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Discussion and Criticism[1]

Early Artefacts from Pakistan? Some Questions for the Excavators

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From the sediments associated with Tertiary rifting in East Africa have come hominid remains and acceptable concentrations of stone artefacts dating before 1.5 million years. Fruitful localities occur from Barogali in Djibouti in the north (Chavaillon et al. 1987) via the Hadar (Johanson and Edey 1981), Omo (Merrick and Merrick 1976, Coppens 1977), East and West Turkana (Leakey and Leakey 1978, Brown et al. 1985, Harris et al. 1988), Senga V (Harris et al. 1987), Olduvai (Leakey 1971), Chesowanja (Gowlett et al. 1981), Peninj (Isaac and Curtis 1974), and Laetoli (Leakey and Harris 1987) to the Chiwondo Beds of Malawi (Kaufulu and Stern 1987) in the south. The footprints from Laetoli and and the hominid remains from several of these sites are clear testimony to the presence of bipedal hominids, and scatters of stone artefacts at all except Laetoli bear witness to their tool-making ability.

The stone artefacts fulfill several criteria that permit us to accept them as such: (1) Pieces that appear worked occur in concentrations (floors). (2) The claimed worked pieces occur out of sedimentary context in finer-grained fluvial and lacustrine sediments, contexts to which they could not have been carried by any natural processes that can be considered to have been operating at the time. (3) The pieces occur in clear association with other clear evidence of hominid presence: butchered bone, hominid fossils, probable structures.

Because these sites and these associations exist it is easier to accept new claimed early occurrences from the region, although each must be expected to justify itself.

Beyond sub-Saharan Africa there is no clear evidence for any hominids earlier than *Homo erectus* and no site that fulfills the three criteria earlier than 1 million years b.p., at 'Ubeidiya (Stekelis 1966, Stekelis, Bar-Yosef, and Schick 1969) and Isernia (Salvatori 1984), for example. Other sites in the time range or perhaps earlier, for example, Vallonnet (de Lumley et al. 1963) and Soleilhac (Bonifay 1986), may fulfill one or two of them, but most are very doubtful. A claim for stone tools dating before 2 million years from this area such as that of Dennell, Rendell, and Hailwood (CA 29:495–98; see also Rendell and Dennell 1987, Rendell, Hailwood, and Dennell 1987) must be evaluated more stringently than an equivalent report from East Africa. From this demanding perspective Riwat comes out poorly: (1) The pieces do not occur in a concentration of probably worked pieces but were extracted from blocks of conglomerate, some more than 10 m^2, scattered for an undisclosed distance along a gully. The worked pieces represent an undisclosed proportion of similar-sized pieces from the locality (Carmona [Bordes and Viguier 1971] in Spain is comparably dubious). (2) The sites occur in a conglomerate in which, according to Dennell et al., they are not out of sedimentary context. (3) The sites are not associated with any other evidence of the presence of hominids.

In the absence of the three criteria that facilitate acceptance of the East African localities, Dennell et al. fall back on criteria of technique (cortex as percentage of probable original surface, number of flakes removed, number of flaking directions, presence of bulbs of percussion, and subjective evaluation), using these to grade the artefacts they have initially extracted from an unknown sample. These criteria were established not by study of accepted collections of very early material but by study of other pieces collected in Pakistan from Lower Pleistocene horizons and regarded as ranging from "the extremely convincing to the almost certainly natural." The use of such a comparative collection will inevitably lead to circularity in attribution. The authors have also failed to test their criteria by studying a sample collected from an active high-energy fluvial environment such as can be provided—quite convincingly if one wants to be convinced—by the beds of most Pyrenean mountain streams. Without such tests, the criteria employed cannot be accepted as defining a distinct early hominid conceptual or manipulative set. Some artefacts from Olduvai would fail on the combination of criteria used (see Leakey 1971:26, 36, 74–78), while the majority are much more complex.

The Riwat "artefacts," failing on the three external criteria, cannot be justified by the use of a priori and untested technological criteria.

Finally, the evidence cited indicates that the sediments had been subjected to folding and erosion before 1.6 million years, and a date of 2.1–1.9 million years for the folding event may well be acceptable. Even so, the accumulation of the conglomerate must have preceded not only the folding event but also the accumulation of the 65 m of overlying sediments. The normal

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event beneath the conglomerate is not in itself dated, so one must suppose a date for the conglomerate that not only predates 2 million years but may predate it considerably.

