

Ethnicity and early medieval cemeteries¹

Etnicidad y cementerios altomedievales

Guy Halsall *

SUMMARY

This article responds to recent work by Michel Kazanski and Patrick Périn, defending the ability of archaeology to recognise ethnic identity in the burial record of the early Middle Ages. After summarising the main outlines of their argument, it takes the components of their hypothesis in turn and subjects them to analysis. This analysis is based around the archaeological evidence and what it can and cannot say without the intrusion of preconceptions drawn from a (usually old-fashioned) reading of historical sources. After finding the argument wanting even on its own terms, the article concludes by looking at the nature of ethnicity itself and whether it is likely to leave such obvious and straightforward traces in the archaeological record.

Key words: Ethnicity, Burial, Archaeology, Early Middle Ages, Western Europe

RESUMEN

Este artículo pretende responder al reciente trabajo de Michel Kazanski y Patrick Périn, que defiende la capacidad de la Arqueología para reconocer la identidad étnica en los registros funerarios altomedievales. Tras resumir las líneas principales de su argumentación, somete a análisis cada postulado de sus hipótesis. Dicho análisis se basa en la evidencia arqueológica y en lo que ésta puede o no aportar, sin introducir preconcepciones extraídas de una lectura (generalmente anticuada) de las fuentes históricas. Tras encontrar el argumento deficiente, incluso en sus propios términos, el artículo concluye planteando la naturaleza de la etnicidad en sí misma, y si es verosímil que deje tan obvias y directas huellas en el registro arqueológico.

Palabras clave: Etnicidad, Enterramiento, Arqueología, Época altomedieval, Europa Occidental

This paper responds to two recent articles by Michel Kazanski and Patrick Périn (KAZANSKI/PÉRIN 2008; 2009), which make an extended and sustained attempt to make a case in favour of archaeology's ability to recognise and identify ethnic identity, particularly in cemeteries. In Britain at least, this would not be a fashionable position to take, since the publication of Siân Jones' monograph on the topic (JONES 1997), even if one can argue that in Anglo-Saxon archaeology its implications have not yet been fully internalised. In the archaeology of mainland

Europe, however, it remains a much more respectable stance and indeed seems currently to be supported by one might call a "counter-revisionist" scholarly offensive. Patrick Périn's knowledge of the archaeological data from France, particularly the burial record, pertaining to the Merovingian era is second to none; indeed one wonders whether it will ever be matched. Similarly, Michel Kazanski has an unrivalled empirical knowledge of metalwork and of the archaeology of the East Germanic-speaking regions of late antiquity. For all these reasons,

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* Department of History, University of York, United Kingdom

these publications deserve to be taken seriously but the ideas they express must be subjected to close scrutiny. This is a mark of the respect with which this work deserves to be considered.

The argument in the longer and more detailed piece (KAZANSKI/PÉRIN 2008) can be summarised as follows:

Kazanski and Périn take their methodological starting point from work published by H.-J. Eggers (1950), which claimed to derive its strength from the avoidance of the 'Mischargumentation' (mixed argumentation) which avowedly characterised earlier work. Instead, it allegedly treated the different bodies of evidence – historical, archaeological, linguistic or onomastic, etc. – separately and on their own terms. These conclusions are then compared to produce an overall theory. This looks exactly the same as the "multidisciplinary" methodology used in my first book, on the Merovingian Region of Metz (HALSALL 1995). It aims at the same advantages and at avoiding the same pitfalls. Ironically, however, I adopted that methodology to avoid pitfalls in work carried out using Eggers' methodology! There is a link between the claims made in these articles and those which Périn is accustomed to make (not untypically in French academic practice), of 'Cartésianisme': that is to say a radical scepticism, making no prior demands on the evidence; everything must be demonstrated through reason. These assertions of methodological rigour and purity ("purification régressive") must be subjected to close examination.

