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Rethinking the Middle/Upper Paleolithic Transition [and Comments and Replies]

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# Rethinking the Middle/Upper Paleolithic Transition<sup>1</sup>

by *Randall White*

DESPITE ITS APPARENT IMPORTANCE, the Middle/Upper Paleolithic transition in Western Europe has been the subject of very little serious and informed debate (but see Bordes 1958; S. Binford 1968; Klein 1969; L. Binford 1973; Mellars 1973). I shall attempt here to reassess past statements concerning behavioral shifts across the transition, pointing out inadequacies in previous formulations and bringing recently assembled data to bear on the problem. In the course of a detailed study of Upper Paleolithic settlement in the Périgord region of southwestern France (White 1980), it became apparent to me that a number of published characterizations of the Upper Paleolithic could be questioned. The following discussion, then, is biased toward the Upper Paleolithic and toward southwestern France, although some observations from other areas are included. It must be emphasized that this is not an attempt to use southwestern France to generalize about the Middle/Upper Paleolithic transition everywhere. Rather, the goal is to use the Périgord data base to elicit comment and thought from scholars working in a number of areas of the Old World.

The first real synthesis of a broad range of data pertaining to the Middle/Upper Paleolithic transition was that of Mellars (1973) for southwestern France. Surprisingly, it stimulated little discussion, despite some obvious weaknesses. Mellars's article is well organized and cogently written. It therefore makes an effective baseline from which debate can proceed. I shall begin by summarizing Mellars's observations concerning continuity and change across the Middle/Upper Paleolithic boundary and offering some critical discussion of each.

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## MELLARS'S VIEWS: SUMMARY AND CRITIQUE

### STONE-TOOL TECHNOLOGY

Following the lead of Bordes (1958), Mellars modifies the view that Middle Paleolithic industries are flake industries while Upper Paleolithic industries are blade industries. He further argues that techniques of retouch differ little between the Middle and the Upper Paleolithic. These points are solid and not subject to serious criticism. However, some minor reservations must be expressed concerning Mellars's third point: that there is a rapid development of new tool forms during the Upper Paleolithic, in contrast to the highly conservative nature of Middle Paleolithic forms.

Binford (1973) has properly raised the question whether the Middle and Upper Paleolithic typologies are measuring the same thing or, indeed, are operating at the same level of resolution. De Sonneville-Bordes and Perrot's (1954-56) Upper Paleolithic typology, in its morphological precision, is clearly founded on typochronological preconceptions. In essence, it differentiates very similar stone tools on the basis of previous knowledge that minor morphological differences are of chronological significance. On the other hand, Bordes's (1950) Lower and Middle Paleolithic typology is based on the assumption of a lack of diagnostic morphological change through time; hence, it has far less tendency to split hairs. It may not be going too far to suggest that the Upper Paleolithic typology is specifically designed to monitor morphological change through time and the Middle Paleolithic typology to monitor contemporary quantitative variation. (This possibility will receive additional attention below.) This does not imply that Mellars is wrong, but it does suggest that a firm judgment must await a more precise knowledge of morphological change through time in the Mousterian and a better way of assessing Middle/Upper Paleolithic similarities and differences than by using two quite different typologies.

### BONE-WORKING TECHNOLOGY

Mellars states that there are only a few examples of the shaping of bone, antler, and ivory before the Upper Paleolithic and argues that there is then a rapid development of new forms in these materials. While the second point is undoubtedly valid, reservations can be expressed with regard to the first.

Freeman (1978) documents 428 artificially worked bone fragments in a Mousterian level at Cueva Morín in Spain. He considers more than 60% of these to be deliberate bone tools. Equally important is the fact that the number of artificially

worked bone fragments compares favorably with the total number of stone tools recovered. Because "the greater part of the bone tools . . . can be recognized *only* by the presence of the retouch they bear, and not by the shape of the bone fragment as a whole" (Freeman 1978:32), it is entirely possible, as Freeman suggests, that such tools have traditionally gone unrecognized. Further meticulous excavation and analysis will be necessary to determine whether such bone working is widespread in Middle Paleolithic contexts.<sup>2</sup>

Assuming for the moment that the bone working at Cueva Morín is characteristic of Mousterian bone working in general, it is possible to see some interesting technological differences between the Middle and the Upper Paleolithic. From Freeman's drawings, much of the worked bone from Cueva Morín gives the impression of having been subjected to retouch techniques similar to those used on stone. This leads one to enquire whether we are not seeing at Cueva Morín the early stages of experimentation with a new medium, but still making use of familiar stone-working techniques. In the Upper Paleolithic we may be seeing the application and refinement of more appropriate techniques.

Perhaps more important is the fact that much of the earliest Upper Paleolithic bone and antler working seems to be operating in a novel context which can be most conservatively described as nonutilitarian (in a technological sense). For example, in the Châtelperronian at Arcy-sur-Cure (Movius 1969) there are already a substantial number of "pendants" and decorated objects, betraying a level of *formal* investment not apparent in Middle Paleolithic bone and antler assemblages. The same is apparently true of the early Upper Paleolithic in Central and Eastern Europe (Valoch 1968:358).

#### PERSONAL ORNAMENTS

Mellars contends that artifacts interpreted as personal ornaments are practically restricted to the Upper Paleolithic. He seems in little jeopardy here. There is, in fact, additional evidence from European Russia (Klein 1969:108) to support this claim. In addition to bone and antler, shell and stone were used for ornamental purposes. It is surprising that this difference between the Middle and Upper Paleolithic has not been the subject of more discussion (but see Binford 1968). An argument can certainly be made that, if these are indeed personal ornaments, they can be interpreted as evidence for increasing awareness of personal—and possibly group—identity.

#### SUBSISTENCE ACTIVITIES

Mellars observes that there is a broadening of the subsistence base during the Upper Paleolithic to include fish and birds and that there is much greater emphasis on a single species of animal (almost always reindeer in southwestern France) as the major food resource (table 1). While the first point is apparently

TABLE 1

MIDDLE AND UPPER PALEOLITHIC SITES IN THE PÉRIGORD  
SHOWING DOMINANCE BY EACH MAJOR  
HERBIVORE SPECIES

PERIOD	NUMBER OF SITES DOMINATED BY EACH SPECIES			
	Reindeer	Horse	Bovids	Red Deer
Mousterian . . . . .	28	8	17	15
Upper Paleolithic . . . . .	86	7	6	1

SOURCE: Mellars (1973:260).

<sup>2</sup> In fact, examples of bone working similar to those from Cueva Morín were documented at La Ferrassie (Peyrony 1934:20) and Le Moustier (Peyrony 1930:69).

valid (assuming that larger mammal bones are not differentially preserved), the idea of single-species emphasis is in need of modification, especially in light of recent evidence from other areas (see Campbell 1977).

In nearly all cases, Mellars's perception of species dominance is based on bone counts rather than on minimum numbers of individuals or live-weight estimates. Bone counts by herbivore species do show great differences between Mousterian and Upper Paleolithic faunal assemblages that seem to support the idea of single-species specialization during the Upper Paleolithic, but upon closer examination the presumed dietary specialization proves illusory. The two extremes (specialization/generalization) in Mellars's data are Level 13/14 at Abri Pataud, where 99% of the identifiable herbivore bones are of reindeer (*Rangifer tarandus*), and Levels 8-10 at Combe-Grenal, where, according to Bordes and Prat (1965:35), 17% are of *Rangifer tarandus*, 38% of *Cervus elaphus*, 20% of *Equus* sp., and 25% of *Bos* sp. When Spiess's (1979:214) data are used to make estimates of average adult live weight for Upper Paleolithic herbivore species—*Rangifer tarandus* 97 kg, *Cervus elaphus* 295 kg, *Equus caballus* 350 kg, *Bos* sp. 1,400 kg, and *Capra ibex* 40 kg—and calculations of minimum numbers of individuals are applied to the Abri Pataud assemblage (table 2), expectations of specialization are not met. Further evidence that bone percentages do not translate into live-weight percentages is available from the Magdalenian site of Gare de Couze in the Dordogne (table 3), although the calculations here are limited, especially in the upper assemblage, by the small

TABLE 2

MINIMUM NUMBER OF INDIVIDUALS AND LIVE-WEIGHT  
ESTIMATES FOR LEVEL 14 AT ABRÍ PATAUD

SPECIES	MNI	NO. OF BONES	TOTAL	% OF
			LIVE WEIGHT (KG.)	TOTAL WEIGHT
<i>Rangifer tarandus</i> . . . . .	16	484	1,552	39
<i>Cervus elaphus</i> . . . . .	2	7	600	15
<i>Equus caballus</i> . . . . .	1	4	350	8
<i>Bos</i> sp. . . . .	1	1	1,400	37
<i>Capra ibex</i> . . . . .	1	1	40	1
<i>Rupicapra rupicapra</i> . . . . .	1	1	35	1

SOURCE: Spiess (1979:chap. 6).

TABLE 3

MINIMUM NUMBER OF INDIVIDUALS AND LIVE-WEIGHT  
ESTIMATES FOR TWO MAGDALENIAN FAUNAL  
ASSEMBLAGES FROM GARE DE COUZE

SPECIES	MNI	NO. OF BONES	% OF ALL BONES	TOTAL	% OF
				LIVE WEIGHT (KG)	TOTAL WEIGHT
Lower (main) assemblage					
<i>Rangifer tarandus</i> . . . . .	71	749	95.4	5917	43.8
<i>Bos</i> sp. . . . .	4	17	2.2	5600	41.4
<i>Cervus elaphus</i> . . . . .	2	4	.5	590	4.4
<i>Equus caballus</i> . . . . .	4	15	1.9	1400	10.4
Upper assemblage					
<i>Rangifer tarandus</i> . . . . .	13	204	94.4	1261	38.1
<i>Bos</i> sp. . . . .	1	1	.5	1400	42.4
<i>Cervus elaphus</i> . . . . .	1	1	.5	295	8.9
<i>Equus caballus</i> . . . . .	1	2	.9	350	10.6
Birds and carnivores . . . . .		8	3.7	?	?

SOURCES: Prat (1962), Spiess (1979:214).

NOTE: These calculations do not include the large quantity of fish present (Delpech 1975:table 28); if they did, the percentage of reindeer contribution to diet would be even less.

numbers of bones used as a basis for minimum-number-of-individuals statistics. However, it is worthwhile to enquire whether the bulk of the larger herbivores necessitated the selecting out, by more complete kill-site processing, of a larger portion of faunal elements. Unfortunately, minimum-number-of-individuals data are unavailable for Combe-Grenal. Little can be said on the basis of the bone percentages reported except that, given the great body weight of large bovids, it is possible that Mousterian diet in Combe-Grenal Levels 8-10 was, in fact, at least as specialized as that of the Upper Paleolithic occupants of Abri Pataud and Gare de Couze.

The fact remains that Upper Paleolithic peoples in southwestern France were killing more reindeer individuals than individuals of other species. In agreement with Mellars, I find it difficult to explain this shift to reindeer wholly in climatic terms, although this may be part of the explanation. If climatic change is only a minor contributing factor, we are left to explain the concentrated hunting of a species which provides a per capita meat return lower than that of other available species. Part of this explanation may lie in the ease of kill and predictability of reindeer during migration (Burch 1972). However, the reindeer has other equally important attributes, including high-quality hide and sinew. One of its most important attributes is often overlooked: both males and females carry substantial racks of antler during most of the year. That this attribute was important to Upper Paleolithic peoples is evident in the fact that several sites in southwestern France contain large quantities of shed antler (Jude 1960, Cheynier 1949). This implies that even the supply available from large numbers of killed animals was insufficient for artistic and technological needs. In contrast, Binford (1978a: 481) has observed that antler is one of the rarest components of Mousterian faunal assem-

blages. Given the rapid introduction of antler working at the Middle/Upper Paleolithic transition, it can be asked to what extent the shift to an emphasis on reindeer was based on the need or desire for antler as a raw material.

#### DIMENSIONS OF SETTLEMENTS

On the basis of site dimensions derived from the literature, Mellars claims that the large settlements of the Upper Paleolithic have few if any counterparts in the Middle Paleolithic. This is taken to suggest that local groups were larger in the Upper Paleolithic. These points are problematic.

In the first place, site size can be the product of more than just local group size. For example, Bordes (1975) argues that many sites may be palimpsests of reoccupation. Moreover, Yellen (1977) has shown that areal extent can be strongly tied to length of occupation. Given present stratigraphic capabilities, there are severe problems in defining and delimiting unique occupations. Furthermore, the site dimensions presented by Mellars are not a representative sample of site dimensions in southwestern France. For the Mousterian, he ignores the seemingly large sites of La Ferrassie and Pech de l'Azé. The biases in his Upper Paleolithic estimates are even more extreme, this time in favor of large sites. This bias is most serious for the Solutrean and Magdalenian, for which he lists no sites smaller than 1,125 m<sup>2</sup>. Figure 1 presents areal-estimate distributions, based on field observations and literature search, for Solutrean and Magdalenian sites in the Périgord. It is obvious, despite the approximate nature of the data, that the vast

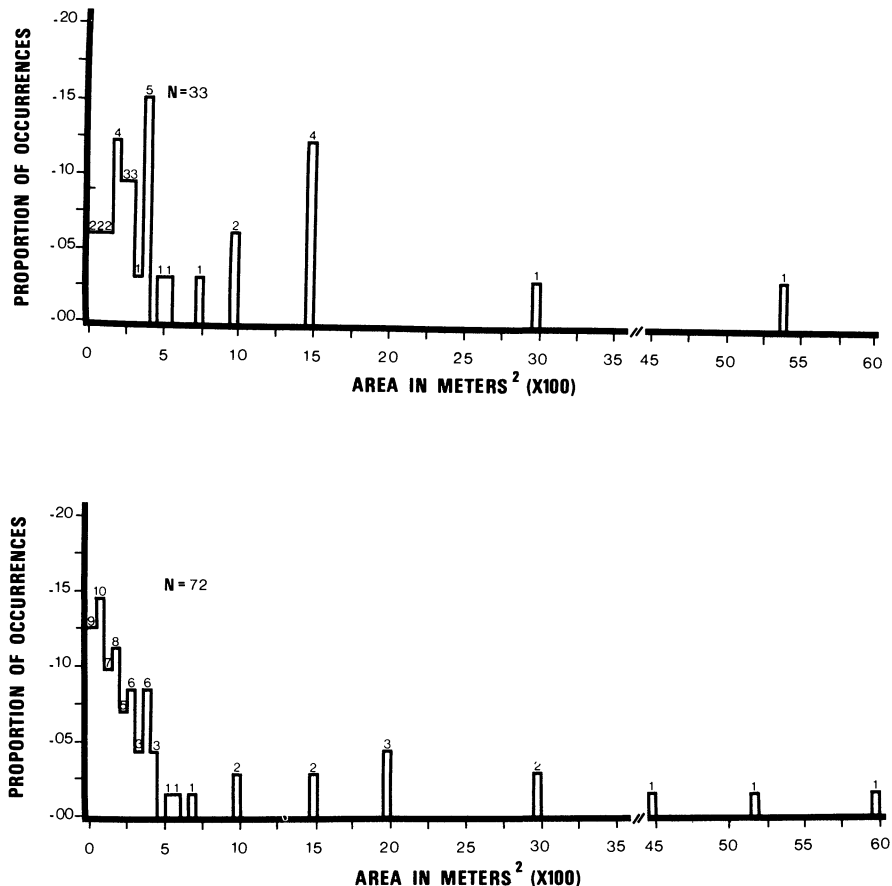


FIG. 1. Areal-estimate distributions for Solutrean (*top*) and Magdalenian (*bottom*) sites in the Périgord. *N* is the number of sites for which area was estimated, not the total number of known sites.

majority of sites during these periods covered relatively little surface area. Large sites are indeed the exception.

