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ARCHAIC MODERNITY VS THE HIGH PRIESTHOOD: ON THE NATURE OF UNSTABLE ARCHAEOLOGICAL/ PALAEOANTHROPOLOGICAL ORTHODOXIES

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Abstract. In this article, the author presents a dialogue regarding various aspects of 'modernity' as it is conventionally expressed in the archaeological and palaeoanthropological literature. The structure of the Africanist model of 'behavioural modernity' is used to illustrate some very troubling aspects of American academics: the construction and use of temporary 'unstable orthodoxies' as means of economic niche construction by the same professionals who also function to police much of archaeological and palaeoanthropological publishing, hiring and grant-reviewing.

Establishing the context of a problem

Before I begin asking questions and opining, let me just state this: I could easily be in part or even completely wrong. The entirety of the essay I have herein assembled is based upon a subjective analysis of the contemporary (as of about late June 2013) archaeological state of past human affairs. New discoveries beyond my grasp could refute part or all of what I will write; in fact, I hope there are both new discoveries and that they contradict at least some of my conclusions so that I can learn from them. I am imperfect and I can only claim knowledge of the relevant literature inasmuch as I have consumed it and as is available, or at least as is published (with the assumption that many pertinent aspects of what I am about to describe are perhaps undiscovered or unpublished for a variety of reasons). I am a product of the establishment 'school' of Americanist archaeology and palaeoanthropology. So stipulated, and given what I have at my disposal, I attempt to answer some questions I have had for some years.

What really is there to 'behavioural modernity'? If we accept and postulate that the suite of materials, behaviours, processes and other congregate phenomena that we archaeologists and palaeoanthropologists singularly lump under the multiple epithet 'behavioural modernity' (BM) actually existed in the human past even roughly as it has been described in the archaeological literature, we need to ask how was it lived as experiential reality (Thompson 2011). Was it perceived? Is it succinctly definable even today? Did it really happen *only* in South Africa to the exclusion of all other places as some loudly and frequently opine (Henshilwood and Marean 2003; Tattersall 1995)? We

should investigate this.

There are four basic issues pertaining to a discussion of behavioural modernity, three of which relate to objective categories of periodicity, locality and identity; the fourth issue involves the actual archaeological material that is appraised to be 'behaviourally modern', on which the previous three are themselves based (Thompson 2011). A fundamental question of modernity's periodicity would be simply, when did it first occur and how long did its developmental process last? The two primary models in the literature contrast with respect to this timing, with one seeing the process as relatively late Pleistocene and sudden in nature (Klein 1994, 1995, 2001; Mellars 1995, 1996, 1999), and the other as a more continuous, synthetic process beginning in the Middle Pleistocene and lasting through the Eurasian Upper Palaeolithic and African Middle and Late Stone Age (Henshilwood and Marean 2003; McBrearty and Brooks 2000; McCall 2006; Thompson 2011).

The current 'state of the state' regarding Old World palaeoanthropology and Palaeolithic archaeology could be quite accurately described as a *détente* between two basic orthodoxies that explain some, but not all, of the evidence in the archaeological and fossil records. Previous research and advanced training seem to indicate the theories and methods whereby individuals choose a side in the divided camps (Thompson 2011). An influential synthetic and quasi-diffusionary model of human modernity obtained after Cann et al. (1987) published a pivotal paper, describing a hypothetical Middle Pleistocene genetic bottleneck event that truncated mtDNA (what about nuclear DNA?) diversity amongst ancient human populations. According

to this interpretation, the population bottleneck eliminated most mtDNA (nuclear DNA?) lineages from the Pleistocene human gene pool, such that subsequent anatomically modern populations featured a low diversity of mtDNA that originated from a single anatomically modern African woman approximately 200 ka ago. This 'recent out of Africa' model (hereinafter ROA) also implies that 'modern humans' diverged evolutionarily from all earlier possible common ancestors perhaps as long ago as 400 to 800 ka, based on a subjective reading of a figurative molecular mitochondrial DNA clock (does nuclear DNA tell the same tale?), with 'anatomically modern humans' (AMH) emigrating to all portions of the planet and replacing, outcompeting, killing or otherwise negatively impacting regional anatomically archaic humans (Cann et al. 1987; Stringer and Andrews 1988; Tattersall 1995; Vigilant et al. 1991). Regardless of what the fossil or archaeological record indicates, for many the selective mtDNA genetic data indicate that a wave of advancing anatomical modernity from Africa swamped the indigenous human populations of Eurasia (some would even question my label of 'human' for earlier, 'archaic' populations).

In opposition to the ROA model would be the 'multiregional model' (hereinafter MRM) (Wolfpoff and Caspari 1996, 1997). According to this orthodoxy, the retention in various regional *Homo* populations of a mosaic suite of purely anatomical symplesiomorphies (general cranial robusticity, lack of mental eminence, supraorbital morphology, sexual dimorphism) indicates long periods of continual gene flow between regional populations of conspecific humans spread across Africa and Eurasia (Hawks and Wolfpoff 2003). The implication is that gene flow would perhaps also be implicative of cultural/behavioural interaction. It would appear that one recent discovery in particular, that of the Siberian Denisova hominins (Reich et al. 2010), substantially buttresses the MRM, with the demonstration that as much as 4% to 6% of the genome of contemporary Melanesians is shared with this isolated Siberian population. How did that combination occur and how were archaic traces retained in the modern human genome, if not by conspecific interfertility? Whereas Cann et al. (1987) and Krings et al. (1997) explicitly removed Neanderthals and other archaics from 'modern' human ancestry, troubles with timing of the 'molecular clock' (Templeton 1993), publication of the Denisova hominin find, and the demonstration that Neanderthals did in fact contribute DNA to contemporary Eurasian populations (Green et al. 2010), among other recent technical analyses (Henry et al. 2010; Peresani et al. 2011; Wu et al. 2010), have undermined allegations of a complete absence of Neanderthal contribution to the contemporary genome. This softening of positions related purely to genetic data may in fact explain some of the recent literary emphasis on 'behavioural' segregation between archaics and moderns. Left without a handy genetic buttress, the ROA enthusiasts opined ever more loudly regarding alleged

behavioural differences that cannot be measured or even sampled.

Despite having somewhat different craniofacial morphology, it is no longer possible to postulate that some of the more recent archaics were not genetically humans. Various modern non-African populations have variable quantities of Neanderthal DNA in their own bodies (Green et al. 2010). One question that looms large would be how exactly were 'archaic' and 'modern' DNA able to come together at all if they were not conspecific according to that most mammalian aspect of biological compatibility. How did that 'archaic' Neanderthal DNA get inside 'modern' humans if the two taxa were reproductively isolated? As adults, we know how the DNA got there. And what do we call groups of organisms that are capable of interbreeding successfully through successive generations? We call those entities species. How widely was behavioural modernity distributed in past African populations? Were there perhaps some Neanderthals who behaved modernly and likewise perhaps some AMHs who lacked modern behaviour? Are there missing pieces to this genetic puzzle? If there are, can we be certain that we have isolated the right puzzle pieces? How and by what means? Who gets to decide? Are there equitable numbers of both interpretive camps represented in academia and the literature?

One may also cogently query the presumed locality of BM's emergence. Was BM confined to an initial discrete florescence only in South Africa with subsequent later diffusion to the Levant and the rest of Eurasia (Henshilwood and Marean 2003; Klein 1994, 1995, 2001; Mellars 1995, 1996, 1999), or did it unfold as a continuous expression of a more gradual process within separate geographies (Barker et al. 2007; Bar-Yosef and Kuhn 1999; Habgood and Franklin 2008; McBrearty and Brooks 2000)? Finally, in terms of identity, which hominin actors accomplished behavioural modernity? In South Africa, modernity proxies are typically associated with AMHs (McBrearty and Brooks 2000; Mellars 1999), while Eurasian Middle and Upper Palaeolithic archaeological sites often present rather ambiguous associations between hominins and material in different areas at much different times (Barker et al. 2007; Habgood and Franklin 2008; Pettitt 2007; Zilhão et al. 2006). Despite some rather bold claims in the literature (Tattersall 1995; Thompson 2008), it is neither obvious nor demonstrably conclusive that 'archaic' humans were prohibited from participation in modernity nor that BM occurred only in Africa, whether southern or not. Such claims appear to be based upon an unstable literary orthodoxy of opinion that is coming under considerable pressure of late due to recent discoveries and interpretations (Bednarik 2007, 2008; Derevianko et al. 2008; Mednikova 2011; Thompson 2011). To the theme of this unstable literary orthodoxy we shall, however, return below.

The fourth basic issue relating to behavioural modernity concerns the nature of the evidence cited for

modernity itself, namely the material of the human fossil and archaeological records and the interpretations rendered from them (Thompson 2011). The material inventory is certainly nothing 'new', of course, consisting of the same art objects, bones, stone tools, human remains and associated landscape features (i.e. 'sites') that have traditionally served as the main focus of Old World prehistorians. What are new are subjective reinterpretations of prior archaeological opinions (Jelinek 1977) combined with more recent and subjective inputs from other disciplines (selective genetics and molecular chemistry, via the African Eve hypothesis, i.e. Cann et al. 1987; Krings et al. 1997) that have resulted in the recent assembly of a 'package' (or theory and trait list), supposedly visible archaeologically and palaeontologically, that can be used to 'spot' behavioural modernity materially and genetically in different times and places (Habgood and Franklin 2008; Pettitt 2007). This trait list or 'package' is composed of things as disparate as human morphology, art, personal adornment and presumed body modifications, blade-based lithic technology, bone, wood and other organic technology, socio-demographic elaboration and population growth and economic intensification (Habgood and Franklin 2008; McBrearty and Brooks 2000). A very key point is that early Upper Palaeolithic subsistence patterns and faunal treatment differed little if at all from the preceding Middle Palaeolithic (Enloe 1993).

How important is lithic technology to this behavioural morass? In two regions with secure associations between hominins and archaeological materials, South Africa and Australia, we find completely different chronologies, patterns of time-successive adaptation and technological change. For the South African MSA, McBrearty and Brooks (2000: 530) note the following diachronic technological trajectory: blades, grindstones, pigments and lithic points by 250 ka; use of aquatic resources, long-distance exchange, mining and bone tools by 100 ka; microliths and presumed composite tools by 75 ka; beads and body art, adornment by 50 ka. For Late Pleistocene Australia, Habgood and Franklin (2008: 211) note a different trajectory: pigments, grindstones and ground-stone tools by 50 ka; long-distance trade, burials, use of aquatic resources by 40 ka; art and adornment by 40 ka; bone tools present by 25 ka; lithic points and microliths among the last items to appear in the toolkit at about 5 ka. Lithic blades appear not to be present in the earliest Australian lithic assemblages (Davidson 2010), while Australian microliths are absent until the very end of the Palaeolithic (Habgood and Franklin 2008). With respect to microlith manufacture, while 'standardised' blade and microlith production (microliths produced on blade segments) are very strongly associated in South African material from Klasies River Mouth (McCall 2006), south Asian microliths appear to have been manufactured on flakes and lack geometric forms, indicating rather a divergent approach (Misra, in comments to James and

Petraglia 2005: S21).

The Australian archaeological example suggests that some fairly diagnostic elements of the modernity package from Africa were lost en route or were perhaps never developed, within a very distinct regional variant from south Asia and Australia that essentially inverted the MSA order and trajectory of technological development. It is by no means clear then that African modernity = south Asian or Australian modernity. In Australia, blades and microliths were not even present in the first iterations of the AMH technological repertoire, whereas these are treated as virtual *fossils directeurs* for the MSA, and are alleged to indicate the material panoply of other modernity proxies. This is an interesting problem. For on the one hand the modernity package is used at least partially to explain the means by which AMHs allegedly 'replaced' our outcompeted Neanderthals in Europe (i.e. possession of 'superior technology'), while for AMHs moving into a previously unoccupied Australian landmass the generally best-attested diagnostic elements of it are absent, at least during the earliest periods. Did the absence of archaic hominins in Australia somehow obviate the need for blade-based lithic technology and microliths? The relatively late development of bone technology in Australia also becomes all the more puzzling since perhaps one might expect bone or composite tools to fill the gaps left by the blades and microliths. The Australian sample therefore generates a number of stimulating questions. Did the Australian modernity package 'devolve'? Do the earliest Australian inhabitants represent an example of non-modern adaptation to influx of modern technology? Two phenomena do, however, seem to loom as potential avenues of productive research: (1) the unfamiliarity of Australian fauna to human predation, based upon (2) the absence of archaic human occupations. It could be that the complete modernity package was simply unnecessary for Australian subsistence. It might also be that the modernity package never existed as past lived human reality in the first place, and that its existence is confined to the demands of certain contemporary cultural phenomena, such as inequitable distributions of archaeological/palaeontological research funding, scholarly publication, and faculty tenuring (Thompson 2011).