The Riwat pieces fail to satisfy the test requirements employed here, and unless such requirements can be satisfied, in the absence of any evidence of hominid presence in Asia so early, the claim that they are artefacts must be doubted.

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Dennell, Rendell, and Hailwood present some data to support their view that five or six unambiguous quartzite artefacts dating to ca. 2 million years B.P. have been found in the Soan Valley. In my opinion, however, their arguments are not conclusive. I do not wish to exclude the possibility of artefacts of such great antiquity, but to my mind it has not been proven that the Pakistan pieces are man-made. Only one piece, interpreted by Dennell et al. as a core, is illustrated in their report, and the photographs are extremely unclear and worthless as documentation of its presumed artificial character.

Some criteria that they use to identify artificial objects are relatively small remaining cortical areas, scars of several removed flakes, the presence of percussion bulbs, and the existence of several planes in which flakes were removed. None of these criteria is sufficient to permit reliable identification of man-made objects, especially for the stratigraphic context described for these finds—a coarse conglomerate, most probably fluviatile in origin. Archaeological research in the Netherlands at sites of the Rhenen industry (Stapert 1981, n.d.) has made clear that great caution should be exercised in identifying artefacts derived from gravelly river-laid deposits (in this case deposits of the River Rhine, dated to 200,000–300,000 years B.P.). "Flake negatives" and especially "retouches" are produced by geological processes in enormous quantities in gravelly riverbeds, and it is by no means easy to establish in every case whether finds from such contexts are man-made or not. The removal of flakes in several planes does not prove human workmanship; geological processes can do the same. Bulbs can also be produced by geological flaking. Small cortical remnants do not constitute proof of pieces' being man-made if no further details are offered. Stones present in river gravels may have had a long geological history, and "old faces" could have been produced prior to deposition of the gravel in which they are now found. Some of these faces may not look like "remnants of cortex," but that does not mean that they originated as a result of hominid activities. In my opinion, therefore, Dennell et al. have not convincingly demonstrated the artificial character of their finds—which does not necessarily mean, by the way, that these pieces are not artefacts.

Other types of evidence crucial for establishing the artificial character of "flaked stones" are not presented, for example, data on flake angles (between the ventral face and the striking-platform remnant or, in the case of "cores," between the flake negatives and the striking platform). Geological processes may produce flakes with angles less than or more than 90°, while in man-made flakes these angles are always greater than 90° (often 100–130°). Do the striking-platform remnants on flakes show signs of preparation, and what are their metrical attributes? If several flake scars are present on the same core (several examples of this are mentioned), what are their metrical attributes? Are they similar in dimensions and form, as is often the case with Palaeolithic cores? Do they all have the same surface modifications, proving that they originated at the same time?

"Ripple marks" are also mentioned as indicating a hominid origin, but these marks can also be produced by geological flaking or when stones split as a result of alternating cooling and heating. With flint, naturally produced ripple marks can often be distinguished from ripple marks on man-made artefacts; this is more problematical in the case of quartzite pieces. Natural ripples are often sharp-profiled in cross section, while ripples on flakes produced by man mostly show smooth, sinus-like cross sections (Stapert 1976). On the Soan Valley cores, are the ripple marks similar to each other within all the flake scars occurring on a core, and what are their cross sections like?

Without sufficient data of these kinds, we cannot evaluate Dennell et al.'s claim. Serious problems regarding the identification of man-made objects are common in Lower Palaeolithic research and often underestimated. In Europe, one of many examples of this problem is the material from the Belle Roche Cave in Belgium (Cordy 1980, 1981), which can best be considered as of uncertain origin (Roebroeks and Stapert 1986). The Pakistan finds should also be considered "incerto-facts" until more convincing data on their presumed artificial character have been produced.

