Triangles and Tripods: Co-ordinating Archaeology, Anthropology, Genetics and Linguistics in San Prehistory

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ABSTRACT

Despite their multiple appearances on the archaeological and anthropological stage archaeologists, at least, need to ask if our wealth of knowledge concerning hunter-gatherers in southern Africa is a difficulty, as much as a source of enlightenment, for reconstructing their past. This paper explores some of the challenges that this situation creates, particularly with regard to how far archaeological selections of San ethnography may be representative of the whole and the time span over which those selections can – or should – be employed. It also considers some of the caveats that arise when trying to establish viable links between the archaeological record and the histories written by geneticists and linguists. It ends by identifying some instances where a multidisciplinary approach may yield better answers about details of San prehistory than any one discipline can offer by itself and by emphasising two particular contributions of the archaeological record: the independence of its chronology and its capacity to document within a palaeoenvironmental context fluctuations in the size, density and distribution of past populations.

My title seems cryptic, but captures, I hope the need to triangulate between disciplines to advance understanding of southern Africa's prehistory. That triangulation must identify questions and strategies for transcending a situation that recognises the important contribution that each makes, but leaves them in a state of autarky, or at best nervous détente (a one-dimensional triangle). It must simultaneously identify appropriate research questions and suitable research strategies for moving toward a situation where, by collectively investigating topics of mutual concern, the combined efforts of different disciplines can provide a more solid, stable and thus informative set of answers (a three-dimensional tripod).

I have just finished introducing another cohort of first year students to Oxford's Archaeology & Anthropology degree. It is difficult to imagine how one can study – or teach – either subject without drawing on southern African hunter-gatherers. But despite their multiple appearances on the archaeological and anthropological stage archaeologists at least need to ask: is our wealth of knowledge concerning hunter-gatherers in southern Africa a difficulty as much as a source of enlightenment for reconstructing their past? If so, what do we do? And where do we find viable links with the histories written by geneticists and linguists? Looking first at how representative our San ethnography is and the time frame over which it can be used, and then at genetic studies, I try to identify instances where working together may yield better answers than any one discipline can offer by itself.
Bound by laws in which they have no representation?

The southern African Later Stone Age, a mostly microlithic set of stoneworking traditions that kicks in around 25,000 years ago, has long been recognised as the handiwork of people similar in material culture and behaviour to ethnographically recorded San. So striking are the correspondences that some would describe it as "Khoisan history". Archaeological mining of the ethnographic record has now gone far beyond technology and subsistence to explore the social relations and worldview of precolonial hunter-gatherers, yet those explorations still depend almost entirely upon just three groups: the now extinct /Xam of the Northern Cape; the Ju/'hoansi of the northwestern Kalahari; and the G/wi of central Botswana.

Their ethnographies have unquestionably massively enriched understandings of the LSA, but archaeologists rarely consider how partial this selection is. Back in 1992 Alan identified some 20 extant San groups so why do archaeologists privilege just these three? What of the rest, the Nharo, !Xõ, Hietshware or Shua, for example? And what of variability within, as well as between, particular groups? Second, all three groups – like most other surviving San peoples – come from just two of southern Africa's eleven terrestrial biomes, the Kalahari Savanna and the Nama-Karoo. The hunter-gatherers who once lived in the other nine, which include Mediterranean-like fynbos environments, highland grassland, evergreen forest and a variety of woodland savannas, have no ethnographic record worth the name. If people even partly structure their behaviour along ecological grains archaeologists are clearly missing a lot by only employing Kalahari or Northern Cape ethnographies. Worse, they risk universalising those ethnographies to all southern African hunter-gatherers, regardless of time or space.

Hxaro provides a good example. First brought to archaeology by Lyn Wadley, for two decades it was identified from the presumed material proxies of its preferred exchange items – beads and arrows – without ever really checking if those same items had moved anywhere at all. Indeed, in archaeological writing hxaro simply stood in for 'gift exchange', despite the fact that not all San practise it and that those who do also engage in other kinds of trade. The circumstances in which hxaro, rather than other kinds of exchange, is practised, whether such circumstances are archaeologically identifiable and how we can show when things did move across the landscape over distances suggestive of exchange all received far less attention. To paraphrase Abigail Adams, wife of John Adams, second president of the United States, archaeologists have ended up binding all precolonial San by laws in which they have little or no representation. Put bluntly, the knee-jerk attractiveness of Kalahari-based analogies can significantly water down the process of robust hypothesis creation and testing, a point I now develop.

