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Symbolism and Modern Human Origins¹

by J. M. Lindly and G. A. Clark

Chase and Dibble (1987) have argued that there is little evidence of symbolic behavior in the Middle Paleolithic of Eurasia. They suggest that hominids associated with these archaeological industries were "paleocultural" (*sensu* Jelinek 1977) and that it is only in the Upper Paleolithic that symbolism appears. Concerned that their conclusion might be taken as evidence for cultural discontinuity between archaic *Homo sapiens* and morphologically modern humans, we examine evidence from all the sites earlier than the Upper Paleolithic that have yielded remains identified as morphologically modern humans and find no indication of symbolic behavior by their criteria. We conclude that neither archaic *H. sapiens* nor morphologically modern humans demonstrate symbolic behavior prior to the Upper Paleolithic and that evidence for symbolic behavior cannot be correlated with hominid taxa as has sometimes been claimed. A model of regional continuity across the cultural transition from the Middle to the Upper Paleolithic and the biological transition from archaic *H. sapiens* to morphologically modern humans appears to be supported by the available evidence. There is no indication that the two transitions coincided in time.

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1. We thank Margaret Conkey (University of California, Berkeley) for a critical evaluation of this manuscript and for sharing her

Chase and Dibble (1987) have argued that there is little archaeological evidence for symbolism in the Middle Paleolithic of Eurasia and that this constitutes a significant cultural difference between the Middle and the Upper Paleolithic in this region. The behavioral system of Eurasian Middle Paleolithic hominids, labelled a "paleoculture" (after Jelinek 1977), is considered to have differed from "modern" systems in that it did not include regular, patterned symbolic behavior as part of the repertoire of human adaptation. This "paleocultural" system is contrasted with that of the Upper Paleolithic, considered en bloc and taken to exhibit a "fully modern" range of behaviors with evidence for numerous kinds of nonutilitarian, "symbolic" artifacts. The possible causes of the apparent difference in evidence for symbolism and the implications of Chase and Dibble's results for the current debate on modern human origins are not addressed, yet their conclusions bear on the nature and timing of the transition from archaic to morphologically modern humans and the question of the role of the Neanderthals in the biocultural evolution of the species (see, e.g., Gowlett 1987; Foley 1987a; Mellars 1988, 1989; Stringer 1988; Stringer and Andrews 1988; Feder and Park 1989; Gargett 1989; Bar-Yosef n.d.; Mellars and Stringer 1989; Otte 1988; Trinkaus n.d.a).

We are concerned that Chase and Dibble's conclusions might be taken by anthropologists inclined to see marked discontinuity across the Middle/Upper Paleolithic transition as further "proof" of a major difference between these periods and, consequently, considerable evolutionary "distance" between archaic *Homo sapiens* and morphologically modern humans. From this perspective, absence of evidence for symbolic behavior in archaic *H. sapiens* would support the contention that archaic *H. sapiens* (including the Neanderthals) was an evolutionary dead end and was replaced throughout its range by humans of "modern" type with little or no genetic admixture (see, e.g., Bar-Yosef et al. 1986, Cann, Stoneking, and Wilson 1987, Valladas et al. 1988). While we think that this evolutionary scenario is extremely unlikely to be correct (either in a particular region or in general), the point is simply that there are clear-cut test implications of pattern in the evidence for and against symbolic behavior.

A pattern corresponding to the distinction between archaic *H. sapiens* and morphologically modern humans

thoughts about the nature of symbolism and its relationship to Paleolithic art and ritual behavior. J. Desmond Clark (University of California, Berkeley), Deborah Olszewski (University of Georgia), C. Michael Barton (Arizona State University), Philip Chase and Harold Dibble (University of Pennsylvania), Brian Hayden (Simon Fraser University), Alexander Marshack (Peabody Museum, Harvard University), and CA referees Bernard Wood, Herbert Ullrich, Paul Mellars, and Susan Pfeiffer also discussed, read, and/or commented upon earlier drafts. An anonymous referee made valuable comments as well. We have tried to incorporate suggestions whenever possible, reconcile different points of view, and clarify ambiguous statements, and doing so has noticeably improved the essay. However, all responsibility for errors of commission or omission remains ours.

would support the argument that fully modern behavior did not evolve until and essentially coincided with the appearance of morphological moderns. If, in contrast, evidence for symbolic behavior crosscut the biological transition, it would support the model of multiregional, in situ evolution proposed by Brace (e.g., 1964, 1967, 1988), Wolpoff (e.g., 1980, 1989; Wolpoff et al. 1988), and others (Clark and Lindly 1988, 1989a, b; Šimek and Snyder 1988; Šimek and Price n.d.; Brooks 1988). As a practical matter it must be assumed in either case that human paleontologists can distinguish unambiguously among archaic *H. sapiens*, morphologically modern human, and Neanderthal populations, although we do not believe that they can actually do this (Clark 1988; Clark and Lindly 1988, 1989a, b). An additional entailment of replacement scenarios is that the biological and cultural transitions should have occurred over approximately the same time intervals in all regions, when in fact it appears that the biological transition took place much earlier than any discernible cultural transition in both the Near East and Europe. The evidence suggests that there is no major change in *adaptation*² until relatively late in the Upper Paleolithic in Europe, perhaps as much as 20,000 years after the biological transition to modern humans had taken place (Šimek and Snyder 1988; Šimek and Price n.d.; Clark and Lindly 1989a; Straus 1977, n.d.a; Straus and Heller 1988).

In order to examine the implications of Chase and Dibble's conclusions for the study of modern human origins, we consider archaeological evidence for symbolism from regions of the Old World in which the remains of morphologically modern humans occur long before the beginning of the Upper Paleolithic (or, in Africa, the Late Stone Age). The survey is exhaustive: these are the only sites in the world that have produced alleged morphologically modern human remains earlier than the Upper Paleolithic/Late Stone Age. If evidence for symbolic behavior can be correlated with hominid taxa, and if (as is widely assumed) "symbolism" has some adaptive significance, we should be able to detect differences between assemblages associated with archaic *H. sapiens* and those associated with pre-Upper Paleolithic moderns. To keep the results comparable, the archaeological evidence used to assess symbolic behavior is limited to the four classes of data examined by Chase and Dibble (1987:265): (1) lithic assemblages, (2) burial data, (3) evidence for ritual behavior other than that associated with burials, and (4) art.³

2. We define adaptation as evolutionary biology does: any structure, physiological process, or behavioral pattern that makes an organism more fit to survive and to reproduce (Wilson 1975:577). Behavior can be viewed as the dynamics of adaptation—a strategy for survival and reproduction (Binford 1972:133).

3. Although we do not intend to develop the argument here, we consider Chase and Dibble's criteria for monitoring symbolic behavior equivocal and inadequate. No argument is presented to warrant their use as unambiguous indicators of the cognitive capacities of the hominids in question, nor are other potentially more informative monitors of symbolic behavior considered. To base a global evolutionary characterization on the limited archaeological evi-

Morphologically modern human skeletal remains dating to the Middle Paleolithic (or, in Africa, the Middle Stone Age) have been reported from Starosel'e (Alexeyev 1976; see also A-M. Tillier, cited in Ronen 1982:315), Darra-i-kur (Angel 1972), Skhül (McCown and Keith 1939; Trinkaus 1982, 1984, 1986), and Qafzeh (Bar-Yosef and Vandermeersch 1981) in southwestern Asia; Dar es-Soltane (Debénath 1975, Trinkaus 1986) and Temara (Ferembach 1976) in North Africa; and Klasies River Mouth (Singer and Wymer 1982, Rightmire 1984), Border Cave (Beaumont, de Villiers, and Vogel 1978, Butzer, Beaumont, and Vogel 1978), Mumba Rockshelter (Bräuer and Mehlman 1988), Laetoli (Rightmire 1984), Omo (Bräuer 1984, J. D. Clark 1988), Porc Epic (Bräuer 1984, J. D. Clark 1988), and Singa (Bräuer 1984, but cf. Stringer 1979) in Africa south of the Sahara. If the contexts surrounding these morphologically modern human remains have little or no evidence of symbolic behavior, it will be clear that no correlation of modern behavior with modern morphology can be proposed.

Review of the Evidence

SOUTHWESTERN ASIA

Four Middle Paleolithic sites in southwestern Asia have been reported to contain the remains of morphologically modern humans: Starosel'e (Soviet Crimea), Darra-i-kur (Afghanistan), Skhül, and Qafzeh (both in Israel).

Starosel'e Cave is located in a dry tributary valley of the Churuk-su River and was excavated by the Soviet prehistorian and anthropologist A. Formozov in 1952–56. Deposits at the site ranged in depth from 60 cm to 4 m. The skeletal remains consist of the partial skeleton of a child, the chin section of an adult mandible, and single fragments of radius and humerus, all considered directly associated with Mousterian artifacts. The infant skeleton, found at a depth of 70–90 cm, is classified as "modern" on the basis of a comparison with modern infant remains of similar age and a reconstruction of the di-

dence from a single region is increasingly problematic, especially in light of the allegedly "delayed" character of modernization events and processes in that small and well-studied region. Conkey (1987a, n.d.) has pointed out that, if hominids were structuring their lives and activities in ways that we would recognize and understand as symbolic, this would probably be manifest in more subtle, contextual kinds of archaeological evidence than those examined by Chase and Dibble—in other words, that we should seek to develop more sophisticated ways of analyzing the archaeological record of symbolism than simply charting the presence or absence of "art," "style," or "ritual activity." While the point is well taken, the exceptionally coarse grain of the Old World Upper Pleistocene archaeological record makes these potentially more sensitive monitors of symbolic behavior exceedingly difficult to operationalize. Conkey has further pointed out (personal communication) that the Chase and Dibble essay is wholly dependent upon a distinction between culture (which seems to be equated with symbolism—the "conscious production of meaning") and paleoculture (consciousness apparently without symbolic behavior), but these terms are left undefined. Without a clear sense of what culture is, what symbolic behavior is, and what relationships might have obtained between them, it is difficult to put much faith in it.

mensions of the skull as it would have appeared as an adult (Alexeyev 1976; see also A-M. Tillier, cited in Ronen 1982:315). The adult remains were found at approximately the same level as the infant, and the mandible fragment is considered "modern" in every sense of the word. None of the remains are judged to represent intentional burials (Klein 1965). The lithic assemblage is identified as Mousterian because of the presence of limaces, discoidal cores, bifacially flaked "Quina-type" sidescrapers, and simple, double, convergent, transverse, and *déjeté* sidescrapers made on flakes and is said to resemble Charentian industries of southwestern France (Klein 1965:63). Despite the recovery of more than 11,000 stone artifacts and some 60,000 unworked bone fragments (dominated by *Equus*), there is no indication of a stylistic component (by anyone's definition [see Sackett 1982, Binford 1989, Clark 1989b]) or of ritual paraphernalia or art.

At Darra-i-kur, a rock-shelter in western Badakhshan, the human remains consist of a right temporal fragment that is "modern in appearance" (Angel 1972). It is not considered to pertain to an intentional burial. The lithic assemblage is Mousterian, comprising more than 800 Levallois flakes and points, handaxes, sidescrapers, flake/blades, and debitage (Dupree and Davis 1972). The only object recovered that might, by a considerable stretch of the imagination, be considered symbolic is a fossil shark's tooth tentatively identified as "worked" (Dupree 1972:79). A so-called bone fabricator is reported to be worked on both ends, but it is not clear from the illustrations how it was "worked" and it is at least equally probable that it is a diaphysis fragment gnawed by carnivores. Since the excavation at Darra-i-kur predated the current concern with taphonomic processes, it is unlikely that the investigators would have distinguished between human and animal modification of bone except where the difference was fairly obvious. There is no art or unequivocal evidence of symbolic activity at this site.

Mugharet es-Skhul, on the Israeli coast, is the smallest of the Mt. Carmel caves investigated by Dorothy Garrod in the 1920s and 1930s. Skhul was also excavated by Theodore McCown in 1931. The human remains constitute one of the best samples in southwestern Asia (>10 individuals and numerous fragments). Both classic and modern researchers consider them morphologically modern (McCown and Keith 1939; Trinkaus 1982, 1984, 1986). Many appear to have been purposefully buried, albeit for the most part without grave goods. Skhul 5 may have been interred with offerings; McCown (1937:104) argues that a boar (*Sus scrofa*) mandible was clasped in its hands because "the left forearm rests upon the broken, hinder ends of the mandible" (p. 100; see his pl. 52[2], reproduced here as fig. 1). Most of the ribs and vertebrae as well as part of the pelvis and most of the right leg are missing, however, and McCown notes some crushing of the lower part of the skeleton by "an ancient disturbance" and reports that "it was impossible to determine the exact limits of the grave" (p. 101). From this and the fact that *Sus* occurs throughout the Skhul depos-

its and is suggested to have been a dietary item (Bate 1937:148) it seems reasonable to conclude that the boar mandible may have become spatially associated with the human remains through some process other than deliberate inclusion in a grave. The skeletal remains are associated with Middle Paleolithic artifacts recently classified as Phase 2/3 Mousterian and on these grounds argued to be "late" (ca. 40,000–50,000 years B.P.) (Jelinek 1982). Levantine Mousterian lithic assemblages have no clear stylistic component and appear instead to reflect variation in raw-material size and/or availability that constrains choice among reduction strategies (Clark and Lindly 1988, 1989a, b; Lindly and Clark 1987).

Qafzeh is an inland cave site located near the village of Nazareth in the lower Galilee. It was excavated by Neuville and Stekelis (1932–35) and by Vandermeersch and Bar-Yosef (1965–79, 1983–present). The cave contains both Middle and Upper Paleolithic deposits. The Qafzeh hominid remains are all morphological modern humans and are considered similar morphologically to the remains found at Skhul (Vandermeersch 1981). Sixteen individuals have been recovered. One, Qafzeh 11 (an infant), is reported to have had associated grave goods; the antlers of a fallow deer (possibly *Dama mesopotamica*) are described as "held in the hand" of the child (Vandermeersch 1970). *Dama mesopotamica* is, however, an economic species that occurs in Level 22, with which Qafzeh 11 is associated (Bouchud 1974). While the remains of this species are not especially numerous, they nevertheless account for 20.4% of the faunal remains in Levels 18–22, and the possibility of a fortuitous association cannot be ruled out. Ochre is present throughout the deposits but not associated with the hominid remains (Vandermeersch 1969). There is no evidence of ritual behavior other than the equivocal burial data or of art.

The lithic industries of the Mousterian levels at Qafzeh have been classified as Tabun B/C or Levantine Mousterian Phase 2/3 and have until very recently been considered "late" (ca. 40,000–50,000 years B.P.) (Jelinek 1981, 1982). Jelinek's interpretation of the age of Qafzeh has been challenged by the recent evidence from geomorphology, the biostratigraphy of the microvertebrate faunas, and amino-acid racemization dates (Bar-Yosef and Vandermeersch 1981, Bar-Yosef n.d.). A series of thermoluminescence dates on burnt flint has yielded an average age of $92,000 \pm 5,000$ years B.P. for the hominid-bearing deposits (Valladas et al. 1988), and a number of electron-spin-resonance determinations average 98,000 years B.P. (Schwarcz et al. 1988). If these dates are correct, the Qafzeh hominids are the earliest dated remains of morphologically modern humans in the world. It should be noted that the dates for the Qafzeh Mousterian levels are approximately the same age as is proposed for Tabun D/Phase 1 "early" Levantine Mousterian assemblage from Tabun, suggesting that the normative phase sequence currently used to organize Middle Paleolithic assemblages in a loose chronological order is in need of serious revision (see, e.g., Lindly and Clark 1987; Clark and Lindly 1988, 1989b).