Reply

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Hemingway's comments display several misunderstandings of both the East African early hominid record and our work in Pakistan. This is shown most clearly by his criticism that we used "a priori and untested technological criteria" in judging some of the pieces from Riwat hominid- rather than geologically flaked. The Oldowan is accepted as hominid-struck because it is clearly different in flaking and fracturing from geologically flaked material. It was on those grounds that it was first recognised by Leakey in 1935 and defined in terms of directionality of flaking in 1951 (see Leakey 1951:34–40), long before it was found associated with hominid remains, probable structures, and so-called living floors. What the discoveries of the early 1960s did was (*a*) to enable Mary Leakey to suggest that the Oldowan also included items that would otherwise have been indistinguishable from geological material—hammerstones, manuports, anvils, modified blocks, etc. (Leakey 1971:3–8) and (*b*) to open a window on early hominid behaviour in general. Even though the trend of research over the last 15 years has been to doubt the strength of association at many of the Olduvai and Koobi Fora sites between hominids, butchered bone, and probable structures, the Oldowan is still recognised as hominid-struck on primarily technological and stratigraphic criteria. Conversely, the main reason the Kafuan material from the MN horizon at Nsonsezi in Uganda (once regarded as arguably second only in importance to Olduvai as evidence for early tool making [see Cole 1954:125]) was discredited was that Clark (1958) was able to demonstrate on technological grounds that it could not be distinguished from naturally flaked material.

In other words, we have simply followed long-established practice in attempting to demonstrate that pieces of flaked stone were modified by hominids and not by geological agencies. For example, Acheulean bifaces were once regarded as thunderbolts: their acceptance as artefacts came about not by finding them associated with hominids, butchered animals, or structures but with the realisation that they could not have been produced naturally. Using the same procedure, we identified handaxes in conglomerate horizons in 1983 (see Rendell and Dennell 1985, Dennell 1986); no one has ever suggested that they were natural because they were not found in association with hominids, structures, floors, and butchered bone.

Hemingway additionally fails to understand our system of grading the Riwat artefacts on a scale of confidence from 0 to 5. A major aim of our survey work in the Pabbi Hills since 1986 has been to gather stone tools. To date, some 500 pieces of flaked quartzite have been found whilst collecting vertebrate fossil assemblages from erosional surfaces between 0.9 and 2.5 million years old. Some pieces—as expected on surface surveys—are more convincing than others as examples of hominid-struck stone; the most convincing include hemispherical disc cores, small flakes with no cortex but with positive and negative bulbs of percussion, and cores with multiple flake scars and little cortex. There is no question that some of this material is hominid-struck; what needs clarification at present is its stratigraphic context. The purpose of grading this material after recording in detail its flaking characteristics was to isolate localities that looked especially promising for future research. The reason for grading the Riwat assemblage the same way in 1987 was simply to place it in the wider context of what we had collected since its discovery. Far from introducing an element of circularity, the point of that ranking was simply to compare the Riwat material with a larger set of data that we had already recorded in detail.

Hemingway is unduly concerned about our collecting strategy at Riwat in 1983. The claimed artefacts in question were "initially extracted from an unknown sample" in the sense that we did not count the total number of clasts exposed in the surface of the conglomerate that we examined. However, I estimate that at least 2,000 were inspected. As stated, only 23 showed any indications of flaking or fracturing: of these, as indicated, only 5 (at most 0.25%) are considered to be extremely convincing as instances of hominid flaking.

He is correct in saying that we did not demonstrate differences between the claimed artefacts from Riwat and a sample from a high-energy fluvial environment. We have, however, inspected conglomerates at Rohtas, Dina, and Jalapur, one in the Pabbi Hills, and many in the Soan Valley since 1981. These conglomerates can be up to 30 m thick and entirely clast-supported. In 1983 and 1985 we inspected as many accessible conglomerate sections as we could, stone by stone, in the search for unambiguous examples of Palaeolithic artefacts (see Rendell and Dennell 1985). Of the several thousands of clasts that we have examined in those conglomerates, the overwhelming majority are unflaked and typically spherical, sub-spherical, or ovoid; some are split; very occasionally one finds clasts with one or two shallow flakes detached; signs of battering are also common. Clasts with more than three flake scars are extremely rare. Whilst this work has no statistical basis, it confirms our view that the pieces under discussion from the Riwat lower conglomerate are highly anomalous.