The ubiquity, or lack thereof, of any of the BM 'package' phenomena in isolation or in tandem seems to depend just as much upon where and when one samples as it does upon which Upper Pleistocene hominin species is sampled. Pettitt (2007: 759) introduces a 'kaleidoscope' metaphor to convey this rather mosaic impression of modernity as a cumulative but irregular set of separately variable, interacting regional trajectories, with no one particular regional variant (including the South African Middle Stone Age) serving as 'the' central driver. Modernity appears to mean different things in different geographic areas. If entire facies of BM, such as the South African MSA, are not 'the' driver of everything subsequent, what can

we say about regional variation in aspects of proxy archaeological criteria? If BM is conceptually valid, why is there such variability in the first place if modernity was diffused *from* South Africa to everywhere else?

What, in fact, *are* the most salient or important BM criteria? Are they discrete or continuous in nature or expression? How do they interrelate? Is mitochondrial DNA the most important aspect of modernity? Why not nuclear DNA also? Is maternal inheritance more important than paternal? What about hominin craniofacial morphology? Are postcrania unimportant or just less important? Exactly how variable was human skeletal morphology during the Pleistocene? Why or why not? Who gets to decide? What about art? Have archaeologists and palaeoanthropologists adequately established what even constitutes art? Do we know what 'art' means within, between and among all variable contexts, such as within, between and among the Lower, Middle and Upper Palaeolithic? Do we understand the symbolic referents in ancient art? If not, why not? Are we certain that what has been described as art by us in the now (i.e. Blombos Cave scratched ochre nodules) was really art as we define it to the humans that made it? Which of these proxy material indices actually equate to recognisable and identifiable human cognitive causes? How?

With respect to art, for example, when in the nearly two million year human archaeological record do we actually find symbolic themes that we can not only identify *as* symbolising or art but, more importantly, *understand* in regards to content? Do we really know what the things we call cave paintings and Venus figurines *were*? Can we state that cave art is even 'symbolic' in the same sense that post-Neolithic art in the Louvre is? What were the referents? Are cave paintings mere representations of pictures taken by the Upper Palaeolithic mind's eye of things distributed about the Pleistocene landscape of, say, Lascaux? If they portray accurate population frequencies of humans in relation to animal subjects, then evidently even during the late Upper Palaeolithic human population densities were apparently extremely low, since humans are barely represented. Were there proscriptions against portraying human form via art during that time period? Are cave paintings totemic badges or insignia of clan ownership? Were they artistic appeals for spiritual intervention in subsistence (sympathetic magic hunting aids) as commonly described (Frazer 1922)? Or something else entirely we have not yet absorbed and accurately classified according to our most analogue minds (Bednarik 1992, 2005)? After decades of research *we do not know*. Since we do not yet know, how likely is it we are going about things the right way?

One can assemble a cogent argument, for example, that it is not until Mesolithic or Neolithic times in the Old World, and Late Archaic times in the New World, that we can detect artistic themes which we can actually interpret to any meaningful extent. We can readily interpret the explicit political propaganda

contained within the Narmer Palette from Egypt, the martial themes of Mycenaean art, or the graphic iconography of Çatal Hüyük, for example, or even the anthropomorphous art of the Hopewell, and the various but much later 'bird-man' motifs from Mississippian Cahokia. We can understand much regarding those and other later themes precisely because our own lifeways were significantly and directly influenced by similar activities and phenomena that resulted in similar behaviours, artefacts, mythologies etc., including many of the very same referents, both symbolic and literal. In some cases, members of contemporary societies actually maintain some degree of continuous culture history with people of the Mesolithic and Neolithic. Perhaps the Mesolithic and/or the Neolithic were the proximate biocultural change agents, the ultimate dividing lines between 'modern' and 'archaic' due to unquestionably wholesale changes in human subsistence postures and footprints, and systemic changes in demography and social organisation. When was 'modern human society' born? If archaeologists and palaeoanthropologists are going to imitate art critics and art historians, they should study the social contexts in which art occurs. If we cannot appreciably 'study' Palaeolithic culture and society except analogically and subjectively, well, then just what are we doing?

If one cannot interpret art, how does one know if something *is* art? A somewhat unconventional means of approaching ancient 'art' might be Bednarik's (1994) taphonomic approach. Perhaps the mere fact that speleological contexts are known to skew archaeological material distributions greatly ought to caution us against over-analysis of art preserved in them. Often caves preserved cave art through simple sheltering from degradational processes. Caves are also known biasing agents in archaeological accumulation, serving as 'behavioural sinks' of long-term diachronic periodicity that portray extremely long accumulations precisely due to long-term intervals of deposition (Thompson 2011). Caves accumulate everything and delete virtually nothing through the physico-chemical degradational, or 'post-depositional', taphonomic processes that occur in open contexts.

Are then lithics perhaps after all the key to modernity? Rocks are certainly durable. Given the incredibly voluminous literature already devoted to lithic studies of various aspects of BM, it seems rather unlikely that in future such analyses will really resolve the very durable debate concerning it. One reason for this is a combination of the accumulating evidence for 'archaic' manufacture of lithic implements and other items heretofore categorised as exclusively 'modern' in authorship (pertaining to manufacture exclusively by AMHs) as well as the demonstration of quasi 'archaic' mimetic errors in what can only be very late AMH (Clovis) lithic assemblages (Coolidge and Wynn 2009; d'Errico et al. 2010; Hamilton and Buchanan 2009; Bar-Yosef and Kuhn 1999; Lemorini et al. 2006; Pawlik and Thissen 2011). If Clovis hunters were AMHs, why did

they make mimetic errors? Were they really errors at all? How do we know that Clovis points are really finished products, instead of just one stage of an entire use life devoted to making bifacial projectiles purposefully intended for later use as knives? For all we know Clovis points were knapped precisely so that when (not if) they broke they could be turned into other bifacial implements. Maybe the projectile point was not the final goal. Why did such mighty moderns use mimetic lithic manufacturing methods at all? Couldn't they instruct one another purely symbolically/linguistically? Were bifacial reduction sequences actually 'efficient' relative to the demands and definitions of the people who formerly used them to survive? An entire sub-literature could be devoted, for example, to the use of contemporary capitalistic notions of 'efficiency' subjectively gleaned from archaeological material.

Some aspects of the current modernity debate involving the role of lithics betrays the same kind of analytical double-standards used to assess possible examples of Lower and Middle Palaeolithic art. How can we be certain that Upper Palaeolithic, or even MSA, beads had symbolic functions as opposed to simple decorative aesthetics? How do we 'know' Lower and Middle Palaeolithic beads are exclusively non-symbolic (Bednarik 2005)? In terms of lithics, since we know anatomical 'archaics' of the Levant made and used precisely the same kinds of Middle Palaeolithic stone implements as alleged anatomical 'moderns', should we treat those lithic assemblages the same? What social controls existed in the fabrication of 'modern' Levantine Mousterian tools as opposed to 'archaic' Levantine Mousterian ones? Were 'modern' implements from the Levantine Mousterian free of the sorts of goal-absent, function-free, purely reduction-based fabrication agencies that some (Dibble 1987; Dibble and Rolland 1990) have alleged to have guided 'archaic' Mousterian manufacturing techniques? To use a contemporary tool-fabrication analogue, would *anyone* claim that the material construction sequence or *chaîne opératoire* employed to make modern claw hammers was more important to determining their final form than was hammer *function* (Thompson 2012)? So why should we deny form-functional relationships in Lower or Middle Palaeolithic tools when we assert them for Upper Palaeolithic ones? This is never actually 'explained', whereas it is certainly confidently asserted (Dibble 1987; Marean and Assefa 2005; Mellars 1995; Tattersall 1995). We might ask when 'we' decided that by disciplinary consensus.

Furthermore, much of the debate regarding the presumptive utility or presumed 'efficiency' (we will also examine the use of such subjective qualifiers in more depth below) of Middle Palaeolithic implements (such as triangular Levallois points) is tainted by assumption. Even the extremely useful experimental study on hafted Levallois points by Sisk and Shea (2009) assumes a basic inferiority in point use on thrust implements as opposed to 'functionality' in ballistic

projectiles, with the simplistic assumption that ballistic is better, or ballistic projectile > thrust projectile. Who decided this? In which situations are ballistic projectiles actually functionally 'better' than thrust projectiles? Is this universal? Can hunting be undertaken efficiently without ballistic projectiles? Are there other means to obtain animal carcasses besides long-distance ballistic targeting or completely passive scavenging? Are ethnographic and experimental ethnoarchaeological analogies with contemporary hunter-gatherers really adequate to explain past variability? Is the BM construct in part or in whole another aspect of Wobst's (1978) tyranny of the ethnographic record? What hunting strategies were available to 'archaic' humans that may have obviated a need for purely ballistic technology? How many ways are or, perhaps more accurately, were available for hunting quadrupeds without use of convenient contemporary lithic diagnostics? Is remote-puncturing of animals the 'only' way to kill or to acquire them efficiently? If so, fishhooks and other means of non-ballistic technology seem oddly non-modern.

In the cognitive domain, some have opined on the concept of 'creativity' as a means of distinguishing between AMH and archaics (Tattersall 1995; Thompson 2008), as though AMH were intrinsically creative and archaics were not. In some publications, one can read many pages of text in vain without finding any definition of 'creativity' even if it is invoked in the same text as a constituent, demonstrable aspect of 'modernity', supposedly confined to the AMH 'species' (Thompson 2008). This is troublesome. What does 'creativity' mean, archaeologically? Is it a useful concept in application to materials accumulated in unobserved episodes over insensible intervals of time? Are Acheulian handaxes and *chapeau de gendarme* Levallois points uncreative? Do such things actually represent *unadroitness*, or *unastuteness* or *uncreativity*? How did such things originally occur except as creative innovations in relation to what preceded them? Or, if it is agreed that such novel and unprecedented items as Acheulian handaxes and *chapeau de gendarme* Levallois points actually occur archaeologically without precedent within Lower or Middle Palaeolithic contexts, should we find something else to refer to them besides 'innovative' or 'creative'? Would examples of Acheulian handaxes and *chapeau de gendarme* Levallois points made after the original 'creative' instances of their manufacture represent devolution or mere copying? We might also ask ourselves *how did they spread across the vast distances over which they occur unless by some sensible method of cultural media and interaction* (i.e. language or trade)? How many mental gymnastics can be performed to deny the humanity of their manufacturers?

This subject of 'creativity' can be particularly interesting, for our own AMH species has seemingly been extremely creative and highly opportunistic in many situations (even though Archaics are described as the opportunistic species; see Stiner 1994, for example). Our own species has, for instance, 'created'

and 'innovated' many things, such as domestication (assertion of *control* over reproduction), for example. Not only that, and not content merely to enslave other members of the mammal class of organisms, the arguably smarter, perhaps better-connected, and unquestionably meaner members of our own 'unique and symbolic' AMH species (Tattersall 1995) also managed to enslave untold numbers of other AMHs into permanently unequal economic and social roles. We often learn about plant and animal domestication (reproductive and functional enslavement) during the Neolithic, for instance, without devoting much attention to the fact that some AMHs obviously extended the paradigm of reproductive and functional 'domestication' to members of their own species, *within their own societies*. And these are descendants of peoples who the dominant anthropological orthodoxy virtually worships as exemplars of 'modernity'. This slavery motif does appear to be a unique innovation of AMH, at least among primates (although not ants, which do enslave other organic species such as aphids and fungi). Why is human slavery not a constituent aspect of 'behavioural modernity'? Is it not creative? Or innovative? Or 'modern'? Or is its absence amongst the archaeological repertoire of things attributed to modernity more a judgment of contemporary aesthetic and political preferences? What does that say about the entire concept of 'modernity'?