'Mischargumentation', an alleged mix of archaeology folklore, linguistics and history thrown together in an ad hoc fashion, was what post-war archaeologists like Eggers claimed, not incorrectly, had lain behind the Germanist, nationalist works of Gustav Kossinna. Kossinna's work, of course, had been popular with the Nazis and had underpinned some of Hitler's claims to territory, in France and in the Soviet Union (FEHR 2002). German archaeologists wanted to distance themselves from this. Similarly, Kazanski and Périn argue that Kossinna's ideas had seen archaeological cultures as simple reflections of ethnic groups –equated with

peoples or nations- in too monolithic a way. Their work, they state, is based on quite different premises. They also claim that it would be unrealistic to expect homogenous or monocultural archaeological manifestations of the barbarians planted on Roman soil and make sensible statements about the fluidity of ethnic identity and the mixed and changing composition of supposedly ethnic groups. This takes account not only of the work of Reinhard Wenskus but also of his successors such as Herwig Wolfram and perhaps even of Walter Pohl (WENSKUS 1961; WOLFRAM 1988; 1997; POHL 1998). So far, one might say, so good. However, whether, or to what extent, these fine sentiments are reflected in Périn and Kazanski's actual conclusions needs to be scrutinised.

The two authors argue that the acculturation of barbarians on Roman soil was 'ineluctable' and demonstrate this through the example of the Visigoths. In the course of a 30-year wandering across Europe, by the time the 'Visigoths' arrived in Gaul in 412, 'where they were tasked with the repression of the *Bagaudae* and formed a kingdom' (KAZANSKI/PÉRIN 2008:188) they had lost their material culture. This 'disacculturation' led to a rapid acculturation in Aquitaine and explains why the Visigoths left no archaeological traces there. When forced into Spain they developed, by contrast, a national material culture, and in this were helped by their contact with the Ostrogothic army of Widimer. This is an argument that Périn has made before (PÉRIN 1993).

Kazanski and Périn then discuss a series of criteria that are relevant to the definition of ethnicity:

- 1: Funerary practices: They claim (KAZANSKI/PÉRIN 2008:191) that burial practices are strictly linked to religious belief in traditional societies and thus deeply rooted within ethnic groups. They are also linked to social factors. All that said, Kazanski and Périn nevertheless conclude that it would be impossible to distinguish, archaeologically, a Barbarian who was perfectly integrated in Roman society or a Roman living in *barbaricum* and buried according to local practice.

2: Ethnic costume: This is a key pillar of Kazanski and Périn's argument but it is developed mostly with regard to female costume, as we shall see shortly.

3: Ethnic weapons: Males were trained in the use of weapons from early boyhood onwards and so, say Kazanski and Périn (2008:195-6), particular weapons can be identified as ethnic markers. The example they use is that of the *francisca*, which (they say) is found throughout the Frankish 'protectorate'.

4: Traditional Female Costume: As mentioned, this is the key support of the argument (KAZANSKI/PÉRIN 2008:196-9). According to Périn and Kazanski, in traditional societies these costumes are sacralised and regulated. Vague reference is made to the work of ethnographers in support of this point, but it is nevertheless claimed to be almost a universal rule, proved over and over by anthropologists (KAZANSKI/PÉRIN 2008:196; 2009:150). Against the back-drop of a claimed ethnic costume for east and west Germanic women, they then discuss a series of tombs, where the brooches are of the 'wrong' type, or where the 'right' brooches are worn in the 'wrong' place as examples of acculturation.

The occupant of grave 756 at Vicq, for example, wore a Visigothic buckle and a pair of bow brooches at the shoulder but also wore, at the chest two small local zoomorphic brooches (KAZANSKI/PÉRIN 2008:196-7 & 195, fig.22).

On the other hand, the subject of grave 140 at Nouvion-en-Ponthieu wore two 'Visigothic' brooches but at the waist rather than at the shoulders, where they 'should' have been (KAZANSKI/PÉRIN 2008:197-8).

Explanations relating to the dead belonging to the second generation of immigrants are adduced (KAZANSKI/PÉRIN 2008:198).

5: Hand-made pottery. This is claimed not to be an object of commerce, but made by women in the settlement and therefore a sign of ethnic identity (KAZANSKI/PÉRIN 2008:198-9).

6: Germanic Animal Art. Kazanski and Périn claim that this has specifically pagan and therefore barbarian significance (KAZANSKI/PÉRIN 2008: 199-201).

On the basis of these points, Kazanski and Périn move on to four case studies:

1. The presence of West Germanic barbarians in northern Gaul in the late 4th and early 5th centuries (KAZANSKI/PÉRIN 2008:201-207).

2. Eastern Barbarians in Gaul at the same time (KAZANSKI/PÉRIN 2008:207).

3. Common Prestige fashions among barbarian warriors in the second half of the fifth century (KAZANSKI/PÉRIN 2008:207-209).

