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# Symmetry in knapped stones is real, not romanced

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# Archaeology and cognitive evolution

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**Abstract:** Archaeology can provide two bodies of information relevant to the understanding of the evolution of human cognition – the timing of developments, and the evolutionary context of these developments. The challenge is methodological. Archaeology must document attributes that have direct implications for underlying cognitive mechanisms. One example of such a cognitive archaeology is found in spatial cognition. The archaeological record documents an evolutionary sequence that begins with ape-equivalent spatial abilities 2.5 million years ago and ends with the appearance of modern abilities in the still remote past of 400,000 years ago. The timing of these developments reveals two major episodes in the evolution in spatial ability, one, 1.5 million years ago and the other, one million years later. The two episodes of development in spatial cognition had very different evolutionary contexts. The first was associated with the shift to an open country adaptive niche that occurred early in the time range of *Homo erectus.* The second was associated with no clear adaptive shift, though it does appear to have coincided with the invasion of more hostile environments and the appearance of systematic hunting of large mammals. Neither, however, occurred in a context of modern hunting and gathering.

**Keywords:** Archaeology, evolution, *Homo erectus,* spatial cognition, symmetry

## 1. Introduction

This article has two goals. The first is to make a case for the relevance of archaeological contributions to studies of the evolution of cognition. The second is to provide an example of one such contribution, a reconstruction of aspects of early hominid spatial cognition based on an analysis of artifactual symmetries. Assuming that human evolution is relevant to understanding the human condition (an intellectual position that is at the core of biological, if not yet psychological, approaches to behavior), then archaeology can supply two important bodies of evidence: (1) actual timing of developments, and (2) the evolutionary context of these developments. The challenge is not epistemological; archaeology can and does supply these things to the study of human evolution in general. The challenge is methodological. How can archaeology inform us about the evolution of mind? (See Gowlett 1979; Holloway 1969; Parker & Gibson 1979 for seminal contributions in the same vein.)

Archaeology is a set of methods for reconstructing past action from traces that exist in the present. These traces include objects made or modified by people in the past – tools, houses, ornaments, and so on – but also less tidy patterns like garbage and refuse of all kinds and evidence of past landscapes (through analysis of soils, pollen, faunal remains, etc.). Because some traces survive the ravages of time better than others, the archaeological record is a biased and nonrandom sample of past action. Stone tools, for example, survive well but wooden tools do not. Also, some environments preserve traces better than others. Tropical environments are poor preservers, but cold, dry, arctic environments are good preservers. Archaeology is an observational discipline. Unlike laboratory scientists, archaeologists cannot duplicate events, and unlike ethologists, archaeologists cannot depend on obtaining corroborating observations, though we certainly hope for them. There is a real element of serendipity in archaeology. Pompeii, the "Ice man," and the recent discovery of 400,000-year-old spears at Schoeningen (Thieme 1997) are unique and wonderfully informative, but they are atypical. The archaeological record boasts relatively few such treasures. Instead it consists largely of more incomplete and mundane traces that allow archaeologists to reconstruct some of what occurred in the past. The primary methodological task of the archaeologist is this reconstruction – translating traces into actions – and archaeology has developed a large body of concepts and techniques for doing this. We are very good at reconstructing diet from garbage, and social/political systems from the size, character, and location of settlements. Can there be an archaeology of cognition? This is in reality a two-part question. First, can traces of action inform us reliably about any aspect of cognition, and second, if so, can archaeologists overcome some rather serious methodological roadblocks inherent to the archaeological record of such traces?

One way that psychologists learn about the mind is by observing the actions of individuals in controlled laboratory settings or in natural situations. Sometimes these actions leave tangible traces that become the focus of the analysis. Children's drawing is one example; block shuffling tests are another. The methodological task of the psychologist is to translate the tangible results into meaningful characterizations of the mind that produced them. Of course, psychologists can also talk to their subjects, but in principle, psychology can and does analyze the traces of action. An archaeologist trying to do the same faces some additional

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hurdles. To make a convincing argument in cognitive archaeology, one must be able to identify specific features of the archaeological record that can inform about cognition in a valid and reliable way. This is the crux of the matter. Unfortunately, the disciplines of archaeology and psychology have never shared much in the way of theory and methodology. For an archaeologist to make a compelling case, he or she must not simply refer to a few selected psychological results. There must also be some understanding of the theoretical and methodological context of the research. With this in hand, the archaeologist can define a set of attributes that can be applied to the archaeological record. This definitional step is indispensable. On the one hand, it is very unlikely that variables taken directly from the psychological literature could be applied to archaeological remains. On the other hand, the traditional categories of archaeology are inappropriate, a point that bears emphasis. During the last century and a half archaeology has developed a large set of categories for the description of archaeological remains. Some of these categories are based on presumed function (e.g., ground stone axe or temple complex), some on presumed usefulness in temporal ordering (e.g., Iron Age), some on social complexity (e.g., "Classic" Mesoamerica), and so on. None, to my knowledge, has ever been defined with cognition in mind, and it would be misleading to use them as such (e.g., to argue that Iron Age people were cognitively different from Stone Age people). The cognitive archaeologist must avoid using these traditional categories and approach the archaeological record from the perspective of psychological theories and methods.

Even after careful definition, archaeology faces a number of roadblocks peculiar to its data. The first is preservation. Not only does preservation produce a biased record, it also presents a sliding scale of resolution. The farther back in time we look, the worse the record. There is less preserved, and there are fewer examples of what is preserved. This alone gives the archaeological record a misleadingly progressive appearance; 10,000-year-old remains appear more complex than 500,000-year-old remains partly (but not entirely) because we have so many more of them. The second caveat is logical. How can we be sure that archaeological remains are a reliable reflection of the cognitive abilities of past people? Might not these people have invested their cleverest thinking in domains that are archaeologically invisible? There is no infallible way around this problem. Archaeologists can only assess the minimum competence necessary to produce a particular pattern. Our only comfort comes from increasing the number and, especially, the variety of corroborating cases.

Finally, cognitive archaeology works best on an evolutionary scale of resolution. The ultimate achievement of cognitive archaeology would be to provide descriptions of the cognitive life-world of human antecedents at many points in evolution. Such descriptions would provide an evolutionary foundation for understanding the modern mind. I have long harbored the desire to provide a comprehensive account of the mind of *Homo erectus,* a very successful ancestor who was the immediate precursor of *Homo sapiens.* Surely, an understanding of *Homo erectus'* cognition would illuminate aspects of the modern mind; there must be much of *Homo erectus* with us still. Unfortunately, I do not think such a comprehensive description is possible, because the archaeological record is too incomplete.

Archaeology can take another approach to the question of evolution, an approach not focused on descriptions of individual antecedents, but one focused on long term patterns of change. Even though poor in detail, the archaeological record is very long, providing a quasi-continuous record of products of action that spans two million years. Archaeologists can use this record to identify patterns of cognitive evolution that provide insights into questions of modern cognitive science. What follows is an example of this approach. The focus is on spatial cognition (generally considered, including shape recognition and image manipulation). The evidence will consist of artifactual symmetry.

## 2. The archaeological record of artifactual symmetry

This article surveys the evolution of artifactual symmetry for three reasons. First, symmetry is a pattern and a concept that is recognized by everyone, which reduces the requirement for definition (but does not eliminate it entirely). Second, symmetry has been incorporated into many schemes of spatial cognitive development, and also into theories of perception, so that it provides a direct way to articulate the archaeological record with the cognitive science literature. Finally, it is amenable to visual presentation.

There are several different patterns to which we apply the term symmetry. The most familiar is *reflectional symmetry,* also known as bilateral or mirror symmetry. Here, one half of a pattern is duplicated and reversed on the opposite side. Implicit in the pattern is a central line, usually vertical, that divides the pattern into reflected versions of one another. Bilateral symmetry is "natural" in the sense that we see this pattern in the natural world of plants and animals. A second symmetry is *radial symmetry,* in which a pattern repeats not across a line, but continuously around a point. Similar to radial symmetry is *rotational symmetry,* in which a pattern is not reflected across a line, but rotated around a point; here the pattern is not reversed or inverted. Finally, there is *translational symmetry,* in which a pattern is repeated in a sequence, without reversal or rotation.

Symmetry is ubiquitous in the natural world and the cultural world. It is a well-known feature of crystal growth, resulting from the chemical structures of the molecules themselves. It also acts as a principle in biological growth and development, as in the symmetrical duplications of supernumerary appendages in beetles (Bateson 1972), where the source probably lies in the genes regulating growth. On a larger scale, symmetry is a feature of the overall body plans of many organisms, from microscopic foraminifera to large vertebrates. In human material culture, symmetry appears in the form of artifacts, buildings, and built environments all over the world. It is a central component of decorative systems in almost all human culture, and also a component of games (e.g., string games) and mathematical puzzles (e.g., tessellations). In many of these cases the symmetry results from the application of transformational rules; simple figures repeated and "moved" to produce intricate patterns. Symmetry is so fundamental in western culture, at least, that it is often a metaphor for balance and regularity (e.g., the symmetrical arrangement of keys in *The Marriage of Figaro*). Moreover, it is often endowed with meaning, carrying explicit and implicit information about fundamental values of a culture (Washburn and Crowe 1988)

Not surprisingly, perception of symmetry has been the focus of psychological research for more than a century (Wagemans 1996). It is now generally accepted, for example, that reflectional symmetry is perceptually more salient than translation and rotation. Indeed, some experimental work suggests that reflectional symmetry can be detected preattentively. Reflectional symmetry across a vertical axis is more salient than reflection across a horizontal axis, with oblique orientations falling a distant third. In addition to such empirical generalizations, there are competing theories of symmetry perception, and it remains a component of general theories of perception (Tyler 1996). It has even come to be a focus in evolutionary psychology, where detection of asymmetry is seen to be a means of mate assessment (Gangestad 1997). Given the ubiquity of reflectional symmetry in the natural world, and its correlation with successful ontogenetic development in many, many organisms, it is not at all surprising that perceptual systems should have evolved a sensitivity to symmetry. It is quite likely, then, that the perceptual saliency of symmetry is not a derived feature of human perception, but is one we share with many complex organisms. The degree to which it is shared, and whether it has evolved independently in several taxa or is instead a very old feature, are interesting questions, but tangential to the current discussion. The archaeological record does not document the development of symmetry perception per se. Instead, it documents the imposition of symmetry on material objects. Detecting symmetry is not sufficient for this task; other cognitive mechanisms must come into play. The importance of the archaeological record of symmetry lies not in the symmetry itself, but in what it reveals about these other mechanisms.

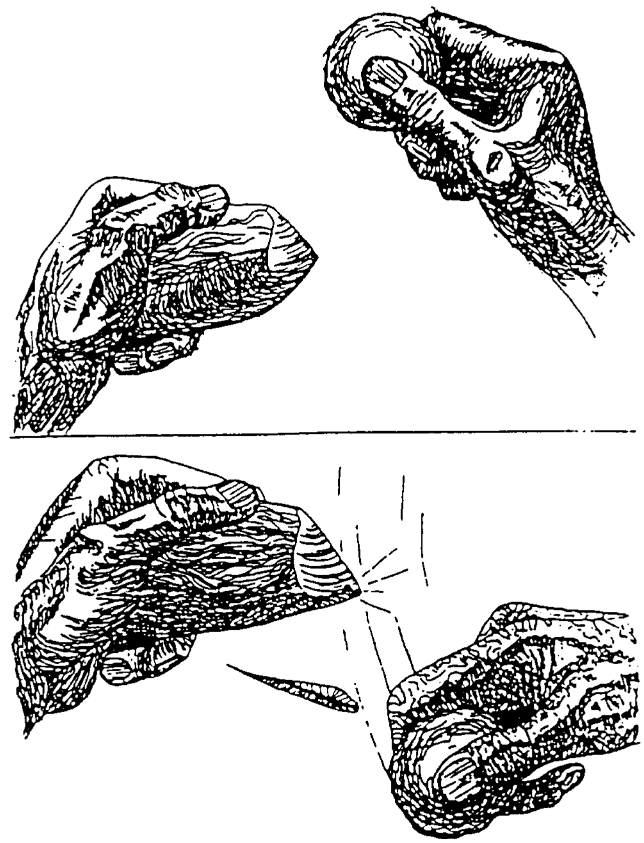

Figure 1. The basic action of stone knapping

### 2.1. Stone tools

Most of the following analysis will focus on stone tools. They are far and away the most abundant material evidence archaeologists possess for the major part of human evolution. The record of stone tools begins 2.5 million years ago and extends to the present. From the tools themselves archaeologists can reconstruct a variety of actions: raw material selectivity and procurement, manufacturing sequences, use, and discard. Archaeologists have been most interested in reconstructing the specific uses of stone tools, and the role these tools played in subsistence and, sometimes, social life. But these reconstructed actions, those of manufacturing in particular, can also document particular cognitive abilities.

Fracturing a stone produces sharp edges; this is the basic principle underlying almost all stone tools (Fig. 1). Archaeologists use the term "knapping" to refer to the stone fracturing process. In the simplest case a stone knapper uses one stone, termed a hammer, to strike another. If the knapper has struck with enough force, and delivered the blow to an appropriate spot at the appropriate angle, the receiving stone, termed a core, will break. In most instances the knapper must direct the blow toward the edge of the core because a blow landing toward the center is unlikely to deliver enough force to produce a fracture. This simple act of knapping results in two potentially useful products, a smaller, sharp-edged piece termed a flake, and the larger core, which now also may have sharp edges. Even this simplest of knapping actions requires directed blows. Randomly bashing two rocks together can produce useful flakes but even the earliest stone tools, 2.5 million years old, resulted from directed blows. The subsequent development of knapping technology included increases in the number of blows delivered to a single core, greater specificity in the location of blows, modification of flakes, longer sequences of action between the initial blows and the final blows, a greater variety of hammering techniques, and more regularly shaped final products.

Recently Stout et al. (2000) have conducted a pilot positron emission tomography (PET) study of basic stone knapping using an experienced knapper (Nick Toth) as the subject. The result showed highly significant activation in several brain regions. Much of this activation is what one would expect from performance of a complex motor task based on hand-eye coordination (primary motor and somatososensory areas, and cerebellar activity [p. 218]), but Stout et al. also recognize a significant cognitive component, implied by the activation of superior parietal lobes.

> The superior parietal lobe consists of what is referred to as "multi-modal association cortex" and is involved in the internal construction of a cohesive model of external space from diverse visual, tactile, and proprioceptive input. (p. 1220)

In other words, simple stone knapping is a "complex sensorimotor task with a spatial-cognitive component" (p. 1221). These results, though preliminary, situate most of the significant cognitive activation within the dorsal pathway of visual processing (Kosslyn 1999; Ungerleider 1995). The ventral pathway associated with object identification and shape recognition is minimally activated, implying that shape is not a significant component of the basic flaking task. These results are preliminary and need confirmation. There was only one subject, a skilled knapper, and the knapping task was brief and basic – removing flakes from a nodule (a Mode 1 procedure; see sect. 2.3.1). Nevertheless, it reinforces the work of cognitive archaeologists who have focused on spatial concepts borrowed from developmental psychology (Robson Brown 1993; Wynn 1979; 1981; 1985; 1989).

The directed action of stone knapping preserves something of the cognition of the knapper. Even in the simplest example, the knapper must make a decision about where to place a blow and how much force to use. These decisions are preserved in the products themselves. It is now common for archaeologists to refit cores by placing the flakes back together in a kind of three-dimensional jigsaw puzzle. Such a reconstruction permits archaeologists to describe in detail long sequences of action including specific location of blows, reorientation of the core by the knapper, and subsequent modification of flakes (Schlanger 1996). But even simple tools can be informative. The pattern of "negative scars" on cores or modified flakes preserves the sequences of blows. Archaeologists interested in cognition can use these preserved action sequences to investigate a variety of cognitive abilities, including sequencing, biomechanical skill, and spatial cognition. Even the simplest knapping required some notions of spatial relations and as stone tools became more complex they often preserved more complex understandings of spatial relationships.

There is a problem with intentionality. All stone tools have a shape, and this shape preserves spatial relationships, but how intentional were they? Here I do not mean the layers of intentionality invoked in theory of mind literature, but the simple question of whether or not a stone knapper intended to produce a particular shape. The basic action of stone knapping will produce useful results without the knapper intending the final core and flakes to have any specific appearance whatsoever. It is even possible for the iterative application of a specific flaking procedure to produce a final core with a regular shape, completely unintended. The shape itself, and the location and extent of modification producing the shape can often, but not always, document intention. For example, the artifact in Figure 5 has extensive trimming on one side that produces a "shoulder" mirroring a natural configuration on the opposite side. This is unlikely to have been an accident.

### 2.2. What about apes?

It is appropriate and traditional to begin discussions of human evolution with a discussion of modern apes. Much of our anatomy and behavior is shared with apes, including characteristics of the brain and cognition. A necessary first step in any evolutionary analysis is the identification of what is peculiarly human, for this allows correct focus of the undertaking. If modern apes, especially chimpanzees, employed all of the spatial abilities used by humans, then our evolutionary understanding must focus on the evolution of apes in general. It is also an axiom of paleoanthropology that human anatomy and behavior evolved out of those of an African ape, so that a description of this ancestor is a logical starting point for any summary. Our best information concerning this common ancestor comes from the living African apes (who, of course, have also evolved, but because their anatomy and habits appear more like those of a "general ape" than the anatomy and habits of the obviously unusual humans, they are a better candidate than ourselves).

Whatever the cognitive requirements of stone knapping are, they are within the abilities of apes, at least at the basic level of using a hammer to remove a flake. Nick Toth and Sue Savage-Rumbaugh have taught a bonobo to flake stone, and the results of their research help identify what might have been different about the cognition of the earliest stone knappers (Schick et al. 1999; Toth et al. 1993). Kanzi, a bonobo also known for his ability to understand spoken English and use signs, learned how to remove flakes from cores by observing a human knapper; he also learned to use the sharp flakes to cut through a cord that held shut a reward box. After observing the procedure, Kanzi perfected his technique by trial and error. It is interesting that his initial solution was not to copy the demonstrator's action, but to hurl the core onto a hard surface and then select the sharp pieces from the shattered remnants. He clearly understood the notion of breakage and its consequences. When experimenters padded the room, he then copied the hammering technique used by the knapper.

From this experiment (and an earlier one by Wright 1972), it is clear that fracturing stone is within the cognitive abilities of apes. Kanzi, however, is not as adept as human knappers. "(A)s yet he does not seem to have mastered the concept of searching for acute angles on cores from which to detach flakes efficiently, or intentionally using flake scars on one flake of a core as striking platforms for removing flakes from another face"(Toth et al. 1993, p. 89). These abilities are basic to modern knapping and, more telling, are evident in the tools made two million years ago. Toth et al. suggest that this represents a significant cognitive development, though they do not specify just what cognitive ability may have evolved. Elsewhere (Wynn et al. 1996) I have suggested that it may represent an evolutionary development in spatial visualization, which is the ability to discriminate patterns in complex backgrounds. If true, this would represent a minor cognitive development, of interest primarily because it is a cognitive ability tied to tool manufacture and use. Kanzi is also not very accurate in delivering blows, and this is harder to assess. It could simply be a matter of biomechanical constraint (i.e., he does not have the necessary motor control), or it could result from an inability to organize action on the small spatial field of the core. It is the organization of such action, fossilized as patterns of flake scars, that developed significantly during the two million years following the first appearance of stone tools.

Although apes can knap stone, they do not produce symmetries. The only possible example of symmetry produced by apes in the wild is that of the chimpanzee sleeping nest, which has a kind of radial symmetry that is produced when the individual reaches out from a central position and pulls branches inward. Here the symmetry is a direct consequence of a motor habit pattern, and one need not posit some idea of symmetry (Wynn & McGrew 1989). There are no other ethological examples, at least to my knowledge. There has been a significant amount of research with captive apes, however, especially chimpanzees, including a fascinating literature about chimpanzee art and drawing, from which one can examine the ways apes arrange elements in space.

Work with ape art has been of two kinds. In the first, researchers present an ape with appropriate media (finger paints, brushes and paint, etc.) and encourage it to create. In the second, researchers control the productions by supplying paper with predrawn patterns (Fig. 2). The former is the more "archaeological," in that researchers have not tried to coax particular pattern productions. Perhaps not surprisingly, these spontaneous productions are patterned

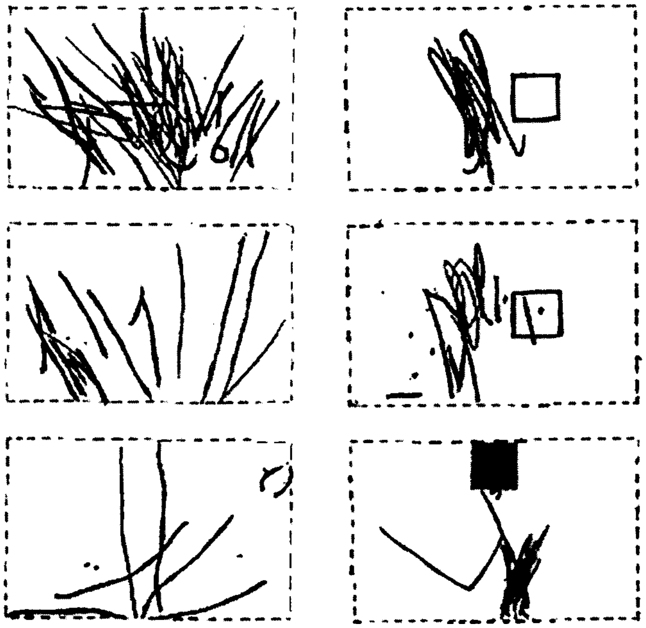

Figure 2. Chimpanzee drawings (Morris 1962). In 2A the fan patterns reflect the prevailing biomechanics of the task, a pull stroke. 2B exemplifies chimpanzees' tendency to fill spaces and mark on stimulus figures. A sense of balance is not necessary for any of these compositions, but cannot be ruled out.

primarily by motor patterns. Fan shapes are common, as are zig-zags produced by back and forth arm motion (Fig. 2A).

Desmond Morris (1962), the most well-known researcher in ape art, thought that these productions may demonstrate a sense of balance, and tried to coax it out with a series of experiments using sheets with already printed stimulus figures (Fig. 2B), following the earlier lead of Schiller (1951). Morris's work led to a number of subsequent experiments by others using similar techniques. The results have been enigmatic at best. Most chimpanzees presented with a figure that is offset from the center of the paper will mark on the opposite side, or on the figure itself (Fig. 2B). Morris suggested, cautiously, that this confirmed a notion of balance. Later Smith (1973) and Boysen (Boysen et al. 1987) confirmed these results, but argued that the pattern resulted from the chimpanzee's placing marks toward the center of the vacant space; balance was an accident.

It is hard to know what to make of this evidence. First, even with the few experimental subjects, there was a lot of individual variability. Indeed, each chimpanzee had an idiosyncratic approach to both the controlled and uncontrolled drawing. Second, most repetitive patterns resulted from repetitive motor actions. Nevertheless, the individuals did appear to place their marks nonrandomly, and did attend to features of the visual field. Other, nongraphic, experiments have indicated that chimpanzees can be taught to select the central element of a linear array (Rohles & Devine 1967), so chimpanzees can clearly perceive patterns in which balance is a component. But they do not appear able to produce symmetrical patterns.

### *2.3. Tools of early hominids*

**2.3.1. Description.** The earliest hominids left no archaeological record. Studies of blood chemistry and DNA indicate that humans and chimpanzees shared a common ancestor as recently as five million years ago. By four million years ago, the evolutionary split between hominids and the other African apes had occurred. There is fossil evidence for these early hominids, but it is fragmentary and more tantalizing than informative in regard to adaptive niches (Tattersall 2000). Between 4 million and 2.5 million years ago several different hominids lived in Africa. They differed from one another in habitat and adaptive niche, but shared the basic suite of hominid characteristics: bipedal locomotion, and relatively small canines and large molars. None had a particularly large brain (though slightly larger, relatively, than that of chimpanzees), and none left any archaeological traces. If any or all of these hominids made and used tools, as modern chimpanzees clearly do, then they have not been preserved. We can assume that tool use must have been in the repertoire of at least one of these hominids, only because it seems unlikely that stone tool manufacture could have developed without antecedents.

To date, the oldest reliably dated stone tools are 2.5 million years old (Harris 1983). These earliest stone tools exhibit no convincingly symmetrical patterns. Archaeologists assign these tools to a category termed "Oldowan," because of their first discovery at Olduvai Gorge in Tanzania. A better label was proposed several decades ago by Graham Clark (1977), who termed them a Mode 1 technology, a term based on technological characteristics, with no time-space implica-

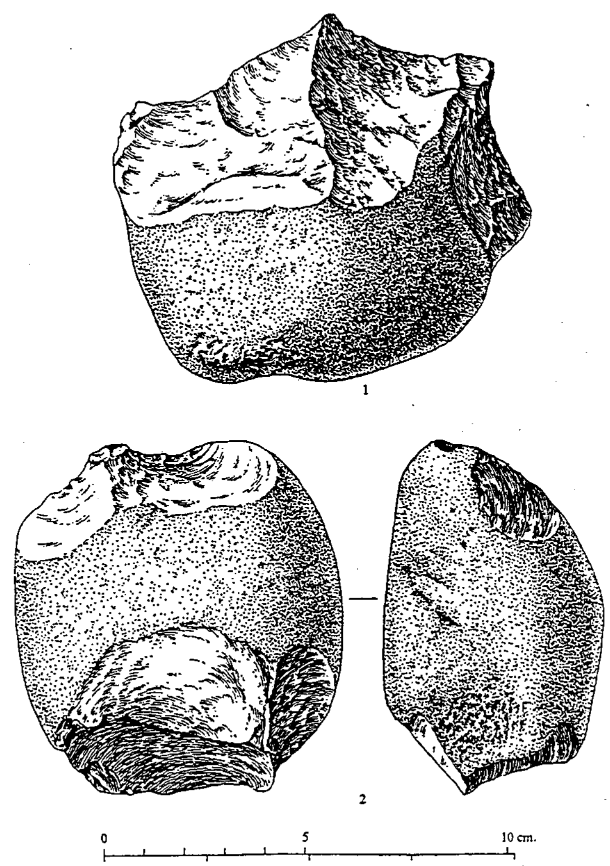


Figure 3. 1.8 million year-old stone tools from Olduvai Gorge, Tanzania (Leakey 1971). The knapper placed blows adjacent to earlier blows, but there is no reason to believe that the overall shape of the artifact was the result of intention.

tions (Fig. 3). Mode 1 tools first appeared about 2.5 million years ago in what is today Ethiopia, and were the only kind of stone technology in evidence for the next one million years. Following 1.5 million years ago, Mode 1 technologies continued to be produced in many areas and, indeed, were made into historic times. As such, Mode 1 represents a common, generic stone tool industry. It was also the earliest one.

The emphasis of Mode 1 toolmaking is on the edges (Toth 1985). Simple stone flakes can have very sharp edges, and are useful cutting tools without further modification. The cores from which the flakes were removed also have useful edges. These are not as sharp as the flakes, but the cores are usually heavier, and the result is a tool that can be used for chopping, crushing, and heavy cutting. Mode 1 tools exhibit little or no attention to the overall shape of the artifact. The only possible examples of a shaped tool occur in relatively late Oldowan assemblages, where there are a few flakes with trimmed projections (termed awls). Here a two-dimensional pattern of sorts has been imposed on the artifact, but it is a very "local" configuration, one that is tied to the nature of the edge itself.

**2.3.2. Cognitive implications.** The work of Stout et al., discussed earlier, supports an emphasis on the spatial cognition required by the basic kinds of stone knapping typical of these Mode 1 artifacts. Cognitive psychology supplies some more specific variables that are also applicable to the analysis of these early tools. Forty years ago Piaget and Inhelder (1967) introduced basic topological notions in their analysis of children's spatial ability, and these still have descriptive power. In particular, the relations of proximity, order, and boundary are all required for the placing of trimming on Mode 1 tools. More recently, Linn and Petersen (1986) have identified "spatial perception," the ability to detect features among complex backgrounds, as one of the four components of spatial cognition. This ability appears to be required when a knapper selects a platform with an appropriate angle for striking. What does not appear to be necessary for these tools is any kind of shape recognition or imagery. Basic flaking procedure and simple spatial relations are sufficient. The knappers imposed no overall shapes.

In this respect, at least, these early hominids were very ape-like. Indeed, when we expand our perspective to other features of toolmaking and use, we find that it was ape-like in most respects (Wynn & McGrew 1989). Yes, use of stone tools to butcher parts of animal carcasses obtained through scavenging was a novel component to the adaptation (Potts 1988; Schick & Toth 1993; Toth & Schick 1986), but at this point in hominid evolution it appears to have been merely a variant on the basic ape adaptive pattern, with no obvious leap in intellectual ability required. Indeed, there is no compelling archaeological reason to grant toolmaking any special place in the selective forces directing the first three million years of human cognitive evolution. But sometime after two million years ago the situation changed.

### 2.4. The first hominid imposed symmetry

**2.4.1. Description.** About 1.4 million years ago hominids in East Africa, presumably *Homo erectus*, began making large stone tools with an overall two-dimensional shape (Fig. 4). Many (but not all) of these "bifaces" were made by first detaching a very large flake from a boulder-sized core using a two-handed hammering technique (Jones 1981).