To be entirely honest, we simply do not know whether the distribution of site areas in the Middle Paleolithic differs from that of the Upper Paleolithic. However, if Mellars's hypothesis is accepted for the moment, it has interesting implications for recent suggestions concerning Upper Paleolithic demography. Conkey (1980) and I (1980) have argued that large Upper Paleolithic sites may represent aggregation sites for otherwise dispersed local groups. The possibility that such sites do not exist in the Middle Paleolithic has some interesting social implications that will be briefly discussed later.

#### SEASONAL OCCUPATION

Mellars's comments on seasonality of occupation are characteristic of the state of knowledge at the time. His main point is that the more extensive habitation structures found in Upper Paleolithic sites may be indicative of more permanent home bases during this period. However, he emphasizes that, on the basis of Bouchud's (1966) studies of reindeer dentition, no obvious differences in seasonal patterning between the Middle and Upper Paleolithic can be seen.

Recent work by Delpech (1975) and Bryan Gordon (personal communication) seem to indicate seasonal occupation at a number of Upper Paleolithic sites. Moreover, Bouchud's methodology, which seemed to indicate year-round occupation at most sites, has been largely discredited by Binford (1973). Nevertheless, we still lack sufficient data to compare seasonal patterns during the Middle and the Upper Paleolithic.

#### LONG-DISTANCE CONTACTS

Mellars notes an absence of exotic materials, such as marine shells and flint from distant sources, in Middle Paleolithic levels, while such materials are frequent in Upper Paleolithic contexts. Klein (1969:108) states that this is also the case in European Russia. The implications of this pattern are more important than has heretofore been recognized.

It would seem that exotic resources can be obtained in two general ways: by human movement to remote areas and/or by exchange. Both of these means can be taken to indicate structured relations between the human inhabitants of different geographic areas—either access to the domain of others or some form of reciprocity. This of course assumes that the areas from which exotic resources were drawn did not comprise part of the area exploited during the annual round. It is of considerable interest to entertain the hypothesis that such structured relations between human groups were not characteristic of the Middle Paleolithic.

#### POPULATION DENSITIES

Mellars argues for an increase in population density across the Middle/Upper Paleolithic boundary on the basis of site frequencies for the Mousterian and each of the Upper Paleolithic periods in the Périgord (table 4). This approach has some problems.

First, the site-frequency calculations, based on data provided by Peyrony (1949), are erroneous. Had Mellars compared Peyrony's cultural attributions with the far more detailed and reliable ones of de Sonneville-Bordes (1960), the problems with Peyrony's data would have been obvious. Moreover, by using a 25-year-old site inventory, Mellars neglected to include approximately 20 sites. The most serious problem with his site-frequency calculations lies with the Mousterian. His Mousterian figure of 32 sites ignores the numerous Mousterian interfluvial open-air sites in the Périgord (Bourgon 1957; personal observation and survey). My guess is that it is in error by at least a factor of 10. Second, Mellars's unit of analysis is,

TABLE 4  
PÉRIGORD SITE FREQUENCIES ACCORDING TO  
MELLARS (1973:268) AND WHITE

PERIOD	NO. OF SITES (MELLARS)	NO. OF SITES (WHITE)
Mousterian . . . . .	32	not calculated
Châtelperronian . . . . .	12	22
Aurignacian . . . . .	44	62
Upper Périgordian . . . . .	42	41
Solutrean . . . . .	38	42
Magdalenian . . . . .	75	94
Total Upper Paleolithic <sup>a</sup> . . . . .	168	184

<sup>a</sup> The total of sites by period is larger than these figures because many sites contain occupations dating to a number of periods.

of necessity, the site rather than the recognized stratigraphic level. There is little question that the number of sites is not proportional to the number of levels. Mellars is in the unfortunate position of placing deeply stratified sites like Combe-Grenal (with 55 recognized Mousterian levels) on an equal footing with single-component sites. In combination with the underestimation noted above, this makes his figure of 32 inapplicable and misleading.

In ignoring the large number of Mousterian open-air sites, Mellars neglects what may be a significant difference between the Middle and Upper Paleolithic in the Périgord: settlement patterns. Figure 2 shows the distribution of known Upper Paleolithic sites in the Périgord. There is a very obvious bias in favor of river-valley occupation. The abundant Mousterian interfluvial occurrences seem to indicate a striking contrast to this river-valley orientation. It is tempting to consider the possibility that this is related to the differences in subsistence emphases noted above. However, there are severe problems arising from traditional sampling strategies.

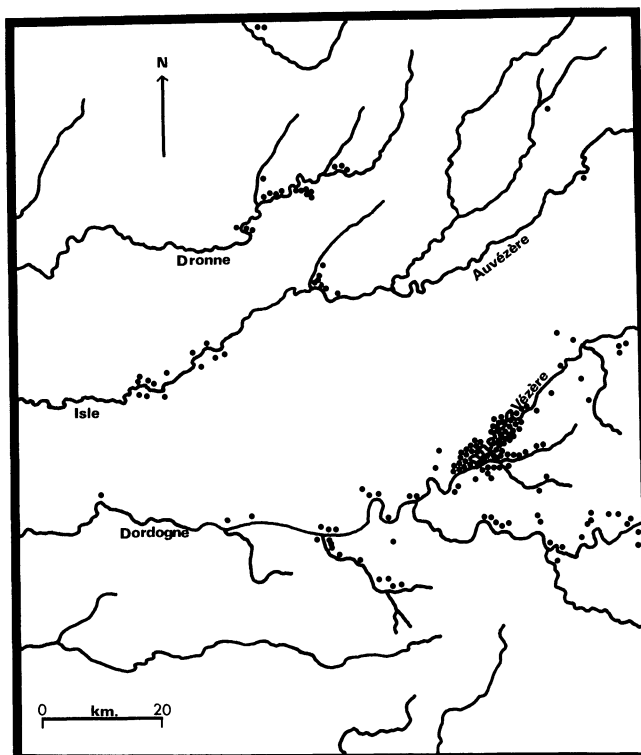


FIG. 2. The distribution of known Upper Paleolithic sites in the Périgord.

It is obvious that, before much more can be said on this topic, systematic surveys must be carried out to determine the kinds of biases built into the traditional search for Paleolithic sites. Moreover, detailed geological study is necessary to determine whether the absence of Upper Paleolithic sites in interfluvial areas is the result of geological factors (for example, an erosional hiatus). It is nevertheless interesting to note that Marks (1979) has argued for a shift in settlement patterns across the boundary in the Levant.

Mellars seems to ignore the fact that preservable forms of art are characteristic of the Middle/Upper Paleolithic transition. Conkey's (1978:74) thoughts on this subject are relevant here: "Essentially, I am suggesting that after 70,000 years ago—if not later, that is, after 40,000 years ago—an 'explosion' of symbolic behavior took place, involving the development of style among and within human groups that enhanced the processes of sociocultural integration and differentiation."

Before leaving Mellars's work, it is necessary to note a somewhat disturbing tone to his arguments. For example, with regard to technology, he states (p. 258) that "the capacity for devising qualitatively *new* varieties of tools appear to be much more characteristic of the upper palaeolithic stage." In the context of a work which is otherwise non-explanatory, statements such as this seem ultimately to account for the Middle/Upper Paleolithic transition in racial or biological terms. "Inventiveness" and "capacity for devising" are explanations only if it is accepted that biological evolution from *Homo sapiens neandertalensis* to *H. sapiens sapiens* is the prime mover. This appears unlikely in light of Lévêque and Vandermeersch's (1980) documentation of a classic Neanderthal in a Châtelperronian level at Saint-Césaire in southwestern France. It is my contention that such explanations must be employed only as a last resort and only after potential ecological and social explanations have been explored and dismissed.

#### ARGUMENTS CONCERNING INTERASSEMBLAGE VARIATION

Bordes and Binford both seem, ultimately, to argue for a different order of variation in the Middle Paleolithic than in the Upper Paleolithic. The argument seems to be that the great contemporaneous variation during the Middle Paleolithic does not correspond to different geographic areas of the Eurasian continent (thus the Lower/Middle Paleolithic typology is applicable to areas as distant from each other as southwestern France and the Near East), while, in contrast, for any given Upper Paleolithic period assemblages within the same geographic area show a striking homogeneity with respect to relative percentages of different tool types (on the Magdalenian, for example, see de Sonneville-Bordes 1960:467). I shall show that this may be a misconception and that the relative frequencies of tool classes in different assemblages show great variation within a given Upper Paleolithic period.

The procedure involves calculating the relative frequencies of burins and scrapers in all available Magdalenian (table 5) and Aurignacian (table 6) assemblages in the Périgord. This restricts the analysis to tools that have been consistently recognized since the time of Lartet and Christy; it excludes types that have been more recently recognized and that, therefore, may have been differentially represented in the quantitative synthesis of de Sonneville-Bordes (1960). It also excludes types that are extremely sensitive to excavation techniques; for example, Bordes and Fitte (1964) have documented immense differences in microlith recovery depending upon the degree of care in excavation. Another reason for choosing burins and scrapers is that they generally make up a substantial proportion of Upper Paleolithic assemblages. There is no necessary assumption here that burins and scrapers

were functionally different, although this seems a logical possibility.

It is frequently assumed that contemporaneous interassemblage variation in the Upper Paleolithic manifests itself at a very specific level of analysis, i.e., in the frequency or presence/absence of specific tool types. If significant quantitative variation can be shown at a very general level (i.e., tool classes rather than tool types), then it can be argued that we have been glossing over some obvious forms of variation. Figure 3 demonstrates that even at this very general level Magdalenian assemblages, and even Aurignacian ones, for which evolutionary systematics are generally thought to be better understood, show wide variation. However, as the keyed plotting of the graph indicates, this variation does not follow traditional chronological lines.

Magdalenian scraper percentages range from 11.0 to 54.0 and, correspondingly, burin percentages from 46.0 to 89.0. Some of this variation probably has to do with excavation and sampling techniques. For example, the high scraper values for Fournieu-du-Diable and Liveyre almost certainly reflect a mixture with closely underlying scraper-rich Solutrean and Aurignacian levels. However, this argument does not hold for most of the other assemblages listed; only Solvieux, Chez-Galou, and Lestruque contained non-Magdalenian levels susceptible to mixture. This leaves only sampling error, cultural differences, differences in human activities, and temporal fluctuations within the Magdalenian as explanations for observed variation.

Sampling error seems most unlikely where burins and scrapers number in the hundreds or thousands. Thus, it is hard to imagine that sampling error can account for the percentage differences between the Magdalenian VI at La Madeleine (39.7% scrapers, 60.3% burins,  $N = 3,756$ ) and the Magdalenian VI at Limeuil (12.6% scrapers, 87.4% burins,  $N = 2,985$ ) or between the Magdalenian III at Laugerie-Haute (41.1% scrapers, 58.9% burins,  $N = 975$ ) and the Magdalenian III at La Forge (25.5% scrapers, 74.5% burins,  $N = 948$ ). It seems that, even when sample size is great, assemblages that have traditionally been attributed to the same phase exhibit substantial differences in lithic assemblage content. While the potential significance of these differences is tempered by the fact that only two tool classes have been considered, it may be predicted that, when a number of well-excavated assemblages become available, frequency differences in other tool classes will be evident. Of course, sample size is only one possible component of sample bias. Comparison of artifact samples drawn from restricted areas of a given archaeological level may be biased by activity-specific concentrations of particular tool types. Given that traditional excavation techniques have not emphasized sampling over large surface areas, we are left to suspect a contribution of this type of distortion to the inter-assemblage variation just documented.

Figure 3 suggests that variation within the Magdalenian and the Aurignacian does not pattern through archaeological time as is currently assumed. Assemblages attributed to the same typologically founded phase are broadly distributed across the range of variation in burin and scraper frequencies. At least four possible explanations, apart from possible sampling error, can be imagined: (1) that the accepted chronology is erroneous and that, given a correct temporal ordering, assemblage variation would pattern directionally through time; (2) that temporal change in assemblage content is fluctuating rather than directional and that the present chronology is more or less accurate; (3) that multiple tool-making traditions, characterized by different artifact frequencies, are present in each Upper Paleolithic period; and (4) that differences in burin and scraper frequencies reflect intersite differences in human activities. (In

fact, it has been shown elsewhere [White 1980] that the smallest Magdalenian occupations in the Périgord [ $<120\text{ m}^2$ ] often exhibit abnormally low scraper frequencies.)

This leaves us to ask whether in fact the Middle and the Upper Paleolithic do differ considerably in the nature of inter-assembly variation. I think that the answer is still yes, but it is possible to argue that differences result from the addition of two new forms of variation to the basic Middle Paleolithic contemporaneous variation.<sup>3</sup>

<sup>3</sup> My earlier warning that the typologies are measuring different things must still be taken into consideration.

First, formal variation through time seems to appear for the first time during the Upper Paleolithic. Second, clear regional differences in artifact morphology seem characteristic of the Upper Paleolithic, necessitating the formulation of a number of regional typologies and making seriation impossible beyond localized concentrations of sites (Collins 1965; see also Kozłowski and Kozłowski 1979 for Eastern Europe). These latter forms of variation have been the focus of Upper Paleolithic typologies because they are diagnostic in terms of time and space. Contemporaneous variation in overall artifact frequencies has been largely ignored, perhaps because it does not pattern coherently in time and space.

