

ceived higher ratings than the same stimuli that had diagonal symmetry. This finding suggests that perceptual fluency, and not symmetry per se, influences liking. Taken together, preference for symmetry seems to be part of a broader preference for fluent processing of incoming stimuli. Processing fluency is hedonically marked because it indicates a positive state of affairs in the interaction with the environment. For example, processing fluency may signal that a stimulus has been encountered before and is thus familiar, or that ongoing cognitive processing progresses toward successful recognition and interpretation of the target (see Winckelman et al. 2003).

If *Homo erectus* began to craft symmetric tools because of a perceptual preference, the question arises whether the change of form from Mode 1 technologies to symmetric tools might reflect a change in preference during the same time period. Is it possible that preference for symmetry evolved between the emergence of Mode 1 technology and the first appearance of symmetry in tools? This seems unlikely to be the case because preference for symmetry has been observed in a wide variety of species, including insects (Giurfa et al. 1996; Møller 1995), fishes (Morris 1998), birds (Møller 1992), primates (Rensch 1957), and humans (Gangestad et al. 1994; Humphrey 1997; Rhodes et al. 1998). This suggests that preference for symmetry had evolved before humans emerged. Moreover, Rensch (1957) has found that primates prefer symmetry in nonmating contexts, suggesting that this preference is present in all primates and thus existed in *Homo erectus* before they began to form symmetric stone tools.

Finally, the view that *Homo erectus* formed symmetric tools because they found them beautiful does not entail the necessity that they had any intention to produce symbolic art. Preference for symmetry is a more basic affective reaction than an evaluation based on aesthetic or symbolic value, as demonstrated by symmetry preferences in subhuman species that have no inclination for symbolic art.

Putting meat on the bones: The necessity of empirical tests of hypotheses about cognitive evolution.

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Abstract: Reconstructing the evolution of cognition requires maximal extraction of information from very sparse data. The role that archaeology plays in this process is important, but strong empirical tests of plausible hypotheses are absolutely critical. Quantitative measures of symmetry must be devised, a much deeper understanding of nonhuman primate spatial cognition is needed, and a better understanding of brain/behavior relationships across species is necessary to properly ground these hypotheses.

Understanding how we came to be the creatures we are is a fascinating and important topic. Wynn believes archaeology should play a critical role in this process. While it is easy to be sympathetic with this view, there are some very real problems – some of which are acknowledged by Wynn – that have to be addressed before this can happen. A lot more can and should be said about how to empirically address the issues Wynn raises.

First, the whole concept of symmetry in stone tools needs to be clarified. Although Wynn notes that there is controversy over the extent to which symmetry in certain kinds of stone tools is real and intended, he nevertheless argues that it does in fact exist and was in fact intended, but then offers little to support this contention. Wynn states that bifaces “do not have the best symmetry, but the economy of means by which the symmetry was achieved reveals that some idea of mirroring must have guided the knapper” (sect.

2.4.1). But how do we judge economy of means, and why is a concept of mirroring necessary to achieve that particular level of symmetry? More fundamentally, how do we compare different levels of symmetry, or determine whether a particular artifact shows a level of symmetry greater than what we would expect from purely random processes? At present, we must rely on the judgments of experienced knappers. However, given that there is controversy even among cognitive archaeologists over these issues, symmetry in lithic artifacts needs to be formalized in some quantitative manner to allow for empirical testing. Dibble and Chase (1981) suggested a measure of symmetry for flakes (the “angle of symmetry”). A version of this measure adapted to bifaces would certainly be a step in the right direction, but an even more comprehensive measure might use radial measurements such as those taken by Wynn and Tierson (1990). A possible metric of symmetry could be obtained by squaring the differences between pairs of corresponding left and right radial measurements (at equal degrees of divergence from the center), summing these squared differences across all corresponding pairs, and finally dividing by the number of pairs of radial measurements. This would provide a single number: the average squared deviation from perfect symmetry across all the different corresponding left and right side pairs of measurements for an individual artifact. Means and standard deviations for this measure could then be obtained for entire assemblages. Different sites (or different time periods) could then easily be compared statistically. This would allow for empirical tests of increasing symmetry. Other types of measures would have to be devised to quantify things such as the “S-twist” in some handaxes, but in principle this is possible, and is really the only way to resolve disputes over symmetry.