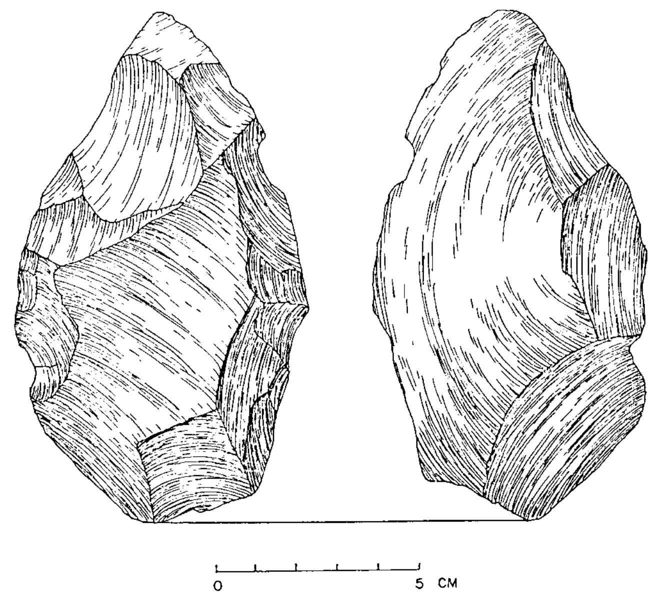


Figure 4. 1.4 million year-old handaxe from West Natron, Tanzania. The artifact has a "global" bilateral symmetry. The lateral edges mirror one another in quality of shape, but are not congruent.

The knapper then modified this large flake by trimming around the margins (usually onto both faces of the flake, hence the term biface). The uses to which these tools were put are unknown, though experimental evidence indicates that they can be effective butchery tools (Toth & Schick 1986). Archaeologists recognize two types of biface, the handaxe and the cleaver. Handaxes have a point or tip, and cleavers have a transverse "bit" that consists of an untrimmed portion of edge oriented perpendicular to the long axis of the tool. Both varieties of biface can have reflectional symmetry, and it is primarily this symmetry that produces the overall shape. Not all bifaces of this age are nicely symmetrical, however, and even the nicest examples look crude compared to the symmetry of later tools. Are we justified in attributing some kind of symmetry concept to the knapper?

Might not the symmetry lie only with the archaeologist, who interprets what was in no way intended by the knapper? This is a knotty problem that has become the center of an interesting debate among cognitive archaeologists (McPheron 2000; Noble & Davidson 1996). Most archaeologists, myself included, argue that the symmetry is real. First, the most symmetrical examples are also the most extensively trimmed, indicating that the knapper devoted more time to production. Second, and more telling, on some bifaces the trimming mirrors a natural shape on the opposite edge (Fig. 5). Such artifacts 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.

In addition to handaxes and cleavers, a third variety of biface occurs in low numbers in some sites in this time period. These are "discoids," so called because of their round shapes (Fig. 6). Like the other bifacial tools, the nicest, in this case the roundest, are also the most extensively modified. Here again we can recognize symmetry, in this case radial rather than reflection.

**2.4.2. Cognitive implications.** In most respects the cognitive requirements of these early bifaces were the same as

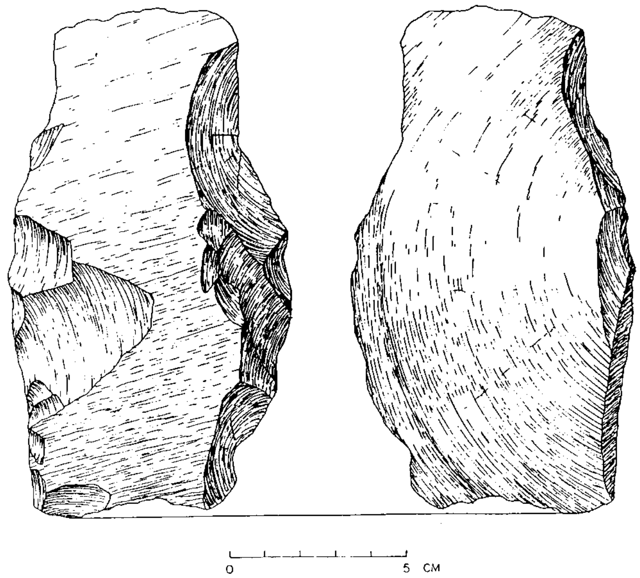


Figure 5. 1.4 million year-old cleaver from Olduvai. The position of negative flake scars indicates overall shape was probably intentional.

those of the earlier (and presumably antecedent) Mode 1 artifacts. But the symmetry presents a puzzle for cognitive interpretation. There are at least three possibilities:

1. The symmetry (and regular radii) are purely a consequence of a technique of manufacture using large flakes as blanks. The placement of trimming on some pieces argues against this, but the absence of congruency means that the symmetry is always crude and, for many archaeologists, unconvincing (Noble & Davidson 1996). Any cognitive significance would have to lie in the techniques of blank production. Although the two handed hammering technique (Jones 1981) clearly qualifies as an invention, its cognitive prerequisites seem no different from those of other direct percussion techniques used in Mode 1 technologies.

2. The symmetry was intended, but was not new. Rather, it is a pattern that is salient in the shape recognition repertoire of apes in general. What was new was the imposition of this shape on modified objects, something other apes never do.

3. Symmetry was a new acquisition in the shape recognition repertoire of these hominids and was applied to stone tools.

Conservatism inclines me toward the second possibility. But even if symmetry in pattern recognition is old, there was still a cognitively significant development associated with these bifaces. The stone knappers produced a symmetry by mirroring or reflecting the shape from one lateral edge onto the other. True, the edges are not exact mirrors. They are rarely if ever congruent in a modern geometric sense, but they are inversions of a two-dimensional shape. It is not even necessary that a particular overall shape had existed as an image prior to manufacture. The knapper could simply have mirrored one of the edges naturally provided him or her. In such a case the knapper would need to invert a shape. More significant, the knapper had to ignore part of the shape of the original large flake to impose a symmetrical edge. This is a kind of frame independence, the ability to see past the constraints imposed by a spatial array (Linn & Petersen 1986). The discoids suggest that the knappers were also able to employ a notion of spatial amount, in this case a diameter. The knappers trimmed the tool until all of the diameters were roughly equal. Although not an abstract quantity like an inch, a diameter is nevertheless a spatial amount, albeit local and limited. But what is most significant is that these biface knappers incorporated a shape component into the knapping problem. This shape component need not have been an abstract concept. It could simply have been shape recognition, matching to unimodal representation (Kosslyn 1999), in this case reflectional symmetry. Such a unimodal representation is almost certainly in the shape recognition repertoire of apes in general. What is significant here is its manifestation in the otherwise spatial task of knapping.

This new development required coordination of spatial abilities with a previously separate cognitive component (or neural network in the sense of Kosslyn 1999 or Martin 2000), that of shape recognition.

The imposition of shape is a feature of virtually all human material culture. But the first time it ever occurred was with these early bifaces. Prior to the appearance of bifaces, stone knappers attended to the configuration of edges and to size. The earlier Mode 1 tools were arguably an ad hoc technology (Isaac 1984; Toth 1985; Wynn 1981) made for immediate use. It is unlikely that they existed as tools in the minds of the knappers. But tools, in the guise of bifaces, almost certainly did exist as a category in the mind of *Homo erectus* (Wynn 1993b; Wynn et al. 1996).

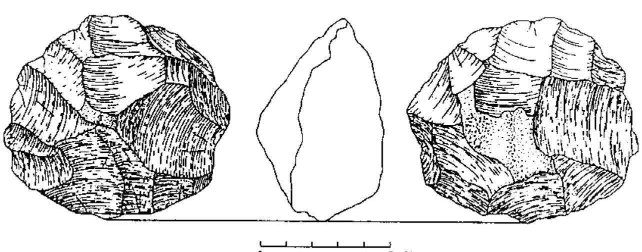


Figure 6. 1.4 million year-old "discoid" from Olduvai. The regularity of diameter suggests attention to a "local" spatial quantity.

### 2.5. Late bifaces: Congruent and three-dimensional symmetries

**2.5.1. Description.** Three developments in hominid imposed symmetry appear in the archaeological record sometime after 500,000 years ago. These are: (1) congruency; (2) three-dimensional symmetries; and (3) broken symmetry.

Although the reflectional symmetry of early bifaces was rough and imprecise, the symmetry of late examples clearly suggests attention to congruency. The mirrored sides are not just qualitative reversals, but quantitative duplicates, at least to the degree that this is possible given the constraints of stone knapping. Many, but certainly not all, late handaxes and cleavers present such congruent symmetries, and this is one of the features that makes them so attractive to us. Such a symmetry was not limited to a single shape. Late bifaces demonstrate a considerable amount of variability in overall plan shape. Some are long and narrow, others short and broad. Some have distinct shoulders, whereas others are almost round. Although there is some evidence that this variability was regional (Wynn & Tierson 1990), much of it is related to raw material, and much appears to have been idiosyncratic. But in almost every assemblage of this time period there will be a few bifaces with fine congruent symmetry, whatever the overall shape.

The second development in symmetry was the appearance of reflectional symmetry in three dimensions. Many of these bifaces have reflectional symmetry in profile as well as in plan. In the finest examples this symmetry extends to all of the cross sections of the artifacts, including cross sections oblique to the major axes, as we would define them (Fig. 7). Once again, this feature is not universally true, and many, many bifaces do not have it, but it is present on at least a few artifacts from most assemblages.

The third development in symmetry was the appearance of broken symmetry. Here a symmetrical pattern appears to have been intentionally altered into a nonsymmetrical but nevertheless regular shape. Several cleavers from the Tanzanian site of Isimila appear bent, as if the whole plan symmetry, including the midline, had been warped into a series of curved, parallel lines (Fig. 8). These are invariably extensively modified artifacts, whose cross sections are symmetrical, and the pattern is almost certainly the result of intention.

A better known example is the twisted profile, or "S-twist," handaxe (Fig. 9). The artifacts give the appearance of having been twisted around the central pole. The result is an S-shape to the lateral edges, as seen in profile. Again, these are extensively modified artifacts and we must conclude, I think, that the pattern is the result of intention.

It is not possible to date these developments in symmetry precisely. Archaeological systematics place all of the examples in the late Acheulean period (sometimes on morphological grounds alone, which leads to a circular argument). All were probably made after 500,000 years ago, perhaps even after 400,000 years ago. The Isimila artifacts, for example, date to between 170,000 and 330,000 years ago (Howell et al. 1972). The twisted profile handaxes probably date no earlier than 350,000, and most may date much later. Although 300,000 years is a long time in a historical framework, it represents only the final 12% of technological evolution.

Several caveats complicate interpretation of these three developments. One is the problem of individual skill; some prehistoric stone knappers must have been more adept than others and better able to achieve congruent, three-dimensional symmetries in the intractable medium of stone. We have no way of knowing how common highly skilled

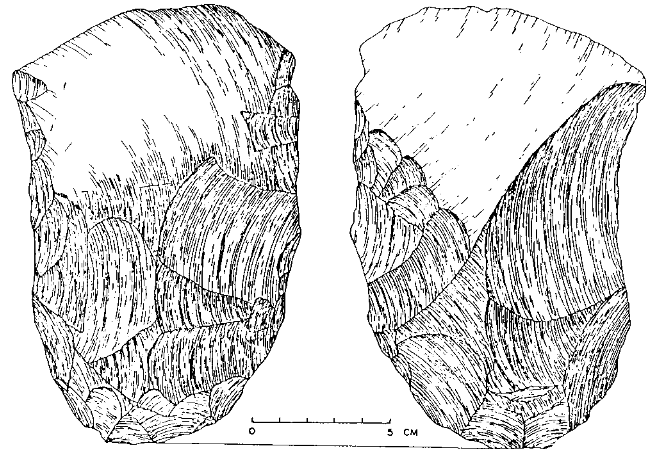


Figure 8. A "bent" cleaver from the Tanzanian site of Isimila. The extensive modification suggests that this broken symmetry was intentional.

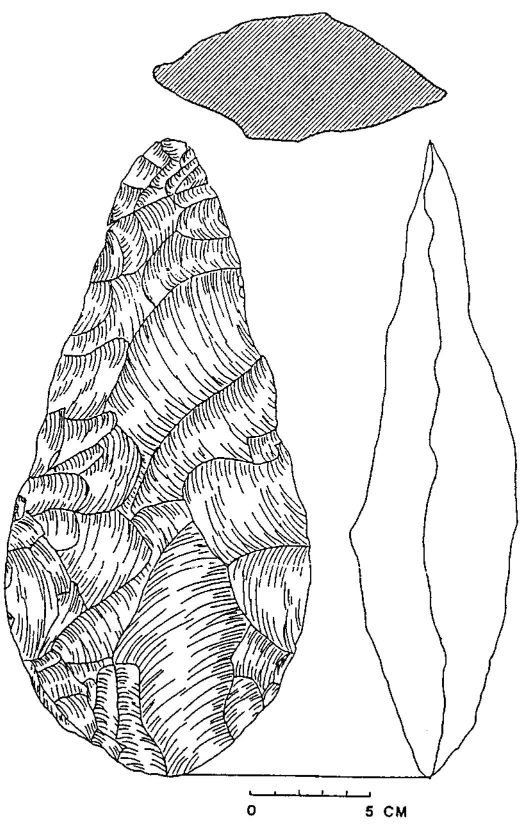


Figure 7. A handaxe from the Tanzanian site of Isimila (about 300,000 years old). This artifact has congruent symmetry in three dimensions.)

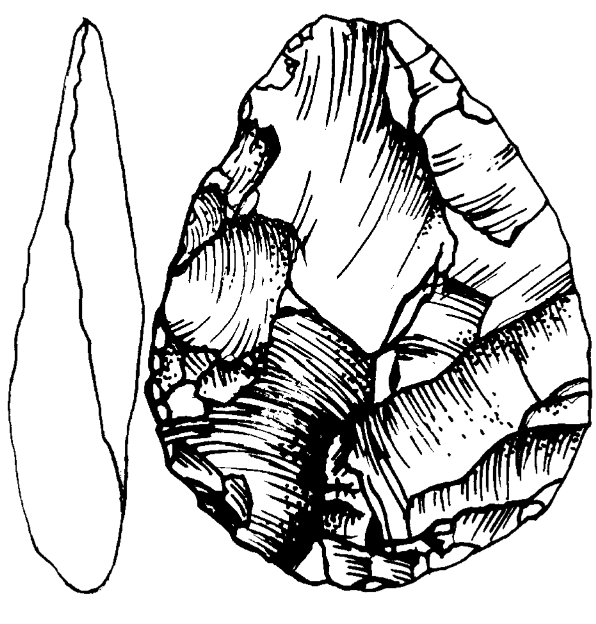

Figure 9. A "twisted profile" handaxe (about 200,000 years old) from the English site of Swanscombe (after Roe 1981). This is another example of broken symmetry.

knappers were. A second caveat is raw material. Some stone is much easier to work than others. I do not think it is entirely coincidence that twisted profile handaxes are invariably made of flint or obsidian, two of the most prized knapping materials. On the other hand, raw material is not as tyrannical as one might think. The bent cleavers from Isimila are made of granite.

**2.5.2. Cognitive implications.** The imposition of three-dimensional, congruent symmetry probably depended on cognitive abilities not possessed by the first biface makers. The cognitive psychological literature suggests some possibilities. The first requirement would appear to be the ability to coordinate perspectives. While flaking the artifact, the knapper has only one point of view. This is adequate to control edge shape, and perhaps even two-dimensional symmetry, but to produce an artifact with three-dimensional symmetry one must somehow hold in mind viewpoints that are not available at that moment, and for the finest symmetries, viewpoints that are not directly available at all (oblique cross sections). The knappers must have understood the consequences of their actions for the shape of the artifact as it appeared from these other perspectives. Such manipulations are akin to "allocentric perception" recognized by psychologists (Silverman et al. 2000), and used in image manipulation tasks such as mental rotation. It is likely that these hominids were able to manipulate mental images of objects. Again, archaeological bias forces a conservative analysis; however, no one has proposed a convincing simpler alternative to this one. Application of a simple flaking procedure, without any image manipulation, could not have produced the kinds of three-dimensional symmetries evident on these artifacts. The second requirement, congruency, is clearly spatial in the narrow sense of perceiving and imaging spatial quantity. As we have seen, basic knapping is largely a spatial problem, and was from the beginning. What is new here is the application of metric spatial relations to a problem of shape. Simple unimodal shape recognition would not have been enough. The sophistication of this symmetrical pattern suggests that shape identification is required. "When we recognize something, we know only that we have perceived it before, that it is familiar [such as the early handaxes described earlier]; when we identify something, we know it has a certain name, belongs to specific categories, is found in certain locales, and so forth" (Kosslyn 1999, p. 1284). 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).

These hominids could manipulate perspectives and spatial quantity, produce congruent symmetries, and even distort these principles to achieve striking visual effects. It is fair, I think, to attribute an intuitive Euclidean concept of space to these stone knappers. A Euclidean sense of space is one of three-dimensional positions. Although the human life-world is certainly organized this way, and we and other primates clearly perceive dimensional space, it is quite another thing to employ cognitive mechanisms that understand space in this way, and which can be used to organize action. Such a mechanism, or mechanisms, underpin our most sophisticated everyday navigational and mapping skills.

### *2.6. After 400,000*

The examples I have used so far have all been knapped stone artifacts. Although symmetry clearly can be and was imposed on many knapped stone artifacts, the medium is not ideal for the imposition of form. It is not plastic, and shaping can only be done by subtraction. Indeed, after the appearance of the symmetrical patterns just discussed, no subsequent developments in symmetry can be recognized in knapped stone. There were developments in technique, and perhaps skill, but the symmetries imposed on even very recent stone tools are no more elaborate than those imposed on 300,000-year-old handaxes. As a consequence we must turn to other materials.

Artifacts made of other materials – bone, antler, skin, wood, fiber, and so on – were undoubtedly part of the technical repertoire of many early hominids (though see Mithen 1996 for a counter suggestion). Because such materials are far more perishable than stone, the archaeological record contains few of them until relatively late in prehistory. There are a few examples that almost certainly predate 100,000 years ago, but all are controversial, either as to age, or as to significance. One is a pebble from the Hungarian site of Tata, on which someone engraved a line perpendicular to a natural crack (Bednarik 1995). Although one might be tempted to argue from it that the maker had some notion of rotation, or radial symmetry, this is too heavy an interpretive weight to be born by a single, isolated artifact. More to the point, even if true, this would tell us little more about symmetry than is supplied by contemporary bifaces. It would be symmetry in a new context, however; a fact which, if confirmed, would have possible implications for cognitive evolution.

It is not until very close to the present, indeed after 100,000 years ago, that the archaeological record presents extensive evidence of artifacts made of perishable materials. Some archaeologists see this timing as entirely a reflection of preservation; others see it as evidence of new behaviors and abilities. The earliest such evidence is African and dates from between 50 and 90,000 years ago (Klein 2000; Yellen et al. 1995). These are worked bone points from a site in the eastern Congo. Although these artifacts are quite important to several current arguments about prehistory, they reveal nothing new in regard to hominid imposed symmetry. The European Upper Palaeolithic provides the best documented examples of hominid imposed symmetries for the time period between 40,000 to 10,000 years ago. Here we find extensive evidence of symmetry in materials other than stone (Fig. 10).

Perhaps most widely known are cave paintings of Franco-Cantabrian art, especially in compositions that are about 15,000 years old. Here we can see possible symmetries as patterns of elements in a composition, not just inherent in a single object. They appear to have resulted from the application of a compositional rule. As such, they do not inform us specifically about spatial or shape cognition and are outside the scope of this discussion.

## 3. Discussion

I suggested at the beginning of this article that archaeology can make two important contributions to the study of the evolution of human cognition: the timing of certain developments and a description of the evolutionary context in which these developments occurred. The sequence of de-

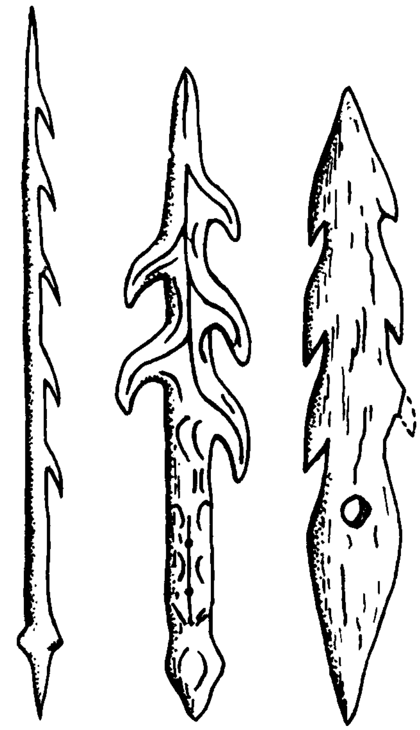

Figure 10. Barbed points made of antler from the European Upper Palaeolithic (about 17,000 years old).

velopment of hominid imposed symmetries just summarized allows us to do both of these things.

### 3.1. Timing

The development of artifactual symmetry was not slow and continuous. Instead, the archaeological evidence suggests that there were two episodes of development, separated by as much as one million years. During the first episode, hominids developed the ability to impose shape on artifacts, an ability undocumented for any living apes. In doing this, early *Homo erectus* employed cognitive abilities in frame independence, mirroring, making simple judgments of spatial quantity, and coordination of shape recognition (symmetry) with the spatial requirements of basic stone knapping. There may have been others, but these are the ones evident in the archaeological record. This development occurred early in the evolution of the genus *Homo,* certainly by 1.4 million years ago. The second episode of development occurred much later and consisted of the acquisition of a modern Euclidean set of spatial understandings. Specific abilities evident from the symmetrical handaxes include congruency, three-dimensional shapes, and coordination of perspectives. The date of this development appears to correlate with the evolutionary transition from *Homo erectus* to Archaic *Homo sapiens.*

This timing of developments has implications for human cognitive evolution. First, the initial hominid adaptation (4.5–1.5 million years ago) apparently included a basic ape-like understanding of space and shape. A generalized ape repertoire of spatial concepts was adequate for this earliest of hominid adaptive niches, including the first manufacture and use of stone tools. A distinctive set of spatial/shape abilities did not appear until relatively late in hominid evolution, after the appearance of *Homo erectus.* Second, because these two later episodes of cognitive development were discontinuous, and indeed rather far from one another in time, it is unlikely that they occurred in response to the same selective factors. Whatever selected for the spatial/shape abilities of early *Homo erectus* probably did not select for the Euclidean abilities that emerged one million years later. But perhaps the most important implication that the development of artifactual symmetry has for the understanding of human shape and space cognition in general, and not just its developmental sequence, is that even the more recent developments occurred in the *very* remote past. In terms of shape and spatial thinking, we have not just Stone Age minds, we have Lower Palaeolithic minds.

### 3.2. Evolutionary context

Evolutionary context is the second body of information archaeology can provide the study of the evolution of cognition. Although it is well and good to describe a sequence of development, it would also be good to answer the questions of how, and perhaps why. In evolutionary science this amounts to answering the question of selection. What selected for these abilities? Evolutionary psychologists (Barkow et al. 1992; Bock & Cardew 1997) answer this question by looking at evidence for adaptive design, on the assumption that past selection is preserved in the modern architecture of the cognitive mechanisms. Paleoanthropologists, and archaeologists in particular, are suspicious of such reliance, and prefer to invoke the actual context of development to help identify possible selective agents. Although our knowledge of the conditions of the evolutionary past is fragmentary and lacking in detail, it is still an account of actual prevailing conditions, not a reconstruction based on presumed selective pressures.

Hominid fossils and the archaeological record constitute the primary evidence for the context of cognitive evolution, supported by a large body of methods used for dating and for reconstructing the physical environment. Hominid fossils provide some direct evidence of cognitive evolution in the guise of brain size and shape. At least at our present level of understanding this does not lead to persuasive arguments about specific abilities, but it can identify times of brain evolution in general, which can support arguments derived from other evidence. Hominid fossils can also inform us about other evolutionary developments in anatomy, which human paleontologists have used successfully to document changes in diet, nutrition, locomotion, heat and cold adaptation, levels of physical stress, and other aspects of adaptive niche that are directly relevant to the context of cognitive evolution. Archaeological evidence, because it is far more abundant than fossils, informs about geographic distribution, habitat use, specific dietary components, geographic range (via raw material transport), and cultural solutions to problems (fire, weapons, boats, etc.), in addition to the evidence for hominid cognitive abilities. Together the fossil and archaeological evidence provide a reliable, if incomplete, picture of the past, including the two time periods in which the major developments in artifactual symmetry occurred.

#### 3.2.1. Early biface makers

*3.2.1.1. Context.* We know much less about the first episode of development than the second. The time of the first biface industries, 1.4 million years ago, with their evidence for the imposition of symmetry and concomitant modest developments in spatial thinking, was also the time of *Homo erectus.* Indeed, the first *Homo erectus* (a.k.a. *Homo ergaster* [Tattersall 2000]) had appeared in Africa (and perhaps elsewhere) several hundred thousand years earlier, so we cannot make a simple equation between *Homo erectus* and biface technology. Luckily, one of the most spectacular fossil finds for all of human evolution is an African *Homo erectus* from this time period. The Nariokotome *Homo erectus* is an almost complete skeleton of a youth who died about 1.55 million years ago (Walker & Leakey 1993). The completeness of the skeleton allows a more detailed discussion of life history and physiological adaptation than is possible with fragmentary remains. The youth was male, about 11 years old, stood about 160cm (63in) at time of death (estimates of adult stature for this individual are 185cm [73in]), had a tall, thin build, and evidence of strenuous physical activity. His brain size was about 880cc, and the endocasts demonstrate the same left parietal and right frontal petalia typical of humans but not of apes. His thoracic spinal diameter was smaller than that of modern humans of similar size, and he had a very small pelvic opening, even for a thin male. This anatomy suggests several important things about his physiology. He had the ideal body type for heat dissipation. Added to the evidence for strenuous activity, this suggests that exertion in hot conditions was common. Earlier hominids had been largely woodland creatures who focused most of their activity close to standing water. Nariokotome had the anatomy to exploit an open tropical grassland adaptive niche. Although 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*). Despite the modern overall shape of the brain, the thoracic spinal diameter (and by extension the number of nerve bodies enervating the diaphragm muscles) suggests that rapid articulate speech was not in Nariokotome's repertoire.

In sum, Nariokotome suggests that the *Homo erectus* niche was significantly different from that of earlier hominids, including earlier *Homo.* It is not clear from the cranial capacity that a significant increase in braininess accompanied this adaptive shift. There is no good reason to think *Homo erectus* had speech, at least in a modern sense (Wynn 1998a). The niche shift itself was very significant, however, and is corroborated by the archaeological evidence.

Archaeological sites from this time period are less informative about hominid activity than many earlier sites. This seeming paradox results from the typical sedimentary context of the sites. Most early biface sites have been found in stream deposits, rather than the lake shore deposits typical of earlier sites. These "high energy" environments move objects differentially, including bones and tools. In effect they destroy the natural associations on which archaeologists rely. Running water also modifies bone, and to a lesser extent stone tools. The unfortunate result is that archaeologists have few direct remains of activity other than the stone tools themselves. There are enough sites dating to this time period to allow archaeologists to assess geographic distribution and environmental context, both of which suggest that a significant change in niche had occurred.

*Homo erectus* left stone tools in stream beds because he had moved away from permanent standing water. Archaeologists presume that the channels of ephemeral streams, or the banks of permanent streams, became one of the preferred activity locales. Given the absence of associated materials, we cannot determine just what these activities were; only the selection of locale is apparent. This fits nicely with the "body cooling" anatomy of Nariokotome. On open savannas, stream channels often support the only stands of trees (in addition to water). Archaeologists have also found African biface sites at higher altitudes than earlier tools sites (Cachel & Harris 1995), and, finally, there are early biface sites outside of tropical Africa. The best known is Ubeidiya in Israel, which in most respects resembles early biface sites in Africa (Bar Yosef 1987).

This archaeological evidence presents a picture of *Homo erectus* as an expansionistic species who invaded new and varied environments. Cachel and Harris (1995) suggest it was a "weed species" – never numerous individually but able to invade new habitats very rapidly. Given the clear reliance on tools, *Homo erectus'* niche was at least partly technological. Control of fire may also have been a component (James 1989). Evidence from this time period at the south African site of Swartkrans includes convincing evidence of the control of fire (Brain & Sillen 1988). Although control of fire may have little cognitive significance (McGrew 1989), the importance to adaptive niche may have been profound in terms both of warmth and predator protection. We have little direct evidence for diet. From experimental studies we know that bifaces could be effective butchery tools, but there are no obvious projectiles and no evidence for greater reliance on meat. There is, in fact, no compelling evidence for hunting.

*3.2.1.2. Selection.* It is not clear from this picture just what might have selected for the development in cognitive abilities evident in artifactual symmetry. At the outset we can consider the possibility that natural selection acted directly on the hominid ability to recognize and conceive of symmetry, which is, after all the pattern that is so salient in the archaeological record. What might the perceptual saliency of symmetry have been good for? There is considerable evidence that body symmetry is, in fact, related to reproductive success for males (Gangestad 1997). According to Gangestad, observable phenotypic asymmetry (away from the reflectional symmetry coded genetically) correlates with developmental stress, so that asymmetry marks lower health. If a potential mate could detect this, he or she could avoid a reproductively costly (in an evolutionary sense) mating. But presumably this is true generally for vertebrates, and not just for humans. Perhaps symmetry gained added importance as a clue to general health when hominids lost thick body hair. Condition of coat is also a good indicator of general health, and its absence may have led to selection for a heightened ability to detect variations away from symmetry. But the real question here is why would *Homo erectus* have imposed symmetry *on artifacts*? Could artifacts have come to play a role in mate selection? Could symmetry have become so salient a pattern for mate assessment that it intruded into other shape recognition domains? Here the saliency of symmetry has been transferred out of the domain of the phenotypic to that of cultural signaling, but the selective advantage is the same. In this scenario both the ability to detect and produce symmetry would have had reproductive consequences. Unfortunately, it is difficult to see how such a hypothesis could be tested. It is provocative only because of the known role of symmetry in mate selection.