In other words, which things are germane either to modernity or to archaism as expressed materially in the archaeological and human palaeontological records? If modernity exists (or, rather, existed) then did archaism exist as well, beyond simply a convenient heuristic device? Do we use the same analytical rules for 'archaic' humans as used for those allegedly 'modern' ones? Could we also then at least generally define and describe a conceptual opposite to BM, perhaps a 'behavioural archaism'? There is an inherent tension between the description and identification of the behavioural modernity 'package' on the one hand (i.e. allegedly modern human behaviour and its various material correlates) through the use of its own conceptual definitions; modern = modern, as though *Res ipsa loquitur*, whereas in my opinion, *Res ipsa non loquitur*. For example, without reference to any authorities in particular, various (sometimes even isolated) technological characteristics of modernity (lithic blade technology for example) are often held to be self-evident and auto-referential proxies of modern human cognition and even other modernity characteristics (i.e. blades = art, recognisable kinship networks etc.); modern human cognition is then often invoked as a necessary and sufficient causal mechanism for the appearance of art and lithic blade technologies and everything else presumed to be in the modernity package. Just find blades and blades = modernity. What it all seems to boil down to is finding material evidence of recognisably human cognition in the archaeological record. This is the supposedly vast theoretical question underlying

the entire modernity debate: what material indices exist of recognisably identifiable human behaviour in the archaeological record? And the apex is supposed to be ...?

Conceptually, BM is also allegedly concerned with human cognition as described by many contemporary scholars. The current literature then at least partially reveals relative personal preferences amongst that scholarly population in extracting proxy material indices of cognition from the archaeological record. Stress should be laid on the 'proxy' indices — indirect in nature and circumstantial — not 'proofs', of various human cognitive activities. Given the vast recent literature devoted to the denial of even rudimentary cognitive ability among archaic humans (see, for example, the odd accusatory tone of Tattersall 1995), it seems reasonable for one to pose the question: just what things would indicate cognition in the Lower and Middle Palaeolithic? In other words, is there perhaps already evidence of cognition in the Lower and Middle Palaeolithic material inventory that has been misidentified or ignored? Can we be certain that if tangible physical evidence were produced suggestive of Lower or Middle Palaeolithic human cognition that it would be received the way evidence alleged to support the South African BM orthodoxy has been? Do the same rules apply or would some prefer to try moving definitional goalposts to try and exclude such evidence (see Mellars 2006a and 2006b)?

One can also see how our very language breaks down somewhat in relation to BM. If, as above, the incipient Australian material expressions of modernity diverge from the South African examples, then we might also question whether possible evidence for 'archaic modernity' would diverge from that cited in support of 'behavioural modernity'. This could actually be an archaic vs. modern debate. Would 'archaic modernity' be commensurate to 'modern modernity'? It is possible that archaics neither were not as primitive as they were supposed to be nor were moderns as innovative as they are alleged to have been in the literature? Accumulation of new evidence from Eurasia, for instance (Denisova, Fumane Cave Neanderthals, Chinese Neanderthal-Modern hybrid), appears to be in opposition to the unstable orthodoxy of 'South African modernity', as Gamble (in comments to Henshilwood and Marean 2003: 639) predicted earlier. Further excavations are likely to produce even more challenges to the orthodoxy. Although important, the research and material bases on which this orthodoxy was constructed does resemble a situation of creeping very far out on rather a thin epistemic and ontological branch before sawing it off behind one's own behind. Proper respect for possible future discoveries would have been prudent in, say, the mid 2000s or so. Such interpretational caveats or contrary interpretations were apparently not granted or published in such high frequencies, however (as even a cursory review of the literature reveals). The imbecilic demand to 'publish

or perish' causes such things since authors receive few rewards for publications which admit the possibility that future discoveries might overturn everything we think we know, including their own publications. But that involves economic and financial risk, as we shall see below. So I conclude this portion of the essay with yet another rhetorical interrogatory: how exactly did the unstable orthodoxy of 'behavioural modernity' arise and spread to a pervasive degree in the first instance?

Archaeological and palaeoanthropological publishing, academic employment, research funding as contemporary economic niche construction and other anti-academic capitalistic trends

I assert that in the archaeological and palaeoanthropological scholarly literature of the past forty years, perhaps even since the late 1960s, our fields have succumbed to a contemporary, highly capitalistic, version of economic niche construction. This niche construction process involves the framing of various literary and cognitive archaeological and palaeoanthropological orthodoxies, in peer-review publication norms, in faculty hiring and tenuring norms, and in research funding norms, in ways that are highly financially lucrative for certain of the same archaeologists and palaeoanthropologists that framed the orthodoxies. Others are left to play along politely, either by conducting research and publishing material in parallel to the orthodox establishment, or by doing one's level best to conduct research and publish material contrary to the establishment. Let us consider, for a moment, the Africanist behavioural modernity orthodoxy that has been recently so popular in and fruitful for the establishment.

The way the mechanism works is basically that the same people who construct explanatory orthodoxies in the first place tend simultaneously to occupy important positions that allow them also to *control* junior faculty hiring and tenuring at academic institutions, to *control* who and what gets published in the 'prestigious' scholarly journals, as well as even having *control* over which research projects get funded by reviewing grant proposals. If one occupies such positions (say, a tenured academic professorship and a position on the editorial or advisory boards of a 'prestigious' journal) one is probably finding them extremely profitable indeed. One is also very well-placed to eliminate research competition and to suppress counterfactual publications from other scholars through the peer-review process. If one also happens to discover a small piece of haematite with some scratches on it, one need only blast the message 'original humans found!' in newspapers and online magazines, and the world beats a path to one's door. Add to the preceding a mastery and shameless willingness to use various online social networking platforms, publishers and all manners of other deafening self-promotion, and the entire fields of Americanist archaeology and palaeoanthropology begin to resemble poorly managed but well-advertised

corporations. It was not supposed to be this way. Anthropologists are supposed to study corporations but not to *imitate* them. Such is the stifling corporate atmosphere of tertiary education in the U.S.A.; even humanistic academic disciplines are starting to resemble corporations. This is especially true regarding the current tendency in archaeology and palaeoanthropology towards shameless self-promotion (in the absence of scientific confirmation) of speculative endeavours.

Is this unilineal model really the best one for a profession containing highly diverse people who research diverse dead people? Given Wobst's (1978) observation that Palaeolithic subsistence most probably involved a variety of strategies without analogue among contemporary hunter-gatherers, how likely do we really suppose it is that the contemporary South African behavioural modernity/African Eve orthodoxy, just to pick one example, is really the 'best' explanatory device? Would we know? Are diverse viewpoints really encouraged in the fields under discussion or are they ruthlessly policed from the disciplines and literature? Much of the orthodox African behavioural modernity model is premised upon contemporary hunter-gatherer ethnography and experimental ethnoarchaeology — in other words, upon the use of analogues with known groups of contemporary hunter-gatherers or fictive past hunter-gatherers living in the minds of contemporary archaeologists and palaeoanthropologists, and certainly through no special knowledge or privileged frames of reference on actual Palaeolithic hunters. Or, to beat up on another theme, *if modern humans and archaic humans are supposed to have utilised raw lithic materials differently in functional terms, how can we know if alleged perceived differences relate to actual differences in intelligence between moderns and archaics?* Who established the parameters? Who got to decide? How far must one carry a chunk of flint to be considered modern? How widely must one distribute pieces of that chunk to be considered archaic?

Above we can see that the developers and purveyors of the orthodox African Eve model simultaneously occupy academic, literary and funding positions that allow them to exert influence on who gets credentialed and hired, who gets published and whose research gets funded. They also control who does *not* get credentialed, who does *not* get hired, published or funded. Some of this has to do with the patron-client relationship between mentors and students that is firmly emplaced within our disciplines. Some of it does not, however, and I submit that people ought to be highly aware of it. To expect this not to have effects on the disciplines and their scholarly literature would be silly. This process clearly colours who enters the fields, who gets published and who gets research funded. At least two groups of primary interested parties also lose out in this neat arrangement: students of contemporary archaeology and palaeoanthropology programs, and graduated junior faculty trying to enter a very unforgiving and hostile field that is increasingly run of,

by and for the calculated benefit of the High Priesthood of Archaeology and Palaeoanthropology. As scholars of the past, we might reflect on the contextually deviant but perhaps sympathetic figure of Akhenaton, whose attempts to change Egyptian society resulted in the virtual erasure of knowledge about the man by the then established priesthood of Amun. Historical students of the French and American Revolutions can also attest that established orthodoxies, when intentionally administered for the benefit of the few against the interests of the many engender revolts from below by default of their own existence and preservation. Power corrupts. Absolute power corrupts absolutely.

Anyone disbelieving what I allege should ask themselves this: what attains one a tenured academic position in Americanist archaeology or palaeoanthropology? Is it technical ability in the field and excellence in teaching those aspiring to membership in our fields or a certain nebulous tally of peer-reviewed publications? Do they correlate at a unity? We all know something about how things get funded and published, and how folks get tenure hires. In other words, do the same things that make good researchers (ability to get slim funding, contacts, access to sexy data) equate to what makes a good instructor (patience, a love of teaching, empathy with students)? How does one join the High Priesthood? Allegedly through the 'hard work' of meeting degree objectives and some evidence for publishing (currently the degree objective is the Ph.D., but who knows what the future may bring ... perhaps a doctorate *plus* 50 peer-reviewed publications and five post-doctorates?). But is that really it? Who reviews journal submissions? Mainly the High Priesthood. Who reviews grant proposals? The High Priesthood. Who chairs doctoral committees? The High Priesthood. The High Priesthood had better be very, exceedingly, minutely careful in today's political and financial environments that in its push to make life easier and more financially predictable for themselves (and few they are) that they do not make the requirements of entering the fields of Palaeolithic archaeology and palaeoanthropology so onerous that they begin to die, leaving a dwindling number of scholars corresponding about dwindling numbers of topics.

Heath and Hanson (1998: 150) describe the following, in relation to the academic classics:

Our present generation of classicists helped to destroy classical education. Yes, what they wrote and said was silly, boring and mostly irrelevant, worse even than the arid (but often valuable) philology that drove away so many undergraduates in the 1960s and '70s. Classicists now, along with the best social constructionists, moral relativists and literary theorists in the social sciences and comparative literature departments, 'privilege,' 'uncover,' 'construct,' 'cruise,' 'queer,' 'subvert' and 'deconstruct' the 'text'.

But while this academic rant may be forgivable — like all fads, it too will pass — what classicists did to the Greeks themselves is not. Our generation of classicists, faced with the rise of Western culture be-

yond the borders of the West, was challenged to explain the importance of Greek thought and values in an age of electronic information, mass entertainment and crass materialism. Here they failed utterly. Worse, the dereliction of the academics grew out of a deliberate desire to adulterate, even to destroy, the Greeks; to demonstrate that, as classicists, they knew best just how awful, how sexist, racist and exploitative the Greeks really were. This was a lie and a treason that brought short-term dividends to their careers, but helped to destroy a noble profession in the process.

Classics was now strangely led by individuals who saw their field as but another stepladder by which to enter the realm of a professional elite. Departments of Greek and Latin were reinvented as places of reduced teaching loads, extended leaves, think-tank hopping, conferences, endowed chairs, grants and petty power politics — often decorated with a patina of trendy leftist ideology or neoconservative scorn, depending on how the volatile winds of budgets and funding sources blew. Teaching and advising students, offering courses on broad topics, writing for a general audience and exploring what the Greeks actually said rather than how they said it — all were abandoned for a little prestige and a handful of perks, the petty recompense for their wholesale destruction of Greek wisdom.

Anyone working in academia should recognise many anthropological, archaeological and palaeoanthropological examples of the above within their own spheres of influence.

Discussion

Bednarik (2008 etc.) often lambasts archaeology for its humanistic mechanisms of scholarship, and perhaps much of this ire is deserved. The field of Old World Palaeolithic archaeology (OWPa) certainly evinces many opportunities to take it to severe task. Whereas I am a product of that scholarly establishment, one of the issues with which I have some profound philosophical issues regards the tendency of OWPa to construct what I term 'unstable orthodoxies' (Thompson 2012), such as the Africanist cognitive modernity model. My primary problem with the concept began rather simply with some questions about the nature of our contemporary lived reality. What *is* modernity? Is it neotony and lifelong retention of juvenile traits? Is it slavery and permanent inequality? Is it corporate efficiency (despite the fact that corporations are not generally efficient at much of anything except making executives and shareholders money)? Is it computers and other information technology? In other words, does it have a beginning or an end? Astronomers and cosmologists do not routinely present their research as the end-state; they deliberately leave caveats in which new discovery can insinuate itself (Sagan 1995). Can the same be said for archaeology or palaeoanthropology? Having not actively sampled the behaviour of matter at the event horizons of black holes, do astronomers studying black holes present their research as the final word on such

phenomena? No, they frame their publications such that they allow for the possibility that future discoveries can upend their conclusions. Why are anthropological discoveries or interpretations then so often presented as end or final states of research (Henshilwood and Marean 2003; Mellars 1999; Tattersall 1995) as though all matters were settled?