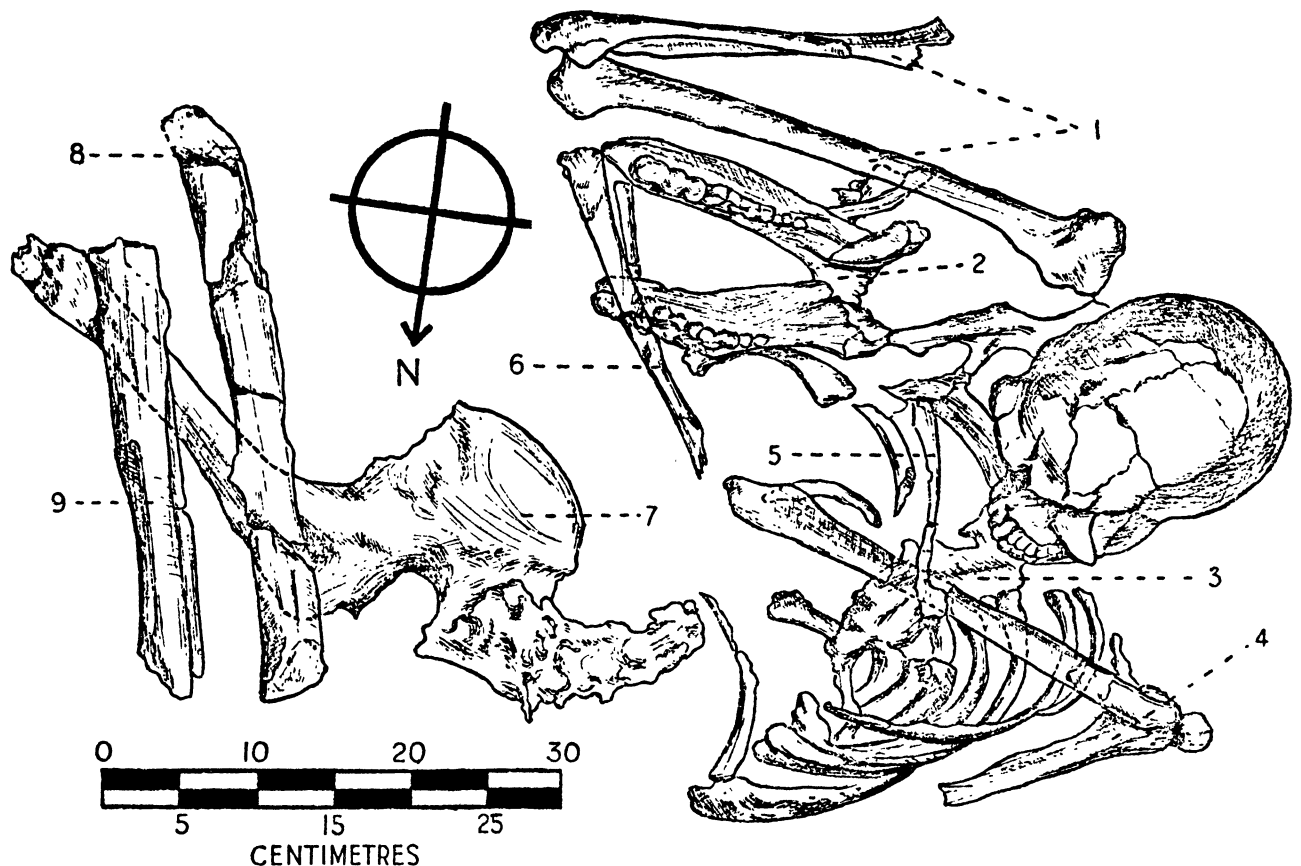


FIG. 1. Plan of the contracted burial of a tall male, Skhūl 5. 1, right arm; 2, *Sus scrofa* mandible; 3, dorsal vertebrae; 4, left scapula and humerus; 5, left clavicle; 6, left radius; 7, right ilium; 8, left femur; 9, left tibia and fibula. (Reprinted from McCown 1937:pl. 52[2] by permission of the publisher.)

NORTH AFRICA

North Africa has also produced several sites with claimed early morphological modern human remains, in every case associated with Aterian lithic assemblages (J. D. Clark 1983, Ferring 1975).

Dar es-Soltane, a cave site on the coast of Morocco, has "lower" Aterian levels that have produced two partial human crania of essentially modern appearance (Debénath 1975, Trinkaus 1986). Single modern parietal and occipital fragments from the Grotte des Contrebandiers (Smugglers' Cave) at Temara are also associated with an Aterian assemblage (Ferembach 1976). There is no indication that these remains were buried intentionally. The Aterian is usually considered Middle Paleolithic. Although it has produced some radiocarbon dates younger than 30,000 years B.P., most Aterian assemblages are probably more than 40,000 years old (Ferring 1975; J. D. Clark 1982, 1983). Radiocarbon-dated Aterian assemblages range from >39,900 years B.P. in the Maghreb and 41,500–45,000 years B.P. (with enormous standard deviations) at the Haua Fteah (Cyrenaica) to >27,000 years B.P. in an upper level and >30,000 years B.P. in a lower one at Dar es-Soltane. Therefore, despite the assertion that the Dar es-Soltane Aterian is typologically "early,"

the site probably dates to the latest part of the Aterian sequence. There are no radiocarbon dates from Temara.

The Aterian "facies" of the North African Mousterian is composed primarily of Levallois debitage including blades with faceted platforms, sidescrapers, points, and endscrapers, but some assemblages include tanged pieces (pedunculates) and bifacial foliate points (Ferring 1975). Nothing in the published accounts of Aterian sites suggests the presence of symbolic behavior in the form of stylistic patterning in stone tool assemblages or of ritual or art (Ferring 1975).

SUB-SAHARAN AFRICA

Several Middle Stone Age sites in southern Africa contain what until the publication of the Qafzeh dates were considered the world's earliest morphologically modern human remains (Beaumont et al. 1978, Bräuer and Mehlman 1988, Grine and Klein 1985, Singer and Wymer 1982). There is some controversy about the age of these sites (Butzer 1982, Shackleton 1982), but there seems to be consensus that they all date prior to the limits of radiocarbon (Volman 1984).

Excavations in a series of caves and rock-shelters at the mouth of the Klasies River, on the east coast of the

Republic of South Africa, have produced five partial mandibles, a maxilla, and various small craniofacial fragments, teeth, and postcranial bones considered to be those of morphologically modern humans (Singer and Wymer 1982, Rightmire 1984). None are thought to represent burials (Singer and Wymer 1982:147). The Middle Stone Age levels at these sites are primarily assigned to Stages 1–4 (including the lamellar Howieson's Poort industries), although there are also overlying Late Stone Age deposits (Singer and Wymer 1982). Some workers (e.g., Butzer 1982) have proposed a date for them as early as >120,000 years B.P. Middle Stone Age lithic assemblages from southern Africa consist mainly of flakes and flake/blades from well-prepared cores with retouched tool types such as points, denticulates, and sidescrapers. The Howieson's Poort is somewhat different from but an integral part of the Middle Stone Age. Instead of the "generic" Middle Stone Age flake tools, it contains an abundance of backed pieces (blade segments) and rather large but well-made geometrics, such as lunates (or crescents) and trapezoids (Volman 1984). The Howieson's Poort is seen by some as an "adaptive response" to environmental perturbation that included changes in mobility patterns and possibly the conservation of more distant and higher-quality raw-material sources (Mellars 1988, 1989; cf. Parkington n.d. for a more critical view), and from this perspective the technological changes it represents need not be considered matters of style.

Evidence for symbolism in the Middle Stone Age levels at Klasies River Mouth (and in all African Middle Stone Age sites) is extremely scarce. The lithic industries show no patterning that can be considered stylistic. The best-defined change is one in retouched tool forms that corresponds to the appearance of the Howieson's Poort industry in Layers 10–21 of Shelter 1A. Above Layer 10, there is a reappearance of the modal kind of Middle Stone Age assemblage. The African Middle Stone Age has been divided into stages according to debitage characteristics such as blank size and shape, percentage of faceted butts, and core types. The extremely questionable but time-honored practice of using retouched pieces in these classificatory schemata is hampered by extremely low tool frequencies (<1% in most cases) (Volman 1984:201). In many cases, it has proven difficult to separate Middle Stone Age stages from one another on the basis of characteristics of the lithic assemblages alone, and stratigraphic and paleoenvironmental information is often utilized. It is difficult to avoid the impression of an essential continuity and homogeneity in these African equivalents of the Middle Paleolithic. At present, the best single criterion for subdividing the African Middle Stone Age industries (including Howieson's Poort) appears to be changes in raw material linked, probably, to changes in the settlement-subsistence systems within which raw-material procurement was embedded (see Binford 1979).

There are four instances of worked bone from Klasies: two serrated rib fragments and one bone with thin, regular parallel grooves from a Middle Stone Age 2 level and a bone "point" from one of the Howieson's Poort levels.

These very rare instances of worked bone correspond to Chase and Dibble's reports for the Eurasian Mousterian. Ochre was also found dispersed throughout the Middle Stone Age levels. Although there is no direct association of ochre with the hominid remains, some of the larger pieces show striations, faceting, and abraded surfaces suggesting use as a colorant. This evidence for symbolic behavior is, however, both equivocal and scarce, and there is no indication of a pattern in its occurrence.

Border Cave, on the Swaziland/Kwa-Zulu border in the Lebombo Range, has yielded a long Middle Stone Age sequence tentatively associated with skeletal material considered morphologically modern human (Beaumont, de Villiers, and Vogel 1978, Butzer, Beaumont, and Vogel 1978, Beaumont 1980). The hominid fossils consist of mandible and cranial fragments from three adult individuals, a relatively complete infant skeleton that is considered a burial, and postcranial remains of uncertain provenience recovered in uncontrolled digging for agricultural fertilizer at the site. One adult mandible and the infant burial were apparently found in situ (Rightmire 1984). The specimens associated with Middle Stone Age artifacts are all considered to be the remains of morphologically modern humans (Beaumont 1980).

Perhaps the best evidence for symbolic behavior from the site is the possible infant burial from the Middle Stone Age 2b level, with an associated perforated *Conus* shell that can only have come from the Straits of Madagascar, some 80 km distant. The Middle Stone Age 3 assemblage contains a notched rib fragment and seven split-tusk "daggers," possibly from a warthog, that show signs of abrasion. It is not clear from the photographs (Beaumont, de Villiers, and Vogel 1978) whether the abrasion on the tusk fragments was produced by human agency (as Beaumont suggests) or, as seems more likely, by natural processes during the life of the animal. (Suid canines are typically broken and abraded in vivo, especially at the tip [Brain 1981].) As at Klasies River Mouth, hematite flecks are found throughout the levels, with some of the larger pieces showing wear facets, striae, and other signs of abrasion (Volman 1984).

Two additional sites in southern Africa are sometimes mentioned as having produced evidence for symbolic behavior. A Middle Stone Age level at Florisbad, in the Orange Free State, has produced a broken curved wooden implement with parallel markings on the end (Volman 1984). Since the famous hominid cranium from the Middle Stone Age deposits at this site has recently been reconstructed and is now considered archaic *H. sapiens* (Kuman and Clarke 1986), its co-occurrence with a worked wooden object might be viewed as further support for the view that there is no link between symbolic behavior and modern morphology. Middle Stone Age 2b levels at Apollo 11 Cave in Namibia have yielded two notched bone fragments, and there are additional incised fragments of ostrich eggshell in the Howieson's Poort (Wendt 1976, Volman 1984), but there are no hominid remains from this site to indicate who the makers of these artifacts might have been.

TABLE 1
Evidence for Symbolic Behavior from Sites in the Old World Associated with Purported Pre-Upper Paleolithic Morphologically Modern Humans

Sites	Evidence	Source
Starosel'e	None	Klein (1965)
Darra-i-kur	Bone "fabricator" "Worked" (?) shark's tooth	Dupree (1972)
Skhül	Boar mandible associated with Skhül 5 burial	McCown (1937)
Qafzeh	Deer antler associated with Qafzeh 11 burial Ochre throughout deposit	Vandermeersch (1981)
Dar es-Soltane	None	Debénath (1975)
Temara	None	Ferembach (1976)
Klasies River Mouth	2 serrated rib fragments and a bone with thin regular parallel grooves in an MSA 2 level Bone "point" in a Howieson's Poort level	Singer and Wymer (1982) Volman (1984)
Border Cave	Ochre with abrasions and facets dispersed throughout MSA levels <i>Conus</i> shell found with infant burial in an MSA 2b level Notched rib fragments and 7 split-tusk "daggers" in an MSA 3 level Hematite with wear facets	Beaumont et al. (1978) Volman (1984)
Mumba Rockshelter	None	Bräuer and Mehlman (1988)
Laetoli Hominid 18	None	J. D. Clark (1988)
Omo 1 & 3, Kibish Formation	None	J. D. Clark (1988)
Porc Epic	Hematite with wear facets	J. D. Clark (1988)
Singa	None	Stringer (1979)

Mumba Rockshelter in Tanzania has produced three hominid molars from Middle Stone Age levels dated to ca. 130,000 years B.P. that supposedly fall within the range of variation seen in modern African populations and are consequently considered morphologically modern (Bräuer and Mehlman 1988). The Ngaloba Beds at Laetoli in Tanzania, dated by uranium series to 120,000 years B.P., have yielded an almost complete skull (Laetoli Hominid 18) with both "modern" (expansion of the vault, rounded occiput) and "archaic" (frontal flattening, supraorbital torus, thick cranial bones) features (Rightmire 1984). The fossil is associated with a Middle Stone Age assemblage that lacks a heavy-duty-tool component (J. D. Clark 1988). The Kibish Formation at Omo in Ethiopia has produced an incomplete calvarium with some associated postcranial bones (Omo 1), a second incomplete calvarium (Omo 2), and some fragmentary cranial bones (Omo 3) (Bräuer 1984, J. D. Clark 1988). Omo 1 was recovered from overbank deposits on the surface of Stratum e at the top of Member 1. It has morphologically modern human features and is associated with a small redeposited Middle Stone Age assemblage containing Levallois flakes. An age of ca. 130,000 years B.P. is suggested by a uranium-series date on shell from the sediments that produced the skull. Omo 2 was a surface find ca. 2.5 km away that could have come from the same geological horizon; interestingly, it has several *H. erectus* features that place it outside morphological modern humans and, indeed, archaic *H. sapiens*. If it is penecontemporaneous with Omo 1, a population of enormous morphological variability is indicated. Omo 3 comes from Member 3 and is probably somewhat (perhaps considerably) younger than the others. An age somewhere in the 100,000–40,000 years B.P. interval is suggested, since

the finds lie outside the range of radiocarbon. In the Ethiopian Rift at Porc Epic, a robust mandible fragment associated with a Middle Stone Age assemblage was recovered by Breuil in 1933. There is a minimum-age obsidian-hydration date of 60,000–70,000 years B.P. from artifacts in the brecciated cave earths from which the fossil was apparently extracted. Measurements and robusticity of the find indicate that it lies within the range of variation of modern humans, but it also has archaic features reminiscent of the Neanderthals (Bräuer 1984, J. D. Clark 1988). Finally, from Singa, on the Blue Nile in southeastern Sudan, a heavily mineralized skull with most of the face missing was found in 1924. It has recently been described both as archaic *H. sapiens* (Stringer 1979) and as "completely modern" (Bräuer 1984). There are no associated artifacts. Evidence for symbolic behavior is absent from all these East African morphologically modern human sites except for the occurrence of hematite with wear facets at Porc Epic (J. D. Clark 1988:299). In no case is the context of discovery primary, and in most cases the skeletal remains have clearly been redeposited by geological agencies. The Middle Stone Age chipped-stone assemblages found with morphologically modern human fossils in this area are indistinguishable from those found with archaic *H. sapiens* fossils. In neither case are there examples of parietal or mobile art, ornaments, bone artifacts, or burials.

