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Tool Standardization in the Middle and Upper Palaeolithic: a Closer Look

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It has been postulated that one difference between Neanderthals and anatomically modern people lies in a 'clearer mental template' of flaked stone tools on the part of modern people. This is thought to have been manifested in greater tool standardization during the Upper Palaeolithic than in the Middle Palaeolithic. Testing of this hypothesis, using three samples of a characteristic Upper Palaeolithic tool class — burins — from one Middle Palaeolithic and two Upper Palaeolithic assemblages, reveals that they are equally standardized for both metric and non-metric traits. Further consideration suggests that most Palaeolithic flaked stone tools are poorly suited to test notions of standardization, although some tool attributes may be well suited when considered in specific adaptive contexts.

Comparisons between the material culture of the Middle and Upper Palaeolithic have a long history, with emphasis usually placed on differences, rather than on similarities (e.g. Mellars 1973). By the 1930s, the defining criteria of the Middle Palaeolithic, as well as some earlier industries, included their almost exclusive flake production (Breuil 1932; Menghin 1937), as compared to Upper Palaeolithic stone work which was characterized by true blade production (e.g. McCurdy 1932). This perceived dichotomy, particularly for tool blanks, became one of the most striking contrasts between the two periods, at times being used as a shorthand to differentiate them (Oakley 1961). While it is now fully established that such a rigid technological dichotomy is not valid for Europe (Révillon 1994), the Near East (Crew 1976; Marks & Monigal 1996) nor for sub-Saharan Africa (Brooks 1996; see also Bar-Yosef & Kuhn 1999), it is still largely held that blade production and the selection of blade blanks for secondary retouch was much more typical of the European and Near Eastern Upper Palaeolithic than it was of the Middle Palaeolithic of the same areas. This very fact, seemingly, has led to the intuitive observation that tools of the Upper Palaeolithic were significantly more standardized than those of the Middle Palaeolithic (Mellars 1989a; 1996a). If, in fact, the vast majority of Upper Palaeolithic tools were made on blades (a dimensional subset of flakes), then it is logical to expect that there would be less dimensional variability in Upper Palaeolithic tools than in their Middle Palaeolithic counterparts, which tended to be made on a more widely shaped range of blanks. Perusal of artefact illustrations from European site reports certainly tends to reinforce this impression of greater tool standardization in the Upper Palaeolithic than in the Middle Palaeolithic.

This proposition of greater Upper Palaeolithic tool standardization, however, has never been tested and is, at best, merely a reasonable impression. It might well remain that way, without additional scrutiny, except that this impression has been used specifically as an argument that anatomically modern people may have been cognitively different from Neanderthals (Mellars 1996a, 526). In particular, Mellars (1989a, 365) suggests that the forms of 'distinctively Upper Palaeolithic tools' and their 'higher degree of *standardization*' and a more obvious degree of *imposed form* in various stages of their production and shaping also 'appear to reflect more clearly conceived *mental templates* underlying their production' than was the case for their Neanderthal predecessors. Thus, this impression has been used to argue for a profound difference between modern people

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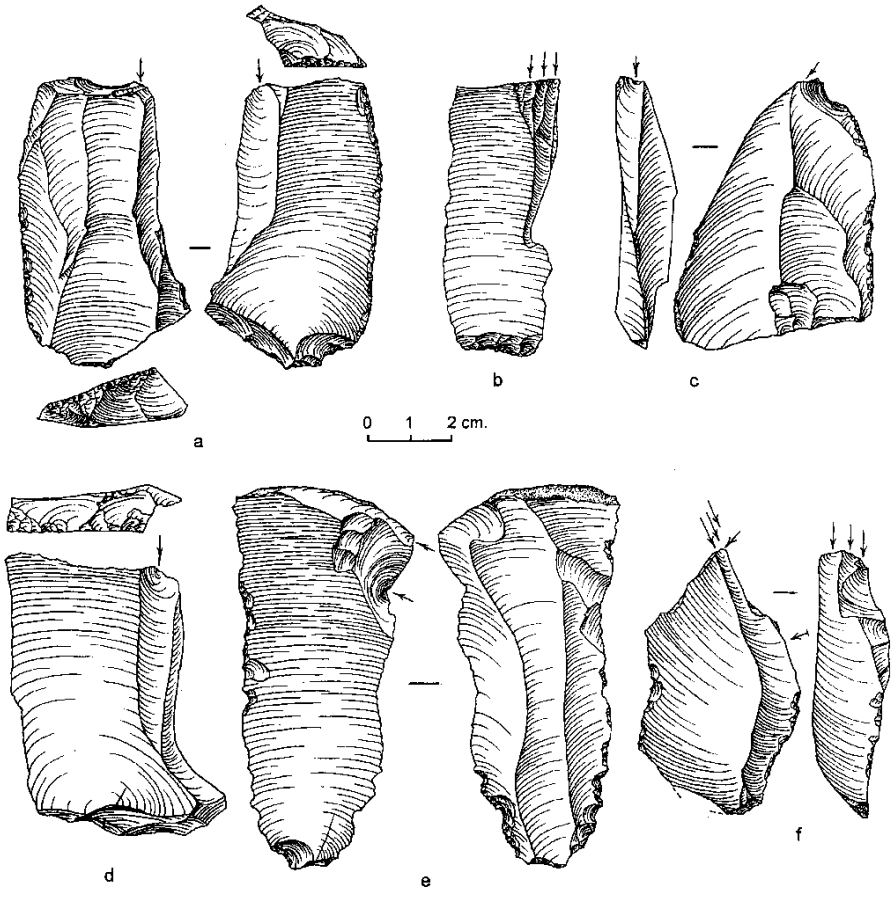

Figure 1. *Middle Palaeolithic burins from Rosh Ein Mor: a) on concave truncation; b) on snap; c) on concave oblique truncation; d) on straight truncation; e) transverse on natural plane; and f) dihedral symmetric.*

and palaeoanthropic ones, particularly Neanderthals, a conclusion needing rigorous justification. Before such conclusions can be drawn about cognitive differences between two groups of hominids, the underlying assumption that, other things being equal, Upper Palaeolithic tools were more standardized, must be empirically demonstrated.

How can the initial impression be tested? Since the tools in question are defined as 'distinctively Upper Palaeolithic', they should not be present in any number in Middle Palaeolithic contexts. Traditionally, these tool classes include endscrapers, burins, perforators, truncations and specific forms of armatures. For the most part, it is true that such tools either tend not to occur in pre-Upper Palaeolithic contexts or, when they do, are found in small numbers. This makes comparisons, even at the tool class level, across the Middle to Upper Palaeolithic boundary extremely difficult. Yet, there are examples of Middle Palaeolithic industries with significant numbers of 'Upper Palaeolithic' tools, one of the clearest being the Early Levantine Mousterian, where burins are common (Garrod & Bate 1937; Copeland 1975, 329). Perhaps the largest number of such burins in Middle Palaeolithic context occurs at Rosh Ein Mor, in the Central Negev, Israel, where 403 were recovered, accounting for over 15 per cent of the restricted Bordian type-list (Crew 1976, 100). Not only does Rosh Ein Mor have a significant burin component, it has recently been dated by U-Series on ostrich eggshell to 210,000 BP (Marks & Schwarcz 1999). Thus, the makers of these burins could not have been even anatomically modern, much less behaviourally modern. Anatomically modern humans do not appear even in Africa until *c.* 100,000 BP (Stringer 1998) and modern human behaviour does not seem to have been present among anatomically modern populations until *c.* 50,000 BP (Klein 1998).

The Rosh Ein Mor burins, a representative sample of which is housed at Southern Methodist University, Dallas, Texas, seemed to be an ideal vehicle through which to test the proposition that Upper Palaeolithic tools were more standardized than their Middle Palaeolithic equivalents (Fig. 1). While the number of Upper Palaeolithic burin samples potentially available for comparison are legion, the most logical comparison would have been with early Upper Palaeolithic assemblages from the Central Negev and adjacent Sinai. The comparison would then be regional, and the potential effects of different raw material constraints would have been avoided. Yet the generalizations about common Upper Palaeolithic tools do not hold well in the southern Levant. Although there are eight published early Upper Palaeolithic assemblages from the Negev and the adjacent Sinai, which may have utilized the same or similar flint outcrops as those represented at Rosh Ein Mor, there was an average of only 17 burins per site (Bar-Yosef & Belfer 1977, 78; Jones *et al.* 1983,

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288). Such samples are way too small for meaningful morphological and statistical comparisons with the much larger Rosh Ein Mor sample, particularly for non-metric attributes, since sample size is directly correlated with typological 'richness' (Grayson & Cole 1998).

By chance, a number of Upper Palaeolithic assemblages from Portugal were at SMU being studied as part of a project on the Upper Palaeolithic of Portuguese Estremadura. Included were two assemblages, one an early Magdalenian from Cabeço de Porto Marinho I, dated to *c.* 16,000 BP (Marks & Mishoe 1995) and the other an undated Gravettian from the site of Tocas (Marks *et al.* 1994; Monigal in press). Both had good burin samples and each represents a different, widespread Upper Palaeolithic tradition (Fig. 2). Again, it would have been optimal to compare these Portuguese Upper Palaeolithic burin samples with burins from Portuguese Middle Palaeolithic assemblages. Unfortunately, not a single burin has been reported from any published Middle Palaeolithic assemblage in Portugal (Raposo & Cardoso 1998; Marks *et al.* in press). Thus, an optimal solution did not exist in either area. While no argument is made that these two Upper Palaeolithic assemblages are modal for Eurasian Upper Palaeolithic burin characteristics, or that burins are more or less standardized than other classes of Upper Palaeolithic tools, the *universal validity* of the claim that the same class of tool in the Upper Palaeolithic will be more standardized than in pre-Upper Palaeolithic contexts may be tested using these three assemblages. If standardization *per se* is directly linked with a significantly increased clarity of mental templates in modern people, as opposed to pre-modern ones, a comparison between samples made by behaviourally modern people and pre-modern ones should be valid wherever they came from.

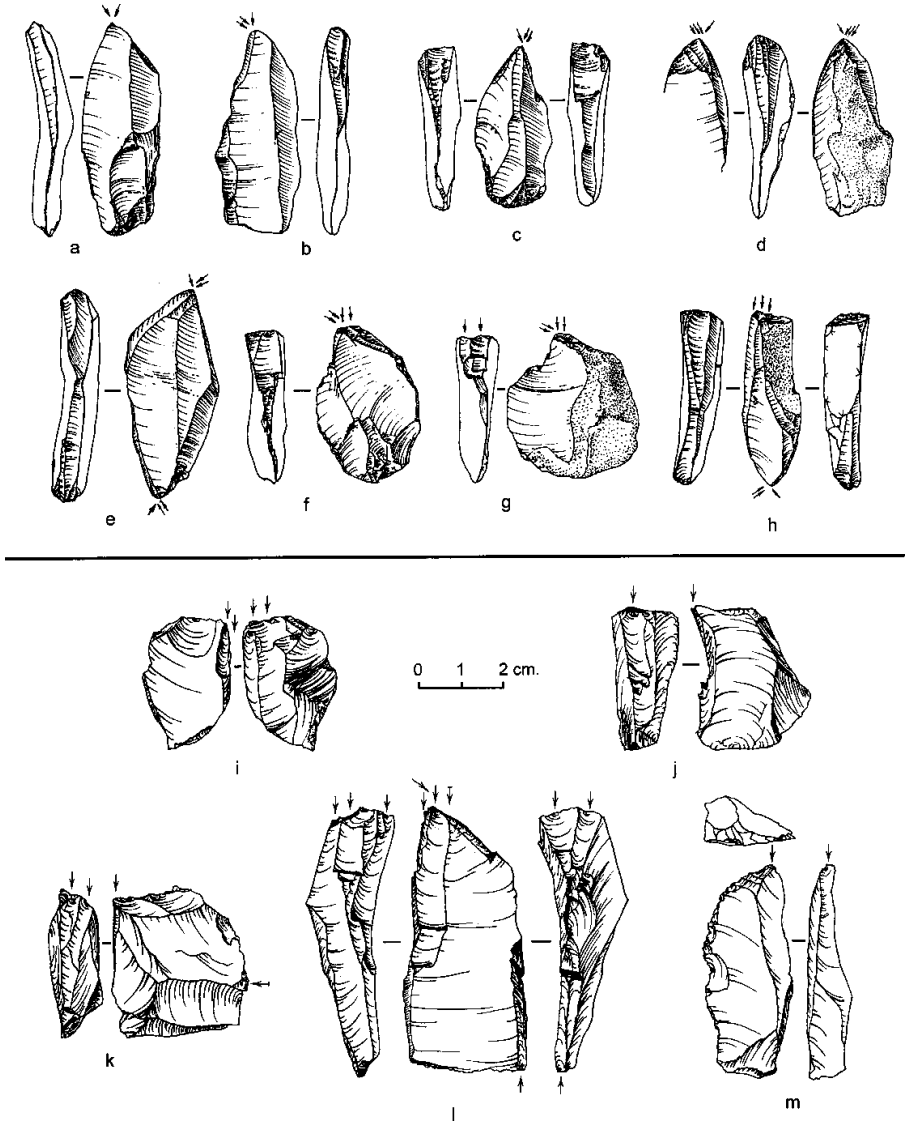

Figure 2. *Upper Palaeolithic burins. Gravettian from Tocas [a–h]: a–c, e–g) dihedral asymmetric; d) busquoid; h) multiple mixed. Magdalenian from Cabeço da Porto Marinho I [i–m]: i) on notch; j) dihedral angle; k) on straight truncation; l) multiple dihedral; m) on convex truncation.*