Given the change in niche associated with early *Homo erectus,* with the accompanying increase in range and the pioneer aspects of the adaptation, it is tempting to posit selection for spatial cognition via navigational ability. Judging spatial quantity would be a useful skill, for example. Although matching diameters on tools and judging distances to water are both matters of spatial quantity, it is not clear that they use identical cognitive mechanisms. Indeed, neurological research suggests that relationships in near and far space are handled somewhat differently by the brain (Marshall & Fink 2001), though there does appear to be some correlation (Silverman et al. 2000). The temporal association of territory expansion with developments in shape and spatial cognition is provocative, but hardly conclusive. Given the emphasis in some literature on sexual division of labor (Eals & Silverman 1994; Silverman et al. 2000), it is also important to note that paleoanthropologists know nothing about division of labor in this time period.

Even though the contextual evidence provides no leading candidate for selective agent, it does describe an adaptive milieu of relevance. Early *Homo erectus* was not much like a modern hunter-gatherer. There is no evidence for human-like foraging systems or social groups (the probable absence of speech would itself obviate the latter). There is not even any convincing evidence of hunting with projectiles, a favorite of several authors (Calvin 1993). Nothing in the contextual evidence warrants direct analogy to the adaptive problems of modern human foragers. The challenge that *Homo erectus* presents to paleoanthropologists, and other students of human evolution, is that there are no living analogs. There is no more reason to invoke a human model than a chimpanzee model, or neither.

### 3.2.2. Late biface makers

*3.2.2.1. Context.* Paleoanthropologists' knowledge of evolutionary context is much better for the time period associated with the appearance of three-dimensional symmetries, congruency, and multiple perspectives. These abilities were clearly in place by 300,000 years ago, and probably by 500,000. Our knowledge is better partly because this time period was much closer to the present (though still remote), but also because *Homo* had expanded into Europe. It is true that some temperate environments have good preservation, but the primary archaeological effect of this expansion is that *Homo* moved into an area where a great deal of modern archaeology has been done. Africa may be the home of mankind, but Europe is the home of archaeology. Based largely on European sites it is possible to draw an outline sketch of the behavioral/cultural context of daily life.

The peopling of Europe is itself a fascinating topic. Some argue that Europe was occupied relatively late, after 500,000 years ago (Roebroeks et al. 1992). Earlier sites are certainly scarce and often controversial. However, there are sites in Italy, such as Isernia (Cremaschi & Peretto 1988), and Ceprano (Ascenzi et al. 2000), and Spain, such as Atapuerca (Bermudez de Castro et al. 1997), that provide strong evidence for the presence of *Homo erectus* (now attributed by some to *Homo antecessor*) prior to 500,000. In many respects these resemble earlier *Homo erectus* sites – poor geological context and little to go on. There is little doubt that after 500,000 years ago the record is better and includes the peopling of northern Europe. There are informative sites in England, Germany, France, and the Netherlands. Some archaeologists have suggested that this represents an adaptive breakthrough, an evolutionary development that opened up harsher environments (Roebroeks et al. 1992). These first colonists in northern Europe did make bifaces, and their bifaces required all of the cognitive abilities identified earlier in this paper. The association is provocative, and suggests that some evolutionary development in cognition may have been partially responsible for this breakthrough. Of course, similar artifacts appeared in Africa, so the breakthrough cannot have been specific to Europe. But what specifically might have selected for cognitive abilities evident in the fine three-dimensional symmetries of bifaces? The archaeological record does provide some clues.

The move into northern Europe (and China) may seem a minor extension of the much more dramatic expansion of *Homo erectus* 1,000,000 years earlier. It may well have required some significant changes in adaptation, however. Northern European climate during the Pleistocene was in more or less constant flux, with cold glacial periods alternating with warmer, forested, interglacial periods. During periods of maximum cold, northern Europe was uninhabitable, and indeed even the anatomically modern of humans of 18,000 years ago were forced to the south. But after 500,000 we have evidence of biface sites in northern Europe during some of the warmer episodes embedded in glacial periods. The environment during these periods was more open and less heavily wooded than today, but also cooler than today. The adaptive problems posed by such an environment are fairly well known. Compared to warmer environments, even in southern Europe, there would have been fewer edible plant species, and a concomitant requirement for increased reliance on animals. Then there were obvious problems of keeping warm, including the likely necessity of controlling and probably even making fire. In effect, these northern temperate environments "pushed the envelope" of *Homo's* adaptation. But here our European bias risks misleading us. We see it clearly because we have the sites. We know, however, that comparable technological developments occurred in Africa, the Near East, and China, and it is unlikely that Europe was in any way central (indeed a cultural backwater is more likely), so what we may be seeing is abilities that evolved elsewhere applied to a European problem.

The fossil remains from this time period present a confusing picture. In some areas of the world, Asia in particular, *Homo erectus* was clearly still present. In Africa the prevailing fossil type appears to have been a larger brained form that still retained many *Homo erectus*-like features of the face. Europe is the biggest puzzle. There are few clear *Homo erectus* fossils (the Mauer mandible is a probable example [Rightmire 1992], as is the Ceprano calvaria [Ascenzi et al. 2000]), but most fossils attributed to the taxon have at least some sapient-like features. Rightmire (1998) now favors a return to the use of *Homo heidelbergensis* for these forms. The key issue for the present argument is that this was a time of evolutionary change in anatomy, as well as technology and cognition. We cannot yet describe the complex evolutionary patterns, but we do know the long period of evolutionary stasis that was *Homo erectus* was coming to an end.

By this time it is almost certain that these *Homo* hunted large mammals. Many sites have included the association of stone tools with animal bone, such as Hoxne (Singer et al.

1993) and Boxgrove (Roberts et al. 1994; Roberts & Parfitt 1999; Wenban-Smith 1989), and in some cases like Hoxne the range of body parts suggests that at least some of these animals had been hunted, not just scavenged (Binford 1985). Until recently the only evidence of hunting technique was an enigmatic sharpened stick from Clacton-on-Sea that could have been a spear tip (Oakley et al. 1977). In 1996, the German site of Schoeningen dramatically confirmed this interpretation. Here, Thieme (1997) and crew excavated three complete spears carved out of spruce, each more than two meters in length. The spears were in direct association with the bones of horses. The center of gravity of each of these spears was situated slightly toward the tip, much as it is in modern javelins, and Thieme argues that *Homo* must have designed these spears for throwing. If true, this suggests a relationship between design and use that has technological and perhaps even cognitive implications. But what bears more on the issue at hand is that hunting with spears was obviously a component of the behavioral/cultural context of these *Homo,* a component that calls up modern human analogs.

The archaeological record does not provide much direct evidence for group organization, or even size, at least not for this time period. For more recent times the size and debris patterns of structures provides much useful evidence of social organization, but such patterns degrade rapidly and in only the most ideal conditions survive for tens of thousands of years, let alone half of a million years. All of the possible campsites from the time period of interest here are controversial. Thirty years ago de Lumley (de Lumley & Boone 1976) presented a dramatic argument for huts, seasonal occupation, and reuse at the French site of Terra Amata, an interpretation that still survives in textbooks (Turnbaugh et al. 1999). The site has never been published in detail, and the one independent analysis cast considerable doubt on de Lumley's interpretation (Villa 1983a). At best, there is evidence for a few post holes, stone blocks, stone knapping debris, and shallow hearths scooped from the sand. These may be the remains of a flimsy shelter, and would certainly fit into a reconstruction of a small hunter-gatherer band. Unfortunately, this optimistic picture is at least premature, and probably unwarranted. Villa's analysis indicates that the integrity of the site is much lower than presented by de Lumley. It is in reality an accumulation of cultural debris that has been moved and altered by natural processes. Yes, this provides evidence of hominid activity, but not of a coherent campsite with multiple activities and an artificial structure. There are no better examples until much later. Indeed, Gamble (1994; 1999) argues that this absence of campsites is an accurate reflection of the life ways of early *Homo sapiens,* and that coherent long-term or multiple use campsites were not part of the adaptive niche. If true, these early *Homo sapiens* were not like modern hunters and gatherers.

There are other ways in which they were not modern. As familiar as some of this evidence is, there is a striking piece of modern behavior that was entirely missing. We have no convincing evidence of art, or personal ornamentation, or anything that clearly was an artifact of symbolic culture. Many sites do have lumps of red ochre, some of which had been scraped or ground. A few sites have enigmatic scratched bones. None of this constitutes indisputable evidence that these hunters and gatherers used any material symbols, of any kind whatsoever. Compared to the abundant use of such items in sites postdating 50,000 this absence is telling.

In sum, the archaeological evidence indicates that by 400,000 years ago *Homo* was a hunter-gatherer who had invaded new, more hostile environments, but who did not invest in symbolic artifacts. Despite similarities to modern hunters and gatherers, these early *Homo sapiens* were different in many respects.

*3.2.2.2. Selection.* What might have selected for the cognitive abilities required for three-dimensional, congruent symmetries? Again, mate selection is a possibility, this time by way of technological skill. An individual who could produce a more regular (symmetrical) artifact would be cuing his or her skill and worth as a potential mate. Other things being equal, the stone knapper who produced the fine three-dimensional bifaces was smarter and more capable, with better genes, than one who couldn't. Especially if knapping skill correlated with other technological abilities, this would be one means of identifying mates with future potential as providers. Kohn and Mithen (1999) take this argument even farther, framing it terms of sexual selection and emphasizing abilities other than technological.

> Those hominids . . . who were able to make fine symmetrical handaxes may have been preferentially chosen by the opposite sex as mates. Just as a peacock's tail may reliably indicate its "success," so might the manufacture of a fine symmetrical handaxe have been a reliable indicator of the hominid's ability to secure food, find shelter, escape from predation and compete within the social group. Such hominids would have been attractive mates, their abilities indicating "good genes." (Kohn & Mithen 1999, p. 521)

Modern people certainly do use material culture to mark their individual success, and it is perhaps not far fetched to extend this behavior into the past, perhaps even to the time of late *Homo erectus* or early *Homo sapiens.*

A second possibility is that selection operated on enhanced spatial and or shape cognition, with artifactual symmetry being just one consequence. Given the co-occurrence of hunting and gathering and modern spatial thinking in the paleoanthropological record, this hypothesis suggests that they are somehow linked. What selective advantage could congruency, three-dimensional symmetries, and image manipulation bestow on a hunter-gatherer? William Calvin (1993) has long argued that aimed throwing was a key to cognitive evolution. Although I find his argument that bifaces were projectiles far from convincing (see also Whittaker & McCall 2001), the Schoeningen spears may have been projectiles. That there is a spatial component to accurate throwing seems beyond question. Calvin himself emphasizes the importance of timed release and the computational problems of hitting moving targets. Would any of this select for image manipulation, congruency, and so on? It is hard to see how, unless ability to estimate distance to target selected for abilities in judging all spatial quantities (e.g., congruent symmetries). The selective agent, throwing, just does not match up well with the documented abilities.

Navigation is again an alternative, and one favored by many psychologists (e.g,. Dabbs et al. 1998; Gaulin & Hoffman 1988; Moffat et al. 1998; Silverman et al. 2000). While route-following using sequential landmarks can work, using basic topological notions like those known for chimpanzees and early hominids, it is difficult to conceive of and follow novel routes without a Euclidean conception of location. Arguably, hunting, especially varieties invoking long distance travel, herd following, or intercept techniques would favor Euclidean conceptions of space. There is now experimental evidence documenting a correlation between navigational skill and the standard psychometric measures of spatial cognition like mental rotation (Moffat et al. 1998; Silverman et al. 2000), though recall that "near" and "far" spaces are not handled identically by the brain (Marshall & Fink 2001). This specific selective hypothesis, then, is a better fit than the throwing hypothesis. Of course, navigation skill might have been unrelated to hunting per se and instead tied to mate searching (Gaulin & Hoffman 1988; Sherry 2000) or any long distance travel. What is provocative is the correlation between the earliest evidence for large-scale hunting and Euclidean spatial relations, as represented by three-dimensional symmetries.

Although the correlation between this development in spatial thinking and navigation is provocative, it does have two weaknesses. First, many animals are fine navigators without relying on the enhanced spatial abilities in question. Of course, what we are seeing here is the *hominid* solution to navigation problems, so I am not too troubled by this objection. The second objection is more bothersome. When modern people navigate, they rarely use the spatial abilities in question. For example, when modern hunters and gatherers move across the landscape they use established paths and routes, often following waterways or animal trails (Baluchet 1992; Gamble 1999). The geometric underpinning of such navigation is largely topological and does not rely on the kind of spatial abilities evident in the stone tools. Yes, it is possible to imagine a form of navigation that relies on such abilities, but this does not seem to be the way people actually move about. Unless there is a compelling reason to think that modern hunter-gatherers rely on Euclidean spatial relations to navigate, it will remain a weak hypothesis.

Of course, these spatial and shape abilities may not have been directly selected for at all. They may be by-products of natural selection operating on other cognitive mechanisms. For example, if Kosslyn's (Kosslyn 1994; Kosslyn et al. 1998) characterization of mental imaging is accurate, the key development may have been in central processing rather than the more encapsulated shape recognition system or spatial assessment system. These are relatively discrete neural networks that reside in different parts of the brain (one ventral, one dorsal). For someone to conceive of congruency, and perhaps alternative perspectives, the two outputs must be coordinated, and this coordination appears to happen in the association areas of the frontal lobe. Here the evolutionary development would be in the area of association and central processing, and there is no reason for selection to have been for shape recognition or spatial ability per se. In other words, the archaeological evidence for the development of three-dimensional, congruent symmetries may inform us about developments in more general cognitive abilities, not just a narrowly encapsulated module of spatial thinking.

## 4. Conclusion

The archaeological record of symmetry reveals two of the times at which significant developments in hominid cognition occurred. The first, a million and a half years ago, encompassed cognitive developments necessary to the imposition of shape on artifacts, the coordination of shape recognition (symmetry) and spatial thinking (stone knapping) being the most salient. This evolutionary development was associated with *Homo erectus,* and the appearance of the first hominid adaptation that was clearly outside the range of an ape adaptive grade. These *Homo erectus* were not, however, like modern hunter/gatherers in any significant sense; indeed, there are no appropriate analogs living today, and the precise agents selecting for these cognitive abilities are not apparent. The second episode evident from artifactual symmetries occurred a million years later and encompassed the development of modern Euclidean understandings and manipulations of shape and space. This was the also time of the transition from *Homo erectus* to Archaic *Homo sapiens.* The appearance of large mammal hunting in the contemporary archaeological record lends some support to evolutionary psychological arguments that hunting may have selected for features of human spatial cognition, either by way of projectile use or navigation. Given the range of evidence documenting the appearance of many features of hunting and gathering at this time, not just spatial thinking, it is perhaps simpler to posit a few developments in associative abilities than a raft of specific cognitive mechanisms. It is also important to reiterate that despite being *Homo sapiens,* these were not modern hunters and gatherers. They lacked the rich symbolic milieu on which modern humans, including hunters and gatherers, rely. Archaeology cannot itself resolve many of the controversies raised by the evidence. Questions concerning the cognitive and neural bases of the actions preserved in the archaeological record must be answered in studies of modern cognition. Archaeology can point to the times and contexts of cognitive evolution, but cannot itself illuminate the workings of the human mind. A comprehensive approach to cognitive evolution must therefore be multidisciplinary.

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# Open Peer Commentary

*Commentary submitted by the qualified professional readership of this journal will be considered for publication in a later issue as Continuing Commentary on this article. Integrative overviews and syntheses are especially encouraged.*

## Artifacts and cognition: Evolution or cultural progress?

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**Abstract:** Lack of symmetry of stone tools does not require that hominids making asymmetric tools are incapable of doing better. By analogy, differences between stone tools of early humans and modern technology arose without genetic change. A conservative assumption is that symmetry of stone artifacts may have arisen simply because symmetrical tools work better when used for striking and chopping rather than scraping.

Making inferences about the evolution of cognition from the archaeological record is a difficult business, because there is not much to go on. Stone tools are probably only a small fragment of the technology used by any group, as contemporary groups (including those that did not use metals) indicate. Wynn's target article and the book chapter on which it is based (Wynn 2000) rely mostly on differences in quality and type of symmetry in a succession of stone artifacts to infer something about the cognitive capabilities of their makers.

Although revealing, the strategy has limitations. The principle one is the assumption that an artifact found at an archaeological site represents the limits of the cognitive abilities of its maker. While it is safe to assume that the artifact's design cannot exceed the capacity of its maker, the reverse assumption is not justified – the maker may be more cognitively sophisticated than the artifact reveals. When digging in my garden, for instance, I may want to level a small lump of earth, and if it's more than my fingers can handle, I'll grab a small nearby rock to finish the job. It's strictly a stone-age technology. I make no improvements in the rock, though microscopic wear marks might indicate to a future archaeologist how I used it. At this point I am somewhat behind the Oldowan level of technology, not having improved my rock. Is the future archaeologist justified in assuming that I haven't quite got up to the cognitive level of early *Homo erectus* yet?

The example, while somewhat extreme, makes the point that tools are as good as they have to be, and no better. Oldowan technology might be perfectly adequate to quickly cobble up a scraper or a cutting blade; there is no need to refine the tool, for its user can make another one in a few minutes, and the broken-pebble level of technology will do as well for some applications as the fine blade.

The difference between early Oldowan artifacts and later more symmetrical ones, therefore, might be more in the application than in the cognitive capabilities of the manufacturer. Further, cultural progress will mean that the same people can make better tools at a later time. It is now fairly well agreed that the upper Paleolithic revolution of about 50,000 years ago was not accompanied by a genetic change that swept across the worldwide range of humans at that time, but rather was based on some key element or elements of cultural progress, perhaps an improvement in language or symbolic conventions (reviewed by Bridgeman 2003).

What might have been the cultural impetus that induced hominids to improve the quality of their stone tools? The key element may have been not cognitive capability, but the demands of a new lifestyle made possible by cultural progress. A stone tool made for scraping will work well whether it is symmetrical or not; when the job is chopping or spearing, though, symmetry suddenly becomes necessary. The symmetrical axe or spear tip simply works better than the asymmetrical one. Anyone who has used an axe, whether stone or steel, knows that it is important to hit the working surface squarely; otherwise the axe will twist and veer off to one side, with unpredictable but usually bad consequences. Symmetrical axe heads are safer, more predictable and more effective.

The need for an axe rather than a scraper or a blade might have come about with new demands being made on tools. Once new uses are found for tools, a demand is created for technical improvements in them. An axe can cut wood for shelters, fires, or making other tools and weapons. A spear can bring down large prey from a safe distance. Progress in technology driven by cultural accumulation of knowledge, rather than neurological evolution, might create demand for such tools.

The contrast between technical level of tools and the biology of their makers is even more clear when we compare the nicely made stone axe heads of 100,000 years ago with the steel axe, the jet engine, or the laptop computer of today. The latter tools are infinitely more sophisticated, yet as far as we know the jet engine maker's genes are no different from those of the fellow who crafted that nicely symmetrical axe head at the very beginning of human technical progress. Admittedly, the rules changed after the upper Paleolithic revolution, when the rate of technical progress clearly began to outstrip evolution, but Occam's razor demands that we seek similar simple solutions for the question of the origin of earlier technical progress as well.

This is not to say that culture rather than biology is definitely responsible for the improvement in tools, but only to point out that the case is not yet made for biological evolution accompanying symmetry of tools or other cultural artifacts. Indeed, the safest working hypothesis is that both brain evolution and cultural progress played a role in the improvement of material culture.

## Rediscovery and the cognitive aspects of toolmaking: Lessons from the handaxe

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**Abstract:** Long before signs of staged toolmaking appeared, *Homo erectus* made symmetrical tools. The handaxe is a flattened tear-drop shape, but often with edges sharpened all around. Before we assign their obsession with symmetry to an aesthetic judgment, we must consider whether it is possible that the symmetry is simply very pragmatic for one particular use in the many suggested.

I like Wynn's (2002) analysis but let me play devil's advocate for a thousand words and consider whether the biface's symmetry was initially pragmatic – and only developed into an aesthetic after a million years of proving its usefulness.

With its flattened-teardrop symmetry, the Achulean handaxe has long invited cognitive explanations. It is the earliest hominid tool that seems "designed" in some modern sense. Yet, for most of the "Swiss Army knife" multipurpose suite of proposed uses (defleshing, scraping, pounding roots, and flake source), an easy-to-make shape would suffice – and indeed the simpler tools continued to be made. None of these uses adequately addresses the "design aspects." Why is the handaxe mostly symmetric, why mostly flattened, why the seldom-sharp point, why sharpened all around (when that interferes with gripping the tool for pounding uses)?

Neither does a suite of uses suggest why this form could remain the same from southern Africa to northern Europe to eastern Asia – and resist cultural drift for so long. The handaxe technique and its rationale were surely lost many times, just as Tasmanians lost fishing and fire-starting practices. So how did *Homo erectus* keep rediscovering the enigmatic handaxe shape, over and over for nearly 1.5 million years? Was there a constraining primary function, in addition to a Swiss-Army-knife collection of secondary uses?

In Calvin (1993, expanded in 2002), I describe the handaxe's extraordinary suitability for one special-purpose case of projectile predation: attacking herds at waterholes on those occasions when they are tightly packed together and present a large, stampede-prone target. Briefly, in the beginner's version that uses a tree branch rather than a handaxe, the hunters hide near a waterhole. When the herd is within range, the branch is flung into their midst. The lob causes the herd to wheel about and begin to stampede. But some animal trips or becomes entangled by the branch. Because of jostling and injury by others as they flee, the animal fails to get up before hunters arrive to dispatch it.

Chimpanzees often threaten by waving and flinging branches but, if such are not handy, they will toss rocks or even clumps of dirt in the same general direction. One can imagine that tree branches were soon in short supply near waterholes. If our waterhole hominids resorted to second best, lobbing a rock into the herd's midst, it would not trip animals but it might knock one down. Because of the delaying action of the stampeding herd, this too might allow an animal to be caught. Even when you miss, the herd will be more tightly packed together on its next cautious visit to the water's edge.

What rocks would work best? Large rocks, but also rocks whose shape had less air resistance. Most rocks tumble, but flat rocks (say, from a shale outcrop) will sometimes rotate in the style of a discus or frisbee, keeping the thin profile aligned to the direction of travel and thereby minimizing drag. Because approach distance will increase with heavy predation, range would become important.

Hunters might also have noticed that stones with sharp edges were more effective in knocking an animal off its feet, even when not heavy. Withdrawal reflexes from painful stimuli, such as a sharp prick from an overhanging thorn tree, cause a four-legged animal to involuntarily squat. Even if the spinning stone were to hit atop the animal's back and bounce free, it might cause the animal to sit down. It is the sudden pain which is relevant, not any actual penetration of the skin.

Handaxes, whether thrown by amateurs or experts, whether lobbed or thrown more directly, usually turn into vertical-plane spinners. Unlike a frisbee which rolls endlessly after landing, handaxes rotate to bury their point and abruptly halt. If the point is momentarily snagged on a pushed-up roll of skin, it would both augment the pain and transfer all of its momentum to the animal, pushing it sideways. Ordinarily, righting reflexes would catch the animal before it toppled, but a simultaneous sit-down withdrawal reflex can override this customary protection.

So this is a beginner's technique for a commonplace high-payoff situation, not a general-purpose hunting technique (it strongly depends on a herd-sized target and the consequent stampede). This proposed path of discovery would also work well in cases of loss of shaping technique, promoting a return to flattened rocks with an all-around edge and something of a point.

Consider also the "life history" of a handaxe. Some new sharp ones would be lost in the mud. Of the ones retrieved, some would have been trampled. A broken classic handaxe may make an excellent cleaver, now having a grip that no longer bites the hand that holds it. Many lost handaxes would be tumbled by a flood and then later discovered in the river bed, with some edges smoothed enough to hold comfortably. So (notwithstanding Whittaker & McCall 2001), I see the shape-defining use as special-purpose, but with broken and tumbled handaxes having many secondary uses, including the "Swiss Army Knife suite."

Channel-cutting floods even set up rediscovery of the best shape by the clueless of a lost generation. In watercourses where the animals come to drink, some of the easily grabbed stones throw farther than others and have better knock-down properties. By the time that this *objet trouvé* supply is exhausted, toolmakers know what the most effective shape is, from having recycled some lost handaxes.

Clearly an ability to imagine that a series of blades in a prepared core was present 50,000 years ago – and equally clearly, little cognitive ability was needed 2.5 million years ago for Glynn Isaac's shatter-and-search method for producing the sharp split cobbles. The latter suffice for getting through the skin and amputating limbs at a joint before the competition arrives; they also allow the limb to be swung club-like against tree trunks to produce spiral fractures and extract marrow. Indeed, shatter-and-search and the handaxe together largely solve the major savanna problems of scavenging and waterhole hunting.

So what cognitive ability was needed by early *Homo erectus* for handaxe design? Not much more than for shatter-and-search. Rather than being seen as an embarrassing exception to 50,000-year modernity, the handaxe can be seen – once the singular controlling use is appreciated – as having a very pragmatic shape, where deviations from the flattened teardrop are more likely to result in dinner running away. The step up to staged toolmaking (first shape a core, then knock off flakes) at 400,000 years ago is far more impressive as evidence of enhanced cognition.

## A complete theory of human evolution of intelligence must consider stage changes

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**Abstract:** We show 13 stages of the development of tool-use and tool making during different eras in the evolution of *Homo sapiens.* We used the NeoPiagetian Model of Hierarchical Complexity rather than Piaget's. We distinguished the use of existing methods imitated or learned from others, from doing such a task on one's own.

An important question that remains unanswered in Wynn's target article is whether the differences seen between earlier tool-making and later tool-making reflect a change in developmental stage attained by hominids during different eras in the evolution of modern *Homo sapiens.* While Wynn's previous work (Wynn 1981) related Mode I tools to the preoperational stage, here he concentrates on the development of specific spatial skills without referring to developmental stage. With more current, NeoPiagetian theories, such as the Model of Hierarchical Complexity (MHC), it should be possible to come up with a valid sequence. This sequence allows the specification of developmental stage both of the earliest tool-related behaviors seen in animals, including apes and early hominids, and of how thoroughly distinct each was from that of modern humans.

To show the developmental sequence most accurately, it is necessary to categorize a much wider set of tool use and tool-making tasks from a variety of species, as well as whatever early hominid behaviors can be inferred from other aspects of the archeological record. Second, the stage-complexity of particular practices becomes clearer if one builds a more complete sequence, adding-in prior stages and later stages. What we have posited (Chernoff & Miller 1995; 1997; Miller 1999; Miller et al. 1999) is that chimpanzees solve social problems that are concrete operational, but not tool-making problems at this stage; instead they are one stage lower, or primary stage tasks. *Homo sapiens* within same-sized groups as chimpanzees solve systematic-stage problems (consolidated formal-operational, Inhelder & Piaget 1958; Kohlberg 1990). The common ancestor of chimpanzees and humans probably did not solve concrete-stage tool-making tasks either. Hominids then had to traverse four stages: concrete, abstract, formal, and consolidated formal.

To have an accurate developmental order of different types of tool use and tool making, a more detailed, complete and accurate model of development than Piaget's is necessary. Such a model is provided by the Model of Hierarchical Complexity (MHC; Commons et al. 1998; Commons & Miller 1998; Commons & Wolfsont 2002). This is a nonmentalistic, NeoPiagetian model of stages of performances based on the fact that *tasks* can be placed in order according to their hierarchical complexity. The orders and stages resemble those suggested by NeoPiagetians (e.g., Case 1978; 1985; Fischer 1980; Pascual-Leone 1970; 1976). All of these added more stages than Piaget's model (14 stages in the MHC), allowing for greater precision in categorizing tasks. MHC has arranged in order problem-solving tasks of various kinds: moral reasoning (Dawson 2000; 2002), reasoning about attachment (Commons 1991; Miller & Lee 1999), social perspective-taking (Commons & Rodriguez 1990; 1993) and evaluative reasoning (Dawson 1998), among others. Such ordered changes can be described by using the MHC in virtually any domain because of this model's universality. MHC posits mathematical definitions of "ideal" actions upon which stages are based (Commons & Richards 2002).

Table 1 shows a brief suggested sequence of "ideal" tool use and manufacture tasks. Note that in understanding the stage demands of a task, it is important to distinguish among using existing methods by imitating or learning (1 level of support, Fischer et al. 1984), doing such a task on one's own (0 levels of support, as used by Piaget), versus discovering new methods of tool manufacturing (-1 level, Arlin 1975; 1984). Each decreasing level of support is harder by one stage.

