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THE SZELETIAN REVISITED

ABSTRACT: Recent dating and other evidence is examined in order to determine whether the hypothesis regarding the nature, chronology, and hominid associations of the Szeletian which was previously supported by the author can still be regarded as correct. The present situation is one in which the partial contemporaneity of Neandertal and anatomically modern man in Central Europe and elsewhere is seemingly admitted on all sides. Their association respectively with the Szeletian (or the Middle Paleolithic) and the Aurignacian (or the Upper Paleolithic) is still valid. Therefore the acculturation hypothesis is still regarded as a tenable model to explain the characteristics of the Szeletian. But more research, involving both re-examination of old materials, and if possible the excavation of new sites, is needed to clarify many outstanding issues relating to the Initial Upper Paleolithic in Central Europe.

KEY WORDS: Szeletian – Aurignacian – Dating – Acculturation – Initial Upper Paleolithic – Neandertal – Anatomically modern man

INTRODUCTION

In an earlier study (Allsworth-Jones 1986, 1990a, 1990b) I concluded that the Szeletian of Central Europe could most convincingly be seen as the product of an acculturation process at the junction of Middle and Upper Paleolithic, that it was most likely the creation of Neandertal man, and that both were replaced, the first by the Aurignacian and the second by anatomically modern man. The Szeletian was regarded as a transitional industry, defined (in terms originally used by Bricker) as one with "mixed technological and typological characteristics", which logically could reflect either an "independent" process of change or a reaction to "an already extant and diffusing tradition". The validity of the model was therefore dependent both on chronological considerations and on the supposed hominid associations of the industries concerned. In the light of current controversies and recent evidence, it is worth taking another look at the adequacy of this hypothesis. For the sake of brevity, I have with a few exceptions included only recent works in the attached bibliography, and the reader is referred to my earlier study for a full list of older titles.

HOMINID ASSOCIATIONS

It is worth remembering that when my earlier study was undertaken, the predominant paradigm favoured a European-wide evolution of Neanderthal into modern man, accompanied by an *in situ* transformation of Middle into Upper Paleolithic. While there are still adherents of this view, it is remarkable how the balance has shifted in favour of the replacement hypothesis, a key component of which is the partial contemporaneity of Neandertal and modern man. Thus, d'Errico *et al.* (1998) refer to an Ebro "frontier" beyond which Neandertals continued to flourish for at least 5,000 years following the arrival of moderns on the other side, and in their recent study Churchill and Smith (2000) envisage a period of coexistence elsewhere in Europe for anything between two and ten thousand years. On current dating evidence, Neandertal man lingered late not only in refugia such as Iberia, the Crimea (Allsworth-Jones 2000a) and the Caucasus (Golovanova *et al.* 1999, Ovchinnikov *et al.* 2000), but also in certain parts of Central Europe (possibly up to 28–29,000 BP in layer G1 at Vindija, Smith *et al.* 1999). Maps (popular and otherwise) illustrating this

TABLE 1. Vindija and Velika Pecina.

Vindija		**Velika Pecina**	
E	8,500±300 Z-2447 Gravettian modern		
Fd	26,970±632 Z-551 Aurignacian	e	26,450±300 GrN-4980 Gravettian
Fd// Fd/d	modern Vi 204 and 302 parietals		
Fd/d	26,600±900 Z-2443 Aurignacian	g	27,300±1,200 Z-189 Aurignacian
G1	33,000±400 ETH-12714 "Olschewian" Neandertal	i	33,850±520 GrN-4979 "Olschewian"
		j	[VP-1 frontal 5,045±40 OxA-8294]
	29,080±400 OxA-8296 Vi 207 mandible 28,020±360 OxA-8295 Vi 208 parietal		
G3	42,400±4300 amino-acid Mousterian Neandertal		
G1 inventory: 62 lithics, including 15 tools: 2 endscrapers, 1 burin, 1 retouched blade, 5 sidescrapers, 4 denticulates, 1 leafshaped bifacial point, 1 "rabot", 1 split-based bone point, 3 non-split-based bone points, 5 bone point fragments.		i inventory: 7 lithics, including 6 tools: 1 endscraper, 1 burin, 1 awl, 3 side-scrapers. 3 probably split-based bone points, 1 non-split-based bone point.	

(Inventories after Karavanic and Smith 1998)

scenario show a world which is a far cry from what was once believed (Norris 1999, Gibbons 2000, Zilhão 2001: Fig. 13). In such circumstances, one would have thought that some kind of interaction between the two populations was practically inevitable, and the debate becomes a more nuanced one as to what exactly that interaction was.

Except in the case of Vindija, so far as I am aware, Neandertal man in Central and South-eastern Europe continues to be associated exclusively with the Middle Paleolithic as well as with the Jankovichian (otherwise known as the "Trans-Danubian Szeletian"). New finds in Germany confirm this association, specifically with the Micoquian, at Warendorf and Sesselfelsgrotte (Orschiedt *et al.* 1999). New investigations at Neandertal itself have revealed traces of a Micoquian industry and have confirmed that a second individual was present at the eponymous site (Schmitz, Thissen 2000). The humerus of this second individual has been AMS dated to 39,240±670 BP (Schmitz, pers. comm.). Following Grote's excavations in 1977 the inventory from Salzgitter-Lebenstedt was re-classified as Micoquian, and this has now been confirmed by a more detailed study of the complete lithic inventory (Pastoors 1998). The bulk of the industry is located in unit 2 (B1), for which two ^{14}C dates of 48,500±2,000 (GrN–1219) and 55,600±900 BP (GrN–2083) BP are now known (Gaudzinski 1999). At Kůlna cave in Moravia the Neandertal remains in layer 7a were also associated with the Micoquian previously dated to 45,660+2,850/–2,200 BP (GrN–6060). New ESR results give a mean age of 46±6 (EU) or 50±5 (LU) kyrs BP for this layer, which are adjudged to be reliable, despite problems with other dates in the sequence (Rink *et al.* 1996). ESR results at Krapina have shown that the sequence there can be dated between 87±7 and 133±15 kyrs BP on average for layers 9 to 1 (top to base) (Rink *et al*. 1995). The bulk of the hominid finds were concentrated in layers 4 and 3, and as Montet-White remarks (1996: 60, 62–68), the considerable age which can now be attributed to them is in accordance with the homogeneous "archaic" aspect which they are now said to possess (in contra-distinction to the "advanced" status which was earlier claimed for some of them at least). Sládek (1999) has also challenged the supposed "progressive" status of other Neandertal finds in the region, particularly Sala.

With regard to the "Trans-Danubian Szeletian", the most significant recent finds were those made by V. Gábori-Csánk during her excavations at the Máriaremete Upper Cave west of Budapest in 1969 and 1970 (Gábori-Csánk 1993). According to her, there were 13 stone artefacts in the occupation horizon, plus one 1 fossil marine shell which must have been brought in from elsewhere. The stone artefacts included 5 bifacially worked tools, 3 sidescrapers, and 5 flakes. In my view, it is legitimate to regard the bifacially worked tools as leafpoints, and in any case this small assemblage is fully representative of the entire

Jankovichian, as she re-christened this group of sites west and south of the Danube. With the assemblage were found three right mandibular teeth attributed to a single Neandertal individual by M. Kretzoi. It is unfortunate, as Churchill and Smith (2000) comment, that as yet no "morphologically detailed defence" of this attribution has been given, but it does agree with the evidence from Dzeravá skala where Hillebrand found an unerupted lower right molar of a child in 1913. It had a pronounced anterior fovea and in that morphological respect was judged similar to the remains from Krapina. It might be recalled that the Neandertal associations of the Châtelperronian were first suspected only on the basis of Leroi-Gourhan's description of the teeth from Arcy-sur-Cure. I therefore think that Svoboda (2001b) is much too cautious in his assessment of these finds, and at the moment there is no ground whatsoever for the assertion that the Szeletian may be associated with Cro-Magnon man (Stringer, Davies 2001).