Degrees of standardization within both clear or fuzzy mental templates may be affected by any number of local and immediate factors, such as intensity of tool use and rejuvenation (Dibble 1995), flaking constraints imposed by raw material characteristics (Moloney 1996), proximity to good raw material (White 1995), responses to different environmental and resource conditions (Kuhn 1995), and individual flaking abilities (White & Dibble 1986). This is in addition to more general questions of the possible relationship between *total* tool form and function and the relevance of our classificatory systems to past cognition. Not all such factors can be

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seen, much less controlled in archaeological contexts. Yet the three samples chosen do avoid a number of the potential factors that might cause differences in standardization unrelated to proposed differences in mental templates. All assemblages studied here utilized high-quality flint, abundantly available in large cobbles within a kilometre of each site. In fact, both the Gravettian and Magdalenian burins were made exclusively from flint from the same Eocene Rio Maior source. The Eocene flint used at Rosh Ein Mor, while obviously not identical to that from Portugal, was likewise available in large, fine-grained nodules and all burins were made of this flint. In none of the samples is there any recognizable 'exotic' raw material used in burin production: given the proximity, fine quality and abundance of local flint to all three sites, this is not surprising. Therefore neither basic reduction strategies nor the imposition of specific form was differentially constrained by raw material. Taking large samples of burins from across sizable sites, the possible effects of individual flaking abilities, specific limited activities, and highly ephemeral uses have been minimized, if not eliminated.

Standardization

Standardization, as a conceptual tool, incorporates the notion that a product has low variability in characteristics that define the product. That is, a technological product is said to be standardized when the produced items tend to exhibit a common set of characteristics which vary little, if at all, from each other. The application of this notion to stone tools (e.g. Arnold 1984), however, is not as straightforward as it might seem. Although Mellars uses standardization as one criterion for his interpretation that modern people had more developed mental templates (Mellars 1989a, 365), he never explicitly defines what he means by the term. It seems that he is mainly referring to standardization of *form*. While standardized *process* might be included within Mellars' concept of 'imposed form', since he relates this to tools in 'various stages of their production and shaping' (Mellars 1989a, 365), it appears that the end product — the finished tool — is the basis used, particularly since burins are specifically cited by Mellars (quoted in Shreeve 1995). In this sense, standardization must be seen primarily as relating to shape and size, and the consistency of both within a given tool class. To measure the degree of tool standardization, at least three objectives should be met. First, the important characteristics of the product must be determined. Second, a coefficient measuring the degree of standardization (i.e. variability) for the relevant attributes must be clearly defined. Finally, the method for objectively comparing the degree of standardization of several assemblages should be experimentally replicable.

For pieces already having the minimal characteristics defining a burin (Brézillon 1971, 166), perhaps the simplest way to intuitively assess standardization is through the number of recognized burin types present in an assemblage. The contrast between the traditional Middle Palaeolithic typology (Bordes 1961) with its two burin types — typical and atypical — and the classic Upper Palaeolithic typology with its 18 types (deSonneville-Bordes & Perrot 1953) could not be more striking. Obviously, it is the Upper Palaeolithic typology that must be used in any comparison.

Assuming for the moment, and only for the moment, that each of the 18 recognized types does, indeed, reflect a distinct 'mental template', it still is not certain what a large number of types within an assemblage might mean in relation to Mellars' arguments. On the one hand, a large number of types in an Upper Palaeolithic assemblage could suggest a lack of standardization. On the other hand, it might document another aspect of the supposed difference between the Middle and Upper Palaeolithic — a marked increase in the number of tool types in the Upper Palaeolithic. Yet it is clear, both from the definitions of Upper Palaeolithic burins types and the way in which some different types are subsequently grouped together (deSonneville-Bordes & Perrot 1953; 1956a), that all types are not comparably different. Thus comparisons at the sub-class level might be more reasonable than at the type level. In addition, all types do not occur in any given Upper Palaeolithic assemblage, as noted by Mellars (1989a, 353). This may suggest, perhaps, that each Upper Palaeolithic group had a more limited range of mental templates than did the Upper Palaeolithic population of Europe as a whole, making the significance of any one type questionable. This particular comparison, however, includes all 18 recognized burin types, including multiple, in case each type does in fact correspond to a distinct mental template. Multiple burins pose a problem in that their number in an assemblage might reflect tool rejuvenation intensity more than any 'mental template' of a distinct tool type. Can more than one burin on a blank be used simultaneously? Given the marked differences among assemblages for multiple burin occurrence (Table 1), multiple burins are probably significant in contexts other than morphological standardization.

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The choice of placement on a blank of specific typological elements, the kinds of retouch and the choice of the blank itself also play a role in standardization or the lack of it, although they are not criteria generally used in the definition of different burin types. To maintain traditional consistency of observations for these elements, all pieces on which burins were made were oriented proximal end down and dorsal surface up and all attributes were recorded relative to that position.

Table 1. *Burin sample frequencies for the non-metric analyses.*

		Assemblage					
		Mousterian		Gravettian		Magdalenian	
		Count	Col %	Count	Col %	Count	Col%
Binary burin type	single	109	87.8%	168	72.1%	124	50.8%
	multiple	15	12.2%	65	27.9%	120	49.2%
Group total		**123**	**100.0%**	**233**	**100.0%**	**244**	**100.0%**

In addition, the dimensions of an artefact are highly significant for metric standardization, since a standardized tool type, by definition, should maintain consistent dimensions with minimal variability. Three primary variables of length, width, and thickness (in mm) were measured for each complete burin. From these measurements, the secondary metrics (derived ratios) of length to width, length to thickness and width to thickness were calculated. A fuller treatment of all these variables is contained in the papers by Hietala (1998; in press). Multiple burins (on a single blank) were treated as single burins, since the measurements of blank length, width, and thickness were common to each burin on the blank. The final sample consisted of 481 burin blanks: 111 from the Mousterian assemblage, 191 from the Gravettian and 179 from the Magdalenian.

The single most important assumption in the analysis of metrics is that the size of burins should not be an important factor in determining whether burins are more quantitatively standardized in one assemblage than in another (i.e. possess less metric variability in the final product). Since large burins exhibit greater size variability than small burins, it would be incorrect to argue that smaller burins are more standardized. That is, burins from an assemblage with smaller blank sizes cannot *a priori* be considered more standardized than burins from an assemblage with larger blank sizes. There may well be a greater technological emphasis on smaller blanks, but this is a technological observation, not an observation about tool standardization or, for that matter, technological standardization. In general, metric variability must be characterized relative to the size (mean) of the measured object in order to correctly address the issue of standardization.

A commonly accepted measure of variability when controlling for the mean is the coefficient of variation, defined as the ratio of the standard deviation to the mean (Thomas 1986, 83). Hietala (in press) has detailed inferential methods for comparing several 'coefficients of variation' when the distribution from which the measurements arise is a positively skewed Weibull distribution; positive skewness is consistent with most size distributions. Confidence intervals for individual 'coefficients of variation' are based on the asymptotic variances of the Maximum Likelihood Estimators. Derivations and solutions are given in Hietala (in press).

If the impression of greater tool standardization in the Upper Palaeolithic than in the Middle Palaeolithic is correct *universally*, then Middle Palaeolithic burin samples should show the following: 1) a different diversity of types than seen in the Upper Palaeolithic; 2) a more random selection of blanks for burin production; a greater diversity in the positioning of burins on those blanks, and a greater diversity of retouch treatments within any given burin sub-class (such as burin on truncation); and 3) their dimensions should exhibit wider variability than Upper Palaeolithic ones, relative to their size.

Analyses

Burins from the assemblages noted above have roughly comparable sample sizes: Rosh Ein Mor, 123; CPM I, 244; and Tocas, 233 (Table 1). Only complete pieces were used; that is, the burin was complete relative to the blank. Thus, a burin on the distal end of a proximally broken flake was not included, since the original length of the tool may not be its present length. If, however, the burin was produced on the proximal end of a proximally snapped flake, then the length of the tool is known and the piece was included. In the following analyses it may be noted that the measure of 'evenness' (directly related to diversity) yields the same conclusions as the measure of diversity. Analytic references to evenness, therefore, are excluded in the remainder of this study.

Burin types

Using the established burin typology (Table 2), the

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Middle Palaeolithic sample has two fewer types than the Gravettian and one fewer than the Magdalenian (Table 3, Richness Indices), suggesting that the Mousterian sample possesses a similar diversity of types as the others. A different sub-class of burin dominates each assemblage: burin on truncation in the Mousterian, dihedral burins in the Gravettian and multiple burins in the Magdalenian. While this is marked, it tells us nothing about the complexity of 'mental templates'. The Upper Palaeolithic assemblages do show somewhat higher percentages for a dominant single burin type than does the Mousterian sample. Yet for the Magdalenian the dominant type is multiple mixed burins, which suggests a rather inconsistent reuse of a blank originally selected for a different type of burin. The Gravettian sample exhibits a dominance of dihedral burins, including multiple ones, which is typical for Portugal (Zilhão 1995). Since multiple burins may possess two different types of burins on the same piece, only single burins were considered when testing for burin type diversity. Each assemblage displays a different dominant single burin type (Mousterian: on straight truncation; Gravettian: dihedral asymmetric; Magdalenian: on concave truncation). Yet Shannon's Diversity Index (Table 3), shows that all assemblages display a high diversity of single burin types (high diversity corresponds to high indices and low standardization). Thus Middle Palaeolithic single burin types are not more diverse than the Upper Palaeolithic types analyzed in this study.

Blank selection

As noted above, a major shift in the selection of tool blanks between the Middle and Upper Palaeolithic has often been cited as an important difference between these two periods. While this may have been true generally and specifically for certain types of tools, such as armatures and backed tools, it is not the case for the burin samples studied here (Table 4). All three samples are dominated by the selection of flake blanks, with the Mousterian sample equalling the Gravettian sample and even out-stripping the Magdalenian sample in the selection of blades. In addition, none of the samples shows any difference in the diversity of blanks chosen. When the dominant burin for each assemblage was examined for blank diversity, however, using the Shannon Diversity Index (Table 3), the Upper Palaeolithic samples exhibit far greater diversity than does the Middle Palaeolithic sample. Thus there is no evidence that Upper Pal-

Table 2. *Counts and frequencies of burin types within each sample.*

		Assemblage					
		Mousterian		Gravettian		Magdalenian	
		Count	Col %	Count	Col %	Count	Col%
Burin type	dihedral on snap	15	12.2%	20	8.65%	15	6.1%
	dihedral on natural surface	10	8.1%	2	.9%	6	2.5%
	dihedral angle	8	6.5%	12	5.2%	11	4.5%
	dihedral symmetric	4	3.3%	27	11.6%	8	3.3%
	dihedral asymmetric	12	9.8%	46	19.7%	11	4.5%
	multiple dihedral	4	3.3%	46	19.7%	17	7.0%
	on straight truncation	19	15.4%	4	1.7%	4	1.6%
	on straight oblique truncation	8	6.5%	11	4.7%	11	4.5%
	on convex oblique truncation	10	8.1%	2	.9%	2	.8%
	on concave oblique truncation	12	9.8%	3	1.3%	31	12.7%
	multiple on truncation	10	8.1%	0	0%	25	10.2%
	multiple on truncations	1	.8%	4	1.7%	21	8.6%
	multiple mixed	0	0%	15	6.4%	57	23.4%
	transverse on lateral retouch	10	8.1%	12	5.2%	11	4.5%
	on transverse notch	0	0%	10	4.3%	14	5.7%
	busquoid	0	0%	19	8.2%	0	0%

Table 3. *Shannon Diversity indices for various characteristics and attributes within each sample.*

	Burin type, single burins			Retouch shape, single burins			Dominant burin, blank type		
Sample	Index	Evenness	Richness	Index	Evenness	Richness	Index	Evenness	Richness
Mousterian	0.971	0.971	10	0.689	0.886	6	0.319	0.529	4
Gravettian	0.929	0.861	12	0.661	0.849	6	0.537	0.893	4
Magdalenian	0.959	0.921	11	0.700	0.899	6	0.460	0.658	5
	Blank type, all burins			**Burin position, single burins**			**Dominant burin, burin position**		
Sample	Index	Evenness	Richness	Index	Evenness	Richness	Index	Evenness	Richness
Mousterian	0.463	0.663	5	0.738	0.949	6	0.579	0.961	4
Gravettian	0.500	0.721	5	0.715	0.919	6	0.696	0.894	6
Magdalenian	0.472	0.675	5	0.723	0.929	6	0.622	0.890	5

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aeolithic blank selection was more standardized, in general, than that during the Middle Palaeolithic. Nor, more specifically, was it more standardized for the most prevalent burin type.