Table 1 (Commons & Miller). *Stages of ideal actions of tool making*

| Stages | Tool-making action |
|---|---|
| 1. Sensory and Motor Actions (actions, perceptions) | Looks at stones, touches, or holds a stone. Each of these actions is done singly and not combined with other actions. |
| 2. Circular sensori-motor actions (organizes 2 actions) | Looks at, reaches, and grabs a stone. Bangs a stone by accident on another stone. |
| 3. Sensory-motor (conceptual activity) | Bangs a stone into another stone or other objects, both singly and in combination. Uses simple concepts such as bashing a nut with a stone. Classifies perceptually. |
| 4. Nominal (words, sequences conceptual actions) | Bashes one stone on the other, such that the second stone strikes the first at a place that is near the immediately previous strike. Creates successive modifications that are nonsystematically different along any dimension. Acts on named concepts as seen by actions. |
| 5. Sentential (sequences nominal actions and words) | Hits one stone with the other in a constant direction of movement (each strike at the stone is done in relation to the previous one). Makes Mode I tools that require just a few bangs. |
| 6. Preoperational (organizes sentential actions) | Does one sequenced set of things after another sequence to the same tool. Focuses on only one dimension or aspect of tool making – bashing edges or just producing flakes. |
| 7. Primary (does single reversible actions) | Uses beginning symmetry or constant spatial amount, as described for early Mode II tools. Follows through on tool making until end of task. |
| 8. Concrete (coordinates reversible actions) | Makes one piece of a tool and then attaches it to another piece (e.g., an arrowhead to a stick). Coordinates two separate reversible actions. Carries and stores tools consistently. |
| 9. Abstract (does norm-based actions; unsystematic uses of variables) | Uses a standard unit of measure to produce symmetrical tools. More precisely, applies constant spatial amount. Follows peer social norms (Wynn 1993b) for uniform tool making. Uses variables including points that vary from dull to sharp; edge sharpness; shapes varying from round to long and narrow; materials effects. |
| 10. Formal (controls and studies effects of variables) | Makes and uses multiple specialized tools for different applications. Instantly decides which to use in which situation (i.e., isolates causal variables). |
| 11. Systematic (forms systems of relationships and multiple causal variables) | Systematically develops tools for different situations (problem finding) for the first time (−1 level of support). Tool making is adapted to materials at hand (causal relation 1), and planned function (causal relation 2), making the best tool for that particular situation. Integrates empirically earlier formal-operational methods of tool making when presented the problem. |
| 12. Metasystematic (compares systems) | Compares two systems each with sets of causal relationships for manufacturing tools. Discovers how formal operational causal relations interact (−1 level of support). |

## Does complex behaviour imply complex cognitive abilities?

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**Abstract:** In this commentary, we propose that the shifts in symmetry Wynn documents may be explained in terms of simpler mechanisms than he suggests. Furthermore, we argue that it is dangerous to draw definitive conclusions about the cognitive abilities of a species from the level of symmetry observed in the artefacts produced by that species.

There is little doubt that cognitive archaeology provides a means of constraining theories of cognitive evolution. Indeed, Wynn's argument that archaeology makes available information regarding the timing of developments and can document attributes that have implications for underlying cognitive mechanisms, is a reminder that cognitive scientists generally should pay greater attention to this line of enquiry. However, we argue that the claimed differences between levels of symmetry in artefacts are underdetermined by the data presented, and need further justification. In addition, Occam's razor dictates that simpler explanations should be considered before more elaborate theories. The assumption underlying Wynn's analysis is that more complex forms of symmetry require the co-ordination of various types of cognitive process. We question whether this is a prerequisite for the production of all of the types of symmetry Wynn considers. We propose that the assumed changes in levels of artefactual symmetry over time may be explicable in terms of the development of a simple biomechanical skill.

Wynn's argument rests on identifying differences between artefacts produced from different points in the archaeological record. However, only a few examples are presented, and often he acknowledges that these examples are not necessarily representative of all the artefacts produced from that period. For example, in relation to the level of symmetry produced by the early hominids, he points out that "not all bifaces of this age are nicely symmetrical." Clearly one can't help but question whether the artefacts illustrated in the target article are representative of the artefactual record in general from the periods considered, and qualitatively different from those of other periods.

There are simple studies that would considerably increase confidence in the differences in the configurations of the tools produced in different periods. Furthermore, it is important to establish variation in the levels of symmetry produced both between and within time periods. In experimental cognitive science it is regarded as *de rigueur* to establish measures of inter- and intra-rater reliability when dealing with the classification of data that are at all ambiguous. In the case of artefactual symmetry, it would be possible to take casts of the artefacts from different periods, randomise them, and then ask participants to freely sort these artefacts, or sort them based on given criteria (e.g., definitions of types of symmetry). This would establish whether (1) it is possible to reliably identify the differences in the artefacts produced at different periods in time, and (2) allow quantification of the variability in types of artefacts produced within and between periods. Additionally, it may be possible to calculate more objective measurements of these artefacts and compute degrees of types of symmetry across a range of artefacts. Such analyses would considerably increase confidence in the differences that are claimed, and would remove the need to speculate about these differences.

Wynn regards the interplay between different components of the perceptual system as an explanation for the differences in levels of symmetry presented in the stages he considers. In the first shift, he argues that early hominids required co-ordination between the shape recognition system and other spatial abilities. However, there are many cases of organisms further down the phylogenetic scale that can produce rather sophisticated geometrical objects without the need for higher cognitive processes. For example, spider's webs illustrate a large degree of complexity and symmetry, but are produced using evolved simple algorithms without the need for sophisticated co-ordination between different cognitive systems. Furthermore, different species of spider illustrate varying degrees of complexity in the types of webs spun. For example, the cobweb spiders (*Theridiidae*) build rather irregular meshes, while the so-called "primitive" web builders, such as the cribellate *Amaurobius* weave a tubular (largely symmetrical) retreat with simple signal or catching threads radiating from its entrance. More complex again are the three-dimensional cones within an orb-like construction built by *Ulobourus bispiralis* from New Guinea. Attributing differences in the degrees of geometric sophistication of the webs produced to complex underlying mechanisms is unnecessary. Wynn dismisses the argument that the symmetry in stone tools may be a product of the technique of manufacture, favouring a more intentional account. In particular, the artefacts which have had more work done on them are argued to be the most symmetrical. However, there is a simple biomechanical explanation for this which may be considered.

Patterns of movements which have evolved in relation to specific technological skills may account for differing degrees of structural symmetry in objects produced. Following Fitts' law, error (standard deviation) is proportional to the amplitude of movement and the amount of force applied to the strike. With greater force of a strike, greater distance is required between target object and striking object, and, therefore, more error is likely to occur. The production of the early artefacts involved fewer strikes with greater force. This would be less likely to result in objects displaying symmetrical properties. With smaller directed blows, error is reduced, but also the object resulting is more likely to look sculptured and symmetrical. Evolving biomechanical algorithms that allow efficient changes in striking patterns over time are likely to be central to the development of the knapping skill. For example, the early discoids illustrated in the paper could be produced by a simple strategy of trimming around the perimeter of the core, resulting in an object that is symmetrical. This argument suggests that earlier artefacts could well be a result of less well-developed biomechanical routines. It has been demonstrated in recent years that similar types of seemingly complex behaviours may be explicable in terms of simple underlying mechanisms (see for example Clark 1997, for a discussion).

We have argued that sophisticated behaviours do not necessarily imply higher-level cognitive processes. It is also the case that simpler behaviours do not necessarily imply the absence of more sophisticated cognitive abilities. For example, it is commonly assumed that there has been no real cognitive development since the Middle/Upper Palaeolithic transition (Mithen 1996). However, the advances in technological achievement (e.g., the construction of buildings and forms of transport) after this shift show that increasing complexities in structural design are not necessarily attributable to changes in basic cognitive abilities. Considerable care needs to be taken when extrapolating from behaviour to cognitive ability or vice versa.

## Is symmetry of stone tools merely an epiphenomenon of similarity?

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**Abstract:** It is proposed that symmetry of stone tools may derive from perceptual similarity rather than from cognitively more complex awareness of symmetry. Although encodement of shapes necessarily involves symmetry (as evidenced by the confusability of enantiomorphs), it does not imply awareness of symmetry. Responses of relatively simple organisms, such as bees, support the notion that the processes involved are likely to be perceptual.

Two distinct approaches are commonly used to study perception of symmetry. At the core of the first approach, which can be thought of as direct, is the perception of symmetrical entities variously presented. At the core of the second is perception of the two enantiomorphic elements forming symmetrical entities. The target article presented by Wynn considers, mostly, the evidence obtained using the first approach. The present commentary examines that provided by the second approach, and tentatively proposes that symmetry of stone tools may derive not from an elaborate cognitive schema involving awareness of symmetry but from a perceptual and therefore cognitively simpler notion of similarity; a proposal according well with Occam's razor.

It is well established that elements which are mutually enantiomorphic (i.e., which jointly form a symmetrical entity and are such that one of them is a reflection of the other) are also mutually confoundable (Corballis & Beale 1976). For example, children often mistake *b* for *d* and *d* for *b* although they are unlikely to have seen those letters in either *bd* or *db* arrays. Letters *b* and *d* will henceforth be used to represent any pair of all mutually enantiomorphic stimuli ranging from pairs of random unfamiliar shapes to pairs of familiar objects. The "*b-d*" confounding when the notional axis of symmetry is in the subject's median plane is markedly more frequent than confounding within pairs of elements having identical shapes but presented in mutually different orientations such as "*b-p*," "*b-q*," "*d-p*," and "*d-q*." It is also markedly more frequent than confounding within "*b-d*" pairs presented with the axis of symmetry in a plane other than the observer's median plane, for example with the axis in the observer's fronto-parallel plane but not vertical, or with the axis in the horizontal plane and inclined to the fronto-parallel plane.

The "*b-d*" confounding when the notional axis of symmetry is in the observer's median plane is, as Wynn acknowledges, in a category of its own. This symmetry is the only kind of symmetry considered here. Confounding of enantiomorphs necessarily implies that presentation of the observer with a visual stimulus results in its perceptual encodement containing both the facsimile of the stimulus in question and its enantiomorph. Thus presentation of *b* results in an encodement incorporating both *b* and *d.* Hence, when the observer is subsequently required to recognize or to reproduce the initiating stimulus to which he has been exposed, his response is one of the mutually enantiomorphic elements. The perceptual similarity may therefore lead to reproduction of *d,* say, where *b* would have been the correct response. This tendency to confound *b* and *d* is affected by the inclination of the vertical plane on which the stimuli are presented, relative to the observer's fronto-parallel plane; it is least when the plane of presentation is fronto-parallel and increases with the increase of the angle between the two planes (Deręgowski et al. 2000).

Consider the knapper at work (see Whittaker 1995). The knapper takes a stone and by knapping alters the shape of one of its faces. This face is face *b.* He then begins to work on the other face and endeavours to make it similar to face *b.* This similarity may be gained either by making this face look like *b,* or to look like *d.* The choice between these two enantiomorphs is not however arbitrary, on the contrary, it is a strongly guided choice, for the edge which has just been made provides a very cogent perceptual vector suggesting that *d* (the enantiomorph of *b*) rather than *b* should be made. Not only does this edge provide perceptual guidance but it is also a strong ergonomic argument; production of *d* calls for much less effort. A combination of these factors furnishes a very strong cognitive vector fostering *d* rather than *b.* The knapper, thus induced to create *d,* has this inducement augmented whenever in the course of his labours, in order to assess his progress, he looks at the emergent tool in such a manner that its cutting edge faces him so that the two faces (*b* and *d*) are at relatively large angles to his fronto-parallel plane. (These angles according to Fig. 7 of Wynn's paper would be of the order of 45° to 65° and would increase with the refinement of the tool's edge.) The result of our knapper's work is an approximately symmetrical tool, the extent of symmetry depending on the knapping skill. This symmetry has been achieved not through consideration of the principles of symmetry, a cognitively complex notion, but by application of a much more rudimentary device, that of perceptual similarity of enantiomorphs. Symmetry of the stone implement is, if this speculation is correct, merely an epiphenomenon.

The above considerations do not, however, imply that symmetry has played no part in the process of toolmaking. It obviously did so because the perceptual encodement of the *b* as *bd* implies symmetry. It is not, however, symmetry of which the knapper is aware. It is an effect which affects his perception but which, like the effect affecting perception of visual illusions, is outside the observer's awareness. Such covert influence of symmetry is also present in the entirely unrelated artefacts of our distant ancestors, in their depictions of animals. It has been argued that such depictions are based on the typical contours of animals' bodies (Deręgowski 1995; Deręgowski & Dziurawiec 1996). In the case of most quadrupeds (e.g., bovids and equines) the typical contours run the length of their spines, and therefore lie in the planes of symmetry of these animals' bodies. Symmetry therefore could be said to contribute to the salience of these contours. Here again, however, the effect seems essentially perceptual.

The ubiquity of symmetrical forms in nature is by and large a result of the gravitational pull. A single vertical force acting on an animal or a plant must, if the body acted upon is to remain stable, be opposed by forces whose resultant is of equal magnitude but of opposite sense. Similar considerations affect the shape of human artifacts. Thus, symmetry of an axe has important practical consequences, for it ensures that the force applied when using it is matched by a reaction free of a twisting moment that would occur had the tool been asymmetrical. It could be argued that these factors affect perception of human beings through perceptual learning. However, since much humbler creatures, such as the bees, perceive symmetry (Giufra et al. 1996) this suggestion lacks force.

The readiness with which symmetrical stimuli are perceived and remembered and discriminated from other stimuli, which is a subject of many a study by workers pursuing the first of the two approaches to symmetry, needs to be placed in the context of the proposed schema. Its postulated origin is simply this: Since every stimulus is perceptually encoded in terms of its facsimile and its enantiomorph, so is a symmetrical stimulus (such as, say, U). However, in the case of a symmetrical stimulus, its facsimile and the enantiomorph are identical (it is encoded as UU), and therefore the bipolar rivalry between the two elements (*b* and *d* in the case of stimulus *b*) is replaced by entirely concordant relationships, which render symmetrical stimuli both perceptually striking and memorable (Deręgowski 1992).

To conclude, this commentary suggests that symmetry of stone tools may not be the most telling attribute as far as human cognitive development is concerned. It follows that it would be of great interest to conduct an analogous exploration of some other attribute, the effect of which could not be explained in perceptual (that is in cognitively relatively simple) terms, in order to evaluate the thesis so ingeniously advanced.

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## Evolution of the reasoning hominid brain

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**Abstract:** Cognition is readily seen to be connected to evolution through plots of the ratio of cranial capacity to body size of hominids which show two regions of sharply increasing ratios beginning at 2.5 and 0.5 million years ago – precisely the critical times inferred by the author from his study of tools. A similar correlation exists between current human brain growth spurts and the onsets of the Piagetian stages of reasoning development. The first goal of the author's target article is stated to be "to make a case for the relevance of archeological contributions to studies of the evolution of cognition" (sect. 1, Introduction). His analysis focuses on spatial cognition.

If Wynn could be satisfied with a more general aspect of cognition, there is a readily demonstrated aspect based on changes in a parameter interpretable as directly revealing increases in brain structural and cognitive complexity.

Evidence for such brain changes come from the ratio of brain or cranial size to body size. Any increase in brain size disproportionately greater than the corresponding increment in body size reflects acquisition of novel brain structures and their derivative functioning.

The data in Table 1 and Figure 1 are taken from Tobias (1987), Hofman (1983), Bauchot et al. (1969), Stephan et al. (1981), and Jerison (1973). Although the body weights increase fairly steadily from 37 kg to 68 kg, the ratio remains constant over two long time periods. Thus, there are two periods with transitions to significantly increased ratios: from about 11.5 cc/kg for Australopithecines to 17.2 cc/kg for *Homo erectus,* and from 17.2 cc/kg for *Homo erectus* to 22.9 cc/kg for *Homo sapiens.* Those increases in brain size are greater than needed just to sense and control any increased body weight.

Any greater-than-proportional increases in brain size should signal increases in the complexity of brain structure and functioning. Such an inference also pertains to the fact that the ratio for Australopithecines is substantially greater than that of *P. troglodytes,* indicating a significantly augmented brain function for the first of the hominids.

The first of the hominid transition periods starts at about 2.5 million years ago and the second about 500,000 years ago; these are just two transition points described by the author based mainly on findings about tools.

Making tools can be transmitted by showing the novice what to do, so it takes only a copying capacity. For that reason, the cognitive status of Kanzi needn't be very great; so the cognitive level associated with the first transition might be presumed to reflect as reaching no more than what the Piagetians call concrete reasoning.

Table 1 (Epstein). *Brain and body data for hominids*

| Name | Brain Weight gm | Body Weight kg | Brain/Body gm/kg |
|---|---|---|---|
| Hominid series | | | |
| *A. afarensis* | 413 | 37.1 | 11.1 |
| *A. africanus* | 441 | 35.3 | 12.5 |
| *A. robustus* | 530 | 44.4 | 11.9 |
| *A. boisei* | 510 | 47.5 | 10.7 |
| *H. habilis* | 640 | 48 | 13.3 |
| *H. erectus* [Tobias] | 895 | 53 | 16.9 |
| *H. erectus* [Holloway] | 929 | 53 | 17.5 |
| *H. e. javanicus* | 937 | 53 | 17.7 |
| *H. e. pekinesis* | 1043 | 53 | 19.7 |
| *H. sapiens* female | 1350 | 55 | 24.5 |
| male | 1450 | 68 | 21.3S |
| Pan troglodytes | 391 | 52.9 | 7.39 |

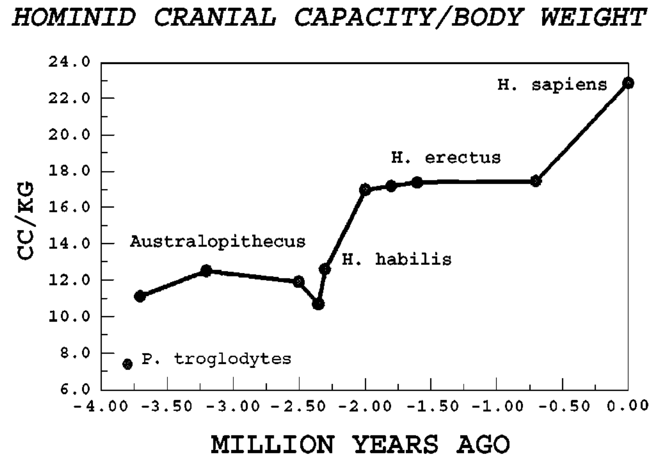


Figure 1 (Epstein). Ratios of cranial capacity to body weight for the hominids plus the value for *P. troglodytes.* There are plateaus for Australopithecines and for *H. erectus,* along with a possible plateau starting with *Homo sapiens.*

The author gives the spatial cognition turning points as 1.5 million years ago and about 500,000 years ago. The first of these is during the span of constant *Homo erectus* ratio and so is not related to any great additional brain change. But the second coincides with the spurt in relative brain size during the transition from *erectus* to *sapiens.* The first point (1.5 million years ago) would then be likely to show a minimal effect compared with that of the second point, which is likely to be part of a substantial increase in complexity of structure and thought.

Two main categories of increments in the ratio are: (1) Brain changes associated with physical properties of individuals; and (2) Changes associated with functional or cognitive aspects. The physical aspects would be likely to be manifested in enhanced agility and/or enhanced manual dexterity. Cognitive aspects could start from overall properties such as the reasoning stages described by Piaget in specific properties such as the spatial cognition used by the author. Accepting the assignment of Australopithecines to the concrete reasoning level, the *Homo erectus* level could be that of formal reasoning, while *Homo sapiens* could be that of post-formal reasoning.

It is useful to remind ourselves of the caution stressed by Churchland (1986) that currently observable behaviors and/or functions may be far-derived from the ones on which evolutionary selection could have acted.

There is precedent for asserting that large increments in brain size are correlated with substantial increments in cognitive levels, because the Piagetian stages are correlated age-wise with the stages of rapid brain growth we have discovered: 2–4, 6–8, 10–12, and 14–16/17 years. Thus, the brain stages occur at the onsets of those Piagetian stages (Epstein 1974a; 1974b; 1980; 1986; 1999).

In present humans, a cranial capacity of about 900 cc is reached by about 2 years of age; this birth-to-age-2 period is called the sensori-motor stage, during which the senses and motor activities become reasonably functional. After this, there is a large increase in cranial capacity reached by of 4 years of age. This 2–4 year period is what the Piagetians call the preoperational stage when children cannot yet reason logically about directly experienced matters, but begin to think about things in symbolic terms. The hominid's brain/body plateau of 11.5 cc/kg corresponds to a cranial capacity of about 950 cc, making it similar to the 900 gm stage in present humans.

"The Great Leap Forward" (Diamond 1993) took place about 60,000 years ago when the hominid suddenly became able to deal with complex or abstract problems. Planned agriculture replaced gathering, planned hunting replaced scavenging, abstract paintings appeared (as in the Lascaux caves), and awareness of the individual as a member of a group that had regular properties, such as dying, replaced just noting when an animal was dead. The Leap originated when some of the hominids discovered how to make use of the new arborization to become able to reason abstractly, which made possible the strikingly novel functions described by anthropologists. Such functions cannot be selected for because they can be taught, so there is neither need nor basis for selection for networks genetically programmed for higher intelligence, provided that persons involved already have the evolutionarily augmented networks. It would be similar to the difficulty of selecting dogs for retrieving newspapers. Thus, humans remain dependent on instruction and experience for acquiring higher reasoning functions. From that time on, education became the means of spreading the new competencies; so education became, and has remained, the main activity of human maturation.

## Tacit symmetry detection and explicit symmetry processing

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**Abstract:** Wynn's claims are, in principle, entirely reasonable; although, as always, the devil is in the details. With respect to Wynn's discussion of the cultural evolution of artifactual symmetry, we provide a few more arguments for the utility of mirror symmetry and extend the enquiry into the tacit and explicit processing of natural and artifactual symmetry.

The centrality of the archaeological record to our knowledge of human cultural evolution can hardly be denied (Renfrew 1993). Although it requires some extra steps in the argument, archaeological evidence can also be expected to throw at least a little light on the biological foundations of cultural development: The study of symmetry in biface tools may well be as relevant to this topic as the study of asymmetry in endocasts (Holloway & DeLaCoste-Lareymondie 1982).

With respect to symmetry, it is crucial to note that mirror symmetry (especially about the vertical axis) is an important determinant of figure/ground discrimination. *Ceteris paribus,* symmetry picks out figures from their backgrounds (Bahnson 1928; Koffka 1935). Indeed, this cue to perceptual parsing of the visual world seems sufficiently "primitive" that one would expect to find it used in many different species. There remains, however, controversy about whether, in *Homo sapiens,* symmetry detection has a significant "innate" component (Bornstein et al. 1981), or is rather derived primarily by learning from statistical regularities in the environment (Brunswick & Kamiya 1953). Beh and Latimer (1997) have suggested that there is a common mechanism underlying the perception of line (edge) orientation and the perception of mirror symmetry. The relative environmental frequencies of different orientations (vertical > horizontal > oblique) are then responsible for tuning the visual system to detect line orientation and mirror symmetry in that same order. *Per contra,* it has also been argued that the bilateral symmetry of the visual cortex is a major factor in the explanation of why mirror (reflectional) symmetry is easier to detect than translational or rotational symmetry (Herbert & Humphrey 1996).

Be that as it may, the implicit use of mirror symmetry as a cue to figure/ground partitioning would seem to be neurobiologically distinct from the explicit analysis of symmetry per se. The most striking examples of this dissociation are found in patients with left visuo-spatial neglect after right parietal lesions. Although these patients can use mirror symmetry to assign figure/ground relationships, they cannot explicitly judge whether a shape is vertically symmetrical or not (Driver et al.1992; Marshall & Halligan 1994). One interpretation is that figure/ground partitioning (on the basis of symmetry) is assigned at an early (preattentive) processing stage where all contours are implicitly coded. By contrast, overt symmetry judgments require that visual attention be explicitly directed to the left side of objects. It is this later processing stage that is impaired in left spatial neglect (Driver et al. 1992; Marshall & Halligan, 1994). It is consistent with preattentive processing that mirror symmetry is tacitly detected even when the task does not demand it: Search for simple figures is influenced by the symmetry (or otherwise) of the background elements (Wolfe & Friedman-Hill, 1992).

We have undertaken this seeming digression in order to emphasize the contrast between the tacit preattentive perception of symmetry (Wagemans 1997) and the explicit analysis of symmetry which is presumably required to make the biface tools discussed by Wynn. As Wynn himself writes, it is one thing to perceive spatial relations and "quite another thing to employ cognitive mechanisms that understand space in this way, and which can be used to organize action" (sect. 2.5.2, para. 2). Artifacts, whether tools, pots, or pictures (Gaffron 1950) may well provide the clearest way to an understanding of the conscious role of visual symmetry and balance in the human mind (Arnheim 1954). With respect to cognitive evolution over a substantial time range, artifacts may be our only clue to the relevant capacities in the hominid lineage from *Homo habilis* to *Homo sapiens.*

Yet, whether our forebears made symmetrical tools for practical or "aesthetic" reasons may require a more detailed specification of what precisely stone age tools were used for: Some tasks may be facilitated by symmetrical tools while others may demand asymmetry. The study of composition in early art and decoration could in principle be more revealing of cognitive capacity, but the available record does not take us to anywhere near the time of *Homo habilis.* Currently, clear evidence for art and ornament can only be traced back to the Upper Palaeolithic (Valladas et al. 2001), some 30,000 years ago, although it has been argued that seemingly geometric designs had been engraved on bone by an even earlier hand 300,000 years ago (Bahn & Vertut 1988).

## Symmetry in knapped stones is real, not romanced

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**Abstract:** It appears that knappers intentionally produced symmetrical stones. Use of the dorsal pathways in knapping does not preclude shape perception, nor does it obviate use of ventral pathways in other tasks in *Homo sapiens* 400,000 years ago. Shape perception precedes production in present-day human infants, suggesting that symmetry perception was used by knappers of symmetrical stones.

Almost all present-day hand tools are symmetrical in shape. Wynn's discussion of early symmetrical tools is crucial to our understanding of the cognitive evolution that led to tool making. Wynn asks whether symmetrical knapped stones from 400,000 years ago show cognitive spatial abilities such as shape perception in these later *Homo sapiens* who made these Mode 2 tools. Some of his questions have to do with earlier *Homo erectus* as well, but for simplicity, I will mainly comment on the findings concerning *Homo sapiens.* His answers to these important questions seem far too cautious and limited on several grounds. For example, Wynn's question of intention is of little concern to the empirical psychologist who is able to use operational definitions in research. All Wynn really needs to establish is reliability in order to demonstrate intention. His work shows that humans reliably created symmetrical tools 400,000 years ago. We can assume that they did this on purpose. They may have imitated the action, or seen the product and attempted to reproduce it, or have repeated their accidental discovery. Any of these possiblities reveal reliable production of one type of object, which would indicate intention. The issue of reliability brings to mind certain methodologies for establishing reliability of another kind. There is no mention here of how reliably tools are judged to be symmetrical by researchers, nor the specific criteria for symmetry. This seems to me to be of greater concern than the issue of intention.

The question of intention is a sidebar to the main issue of spatial cognition in *Homo sapiens.* Wynn cites a PET scan study (Stout et al. 2000) showing that activation during knapping is localized in dorsal pathways in a modern knapper. He takes from this study the suggestion that perhaps, then, knappers do not need shape perception in order to produce a symmetrical object. He suggests that the ventral pathway, which is associated with object identification and shape recognition, is minimally activated during knapping. But Milner and Goodale (1993) have obtained evidence that the ventral pathways are involved in shape perception in terms of actions on shapes. More recently James et al. (2002) have found in fMRI studies that dorsal pathways are sensitive to changes in views of objects, while ventral pathways show priming effects to different views of the same object. Thus, it is not fair to conclude that dorsal involvement precludes shape perception, although dorsal shape perception may be uncoscious. Furthermore, because the dorsal pathways are activated during knapping it is not fair to conclude that the knapper is unable to use or does not have functional ventral pathways during activities that precede knapping.

Wynn has addressed developmental issues elsewhere (Wynn, in press). He has compared the known spatial cognitive abilities of children to those of ancient knappers. This seems to be a reasonable comparison. I have argued (Humphrey, in press) that the development of the perception of symmetrical patterns precedes the development of the production of symmetrical patterns, probably because of the symmetrical location of sensory systems on a symmetrical body primarily designed for orientation and locomotion. The production of symmetries, on the other hand, requires the use of an asymmetrically organized cognitive system and asymmetrical use of bimanual coordination. This is likely to be more difficult, and to lag developmentally. It thus seems reasonable to argue that *Homo sapiens* 400,000 years ago could recognize symmetries before they could produce them. It is fairly likely, then, that their production was indeed intentional, and involved shape recognition.

The development of three-dimensional productions may be less clear. Golomb (1993; see also Golomb & McCormick 1995) has presented evidence for early use of three-dimensional shapes in sculpture. Arnheim (1974), however, had suggested that early constructions emerge as one-dimensional "worms," that are more likely to be flat and two-dimensional, followed by three-dimensional constructions. Golomb finds little evidence for early production of one-dimensional "worms," but rather, sees three-dimensional sculptures produced alongside flat constructions during development. My own findings in children's constructions (Humphrey 2002) suggest a sex difference in the development of the production of three-dimensional constructions. While younger girls (under eight years-of-age) make more three-dimensional constructions than do younger boys, after eight years-of-age boys are making far more three-dimensional constructions than are girls. Neither sex made one-dimensional constructions in my study.

Interestingly, Wynn refers to many authors who study sex differences in spatial cognition, but skirts the issue of sex differences in knapping and of the spatial abilities required for knapping. If we knew which sex had done the stone knapping it would enhance our understanding of the cognitive and perceptual abilities entailed. He suggests that early knapping was more two-dimensional in shape than were objects knapped later. This sequence does not resemble the ontogenesis of three-dimensional constructions, except perhaps for some boys. We need to know how much temporal overlap there is in the evolution of the production of three-dimensional and two-dimensional tools.

It seems clear that the symmetrical tools made 400,000 years ago were made with symmetry in mind. Other interesting questions of cognitive evolution remain unanswered, such as the sequence of design and construction evolution between the making of symmetrical tools 400,000 years ago and symmetrical decorative markings 70,000 years ago (Henshilwood et al. 2002). The role of imitation and mirror neurons (Rizzolatti et al. 1997) in the ability to make symmetrical tools might also be important.