TABLE 5  
RELATIVE FREQUENCIES OF BURINS AND SCRAPERS IN MAGDALENIAN ASSEMBLAGES IN THE PÉRIGORD

SITE	N <sup>a</sup>	SCRAPERS		BURINS	
		No.	%	No.	%
Château des Eyzies					
Middle Magdalenian	78	21	26.9	57	73.1
Magdalenian V-VI	134	42	31.3	92	68.7
Liveyre (V-VI)	124	55	44.4	69	55.6
Longueruche					
Magdalenian IV	117	44	37.6	73	62.4
Magdalenian V	243	68	28.0	175	72.0
Limeuil (VI)	2,985	378	12.6	2,607	87.4
Font-Brunel (VI)	152	59	38.8	93	61.2
Soucy (VI)	1,628	319	19.6	1,309	80.4
Fourneau-du-Diable (VI)	171	88	51.5	83	48.5
Mège (V)	138	18	13.0	120	87.0
La Mairie					
Magdalenian V	102	20	19.6	82	80.4
Magdalenian VI	220	37	16.8	183	83.2
Crabillat (II)	261	31	11.9	230	88.1
Jolivet (III)	140	37	26.4	103	73.6
Cap-Blanc (III)	237	53	22.4	184	77.6
La Forge (III)	948	242	25.5	706	74.5
Reverdit (III)	1,064	396	37.2	668	62.8
Solvieux (?)	611	83	13.6	528	86.4
Recourbie I and II (III)	741	198	26.7	543	73.3
Chez-Galou (V-VI)	200	37	18.5	163	81.5
Laugerie-Haute Est (III)	975	401	41.1	574	58.9
La Madeleine					
Magdalenian IV	2,900	1,029	35.5	1,871	64.5
Magdalenian V	2,445	613	25.1	1,832	74.9
Magdalenian VI	3,756	1,493	39.7	2,263	60.3
Villepin					
Magdalenian VI 1	126	50	39.7	76	60.3
Magdalenian VI 2	278	150	54.0	128	46.0
Roc Saint-Cirq					
Red Layer (Middle Magdalenian)	198	50	25.3	148	74.7
Brown Layer (III)	755	295	39.1	461	60.9
Lestruque (Upper Magdalenian)	222	50	22.5	172	77.5
Roc d'Abeilles (VI)	539	109	20.2	430	79.8
Plateau Parrain (Upper Magdalenian)	135	20	14.8	115	85.2
Grand Rochers (III)	145	25	17.2	120	82.8
Rochereil					
Level IIa (VI)	1,514	267	17.6	1,247	82.4
Level IIb (VI)	72	21	29.1	51	70.9
La Gaubert (III)	80	16	20.0	64	80.0
La Caillade (Middle Magdalenian)	174	37	21.3	137	78.7
Mas de Sourzac (Middle Magdalenian)	71	17	23.9	54	76.1
Flageolet II (III)?	173	53	30.6	120	69.4
Gare de Couze					
10-20 cm (VI)	99	22	22.2	77	77.8
22-30 cm (VI)	99	29	29.2	70	70.8
45-55 cm (VI)	79	30	38.0	49	62.0
Jardel II (VI)	453	50	11.0	403	89.0

SOURCES: Font-Brunel, Daniel (1970); Soucy, Daniel (1972); Lestruque, Lenoir (1970); Roc d'Abeilles, Champagne and Espitalié (1970); Plateau Parrain, Bordes and Gausson (1970); Grands Rochers, Blanc (1934); La Gaubert, Daniel (1962); Delage (1923); La Caillade, Gausson (1980); Flageolet, Rigaud (1970); Gare de Couze, Fitte and de Sonnevile-Bordes (1962); Jardel, Jardel and Roussot (1967); all others from de Sonnevile-Bordes (1960).

<sup>a</sup> N = burins (de Sonnevile-Bordes and Perrot Types 27-44) + scrapers (de Sonnevile-Bordes and Perrot Types 1-15). To control for sampling error, only assemblages with N = 70 or more are considered.

TABLE 6  
RELATIVE FREQUENCIES OF BURINS AND SCRAPERS IN  
AURIGNACIAN ASSEMBLAGES IN THE PÉRIGORD

SITE	N <sup>a</sup>	SCRAPERS		BURINS	
		No.	%	No.	%
La Ferrassie					
Aurignacian I.....	1,557	1,297	83.30	260	16.70
Aurignacian II.....	3,491	2,592	74.25	899	25.75
Aurignacian III.....	281	220	78.30	61	21.70
Aurignacian IV.....	382	325	85.05	57	14.95
Laugerie-Haute (V).....	1,230	701	57.00	529	43.00
La Faurelie I (II).....	425	271	63.75	154	36.25
Abri Lartet (I).....	549	449	81.80	100	18.20
Abri Poisson (I).....	310	237	76.45	73	23.55
Abri Cellier					
Aurignacian I.....	184	151	82.05	33	17.95
Aurignacian II.....	296	211	71.30	85	28.70
Le Moustier.....	95	76	80.00	19	20.00
Abri du Renne.....	344	248	72.10	96	27.90
La Metairie.....	237	196	82.70	41	17.30
Abri Blanchard (I).....	313	288	92.00	25	8.00
Abri Castanet					
Aurignacian I.....	1,395	1,339	96.00	56	4.00
Aurignacian II.....	859	826	96.15	33	3.85
Le Patary (I).....	147	131	89.10	16	10.90
Caminade-Ouest					
Aurignacian I.....	125	101	80.80	24	19.20
Aurignacian II.....	252	143	56.75	109	43.25
Caminade-Est					
Aurignacian I (G).....	93	58	62.35	35	37.65
Aurignacian I (F).....	132	105	79.55	27	20.45
Aurignacian II (?) (D <sub>2i</sub> ).....	185	82	44.30	103	55.70
Aurignacian II (?) (D <sub>2a</sub> ).....	120	49	40.85	71	59.15

SOURCE: de Sonneville Bordes (1960).

<sup>a</sup> N = burins (de Sonneville-Bordes and Perrot Types 27-44) + scrapers (de Sonneville-Bordes and Perrot Types 1-15). To control for sampling error, only assemblages with N = 70 or more are considered.

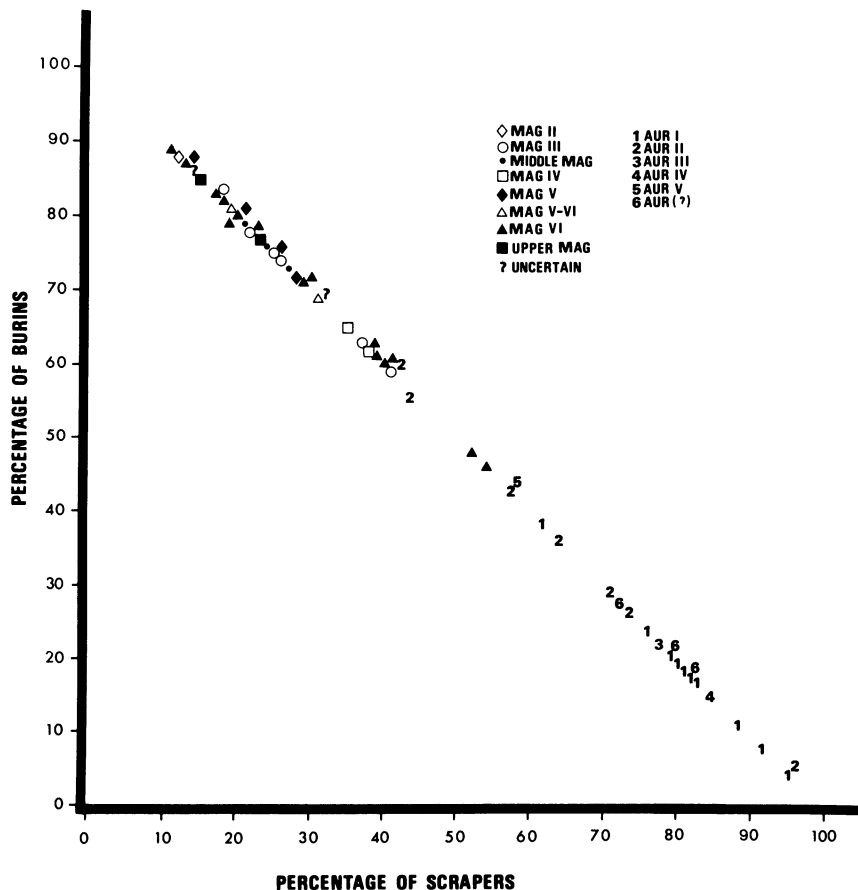


FIG. 3. The distribution of relative frequencies of burins and scrapers for Magdalenian (Phases II-VI) and Aurignacian assemblages in the Périgord (after data in tables 5 and 6).



In searching for a thread to tie together all of these seemingly disparate trends, it is intriguing to consider Binford's (1968) conclusions from a comparison of Middle and Upper Paleolithic modes of disposal of the dead. Across the Middle/Upper Paleolithic boundary, she finds convincing evidence for an increase in the degree of corporate involvement in mortuary ritual and increasing use of means of symbolizing the status of individuals. Moreover, L. Binford (1973:237) questions whether ethnicity as we know it was present among Mousterian populations and elsewhere (1972:291) speaks of increasingly "complex cultural geography" during the Upper Paleolithic. Ranov and Davis (1979:256) give credence to this latter idea and cite the ongoing debate on the matter in the Soviet literature. The greater cultural complexity of the Upper Paleolithic finds further support in Central Europe, where, for example, Kozłowski and Kozłowski (1979) have identified ten regional variants of the Central European Gravettian. These ideas lead us to question whether many of the trends discussed here cannot also be accounted for in social rather than natural-environmental terms.

It is worthwhile to review these trends. The reader is asked to tolerate, for the sake of argument, the interpretations implicit in the following list:

1. The Upper Paleolithic is possibly characterized by greater population density than the Middle Paleolithic.

2. There is a possible absence of regular social aggregation during the Middle Paleolithic, in contrast to its probable presence during the Upper Paleolithic.

3. During the Upper Paleolithic there is a greater stylistic<sup>4</sup> component to lithic artifacts. This component is patterned in both space and time and is imposed on contemporaneous variation in artifact frequencies. Regional variation in tool morphology, evident from the beginning of the Upper Paleolithic<sup>5</sup> (Valoch 1968; de Sonneville-Bordes 1961, 1963, 1966), is of particular interest. It is not unreasonable at least to hypothesize that these morphological differences are playing a significant role "in information exchange, as, for example, in the symboling of territory or social boundaries, in the context of ritual, in the support of ethnicity, or in maintaining and strengthening mating networks, exchange relationships, and structural poses" (Wobst 1977:320).

4. During the Upper Paleolithic, there is a far greater emphasis on the working of antler and bone, which is characterized by the imposition of formal standards possibly communicating individual and/or corporate identity by means of purposeful stylistic variation (Conkey 1978). Such may also be the case with the emerging importance of parietal and mobiliary art.

5. There is a dramatic shift toward the hunting of large numbers of individuals of a herd species in which both sexes carry antler for the majority of the year.<sup>6</sup> Interestingly, individuals of this species provide a smaller per capita meat yield than individuals of other available species.

6. The Upper Paleolithic sees the first indications of the use

<sup>4</sup> For present purposes, I prefer Kroeber's (1976:125) definition of style, presumably based on ethnographic observation: "a way of achieving definiteness and effectiveness in human relations by choosing or evolving one line of procedure out of several possible ones, and sticking to it."

<sup>5</sup> For example, the regional "cultures" identified for the earliest Upper Paleolithic in Europe include the Uluzzian, the Châtelperronian, the Altmuhlian, the Szeletian, the Jermanovician, and the northwestern European leaf-point industries.

<sup>6</sup> In more southerly latitudes of Western Europe, reindeer were not available. Moreover, in Spain it is difficult to detect any striking differences in faunal exploitation across the Middle/Upper Paleolithic boundary (Freeman 1973).

of personal ornaments capable of communicating individual or corporate identity.

7. At the beginning of the Upper Paleolithic, humans began obtaining materials from distant sources, presumably through structured exchange and/or with the approval of groups inhabiting distant locales.

All of the above is consistent with the idea of a total restructuring of social relations across the Middle/Upper Paleolithic boundary in the course of which corporate and individual identity become important and are enhanced by stylistic input and regional differences in the working of stone, antler, and bone, the fabrication and wearing of ornaments, and the regular aggregation of a set of otherwise dispersed local groups. The desirability of antler as a communicative and technological medium is reflected in a shift to the hunting of large numbers of reindeer. Structured relationships between the inhabitants of different geographic areas are evident in the presence of materials from distant sources. The possible increase in population density is a tempting prime mover in this hypothetical restructuring, but without more reliable demographic data this possibility can be taken no farther.

There are undoubtedly other constructs which would account for the "facts" equally as well as the house of cards that has been built here. For the moment, however, this construct has the advantage of avoiding both biological and environmental determinism. Moreover, it raises some important questions, not the least important of which are the following:

1. To what extent are material manifestations a reflection of nonmaterial aspects of Paleolithic culture?

2. Accepting what has been suggested here, what was the nature of social relations during the Middle Paleolithic?

3. To what extent did Upper Paleolithic survival become a matter of human groups' adapting to the presence of other human groups?

## Comments

by NICO ARTS

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7 x 81*

White's critical discussion of Mellars's (1973) analysis provides a useful and stimulating base for further study of both the Middle/Upper Paleolithic transition and the Paleolithic in general. In broad outline I agree with his criticism, but I do have two points of comment.

White leaves open the possibility that aggregation sites occurred in the Late Paleolithic and not in the Middle Paleolithic, which might have some interesting social implications. I want to take this opportunity to warn that we still do not know how an aggregation site might be recognized archeologically. We might appeal to the ethnography of hunter-gatherers, but there is a general lack of useful descriptions.

There seem to be two kinds of aggregation sites. The first kind consists of a moderately extensive area of closely grouped dwellings, between which there is an area in which daily activities are performed. Examples are given by Yellen (1976:63-64); this is what archeologists mean by "aggregation site." The second kind is larger than the first, and in it the dwellings are far apart. Activities take place around the separate dwellings, and between them there is an extensive area which may not be used at all. Examples are given by Gould (1980:25), Silberbauer (1981:197), and van den Steenhoven (1964:57). Translating the second type into archeological terms could have serious consequences for the recognition of Paleolithic aggregation sites (Arts and Deeben 1981:chap. 5). Concentrations of Paleolithic artefacts might indicate only part of an aggregation

site, other parts of which might be situated dozens of metres away and might either not be noticed during the excavation or considered as separate settlements. It is difficult, if not impossible, to speak about Paleolithic aggregation sites when we do not even know what a subrecent aggregation site looks like.

My second point of comment is that there do seem to be some fundamental differences between the Middle Paleolithic and the Upper Paleolithic, and one lack in White's stimulating article is a concrete contribution to the explanation of these differences.

by PAUL G. BAHN

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White's paper is a timely outline of a subject which has just entered a new phase owing to the finds at Saint-Césaire. The Châtelperronian is emerging as the crucial phase of transition rather than merely the first stage of the Upper Palaeolithic. Although it was a good idea to examine the transition in terms of the various aspects discussed by Mellars, it would have been useful—since White is writing primarily about France—to start with a summary of earlier French work on the problem, such as that of Leroi-Gourhan or Delporte (1963, 1970). Moreover, in those sections devoted to stone tools, some attention could profitably have been given to the alternative views and typological method of Laplace (1966).

The suggestion that deer became important to man because of their antlers was also made by Lantier (1974), who believed that, in the Upper Palaeolithic, meat was obtained principally from bovinds and horses, whereas cervids were exploited for antler, skin, and sinew. Unlike White, however, I find it very hard to believe that a desire to adopt antler as a "communicative technological medium" (why were bone and wood not sufficient in this role?) led to a radical change in economic strategy. This is clearly a "chicken-and-egg" problem, since it could equally be argued that antler working arose on a large scale because new economic strategies based on exploitation of gregarious cervids led to the amassing of great quantities of this previously neglected raw material. Both arguments are simplistic, and the truth, as usual in prehistory, probably lies in a complex combination of these and other explanations.