Vindija obviously deserves special mention. The situation here has been described a number of times (Wolpoff *et al.* 1981, Montet-White 1996, Karavanic 1995, 2000, Karavanic, Smith 1998, 2000, Karavanic *et al.* 1998, Smith *et al.* 1999) and is summarised in *Table 1*, which also includes a comparison to Velika Pecina. Until recently, the Neandertal remains in layer G1 at Vindija were believed to be essentially contemporary with the modern human frontal in layer j at Velika Pecina, suggesting the possibility of direct contact between the two. The Velika Pecina specimen has now been shown to date to the Holocene, and was therefore presumably intrusive, but the general stratigraphic and archaeological parallel between the two sites still stands. On the assumption of contemporaneity, and of the homogeneity of the finds from Vindija layer G1, Karavanic and Smith (1998) originally took the view that some of the Upper Paleolithic tools associated with the Neandertals in this layer, particularly the bone points, may have resulted from "imitation of or trade with early modern people". In this they specifically followed the suggestion made by Hublin *et al.* (1996) in relation to Arcy-sur-Cure. I already remarked that, if homogeneity could be guaranteed, there was also a broad parallel here to the situation at Istállóskő and Szeleta, with the "archaeologically rather ambiguous contemporary occurrence of bone points" at these two sites (Allsworth-Jones 2000a). Churchill and Smith (2000, Table 6) have now reformulated the original idea by referring to the industry from layer G1 at Vindija as an "Initial Upper Paleolithic with Aurignacian elements". This is in contrast to the interpretation put forward by Montet-White (1996) and now by Karavanic (2000) that it would be more appropriate to call both Vindija G1 and Velika Pecina layer i simply "Olschewian". The adequacy of this suggestion will be considered below.

In the meantime, it has been pointed out by more than one author that there is an entirely different possible explanation for the situation at Vindija. As Montet-White emphasised (1996) the excavation methods used at the site were hasty, and it is obvious from the account given by Karavanic and Smith (1998) that many of the finds lack secure provenance. In addition, there were obvious signs of cryoturbation in layer G1. Karavanic and his colleagues maintain that cryoturbation did not affect the entire layer, and he points out (1995) that both the hominids and the bone tools were impregnated with fine particles of red-brown sediment which are characteristic of this layer. Even so, that does not guarantee its homogeneity. On the basis of the available ^{14}C dates, it is admitted that the layer spans "a minimum of 3 kyrs of radiocarbon time" (Smith *et al.* 1999) and considering the paucity of the archaeological finds it is obvious that it constitutes a palimpsest witnessing, as Zilhão and d'Errico (2000) put it, no more than "sporadic human incursions into the cave" (Mellars *et al.* 1999: 355). This is the kind of situation analysed by Pettitt (1999: 225–228) where the level of resolution is such that it is difficult or impossible to sort out what was really associated with what. In my view therefore, the problems presented by this site are such that it cannot be taken as a convincing exception to the general rule, and certainly not as proof that the Aurignacian was created by Neandertal man.

Apart from Vindija, so far as I am aware, no new finds in Central and South-eastern Europe have shaken the equation that the Aurignacian and later industries are always associated with anatomically modern man. The recent review of the evidence by Churchill and Smith (2000) is most judicious in this respect. They provide a detailed reconsideration of four sites, Bacho Kiro, Mladeč, Zlatý kůň, and Vogelherd, which are of particular importance in this context. At Bacho Kiro a mandibular fragment with one deciduous molar (possessing a nontaurodont root) was found with the "Bachokirian" in layer 11, and seven other fragmentary remains, mainly teeth, were found in later Aurignacian layers. Despite frustrating ambiguities, Churchill and Smith's overall evaluation is that these remains can be classified as modern. Mladeč is described as "the largest, most important, and best-studied assemblage" of "modern skeletal material associated with the Aurignacian" in this area. There are significant differences between the male and female crania, but nonetheless "if the entire sample is considered" these specimens are "clearly distinguishable from Neandertals" in their overall morphological form. The importance of this conclusion is enhanced by the new dating evidence for this site, which suggests that these specimens are at least 34,000 years old (Svoboda *et al.* 2002). Zlatý kůň is described as "a robust yet morphologically modern individual". In this case, however, the new dating evidence suggests that the specimen is not Early Upper Paleolithic at all, but rather Magdalenian in age (12,870±70 BP, Svoboda *et al.* 2002). The conditions of deposition at Mladeč and Zlatý kůň have been re-examined by Svoboda (2000, 2001a), who emphasises that both are best interpreted as debris cones, where the fossils probably fell in through chimneys, rather than primary living sites. Finally, the importance of the remains found in Vogelherd layers IV and V is clear. They

come from at least three adult individuals. With three (H-series) ^{14}C dates ranging from 30,730±750 to 31,900±1,100 BP, they are regarded by Churchill and Smith as "the earliest well-provenanced and confidently classified modern human fossils in Europe". No doubt few would quarrel with such a classification and such an association at ca. 32 kyrs BP, but the authors do add that there is also a "strong suggestion" that modern humans had settled in Europe before then, by ca. 36 kyrs BP. The new evidence from Mladeč is congruent with that.

There is no need to emphasise here the obvious importance of recent studies on the genetics of Neandertal and modern man, but they do serve to demonstrate what divided us rather than what united us. As is now well known, mitochondrial DNA sequences at Neandertal itself, and also at Mezmaiskaya and Vindija, show the two groups to be distinct, with little possibility of interbreeding, and on the basis of these studies it is suggested that the divergence between them may go back for at least 550,000 years (Krings *et al.* 1997, 2000, Ward, Stringer 1997, Ovchinnikov *et al.* 2000, Höss 2000, Richards *et al.* 1996, 2000). From the paleoanthropological point of view Silvana Condemi (2000) suggests that the evolution of the Neandertals may have taken place in Europe over a period of about 450,000 years, and Y chromosome data provide further evidence for what may have happened over the last 40,000 years (Gibbons 2000, Semino *et al.* 2000).

