply themselves with constant vegetable food'. I think it is time to revisit that contrast, which I then unquestioningly accepted. I do this in the spirit of Gosden and Head (1999) who point out that 'the deepest divide we see [between Australia and New Guinea] is that imposed by European thought' (1999: 233), especially its division between hunter-gatherer 'savages' and agricultural 'barbarians'. They then go on to argue that while food obviously looms large in life, there is much greater similarity in the overall cultural structures of New Guinea and Australia than is generally accepted. Part of the problem is the misconception that all of each area can be unambiguously placed within a single economic category. Unlike Gosden and Head, however, I propose to remain focussed on subsistence and on the assumptions which are built into our archaeological considerations. I draw attention first to the way in which most commonly accepted definitions of 'agriculture' raise problems when faced with actually trying to apply them to the archaeological record and second how Australia has been downplayed in discussions of agriculture both because of these definitions and because of a mind-set that has a massive historical inertia.

AGRICULTURE

I start with the concept of agriculture which, as Harris (1996: 3) notes, 'is characterised by a confusing multiplicity of terms for [our] conceptual categories'. Among the archaeologists, Smith's recent interesting discussion (2001) agrees with Harris (e.g. 1989), Yen (1989, 1995) and Winterhalder and Kennett (2006: 3) that agriculture is defined entirely by the presence of domesticated plants. Conceptually, he centres the process of domestication firmly in the divide between hunter-gatherer and agricultural societies, encouraging us to consider societies which are neither of these as 'low-level food producers', whether or not domesticates are present. Smith (2001: 14) defines the two main characteristics of domestication as: a) species-specific genotypic and phenotypic change, and b) the reliance of these changed variants on humans for survival. This gives us a 'clear and constant vantage point'. Agricultural societies are then defined as those relying on domesticates for >50% of the annual calorific budget; societies whose reliance on domesticated plants is less than this are 'low-level food-producers'. Harris (1996: table 1) similarly draws a line between cultivation, which may include land-clearance and systematic tillage, and agriculture, which is 'largely or exclusively' based on domesticated plants.

Some archaeological definitions of agriculture encompass a wider range of data. Nesbitt (1996: 19) for example, considers that some or all of the following should be used in making claims about the presence of agriculture: a) physical landscape changes, either direct (e.g. field systems) or indirect (e.g. pollen, charcoal), b) plants, including

types and number of varieties, and c) human diets deduced directly from bone chemistry and morphology. Despite this wide-ranging approach, he continues to define agriculture by the presence of domestication. In other words, among a polythetic set, one criterion is necessary and the others are ancillary.

On the other hand, some archaeological discussions have suggested either broadening the concept or that it should be abandoned. Spriggs (1996: 525) used the concept of 'agro-ecosystems that limit subsistence choice because of environmental transformation or labour demands', pointing out that Yen considers morphological or genetic evidence of change in plants is unlikely to exist in many tropical root and tree crops. Note that this implies domestication in Smith's sense [a) above] will be impossible to find. Recently, Terrell et al. (2003) have argued for replacing the graded steps towards agriculture proposed by Smith and Harris with 'domesticated landscapes' and a 'provisioning table' which focuses on the strategies used to acquire and maintain specific plant resources.

It is noticeable that all these definitions can be shown to be quite workable when present-day societies are discussed, but are clearly much harder, if indeed possible, to operationalise in an archaeological context. Smith notes that determining the calorific intake of prehistoric societies is unlikely to be feasible, though he argues that seeing genetic and morphological changes in species should be easier. His specific examples are drawn from South-West Asia and the Americas, where seeds and other hard plant remains are common. He rather dismisses other kinds of plant/human interaction: 'Given the multitude of overlapping and conflicting definitions and applications assigned to horticulture and garden over the years, these labels tend to confuse rather than to clarify when they are employed in attempts to characterise any of the regions on the conceptual landscape between hunting-gathering and agriculture' (Smith 2001: 22). His discussion thus suggests root and fruit crops are relatively unimportant. Nesbitt's comprehensive criteria may allow determination of whether societies were actually reliant on specific crops, but such a range of data is rarely obtainable from a particular archaeological record. Terrell et al. (2003: 354) say that for any prehistoric resource 'one must try to gauge the likely harvesting skills that were used', a process for which the guiding principles are unclear unless we assume that the past is like the present.