Shape of retouched edge

This observation is limited to the retouched portions of various types of single burins. The shape and orientation of the edge have been used in the classic typology to differentiate six different types, including the Bec-de-Perroquet (types 33–39). Since Mousterian burins are often considered 'atypical' and one aspect of that might be a roughness of the prepared, retouched edge, another category was added for when the 'truncated' edge was formed by a single blow notch. While this form occurred on 5.1 per cent of the Mousterian burins on truncation, it occurred on 23.3 per cent of the sample class of burins in the Magdalenian! There are differences in the dominant shape of retouched edge among the samples: in the Mousterian, straight truncations dominate, while in the Magdalenian it is concave truncations, and in the Gravettian both straight and concave are abundant (Table 5). In terms of standardization, however, they are equally diverse (Table 3).

Burin position

The area of a blank where the burin was produced presumably remains more consistent as standardization increases. As revealed in Figure 3, burination occurred predominantly on the distal left edge of the blank in the Mousterian sample, whereas the Gravettian and Magdalenian burins were produced equally on the distal left and right edges. All samples are moderately diverse, including samples analyzed for the dominant burin in each assemblage, as seen in Table 3. For this observa-

Table 4. *Counts and frequencies for single burin blank types within each sample.*

		Assemblage					
		Mousterian		Gravettian		Magdalenian	
		Count	Col %	Count	Col %	Count	Col%
Blank type	flake	68	64.8%	95	58.6%	67	58.8%
	blade	18	17.1%	26	16.0%	12	10.5%
	primary flake	11	10.5%	25	15.4%	29	25.4%
	core elements	3	2.9%	14	8.6%	2	1.8%
	core, chunk	5	4.8%	2	1.2%	4	3.5%

Table 5. *Attributes related to the retouched portions of single burins.*

		Assemblage					
		Mousterian		Gravettian		Magdalenian	
		Count	Col %	Count	Col %	Count	Col%
Retouch type	straight	27	45.8%	10	23.8%	6	8.2%
	straight oblique	10	16.9%	17	40.5%	20	27.4%
	convex oblique	10	16.9%	2	4.8%	2	2.7%
	slightly concave	6	10.2%	1	2.4%	11	15.1%
	markedly concave	3	5.1%	9	21.4%	17	23.3%
	single blow notch	3	5.1%	3	7.1%	17	23.3%

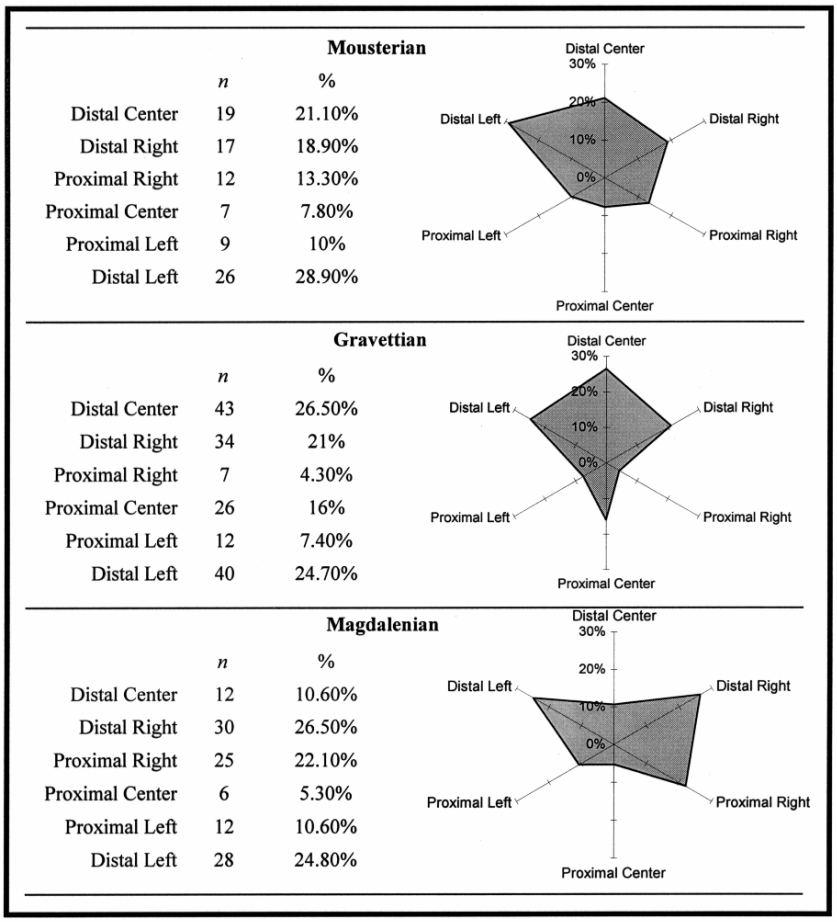

Figure 3. *Burin position frequencies for each assemblage, revealing the area of the blank where the burin was produced.*

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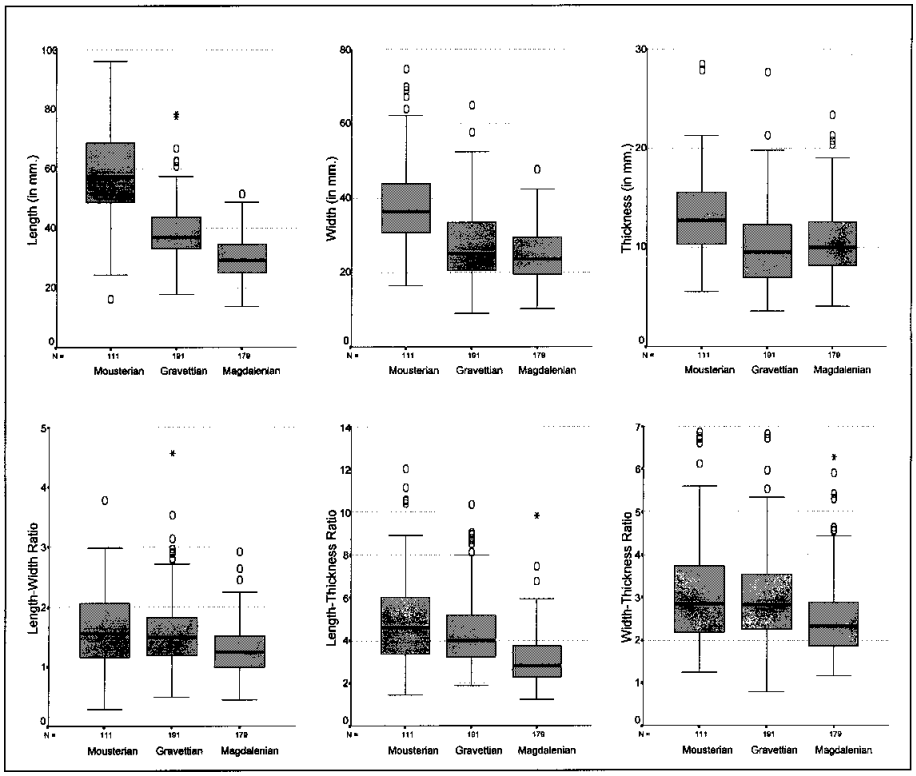

Figure 4. *Boxplots revealing the metric dimensions for burins in each assemblage.*

Table 6. *Goodness of Fit test* p*-values based on the one sample Kolmogorov-Smirnov statistic.*

Weibull tests			**METRIC**				
All data	**Sample size**	**Length**	**Width**	**Thickness**	**L / W**	**L / T**	**W / T**
Mousterian	111	.935	**.013**[a]	.362	.959	.365	.358
Gravettian	191	**.016**[a]	.138	.473	.088	.137	.357
Magdalenian	179	.241	.077	.195	.462	.109	**.029**[a]
Box plot outliers removed[(b)]	**Range of *n***						
Mousterian	106–110	.979	**.034**[a]	.803	.944	.794	.378
Gravettian	182–189	.121	**.043**[a]	.712	.893	.333	.447
Magdalenian	170–178	.249	.062	.203	.594	.144	.156

[(a)] Test fails at the type I error level of .05.
[(b)] The number of outliers depends on the individual metric.

Table 7. *Sample and Weibull coefficients of variation by metric and assemblage; the first value is for all data, the second value is with box plot outliers excluded.*

		METRIC					
Coefficient	**Assemblage**	**Length**	**Width**	**Thickness**	**L / W**	**L / T**	**W / T**
	Mousterian	.277,.268	.301,.261	.306,.272	.382,.367	.435,.368	.397,.342
Sample	Gravettian	.238,.200	.353,.335	.389,.365	.371,.304	.389,.339	.348,.317
	Magdalenian	.231,.227	.288,.283	.326,.298	.326,.301	.390,.335	.372,.299
	Mousterian	.277,.270	.325,.281	.331,.279	.386,.366	.440,.371	.406,.350
Weibull	Gravettian	.278,**.218**	**.369,.347**	**.400,.369**	.392,.311	.399,.350	.364,.325
	Magdalenian	.250,.243	.301,.292	.344,.311	.344,.311	.413,.347	.393,.317
	Combined	.268,.240	.335,.313	.364,.328	.374,.324	.414,.353	.385,.328

tion, all three samples, again, are equally diverse.

These non-metric attributes therefore provide little basis for postulating that the Upper Palaeolithic burin assemblages studied here are more morphologically standardized than the burins from the Mousterian site of Rosh Ein Mor.

Metrics

Two or more Weibull distributions have common coefficients of variation if their shape parameters are equal (Hietala in press); higher shape parameter values have lower coefficient of variation values. It is clear in a direct metric comparison of the primary variables and their ratios (Fig. 4) that there are extreme differences between the Middle and Upper Palaeolithic assemblages. This is to be expected since the sizes of Mousterian, Gravettian and Magdalenian blanks are very different, and metric variability increases with mean size. The box plots also show that the underlying distributions are generally positively skewed with outliers.

Hypotheses of Weibull distributions were tested using the one sample Kolmogorov-Smirnov statistic. Table 6 gives the *p*-values for testing the hypotheses of Weibull distributions. It is clear from this table that the Weibull distribution fits well. For example, only 17 per cent (3 of 18 tests) fail when box plot outliers are included, while 11 per cent (2 of 18) fail when box plot outliers are excluded. Consequently, the Weibull model was used to perform hypothesis tests of equal coefficients of variation for the three assemblages. Ta-

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ble 7 gives the sample (corresponding to a Normal theory model) and Weibull model coefficients of variation for the three assemblages, as well as the common Weibull coefficients when the assumption of 'no coefficient differences' is assumed. Bold face entries correspond to interpretive differences discussed below.

Table 8 gives the *p*-values for testing the null hypotheses that the coefficients of variation are equal for the three assemblages. In this table, it can be seen that the three assemblages are not equally standardized for length (outliers excluded *p*-value equals .031), width (p = .011 with outliers not excluded and p = .010 when they are excluded) and thickness (p = .019 with outliers not excluded and p = .002 when they are). Seven of the 12 comparisons, however, including all the ratios fully support the equal coefficient of variation hypotheses.

Figure 5 gives the graphic portrayal of 95 per cent confidence intervals for the coefficients of variation. It is very clear from these plots that confidence intervals for the derived ratios of length to width, length to thickness and width to thickness strongly overlap both for situations where all data were analyzed and when box plot outliers were excluded. This visual confirmation of equal coefficients of variation for the ratios is supported by their respective *p*-values (*p*-values ranging from .090 to .695).

Length has *p*-values of .260 (all data) and .031 (outliers excluded). Figure 5 clearly shows that, in the case of all data, the confidence intervals overlap. When the outliers were excluded, the Mousterian interval strongly overlaps with the Magdalenian, but not with the Gravettian. The Gravettian shows a higher degree of standardization (lowest coefficient of variation) than either the Mousterian or Magdalenian, but the Mousterian is not different in this respect from the Magdalenian.

For width, the null hypotheses of equal coefficients of variation for the three assemblages are strongly rejected by the Weibull model (*p*-values of .010 and .011), although there are no differences between the Mousterian and Magdalenian coefficients. When all data are employed, the Magdalenian coefficient is slightly smaller than the Mousterian (Table 7; Fig. 5), but when the outliers are excluded, the reverse is true. In general, standardization is lowest for the Gravettian (i.e. highest coefficient of variation), while the Mousterian and Magdalenian display no significant differences.