Wynn argues that “The mirrored sides [of bifaces] are not just qualitative reversals, but quantitative duplicates, at least to the degree that this is possible given the constraints of stone knapping” (sect. 2.5.1). How do we evaluate empirically how close the bifaces are to the theoretical limit of symmetry given the material and the techniques used? This would be possible to assess, once a quantitative measure of symmetry is decided upon. One could use the most symmetrical bifaces produced by expert knappers (using the same techniques and materials used by hominids) as the “gold standard” against which to compare the degree of symmetry in bifaces at archaeological sites.

A deeper problem remains, however. To what extent is biface shape the goal of the stone tool maker, instead of being simply the unintended side effect of a reduction sequence? McPherron (2000) argues that bifaces were likely modified with use, and that this very fact challenges the idea that the shapes we see today are the desired end-product. He also suggests that geographic differences in artifact shape are more simply explained as being differences in degrees of artifact reduction, rather than differences in underlying mental templates of the tool makers. Similarly, Dibble and colleagues (Dibble 1987; Rolland & Dibble 1990) have long argued that a large part of the variability in Middle Paleolithic tool assemblages is best explained by different degrees of artifact reduction. If such factors strongly affect shape differences between sites and across time, then what we are seeing is not changes in mental concepts, but rather changes in how hominids made use of various resources available in their environments. However, if it can be shown that repeated lithic reduction – driven only by utilitarian usefulness – typically results in statistically significantly less symmetry than that shown among the gold standards produced by expert knappers, then there might be a way to demonstrate that some sort of mental template was involved in the production of assemblages of bifaces: that is, determine whether the degree of symmetry of archaeological artifacts exceeds that shown in the utilitarian models.

Another area in need of empirical testing concerns exactly what nonhuman apes can and cannot perceive and produce with respect to symmetry. The target article focuses on what kinds of behaviors apes demonstrate naturally, rather than the equally important question of what they can be trained to do. When an

animal does not demonstrate a particular behavior in its natural environment, this does not constitute evidence that the animal lacks the cognitive requisites to perform that behavior. A clear example of this can be seen in the abilities of captive trained apes to understand and manipulate arbitrary signs (Gardner & Gardner 1984; Premack & Premack 1972; Savage-Rumbaugh et al. 1993). Whatever one believes these studies say about language abilities, at the very least these apes are doing quite sophisticated things that they don't show in the wild. Why don't they? Do they lack some fundamental cognitive abilities that humans have? A simpler argument is that their natural environment just doesn't provide the appropriate rewards for learning such obscure behaviors. Showing that apes do not demonstrate some behavior in the wild actually tells us very little about the cognitive differences between them and us. We need detailed studies of what apes can be trained to do. Is it the case that apes can create symmetrical objects if properly motivated? If so, then any neurocognitive evolution that has occurred (if any) would have been in the motivation system, and not in specialized spatial abilities.

However, even if an animal does not demonstrate some behavior in a lab experiment after extensive training, this does not constitute proof that the animal lacks the cognitive capacity for that ability (Essock-Vitale & Seyfarth 1986). An animal might fail to demonstrate some cognitive ability because the experiment did not provide appropriate motivation. A clear example of this is the case of cross-modal perception, which is the ability to integrate information from different modalities (e.g., vision, hearing, touch, etc.) about a single object. Cross-modal perception is thought to be crucial for language, because concepts are brought to life purely through a single modality (the auditory channel for most people). It was once thought – on the basis of studies of experiments on captive primates – that monkeys lacked the neurocognitive circuits underlying cross-modal perception. It turned out, however, that when an appropriately designed study was constructed, monkeys did show cross-modal perception (Cowley & Weiskrantz 1975). The point here is that we cannot rely too heavily on limited data concerning ape cognitive abilities when trying to reconstruct hominid cognitive evolution. Apes may well be different in the spatial abilities they exhibit, but exactly what (if any) neurocognitive differences underlie any such differences in behavior is entirely guesswork at this point.