Concluding Remarks

The results of our survey (table 1) suggest that, as Chase and Dibble have reported for archaic *H. sapiens*, the daily activities of pre-Upper Paleolithic morphologi-

cally modern humans had no archaeologically discernible symbolic component, at least in the regions we examined and quite probably in any region of the Old World that has produced an Upper Pleistocene archaeological record. Does this apparent "fact" render these morphologically modern humans significantly different from or less human than morphological moderns associated with Upper Paleolithic industries in Europe, who admittedly do exhibit symbolic behavior? Are they to be considered outside the evolutionary trajectory of modern humans? Clearly, the answer is no. Yet, on the basis of similar lack of evidence for behavior like that observed in the European Upper Paleolithic, Eurasian archaic *H. sapiens* has been considered different enough from morphologically modern humans to warrant rethinking of the biological and cultural relationships between these hominids (Bar-Yosef 1987, n.d.; Gargett 1989; Foley 1987a; Gowlett 1987; Mellars 1988, 1989; White 1982, 1989a; Clark and Lindly 1989a). Support for a hypothesis of no difference in this regard between archaic *H. sapiens* and morphologically modern humans of the Middle Paleolithic/Middle Stone Age calls into question the credibility of the replacement scenario for modern human origins and suggests that archaic *H. sapiens* cannot be relegated to an evolutionary backwater (see also Marshack 1988a, n.d.).

That the pattern in the evidence for symbolic behavior is the same whether the hominids associated with Middle Paleolithic/Middle Stone Age archaeological assemblages are archaic *H. sapiens*, Neanderthals, or morphological moderns implies that the taxonomic units themselves are unreliable (which we think very likely) and/or that the major shift in adaptation occurred late in the Upper Paleolithic/Late Stone Age and was largely unrelated to the perceived transition from the Middle to the Upper Paleolithic (see Chase 1986, 1989; Šimek and Price n.d.; Šimek and Snyder 1988; Brooks 1988; Svoboda 1988, n.d.; Geneste 1988; Böeda 1988; Straus n.d.a; Straus and Heller 1988; Marshack 1988a, b, n.d.; Clark and Lindly 1988, 1989a, b).

The latter conclusion is likely to be disputed by workers who argue from European data for a "symbolic explosion" at the beginning of the Upper Paleolithic (e.g., and esp., White 1982, 1989a, b). Though Europe, since it has not produced any early morphologically modern humans, is tangential to our major argument, we therefore wish to make our views on the European situation absolutely clear. We readily acknowledge an apparent latitudinal component to the archaeological evidence for symbolism that implies possible differences in social complexity between western Eurasia and the rest of the Old World at ca. 20,000–15,000 years B.P. To argue that the Middle/Upper Paleolithic transition at ca. 38,000–35,000 years B.P. is a major threshold in cultural evolution (cf. White 1982, 1989a, b) is, however, an oversimplification.

We are not suggesting that there is no evidence for symbolism in the early Upper Paleolithic of Europe (conventionally dated ca. 38,000–20,000 years B.P.). Indeed, Marshack (1988a, b, n.d.) has demonstrated, through a series of exhaustive studies, that evidence for symbolic

behavior, while rare and sporadic, extends well back into the Middle Paleolithic and, in aggregate, strongly supports the idea of behavioral continuity across the Middle/Upper Paleolithic transition (see also González Echegaray 1988). We do, however, seriously doubt that much of the early Upper Paleolithic evidence (i.e., ivory sculptures from Aurignacian sites in Germany; limestone engravings, ornaments from French Aurignacian sites; Aurignacian bone points; the Sungir and Dolní Věstonice burials; the Dolní Věstonice clay figurines; the Mladeč ornaments; etc.) can be shown to date to the beginning of this period. Compared with the late Upper Paleolithic, the early Upper Paleolithic has relatively few radiometric dates, and in most cases "symbolic artifacts" are considered "early" only on the basis of allegedly time-sensitive "index-fossil" tool types and normative characterizations of assemblage sequences—both notoriously unreliable as temporal indicators (Clark and Straus 1986, Straus 1987a, Straus and Heller 1988). It should be kept in mind that the conventional early Upper Paleolithic analytical units (Aurignacian, Perigordian, Uluzzian, Gravettian, Szeletian, etc.) span ca. 18,000–16,000 years and that aggregation of the evidence from any such unit may make change appear "explosive."

Again, while there is some unambiguous parietal and mobiliary art from the early Upper Paleolithic, when sites that have produced absolute determinations are examined the overwhelming majority of it postdates 20,000 years B.P. (Conkey 1983, 1987, personal communication). This also applies to the worked bone and antler inventories and the burials (cf., e.g., White 1987, 1989a with Julien 1983, May 1986). All but 3 of the 74 relatively unambiguous Upper Paleolithic burials studied by May (1986) are not only from the late Upper Paleolithic (25,000–12,000 years B.P.) but from its latest phases. The recently discovered triple burial at Dolní Věstonice is radiocarbon-dated at 27,600 and 26,600 years B.P. (Bahn 1988). The Sungir burials are 20,000–25,000 years old (Fisher 1988). If one were to "scale" the incidence of art, bone/antler artifacts, and/or burials per unit time (e.g., number of items or occurrences per millennium), it would immediately become evident that, *contra* White (1987, 1989a), the "symbolic explosion" occurred not at the Middle/Upper Paleolithic transition but in the late Upper Paleolithic, at ca. 20,000–15,000 years B.P. The rate at which such evidence accumulates increases slowly during the early Upper Paleolithic, more rapidly during the late Upper Paleolithic, and even more rapidly in the Mesolithic and beyond (Clark and Neeley 1987).

Finally, abundant European archaeofaunal evidence shows few marked changes coincident with the local Middle/Upper Paleolithic boundaries, and those that are detectable can usually be attributed to climate (Straus 1977, Delpech 1983, Clark 1987). Since few would dispute that the character of faunal assemblages is a much more direct monitor of human adaptation than art, ornamentation, or mortuary practices, it is not unreasonable to conclude that here, too, is evidence for clinal, relatively gradual change, in some regions (e.g., northern

Spain) accelerating sharply about 20,000 years ago (Clark and Straus 1983, 1986). In short, a model of regional continuity between Middle and Upper Paleolithic and between archaic and modern *H. sapiens* (see, e.g., Brace 1967, 1988; Wolpoff 1980, 1989) appears to be supported in Europe as well as elsewhere.

This is not to say that we or any of the other researchers cited in support of the continuity position consider the two transitions to have been simultaneous, but this is in fact a clear implication of the replacement scenario. In evolution, behavioral change always occurs well in advance of related morphological change. If some kind of relationship obtained between the emergence of morphologically modern humans and the Upper Paleolithic/Late Stone Age, the morphological changes that supposedly allow human paleontologists to distinguish between morphologically modern humans and archaic *H. sapiens* should have been preceded by many thousands of years by significant adaptive shifts. There is *no evidence whatsoever* for such adaptive shifts in Eurasia, Africa, or the Levant—although, admittedly, they would be exceedingly difficult to detect given the inadequacies of the time/space grid.

We irreverently conclude that the replacement scenario is very likely a product of sampling bias and entrenched regional research traditions (Binford and Sabloff 1982). While no one disputes the European evidence for Upper Paleolithic symbolism, the case for the rest of the Old World is wholly dependent upon argument from negative evidence: areas outside of Europe are judged not to have been characterized by the same level of social complexity as Europe during the 25,000–10,000-years-B.P. interval because they have not produced comparable evidence of symbolism. Such areas are, of course, much less intensively investigated than Europe, and taphonomic and macroclimatic factors may have combined to erase any evidence of social complexity comparable to that of Europe. We do not imply, however, nor do we believe, that *rates* of Upper Pleistocene biocultural evolution were everywhere the same.

The question of symbolic behavior on either side of the archaic *H. sapiens*/morphologically-modern-human transition is one small part of the larger issue of the appearance of modern humans. In the absence of any evidence for differences in adaptation among archaic *H. sapiens*, Neanderthal, and early morphologically modern human populations—differences that would be expected if in fact these taxa were really distinct—the case for replacement of archaic *H. sapiens* by moderns rests solely upon the assertion that morphologically modern humans displaced archaic *H. sapiens* because they were more “advanced.” We submit that the Old World Upper Pleistocene archaeological record exhibits none of the discontinuity implied by the replacement model and that it is incumbent upon its advocates to show how replacement could have occurred without leaving traces of disjunction in the typological and technological aspects of archaeological assemblages, in those aspects of the archaeofaunal record that monitor subsistence, and in the evidence from settlement-pattern studies (Clark

and Lindly 1989a). In light of this continuity, acknowledged even by archaeologists who support biological replacement (e.g., Bar-Yosef and Meignen 1989), it seems more reasonable to suggest that the taxa employed by replacement systematics are defective and that archaeologists would be well advised not to take them at face value. A satisfactory explanation of the origins of modern humans must reconcile the archaeological and fossil evidence and the evidence from molecular biology. In our opinion, only a multiregional model of cultural and biological continuity can do so.

Comments

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Lindly and Clark have attempted to deal with the questions raised by the new thermoluminescence and electron-spin-resonance dates from Kebara, Qafzeh, and Skhül (Valladas et al. 1987, 1988; Schwarcz et al. 1988, 1989; Stringer et al. 1989). Until two years ago, most scholars ignored the biostratigraphic evidence and the alternative palaeoclimatic interpretations for the Tabün sequence and other major cave sites, placing the Western Asian Neandertals (Amud, Kebara, Shanidar, Tabün) earlier than the early anatomically modern humans (Skhül, Qafzeh) to argue for local evolution of modern humans in Southwestern Asia (Bar-Yosef 1989). The acceptance of the greater antiquity of the early anatomically modern *Homo sapiens* from Qafzeh and Skhül has raised many interesting questions concerning human evolution in the Upper Pleistocene, especially questions of taxonomy, the relationship between archaeological assemblages and fossils, and the anthropological meaning of the transition from the Middle to the Upper Paleolithic—all of which are confused by Lindly and Clark.

They are correct in pointing out the paradoxical lack of any discernible behavioral differences in the archaeological record between early anatomically modern and archaic *H. sapiens*. We agree that there is evidence for significant differences of symbolic behavior between the archaeological records of the Middle and Upper Paleolithic and that symbolic expressions become much more prevalent and elaborate during the late glacial maximum/oxygen-isotope stage 2 (24,000–14,000 years ago). We strongly disagree, however, that the absence of archaeological evidence for different symbolic behaviors associated with archaic and modern *H. sapiens* during the Middle Paleolithic constitutes either support for the multiregional hypothesis or refutation of the single-origin hypothesis.

Lindly and Clark make a number of taxonomic and phylogenetic errors. Fossil taxa such as archaic and mod-

ern *H. sapiens* can only be defined on the basis of morphological criteria, some of which may imply behavioral differences (Mayr 1942). Although Lindly and Clark are justified in looking for different behaviors that are specific to these taxa (as predicted by the evolutionary principle of competitive exclusion), the absence of archaeological evidence for behavioral differences—particularly in connection with something as ephemeral as symbolism—is a poor basis on which to criticize taxonomy. Fossil species are defined not by their archaeological traces or by their presumed evolutionary relationships to modern humans but by the range of variation of their morphologies. Whether or not Neandertals and modern *H. sapiens* are different species is difficult to test and hence open to debate; however, to argue that there are no behavioral differences between these taxa (and hence that these taxa are incorrectly defined) ignores the large body of functional morphological data that clearly demonstrates otherwise (Trinkaus 1986).

Lindly and Clark incorrectly attempt to fit archaeological data (actually the absence of evidence for symbolic behavior) to the biological question of whether anatomically modern humans evolved in one region or in numerous regions. The evolutionary relationships between taxa such as archaic and modern *H. sapiens* can only be determined by analyzing their morphological characteristics (whether by cladistic and/or phenetic methods). For the Middle Paleolithic of the Levant, there is no close correspondence between hominid morphotypes and the lithic industries with which they are found. For example, hominids such as the Neandertal woman from Tabūn layer C are associated with the same industry as the hominids from Qafzeh (layers 17–24). One can argue that the associations between burials and industries in Levantine caves are coincidental, but this argument ignores their repetitive nature. Lindly and Clark's argument concerning the purportedly "disturbed" Skhūl and Qafzeh burials is at variance with photographic records and the illustration included in their article. (Perhaps they have confused McCown's reference to disturbance in the "lower part of the body," which clearly refers to the lower limbs of the Skhūl 5 burial, with the stratigraphically "lower" portion of the thoracic region.) Again, it is true that fallow deer remains were uncovered at Qafzeh, but only as small fragments; the occurrence of the large antlers across the chest of the dead child can hardly be conceived as accidental. The mere observation that Mousterian burials are somewhat different from expectations based on Upper Paleolithic, Mesolithic, and Neolithic burials does not mean that these were unintentional or unaccompanied by offerings. This is why accurate, well-recorded field observations are essential (Villa 1989). Moreover, the nearly complete articulation of many of these skeletal remains at sites demonstrably frequented by hyenas is compelling evidence for their integrity (Trinkaus 1989).