Thickness has *p*-values of .019 (all data) and .002 (outliers excluded) for the hypothesis of equal coefficients of variation using the Weibull model. Table 8 and Figure 5 demonstrate, both all data and with outliers excluded, that the Gravettian coefficients are higher than the Magdalenian and Mousterian coefficients, while the Mousterian is

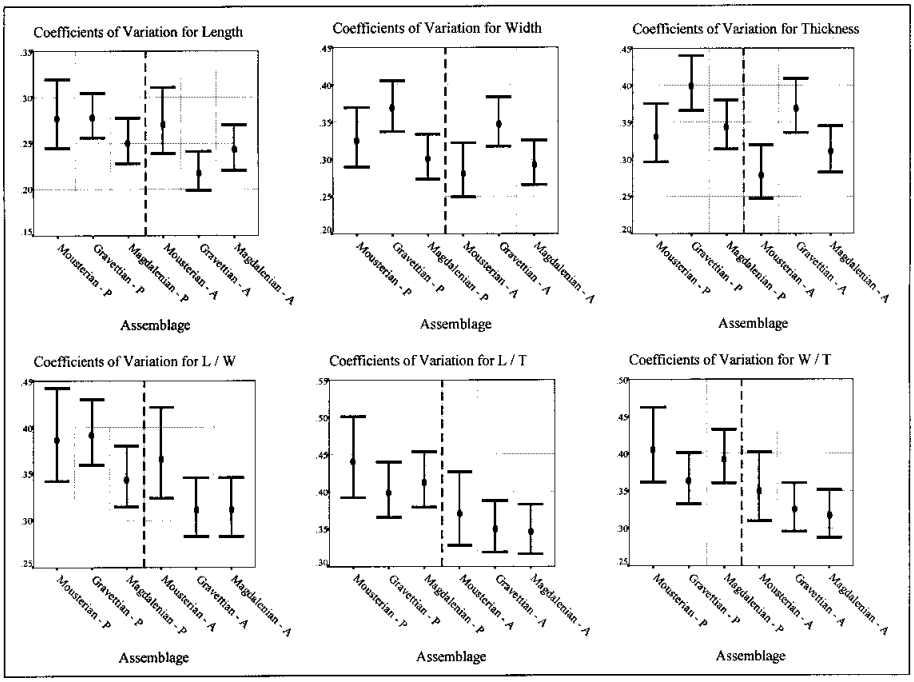

Figure 5. *Confidence Intervals for the Coefficient of Variation using the Weibull Model: P indicates outliers are present; A indicates outliers are absent.*

Table 8. *Equal coefficient of variation test* p*-values based on the Log-Likelihood Ratio statistic.*

	METRIC					
Metric distributions assumed to be Weibull	**Length**	**Width**	**Thickness**	**L/W**	**L/T**	**W/T**
All data: total sample size = 481	.260	**.011**[a]	**.019**[a]	.123	.453	.312
Outliers excluded: sample sizes of 463 to 473	**.031**[a]	**.010**[a]	**.002**[a]	.090	.695	.472

(a) Test fails at the type I error level of .05.
(b) The total number of outliers depends on the individual metric.

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slightly lower than the Magdalenian. The results of these thickness tests indicate that Gravettian burins in this sample exhibit less standardization (i.e. highest coefficient of variation) than either the Mousterian or Magdalenian, while no difference was found in the degree of standardization in the Mousterian and Magdalenian samples.

In conclusion, these data provide no differences in metric standardization between the Mousterian and Magdalenian burin samples. The Gravettian sample, on the other hand, shows a tendency for greater length standardization but less standardization for width and thickness than do the others. These results clearly demonstrate that these Upper Palaeolithic burin samples are not more metrically standardized than is the Mousterian burin sample.

Discussion and conclusions

This study of a single tool class, burins, has amply shown that for these samples, at least, there is no difference in the degree of standardization between the Middle and Upper Palaeolithic, whether standardization is defined by non-metric or by metric attributes. That is not to say that some unstudied Upper Palaeolithic burin sample might not show different standardization in either metric and non-metric attributes compared with the Middle Palaeolithic, Rosh Ein Mor sample. What it does show is that, accepting the implicit premises behind the original observation, there is not, *in all cases*, greater standardization for the same tool class in the Upper Palaeolithic than in the Middle Palaeolithic. If, therefore, standardization and the additional number of within-class types reflect directly upon increasing clarity of mental templates, there is no evidence from these burins that there was any difference between the clarity of mental templates possessed by the makers of burins at Rosh Ein Mor at 200,000 BP and for those who made the burins at Tocas after 20,000 BP. It is far from clear, however, that the original assumptions behind Mellars' observation (1989a, 365) are valid.

The stated impression of Mellars (1989a; 1996a) that Upper Palaeolithic tools are more standardized than those of the Middle Palaeolithic and that this difference relates to mental templates, makes a number of assumptions:

1. that increasing clarity of mental templates is an evolutionary trend;
2. that increasing clarity of mental templates results in increasing standardization within tool classes and, therefore, that the degree of tool class variability should approximate the relative clarity of the mental template of their makers; and,
3. that increasing clarity of mental templates can be effectively monitored through time by an increase in the number of definable types within a tool class.

While it is quite possible, even probable, that increasing clarity of mental templates was part of hominid evolution, measuring the clarity of mental templates through tool standardization is more problematic. The last two assumptions noted above cannot be taken for granted because differences in relative standardization can result from sources other than differences in mental template clarity. Traditionally defined stone tool types are neither necessarily appropriate vehicles through which to judge relative standardization, nor do their numbers necessarily reflect original mental templates.

Are mental templates, in fact, manifested in the archaeological record by increasing standardization? Deetz (1967, 45) defined a mental template as 'the idea of the proper form of an object (which) exists in the mind of the maker'. If, in fact, standardization results from this idea, it takes two major forms: standardization of process and standardization of product. Standardization of process is well attested from the Acheulean, in both reduction strategies (Bordes 1961; Boëda 1986) and in bifacial tool production (Isaac 1972; 1977; Jöris 1997; Saragusti *et al*. 1998), but this is a step removed from the concept of 'proper form'. While standardization of both process and form are recognized in pre-modern contexts, there is no agreement as to their meaning in those contexts. Standardization has been 'explained' in terms which range from developed mental abilities (Gowlett 1984), to a fancifully Freudian interpretation (Kohn & Mithen 1999), although its *a priori* association with symbolic behaviour, even in Upper Palaeolithic contexts, has been convincingly questioned (Chase 1991).

While its meaning or meanings are still conjectural, standardization existed well before the appearance of behaviourally modern people. Yet standardization of process does increase standardization of blank form, which, in turn, affects dimensional standardization of 'final products'. This is most clearly seen in the significant role that consistent blade reduction strategies had in resulting blank standardization, both by modern and pre-modern peoples (Bar-Yosef & Kuhn 1999). The extreme example of this may be the Terminal Gravettian of Portugal where a sub-set of blade production — carinated reduction — produced bladelet blanks which were morphometrically much more standardized than the elongated blanks produced from normal

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blade cores in the same assemblage (Almeida 2000). Thus, since standardization of process has a very long history in the archaeological record, consideration of relative standardization as it may reflect upon different developmental levels of mental templates *across the Middle to Upper Palaeolithic boundary* should be limited, as much as possible, to standardization of form, and not process. Even this, however, raises additional questions.

Standardization of form, as defined above, is relative, and its measurable attributes are potentially subject to a large number of variables unrelated to mental templates. Furthermore, the doubts expressed about the past cognitive 'reality' of Middle Palaeolithic tool types (e.g. Dibble 1995), on the one hand, and the explicit acceptance of their cognitive 'reality' (Bordes 1981; Perlès 1993), on the other, would seem to call for considerable justification before any defined tool type, Middle Palaeolithic or Upper Palaeolithic, be directly linked with a mental template.

Is there any *a priori* reason to believe that the tool types defined by deSonneville-Bordes & Perrot (1953; 1954; 1956a,b) or anyone else, for that matter, had cognitive reality to those who made the tools? Is it not as likely that those recognized types more closely reflect the mental templates of the classifier than of the maker? While a high level of morphological redundancy within an assemblage intuitively lends credence to this approach, it is by no means certain that it is a universally reasonable assumption. Even in ethnographic contexts, there are people who view individual retouched items not as single tools but as a conglomeration of functionally unrelated tools on the same blank (White 1969). In such cases, morphological or even metric standardization may only reflect basic technological processes and use, rather than mental templates of what a 'tool' should be.

Even if defined tool types, in their entirety, should be appropriate for judgements about standardization, what reason is there to believe that the number of types recognized in any type-list corresponds to the actual number of different mental templates held in the minds of any group of makers? The vagaries of type-list formulation are legion, since each type-list, including that of deSonneville-Bordes & Perrot, was created to elucidate a specific archaeological problem, rather than as an objectively derived morphological division of lithic artefact groups. Can the different number of types in the type-list for the Upper Palaeolithic of Western Europe (deSonneville-Bordes & Perrot 1954) and that for the Middle Palaeolithic of Western Europe (Bordes 1961) actually reflect the number of 'proper forms' recognized by Upper Palaeolithic vs. Middle Palaeolithic peoples when they monitor different things (Simek & Price 1990)? Does the addition of defined types mean clearer mental templates? Did the Epipalaeolithic folk of the Mahgreb, with their 111 types (Tixier 1963), have clearer mental templates than the Upper Palaeolithic population of Western Europe, who had only 95 types? There are many examples where the number of types recognized by one archaeologist differs markedly from the number recognized by another archaeologist for the same period. For instance, while Bordes (1961) recognizes 27 biface types for all of Western Europe, Guichard & Guichard (1968) recognize 37 for just a small area of Nubia. These and other problems are clearly laid out by Grayson & Cole (1998, 929–30) and need not be elaborated here.

It is equally important to ask whether it should be accepted, without specific evidence, that retouched tools, as they are recovered from excavations, represent a 'desired end product': Deetz's, 'proper form of an object'? A strong argument can be made that many of the tools found in Palaeolithic contexts represent what in fact were considered to be no longer appropriate, or at least represent a distorted version of the original 'proper form'. The tendency for flint and other similar rocks to blunt quickly when used in contact against almost any material necessitates frequent resharpening of the working edge or edges. If the tool was used even for a relatively short time, it required a number of rejuvenations, the cumulative effect of which was significantly to change the size, and perhaps the shape, of the original tool (e.g. Dibble 1984; 1987; 1995; Jelinek 1976). It seems unlikely that the last, abandoned state of any tool reflects the maker's mental template of the tool as initially made.

There are numerous examples of such changes in tools, both ethnographic (e.g. Gallagher 1977) and archaeological (e.g. Nijs 1990). Perhaps the best ethnographic example is of Ethiopian hide scrapers (Clark & Kurashina 1981), where the exhausted, discarded scrapers are significantly different both typologically and metrically from the unused 'desired' form. In Upper Palaeolithic typological terms, the unused scrapers are classifiable as double end-scrapers on retouched flakes, while the exhausted examples are circular scrapers. Had the discarded sample been from an archaeological context, a reasonable interpretation would have been that the recovered scrapers were desired end-products, exhibiting a high degree of morphological standardization (Clark &

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Kurashina 1981, 309–10). Statistical comparisons of the coefficients of variation between the unused and the used (discarded) scrapers show the latter to be more standardized. If this ethnographic example can be generalized, then it might be expected that the greater the in-class standardization, the greater the likelihood that the objects represent not 'desired end products' but exhausted, no longer desirable, tools.

Studies of archaeological material have provided clear examples of such changes in Middle Palaeolithic tool morphology and size (e.g. Dibble 1984) and there is reason to suppose it is also true in the Upper Palaeolithic. A specific example is provided by a refitted sequence of 'a tool' at Boker Tachtit in the Negev, Israel (Marks & Kaufman 1983, 103, fig. 5–20b). The recovered tool was typologically a multiple dihedral burin. Its initial form, however, was that of a transverse convex side-scraper that then passed through two stages of being different transverse burins on lateral retouch, before being transformed into its final state.

There is obviously considerable distance between the assured modern mental template of the hide worker and the admittedly unknowable mental template of the burin maker. In the ethnographic example, the discarded scrapers demonstrate that the workers had a clear mental template of what was no longer useful, mainly conditioned by the limits imposed by the haft and increasing edge angles (Clark & Kurashina 1981, 310). In the archaeological record it is difficult, if not impossible, to ascertain where a specific recovered tool was positioned in the production-use-discard cycle. In the Boker Tachtit example, there is no morphological indication from its discarded state that this burin ever passed through a series of other types. In addition, since its discarded state conforms to a typical, defined tool type, there is no way to tell from its morphology whether it was discarded because of some no longer usable attribute, whether it was discarded merely because the job at hand had been completed, or whether the type itself represents an exhausted, rather than a desired, 'proper form'. All of this reinforces the notion that existing, defined tool types, whether Middle or Upper Palaeolithic, do not necessarily have a direct association with 'proper form' or clarity of mental template as applied to tool production.

This far-from-exhaustive discussion raises sufficient doubts as to the relationships among morphological and metric standardization of within-class tools types, the Palaeolithic cognitive reality of our current types and type-lists, and clarity of mental templates. It suggests that standardization (or any other unstandardized measure) is not an appropriate vehicle to discriminate between modern and pre-modern mental abilities. On the other hand, standardization studies of both process and form, may be an effective, albeit indirect, means of getting at aspects of the original archaeological record which tend not to survive. In this case, it is important to ask when standardization of either process or form would have been adaptive. What constraints might there have been which would have encouraged standardization?