Looking beyond the archaeologists' attempts to capture the concept of agriculture, even less clarity is apparent. Ingold (1996), for instance, would dissolve any distinction between collection and production and replace both with an analysis of

growth in relations between humans and other species, i.e. the relative scope of human involvement with particular plants (or animals). He notes that this may be easier to see with short-lived plant species than with trees which may outlast several human generations. Appealing as this concept is, and others like it (e.g. Rindos 1989), the problem of observing such processes in archaeological evidence, unaided by direct ethnographic analogy, seems to me to be even more apparent than it is with the archaeologists' definitions.

If, as the literature demonstrates, concepts of 'agriculture' revolve around the domestication of plants, how extensively can this be observed or inferred in New Guinea and Australia?

NEW GUINEA

Many New Guinean societies, both highland and coastal, were, to European eyes, agricultural when contacted. Their staples were often a few varieties of plants which were grown in fenced gardens, but these were nearly always supplemented by many other species, which were cultivated, curated or collected. The phenotypes of some of these plants, notably bananas, taro, sugar cane and, more recently, sweet potato, were certainly modified from their original forms and some of the new phenotypes would not survive without human help. In some localities agricultural practices resulted in visible changes to the landscape, such as ditches, mounds, walls and terraces. As archaeologists, our primary assumption has been that these visible endpoints, agricultural in the restricted sense described above, have a history. But until recently this history has been based entirely on direct and proxy evidence of landscape modification, inferences from settlement sizes and assumed permanency, and the appearance of exotic domestic animals and pottery (cf. e.g. Golson 1977, Denham et al. 2004a for the highlands, Spriggs 1996 for lowlands, but cf. Denham 2004 for a contrary view), rather than on actual plant evidence. In the last few years some actual plant evidence has been forthcoming. The history of morphological change in some tree crops is being provided by macroscopic remains and pollen data (e.g. Lepofsky et al. 1998). Microfossils such as starch grains and phytoliths, along with proxy evidence of soil modification and landscape firing, demonstrate that humans were intimately involved with the same plants as are widely used today (e.g. Denham et al. 2004b; Fairbairn 2005). But it is also true that none of the critical criteria outlined above – morphological changes in species, primary dietary reliance on them, limited subsistence choice or 'planned and repeated environmental manipulation' (Terrell et al. 2003: 352) – are archaeologically yet documented for any specific

crops in prehistoric New Guinea (e.g. Haberle 1995; cf. Hather 1996).

In New Guinea, it is the ethnographic evidence of a wide-ranging spectrum of reliance on plants often organised into recognisable gardens (although crops such as sago and pandanus are usually not), that has given us an end-point back from which we work. If there were no ethnographic evidence, our reconstruction of agricultural history would be much less certain. When New Guinea societies became 'agricultural' in terms of any of the above general definitions is almost impossible to assess – the best example of this being Kuk Swamp in the Highlands (Denham et al. 2004b, and references therein). In fact, according to these definitions many New Guinea societies are probably not 'agricultural' even today (Guddemi 1992; Roscoe 2002).

AUSTRALIA

Australia has been generally regarded as populated by hunter-gatherers ever since Captain Cook in 1770 described its people as having '... no fixed habitation but move about from place to place ... in search of food ... [and] ... we never saw one Inch of Cultivated land in the whole country' (Journal 23 August 1770). Research over the past 30 years has shown that while this characterisation may partially apply to some Aboriginal groups Cook encountered (e.g. Keen 2004: 117–120), in other areas of the country things were different (for a general summary Keen 2004: 94–96). However, the Australian ethnographic picture is heavily obscured by the European invasion and its accompanying mindset. Until recently, the rapidity with which Aboriginal societies were destroyed and the extent of that destruction have been underestimated by scholars (Butlin 1983). The invasion was accompanied by diseases, which sometimes seem to have preceded the arrival of actual people. Many Aboriginal groups lost half or more of their population within a very short time, sometimes as little as a year (e.g. Attenbrow 2002: 21–22). Because of this social and economic destruction, we need to consider whether some ethnographic and archaeological data point to a need for a more directed search for Australian economic behaviour prior to this event.