Is modernity a mindless conformity of amplification, whereby the loudest, most persistent and most published, voices hold sway because they are loudest, most persistent and most published? How does modernity relate to archaeology's conceit of quantification: wherein we find a race to statistice the entire field and to reward those who most deftly appropriate statistics and quantitative models from other disciplines? This, despite the fact that most of us are taught about Franz Boas, a former physicist who eschewed natural science in favour of social science, who wanted to study *people* as opposed to minute properties of matter.

Archaic humans, for example, probably did not sit around mutely knocking out functionless tools. If Lower and Middle Palaeolithic stone tools were functionless then why were they made? Boredom? Amusement? Yet, we are also chided that Lower Palaeolithic Acheulian handaxes were made to be capable of serving multiple functions. So, on the one hand Acheulian handaxes are rudimentary barely-tools that contain no form/function correlation, and on the other hand handaxes are multifunctional implements. The entire act of flaking (not moulding, or pouring, or baking) a handaxe out of siliceous material implies thinking and conception. If Archaics could not conceive of a need for task-specific tools, then how could they conceive of needs for tools capable of serving a broad spectrum of functions? The modernity orthodoxy would have us understand that archaic humans made functionless tools that nonetheless were used generally for multiple functions, somewhat like Swiss army knives, tools for all seasons. This is nihilism. And since much of the BM orthodoxy was (and still is) premised upon craniofacial morphology, precisely how much craniofacial diversity does the human species exhibit? Dogs and horses for example, exhibit a much greater range of craniofacial plasticity than contemporary humans. Yet, dogs are not broken into different species based upon such phenotypic traits. Why are humans? Mainly because it benefits some people in academia. It benefits the same people who police research and teaching.

By way of example, one possible study would be to make some handaxes on a variety of materials, and then to take those handaxes to as many students, craftsmen and artisans as one might reasonably find and ask them what uses *they* could make of such things. Craftsmen rely on their tools and their own physical abilities to subsist; professors do not. Archaeologists and palaeoanthropologists should therefore be studying contemporary craftspeople who work with their hands and their tools in addition to the few surviving groups of compromised and variably colonised foragers.

Interesting as lithic replication studies are they do not really inform us about much beyond the replicator's personal techniques and opinions. Even the most adept tenured flintknappers *do not rely on their technical or physical prowess to subsist*. In a Socratic dialogue one might then ask, 'So just what makes us archaeologists experts on tools and tool-use if we don't demonstrate any reliance on them for our own subsistence?'

I think unstable orthodoxies, such as the African modernity structure, especially as they are used in North America and Europe, are to some degree conscious self-promotion and turf-protection schemes for professional academics who simply happen to find themselves involved in archaeology and palaeoanthropology because those happened to be their routes to the throne. Such orthodoxies are policed against outside unorthodox offerings or against general heterodoxy. Such narrow thinking is neither effective nor is it sustainable. The African Eve model did not ever predict Dmanisi or Denisova, new fossil finds that were forced by some members of the High Priesthood of Archaeology to fit inside its preferred Africanist model (post-hoc accommodation). New thinking is not developed to deal with new material, but rather the material is forced to 'fit the policy', in a manner somewhat akin to the origins of a recent war in the Near East premised upon the existence of fictional weapons. We won't and cannot understand if we continue to pretend that unstable explanatory orthodoxies, such as that of Africanist behavioural modernity, are anything but heuristic devices. We should always ask: '*Cui bono?*'

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COMMENTS

Bones from the Barbary Coast

By AHMED ACHRATI

A question that preoccupies palaeoanthropological research is modernity, a behaviour which many researchers think is uniquely a human characteristic that emerged sometime in the Middle Palaeolithic as a result of mutation. In its extreme form, this view limits cultural innovation and cognitive development to modern humans, excluding other sister species such as the Neanderthals and Denisovans. This view

of behavioural modernity, J. R. Thompson argues, has misdirected research efforts relating to human origin and development.

Although not new, Thompson's criticism is supported by recent discoveries from Dmanisi (Lordkipanidze et al. 2013), and Sima (Meyer et al. 2013). While the Dmanisi skull analysis points to the pitfalls involved in the taxonomic typologies that have guided phenotypic and behavioural theorising in anthropology, the DNA results from Sima confirm the chequered genealogies of modern humans (Reich et al. 2010; Green et al. 2010), thus calling into question the replacement views.

Thompson also rightly points to problems relating to academic culture and the ways anthropological researches are conducted, including narrow disciplinary focus, theoretical biases, institutional allegiances and personal career concerns. These conditions are not without costs to the discipline in terms of lost opportunities, misallocation of resources and inconclusive investigative results. Another problem that could be added is that, in spite of the impressive innovations in research tools and techniques, old assumptions still persist, making the learning curve steeper. For example, recent studies on the retrogression of Neanderthal DNA in modern human lineages indicated that non-Africans inherited 1–3% of their genomes from this sister species but the Africans have no sign of Neanderthal DNA (Reich et al. 2010; Green et al. 2010). But, as subsequent research found, Neanderthal genes are present in north Africans (Sánchez-Quinto et al. 2012; Henn et al. 2012). Indeed, compared to sub-Saharan Africans, north African populations have a level of derived alleles shared with Neanderthals similar to that found in non-African humans, which may simply reflect a Holocene influx of people from Europe or the Middle East. However, it was also found that a higher level of Neanderthal's genetic signals are present in the autochthonous Berber populations of Tunisian. This, it was hypothesised, may be due to a pre-Holocene back-to-Africa movement, which may or may not be the case. Still, the problem of all these studies is that they do not account for the north African hominins of the Middle and Upper Palaeolithic and the descendants of Irhoud, Dar as-Sultan, the Aterian and Iberomaurusian people.

What this indicates is that, in their enthusiastic espousal of the African origin, recent studies of modernity have eagerly sought ancestral linkages between sub-Saharan peoples (Yoruba, San, Mandenka, Dogon, Dinka, Bakola, Baka, Biaka, Mbuti, Luhya) and the rest of the hominin world, while ignoring ancient north Africa (Hammer et al. 2011; Sankararaman et al. 2014). A focus on the Levant as a geographical and chronological bridge has also contributed to the marginalisation of the archaeological importance of north Africa and a culture whose makers lived at the doorsteps of Asia and Europe!

But it takes time for old prejudice and established paradigms to be overcome. For many, north Africa was

a 'dead end' (Klein 2008; see also Balter 2011), and as a consequence, the Aterian culture was chronologically assigned to an Upper Palaeolithic horizon. Renewed interest and fresh discoveries, however, indicate that north Africa may have more to tell about the hominin story in general, and behavioural modernity in particular (Jacobs et al; also 2011; Barton et al. 2009; d'Errico et al. 2009; Hublin et al. 2012; Vanhaeren et al. 2006; Debénath 1994). At Toforalt (Grotte des Pigeons), for example, researchers recovered Aterian points along with sea-shell beads, some of which were intentionally perforated and dyed with red ochre. These artefacts were dated using multiple and single grain OSL to 82 ka (Bouzougar et al. 2007), much earlier than Blombos. In 2009, the skull of a hominin child was found at Grotte des Contrebandiers dating to 108 kya (Balter 2011).

The point to be made here in relevance to Thompson's criticisms of behavioural modernity is that north Africa's archaeology, like the Dmanisi skull is another cautionary tale against a rush to generalisation before all facts are ascertained. And one of the most crucial facts to be ascertained to validate the modernity argument is language: at what level of development did the hominin brain produce verbal communication?

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RAR 31-1124

Will the Empire strike back?

By ROBERT G. BEDNARIK

'It has been said that though God cannot alter the past, historians can; it is perhaps because they can be useful to Him in this respect that He tolerates their existence' – Samuel Butler.

At first reading, Thompson's tour de force may seem a little provocative, but a closer look soon reveals that it is a perfectly realistic commentary on the direction the discipline has been taking. Some readers may identify his assertion that it has become a version of 'economic niche construction' as the most significant pronouncement of the paper. Although quite an original sociological observation, it is not entirely unexpected: archaeology and palaeoanthropology do 'resemble poorly managed but well-advertised corporations', as Thompson observes, and the intellectual inbreeding in their 'upper echelons' does indeed stifle innovation. Some readers may find Thompson's warning of the consequences of 'dereliction of the academics' as it has occurred in classical Greek studies particularly apt. But

wherever one looks in this powerful essay, the author seems to be almost universally spot on.

It is impossible to justly disagree with any of the fundamentals of Thompson's thought-provoking paper, for instance about purported behavioural modernity being the reason for 'African Eve's' progeny taking over the world. Since we 'have never been modern' (Latour 1993), what does this nebulous construct actually mean? It is here that I can at last disagree with one minor aspect of Thompson's paper. He defines two 'primary models' of the timing of hominin modernity's appearance: one placing it in the final Late Pleistocene (the big bang of consciousness), the other in the late Middle Pleistocene (in Africa, of course). However, there is a third position: irrespective of how one defines cognitive modernity (and there is a considerable range of possible characterisations), both of these two hypotheses are false (Bednarik 2011, 2012, 2013). To see this one needs to ignore archaeology and palaeoanthropology and delve into the sciences, especially the cognitive and neurosciences. From their perspective both of the versions Thompson mentions are absurdities. Cognitive modernity was not acquired by humans until the most recent centuries — even the people of the Middle Ages existed in very different cognitive frameworks. Consider, for instance, the changes to the human brain engendered in the near-universal adoption of writing in the last couple of centuries (Helvenston 2013). Archaeologists who 'communicate' with the palaeoartists of the Final Pleistocene only deceive themselves with their necromancy; there is nothing modern about the Franco-Cantabrian cave art and there is nothing about it that they should expect to be able to comprehend (ignoring for the moment that a good part of this corpus was in any case made by hominins *they* choose to regard as a different species, such as Neanderthaloids; Bednarik 2007). On the other hand, if with the byword 'mind' we refer to the state and operation of the neural structures that are involved in moderating behavioural patterns, the hominin 'mind' must have been essentially modern at least since the end of the Lower Pleistocene. Anything else is incompatible with what the sciences know about theory of mind, self-awareness and consciousness (see Bednarik 2013 for exhaustive discussion). Thus all archaeological notions of cognitive modernity are illusions, and those of somatic modernity are probably not much better. Human behaviour is not only determined by the intrinsic neural and endocrine systems giving rise to it. These are influenced by ontogenic experiences of the individual and their effects on these neural configurations (Maguire et al. 2000; Draganski et al. 2004; Smail 2007; Malafouris 2008; Helvenston 2013).

From my perspective the most crucial point made in this paper is when Thompson raises the issue of 'misidentified or ignored' Lower or Middle Palaeolithic evidence of cognition. The key factor in 'becoming human' is none of the many candidates that have been promoted over the last century (upright walk, tool use,

language, symboling etc.); it is the introduction and skilled use of exograms. The vast corpus of surviving evidence of the use of exograms that has long been available from the Early and Middle Pleistocene has been systematically ignored or explained away by archaeology, in its endeavours to preserve the African Eve fallacy. Under that dogma such evidence of what archaeologists simplistically and without proof call 'symbolism' or 'art' was just not acceptable for early times, as it demanded the preservation of the concept of earlier hominins having been very primitive. The African Eve advocates failed to see that, from the scientific perspective, their catastrophist scenario of cognitive development was without support. Firstly, theory of mind, self-awareness and consciousness are available to extant primates (and various other species) and therefore can safely be assumed to have been with all hominins and hominids. Secondly, their model implied that millions of years of biologically very costly encephalisation involved no significant advantages, an evolutionarily naive proposition that illustrates the differences between archaeology and the sciences. And then, of course, there is the issue of archaeological illiteracy in taphonomic logic, which remains so widespread (see 'Taphonomic logic for dummies', http://www.ifrao.com/epistem/shared_files/dummies.PDF).