DATING

Dating, as well as the nature of the assemblages, is obviously vital when considering the Aurignacian and the Szeletian in Central and South-eastern Europe, and the possible relationship between them. The majority of the evidence, some of which has already been mentioned, consists of ^{14}C dates, and the current controversy about the reliability of these dates cannot be ignored. It is now evident that the ^{14}C content of the atmosphere has varied through time, depending largely but not entirely on changes in the intensity of the earth's magnetic field (Mazaud *et al.* 1991, van der Plicht 2001: Fig. 2). It is clear that this has implications for the correction, if not the calibration of ^{14}C dates in the time period with which we are concerned (van Andel 1998, van der Plicht 1999, Kitagawa and van der Plicht 1998, 2000). The broad conclusion a short while ago was that up to about 25 kyrs BP a reasonable correlation between the two sets of data could be made, since "the mean geomagnetic field intensity describes the general trend of the atmospheric ^{14}C variations very well" (van der Plicht 2001: 319), after that things were much more uncertain, but not unmanageably so. Thus Hedges and Pettitt (1998) made the case that the great majority of radiocarbon dates could still be trusted "up to at least 40 kyrs BP", and on the assumption that such dates were likely to be ≥3,000 years younger than those achieved by other methods (particularly TL), it seemed possible by making rough corrections to compare ^{14}C and other dates in a single diagram (Mellars *et al.* 1999: Fig. 1).

The uncertainties inherent in the situation have been much increased due to the work of Beck and Richards *et al.* in comparing Th^{230} and ^{14}C dates obtained on Bahamian stalagmite GB–89–24–1 (Beck *et al.* 2001: Fig. 3). The problem with their diagram is not so much the increasing offset between calendar and radiocarbon ages after about 25 kyrs BP, but the existence of marked "plateaux" and "reversals" after that time (Richards, Beck 2001). As they say, this may "cause clusters of indistinguishable radiocarbon ages for material that was formed at different times over extended periods of up to 3000 years", and this is well illustrated at Vindija (Richards, Beck 2001: Table 1). Nonetheless, I do not think that because of such factors "archaeologists may finally be obliged to give up using the ^{14}C method altogether" for the time period in question (Jöris, Weninger 2000: 17). For our purposes the age determinations retain their validity, because we are concerned above all with the comparison of sets of dates, within a given time range. In other words, it is their position relative to each other which counts. Unless we are dealing with a completely random kaleidoscope, what affects one should affect all at any particular point. Moreover, an examination of many carefully dated sequences (such as Willendorf II) shows an impressive internal coherence which inspires confidence. It would be altogether premature to throw all this work overboard and in fact good sense can be made of it. Notwithstanding the "many more surprises in store" which Richards and Beck promise us.

Bearing the above in mind, a review will now be made of the key sites themselves, beginning with the Aurignacian.

THE AURIGNACIAN

We will begin with the Geissenklösterle cave, excavated by the late Joachim Hahn between 1973 and 1991. His published volume (Hahn 1988), intended to be the first of a series, takes the story up to 1983 only. From our point of view, it is fortunate that it concentrated on the stratigraphy and settlement structure of the cave, as well as the Aurignacian in particular, but still it is an unfinished story. Hence it is vital to take into account also further work which has been conducted since then (Richter *et al.* 2000). The Aurignacian deposits consisted of 7 excavated levels which were divided by Hahn into two groups. The upper group consisted of levels IIn IIa and IIb, whereas the lower group consisted of levels IId, III, IIIa and IIIb (Hahn 1988, Fig. 11). For some purposes, but not all, Hahn lumped the finds together as Aurignacian horizons II and III, for example in the de Sonneville-Bordes type list which he presented for the two of them (Hahn 1988, Table 35), where the tool totals came to 233 and 96 pieces respectively. These horizons are referred to by Richter *et al.* (2000) as Typical and Early Aurignacian respectively. In his reconstruction of the sequence of events at the cave (Hahn 1988: Fig. 25),

the excavator was very clear that there were only two real primary horizons, IIb and IIIa. IIb was characterised above all by a large ash lense, whereas IIIa had a hearth and distinct accumulations of bones, artefacts, and red ochre. Partly on the basis of refittings, Hahn regarded the levels immediately above and below these horizons as containing secondarily displaced material from these two primary contexts. Even those who wish to query the interpretation given to the site recognise that "the archaeological definition of levels IIn–IIb is uncontroversial" representing "a typical Aurignacian context with some of the more sophisticated art objects of the period" (Zilhão and d'Errico 1999: 35, the latter include the well-known ivory figures of bison, human, mammoth, and bear, Hahn 1988: Figs. 88–91). The current controversy therefore mainly concerns levels IId–IIIb.

In the original report Hahn (1988, Table 2) published 9 ^{14}C dates for the Aurignacian levels in the H– and Pta-series. These were reasonably coherent, including two particularly interesting ones for level III: 34,140±1,000 (H 5118–4600) and 36,540±1,570 (H 5316–4909) BP. Nonetheless for reasons given by Richter *et al.* (2000: 75–76) they were felt to be unsatisfactory and further samples were presented for AMS dating. In their Table 1, Richter *et al.* give only 5 AMS dates for levels IIa–b and 3 AMS dates for levels III–IIIa. They explain that "identifiable bones were preferentially selected from species considered to be human prey animals and/or showing cut marks", and they did not include cave bear bones or "results from samples in stratigraphically intermediate positions". Hence the Table consists exclusively of samples from a "clear archaeological context". The 5 dates for levels IIa–b are in the range 32,300–36,800 BP, whereas those for levels III–IIIa are in the range 37,300–40,200 BP. The mean ages in both cases respectively are 33,500±350 and 38,400±850 BP. In addition, TL dating of burnt flint from the major occupational layers (with ash lenses and hearths) was undertaken, producing two dates from Aurignacian horizon II and seven from Aurignacian horizon III (Richter *et al.* 2000, Table 6). One of the dates from horizon III (61,600±3,800 BP) was rejected as obviously anomalous. The two dates for horizon II average at around 37,000 BP, whereas the mean for the six acceptable samples from horizon III is 40,200±1,500 BP. As Richter *et al.* point out, the difference between these ages and the AMS results is exactly what would be expected given what we now know about the validity of ^{14}C dates at this time. An average ESR result of 43,300±4,000 BP for the underlying Middle Paleolithic is concordant with the above (Richter *et al.* 2000, Table 9).

On the face of it therefore we do have strong evidence here for an Early Aurignacian in Central Europe dated to around 38,400±850 BP in radiocarbon years. The objections raised by Zilhão and d'Errico (1999) are three-fold.

(1) In their list of dates for the site, they use the same ones as Richter *et al.* (2000), but add two more for level IIIa: 33,100±680 (ETH–8268) and 33,500±640 (ETH–8269) BP (Zilhão and d'Errico 1999, Table III). They then proceed to rearrange the dates in chronological order and suggest on that basis that three rather than two archaeological occupations are documented: the first at 40,200 BP (OxA–4595 from level IIIa), the second at about 37,300 BP (ETH–8267 and OxA–5163 from level III, OxA–4594 from level IIa), the third at about 33,200 BP (all other samples, including ETH–8268 and ETH–8269 from level IIIa, and the remainder from IIa and b). There is however absolutely no basis in Hahn's account to support the existence of three occupations of this kind. With regard to the two extra dates, Richter *et al.* comment that the incorporation into the analysis of results which do not conform to their own strict criteria has led to the "misinterpretation" of the Aurignacian at the site as being "mixed" (Richter *et al.* 2000: 75–76).

