

disciplines as ethology, psychology, cognitive sciences and neurobiology. Archaeology has limited input here, and palaeoart provides at best the raw material. Cognitive archaeology is thus not a sub-discipline of archaeology; it needs to be quite deliberately interdisciplinary. Hodgson and Helvenston demonstrate this with authority in their well-crafted paper. I find it particularly impressive how they follow up early pointers, develop them and provide them with neurophysiological substantiation or depth, showing elegantly how questions of human evolution can be usefully addressed via the cognitive sciences. For instance they develop my proposal of twenty years ago, that 'visual misinterpretation would have favoured objects that dominated the taxonomic visual system of hominins, namely those that provoked desire and fear, i.e. large mammals'. Another such key statement in this paper that illustrates the kind of direction we need to take is this:

As LeDoux (1994) makes clear, certain key features may be enough to trigger an emotional response by way of the amygdala before an object, such as a dangerous animal, is consciously recognised. Thus, there seems to be an early warning system that proceeds directly from the retinal-thalamic visual pathway to the limbic system for the rapid discrimination of potentially threatening objects that primes conscious awareness for action if required, and over the course of hominin evolution these abilities would have been highly adaptive (p. 7).

Clearly, the main strength of this paper is its use of what we have learned in recent years about the operation of the human brain. Another strong point is the emphasis on the role of animals (or, rather, the role of other animals) in the lives and preoccupations of people in pre-industrial societies. Whether these were hunters and gatherers or herders, the importance animals played in their lives is hard to comprehend from outside their cognitive realities. As an example one might consider the Nuer, the Nilotic people whose daily life revolves entirely around their cattle, who have deep psychological bonds with them, and whose language includes a huge vocabulary defining nuances of appearance and character in the cattle. They lavish more care on the appearance of their stock animals than a film actress does on her make-up. The many expressions of kinship hunters feel for their quarry have often been documented (and they are very tangible in totemic systems), as has been the universal ability of hunter-fisher-foragers to empathise with and mimic animals. It is therefore easy to agree with the authors of this fine paper that the importance of animals to the people who produced the world's rock art can hardly be overstated. This is obvious enough, surely, from the iconic content of much rock art. Moreover, large mammals have been the principal source of food in Pleistocene Europe since the times of *Homo heidelbergensis*, much in the same way as for modern-day Inuit. The cognitive reality of such peoples must be viewed from that perspective. The late 'Neanderthals' (and I use this term without endorsing it) need to be assumed to have had a preoccupation with 'dangerous animals' (cave bears and lions, pachyderms), expressed in several cultural forms, and their therianthropes may result from this (it should be noted, conversely, that there are now two Aurignacian lion-headed therianthropes from the Swabian Alb of south-western

Cognitive archaeology and cognitive sciences

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This important paper demonstrates that the issues of human cognitive origins are the domain primarily of such

Germany: the possibly female, large specimen from Hohlenstein-Stadel (Schmid 1989) and the much smaller one from Hohle Fels (Conard et al. 2003).

A few minor points in the paper require clarification. It has not been demonstrated that the Makapansgat cobble (which is of jasperite, not ironstone; Bednarik 1998) was carried 'over 20 miles from its place of origin'. Certainly it was carried into the dolomite cave for some kilometres and is clearly a manuport, but Dart's original claim of 32 km is not valid. If the cobble were of ironstone, as has been said incorrectly, it could come from 4.8 km away, but it eroded in fact from an ancient conglomerate and could be from any fluvial deposit.

Concerning the Tan-Tan proto-figurine, I am not sure what Hodgson and Helvenston mean when they say that my 'interpretation' has not yet been confirmed by other investigators. After all, my paper (2003a) avoids simplistic interpretation quite explicitly. If they mean that the object's significance has not been appreciated by archaeologists who have examined it, they are certainly wrong. It was discovered by Professor Lutz Fiedler, a specialist of the Acheulian tradition, and it was examined by Martin Kuckenburger, among others. These scholars agree with me concerning its significance.

I have several problems with the statement that 'the only Neanderthal representational artefact (yet to be confirmed by other researchers) takes the form of two stones intentionally fitted together to look like a human or feline face dating to about 32 000 BP'. First, my unimportant objection: I do not think Marquet and Lorblanchet (2003) have presented a credible case that this is an iconic 'artefact'. There is no indication of any modification, nor is intentionality apparent or even demonstrable. Literally thousands of such supposedly early stone 'sculptures' of purported Lower and Middle Palaeolithic ages have been reported from France to northern Germany, a massive amount of literature exists about them, and archaeologists consistently reject this material. Many of these stones bear more obvious resemblances to objects (usually animals and human faces) than does the stone from Le Roche-Cotard, hence I fail to see its relevance.

My more important objections, however, are more complex. The cited comment suggests that the authors believe no other iconic material is the work of 'Neanderthals'. This is firstly false, and secondly a premature assumption. It is almost certainly false because one of the three Micoquian engravings from Oldisleben appears to be iconic (Bednarik 2006a), and it is premature because the authors cannot know which of the indisputably iconic artefacts of the Early Upper Palaeolithic (EUP) were made or not made by Neanderthals. Unless they have proof, one way or the other, they need to withdraw this point.

