Primordialism and the ‘Pleistocene San’ of southern Africa

Justin Pargeter, Alex MacKay, Peter Mitchell, John Shea and Brian A. Stewart

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Analogies are an important tool of archaeological reasoning. The Kalahari San are frequently depicted in introductory texts as archetypal, mobile hunter-gatherers, and they have influenced approaches to archaeological, genetic and linguistic research. But is this analogy fundamentally flawed? Recent arguments have linked the San populations of southern Africa with the late Pleistocene Later Stone Age (c. 44 kya) at Border Cave, South Africa. The authors argue that these and other claims for the Pleistocene antiquity of modern-day cultures arise from a fundamental misunderstanding of the nature of cultural and archaeological taxonomies, and that they are a misuse of analogical reasoning.

Primordialism and the ‘Pleistocene San’ of southern Africa

Justin Pargeter1,2, Alex MacKay3, Peter Mitchell4,5, John Shea6 & Brian A. Stewart7,8

Introduction

That living humans have late Pleistocene ancestors is beyond dispute. All humans alive today descend from people who lived in Africa between 100 000 and 200 000 years ago (Stringer 2012). None of us is morphologically identical to those ancestral Africans; rather, we share with them an evolved capacity for wide behavioural variability (Shea 2011). These facts stand in contrast to ‘primordialist’ claims that particular ethnic groups have survived largely unchanged since Pleistocene times (e.g. Sollas 1911). Here, we review recent arguments linking the San populations of southern Africa with the late Pleistocene Later Stone Age (LSA) (c. 44 kya) at Border Cave, South Africa (d’Errico et al. 2012). These and other claims for the Pleistocene antiquity of modern-day cultures arise from a fundamental misunderstanding of the nature of cultural and archaeological taxonomies, and are a misuse of analogical reasoning. Our discussion is relevant not only in southern Africa, but to archaeologists everywhere.

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Background

Analogies are a critical component of archaeological thinking (Wylie 1989). They come in two primary forms: direct historical, and relational. Direct historical analogies require that a demonstrated cultural continuity exists between source and subject. They are equivalent to proposing homologous relationships in genetic and cultural evolutionary studies (Shennan 2009). Due to the difficulties with using archaeology to trace living communities directly back into the past, analogies that assume heritable continuity are rarely unproblematic (Lane 1994–1995).

Relational analogies require that a relational link (functional, raw material or ecological resemblances, for example) exists between source and subject. By this logic, relational analogies explore the causes for apparent similarities and differences. This form of analogy requires that the quality of an analogical argument be assessed by examining whether the relevance and relational structure of the analogy are valid in the first place.

The San populations of southern Africa are among the best known and best documented ethnographic hunter-gatherers; they have been studied extensively over the past 150 years (Biesele et al. 1986). These studies have generated models of foraging, mobility, site formation, kinship and exchange (among other cultural facets), each taken to have broad analogical relevance to our understanding of hunter-gatherer behaviour (e.g. Lee & DeVore 1976). This relevance extends up to and includes employing the Kalahari San as archetypal, mobile hunter-gatherers in many introductory anthropology texts. The data underlying these models, however, derive predominantly from just three groups: the now extinct /Xam of South Africa’s Northern Cape Province, the Ju’hoansi of the north-western Kalahari and the G/wi of central Botswana (Mitchell 2010). All three groups come from what are now relatively resource-marginal, arid and semi-arid areas (Figure 1). Measured on a range of variables—meat consumption, mobility, plant food consumption, use of aquatic resources and so on—they are far from archetypal hunter-gatherers (Kelly 2013). More fundamentally, their location, behaviours and identities are the product of centuries of interaction and integration in complex, shifting socio-political landscapes, the influences of which were active in the relatively recent past and remain so today (Solway & Lee 1990).

Notions of a mutable and evolving San identity propelled the so-called ‘Kalahari debates’, which questioned the ‘pristineness’ of populations depicted in the classic Kalahari ethnographies of the mid twentieth century, and their usefulness as analogues for prehistoric populations (see Denbow 1984; Schrire 1984; Solway & Lee 1990; Wilmsen & Denbow 1990; Gordon 1992). Yet conceptualisations of ethnographically documented San groups as, in some sense, holotypes of a deeper, southern African hunter-gatherer identity continue to affect how archaeological, genetic and linguistic research is carried out (e.g. Kim et al. 2014: 6).