The most obvious adaptive advantage in standardization came with the introduction of composite tools and the necessity to haft stone tools, as noted by Bar-Yosef & Kuhn (1999). They link this with the appearance of consistent blade production in some Upper Palaeolithic contexts, but it was without question most clearly manifested later with the appearance of geometric microliths. Within the late Natufian of the Levant, for instance, the abundance of geometric microliths reflects *both* standardization of process (microburin technique) and standardization of form (crescents, as opposed to triangles or trapezes) (Henry 1989, 89–93). The recovery of mastic on comparable geometrics in Sinai (Bar-Yosef & Goring-Morris 1977, 199) provides direct evidence that these were hafted, representing an excellent example of replaceable, as opposed to maintainable, tools (Bleed 1986). Obviously the more standardized the replaceable tool, the more adaptive it would be. Such clear evidence for standardization associated with composite tools is unfortunately very rare in the Middle Palaeolithic and in the early Upper Palaeolithic. It is this rarity that makes standardization studies potentially valuable in Middle and early Upper Palaeolithic contexts. Hafting has been inferred for Middle Palaeolithic tools from use-wear (e.g. Anderson-Gerfaud 1990, 406–7; Beyries 1987), from edge damage (e.g. Shea 1998), as well as from the presence of mastic (Boëda *et al.* 1996), but whether these tools also exhibit relatively more standardization than others has not yet been addressed.

In standardization studies it is important to distinguish between the stone tool *in its entirety* and that portion of the tool that fits into the haft, for it is the latter that would be directly constrained in shape and/or dimensions by the pre-existing haft (e.g. Straus 1990). Thus in dealing with standardization, it is most likely that different parts of hafted tools would be differently standardized. Hafted tools recovered from archaeological contexts are likely to represent the end of use-discard cycles, where standardization would be seen not on the rejuvenated

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portion of the tool but at its opposite, hafted end. Such a situation might well be used to infer the use of hafting, in placement of use-wear and the presence of mastics, when no direct evidence exists of organic hafts. In fact, while no one doubts that the vast majority of American Paleo-Indian points were hafted, not a single point has ever been found in a haft. Thus the recognition of hafting must be by inference in the vast majority of cases.

A combination of probable mastic, use-wear, and standardized basal forms on Eastern Micoquian bifacial foliates from Starosele, Crimea (Kay 1999; Hardy & Kay 1999), actually links standardization of form with the other means for inferring hafting. In addition, it is significant that only the basal third of the foliates exhibit the same form. The distal two-thirds of each foliate differ markedly, depending upon the degree of rejuvenation each underwent *while in the haft.* Comparable differential standardization is clear on Clovis points in Texas, since points were resharpened in their hafts and were made 'to fit hafts, and not visa versa' (Meltzer & Bever 1995, 70). These examples indicate that a tool *in its entirety* is probably not optimally suited for meaningful studies of standardization.

Although it is clear that standardization of process in blank production, in and of itself increases standardization of form, a second step is also important for relative standardization of tools: the selection of blanks for tool production. In Middle Palaeolithic contexts, a much higher percentage of the largest blanks tend to be retouched than of the smaller blanks (e.g. Chabai 1998, 250). During the Upper Palaeolithic, the retouch of blades, randomly selected from all blades produced, will have resulted in more standardized tools than if tool blanks were selected randomly from all blanks produced, including both blades and flakes. Even within assemblages that are dominated by blade production, such as the Ahmarian of the Levant (Gilead 1981), there appear to be quite different degrees of standardization based not upon imposed form but on differential blank selection for different tool classes. While end-scrapers and burins were made on the blanks with the largest available dimensions, it was essentially overall size that determined selection. On the other hand, blank selection for El Wad points shows that width was significantly more standardized than either length or thickness, given their respective standard deviations (Jones *et al.* 1983, fig. 9–5, 294). Since the retouch on El Wad points is marginal, it scarcely modifies the chosen blank. Therefore it was in blank selection that standardization was manifested.

While this study of a limited sample of burins clearly negates the universal truth that Upper Palaeolithic tools should be more standardized and have more within-class types than their Middle Palaeolithic equivalents, it also forced a closer look at the assumptions underlying those factors that might have made standardization and a greater number of types adaptive, and how these are presently defined. Although increasing clarity of mental templates might well have been an evolutionary trend, it is unlikely to have played any significant role in degrees of standardization across the Middle to Upper Palaeolithic boundary. There were simply too many other, more mundane, variables at play. It would appear that degrees of within-tool-class standardization across this boundary are best explained in terms of specific adaptive situations, rather than by any inherent differences in hominids. The challenge now is to use the concept of standardization to help elucidate some of the vast variability seen in Palaeolithic assemblages.

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Comments

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This is a stimulating and well-structured discussion of the long-debated issues of changes in tool morphology across the Middle to Upper Palaeolithic transition and their possible behavioural implications. My only negative reaction is that by focusing so strictly on the issue of 'standardization' the authors may have thrown away the baby with the bath water**.** As the article points out, standardization is a difficult entity to define and measure, but so far as I am aware neither I nor anyone else has ever suggested that *all* Upper Palaeolithic tool types are more morphologically standardized than *all* Middle Palaeolithic types. Burins, of course, are notoriously variable tool forms whose final (i.e. discarded) shape is inevitably heavily influenced not only by largely functional considerations, but also by the history of repeated resharpening and reduction applied to the

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individual tools. So to demonstrate that two samples of simple burin forms from an Upper Palaeolithic site in Portugal are not demonstrably more morphologically standardized than a single sample of burins from a Mousterian site in Israel need not come as any great surprise. If the analysis had focused on some of the more specialized and distinctive Upper Palaeolithic burin forms, such as Noailles, Raysse, busqué or parrot-beak burins, the results no doubt would have been rather different.

But the main limitation of the article is that by focusing so narrowly on the issue of standardization it largely sidesteps what I have always emphasized as the main contrast between Middle and Upper Palaeolithic tool morphology — i.e. the element of deliberately 'imposed form' in tool manufacture (Mellars 1989b; 1991; 1996a,b; 1998 etc.). In 1996 I defined this as 'a deliberate attempt to influence and control the overall shapes of the retouched tools which went beyond their immediate functional requirements. Typically this involves large-scale reduction of the original flakes in a way which influences not only the active working edges of the finished tools but also their visual appearance' (Mellars 1996a, 133). The whole emphasis in this definition therefore is not on standardization *per se* (especially if defined in a purely metrical sense) but on the deliberate *reduction* of the original flake blanks in order to impose some deliberate and visually distinctive appearance on the finished tools. In practice of course this usually results in increased metrical standardization, but that is not the main element in the definition. I have often chosen to illustrate this contrast by the comparison between Mousterian backed knives and the technologically and probably functionally similar Châtelperron points, which illustrate the point in a particularly striking and unambiguous way (see Mellars 1989b, figs. 20.2 & 20.3; 1991, figs. 2 & 3). Whereas on the Mousterian backed knives the retouch is normally relatively light and usually adheres closely to the original margins of the parent flakes or blades, in the case of the Châtelperron points the retouch bites deeply into the central parts of the parent blanks in order to impose a distinctively arched and pointed form to the tool as a whole. That this is not due simply to the manufacture of Châtelperron points from blade rather than flake blanks is shown by the fact that a good proportion of Mousterian backed knives are also made from blades, with no apparent attempt to impose any special shape on the finished tools (see Mellars 1996a, fig. 4.30).

The above comparison provides the most convenient way of illustrating the notion of imposed form, but the same point could have been made by reference to many other distinctive and widely recognized Upper Palaeolithic tool forms, such as Font Robert points, Solutrian willow-leaf, tanged and shouldered points, Gravette points, *fléchettes*, '*elements tronqués*', triangles, Noailles burins, Creswellian and Cheddar points, Hamburgian and Ahrensburgian points, Azilian points, Steletskaya points, Szeletian foliates, Uluzzian crescents, etc. (see Djindjian *et al.* 1999, fig. 6.7). For all of these forms it seems to me totally impossible to escape the notion that some specific mental templates prescribing the overall shape and appearance — as well no doubt as the intended functions — of the tools must have existed in the minds of the people who made them. Indeed, one of the most striking and widely recognized overall contrasts between Middle and Upper Palaeolithic tool morphology is the vastly greater range in individual, idiosyncratic, and for the most part relatively short-lived 'type-fossils' that can be identified within the 30,000 years of the Upper Paleolithic sequence, compared with the conspicuous scarcity of similar, recognizable type-fossils within the preceding 250,000 years or so of the Middle Palaeolithic. The increased complexity, variety and visual distinctiveness of Upper Palaeolithic tool forms is an empirical feature of the archaeological record, not a hypothetical construct or an optical illusion created in the minds of the typologists involved. Incidentally, I never suggested that this kind of imposed form was *entirely* lacking from the Middle Palaeolithic (as the forms of some triangular and foliate bifaces seem to reveal) simply that it was a very much rarer and more isolated phenomenon in the Middle Palaeolithic tool repertoire than it was in the Upper Palaeolithic (Mellars 1996a, 133–6).

The critical question, of course, is what this greatly increased element of variety and imposed form in Upper Palaeolithic tool types means. Here there is no doubt scope for a spectrum of responses, ranging from the primarily functional to the explicitly social or symbolic. My own suggestion was that this rapid proliferation in imposed form in tool manufacture might be reflecting a parallel increase in the complexity and structure of language — especially the range and complexity of linguistic vocabularies (Mellars 1996a,b; 1998, etc.). To cut a long story short, I was suggesting that the highly distinctive, repetitive forms of many Upper Palaeolithic tools could be reflecting an explicitly visual and symbolic, as opposed to purely functional, dimension of the tools, in which these distinctive visual and symbolic forms were in some way way reflecting, and perhaps sym-

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bolizing, the names applied to the tools themselves. I argued that this kind of imposed *visual* dimension in tool manufacture is even more striking and explicit in the forms of many Upper Palaeolithic bone and antler tools, and also parallels closely the first emergence of both personal ornaments and naturalistic and abstract art in the initial stages of the Upper Palaeolithic. In other words, I suggested that the proliferation of new, visually distinctive imposed form in Upper Palaeolithic tools (in both stone and bone/antler) could be simply part of a far more general and widespread 'symbolic explosion' which could well reflect a similar explosion in the structure and complexity of language. This remains an hypothesis of course, which may or may not turn out to be true. And it also raises issues about the nature of language among the final Neanderthal (e.g. Châtelperronian) communities in Europe which are far too complex to discuss here! (see Mellars 1999). But there is certainly much more to changes in tool morphology across the Middle–Upper Palaeolithic transition than a question of 'standardization', and the ultimate explanation of this increased complexity, diversity and visual distinctiveness in tool manufacture will inevitably require a number of functional, as well as social, symbolic and cognitive components.

From Steven L. Kuhn, Dept. of Anthropology, University of Arizona, Tucson, AZ 85721-0030, USA.

Marks, Hietala & Williams make a strong case against the generalization that Upper Palaeolithic artefacts always show greater levels of 'imposed form' than Middle or Lower Palaeolithic artefacts, at least according to widely-used measures of standardization. While I am quite comfortable with the goals and findings of this study, I am less comfortable with measures of standardization commonly used by archaeologists. It appears that the authors share this discomfort, to the degree that some of the caveats they express in the final section of the article threaten to undermine the rationale for their own analyses.

Following the lead of past studies, Marks *et al*. adopt two different strategies for evaluating whether one assemblage of artefacts is more standardized than another. The first is based on the argument that that a more standardized assemblage should show stronger evidence of having been created in terms of several discrete designs, or of having a greater number of distinct designs. In pursuing this strategy, Marks *et al*., like others (Grayson & Cole 1998) assume that the most common (deSonneville-Bordes) Upper Palaeolithic typology has some — admittedly undemonstrated — significance in terms of prehistoric design criteria and that these types actually conform in some way to 'mental templates'. As decades of debate over the significance of formal variation in Palaeolithic tools demonstrates, this is by no means self-evident. To be fair, the authors did not select the deSonneville-Bordes typology arbitrarily. Still, we need to have some confidence in the meaningfulness of the category distinctions in order to have confidence in the results. Just as the way one partitions a continuous measure can affect the shape of a histogram, the use of arbitrary (and non-equivalent) categories can alter the appearance of evenness or bias in category frequencies.

A second strategy for assessing standardization, examining how closely different exemplars might fit an ideal model, stands behind the analysis of coefficients of variation in artefact metrics. Standardization is an active cognitive process, the imposition of design criteria. Of course, archaeologists do not directly observe the active cognitive process. Instead, they measure one possible outcome of it, morphological redundancy. Redundancy in form *can* result from application of design standards, but it can also be a result of factors such as raw material constraints, technological constraints, life-histories of stone tools, and even imposed categories (e.g. Chase 1991). The simple fact that things end up being close to the same size and shape does not constitute *prima facie* evidence that prehistoric artisans intended them to be that way. By that same token, comparable levels of morphological redundancy could well result from the operation of different processes in different assemblages (imposed standards in one case, artefact life histories in another).