4. Germanic minorities in Gaul in the late fifth and early sixth centuries (KAZANSKI/PÉRIN 2008:209-12).

So much for Kazanski and Périn's argument, which originates in work by Kazanski (KAZANSKI 1997). As stated, it is based upon a very thorough knowledge on the one hand of Merovingian archaeology in France and, on the other, of the material culture, particularly the metalwork, of the cultures from around the Black Sea and Danube areas and their presence in Gaul. As far as empirical awareness of data is concerned I cannot hope to equal these authors. However, the logical and methodological coherence of the arguments they present, in support of the idea that ethnicity can be detected through the archaeology of Gaul at the time of the Migrations, can be examined more closely.

Serious engagement with these ideas is a mark of respect for the work (and knowledge) of their authors. This makes it all the more disappointing that Périn and Kazanski do not deal with the growing literature on early medieval cemeteries that rejects their model and its antecedents. This work is simply ignored. Under this heading I could include Sebastian Brather's monumental *Ethnische Interpretation in der frühgeschichtliche Archäologie* (BRATHER 2004a), or Bonnie Effros' writings on the supposedly conservative dress of barbarian women (EFFROS

2004) or Philipp von Rummel's discussions of the shortcomings of ideas about barbarian dress (VON RUMMEL 2007; see also von RUMMEL 2010), or other Freiburg School studies casting doubt on the geographical origins of key classes of object (GAUSS 2009), or my own or Frans Theuws' studies of the late Roman and Merovingian cemeteries of Gaul (HALSALL 1992; 2000a; 2010:131-67; THEUWS 2009; THEUWS/ALKEMADE 2000), and so on. It is possible to read between the lines and to see these pieces – implicitly – as part of a growing counter-offensive by traditionalist archaeologists against new readings of the excavated data, prompted mainly by the publication of Brather's book (BIERBRAUER 2004; BROGILO/CHAVARRÍA ARNAU 2010; VALENTI 2009), but one would have preferred a closer engagement with the specific arguments proposed in the newer works, rather than a simple restatement of the old views, and the application of an unjust *damnatio memoriae* on revisionist work.

In British archaeology, the approach taken would usually be to address the nature of ethnicity and whether, theoretically, such forms of identity would or could be identifiable in the archaeological record. Adopting that line of argument would, however, leave us in a position (for reasons we shall encounter at the end) with little to say about Kazanski and Périn's works other than simply to restate an important methodological difference between British and much of mainland European early medieval archaeology. Instead, therefore, it has been felt more profitable to examine, in depth and on their own terms, the arguments of this recent and detailed defence of the traditional viewpoint. Other problems with the project of detecting ethnicity in the cemetery evidence will then be discussed.

To what extent does Kazanski and Périn's argument live up to the claims of methodological purity made for it? One obvious point must be made at the very outset, and cannot be made too forcefully: an object does not have an ethnicity. This is perhaps an insultingly obvious point, but how many times do we read in archaeological literature (not just in the work of the two authors under discussion) about a *Visigothic* belt buckle, or a *Lombard* brooch?

At the 2010 International Medieval Congress in Leeds Philipp von Rummel, was asked what we should call belt buckles or brooches if not Gothic or Vandal or whatever. He replied by drawing attention to the fact that no one has any difficulty in talking about pottery without using ethnic terminology. An amphora is Spanish or Eastern Mediterranean, a fine ware bowl is African Red Slip or a *dérivée sigillée paléochrétienne*, or whatever. There is no reason why we cannot use such general terms for brooches as well, or (perhaps better) just describe them in terms of their principal features (as Anglo-Saxon archaeologists do, with their "Great Square-Headed" Brooches &c.). One really must wonder how much clearer the archaeology of the *Völkerwanderungszeit* would become if all these superfluous ethnic terminologies were abandoned.

Indeed assigning *any* ethnic name to archaeological evidence is quite impossible on archaeological grounds alone. *No* specific ethnic identity of *any* sort can ever simply emerge from the archaeological record on its own, whether that record be studied through artefact design, or from distribution maps or charts of percentage frequencies (as, e.g. in SIEGMUND 1998; 2000). Such an interpretation can only – ever – arise through the application to the archaeological data of a series of assumptions derived from written sources. In other words, the ethnic interpretation of material cultural data can never, ever result from looking at archaeology alone, and taking it on its own terms. Put another way, *no* ethnic interpretation of archaeology can ever claim to be 'Cartesian', or to be using 'pure' archaeological reasoning. To assign any of these names to an object immediately contaminates the archaeological evidence with the influence of an historical narrative. Indeed, a number of Kazanski and Périn's examples are made entirely in accordance with one particular view of the period's history.