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## The explanatory limits of cognitive archaeology

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**Abstract:** I make two claims about cognitive archaeology. I question its role, seeing psychology as yet another contributor to the archaeological tool-kit rather than as something unique. I then suggest that cognitive archaeology is not in a position to provide evolutionary contexts without other disciplines. As a consequence it cannot deliver on the provision of evolutionary contexts for cognitive evolution.

Thomas Wynn's claim for cognitive archaeology is that it can contribute to our understanding of cognitive evolution by providing information about the times and context of the evolution of cognitive faculties present in *Homo sapiens.* I take issue with two facets of this general claim: (1) cognitive archaeology's role in archaeological practice generally; and (2) the ability of cognitive archaeology to provide information about the context for cognitive evolution.

Archaeology is the business of reconstruction. It takes the traces of past activities and attempts to reconstruct past behaviours from those traces. And, as Wynn points out, archaeology has developed certain techniques and concepts for achieving this. Many of those ideas, theories, and methodologies are borrowed from other disciplines, mostly anthropology and ethnography, but also art history, economics, and biology. These "borrowings" help frame questions that can guide research projects in archaeology and aid the reconstruction process.

Naturally, some of the arguments within archaeology are to do with the appropriateness of these methods of interpreting remains, and the validity of reconstructions. Equally naturally, archaeologists cluster depending on their preferences, and as a consequence there has been a proliferation of archaeological schools, such as behavioural archaeology, interpretative archaeology, processual archaeology, selectionist or evolutionary archaeology, and so forth.

So, one way of viewing cognitive archaeology is that it adds new techniques and concepts to the archaeological repertoire – the techniques and concepts of cognitive science. If we interpret cognitive archaeology in this way, then an archaeological practice that takes into account the psychological dimension of the subjects under study aids archaeologists, rather than evolutionary psychologists. Importantly, it should be able to help constrain explanatory hypotheses regarding the changes in material culture. If we can say that *Homo erectus* was incapable of some cognitive trick, but archaic *sapiens* were capable, then we have helped provide some insight into our evolutionary past, and potentially eliminated some evolutionary scenarios. So long as cognitive archaeology stays testable, it provides archaeologists with further means of assessing their reconstructions of the past.

The inclusion of cognitive science in the study of human evolution is a particularly important addition to the tool-kit. There has been a tendency to assume that hominid minds are blank slates capable of whatever the particular researcher wants them to be capable of. Frequently, researchers have noted the increased cranial capacity of hominids, and inferred increased behavioural flexibility as a direct consequence. Saying that hominids brains got bigger, and assuming a raft of flow on effects from such increases – everything from better hunting abilities and larger social organisation right through to the emergence of language – has been a somewhat frequent failing of paleoanthropologists. By including concepts from psychology in the reconstruction of past behaviours, archaeologists and paleoanthropologists give themselves access to new insights, and can potentially cross-check their reconstructions against independently testable, plausible, psychological constraints. The possibility of timing cognitive developments is particularly important, but so too is being able to assess actual capabilities. For instance, reconstructing changes in resource use has always been problematic. We can improve our chances by framing various resource use hypotheses, analysing the cognitive requirements of them, and matching them to archaeological data.

So the role of a cognitive archaeology should not only be to provide for time and context, it should also help to explain and secure our general understanding of the evolution of *Homo erectus* and other human ancestors. This first issue is really a cheer for cognitive archaeology. It should be an active contributor to the project of understanding human evolution, rather than just clarify adaptive stories for evolutionary psychologists.

However, in order to be an active contributor to reconstructing the past, cognitive archaeology has to be well integrated with other disciplines that play a role in archaeological reconstructions. Wynn's view of this integration is problematic in light of his claim that cognitive archaeology should be able to provide the context for cognitive evolution. Now, I interpret this claim to mean that cognitive archaeology can provide some idea as to the evolutionary context for a cognitive adaptation; certain selective pressures were present, and a certain cognitive capability enhanced an organism's fitness. Yet, the evolutionary context is the most unsatisfying part of Wynn's article.

For example, Wynn's context for the emergence of new cognitive features associated with spatial manipulation and shape recognition is that of a species which was responding to sexual selection, or to selection for improved navigation. But if either of these is right, how does this produce the cognitive feature upon which Wynn focuses? Wynn's context never connects to the specific uses of the stone tools in question. He provides us a good broad picture of the ecological context of *H. erectus,* but he never really tells us about the context for the tools themselves. For that he needs to provide a description of how the tool was used, and how it fitted into the everyday life of the hominids concerned. Were these tools created and re-used, or were they single use items?

To demonstrate where I think Wynn went astray and why I find his contexts unconvincing, we need to look at the way he frames his questions. Dan Sperber distinguished between an Actual Domain for cognitive faculties, and a Proper Domain (Sperber 1997). The Actual Domain is the area of behaviour or information where a cognitive faculty gets used. The Proper Domain is the area of behaviour or information that a cognitive ability evolved to deal with. So, for the adaptive context of a cognitive evolutionary event we need to identify its Proper Domain. It is not at all clear how to do that in the particular case of symmetry.

My concern is on two fronts. First, there is the issue of how we test claims about the evolutionary context of a cognitive adaptation. What are our means of checking its plausibility? The second, is Wynn's failure to identify the specific nature of the cognitive adaptation. Is the behaviour identified in the archaeological record a by-product of other adaptive behaviours, or is it the primary adaptation?

To understand a tool or artifact beyond its method of manufacture, we need to know its role within the subsistence economy of the user. Wynn has started this, but the tool's use does not seem to be connected to the cognitive faculty being examined. It is a cognitive faculty used in the manufacture of a tool, not in its everyday use. In fact, it is used in only one aspect of its manufacture; the imposition of symmetry.

To determine the context for a cognitive adaptation associated with this particular aspect of tool manufacture we should not ask "Why is this cognitive feature adaptive?" as Wynn seemed to do. That is too general. Rather our questions ought to be: "Why is imposing symmetry important in stone tool making? What's so great about symmetry in tools that it is worthwhile investing in cognitive structures to make sure tools have it?" These are very specific questions about the structure of stone tools and their manufacture. If the answer to these questions is "Nothing, symmetry serves no functional role in enhancing the benefits of a stone tool," then the context for the evolution of the associated cognitive feature, its "Proper Domain" in Sperber's parlance, hasn't been found. We may have found one of the actual domains for a cognitive faculty, but we have not identified the reasons for its maintenance within a population. No amount of studying bifaces with the tools of cognitive science will determine the selective forces that maintain these cognitive faculties within a population.

The only context that Wynn provided for the supposed adaptation which came close to asking "why symmetry?" was the hypothesis of Kohn and Mithen, namely, that the symmetry of biface handaxes played some role in sexual selection (Kohn & Mithen 1999). Other possibilities offered – navigation and shape cognition – saw artefact symmetry as by-products of other selective forces. These alternatives are not contexts that shaped the adaptation. The making of symmetrical stone tools may have been within the Actual Domain of the cognitive faculties, but is not the Proper Domain, its evolved function.

So, Wynn's catalogue of adaptive pressures doesn't distinguish between the Actual and the Proper Domain of the cognitive function. Admittedly, that's always going to be difficult. Evolutionary psychologists have access to working minds; they can examine behaviours that leave no physical traces. Archaeologists only have access to behaviours that leave some kind of lasting physical trace on the world. Should the Proper Domain of a cognitive function not be such a behaviour, then the archaeologist can only point to the behaviour's presence, not its adaptive function.

The ability to test hypotheses about cognitive evolution's context is constrained by the archaeologist's ability to precisely reconstruct the specific benefits of a behaviour. In order to make the claim that a cognitive function was specifically for a particular behaviour, rather than all behaviours associated with the cognitive ability, the burden of proof lies on the reconstruction of the particular application's fitness-enhancing qualities, not the cognitive faculty applications in general. To put this in another way, suppose we suspect that a cognitive faculty can be used in multiple ways, it has multiple actual domains. If that is the case, then in order to determine which domain is the proper one, we have to precisely reconstruct the evolutionary pressures on the various actual domains to determine evolutionary context. So we need to look at the various applications of the cognitive ability, rather than the cognitive ability.

In many cases we will not have a clear picture of all the Actual Domains that a cognitive function operates in. If a behaviour leaves no physical evidence, we can't see it. Consequently, many hypotheses about the Proper Domains of cognitive abilities will be untestable. But in the example used by Wynn, he had unambiguous access to one domain, the imposition of symmetry on stone tools. What he needed to determine was whether that particular application of the mental faculty could in fact be the Proper Domain.

I think that Wynn could have done this, but he has ignored other tools in the archaeologist's tool-kit which would have helped: in this case, two distinct contributions from other sciences. On the one hand, Wynn needed to use concepts and techniques related to the physical makeup of stone tools. So, input from material scientists and engineers is needed to ask crucial questions about the tool's shape. Does symmetry do anything for the tools' functionality? The other contributing discipline would be behavioural ecology: Is there some cost and benefit to the hominid from making tools in this way?

Imagine for a moment that there was some structural property of the tool that meant that symmetry was a useful indicator of quality. Perhaps the nature of the materials meant that symmetry was an indicator of a well-made tool, with minimal surface re-entrants reducing the risk of mechanical failure, the tool breaking through cracking during use. So increased symmetry somehow tracked the structural integrity of a tool. In this situation, the life-span of the tool would be increased, and less time would be spent by an individual remaking tools. The toolmaker would accrue the benefits of the tool while the costs and potential risks of injury from tool making (Schick & Toth 1993) would be decreased. Any hominid that could maximise symmetry in such a situation would be better off, and we could talk confidently about the increased fitness that would accrue to the possessor of the cognitive feature. Note here that this doesn't rule out the Kohn and Mithen suggestion outlined by Wynn that symmetry played a role in mate assessment. It just suggests that tool symmetry was a genuine indicator of fitness on which sexual selection could hitchhike.

This kind of analysis would provide us with the specific evolutionary context for the application of a cognitive faculty, by assessing the benefits and costs that result from behaviours enabled by a new cognitive structure, rather than attempting to assess the benefits of the cognitive structure per se. New cognitive architectures allow organisms to behave in new ways, and it is the behaviour that bears the brunt of the selective environment, not the cognition. Wynn lost the link between the environment and the mental process, the behaviour that the cognitive process enables. Cognitive archaeology has much to offer archaeologists, and it should enhance the archaeological tool-kit in reconstructing past behaviours. But in order for cognitive archaeology to achieve its potential, it has to know when it can't provide the answers and other disciplines can. Psychology helps clarify what minds can do, but the adaptive context of minds isn't a question that psychology can answer. Without other archaeological tools for reconstructing the past, in this case behavioural ecology, cognitive archaeology is incomplete.

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## Deriving intentionality from artifacts

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**Abstract:** Cognitive psychologists tend to treat intentionality as a control variable during experiments, yet ignore it when generating mechanistic descriptions of performance. Wynn's work brings this conflict into striking relief and, when considered in relation to recent neurophysiological findings, makes it clear that intentionality can be regarded mechanistically if one defines it as the planning of distal effects.

Wynn argues that the traditional temporal schemes of archeologists (e.g., bronze age, iron age) do not work well for the cognitive archeologist. Instead, he utilizes the conceptual framework of cognitive psychology and dimensionalizes mental evolution in terms of the spatial cognitive abilities necessary to produce symmetry within artifacts. Before granting cognitive content to such symmetries, however, he first works to determine whether or not the symmetries were intended. The deciding factor in this decision proves to be the degree of complexity in the symmetry. That is, he assumes that the more complex the pattern of effects, the more likely the effects were intended.

Though inferences of intentionality may appear to constitute a weakness in Wynn's methodology, such inferences are almost always implicitly at work in cognitive psychology. Subjects in experiments are given "instructions," and it is assumed that by giving such instructions one has reduced or negated the impact of any "intentional" states upon the data. The artifacts (i.e., data) resulting from the experiment are then accounted for via a mechanistic language entailing terms such as "perception," "cognition," and "behavior." In short, intentionality is controlled for via experimental instructions, and although its designation as a control variable reveals its role in performance, it finds no place in the mechanistic description of how we do what we do.

At first glance, this seems appropriate. The concept of intentionality connotes a dualism that psychology has attempted to avoid for over 100 years. But more and more data are coming to the fore that indicate that instead of avoiding intentionality as a mechanism, cognitive psychology should perhaps re-think its use of the term and its relationship to concepts such as perception, cognition, and behavior. Research on the perception of behavior (Jordan & Hershberger 1989), for example, indicates that observers perceive the actions of others, not in terms of limb movements, but in terms of the pattern of effects the limb movements seem to be producing. Thus, when we describe what another is doing, we say, "he is going to the store," "she is reacting to a stimulus," or "they are playing soccer." What the other is doing in these descriptions is producing a pattern of effects in the environment. When such performance is translated into the language of cognitive psychology, it is described in terms of behavior. The term "behavior" however, carries with it the dual role of referring to limb movement from an empiricist perspective, and to "what one is doing" from a folk psychological perspective. Given our commitment to the empiricist notion that "behavior" is the only thing we can see about another, we end up asserting that what people do is move their limbs. Such usage of the term "behavior" seems to carry with it the conflict between recognizing intentionality as an aspect of performance, on the one hand, and harboring a conceptual scheme that negates its role as mechanism, on the other.

One approach that seems to deal with this issue is known as the Theory of Event Coding (TEC; Hommel et al. 2001; Prinz & Hommel 2002). TEC asserts that actions are planned in terms of the distal effects they are to produce. TEC also asserts that action planning and perception utilize common neural resources. Neurophysiological support for TEC derives from experiments that reveal populations of pre-motor neurons that are active during both the planning and the observation of action (Buccino et al. 2001; Fadiga et al. 1999).

An advantage of TEC is that it is consistent with the way we describe the actions of others (i.e., in terms of effects they are producing in the world). In addition, it provides a means of treating intentionality as a mechanism (i.e., the planning of distal effects). As a result, intentionality can play a role in scientific accounts of how we do what we do, and the term "behavior," freed of its dual role, need only refer to limb pattern.

By freeing cognitive psychology of the conflicting connotations of "behavior," TEC makes it clear why Wynn's work to establish the "intentionality" of artifacts is wholly appropriate. All cognitive psychologists, via the use of instructions, work to establish (i.e., control for) intentionality. Since Wynn is not able to instruct his subjects, however, he must "fess up" to the role intentionality may have played in the production of the artifacts he studies. TEC seems to provide a means by which cognitive psychology might do the same.

## Was early man caught knapping during the cognitive (r)evolution?

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**Abstract:** Wynn describes a revolution in cognitive abilities some 500,000 years ago, which added new sophistication to the curiosity of early man – the ability to form hypotheses. This derivative of archaic curiosity is a fundamental feature of learning, and it is our contention that the naive hypothesis testing behavior of early man will have left a distinctive trail in the archaeological record.

*Learning,* along with the basic reflexive behavioral repertoires exhibited by all organisms, is a biological imperative, which provides a "powerful evolutionary advantage" – the ability to collect, collate, and develop knowledge pertinent to survival (Claxton 1997). The study of human cognition, in its many guises, has consistently signaled that two forms of knowledge are accumulated during learning. One form is tacit, implicit, or nonconscious, whereas the other is declarative, explicit, or conscious (e.g., Anderson 1987; Polanyi 1967; Reber 1993). Evolutionary psychologists argue that "sophisticated unconscious perceptual and cognitive functions" (Reber 1983, p. 86) preceded the emergence of explicit, conscious functions by some way.

Implicit unconscious learning is seen as a gradual encoding of frequency information relevant to action-outcome contingencies (Hasher & Zacks 1979). Curiosity, a characteristic of survival in most higher organisms, including early man, was likely to have been selected for because it supported implicit learning processes. By initiating exploration and aggregation of information about the environment, curiosity would have provided valuable information, for example, when the need for an escape route arose.

The shift to new environmental niches some 1.5 million years ago and the concurrent development of primitive tools provides circumstantial evidence of the innate curiosity of early man. But evidence from the archaeological record suggests that one million years on, the existing unconscious cognitive abilities were substantially augmented by the arrival of conscious manipulation of information, bringing about a revolution in learning. Production of the three-dimensional symmetry of biface tools, such as the S-twist axes found at Swanscombe (England), required a cognitive work space or desktop to "hold in mind viewpoints . . . not available at that moment" (target article, sect. 2.5.2). Epistemologically speaking, this was an evolutionarily defining moment for *Homo,* for this work space, now most commonly described as working memory (Baddeley & Hitch 1974), brought with it the potential for speech and verbalization and the storage of verbal knowledge in an explicit, consciously retrievable manner.

One consequence of this development was that a new layer was added to the process of curiosity. The ability to manipulate information about the environment meant that curiosity began to result in hypothesis testing – the intuitive judgment of how best to accomplish a task, followed by the selection and storage of the best attempts for future performance and the avoidance of failed attempts (Maxwell et al. 2001).

In particular, the evolution of the spatial abilities of *Homo erectus,* as signaled by the record of biface development, with its increased diversity of tool symmetries and advanced complexity of manufacture (e.g., a greater variety of hammering techniques, more specific location of blows, longer sequences) indicates that explicit hypothesis testing was likely. The differences between the bent cleavers of Isimila and the S-twist axes of Swanscombe may occur because they were used for different purposes; but, just as likely in our opinion, they represent the unique hypothesis testing strategies of separate groups with the same requirement of the tool, though guided perhaps by adaptations necessary for use in the different environments. It is not surprising that the record is demarcated at roughly this time by an increased sophistication of the weapons and tools crucial to survival, as the cleverest thinkers (perhaps) tested hypotheses about the effectiveness of their implements in a search for better performance. The introduction of new materials, such as bone, wood, and antler, may reflect the search for greater power, distance, or control of performance.

This conscious derivative of curiosity is mirrored in the modern day equivalent of the battle for survival. Today's archaeological record shows that hitting implements, such as tennis racquets, have become lighter and more flexible as new materials have been experimented with. The heads have become larger and the hitting area (or sweet spot) has expanded. Grips have changed from wood through leather to toweling and now suède. All of these changes have come about in response to explicit hypothesis testing behaviors as performers have searched for improved motor output in their bid for *survival* in the rankings.

The ability to produce functional implements from new materials would have required a degree of craftsmanship in early man, just as it does today. A fundamental precursor of the skilled motor output of any craftsman is, of course, learning through repeated hypothesis testing: practice. In fact, Ericsson et al. (1993) have argued that the realization of expert motor output requires a minimum of approximately ten years of deliberate practice. Wynn argues that Paleolithic stone knappers had a degree of skill and, while they may not have been experts in the Ericsson et al. sense, it seems logical that they would, nevertheless, have refined their skills through practice.

Contemporary evidence shows that novice learners leave behind characteristic products of their hypothesis testing (e.g., commission of numerous errors, aborted attempts). Novice stone knappers should have left their own characteristic products of hypothesis testing in the archaeological record.

Most obvious should be under-worked stones, discarded by the knapper if they were incorrect or unsatisfactory. Over-worked stones may be evidence of the knapper reworking the stone, refining his technique. In order to avoid wastage, at sites where materials were in short supply, a higher degree of over-working would have occurred. Plentiful materials at these sites would have been under-worked or discarded, as wastage was not a problem. These principles have their modern day cousins in the form of the unfinished canvasses in Picasso's studio. Another observation is that the differentiation between practiced and unpracticed knappers should show up in the degree of randomness in the sequence of strikes. Practiced knappers would have followed a more predictable strike path than unpracticed knappers, or adapted more easily to flaws in the materials that they worked with. Additionally, expert knappers would have exhibited transferable skills, showing few signs of under work or over work, for example, when they changed to new materials. Finally, rare nonfunctional anomalies, such as chiseled grooves (Bednarik 1995), may indicate hypothesis-testing behaviors or the practice of particular techniques that were later applied in the production of specific items.

## Coincidental factors of handaxe morphology

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**Abstract:** Handaxe morphology is thought to be the first example of the imposition of arbitrary form. Handaxes may thus inform researchers about shared mental templates and evolving cognitive abilities. However, many factors, not related to changes in cognition (e.g., material type, function, resharpening processes), influence handaxe shape over time and space. Archaeologists must control for these factors before making inferences concerning cognition.

Wynn is without a doubt a pioneer in the study of cognitive archaeology, and his innovative approaches have inspired others to extend the boundaries of traditional archaeological inquiry (Nowell 2001). Many cognitive archaeologists, as well as those who would not describe themselves as such, have studied the Acheulian handaxe. The primary reason numerous archaeologists have sought to explain handaxe morphology and variability is because handaxe shape is thought by many to be the first true example of the imposition of arbitrary form. This form is often assumed to be the result of a shared mental template.[1] If this is the case, then it is believed that temporal and spatial changes in handaxe shape may inform researchers about evolving mental capabilities (McPherron 1994; 2000; Rolland 1986).

Arguments concerning human cognition based on handaxe shape and manufacture, made by Wynn (e.g., the target article; 1979; 1989; 1995; 1998b) and others (Crompton & Gowlett 1993; Gowlett 1996; Mithen 1994; Shick & Toth 1993), only hold true if it can be demonstrated that handaxes are indeed the result of purposeful intent. The question becomes, is the handaxe form the intended shape, or is it the unintended by-product of another goal? For example, for White (1995, p. 18), "the mental template involved in biface manufacture revolved around the idea of an adequate functional unit suited to do its job and little else. *Preferences are suggested to have existed for a circumferential working edge*" (emphasis in the original).

There is a great deal of evidence to suggest that handaxe shape may not be as intentional as commonly thought. It is important to note that while handaxe shape may not be random, "the lack of randomness does not itself necessitate forethought or conscious standards" (Dibble 1989, p. 422). It is well known that raw material variability constrains and influences handaxe shape (Ashton & McNabb 1994; Jones 1979; 1981; 1994; McPherron 1994; 2000; Villa 1983b; White 1995; Wynn & Tierson 1990). Furthermore, McPherron (2000) argues that handaxe shape is the result of resharpening processes that are quite similar throughout the Acheulian. Specifically, he observes that "handaxes begin large, elongated, pointed and relatively thick. As the biface edge is continually reworked, and as the edge expands to encompass more of the original nodule or flake blank, the handaxe becomes smaller and the shape gradually becomes broader, more rounded and relatively thinner." This observation explains, for example, the radial symmetry of the discoid bifaces. Furthermore, evidence suggests that handaxe morphology is a continuous phenomenon and cannot be divided into discrete (modal) types (Bordes 1981; Nowell 2000). Therefore, while there may be some striking examples of symmetrical handaxes, we need to place them within the larger context of the artifactual assemblage from a site as a whole.

This leads to a discussion of what Davidson and Noble (1993) refer to as the "finished artifact fallacy." Unlike the shape of a metal tool that can be melted down and recast when its shape is no longer desired, lithic artifacts carry with them the history of their use-lives. Artifacts in the archaeological record represent only the final form of the tools and the last uses to which they were put, and not necessarily a desired end-product. In addition, it is becoming increasingly clear that many factors that have little or nothing to do with evolving mental capabilities (e.g., blank morphology, raw material type, technology of blank production, blank selection, retouched tool morphology, function, resharpening processes, and the imposition of a classification system) influence tool morphology. I refer to these as "coincidental" factors of standardization and symmetry (Nowell 2000; Nowell et al., in press). As Barton (1990, p. 70) notes, "virtually all lithics found at sites entered the archaeological context because they were no longer of value to their makers and users." In most cases, assuming that the final form of a stone tool is its intended morphology, is analogous to assuming that cars in a junkyard began their use-lives as broken down heaps (Dibble, personal communication).

The implication of these observations for the present discussion is that archaeologists must take into account all of these more mundane coincidental factors that influence tool morphology before turning to changes in cognitive abilities as an explanation. For example, Wynn's contention that the twisted profile or S-shaped handaxes from Swanscombe and the bent cleavers from Isimilia represent an intentional violation of symmetry requires further examination. The S-curve handaxes and bent cleavers are a consequence of a particular sequence of blows used to create the specimen. Striking alternating faces creates a zigzag edge that is gradually smoothed out as long as the knapper continues to turn and strike the piece (i.e., alternating edges). With Wynn's example it is the raw material that is responsible for the saliency of this phenomenon. The twisted S-shape shows up better with fine-grained material. If, when making a handaxe, the knapper works down one side, then the other face on the same side, this S-curve is not created. Therefore, these shapes can be explained simply as a result of the flaking technique chosen and not necessarily as the result of an intentional violation of symmetry (Bisson, personal communication; Nowell 2000).

NOTE

**1.** The term mental template is being used here in the conventional sense that most archaeologists use the term – namely, that it is a preconceived idea in the mind of the knapper of the exact type and shape of tool that he or she desires to knap.

## Locating early *Homo* and *Homo erectus* tool production along the extractive foraging/cognitive continuum

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**Abstract:** This commentary contests Wynn's diagnosis of the cognitive implications of the earliest stone tools and Acheulian tools. I argue that the earliest stone tools imply greater cognitive abilities than those of great apes, and that Acheulian tools imply more than the preoperational cognitive abilities Wynn suggests. Finally, I suggest an alternative adaptive scenario for the evolution of hominid cognitive abilities.

In his target article, Wynn takes on the challenging and inherently speculative task of diagnosing cognitive implications of various hominid tool technologies. First, he argues that the earliest worked stone tools, appearing about 2.5 million years ago, reveal no clear advance in the spatial cognition of early *Homo* as compared to that of apes, Although, as he notes, chimpanzees apparently are capable of topological notions of space characteristic of 3- to 4-year-old human children (e.g., Parker & McKinney 1999; Russon et al. 1996), it is important to note that they differ from humans in some aspects of spatial and logical cognition: (1) their cognitive development is asynchronous across domains (Langer 2000); (2) they do not arrange objects sets on the ground after touching them together in the air (creating "mobile" as opposed to "stabile" sets) (Poti' 1996); moreover, (3) as Köhler noted, chimpanzees do not understand gravitational relations between static objects such as two stacked boxes or a ladder and a wall. (Interestingly, Köhler associated the contrasting human ability to orient objects above and below each other in space with humans' habitual upright posture.) Moreover, it is difficult to believe that bipedalism, (two-times) larger brains; and stone tool manufacture and use by early *Homo* (Conroy 1997) entailed no cognitive differences from great apes who show none of these characteristics. Therefore, I am skeptical about Wynn's conclusion that the manufacture and use of Mode I tools entailed no obvious leap in the intellectual abilities of early *Homo*.

Some of the earliest worked stone tools are found with the new species, *Australopithecus garhi* from East Africa, dated about 2.5 million years ago. These tools are associated with cracked long bones of antelope (Asfaw et al. 1999a; 1996b) providing the first clear evidence of bone marrow extraction, presumably associated with scavenging (Blumenschine et al. 1994; Blumenschine & Selvaggio 1994). This new form of foraging – associated with the invention of the first worked stone tools – opened up a rich source of fat and protein. Similar practices probably occurred in early *Homo* species, *H. habilis* and or *H. rudolfensis,* which some paleoanthropologists reclassify as Australopithecines, based on their small brain size and primitive bipedalism (Wood & Collard 1999).

I think at least three cognitive advances over apes are implied by early *Homo*/late *Australopithecus*' exploitation of bone marrow: (1) *the use of tools to make tools* (M. Leakey 1971); (2) extension of tool use to acquire a cryptic and hazardous new class of embedded foods; and (3) extension of apprenticeship to tool making and tool use in extracting scavenged food. Stone tool manufacture implies additional steps in planning (Parker & Milbrath 1993), and some projective notions of sharpness and angle characteristic of late preoperational cognition of human children 4 or 5 years-of-age (Parker & Gibson 1979; Piaget & Inhelder 1967). Stone tool manufacture and use in the potentially hazardous enterprise of bone cracking suggests specialized apprenticeship of adolescent males by adult male tutors (supplementing the kind of mother-offspring apprenticeship in nut cracking and termite and ant fishing seen in chimpanzees [Parker 1996]).

Second, Wynn argues that about 1.4 MYA East African hominids, presumably *Homo erectus,* associated with an adaptive shift to open country, began to manufacture a variety of large tools whose overall two-dimensional shapes were characterized by various forms of symmetry. He argues that production of these tools entailed coordination of visual perspectives. He also argues that at about 500,000 years ago, the late Acheulian tools suggest three developments of imposed symmetry, congruency, three-D and broken symmetry, which imply intuitive Euclidean concepts of space (which he says is characteristic of the late preoperations period in human children of 4 or 5 years-of-age). In contrast to Wynn, I think that these spatial concepts may have been operational, that is, characteristic of the cognitive level of 6- or 7-year-old human children. Sectioning and rotation of solids and coordination of perspectives certainly imply concrete operations (Piaget & Inhelder 1967). I agree with Wynn that the (one-third) larger brains and longer period of immaturity of *H. erectus* as compared to australopithecines and early *Homo* (Walker & Leakey 1993) are consistent with the idea that these hominids were more cognitively sophisticated than their predecessors (Parker 2000), but consistent with my diagnosis of late preoperational intelligence in early *Homo,* I expect *H. erectus* cognition to have exceeded that level.

Wynn briefly suggests three selective forces that might have favored hominid spatial concepts: (1) selection for navigational skills; (2) selection for appreciation of symmetry in tools derived from assessment of symmetry as an index of developmental health; and (3) mate choice for the capacity for symmetrical tool production. All these proposals lack the phylogenetic analysis, evolutionary continuity, and heuristic power (Parker & McKinney 1999) offered by the extractive foraging hypothesis (Parker & Gibson 1979), which Wynn cites but doesn't discuss.