That multiple analysts put ‘San’ and ‘Khoisan’ peoples close to the genetic root of the Homo sapiens family tree may itself encourage a view of them as ‘primordial’. Proponents of primordialism see ethnic identity as fixed and persistent over long stretches of time (see Geertz 1963; Isaaacs 1974). In archaeology, primordialism is conceptually aligned with culture-history, which assumes that “bounded, homogeneous cultural entities correlate with particular peoples, ethnic groups, tribes, and/or races” (Jones 1997: 24). Beginning in
the 1960s, primordialism and traditional cultural-historical archaeology suffered a series of devastating critiques centred on their inability to explain the permeability of ethnic boundaries, the historical and situational variability of individual and group identity, and processes of cultural change. In this vein, claims that a cultural pattern called ‘San’ can be traced back into the Pleistocene ignore the effects of servitude, assimilation, political landscape change, genocide and interbreeding on the demography and political economy of click-speaking groups, as well as the fact that the ‘San’ fall into three distinct language families (Güldemann 2008). Add in the impacts of successive shifts in climate and ecology on human demography, and tracing cultural identities back into the Pleistocene becomes a theoretically flawed exercise.

**Border Cave and the origin of ‘San’ material culture**

Unit 1WA (>40 kya) at Border Cave has been claimed to show the origin of San material culture and the LSA in southern Africa (d’Errico et al. 2012). Recent analyses demonstrate the presence, by c. 44 kya, of poisons, bone and tusk implements, shell beads, wooden digging sticks and ground stone artefacts said to be similar in form to those used by Kalahari San groups. These finds have revived notions of a late Pleistocene ancestry for the ‘San’. Here we employ a formal approach (see Van Reybrouck 2012) to examine the direct historical analogy between the evidence from Border Cave and the ‘San’.

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Table 1. Select examples of Border Cave ‘San’ material culture found beyond the Kalahari.

<table>
<thead>
<tr>
<th>Trait</th>
<th>Presence beyond the Kalahari</th>
<th>Reference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Notched bone</td>
<td>Domuztepe, Turkey (Neolithic)</td>
<td>Carter &amp; Campbell 2000</td>
</tr>
<tr>
<td>Bone awls</td>
<td>Yámana, Argentina</td>
<td>Borrero &amp; Borella 2010</td>
</tr>
<tr>
<td>Bone points</td>
<td>Netsilik Inuit, Canada</td>
<td>Balicki 1970</td>
</tr>
<tr>
<td>Use of poison</td>
<td>Mbuti, Congo-Kinshasa</td>
<td>Ichikawa 1983</td>
</tr>
<tr>
<td>Digging sticks</td>
<td>Yiwara, Australia</td>
<td>Gould 1969</td>
</tr>
<tr>
<td>Digging stick weights</td>
<td>Chumash, USA</td>
<td>Sutton 2014</td>
</tr>
</tbody>
</table>

In discussing the relevance of the Border Cave materials, d’Errico *et al.* (2012) chose only a single ethnographic example for comparison: the ‘San’. When this comparative net is cast wider, as shown in Table 1, it is clear that the San are not the only groups to employ such items. Bone awls, bone points, digging sticks and digging stick weights are all found in ethnographic contexts wholly unrelated to the San. *Flueggea virosa*, the wood species probably used to make the Border Cave digging stick, is widely employed across southern, western and eastern Africa (d’Errico *et al.* 2012), implying a general, rather than culturally specific, application for this material. Digging stick weights are far from universal among ethnographically attested San and go unmentioned in Kalahari ethnography, although they were used by nineteenth-century /Xam (Ouzman 1997). If indeed these objects are so inseparable from San identity and social organisation as to signal their presence at Border Cave, then their absence from certain San groups, and presence in non-San groups, needs to be explained.

The choice of source material in the Border Cave analogy is also questionable. Which San groups are being referenced: the desert-dwelling Ju/'hoansi, riverine fisher-herders such as the Deti, or the now goat-keeping G/wi? Each of these groups is the product of different historical contingencies far removed from the mountainous contexts of south-east southern Africa where Border Cave is located (Figure 1). These contingencies are manifest in variable worldviews and economies (Barnard 1992) that represent anything but an “unambiguous parallel” (d’Errico *et al.* 2012: 13218) with the artefacts at Border Cave.