Thompson's questioning of the belief that ballistic hunting technology has to be the most advanced reminds me of the many sites I have seen in large European caves where the distribution and form of cave bear scratch marks seems to indicate hunting by snares, which would have been by far the most effective strategy of harvesting the large and dangerous animals in their hibernation haunts. Again, Thompson is right to challenge the simplistic mindsets promoted by the defence of simplistic archaeological dogmas, created and defended by the discipline's high priesthood that he defines so well.

I can only say that I admire Thompson for his courage in saying it as it is. After all, the Empire has a tendency of striking back, and the long list of its targets, from Boucher de Perthes to the present, shows that only the most dedicated and determined scholars persevered. Those who were *only* dedicated, like de Sautuola, died young, destroyed by the vicious Empire.

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What is art?

By TONY CONVEY

Jason Thompson has written a provocative paper on what he calls 'Unstable archaeological/palaeoanthropological orthodoxies'. I wish to comment on the section referring to art. He poses the question 'Do we really know what the things we call cave paintings and Venus figurines are?' The difficulties involved in answering this question are magnified when we look at the varied processes which result in what is called 'art' by art historians in our own AMH species. The literature on what Dubuffet calls *art brut* is full of studies of people who were culturally marginalised but produced artefacts which observers later classified as art. It is unlikely, however, that the makers of these artefacts intended them to be seen as art.

My own experience is relevant here. As a distraught eight-year-old I was incarcerated in a boarding school a considerable distance from my home. On the beach below the school I assembled small boats and vehicles out of driftwood and 'rubbish' washed up by the tides. Years later, as a young customs officer on the Melbourne waterfront I marked my blotters and scraps of waste paper with marks and patterns. A decade later, inspired by my wife, an artist since childhood, I began painting images on canvas and boards and exhibited them in art galleries. The artefacts produced by these three activities — crude models of boats and vehicles, random marks on paper and framed oil paintings — could all be described as 'art' by an observer unaware of their genesis. However, in the first instance it would be more accurate to describe the artefacts as the results of behaviour prompted by 'magical' or 'wishful' thinking on the part of a lonely traumatised child. The second set of artefacts would be more accurately described as the meaningless by-products of behaviour induced by boredom. I submit that only the artefacts produced when I began my career as an artist, framed oil paintings, could be accurately described as 'art' according to the criteria usually employed to define art in our modern Western culture.

I suggest that it is a reasonable assumption that the first two episodes of image making I experienced could have been the trigger for the creation of at least some phenomena which we categorise as pre-Historic art and furthermore that there could be many more motivational triggers of which we are unaware which could have resulted in the creation of what we call rock art.

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RAR 31-1126

Present control of the past in the interpretation of human evolution

By ROBERT B. ECKHARDT

'Who controls the past controls the future. Who controls the present controls the past' – George Orwell, 1984

In his logical and highly courageous paper, Thompson identifies himself as 'a product of the establishment "school" of Americanist archaeology and palaeoanthropology'. Reciprocally, I am a member of the 'Michigan school' interpreting human evolution (Eckhardt 1982), within which I am the only graduate who earned a joint Ph.D. in Anthropology and Human Genetics, individually personifying the school's insistence that new genetic-based interpretations should not be old wine rebottled (earlier Howell 1952; later Stringer and Andrews 1988) but instead used to formulate and test specific hypotheses against data furnished by fossils. This point is pertinent to Thompson's comments on archaeological and palaeoanthropological orthodoxies at the heart of the 'archaic modernity' problem, which requires synthesis of fossil and molecular evidence. Been there, done that.

Learning of my thesis research, *Scientific American* solicited an article. 'Population genetics and human origins' (Eckhardt 1972) dismissed the most popular mid-60s candidate for earliest known hominid, 'Ramapithecus', known chiefly since the 1900s from jaws and teeth collected at 14–15 Ma sites in India and Pakistan. My exhaustive review of the literature on all Eurasian and African dryopithecine teeth showed that regional metric dental variation, conventionally partitioned taxonomically among multiple genera and species, was less than for most teeth in a single natural population of Liberian chimpanzees. The few dimensions exceeding this range could be explained by trivial amounts of selection over time, given the heritability of tooth size estimated from my quantitative genetic studies in living chimps. The second point concerned the timing of the divergence between lineages of humans and chimps, which was estimated by Vincent Sarich from crude, pioneering molecular work to be no greater than 8 Ma. My synthesis of the molecular and morphological evidence supported a divergence time in the range of 6–8 Ma. A third tentative inference, given the shift to a later divergence date and gnathic resemblances to robust australopithecines (noted by John Robinson 1972), was that a case could be made for a large dryopithecine, *Gigantopithecus*, as the ancestor of later hominids.

The *Scientific American* article was assailed as heresy. A letter to the editor was signed by the two principals whose work mine directly challenged, plus many other authorities included to intimidate. I had grown up dealing with real physical bullies, so an academic mob inspired notably less terror. The journal required me to answer the letter, with the exchange

to be published. I promptly submitted a response. Through the editor the signatories then suggested that we should 'negotiate'. I refused, not knowing then, and still not knowing now, how one 'negotiates' reality in science as if one were setting geopolitical boundaries or haggling prices for plucked chickens. I follow Sir Peter Medawar, who considered politics to be the art of the possible, but science to be the art of the soluble. What was the scientific resolution here? The hostile letter was withdrawn, never to be seen again openly; the covert attacks had a long, strong tail, including unusually high rejection rates for my manuscripts, grant applications and book proposals. As one example, John Buettner-Janusch, then Vice President of the American Association of Physical Anthropologists (subsequently convicted first for using his New York University lab to produce recreational drugs for street sale, then again for attempting to poison the judge who sentenced him), contacted my Department at Penn State (where I was an untenured Anthropology assistant professor) and demanded that I be fired for publishing such heterodox views. Reasoning that the fury of the attacks signalled that my findings had merit, colleagues refused. In four decades I have had only one abstract rejected for presentation at the AAPA annual meeting, that in the year that Buettner-Janusch was Program Chair. Overall, advancement was slowed markedly as I faced a higher bar for publication and funding. As one example, when a Michigan colleague sought to include my *Scientific American* article in a reader, the journal-affiliated publisher was threatened with a boycott of all its books by various universities; the article was not included. In such situations one learns of only some of the manipulations, the visible tip of a career-chilling iceberg, the greater mass of which lies unseen beneath the surface, sensed but beyond full perception, also beyond effective direct response. As argued cogently by Thompson, bucking orthodoxies can be expensive in professional terms, with strong and long-lasting impact.

Despite intended professional harm, overall my story is one of career success, not failure. Metaphorically, levees thrown up around a strong river need not confine the waters until they evaporate, but may merely shift the locus of breakthrough. Doctoral degrees conferred for specific subjects mastered and research completed also can indicate a broader capacity for learning in other contexts. For me this has included analysis of company balance sheets as candidates for value investing operations, where objective analysis counts for much and social validation for little. To quote the pioneer value investor, Benjamin Graham, 'in the short run the market is a voting machine, but in the long run it is a weighing machine'. Real science parallels this; after three decades our research group (Galik et al. 2004) established the earliest evidence for bipedal locomotion at 6 Ma, confirming my 1972 estimate. Though not on the same field-transformative scale as Raymond Dart's, such delayed affirmations nonetheless are

satisfying. But how does one survive professionally and economically over the long run? Modest personal savings from whatever source can be compounded, making it possible to self-fund some research. Such activities also bring one into contact with counterparts in honest corners of the corporate realm (contrary to the facile assumptions of many faculty members, corporate officers are no more uniform in their qualities than are academics). One like-minded ally I discovered is Patrick M. Byrne, who earned a B.A. in Chinese Studies at Dartmouth, M.A. as a Marshall Scholar at Cambridge, and Ph.D. in Philosophy at Stanford. After purchasing a small equity stake in an online closeout company, Byrne expanded it into Overstock.com, which he then had to defend against massive illegal market manipulation (including naked short selling) and gangland threats against his own life. Facing greater hazards than in academia, such people can recognize and support risk-taking in research. Many more of them are needed, acting on their own and through foundations, as are scholars who are willing to openly fight thought control.

Scholars under threat for originality should do their best to force attacks into the open, where they can be subjected to scrutiny and critical thought by many people rather than being controlled by the selective suppression of the few. As Maciej Henneberg and I have discovered in our evaluation of the Flores skeletons (Jacob et al. 2006), the Internet can make this easier (e.g. www.liangbuacave.org). Another way to widen the exposure for one's ideas is to escape the confines of artificially-constricted disciplines; I sometimes publish in genetics and engineering journals beyond archaeological and palaeontological spheres of influence. In the current economic environment that is intensifying all academic resource struggles, the best, most independent-minded scholars may have to survive financially by employment outside the academy. There is an honourable precedent for this route: *Torah* scholars knew learning had value beyond that of 'a spade to dig'. As far back as the third century CE, the *Mishnah* (a written redaction of Jewish oral traditions commenting on the *Torah*), one respected body of opinion, held that scholars should not use knowledge of the *Torah* as a means of support and many Jewish scholars of antiquity also were artisans and traders. More recently Charles Darwin lacked tenure but had an independent income based on successful reinvestment of inherited wealth. His foremost defender, the energetic and articulate Thomas Henry Huxley, at first made his way economically as the nineteenth century embodiment of a high-level gypsy scholar. Although academic appointments were rare at the time, in his early years he managed to support himself on a stipend from the British Navy and by writing popular science articles. Later he secured a lectureship at the London School of Mines. It often has been said that before Huxley, science was mostly a gentleman's occupation as typified by Darwin's work, and that after Huxley it

became a profession. However, although Huxley's biographies list impressive lists of professorships and appointments to various commissions, he often struggled financially and took on extra work to support his family. In his last years he was supported by a fund raised through subscription among his friends. Professional accomplishments require economic means, and academic freedom can be impossible if scholarship is constrained by any power structure that rewards sycophancy over originality.

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Palaeoanthropology's persistent memory hole: the 'Homo floresiensis' affair

By MACIEJ HENNEBERG
 and ROBERT B. ECKHARDT

'For, after all, how do we know that two and two make four? Or that the force of gravity works? Or that the past is unchangeable? If both the past and the external world exist only in the mind, and if the mind itself is controllable — what then?' — George Orwell, 1984

Another piece [RBE] in this collection of responses to Thompson's paper recounts a palaeoanthropology's past attempt to control the perception of reality in the field. This essay provides evidence that nearly five decades later most palaeoanthropologists continue to operate in the same way. In a field in which the number of practitioners outnumbers the numbers of diagnosable primary specimens by a factor of at least ten, and where it is thought acceptable to shield important specimens from access by colleagues and hence prevent replication of observations, the scenarios offered to explain the evidence may overshadow the evidence itself.

Against this background, efforts at thought control continue unabated to ensure that attention is diverted from observable facts to elaborate mental constructs that seem to weave together a warp of misreported data with a woof of misleading memes. The result is social validation of a complex set of contradictory hypotheses that, separately or together, cannot account for

observed facts.

The two initial reports on the Liang Bua Cave specimens were published a decade ago (Brown et al. 2004; Morwood et al. 2004), amidst widespread media buzz, including frequent references to the find being the most important discovery in human evolution for the last century. From the beginning, however, several researchers had strong and very specific doubts about the validity of the 'new species'. While one of us [RBE] was working to correct what manifestly appeared to be an exaggeratedly low estimate of stature, the other of us (MH), in an interview on Australian Broadcasting Corporation (ABC) radio, suggested that the unusual features of the only known skull (still now, a decade on, as then) were due to abnormal individual development. This pathological interpretation was quickly picked up by other media, and gained some public attention. Within days Alan Thorne, a palaeoanthropologist at the Australian National University expressed doubt in the designation of Liang Bua finds as a new species. Eventually the substance of this initial alternative interpretation by two Australian human biologists, after having been rejected by *Nature* within hours of its submission, was published in December 2004 in an on-line archaeological journal *Before Farming* in the UK (Henneberg and Thorne 2004). Other sceptics included Teuku Jacob, Head of the Laboratory of Bioanthropology and Palaeoanthropology at Gadjah Mada University in Yogyakarta, Indonesia, as well as the Head of the Indonesian National Archaeological Research Centre, Raden Soejono, who had been included without much consultation as a co-author of original reports published by *Nature* in October 2004. Soejono asked Jacob to examine the specimens, and had them transported to his laboratory in Yogyakarta. Within several days Jacob announced in a press conference that it also was his conclusion that the LB1 skull was developmentally abnormal. Eventually the four of us (Jacob, Henneberg, Thorne and Eckhardt) coalesced with several others into a working group. In February of 2005 we were able to study the Liang Bua Cave bones at Jacob's laboratory. As a result our working group submitted the manuscript below to *Nature* in March of 2005:

Large errors in the depiction of small humans

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Observations made by all of us on LB1 from Flores, Indonesia, document numerous material errors and omissions in a report¹ that continues to shape extrapolations² and speculations³. Our findings challenge the empirical basis of conjectures that the Liang Bua remains represent a new

species of the genus *Homo*.