(2) The careful refitting of artefacts carried out by Hahn is used as an argument to show that the layers attributed to the Aurignacian at the site are in fact not homogeneous, and two of his diagrams are reproduced to support this assertion (Hahn 1988, Table 4, Fig. 20, combined in Zilhão and d'Errico 1999, Fig. 11). Hahn succeeded in refitting 350 artefacts from the Aurignacian levels (mostly IIb, III, and IIIa), and he was also able to plot multiple refittings which could be traced back to 5 particular nodules, for example A9 (Hahn 1988, Plate 6). They do indeed show a degree of vertical displacement. This however is by no means the whole story. The number of refittings should not be taken in isolation. Hahn (1988, Table 7: 79) considered it necessary to compare them with the total number of artefacts in the various Aurignacian levels. There are 3,257 altogether, hence refittings account for about 10.7% of the total. As he said, this proportion is not particularly high when compared to some other sites where he carried out the same procedure, for example Spitzbubenhöhle, where the proportion was 18.5%. It is ironical indeed that Hahn's meticulous methods could be used as an argument against the conclusion he himself reached, that there were in fact two well defined primary horizons at the site. There are only a handful of cave sites in Europe where such painstaking work has been carried out. I suspect that what Hahn found at Geissenklösterle would be repeated all over the continent were his same procedures to be followed, the most classic of Western European sites included.

(3) As a corollary to the above, Zilhão and d'Errico question whether the lower assemblage can really be called Aurignacian. According to them, the principal Aurignacian-like characteristic which it possesses is the presence of carinate and nosed endscrapers. But, they claim, "we know only that they were reconstructed as belonging to level III, not what their original stratigraphic position was". It is suggested that "a significant proportion of the aurignacoid material in reconstructed level III", including the carinate and nosed endscrapers, is "related" to the later occupation (Zilhão and d'Errico 1999: 38). In other words, they were derived from the typical Aurignacian in level II. Hahn's account does not bear this out. Apart from the de Sonneville-

TABLE 2. Austrian and Czech Aurignacian sites.

Willendorf II Aurignacian	**Milovice**
4 31,210±260 GrA-501	29,200±950 GrN-14826
4 31,700±1,800 H 249-1276	**Stratzing layer 2**
4 32,060±250 GrN-1273	29,950±370 ETH-6023
3 34,100+1,200 GrN-11192 –1,000	31,230±430 ETH-6025
3 37,930±750 GrN-896	31,450±440 ETH-6024
3 38,880+1,530 GrN-17805 –1,280	31,790±280 GrN-16135
2 39,500+1,500 GrN-11190 –1,200	**Grossweikersdorf**
2 41,600+4,100 GrN-17806 –2,700	B 31,790±280 GrN-16135
2 41,700+3,700 GrN-11195 –2,500	C 32,770±240 GrN-16263
Schwallenbach (Willendorf VII)	**Stránská skála**
36,700+1,400 GrN-16326 –1,200	IIIa.3 30,980±360 GrN-12605
Senftenberg	IIIa.4 32,350±900 GrN-14829
36,350±600 GrN-16887	IIIb.4 32,600+1,700 GrN-16918 –1,400
Krems-Hundssteig	**Mladeč I**
35,500±2,000 KN-654	34,160+520 GrN-26333 – 490
	34,930+520 GrN-26334 –490

Bordes type list, which is presented according to the amalgamated information for horizons II and III, details about the endscrapers in particular are given according to exact provenance in his Table 19 (Hahn 1988: 153). According to this Table (which differs slightly from the type list) there were 15 carinate and nosed endscrapers in horizon II and 30 in horizon III. They constitute 6.4 and 26.0% of the tool inventories respectively. It would seem to be very odd if proportionately so many of these endscrapers made their way down to the lower horizon while, as Richter *et al.* remark, other characteristic elements stayed obstinately up top. In addition, Hahn's refittings showed among other things how the relatively frequent cores in the lower horizon could be linked directly to the manufacture of such tools in that horizon (Hahn 1988, Plate 19). There is no reason to doubt that the endscrapers, together with for example non-split-based bone points (Hahn 1988, Fig. 84), formed an integral part of horizon III. As a parting shot, Zilhão and d'Errico (1999: 39) claim that, even if carinate and nosed endscrapers can be shown to belong in the lower horizon, still it cannot constitute a "true" Aurignacian because it lacks its "full cultural repertoire". But that is to assume that the Aurignacian must have sprung at once fully armed like Athena from the head of Zeus and this is not at all likely. It would be more logical to expect that the beginnings in Central and South-eastern Europe were more tentative than that, and other sites in the same time bracket tell the same story.

For the above reasons I conclude that the lower horizon at Geissenklösterle is rightly called Early Aurignacian.

Other dated sites in Germany support the evidence from Geissenklösterle. There are three early dates from Keilberg–Kirche of 37,500±1,450, 37,500±1,250, and 38,600±1,200 BP (Uthmeier 1996). All are conventional ^{14}C dates on charcoal, and they form as Street and Terberger (2000) suggest a "very consistent series". The charcoal fragments were associated with partially burned stone artefacts from the remains of a fireplace in an *in situ* cultural layer. The number of finds located *in situ* was small, but Uthmeier argues convincingly that the great majority of the material from the site can be regarded as having originally belonged here. In his view it constitutes a homogeneous Early Aurignacian inventory. There were 118 tools with 129 "working ends", among them 24 endscrapers and 77 burins. There are 10 carinate or nosed endscrapers and 28 carinate or busked burins. As at Geissenklösterle, these are the elements which are considered sufficiently diagnostic to label the site as Aurignacian. The remaining sites in southern Germany and the Rhineland (Vogelherd, Hohlenstein-Stadel, Bockstein-Törle, Lommersum, and Wildscheuer III) have dates which are comparable to level II at Geissenklösterle, or as Street and Terberger (2000)

TABLE 3. Bacho Kiro and Temnata.

Bacho Kiro		**Temnata TD-V and TD-I (interior) and TD-II (talus)**	
6a	29,150±950 Ly-1102 Typical Aurignacian		
7	32,200±780 OxA-3181 Typical Aurignacian		
6b	32,700±300 GrN-7569 Typical Aurignacian		
6b/8	33,300±820 OxA-3182 Typical Aurignacian	TD-V	3g/3h >31,100 Gd-4595 >32,200 Gd-4693 Evolved Aurignacian
			Tephra
10/11.I	34,800±1,150 OxA-3212 Bachokirian	TD-V 4a	33,000±900 OxA-5174 Early Aurignacian
11.I	37,650±1,450 OxA-3183 Bachokirian	TD-V 4b	36,900±1,300 OxA-5173 Early Aurignacian
		TD-V 4b	38,300±1,800 OxA-5172 Early Aurignacian
11.III	38,500±1,700 OxA-3213 Bachokirian	TD-I 4A	31,900±1,600 Gd-2354 Early Aurignacian
11.IV	>43,000 GrN-7545 Bachokirian	TD-I 4B	38,200±1,500 OxA-5171 Early Aurignacian
		TD-I 4B	38,800±1,700 OxA-5170 Early Aurignacian
		TD-I 4B	39,100±1,800 OxA-5169 Early Aurignacian
11a	33,750±850 OxA-3184 indeterminate EUP	TD-II VI	>38,700 Gd-4687 transitional industry
13	>47,000 GrN-7570 Mousterian		

(simplified after J. K. Kozłowski 1999)

put it, "the classic Aurignacian" was present in this region from about 34,000 BP onwards.