D’Errico *et al.* (2012) claim that the bone points and other items of material culture found at Border Cave are similar enough to those found amongst San communities to suggest a direct cultural link. To explore the issue of similarity between the Border Cave materials and various source data more systematically, we conducted statistical tests on the Border Cave bone point morphological data presented in SI Table 2 of d’Errico *et al.* (2012) (Table 2). We used their comparative data on width and thickness for San bone points in the Fourie ethnographic collection from Namibia (n = 50), and bone points found at the South African Iron Age complex of Mapungubwe (n = 25). For our comparisons, we employed Kruskal-Wallis tests, a form of non-parametric ANOVA. Our results (Table 2) demonstrate that the Border Cave bone points are statistically indistinguishable from those in the Fourie collection, but that they are also indistinguishable from those made at Mapungubwe (non-San). D’Errico *et al.* (2012) left this similarity between the Border Cave and non-‘San’ bone points undiscussed.
Table 2. Statistical comparisons of the d’Errico et al. (2012) bone point morphological data.

<table>
<thead>
<tr>
<th>Measurement</th>
<th>Comparison</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Width at 5mm</td>
<td>Border Cave-Mapungubwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
<tr>
<td>Thickness at 5mm</td>
<td>Border Cave-Mapungubwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
<tr>
<td>Width at 10mm</td>
<td>Border Cave-Mapungubwe</td>
<td>&gt;0.01</td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
<tr>
<td>Thickness at 10mm</td>
<td>Border Cave-Mapungubwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
<tr>
<td>Width at 30mm</td>
<td>Border Cave-Mapungubwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
<tr>
<td>Thickness at 30mm</td>
<td>Border Cave-Mapungubwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Border Cave-San</td>
<td></td>
</tr>
</tbody>
</table>

Table 3. Putative ‘San’ traits and their presence in southern Africa’s major late Pleistocene technocomplexes. 0: absent, 0.5: rare; 1: common. Data from Mitchell (2002) and Lombard et al. (2012).

<table>
<thead>
<tr>
<th>Still Bay c. 77–70 kya</th>
<th>Howiesons Poort c. 66–58 kya</th>
<th>Post-Howiesons Poort c. 58–45 kya</th>
<th>Early LSA c. 44–22 kya</th>
<th>Robberg c. 22–12 kya</th>
<th>Oakhurst c. 12–7 kya</th>
<th>Wilton c. 8–4 kya</th>
</tr>
</thead>
<tbody>
<tr>
<td>Confirmed poisons</td>
<td>0</td>
<td>0</td>
<td>0.5</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Bone ornaments</td>
<td>1</td>
<td>0</td>
<td>0.5</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Wooden digging sticks</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0.5</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Bone points/ awls</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0.5</td>
<td>0.5</td>
<td>1</td>
</tr>
<tr>
<td>Bored stones</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0.5</td>
<td>0</td>
<td>0.5</td>
</tr>
<tr>
<td>Marine shell beads</td>
<td>1</td>
<td>1</td>
<td>0</td>
<td>0.5</td>
<td>0.5</td>
<td>0.5</td>
</tr>
<tr>
<td>Ostrich eggshell ornaments</td>
<td>1</td>
<td>1</td>
<td>0.5</td>
<td>0.5</td>
<td>0.5</td>
<td>1</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>3</strong></td>
<td><strong>3</strong></td>
<td><strong>1</strong></td>
<td><strong>3</strong></td>
<td><strong>1.5</strong></td>
<td><strong>4</strong></td>
</tr>
</tbody>
</table>

Editor’s note: please see comments on this table in d’Errico et al., and in the present authors’ final reply, both of which are in this issue.

D’Errico et al. (2012) focus only on how the poisons, bone implements, ostrich eggshell beads and ground stone artefacts are similar to those found at Border Cave, not on the ways in which these items might differ or be discontinuous over time. For example, the Nassarius kraussianus beads found there have no referent in Ju/’hoan ethnography (Lee & DeVore 1976). Bone implements are also exceedingly rare for the 30,000 years following Unit 1WA at Border Cave, as are bored stones (Mitchell 2002).

Table 3 shows the occurrence of the Border Cave material claimed to be ‘San’ compared to the major late Pleistocene Stone Age techno-complexes of southern Africa. These data © Antiquity Publications Ltd, 2016
show that six of the seven techno-complexes exhibit very little by way of material culture patterning that can be referred to as ‘San’-like by Border Cave standards. The only period containing these items consistently is the Wilton (c. 8–2 kya). Thus, in the case of the Border Cave finds, there is currently very little evidence of continuity in this package of material culture across the approximately 40,000 years plus implied by d’Errico et al. (2012). Moreover, as most of these organic items have preservation biases, they should not be taken to indicate the presence/absence of the ‘San’ in the Pleistocene archaeological record (cf. Henshilwood & Marean 2006).