Although the archaeological record can tell us little about the phylogeny of prehistoric hominids, behavioral inferences from archaeological residues can inform us about their coevolutionary relationships. There is no

credible archaeological evidence for prolonged contact, interbreeding, or evolutionary continuity among Neandertals and early modern humans in the Levant, and current data suggest that they would have been ecologically incompatible. Lithic use-wear analysis (Shea 1989) indicates that tool functions were virtually identical in kind and in relative frequency among Levantine Mousterian sites located in the Mediterranean woodland phytogeographic zone. Moreover, the large-mammal components of the archaeofaunas from these sites are essentially the same (Tchernov 1988:219–22). Neandertals and early modern humans, it seems, utilized this part of the Levant in essentially the same way for a considerable period of time, which would likely have placed them in competition for the same set of plant and animal resources—a profound obstacle to prolonged coresidence. Not surprisingly, Neandertal and early modern human skeletal remains have not been found in the same strata of any Levantine site. Accordingly, the Levantine Mousterian archaeological record can most parsimoniously be modeled as documenting shifts in the occupation of what is today northern Israel by at least two distinct hominid taxa competing for approximately the same ecological niche. Indeed, a similar interpretation is possible for Late Pleistocene prehistory throughout western Eurasia.

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The nature of Upper Pleistocene hominid behavioral and biological changes is the subject of considerable recent controversy. Three distinct issues, each entailing a quite different kind of evidence, are being debated simultaneously:

1. Whether the differences between archaic *Homo sapiens* and morphologically modern *H. sapiens* should be interpreted as inter- or intraspecific. Resolving this question depends principally on biological evidence, including functional anatomy and the growth and development of diagnostic skeletal features, and the geographic, environmental, and temporal distributions of hominid forms.

2. Whether there was a relatively recent replacement of populations in various parts of the Old World or whether each region exhibits local continuity of biological forms. Archaeological, chronological, paleoanthropological, and genetic data are all relevant here to show movements of and contacts between various populations.

3. The nature of the behavioral adaptation of Upper Pleistocene hominids. The most direct evidence for behavior is, of course, archaeological, though skeletal evidence may be important when the relationship between behavior and biology is well understood.

We concentrated on the last of these, reviewing the evidence that had been put forward by others for the

presence of symbolic behavior in the Middle Paleolithic. Lindly and Clark expand on our review, with essentially identical results, but because they confuse the three issues they draw very different conclusions.

Lindly and Clark are clearly addressing the issues of taxonomy and replacement/continuity. In their introduction they state: "We are concerned that Chase and Dibble's conclusions might be taken . . . as further 'proof' of . . . considerable evolutionary 'distance' between archaic *Homo sapiens* and morphologically modern humans." This is a question of taxonomy. They go on to say that "absence of evidence for symbolic behavior in archaic *H. sapiens* would support the contention that archaic *H. sapiens* (including the Neanderthals) was an evolutionary dead end and was replaced throughout its range by humans of 'modern' type with little or no genetic admixture." This, of course, relates to the continuity issue.

If we consider the archaeological evidence for symboling as a behavioral issue divorced from taxonomy, we see nothing in Lindly and Clark's paper that would cause us to alter our position—in fact, the evidence they present concurs with ours. Nonetheless, they criticize our work, citing Conkey's objection that symbolic behavior might be manifest in more subtle ways than the presence or absence of art, style, and ritual activity. As we said (Chase and Dibble 1987:284), "It could be that most of the symbolic behavior of Middle Paleolithic hominids left no archaeological traces simply because Middle Paleolithic culture did not express symbolism in any archaeologically preservable form. If this is the case, then we as archaeologists will be in error because of the very nature of our data base. But it is an error that must be risked in order to avoid assuming that which we are trying to demonstrate." To argue that symbolism (or any other trait) may have existed in forms other than those that are available to us is not a very strong argument that it *did* exist.

A second criticism of our paper, again attributed to Conkey, is that it is "wholly dependent upon a distinction between culture . . . and paleoculture." Here it is clear that Lindly, Clark, and Conkey have missed the point of our study. We simply reviewed phenomena proposed by others as evidence of symbolism in the Middle Paleolithic. It turned out that much of the "evidence" was shaky because of taphonomic conditions, dubious dating, or inadequate documentation. Moreover, other claims of symbolism usually required the unwarranted assumption of links with phenomena such as esthetics. When we used the word "paleoculture" we were simply acknowledging that the lack of evidence for symbolic behavior in the Middle Paleolithic was consistent with a difference in behavior between Upper Paleolithic and behavior from earlier periods (as was noted by Jelinek [1977], who coined the term) and that this difference did not appear to be due to lack of intelligence on the part of Middle Paleolithic hominids.

When Lindly and Clark review evidence for symbolism from Middle Paleolithic sites associated with anatomically modern human remains, they use essentially the same method we did. While we are encouraged that

their findings agree with ours, we disagree with the conclusions they draw from them. Their argument is structured as follows: (1) Biological change and speciation result from changes in adaptation. (This premise is unstated.) (2) Symboling has adaptive significance. (3) "In evolution, behavioral change always occurs well in advance of related morphological changes"; therefore the development of symboling in hominids should precede biological change and speciation. (4) "The results of [this] survey suggest that, as Chase and Dibble have reported for archaic *H. sapiens*, the daily activities of pre-Upper Paleolithic morphologically modern humans had no archaeologically discernible symbolic component." (5) The lack of evidence for symbolic behavior in both forms "implies that the taxonomic units themselves are unreliable," and therefore they should be considered the same species. (6) If they are the same species, they have the same adaptation, and therefore "the assertion that morphologically modern humans displaced archaic *H. sapiens* because they were more 'advanced' " is untenable. We do not, of course, dispute the first two premises or the fourth. We must, however, take issue with the rest.

The chronological primacy of behavioral over biological change (the third point) provides the logic for their review of early modern *H. sapiens* sites. However, this is valid only if it can be shown that the behavioral change in question (the adoption of symbolic behavior) is the one responsible for changes in hominid morphology. If a different behavioral change underlies the morphological differences between archaic and modern *H. sapiens*, then the timing of the first appearance of symbolic behavior in the archaeological record is irrelevant to the evaluation of these morphological differences. Generally speaking, the primary skeletal differences between archaic and modern *H. sapiens* relate to overall robustness and details of cranial morphology. We are not aware of any demonstration that symboling is related to these features. In fact, it seems that one of the points that Lindly and Clark want to make is that symboling cannot be linked to changes in biology—a conclusion that we would endorse. But if symbolic behavior and biology are not linked, then the presence or absence of symbolic behavior has no implications for the interpretation of taxonomic differences.

As we pointed out, "it is highly probable that Middle Paleolithic hominids had some capacity for symbolism" (1987:285). What we question is that the regular use of symbols was an integral part of their behavioral adaptation. Contrary to the characterization of his work by Lindly and Clark, Marshack (1988, 1989) has not demonstrated habitual use of symbols in the Middle Paleolithic. What he has done is to argue for the Neanderthal *capacity* for symboling—a capacity more clearly suggested by the evidence from Saint-Césaire and Arcy-sur-Cure than by his analysis of isolated finds.

Thus, that there is no solid archaeological evidence for symbolism before the Upper Paleolithic, even at sites associated with modern *H. sapiens*, has no apparent bearing on the question of the biological differences between archaic and modern hominids. It just means that the appearance of this particular behavior and the devel-

opment of this particular set of biological traits are neither causally nor temporally linked.

Nor does the absence of archaeological evidence for symboling from sites associated with either form of pre-Upper Paleolithic hominid “imply that the taxonomic units themselves are unreliable” (point 5). It makes no sense to argue that the shared lack of any particular behavioral trait (especially one not linked to biology) in two populations implies that the forms are taxonomically the same. Frogs and humans share the absence of the ability to fly, but that does not make us one species. Even within the hominid line, no one would argue that australopithecines and modern *H. sapiens* from the Middle Paleolithic belong to the same species just because there is no archaeological evidence that either regularly used symbols.

In point 6, Lindly and Clark confuse the issues of behavior and taxonomy with the issue of replacement. It is true that different species often have different adaptations, at least if they overlap temporally and geographically. It does not follow, however, that there are no differences in adaptation within a species, especially one as plastic and as dependent on learned behavior as the human. One need only look at the differences in adaptation between traditional Australian peoples and modern inhabitants of Melbourne. Nor does it follow that one population cannot displace or genetically swamp another population of the same species. There may have been a population movement into Europe, for example, whether or not modern *H. sapiens* is a species distinct from the Neanderthals. Such movements have virtually no taxonomic implications in the sense that replacement of one population by another cannot be taken as evidence of genetic distance. An obvious historical example is the colonization of the Caribbean, where the indigenous population was almost totally replaced by newcomers from first Europe and then Africa. In other words, an important implication of the “extrasomatic means of adaptation” characteristic of hominids is that significant changes in behavior need not be associated with significant changes in morphology.

There does appear to be good evidence for population replacement in Europe at the beginning of the Upper Paleolithic (Mellars 1989). Moreover, the coincidence of this replacement with the first good archaeological evidence of the habitual use of symbols is striking. Even if Conkey (as cited by Lindly and Clark) is right that symbolic behavior may have been too subtle for the archaeological record to monitor (an argument that is, *ipso facto*, difficult to support empirically), the common evidence of symboling in the archaeological record of even the early Upper Paleolithic of Europe implies a new and significantly different role for symbols in a new adaptation (see Gamble 1983, White 1985, and Whallon 1989 for ideas about what this may have involved). The temporal coincidence implies that it was this new adaptation that permitted the newcomers to replace or swamp the area’s former inhabitants. The association of both Neanderthals and archaeological evidence for symboling with the Chatelperronian, on the one hand, and the absence of links between symboling and both modern and

archaic *H. sapiens* elsewhere until considerably later, on the other, demonstrate that this new adaptation was purely behavioral in nature and neither the cause nor the result of biological change. Nor, as we have pointed out elsewhere (Chase and Dibble n.d.), does replacement in Europe necessarily argue for replacement elsewhere in the world.

In sum, we hope that Lindly and Clark’s findings will not be taken by anthropologists as “proof” that behavior did not change significantly during the early Upper Pleistocene or that there could not have been population movements during this time.

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This paper is an ingenious exercise in moving the chronological goalposts in order to accommodate the argument for regional continuity between archaic *Homo sapiens* and anatomically modern humans. While the data that Lindly and Clark present come from throughout the Old World, the focus is strongly European, since it is here that the cultural, symbolic, and anatomical evidence most strongly favours the replacement model to which they are opposed. Their main point is that the European early Upper Palaeolithic (ca. 40,000–20,000 B.P.) produces few data that can be interpreted as symbolic. Similarly, in sub-Saharan Africa modern skulls and mandibles predate the Late Stone Age/Upper Palaeolithic “revolution” at 40,000 B.P. and are not associated with any symbolic objects. This situation contrasts with the creative explosion after 20,000 B.P. in Europe and 12,000 B.P. in southern Africa (Deacon 1990). This extra 20,000 years, they argue, is ample time for symbolically informed behaviour to have developed, and as a result we do not have to posit rapid replacement to explain its appearance.

While welcoming Lindly and Clark’s negative review of symbolic data from the Old World, I can see some problems. Not least among these, given the chronology just mentioned, is their contention that behavioural change always precedes anatomical change if, as they claim, the symbolic explosion after 20,000 B.P. is somehow more significant because of the volume of symbolic objects recovered after that time. Symbolism, once available, is not something that is either turned off and on or varies in intensity. For example, the colonisation after 13,000 B.P. of the North European Plain and the entry of humans into North America are marked by very few if any symbolic artifacts. The world at the last glacial maximum (18,000 B.P.) has huge areas with no symbolic objects and a northern fringe with abundant art (Gamble and Soffer 1990, Wobst 1990). This does not mean that the populations settling the former areas for the first time or adapting to refuge conditions there had switched off their capacity for symbolism.

I am therefore surprised to see Lindly and Clark recognise, at a world scale, a “latitudinal component to the

archaeological evidence for symbolism" but then ignore this component *within* Europe at different stages of the last glacial cycle. Nor can their argument be applied to the objects from the early Upper Palaeolithic of Europe, especially now that the Aurignacian is getting older thanks to accelerator mass spectrometer dating (Mellars 1989). Consequently, it is unreasonable to dismiss such data as the southern German figurines, all dated to over 30,000 B.P., as unimportant because they are rare. As to the pieces from the Middle Palaeolithic championed by Marshack, I follow Chase and Dibble (1987) and Davidson and Noble (1989) in considering their symbolic status improbable.

Moving the goalposts to make the pitch longer puts off answering the main problem, which supporters of regional continuity are often reluctant to address—the very rapid colonisation by humans of the whole earth (Cavalli-Sforza et al. 1988:6005). This occurs not with the first modern skulls but in a pulse beginning 50,000–30,000 years ago with, for example, the colonisation of Australia and Melanesia (Allen, Gosden, and White 1989, Jones 1990) and continuing for 30,000 years. It seems likely that expansion on this scale required the "conscious production of meaning" (n. 3); Whallon (1989) has argued that it points to the development of efficient language and memory that increased the scale and intensity of interaction and resulted in humans' colonising all the world's habitable zones. If this provided a context for selection and by proxy a chronology for modern human origins (irrespective, by the way, of the shapes of their skulls and stone tools), then we need be less concerned with negative evidence for symbolic artifacts. We can also avoid the problem of supposing different rates of biocultural change and social complexity in different regions of the world—a notion that is reminiscent of Coon's (1962) conclusion, from which for obvious reasons most supporters of regional continuity are keen to distance themselves, that some regional populations (no guesses which) crossed the line to humanity later than others.

What Lindly and Clark have not addressed is the fact that the massive extension of range, while not always associated with the evidence for symbolic artifacts they discuss, does *not* take place before such objects have appeared somewhere in the world. Current time scales indicate that this process is more readily explained by dispersion, which involved replacement in some parts of the Old World. A more adequate test for Lindly and Clark's model would therefore be the convincing documentation of Middle and Early Upper Pleistocene human presence in the Americas, Sahul, or any of the major environments of the Old World that remained deserted until after 50,000 years ago.

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Lindly and Clark find no unequivocal archaeological evidence for "symbolic behavior" in association with mor-

phologically modern *Homo sapiens* in the Middle Paleolithic. They conclude that because neither Middle Paleolithic archaic *H. sapiens*, *H. sapiens neanderthalensis*, nor morphologically modern *H. sapiens* left evidence of ability to think symbolically, none can be ruled out as a potential ancestor of behaviorally modern humans. Exposing as specious the old equation of modern form with modern capacity does not, however, radically undermine the replacement hypothesis; it does force a reconsideration of the mode of replacement and of what constitutes good evidence of modern behavior.

Lindly and Clark present "evidence" for continuity across technological (i.e., Middle-to-Upper Paleolithic) and morphological (i.e., archaic-to-modern) "boundaries" that they contend should lay the replacement model to rest. Laid to rest, however, is any notion that the evidence they introduce can be used to refute the model of replacement of archaic *H. sapiens* by modern humans in Europe about 38,000 years ago. First, citing Marshack (e.g., 1988a), they assert that the human capacity for symbolic thought reaches back into the Middle Paleolithic; but Marshack's arguments are not universally accepted (see, for example, d'Errico 1989) and cannot in any case be taken as unequivocal evidence for the kind of cognitive abilities that characterize modern humans. They continue with the rather startling proposition that we should ignore the evidence of modern "symbolic behavior" from the early Upper Paleolithic and view the identical but more plentiful evidence from the late Upper Paleolithic as the "symbolic explosion" heralding the arrival of humans with modern abilities. One is left to infer that it is not the human ability to manifest "symbolic behavior" in sculpted antler and bone and painted and incised representations on stone of humans and animals but the ability to leave such artifacts around in quantity that makes modern humans modern. In producing an arbitrary quantitative distinction to demonstrate continuity, Lindly and Clark have masked a marked qualitative difference between the archaeological records of Europe before and after the appearance of modern humans.