According to present information (Brandtner 2000, Broglio 2000, Haesaerts *et al.* 1996, Neugebauer-Maresch 1999, Svoboda, Simán 1989, Svoboda *et al.* 1996, Valoch 1996) there are several radiocarbon dated Aurignacian open-air sites in Austria and the Czech Republic, to which the cave site of Mladeč must now be added (Svoboda *et al.* 2002). The data are summarised in *Table 2*. The more recent of these sites – Willendorf II layer 4, Milovice, Stratzing, Grossweikersdorf, and Stránská skála – are more or less comparable to the Aurignacian II assemblage at Geissenklösterle, both chronologically and in terms of their material culture. The dates for Stratzing relate to layer 2 only and are those considered most reliable by the excavator (Neugebauer-Maresch 1999: 67). One of them is associated with the human figure made of schist for which the site is well known (Neugebauer-Maresch 1999, Fig. 38). According to her, the assemblages from Willendorf II layer 3, Schwallenbach, and Senftenberg also contain typical Aurignacian elements, and these sites are of course chronologically closer to the Aurignacian III assemblage at Geissenklösterle. According to Haesaerts *et al.* (1996) Aurignacian layers 3 and 4 at Willendorf II can be linked to interstadial episodes labelled Schwallenbach I and II. The three oldest dates for Willendorf II come from unit D1 and therefore provide a minimum age for layer 2 in unit D2 (Haesaerts *et al.* 1996: Fig. 3). Opinions are divided as to whether the material from layer 2 can be called Aurignacian or not. Since it has produced only 31 artefacts, this is not surprising. The situation is a good deal clearer at Krems-Hundssteig, discovered in the closing years of the 19th century. According to Neugebauer-Maresch (1999: 60–64, 73, 109) this can be considered the richest and most important Aurignacian site in Austria, and it is regrettable that it was not the subject of a controlled excavation. Apart from an abundant lithic inventory (with carinate and nosed endscrapers as well as predominant lamelles Dufour), many pierced shells were found, decorative objects, suggesting links to Lake Balaton and in particular to the Mediterranean (Broglio 2000). The dates now available for Mladeč, in the range 34–35,000 BP, are on calcite from immediately above the human remains found at the site. They therefore provide a minimum age for these remains and, as Svoboda *et al.*

(2002) comment, they compare well with those from Vogelherd. Taken as a whole, the information from this area provides another indication that the typical and early Aurignacian from Geissenklösterle does not stand alone, and forms part of a larger continuum which may well date back 38,000 radiocarbon years.

The same story is revealed by the Balkan sites of Bacho Kiro and Temnata, excavated by J. K. Kozłowski and his team (Kozłowski 1982, Kozłowski 1999, Ginter *et al.* 1996, Hedges *et al.* 1994). The sequence of dates relating to these two sites (simplified after Kozłowski 1999) is set out in *Table 3*. All the ^{14}C dates obtained at various laboratories have been included, but not the TL dates, having regard to the critical comments made by Richter *et al.* (2000: 85). According to Bluszcz and his colleagues (1992, Tables 2 and 3) there are serious discrepancies between the TL results for their first and second set of burnt flints from Temnata. Two of the results first obtained for layer 4 (45,000±7,000 and 46,000±8,000 years BP) seem reasonable in the circumstances, but in view of the quite different results for the same layer obtained the second time round, it is probably best to leave them out of account for the moment.

Obviously the most important unit for our purposes is the "Bachokirian" Early Upper Paleolithic industry distinguished by Kozłowski in layer 11 at Bacho Kiro and its counterpart in Temnata layer 4. If, as Kozłowski asserts, it would be better to disregard the date at the boundary of Bacho Kiro layers 10 and 11 (OxA–3212), the occupation on the face of it still stretches for some 5,000 radiocarbon years, which he finds "puzzling" in the light of the "homogeneity" of the cultural remains (Kozłowski 1999: 102). If we exclude the infinite date from layer 11 phase IV (GrN–7545), we are still left with sure evidence for this unit at about 38,000 years BP, and the same is true for layer 4 at Temnata. The date for layer 11a at Bacho Kiro, as Kozłowski remarks, is obviously "too young", whereas the one for the "transitional" industry in layer VI on the talus at Temnata is infinite. Kozłowski emphasises that at neither site is there a convincing link to the local Middle Paleolithic. Layer 11a at Bacho Kiro is characterised as an "indeterminate" non-standardised Early Upper Paleolithic (Kozłowski 1999: 105–106). The "transitional" industry at Temnata is so defined because it contains evidence of a technological evolution from Levallois to Upper Paleolithic blade technology, hence it is compared to the Bohunician, but there is no discernible connection between it and the Early Aurignacian in layer 4 (Ginter *et al.* 1996: 175, 190). At the newly discovered site of Klisoura Cave 1 in Greece, an equivalent to the Balkan Early Aurignacian is absent, and the Upper Paleolithic succession, which begins with what is apparently an Uluzzian horizon, is said to be more similar to the Italian one (Koumouzelis *et al.* 2001).

Since the dating evidence for the Early Upper Paleolithic at Bacho Kiro and Temnata appears to be quite firmly based, then once again as at Geissenklösterle the argument turns on the claim that this is not a "true" Aurignacian (Zilhão and d'Errico 1999: 42–43). In the later part of the sequence at Bacho Kiro there are elements which presumably can be accepted as typical Aurignacian without any difficulty, including both split-based and non-split-based bone points, as well as carinate and nosed endscrapers. In coining the term "Bachokirian" to emphasise the distinctiveness of the assemblages in layer 11 at the site, Kozłowski may have unwittingly facilitated the kind of argument referred to, but in fact he has always insisted on the Aurignacian affinities of these assemblages. The Bachokirian, or "the oldest Balkan Aurignacian" as he puts it (Kozłowski 1999: 106), reveals a "fully-developed Upper Paleolithic technology with a relatively low proportion of Aurignacian diagnostic forms", including Aurignacian retouched blades and nosed endscrapers, and at Temnata carinate endscapers as well (Ginter *et al.* 1996: 192). As at Geissenklösterle, it is not reasonable to expect the "full cultural repertoire" of the Aurignacian to have sprung into being at once, indeed it would be quite reasonable to expect the opposite. The dating evidence summarised above shows that the Aurignacian in the Balkans is at least as old as at Geissenklösterle, and in my view Kozłowski may well be right in his claim that "this culture unit" began "in that territory earlier than in other parts of Europe" (Kozłowski 1999: 116).