The holotype statement mentions “femora,” and the report describes a “right” complete femur with damage to its lateral condyle. The true right femur lacks the head, most of the neck and greater trochanter; its condyles appear unusual, though possibly from reconstruction or matrix obscuring the intercondylar fossa. The only nearly complete LB1 femur is the *left* (our Figure 1), misrepresented as right in Figure 7 in the original publication¹. Estimated stature of 106 cm¹ was extrapolated from pygmies, a model rejected elsewhere² and 20-25% below regressions based on extant Javanese females⁴ that produce an estimate of about 120 cm.

Description of the skull misleadingly notes (p. 1056) that the “cranial vault is long...” Supplementary Table 1 gives maximum cranial width (113 mm), length (143 mm), and cranial index of 79.1, approaching the upper limit of mesocephaly (brachycephaly being ≥ 80.0 , dolichocephaly < 75.0). The vault also is markedly asymmetrical. In inferior view of original¹ Figure 1, a line from orale (center anterior hard palate) through staphylion (center posterior hard palate) would pass well left of basion and opisthion on the foramen magnum and opisthocranium at rear of the vault, reflecting bulging in the right occipital region.

That abnormal orientation of the palate was antemortem and developmental is supported by asymmetric tooth wear that we observed on the original specimen, obscured by poor contrast and focus in the authors’ Figure 4. Their statement (p. 1058) that “CT scans indicate that the maxillary right M³ was congenitally absent” appears incorrect. Our examination shows an alveolus for the right M³ containing a worn dental fragment or root remnant verifiable by further CT or X-ray studies. Presentations of other data also are confusing: On p. 1058 it is stated that LB1 is “megadont ... relative to *H. sapiens*.”

However, Figure 5 shows that the *absolute* tooth dimensions of LB1 correspond very closely to the authors’ own sample of *H. sapiens*. This is the situation that would be expected if characteristics of LB1 reflected developmental processes reducing brain and body size, not exclusively phylogenetic ones. Indeed, the authors’ statement about “megadontia” is contradicted on p. 1061, where they refer (here, correctly) to “a masticatory apparatus most similar in relative size and shape to modern humans...”

Given these points and many more not detailed here, assertions about the phylogenetic novelty of LB1 are questionable in the presence of alternative explanations⁵. Characterizing features as “derived” reifies a taxon that remains nebulous, since beyond LB1 only a single additional tooth was described in the original paper¹. Since more than a year has elapsed between the discovery and description of these materials, we call upon the authors to provide a full, accurate presentation of all the evidence, as is their prerogative and obligation.

1. Brown, P., et al. *Nature* **431**, 1055-1061 (2004)

2. Falk, D., et al. *Science Express Reports* **1109727** (2005).

3. Diamond, J., *Science* **306**, 204-205 (2004).

4. Eckhardt, R. B., et al., *Proceedings of the Joint meeting of the International Society of Biomechanics and American Society of Biomechanics* (2005, submitted).

5. Henneberg, M., Thorne, A. *Before Farming: archaeology and anthropology of hunter-gatherers* (online journal), article 2 (2004).

After consultation with Peter Brown and another unidentified reader, *Nature* declined to publish our submission. This was not surprising given that journal’s continuing advocacy position in favour of the new

species hypothesis. What *was* unexpected was that our manuscript and the referees’ comments on it were made available to Michael Morwood and used without our permission in a later book (Morwood and Van Oosterzee 2007), where they quoted an unidentified referee who allegedly said that ‘... the paper of Jacob et al. [of 2005] had no real substance’.

To say that such behaviour is irregular is an understatement. It is a manifest ethical violation by conventional scientific standards:

‘Manuscripts under review are confidential documents, and should be treated as such. They contain unpublished data and ideas that must be kept confidential. You cannot share the paper or its contents with your colleagues.... Moreover, you cannot use the information in the paper in your own research or cite it in your own publications. You should not discuss the review or its outcome with your colleagues.’ (Rockwell 2006; see also Graf et al. 2007).

Eventually all of the points that we made in our 2005 manuscript rejected by *Nature* were conceded by Brown, Morwood or their supporters, verified independently by others, or appropriated into one or another of their own publications. However, *Nature*’s refusal to publish our paper early in 2005 very largely accomplished what must be assumed to have been its intended purpose, to suppress evidence contrary to the existence of ‘*Homo floresiensis*’ and to marginalise researchers who would dare to present such evidence. The time was critically important for the reification of the new taxon, because it allowed for publication of numerous reiterative and derivative papers that built on the earliest ‘definitive’ – but incorrect – descriptions and inferences.

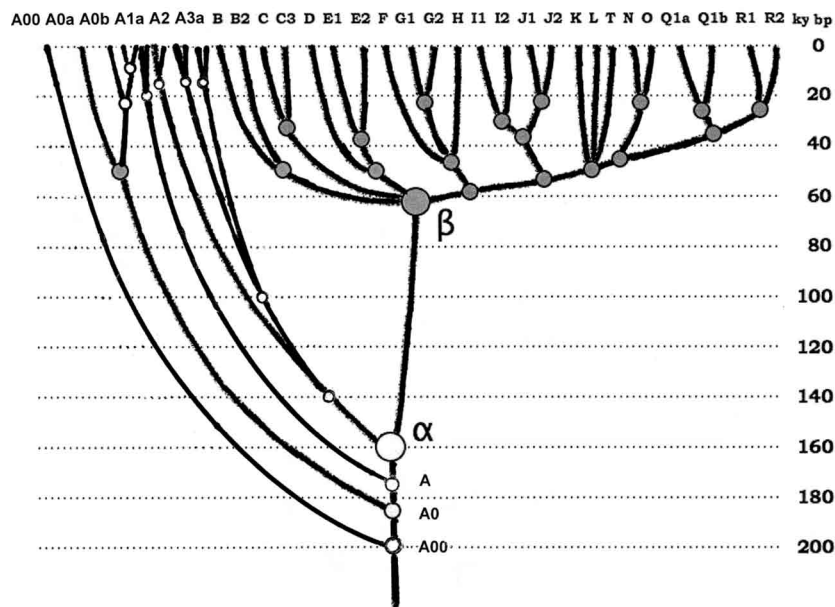
While returning from the study of LB1 skeletal remains in Indonesia, MH found in Sydney airport a major newspaper where Peter Brown described MH’s findings as ‘scratchings on the toilet wall’. Clearly, any opinion not published in *Nature* was to be considered no more than bathroom graffiti. This style of attacks continued. A short time later a debate between supporters of the ‘new species’ interpretation and MH was scheduled on the ABC national television. During this late evening debate, one of the new species protagonists, Bert Roberts, a specialist in modern methods of dating of archaeological sites (bones from Liang Bua Cave have never been directly dated to this day, however), said that MH should be punished by his university for opposing the views of the ‘discoverers’. He also called MH’s stance ‘unethical behaviour’. Next morning a telephone rang in Henneberg’s office. At the other end was the Vice-Chancellor and President of the University of Adelaide himself. He pledged the full force of the University’s legal services to defend MH from any allegations of wrongdoing. Loose allegations of an ethical breach never made it anywhere and we eventually published a book recounting the Liang Bua affair on the background of the socio-political situation in the 21st century academia (Henneberg et al. 2010).

When several papers questioning the ‘new species’

status of the Liang Bua finds eventually were published in other international journals such as *Proceedings of the National Academy of Sciences of the U.S.A.*, *PLOS* and *Proceedings of the Royal Society*, *Nature* responded with an editorial titled 'Rude palaeoanthropology' (Anon. 2006) in which it stated that it is good to have a 'robust' debate regarding some skeletal finds since it shows the vitality of the discipline. Such antics are difficult to satirise adequately, but it is possible to rephrase Orwell's question thus: if not all minds are controllable and their thoughts can make it into print, can the external world and its past be denied existence? All fingers might be chopped off to prevent the observation that two fingers plus two fingers equal four fingers, but would no one recall that the maimed hand but a short time ago had been whole, or notice that bloody digits dropping to the floor at least showed that gravity is real?

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haplotypes and subclades (see Fig. 2) with an addition of the recently discovered haplogroup A00, and with an updated nomenclature of haplogroups and subclades compared with those in Figures 1 and 2. The timescale on the vertical axis shows thousands of years from the common ancestors of the haplogroups and subclades. The tree shows the α -haplogroup, which is apparently equivalent to haplogroup A1b in the current nomenclature, and is ancestral to both the African and non-African haplogroups (its common ancestor lived $160\,000 \pm 12\,000$ ya), and the β -haplogroup, which is equivalent to haplogroup BT in the current classification (its common ancestor lived $64\,000 \pm 6000$ ya). From Klyosov (2014).

Clarifying the 'African Eve' concept

By ANATOLE A. KLYOSOV

Since *RAR* Comments are restricted by length I will comment only on DNA-related matters, particularly since they seem to be the most puzzling to the author of the paper commented on, if to judge by the number of questions addressed.

To establish a background for the DNA-related matters with respect to the 'out of Africa' and the 'African Eve' concepts, let me specify a few basic items:

1. There was no 'out of Africa' exodus of the 'anatomically modern humans', either 60 000–70 000 years before present (ybp) or 100 000–120 000 ybp, or 180 000–200 000 ybp. By stating this I do not mean some isolated cases when someone wandered out of Africa to end his or her life nearby, or slave exports from Africa to elsewhere. I mean 'out of Africa' as a basis for modern humankind on the planet. It has never happened, or at least it has never been proven (Klyosov and Rozhanskii 2012; Klyosov et al. 2012; Klyosov 2014). All 'academic' papers which have said otherwise were not qualified in their presentations, they misinterpreted the data, they bent their interpretations, they ignored other evidence, or they invented their 'evidence' (Klyosov 2014).
2. Y-chromosomal DNA-lineages of Africans and non-Africans went apart around 160 000 ybp (see Fig. 1). Before that a number of African haplogroups and subclades (A00, A0, A1a, A1b1) split one by one from the principal non-African lineage between 210 000 and 160 000 ybp and left for Africa. In fact, we do not know when they moved into Africa; all that we know is that they live in Africa NOW. There is no single DNA from ancient Africans analysed to provide any data on their haplogroups (Klyosov 2014).
3. The mtDNA lineages of Africans (haplogroup L0) and non-Africans (haplogroups L1–L6, see Fig. 2 below) went apart around 160 000 ybp (Klyosov 2014).