It is true that, in southern France, Middle Palaeolithic sites tend to display economies that are based on two or three large herbivore species, but in the Pyrenees (Bahn 1979, n.d.) there is an important open site of the period at Mauran which contains almost exclusively the remains of adult bovinds, though it is not yet possible to claim that these formed the basis of the economy throughout the year. A very similar site exists at Livernon, to the north in Quercy.

It is also true that much material has been found in open-air interfluvial sites, but, in the Pyrenees at least, most of these are surface finds and therefore often undatable. The material is more safely described as Mousteroid and is known to occur in all periods of prehistory: crest routes have always been well-frequented, and I therefore feel that Mellars was wise to omit such sites from his calculations. Surface finds are always difficult to include in any estimate of site numbers. A further complication—again, in the Pyrenees—is that very few Upper Palaeolithic sites have been dug down to bedrock, since early excavators were often primarily interested in Magdalenien art objects: thus there may be many more pre-Magdalenian and Mousterian sites than we know (Bahn n.d.), a fact which must be taken into account in estimates of site numbers and demographic trends. In fact, the rate of population increase in the Palaeolithic seems to have been extremely slow.

It is hard to prove the presence or absence of aggregation sites in the Mousterian, since indicators of movement and contact such as seashells or art objects are totally lacking.

Proofs of intersite and interregional contact are numerous from the Aurignacian onwards, and I have argued (1977) that they can often be explained in terms of seasonal migration linked to subsistence strategies; but it is of course probable that many other factors, such as trade and exogamy, were involved in the movement of objects.

With regard to ornamentation, it is possible that decorative objects of perishable materials existed in the Middle Palaeolithic, but it is nevertheless intriguing that the first evidence of exotic objects occurs in the early Upper Palaeolithic. In the French Pyrenees, for example, the earliest item of this type yet published is a seashell from the Châtelperronian layer in the cave of Gargas (Bahn 1979)—though it can no longer be assumed automatically that Cro-Magnon man was responsible for its presence.

What credibility, then, can still be attached to a Middle/Upper Palaeolithic boundary? Its traditional position lies between the final Mousterian and the Châtelperronian, but in many regions such a limit is completely artificial, since there is clear and strong continuity between the two in terms of site locations, subsistence, and stone-tool typology. The human anatomical boundary at this point was little more than supposition and must now be displaced. Hence, the only discernible innovations at present are the rise of ornamentation, artistic representations, and formalised bone tools. This is why the most fruitful avenues of research on the transition seem to lie in the social sphere and its interaction with subsistence practices.

by LEWIS R. BINFORD

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White's timely and well-organized review of advances in data base and thinking relative to the Middle to Upper Paleolithic transition is welcome. I will arrange my comments in terms of some of his topical headings.

*Bone-working technology.* White accepts Freeman's claim that worked bones in considerable numbers are present in Mousterian Level 17 at Cueva Morín. The specimens illustrated by Freeman (1971, 1978) are without exception indistinguishable from canid-gnawed bone fragments. I have illustrated many "pressure-flaked" bones with the properties he describes, including the diagonal scoring on the external surface (the "pressure flaking" tends to be on the internal face). I have described this scoring as diagnostic of canid gnawing, since it is produced when chips give way on the inside of the bone and the tooth slides down the external face. Most of the "flaking" is produced by animals during the course of collapsing a bone cylinder (fig. 1). Once the cylinder splits longitudinally, the "pressure flaking" remains on the ends of the splinters (see Binford 1981: figs. 3.19, 3.20). Bones similar to those described by Freeman have been reported by Dart (1960:5) from Makapansgat, by Breuil (1939) from Chou-kou-tien, and by Veyrier and Comber (1952) and de Lumley (1969) from ancient sites in France. In all these cases, the "manufacturers" were most certainly gnawing animals. (For comparative illustrations, see Binford 1981: figs. 3.09–3.17.) The "worked" bone reported by Freeman is relevant to the Mousterian only in that it betrays the common use of sites by man and other animals. We need to know much more about the factors conditioning the associations between man and evidence of carnivore occupation at the same sites; there are apparently strong geographic variations in such associations. Freeman's misidentifications obscure this interesting research area rather than opening up "traditionally . . . unrecognized" accomplishments by men of the Mousterian era.

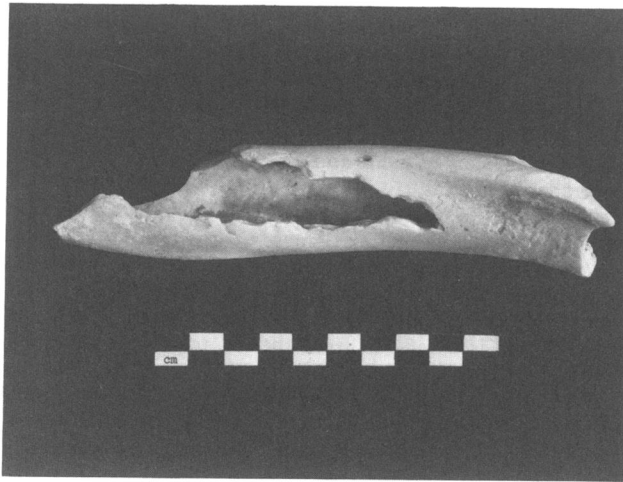


FIG. 1. Channeled bone gnawed by wolves.

*Subsistence activities.* White challenges some of the generalizations offered by Mellars regarding the Upper Paleolithic hunting "specialization" on the basis of recent faunal summaries by Campbell. What he ignores in citing Campbell's summaries is that in only one case (Campbell 1977:75) is there any suggestion that the artifacts referable to human occupation are not also mixed with the remains of carnivore lairs. Judging from the few tools (7,000–9,000) from the Upper Paleolithic in Great Britain, most of the animal remains observed at the sites where these tools were obtained almost certainly refer primarily to the subsistence activities of animals, not man. I am not surprised that the faunal summaries look like the behavior of predatory generalists.

White further points out that Mellars compares bone counts rather than minimum numbers of individuals, arguing that when MNI's are used the apparent specialization in the Upper Paleolithic "proves illusory." His example, the work of Spiess (1979), is misleading; Spiess assumed that man consumes animal foods in living-animal units, and this is demonstrably not the case most of the time (Binford 1978a:69–72, 478–79). Parts of animals are discarded, transported, stored, and consumed differentially. This means that the presence of a mandible on a site does not mean that a whole animal was consumed there, but probably only that a mandible with attached tongue was used there. For Spiess, "a minimum count of one individual . . . was often made upon the basis of a scrap of one piece of bone" (p. 183). As I have pointed out, this is a fairyland exercise (Binford 1980b:630).

It is true that there are situations in which only a few bones might be transported yet a substantial amount of meat might be introduced to a site—for example, when the meat was stripped from the bones in order to reduce the transport load and the bones discarded at the kill or processing location. The anatomical parts which would be transported in such a situation (Binford 1978a:107–9, 285, 238–45) would be parts of moderate utility with a low meat-to-bone ratio, for example, rib slabs. In the cases cited by White as possibly indicative of a generalist's diet, the parts of large animals represented are predominately from the head (see Bouchud 1975). This is far from a high-utility part, and certainly not one for which a numerical bias could be expected to arise from the differential transport of processed or stripped meat. The pattern of anatomical-part bias from many of the sites where MNI conversions based on limited bone-counts-to-meat estimates have been made appears bizarre if one assumes the parts were transported incidentally to the introduction of all the meat, presumably removed from the bones at the kill site. For instance, for Level 14 at the Abri Pataud, using a MNI convention leads to the estimate of

1,400 kg of meat, said to represent 37% of the total meat diet, contributed by auroch. This estimate is based on the presence in the level of one upper M2 (Bouchud 1975:134), while reindeer, said to represent 39% of the diet, are represented by 1,481 bones (Bouchud 1975:134). Clearly, a large number of hidden assumptions must stand behind such an inference. We must discard such simple conventions and seek to understand the conditions which might have generated the archaeological patterns.

Hunters and gatherers do not begin eating an animal at the nose and proceed to the tail; they segment the animal and differentially consume parts in different places and at different rates. The archaeological record reflects this differential usage, and we cannot ignore it. Further, both ancient and modern hunter-gatherers used various tactics—hunting, trapping, and scavenging from both the natural death sites of animals and the kill sites of other predators—and at least the latter two are associated with a complicated set of interactions with other predator-scavengers. To equate a set of lower legs and a skull scavenged from the death site of a large bovid with the usable meat in a live animal is naive at best. We must have the methods for identifying the food procurement tactics represented by a faunal assemblage before assuming that all animals were hunted and, further, that all usable parts of hunted animals are represented by a single molar tooth.

Is there any evidence for a contrast between Upper and Middle Paleolithic in degree of hunting specialization? Techniques for identifying hunting tactics are just beginning to appear and have not yet been applied to the relevant materials. This means that, while we may appreciate the inadequacy of interpretive conventions, we must, nevertheless, fall back on impressions and judgments. It is my judgment that, prior to the Upper Paleolithic, (a) scavenging of large body-sized animals was a regular and important part of the food-procurement strategies, (b) gregarious and migratory animals such as reindeer were hunted as if they were territorial game (that is, I see no evidence for large mass kills, but only for the killing of individuals and resumption of hunting after short periods of consumption); and (c) storage of meat was not a regular part of the subsistence strategy. This latter is important because hunting specialization is in my opinion generally linked to storage. Specialization is commonly linked to the periodic aggregation of a species, which renders it a prime target. This is an optimal condition for putting up stores if the schedule of the prey localization and aggregation can be synchronized with the onset of the nongrowing season and reduced temperatures which make storage of meat more reliable (see Binford 1978a: 91–133; 1980a).

Storage contrasts, if they can be demonstrated, appear to me to be symptomatic of a still more provocative contrast between the earlier time ranges and the Upper Paleolithic in its "modern man" manifestations. Early adaptations appear to me to be based on tactics which do not require much planning ahead (that is, beyond one or two days); in addition to the absence of storage (assuming for the moment that my impressions are correct) there is an absence of curated technologies (Binford 1976, 1979) and of the tactical use of such resources as salmon, the exploitation of which in large quantities requires the anticipation from one year to the next of spawning runs, etc. Perhaps of similar relevance is early populations' inability to penetrate the Eurasian steppe, where both storage and the anticipation of herd movements would seem prerequisite to adaptation. It is my impression that the *ability to anticipate events and conditions not yet experienced* was not one of the strengths of our ancestors prior to the appearance of clear evidence for symboling, e.g., personal ornaments, graphics in the form of painting, "art," and "notation" (Marshack 1972), graphic decoration of other things, including other persons, etc., things which mark the appearance of "culture" as we know it (cf. White 1949:363). In my opinion the appearance of

symboling is a major change and not one which can be understood by seeking exclusive "social explanations."

White points out that antler is very common in Upper Paleolithic sites, suggesting that this can be understood in terms of an increased demand for antler as a raw material for "artistic and technological" productions. The view that "the desirability of antler as a communicative and technological medium is reflected in a shift to the hunting of large numbers of reindeer" seems to be forcing a "social explanation," to say the least. In the first place, shed antler is almost useless for the manufacture of most kinds of bone tools requiring strength and resiliency. Secondly, a group hunting reindeer for food, particularly one with storage, would have had more antler than it could use for the production of the relatively minor antler-bone component known from Upper Paleolithic sites. This lack of scarcity is illustrated by the variety of functions served by unmodified antler around sites of reindeer-hunters (Binford 1978a:117, 480-81). Far from a situation in which one kills reindeer to get antler for purposes of communicating, the high frequency of antler reflects its abundance where hunters are exploiting reindeer as a focal resource. It is much more likely that the comparative absence of antler in Middle Paleolithic sites reflects the absence of a focus on reindeer.

The societies of early hominids cannot be understood by projecting backwards from what is in many cases a poor understanding of modern hunter-gatherers. This is a point made recently by a number of writers (e.g., Conkey 1980:610). My research on temperate- and cold-environment adaptations will, I feel, greatly aid our understanding of the Upper Paleolithic, but it is only by way of contrast that it directly illuminates the Middle Paleolithic. Figuring out how the Middle Paleolithic was organized remains one of our greatest challenges. I am quite certain that Bordes's arguments about ethnicity are wrong, but I am equally convinced that functional arguments projected from modern logistical hunter-gatherers are likely to be inaccurate.

*Dimensions of settlements.* White is correct in pointing out that we have no reliable and unbiased data with which to evaluate the impression that Middle Paleolithic occupations were by small groups for short durations. (It should be kept in mind that some have thought they were large, essentially sedentary groups [Bordes 1968:144].) It remains my impression that Middle Paleolithic sites are generally palimpsests composed of numerous small and short-duration episodes of occupation. In the materials known from northern Europe, even the recognizable "levels" are small and yield small inventories. Analysis of the Combe-Grenal data has convinced me that I can demonstrate the composite character of those assemblages, and I will soon present a series of arguments regarding site structure pointing to the conclusion that the Mousterian assemblages as known from Combe-Grenal are *all* palimpsests. My analysis of site-formation observations regarding logistically organized hunter-gatherers (Binford 1982) shows that there should be assemblage heterogeneity arising from changes in the relative economic potential of different places as a simple consequence of the movement of residential hub locations. This should result in variation even within deposits accumulated under the same general pattern of site use. This type of variation is *not* demonstrable when relatively thick levels like K, L, and M from Combe-Grenal are partitioned vertically and within- and among-sample variance measures calculated; these Mousterian levels are internally homogeneous. I cannot see how this could occur if hunter-gatherers organized into minimally differentiated residential and logistical locations were responsible. A further implication of this pattern is that there are no relative changes in position or activities within a region during long periods of occupation. Given current understanding, this implies that all sites were residential—like those of foragers—or that there was no fine-grained response by the hominids to minor environmental dynamics within their habitual ranges. Either

way, the existence of very different forms of social and economic organization within the Middle Paleolithic is implied. The task remains to figure out what these forms were. Nevertheless, I would be very surprised if Mellars's generalizations were to fall as we develop better methods for inference; both the variance in and the absolute size of settlement achieved are in my opinion greater in the Upper Paleolithic.

White speculates that "aggregation sites" may distinguish Upper from Middle Paleolithic. Upper Paleolithic settlement systems were very different from those of the Middle Paleolithic, but the extent to which the concept of "aggregation site" is useful for describing the nature of the differences is another matter. For instance, Conkey (1980:612) tells us that "an aggregation refers to the concentration of individuals and groups that are otherwise fragmented." She recognizes that the archaeological manifestations of aggregation sites will vary with the "conditions under which aggregation takes place," among them duration of occupation, spatial extent of an occupation, personnel, and context. To my way of thinking, these conditions can be expected to be relevant to almost any type of settlement contrast one might choose to make. The degree to which variation in these conditions permits one to discriminate between an aggregation site and other types is not at all clear. Conkey recognizes the problem (p. 612) when she states that "the real methodological challenge . . . is to develop the test implications for each combination of possible conditions under which an aggregation might take place." Obviously unable to do this, she tells us that "relative diversity is the key; even if a considerable range of activities is indicated we must still investigate the degree to which intrasite variability is greater or less than that between sites, between levels in a site, or between regions." This seems to me to be an unsatisfactory approach. Nowhere is the argument warranted that there is a necessary connection between aggregation and diversity. Many other contexts not particularly associated with aggregation could result in regular and regionally patterned differences in diversity. The potential implications of differences for variations in social organization and settlement system are provocative, but the methods for recognizing such differences from archaeological remains have not yet been developed.