The remaining key site that needs to be considered is Istállóskő. This will be done in connection with the evidence for the Szeletian in Hungary, but before leaving the Aurignacian a few words should be said about the "Olschewian", which came up in connection with Vindija and Velika Pecina. The concept of the "Olschewian" was created by Bayer in 1929 following the first excavations by S. Brodar at Potocka Zijalka. According to him, it was distinguished by the following features: (1) a predominance of bone points over stone artefacts, (2) the bone points were exclusively non-split-based, (3) many of the animal bones were artificially pierced, and (4) the culture was confined to caves where the inhabitants hunted mainly cave bear. The Olschewians were said to be contemporary with the Aurignacians, but they lived in a degree of isolation and formed a separate race (Bayer 1929). When the complete record from Potocka Zijalka was published, many years later, it was shown how erroneous many of these ideas were (Brodar, Brodar 1983). The site does have a predominance of bone tools (130) over stone tools and cores (76) but the bone points do include one split-based example, and there are characteristically Aurignacian elements among the stone tools, including carinate and nosed endscrapers. The site was therefore published as "eine hochalpine Aurignacjägerstation", as Mokriska Jama had been some time before (Brodar 1960). Hahn had already suggested an alternative interpretation, whereby these sites could be regarded as a specialised variant of the Aurignacian, and for reasons already advanced I share this view (Allsworth-Jones 1990a: 160, 193–196). It was already known in Bayer's time that non-split-based bone points had been found in open-air sites such as Willendorf II layer 4

(Neugebauer-Maresch 1999, Fig. 30), so this is an additional argument in favour of the unity of the complex. Cave sites broadly similar to Potocka Zijalka, most with smaller inventories which may include only non-split-based bone points, occur quite widely in the area. In the Austrian Alps, Neugebauer-Maresch (1999) and Fuchs (2000) mention Drachenhöhle, Grosse Badlhöhle, Tischoferhöhle, and Liegl loch. Mladeč has already been mentioned, but Zlatý kůň must now presumably be discounted (Svoboda 2000, 2001a, Svoboda *et al.* 2002). The few artefacts in layer 8 at Pod Hradem, with four dates in the range 28,200–33,300 BP, can probably be regarded as Aurignacian as well (Valoch 1995, 1996). In Trans-Carpathian Ukraine, the small site of Molochnyi Kamen' has a (rather recent) ^{14}C date of 25,550±350 BP (GrN–7761) and a good pollen record, apart from the usual kind of inventory found at sites like this: one fragment of a bone point, and 24 stone artefacts, including 13 retouched tools, but no cores (Gladilin, Pashkevich 1977). The Aurignacian sites in the Bükk mountains have also sometimes been referred to as Olschewian, so it is worth recalling that Peskö (dated to 34,600±530 BP: GrN–4950) had 13 bone or ivory artefacts (including both non-split-based and split-based bone points) and 27 lithics. At Istállóskő, Vértes established that although the lower Aurignacian layer had mainly split-based bone points there were a couple of non-split-based ones, and the opposite holds true in the upper Aurignacian layer. The lower layer incidentally had very few lithics, which are not markedly Aurignacian in character, so this reinforces the point that the "full cultural repertoire" of the Aurignacian should not necessarily be expected in every case (Allsworth-Jones 1986: 91–92).

In reviving the concept of the "Olschewian" Montet-White (1996, chapter 6, Figs. 44–45) lays particular emphasis on the occurrence of non-split-based bone points in cave sites with sparse inventories. Karavanic (2000) suggests that the term should be used to designate an industry of a "regional character", which usually does not conform to that of the "classic European Aurignacian", and which may represent an "indigenous cultural development" in this area (Karavanic, Smith 1998). For the reasons mentioned above, I think that this viewpoint is difficult to sustain. I furthermore think that split-based bone points in particular can be taken as a reliable hallmark of the Aurignacian as such (cf. Svoboda 2001a). Of course it is now known that bone tools were produced in the Middle Paleolithic, for example at Salzgitter-Lebenstedt (Gaudzinski 1999). Nonetheless, as Heidi Knecht's studies have shown (Knecht 1999), split-based bone points (unlike for example sidescrapers or leafpoints for that matter) were by no means an obvious idea, and wherever they occur in an unambiguous context that context is always Aurignacian. If my view is correct, and the "Olschewian" can be removed from the equation, the alternative explanations for the situation in layer G1 at Vindija become more sharply opposed, and in my opinion we would probably be on safer ground to regard this layer as not homogeneous.

THE SZELETIAN

Turning now to the Szeletian, it must be emphasised that strictly speaking this is only one of a number of entities in Central Europe which are characterised among other things by leafpoints, and not all of these (for example the Altmühlian in southern Germany) could be described as transitional by any means. In this respect the situation is a good deal more complicated than in western Europe. It seems to be generally accepted that the Bohunician, described by Svoboda as "Levalloisian-leptolithic", should be separated from the largely non-Levallois Szeletian, and based mainly on the finds from Nietoperzowa cave a separate "Jerzmanowician" entity in southern Poland has also been recognised for quite some time. Reference has already been made to the Hungarian sites west of the Danube which Gábori-Csánk (1993) re-christened Jankovichian in order to distinguish them from the Szeletian in the Bükk. The available dates for the Bohunician, Szeletian, and Jerzmanowician sites outside Hungary are listed in *Table 4*, taking account of recent results (Gladilin 1989, Gladilin, Demidenko 1989, Kozłowski 2000, Kozłowski, Kozłowski 1996, Oliva 1991, Svoboda, Simán 1989, Svoboda *et al.* 1996, Usik 1989, 1990, Valoch 1989, 1995, 1996).

The three older original dates from Bohunice have been complemented by a more recent one of ca 36,000 radiocarbon years obtained during excavations in 1985 (Svoboda *et al.* 1996: 206–207). The published dates for Stránská skála are approximately of the same order, but they are noticeably older than the dates for the Aurignacian at the site. This is not surprising since, as Svoboda ascertained, the Bohunician at least in some places is stratified below the Aurignacian at Stránská skála (Svoboda, Simán 1989, Table II, Svoboda *et al.* 1996: 230–234). It is understood that, thanks to more recent excavations, further dates have been obtained, in the vicinity of 35,000 years BP, and in that case the difference between the Bohunician and the Aurignacian at the site would be less marked (Svoboda, pers. comm.). In general there is a good parallel to the Bohunician at Korolevo in Trans-Carpathian Ukraine. There are several localities at this site, and the parallel to Bohunice is provided by the industry at Korolevo II layer II (Gladilin 1989, Gladilin and Demidenko 1989: 143–163, Usik 1989, 1990, Valoch 1989). This has a ^{14}C date of 38,500±1,000 BP (GIN–2774). The Aurignacian is represented at Korolevo I layer Ia, with a ^{14}C date of 25,700±400 BP (GIN–2773) (Gladilin, Demidenko 1989: 163–177). Hence, while there is no direct stratigraphic superposition, the Aurignacian is once again later than the Bohunician equivalent industry at this site. Valoch considers that the date for the Aurignacian here is too young and probably contaminated. The site has also produced TL dates of 35±6 and 60±8 kyrs stratified in the composite profile above each of the two industries concerned (Gladilin 1989, Fig. 4) but they seem to be much too old, and the ^{14}C dates provide a surer guide.

TABLE 4. Bohunician Szeletian Jerzmanowician dated sites.