Twenty-three years after Gibson and I published our extractive foraging hypothesis for the origins of hominid intelligence, new evidence supports the claim that hominoid tool cultures and subsistence modes fall along an *extractive foraging/cognitive continuum:* (1) from seasonal extractive foraging with tools for such embedded foods as termites and ants and nuts by the common ancestor of chimpanzees and hominids, (2) to year round extractive foraging with tools on a greater variety of foods by australopithecines, (3) to simple stone tool manufacture and use in extracting marrow from scavenged bones by early *Homo,* (4) to Acheulian tools and extraction of meat through butchery of large game, and excavation of underground storage organs (USO) by *H. erectus* (O'Connell et al. 1999), (5) to hunting/puncturing big game with aimed projectiles by archaic *H. sapiens.* According to the extractive foraging model, each new adaptive radiation of hominids has maintained and elaborated on the extractive technologies of its predecessors. Likewise, each new technology entailed more sophisticated physical, logical, and social cognition. In contrast to other models, this model interrelates evolutionary potentialities and continuities in subsistence technology and cognition in the hominoid clade.

## Reasons for the preference for symmetry

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**Abstract:** Why did *Homo erectus* begin to craft symmetric tools? A parsimonious account assumes that preference for symmetry is inherent in all visual systems. This preference can be explained by a broader preference for perceptual fluency. The perceptual fluency account does not assume that selection for mate health or the production of symbolic art is a prerequisite for symmetry preference.

What are the possible explanations why *Homo erectus* began to form symmetric stone tools some 1.5 million years ago? One possibility is that hominids imposed symmetry on stone tools because symmetry in faces and bodies of prospective sexual partners signals health (e.g., Thornhill & Gangestad 1993). Wynn acknowledges that this is a provocative explanation. Specifically, any theoretical account that assumes that the production of symmetrical tools is caused by preference for symmetric features in sexual partners has to explain how the more general preference for symmetry in stone tools emerged from the very specific preference for face and body symmetry. Moreover, the notion that human facial attractiveness – which includes symmetry – signals health is one that has been challenged recently (Kalick et al. 1998). I propose a more parsimonious explanation that assumes that a general preference for symmetry is due to a hidden preference inherent to all sensory systems.

Symmetry has been found to determine facial attractiveness (e.g., Gangestad et al. 1994; Rhodes et al. 1998). Importantly, humans prefer symmetry in nonmating contexts (e.g., Humphrey 1997). This suggests that *Homo erectus* might have begun to form symmetric tools just for pleasure of the eye. By means of computer simulation, it has been shown that preference for symmetry may be a hidden preference inherent to all sensory systems and is not necessarily linked to selection for mate health (e.g., Enquist & Arak 1994; Johnstone 1994). This suggests that preference for symmetry is a by-product of general properties of visual systems (see Enquist & Johnstone 1997).

Preference for symmetry can be explained by a broader preference for perceptual fluency (Reber et al. 1998; Reber & Schwarz 2001; Whittlesea 1993; Winkielman & Cacioppo 2001). Perceptual fluency is the subjective ease with which an incoming stimulus can be processed (see Jacoby & Dallas 1981). In several experiments, Reber et al. (1998) manipulated perceptual fluency by changing physical features or the use of priming procedures and found that in each case, perceptual fluency was affectively positive. More specifically, simple shapes were liked more if they had higher figure-ground contrast, were presented for a longer duration, or were preceded by matching rather than nonmatching primes. Winkielman and Cacioppo (2001) replicated these findings and provided physiological evidence for the link between high fluency and positive affect. Findings by Palmer (1991) support the notion that symmetry may be based on preference for perceptual fluency. Reaction time studies have revealed that vertical symmetry is easier to detect than horizontal symmetry, which in turn is easier to detect than diagonal symmetry (Palmer & Hemenway 1981; Royer 1981). Palmer (1991) presented dot patterns in vertically, horizontally, or diagonally symmetrical arrangements. He thus manipulated ease of processing while controlling for symmetry (and amount of information). Stimuli that had vertical symmetry received higher ratings of figural goodness than identical stimuli that had horizontal symmetry, which in turn received 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 Winkielman 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 of 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 detectible 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 $\sim$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.

## Symmetry and human spatial cognition: An alternative perspective

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**Abstract:** Wynn's thesis that the acquisition of the rules of symmetry comprised the formative factor in the evolution of human spatial cognition is questioned on several grounds, including the ubiquity of symmetry across species and the apparent hard-wired nature of its evolution in both humans and animals.

Mithen (1996) posed the rhetorical question, "Why ask an archeologist about the human mind?" to which he later replied, "we can only understand the present by knowing the past" (pp. 9–10). In its broadest sense, this is an uncontestable statement. But the archeological record is highly selective, which may readily bias any general theory about the origin of human cognition based on evidence from artifacts. Inasmuch as stone tool fragments provide the most copious, continuous, and concrete human archeological data, and symmetry is a cardinal attribute of tool making, it should not be surprising to find a theory placing symmetry at the core of human spatial cognition. If wayfinding, or migratory patterns, or the capacity for future planning provided as durable and detailed records as stone tool making, the perspective might be quite different.

That said, we might nevertheless ask if Wynn has made his case? The evidence that symmetry and efficacy in stone tools were closely associated in hominid evolution is convincing, but does not necessarily imply the direction or even the fact of a cause-effect relationship. Nor does it render the proposition evident that symmetry was the essential adaptation in the evolution of human spatial cognition. Thus, we are left to consider the validity of the proposition itself.

First, we might ask whether it is efficacious to seek a common element underlying all of human spatial cognition. The prevailing theory of evolutionary psychology emanated from the concept of modularity, particularly as applied in Cosmides and Tooby's (1992) model of mind as a collection of domain specific mechanisms that evolved in response to separate selection pressures. Mithen (1996) provided archeological support for the modularity model within his theory of "specialized intelligences." Wynn's theory, however, represents a return to the domain general approach, in that he regards the acquisition of the rules governing symmetry as basic to the evolution of human spatial cognition in all of its aspects.

Competing theories are, however, the stuff that moves science; hence, the question that follows is whether it is feasible to regard symmetry as the essential element in the evolution of human spatial cognition. My own doubts on this point stem from the premise that human thought processes are qualitatively and profoundly different from other species, which leads to the assumption that any attribute fundamental to the evolution of modern human cognition will be unique to humans. Accordingly, domain general models of human cognitive evolution tend to focus on distinctly human capabilities, such as language, self-reflection, abstract thought, manipulation of symbols, and so on. Symmetry, on the other hand, in all of its forms and functions, is ubiquitous in the animal kingdom. It is found in both invertebrates and vertebrates, in their edifices, migrations, formations, calls, songs, courtship rituals, and even as cues to developmental stability in mate selection.

Wynn might answer that in contrast to humans, behaviors reflecting symmetry are "hard-wired" in animals, in that they entail relatively little in the way of learning or generalization of rules. In fact, he makes frequent reference to the "idea" or "intention" of symmetry as the distinguishing factor between both animals and early hominids compared to modern humans. But is this a fair distinction? Does a person getting into position to catch an object in the air possess a more refined concept of rotational symmetry than a canine doing the same? Does a tribal drummer know more about translational symmetry than a songbird? Of course, unlike animals, humans can *learn* the rules of symmetry in the abstract, but there is nothing to suggest that this higher order capacity is or ever was required for the acquisition of day-to-day, symmetry-related perceptions and behaviors.

Wynn leaves it largely to the reader to intuit what is meant by the "idea" of symmetry, but does offer a brief description of the underlying mental processes. He proposes that in order to construct the fully symmetrical, three-dimensional stone tools of the late Paleolithic, hominids must have had the capacity to "hold in mind viewpoints that are not available at that moment." He contends further, citing our own work (Silverman et al. 2000) as support, that this capacity is reflected in three-dimensional mental rotations tasks (whereby subjects determine whether individual three-dimensional test figures are the same as a target figure viewed in a different position).

Our conclusion, however, was that those three-dimensional mental rotations tasks could *not* be effectively solved by analytic means, but only by the simple and concrete process of visually rotating the figures. In fact, we have found seven- and eight-year-olds who are more facile in this task than the average university student. From these data, and the findings that three dimensional mental rotations occupies a separate factor among other spatial tasks, we asserted that this ability was "no more part of a general domain of spatial reasoning than the capacity of an infant to avoid crawling over precipices or a squirrel to leap between branches" (Silverman et al. 2000, p. 211).

The phenomenon of shape constancy may also be relevant to the question. This refers to the innately based tendency to retain the perception of bilateral symmetry of squares and rectangles, even when they are viewed from the side and appear on the retina as trapezoids. Ames created two perceptual illusions demonstrating shape constancy, the Rotating Trapezoidal Window and the Distorted Room (Ittelson 1952). The trapezoidal window is fashioned so that one side always remains longer in length during rotation; thus, the window cannot be perceived simultaneously as rotating and having the bilateral symmetry of a rectangle. The conflict is unconsciously resolved by the viewer in favor of maintaining symmetry, by perceiving oscillation rather than the actual rotation. The distorted room is viewed by the subject from a small opening, and is constructed as a normal room appears on the retina from that perspective. Windows, doors, walls, and the floor and ceiling are trapezoidal, with vertical borders decreasing in size as a function of distance from the viewer. The viewers' customary perceptual adjustments to restore symmetry when entering an ordinary room are simply enhanced by these added distortions; hence, the room and all within it are perceived as normally shaped.

Both of these illusions persist even when viewers are made aware of the true nature of the stimuli and the bases for their misperceptions. Furthermore, subjects will experience percepts that defy reality when necessary to retain the illusions. Hence, a stick hanging inside the window will continue to appear to rotate while the window appears to oscillate, and will be perceived as moving through the solid surface of the window frame. People crossing the far wall of the distorted room will be perceived as becoming either taller or shorter, depending on the side from which they began.

In one sense, these demonstrations support Wynn's general contention about the salience of symmetry. In another, however, they provide additional evidence of its modular character and dissociation from reason.

## Tools evolve: The artificial selection and evolution of Paleolithic stone tools

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**Abstract:** I claim that the increase in complexity in the (known) trace of Paleolithic stone tools can be parsimoniously explained by postulating the emergence of effective mechanisms for the social transmission of representations. I propose that Paleolithic tools, similar to more contemporary tools, were subject to a process of evolution by artificial selection based on functionality.

Wynn puts forward a theoretical account for the increase in the complexity of Paleolithic stone tools by associating each major improvement in tool manufacture (mostly in symmetry type), with the emergence of a new cognitive skill. This perspective on the evolution of human cognition, where behavioral changes are mapped in a straightforward way to changes in cognitive architecture, is problematic because it leads to a view where each substantial innovation must be explained by the emergence of a new neural-cognitive skill. This becomes particularly unsatisfactory for later stages in human evolution where the diversity and sophistication of artifacts increases dramatically – most notably, in the Upper Paleolithic. Therefore, invoking the emergence of new spatial reasoning skills to explain the origins of symmetrical stone tools begs the question of how to explain other recorded innovations (e.g., tools with multiple components [Mithen 1996]). Moreover, the evidence presented by the author does not exclude the possibility that apes are also capable of similar kind of spatial reasoning, but are unable to produce symmetrical stone tools due to a constellation of other reasons.

An alternative, and more parsimonious, interpretation of the known trace of Paleolithic stone tools is postulating the gradual emergence of a combination of factors and mechanisms that enabled the effective social transmission of representations (Boyd & Richerson 1985). Namely, representations coding for functional sensorimotor couplings used in the copy-production of tools – such as motor sequences and routines – probably along with communication protocols (not necessarily verbal) that facilitated the transfer of knowledge between expert tool-makers and learners (Dautenhahn & Nehaniv 2002). In this hypothetical scenario, paleo-tools may have been subject to a process of evolution by artificial selection based on functionality and efficiency: that is, the selective recreation of tools depending on how well they fitted their target function(s). Such a process, coupled with the occasional improvement in tool design (sometimes resulting only from the variation in their production), is sufficient to generate the recorded increase in tool complexity over a sequence of many generations of tools and tool-makers – both in earlier and later stages of human evolution.

This is not a surprising hypothesis. In many, if not most, ancient and contemporary tools for which the historical development has been recorded, we can identify similar evolutionary processes taking place (e.g., jet engines; see Gunston 1998). In contrast with Wynn's perspective, in this alternative view, new features of paleo-tools are not explained as a result of directly related changes in neuropsychological mechanisms of human ancestors – which somehow made those new tools cognitively feasible and/or understandable. Rather, they resulted from a process of cultural retention and transmission, where cultural artifacts were trans-generationally preserved (Tomasello 1999/2001). For example, symmetry in bifaces can be functionally explained by noticing that symmetry puts the center-of-mass of the tool in the line corresponding to direction of motion of the tool at the instant of impact – thus avoiding torque and, consequentially, maximizing power. Additionally, more regular surfaces distribute the reaction force at impact time more evenly through the hand of the tool's user, which increases comfort. Since more symmetrical bifaces tended to be more regular, they are also more ergonomic. (Both of these facts can be easily checked by inspecting the behavior of modern objects with comparable shapes.) This means that more symmetric tools would have been more likely to be recreated and used by human ancestors, something that over the eons led to the recorded tradition of ever more symmetric tools. Although it may be useful to discretize the paleo-tool's trace for taxonomical purposes, by demarcating a few periods and transitions of special relevance, the evidence presented by the author shows that the trace is essentially continuous. This suggests that a more or less gradual evolutionary process took place.

A possible objection to using the above account to explain the evolution of earlier Paleolithic tools is the claim that the effective social transmission of complex representations emerged later in human evolution – say, during the last million years – and, therefore, was not available to *Homo erectus* and earlier hominids. In support of the opposite hypothesis, though, considerable neurological evidence has been gathered during the last decade or so which (indirectly) suggests otherwise. Specifically, it is now widely believed that most (if not all) of the functional specialization of cortical brain areas during vertebrates' (including humans) development is not determined by internal constraints (e.g., some kind of genetic-molecular markers), but rather emerges as a result of a self-organizing process of statistically partitioning the complex information structures presented by the environment (mediated by the individual's physical embodiment) (Edelman 1987; Elman et al. 1996; Reed 1996). This said, it is plausible that a substantial part of the long juvenile period of *Homo erectus* (estimated to have been fully grown between 14 to 16 years of age; Dean et al. 2001) was dedicated to incorporate in its enlarged neural substrates behavioral routines observed and performed by conspecifics. Even if one is willing to contest this hypothesis, by arguing that much of the internal neural encoding that shaped the adult's brain occurred as a result of the individual's self exploration and learning (as opposed to a more caregiver assisted, social type of learning), this individual learning necessarily took place in the context of the juvenile's developmental niche – which would include all the artifacts of its primitive culture (Laland et al. 2000). It is thus unlikely that developing brains would discard such rich sources of information, "reinventing" the same new set of tools afresh in every new generation. Furthermore, the presence of such a complex niche would have the effect of further increasing the selective pressure to use the available information. A speculative, and rather minimalist hypothesis is that brains were selected to incorporate new motivational components (drives) to attend and produce motor sequences that replicate observed conspecific's behavior – that is, to develop a *true cross-modality imitation* ability (Dautenhahn & Nehaniv 2002).

Likewise, it has been proposed by others that the evolution of the morphological and neurological requirements for the acquisition and production of language might also have benefited from similar self-reinforcement processes, where more complex structures in the environment (in this case, sound patterns) coevolved with the ability to use/produce them (Deacon 1998). Thus, a more general point here is that although the increment in the complexity of Paleolithic tools did not have a linear correspondence with (chronological) time, this does not require us to postulate that a set of almost punctuated events occurred during human evolution (such as, the acquisition of the ability for complex spatial reasoning). The fact that we can record an exponential (super-linear) increase in the complexity of the material and symbolic cultures of the human lineage, might just be the expected outcome of having the cultural generation and transmission infrastructures coevolving with the complexity of the cultures themselves (Donald 1993), a process that may be operating until modern days.

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## Footloose and fossil-free no more: Evolutionary psychology needs archaeology

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**Abstract:** Evolutionary theories of human cognition should refer to specific times in the primate or hominid past. Though alternative accounts of tool manufacture from Wynn's are possible (e.g., frontal lobe function), Wynn demonstrates the power of archaeology to guide cognitive theories. Many cognitive abilities evolved not in the "Pleistocene hunter-gatherer" context, but earlier, in the context of other patterns of social organization and foraging.

Wynn's target article on cognitive archaeology brings a much needed perspective to research on the evolution of human cognitive mechanisms. Theories in evolutionary psychology often present hypotheses about adaptive pressures that shaped psychological abilities without referring them to specific times in the hominid past. Wynn adds precision to the definition of the "environment of evolutionary adaptedness" (EEA) for spatial cognition and tool manufacture, and questions whether our early Pleistocene ancestors were at all adapted to a hunter-gatherer lifeway. Whether or not Wynn's theory of the cognitive skills required for tool manufacture is correct, his work represents an often-missed step in developing evolutionary theories of cognition. Below, I outline the steps involved in developing such theories, and discuss Wynn's contributions within that framework.

Cosmides and Tooby (1987; 1992) have outlined the usefulness of Marr's (1982) computational theory approach to developing theories of cognition. There are several steps involved:

1. Specify the adaptive function of the computation, that is, what is it that having this cognitive ability allows us to do?
2. Identify the time period during which that adaptive problem existed.
3. Identify the EEA, the relevant selection pressures that prevailed during that time.
4. Propose a set of processes and representations that could serve the identified function. These must be powerful enough to solve the problem.
5. Make predictions about patterns of behavior the proposed computations would generate.
6. Devise tests between alternative theories that could explain the same pattern and one's own computational theory.

Though not subscribing to this framework, Wynn emphasizes the power of using archaeology for steps 2 to 3; in his words, defining the *timing* and *context* of developments in human cognition. Archaeology can also make contributions to the other steps.

For all its emphasis on evolutionary forces, evolutionary psychology seldom discusses the archaeological record of hominid evolution. Wynn shows us why archaeology is necessary. Evolutionary psychologists refer frequently to the EEA for humans, usually characterized as the selection pressures acting on "Pleistocene hunter-gatherers" 2,000,000–10,000 years ago, who are modelled as being like current hunter-gatherers.[1] However, the definition of the EEA for a particular adaptation is the set of selection pressures that occurred while that adaptation was evolving (Tooby & Cosmides 1992); thus not all cognitive mechanisms have the same EEA. Developing a computational theory of the adaptive function of a mental process requires specifying the conditions that prevailed while it was evolving. Knowing those conditions depends on archaeology.

Wynn never uses the term "EEA" but does define the time frames for particular adaptations in spatial cognition, which is crucial for identifying the relevant selection pressures. The period of adaptation for an ability predates appearance of the fully developed ability. Thus, if the spatial skills required for making Mode 1 stone tools are present in other apes, then the EEA for these skills includes conditions present for Miocene apes. (However, the recent finding that crows spontaneously impose shape on tools raises questions about whether this skill is unique to primates; Weir et al. 2002.) The EEA for imposing bilateral symmetry in toolmaking comprises those selection pressures acting on *Homo habilis* and *erectus* 2.5–1.5 million years ago, from when flaked stone tools first appeared to when clear evidence of symmetry appeared. The EEA for imposing more elaborate forms of symmetry includes the changing selection pressures acting on *Homo erectus* and archaic *Homo sapiens* 1.5–0.5 million years ago. *Homo erectus* occupied a wider variety of habitats than earlier hominids – Africa, Asia, Europe (Vekua et al. 2002) – and foraged but did not hunt large game. The major selection pressures acting on *Homo erectus* seem to have been those of foragers moving into new habitats with unfamiliar food resources. Archaic *Homo sapiens,* in contrast, were big game hunters, and faced somewhat different selection pressures.

Wynn wrestles with a difficult problem in doing steps 1 and 4, above. Steps 1 and 4 are related: Knowing the function of this new spatial ability would clarify the necessary representations and processes. Wynn identifies the ability that is of interest: imposing form and symmetry on created objects. However, what adaptive problem does this ability solve? What is the function of imposing symmetry? It is unclear why it was more adaptive to make symmetrical than asymmetrical tools. There is a link between step 1 and steps 2–3: Knowing the context and selection pressures acting during a period of time allows one to specify adaptive function. However, Wynn does not take full advantage of the power of archaeology here. He has done an excellent job of describing the relevant context, yet he does not refer the question of adaptive function to the specific context of *Homo erectus* or archaic *Homo sapiens.* Instead, he considers and rejects adaptive explanations based on preference for symmetry, mate value, and navigation, none of which are problems specific to those time periods. Focusing on the evolutionary context of those species would strengthen his analysis here.

Archaeology can also contribute to steps 5–6, comparing the evolutionarily derived theory to alternative accounts. One alternative theory to Wynn's is that imposition of symmetry depended not on new cognitive abilities, but on manual dexterity absent before 1.5 million years ago. Analysis of muscle attachments on hands and wrists of fossil skeletons could illuminate this. Another possibility is that the necessary spatial skills were already present, but using them for innovations in tool use required greater frontal lobe capacities. I believe domain-general frontal executive functions would be more likely candidates than the more general "associative abilities" Wynn discusses, as unspecified associative abilities fail the solvability criterion of step 4. Anticipating a future need for a tool (Suddendorf & Corballis 1997), planning, and working memory might be the crucial cognitive skills. Here, archaeology and neuroscience together can supply answers. Semendeferi and colleagues showed that parietal cortex, seat of our spatial skills, is not proportionately larger in humans than in other primates relative to body size (Semendeferi & Damasio 2000), whereas the frontal pole, involved in executive function, is disproportionately larger in humans (Semendeferi et al. 2001). Changes in skull morphology that significantly distinguish our species – a domed skull and a less retracted face – allowed more room for the frontal lobes (Lieberman et al. 2002). These two sources of data imply that selection was for frontal lobe abilities, not spatial skills. Analysis of hominid endocasts to determine the extent of key sulci and gyri could also shed light on the relative size of parietal and frontal lobes (Falk 1987).

One of Wynn's most significant contributions is clarifying the evidence that a hunter-gatherer lifestyle did not emerge until 200,000 years ago, that our ancestors were *not* like modern hunter-gatherers. What were they like? Wynn's conclusion that *Homo erectus* did not live in groups because they did not have speech is odd, given the many group-living social primates who lack speech. Like archaic *Homo sapiens, Homo erectus* could have lived in groups, even if those groups lived differently from modern hunter-gatherers. Both species were social foragers facing different adaptive problems.

One conclusion to draw from the recency of hunter-gatherers is that the hunter-gatherer way of life is *the result, not the cause,* of evolution in human psychological mechanisms. Between the emergence of a hunter-gatherer lifestyle 200,000 years ago and the spread of anatomically modern humans out of Africa 80,000 years ago (cf. Capelli et al. 2001; Thorne et al. 1999), only 120,000 years, or 6,300–8,000 generations[2] elapsed. The claim that humans have a large number of psychological adaptations with special design features for anything like the modern hunter-gatherer lifestyle is difficult to reconcile with these numbers.

I hope that collaborations between archaeologists and cognitive psychologists will become more common. The type of task analysis Wynn does for hominid toolmaking over time should be taken as a model for steps 2–3 in characterizing a psychological mechanism. Archaeology can help define adaptive functions for certain abilities by identifying the relevant time and selection pressures. Archaeology can also rid evolutionary psychology of vague assertions about "Pleistocene hunter-gatherers." Spatial cognition, cooperation,[3] living in small groups, and hierarchy negotiation are all adaptive problems that should be referred not to "our hunter-gatherer ancestors," but to earlier time periods, with other patterns of social organization and foraging.

Knowing one's ancestors is centrally important in the mythology of hunter-gatherers all over the world. If evolutionary psychologists really want to take a lesson from hunter-gatherers, we had better start talking to our ancestors. Wynn has shown us one way to do so.

NOTES

**1.** For example, Buss 1999; Cosmides 1989; Cosmides & Tooby 1987; 1992; Ellis 1992; Kurzban et al. 2001; Silverman et al. 2000; Wright 1994.

**2.** This assumes generation times ranging from 15–19 years of age (Bogin & Smith 1996; Dean, personal communication, 4/12/02; Dean et al. 2001; Smith & Tompkins 1995).

**3.** Stone et al. (2002) define the EEA for social exchange as at least as long ago as the Miocene.

## Thinking and doing in cognitive archaeology: Giving skill its due

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**Abstract:** Wynn shows that intentionally standardized artifacts (handaxes) provide evidence of the ability to conceptualize form (symmetry). However, such conceptual ability is not sufficient for the actual production of these forms. Stone knapping is a concrete skill that is acquired in the real world. Appreciation of its perceptual-motor foundations and the broader issues surrounding skill acquisition may lead to further important insights into human cognitive evolution.

Wynn presents a valuable example of the way in which archaeology can contribute to our understanding of human cognitive evolution. Particularly important is his insistence that cognitive archaeologists should avoid traditional archaeological typologies in favor of psychological theories and methods. However, this still leaves the question of *which* psychological theories and methods should be applied.

Insofar as a Grand Unified Theory of human mental life does not appear to be on the horizon, a somewhat pluralistic approach to this question is probably most appropriate (Pickering 2001). The set of theories and methods that proves to be most illuminating will largely depend on what questions are being asked. Wynn chooses to base his analysis of the archaeological record on a fairly traditional theoretical framework derived from cognitive and developmental psychology. This framework is essentially *computational* in that it seeks to explain diverse overt behavior in terms of underlying formal cognitive operations (e.g., "frame independence" or "coordination of shape recognition"). Because Wynn, like many cognitive archaeologists, is primarily interested in using artifacts as evidence of abstract conceptual capacities, this framework is particularly well suited to the questions he is asking.

However, abstract conceptualization is not the only (nor perhaps even the most important) mental process involved in stone tool making, a fact that is reflected in some of Wynn's previous work (e.g., 1993a; 1995). Stone knapping is, first and foremost, a concrete and practical skill that is acquired and performed in the real world. The implications of this for cognitive archaeology are best appreciated from a theoretical perspective that draws on elements of ecological psychology (Gibson 1979; Michaels & Beek 1995), cultural psychology (Bruner 1990; Vygotsky 1978), and the dynamic systems approach (Bernstein 1967; Thelen & Smith 1994).

As Wynn states, "even [the] simplest of knapping actions requires directed blows" (sect. 2.1). In fact, many archaeologists have noted the perceptual-motor skill evident in the earliest stone tools (Ambrose 2001; Ludwig & Harris 1998; Semaw 2000). A great deal of experimental work is needed to describe more rigorously the skills associated with particular prehistoric technologies, but the preliminary PET research (Stout et al. 2000) cited by Wynn does suggest that even simple flake removal places significant demands on the dorsal visuomotor control system (Milner & Goodale 1995) of modern humans. Although perceptual-motor skill is often dismissed as trivial or primitive compared to abstract conceptualization, such skill is an impressive achievement requiring the discovery of dynamically stable behavioral solutions to inherently variable motor problems (Reed & Bril 1996). Huge portions of the modern human brain are involved in this process, including areas like the cerebellum, superior parietal lobule, and premotor cortices that appear to have experienced preferential expansion during human evolution. The sophisticated perceptual-motor skills that typify human sport, art, and craft can take years of dedicated practice to acquire, and are as reflective of human mental uniqueness as more "cognitive" behaviors like visualization and language.

Ethnographic studies of stone knapping (Roux et al. 1995; Stout 2002) indicate that, even in sophisticated modern technologies, mastery of the elementary percussive action is the most fundamental and time-consuming aspect of skill learning. Effective flaking is a specialized form of perception-though-action that allows for the discovery and stabilization of larger scale patterns (strategies) in necessarily variable reduction processes. Less skilled knappers can readily conceptualize or describe an appropriate reduction strategy, but they do not actually *comprehend* it in the concrete sense required for performance.

Wynn has previously pointed out (Wynn 1995) that skilled tool use is only developed through long periods of practice and observation. In modern humans, such learning occurs through guided participation in a *community of practice* (Lave & Wenger 1991). The social situation *scaffolds* (Wood et al. 1976) learning by providing opportunities for participation at appropriate levels of difficulty (i.e., within the *zone of proximal development* [Vygotsky 1978]) using culturally provided material and conceptual tools. Motivational and affective elements critical to learning (Damasio 1994; Greenspan 1996) derive from the culturally constructed meanings (Perret-Clermont et al. 1991; Fogel 1997) of participation. This is exemplified in the modern stone knapping craft of Langda village in Indonesian Irian Jaya (Stout 2002).

Over evolutionary time, this distinctly human, cultural, mode of learning came to replace the primitive hominoid condition. Modern chimpanzee societies scaffold skill acquisition to a degree (Boesch 1991), but lack the added dimension of cultural meaning and structure. In the absence of cultural facilitation of more intensive and/or protracted learning (as seen, for example, in captive "enculturated" apes), efficient nut cracking may approximate the upper limit of skill acquisition possible in chimpanzee societies. Premodern hominids clearly came to exceed this limit, perhaps through some "proto-cultural" adaptation such as the *mimetic culture* proposed by Donald (1991). Careful attention to the level of skill indicated by premodern stone artifacts may help to chart the course and timing of this critical development in human cognitive evolution.

As Wynn has shown, the conceptualization of form and symmetry is necessary to the production of standardized artifacts like later Acheulean handaxes. This is just the tip of the iceberg for cognitive archaeology, however, because such conceptualization is by no means sufficient for actual tool production. Thinking about knapping and actually knapping are closely related but diagnostically different mental behaviors (cf. Thelen & Smith 1994). Wynn has demonstrated the promise of psychologically informed research on stone tools, and it is to be hoped that he and others will continue in this vein to address the many exciting questions that remain.

## Natural selection of visual symmetries

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**Abstract:** Implicitly, Wynn's target article starts from the transformational definition of symmetry. Unlike his suggestion, this traditional definition and the recent holographic definition are relevant to the discussion on the cognitive evolution of visual symmetries. These definitions reveal underlying properties and, thereby, they support the natural selection hypothesis. The holographic definition even agrees with an indirect test of this hypothesis.