*Population densities.* White is correct in pointing out that we have no reliable instruments for measuring even relative population densities across the Middle-to-Upper Paleolithic boundary. Again he is prudent to suggest that the settlement patterns are apt to have been quite different. He fails, however, to acknowledge that even if it were demonstrated that there were absolutely more "Mousterian" sites, particularly undated open-air sites, it could still be quite likely that the Upper Paleolithic sites represented greater numbers of people. The duration of assemblages typologically referable to the Mousterian from geologically undated contexts is not at all clear. According to the French convention, the Mousterian is an assemblage from the first two stadials of the Würm glaciation; assemblages of Riss age are by definition Acheulian, even when typologically indistinguishable from the Würm assemblages. Thus typologically recognized "Mousterian" assemblages may span as much as 200,000 years, while the Upper Paleolithic sites represent 25,000 to 30,000 years at most. From this perspective, White's objections to the accuracy of Mellars's sample become somewhat less critical to the point at issue.

*Differences in interassemblage variation.* White rightly points out that Bordes and I have both recognized "something different" about the Upper Paleolithic patterning relative to that of the Middle Paleolithic. From the very beginning, the regionally correlated variation seemingly indicated by at least some of the Upper Paleolithic systematics has been much more consistent with the generalizations of previous workers (Wissler 1914, Kroeber 1953) pointing to geographical continuity in

material-culture "stylistic" markers. At the time of many of my arguments with Bordes I was also aware that when "assemblages" were looked at in a configurational sense—that is, when all tool types were taken as of equal value and the only characteristic deemed important was the pattern of overall assemblage composition—this picture of regional continuity tended to break down. Different *conventions* in comparative study produced very different pictures of the past.

For instance, Winters (1963*a, b*, 1969) investigated a variety of sites in the Wabash River valley from which the same types of projectile points were regularly recovered. Sites in the same region yielding similar projectile points and/or pottery were conventionally considered as produced by the same or culturally similar peoples, and any differences between them were accepted as referring to conditioners other than cultural identity. The range of variation observed by Winters among sites coming from the same region and judged to be of the same cultural unit is shown in table 1. Bone and shell preservation were good in all of Winters's sites, and therefore it was difficult to visualize the percentages given in the table exclusively in terms of stone tools; nevertheless, it is clear that frequencies among sites vary widely. These differences were shown to covary largely with indicators of seasonal differences, and consequently Winters interpreted them as referable to seasonally variable structural poses coupled with some seasonal mobility.

Again, Judge (1973) found considerable variation among the assemblages recovered from Rio Grande Valley sites yielding typologically similar projectile points; for example, some sites yielded as low as 2% projectile points and others as high as 23%. Difference of this magnitude is sufficient to qualify the assemblages as different "cultures" by European standards (see Bordes and de Sonneville-Bordes 1970:62–64), yet all the sites were of the Folsom culture, accepted as a "culture" by Bordes himself (1968:216). Like Winters, Judge (1973:318–39) demonstrated a fair correspondence between types of assemblage and the geomorphology of the site locations. This would support the view that the differences in assemblage content and the differential placement of sites of different form could be taken as indicative of similar persons doing different things in different places.

Support for this interpretation can be drawn from a very different pattern in the archaeological record. Irwin and Wormington (1970) described and compared chronologically distinct "cultures" defined by projectile-point styles recovered in stratigraphic order from the Hell Gap site near Gurnsey, Wyoming. This study is of the greater interest for its attempt to utilize the taxonomic principles developed by Bordes (1950,

1961*a*) for describing the assemblages. Essentially no differences were found between the assemblages associated with Clovis, Folsom, and Midland points. Similarly, there was little difference between the assemblages associated with Hell Gap and Agate Basin points and between Frederick and Cody assemblages. Indeed, by European standards there was very little difference between any of these groupings. There was not as much variation among the assemblages at Hell Gap, regardless of "culture" or time period, as was demonstrated by Judge among assemblages found in association with a single culture as defined by projectile-point styles, yet at Hell Gap the assemblages were associated with very dissimilar projectile-point styles.

Still a more recent example of the seeming lack of assemblage variation in spite of variation in artifact styles in terms of the "typological method" (Krieger 1944) has been described by Vierra (1975). Vierra showed that there was no significant change in the relative frequencies of artifact classes (e.g., end scrapers, projectile points, differing forms of side scrapers, etc.) through a long stratigraphic sequence at Puente (A.D. 158) in the Ayacucho Basin of highland Peru. This was true in spite of considerable directional style shifts in projectile-point types and in subsistence base for the system as a whole as seen in a regional perspective. (The sequence spanned the origins of agriculture.)

What these studies demonstrate is that when one's typology is developed to measure variation (see Krieger 1944) of the kind characteristic of New World ethnographic data (Kroeber 1953, Wissler 1914), variation as measured by assemblages summarized in ways similar to the "Bordes method" can be shown to be independent of the patterns demonstrable using the "Krieger method." Assemblages from different sites of the same "culture" are shown to be quite varied in content, while assemblages from the same site but representing different occupational components which were also demonstrably representative of different "cultures" are shown to be roughly the same. There is no escaping the conclusion that variation as measured by assemblage composition conceived in technomorphological terms, as in the Bordean typology for the Middle Paleolithic, does not correspond to variation conceived in ethnic or culture-historical terms. Materials ordered by Bordes "typology" do not vary in the way characteristic of "culture" (that is, little geographical clustering of cultural similarities, with continuity both temporally and spatially between formal varieties of "cultural" products).

It was in the light of all this that I suggested that the varying frequencies among the same artifact types as summarized in assemblage "types" would most likely be informing us about organizational differences arising from the internal dynamics of Middle Paleolithic systems, not from "cultural" differences among systems. Of equal interest was the seeming inability of taxonomists to isolate modes of variation in the Mousterian which patterned according to "cultural" expectations of temporal and spatial continuity. As much as the Europeans tried, they could not find "historical index" types (Steward 1954:54) as had been discussed by Krieger (1944). I suggested that this "failure" was perhaps telling us that culture as we know it was not manifest in Mousterian materials. Certainly the lack of "continuity" patterning is hard to reconcile with a symbol-based mode of adaptation (including language), if it was actually present (Binford 1972:161).

While all the attention was seemingly focused on the meaning to be assigned to Middle Paleolithic interassemblage variation, there were clear implications in our arguments for Upper Paleolithic systematics, particularly as it had been developed by the French. These implications had been recognized by de Sonneville-Bordes (1966), and it was her claim that functional variability between assemblages did not characterize the Upper Paleolithic. If this claim was true, then clearly the "inter-assemblage variation" problem was even greater than imagined.

TABLE 1  
ASSEMBLAGE VARIABILITY BY TYPE OF SITE IN THE  
WABASH RIVER DRAINAGE

SITE CLASS	TOOL CLASS		
	General Utility <sup>a</sup>	Weapons <sup>b</sup>	Fabricating, Processing, and Domestic <sup>c</sup>
Settlement.....	10 ± 5%	15 ± 5%	75 ± 5%
Transient camp.....	15 ± 5%	30 ± 5%	55 ± 5%
Base camp.....	15 ± 10%	40 ± 10%	40 ± 5%
Specialized hunting camp..	25 ± 5%	55 ± 10%	20 ± 10%
Generalized hunting camp.....	55 ± 20%	35 ± 10%	10 ± 10%

SOURCE: Winters (1969:35).

<sup>a</sup> Knives, side scrapers, end scrapers, spokeshaves, choppers, and hammerstones.

<sup>b</sup> Projectile points, atlatl weights.

<sup>c</sup> Flakers, punches, awls, and needles; anvils, drills, perforators, graters, abraders, chisels, and gauges; manos, metates, and shell spoons.



For example, if (1) interassemblage variation, as measured by categories of tools defined in ways similar to Bordes's Middle Paleolithic typology, was demonstrable among relatively recent New World assemblages and (2) this variation patterned in an independent way from variation measured by typical New World "stylistic" typologies, then New World data appeared to be consistent with patterning known from the Middle Paleolithic and earlier time ranges, at least in the form measured by the "Bordes method." If the difference between the earlier and later periods was referable to the absence of "culture," then in the Upper Paleolithic, where clearly "culture" was present, we should see both forms of patterning: (a) continuity patterning, as illustrated by style-based taxonomies, and (b) interassemblage variation independent of stylistic patterning and analogous in a temporal-geographical sense to that seen in the Middle Paleolithic. The French for years maintained that assemblage systematics as developed under the "Bordes method" worked for the Upper Paleolithic and yielded continuity patterning. This demonstration by White that variation among technofunctional classes of tools is substantial and independent of the generally accepted "cultural" systematics brings Old World material back into line with what was already known from the New World.

The implications, however, will not be appreciated by many Old World workers for some time, since his demonstration implies that Upper Paleolithic systematics is not based on the "Bordes method" (de Sonneville-Bordes 1977:19), but, as was suspected, incorporates many "historical index types" (Steward 1954:54) selected because of their recognized historical patterning. When two different things with different conditioning causalities are treated as the same thing with similar causalities, the picture of the world one obtains is distorted and unrealistic. I think we can expect a general deterioration of the seemingly "clean" cultural picture of Upper Paleolithic times in direct proportion to the degree to which the "Bordes method" is the basis for the systematics.

The question of interassemblage variation remains complex. My work has convinced me that most of the dynamics which have been recently explored—curation (Binford 1976, 1979), seasonal variability (Yellen 1977, Binford 1978a), activity differentiation among different sites (Binford 1978a, 1980a), and variation arising from the tempo of site reuse (Binford 1982)—are primarily of direct relevance to logistically organized hunter-gatherers. The Upper Paleolithic societies of Pleistocene Europe were probably so organized. White's demonstration that Upper Paleolithic culture groupings exhibit substantial interassemblage variation is most encouraging. We are gradually building up a body of understanding regarding the processes of site formation among logistically organized hunters which should permit the interpretation of Upper Paleolithic materials.

Our methods of inference are thus far inadequate to provide a picture of Middle Paleolithic systems in organizational terms. We need much more understanding of forager (Binford 1980a) systems and better methods of recognizing the results of subsistence tactics not commonly considered in the past, such as scavenging, hunting small-body-sized prey, and the organization of hunting and scavenging in a forager mode of food procurement. (Even the predominantly forager tactics of such groups as the San Bushmen are not relevant, for their hunting activities are generally organized logistically.) We need to know much more about modern groups that are foraging hunters, among them, apparently, tropical hunters such as the Aché of eastern Paraguay (K. Hawkes, personal communication). In addition, we need to look for further patterned contrasts between the Middle and Upper Paleolithic. For instance, I have noted a general absence of fire-cracked rock from the Mousterian and earlier sites I have examined. This betrays a lack of means for maximizing the radiant potential of fuels. This seems strange when one recognizes that populations were living in near-arctic settings in which, judging

from the pollen (a near-absence of arboreal pollen from Levels K, L, and M in Combe-Grenal), fuels must have been a scarce necessity. There was also a general absence of prepared hearths; fires seem to have been mainly kindled directly on the surface, and this would have made draft and heat dissipation hard to control and techniques such as "banking" a fire to last the night difficult at best. In contrast, in the Upper Paleolithic we see extensive use of stone liners and probably a kind of radiant oven in which the hot stones were banked with ash and items buried for cooking over a considerable period of time. In addition, there are large roasting pits such as the entire Perigordian IV (Level 5) "level" at the Abri Pataud (Movius 1977:91) and the feature in Level C at the rockshelter of Le Malpas (Montet-White 1973:20). Almost all the types of hearths recognizable among protohistoric and historic American Indians are represented in the Upper Paleolithic (Movius 1965, 1966).

*General comments.* An attitude running through White's writings is consistent with the bias recently expressed by Redman et al. (1978:14) as "a loosely defined direction toward which many researchers are moving in order to remedy some of the shortcomings of previous research." Clearly implied in the subtitle of the Redman et al. volume, *Beyond Subsistence and Dating*, the thrust is toward the discussion of "nonmaterial aspects of Paleolithic culture" as causes of the material aspects remaining for us to observe. This was of course the basic point of one of my earliest works (Binford 1962), so I can hardly be in basic disagreement. What the advocates of "social archaeology" seem to ignore, however, is that, as was pointed out by Radcliffe-Brown (1958[1929]:40-41) many years ago, there is a big difference between a set of synchronic functional interactions and mutual determinancies within a system and the factors which might impinge on a system to modify it through time. We might well observe features in the archaeological record which are directly referable to beliefs and/or forms of socially instituted ritual and interaction. These connections refer to the *functional* relationships between the operation of a system and its derivative materials. The existence of such relationships in no way implies that the dynamics operating to bring about change in the organization of the system are to be understood in terms of them. Understanding the relationships between "material aspects remaining for us to observe" and the internal organization of the nonmaterial aspects of past culture is a matter of understanding the dynamics of roughly synchronic interactions and mutual determinancies operative within the organization of a past system.

One of the greatest confusions to have plagued the social sciences is the confusion between regularities in the internal dynamics of cultural systems (synchronic and internal-functional) and the nature of the dynamics which conditioned changes in the organization of systems themselves and in their evolutionary diversification and change (diachronic and external-ecological). I have tried to suggest that with regard to the former problem archaeologists seek to understand the dynamic conditions which produced the statics remaining for us to observe. This may well involve us in many arguments regarding the relationships between "nonmaterial" or "nonpreserved" aspects of past systems and material derivatives of these "non-material" dynamics. I have called this middle-range research, and it is obviously research which would ideally permit the accurate *description* of past conditions. When we turn to the interesting job of explaining the nature of past systems, we move into the mode of diachronic patterning and ecological-evolutionary theory building. Functional understandings can never serve as the explanations of evolutionary changes. This fundamental distinction seems to have been overlooked and to be merged in a confusing way in the discussions of White and many of his colleagues advocating a "social archaeology."

White's article is a most interesting contribution to the study of the problems surrounding any comparison of the Middle and Upper Palaeolithic. It is really a matter of choosing a series of supposedly significant attributes which will serve to demonstrate cultural differentiation. However, the choice of attributes will be restricted to the material data at the disposal of the prehistorian. White himself compares his hypothetical construction to a house of cards. I would compare it to the successful arrangement of one face of a Magic Cube, the player proposing the hypothesis that the five other faces ought to be correct without ever having the possibility of seeing them. In fact, I am in perfect agreement with White concerning the basic problem, though I would like to add the following remarks:

1. With respect to bone-working technology, it is quite possible that many a posteriori bone artefacts already exist in the Middle Palaeolithic. I have encountered such artefacts only very rarely in the Upper Palaeolithic assemblages that I have studied. What characterises the Upper Palaeolithic is the use of a different bone-working technology consisting of preparation and "retouch" which radically modify the original form of the raw material. The dual process parallels somewhat that found in lithic technology.