But even if we are to assume that a level of uniformitarianism existed in the Border Cave material, it need imply nothing more than strong, sustained and stabilising selective pressure on those artefact designs, not on ‘San culture’ as a whole. All late Pleistocene southern Africans were *Homo sapiens* with human cognitive abilities (Lombard & Parsons 2011). Facing a need for bone tools, glues and projectile weapons of one kind or another, they surveyed their environment, devised a range of effective solutions and stuck with them until those needs changed.

**Conclusion**

The fundamental problem with trying to identify a ‘San culture’ in the Pleistocene is that this taxonomic unit holds little internal or external validity. Scientists generate taxonomic units to explore patterns in data. Named ethnographic cultures are abstract taxonomic concepts created by anthropologists and historians in reference to fluid human identities (Bayart et al. 2005). There is a difference between cultures as anthropological taxa and the cultural traits that they comprise. Taxa are stable and finite (albeit variably well defined). Cultural traits, such as ideas, languages and ways of doing things, are fluid, being transmitted with varying degrees of fidelity, altered, combined and recombined in multiple different configurations over long time-spans. Genes behave in a similar way. At any given time slice in this fluid continuum—including the ‘ethnographic present’—it is possible to identify a given cultural group in terms of its particular combination of traits, and to assign it a name. Yet if we trace a descendant population through multiple time slices, its cultural traits will be variously inherited, blended, lost or reinvented depending on selective pressures mediated by historical contingency (McGranaghan 2014). The characteristics of these cultures are not immutable. These factors make the use of direct historical analogy (or homology) in the search for specific named ethnographic cultures in the deep recesses of the archaeological record a fundamentally flawed endeavour.

Finally, why should anthropologists working outside southern Africa care about this issue? They should care because knowledge claims based on science have power that those derived from other sources do not. The concept that particular human cultures can be traced into deep time, and have thereby remained essentially unchanged over vast periods and across evolutionary timescales, is one such idea. Unchallenged, politicians and other demagogues have used, and will use, pseudo-scientific claims—about one culture’s purity and lost glories, and another’s inability to change with the times—to justify war, economic injustice, development agendas and even genocide (Kuper 1988). Just as a former generation of anthropologists spoke out forthrightly against fixed and invariable ‘racial’ taxonomies as...
an organising principle for society (Montagu 1945), anthropologists today also need to speak out when we see claims about fixed and invariable human ‘cultures’ used for similar purposes. We must not forget that it is not that long since links between some San groups and their ‘Stone Age culture’ were invoked as a pretext for resettling them away from those fragments of land that they still retained (Survival 2010).

References


The ‘to be or not to be’ of archaeological enquiry

Francesco d’Errico1,2, Paola Villa1,3, Ilaria Degano4, Jeanette Lucejko4, Maria Perla Colombini4 & Peter Beaumont5

Pargeter and colleagues do not escape the dangers inherent in the exercise they embark on. The first is that of creating a straw man argument in which one exaggerates and misinterprets what was said in the article being criticised. The second is that of using your time to look

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Francesco d’Errico et al.

at the speck of dust in your brother’s eye instead of paying attention to the plank in your own. The third, if you are lucky enough to find a sympathetic journal, is to rehash the same criticism over and over in multiple articles, changing the tone from very moderate (Mitchell 2012) to more aggressive (Pargeter 2014), which inevitably pushes your opponents and any sensible reader to wonder about your motivations.

Pargeter et al.’s attempt to make us appear as the followers of Kossinna “ein Volk, ein Topf” is ridiculous. Let us, to start, make it short and clear. Did we ever claim in our articles (d’Errico et al. 2012; Villa et al. 2012) that our results indicated that the people living at Border Cave 44 kya were San, ancestors of the San, speaking a San language or in any way biologically related to the San? No. Did we write that our results supported cultural continuity—meaning a persistence of beliefs, morals, laws, rituals, customs, social organisation and language—between the Border Cave inhabitants and present-day or historically known San? No.

In our articles, we directly date, reassess the stratigraphic provenance and analyse with a number of advanced analytical techniques never previously applied in combination to a site of this period—an input that Pargeter et al. try their best to hide—the lithic assemblages and a number of organic artefacts recovered from Border Cave. We retrace in detail changes in lithic technology occurring at the site between 60 kya and 40 kya, and observe that, although not identical, a number of items dated to or occurring in the 44–40 kya layers, and one dated to 24 kya, resemble those found at Later Stone Age (LSA) sites known to be used by historical or contemporary San hunter-gatherers of the Kalahari. We relied upon this similarity to reach the conclusion that a cultural adaptation—meaning a set of technological innovations and specific artefacts types, including symbolic items—similar in a number of respects to those known at LSA sites and among San populations is observed at Border Cave c. 44 kya. It is in this sense, and in this sense only, that we proposed a degree of continuity with more recent hunter-gatherer adaptations.