Finally, they shift the discussion from evidence for "symbolic behavior" to "the character of faunal assemblages," on their account a much better "monitor of human adaptation" than traces of symbolic ability. They maintain that the changes in faunal composition visible across the technological and morphological boundary in Europe can be attributed to climatic changes and not to any adaptive differences between the two types of *H. sapiens*—that Neandertals and modern humans in Europe lived off the meat of just those animals available to them. They are right to point out that the faunal evidence may not be used to support the replacement model, but neither can it be used as a refutation. In order for the faunal evidence to serve as support for the continuity hypothesis, we would need to know, at a minimum, whether Neandertals acquired their food using cognitive abilities similar to those ascribed to behaviorally modern humans. Only then would it be possible to say with relative certainty that there is "evidence for clinal, relatively gradual change" in "human adapta-

tion" until about 20,000 years ago. In sum, the argument for a multiregional model rests on equivocal "evidence" for continuity and on the unsuccessful attempt to use that evidence to refute the replacement hypothesis.

What, then, is the status of the two competing hypotheses for the emergence of modern humans, given the early evidence (e.g., at Qafzeh about 90,000 years ago [Valladas et al. 1988]) for morphologically modern humans in the Middle Paleolithic? At Klasies River Mouth 40,000–50,000 years ago there is an arguably modern tool kit in association with morphologically modern humans (Singer and Wymer 1982, and see Mellars 1989), suggesting that some as yet unknown process of cognitive evolution had already taken place. This evidence comes to us from a time when Neandertals were still working stone with the technique that they had once shared with the morphologically modern form and that they had used without any real change for about 50,000 years. If we can take this as one line of evidence that what occurred in Africa did not happen in Europe (possibly because of geographical isolation), this should suggest that while a transition to modern behavior did not, after all, coincide with the emergence of modern skeletal morphology, *it did take place within the modern morphotype*.

The changes in question may have involved such uniquely human capacities as language and the (possibly) related ability to create archaeological traces of "symbolic behavior," as Davidson and Noble (1989) have persuasively argued. This could have profoundly affected two otherwise similar populations of protohumans by rendering them too different psychologically for mating to occur. Thus, while Neandertals and modern humans may have been potentially interfertile owing to their close phylogenetic relationship, they may have been behaviorally isolated—a potent mechanism of speciation that has not received much consideration in this debate. That modern humans resemble morphologically modern populations of archaic *H. sapiens* more than they do Neandertals or other archaic forms may be viewed, quite plausibly, as the result of reproductive isolation and not of the willy-nilly gene exchange implied by the multiregional-origins scenario.

Lindly and Clark have given us a valuable summary of the archaeological record of Middle Paleolithic morphologically modern *H. sapiens*. The caution they display in making inferences of "symbolic behavior" from archaeological sediments is a position with which I am much in sympathy. Their arguments for continuity ultimately fail, however, to persuade me that the multiregional-origins hypothesis in any way accurately depicts recent human phylogeny.

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Lindly and Clark are to be congratulated for a thorough review of many less well-known Eurasian Upper Pleistocene hominid-bearing sites. They argue, with good

cause, for the decoupling of the marked cultural discontinuity across the Middle/Upper Paleolithic transition from the putative biological discontinuity between "archaic *Homo sapiens*" and "morphologically modern humans." This suggested decoupling has been far too long in coming, and to have it so well presented and documented is helpful. However, several comments are called for:

The classic dichotomies of Middle versus Upper Paleolithic and archaic *Homo sapiens* (a euphemism for Neandertal or Neandertal-like) versus morphologically modern humans have hampered our efforts to understand the evolutionary processes involved in the "transition" between arbitrarily defined, and then reified, categories. We learn these categories, accept them, are beholden to them, and allow them to shape the trajectory of our research and discourse. Yet the underlying evolutionary dynamics are rarely addressed. For example, given the emphasis here on purposeful burial, it would have been interesting to see a discussion of why such burials, common in the "Middle Paleolithic," are virtually unknown in the "early Upper Paleolithic" yet become the rule in the "late Upper Paleolithic." The absence of sites in which "Upper Paleolithic" burials are found in sediments overlying sediments with "Middle Paleolithic" burials, which suggests different strategies and norms regarding site usage, would also seem pertinent to the themes discussed here.

Further, Lindly and Clark only briefly discuss the fact that major changes in symbolic behavior appear only at the beginning of the "late Upper Paleolithic," a time roughly corresponding to the last glacial maximum. This coincides nicely with the results of Soffer (1987), who has underlined the archaeological correlates of adaptive (including symbolic) strategies in an increasingly stressful environment. Similarly, I have shown (Jacobs 1985) that major human postcranial changes in sexual dimorphism and robusticity accelerate with the last glacial maximum. In the absence of an explicit model, one gets the impression that Lindly and Clark are suggesting that changes in the nature or intensity of symbolic behavior have little or no impact on human population biology. While their rejection of the classic Upper Pleistocene biocultural coupling is welcome, further and more informative investigation of the links between symbolic behavior and population biology is needed.

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Whilst I have a good deal of sympathy with many of Lindly and Clark's comments, I believe that their article reflects a number of persistent and recurrent confusions in discussions of the biological and cultural transition from archaic to modern humans. First, no one would disagree with them on the absence of a simple, one-to-one correlation between anatomically modern humans and characteristically Upper Palaeolithic culture (with

its rich symbolic manifestations) throughout the world. The question is whether this is at all relevant to the issue of population continuity versus population replacement over the archaic/modern-human transition. As Lindly and Clark point out, there is absolutely no reason to assume that biological and cultural changes must have gone strictly hand-in-hand. A far more likely scenario is a pattern of "mosaic" evolution, in which behavioural changes in some cases preceded major biological changes and in other cases followed them. If this was the case, then there is no reason whatever to assume that either the initial emergence of anatomically modern populations in Africa or their subsequent postulated dispersal into more northern latitudes should have been connected in any simple or direct way with dramatic changes in the associated archaeological record. The lack of such correlations in no way "refutes" the hypotheses of either an initial emergence of anatomically modern humans in one particular area or their subsequent dispersal to other regions of the Old World.

Leaving aside these theoretical issues, Lindly and Clark skate lightly over a vast amount of evidence that over a large region of the Old World (i.e., Central and Western Europe—where the major debate has always centred) there were in fact fundamental changes in human behaviour that can be shown to correlate remarkably closely with an equally abrupt transition from anatomically archaic to anatomically modern forms. The whole character of "Aurignacian" culture (with which the earliest forms of fully anatomically modern humans seem invariably to be associated in this region [Howell 1984]) shows a dramatic contrast with earlier "Middle Palaeolithic" culture not only in the character of the lithic industries but in such features as complex personal ornaments, elaborately shaped bone, antler, and ivory artifacts, far-travelled marine shells, increased use of other "exotic" materials, and the earliest well-documented (and remarkably complex) art. The relatively sudden and abrupt appearance of these features over such a large area within such a short space of time (Mellars 1989:372–75; White 1989a; Bischoff et al. 1989; Cabrera Valdes and Bischoff 1989; Kozłowski n.d.) is far more consistent with the hypothesis of a major episode of population dispersal than with that of a gradual, in situ evolution of the local (and highly varied) Middle Palaeolithic industries within the same regions. Lindly and Clark also fail to mention the very late (and very typical) Neanderthal hominid from Saint-Césaire (western France), which is almost certainly contemporaneous with the earliest forms of anatomically modern humans in Western Europe and demonstrably much *later* than the appearance of these forms at sites in the Middle East. Nor do they mention the demonstrable 30,000–40,000 years' overlap between "modern" and "archaic" forms that has now been documented within the Middle Eastern sites (Valladas et al. 1988, Schwarcz et al. 1988).

A more general weakness in many recent discussions of the origins of modern humans is an apparent implicit equation between "advanced lithic technology" and "advanced culture." Even if many of the lithic industries

associated with the earliest forms of anatomically modern hominids (e.g., in southern Africa and the Middle East) were relatively "simple" in technological terms, this may give little indication of other—potentially much more significant—aspects of culture, such as social organization, subsistence strategies, or language. Here again, Lindly and Clark downplay the available evidence. The fact remains that the boar's jaw and large deer antler associated with early (ca. 90,000–100,000 B.P.) anatomically modern hominids at Skhül and Qafzeh, respectively, are by far the most convincing examples of deliberate grave offerings yet recorded from pre-Upper Palaeolithic contexts in Eurasia. Similarly, the perforated *Conus* shell from Border Cave and the regularly notched bones from Klasies River Mouth, Apollo 11, and other African Middle Stone Age sites provide much more convincing evidence for early "symbolic" artifacts than anything so far recorded from the Middle Palaeolithic/Neanderthal sites of Europe. And the character of the African Howieson's Poort industry is not simply "aberrant Middle Stone Age" but fully "Upper Palaeolithic" in almost every recognized technological and typological sense (Mellars 1988, 1989). It may well be, therefore, that the total "culture" associated with these early forms of anatomically modern humans in southern Africa was significantly more complex and advanced than anything so far documented from the contemporaneous Middle Palaeolithic/Neanderthal sites of Eurasia.

Finally, I have never really understood the argument that the significance of the symbolic and technological "explosion" at the start of the Upper Palaeolithic is in some way diminished by the evidence of further increases in "cultural complexity" during the later stages of the Upper Palaeolithic sequence. I would see this as a natural and predictable outcome of progressive increases in population densities and other demographic and social pressures in some of the more ecologically favoured areas, such as the Franco-Cantabrian region or the South Russian Plain (Mellars 1985, Soffer 1985b). To argue that this evidence for later Upper Palaeolithic cultural "intensification" rules out the significance of the far more radical innovations in behaviour at the *start* of the Upper Palaeolithic would seem akin to dismissing the significance of the "Neolithic Revolution" on the grounds that things became even more complicated during the Bronze Age.

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I consider two major aspects of Lindly and Clark's argument problematical. The first is their use of the concept of behavioral adaptation rather than culture. In my view, their definition of adaptation (n. 2) cannot encompass social and cultural change. Culture is not like any other "structure, physiological process, or behavioral pattern"

that contributes to the reproductive "fitness" of a species, and its development cannot be monitored in the same manner. For modern humans, the habitual use of symbols through language defines the environment within a social and historical context. Changes in human adaptations involve not only the environmental stimuli but the group's response to them, grounded in its unique historical circumstances (see Bettinger 1980, Conkey 1987b).

Lindly and Clark's concept of behavioral adaptation directs the argument that negative evidence for symbolic behavior supports "a hypothesis of no difference" between archaic *Homo sapiens* and anatomically modern humans. I completely agree that "no correlation of modern behavior with modern morphology can be proposed." The former deals with social and cultural adaptations (including symbolic behavior), or what Conkey (1987b:65) has termed "human-human relationships," and the latter concerns biological adaptations. I disagree, however, with the assumption that the mechanisms that shape these two dimensions of human change and variability are the same.

Lindly and Clark also contend that the negative evidence for symbolic behavior on the part of the earliest moderns from the African continent challenges the replacement scenario. I find this troubling on two counts. First, the "out-of-Africa" model has never relied upon the premise that anatomically modern humans arrived in Europe fully equipped with symbolic behavior. Second, Lindly and Clark explicitly question (n. 3) the appropriateness of the categories that have been employed to monitor symbolic behavior, calling them "equivocal and inadequate," but they propose test implications for the replacement model based on these same classes of evidence.

The second problem in Lindly and Clark's argument lies in the assumption that the similarities and differences between Middle Paleolithic and early Upper Paleolithic subsistence adaptations have been adequately assessed. While the European archaeofaunal evidence may initially suggest few marked changes "coincident with the local Middle/Upper Paleolithic boundaries," when we are able to go beyond the relative frequencies of prey species we begin to see differences suggestive of change through time. For example, studies such as that of Delpech and Rigaud (1974) on the systematic processing of bone and marrow provide insight into early Upper Paleolithic innovation that would have been otherwise overlooked. Interpretive frameworks recently developed for understanding the age profiles of Middle and Upper Paleolithic ungulate prey (esp. Stiner 1989a, b) allow us to consider planned, corporate involvement in prey acquisition in the Middle Paleolithic and early Upper Paleolithic (Pike-Tay 1990). For example, comparative study of strategies involved in seasonal red-deer hunting in the Gravettian and Final Magdalenian of southwestern France (Pike-Tay 1989) suggests that the success of the early Upper Paleolithic group of hunters may be attributed to cooperation rather than technology.

Testing for *seasonal* use of prey species in well-

controlled archaeological contexts can now aid in monitoring changes in subsistence systems that might appear identical if only relative frequencies of species, anatomical parts represented, and age profiles were considered. In addition, indicators other than faunal assemblages, such as paleonutrition, must be considered in examining change in subsistence adaptations in the Middle and Upper Paleolithic. For example, Brennan's (1986) work with biological stress indicators (i.e., enamel hypoplasias and Harris lines) has demonstrated statistically significant differences between the Middle Paleolithic and the early Upper Paleolithic in southern France. Pending further analysis of this kind, it is perhaps premature to argue that the subsistence strategies of the Middle Paleolithic and the early Upper Paleolithic were identical or different.

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I cannot but agree with Lindly and Clark's major argument for a slow accumulation of traces of "symbolic" behavior and a "symbolic explosion" only at ca. 20,000 years B.P. Yet I think we should also bear in mind both the irregularity of historical development and the wide variety of its particular forms that are suggested by the irregular distribution of archaeological traces of such activities over time and space. Further, the time and space distribution of particular types of "symbolic" activities throughout the world from ancient to modern times testifies to the existence of certain zones of aggregation (centers) that contain the majority of sites producing material evidence of this or that kind of "symbolism." Thus, there are centers of primitive art (Formozov 1983) and centers of taphological activity (Smirnov 1989). In all probability, we can also speak of centers of zoolatry cults, e.g., bear caves representing a bear cult. The existence of such centers does not, however, exclude the possibility that similar "symbolic" activities went on in other places, although those activities evidently took other forms undetectable by archaeological means (e.g., drawings on perishable materials, exposure rather than burial of dead bodies or the objects of zoolatry cults, etc.). There is good reason to believe that there were in fact no human societies that did not go in for some sort of "symbolism." Both the structural complexity of the human brain (Kochetkova 1973) and the evolution of the particular parts of the brain responsible for thinking and speech suggest the existence of various kinds of "symbolic" activity as far back as *Homo erectus*.