2. With respect to apparently nonutilitarian objects and objects of personal adornment, several further problems spring to mind. First, we must recognise them without being too ethnocentric. For example, a small perforated bone object may be a pendant, but it may just as easily be a drumstick (Birket-Smith 1965:pl. 55), an ear-pick, or a gaming piece such as that for a Batak puzzle (Tichelman 1953). A second problem lies in discovering whether or not the people of the Middle Palaeolithic used objects of adornment made of perishable materials (feathers, leather, wood, etc.) or whether they tattooed themselves, for any of these could also have given them personal or group identity.

3. Concerning subsistence activities, it must be admitted that we have at our disposal only incomplete data. We cannot be certain that the bones collected from a "layer" which is not a true occupation floor represent only the game hunted by man and not also that hunted by other carnivores which may have frequented the site. Nor do we know how much of their game the Palaeolithic hunters consumed at their living sites or whether such sites do not sometimes represent temporary camps for specialised hunting of reindeer, horse, or bison, activities that would of course have an important effect upon the day's "bag." Furthermore, we do not know whether certain groups might not have followed a preferred game species at some times and hunted anything in proximity to their living sites at others.

It should not be forgotten that shed reindeer antler is highly nutritious because of the blood it contains and that such antler may have been collected for food as well as for raw material for artefacts.

4. The notion of group territories is most difficult. Were these merely spatial entities, or was there also a temporal aspect, for example, in the sense of a trail followed through both time and space? Did territories exist at all?

5. Concerning variation in lithic assemblages, it is clear that typologists have insisted too much upon certain quantitative relationships between types. Moreover, the lithic industry alone is insufficient to represent fully the material evidence of the Palaeolithic. We also need to take into account bone material and all other evidence of past behaviour.

It can be demonstrated that there is considerable variation within Later Upper Palaeolithic culture groups which are more or less contemporary. Indeed, it would appear to be impossible to use the type-list of de Sonneville-Bordes and Perrot to attempt an inventory of the Creswellian or the Hamburgian,

and the Magdalenian of Belgium cannot be directly compared with the Magdalenian of the Dordogne.

What appears to me important and useful is not the testing of the validity of White's hypotheses against those of Mellars, but, as White stresses, the questioning of received wisdom before it becomes fossilised.

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We applaud the kind of regional perspective in Paleolithic studies first adopted by Mellars (1973) and displayed by White in this paper. Most overviews of Paleolithic variation tend to make broad interregional industrial comparisons. At the same time, they demonstrate little eagerness to reexamine existing information in ways that permit discovery of new relationships within specific areas. White's study suggests that social and other processes responsible for producing the Paleolithic record can best be understood through more sophisticated comparative analyses of a great variety of relational data within the contexts of restricted geographic landscapes. However, there are a number of weaknesses in his approach. Actually, he does not rethink the transition from Middle to Upper Paleolithic at all; he only reevaluates and sometimes updates the distinguishing characteristics of the two major periods. This classificatory approach leads to several problems in analyzing what is really a dynamic process.

First, because the ideal characteristics that define the Middle Paleolithic seldom overlap with those that define the Upper Paleolithic, it is difficult to deduce the nature of the transition between them. Is it a transition in form or quantity? For example, Bordes (1958, 1972) defines the transitional nature of the Mousterian of Acheulian Tradition, Type B, of Pech de l'Azé I in terms of increasing numbers of Mousterian backed knives, while Marks (1977) identifies this transition at Boker Tachtit on the basis of new tool forms on Levallois flakes. This difference reflects an aspect of the transition that truly needs rethinking, and White should have addressed it with regard to all of the differences listed between the Middle and the Upper Paleolithic.

Second, some of the evidence used is irrelevant to the transition itself. Although temporally far removed from the transition, the Magdalenian is the "typical" example of Upper Paleolithic used throughout the paper. It would have been far more useful to look for similarities and differences only between late Middle and early Upper Paleolithic industries, the ones most crucial to our understanding of the problem at hand.

Another point that deserves comment is White's quick dismissal of the notion that a biological change from Neanderthals to modern *Homo sapiens* may have some bearing on the behavioral changes reflected in the industries. In the first place, it is not vitalism, as he says, to suggest that there are biological bases for some differences in behavior; this is a valid and important research question. Moreover, because the biological and industrial changes appear at roughly the same time, it is clear that the former must be taken into account *along with* social and ecological considerations, not as a last-resort alternative.

Finally, White is wrong when he states that formal lithic variation on a temporal and regional scale appears for the first time in the Upper Paleolithic. In fact, there is evidence for regional variation within the Mousterian (Crew 1975, Fish 1978, Bordes 1980, Dibble n.d.) and even within the Acheulian of France (Bordes 1968). Moreover, Copeland (1975) and especially Jelinek (n.d.) have demonstrated clear directional changes in lithic morphology through time in the Levant. These temporal trends may or may not exist in the French Middle Paleolithic, but in any case such variation cannot be considered restricted to the Upper Paleolithic.

It is time, as White points out, to rethink the Middle/Upper Palaeolithic transition. This particular nonconformity in the archaeological record has long been recognised as a possible starting point for the understanding of both synchronic and diachronic variation in Palaeolithic material. Nor is it surprising that a rethinking should return to the familiar data base of southwestern France and Mellars's (1973) clear exposition of what constitute the differences in the archaeological record on either side of the transition.

However, while White's paper contains points of interest I find it unsatisfying that the issue should be framed yet again in this standard space/time framework. A true rethinking should break out of this arbitrarily imposed framework (Mellars 1973:255-56) and look for a wider appreciation of the potential of such an investigation. Moreover, I would like to see a proper distinction drawn between investigations of variation within Middle and Upper Palaeolithic entities and the study of the transition process itself. For example, a comparison of stone-tool types between Middle and Upper Palaeolithic assemblages might lead to the conclusion that the latter displayed greater efficiency in performing certain critical tasks, but a comparison of entities and the discovery that they are different, though related, does not necessarily contribute to an explanation of the process of change. If we were to explain the transition as the product of increased efficiency, all we would be doing is fitting the dynamic of change to an interpretation of the end result. Hence our explanation of change merely accommodates (Binford 1980) the description of an entity defined in time/space.

I gain the impression from White's paper that he believes more sophisticated measures of assemblage variation on either side of the transition will lead to an explanation of the transition. Treating the transition as some sort of black box, he has produced a plausible-sounding mechanism that could account for both the transition and the changes in assemblage variation. Hence we learn of the possibility of "a restructuring of social relations across the boundary" in which the "structured relations between human groups" in the Upper Palaeolithic "were not characteristic of the Middle Palaeolithic." This is sufficiently vague but plausible to keep us patiently waiting until more measures of assemblage variation arrive to relieve the theoretical fort. Unfortunately, we are not told what these structured relations consisted of and why their absence should necessarily affect material culture. White's final three questions are extremely pertinent in this regard and do point towards some challenging new lines of enquiry. However, as the implications are not pursued it is difficult to ascertain how a notion of changing social relations can either be investigated or related to the archaeological record.

This is a criticism about methodology and the procedures by which we move from statics to dynamics (Binford 1981). A case in point refers to the interpretation of personal ornaments as a measure of increasing social complexity, formal investment, and the signalling of individual and corporate identities. I agree that such an interpretation seems *likely* and more plausible than, say, the suggestion that these objects served to keep folks' trousers from falling down, but I would prefer an independent check on the interpretation of social change. Otherwise we are just searching the archaeological record for suitable aspects of material culture which appear, since we believe we know how they function in a living system, to justify our interpretation of the past.

The data set chosen by White could be more profitably used to rethink the procedural problems concerning regional frameworks and in particular the analytical concepts which are required in order to investigate variation on a local scale. Certain

models, such as mating networks and alliance systems (Wobst 1974, 1976; Bender 1981; Gamble n.d.), give us a means to examine local outcomes in assemblage composition and variation while retaining a wider framework for the investigation of regional change. Moreover, such models focus attention on the principles which produce such general structures and which would be expected to change in such a major transition as that between the Middle and Upper Palaeolithic periods is held to be. The dimensions of a regional approach force us to look outside the 12,000 km<sup>2</sup> of the Périgord and consider area-level adaptations (Hill 1978) by Palaeolithic groups to the European continent. At once we find that the rich data base of the Périgord is not mirrored in other areas in northern and central Europe and that their meagre settlement evidence more closely reflects the extreme climatic conditions that such areas endured between 30,000 and 18,000 B.P. However, that there was settlement at all in these areas, under such conditions, is highly significant, especially as the presence of Middle Palaeolithic settlement in the same regions ebbed and flowed with the changing climate (Gamble n.d.). This measure from the archaeological record seems to me of potentially more use in examining differences between the Middle and Upper Palaeolithic than just detailing cultural inventories and their typological attributes. It raises questions about the organisation of such factors as technology, subsistence, population, and information systems, as well as the underlying organisational structures which required changes in these factors. *Why* change occurred should not be argued in terms of why these variables exhibit diachronic change; rather, the door should be opened to a fuller discussion of the principles that structured Palaeolithic society, why these changed, and what the links are with the archaeological record. That will require a good deal of rethinking.

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White has provided us with a clear departure point for discussion of this crucial transition. I applaud his concise discussion and clear conclusions, and I see particular value in the use of live-weight estimations rather than total bone counts in the discussion of faunal remains, the suggestion of different usage of reindeer antler in Middle and Upper Palaeolithic contexts, and the indication of the major difference in approach of the Bordes and de Sonneville-Bordes/Perrot typologies. While in general agreement with the content and thrust of the article, I would like to raise some points that I feel require comment; in some of these I hope that the discussion will clarify issues that emerge from the literature.

The idea of using personal ornaments as an approach to group identity and interaction appears to be very strong; it has certainly borne fruit for later periods. I do, however, have some queries on the suggestion that aggregation sites may be absent in the Mousterian and the hypothesis that structured relationships between groups were not present at this time. Whereas the later comment on absence of ethnicity does not bother me, I see real stumbling blocks to a view that seems to imply the essential isolation of local populations. There are clear limits to the minimal population size that can be maintained *over any extended period of time* (see Meiklejohn 1978), and these limits are well above the expected size of the local group. I would predict that an area the size of southwestern France would be too small to support more than one effectively isolated population during the Upper Palaeolithic and Mesolithic. Even if social structuring of the interaction network was weak, a biological network would be necessary for the survival of the population as a whole. From what we can deduce about



population densities, the kind of biological separation required to support a model of isolated populations maintaining the various technologically distinguishable Mousterian variants is impossible. I would argue that the appearance of regional differences in the Upper Palaeolithic marks the point at which local population size passes the minimal level necessary to support a locally viable mating system. I have suggested elsewhere that a valid ethnographic analogy can be drawn between the nature of archaeologically identifiable regional groups within the Upper Palaeolithic and the types of identifiable subdivisions within subarctic hunting populations (Meiklejohn 1977).

In the discussion of dietary preferences, I question the inclusion of a species of animal as part of the diet on the basis of a single bone. The presence of *Bos* in the upper assemblage at Gare de Couze is a glaring example (0.5% of the sample represents 42.4% of the calculated live weight of food). This is not a criticism of the general stance adopted in the paper; I simply have a nagging suspicion that misattribution of bone brought to a site for reasons other than meat consumption could bias the conclusions. I have expressed similar reservations elsewhere about the use of apparent faunal elements in the calculation of length of possible occupation of a site when those elements have been used for dwelling structures on the site (Meiklejohn 1974).

With reference to the problem of definition and delimitation of individual occupation units, I would simply like to point out that approaches have been formulated for its solution (e.g., Newell and Dekin 1978). It is to be hoped that they will come into more general use.

I would also like to ask whether there is any indication of possible correlation between the river-valley/interfluvial difference noted in the occupation patterns of the Upper Palaeolithic and Mousterian and factors of climate. It has been noted by many (e.g., Mellars 1969) that climatic conditions were generally less harsh in the first half of the Last Glacial than in the last half. Abandonment of the interfluvial zone could be a simple marker of a colder climate. This should be most evident during the earlier phases of the Magdalenian, a period for which total depopulation of parts of northwestern Europe has been suggested (Campbell 1978, Meiklejohn 1974).

In the case of the nature of the Châtelperronian and its role in the transition, it is possible that a priori arguments are hindering understanding. From the apparent fact that techniques of retouch differ little between the Middle and Upper Palaeolithic, White seems to go on to imply an effective continuity between the two traditions as a whole, the primary addition to the Upper Palaeolithic being the appearance of a full punched blade repertory, though elements of the latter do appear earlier. In light of the above I would like to raise a question relating to the designation of the Châtelperronian as the first full manifestation of the Upper Palaeolithic. Lynch (1966) argued that much of the underlying technique in the Châtelperronian had essential affinities with the Mousterian rather than with succeeding Aurignacian manifestations. His overall impression was that the Châtelperronian was, in essence, a late Mousterian industry with the addition of certain "fossiles directeurs" such as the Châtelperron point that resulted in its identification as Upper Palaeolithic. This view was supported by the observation that (following de Sonneville-Bordes) Châtelperronian levels always overlay Mousterian levels in excavated exposures (the sole exception being Combe-Capelle); continuity with Aurignacian levels in the same sites was not nearly as clear. The suggestion of technological links between the Mousterian and the Châtelperronian is also supported by the observations of several workers that there is little, if any, difference between the late Mousterian of Acheulian Tradition and the early Châtelperronian (e.g., Bordes 1961b), a point that is underlined by Mellars's (1969) suggestion that the MTA is invariably a terminal Mousterian

manifestation. In the light of these comments it is interesting to note the general acceptance of a considerably greater break between the Châtelperronian and Aurignacian (as suggested by Bordes, Cheynier, and Delporte, among others [see Lynch]).

Now, a principal argument for a continuity of tradition from the Middle to the Upper Palaeolithic in western Europe has been the "fact" that the technological breaks within the early Upper Palaeolithic are greater than those at the Middle/Upper Palaeolithic interface. This argument is, however, largely dependent upon the allocation of the Châtelperronian to the Upper Palaeolithic.

This discussion might be simply dismissed as "semantic" if it were not so intricately interwoven with the transition from Neandertal to *Homo sapiens sapiens*. This is made even more critical by the discovery of a Neandertal skeleton in the Châtelperronian levels at Saint-Césaire. The association of *H. sapiens sapiens* with the Châtelperronian is primarily based upon the Combe-Capelle burial. However, that allocation has been queried (Asmus 1964). It therefore appears increasingly likely that the major cultural and biological discontinuities are both aspects of the same event, the replacement of the Châtelperronian by the Aurignacian. I refer to the latter aspect as a discontinuity given the continued lack of a credible intermediate specimen between the two generally accepted populations (at whatever taxonomic level one wishes to use).

I raise these points because I feel that the impression has been given in the recent literature that direct in situ evolution from Neandertal to *H. sapiens sapiens* is a proven fact. While the evidence emerging from central and southeastern Europe is becoming increasingly convincing, the same cannot be said for western Europe. Indeed, the discovery at Saint-Césaire weakens the case, as this specimen is almost contemporary with recently discovered specimens of essentially modern morphology (Henke and Protsch 1978, Protsch and Semmel 1978). A strong case can, I feel, be made for the replacement of Neandertal by *H. sapiens sapiens* at the interface between the Châtelperronian and the Aurignacian in western Europe. Replacement associated with a variable boundary between the populations can also explain the interdigitation of Châtelperronian and Aurignacian levels at Roc de Combe and Le Piage (Lorblanchet 1976).