Because the measures commonly used are not perfect, the final chapter on issues of differential standardization in the Middle and Upper Palaeolithic has yet to be written. The researchers who first made the statements about imposed form in the Upper Palaeolithic are experienced observers, and we need to ask what might have led them to make such generalizations in the first place. The authors are probably correct in asserting that a preference for blade blanks confers a greater appearance of formal regularity on some Upper Palaeolithic assemblages, but this begs the question of why blades became more common in the Upper Palaeolithic. At the same time, there are many other ways in which design criteria might be addressed. The final section of the article offers some creative and useful suggestions, particularly with respect to treating the whole tool, the working margin(s) and the hafting element individually.

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From William Davies, Department of Archaeology, Downing Street, Cambridge, CB2 3ER.

Discussions on the morphological 'standardization' of stone tools have not been as prominent or numerous as they deserve, and this article makes an extremely valuable contribution to the subject. If it helps us to progress beyond looking at 'standardization' within assemblages without questioning why this might be so, then it will have performed a very useful service. I have long thought that the most interesting perspectives on lithic tools are the ones which emphasize their underlying *behavioural* characteristics and choices in manufacture, rather than their simple typological attributes. Such approaches at least enable us to set any 'standardization' we might see in the rather more interesting area of human responses to raw material qualities (their availability, nodule size, [grain] quality, whether 'exotic' or 'local', etc.).

Burins, as the authors readily note, are of equivocal use for studies of tool standardization. This, I venture, is because we define them by a manufacturing technique: they are made by the removal of burin spalls, which might have been done for a variety of reasons, perhaps as a simple way of obtaining a sharp edge, or for thinning/reducing the blank. We could thus be lumping artefacts together which were made to perform a wide variety of functions. I should be very interested to see similar work done on other groups of tools, so that we can begin to assess the range of human responses to lithic blank attributes in the manufacture of different tools. My own work on endscrapers from the Aurignacian of Europe (Davies 1999) reaches some similar conclusions to those outlined by Marks *et al.*, although I would strongly dispute their claim that endscrapers and burins are equally expedient and subject to resharpening. The range of endscraper forms which I encountered across Europe in the early Upper Palaeolithic does not appear to support a resharpening hypothesis, where one might expect greater formal similarity in assemblages as exhausted tools were discarded. I found plenty of endscrapers which were made on blanks which were by no means exhausted. The general assumption of 'resharpening', based upon ethnographic parallels, risks being just as much of a would-be universalist paradigm as 'standardization'. Likewise, hafting cannot be assumed to have been universal for all Upper Palaeolithic tools. These concerns are essentially all hypotheses which require testing.

One interesting aspect of Marks *et al.*'s work is their discussion of blank selection for tools, and how this might vary between different classes of tools. I encountered such selection in my work on endscrapers (especially for blank thickness in ordinary endscrapers). One surprise, though, was the use of 'exotic' (long-distance) materials for the production of endscrapers, tools presumed to be 'mundane'. We have to confront the strong possibility that Upper Palaeolithic people were more flexible and adaptable in their values and behaviours than we often assume. Furthermore, our imposed dichotomy between 'tools' and 'cores' is, I believe, false. Carinated scrapers may or may not have been used as bladelet cores, but they also show evidence of use as tools: why should 'tools' and 'cores' have been mutually-exclusive to Palaeolithic peoples? This assertion of course impinges upon our ideas of 'mental templates', but at the same time it alerts us to the probability that Upper Palaeolithic people often saw multiple tool possibilities in lithic blanks. This behavioural flexibility, rather than just standardization of form, might be a fruitful avenue to pursue in assessing differences between what Marks *et al.* call 'modern and pre-modern mental abilities'.

From Geoffrey A. Clark, Department of Anthropology, Arizona State University, Tempe AZ 85287-2402, USA.

For a long time now, many American workers have been sceptical of generalizations about the nature of the Middle–Upper Palaeolithic transition — in Europe, and everywhere else. As I, and others, have pointed out repeatedly in various journals (including *CAJ*: Clark & Willermet 1995), the perceptions of 'imposed form' and 'formal standardization', of which Mellars makes so much in respect of implied differences in the cognitive capacities of Neanderthals and moderns, are illusions; an inevitable consequence of the application of two different typologies, one (the UP) more fine-grained than the other (MP) (Mellars 1989b; 1996a; Clark 1997; 1999). In addition, the Upper Palaeolithic typology emphasizes allegedly time-sensitive stylistic markers and overall morphology; the Middle Palaeolithic typology emphasizes edge modification and edge shape. Most workers make *a priori* decisions at the outset of an investigation (usually based on archaeological index types, and/or dates) which predetermine whether assemblages are considered 'Middle' or 'Upper' Palaeolithic. They then apply the 'appropriate' typology. But there is an enormous amount of equifinality (or formal convergence) in lithic reduction, and — given similarities in raw material pack-

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age size and quality (which in turn affect the size and shape of the blanks upon which retouched tools are made) — different processes can produce artefacts which are typologically identical to one another. In fact, Marks himself demonstrated this conclusively at the stratified and dated Middle–Upper Palaeolithic transitional site of Boker Tachtit, in the central Negev highlands. Through extensive refitting of cores, he was able to show that typologically identical Levallois points were made early on from classic 'Middle Palaeolithic' Levallois point cores and, toward the end of the sequence, from classic 'Upper Palaeolithic' single platform blade cores, the one production stream gradually replacing the other (numerous publications, but esp. Marks & Kaufman 1983). The major implication of this work is that typology can vary independently of technology (something very few European workers realize). This very significant conclusion is practically always obscured in studies that concentrate on the typology of retouched stone tools.

In my opinion, Mellars has a completely unwarranted faith in the ability of Bordesian typological systematics to answer significant questions of Palaeolithic pattern and process. I think the reason for this faith is that he digests and analyzes literature — he doesn't do primary fieldwork himself. People who do (Marks, Straus, myself, etc.) recognize immediately just how limited, subjective and essentialist typological systematics can be. And the French, who do a lot of primary fieldwork but who exhibit neither a concern with, nor even an awareness of, epistemology in their research traditions, don't see the problems with this (Binford & Sabloff 1982; Cleuziou *et al*. 1991; Sackett 1988; 1991; Clark 1993; 1996; Coudart 1999; Audouze 1999; Scarre & Stoddart 1999; Bisson 2000). I've written extensively on the different assumptions, preconceptions, and biases that underlie differences in the logic of inference between the research traditions of Latin Europe and those of the anglophone world (Clark 1993; 1996). Intellectually, Mellars belongs more in the Latin European camp than he does in the anglophone one.

The literature is replete with examples of this unwavering commitment to typology, and the lengths to which some will go to preserve its shopworn, threadbare, moth-eaten logic of inference (e.g. Zilhão & d'Errico 1999; Goren Inbar *et al.* 2000). Card-carrying typologists tend to accept without question the reality of the conventional, normative, analytical units created by several generations of European prehistorians, and to assume that they detect pattern at the levels of 'cultures' or 'technocomplexes', which are seldom defined. They also exhibit an annoying tendency to correlate typologically-defined archaeological assemblages with biological taxonomic units (usually 'Neanderthals' and 'modern humans') when the credibility of doing this, and the credibility of the units themselves, are never explicitly called into question. To these workers, Palaeolithic archaeology is essentially history projected back into the Pleistocene, and patterns are typically explained *post hoc* by invoking processes (e.g. migrations) analogous to those operating in recent historical contexts. The whole approach is predicated on (1) the existence of tool-making 'traditions' manifest in artefact form that are detectable over hundreds of thousands (even millions) of square kilometres; (2) that such 'traditions' (ways of making stone tools transmitted in a social context from one generation to the next) persisted unchanged and intact over tens (or, in the case of the Lower Palaeolithic, hundreds) of millennia, and (3) that they are detectable at points in space (e.g. Europe, the Levant) separated by thousands of kilometres. While the pattern similarities themselves are uncontested, what is supposedly causing them to occur (historical connectivity over vast geographical areas and time ranges) is deeply problematic. It is possible, however, to explain pattern similarities in Palaeolithic archaeological assemblages without recourse to typology-based tool-making traditions. I make two points.

First, there are serious logical and conceptual problems with the notion of a cultural component in the form of (most) Palaeolithic artefacts. For one thing, the time–space distributions of prehistorian-defined analytical units (e.g. the Aurignacian) *exceed by orders of magnitude* the time–space distributions of any real or imaginable social entity that might have produced and transmitted them. Unless one resorts to essentialism (e.g. there is an ineffable 'Aurignacianness' manifest in the appearance of, for example, Dufour bladelets), there is simply no behavioural or cultural mechanism whereby a hypothetical tool-making tradition could have been transmitted over thousands of years and millions of square kilometres. Thus, something other than historical connectivity must account for pattern similarities.

For another, we have no guarantees that the basic analytical units themselves are discrete in space and time, and are 'the same thing' whenever and wherever they are found. In fact, it is highly likely that they are not. The Aurignacian is a good illustration of this problem. The French Aurignacian is defined typologically by the presence of carinate endscrapers, blades, blades with scalar retouch

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('Aurignacian blades'), strangled blades, Dufour bladelets, split-based bone points, *sagaies*, etc., as well as by a range of non-lithic criteria (e.g. ornaments, 'art', 'well-organized' campsites). The Levantine Aurignacian is a flake industry bearing no resemblance whatsoever to its French counterpart (Marks & Kaufman 1983; Coinman 1998). It almost entirely lacks personal ornaments, bone or antler tools, figurines, portable art in general, parietal art, burials, and 'well-organized' campsites, and, when these features of 'Aurignacianness' do appear together as a package, it is only in the later phases of the Epipalaeolithic, in the Natufian, after *c.* 12 kyr ago (Marks 1994). Apart from the occasional appearance of carinated tools in a few Levantine Aurignacian levels (e.g. K'sar Akil, Level 13), the only similarity between the French and the Levantine Aurignacians is the name itself, imported from France by several generations of Levantine scholars trained in the francophone tradition. So whatever the Aurignacian is, it is manifestly not a 'culture' or a 'tradition'.

Second, there is the question of resolution and its consequences for identifying a tradition 'on the ground'. No known Palaeolithic site sequence, or series of site sequences, is anywhere near fine-grained enough to allow us to identify the remains of the hypothetical social units that would have been the bearers of these lithic 'traditions' (i.e. assemblage resolution and integrity are far too low). Moreover, the generally acknowledged fluidity of forager territorial boundaries would, in short order, have impossibly confounded any stylistic patterns that might have been manifest in stone tool form in the archaeological context. So, even if there were a 'cultural' component to the form of stone artefacts, we couldn't possibly detect it (Clark 1989). It is not enough to claim, as some have done, that we cannot yet model 'palaeoculture' adequately. In fact, we can model it very well (e.g. Stiner 1994; Kuhn 1995). By invoking identity-conscious 'migrants' whose peregrinations are supposedly manifest in timeless, changeless tool-making traditions, process in the remote past is treated as if it were analogous to process in recent historical contexts. From an Americanist standpoint, this makes no sense at all.

Marks and colleagues are not the first to point out that Palaeolithic stone tools are poorly suited to test notions of standardization, nor are they the first to document this empirically. Dibble's work showed that all of Bordes' Middle Palaeolithic sidescraper types could be derived from a few generalizable reduction sequences aimed at producing blanks of particular sizes and shapes, and by subsequent resharpening (e.g. Dibble 1984; 1987; 1995). Kuhn and Stiner have in addition identified a small number of contextual factors (e.g. quality, package size and availability of raw material, whether meat is scavenged or hunted) that affect choice of reduction strategies, blank dimensions and, ultimately, assemblage composition, especially as these are influenced by the extent to which foragers moved about the landscape (e.g. Stiner 1994; Kuhn 1995). There are other examples (e.g. Barton 1991; Barton *et al.* 1996; Neeley & Barton 1994) that span the Middle Palaeolithic to the Epipalaeolithic, suggesting that the widespread convergence of form in the Palaeolithic has very little to do with mental templates and alleged differences in cognitive evolution on the part of the hominids involved. The physics of lithic reduction are well-understood, and indicate an enormous amount of equifinality in the form of chipped stone artefacts. A substantial literature identifies the processes that affect technological variables in Palaeolithic contexts. It is a virtual certainty that formal convergence is almost entirely due to constraints imposed on form by the interaction of contextual factors and rock mechanics, and that those constraints override any hypothetical cognitive or cultural component manifest in mental templates, relative degrees of 'imposed form' or tool-making traditions. Thus formal convergence has little or nothing to do with history (in the narrow sense of the term). It is a consequence of repeated combinations of these relatively few factors.

Although not without its defects, the article by Marks and colleagues addresses an extremely important question — how we go about assigning meaning to pattern in the form of Palaeolithic stone artifacts. Too much archaeology (of all kinds) is exceptionally myopic — concerned with 'little questions' of interest only to a few specialists — is obsessed with methodology, and is unconcerned with (or unaware of) the necessity to confront the logic of inference underlying its knowledge claims. If archaeology is to be taken seriously as a science (or even 'an intellectually credible enterprise' — a big and growing problem over here), it must address big questions that are important to more than a handful of people. This article does that, and does it very well.