This is not a pedantic observation on my part, it is a very deliberate measure to demonstrate that the archaeological paradigm within which we conduct debates of this nature is itself largely false. For instance, we have no evidence whatsoever that Aurignacian art was not made by 'Neanderthals'. All the unambiguous human fossil evidence we do have from EUP sites is that these occupations were

by 'Neanderthals' (currently five instances). We have no unambiguous 'Graciles' from any clear Aurignacian context, and there is no evidence of gracile trends before the Peștera cu Oase mandible. All the known European hominins between 35 ka and 25 ka are either 'Neanderthals', or what should be called 'post-Neanderthals' or 'Robusts' — people of distinctive sexual dimorphism, whose males were almost as robust as 'Neanderthals', while the females were far more gracile, preceding in physical evolution the males by many millennia. In other words, there is rapid gracilisation, which is a global trend at about that time, emphasising continuity and uniform evolutionary processes from Europe to Australia. Moreover, precisely the same applies to all tool traditions ever identified in the EUP of Europe, such as the Aurignacian, Châtelperronian, Uluzzian, Uluzzo-Aurignacian, Proto-Aurignacian, Olshewian, Bachokirian, Bohunician, Spitsyn culture, Szeletian, Jankovician, Streletsian, Altmühlian, Lincombian or Jerzmanovician. All of them evolved locally, and to the best of our current knowledge they were all the work of robust people, including those usually pigeonholed as 'Neanderthals'. Fully gracile humans do not occur in Europe until the Late Upper Palaeolithic, and even they are still considerably more robust than those of the early Holocene, which in turn are more robust than those of the late Holocene. In short, there is no apparent intrusion of either a new tool industry from elsewhere, or of a sudden change in human morphology. The change from Robusts to Graciles is gradual and simply mirrors a universal development in all human populations of the second half of the Late Pleistocene. The 'replacement hypothesis' is a farce, based on the fake datings by R. Protsch and others, and on a long series of mistakes (e.g. concerning the four Stetten specimens, Hahnöfersand, Velika Pečina, or the Crô-Magnon and Mladeč samples; see Bednarik 2006b and work in press).

What concerns me is not just how the African Eve advocates overlooked all of this, but the fact that their fervour has discouraged the consideration of more important issues. For instance, there is the question: why would human evolution tolerate the unprecedented developments of the last fifty millennia? That period has witnessed humans evolving into inferior forms. Their brains shrank (despite increased demands made on them), as did their muscles, while their bone architecture became significantly more fragile. None of this makes any evolutionary sense. There is a rational explanation but it attracts no interest whatsoever. This is a result of the African Eve fad. Now that we face the very real possibility that Aurignacian palaeoart is the work of Neanderthaloids (Bednarik in prep.), it is high time to start considering much more likely scenarios than the replacement dogma. In particular, the model offered by Fedele and colleagues not only provides a realistic explanation of demography, it has the complete support of the archaeological evidence, which the replacement dogma has always lacked (Fedele et al. 2002, 2003). I have been warning for many years that we have no evidence of what kind of people the 'Aurignacians' were (e.g. Bednarik 1995), and now that so many fake datings of European hominin

finds have been exposed we have discovered that I merely understated the problem.

Archaeology is based on beliefs, science operates by falsification. The proposition that is currently on the table for falsification is that the EUP traditions *are not the work of 'fully modern' people*. Until it is falsified by presenting fully modern human remains (not intermediate or 'post-Neanderthal' Robusts) from a clear EUP context, the replacement model remains devoid of any archaeological or palaeoanthropological evidence. It is based purely on unresolved and dubious genetic claims (see Barinaga 1992; Templeton 1996; Brookfield 1997; Gyllensten et al. 1991; Kidd et al. 1996; Gutierrez et al. 2002; Strauss 1999 for some clarifications), i.e. it is without credible evidence. It never had any archaeological evidence to begin with, and all the palaeoanthropological evidence cited in its support is now exposed as either fake or mistake.

None of this has much effect on the authors' hypothesis, but it shows the inadequacies of the archaeological paradigm we start from: it should be assumed to be largely false. One of the many errors made by the 'short range' lobby has been its claim that to demonstrate symboling the evidence must be very numerous, because only often repeated use of symbols can demonstrate their use (Chase and Dibble 1987; Davidson and Noble 1998 and subsequent debates). This has been that lobby's standard response whenever I confronted it with pre-Aurignacian palaeoart evidence (e.g. Bednarik 1992) — as if a pre-Aurignacian status even mattered if the Aurignacian is by Robusts, as it apparently is. The work of Merlin Donald, mentioned by Hodgson and Helvenston, demonstrates the falsity of the cited belief: it is impossible for symbol use to exist in isolation, or as a 'running ahead of time' (Vishnyatsky 1994). One of the several reasons for this is that symbols are a form of memory storage external to the brain. Not only do they indicate cognitive 'modernity', a single instance suffices to demonstrate such 'modernity'. In that sense, Donald's work is invaluable, even if the lack of relevant data available to him has limited the utility of his sequence, and particularly its timing. If Donald had had the benefit of a catalogue of relevant material (Bednarik 2003b), his chronology would have been somewhat different. This may detract from the details of his model, but it does not affect the validity of his central thesis of external storage. Hodgson and Helvenston have made good use of this idea, and they have made equally good use of neurophysiology. The result is a superb paper pointing the discipline in what I have long regarded as the right direction.

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