In the spirit of debate that this journal encourages, I hope that these queries will be seen as positive in nature. This is an excellent article that raises issues further stressing the need for a full understanding of this transition.

by MILLA Y. OHEL

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Since I am on White's side regarding most issues discussed, I would like to restrict my comment to two more general points that seem crucial to me. I fully concur with White that "systematic surveys must be carried out to determine the kinds of biases built into the traditional search for Paleolithic sites." I wonder whether he realizes how much this call is needed. Many (perhaps most) archaeologists, particularly Old World ones, do not really understand—or at least do not perform—any kind of systematic survey (random or otherwise) before they launch excavations at a spot that seems to them attractive or important. The implications of this are far more damaging than the behavior itself. An idiosyncratic "culture," "style," "tradition," or whatever, temporally and/or spatially bounded and distinctive, is created or decided upon. Entities are established on the basis of hasty conclusions from *partial* data, and these go straight into publications, regional reports and summaries, textbooks, etc., to become "accepted fact." To drag the wagon out of the mud at that point is much more difficult than running it into the mud in the first place. Do I have to give examples? It should be clear once and for all that the

data emanating from a given excavation chosen subjectively, no matter how attractive and important, *can never represent anything more than that site*. Unless a general, valid representative picture is gained by some systematic survey beforehand or a *great* number of sites are compared with it afterwards, any wider generalization concerning time and/or space is premature, more often than not misleading, and a Sisyphean task to correct later.

White's statement that "there are undoubtedly other constructs which would account for the 'facts' equally as well as the house of cards that has been built here" has the modesty I think is vital for a scientist, particularly a prehistorian, and it points to the probable existence of alternatives, sometimes not excluding each other but interfingering or interweaving. Many among us, although witnessing with their own eyes that there is almost never a single cause, answer, or explanation for any question in contemporary life, cannot tolerate anything but a clear-cut definite, single answer to any prehistoric question. This is illustrated by the reactions of a number of colleagues to the fact that I left room for two different interpretations of the Clactonian based on the same set of data (CA 18:329-31; 20:685-726): they seemed not to understand at all how this was possible. I contend that the search for applicable alternative interpretations is not just modest and realistic in terms of actual human life in the past, but also a clear sign of broad- and open-mindedness.

Thus, although White's paper did not dwell on these two points, I commend it for mentioning them, lending me the opportunity to stress their importance.

by JOHN PFEIFFER

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I hope White's important paper will help stimulate research on the Middle/Upper Paleolithic transition. For some time now—far too long, in my opinion—practically all the publicity and a disproportionate share of available funds have gone to investigators concerned primarily with hominid origins in Africa. While conceding the significance of the African studies, a case can be made for the fullest possible support of research on a period which saw a notable increase in the rate of human evolution, a process which continues to accelerate in our times.

White clears the deck for future insights by discounting the notion that a major factor in the transition was the replacement of *Homo sapiens neandertalensis* by people like ourselves, representatives of the cerebrally superior, doubly wise breed, *H. sapiens sapiens*. Although this notion explains nothing and discourages explanation, it seems to have a wide appeal. In at least two novels the Neanderthals are portrayed as individuals incapable of abstract thinking, barely able to talk (Golding 1955, Auel 1980)—and it should be noted that both novels are based on consultations with prominent paleoanthropologists. As far as the anatomical facts are concerned, however, there is no reason to believe that our robust relatives were a bit weak in the head, no reason to assume "any difference in intellectual or behavioral capacities" (Trinkaus and Howells 1979).

On the other hand, the archeological record as reviewed critically and constructively by White argues for "a total restructuring of social relations" in Western Europe. His suggestion that antler served a communicative as well as a technological function is of special interest. Communication was a central theme, perhaps the central theme, of the Upper Paleolithic. The appearance of personal ornaments and long-distance exchange and the possibility of rising population densities may be regarded as signs of people living under pressure and in the process of forming wider associations. In this context the spectacular increase in cave art and ceremony may represent an effort to reduce conflict or, more positively, to create allegiances beyond those of kith and kin, and status burials may indicate the emergence of individuals specializing in the orga-

nization of ceremony (Eastham and Eastham 1979; Denis Vialou, personal communication, 1980).

I have only one small bone to pick with this paper, specifically with the last paragraph, where White states that "there are undoubtedly other constructs" to account for the Middle/Upper Paleolithic transition and refers to his own as a "house of cards." Are there indeed other constructs? If so, I should like to have them spelled out; they are not obvious to me. If not, the gesture is uncalled for. This paper is sufficiently solid to stand on its own, without apologies.

by LAWRENCE GUY STRAUS

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White is to be commended for taking up the important question of the Middle/Upper Paleolithic transition, but it is not a topic which can be said to have been abandoned in recent years (see, for example, Freeman 1975, Bricker 1976, Straus 1977, Harrold 1980, and numerous topical chapters in *La préhistoire française* [de Lumley 1976]). New ideas are always welcome, however, in the search for understanding of this crucial issue. The main difference I think I have with White is that I believe the evidence tends to indicate a fairly long period of continued adaptive change (cultural and morphological) between Würm II and Würm IV. It is true that some developments, such as the appearance of ornaments and art (with isolated exceptions such as those noted by Marshack [1976]), do occur in the record rather suddenly. Nonetheless, the key changes in technology, settlement, and subsistence patterns, just like the anatomical evolution from archaic to modern *Homo sapiens*, clearly did not take place overnight during the Würm II/III interstadial, even though the tempo of change obviously had accelerated remarkably since the Middle Pleistocene.

Legitimate questions concerning the difference between the Bordesian Middle and Upper Paleolithic typologies aside, I think White could have brought up important developments such as the manifest increase in importance (or invention) of hafted and composite tools and weapons in the Upper Paleolithic. With the caveat that blades, endscrapers, burins, and backed knives are found in Middle Paleolithic assemblages, while sidescrapers, flake denticulates, bifaces, and even choppers are found in Upper Paleolithic ones (see Straus 1978a), true tanged and shouldered points, backed bladelets, and eventually other microlithic types are found only in the latter and suggest the development of technologies far more complex and specialized than those of the Middle Paleolithic. Indirect (faunal) evidence suggests the presence in Upper Paleolithic time periods of nets, traps, weirs, etc., as well as of atlatl and even bow and arrow "weapon delivery systems." The question of whether or how many of the Cueva Morín (and El Pendo) Mousterian "worked" bones are deliberately fashioned tools is an open one (see Freeman 1980, Binford 1981). Nonetheless, the complete, standardized alteration of bone, antler, ivory, and tooth into new forms beginning in the early Upper Paleolithic is clearly on a scale and of a nature quite different from the possible instances of Mousterian bone working.

I agree with White that Mellars's (1973) characterization of Middle/Upper Paleolithic subsistence differences is inadequate. Nevertheless, I feel he was onto something important, however poorly expressed. The faunal data from north-central Spain with which I am most familiar indicate important changes in subsistence activities, particularly in the late Upper Paleolithic (Solutrean and Magdalenian). I have argued (Straus 1977) that these changes entailed *both* diversification and situational specialization as elements of a much intensified food quest. New food sources—often harder to exploit—were added to the extant subsistence base, while others were acquired

with new techniques and technologies (e.g., mass hunting of cervids and caprids). My findings and those of Freeman (1973) concerning subsistence intensification in Cantabria have been supported by recent faunal analyses of primarily Solutrean and Magdalenian assemblages from Ekain (Altuna 1979) in Guipúzcoa, Rascaño (Altuna 1981), El Juyo (Klein et al. 1981), and El Pendo (Fuentes 1980) in Santander, and La Riera (Straus et al. 1981), Tito Bustillo (Altuna 1976), and Las Caldas (Soto and Meléndez 1981) in Asturias. Large numbers of red deer or ibex are found in thin, carefully excavated late Upper Paleolithic levels at these sites. Small numbers of horses and bovines—species so important to Mousterian diets, perhaps in the form of carrion—do continue to be present in Upper Paleolithic assemblages. If fully consumed, they would always have made significant contributions to Upper Paleolithic diets, as White demonstrates. The point is, however, that red deer (and—at mountain or cliffside sites—ibex) were now present not just in small numbers, as in the Mousterian, but in large quantities, clearly suggesting the development of (and need for) specialized mass hunting practices (see Klein et al. 1981, Straus et al. 1981). The acquisition of antler (and, probably even more critically, hides) may have been an important by-product of such hunts, but food would usually have been primordial. In terms of subsistence resource diversification, new evidence from La Riera (Straus et al. 1980, 1981) and from El Pendo (Madariaga 1980) confirms the growing albeit limited role of shellfish, as well as fish and birds, during the late Upper Paleolithic.

Along with changes in technology, hunting methods, and prey, subsistence intensification over the course of the Upper Paleolithic involved the development of more elaborate systems of site placement and activity scheduling to take advantage of resources available in different habitats at different seasons. There seems to have been a multiplication of specialized site types and locations (see, for example, Straus 1979). With regard to “long-distance contacts,” much work has been done recently which is not mentioned by White, although it would tend to bolster his arguments (e.g., Bahn 1977; Sieveking 1976, 1978; Straus 1978*b*, n.d.a).

Naturally, the evidence for Upper Paleolithic subsistence intensification leads to the key question of causation. White's own work contributes significantly to the formation of a modern-quality data base on relative site densities. It is unfortunate that he can only “guess” that Mellars is wrong by “at least a factor of 10” in estimating Périgord Mousterian site numbers. Added evidence for real population pressure in the Upper Paleolithic of southwestern Europe includes the first peopling of the Pyrenees in the Magdalenian (Clottes 1976), as well as my recently revised site-density figures for Vasco-Cantabrian Spain, which show about 0.03 sites per millennium for the Acheulean, 0.2 for the Mousterian, 1.2 for the Aurignaco-Perigordian, 11.0 for the Solutrean, 11.7 for the Lower/Middle Magdalenian, 12.0 for the Upper Magdalenian, 11.0 for the Azilian, and 14.0 for the Asturian.

I most strongly agree with White that the assemblages of individual Upper Paleolithic culture-stratigraphic units can be highly variable in terms of tool-type relative frequencies. White's Périgord Magdalenian data fully parallel my Vasco-Cantabrian Solutrean data (e.g., Straus 1976, 1978*c*, n.d.b). Solutrean collections from both old and modern excavations vary widely, but in regular patterns which I have interpreted functionally. Endscraper percentages range from 5 to 35, burin percentages from 5 to 37, and even percentages of Solutrean points from 0 to 32. Site role (as well as sampling error) is clearly involved in this variation.

I have no doubt that new types of social relations, territorialism, exchange (of information as well as of goods), etc., all played major roles in allowing our Upper Paleolithic ancestors to adapt not only to the vicissitudes of Last Glacial physical environment (fluctuations which were, after all, not new to

the hominids), but also to increasingly densely packed human populations, especially in peninsular regions such as south-western Europe. I, like White, have, however, no sure cement for the “house of cards” he so tentatively erects. The apparently increased importance of artistic objects and styles in the Upper Paleolithic, to which some try to assign symbolic meaning, certainly has significance in the overall picture of changing human adaptations.

by THOMAS WEBER

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White's paper shows the difficulty of finding a reasonable position on the problem of the boundary between the Middle and the Upper Palaeolithic. I agree with him that we should seek to understand Middle/Upper Palaeolithic differences with reference to a “restructuring of social relations across the boundary,” but the sources for such an approach are few and therefore there will be numerous uncertainties in our results.

This is the case, for example, with the problem of bone, ivory, and antler technologies during the Middle Palaeolithic. It was long believed that these techniques played a very limited role and one that increased with the beginnings of the Upper Palaeolithic. Recent excavations, for example, in the Middle Pleistocene travertine complex of Bilzingsleben, have shown specialized production of such artifacts in the Lower Palaeolithic. The typical technique for modifying bone was the same as in the Clactonian “hard” retouch on flint artifacts. Antler was used for making mattocks of different types (Mania 1979: 708–17), which are also known from the Eemian Interglacial site of Taubach (Eichhorn 1909). Serious difficulties arise in attempting to reconstruct habitation structures. In recent years, however, structures (tents or “huts”) older than the Upper Palaeolithic have been excavated at Bilzingsleben (Mania n.d.) and Rheindahlen (Thieme n.d.). Therefore a clear distinction in this regard is problematic.

I agree with White's critique of Mellars's calculations of population density during the Mousterian. There are difficult problems involved in identifying a general level of “site” or “stratum” for comparisons of different cultures (with different behaviour), and therefore the Mousterian is clearly “under-represented” with only 32 sites. I cannot believe, in fact, that the predominant river-valley occupation in the Upper Palaeolithic is only a consequence of sampling bias, given the Mousterian interfluvial occurrences. In our region we find only a few Middle Palaeolithic and Aurignacian/Perigordian surface sites (probably because of climatic conditions and erosion) and a respectable number of more than 50 Magdalenian ones. I think it rather likely that it is the *older* sites that have been destroyed by Late (Peri-) Glacial erosion.

White's analysis of the differences in the nature of inter-assemblage variation is inadequate to answer the various questions concerning continuity/discontinuity in artifact assemblages. Even if we accept the monothetic typological concept (and this is very problematic for most of the Middle Palaeolithic inventories), we have to recognize that differences in tool percentages (especially differences in only *two* tool groups) are not the sole dimension for measuring variation. To obtain a real picture of the Middle/Upper Palaeolithic boundary it is necessary to examine the technology of *all* artifacts (cores, flakes, unmodified blades, and “waste” included) and the characteristic features within types with an elaborated system of attributes. Such investigations of Lower and Middle Palaeolithic assemblages have shown a clear distinction in flake technology between the Clactonian, Bilzingsleben 1/Vértesszöllös and Weimar/Bilzingsleben 2 (two typologically and chronologically [?] Middle Palaeolithic industries) inventories, on the one hand, and the Acheulean, the Interglacial, and the Königsau Middle Palaeolithic finds, on the other (Weber 1980,

1981; Schäfer 1981). These differences are characterized by metrical data, e.g., length-width indices of flakes near 1.0 for the first group and near 1.3 for the second, relative-thickness indices near 30 and 25, respectively, and so on. In the absence of analytical techniques of artifact interpretation, it was impossible to recognize these technological groups. The use of multivariate methods, combined with statistical tests, especially for the various artifact categories, may permit us to draw a clearer picture of the Upper Palaeolithic technocomplexes and consequently of the Middle/Upper Palaeolithic transition.

[As the result of an oversight in the Editor's office, Paul Mellars was unable to comment in time for publication here; his remarks appear in the Newsletter (pp. 238-40), and White's response to them can only appear in the June issue.—EDITOR.]

## Reply

by RANDALL WHITE

*New York, N.Y., U.S.A. 3 XII 81*

In many ways it is presumptuous to attempt a broad-scale synthesis of knowledge and ideas pertaining to the Middle/Upper Paleolithic transition. Few of us possess the linguistic capabilities and/or firsthand experience with the many regions, sites, and assemblages pertinent to the subject. For this reason, I chose to use my knowledge of the Périgord data base to stimulate colleagues to contribute ideas and information drawn from other areas and other theoretical perspectives. It is gratifying that most comments respond constructively to this call for collective synthesis. My reply will attempt to perpetuate this constructive and conciliatory spirit.