From Thomas Wynn, University of Colorado at Colorado Springs, 1420 Austin Bluffs Parkway, PO Box 7150, Colorado Springs, CO 80933-7150, USA.

This is an odd article. The authors set out to test, empirically, a hypothesis of Paul Mellars concerning standardization and cognition. They conclude by re-

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jecting the methodological assumptions of the hypothesis, and by implication obviate their own empirical argument. It helped me to think about the article by posing three questions.

First, do the authors make their central empirical point that Upper Palaeolithic burins are not, in fact, more standardized than Middle Palaeolithic burins? The answer here is yes, at least for their sample. There is, however, far too much special pleading (e.g. the reasons for using Iberian Upper Palaeolithic and Israeli Middle Palaeolithic burins) to allow a general conclusion concerning Middle and Upper Palaeolithic tools, a point the authors themselves acknowledge. The statistics are more than normally obscure, but there is no reason to doubt their validity or the reliability of the results.

Second, have the authors effectively rejected or weakened Mellars' initial hypothesis? Here the answer is no. The special pleading concerning sample alone removes this possibility. But even if Middle Palaeolithic burins everywhere were as standardized as Upper Palaeolithic burins, Mellars' hypothesis would be unaffected. Nowhere in Mellars' 1989 article (the source most quoted by the authors) does Mellars suggest that standardization in burins should be expected. Indeed, the illustration on p. 365 (Mellars 1989a) implies that Mellars' primary interest is in bifacially shaped stone tools and bone and antler armatures. Rejecting a difference in the standardization of burins does nothing to weaken an argument about mental templates when bifacial points and bone harpoons are not considered. Having now used 'mental template' it is appropriate to pose my final question.

Have the authors made any contribution to our understanding of the evolution of cognition, or to the methodological basis of cognitive archaeology? The answer here is yes, albeit a modest contribution. The conclusion and discussion sections of the article are devoted to challenging the idea of standardization as a measure of the 'clarity of mental templates'. The authors make the necessary distinction between standardized technique and standardized form, and reasonably argue that standardization need not imply clarity of mental templates. In other words, they challenge one of Mellars' methodological assumptions. Much of this discussion, especially that of the 'reality' of type lists, is not at all new or particularly insightful. Archaeologists covered this ground 25 years ago and more, though I suppose it does not hurt to remind us. What is more troubling is what is missing; the authors do not *add* anything to the discussion. How *should* we document 'clarity of mental templates?' Better, why not challenge the whole notion of mental template? It was of questionable utility twelve years ago when Mellars used it; it is effectively useless now. Cognitive archaeology has made methodological strides in the last decade primarily by rejecting such ill-defined terms as 'mental template'. Instead, cognitive archaeology has turned to cognitive psychology and cognitive science for its concepts, and 'mental template' is nowhere to be found. There is an extensive literature on imagery (e.g. Kosslyn 1994) and intentionality (Dennett 1987). Indeed, some of this literature has been invoked by archaeologists (Noble & Davidson (1996) and Mithen (1996) have made especially effective use of the psychological literature).

The challenge of cognitive archaeology is not simply to debunk common-sense notions like 'mental template'. Rather it is to identify abilities that have been confirmed by psychological research and which can be reliably applied to the archaeological record.

From João Zilhão, Instituto Português de Arqueologia, Av. da India, 1300-300 Lisbon, Portugal.

Marks *et al*. effectively demonstrate that no increase in tool standardization differentiates the Upper Palaeolithic as a whole from the preceding Middle Palaeolithic. They further question with convincing arguments Mellars' use of tool standardization as a proxy for 'more clearly conceived mental templates' and, hence, as a measure of cultural modernity and symbolic behaviour. This hypothesis can now be considered as having been formally tested and rejected. Perhaps this should come as no surprise, given the intrinsically nonsensical nature of the concept of 'modern behaviour' itself.

This said, Marks *et al*.'s use of burins as the tool class of choice to perform the test is open to question. Although traditional typologies consider them as tools, in most cases burins are in fact, functionally, bladelet cores, as I suggested for the Upper Palaeolithic of Portugal on the basis of the following arguments:

1. the fact that the bevels of most burins tend to be rather thick, making them totally inappropriate for the kinds of utilizations suggested by some use-wear studies;
2. the frequencies of burins and prismatic bladelet cores vary in an inversely proportional way;
3. ratios of burin spalls to burins are too small (often <1) to warrant the interpretation of the former as discarded residues of retouch, suggesting in-

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stead that burins are themselves the real residues;
4. blanks of microlithic points, particularly in the Magdalenian, can often be recognized as burin spalls.

When the volumes available for extraction are thin parallelepipeds (as in contexts where raw-material must be economized and broken, or used-beyond-repair tools re-enter the lithic production system), the *coup de burin* technique represents the most efficient way of extracting elongated pieces to be used as unretouched barbs or as blanks for the manufacture of armatures. This view of burins is consistent with Marks *et al.*'s finding that it is in the width/thickness ratio that their samples seem to differ less.

The fact that the Gravettian sample they use is more standardized for length than the others is clearly the direct consequence of a higher proportion of blade blanks. Once variability in the basic technology, i.e. in the nature of the used blanks (flakes or blades), is controlled for, the interpretation of the burins as a special core type predicts that the degree of standardization in dimensional factors must be identical across all samples.

In my view, therefore, the really significant finding of Marks *et al.*'s study is that no difference exists in the standardization of the non-metric attributes analyzed.

These reservations do not invalidate Marks *et al.*'s test of Mellars' hypothesis. The latter is phrased in the terms of the classification of stone tools provided by Bordesian typology and its internal logic assumes the validity of such a typology. That most burins are in fact cores, not tools, only highlights a further weakness of Mellars' position: the argument about the imposition of form only makes sense if form and function can be separated (i.e. different forms can have the same function), but the device used to monitor the real function of artefacts (Bordesian type-lists) performs the task very poorly, even at the basic level of discriminating between what are in fact cores and what may actually have been tools.

Reply from Anthony Marks, Harold Hietala & John Williams

The six who provided comments to our article are to be thanked for their efforts and the ideas they presented. While not all related directly to our article, it is certainly reasonable that our article should have led some to expand on our theme. A number asked why we didn't do something different: Kuhn wanted us to explain why blades became more common in the Upper Palaeolithic; Wynn wondered why we did not challenge the whole notion of mental template; and Mellars asked why we emphasized 'standardization' when he would have preferred us to focus on 'imposed form'. All these points are worthy of detailed, thoughtful treatment, but they were not central to our concerns in this case. We would love to know why blades became more common in the Upper Palaeolithic in most contexts but not all, and we are sure that Kuhn would have told us had he known (Bar-Yosef & Kuhn 1999). It is certainly a more profound and interesting question than that of whether or not standardization was different across the Middle to Upper Palaeolithic boundary. We did not challenge the whole notion of mental template because: first, none of us is steeped in cognitive psychology or cognitive archaeology; and, second, it appears to us that articles which just question the logic of underlying assumptions of collateral archaeological discourse tend to fall on deaf ears. While standardization might have been less important to Mellars than 'imposed form', it was explicitly included in his arguments, as cited below. His wish to separate standardization from 'imposed form' (visual distinctiveness) truly bemuses us. How else can a group of artefacts (e.g. Bassaler burins) with a visual distinctiveness relative to other artefacts of the same class (burins on truncation), be recognized or defined, if not by some form of standardization? Simply introducing a different term does not remove standardization as a central aspect of redundant form, whether non-metric, or metric. We will address artefact form in relation to type-fossils later in this response, although not in the detail it deserves.

We chose to deal with standardization, as seen by Mellars, within the constructs implicit in his writing (that recovered tools reflect the mind of the maker, for instance) not because we agreed with them but because we wanted to show that even if the underlying assumptions were accepted, the conclusion based upon them was false. It is true we have made but a modest contribution. It is what we intended to do: to remove one small element from the myriad of ideas, conjectures, guesses and intuitive observations that are being used to justify the belief that Neanderthals were profoundly different from *Homo sapiens sapiens*. While they may have been different, a point still in contention, the use of tool standardization as a measure of clarity of mental templates between them is not valid.

Kuhn's comments are, as usual, tactful, thoughtful, useful and to the point. We hope the previous two paragraphs have clarified that we did not accept Mellars' underlying assumptions: we merely used

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them heuristically. In this case, our intended audience is those who feel no discomfort in measures of standardization and the intuitive explanations that arise from them. We agree with Kuhn that morphological redundancy is not necessarily a reflection of the intent of the flintknapper, and that is why we tried to minimize the influence of other factors in our choice of samples (raw material constraints, individual flaking abilities, specific limited activity areas, and highly ephemeral use). Perhaps, most of all, we agree with him that those who posit greater standardization and/or imposed form in Upper Palaeolithic versus Middle Palaeolithic stone tools need to lay out their reasoning in detail.

The comments by Davies add dimension to our discussion and are welcome. In places, perhaps, he has read more into our article than we wrote, or intended to write. We have only minor disagreements with him. We would suggest that all retouched tools have their form determined by manufacturing techniques, whether it be the removal of a burin spall, a series of tiny chips, or twisted bladelets. The typological aspect of the tool, on the other hand, is its state after the completion of the manufacturing/rejuvenation processes. In this sense, burins are typologically no different from any other class of tool. It is important not to link typology, or redundant form, with function. We know that typological burins were used for a number of functions but this is also true for scrapers. Recent work has shown that quite typical scrapers were used solely for scraping, only for cutting, or for both scraping and cutting, at times alternate (Kay 1999). While form certainly may limit function, there are very few forms within the Palaeolithic tool repertoire that cannot be used for more than a single function. With this caveat in mind, we agree that similar studies should be done on other tool classes, provided that sufficiently large Middle Palaeolithic samples could be found of Upper Palaeolithic types or that a sufficient number of typical Middle Palaeolithic tools could be found in Upper Palaeolithic contexts. We do not believe that burins were particularly expedient tools, and we see no essential difference between them and scrapers in that regard.

Nostra culpa. We did not mean to suggest that all scrapers, or all burins, for that matter, were only discarded when exhausted. The use of rejuvenation is dependent upon a number of factors, including but not limited to the proximity of suitable raw materials to the activity being carried out, the duration of the activity being undertaken, the length of occupation, and the number of times that the artefacts themselves served as a new source of raw material during re-occupations. One might well postulate that at sites which were not intensively or repeatedly revisited, such as those which characterize much of the Aurignacian, few non-curated tools would be rejuvenated and those that had been would exhibit relatively little rejuvenation. This is a hypothesis that can be tested. Again, we did not imply that all Upper Palaeolithic tools were hafted. Rather, we were arguing that when hafting occurred, standardization of the haft element would be adaptive.

There is no *a priori* reason why a core may not have functioned as a tool and vice versa. The traditional type, *rabot*, recognized this possible duality. Today, however, use-wear studies document use, or the lack of it. For carinated pieces, at least, these studies (e.g. Almeida 2000, 137) have been unable to document any significant use-wear. If Davies' work shows a different pattern, it will not be unexpected, since universals in archaeology are highly unlikely. We look forward to seeing his results.

We find Geoff Clark's comments to be tangential to the central theme of our article. While we certainly believe that typology can vary independently of technology, technological methods and the equifinality in lithic reduction are a step away from testing Mellars' ideas about differential typological standardization. We made no claim that we were the first to show that stone tools are poorly suited to test notions of standardization. We are the first, however, specifically to test Mellars' conclusion that tools in the Upper Palaeolithic were more standardized than their equivalents in the Middle Palaeolithic. While we dealt to some extent with 'how we go about assigning meaning to pattern in the form of Palaeolithic stone tools', it was by specific example. This article was not intended to be, and should not be read as, a general, theoretical treatment of the 'logic of inference underlying its (archaeology's) knowledge claims'.

In relation to Wynn's comments, we hope that our earlier explanation covers the issue of why we used the methodological assumptions of Mellars' hypothesis, although we ultimately rejected them. While Mellars never specifically refers to burins in his article (1989a), his later writings (including his comments here) make clear that he is referring to Upper Palaeolithic stone tools in general and, in the quote provided by Shreeve (see below), it is clear that burins are included. We think that Wynn has somewhat misunderstood the purpose of our article. It was not to falsify Mellars' claim that there were profound differences in the mental templates of Neanderthal and modern people. It was to show that

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the proposed general, differential standardization of typologically comparable tools across the Middle Palaeolithic/Upper Palaeolithic boundary cannot be verified in terms of one of the most characteristic Upper Palaeolithic tools — burins. Since bone harpoons do not occur in the Middle Palaeolithic, it would be impossible to test their relative standardization in the Upper Palaeolithic, as opposed to the Middle Palaeolithic. Bifacial tools are another matter; one worthy of consideration. Yet, if by chance Upper Palaeolithic bifacial tools were relatively more standardized than Middle Palaeolithic ones, would a different clarity of mental template *a priori* be the preferred explanation? We think not, because among other reasons it would be truly strange for clarity of mental template to manifest itself only in bifacial reduction and not in unifacial reduction.