With that established, we can move on to other claims that Pargeter et al. make. Did we raise the possibility that this novel cultural adaptation may reflect the emergence of more evanescent cultural traits, on which archaeology notoriously has little grasp, characteristic of historically known San? Yes. Did we claim to have demonstrated that such a link exists? No. Do we consider this to be a matter of archaeological enquiry? Yes. In fact, the field of genetic research has successfully accumulated data on this issue for the last 30 years, and what do we know about the possible resilience of cultural traits in hunter-gatherer populations living in southern Africa between 40 kya and the present? Virtually nothing. Is it the destiny of archaeology to remain mute on this topic, or do we have the means to sharpen our heuristic and analytical tools, ‘take arms against a sea of troubles, and, by opposing, end them’? Pargeter and colleagues are free to think that it is ‘nobler in the mind to suffer the slings and arrows of outrageous fortune’—in other words, to be unequivocally against the idea that such an endeavour should be embraced.

The ways in which Pargeter et al. attempt to show that our approach is, however, not feasible or that our inferences can be rejected are inadequate. A six-entry table (Pargeter et al. above: tab. 1) indicating that ‘bone points’, ‘bone awls’, ‘use of poison’ and ‘notched bone’ are used elsewhere on the planet by traditional societies cannot be considered a serious means of demonstrating or disproving anything. These categories are too broad to carry

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any significance in this debate. From this table, we know nothing about the technology, appearance, size or function of the artefacts, and the cultural contexts in which each of them occur apparently do not feature the others, rendering such examples even more irrelevant here. Are the ‘bone points’ of the Netsilik Inuit hafted and used as spear points or as arrow tips, and, if the latter, with what kind of bow? Do the tips of their points display tiny incisions similar to those that we have identified at Border Cave, reminiscent of the marks of ownership made by San on their bone arrow points? How do the Mbuti from Congo use poison? Do they apply it with poison applicators consisting of thin, notched wooden rods similar to that found at Border Cave, which still retains traces of poison and is virtually identical to those used historically and at present by San hunters? Do the digging sticks used by the Yiwarea in Australia have a bevelled tip similar in size and morphology to that found at Border Cave and at LSA sites, and, as with the Border Cave example, a diameter falling in the range of those found at LSA sites and in San ethnographic collections? Clearly, the degree of refinement that we have applied in our analysis of material culture from Border Cave and other Middle Stone Age (MSA) sites in the last 15 years is not one that Pargeter et al.

fail to understand that similarities between Border Cave artefacts and LSA or more recent subsistence strategies need to be measured in the light of differences with previous MSA cultures. Of the ten novel cultural features appearing at Border Cave in early LSA layers (ostrich eggshell beads, small bone points, digging stick, bored stones, beeswax mixed with resin, tar made from yellow wood used for hafting, poison, a poison applicator, notched bone objects and Nassarius shell beads) only two are known in the MSA, and for one of them (Nassarius shell beads) we were able to show that the way in which they were strung does not conform with that observed at MSA sites (Vanhaeren et al. 2013).

Pargeter et al.’s Table 3 is nonsensical in that it grossly underestimates the potential role of taphonomic processes and excavation techniques on the occurrence of the artefact types they list in the table. Organic material is exceptionally well preserved at Border Cave for reasons linked to the special geological setting of this cave (Beaumont 1978; d’Errico et al. 2012). Most of the items discussed here are very small in size. How many were recovered at LSA excavations conducted without sieving or with 5mm sieves? A number of these items are made of perishable and fragile material that disintegrates during excavation if time-consuming precautions are not taken. It is very telling that no bone tools, engraved ochre or shell beads were found at Still Bay sites; no engraved ostrich egg shells at Howiesons Poort (HP) sites; and no bone tools at HP and post-HP sites before the careful excavations conducted at Blombos, Diepkloof and Sibudu caves; and this in spite of dozens of sites

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of these periods having been excavated previously. In addition, we notice that Table 3 contains a number of factual errors. Contrary to what Pargeter and colleagues put in their database, no bone ornaments and no ostrich eggshell ornaments are known at Still Bay sites. These sites have instead yielded bone points and bone awls, which are listed as absent in their Table 3. Also, no ostrich eggshell ornaments have been found, to the best of our knowledge, in HP layers, and no bone ornaments are signalled at post-HP sites. First looking at the plank in your own eye is advisable before criticising someone else’s work.