A few words about some archaeological evidence that Lindly and Clark have regrettably not taken into account: From Qafzeh we have evidence not simply of the burial of the dead but of more sophisticated forms of preburial treatment including ritual cannibalism (e.g., pathology of the occipital part of the Qafzeh 6 skull). Both the condition and the distribution of the skeletal

remains in burials testify to the practice of mortuary decapitation (Qafzeh 6), defleshing (Skhül 1?), and reburial (Skhül 2?). They also indicate that there were two types of burial, of the whole body and of only parts of it (Skhül 2, 6?, Qafzeh 6, 10, 15), and there are grounds for belief that the Mousterians ritually substituted isolated teeth for the cranium or the mandible (Qafzeh 3) (Smirnov 1989:223). Finally, Lindly and Clark have omitted mention of two pieces of ochre found in association with the Qafzeh 8 burial (Vandermeersch 1969:2563). I think that the interred(?) bull's skull that partially interfered with the Skhül 9 burial can be considered further evidence of "symbolic" activity.¹

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This is a useful article and one with whose perspective and conclusions I am in fundamental agreement. I have argued for a number of years that, at least in the specific case of Cantabrian Spain, cultural evolution in the second half of the Upper Pleistocene was overall gradual, cumulative, and mosaic in nature (e.g., Straus 1977, 1983, n.d.a; Straus and Heller 1988). This is not to deny the apparently rather abrupt effect of human abandonment of northwestern Europe and the southward recession of the hominid range during the last glacial maximum on population densities and hence on subsistence strategies, social organization, and symbolic/ceremonial activity in the Franco-Cantabrian region after about 20,000 years ago (Straus n.d.b, c), but such cases of relatively rapid change need to be identified and analyzed individually and in local context.

I continue to be surprised that archeologists and human paleontologists can argue so intensely about notions such as "the Middle-to-Upper Paleolithic transition" as if they were real and the true object (as opposed to prehistoric human adaptations) of paleoanthropological research. We have not yet escaped the unilineal evolutionism of our scientific ancestors. With each new reassignment of key fossils (e.g., Skhül, Qafzeh) or industries (e.g., Chatelperronian, Szeletian, Bohunician), the supposed "transition" is moved in time and redefined. We tend to jump on bandwagons, the current one being the punctuated-equilibrium paradigm. However, depending on one's time frame (long or short), the same phenomena may appear to be the results of either gradualistic or punctuated change. We continue to make the basic mistake of assuming that new forms of fossil hominids must be strictly correlated with new behaviors (and vice versa) in all or at least most domains of human activity (this despite the Skhül, Qafzeh, and Saint-Césaire discoveries). Lindly and Clark clearly show the error of this assumption in the supposedly critical realm of symbolic behavior (however that may be defined).

A few points may be added to their exposé:

1. Many/most Upper Paleolithic human remains (even in the Late Upper Paleolithic, even in France) lack clear grave goods; indeed, unequivocal burials are still rare, particularly in such regions as Vasco-Cantabria (Quechón 1976, Harrold 1980). Grave goods are variably present even at the same site even in the Late Upper Paleolithic; for example, in the Upper Magdalenian of Duruthy (southwestern France) the (unsexed) individual found in 1874 by Lartet and Chaplain-Duparc was associated with some 40 perforated and engraved lion and bear teeth, while the (female) individual found in 1961 by Arambourou (1978:28-29) lacked "offerings."

2. Most rupestral and even mobile art in Vasco-Cantabria can be argued to be of Late Upper Paleolithic (Solutrean and Magdalenian) age; its appearance in this region may be linked to the specific density-dependent conditions of human settlement alluded to above (Straus 1982, 1987b).

3. There are of course some well-dated, early, unequivocal works of art (e.g., at Geissenklösterle in southwestern Germany, ca. 32,000 years b.p. [Hahn 1988], and at Apollo 11 in Namibia, ca. 27,500 years b.p. [Wendt 1976]), but their distribution is geographically spotty: some regions have "much" early art, others little or none. Not all of this variation can necessarily be explained by differential preservation.

It is futile to debate whether "the transition" or even one aspect of the transition (i.e., "symbolic behavior") took place universally at one time or another. These phenomena are regionally variable in timing and mosaic in nature. Although hominid distributions and adaptations worldwide did end up looking very different by the end of the Upper Pleistocene than they had at its beginning, this was the cumulative result of long-term, non-teleological, adaptive changes. Symbolism undoubtedly does have adaptive value—in the context of certain physical, demographic, and social environments such as those of the resource-rich but relatively cold and crowded Franco-Cantabrian region.

It is ironic that, while the authors (correctly) imply that specific attribution of individual fossils to archaic or modern *Homo sapiens sapiens* is often tenuous, they seem to accept several very fragmentary or juvenile remains as definitely "modern." In addition, some of the supposed associations of "modern" fossils with Middle Paleolithic artifact assemblages are questionable (e.g., Border Cave, Starosel'e Cave). Finally, the relevant deposits at Skhül are now "dated" by electron spin resonance to ca. 90,000 B.P. (Stringer et al. 1989), but, being based on the same theoretical assumptions as thermoluminescence dating, this need not be taken as an independent check on the dates from Qafzeh (Valladas et al. 1988).

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The realisation that the conventional archaeological division between Middle and Upper Palaeolithic recog-

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nised by most archaeologists does not neatly correspond with the distinction (whether subspecific or specific) between anatomically non-modern and modern skeletal morphologies recognised by virtually all palaeo-anthropologists should have been with us for a long time now. In this paper, Lindly and Clark use "symbolism" as their main criterion for recognising "modern" behaviour and conclude that a lack of symbolism prior to the Upper Palaeolithic therefore indicates a lack of "modern" behaviour, thus (in their opinion) supporting a multiregional model of modern human origins. However, they do recognize a number of limitations in their archaeological arguments, and I will leave those to be dealt with by other commentators.

Before going on to deal with their arguments concerning the origin of modern humans, I would like to clarify a few points concerning the sites they discuss in their useful review. First, concerning the supposed "grave goods" with Skhül 5 and Qafzeh 11: it is true that the species represented were common in the layers concerned outside the purported grave area, but it is also worth noting that the remains in question were well preserved, suggesting that they, like the skeletons, may have been protected by intentional burial. Regarding Skhül, there is further evidence to link the sample with the Qafzeh hominids from electron-spin-resonance age estimates (Stringer et al. 1989). The accuracy of the claim that the Qafzeh hominids represent "the earliest dated remains of morphologically modern humans in the world" depends on interpretations of a number of African hominid sites. Omo Kibish 1 (Day and Stringer n.d., Day, Twist, and Ward n.d.), KNM-ER 3884 (Bräuer, Leakey, and Mbua n.d.), and the Klasies MSA 1 sample (Grün, Shackleton, and Deacon n.d.) may all be of comparable or greater age, and this would also apply to Laetoli hominid 18 and the Singa calvaria (Grün and Stringer n.d.) if they are considered to be anatomically modern (for further evidence of an archaic morphology in Singa, see Stringer, Cornish, and Stuart-Macadam 1985). The dating of the Aterian hominids of North Africa also remains unclear, with some workers arguing for much greater ages (>70,000 years) for Aterian assemblages (Wendorf et al. n.d.). Finally, regarding Florisbad, it is likely that the hominid considerably predates the Middle Stone Age levels above Peat 2 (Clarke 1985).

The implication of Lindly and Clark's arguments is that if the archaeological evidence (which they claim supports a multiregional model) does fit with the palaeontological evidence, it must be the latter, specifically what they term "replacement systematics," that is at fault. Here they are not just taking on advocates of replacement models but attacking the view accepted by most workers that there are significant morphological differences (whether specific or subspecific) between anatomically non-modern and modern humans. As they recognise, acceptance of the latter view completely undermines their arguments, for given their assertion that "behavioral change always occurs well in advance of related morphological change," such behavioural changes should be observed in the Middle Palaeolithic of Europe and Western Asia if the Neanderthal populations

there were to transform themselves into anatomically modern ones. This brings me to a very important point stressed on numerous occasions by Trinkaus (e.g., 1986, n.d.; see also Stringer n.d.), who can hardly be characterised as favouring overall replacement: if we *do* accept the reality of the appearance of a new, more gracile skeletal pattern with anatomically modern humans, and we also accept that such a pattern is a reflection of selection and adaptation for a habitual life-style, then there *must* have been significant behavioural differences between the Skhül-Qafzeh hominids and the Neanderthals, whatever the lithic remains are supposedly saying. Otherwise, why were the Neanderthals (and other archaic hominids throughout the Pleistocene) carrying around all that physiologically and nutritionally demanding muscle and bone?

Presumably, if the more comprehensive genetic analyses now being conducted (e.g., Vigilant et al. n.d., Long et al. n.d., Nei and Livshits n.d.) also do not fit with Lindly and Clark's multiregional interpretations, these too must all be at fault. Although I agree that "a satisfactory explanation of the origins of modern humans must reconcile the archaeological and fossil evidence and the evidence from molecular biology," I think we are some way away from such a reconciliation, and I consider it unlikely to come from the view that nearly everyone else has got it wrong (see also Clark 1988; Clark and Lindly 1989). It *might* come, however, from a recognition that at the moment we are missing things that we need to achieve a resolution. Each area of research needs to recognise its own limitations, both in data and in interpretation.

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The ongoing debate on the nature, the timing, and especially the constituent processes of what we call "the origins of modern humans" appears to be becoming increasingly polarized just at a time when new data, analyses, and insights should be taking us away from the narrow, polemical, and nonproductive arguments formerly justifiable by major gaps in our paleoanthropological knowledge and understanding. This paper by Lindly and Clark appears to be another contribution to this polarization rather than to our understanding of what might have happened in the past.

The substance of their text requires little comment. It is difficult to make a convincing argument one way or the other concerning the "evidence" for symbolic behavior among Middle Paleolithic-associated early modern humans based on a mix of remains from some old excavations (Starosel'e, Skhül, Témara, Porc Epic, Singa), some recent carefully done excavations (Qafzeh, Klasies River Mouth, Mumba), mixed deposits (Darra-i-Kur), and surface finds (Laetoli-Ngaloba, Omo-Kibish), not all of which are sufficiently complete to be morphologically diagnostic as to their affinities with late archaic versus early modern humans (Darra-i-Kur, Témara, Porc Epic,

Mumba) and some of which are better considered as late archaic humans (Laetoli-Ngaloba, Singa). Their argument is based on negative evidence when there is little possible confidence in the representativeness of the samples assessed.

More important, they seem to be particularly concerned to eliminate any hint of biological determinism, as in their statement "it will be clear that no correlation of modern behavior with modern morphology can be proposed." Yet they are perfectly content to use supposed evidence of behavioral continuity, including typotechnological data, archaeofaunal analyses, and settlement-pattern inferences (only the first of which have anything resembling a secure basis in current analyses of the archaeological record, and that only for select regions of the northwestern Old World), to argue that there must have been human biological (i.e., genetic) continuity. Isn't there a problem here? If we cannot use past human morphology to say anything about past human behavior (and I believe that in fact we can), then we should be more careful about using highly debatable archaeological inferences concerning "cultural" continuity to make inferences about human phylogeny.

Perhaps more concern with sorting out the actual behavioral processes during the general period related to the origins of modern humans, combining human paleontological and Paleolithic archaeological data and inferences, is in order. It is becoming increasingly apparent that it was a considerably more complex period of human evolution than many of us thought even a few years ago, and perhaps our efforts would be better spent focusing on the past rather than contributing to anthropologically generated controversies that tell us little about it.

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I wish to restrict my comments to two subjects: (1) the out-of-Africa hypothesis as seen from the perspective of the European Paleolithic and (2) Early Upper Paleolithic evidence for symbol use. With respect to the first issue, I am in substantial agreement with Lindly and Clark. As for the second, I cannot imagine how we could be farther apart, either theoretically and factually.

It remains uncertain whether the out-of-Africa model of later hominid evolution will stand the test of time, although there is much reason to give it serious consideration. However, in many respects the Aurignacian is irrelevant to the question, since dates for the Near Eastern Aurignacian are substantially later than the earliest Aurignacian dates for Europe. Thus, the early Aurignacian must be viewed as a completely European phenomenon, with some later spill-over into the Levant. If modern humans came to Europe as the result of an out-of-Africa radiation, it was not with already developed Aurignacian culture in hand. Archaeological evidence for an out-of-Africa radiation, if it exists, must be sought

in earlier European/Near Eastern similarities such as those between the little-known East European Bohunician (Svoboda 1990) and industries from Boker Tachtit in Israel (Klein 1990; Marks 1983, 1990). The success of Upper Paleolithic culture in replacing the Mousterian is understandable, but we remain without any archaeological, biological, or behavioral explanation for a preceding movement out of Africa. The selective advantage of being an anatomically modern human is simply not self-evident.

Whether the out-of-Africa model proves to be a myth or a reality, I agree with Lindly and Clark that there is little if any pre-40,000-year-old symbolic evidence anywhere. But they imply that, if the out-of-Africa model were correct, there would be such evidence earlier in Africa. This is not a valid test implication unless one believes that symbolic behavior was necessarily a neurological/genetic correlate of the emergence of anatomically modern humans. The cultural developments of the Aurignacian, including the first known representational art and personal adornment, took place at least 50,000 years after the first anatomically modern humans appeared in Africa. Therefore, as I have previously emphasized (White 1982, 1985, 1989b), the developments across the Middle/Upper Paleolithic transition are not susceptible to neurological/biological explanations but may be understood solely in cultural evolutionary terms.

In a peculiar twist, Lindly and Clark go so far as to question the taxonomic distinction between morphologically modern humans and archaic modern humans on grounds that neither demonstrates symbolic behavior. In their sociobiological view of things, they have lost sight of the social and ideational dimensions of cultural evolution. Leslie White, who had no trouble reconciling a symbolic with an adaptational definition of culture, would have been surprised by the view that "the character of faunal assemblages is a much more direct monitor of human adaptation than art, ornamentation, or mortuary practices." In fact, the nature of faunal assemblages is directly linked to culture (no matter how defined) through ideas, beliefs, technology, and social organization. As I have recently emphasized (White 1989b:99), the first symbolic representation was at least as significant adaptively and evolutionarily as the first use of fire or stone tools. The consequences for innovation and change were profound. The Aurignacians and their Upper Paleolithic descendants were able to realize with increasing rapidity a wide range of social, technological, and ideational possibilities. In my view, much of this rapid evolutionary development, as is the case today, was due to the forming, manipulating, and sharing of images.

But Lindly and Clark dispute the very existence of *abundant* symbolic evidence in the Early Upper Paleolithic. Here, our disagreements are not ones of perspective but of fact. For the past four years, I have been struggling to understand the rich body of Aurignacian and Gravettian symbolic evidence, especially body ornaments, from Western, Central, and Eastern Europe. The

quantity of material is staggering, and most of it carries adequate stratigraphic provenience, in some cases with radiocarbon dates. Lindly and Clark mistakenly state that for the early Upper Paleolithic symbolic artifacts are dated "only on the basis of allegedly time-sensitive 'index-fossil' tool types and normative characterizations of assemblage sequences." In my view, it is undeniable that the Aurignacian is a culture-stratigraphic entity with relatively well-defined chronological limits established by radiocarbon dates (see Mellars et al. 1987). Peyrony's classic Aurignacian sequence has been much amended, but his Aurignacian I, characterized by split-based points, maintains its stratigraphic and chronological validity, with no radiocarbon dates placing it later than 30,000 b.p. and several placing it as early as 33,000–40,000 b.p. (see Cabrera Valdes and Bischoff 1989).