We have also documented in detail the flaking and fracturing of clasts in the lower conglomerate in Riwat. In 1988, an additional piece of flaked quartzite that we consider to be hominid-struck (fig. 1) was found in section 40 m from where the piece of which a cast is available was found in 1983. To demonstrate that these came from the same horizon, this 40-m section was drawn at a scale of 1:20, and every clast more than 2 cm in size was plotted and noted (Rendell and Dennell n.d.). In

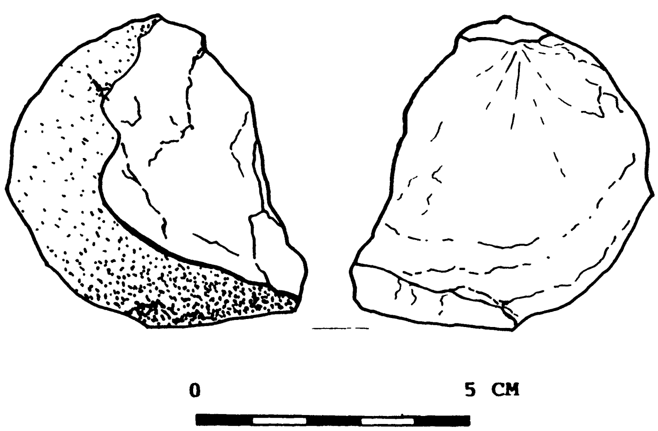

FIG. 1. *Quartzite flake (R88/1) from lower conglomerate horizon at Riwat. (For comparison with a utilised light-duty flake from DK, Lower Bed I, Olduvai, see Leakey 1971:fig. 18, no. 3.)*

all, 1,277 were inspected. Of those, 10 were gritstone nodules, 2 were sand casts of bone, and 1 was a mud ball. Only 1 had been fractured, and none of the other 1,263 clasts exhibited any evidence of flaking. Thus the overwhelming majority of clasts in the type of moderate- to high-energy conglomerates represented at Riwat are very unlike the pieces under discussion, and pieces regarded as hominid-struck form a minute proportion of the total.

In addition, I have recorded cobbles in colluvial conglomerates eroding from the base of Upper Pleistocene loess nearby at Riwat. These were near scatters of freshly struck flakes and blades and were recorded to see whether or not these were localised: in other words, one would expect to find evidence of human modification. Of a total of 310 cobbles, 249 were unflaked and 24 were split; 24 had one flake scar, 2 had two, 1 had three, and 10 were flakes. Almost all clasts are complete, and none could be described as a core or like the piece under discussion here.

I have also recorded 1,218 clasts in a conglomerate surface at Site 55, Riwat, that was covered by loess ca. 45,000 years ago. This particular conglomerate contained on its surface a fresh quartzite blade, flake, and core assemblage. The interest in this particular conglomerate was that some of the cobbles in its surface might either have been naturally flaked when first incorporated into the conglomerate or used as a source of raw material either when or before the structure was used. Consequently, cobbles were recorded in detail in respect to numbers of flakes removed. There were 700 complete clasts, 201 split ones, and 12 quartered ones. Seventy-seven clasts had had one flake removed, 83 two flakes, 88 three flakes, 45 four flakes, 11 five flakes, and 1 six flakes. The 104 cores and 65 fragments from this site were usually flaked considerably more than six times. In general, they were usually flaked in several directions, displayed clear bulbs of percussion, and had most of their cortex removed. In 11 cases, they could also be joined to flakes and/or blades. We therefore consider the piece from the lower conglomerate at Riwat far more like the archaeological material from Site 55 than like the clasts from conglomerates that we have examined but not documented in detail, from the 1,277 clasts in the 40-m section at Riwat, from the 310 clasts recorded in colluvial deposits eroding from the base of Upper Pleistocene loess, and from the 1,218 recorded in the conglomerate at Site 55.

We feel, therefore, that we can demonstrate significant differences between geological flaking and the key items from the Riwat lower conglomerate assemblage. In addition, we have shown a cast of this specimen (which Hemingway could easily have seen for himself) to anyone expressing interest. To date, no one who has seen it has regarded it as natural; instead, discussion has concerned its context and dating.

Two points can be raised here on context and dating. In 1988, a latex-and-plaster mould was made of the socket left after chiseling the main piece (R001) from the gritstone five years previously. This shows clearly (*a*) that the impression of the artefact in the socket is still visible and (*b*) that all the flakes must have been detached before incorporation into the gritstone. In 1988, we traced the artefact-bearing horizon at Riwat over 5 kilometres and can confirm that pieces that we regard as hominid-struck and clearly embedded within that conglomerate are localised to within 100 m of the pieces found in 1983. Hemingway is probably incorrect in asserting that the conglomerate is "substantially older" than 2 million years. As indicated by Rendell, Hailwood, and Dennell (1987), the normal event under the conglomerate is likely to be Reunion rather than Gauss: further palaeomagnetic analyses by Rendell are in hand to confirm this.