Bohunice		**Vedrovice V**	
36,000±1,100	GrN-16920	35,150±650	GrN-15513
40,173±1,200	Q-1044	37,600±800	GrN-15514
41,400+1,400 –1,200	GrN-6802	37,650±550	GrN-12374
42,900+1,700 –1,400	GrN-6165	39,500±1,100	GrN-12375

Stránská skála			**Dzierzyslaw I**	
III.5	38,200±1,100	GrN-12297	36,500±5,500	GdTL-349
III.5	38,500+1,400 –1,200	GrN-12298		
			Čertova pec	
IIIa.4	41,300+3,100 –2,200	GrN-12606	38,400+2,800 –2,100	GrN-2438

Korolevo			**Nietoperzowa**		
II.II	38,500±100	GIN-2774	4/5a	30,500±1,100	Gd-10023
			6	38,500±1,240	GrN-2181

The dates for Vedrovice V, still the only single-layer *in situ* Szeletian site excavated in Moravia, run more or less in parallel with the Bohunician (Valoch 1996), but according to Kozłowski and Foltyn at Dzierzyslaw I in southern Poland the Szeletian has been found stratified above the Bohunician (Bluszcz *et al.* 1994, Kozłowski, Kozłowski 1996: 52, 105, Kozłowski 2000). As a result of his excavations, E. M. Foltyn established that there were two occupations at the site. The lower occupation is in layer 4, a pseudo-gley, and the upper occupation is in layers 3a and b, a solifluction series (Bluszcz *et al.* 1994: Fig. 2). The TL date obtained, 36,500±5,500 BP, apparently relates to the upper occupation. In terms of the tool types represented, there does not seem to be much difference between them (Bluszcz *et al*. 1994: Table 2). The dated industry from Čertova pec unfortunately is small and hardly diagnostic. Layer 6 at Nietoperzowa, with the bulk of the finds attributed to the Jerzmanowician, has long been dated to 38,500±1,240 BP (GrN–2181). There is now a further date of 30,500±1,100 BP (Gd–10023) for layer 4/5a (Kozłowski, Kozłowski 1996, Fig. 10) but there is only a little material in this layer, and if the dates are taken at their face value the cave can only very sporadically have been visited over a space of 8,000 radiocarbon years (Allsworth-Jones 1990a: 192–193). Finally there is some new evidence from Oblazowa cave in the Polish Carpathians (Kozłowski 2000: Figs. 6 and 7). Layer XI with an industry described as Szeletian is surmounted by sparse indications of an Aurignacian occupation and then by layer VIII with a Gravettian occupation dated between 30,600 and 32,400 BP. There is an AMS date for layer XI of 23,420±380 BP, but as Kozłowski says this cannot be regarded as acceptable in view of the disagreement with the Gravettian layer above. The finds from layer XI, which are not abundant, include one bifacial leafpoint and one non-split-based bone point.

The evidence summarised above is not too abundant, compared with what we have already seen for the Aurignacian in Central and South-eastern Europe. The dates for the Aurignacian have been listed and the case has been made that it was present in the area by at least 38,000 radiocarbon years BP. Some of these dates, particularly for the Bohunician, go back beyond that time, and the stratigraphic superposition of the Aurignacian above the Bohunician at Stránská skála is obviously significant as well. But it has always been supposed that the transitional technologies had their origins in the Middle Paleolithic, so this should occasion no surprise, and it is more significant that they apparently carried on alongside it. Conceding the early dates for the Aurignacian which do exist, and comparing them with the evidence from Moravia, Svoboda recently attempted to square the circle by suggesting that "in a Europe dominated by transitional cultures, the Aurignacian dispersed quite rapidly over the continent and accelerated the Upper Paleolithic development processes" that had already begun (Svoboda *et al.* 1996: 103). I think this is a very fair way of putting it. Did such technologies lead the way in pioneering an independent and fully realised transition to the Upper Paleolithic? In an earlier study Svoboda reconstructed the way in which a technological development could be traced – in theory at least – from Middle to Upper Paleolithic starting from a Levallois base at Ondratice (Svoboda 1980, Allsworth-Jones 1990b, Figs. 3, 4, 6). At Korolevo, Usik has improved on this by reconstructing literally – by means of refittings – a plausible technological succession from a local Levallois-Mousterian base (I.IIB) via a transitional phase with leafpoints (II.II) to the Aurignacian (I.Ia). Nonetheless in my view there is still a gap between the last two. This is Valoch's opinion also. As he puts it, a "direct genetic relation between the industries from Korolevo II and Korolevo I does not seem to be probable" (Valoch 1989: 90). The same viewpoint is

TABLE 5. Szeleta and Istállóskő.

Szeleta	**Istállóskő**
(layer numbering after Kadic text 1916, 1934)	(layer numbering and designations after Vértes 1955)
new dates at cave entrance	layer 8 Aurignacian 2
* 11,761±62 ISGS-A0 128	30,900±600 GrN-1935
* 13,885±71 ISGS-A0 129	charcoal from hearth
(hearth, anomalous)	31,540±600 GrN-1501
	charcoal from hearth
layer 7/a (top of section)	* C/D 27,933±224 ISGS-A0 186
* 22,107±130 ISGS-A0 131	Aurignacian 2
layer 4 (base of section)	* D 31,604±295 ISGS-A0 188
* >25,200 ISGS-4460	
* 26,002+182 ISGS-A0 189	* E 29,035±237 ISGS-A0 185
layer 7 Developed Szeletian	* G (above and below)
32,620±400 GrN-5130	32,701±316 ISGS-A0 187
	33,101±512 ISGS-A0 184
	Aurignacian 1
layer 4 Early Szeletian	layer 9 Aurignacian 1
>41,700 GXO-197	39,700±900 GrN-4658
	bone collagen
layer 4 base central chamber	layer 9 Aurignacian 1
* 42,960±860 ISGS-4464	44,300±1,900 GrN-4659
	bone collagen near base of layer
layer 3 Kadic level XII	
43,000±1,100 GrN-6058	

(* Dates provided by Brian Adams (2001) from his recent excavations with Arpad Ringer; his Szeleta layer numbering revised to agree with Kadic's text)

expressed by Kozłowski in relation to the "transitional industry" from layer VI on the talus at Temnata (Ginter *et al.* 1996: 190, 198–199). Hence the argument I previously advanced that to cross that threshold the Szeletian required the impact of an external force.

Up to now, in my view, one of the most compelling cases to prove this point has been the Szeletian of the Bükk, and more particularly Szeleta itself in comparison with Istállóskő. The thesis that these two sites ran in parallel has been buttressed both by dating and by some archaeological evidence. Thanks to research conducted by the late Laszlo Vértes, ^{14}C dates were obtained for the two sites. At Istállóskő, the upper Aurignacian 2 layer has two dates of 30,900±600 and 31,540±600 BP, whereas the lower Aurignacian 1 layer has two dates of 39,700±900 and 44,300±1,900 BP. Obviously there is nothing too controversial about the dates for the upper Aurignacian layer. Some unease has been expressed about the oldest date, which was found near the base of the lower layer, but as we have seen the date of 39,700±900 BP is not impossible by comparison with other dates for the Early Aurignacian in this part of Europe. At Szeleta, the Developed Szeletian (using Vértes's terminology) had a date of 32,620±400 BP and the Early Szeletian an infinite date of >41,700 BP. A date at the base of the sequence of 43,000±1,100 BP could best be explained (Allsworth-Jones 1986) as preceding the Szeletian, possibly related to a minor Middle Paleolithic occupation deep in the interior of the cave. The archaeological backing is provided by a few finds in each site which seem to suggest contact with the other. In 1928 a split-based bone point was found in the Early Szeletian layer at Szeleta and another fragmentary such point was found in the same layer in 1947 (Allsworth-Jones 1978, 1986), whereas two characteristically Szeletian leafpoints were found by Vértes in the upper and lower Aurignacian layers at Istállóskő.