In the course of evolution, our visual system became attuned to only a few of the innumerably many kinds of regularity in the world. A common idea in perception research is that each of these few regularities was selected because of its individual functionality – for the rest, these regularities are considered to be unrelated to one another. Remarkably, however, the visual regularities are practically the same as the regularities that are relevant in nonvisual domains such as crystallography and molecular biology. This domain-transcending relevance suggests that there might be a more fundamental property that is characteristic for only these few regularities. In fact, two such properties have indeed been found. One is the property of invariance under motion, as put forward in the traditional transformational approach (see, e.g., Palmer 1983). Another is the property of invariance under growth, as put forward in the more recent holographic approach (van der Helm & Leeuwenberg 1991; 1996; 1999).

As I elaborate in a moment, the transformational property relates to the external structure of regularities and is relevant in object recognition; the holographic property relates to the internal structure of regularities and is relevant in object perception (which precedes object recognition). Each of these two properties is, in a formal mathematical sense, characteristic for only a small set of regularities. The two regularity sets, thus defined, not only overlap largely, but also agree well with the regularities that are generally considered to be the visual regularities.

Although Wynn argued that such definitions are hardly required, he used the very specific transformational terminology by referring to the visual regularities as being symmetries that are reflectional, radial, rotational, or translational. Reflectional symmetry corresponds to mirror symmetry which, together with a kind of broken symmetry, forms the holographic regularity called bilateral symmetry; radial, rotational, and translational symmetries are variants of the holographic regularities called repetition and alternation.

Be that as it may, Wynn does not seem to realize that the transformational and holographic properties open the possibility that evolution has selected a central visuo-cognitive system that embodies one or both of these underlying properties. In other words, the existence of these underlying properties supports the idea that certain regularities became visual regularities by natural selection at the level of regularity-processing systems, rather than by, say, sexual selection at the level of individual regularities. Moreover, as I discuss next, favourable towards the survival of such a naturally selected system, are factors that run parallel to the transformational and holographic properties.

First, the transformational property of invariance under motion specifies visual regularities as being configurations which, if present in an object, yield the same retinal image after translations and rotations that let the object move as if it were rigid, even if it is not. This transformational invariance is a property of many flowers and crystals, for instance. The functionality of transformational invariance in object recognition is favourable towards its survival embodied in a regularity-processing system. That is, successful recognition of a transformationally invariant object, like a cube, can occur fairly independent of the viewpoint position taken by the observer (see, e.g., Enquist & Arak 1994).

Second, the holographic property of invariance under growth is the primary characteristic in van der Helm and Leeuwenberg's (1991) definition of visual regularity, and may be illustrated as follows. Living organisms generally grow such that their body shape remains basically symmetrical – that is, the symmetry structure is invariant under body growth. Similarly, the repetition structure of, for instance, a queue of virtually identical penguins remains a repetition structure when the number of penguins increases – that is, it is invariant under queue growth. The symmetry structure of a body grows cell by cell, and the repetition structure of a queue of penguins grows penguin by penguin, so that the holographic growth steps can be said to specify the constituent parts of each regularity.

The foregoing illustrates that holographic invariance relates to the internal growth structure of regularities – as opposed to transformational invariance, which relates to the external motion structure of regularities. Despite this difference, the functionality of transformational invariance in object recognition is also favourable towards the survival of a regularity-processing system that embodies the holographic property. After all, as mentioned, the holographic and transformational regularity sets overlap largely. By specifying the constituent parts of regularity, however, holographic invariance seems more fundamental: It specifies the intrinsic character of regularity, rather than just a transformational consequence of regularity. Furthermore, holographic growth seems a useful model of the way in which the visual system builds up its representation of regularities. Indeed, in contrast to the transformational approach, the holographic approach provides a fairly comprehensive explanation of the human perception of not only perfect but also imperfect regularities (see van der Helm & Leeuwenberg 1996; 1999).

For instance, the well-known phenomenon that mirror symmetry is the best detectable visual regularity by far (see, e.g., Barlow & Reeves 1979) is holographically explicable. Holographically, it is therefore no surprise that mirror symmetry intruded into various visuo-cognitive domains – including the domain of mate assessment, where a preference for more-symmetrical mates has been found (see, e.g., Møller 1992). Related to biological growth, these domains provide two further factors that are favourable towards the survival of holographic invariance embodied in a regularity-processing system. First, in scene perception in general, mirror symmetry is preeminently a cue for the presence of a living object. Second, in mate assessment in particular, the degree of (a)symmetry in an organism's body shape seems to be correlated with the organism's health in terms of genetic quality, developmental stress, and reproductive success (see, e.g., Møller 1990). Hence, the holographically-explicable high salience of mirror symmetry is functional in both domains.

Finally, several holographically explicable peculiarities suggest that our far ancestors indeed perceived regularities in the same way as we do. First, depending on the context, modern humans may either overestimate or underestimate the amount of symmetry in an imperfect mirror symmetry (Freyd & Tversky 1984; van der Helm & Leeuwenberg 1996). Second, for modern humans, the detectability of a mirror symmetry is not strengthened but is weakened by salient substructures (Csathó et al., submitted). These peculiarities do not seem to be explicable from sexual selection at the level of individual regularities. Hence, in sum, the holographic approach provides not only direct theoretical evidence, but also indirect empirical evidence that the visual regularities emerged by natural selection at the level of regularity-processing systems.

## Symmetry for the sake of symmetry, or symmetry for the sake of behavior?

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**Abstract:** Wynn suggests that the imposition of symmetry on stone tools is indicative of the evolutionary development of cognitive abilities of the tool makers, particularly that of creating mental images. I suggest that it is more likely indicative of the evolutionary development of the perceptual ability to detect resources for behavior of hand-held objects.

In his target article, Wynn asks whether there can be an archeology of cognition. That is, he asks whether the evolutionary development of human cognition can be inferred by a systematic investigation of "the traces of action" as revealed by archeological artifacts. Wynn primarily addresses this question with respect to the symmetry of rudimentary stone tools.

From the perspective of cognitive psychology, (human) tool use is typically equated with a logical problem-solving task. Thus, the focus of most research has been on the cognitive mechanisms expected to underlie such behavior. Hence, cognitive mediators (e.g., representations, mental images, etc.) are often brought to bear. Thus, it follows that the evolution of tool use (and tool making) can be used as an index of the evolution of these cognitive mediators.

Although I agree with Wynn that it behooves the cognitive archeologist to make contact with the psychological literature, I suggest that traditional cognitive psychology may be inappropriate for cognitive archeology. Alternatively, a more appropriate psychology may be the ecological psychology developed by James J. Gibson and colleagues (see Gibson 1966; 1979; Michaels & Carello 1981; Reed 1996; Turvey et al. 1981).

A fundamental tenet of ecological psychology is that environmental properties lawfully structure patterned energy arrays (such as reflected light or compression waves), such that the structure is specific to its source. This structure provides information about its source to a perceiver capable of detecting it (Gibson 1979; Michaels & Carello 1981; Reed 1996; Turvey et al. 1981). Specifically, it provides information about *resources for behavior* (Reed 1996; i.e., affordances; Gibson 1979) provided by the layout of substances and surfaces including the availability of prey, predators, shelter, and potential mates.

Presumably, evolutionary development takes place with respect to the differential ability of animals to utilize these resources for behavior (Reed 1996). In order to do so, however, a given animal must first: (1) be aware that such resources exist; and (2) regulate its behavior such that the resources are utilized (see Turvey 1992; Turvey & Carello 1986). According to ecological psychologists, perception yields an awareness of these resources and provides a means by which to regulate behavior (Gibson 1979; Michaels & Carello 1981; Turvey et al. 1981). Hence, successful perceiving is fundamental to the evolutionary process (Reed 1996).

In traditional (i.e., cognitive) accounts of perception, epistemic mediators (such as mental representations) stand between the perceiver and that which is perceived. Since there is no lawful link between representations and that which they represent, the presence of epistemic mediators in the perceptual process is a barrier to realism (see Turvey & Shaw 1979). Thus, epistemic mediators interfere with a law-based account of successful perceiving (Turvey et al. 1981).

If so, and if successful perceiving is fundamental to the evolutionary process (see above), then *epistemic mediators interfere with a law-based account of evolution.* In short, mediated perception is not a particularly "phylogenetically friendly" concept (see Brooks 1999; Reed 1996; Turvey et al. 1981). Cognitive archeologists require a psychology that promotes *unmediated epistemic awareness.* Some authors (e.g., Dent-Read & Zukow-Goldring 1997; Michaels & Carello 1981; Turvey et al. 1981) suggest that ecological psychology provides the only tenable account of such awareness.

There can be no question that the imposition of symmetry on stone tools is indicative of phylogenetic development. However, such development is less likely the refinement of the ability to form a mental image and more likely the refinement of the ability to perceive resources for behavior of a hand-held object. From the ecological perspective, tool use and its development (like perception and behavior) are law-based phenomena. A hand-held tool is an object temporarily attached to the body so as to extend the capacity for perceiving and acting (Gibson 1979; Shaw et al. 1995; Smitsman 1997).

Successful use of a hand-held tool requires an appropriate scaling and directing of muscular forces so as to regulate the relationship between tool and to-be-affected-surface (Carello & Turvey 2000; Smitsman & Bongers 2002; Wagman & Carello 2001; under review). This implicates dynamic touch, the haptic subsystem used when wielding or manipulating objects via muscular effort (Gibson 1966).

Given that objects have different mass distributions, they will differentially resist being rotated in different directions. This resistance to rotation in different directions is quantified by the inertia tensor ($I_{ij}$), represented by a $3 \times 3$ matrix. A large body of research has shown the relevance of rotational inertia to perception of both geometric and functional properties of objects via dynamic touch (see Carello & Turvey 2000 and Turvey 1996 for reviews).

Recent efforts have been directed at the relevance of the tensor to the *dynamic symmetry* of a hand-object system. Dynamic symmetry describes the ways in which that object can easily be moved and constrains the *resources for behavior of the hand-object system.* The volume $V$ and symmetry $S$ of the inertial ellipsoid (Shockley et al. 2001; Turvey et al. 1999):

$$V = 4\pi/3\,(I_1 \times I_2 \times I_3)^{-1/2} \tag{1}$$

and

$$S = 2I_3/(I_1 + I_2) \tag{2}$$

respectively, constrain how much force is required to control the object and how that force should be directed (Carello & Turvey 2000; Shockley et al. 2001; Turvey et al. 1999). These variables have been shown to constrain both perception of the utility of and choice of grip position on a hand-held tool in ways that reflect the power or precision constraints of the tool-use task (Wagman & Carello 2001; under review).

The resources for behavior of a given object depend on the mass distribution of the hand-object system about a rotation point in the wrist. This distribution depends on where (and how) the actor chooses to grasp the object. A given object gripped in a particular location (or in a particular manner) has different resources for behavior than the same object grasped in a different location or in a different manner (Napier 1993; Wagman & Carello, under review). Modifying the shape of the tool itself (such as in the stone knapping described by Wynn) has similar consequences for resources for behavior of the limb-object system (see Hart et al 2001).

As Wynn points out, the imposition of symmetry on tools seems to have played a large role in the evolution of tool making. However, this imposition need not be indicative of the evolution of cognitive mechanisms. From the perspective of ecological psychology, the evolution of tool use and the evolution of perceiving-acting capabilities are symbiotic processes (Smitsman 1997; Wagman & Carello 2001). Along these lines, the evolutionary development of hammering tools may be indicative of attunement to the complex inertial quantities that specify the resources for behavior of a hand-object system.

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## The fossil evidence for spatial cognition

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**Abstract:** Wynn's model for the evolution of spatial cognition is well supported by fossil evidence from brain endocasts, and from neurological studies of the cerebellum and the posterior parietal region of the cerebral cortex. Wynn's intriguing hypothesis that the spatial skill reflected in artifacts is an index of navigational ability, could be further explored by an analysis of lithic transport patterns.

Thomas Wynn has steadfastly insisted for many years that archaeology can inform us about the evolution of human cognition only if we evaluate the evidence within a well-developed theory of intelligence, and resist the temptation to over-interpret the evidence. His ground-breaking attempts to link Paleolithic material culture with psychology have provided an important model for cognitive archaeology (Wynn 1979; 1981; 1985; 1989; 1996). The target article brings Wynn's ideas before a wider audience, integrates observations about chimpanzees' "artistic" activities; offers speculation about the adaptive pressures on early hominids to adopt technical innovation; and invites a more structured and explicit interdisciplinary collaboration between the cognitive sciences and archaeology.

Wynn's analysis is well supported by the fossil evidence for the timing of brain evolution. He has highlighted the Nariokotome skeleton in his discussion. However, he has neglected a larger body of evidence available from many other endocasts and documented by physical anthropologists. The transition from sensorimotor to a concrete operational level of intelligence occurred in parallel with profound morphological changes in the neocortex (Holloway 1996). Notable among these changes was the expansion of the posterior parietal region, where spatial perception, visuospatial integration, and other cognitive spatial operations are processed. Although the morphology of this region in australopithecines is a matter of chronic debate (e.g., Falk et al. 1989; Holloway 1984; 1985), there is a general consensus that the posterior parietal cortex had expanded in *Homo habilis* and subsequent hominids (Begun & Walker 1993; Geschwind 1965; Holloway 1981; Schepers 1946; Tobias 1987).

The trajectory of encephalization during the Pleistocene is also relevant to Wynn's model. Encephalization in *Homo erectus* appears to be similar to that of *H. habilis,* but between 600,000 and 300,000 years ago a rapid increase in brain mass occurred (Ruff et al. 1997), resulting in dramatic encephalization in Middle Pleistocene hominids. The makers of the highly refined, symmetrical late bifaces were considerably more encephalized, and had unambiguously larger posterior parietal cortices, than earlier hominids.

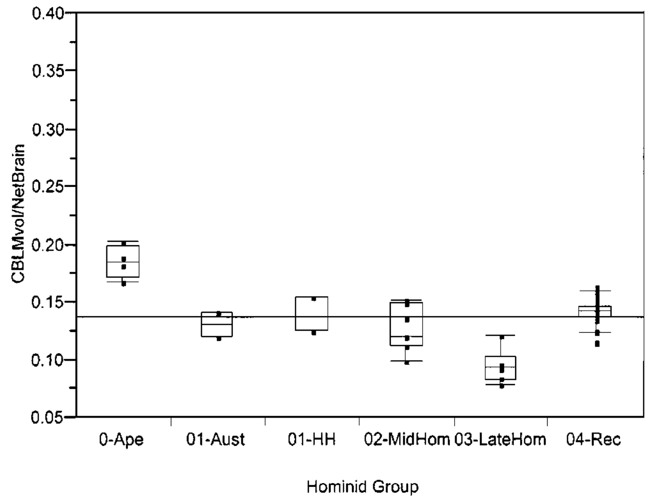


Figure 1 (Weaver). Cerebellum volume (cc)/NetBrain volume (cc) in apes and hominids. Ape = *Hylobates, Pongo, Gorilla, Pan;* Aust = australopithecine; HH = *Homo habilis;* MidHom = *Homo erectus;* LateHom = Kabwe, Swanscombe, Neandertals, Cro-Magnon I; Rec = Recent anatomically modern humans.

The cerebellum, as well as the posterior parietal cortex, appears to be involved in the production of stone tools (Stout et al. 2000). The cerebellum governs timing and sequencing of neural events, and is active during activities requiring procedural memory rather than declarative knowledge (Daum et al. 1993; Ullman, in press). This remarkable brain region appears to enhance and sharpen precise timing of neural events to contribute to efficient performance of difficult tasks, and to promote the smooth control of either thoughts or motor sequences (Paradiso et al. 1997), regardless of whether it is processing sensory, motor, electric, or "cognitive" signals (Butler & Hodos 1996; Ito 1993; Leiner et al. 1993).

My own (unpublished) data based on measurements from digitized endocasts and CT scans of fossil hominids[1] suggests that there was a slight increase in relative cerebellar volume in the early tool-maker, *Homo habilis* over the australopithecines (Fig. 1). It is possible that the motor and perceptual skills required to produce Mode 1 technology made increased demands on the cerebellum for finer ballistic control during stone knapping.

Later, in the early biface makers (*Homo erectus*), neocortical expansion began to outpace cerebellar expansion. This suggests that the manufacture of bifaces involved evolution of neocortical functions related to declarative knowledge such as spatial concepts, rather than procedural processing by the cerebellum.

The trend of rapid neocortical expansion, and slower cerebellar expansion, continued through the Late Pleistocene in Neandertals and early modern humans. Although the cerebellum continued to expand in absolute terms, it was small compared to the rest of the brain in Late Pleistocene hominids, including the Swanscombe and Kabwe hominids, who were representative of the late biface makers. Relative cerebellar volume reached its lowest point in the highly encephalized Neanderthals. That is, neocortical, rather than cerebellar, evolution coincides with the refinement of biface manufacture, the development of prepared core techniques, and even the early Upper Paleolithic. Significant cerebellar expansion occurred later, in recent modern humans.

In sum, there appears to be a correlation between the emergence of preoperational intelligence and expansion of the cerebellum and posterior parietal cortex in early Pleistocene *Homo habilis.* On the other hand, the emergence of concrete operational intelligence in *Homo erectus* was accompanied by a relatively rapid increase in the rest of the brain, with a slower rate of cerebellar expansion. In later Pleistocene hominids, who produced late bifaces, prepared core technology, and early Upper Paleolithic tools, neocortical expansion again made the more important cognitive contribution. By contrast, recent modern humans are characterized by cerebella that are both absolutely and relatively larger than those of Late Pleistocene hominids, suggesting that the automation of routine tasks characterizes the cognitive difference between Late Pleistocene hominids (Neanderthals and early modern humans), and recent humans.

Wynn's extrapolation of Gangestad's (1997) discussions of symmetry to the evolution of spatial intelligence is intriguing. It is more likely that the ability of a scavenger, gatherer, or hunter to navigate within a large territory and map a diversity of lithic resources, seasonal fruits, game trails, and water resources onto it, would make a greater contribution to fitness than the ability to enhance the aesthetic properties of utilitarian objects. Wynn's hypothesis that symmetry in stone tools was a secondary manifestation of other developments in spatial competency could be addressed in part by evaluating patterns of lithic transport over time. For example, chimpanzees transport hammer stones for cracking nuts over distances of several hundred meters (Mercader et al. 2002); whereas makers of Mode I tools and early bifaces at Olduvai transported lithic raw materials up to 2–3 kilometers and may have carried meat and stones to central processing sites (Isaac 1978; Potts 1988). *Homo erectus*' increased stature and the transition to open savannah conditions around 2 million years ago (Wood 1992) may well reflect an extension of foraging territory requiring enhanced navigational ability. Middle Paleolithic hominids were wide ranging in their procurement strategies, transporting raw material and unfinished blanks for tools over distances of dozens of kilometers (Roebrooks et al. 1988 and references therein).

Wynn's model invoking two episodes in the emergence of human spatial competency fits well within a broader anthropological context and offers a useful platform for testing further hypotheses about human cognitive evolution.

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NOTE

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## Intentions, goals, and the archaeological record

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**Abstract:** The underdetermination of intentional explanation by motor behavior complicates inferences drawn from preserved artifacts in the archaeological record to intentions in their production. Without knowledge of a producer's intentions, inferences drawn from those intentions to required cognitive abilities for having those intentions is also complicated.

In the target article, Thomas Wynn argues that the emergence of symmetrical knapping practices preserved in stone artifacts produced during the period between 1.5 million years ago and 300,000 years ago provides evidence for evolution in spatial symmetry recognition abilities.

When we examine artifacts such as Anasazi pictographs and petroglyphs, we readily admit that they are the result of intentional cognitive activity rather than random doodles. That admission is the result of an inference premised, in part, on the assumption that when *we* draw something (rather than doodle) we *intend* to draw something. When we encounter other visual representations, we extend that same assumption to its creator. Such *intentional hypotheses* are neater, more economical, and more plausible than other, nonintentional, hypotheses. Although it's physically possible, it just isn't plausible to think that Anasazi pictographs are the result of unintentional motor behavior.

Consider, then, knapping and its products, stone artifacts with particular shapes. Suppose a stone with a characteristic knapping pattern is discovered. We assume that since we would have some intention were we to produce such an artifact, then those who created the artifact must also have had some intention in doing so. We thus extend to those in the distant past the benefit of the kind of intentional explanation we use for explaining our own behavior and the behavior of our contemporaries. Since our intentions in knapping a stone would be to produce an artifact with a particular feature *F* for purpose *G*, we project similar intentions to those who actually produced the stone. More significantly, whatever cognitive abilities we possess that are necessary for having those intentions, can also be read back into those who had relevantly similar intentions. Thus, if our intention to knap a stone with *F* for *G* presupposes the cognitive ability to recognize spatial symmetry in the artifact, then so too does the intention of those in the distant past to knap a stone with *F* for *G* presuppose the cognitive ability to recognize spatial ability in the artifact.

This explanatory structure is persuasive. Yet, I would like to ask a few skeptical questions. I shall not be concerned with the general skeptical question about having intentions – I assume that our distant ancestors formed, consciously entertained, and acted from intentions. Even if the glimmer of conscious reflection upon intentions was fleeting and sporadic, it is far more likely that they engaged in knapping with *some* intention, than that they engaged in knapping with *no* intention. But *what* intentions can we reasonably attribute to these distant ancestors? Second, what can we reasonably infer about their cognitive abilities from those attributed intentions? And, third, are those inferred cognitive abilities such that we could plausibly maintain that their evolution can be inferred from a change in knapping practices over time?

Take the first question. Granted that our distant ancestors had some intentions when they engaged in knapping. Are those intentions the same intentions we would have were we to knap? Perhaps. But, just as my putting my finger in my ear can either be a sign that I am cleaning my ear, or that I am signaling to my accomplice that the coast is clear for her to start drilling through the wall again, so too knapping a stone can either be the result of intending to make a tool or intending to create an object pleasing to the gods, or something else. This problem is quite general: Any motor behavior *M* underdetermines the intentional explanation of *M*. That's just a fact of life, and, faced with it, we sensibly opt for the most likely attribution. In the case at hand, the most likely intention to attribute to ancient knappers is the one Wynn attributes to them – they wanted to make tools.

Grant that our distant ancestors wanted to make tools when they knapped away. What can we reasonably infer about their cognitive abilities from that attributed intention? Well, we can infer that they understood that artifacts with particular features served particular functions, that knapping would have certain consequences on the shape of stones, that stones were good knapping resources, and a host of other intentions. However, from the knapped stone having a particular feature post-knapping, can we infer that that feature was one of the knapper's intentions, or a part of the knapper's understanding of what it takes to make a tool? Not directly: Perhaps the knapper only intended to create a tool with a sharp edge or perhaps the knapper's only thought was to make a thing that could cut. Shape – whether symmetrical or not – might not have been a component of any of the knapper's intentions. Again, this skeptical possibility derives whatever force it has directly from the underdetermination of intentional explanation by motor behavior. Unlike the previous case, however, it seems to me that Wynn's case that cognitive abilities are revealed by intentions, is persuasive only if a strategy for ruling out other inconsistent explanations is available. I suspect that such a strategy is available, and on similar grounds to those suggested above, but the need here is more pressing.

Finally, suppose that we can infer the cognitive ability of recognizing symmetry in artifacts from knapping intentions. Does its nonappearance prior to some time *t* and its appearance after *t* provide evidence that it evolved? Again, not directly. For, again, it is possible that knappers prior to *t* possessed the cognitive ability to recognize spatial symmetry, but were compromised by some other cognitive or motor ability (or lack thereof) that prevented their taking advantage of their capacities for symmetry recognition.

None of what has been suggested here rebuts Wynn's argument. Rather, what is called for are certain methodological refinements that will make the case more persuasive than it already is. Only if those refinements are incompatible with the available archaeological resources would these reflections be damaging. I doubt very much that such is the case.

# Author's Response

## The devil in the details

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**Abstract:** Despite challenges on minimum necessary competence, intentionality, reliability, and context, the example of cognitive archaeology presented in the target article holds up well. The commentaries also present perspectives on cognition and symmetry that suggest an alternative to the target article's characterization of the cognitive abilities of early *Homo erectus.* However, the major conclusion of the initial argument – that the human ability to coordinate shape recognition and spatial cognition evolved hundreds of thousands of year ago in conditions unlike those of the modern world – remains intact.

In my reply I will follow the same order of topics, more or less, as in the target article.

### R1. Cognitive archaeology

Although all of the commentaries endorse the basic idea of a cognitive archaeology, not all endorse my characterization of it or my particular example. Concerns about methodology include the logic of the argumentation (and of my specific argument) and the nature of appropriate theory.

#### *R1.1. Minimum necessary competence*

Several commentators, most notably **Bridgeman** but also **Coventry & Clibbens**, were troubled by what I have termed the problem of minimum necessary competence. This is the possibility that the maker of a stone tool, or any prehistoric actor for that matter, "may be more cognitively sophisticated than the artifact reveals" (Bridgeman). Bridgeman then employs the example of using a rock in his garden as a tool, suggesting that a future archaeologist might misread his abilities. But the future archaeologist would also likely find the foundations of Bridgeman's house, his bone china plates, ceramic toilet, glass light fixtures, and so on, rendering such an error far less likely. Bridgeman is of course correct that minimum competence can potentially mislead, which is why archaeologists rarely rely on isolated finds and try very hard to document a broad range of activities. The stone tools that I used in this article represent the most sophisticated products of their times; nothing in the contemporary archaeological record suggests more sophisticated abilities.

**Coventry & Clibbens** take an opposite tack and invoke Occam's razor to suggest that the stone-knappers may have been *less* sophisticated than my analysis suggests. They point to spiderwebs as examples of "rather sophisticated geometric objects" produced "without the need for higher cognitive processes," and suggest that simple mechanics is a more parsimonious explanation. Citing spiders (or bees) is simply a red herring. Modern humans are the only primates that impose geometrically regular shapes on objects. We do this by employing a suite of abilities (skill, spatial thinking, shape recognition, etc.). Moreover, the artisans who produced these stone tools were our immediate and distant ancestors. The parsimonious solution is to posit that they imposed shape in the same way that we impose shape, a point made nicely by **Welshon**. Of course, we expect that these abilities have evolved, but from abilities and circumstances similar to those of apes and other primates, not from spiders or bees. The burden of proof lies on those who assume simple mechanics, and innate procedures are sufficient to account for the imposition of shape on knapped stone artifacts.

#### *R1.2. Intention*

In the target article I defended the idea that symmetry was an intended quality of handaxes, both early and late. This position provoked the most serious methodological challenges, including comments by **Coventry & Clibbens**, **Jordan**, **Humphrey**, **Nowell**, **Schoenemann**, and **Welshon**. Nowell and Schoenemann nicely present the objections to an intentional interpretation of artifact shape, citing in particular the arguments of Dibble (1987) concerning Middle Palaeolithic tool types, McPherron (2000) concerning handaxes, and Davidson and Noble (1993; see also Noble & Davidson 1996). In essence, they suggest that raw material, resharpening, and the typical use-life of a stone tool are sufficient to account for its shape, including symmetry, and that specific cognitive mechanisms need not be invoked.

Is it ever reasonable to assume intention? **Welshon** has most clearly presented the dilemma and has also pointed the way out. Granting the knappers the intention to make tools, he asks whether we can infer that a specific feature (e.g., symmetry) was intended. He concludes that we cannot because of "the underdetermination of intentional behavior by motor behavior." He further concludes that "Wynn's case that cognitive abilities are revealed by intentions is persuasive only if a strategy for ruling out other explanations is available." Fair enough. Such a strategy exists, though it is more persuasive for the 300,000-year-old hand axes than for the 1.5-million-year-old handaxes. The strategy invokes characteristics of the archaeological record and has four components:

1. The first is simply quantitative and is suggested by **Nowell** herself: How common were nicely symmetrical hand axes within individual assemblages? For some late handaxe sites with high-quality raw material, such as Ma'ayan Barukh in Israel (Steklis & Gilead 1966) or Kalambo Falls in Zambia (Clark 2001), three-dimensional symmetry is virtually ubiquitous, with "ugly" examples being the exception.

2. At a few sites there is evidence for the production of a specific handaxe as a finished artifact. At the relatively late site of Boxgrove in England (Roberts & Parfitt 1999), the debitage from individual handaxe productions has been refitted into the original cores. In the best of these cases, the artifact had been carried to the location as a rough roughout, which was then completed. The debitage consisted of 1,715 pieces more than 5 mm in size and "thousands" between 1 and 5 mm. Although some of these flakes could have been used, most could not. The bulk of this debitage was restricted to an area of less than a square meter and arranged in a pattern consistent with the knapper sitting in a single position with his or her legs apart. The knapper's goal appears to have been the handaxe itself, which was not found.

3. The third component considers the nature and location of trimming on individual handaxes. Many late assemblages have nicely shouldered handaxes in which one lateral edge has been knapped to mirror the other, a possible accident once but not if the shape recurs again and again, as it does at Ma'ayan Barukh. The lateral edges of handaxes often have scores of very small flake removals located in discontinuous positions. The resulting flakes are too small to have been used (see point 2 above), and resharpening (a possible reason for removing small flakes) is unlikely to have been discontinuous.