I am totally at a loss to understand what Hemingway means by "The sites occur in a conglomerate in which, according to Dennell et al., they are not out of sedimentary context," but I assume that he is trying to make some point about the contextual evidence. We would certainly not claim that we have found "sites" in the normal sense of the term. As indicated in Rendell's section (Rendell, Hailwood, and Dennell 1987: fig. 2), the conglomerate/gritstone is sedimentary. It is clearly a fluvial deposit associated with cross-bedding, pebble stringers, and pebble orientation and varies laterally over a short distance, from a largely clast-free gritstone near where the main piece was found to a predominantly clast-supported one 20 m downstream. Whilst the claimed artefacts are clearly not in their original context of discard, they are unlikely to have been transported far, as is evidenced by the freshness of the flake scars.

He inadvertently makes one useful point with the references he cites in his first paragraph. The overwhelming amount of early hominid research in the last few years has taken place in East Africa. It is worth bearing in mind that absence of evidence is not evidence of absence: whereas East African fieldwork can demonstrate a marked degree of continuity from 1931 to today, virtually all the major field programmes in China, India, and Java were terminated by the outbreak of the war with Japan, and little significant research into Asian hominid origins occurred until the late 1970s. Merely that there are few additional data yet from Asia is no guarantee that they are not there; at present, it may well reflect the lack of fieldwork since 1939.[1]

Stapert's comments are useful and informed. I agree that the photographs are not very satisfactory, but he is free to inspect the cast of the one illustrated. His main point concerns the crucial question of differentiating hominid from geological flaking. He does not specify the type or size of stone(s) making up the gravel deposits he mentions from the Rhine, but I assume that they are largely flint/chert, which fractures more easily than quartzite, and perhaps smaller on average than the clasts from the Riwat horizon. The Rhine deposits appear to differ substantially from the sections we have inspected. Far from finding "enormous quantities" of "flake nega-

1. I thank Linda Hurcombe for critical comments and P. Smith for some of the raw-data estimates.

tives" or "retouches," we have found these in only negligible frequencies. As indicated above, flaking of any kind is extremely rare in the fluvial horizons that we have examined. None of the pieces that we consider artefacts show signs of thermal fracturing, and we do not feel that this factor can account for the multiple flaking seen here.

I thank him for emphasising the importance of regularity of flake size and proportion, obliqueness of striking angle, and presence of sinuous ripple scars as criteria of hominid flaking that we could have included. Details of piece R001 (taken from the cast) are as follows: Flake sizes (mm) are (Face 1) 107 × 103*, 32 × 40, and > 46 × 105*; (Face 2) 60 × 78, 66 × 64, and 35 × 40; (Face 3) 52 × 26 and 70 × 74*. Those asterisked have clear bulbs of percussion and striking angles of 112°, 110°, and 96° respectively; flake angles on the other five could not be measured as precisely but are more than 90°. Length/breadth ratios are 1.04, 0.80, > 0.44, 0.77, 1.03, 0.88, 2.0, and 0.95. Ripple marks on R001 are sinuous and not sharp-profiled in cross section; they are also consistent with one another where they occur. Five of the flakes from R001 are similar in size and proportion to some of the larger ones in Oldowan assemblages from Bed I and Lower Bed II at Olduvai. Maximum lengths of flakes at DK, FLK, and FLK North were 64, 65, and 57 mm and maximum breadths 36, 80, and 54 mm; average length/breadth ratios for quartzite flakes from these localities were 1.30, 1.17, and 1.31 if end-struck and 0.87, 0.77, and 0.96 if side-struck (Leakey 1971: 37, 55, 83). The other three flakes on R001 are within the size range of the longest (110 mm, MNK occupation floor) and the widest (113 mm, EF-HR) flakes in Upper Middle Bed II (Leakey 1971:155, 136). Striking angles are also similar in that the Olduvai ones are mostly oblique; Stapert should note that striking angles of hominid-produced flakes are *not* always > 90°, as up to 10% of the flakes from FLK and HWK East at Olduvai had angles of <90° (see Leakey 1971:58, 106). Nevertheless, it is worth mentioning that the striking angle of piece R88/1 (fig. 1) is strongly oblique and that its size and proportion are within the Olduvai range (length 58 mm, breadth 47 mm, length/breadth 1.23).