*Stone-tool technology.* Straus correctly points out that the Upper Paleolithic sees the development of far more complex stone technology and "weapon delivery systems." My discussion was perhaps too typological. We could definitely profit from a more serious consideration of the evolutionary implications of the technological developments noted by Straus. First, however, we need to know much more about the details of this increased technological complexity. Such knowledge is in fact within reach, given the appropriate research design. There is, for example, irrefutable evidence for the hafting of blades during the Lower Magdalenian at Lascaux (Allain 1979), the hafting of a laurel-leaf in the Solutrean at Badegoule (Cheynier 1956), and the arming of a bone projectile with fragments of flint in the Magdalenian at Saint-Marcel (Allain 1957). There is undoubtedly a good deal more of this kind of evidence, most of which is contextual in nature, from areas other than southern France. It is perhaps time to synthesize this information in order to go beyond simple statements of shifts from flakes to blades which, apart from being inaccurate, tell us little of importance.

*Bone-working technology.* Ordinarily, I would be pleased that both Dewez and Weber provide supporting evidence for the presence of retouched bone in Middle Paleolithic contexts. However, given Binford's (1981) recent and rigorous studies of nonhuman bone alteration, the only realistic stance is a conservative one—nonacceptance of claims for Middle Paleolithic bone and antler working until the possibility of natural action has been considered and justifiably dismissed. With respect to the Cueva Morín material, the responsibility now rests with Freeman to substantiate his claims in light of Binford's observations.

*Subsistence activities.* Binford and Meiklejohn both emphasize that minimum-number-of-individuals estimates, and the resulting live-weight figures, are of dubious value when the number of bones used to establish these estimates is minuscule. Their arguments are sound. The use of these data was based on the need to grasp at straws imposed by the way in which faunal

statistics are calculated and expressed by most French paleontologists. In other words, there are so few faunal assemblages for which element/species inventories have been published that one tends to employ, perhaps uncritically, those assemblages for which such data exist. The alternative—the use of bone percentages—is, in my opinion, even less desirable a method. However, the problem transcends the methodology of faunal analysis. Questions pertaining to the nature of traditional site definition and excavation must be addressed.

It is shocking to be able to state with confidence that, from the time of Lartet and Christy until the present, not a single Paleolithic site in the Périgord has been excavated in its entirety and/or sampled in such a way that statements concerning all areas of the site and its environs could be made. Excavations at Laugerie-Haute (Peyrony and Peyrony 1938), Abri Pataud (Movius 1975, 1977), and La Gravette (Lacorre 1956) serve to illustrate the problem. Laugerie-Haute, one of the largest and richest Upper Paleolithic sites in western Europe, is known from massive excavations inside the collapsed rock-shelter. However, there is more to Laugerie-Haute than meets the eye: a probable reindeer kill site immediately in front of the site, on the banks of the Vézère. According to Elie Peyrony (personal communication) and Hallam Movius (personal communication), a test trench by Denis Peyrony revealed reindeer skeletons in various stages of disarticulation. Apparently because there were few artifacts, the excavations were never continued or published. At La Gravette, Lacorre briefly mentioned the recovery of spongy tissue from waterlogged deposits in front of the shelter, but to my knowledge these excavations were never followed up. More recently, Jean-Michel Geneste, excavating the Abri Vignaud, a continuation of the Abri Pataud, has uncovered a probable kill/butchering locality in riverine sands in front of the shelter. The point of this excursion is to suggest that studies of fauna derived from the shelters themselves will be of little utility in behavioral reconstruction unless the materials can be related to the assemblages remaining at kill/butchering localities. Without evidence from kill/butchering localities, it is even difficult to refute a suggestion as far-fetched as that the occupants of the Abri Pataud were specialized mammoth hunters who, intelligently enough, never dragged the bones of their preferred prey back to camp. In sum, Binford and Meiklejohn are both correct in attacking the faunal data presented and used in MNI/meat-weight calculations. However, larger samples of bones from the species concerned would be no more accurate unless considered alongside data from kill sites. Dewez is right: understanding subsistence patterns is a complex matter. Nevertheless, the underlying point of my argument remains intact: greater numbers of reindeer bones do not automatically indicate dietary specialization on reindeer. The additional point made here is that a firm conclusion on this matter will require not just more refined faunal analytic techniques, but a major change in the way Paleolithic archeologists attack the archeological record.

Straus's observation that in Spain the Solutrean and Magdalenian exhibit simultaneous trends toward subsistence diversification and situational specialization finds full support in the Périgord, where fish, birds, rabbits, and hares become important at the same time that site location strategies become highly redundant and predictable (White 1980).

*Dimensions of settlements.* Binford is right in observing that "both the variance in and the absolute size of settlement achieved" increase in the Upper Paleolithic. This is my point exactly. My criticism of Mellars's work is that, while recognizing the increase in maximum size of settlement during the Upper Paleolithic, his bias in favor of large Upper Paleolithic sites does not give one a sense of size variation.

Whether the large Upper Paleolithic sites reflect aggregation of otherwise discrete groups is a legitimate question posed by

both Arts and Binford (as well as by me). This important practical problem notwithstanding, I have fewer doubts than Binford regarding the value of the concept of aggregation site. Moreover, I am not convinced that a strict delineation of the immediate conditions under which aggregation takes place is prerequisite to an understanding of the long-range adaptive value of aggregation, although a knowledge of such conditions would certainly aid in the formulation of test implications for the hypothesis that any given site is an aggregation site.

*Population density.* Binford's arguments concerning the homogeneity of Mousterian levels and the corresponding lack of fine-grained response to minor environmental dynamics are important. They provide one possible explanation for the ubiquity of Mousterian open-air sites in much of the Périgord. On the basis of these arguments, we must reassess the simplistic equation of site numbers with population density. The number of sites resulting from Mousterian nomadic foraging, as it is described by Binford, and Upper Paleolithic logistically organized collecting should be expected to be radically different and seemingly independent of population density.

In wishing to attach behavioral significance to settlement differences and differences in site numbers, we should not lose sight of the complex geological forces of site preservation and destruction and the lack of knowledge of the chronological relationships between most of the sites and assemblages recovered thus far for the Mousterian. One point of interest is that Straus's calculations of site numbers per millennium for the Spanish Upper Paleolithic seem to match my own for the Périgord. However, while Straus argues for very gradual population increase through time, I have interpreted my results as probably reflecting a greater rate of site preservation for more recent time periods, among other biases.

Bahn's position that Mellars was prudent to disregard open-air Mousterian occurrences on the interfluvies because they are undated is untenable. First, it ignores the fact that many of the shelter and cave levels used by Mellars were also undated, and many of them (Laussel, for example) result from early 20th-century "mining operations." Second, far from being the result of the transitory frequenting of crest routes by Mousterians, many of these surface sites exhibit rich and spatially discrete accumulations of Mousterian materials. This is reason to focus more research energy on these areas rather than to ignore them because they are undated. Such research should help to answer Meiklejohn's question as to whether these open-air sites reflect occupation during periods of favorable climate. It should also help to answer Binford's query as to the possibility that these are in fact Acheulian sites. One final attraction of these interfluvial sites is that they represent a large sample of Mousterian assemblages from a very restricted geographic area. Chronological problems notwithstanding, the contribution of such a data base to the assemblage variation problem is self-evident.

Ohel's point with respect to the need for systematic survey and sampling strategies is well taken. The Périgord is perhaps one of the best examples of a situation in which a lack of systematic regional survey has resulted in a sample of sites biased in a variety of complex ways. Without systematic regional survey, the representativeness of the sample will remain unassessable.

*Differences in interassemblage variation.* I agree with Binford that the demonstration of blatant interassemblage variation in the Upper Paleolithic is an important contribution of the paper, one which echoes earlier work by Straus (1978c) on the Spanish Solutrean. This demonstration is but a first step. Nevertheless, it opens the door to attempts at understanding the relationship between stone tool assemblages and the dynamics of human/environmental interaction.

I do not believe, as Gamble suggests, that more sophisticated measures of assemblage variation will *explain* the transition. This is clearly ludicrous. However, without means of objective

measurement of variation, we cannot even define the "statics," much less move on to "dynamics" (Binford 1981).

Fish and Dibble, as well as Weber, make it clear that techniques of analysis that go beyond traditional typology are potentially capable of changing our conception of the nature of differences in variation across the transition. Nevertheless, an interesting contradiction is apparent. While arguing for region-specific variation in the Middle Paleolithic, Fish and Dibble find it permissible (and, one presumes, helpful and appropriate) to apply the term "Mousterian of Acheulean Tradition" in both France and the Levant. At present, it still seems that, if there is geographic variation in the Middle Paleolithic, it does not even approach the scale of that of the Upper Paleolithic; for example, Dewez observes that the de Sonneville-Bordes/Perrot type list is inapplicable to areas as close to France as Germany, England, and Belgium.

Again in response to Fish and Dibble, it remains to be argued whether "clear directional changes in lithic morphology" are referable to style or continuity patterning in the Levantine Mousterian or in Europe. Fish and Dibble provide few data as a basis for meaningful dialogue on the question of directional change through time. As I urged, new approaches to measuring change through time in the Mousterian must be developed. If these approaches are to be metric in nature, as I suspect those of Fish and Dibble are, there must also be accompanying epistemological developments. I trust that the next few years will witness substantial debate over the significance of assemblage differences perceived by means of artifact measurement and flake morphology.

*Relations between biological and cultural evolution.* I remain firm in my contention that biological differences should be employed only as last-resort explanations. In fact, they are not explanations at all unless neurological changes and their implications can be specified. Empirically, such a stance seems justified given the recent discoveries at Saint-Césaire and in light of the detailed firsthand work of Trinkaus and Howells (1979) and Trinkaus (n.d.), which suggests that, if there is a relationship between culture and biology across the boundary, cultural developments, such as better control of heat and shelter, are stimulating biological change rather than vice versa.

Meiklejohn comments on the demographic feasibility of isolated local populations. However, I did not mean to imply the reproductive isolation of local groups. If I had to be pinned down, I think that I would argue for open-ended mating networks during the Mousterian. In this light, "structured relations" can in part be taken to mean mating-network closure and the related imposition of cultural boundaries.

Meiklejohn is perhaps too eager to attribute the Châtelperronian to the terminal Mousterian, an attribution that would make Saint-Césaire conform all too readily to our expectations. Besides, I am not certain that it makes any difference whether the changes occur during the terminal Mousterian or during the initial Upper Paleolithic. The more refined we wish to become in identifying the date of the boundary, the more arbitrary it becomes. What we are dealing with is not an event at some specifiable point in time, but a time-transgressive process.

While Meiklejohn is accurate in stating that the Châtelperronian most often overlies Mousterian occupations, it is not true that this indicates a greater affinity in settlement location than that between the Châtelperronian and Aurignacian, for example. Of 25 Châtelperronian occurrences in the Périgord, 20 of them are succeeded by Aurignacian occupations in the same stratigraphic column; of the remaining 5, 3 are single-component Châtelperronian occupations.

Meiklejohn's comments on the relationship between the Aurignacian and Châtelperronian ignore a staggering literature that accepts the contemporaneity of the Aurignacian with the Châtelperronian/Upper Périgordian (see Laville, Rigaud, and Sackett 1980 for a detailed statement). This is by no means a dead issue.



Binford's point that the first evidence for symboling across the transition marks the emerging "ability to anticipate events and conditions not yet experienced" is a provocative one. However, it must be asked to what extent the absence of symbolic behavior indicates the lack of capacity for such behavior. Of course, the more immediate question is to what extent the archeological record indicates a lack of symbolic behavior. It strikes me that formal redundancy in tool manufacture indicates an ability to anticipate the desired form in an amorphous mass of raw material. Similarly, the association of grave goods with dead Mousterians seems indicative of concern for the future. These important questions aside, Mousterian symbolic abilities seem to have been far less manifest than those of Upper Paleolithic peoples.

*Transitions versus differences.* Fish and Dibble, as well as Gamble, point out that I have focused on differences between the Middle and Upper Paleolithic and have thus ignored the transition itself. This is both true and justifiable. It is certainly justifiable in the sense that what went before and after the transition actually defines the transition itself. For purposes of explanation, the transition is a black box. A major thrust of my argument is that this is so because the causative factors are not of the kind that have been readable archeologically, given traditional theoretical and methodological orientations (climate change, for example). Rather, they are reflected indirectly in several aspects of the archeological record, a record which at 35,000 B.P. is woefully fragmentary anyway.

Fish and Dibble's notion that the use of evidence drawn from the Magdalenian is irrelevant to a discussion of the transition is ill-conceived. I consider it self-evident that the Magdalenian is as much a product of the transition as are earlier time periods. In certain scenarios one can see it as the logical outcome, for example, of Binford's ever more complex social geography. In any case, I do not use the Magdalenian to typify the Upper Paleolithic. Magdalenian assemblage data are instructive because it has been consistently maintained that Magdalenian assemblages are more quantitatively redundant than those of earlier periods. If these can be shown to exhibit considerable variation, then variation should be equally or more pronounced for other Upper Paleolithic periods. My use of Aurignacian data for comparison (any other period could have been employed) shows this to be true.

*Social facts and artifacts.* Binford presents a complex and crucial argument in response to "social archeology." The turnaround time allotted for submission of this response has not allowed the amount of thought necessary to digest thoroughly the nuances of the arguments. Therefore, some general reflections will suffice in anticipation of much more thought and debate. The thrust of Binford's statement seems to be that, while certain manifestations of the archeological record can be accounted for with reference to synchronically operating, non-material aspects of culture (social relations, for example), there is no reason to think that change in a system is referable to these nonmaterial, synchronic factors. I am less willing than Binford to draw clear distinctions between synchronic and diachronic processes or between "internal-functional" and "external-ecological" dynamics. I am also less willing to see causal arrows always drawn from material to nonmaterial. This is not to say that all change is exclusive of external causality, but to suggest that ideological, social, and even technoeconomic change can be the result of adjustments to changes in nonmaterial conditions. I find it peculiar that we are more than willing to accept that technoeconomic innovations such as the invention of the bow, storage capabilities, and plant and animal domestication can be epoch-making in their impact but unwilling to attribute the same clout to "social inventions" such as the incest taboo, exogamy, and countless other organizational institutions. In sum, my argument is merely that change results from a complex interplay

between human organization and the resources to the extraction and distribution of which that organization is dedicated. Across the Middle/Upper Paleolithic transition, and during the entire Upper Paleolithic, this interplay was becoming progressively more complicated. In a general sense, this is what I take Binford to mean when he speaks of increasingly complex social geography.

In conclusion, I believe that this article and the comment stimulated by it have laid bare most of the problems and issues surrounding an understanding of the transition. Moreover, a collective knowledge of the literature has resulted in a significant bibliography pertaining to the transition. It is obvious from the responses that efforts are under way on several fronts to solve the problems discussed. I hope that I have been able to lend some direction to these efforts. Pfeiffer's query remains appropriate: "Are there indeed other constructs?"

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