This has been the position hitherto, but there are two new sets of observations to add to it (Adams 1998, 2001). In the first place, new excavations have been conducted by Brian Adams and Arpad Ringer both at Szeleta and at Istállóskő in 1999 and 2000. Adams very kindly made the new ^{14}C dates for the sites available to me, and they are shown together with the old ones in *Table 5*. Secondly, he undertook a re-examination of the record from the Bükk mountain region in a thesis which was published in 1998. All the points which he raised in his thesis cannot be discussed here (they are discussed in detail in Allsworth-Jones, 2000b) but mention must be made of his main conclusions, which are relevant in this context.

There are five new dates for Istállóskő. The dates for layers C/D, D, and E relate to the upper portion of the deposits and, while they are not internally consistent, they are not in contradiction with the previous two dates for Vértes's Aurignacian 2. Layer G at the base of the sequence

was identified as the equivalent of Vértes's Aurignacian 1, and two dates were obtained, one above and one below it, at 32,701±316 and 33,101±512 BP. As Adams says, if these results are accepted as the only valid ones for the lower Aurignacian layer, then the effect would be to rejuvenate it by at least 7,000 years. The results for Szeleta are more problematic. One sample (ISGS–4464) came from the central part of the cave, the rest were from the entrance. The one from the central chamber, at the boundary between layers 3 and 4, agrees very well with the previous evidence, and provides an oldest finite date for the Early Szeletian in the cave. The other dates are much younger than what has previously been available. Two of them (ISGS –AO128 and –AO129) are agreed to be anomalous and if valid relate to the end of the Pleistocene. The other dates are said to refer to the Developed Szeletian (ISGS–AO131) and the Early Szeletian (ISGS –4460 and –AO189). Without necessarily querying the dates, my doubts concern the proposed stratigraphic correlation between them and the deposits in the rest of the cave. Kadic referred to the presence of a light yellow layer (which he did not number) at the entrance to the cave at the top of his layer 7, and I am wondering whether the date of 22,107±130 BP relates to this. There are finds in the cave which typologically are Gravettian, and there are two dates for a Gravettian occupation at the nearby Balla cave of 20,000±190 and 22,300±180 BP (GrN–4661 and –4660). This date would fit in very well with that. Adams raises a doubt as to whether Vértes's date of 32,620±400 BP really relates to the Developed Szeletian. I do not see any reason to query Vértes's attribution, but if for the sake of argument we do so, and we also accept the new date of 42,960±860 BP for the base of the sequence, that would still give us a perfectly acceptable time bracket for the Early Szeletian in the cave. A Developed Szeletian stretching from ca. 32,000 to ca. 26,000 BP (despite the attribution given to the latter date) would then be somewhat on the young side, but by no means impossible. Apart from the dates, interestingly enough, the new excavations have provided another indication of contact between Istállóskő and Szeleta. Adams reports that "a broken leafpoint made from the same material used to produce the majority of leafpoints at Szeleta" was found 40 cm above the top of layer G (Aurignacian 1) at Istállóskő. Whatever the story eventually turns out to be in regard to the dates therefore the claimed archaeological parallel between the two sites continues to hold good.

With regard to the interpretation to be given to the Aurignacian and the Szeletian in the Bükk as a whole, Adams's view (1998) is that there was no transition from Middle to Upper Paleolithic in this area, and that the Szeletian was no more than a specialised activity variant of the local Aurignacian. The leafpoints at Szeleta are regarded as the functional equivalent of Aurignacian bone points. Indeed the raw material used at that site is rather special, and its suitability for bifacial working has been remarked upon by many authors including myself, but I do not think this provides an adequate justification for the conclusion reached. Within the Bükk, the differences between the two entities seem to me to stand decisively in the way of an acceptance that they are in fact identical, nor do I think that the evidence from this area can be treated in isolation. All the other evidence mentioned above has to be taken into account as well. I do not believe there is a convincing parallel elsewhere in the European Early Upper Paleolithic for a wholesale substitution of artefacts such as hypothesised by Adams, although the author does incline to the view that the Châtelperronian and the Aurignacian in Western Europe can be treated as identical for the same reasons as are advanced by him in relation to the Aurignacian and the Szeletian in the Bükk (Adams 1998: 14).

CONCLUSION

When all caveats are admitted and all allowances made for the fragility of the evidence at a period of 30 to 40 thousand years ago, I submit in the light of the evidence summarised above that the hypothesis I originally advanced whereby the Szeletian (and related entities) were defined as the product of acculturation at the boundary of Middle and Upper Paleolithic can still be defended. Obviously the argument about the interpretation to be given to the evidence from Central and South-eastern Europe forms part of a larger debate about Neandertals and modern humans, much of which is concerned with the Châtelperronian and the Aurignacian in Western Europe, and evidently my interpretation tends to favour one school rather than the other (Davies 2001, Hublin 1999, Hublin *et al.* 1996, Kozłowski, Otte 2000, Mellars 1992, 1998, Mellars *et al.* 1999, Mercier *et al.* 1991, Stringer, Grün 1991, Taborin 2002, White 1993, 2000, 2002, White, Taborin 2000, versus Bahn 1998, d'Errico *et al.* 1998, Zilhão 2000, 2001, Zilhão, d'Errico 1999, 2000). Nonetheless, as I said at the beginning, in a situation where the contemporaneity or overlap of Neandertal and modern man is seemingly admitted on all sides, the precise nature of the interaction between them and the role of what have been called transitional industries becomes a matter of nuance rather than stark contrast. As White (2000: 46) has said, "we need not link an acculturation model to the presumption of cognitively handicapped Neandertals", but the fact is that at the end of the process, for good or ill, only *Homo sapiens sapiens* survived in Europe.

So far as Central and South-eastern Europe is concerned, obviously there is a lot to do, and quite possibly the situation may look very different in a few years' time.

It is clear that the dating record needs to be improved. Now that AMS facilities are available, I hope that museums and other institutions in this area can be persuaded to release samples of bone artefacts, including those from old excavations at sites which have practically been emptied of deposits, for direct dating. This in itself may change the picture considerably.

So far as archaeological nomenclature is concerned, I think there is a good deal of merit in the proposal by Churchill and Smith (2000: 74–75 and note 4) to rephrase the debate in terms of an Initial Upper Paleolithic. They have borrowed this term from Kuhn *et al.* (1999: 504–505, in fact it originated with A. E. Marks, Kuhn and Stiner, pers. comm.) who have applied it in the Near East instead of the hitherto accepted "transitional" formulation. IUP is preferred as being more "neutral and appropriate", since as they say "transitional" "presumes a phylogenetic relationship between Mousterian and Upper Paleolithic based on a simple combination of technological traits", which may or may not be demonstrated. The relevance of this is evident in view of what has been said above about the Bohunician in particular. In fact, as Churchill and Smith point out, it is "reasonable to ask if IUP assemblages might better be considered as belonging to the final Middle Paleolithic". These and other questions will need to be addressed.

Finally, I do agree it is time more attention was paid to other, behavioural, aspects of the change which occurred in Central and South-eastern Europe in this period. Féblot-Augustins (1993) for example showed what could be done in terms of the study of mobility strategies as revealed by raw material transport patterns. What could be derived from the study of the old excavations was necessarily limited because of the restricted data base which they provided. With new methods, new sites, and new research goals it should be possible for this limitation to be overcome.

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