

Letters to the Editor

A Forum for Commentary on Articles and Research Issues

Handaxes and Sexual Selection: A Reply to Nowell and Chang

ABSTRACT

Nowell and Chang's (2009) article in *PaleoAnthropology* provided a critique of the theory that sexual selection may explain some of the variability in handaxe morphology, as proposed by Kohn and Mithen (1999) and described in Mithen (2005). While debate about such a theory is to be welcome, Nowell and Chang seriously misrepresent the arguments forwarded by Kohn and Mithen. This short paper specifies where such misrepresentation has occurred.

DELIGHT TURNING TO DISMAY

I was delighted when I learned that *PaleoAnthropology* 2009 (pp. 77–88) had an article entitled 'The Case Against Sexual Selection as an Explanation of Handaxe Morphology' by April Nowell and Melanie Lee Chang (2009). The abstract indicated, as I had hoped, that their paper would provide a critique of the theory and archaeological evidence that Marek Kohn and I had forwarded within an article published a decade ago (Kohn and Mithen 1999). Advancement of knowledge depends upon vigorous academic debate and the worst fate for any theory is to be ignored. But I quickly became dismayed when I began to read Nowell and Chang's article to discover that they relied on an appalling misrepresentation of our arguments. This short note is not to continue the debate regarding the applicability of sexual selection to handaxes (e.g., see Hodgson 2009; Machin 2008; Mithen 2008), but simply to put on record that Nowell and Chang have provided an inaccurate characterization of the arguments that Kohn and Mithen (1999, Mithen 2005) put forward. As such, the academic credibility of their own paper is severely diminished. Having provided an entirely misleading representation of our work, one can have limited confidence in how they represent the work of other authors cited within their paper.

Nowell and Chang begin (p. 77) by stating that Kohn and Mithen have proposed an 'evocative and romantic' theory explaining the morphology of Acheulian handaxes. That is their phrase, not ours: I have no idea what is 'romantic' about sexual selection; our arguments were written in as plain and un-evocative language as possible. By describing it as 'romantic and evocative' they immediately seek to discredit its academic basis in the mind of the reader.

Nowell and Chang continue by claiming that we posed "five fundamental questions that a theory of handaxe morphology needs to answer" (p. 78). That is quite different to what we actually wrote, which simply referred to "five fundamental questions that remain unanswered" (Kohn and Mithen 1999, 518). We later (p. 524) suggested that our

theory of sexual selection as applied to handaxes did indeed help to address these questions. But that is quite different from the position that Nowell and Chang attributes to us—there is no need for one theory to necessarily provide answers to every one of these questions.

The next inaccuracy from Nowell and Chang comes a few paragraphs later when they state that "Mithen (2005) argued that the overwhelming majority of handaxes are symmetrical" (p. 78). This claim is repeated on pages 36 and 37 of their article. Well, I have read and re-read my own publications and I cannot find any such argument. What I can find is a statement that says "many [handaxes] have a very high degree of symmetry" (Mithen 2005, 188) and that "the archaeological evidence shows the entire range, from handaxes in which symmetry is hardly present at all to those that are practically perfect in all three dimensions" (Mithen 2005, 188). Kohn and Mithen (1999, 521) argued that such a range of morphological variation was essential to their theory: "critical to this argument is the wide range of variability found in artefacts categorized as handaxes ...which is essential for selection. Axes range from classic symmetrical forms to non-classic asymmetrical handaxes." In light of such statements within our work, I fail to see how Nowell and Chang (2009: 82) can then claim that "by focusing exclusively on classic, teardrop shaped artifacts, Mithen is artificially isolating types from what appears to be a continuous variation in handaxe morphology." We have done the precise opposite.

Such inaccurate characterizations of our work continue. The next is when Nowell and Chang state that "the accumulations of handaxes seen at some sites may be the result of display arenas (essentially, leks)" (p. 78), a sentence which is then transposed a few pages later to read "he [Mithen] argues that there are large accumulations of handaxes at archaeological sites because these sites were essentially hominin leks (or display arenas)" (p. 81). Again, I have read and re-read my own publications and I cannot find any reference to either 'display arenas' or 'leks.' All that we said was that "to fulfil their social function, members of the opposite sex had to witness the act of handaxe manufacture" (Kohn and Mithen 1999, 524). Such observation could happen in the context of making and using handaxes for a variety of tasks without any deliberate display.

That handaxes were indeed made and used for multiple functions was explicitly stated in our work, the social function of indicating 'good genes' being just one additional factor for archaeologists to consider. Kohn and Mithen (1999, 521) wrote that: "We propose that handaxes functioned not just to butcher animals or process plants but as Zahavian handicaps, indicating 'good genes'." In conclusion to our article we stated that "A complete explanation

for the form and distribution of handaxes in the archaeological record will require many factors to be invoked. The nature and distribution of raw materials was no doubt a major influence on their form, and handaxes were clearly efficient butchery implements" (Kohn and Mithen 1999, 524). Nowell and Chang, however, state that Kohn and Mithen "allege that *Homo erectus* (sensu lato) males made handaxes primarily to attract females for mating purposes" (p. 78) and that they "argue that males were making vast numbers of these artifacts to advertise their qualities as potential mates" (p. 81).

The level of mismatch between what Nowell and Chang claim was argued by Kohn and Mithen and what is actually present in our publications leads to two possible conclusions. One possibility is that Nowell and Chang have read neither Kohn and Mithen (1999) nor Mithen (2005) and relied instead on second hand characterizations of our work, either in the references they cited where it may have also been mis-quoted or in the popular press where crude characterizations are the norm. The other possibility is that they have deliberately misrepresented our work to set up a straw man to knock down. Either way, their work falls

short of what is acceptable academic scholarship.

REFERENCES

- Hodgson, D. 2009. Symmetry and humans: reply to Mithen's sexy handaxe hypothesis. *Antiquity* 83, 195–198.
- Kohn, M. and Mithen, S.J. 1999. Handaxes: products of sexual selection? *Antiquity* 73, 518–526.
- Machin, A. 2008. Why handaxes just aren't that sexy. *Antiquity* 82, 761–766.
- Mithen, S.J. 2005. *The Singing Neanderthals: the Origins of Music, Language, Body and Mind*. Weidenfeld and Nicolson, London.
- Mithen, S. 2008. 'Whatever turns you on': a reply to Machin. *Antiquity* 82, 766–769.
- Nowell, A. and Chang, M.L. 2009. The case against sexual selection as an explanation of handaxe morphology. *PaleoAnthropology* 2009, 77–88.

Steven Mithen

Faculty of Science, University of Reading, Whiteknights, PO Box 220, Reading RG6 6AF, Berkshire, UK; S.j.mithen@rdg.ac.uk