There are ways to move on in this debate constructively. The first is to document San material culture better from an ethnoarchaeological perspective. The San populations have been widely documented ethnographically, but with the exception of few amendable studies, little has been done to produce data describing their technology with the degree of precision that can be used by archaeologists when analysing past material culture. We are carrying out this work (see, for example, Wadley et al. 2015) with the help of San elders and the analysis of unpublished museum collections. Second, Border Cave and other early LSA sites have not revealed all of their secrets. The ongoing reappraisal of some material found at Border Cave (d’Errico & Backwell 2016) and the new excavations recently commenced at this site may provide better insights into the processes that led to the emergence of the cultural innovations discussed here and their eventual maintenance in more recent populations.

References


Nothing wrong with reasoned speculation

Alan Barnard

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the contexts of change and variation in which they were embedded, in that way deterring from those contexts, and deflecting from the real problems of the present and its pasts.

References


Nothing wrong with reasoned speculation

Alan Barnard*

In essence, Pargeter *et al.* argue that claims for the antiquity of modern San ‘cultures’ involve a misuse of analogical reasoning. In general, I agree. But, let me take issue with a few of the specifics they discuss, and argue instead that things may be more complicated than they seem.

It is perfectly true that I once claimed a greater diversity for San peoples than had previously been recognised (Barnard 1992). This is especially true for aspects of kinship and for settlement patterns. For other things, however, particularly religious beliefs and practices, it is less true (see Barnard 1988). From one end of southern Africa to the other, we do find diversity, although we also find great similarities, even in the vocabulary employed to discuss such things: words for ‘God’ and for ritual activities, for example. How old these are, we of course do not know, but the possibility exists that they are very ancient and even part of an *Urkultur* such as that which seems to exist both in High God concepts and in mythological systems. The latter may even exist across the continents, which means, therefore, through long periods of time (see Witzel 2012).

I have no idea how ancient Border Cave cultural practices may be. The authors cite Güldemann (2008), however, on the fact that the ‘San’ occupy three different language families. What they do not say is that he has characterised the Kalahari Basin as a whole as a *Sprachbund*, a collectivity of languages that have through time merged together, rather than having split from a common source as with Indo-European. This makes every bit of sense to me: language families can only be traced 7000 or 8000 years back, but this is

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not to say that there may not be a degree of continuity over a longer period. Languages diverge and converge over very long periods, and it is quite normal for hunter-gatherers to speak many languages. During my last visit to the Kalahari in 2011, I met a N!aqriaxe man who could speak languages in five different language families (the three ‘San’ ones and two others). Some of these have grammars that are among the most complex in the world. Naro (my primary fieldwork language) has 86 person-gender-number markers. /Xam has at least six verbal suffixes, 24 verbal prefixes and some 14 ways to make a plural.

How do we know the linguistic history of the subcontinent? What is to say that the situation 40 000 or 50 000 years ago was significantly different? In Language in prehistory (Barnard 2016), I discuss the probability that every Pleistocene hunter-gatherer spoke many languages, and that these languages existed for a purpose, namely the transmission of ideas through myth: myths spread far and wide and across language-family boundaries. Through genetic and linguistic evidence we know that the Naro probably spoke a Ju/'hoan-like language before they acquired a Khoe one (Pickrell et al. 2012). This is also borne out by changes in Naro kinship structure, and of course the 15 000 Naro alive today were not the same ‘people’ as then. Those who eventually became ‘the Naro’ were, a few hundred or thousand years ago, probably a very small, multilingual population, numbering perhaps only in the hundreds.

If we put all this together, who is to say exactly what the prehistory of Border Cave may have been. At times, I prefer reasoned speculation over either direct historical analogy or relational analogy. I leave it to archaeologists to explore this idea further.

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The analogy generation game

H. Martin Wobst

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The analogy generation game

H. Martin Wobst*

I have grown increasingly allergic to arguments about analogies and origins in archaeology. Analogies are simply devices to stimulate the invention of ideas. The source of intuition is fairly irrelevant for the power of the ideas thus generated. The history of science is full of exciting hypotheses that had amusing sources, often far removed from the actual contexts to which the idea ultimately applied.