While we thank Wynn for providing us with a definition of his research focus, archaeology is a broad discipline that cannot ignore even common-sense notions when they appear repeatedly in the mainstream literature. Problems in archaeology should come from subject matter, not from our broader questions of humanity. Imposing our ideas onto the archaeological record or otherwise using a top-down strategy opens the door for accommodating the evidence to fit our preconceptions.

We are gratified that Zilhão has taken our article as intended and agrees with our limited conclusions. He questions whether burins should have been used because, in his mind, most are really cores. This is similar to Davies' point that burins, as a class, have multiple functions. Our position is that Mellars' argument accepts the traditional interpretation of burins as tools and they could be tested in that framework. In any case, whether core or tool, or a bit of both (as Davies might reasonably like), burins are defined morphologically and, so, may be tested for relative standardization, whatever their function(s).

We are grateful that Paul Mellars has taken the time to elaborate his published positions and to acknowledge that there was some bath water along with his baby. To continue with his analogy, however, we have now got rid of some of the bath water, peered at what remains and, to us, there still seems to be a lot of bath water. There may be a baby in there somewhere but it is unlikely to have been made of stone. Thus, we limit our considerations to lithics with the recognition that patterns of bone or antler personal adornments may well have been conceptually such that inferences associating them with symbolic behaviour might be justifiable. We doubt, however, that the same applies to stone tools.

In our following discussion we have dropped the term 'imposed form' in favour of redundant form. The very use of the term 'imposed' presumes cognitive decision-making (imposition). Since it is hardly clear that all redundant form was intentional, it seems to be inappropriate to use language which, at the very least, predisposes the reader to unthinkingly accept the idea that artefact form arose first in the mind of the maker.

The question is whether redundant form, combined with limited distribution in time and space, transcends the mundane and the functional to become a metaphor for a symbol. It is absolutely true that there are many more lithic type-fossils claimed for the Upper Palaeolithic than for the Middle Palaeolithic. It is also true that many of these exhibit significant redundant form. Of course, there are many tools with redundant form in both the Middle and Upper Palaeolithic that are not claimed to be type-fossils. Causing form by retouch is how the vast majority of tools were made in all periods. Retouch, by removing portions of a blank, modifies its form, whether the removal is a burin spall or a tiny chip. It is a matter of degree. Even a 'purposeful' snap may result in significant modification of form by radically changing the shape and relative dimensions of a blank. Without some level of redundancy, of course, no two tools would look alike and we would have no typologies (as in the case we cited of White's work (1969) in New Guinea).

Redundant form, as recovered in artefacts, even if complex in and of itself, has so many possible causes (some discussed in our article and in the comments of Clark and Kuhn) that attributing it *a priori* to symbolic behaviour is unwarranted. In spite of this, might not the comparatively large number of type-fossils in the Upper Palaeolithic (compared with those in the Middle Palaeolithic) have additional meaning, particularly if temporally linked with a more general 'symbolic explosion' at the beginning of the Upper Palaeolithic, as suggested by Mellars? This is, however, a questionable linkage for lithic type-fossils, since the early Aurignacian has no lithic tools that are specific to it. Certainly, the Aurignacian retouched blade is characteristic, but it occurs throughout the Palaeolithic in assemblages with a large blade component. It is really only in the later Aurignacian that a few true type-fossils appear, such as the Grattoir Caminade (Demars & Laurent 1989, 38) which has a very limited distribution in time and space but which, although distinctive, has little imposed form, in Mellars' sense. Second, most type-fossils come from mid- to late Upper Palaeolithic

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contexts. Thus, they would not be part of an original 'symbolic explosion' but might well be attributed to situational factors (i.e. increasing population densities, new technologies such as the bow and arrow, even stylistic drift).

It is important that the reality of type-fossils be considered. Some have only trivial differences from more common tool types, while others are defined so broadly that they fall well outside the idea of visual distinctiveness. For some, their distribution in time and space may be much larger than perceived by the typologists who defined them, while for others their status may be secure. As a group, they are insufficiently homogeneous in concept and definition to be considered significantly different from other stone tools. A good example is found within burins.

While burins are now 'notoriously variable tool forms', not too long ago Mellars was quoted using burins as his example. As reported by Shreeve (1995, 303), Mellars gave the following example of how an Upper Palaeolithic person would have thought about a tool, in contrast to a Neanderthal.

'A typical Mousterian might make a tool and think, "As long as this does the job, I don't care what it looks like" . . . But an Upper Paleolithic fellow says, "this thing is a burin, I call it a burin, I use it like a burin, and by God, it better look like a burin."' In addition, with burins being both one of the main diagnostic Upper Palaeolithic tool classes and being generally abundant in the Upper Palaeolithic, their exclusion from the affects of Upper Palaeolithic imposed form would be strange indeed. Therefore, testing the notion of increased standardization in the Upper Palaeolithic using burins is reasonable, with regard to the statements made by Mellars.

We are a little confused by Mellars' characterization of our study as one involving 'simple burin forms' from two sites, since the study included all major burin types traditionally recognized for the Upper Palaeolithic of southwestern Europe and three sites were used, including one Gravettian and one Magdalenian.

Mellars' suggestion that a different result would have occurred had we used 'more specialized and distinctive Upper Palaeolithic forms, such as Noialles, Raysse, busqué or parrot-beak burins' is worth considering. First of all, busquoid burins *were* included (those busqued forms with and without a terminating notch). In fact, they occurred only in the Gravettian sample but were insufficient by themselves to affect measures of diversity. Burins Noialles, Raysse, and *bec de perroquet* are traditional examples of type-fossils recognized for the Upper Palaeolithic (Demars & Laurent 1989) — for Mellars, those distinctive, special tools with 'imposed form and visually distinctive appearance' which, taken together, are an 'empirical feature' of the archaeological record. A somewhat closer look at these forms will show just how careful one must be in accepting traditional typological constructs.

All three burin types fall within the sub-class 'burin on truncation'. Of the three, the *bec de perroquet* has the most 'distinctive' appearance but, at the same time, exhibits only minimal modification of the original blank (Demars & Laurent 1989, 67). Its distinctiveness, in the context of the Late Magdalenian, lies in the selection of a large, thin flake as a blank and in the steep but non-invasive retouch around its whole edge. The burin part, itself, is unremarkable and a small distal fragment would most likely be classified simply as a burin on convex truncation. In spite of the only minor shaping, its morphological distinctiveness, as well as its temporal and geographic parameters, make it a reasonable type-fossil.

The Noialles burin, on the other hand, is distinguished from other burins on truncation by a single attribute: a bit width of <2 mm. In fact, this criterion is so important that a burin on snap is included if the bit is <2 mm (Demars & Laurent 1989, 68). While this type-fossil is often multiple, that is not a necessary feature. It may also have a notch to terminate the burin spall but this, too, is not required. Traditionally, it has been seen as a type-fossil of the old Périgordien Vc. What is striking about the Noialles burin, in that context, is its abundance, not any particular redundant form. Simply, it is a burin on truncation on a very thin blank. There is no greater shaping than on any other burin on truncation. Of course, very thin blanks tend to be smaller overall and, so, Noialles burins are often smaller than other burins on truncation, although they can be on blanks over 4 cm long (Demars & Laurent 1989, 69, figs. 1, 10 & 20). Using the bit width criterion, and limiting it to burins on snap or on truncation, Noialles burins occur in numerous contexts beyond Périgordien Vc. In fact, of the samples used in our study, the Middle Palaeolithic contained two, the Gravettian sample had three, and the Magdalenian sample had two. Just as importantly, there were even more examples with bit widths between 2 mm and 3 mm.

How can it be argued that, as a morphological type, the single attribute — width of burin bit <2 mm — makes Noialles burins into a special, 'visually distinctive' type-fossil? Short of modern measuring devices, can even a well-experienced typologist visually distinguish a burin on truncation with a bit width of 2.1 mm from one with a 1.9 mm bit width?

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A dichotomy might be visually apparent if, in fact, there were two quite distinct populations of burins on truncation, one with very narrow bit widths, and another where the bit width was significantly wider. Experience in Portugal, however, suggests that the very narrow bit widths are not part of bimodal bit width distributions. At the Gravettian site of Picos (Marks *et al.* 1994) 11 of 25 burins on truncated blades/bladelets have bit widths of <2 mm, while six others, otherwise identical morphologically, have bit widths between 2.0 mm and 2.7 mm.

Thus, the Noialles burin is not very limited either in time or space, while its definition depends upon a single attribute separating it from the very common burins on truncation. Even that attribute does not involve additional or different form from the common sub-class. This is hardly the image of a type-fossil that Mellars' comments are meant to bring to mind.

The Raysse burin (also referred to as a Bassaler burin) is essentially a burin on truncation that has been modified at its bit, resulting in a final plan facet. This is considered to be good type-fossil, limited in time and space, since its production sequence is quite complex and it is difficult to confuse with other burin types (Demars & Laurent 1989, 72). Yet its production complexity involves the modification of the same portion of the burin, at least two, and often three times. It is striking that the second and third modifications involve the same technical procedures, presumably resulting in the same kind of working bit, although the spalls removed are on different planes. Is it not possible that the Bassaler burin is a burin on truncation which has been rejuvenated two or three times? Since the final spall, either during the second or third stage, is always *plan*, perhaps, that was the motivation for discard. Until use-wear studies are carried out to see if the burin was used after the *plan* spall, it is impossible to tell which scenario, if either — a complex tool or a rejuvenated simple tool — is accurate. While it does have a visual distinctiveness, why it came about is uncertain.

It would be possible to continue this detailed critique of virtually all type-fossils, but we hope the point has been made: in traditional typology, little should be taken at face value (Demars & Laurent 1989, 15–21). That does not mean that there are no real type-fossils with significant, redundant form, as well as limited distributions in time and space, since there are such (e.g. Font Robert points, *fléchettes*, laurel leaf points, Streletskaya points). There are merely not quite as many in the Upper Palaeolithic as it might seem. In fact, the number attributable to the early Upper Palaeolithic is quite small — perhaps not many more than those attributable to Neanderthal craftsmen of the late Middle Palaeolithic.

Why do there appear to be few type-fossils in the Middle Palaeolithic? Partly, as every Palaeolithic archaeologist must know by now, it is because Bordian Middle Palaeolithic systematics and the traditional West European Upper Palaeolithic systematics (i.e. deSonneville-Bordes-Perrot) measure quite different aspects of lithic artefacts (noted, yet again, in Clark's comments, as well as in our article). In fact, Bordian systematics discourage the recognition of variability not represented in his type list. For instance, even within the limited view that the shape of a retouched edge is the diagnostic criterion for scraper type designation, edges which are sinuous are classified according to whatever shape (concave, convex, or straight) covers the largest portion of the retouched edge (Bordes 1961, 12). Even multiple tools are excluded, with the least-represented tool type on the piece being counted to the exclusion of the other(s) (Bordes 1961, 11).

Combined with Dibble's (e.g. 1987; 1995) suggestion that Middle Palaeolithic scraper variability could be explained merely as a result of rejuvenation, this has permitted those who do not want to see redundant form and visually distinctive tools in the Middle Palaeolithic to down-play, if not ignore, Middle Palaeolithic variability. It must be said in their defence, however, that since most Middle Palaeolithic assemblages in southwestern Europe are described in Bordian terms (even those published by Dibble: Dibble & Lenoir 1995), it would be impossible to recognize and document Middle Palaeolithic type-fossils from the West European literature.

A quite different approach was taken for the Central European (Bosinski 1967) and East European Middle Palaeolithic (Gladilin 1976; see Chabai & Demidenko 1998 for an English discussion of Gladilin's systematics). Both of those systems encourage the recognition of morphological variability, and, not surprisingly, it is present in abundance. In Eastern Europe, there are numbers of even unifacial tool types with significant redundant form and visual distinctiveness that cannot be explained as rejuvenation stages within Dibble's model (Chabai & Demidenko 1998, 41–7). Are they type-fossils, in the best sense of the West European Upper Palaeolithic? To date, too little is known about their temporal and spatial distributions to say, but, without question, their morphological patterning is fully comparable to that of many legitimate Upper Palaeolithic type-fossils. It is a question still to be resolved. Thus the statement that there are fewer type-fossils in the

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Middle Palaeolithic than in the Upper Palaeolithic is without demonstrable foundation. At best, it is a hypothesis still to be tested. Until it is shown to be true, no generalization on which it is based will be meaningful. We feel it is incumbent upon those who accept the questionable clichés of traditional West European typology to document their validity before using them as even collateral evidence for generalizations about symbolic behaviour that may be more convincingly seen elsewhere. We hope, therefore, that assertions of intuitively perceived degrees of standardization that have been used as evidence for differing mental templates across the Middle to Upper Palaeolithic boundary will disappear from the literature. It might be only a modest improvement, but it would be a step in the right direction.

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