Some of the best-dated Aurignacian I levels are precisely those that have yielded the earliest known representational art, for example, the three-dimensional ivory animal figures from Geissenklösterle, dated to well before 30,000 b.p. (Hahn 1986, 1988), and a red-deer canine replicated in steatite from Castillo with accelerator dates (Straus 1989) of 37,700–39,900 b.p. Most Aurignacian I assemblages in which organic materials have been preserved have yielded personal ornaments and/or decorated objects, not to mention items of bone and antler technology. However, as in the Magdalenian, in which 90% of all the mobiliary art in Europe comes from a dozen or so sites, there are great intersite differences in quantity.

From the beginning to the end of the Aurignacian (at about 28,000 b.p.) in Europe, there are approximately 2,500 personal ornaments (see White 1989a, Lejeune 1987). Indeed, the number of basal Aurignacian beads, identical to those recovered from older excavations at several other sites, is growing rapidly with the meticulous recovery of dozens of these objects from Henri Delporte's ongoing excavations at Brassempouy. Aurignacian sites have yielded about 70 decorated, engraved, or painted slabs (Delluc and Delluc 1978), about 30 three-dimensional ivory carvings (Hahn 1971, 1972, 1975, 1983, 1986, 1988; White 1989b), at least one bone flute (Passemar 1944: pl. 7), and several thousand bone, antler, and ivory tools/projectiles (cf. Léroy-Prost 1975, Knecht 1990). Bone/antler/ivory implements made by complex grinding and polishing techniques (see White 1990) are very abundant. For example, there were 70 split-based antler points in the Aurignacian I at Isturitz, 54 at Abri Castanet, and 114 at Abri Blanchard (H. Knecht, personal communication).

The number of such objects increases steadily in the succeeding Gravettian, with perhaps fewer bone and antler tools/weapons and hundreds more engravings and paintings. It is worth noting that there are more personal ornaments (ca. 12,000) from the 28,000-year-old (Hofecker 1987, Bader 1978) Gravettian site of Sungir than exist in all French Magdalenian sites combined.

These observations clearly indicate that Lindly and Clark are unjustified in wishing to move the "symbolic explosion" forward to 20,000–15,000 b.p. While I have

no doubt that there is an increase in representational art in the Magdalenian/Epigravettian, this change is quantitative. The appearance, in the Aurignacian, of substantial numbers of representational objects (after 2.5 million years in which they apparently did not exist) is a qualitative and revolutionary development with general evolutionary consequences (see White 1989a) at least as profound as those of such landmarks in cultural evolution as the emergence of food production.

Pre-Upper Paleolithic hominids, whether in Africa or Eurasia, collected interesting forms and colors but seldom if ever created such forms. One possible exception is the Bacho Kiro festoon (Marshack 1982). I share fully Chase and Dibble's (1987) skepticism and feel that Marshack (1988) has overestimated the symbolic qualities of the very few well-provenienced "curiosities" while ignoring the severe provenience problems of others out of a firmly held belief in a gradualist versus punctuated view of symbolic evolution. In fact, however, these pre-Upper Paleolithic specimens contradict a gradualist perspective in lacking technical and formal redundancy through time and across space, attributes immediately visible from the outset of the European Upper Paleolithic.

Reply

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We thank those who have taken the trouble to address some of the implications of our paper for the issue of modern human origins. It would appear that the modern-human-origins debate is not for the faint of heart. Yet, although we disagree wholeheartedly with many comments, we think that a resolution of these problems will only come from such frank exchanges. It is unreasonable (*contra* Trinkaus) to expect everyone to agree about which data are "relevant" and what data "mean." Since data have no "meaning" (even existence) independent of the conceptual frameworks (or paradigms) that define and contextualize them, many of the differences of opinion expressed here are due to different conceptions of biological and cultural evolutionary models (or parts thereof).

Our objectives in this essay were considerably less ambitious than many commentators perceive them to be. If, for the sake of argument, one accepts the reality of the taxonomic infrastructure (i.e., the reality of archaic *Homo sapiens*, Neanderthal, and morphologically modern taxonomic units), one of the primary issues becomes the relative contributions of these different (or not so different) hominids to "the origins of us." Chase and Dibble (1987) argue for a significantly different adaptation in the Middle Paleolithic than that seen in the Upper Paleolithic of Eurasia, basing their case on an *absence* of evidence for symbolism in the Middle

Paleolithic and its *presence* in the Upper Paleolithic. Their construal of pattern in the Middle Paleolithic is used to juxtapose this "paleocultural" system to a cultural system of "modern" form (i.e., the Upper Paleolithic) for the purpose of demonstrating the "nature" of the Middle Paleolithic adaptation. It seems clear, however, that this approach can only take us so far in the examination of this complex series of interrelated issues and problems.

What we tried to do was to turn the Chase and Dibble argument around and look instead for evidence of symbolic behavior in Middle Stone Age/Middle Paleolithic sites associated with the skeletal remains of pre-Upper Paleolithic morphologically modern humans. If there were significant differences in symbolic behavior between archaic *H. sapiens* and morphologically modern human populations of pre-Upper Paleolithic/pre-Late Stone Age date, one would expect them to be manifest in the comparisons made here. So far as we can tell, however, the pattern is *exactly the same* whether the hominids associated with Middle Paleolithic/Middle Stone Age archaeological assemblages are archaic *H. sapiens*, Neanderthals, or morphologically modern humans. This implies (1) that the taxonomic units themselves are suspect (which we think very likely) and (2) that the major quantitative shift in adaptation occurred relatively late and was largely unrelated to the perceived transition from the Middle to the Upper Paleolithic. The evidence for and against symbolism is only a small part of this complex equation, albeit one that, because of its ambiguity (conflicting conceptual and operational definitions), seems to have evoked strong feelings.

We simply do not believe that *any* version of the replacement scenario, *anywhere in the world*, can reconcile obvious inconsistencies in the biological and cultural records of Upper Pleistocene hominids. If one subscribes to the "out-of-Africa" hypothesis (Cann, Stoneking, and Wilson 1987, Stoneking and Cann 1989), the implication is that morphologically modern humans replace archaic *H. sapiens* (or Neanderthals) throughout their range with no admixture and therefore the two *must be different species*. If these two different species were living in the same region at the same time, given what we know about evolutionary ecology, *they must have had fundamentally different adaptations*. Our previous work suggests that in southwestern Asia, and probably in Europe, they did not (Clark and Lindly 1988; 1989a, b). An absence of symbolic evidence associated with both hominid "types" provides further, albeit circumstantial, evidence for a similar adaptation. We cannot comprehend how Mellars can argue that an "out-of-Africa" perspective, wherein anatomically modern populations migrate throughout the rest of the Old World, would not also be accompanied by discernible changes in the archaeological record. If there are no actual or conceivable empirical referents for such an event or process, then it can be no more than speculation. According to the competitive-exclusion principle (Mayr 1950), populations coming together in any region from different environments must show adaptive differences,

especially if they were different but closely related species. The archaeological record would, therefore, reflect discontinuity wherever this replacement "event" or "process" occurred. Mellars (along with some other commentators) appears to regard the concept of migration as a plausible mechanism to explain his construal of pattern. We do not. With Trinkaus (1982) and Jelinek (1982), we consider migration to be a density-dependent phenomenon essentially confined to the latest protohistoric and historic periods (i.e., those periods when human population densities were locally high in some areas). We simply do not believe that the physical migration of peoples played a significant role in human macroevolution and are hard-pressed to come up with a single instance in which a more compelling alternative explanation is not possible (including the Aurignacian in Europe [see Straus 1989]).

It seems clear that many view symbolism as something modern humans "do" as a matter of course. Despite assertions by Dibble and Chase, Mellars, Stringer, and White that no one believes this anymore, we see plenty of evidence to the contrary (e.g., Gargett 1989, Smirnov 1989). We think that our survey indicates that symbolic behavior is not "species-specific" but situational in human adaptation. In other words, it solves an adaptive problem for humans that is probably related to information-processing requirements created by increased social complexity, population density, and/or subsistence uncertainty (Mithen 1988a, b). We would suggest to Gamble that symbolic objects do not occur during the colonization of the North European plain in the late Upper Paleolithic or in the initial colonization of the New World essentially because population densities were so low that there was no need for them. In other words, it would appear that symbolism of the kind that would leave unambiguous empirical referents conferred no particular adaptive advantage on these colonizing populations under conditions of low population density. We do *not* contend that symbolic behavior of other sorts did not occur—only that we cannot monitor it archaeologically. In our opinion, this is the major stumbling block to the study of symbolic behavior in the remote past; we simply do not know what aspects of the total repertoire of symbolic behavior are likely to leave traces in the archaeological record.

Our study concluded that evidence for "modern behavior" (*sensu* Chase and Dibble 1987) does not occur in the archaeological record until the Upper Paleolithic, regardless of the hominid associated with it. As this "modern behavior" is associated with a suite of adaptations present *after* 35,000 years B.P., it is logical to suppose that *before* this time a different suite of adaptations related to "nonmodern" behavior (however defined) must have existed. In these adaptations neither Neanderthals nor archaic *H. sapiens* nor morphologically modern humans appear to have utilized symbolism as part of their daily existence. As we see it, we are criticized for being interested in the variability of these Upper Pleistocene adaptations, what they might mean in terms of the archaeological record, and how they might be related to

the various hominid taxa with which they are associated. While we admit to no great admiration for the present state of systematics in human paleontology, we are manifestly *not* using symbolism—a behavioral trait—to question the *identification* of fossil taxa (*contra* Bar-Yosef et al., Dibble and Chase, Trinkaus, Stringer). We are using a behavioral trait in conjunction with other behavioral traits to examine whether human adaptations in western Eurasia during the Upper Pleistocene are similar or different in respect of the fossils with which they are associated. That Bar-Yosef et al. and Dibble and Chase take our statement about the “evolutionary distance” between Neanderthals and moderns to mean “taxonomic distance” is troubling. Taxonomies do not evolve; individuals in a population do. By attempting to monitor one aspect of the adaptations of pre-Upper Paleolithic hominids in conjunction with what we already know about other aspects of these adaptations, we feel better able to assess similarities and differences in order to begin to understand the evolutionary relationship that might have obtained between the hominids themselves.

We think that archaeologists (and many paleoanthropologists) tend to take biological taxonomic units at face value. Doing so often constrains debate along the lines established by the taxonomic categories themselves. Although we would think it obvious, it is very important to emphasize that *taxonomic categories do not always correspond to biological categories* (Futuyma 1979:507). Most of this literature is concerned with the taxon “species.” Biologists define species as “groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups” (Mayr 1942). Morphology by itself is *not* the only criterion for the identification of species (Futuyma 1979:190). Dependence on taxonomies created with reference to morphological traits in attempts to understand *evolutionary* relationships (as suggested by Bar-Yosef et al., Dibble and Chase, Stringer, and Trinkaus) is precisely what is wrong with the cladistic approaches to taxonomy that are currently so popular in replacement systematics. Taxonomy is a means to an end, not an end in itself. That Trinkaus, who has consistently produced credible functional explanations for morphological features grounded in behavioral changes, should make this particular criticism is especially worrisome. Perhaps we are missing something here, but it seems entirely inconsistent with his functional morphological approach (an approach we regard as the most defensible in a field littered with the wreckage of discredited empiricist systematics). We do not believe it is possible nor are we interested in trying to understand the dynamic evolutionary relationships of hominid species strictly in terms of sterile classification systems. To suggest that taxonomies are somehow “real” is reminiscent of the typological thinking in biological systematics pilloried by Mayr (1963) more than 25 years ago. We are also reminded of Maynard Smith’s (1988:9) reference to essentialism, wherein species are identified because they are “obviously different from one another,

and in possession of a common essence,” rather than because they do not interbreed. Lest we be accused of being overly critical of paleoanthropology, we hasten to add that there are clear parallels in those Old World archaeological systematics that use retouched-stone-tool typologies as if they were somehow “real” or “meaningful” in their own right (or more meaningful than other categories of evidence) (Barton 1988; Clark and Lindly 1989c; Dibble 1987, 1988).

In order to identify species, one must first determine whether individuals could have interbred under natural conditions. Characteristics that prevent interbreeding are known as isolating mechanisms, and these can be morphological, physiological, environmental, and/or *behavioral* in nature. In evolutionary biology, behavioral criteria are perfectly acceptable for assisting in the identification of species by assessing the likelihood that interbreeding might have taken place between them. This is especially true of subspecies (such as *H. sapiens sapiens* and *H. sapiens neanderthalensis*)—a concept that most evolutionary biologists consider arbitrary (Futuyma 1979:205). Although in this essay we examine one particular aspect of behavior (symbolism), we have elsewhere examined others (Clark and Lindly 1988; 1989a, b). In our opinion, there is no compelling biological evidence that Neanderthals and modern humans could not have interbred and, given their nearly identical adaptations, much to suggest that they did.

Dibble and Chase in particular confuse the taxonomic and biological species concepts by contending that differentiation of species depends solely on biological evidence. In addition, they mistakenly claim that we suggest that symbolism *causes* the morphological differences between archaic and modern humans. This is related to a misconstrual of evolutionary process. Behavior need not promote speciation or enhance reproductive success to be important to the survival of a species. Symbolic behavior is contextual. It was probably a latent capacity in all later Upper Pleistocene hominids, “activated” when it was adaptively advantageous to do so. Granted, modern humans have a multitude of different adaptations and degrees to which evidence of symbolic behavior is reflected archaeologically. Yet there is no question that we comprise a single species morphologically, physiologically, and behaviorally. It is, moreover, quite an inferential leap to argue that two populations with nearly the same morphology and exactly the same adaptation are different species (i.e., *do not* interbreed) simply because they have been *classified* as such. That humans are extremely flexible in their behavior and variable in their morphology does not mean that we can ignore similarities and differences in adaptation if we hope to understand evolutionary relationships. In short, we think that the approach advocated by Dibble and Chase and by Bar-Yosef and his colleagues ignores the interplay that must exist between archaeological, morphological, and molecular evidence.

Stringer suggests that, if morphological differences between moderns and Neanderthals at Qafzeh, Skhül, and Kebara are an indication of different adaptations, there

might be behavioral differences manifest in the skeleton and not reflected in the archaeological record (essentially the reverse of what we are arguing). This is an interesting idea but one that is difficult to test. The only kind of test that we would imagine to be widely convincing would be one based on significant differences in functional morphology, and Trinkaus, who has studied most of the western Asian material, does not detect any such differences. How much morphological variation is enough to suggest major differences in adaptation? Stringer seems to think that enough variation can already be documented between these two taxa to allow for the possibility of different adaptations. We disagree, but if he is correct archaeologists will have to reevaluate what our data and theories can really tell us about the past. We remain optimistic that the archaeological record is at least as informative about Upper Pleistocene hominid adaptation as is the morphology of the hominids themselves. The point, though, is that we really need both perspectives to address these evolutionary questions adequately.