An ethnography for all seasons?

If Sir Thomas More, the early sixteenth century Tudor statesman and author, was a man for all seasons, can we say the same of San ethnography? In other words, how far back can we credibly use San ethnographies to explain the archaeological record? Three decades ago the answer would have been no further than the start of the LSA because only then did we find microlithic toolkits, bone points, ostrich eggshell beads and – by a slight of hand as they actually come from terminal Middle Stone Age deposits - the first rock art. Two decades ago
Janette Deacon and Lyn Wadley cut this timeframe in half, to 12,000 BP, as before then LSA assemblages have so few beads or bone points (the *hxaro* idea again!) that "social practices not unlike those observed historically among San, may have had their origin late in the Pleistocene." At broadly the same time, however, Hilary Deacon argued that 60,000-year-old Howiesons Poort segments at Klasies River were equivalents of mid-Holocene segments and that as the latter supposedly functioned as arrow armatures and arrows were used in *hxaro*, so *hxaro* was practised back in the Howiesons Poort, one of several components to his overall thesis that anatomically modern people at Klasies River behaved in modern (i.e. San-like) ways.

We now know that some Howiesons Poort segments probably did arm arrows and that humans have used material culture in complex symbolic ways for at least 100,000 years. With South African sites in the vanguard, this paradigm shift may tempt us to generalise our knowledge of San ethnography well back into the Pleistocene, but we should recall here the words of the Lord's Prayer, "Lead us not into temptation". Why? Because San ethnographies belong not only to quite specific spatial, ecological contexts, but also to equally specific temporal ones, the core contention, of course, of the Kalahari Debate. A whole series of observations make this plain, not least, as Larry Barham has pointed out, that we have virtually no data to connect ethnographically recorded San to stone tools and even fewer regarding their use of rockshelters, two of the major datasets on which archaeologists depend.

Here we get to the core of the embarrassment of riches with which southern African archaeologists must deal: we know that the ethnography is – in many detailed ways – linked to the archaeology and we desperately wish to make maximum use of it, but we must also recognise that it is partial, unrepresentative and the product of particular historical trajectories. To overcome Martin Wobst's 'tyranny of the ethnographic record' and 'de-!Kung' southern African prehistory, in John Parkington's compelling phrase, I offer three suggestions, none new, but all increasingly in need of application and exploration.

First, archaeologists could prioritise investigations of San ethnographies. Though Larry Robbins' work in and around the Tsodilo Hills is an exception, we still know shockingly little of the history of Kalahari San peoples. Even for the /Xam, the source of the Bleek/Lloyd archive that is crucial to understanding San rock art, we have very little archaeological detail beyond Mark's work and a few other studies. Extending our ethnographies from Braudel's *évenements* in the direction of his *longue durée* has yet to be achieved.

Second, archaeologists must invert their gaze. Instead of asking what San ethnography can do for them they should ask what archaeology can do for San ethnography, following the lead not just of Jack Kennedy but of scholars like Ann Stahl, whose work in Ghana stresses the importance of working back from the present into the past to identify when and where ethnographically recognisable behaviours first appear, how far they can be reliably traced, and how best we can identify convincing material correlates for aspects of social relations.

Third, archaeologists should look beyond the Kalahari to situate southern Africa's hunter-gatherers in a global context. Mobility and group size, dispersal and aggregation, diet breadth and technology, exchange and land tenure, egalitarianism and storage all show patterning in
relation to ecological variables. By exploring such patterning and being more open to the experience of hunter-gatherer research in other parts of the world archaeologists may be able to transcend some of the limitations that the Kalahari/Karoo record inevitably creates. But if they – we - shy away from all this, then I see little escape from extrapolating San ethnography across time and space regardless of southern Africa's enormous ecological diversity, despite the major climatic and environmental shifts of the last 100,000 years and in ignorance of the fact that far from being living fossils today's San have rich histories of their own making. Neither the academic nor the political implications of this are appealing.