Wynn argues that stone knappers as far back as 500,000 years ago had “an intuitive Euclidean concept of space,” and further that even though “we and other primates clearly perceive dimensional space, it is quite another thing to employ cognitive mechanisms that *understand* space this way, and which can be used to organize action” (sect. 2.5.2, my emphasis). But what exactly does it mean to “understand” space in a Euclidean manner? How *does* this differ from how other primates understand space? On what basis should we accept that primates really are different from these hominid toolmakers in this regard? This contention is central to the argument, but it is not clear how it can be supported. Wynn also argues that

These handaxes were almost certainly categories, and categories are abstract, multi-modal, and rely on associative memory. As such they reside in declarative memory, which “requires associative links between several types of information that are stored in different areas” (Ungerleider 1995, p. 773). (target article, sect. 2.5.2)

This description, while quite reasonable, simply does not obviously differentiate humans from apes (and suggests that a key aspect of cognition necessary for language predated hominids).

The behavioral abilities of early hominids is of course central to Wynn's argument, and here he uses one specimen – Nariokotome – to characterize important aspects of an entire fossil species. Unfortunately, the Nariokotome skeleton has a number of anatomical abnormalities suggestive of axial dysplasia, which would mean, for example, that its vertebral foramen should not be considered representative of *Homo erectus* (Latimer & Ohman 2001). Wynn notes that, “While the brain size of Nariokotome was larger than

earlier hominids, so was his body size; there was only a small increase in relative brain size (compared to, say, *Homo habilis*)” (sect. 3.2.1.1). This reflects long-standing but poorly supported assumptions about size/function relationships, which take relative brain size to be the most behaviorally relevant variable (see e.g., Wood & Collard 1999). It is, however, not at all clear that this is the case, and in fact a good argument can be made that *absolute* brain size is in fact the most behaviorally relevant variable (Schoenemann 2001). Studies actually suggest that absolute brain size is more closely associated with behavioral differences between species than relative brain size (Passingham 1975; Riddell & Corl 1977; Rumbaugh 1997; Schoenemann 2000). Thus, to the extent that brain size means anything (and it must mean something, otherwise it wouldn't have increased so dramatically in hominids), it really is not correct to suggest that the early *Homo erectus* was cognitively little different than earlier hominids.

Exactly how the question of brain size is relevant to spatial skills necessary for producing symmetrical handaxes is not clear. However, there is a huge amount of variation in brain size in modern humans, and if the change in brain size had anything to do with spatial processing, one might be able to find evidence of it in the form of a correlation between brain size and spatial ability among individuals today. My own study on this question did not show any detectable relationship between brain size (as measured with MRI) and a version of the widely used “mental rotation” task (Schoenemann et al. 2000). It is possible that a statistically significant association would be apparent, given a much larger sample size, but in any case the relationship appears to be very weak. It remains to be seen whether the spatial task correlates strongly with some subset of the cortex (e.g., parietal regions known to mediate spatial processing of various kinds). If so, one could then ask whether there is any way to detect evidence of evolutionary changes in this region from the study of fossil endocasts. At this point, we don't know enough to make any definitive statements.

I would also take issue with the claim that there is “no compelling evidence for hunting” in *Homo erectus*. Isotope analysis suggests that early hominids (including *Homo erectus*) regularly ate meat (Lee-Thorp et al. 2000). Furthermore, chimpanzees hunt fairly extensively (Boesch & Boesch 1989; Stanford 1996). It is therefore highly unlikely that *Homo erectus* did not also hunt at least some of the time.

The idea that symmetry of tool manufacturing would be a sexually selected trait (because it correlated with better genes, or greater likelihood of survival of offspring) is extremely speculative. Nevertheless, it is potentially testable. There are modern hunter-gatherer groups in which one could ask whether tool-making prowess is in fact rewarded in some manner (reproductively or otherwise). To make the idea of the sexual selection for symmetry in stone tool making to be more than speculation, such studies must be done.

Lastly, it should be pointed out that there is a large sex difference in the mental rotation spatial tests that Wynn discusses. In fact, it is one of the largest cognitive ability differences that have been found (averaging ~1 standard deviation difference favoring males, Linn & Petersen 1985). What are the implications of this for human cognitive evolution? If mental rotation ability is critical for symmetrical stone tool manufacturing, does this mean that males likely were the stone tool manufacturers? Do people who score higher on mental rotation tests learn stone tool manufacturing faster and easier than those who score lower? These are the kinds of empirical tests that need to be done to put some meat in the arguments about human cognitive evolution.