We feel that the flake scars on R001 originated at the same time and see no reason to suppose that this and the other pieces were reworked. The flake scars on R001 and R88/1 were crisp, with no evidence of the kind of battering and abrasion one would expect in reworked material that had been repeatedly flaked over a long period. Overall, therefore, we feel that the pieces we consider to be hominid-flaked differ very substantially from the type of natural flaking that we have observed in fluvial and colluvial conglomerates.

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STEKELIS, M., O. BAR-YOSEF, AND T. SCHICK. 1969. *Archaeological excavations at 'Ubeidiya 1964–66.* Jerusalem: Israel Academy of Science and Humanities.

On the Evidence for Neandertal Burial

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24 X 88

Gargett (CA 30:157–77) gives plausible alternative taphonomic explanations for the major cases traditionally presented as proof of—be it incidental—purposeful burial of Neandertal man by his fellow human beings. We were prepared for his conclusion by the paper of Chase and Dibble (1987) on the totality of Neandertal symbolic behaviour, and it is this broader view that one misses here, especially in this anthropological journal.

For one thing, we should try to discriminate between the burial or covering of corpses for very practical, for example, hygienic, reasons and burial with ideational motivation. Burial in itself does not say very much about Palaeolithic man's ideas or level of abstract thinking, and it is precisely here that some of the basic and most challenging questions lie. One should first distinguish intentional burials and then try to identify burials with "symbolic meaning," and, while it is difficult to identify sound archaeological correlates for the latter that work in the Palaeolithic, one should try. Intentionally placed grave goods (other than adornment) seem to be the best; considerable quantities of red ochre rank second, while specific, traditional body postures could be a clue, too.

Gargett's critical approach to Neandertal burials tends to overstress the Middle/Late Palaeolithic contrast when Late Palaeolithic burials are not given similar attention. Quite a number of Late Palaeolithic "burials" might need to be rejected as such or treated more cautiously as a result of more critical analysis. I am especially thinking of multiple burials and burials at various levels in a single cave. Adornment is no criterion, since this can become fossilized by accident as well. If some of the Neandertal "graves" were to withstand the present critique and some of the early Upper Palaeolithic "graves" were to be explained away by referring to taphonomic processes, then the Middle/Late Palaeolithic transition would be more gradual. A major difference is not so much the symbolic behaviour reflected in burial itself but the presence of adornment, absent with Neandertal man and present in Late Palaeolithic burials from the Aurignacian onward (for instance, Grotte des Enfants, Menton) concurrent with the appearance of rock art as another expression of symbolic thinking.

Chase and Dibble (1987) state that "it appears that the actual number of cases where intentional burial is more or less certain is small compared to the total number of burials found to date" (p. 272). Perhaps the number is even zero and it is all pure taphonomy, just like "cannibalism" and the "cave bear cult." After a period of upgrading, Neandertal man is now being downgraded on the modernity scale. In respect to burial it would, however, be interesting to investigate whether other animals are *also* accidentally fossilized more or less complete and articulated and in what numbers. The cave bear (*Ursus spelaeus*) might rank first because of its use of deep caves as winter sleeping dens. Are there any other animals on this list, and, if not, can this be explained by man's living and/or sleeping in caves, in contrast to animals? Perhaps some arguments for the purposeful covering of bodies could be found along this line.

Since "the evidence from Middle Palaeolithic burials does not demonstrate the presence of symbolism or culturally defined values" (Chase and Dibble 1987:276), we should look to other fields, and I would like to point to the sometimes very careful and sophisticated shaping of handaxes, far beyond the practical requirements for a ready-made, transportable cutting edge and raw-material supply. There is no contextual evidence pointing in this direction, but one should not exclude the possibility that handaxes as such or especially carefully made specimens had special meanings in Middle Palaeolithic society. There are artifacts showing a beginning of regionalisation of material culture in the later Middle Palaeolithic—the differentiation of a Central European province with *Blattspitzen* and a Western European one lacking them. This is a process that becomes more marked in the Upper Palaeolithic—again, a gradual rather than an abrupt transition.

I think that we are confronted with Neandertals as essentially non-modern and that this is of great importance for our study of their cultural remains. This view is corroborated by the fact that Mousterian interassemblage variation cannot be well understood in terms of ethnoarchaeological analogy. The organisational and behavioural system of Neandertals must have been quite different from those of modern hunter-gatherers. The solution to the Mousterian problem calls for "independent" prehistoric reasoning. Perhaps Neandertals did not