4. The final component relies on the authoritative stance of modern stone-knappers, who almost universally consider handaxe shape to be intentional. Stephen Edwards, for example, is an experienced stone-knapper who has replicated handaxes like those excavated at Kalambo Falls. He concludes:

> Put another way, if handaxes were merely accidental byproducts of flake production, the Acheulean occupation horizons would be littered with: a) cores that look like "burnt out" handaxes, i.e. handaxes that are small, not carefully shaped in plan form, and with markedly wavy edges; and b) cores that could only with difficulty be assigned to handaxes vs. other categories. These would clearly outnumber larger, symmetrical 'finished' hand axes, even in the Late Acheulean assemblages. Such is not the case. (Edwards 2001)

Does this mean that there had to be a "mental template," that is, some preexisting image? No, but it does mean that the knappers attended to shape and coordinated shape with spatial cognition. In sum, what we know of late handaxes indicates that a flaking strategy alone (biomechanics) could not have produced these artifacts. I challenge anyone to teach a novice to knap stone, supply him or her with appropriate raw material, and see if a handaxe with three-dimensional symmetry emerges.

The early handaxes are another matter. Only point 4 above, and possibly point 3, can be brought to bear. Although this would appear to rule out a simple flake production strategy, the case is much weaker than that for the late handaxes.

Interestingly, the question of intention appears to have troubled paleoanthropologists more than psychologists, perhaps because, as **Jordan** notes, intention is a control variable in most psychological experiments and its presence or absence is not an essential piece of argumentation. **Humphrey** is comfortable with the attribution of intention as long as symmetry is reliably documented. Indeed, she views this as more critical than the question of intent.

#### *R1.3. Reliability*

**Schoenemann** and **Coventry & Clibbens** are uncomfortable with my qualitative analysis. Schoenemann suggests, reasonably, that I could have employed a metrical measure of symmetry and quantified how close or far an artifact, or average for an assemblage, was from the ideal. He even tactfully reminds me that I myself have appropriate measurements on hand (Wynn & Tierson 1990). As a matter of principle, I must concede this point. A quantitative measure of increasing symmetry, from none in the Oldowan to high in the late Acheulean, might have bolstered my case. As a practical matter, it was not possible. First, although I have measurements for 1,400 late handaxes, I do not have comparable measurements for early examples. Second, these simple metrics omit important attributes like amount and location of trimming, which would run the analysis afoul of the "intention" problem discussed above. But there is an additional counter to Schoenemann's point that bears on the whole methodological issue of quantitative methods in cognitive archaeology. Although quantification may provide greater reliability, it does not in itself assure validity. If we can reliably document an increasingly accurate approach to symmetry, but if this symmetry was not "on the screen" of the prehistoric mind, it would be of little interest. How the artifact was seen and conceived is the issue, and if I can demonstrate that even some of the symmetry was intentionally imposed, this is enough to ground a qualitative analysis. There is an established tradition of qualitative research in both psychology (e.g., Piaget) and archaeology; for better or worse, mine is a qualitative argument. But perhaps my attribution of symmetry reveals more about me than about early hominids? Coventry & Clibbens propose a novel way around this. They suggest showing a range of artifacts to a sample of people and asking which are symmetrical. This would certainly provide some reliability (assuming it worked out as hoped), but what would I be testing? Might not these modern humans be influenced by the same symmetrical bias that clouds my vision? Perhaps, but it is an interesting suggestion nonetheless.

#### *R1.4. Theory*

Although it has always been my contention that cognitive archaeology is best characterized as a method of inquiry, it is also true that the resulting analyses and interpretations are guided by theories of cognition. I have tried to be clear about my preferences, which currently lean toward cognitive neuroscience. This is not, however, the only theory of cognition that can be brought to bear on the archaeological record. Indeed, **Wagman** argues that cognitive psychology in general is inappropriate to the undertaking because it requires describing epistemic mediators that "stand between the perceiver and that which is perceived." He suggests that ecological psychology, which does not posit an epistemic mediator, would be a more appropriate theory. He has a point. Certainly the "homunculus" problem has plagued psychology, and the difficulties of describing features of an internal mind are exacerbated when confronting the evolutionary past. There have been, in fact, some initial forays into ecological psychology by cognitive archaeologists (Stout 2002), but it remains to be seen whether this approach will yield insights into human evolution.

**Epstein**, **Weaver**, and **Commons & Miller** favor a Piagetian or neo-Piagetian approach. Having spent roughly 15 years advocating such an approach (e.g., Wynn 1989), I find these suggestions both comforting and ironic. Space prohibits a long self-critical discussion of my fall from Piagetian grace, but a few comments are appropriate. The stage organization of Piaget's scheme is very seductive because it describes a series of steps culminating in the modern adult mind. What better model for evolution? Even though Piaget argued that the sequence was logically necessary (and applicable to other sequences like the history of science), the argument still presumes a parallel between ontogeny and phylogeny that is controversial and far from understood (Gould 1977; 2002). More importantly, the Piagetian scheme ultimately failed to account for some very real features of the archaeological record. For example, in Piaget's scheme of spatial cognitive development (Piaget & Inhelder 1967), projective concepts should emerge prior to Euclidean ones (indeed, they are logically prerequisite). In the archaeological record they appear simultaneously (Wynn 1989). Indeed, the archaeological record indicates that modern adult spatial cognition was in place by 300,000 or more years ago, yet little else about the behavior of these hominids resembles the behavior of modern adults. Yes, Piagetian approaches have yielded some important insights (Parker & Gibson 1979; Wynn 1979; 1981; 1985; 1989), and it is possible that the neo-Piagetian approach advocated by Commons and Miller will do even better. However, I now believe that such global theories as Piaget's are premature and that we are on more secure ground investigating more narrowly circumscribed abilities. I also agree with **Stout** when he writes that "a Grand Unified Theory of human mental life is not on the horizon" and that for the foreseeable future a pluralistic approach in cognitive archaeology will yield the best results.

### *R1.5. Context*

I conclude this section on cognitive archaeology with a response to **Jeffares**'s very thoughtful commentary. He has set out the role of cognitive archaeology quite clearly and has taken a close look at my claim for timing and context, finding the first laudable but the second well short of success. His discussion of Sperber's actual and proper domains underscores the challenge – and the frustration – of most cognitive archaeology. Archaeology is good at reconstructing the actual domain of use but has a much harder time with the proper domain. This criticism can also be leveled at evolutionary psychology, where the two are often conflated, so we archaeologists are not alone. For my particular case I can only reply that, had I been able to specify the proper domain, I certainly would have. None of the proposed functions of handaxes can adequately explain the symmetry, which suggests that we have not identified the proper domain. This is why I have opted for the "by-product" solution, that is, some domain other than toolmaking selected for these abilities, and their appearance on stone tools was secondary. In spite of the weakness of this specific example, I think it is important to reiterate that the actual domain supplied by archaeologists makes for a stronger evolutionary argument than the hypothetical actual domain proposed by the evolutionary psychologist. Neither the psychologist nor the archaeologist is in a privileged position to supply the proper domain. Even a vague, imprecise description of context is preferable to no context at all and at a minimum sets the appropriate stage for evolution.

## R2. The archaeological record

### *R2.1. Symmetry*

The target article was not *about* symmetry per se. Rather, it was about the coordination of shape recognition and spatial cognition, and used symmetry as the central example. This caused some confusion. **Silverman**, for example, concluded that I believe that "symmetry was the essential adaptation of human spatial cognition." This is certainly *not* the case, and, as I am unable to find any passage in the original that even implies it, I must attribute his misreading to the saliency of symmetry itself. Not only is symmetry perceptually salient, but it has been the focus of a very large array of scholarly studies, from crystallography to art (Washburn & Crowe 1988). The nature of the perception of symmetry prompted a number of very thoughtful commentaries. Most focus on detection of symmetry by the visual system. **Reber** and **van der Helm** argue that the saliency of symmetry results from more fundamental features of the visual system, perceptual fluency for the former, and invariance under transformation and growth for the latter. **Deregowski** argues that detection and imposition of symmetry derives from the simultaneous encoding of a facsimile and an enantiomorph by the visual system. **Wagman** avoids the visual system almost entirely and derives object symmetry from the haptic system, differential resistance to rotation in particular. Finally, **Gurd**, **Fink & Marshall** (henceforth **Gurd et al.**) distinguish between the preattentive detection of symmetry in figure-ground discrimination and judgments of symmetry, which require attention. I am in no position to argue the inherent merits of these various perspectives on perception. Several of these commentators also address the evolutionary significance of symmetry perception. Although this is an important and interesting subject in its own right, it was not the real focus of my analysis. Presumably, natural selection for the perception of symmetry was a much older evolutionary event than those treated here. Nevertheless, some of the alternative perspectives on the perception of symmetry do have implications for how one interprets artifact symmetry, especially the understanding of the earliest handaxes (see sect. R2.3).

### *R2.2. Apes and the Oldowan*

In light of the commentaries, it is necessary to revisit my specific assertions about the shape and spatial cognition of apes and early hominids.

With the exceptions of **Schoenemann** and **Parker**, most commentators appeared comfortable with my assessment of the ape baseline. Schoenemann decries my reliance on what apes do naturally rather than what they can be trained to do, and cites ape language acquisition studies and cross-modal perception in monkeys as examples of behaviors that do not occur naturally. At first consideration, he has a point. What if Kanzi, the bonobo trained by Nick Toth and Sue Savage-Rumbaugh, could be trained to knap a handaxe, or even draw one? If he could, then the cognitive significance of this development in human evolution would certainly be weakened. But Schoenemann is proposing an unfair test in having us compare a trained group (chimpanzees) with an untrained group (*habilis,* or *erectus,* or whoever); perhaps a trained *erectus* could produce three-dimensional handaxes – or bronze axes. Toth et al. (1993) have argued that Kanzi cannot even produce decent Oldowan tools, let alone a handaxe, so this is probably a moot point. Yes, it would be useful to know if any nonhuman primate could be trained to produce symmetries, that is, coordinate shape recognition (and no one doubts that they can recognize symmetry) with spatial cognition, and use this to guide action. But the fact is that none ever has. Schoenemann's musing that they might if properly motivated just does not bear much argumentative weight. Apes do not produce symmetries naturally and have never been trained to do so. Until they have, Schoenemann's point is merely another red herring. Parker is on more solid ground. Rather than speculate about what apes might be able to do, she points to important features of ape spatial cognition that augment my more cursory description of ape abilities. She notes, for example, that apes do not arrange objects sets and do not appear to understand gravitational relations (box stacking). The inability to arrange objects sets certainly bears on spatial ability but, if anything, it attests to less-developed spatial ability than I have suggested.

Of course, **Parker**'s major disagreement with me concerns my characterization of the hominids who made Oldowan tools. She first notes that it is difficult to believe that bipedalism and stone tool manufacture entailed no cognitive changes in Oldowan hominids away from an ape-like cognition. She further maintains that these hominids employed a new form of foraging – bone-marrow extraction using stone tools – and that this was based on three cognitive advances: tools to make tools, tools to access embedded foods, and the extension of apprenticeship to toolmaking. Also, she attributes projective notions of sharpness and angle to the Oldowan knappers and places them in Piaget's late preoperational stage. I do not disagree with her characterization (though **Epstein** appears to), but I do question whether or not this places these hominids outside the range of ape cognition (or what W. C. McGrew and I termed an ape adaptive grade [Wynn & McGrew 1989]). Although "preoperational" is certainly appropriate (Wynn 1989), application of subdivisions within the stage are just too subtle to be convincingly applied (Wynn 1981). **Commons & Miller**'s "primary" stage in their neo-Piagetian scheme may be a more appropriate one for the Oldowan, but the status of apes still remains unresolved. I admit to being very conservative here; it is tempting to give Oldowan knappers the benefit of the doubt because, as Parker points out, they were bipedal, did have bigger brains, and may have been our ancestors. But the archaeological evidence itself does not provide any examples of cognitive abilities that we know are not possessed by apes. Perhaps Parker and I are simply looking at the proverbial half-filled glass; I see it as half empty.

### *R2.3. The early handaxes*

Early handaxes present a different picture. Apes do not produce symmetries; *Homo erectus* could. This section of my analysis was, I feel, the most controversial. It is here, for example, that commentators like **Nowell** and **Schoenemann** have the strongest case, and it is here that commentators provide some of the more provocative alternatives to my cognitive assessment, though not always explicitly. Is it possible to account for the knapping of these early handaxes without recourse to coordination of shape recognition and spatial cognition (my contention)? **Deregowski** suggests that no awareness of symmetry is required. When the knapper of a handaxe worked to shape an edge, he or she would naturally impose the enantiomorph of the opposite side because the facsimile (copy) and enantiomorph (reverse) are easily confounded in perception, and the difficulties of knapping would naturally lead to the enantiomorph. I rather like this idea, because it can account for the symmetry without recourse to an image or "mental template," an ill-defined term popular among archaeologists. Moreover, it does not negate my primary conclusion; even if there was no awareness, the knapper still needed to coordinate shape recognition (facsimile or enantiomorph) with the spatial task of knapping. Similarly, **Gurd et al.**'s distinction between implicit and explicit knowledge of symmetry is potentially useful. Could the saliency of symmetry as a figure-ground feature be sufficient for its imposition on artifacts, with symmetry not being a component of explicit (declarative) knowledge? This strikes me as a very real possibility. It is even possible that **Wagman**'s modifications tied to "resources for behavior" might be sufficient to account for these early handaxes, though Wagman is less clear than Deregowski or Gurd et al. as to exactly how this would work. Finally, the "implicit" knowledge interpretation fits well with **Masters & Maxwell**'s discussion of curiosity and even **Silverman**'s emphasis on the preattentive status of symmetry. In other words, commentators have convinced me that a cognitive middle ground exists in which stone-knappers could impose symmetry preattentively, using implicit knowledge, without relying on images, mental templates, or conscious awareness of a concept of symmetry.

### *R2.4. The late handaxes*

I do not, however, believe that this middle ground can adequately account for the three-dimensional, congruent symmetries of late handaxes. First, to repeat a point made earlier, **Nowell**'s and **Schoenemann**'s suggestions that these bifaces could be the unintended result of a flaking procedure simply do not hold up to scrutiny (see sect. R1.3; Edwards 2001). It is an arguable position in regard to the early handaxes, but the symmetry imposed on these late examples cannot have been an accident. Schoenemann further challenges my attribution of an intuitive Euclidean concept of space to the hominids who made these tools. He rightly sees this as a central contention of the target article but wonders how I can deny this ability to apes. The answer is again quite simple: Nonhuman primates have never done anything that requires that we attribute to them such an understanding. Certainly nothing about chimpanzee tools requires it and nothing about their foraging requires it. Indeed, Silverman et al. (2000) argue that an essential element of Euclidean understanding, size constancy, is a specific evolved cognitive ability of humans.

**Silverman**'s specific challenge to my cognitive assessment is better formed and driven in part by the much more general debate concerning modular and domain-general interpretations of the human mind. In the target article I conclude that the ability to produce three-dimensional symmetries hinged on allocentric perception and I include mental rotation in the general category of image manipulation. Silverman prefers a more modular interpretation in which a narrower mechanism, size constancy, underpins mental rotation and wayfinding. Moreover, he has experimental evidence to support his contention (Silverman et al. 2000). Silverman's position is actually congruent with my own. Even if size constancy is a separate isolable mechanism, it was certainly required for production of these late handaxes. Indeed, spatial quantity is one of the abilities I identified as required. My analysis has simply provided a time frame for Silverman's evolutionary hypothesis.

**Stone** and **Masters & Maxwell** suggest that developments in frontal lobe functions, working memory in particular, make a better domain-general interpretation for the coordination found in later handaxes than my admittedly superficial reference to associative abilities (derived from Kosslyn 1994; 1999). This is, coincidentally, an area in which I am currently interested (Coolidge & Wynn 2001). Working memory is Baddeley's (1986; 2000) term for the ability to "hold in mind" information necessary to formulate plans. In its classic form it posits a central executive and two slave systems, a phonological loop and a visuospatial sketch pad (Baddeley 2000). The latter are functionally separate and governed by separate areas of the frontal lobe. Goldman-Rakic (2000), who also studies working memory, questions the need for the central executive, suggesting that the phonological loop and visuospatial sketch pad are sufficient. Either way, working memory is a key component of complex plans of action and, as Masters & Maxwell note, of hypothesis testing. To build on Stone's and Masters & Maxwell's suggestion, one could posit that evolutionary developments in working memory, in the visuospatial sketch pad in particular, are the basis for the handaxe knappers' ability to coordinate shape, spatial, and procedural information into a plan of action. Baddeley also associates working memory with declarative memory, so that Stone's and Masters & Maxwell's suggestion conforms well with the commentary of **Gurd et al.** Both working memory and declarative memory require attention and both are involved in explicit understanding. The explicit application of symmetry must have been a function of working memory. Interestingly, it may not have been necessary to engage the phonological loop; most tool use and toolmaking are performed and learned in nonverbal circumstances (Keller & Keller 1996; Wynn 1991). In other words, even if handaxes have implications for working memory, they do not have necessary implications for language.

### *R2.5. Timing and context*

A major thesis of the target article was that archaeology is in a privileged position to provide the timing and describe the context of cognitive evolution. Several commentators, most notably **Jeffares** and **Stone** but also **Humphrey**, find this aim to be overly cautious and suggest that archaeologists can do more. Jeffares has provided an excellent characterization of the role of cognitive archaeology and suggests that archaeologists have much to contribute to our understanding of human cognitive evolution. Of course I agree. But what archaeology, *sensu strictu,* adds to the discussion is timing and context; our further interpretations must draw on psychology, human paleontology, and anthropology. Stone, citing Cosmides and Tooby (1987; Cosmides & Tooby 1992) and Marr (1982), places archaeology within the context of a set of formal steps to a theory of cognition. My analysis focused on steps 2 (timing) and 3 (context). Stone suggests that archaeologists may be able to provide steps 1 (function of a cognitive computation), 4 (processes to serve the functions), 5 (provide predictions), and 6 (devise alternative theories). Again, I agree in principle and, even in my less stodgy moments, try to do all of them. Some cognitive archaeologists, most notably Steven Mithen (1996), are more sanguine than I about these possibilities. I find assessment, timing, and context to be daunting enough goals.

**Stone** and **Jeffares** have also identified what I now believe to be a weakness in my specific argument: My characterization of context was incomplete. As Jeffares puts it, "Wynn's context never connects to the specific uses of the stone tools in question." Stone is even more specific: "What is the function of imposing symmetry? It is unclear why it is more adaptive to make symmetrical than asymmetrical tools." What I supplied was a snapshot of the prevailing conditions – anatomically, environmentally, and culturally – at several points in human evolution. What I did not do was delineate those contextual features that were directly relevant to understanding artifactual symmetry. I did not because, quite frankly, I could not. We do not know the function of symmetry, though hypotheses abound (see section 3). I view this as only a partial failure. The specific function of symmetry must still be identified within a general context, and general context, which in these cases was not modern hunting and gathering, constrains the kinds of interpretations we can make.

Also relevant to timing and context, **Epstein**, **Schoenemann**, and **Weaver** discuss the significance of fossil endocasts. Although their summaries disagree in detail, they do agree on several conclusions that support my interpretation of timing. In particular, they all point to a period of dramatic encephalization at roughly the same time as the appearance of three-dimensional, congruent symmetries (between 600,000 and 300,000 years ago). Although they appear to agree on this point, they disagree on the significance of early *Homo erectus* brain size: Weaver emphasizes the lack of increase in brain size relative to body size when *Homo erectus* is compared to *Homo habilis;* Schoenemann emphasizes the absolute increase in *erectus* brains compared to *habilis;* and Epstein focuses on the timing of the first handaxes, which correlate with no obvious change in brain size. All three acknowledge that endocasts are not ideal measures of behavior. Although Schoenemann makes the point that modern brain size does not correlate with spatial ability, Weaver concludes that the pattern of endocast change supports an increase in parietal size with the encephalization at 600,000 to 300,000 years ago. This *might* reflect spatial ability, but she also emphasizes the importance of declarative knowledge, which is in keeping with suggestions by **Stone** and **Gurd et al.** Just what the pattern of cerebellum development means is still a puzzle. A priori one might expect timed, procedural activity, which Weaver associates with the cerebellum, to have been more important for *Homo erectus* than for modern humans. But this is the opposite of what Weaver's evidence suggests. In the target article I intentionally avoided a long discussion of endocasts in order to emphasize the archaeological record. It is heartening that some correlation exists between changes in brains and changes in cognition as revealed in stone tools. But it is also not surprising that there is not a tight fit, given our current limited ability to relate changes in gross brain anatomy to changes in behavior. Together, cognitive archaeology and human paleontology may eventually be able to paint a coherent picture of the evolution of the brain and cognition, but there is still a very long way to go.

Finally, **Stone** wonders how I could claim that *Homo erectus* did not live in social groups. What my rather awkward original sentence intended was that *Homo erectus* did not live in human-like social groups.

## R3. Evolutionary scenarios

The target article devoted relatively little space to developing an evolutionary scenario with hypotheses concerning selective advantage of shape imposition, or Euclidean space, or even symmetry. This poverty stems in part from my natural reluctance to tell evolutionary stories (we have too many poorly founded examples of this genre), but also from a realization that our knowledge (certainly my knowledge) is too incomplete. As **Stone** noted, I do not even propose a function for artifactual symmetry, which renders a simple Darwinian explanation difficult to formulate. Instead, I discuss several possibilities, including sexual selection and selection for navigational ability (wayfinding is **Silverman**'s more mellifluous term), and found none convincing, Indeed, if anything, the target article suggests that artifactual symmetry was likely to have been an evolutionary by-product. My reticence to propose a well-developed scenario has inspired several commentators to fill the void.

**Bridgeman** and **Simão** maintain that it is more parsimonious to explain changes in palaeolithic tools by recourse to culture change rather than biological evolution. Both believe that there is no need to invoke changes in the genes that control cognitive architecture, though their alternative scenarios differ in significant respects. Bridgeman is an advocate of progress: "Further, cultural progress will mean that the same people can make better tools at a later time," and efficient function will lead to improvement in tool design. Bridgeman's is a very old idea in anthropology and archaeology, and can be traced at least as far back as Victorian scholars such as Spencer and Pitt-Rivers (Wynn 1990). For the recent past it even has a good deal of descriptive value (though the notion that tool design is directly linked to function is probably wrong [Pye 1964]), but it fails to fit the vast majority of the prehistoric record. The simple fact is that people almost never made better tools at a later time. The palaeolithic record includes truly immense spans of time during which there was no progress whatsoever. And those changes that did occur happened on a chronological scale appropriate to biological evolution, not culture change. It is only after 50,000 years ago that the rate of change accelerated, but even this "cultural explosion" may have been enabled (contra Bridgeman) by a genetic change (Coolidge & Wynn 2001; Klein 2000).

**Simão**'s scenario is based on an alternative psychology in which an effective mechanism of social transmission enables culture change. He further maintains that "it is widely believed that . . . most functional specialization of cortical brains areas . . . is not determined by internal constraints" but instead is self-organizing in the presence of environment. This position is certainly on an opposite theoretical pole from that of evolutionary psychologists like **Silverman**. The same counter holds for Simão as for **Bridgeman**. Simão's mechanism would result in continuous, perhaps accelerating, culture change. This is simply not what happened. I must also object to another statement that Simão makes. He attributes to me the belief that "behavioral changes are mapped in a straightforward way to changes in cognitive architecture . . ." This is a position that I certainly do not hold; Many important developments occurred in the Palaeolithic that cannot be explained by simple reference to cognitive architecture (the appearance of prismatic core techniques, for example). I do not even deny the existence of culture change. But I do aver that culture change alone is insufficient to account for the developments of the Palaeolithic, that changes in cognitive architecture did occur, and that they were relevant.

The other 21 commentators appear comfortable with my position on evolution in general, but several disagree with my specific scenario (or lack thereof). Priority here must belong to **Parker**, whose 1979 article in *BBS*, written with Kathleen Gibson, is one of the seminal works in cognitive evolution (Parker & Gibson 1979). It was one of the very first arguments to integrate established psychological theory (Piaget) with paleoanthropological evidence (fossil and archaeological). In it Parker and Gibson laid out the hypothesis that extractive foraging of embedded foods was the prime selective agent in early hominid cognitive evolution. The hypothesis still has explanatory power. I did not discuss it, however, because I do not think it can account for the narrow range of cognitive ability that is the focus of the target article. Although extractive foraging certainly has a spatial component, it is not at all clear to me that it could select for the coordination of shape recognition and spatial thinking or for Euclidean spatial cognition, except perhaps as by-products.

**Stout**, **Coventry & Clibbens**, **Stone**, and, by implication, **Wagman**, wonder whether or not developments in skill might better account for the changes in the refinement of symmetry. Compared to spatial cognition, skill has received less attention in cognitive science and has only recently become the focus of serious research in cognitive archaeology (Stout 2002). That there is a component of fine motor control in stone-knapping is beyond question, and the experience of novice stone-knappers supports the contention that the fine three-dimensional symmetrical handaxes are more difficult and take longer to make than the cruder earlier variety (Edwards 2001). It is reasonable to suppose that skill has evolved. However, I do not think that skill alone will produce a symmetry. There must also be a shape and space component to the cognitive task, and this component must have evolved between the times of the earliest and latest handaxes. Yes, skill may have played a part, but it was not the whole story.

**Calvin** is the only commentator to have proposed a detailed alternative scenario for symmetry itself. He folds symmetry into his well-known hypothesis (Calvin 1993; 2002) on the role of aimed throwing in cognitive evolution and in particular the role of handaxes. In a nutshell, Calvin proposes that handaxe shape was determined by its function as a projectile. This hypothesis has shown considerable resilience, despite the skepticism of most archaeologists (Whittaker & McCall 2001; we archaeologists are a stodgy lot). For the purposes of this discussion, the only relevant question is whether or not Calvin's hypothesis could account for the cognitive abilities required for symmetry. My answer is yes – and no. If, as Calvin suggests, the key feature of handaxes is pointyness and sharp edges, then this could select for an ability to coordinate shape with the spatial task of knapping. It would not, however, select the ability to produce three-dimensional congruent symmetries. Calvin himself appears to appreciate this when he writes that "symmetry was initially pragmatic – and only developed into an aesthetic after a million years of proving its usefulness." It is precisely this "aesthetic" that makes the late handaxes so informative.

## R4. Other issues

### *R4.1. Modularity*

Lurking behind several commentaries and explicitly raised by **Silverman** is the conflict between modular and domain-general theories of cognition. From Silverman's theoretical vantage point, my brief discussion of central executive coordination represents a return to archaic views of domain-general intelligence. This even leads him to attribute to me the notion that "human thought processes are qualitatively and profoundly different than those of other species." I certainly do *not* hold such an opinion, and nothing in the target article justifies such an attribution. Silverman has simply lumped me with the group of indistinguishable "others" who do not espouse a massively modular view of the mind, some of whom may even consider human thought processes to be qualitatively different (though I cannot think of any). **Simão** falls on the other extreme, proposing that a very general "mechanism for social transmission" is all that is necessary to account for cognitive evolution. My own theoretical position appears to lie closer to that of **Gurd et al.**, **Masters & Maxwell**, and **Stone** – some encapsulated or dedicated modules are apparent, but there is also a need to posit some executive functions. The archaeological evidence will contribute little to resolving this conflict of views.

### *R4.2. Sex difference*

**Schoenemann** and **Humphrey** raise the issue of the sex difference in spatial cognition. I have treated this issue at much greater length elsewhere (Wynn et al. 1996), but a brief discussion is in order. As Schoenemann notes, the sex difference is real and, for mental rotation, robust. The leading evolutionary hypothesis is that of Silverman and colleagues (Eals & Silverman 1994; Silverman & Eals 1992; Silverman et al. 2000), who argues that the difference evolved as a result of different selective pressures on males and females in hunting and gathering economies. As initially proposed, selection acted on females for the ability to detect patterns in a complex background, an ability that was presumably a feature of female foraging activity. This initial version of the hypothesis failed because the sex difference in spatial cognition results from prenatal developmental events in males, and it is difficult for natural selection to favor females through male development. Palmer, Tierson, and I suggested (Wynn et al. 1996) that the sex difference in spatial cognition, linked as it is to male fetal development, makes a very good candidate for an evolutionary by-product. Our reasoning was based largely on the failure of several adaptive hypotheses, including Silverman's. The latest iteration of Silverman's hypothesis (Silverman et al. 2000) is much stronger because it posits that a male advantage in size constancy underpins the performance difference in mental rotation and wayfinding. What the archaeological record adds to the discussion is timing and context. Silverman's size constancy was in place by 300,000 years ago, probably earlier. The stumbling block is that modern hunters and gatherers were not present 300,000 years ago, so we cannot use them as a surrogate for the environment of evolutionary adaptedness, as Silverman has done. Although we have good evidence for hunting (the Schoeningen spears [Thieme 1997]), the archaeological record presents a picture of opportunistic hunting (Gamble 1999), not the kind of managed or intercept hunting that we associate with modern hunters and modern wayfinding. In other words, the timing and context just do not work. In sum, the archaeological evidence strengthens one aspect of **Silverman**'s argument (the importance of size constancy) but weakens another (wayfinding as a selective agent).

## R5. Conclusion

To conclude, I return to the two goals of the target article. From the commentaries it is clear that I made a successful case for the relevance of archaeological contributions to the evolution of cognition. Although the archaeological record can never stand alone, it can and will provide important evidence for the evolution of specific cognitive abilities and contribute a line of argument parallel to those of human paleontology and evolutionary psychology. My specific example proved more controversial but survived largely intact, with modifications derived from the commentaries. The evidence from artifactual symmetry marks at least two milestones in the evolution of human spatial cognition, one associated with early *Homo erectus,* and the other with the transition from *Homo erectus* to early *Homo sapiens.* However these are interpreted, they indicate that human spatial cognition evolved a very long time ago in circumstances very different from those of the modern world.

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**Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.**

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