Figure 1. Haplogroup tree of the *H. sapiens* Y-chromosome derived from

4. Regarding 'Cann et al. (1987) published a pivotal paper', as it was mentioned in Thompson's paper (p. 131), I can only note that the Cann et al. paper entitled 'Mitochondrial DNA and human evolution' was rather weak not only by contemporary criteria, but also by those in the 1980s, and one can only wonder how that paper got through the *Nature* reviewers, if there were any. It would be enough just to mention that the Abstract, published prior the main text, said that the mtDNA studied in the paper 'stem from one woman who is postulated [! – AAK] to have lived about 200 000 years ago, probably [! – AAK] in Africa'. It was not surprising that the paper was actually denounced four years later by two former authors, Stoneking and Wilson, along with three new co-authors and Cann absent (Vigilant et al. 1991), and the new paper informed that the Cann et al. (1987) proposal 'that all contemporary human mtDNAs trace back ... to the ancestral mtDNA present in an African population some 200 000 years ago' was at first 'rejected because of confusion over conceptual issues', and pointed at 'perceived weaknesses of the Cann et al. study'. Among those weaknesses the authors (Vigilant et al. 1991) count that 'it used an indirect method of comparing mtDNAs ...; used a small sample made up largely of African Americans to represent Native African mtDNAs; used an inferior method ... for placing the common DNA ancestor on the tree of human mtDNA types; gave no statistical justification for inferring an African origin of human mtDNA variation; and provided an inadequate calibration of the rate of human mtDNA evolution'. In other words, its authors recognised the weakness of the paper (Cann et al. 1987) that formed a ground for the 'out of Africa' concept. However, the concept was already accepted by the 'consensus', and it was too late to turn it back. Therefore, the 1991 paper aimed at throwing the Cann et al. (1987) paper out as a weak one, but justified the concept itself. A recent overview paper (Klyosov 2014) shows what kind of 'justification' it was, along with other 'justifications' in the area. All those 'justifications' actually postulated the 'out of Africa' event, and then bent all the data and their interpretations to 'prove' it (Klyosov 2014).
5. Some academic papers appear from time to time which describe migrations 'into Africa'. For example, a recent paper in *Nature* in August 2013 (Hayden 2013) describes migrations into Africa 3000 and 900–1800 years ago. Did they add to the 'genetic diversity' in Africa, which is commonly used as a major 'proof' of the 'out of Africa' concept? Sure they did. Furthermore, the migrations were to the sub-Saharan region, where Cann et al. (1987) sampled mtDNA and found a 'high genetic diversity'. In

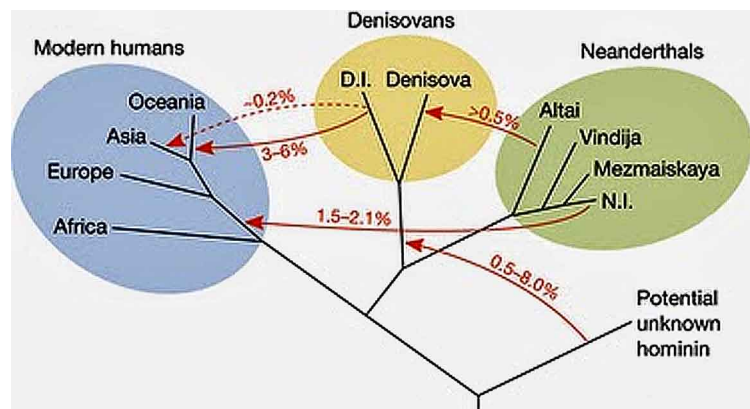


Figure 3. The tree of *Homo* showing alleged admixtures (in red). The blue area on the left shows that the non-African branches arose not from the African branch. From Prüfer et al. (2013) with permission.

a recent study Prüfer et al. (2013), after studying African genomes, noted: 'These results mean that we have not identified any sub-Saharan African sample that we are confident has no evidence of back-to-Africa migration'. The principal results of their study are summarised in Figure 3, which shows that non-Africans did not descend from Africans.

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Sociological approaches to archaeological research

By OSCAR MORO ABADIA

Thompson's paper makes a reasonable case for addressing some of the problems associated to the concept of 'behavioural modernity'. The author rightly argues that the criteria used by archaeologists and anthropologists to define 'behavioural modernity' constitute a kind of 'disparate' list that includes genetic data, artwork and personal ornaments, blade-based lithic technology, bone, wood, and other organic technology, socio-demographic elaboration and population growth, and economic intensification. The first part of the paper examines some of the problems related to each of the abovementioned criterion. For instance, the author demonstrates how in South Africa and Australia, two regions that have been the object of intense debate concerning the origins of modern human behaviour, Pleistocene groups developed very different kinds of technology, some of which do not fall comfortably within the remit of Upper Palaeolithic

industries (for example, lithic blades and microliths are mainly absent from Australian lithic assemblages). In this sense the first part of the article provides a critical (and necessary) assessment of many of the inconsistencies associated to recent debates on human paleoanthropology.

While the review of the criteria used by archaeologists to define the concept of 'behavioural modernity' is accurate, the explanation concerning the current popularity of this term in the fields of archaeology and anthropology is problematic. In section number 2 ('Archaeological and palaeoanthropological publishing, academic employment, research funding as contemporary economic niche construction and other anti-academic capitalistic trends'), the author argues that the concept of 'behavioural modernity' has been imposed to archaeological research by the 'high priesthood' of archaeology, i.e. by a tenured academic professorship that controls academia by deciding who is hiring at academic institutions, who and what gets published in mainstream journals and which research projects get funded. In other words, according to the author, the reasons explaining the popularity of the concept of 'behavioural modernity' are not scientific, but sociological. Without denying the impact of sociological factors in archaeological research, I think there are a number of academic reasons explaining the current prevalence of the idea of 'modern human behaviour' in academic debates. In fact, this concept became popular in the 1980s to explain the 'Upper Palaeolithic revolution', i.e. the 'cultural revolution' associated to the arrival of modern humans in Europe 40 000 years ago. This revolution was defined by a number of archaeological traits, including the standardisation of lithic technologies, the use of pigments such as ochre, the collecting of beads and personal ornaments, the specialised hunting of large animals, the burial of the dead, and the development of art and symbolism. Later, archaeologists began to use these criteria, that initially only referred to the beginnings of the Upper Palaeolithic in Europe, to explain the emergence of modern human behaviour all around the world. In my view, the popularity of the concept of 'behavioural modernity' is less related to the capricious desires of the aforementioned 'archaeological priesthood' than it is to a profound Eurocentric bias in archaeological research that is the result both of the history of research and of the privileged position of the European record in debates concerning the origins of most cultural innovations.

The second point I want to make is a consequence of a virtue of the paper, its reference to the sociology of science. While the emphasis on sociological explanations is original and thought provoking, this strength of the article is also its weakness. What is missing in many respects is a more detailed and subtle understanding of a number of non-scientific factors influencing archaeological research. The paper somewhat promotes a monolithic characterisation of the field of scientific archaeology. According to the author, this field 'has

succumbed to a contemporary, highly capitalistic, version of economic niche construction' based on 'patron-client relationship between mentors and students that is firmly emplaced within our disciplines'. However, the author does not explain in what *concrete* ways archaeology has succumbed to capitalism or how the patron-client relationship articulates modern archaeological research. In my view, this pejorative view of academia would have benefitted from a more detailed contextualisation in the field of the sociology of science, including a reference to the works by Thomas Kuhn, Robert Merton, David Bloor, Pierre Bourdieu and other sociologists.

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The tip of the iceberg

By ROY QUEREJAZU LEWIS

I want to congratulate Jason R. Thompson for his brave and interesting paper 'Archaic modernity vs the High Priesthood: on the nature of unstable archaeological/palaeoanthropological orthodoxies'. Thompson covers many vital points on the subject, although I esteem that it is only a good part of the tip of the iceberg.

In these comments I wish to increase that tip of the iceberg with some other themes, and leave it to other scholars to deal with the body and substance of it. I will mention, therefore, very briefly some other areas that could be taken into account in this debate. Themes such as 'unstable orthodoxies' and 'how can we be certain' are so evident that I am sure that other scholars will comment on them, in a better way than I could do.

Jason Thompson puts much emphasis on the theme of 'material evidence'. Well, on this point, I consider that there is a lot to be said, and a lot to be analysed. And here is where we have the great segregation between archaeology research and rock art research. Archaeology research covers mostly the supposed 'material evidence', whereas rock art research, while analysing material evidence, deals mostly with intellectual, cognitive and spiritual contents.

The relevance of archaeology to the science of rock art has been questioned. Yes, on close examination the relevance of archaeology is found to be limited and the methods of that discipline are inexpedient, whereas several other disciplines are more closely related to rock art science, such as conservation science, ethnography and anthropology. This reality will be evidenced in

the 'First International Rock Art and Ethnography Conference' to be held in Cochabamba (Bolivia) in September 2014.

Related to the subject just commented on, there is another preoccupying theme which has to do with the way some self-taught scholars in rock art carry out their activities in rock art research and conservation. Practically, the majority of rock art researchers are self-taught and adopt divergent types of analysis and different methodologies, mostly based on descriptions of the remaining rock art. There are those that follow previously established archaeological practices, and therefore try to situate themselves in the 'accepted academic establishment'. On the other hand, there are self-taught researchers that consider rock art research as a science in itself, and work encouraged by passion and profound eagerness to learn, and put personal ambitions behind scientific and rock art conservation results. Rather than trying to diminish the excellent results self-taught rock art researchers have obtained since the beginnings of rock art research until nowadays I wish to point out that some self-taught rock art researchers have led the way so that rock art research has finally established itself as a science in its own right.

This leads me to the next point. Thompson mentions the benefits that some people gain in academia. This theme is vast and complicated and it occurs worldwide. Each case, without doubt, has its particularities. If we try to incorporate a common denominator, we could include the benefits some people receive in political and administrative executive positions dealing with cultural heritage. In many cases and in some countries, especially in the Southern Hemisphere, decisions on cultural and heritage themes are decided by politicians, public administrators and other people related to the government. This leads to, and has led to, catastrophic and shameful results.

Finally, a small comment in favour of replication studies, just to mention that they enable us to understand better what could have been the reality of the human past.

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Homo querulosus

By IAN TATTERSALL

Many of the individual points in Professor Thompson's admirably impassioned review are very well taken. But reading his tirade tempts me to think that

perhaps his disposition is not optimally suited to measured scientific discourse. For after all, science is a way of knowing which does not seek definitive answers about the universe (heaven forbid that I should utter the word 'truth'). Instead, it is an unstable system of provisional knowledge in which our descriptions of natural phenomena are ceaselessly refined by the rejection of false beliefs about them. As a result, the scientific framework at any one time is not only pluralistic, but is glued together as much by questions about what we think we know of the world as it is by enduring 'facts'. What is more, the field about which Professor Thompson has chosen to complain so eloquently and vociferously is (or certainly should be) an area of historical biology. Of all of the major divisions of science, biology is indisputably the most untidy; and among all the areas of biology its historical branch, which deals with phenomena that cannot be replicated in the laboratory, is untidiest of all. Professor Thompson is evidently of a highly reductionist mindset (or is, at least, extremely unhappy in the absence of reductionist answers); and in light of this it might be fair to speculate that he might be happier when engaging, say, with the phenomena investigated by physicists, than when grappling with the slippery subject-matter of palaeoanthropology.

These are matters of temperament, of course. But they do intersect with questions of substance. Professor Thompson complains, for example, that 'Modernity appears to mean different things in different geographic areas' (p. 133). But why should it not? Even chimpanzee 'culture' means different things in different places. Indeed, if we try to equate 'modernity' with the 'human condition' that we modern people tend to agonise so greatly about, we will seek with great difficulty for any usefully defining attributes of modernity. Human behaviours exist on continua; and you may easily find someone to illustrate each pole of any pair of behavioural/dispositional paradoxes you might wish to specify. Indeed, you may often find them both within the same person. While pondering the easy reductionisms of evolutionary psychology I have tried many times to think of a true 'human universal' that characterises every human being alive; and the best I have been able to come up with is 'cognitive dissonance'. This is hardly a reassuring thought for anyone seeking proxies for 'modern' behaviour patterns in the archaeological record; but it may help explain some of the difficulties that so deeply unsettle Professor Thompson. And it might be of additional comfort to him to offer the thought that eluding easy definition doesn't imply non-existence.

As to many of Professor Thomson's specifics, it is vaguely disquieting that he so frequently allows hyperbole to get the better of him. I was a little surprised, for example, to learn that he thinks I many years ago insisted (p. 136), in an 'odd accusatory tone,' that 'our 'unique and symbolic' AMH species ... managed to enslave untold numbers of other AMHs'.

But then, maybe I shouldn't have been particularly astonished. After all, I once heard the great novelist Anthony Burgess gripe memorably about how amazing it was that critics continually managed to discover all kinds of allusions and meanings in his writings that he himself had been totally unaware of. Still, that was literary criticism; and Professor Thomson claims to be writing about science.

All this notwithstanding, Professor Thompson is not distressed merely by the details of one particularly elusive area of science. He is upset about the larger processes of science itself, or at least about how they are played out on the academic stage. And here he is fully entitled to his dismay, especially as regards his bugbear science of palaeoanthropology. Science is a distributed activity that is necessarily done by people (albeit increasingly algorithmically assisted); and, like all communal human endeavours, it has its politics. Here it is the parochial politics of scientific academia that earn Professor Thompson's particular disdain: matters of power, publicity, funding and faculty hiring, and their effects on the distribution and penetration of ideas. But even more importantly, his observations apply equally to the larger dynamic of the scientific enterprise, and to the sheer inertia that is built into the system, partially at least as a function of long human generation length.