Archaeologists have tended to generate analogies in a kind of a typo-numerical exercise: you start with the elements/traits that you are archaeologically faced with and have questions about. You then scan the present, the ‘ethnographic present’, or other pasts, for contexts that you assume to be more complete than your starting point. That context becomes your analogy generator that offers the most trait/element congruences with your archaeological case. The more congruences, and the closer in time and space that case is to your archaeological context, the more easily the hypotheses that it generates will convince you. This method of hypothesis generation is exactly that—a method of hypothesis generation—and the hypotheses thus generated are not inherently superior to those generated any other way, with some rather amusing examples from the history of science. They now need to be as vigorously evaluated as any other idea. One should harbour the suspicion that hypotheses generated by analogy in the context of foragers might be considerably more pedestrian than those inspired by other contexts because of the many problems inherent in the ‘ethnographic present’ of foragers (after all, as with the archaeological record, it is also one where pre-contact behaviours usually were not observable any more).

Similarly, the search for origins is simply a typological dead-end. The descriptive elements of one’s present (often, the ethnographic present or a select set of its material elements) are chased into the past until all formal coincidence ultimately disappears (when a ‘point of origin’ has been located). Or a complex of descriptive elements (e.g. ‘some archaeologically preservable material elements of recent Kalahari populations’) is matched with precedent co-occurrences of the same elements until the coincidences eventually fizzle out (with ‘the culture of origin’ just overlying that point). Either procedure is processually vacuous. The resultant data structure (I have called it ‘origination cone’ in a previous publication (Wobst 1989)) is simply the result of the mindless sorting procedure. Its shape is pre-given, and its ultimate spatial and temporal referents are the result of the number of attributes one starts with, rather than any anthropologically interesting relationships or processes. Yet forever after, these points of origin will motivate our explanatory efforts and the largesse of archaeological supporters.

There are no easy processual shortcuts that relate Pleistocene and Holocene populations to each other. Searches for analogies and for origins merely extract cultural elements from

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the contexts of change and variation in which they were embedded, in that way detracting from those contexts, and deflecting from the real problems of the present and its pasts.

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In essence, Pargeter et al. argue that claims for the antiquity of modern San ‘cultures’ involve a misuse of analogical reasoning. In general, I agree. But, let me take issue with a few of the specifics they discuss, and argue instead that things may be more complicated than they seem.

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Analogy and the danger of over-simplifying the past

James Denbow

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Analogy and the danger of over-simplifying the past

James Denbow*

This is an excellent and tightly written argument against the indiscriminate and essentialist extension of invented anthropological typologies, such as ‘the San’, back into the Pleistocene. While analogical arguments that relate similarities in excavated tools, poisons and so on to the repertoire of items used by extant peoples in order to interpret their function is a common approach in archaeology, as the authors rightly point out, the extension of these analogies to include particular cultural and linguistic forms is ‘a theoretically flawed exercise’.

More seriously, such approaches short-circuit more detailed investigations that seek to identify and investigate differences between prehistoric assemblages that would point to greater diversity in hunter-gatherer adaptations than were recorded in the few detailed ethnographies we have of living peoples. No detailed ethnographic or ecological studies, for example, have ever been carried out amongst the contemporary Khoe who neighbour the Ju/'hoan and hunt, gather, fish and tend small stock along the waterways of the Okavango and Boteti, as well as the vast plains surrounding the salt pans of the Makgadikgadi. Such studies would certainly expand our understanding of the historicity and diversity of hunter-gatherer adaptations in southern Africa. They would also problematise overly simplistic relationships between simple artefact types, such as bone points and beads, and a static and essentialist social identity that presupposes cultures living in a time-warp outside the ordinary flow of history (Wolf 1982; Fabian 1983). Sadly, the fact that corrective arguments such as those made by Pargeter et al. still (rightly) need to be made, despite the issues raised in the Kalahari Debate over 25 years ago (Denbow 1984; Wilmsen & Denbow 1990), suggests that the film Groundhog Day reflects a temporal reality.

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‘Primordialism and the ‘Pleistocene San’ of southern Africa’: final reply

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We thank our colleagues for their insightful comments. The weight of modern evidence is against the notion that contemporary human cultures can be tracked backwards into the Pleistocene (e.g. Lee & DeVore 1976; Kuper 1988; Wilmsen 1989; Solway & Lee 1990; MacEachern 2000). Modern-day hunter-gatherers are not our Stone Age ancestors. Current protestations notwithstanding, the provocative title that d’Errico and colleagues (2012) chose for their paper, ‘Early evidence of San material culture represented by organic artifacts from Border Cave, South Africa’, unambiguously asserts the opposite. Our critique of that paper’s content does not question the robusticity of the methods employed at Border Cave (for this, see Evans 2012). Rather, our comments focus on the theoretically flawed search for a specifically ‘San’ “cultural adaptation” (d’Errico et al. 2012: 13214) at any Pleistocene archaeological site.