**Genes and languages**

Where does the work of geneticists and linguists fit? Without debating any specific claims in detail I make some general points. First, a confession of ignorance: archaeologists are, by and large, ignorant of – and unable critically to assess - the detail of your methods. Second, because of different publication outlets they also often seem unaware of your work. As well as workshops such as this, we could do worse if, from time to time, archaeological journals would accept or commission state-of-the-art surveys like Tom and Mark's paper of 2008. Third, many genetic studies need to become much more accurate about the ethnic and geographic origins of the samples they employ and – at least when dealing with questions that transcend Kalahari prehistory alone – must avoid homogenising those ethnicities and linguistic identities under a single 'Khoisan' umbrella. Fourth and most importantly, because almost all published 'Khoisan' genetic studies relate to communities drawn from the far north of southern Africa and sample only a few populations even there, much more extensive sampling of extant populations living elsewhere in the Kalahari needs to be undertaken, plus sampling of San-descended communities to its south, such as in Lesotho, Mpumalanga and the Karoo. It is reassuring that geneticists are starting to do just that, not least by exploring whole genome studies and by seeking palaeoDNA in human remains from the non-tropical Cape, the one part of southern Africa where conditions are probably most propitious for its survival.

With an expanded dataset we can better tackle questions of common interest. Some examples: first, the Ju-≠Hoan and Tuu language families appear to have no demonstrable genealogical link and occupy quite distinct parts of southern Africa, even allowing for the later expansion of Khoe between and around them. Is this because of expansion from separate northern and southern refugia to which they were once confined in a context of environmental stress? If so, was this Isotope Stage 2 (25-12,000 years ago) or much earlier, in Stage 4 (70-59,000 years ago)? Can genetics help answer this? Do Ju-≠Hoan and Tuu speakers, in short, show evidence of different ancestries and, if so, when did they last share a common ancestry?

Second, archaeology suggests a major recolonisation of South Africa's interior as climate ameliorated across the Pleistocene/Holocene boundary with populations south of the Limpopo and the Orange sharing broad outlines of their artefact traditions with others in Zimbabwe and Namibia. Can we pick up genetic, or even linguistic, traces of this expansion?

Conversely and controversially in so far as his work is based heavily on lexicostatistics, if Nigel Crawhall is nevertheless correct in his glottochronological estimate that N/u and !Xo
speakers diverged at least 2500 years ago, expanding north from somewhere in the Free State or southern Cape, can genetics and archaeology provide supporting evidence of the same?

Fourthly, what more can we learn about the timing and quality of San interactions with Bantu-speaking immigrants from integrating more detailed genetic analyses with studies of linguistic and cultural borrowings? For example, will they confirm what archaeology appears to suggest, namely a very rapid displacement/assimilation across most of Zimbabwe compared to many areas further south? And can they discriminate in South Africa between interactions of the first and second millennia AD given that archaeology suggests Nguni and Sotho-Tswana languages only arrived in the region around 1100 and 1300-1400 respectively?

Finally, can we learn something about the chronology of language change by taking advantage of the fact that the archaeological record documents the relatively well-dated introduction of a whole series of new technologies and species within the last 2000 years? Relevant examples include the dog, iron, pottery, and perhaps cannabis, along with the better-known instances of cattle and sheep. Much more recent, because European in ultimate origin, are maize, tobacco and horses. What terms are used for these novelties and how do the terms in one language relate to those in others?

In all these cases, two particular aspects of the archaeological record demand attention. First, that record can be securely dated independent of assumptions about how its content formed or changed. Second, its dating unambiguously shows that human occupation of every region of southern Africa has varied in presence/absence and in scale over time, in large part probably because of climatically linked changes in resource productivity. Archaeological data and the palaeoenvironmental evidence recovered from archaeological excavations thus help create a context for thinking about fluctuations in the size, density and distribution of populations that presumably affected the spread and transmission of genes and languages alike.

I began with one quotation from the American Revolution. Let me end with another, Benjamin Franklin's well known assertion that we must, indeed, all hang together, or assuredly we shall all hang separately. Perhaps KhoeSan studies need not fear quite such a dramatic fate, but we shall certainly make much more progress through concerted action than by pursuing what have historically often been too separate a series of paths.