One single publication, appearing in an instant of time, has the instant potential to change an influential scientific paradigm (think of Eldredge and Gould 1972, and punctuated equilibria). Nonetheless, as has most certainly happened in palaeoanthropology, it often takes many decades for the implications of important, insightful and revolutionary studies of this kind to permeate the areas of science to which they pertain. Despite the discovery of huge quantities of hominid fossils that scream of how diverse the history of our family (or subfamily, or whatever) has been (and thus of how typical it is of successful taxa), most palaeoanthropologists today are still mired in various versions of the rigorously linear notion of human evolution that was foisted on us by Ernst Mayr over 60 years ago (Mayr 1950). For this lamentable and destructive persistence, blame the power of Professor Thompson's eloquently characterised (though informally constituted, and alas probably inevitable) 'High Priesthood.'

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Editor's note: several of the leading protagonists of the replacement hypothesis ('African Eve') were invited to comment on Thompson's paper. They declined.

REPLY

Response

By JASON RANDALL THOMPSON

I thank the commentators for providing their commentary.

Achrati correctly asserts that north Africa presents problems for the various replacement hypotheses, and observes the Dmanisi material also complicated matters. I will admit that I was hesitant to submit this paper for review, because it is provocative and was intended to be. I used the comparison with the academic Classics as a mechanism to show how material anthropology (at least in the US) is growing to resemble that fossilised discipline. If, as Bednarik suggests, the Empire does in fact strike back then I suppose my fate will be not terribly different from most newish advanced degree-holders in the US, which is employment in various 'service sector' industries and moving in with the parents. Convey very wisely notes the global tendency for objects produced by marginalised aboriginal peoples to be described as 'art' by Western scholars despite its lack of such status among the makers. The inverse of this process is of course where professional academics perform mental gymnastics to explain away archaic human capabilities: i.e., Dibble's 'explanation' of Mousterian scraper morphology as a mindless reductive process absent of any goal-direction. Some Mousterian scrapers unquestionably *were* fashioned into their sizes and shapes through sequential retouching and re-use. Yet from Dibble it takes a very large jump across a vast *non sequitur* gulf to arrive at the beach of 'that means archaics were stupid' as so many have opined so loudly for so long.

Eckhardt's very welcome comments have stiffened my spine a bit, I must confess. He observes that performing on the stage of academic science has never been easy and that colleagues can and will attack our livelihoods if their research is challenged. Dr Klyosov produces a very welcome recapitulation of the problematic adoption of the replacement model post-Cann et al. (1987). Actually, I think I understand the genetic data quite well. Its primary interest for me is its evident portrayal of hybridisation and admixture between archaic and modern human lineages, which refutes a primary aspect of the replacement hypothesis: namely, the assumption that modern and archaic humans were separate 'species'. I haven't as yet had adequate time to review Klyosov's publications, but I am familiar with Alan Templeton's (i.e. 2010) genetic research in which he has been severely critical of the misuse of the biological concept of species by many archaeologists and palaeoanthropologists. Too many scholars for me to recount have suggested that the immense landmass of Asia was the main area in which most human evolution occurred, and the new

genetic data also buttress that. Moro Abadía observes primarily that I am a newer scholar and haven't read enough about the sociology of science. On that count the author pleads a meek 'guilty', and hopes to rectify that ignorance as soon as he finds the time! Henneberg and Eckhardt suggest that the Liang Bua specimen has been misattributed taxonomically into a new 'human' species instead of being a pathological human being. This is a serious charge, and I have elsewhere read similarly severe critiques of interpretations based on this fossil material. An even more serious charge is that the business of science can be a bloody enterprise because of competing egos, too few specimens and too many colleagues, and a bloody general absence of money!

Homo superciliosensis

I was quite pleased when I learned of Dr Tattersall's participation in our commentary. It becomes abundantly clear, however, and regrettable if not absurd that, instead of joining in dialogue he chose merely to sneer down at us — or at least to me — from his throne, robbing all of us of an opportunity to learn. Since this paper was written in November 2013, several recent publications were presented that offered all of us excellent opportunities to reconsider long-held assumptions regarding the conspecificity of anatomically modern and archaic humans (Sankararaman et al. 2014; Vernot and Akey 2014). Dr Tattersall is unquestionably aware of them, but chose to ignore those sources along with his artful avoidance of any of the matters at-issue in our mutual dialogue. This is not at all how I hoped this dialogue would unfold. None of us are ignorant of the contributions Dr Tattersall has made to the study of human pre-History. I fully acknowledge that he is a major player, which makes his quizzical mockery and scorn all the more disappointing. It seems a bit ironic, however, that Tattersall's (Tattersall 1995; Tattersall and Eldredge 1977) models for human origins now appear, in the light of the new genetic and archaeological data, to represent a form of the special-pleading he so powerfully derided in print in *The fossil trail* and elsewhere.

I have previously criticised the opportunistic framing of self-aggrandising 'unstable orthodoxies' in anthropological publishing (Thompson 2011, 2012, 2014). This is not a new theme of mine nor is it even *mine*. I think the most salient aspect of my paper regards that: the critique of shameless self-promotion via framing literary orthodoxies amongst many of the louder self-elected grandees of human pre-History, especially on the relatively non-controversial assumption that human origins matters, and getting it right ought to matter too. What are the costs of failure, especially in the United States, where people claim to 'disbelieve' in human evolution as though it were a matter of preference? This is sociopolitics in anthropology. If members of the replacement clique were physicians or worked with material media, then instances of wrongness could be

demonstrated relatively quickly. Yet, the self-centred elite I describe works outside ultimate proofs with a plastic medium whose dimensions and attributes *they chose*. Refutation of the African replacement model for human origins had to await technological evolution of methods and mechanisms for isolating and amplifying ancient DNA. What does it mean to have academic status and prestige if the science one has done to attain it is inaccurate or wrong? How does it help Science if we get our own origins wrong?

In his comments, Tattersall simply talks right past the fact that such self-promotional careerism is decidedly a negative 'matter of temperament', and certainly adds nothing to our scientific 'questions of substance'. It is all the more ironic that he uses the metaphor of phyletic gradualism as 'how things are done in Real Science' but then cites the model of punctuated equilibria. For example, I deliberately wrote that I hoped *future discoveries would challenge me and all of us in regards to human origins because this stuff matters*. It matters all the more in an interval in which religious and corporate intransigence mutually agitates against American science in a bewildering profusion, but with special venom reserved for sciences of human origins. We have to get our facts right! Being wrong comes at a perilous cost now because science is being attacked from so many different sides! It is quite simply a demonstrable fact that the majority of the palaeontological and archaeological scholarly establishment *chose* to advocate for the replacement model of human origins, and it is equally factual to note that the genetic data now seriously undermine that model. Note how the debate is shifting beneath our very feet, as replationists are now in fall-back positions of arguing that moderns and Neanderthals were 'becoming reproductively-isolated' instead of *were* reproductively-isolated. If anything, it now looks in the light of the recent genetic data that *both* the replacement and multiregional models for human origins are inherently flawed, suggestive of some rather extreme problems with the assumptions we have traditionally made. Our origins were much more nuanced than either model can tolerate. We don't actually 'know what we think we know' yet, to make an obvious epistemological observation (Tattersall 1995).

This is not simply inevitable or 'how science gets written'. This is how one particular brand of scientific palaeoanthropology/archaeology gets written by a subset of some professional prehistorians. Not all. Nothing Don Fowler has written, for example, or Joe Schuldenrein, just for two anecdotal examples, was framed as though it was unassailably inscribed in tablets handed down from the mountain. I will observe, again, that astronomers and cosmologists routinely avoid framing their published professional research such that the last word on phenomena has been written. Carl Sagan (1995), a highly-regarded, influential and open-minded scientist and author, actively warned against this tendency. Along with many other highly-placed scholars of prehistory,

including legions of Lewis Binford's students, Don Johanson, Paul Mellars, Harold Dibble, Chris Stringer, Chris Henshilwood and Curtis Marean, Dr Tattersall chose to frame his own publications as constituting the 'last word' — in this instance, the peculiar fixation on 'behavioural modernity' and the monotonous droning of the theme of archaic simpletons overrun by the invaders from South Africa, who allegedly brought the gospel of 'modern human uniqueness' along with art, music, language, symbols, and everything else in our sociocultural repertoire (Tattersall 1995). Dr Tattersall has been singing this particular dirge since at least 1977 (Tattersall and Eldredge 1977).

It appears that archaic humans were not the only things subject to replacement; archaic literary orthodoxies are also subject to it. As I observed in Thompson (2011, 2012), we once used to say very authoritatively that *Australopithecus afarensis* was unquestionably the basal stem hominid. That has gone the way of the dodo. It was also once possible to pretend that *Homo erectus* left Africa only one million years ago en route to everywhere else in the Old World. Pffffffft. The Dmanisi site has convincingly demolished that hypothesis. We also 'knew', or were rather scolded into knowing, often by Dr Tattersall, that Neanderthals were cognitively simple. How did we know that? Negative evidence (Speth 2004)! Yet that bubble has also burst from extremely recent archaeological review of Neanderthals, concluding that Neanderthal parents were basically ... well, like other human parents: they loved their children (Spikins et al. 2014). Imagine someone writing that in the late 1980s through the 2000s. It would have been received by the League of Really Serious Anthropological Bigshots with taunts of over-emotionalism, feminine hysteria etc. Which, I will observe, was one of the implicit damnations in Tattersall's faint praise for me: dismissal by invocation of alleged hysteria.

An accumulating body of objective *genetic* data, not subjectively and variously interpretable fossils or lithics, but DNA, presents us with the refutation of the African replacement model for human origins, since that model was premised upon complete genetic isolation between the allegedly creative and unique Africans and the mythically crude archaics (ironically including the participation of Svante Pääbo, who the replacement advocates once thought pruned Feldhofer from the human family tree). If 'anatomically modern humans' and Neanderthals, Denisovans and *heidelbergensis* were interfertile then they were not separate species, and the replacement model is *wrong*. That matters, a great deal. If but only we could derive DNA from the Petralona and Monte Circeo hominins! Arago, anyone? Alas! The replationist fossilised literary orthodoxy served as the primary explanation of *human origins*, not a hypothesis, but as virtually received wisdom; for decades it has been presented as 'the Truth', axiomatically. It was simply a given. Recall the nasty barbs and arrows that flew between Wolfpoff and Stringer for years regarding

this very subject.

Recall the mockery that many multiregionalists received. Recall how Don Johanson, Curt Marean and Chris Henshilwood navel-gazed at their fossil hominin, clamshells and scratched ochre piece in the big-budget (funded by the Koch oil barons) PBS Nova series 'Becoming human'. Recall their mighty opining. In that recent but now obsolete miniseries Henshilwood and Marean present their interpretations of 'behavioural modernity' as objective facts, veritable *fossils directeurs* using words such as 'we know ...' as opposed to 'we think ...'. Or even the more scientifically-appropriate 'we hypothesise'. That is not science. There is a difference, in terms of tone, tenor, but also in terms of egoism and narcissism and our students (at least, for those of us who are fortunate enough to teach, to reference another of Tattersall's weak slaps) deserve better. I know some of those familiar refrains and themes earn one a seat at the High Table of Really Serious People and tend to impress other grandees who review grant proposals and tenure reviews, but let us not pretend that 'that is just the way science gets written'. Another set of Really Serious People also thought it beyond question that the US should invade Iraq based on mere vapours and rumours, and a lot of others also think that measured scientific discourse = whatever they happen to think and deign to share with the rest of us mere mortals. Dr Tattersall is wedded to the upended replacement model, and he *chose* that particular bride all by himself and published many flattering pieces in her honour.

Such things are now moot, since genetics has established the interfertility of *Homo antecessor*, Neanderthals, Denisovans, contemporary humans, and even something else underlying all of those, perhaps *Homo erectus* (Reich et al. 2010; Sankararaman et al. 2014; Vernot and Akey 2014). Interfertility indicates that these and other various human fossil identities are essentially bogus taxonomic categories, heuristic devices organised for our own benefit as opposed to factual constructs indicative of shared human ancestry. This is where Tattersall could have joined us, especially in discussing how bogus human fossil categories reflect bogus, nearly meaningless lithic categories. His voice would have been very welcome and particularly powerful in those regards. It seems Weidenreich had it basically right so long ago, but think how many awesome, creative, symbolic, self-elected modern human grandees would have failed to get their own keys to the kingdom if we would have been content to stop there with Good Ole Franz. I am reminded, here, of the day that Strepsades went to the Phrontisterion on legitimate (to him, anyway) business only to be mocked and scorned by Socrates, carried in a basket, making the worse argument seem the better, blowing a squid-ink smokescreen to distract from the erosion occurring beneath his pedestal.

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