One of the best studies using the very earliest European accounts is Hallam's synthesis of southwest Australian ethnography (Hallam 1986, 1989). In the early 19th century, a number of alluvial river flats, spread over several hundred kilometres north from Perth, were noted as having 'superior huts, well marked roads, deeply sunk wells and extensive warran [yam] grounds' by the shipwrecked George Grey. He and other early observers remarked upon the relative permanence of occupation, with its concomitant proprietary

rights to areas for harvesting both yams (*Dioscorea hastifolia*) and reed rhizomes (*Typha* sp.). Firing the country away from yam gardens was frequent, and the rights to this were also carefully controlled. Hallam notes (1989) that in Western Australia: a) *Dioscorea hastifolia* was grown further south than any other yam, b) the alluvial flat environments in which these yams were primarily grown by Aborigines were very different from those of their natural habitat of open woodland with granitic and basaltic soils, and c) their growing period was the reverse of that of tropical yams of the same species. That these differences were entirely the result of chance seems remote: a process of 'domestication' (following Rindos 1989) is likely to be at work. Whether this may have been deliberate morphological transformation followed by clonal selection leading to the new varieties, along the lines documented by Doumont and Vernier (2000) in Benin, may not now be determinable. But, as they note, domestication involves creating genetic diversity within the germ plasm, and this might be visible genetically. But what are the chances of finding this in the archaeological record?

We rarely have such detail elsewhere. In south-eastern Australia the daisy yam *Microseris lanceolata* (formerly *scapigera*) was the staple food for people in many regions, with dense concentrations of plants over large areas being recorded by some early reporters; one statement, for instance, describes plants by 'the millions' (Gott 1982, 1983). Whether its morphology or genetics were altered by Aboriginal cultivation remains to be determined since all the fields were rapidly destroyed by sheep and the plant is so far unidentified in archaeological contexts. The botanist Beth Gott considers that it was a staple resource 'which could be used year-round' (1982: 10) and notes that recorded Aboriginal treatment of the plants in the form of soil preparation, fertilising by burning, and tilling by harvesting would have increased harvests (see also Gott 1999a). It may also have altered the plants in ways similar to those recorded by Chikwendu and Okezie (1989) for another variety of root vegetable in Nigeria. Additionally, in limited areas there is clear evidence of landesque capital investment, such as the elaborate water-control systems for eeling (Coutts et al. 1978; Lourandos 1987), with their 'thousands of yards' of trenching and stone banking and their ethnographically described (if archaeologically hard to detect: Clarke 1994) villages. European reports note frequent warfare over territorial rights and quite dense populations even after the early epidemics. Such data imply low mobility and considerable landscape reorganisation, though this is not documented to relate specifically to curation of plants.

There is strong evidence that similar styles of

economic behaviour existed elsewhere before contact, but have either gone unreported or the implications have not been fully explored. The most obvious case is the Central and Lower Murray Valley (Pardoe 1994, 1995) and perhaps other riverine areas. Pardoe's remarkable syntheses of Murray River evolution and human skeletal biology (Pardoe 2006), along with evidence of the development and nature of cemeteries (Pretty 1977; Pardoe 1988; Littleton 1999) argues strongly for very high population densities and a territorial organisation based on strongly exclusionary principles. This is supported by Webb's (1984, 1995) research on tooth wear, pathologies, stress and decrease in stature, all of which lead him to describe the Central Murray communities as 'very settled', and consisting of a 'large, sedentary population intensifying its economy to feed itself' (1995: 280). Further, a heavy starch diet is supported by Webb's observation of thick calculus build-up on teeth from burials around the Willandra lakes dating back to at least mid-Holocene. The calculus is similar to deposits on teeth from the Papuan coast, where the standard diet was sago (Webb 1995: 279–280). Proprietary rights to plant food sources and the elaborate manipulation of these may be legitimately inferred in contexts in which pathologies and diets of the past are so similar to those of many undoubted agricultural communities (Cohen and Armelagos 1984; Ulijaszek 1991).

A range of plants was probably used to sustain these very dense Murray populations. Given the swampy billabong nature of the river valley itself, roots and rhizomes from rush-type water plants such as cumbungi (*Typha* spp.), *Scirpus* spp. and *Triglochin procera* seem likely to have dominated (Gott 1983, 1999b). In drier country *Microseris lanceolata* is probable. Aboriginal field treatments of burning, aerating the soil by digging and harvesting are likely to, and may have been intended to, have synchronised and increased harvests, but in the longer term they will have changed plant phenotypes. The start of this riverine economy can be traced into at least the early-mid Holocene (Pardoe 1995).