Dibble and Chase, Mellars, and White, while emphatically agreeing that there is no discernible relationship between symbolism and fossil taxa, cannot help remarking the apparent uniqueness and significance of symbolic objects associated with the Aurignacian of Europe. The "abrupt appearance" of the Aurignacian in Europe, allegedly bolstered recently by early (ca. 40,000 years B.P.) accelerator dates from L'Arbreda and El Castillo caves in Spain, can be interpreted in several ways (Bischoff et al. 1989, Cabrera Valdes and Bischoff 1989; cf. Straus 1989). Mellars suggests that the early dates could signify a rapid replacement of indigenous Neanderthals by a dispersing population of moderns. In our opinion, it is just as likely that the Aurignacian "represents a simultaneous technological development, largely the product of convergence" (Straus 1989:477). These dates, at the *far western end* of the Aurignacian geographical distribution, are as old as or older than early Aurignacian dates from eastern Europe (Svoboda and Simán 1989). The "facts" evidently do not "speak for themselves," and the Spanish dates could mean many things: (1) that the dates from eastern Europe are too young and older dates will eventually be found, (2) that the conventionally dated early Upper Paleolithic sites in western Europe are too young and will turn out to be a good deal older when the accelerator technique is more widely used, or (3) that Aurignacian technology developed *in situ* at different times in different places out of the local Middle Paleolithic technology (Straus 1989). We think that a good case can be made for multiregional continuity and *in situ* development, at variable rates, of Aurignacian technology in Europe. No human fossils are associated with the dated early Aurignacian levels at these sites, and if fossils with Neanderthal morphology are eventually found the replacement scenario will collapse like a house of cards. The same will be true if fossils of modern morphology are eventually discovered in a Chatelperronian context. White appears to support the possibility of an *in situ* European development of the

Aurignacian, but we see no evidence to suggest the "ancestral" relationship he suggests between the European Aurignacian and the industries of Boker Tachtit in the Levant (Marks 1983, 1985; Marks and Volkman 1983), or the Bohunician of eastern and central Europe (Svoboda and Simán 1989). These industries are based on Levallois reduction strategies that are not usually seen in the European Aurignacian.

Gargett's assertion to the contrary, we never suggested that the evidence for early Upper Paleolithic symbolic behavior in Europe should be ignored. We only wished to point out that the "early" evidence is, for the most part, undated or dated in terms of *fossiles directeurs* that, *contra* White, are manifestly *not* an accurate means of arranging archaeological assemblages in a chronological sequence (Straus 1987, Straus and Heller 1988, Šimek and Snyder 1988). Although the numbers of objects "dated" to the early Upper Paleolithic are impressive, we remain unconvinced that most of them can be accurately placed in time. Those radiometrically dated show a pattern of relatively few early Aurignacian objects, with an increase in frequency in the later Aurignacian and Gravettian. This fact does not diminish the importance of these objects or their significance to the study of early Upper Paleolithic adaptations but only indicates that symbolic behavior became more important through time and that the trend continues unabated (in fact, accelerates) in the later Upper Paleolithic. It is clear that we do not know as much about the Aurignacian as we should and that dependence on differences in retouched-tool-type frequencies and undated "art" objects does not present a complete picture of the relationship of the Aurignacian to the Chatelperronian, the late Middle Paleolithic, or even the Gravettian.

We are pleased that Jacobs, Smirnov, and Straus support our argument for a significant adaptive shift at around 20,000 B.P. rather than at the beginning of the Upper Paleolithic. In addition, Jacobs's research on concurrent human morphological change during the late Upper Paleolithic complements the archaeological data in suggesting major adaptive change. Dibble and Chase claim that our construal of biocultural evolutionary patterns cannot be reconciled with a mosaic configuration. We emphatically disagree. Our viewpoint is entirely consistent with mosaic evolution—with the crossing of behavioral and biological "thresholds" (however defined) at different times in different regions (Clark n.d.). Differences in *rates* of change do not contradict and in fact are predicted by the multiregional-continuity model.

Bar-Yosef et al., Mellars, and Stringer think that we undervalue the "grave goods" associated with the Skhūl V and Qafzeh 11 burials, yet they are unable to explain why these "offerings" are any more significant than similar objects associated in similar ways with archaic *H. sapiens* that are dismissed as "utilitarian" and/or as inadvertently included in grave fills. We think that Gargett (1989) is correct in questioning the intentionality of Middle Paleolithic burials, many of them enshrined in the literature as if they were established

"facts." In respect of Qafzeh and Skhül, however, we are questioning not the burials themselves but only the purported "grave goods." Finally, contrary to Bar-Yosef et al., it is crystal clear from McCown's comments and from his illustration (taken from a photograph) that Skhül V, while probably a grave, was in fact disturbed.

We welcome the comments of Smirnov, who represents a research tradition quite different from our own. He embraces a more eclectic view than ours (and most other commentators') in respect of what can be regarded as "symbolic" in the Upper Pleistocene archaeological record (especially the evidence for ritual treatment of several of the Qafzeh and Skhül burials). Although we have not been able to evaluate these claims directly, we adopt a more conservative stance. Taphonomic research over the past decade has forced a much-needed reassessment of conventionally accepted evidence for cannibalism, ritual, burial, etc., and has provided a range of alternative interpretations as to what such evidence might "mean."

Straus's point regarding the scarcity of grave goods and unambiguous graves even in the Upper Paleolithic is well taken. Rather than accepting on faith the conclusions of earlier generations of prehistorians, we have an obligation to reanalyze grave contexts and possible associated objects on a case-by-case basis. We agree with Jacobs that the reasons behind apparent differences in the frequency of Middle and Upper Paleolithic burials need to be explored (see also Clark and Neeley 1987).

Gargett and Mellars contend that the Howieson's Poort industry found in the MSA sequence of South Africa is very "modern" in appearance, but we do not know what that means. If it is the presence of prismatic blades that makes a lithic industry "modern" in appearance, then there are numerous examples of "modern"-looking industries in the Middle Paleolithic of southwestern Asia and Europe, and industries lacking blades are found all over the world and throughout prehistory. Gargett's assertions about a correlation between modern behavior and morphology cannot be tested in default of adequate operational definitions of "modern" behavior and morphology.

We agree with Jacobs and Straus that the dichotomies utilized in the modern-human-origins debate (Middle vs. Upper Paleolithic, archaic vs. modern morphology) are too restrictive for the investigation of what was surely a dynamic process rather than a series of more or less discrete stages. We also share Jacobs's concern that the underlying evolutionary dynamics be examined and discussed. The biological and cultural transitions must be studied *as transitions*. En bloc comparisons between normative characterizations of the Middle and Upper Paleolithic or between archaic and modern *H. sapiens* cannot fail to throw differences into sharp relief, but they tell us nothing about process.

Trinkaus takes issue with our inclusion in the survey of certain fossils whose "early modern" status is debated. It should be clear, however, that one must depend on the published accounts of these finds. It is simply a fact that the taxonomic status of some of them is con-

tested by reputable scholars. One of the things that struck us very forcefully is just how poor the fossil evidence really is for pre-Upper Paleolithic morphologically modern humans. It amounts to *almost nothing*, and it is characterized by the most inadequate time/space grid imaginable. The impression conveyed by general and popular accounts is that there is a lot of solid, unequivocal evidence for early moderns, but it just isn't so.

Pike-Tay, apparently making a distinction between cultural adaptation and biological adaptation, chastises us for emphasizing the latter. We readily admit to an "adaptationist" bias in respect of human social behavior (Binford 1962, 1964, 1965), mainly because "adaptation" has realistic empirical referents whereas many construals of "culture" do not. Obviously, much depends here on what is meant by "culture" and what is meant by "biology." We define adaptation as evolutionary biology does (see n. 2). Behavior can be viewed as the "dynamics of adaptation"—a strategy for survival and reproduction (Binford 1972:133; see also Maynard Smith 1978, Jochim 1981, Foley 1987b). Natural selection operates on the behavior, morphology, physiology, and biochemistry of an organism, through reproductive success, to minimize or solve problems posed by an organism's physical and social environment (Foley 1987b:61). Culture, in contrast, is so ambiguous a concept that it has defied any consensus definition for over a century (and is, therefore, analytically useless). At a minimum, Pike-Tay must tell us what she means by "cultural adaptation." In a backhanded way, though, she has put her finger on an important conceptual issue: can we in fact neatly separate biological and cultural components of adaptation, and, if so, what are the empirical referents of each?

In modern-human-origins research, one might be tempted to look for "cultural origins" in an effort to differentiate "culture" from "paleoculture," as Chase and Dibble (1987) have done. In our opinion, however, this is a dangerous tack because of the generally acknowledged mosaic character of cultural evolution. What aspects of behavior are to be considered exclusively "cultural"? How many such traits must a population exhibit before it can be said to be "cultural" rather than "paleocultural"? Even assuming that one could come up with unambiguous empirical referents for "culture" and "paleoculture" (which we regard as an impossibility), there is no parsimonious solution to this problem, since it is based on a false dichotomy. Behavior usually considered "cultural" can be found in a number of nonhominid primates (Foley 1987b). Some chimpanzees make, use, and transport tools and thereby exhibit displacement (a capacity to take future contingencies into account). Some (perhaps all) chimps have the capacity to symbol and can be taught to communicate through sign language. Lowly vervet monkeys have a communication system with vocalizations that are the functional equivalents of words in human speech (Foley 1987b:4). Many animals exhibit learned behavior to a greater or lesser degree and so can transmit (to a greater or lesser degree) adaptive behavior from one generation

to the next. In short, we agree with Foley (1987*b*) that culture is not a useful concept in the study of human origins because the ambiguity of its empirical referents obscures our perceptions of the dynamic development and evolution of behavior in the archaeo/paleontological record.

We are astonished that Gargett, Pike-Tay, and White (in part) take issue with our contention that faunal remains are a comparatively direct monitor of adaptation and with our conclusion that subsistence patterns over the Middle/Upper Paleolithic transition in Europe demonstrate continuity (Clark 1987, Clark and Lindly 1989*a, b*). Admittedly, the data available to evaluate this proposition are somewhat inadequate (though better in Europe than anywhere else), but (*contra* Pike-Tay) we are not basing our conclusions on anything so simpleminded as species lists (Chase 1986, Clark 1987, Straus 1986). Neither Gargett nor Pike-Tay presents data to contradict our findings. We, too, have read Stiner (1989*a, b*), and to us her results, from an analysis of age profiles across the late-Middle/early-Upper-Paleolithic transition in Italy, appear to suggest continuity between these periods. Subsistence organization is indeed difficult to understand, but we would remind Gargett that all we can do is to utilize the data in hand. While White's linkage of subsistence organization and culture is sometimes (although not always) demonstrable in ethnographic contexts, our inability to identify "cultures" unambiguously makes it impossible to link these aspects of adaptation archaeologically.

One might imagine that this last great hominid transition would be the best-understood of all, but, as the present controversy shows, there is as much diversity of opinion as ever. If we are ever going to understand the transition to modern humans, it must be studied *as a transition*. We can no longer afford to compartmentalize it into archaeology, paleoanthropology, and molecular-biology components. Preconceptions about biological evolution have important consequences for interpretations of the archaeological record (and vice versa), and archaeologists can ignore the findings of other disciplines only at their peril. The ancestors of modern humans represent a long-lasting adaptive phase immediately preceding us. To claim that they were extinguished without issue over most of their range without coming up with a plausible explanation of *why* and *how* does little to engender confidence in the explanatory potential of anthropological research designs.

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Calendar

1990

- June 18–July 20. National Endowment for the Humanities Institute on Perspectives on the Indo-European World, Austin, Tex., U.S.A. Write: Edgar C. Polomé, Oriental and African Languages and Literatures, University of Texas at Austin, 2601 University Ave., Austin, Tex. 78712, U.S.A.
- July 28–31. International Symposium on Primates, Delhi, India. Write: P. K. Seth, Department of Anthropology, University of Delhi, Delhi 110 007, India.
- August 2–5. Language Origins Society, 6th Annual Meeting, Volendam, The Netherlands. Write: J. Wind, Institute of Human Genetics, Free University, P.O. Box 7161, Amsterdam, The Netherlands.
- August 19–23. 7th Inuit Studies Conference, Fairbanks, Alaska, U.S.A. Theme: Looking to the Future. Write: Lydia Black, Department of Anthropology, University of Alaska, Fairbanks, Fairbanks, Alaska 99775, U.S.A.
- August 26–30. European Anthropological Association [concerned mainly with human biology], 7th Congress, Wrocław, Poland. Write: Tadeusz Bieliński, Zakład Antropologii, Polish Academy of Sciences, ul. Kuznicza 35, 50–951 Wrocław, Poland.
- August 31–September 2. European Association of Social Anthropology, 1st conference, Coimbra, Portugal. Themes: Historical Approaches in Anthropological Analysis (Kirsten Hastrup, convener), Conceptualizing Society (Adam Kuper, convener), Emerging Trends in the Anthropological Study of Gender (Teresa del Valle, convener), Understanding Ritual (Daniel de Coppet, convener), with a day set aside for panels and groups proposed and organised by members. Write: Daniel de Coppet, Ecole des Hautes Etudes en Sciences Sociales, 44, rue de la Tour, 75116 Paris, France.
- August or September. Pan-African Association of Anthropologists, 1st Annual Meeting, Nairobi, Kenya. Theme: The Role of Anthropology in African Development. Write: Paul Nchoji Nkwi, University of Yaounde, P.O. Box 755, Yaounde, Cameroon, or A. B. C. Ocholla-Ayayo, Institute of Population Studies, University of Nairobi, Nairobi, Kenya.
- September 3–9. International Association for the History of Religions, 16th Congress, Rome, Italy. Theme: The Notion of "Religion" in Comparative Research. Write: XVI International Congress of the History of Religions, Dipartimento di Studi Storico-Religiosi, Facoltà di Lettere e Filosofia, Università di Roma "La Sapienza," Piazzale Aldo Moro, 5, I-00185 Rome, Italy.
- September 18–23. 4th International Symposium on the Mesolithic of Europe, Leuven, Belgium. Write: Pierre M. Vermeersch, Mesolithic Symposium, Redingenstraat 16bis, B-3000 Leuven, Belgium.

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- July 7–11. 47th International Congress of Americanists, New Orleans, La., U.S.A. Write: Secretariado ICA 1991, Roger Thayer Stone Center for Latin American Studies, Tulane University, New Orleans, La. 70118–5698, U.S.A.
- September 1–7. International Union of Prehistoric and Protohistoric Sciences, 12th Congress, Bratislava, Czechoslovakia. Theme: Archaeology—Present—Future. Write: Archeologický ústav Slovenskej akadémie vied, Sekretariát XII. kongresu UISPP, 949 21 Nitra-hrad, Czechoslovakia.

1992

- September 7–10. European Anthropological Association, 8th Congress, Madrid, Spain. Write: María Dolores Garralda, Sección de Antropología, Facultad de Biología, Universidad Complutense de Madrid, Ciudad Universitaria, 28040 Madrid, Spain.