Historically, southern African archaeologists have used ‘San’ ethnography to frame Later Stone Age archaeology (e.g. Goodwin & van Riet Lowe 1929), but this practice has more recently been roundly criticised (e.g. Parkington 1989; Mitchell 2003; Humphreys 2005; Sealy 2006). The societies lumped together as ‘San’ reflect millennia of interaction, contact and acculturation, resulting in their diverse material cultures and worldviews. Barnard’s (above) encounter with a Kalahari man who spoke five distinct languages highlights the degree to which the term ‘San’ oversimplifies contemporary cultural variation. As Denbow notes above, extracting so-called ‘San’ cultural elements from the “contexts of change and variation in which they were embedded” greatly reduces their variability and complexity. In ignoring this variability/complexity, we allow ethnography to dictate higher-order theory in archaeology. Our alleged ‘re-hashing’ of this point in several venues is a measure of a problem that d’Errico and colleagues dismiss.
D’Errico et al.’s (2012) choice of examples for “early evidence of San material culture” at Border Cave, and their failure to comprehend our Table 1, highlights the variability and complexity that has been overlooked. For example, they lump the Border Cave bone ‘tools’ into morpho-functional categories whose integrity remains untested. Do we know that the pointed objects at Border Cave were used as weapon tips rather than as needles, awls or any of the other myriad functions to which a pointed bone object might have been used? Plainly, we do not. The fact that both the Netsilik Inuit and some ‘San’ groups used bone tools does not prove relatedness, but it does point to broader patterning in this class of object. We partially agree with d’Errico and colleagues that such classes are “too broad” to be a “serious means of demonstrating or disproving anything”. Consistency in coarse classes does not prove continuity. Where coarse classes are absent from large numbers of assemblages, we cannot simply draw a dotted line through time and choose to believe that they are masked by taphonomy or recovery techniques.

Discoveries of bone tools or ostrich-eggshell ornaments at sites beyond southern Africa (e.g. Kabwe, Mumba and Magubike) suggest patterning outside the regions typically associated with ‘San’ ethnography (see Wadley 2015 and references therein). Their discovery at sites pre-dating the Later Stone Age (e.g. Sibudu Cave and Klasies River Mouth), at sites excavated before the “careful excavations conducted at Blombos, Diepkloof and Sibudu caves” (d’Errico et al. above) (including Boomplaas, Cave of Hearths and Bushman Rockshelter), and their absence from recently excavated sites with excellent preservation (e.g. Klein et al. 2004) surely attests to the fact that their absence in various parts of the southern African archaeological record is more real than apparent. The fickle nature of these remains is further accentuated upon removing the Still Bay ostrich-eggshell ornaments from our Table 3 (their original inclusion in our manuscript was an error, and one inconsequential to our argument; we thank d’Errico and colleagues for noting this error, although they were unwilling for it to be corrected in our paper as published). We observe, however, the presence of notched bone in both the Still Bay and post-Howiesons Poort techno-complexes at Apollo 11 and Sibudu Cave (Cain 2004; Vogelsang et al. 2010), the purpose of which is currently unknown. This pattern cannot simply be ascribed to poor organic preservation, inadequate recovery techniques or the size of excavations. As Lombard and Parsons (2011) note, variation of this kind in the late Pleistocene of southern Africa now forms one of the most exciting topics in the region’s hunter-gatherer prehistory.

D’Errico and colleagues (above) rhetorically ask, “What do we know about the possible resilience of cultural traits in hunter-gatherer populations living in southern Africa between 40 kya and the present?”; they then answer, “Virtually nothing”. Actually, we know quite a bit (e.g. Deacon 1984; Parkington 1984; Mitchell 1988; Wadley 1993; Sealy 2006). We do not know as much as we might wish, but we know enough to reject the argument that ‘San culture’ remained recognisably the same for 40 millennia.

Finally, it is encouraging to note that d’Errico and colleagues have begun more systematic ethnoarchaeological research among Kalahari populations, and that Border Cave is being reinvestigated. These more behavioural, ecological and comparative approaches, grounded in sounder understandings of both ethnographic and archaeological contexts, are surely better ways to move forward (e.g. Shea 2011). We join Wobst (above) in hoping that this work leads to the investigation of more “anthropologically interesting relationships or processes”
than can be attained from a misplaced search for ‘origins’ or primordial ‘Pleistocene San’ cultures.

References