Another area where a similar society and economy is more tenuously inferred is coastal south-east Queensland. By inference on the basis of the Murray data, Broadbeach and other cemeteries (Haglund 1976: 79; McNiven 1991: 14) firmly mark group areas. Hall and his students have documented rich resources, both plant, especially bungwall (*Blechnum indicum*) (Hall et al. 1989), and marine (Hall 1982, 1999), which allowed high density populations, at least during the recent past. Apart from the cemeteries, little landesque capital has been reported.

DISCUSSION

Australian societies have always been characterised as hunter-gatherer. Further, it is largely on the basis of ethnographic evidence, starting with Cook and continuing until the 20th century, that the Australians have been described as 'domesticating the environment' rather than domesticating plants (Yen 1989, 1995). In Smith's terms, they are located among the low-level food producers. But the ethnographic and archaeological evidence I have cited suggest that some Australian societies may have altered the plants, and clearly developed the infrastructure, in ways very much equivalent to some New Guinea situations. I suggest that had the Australian ethnography described above been found in New Guinea, the 'hunter-gatherer' cachet would have been less readily applied. Further, because Australians are 'hunter-gatherers', there has been little incentive to investigate the archaeological record for the presence of agriculture or domestication in the senses described above.

It is, I believe, time to break away from the current attitude and accept that agriculture may have occurred in parts of Australia. In looking further for evidence, there are two obvious avenues to explore.

The first is to expand on Sylvia Hallam's pioneering research into the historical and ethnographic evidence of landscape alteration. How much of this evidence remains in the face of the large-scale and continuing European transformation of the landscape is probably questionable. Earthmoving for subsistence in Aboriginal Australia had no reason to be quantitatively large per unit area. The smaller scale activities observed by Grey – paths, mounds, wells etc. – were quite quickly obliterated and the same is to be expected elsewhere, wherever the white invaders settled. Today, traces are likely to be found only where European subsistence activities have not found the ground useful. Otherwise, our sources will be in the historical records, and it is hard to believe these have been exhausted.

The second avenue capitalises on a recently developed area of research, plant microfossils such as starch (Torrence and Barton 2006) and phytoliths (Piperno 2006). Since Australian staples were roots, there will be no evidence of them in the form of hard seeds or skins. But they did consist of starch and may well have contained phytoliths. Research in Australia has already demonstrated that plants processed by stone tools can be identified, such as *Blechnum indicum* and *Marsilea* sp. (Fullagar 2006: 181–182). Elsewhere in the world, changes in starches have been used as signals for domestication (e.g. Piperno et al. 2000; Perry 2002), but similar developments remain to be pursued here.

Another potential area of investigation through microfossils is study of their changing occurrences through time as indicative of patterns of human use of the local environment. This approach has been successfully carried out in New Guinea (Lentfer 2003) as well as in other parts of the world. It is both highly technical and time-consuming, but is probably the best chance of reaching back into the real economic world of the past which, I suggest, may have been quite different to the one we think we know.

CONCLUSION

Was there ever a 'Neolithic Problem'? My answer now is yes, but primarily in our own minds, guided by the definitions and assumptions deriving from models imported from elsewhere. What the last 30 years of research have shown us is that these models are difficult or impossible to apply even in the real-world contexts for which they were supposedly designed, and that they are probably quite inapplicable to Australia or New Guinea. Data from these two areas demonstrate how strongly our preconceptions have shaped our thinking about subsistence behaviour.

Our current difficulties in this area may be largely to do with scale. Instead of contrasting Australia and New Guinea, the research focus needs to be narrowed down to specific environments and subsistence practices. How did different communities change their resource organisation over time? What was the impact of this on their plants – and vice versa? Which communities were involved and are common variables visible? What was the impact of such changes on population numbers, distribution and social organisation? In the face of such questions, definitions of 'agriculture' and the hierarchical ordering of groups into hunter-gatherers, small-scale food producers and agriculturalists becomes irrelevant. The 'Neolithic Problem' disappears, replaced by a more humanly scaled prehistory.

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