Let us take, for example, the case of the Visigoths in Aquitaine and Spain. The first thing that needs to be said is that the whole problem is driven – indeed the 'problem' is *created* – by the historical narrative. Without a historical record that told us that people called Goths

came to Aquitaine in the second decade of the fifth century, there would be no problem, there would be no 'absent' or 'invisible' Goths to explain. The second problem, and it is one to which I will return, is that it assumes that the historical record, in the form of historically-attested ethnic identities, would be simply, passively reflected in the archaeological record, and that the variability in that record will automatically reflect the variability or oppositions suggested in the written data.

The next issue with this case study concerns Widimer and his army. For the last twenty years Patrick Périn has used this 'Ostrogothic' army as the explanation upon which to hang the appearance of an apparently Gothic material culture in Spain (PÉRIN 1993). Unfortunately, there is little or no evidence for this army. Widimer is not attested in any contemporary sources: only in Jordanes' *Getica* from the middle of the sixth century. A Billimer mentioned by Paul the Deacon has been suggested to be the same man but neither source is very trustworthy on these matters. A Wittimer appears in two letters of Ruricius of Limoges who might or might not be the same man, but they say nothing about his arrival from Italy or anything that would confirm Jordanes' story (HALSALL 2007:278-9; 2010:70). Even in the late, unreliable stories we have, there is insufficient evidence to say what became of this army. It is nowhere said that it ever went to Spain. All told, this example is about as far away as one could possibly get from being an example of a 'Cartesian' approach to the archaeological evidence, without being influenced by data from other sources.

A second instance can be found in the citation of the *francisca* as a diagnostically Frankish weapon. Isidore famously, and incorrectly, said that the Franks drew their name from the *francisca* (Isidore, *Etymologiae* 18.6.6) but the association between the Franks and this weapon actually goes back no further than the middle of the fifth century, and Sidonius Apollinaris. Accounts of the fourth-century Franks make no mention of the weapon and the archaeological record of *franciscas* equally does not go back earlier than the fifth century. Moreover

the *francisca* is found overwhelmingly in Gaul and very rarely in *barbaricum*. There is quite abundant evidence that the axe was in use within the Roman army (HALSALL 2010:134-5). Looked at in purely archaeological terms one would argue that the *francisca* was a weapon that appeared in Gaul in the fifth century and was occasionally, usually later, found beyond the Rhine. The archaeological record, when set alongside a more critical reading of the documentary and epigraphical sources, suggests that the *francisca* was a weapon used by the very late Roman armies in Gaul and that the Franks adopted it from their service in those armies. Whether or not one accepts that, the interpretation of the *francisca* as diagnostically Frankish could not emerge from the archaeological record on its own.

Indeed, in many cases the archaeological evidence is not being taken on its own terms at all. The example of Germanic barbarians in Late Roman northern Gaul, claimed to be an 'assured' case of archaeology showing the presence of an intrusive ethnic group in Gaul (KAZANSKI/PÉRIN 2008:201), is a good illustration. Almost none of the standard interpretation of so-called federate graves in northern Gaul in the fourth century would emerge from a purely archaeological reading (HALSALL 1992; 2000a; 2010:131-67). Were this evidence generated in a prehistoric context, as I wrote nearly twenty years ago (HALSALL 1992:201), no one would ever find in it any evidence of a migration. Almost all of the material culture found in the burials is of Roman Gallic origin; the rite itself is basically the standard rite of Roman Gaul, but with more grave-goods; the rite is actually quite different from that used in the barbarian territories; etc. The traditional argument finds its strongest support in a series of brooch types buried with some of the women in these graves but when one studies the distribution of these objects one finds that it is almost exactly the same as that for other items of metalwork such as belt buckles and other belt appliquéés, or of Roman pottery and metal vessels, which, as no one is in any doubt about, were produced in Gaul and exported beyond the Rhine to *Germania*. This alone begs the question of why one interpretation is followed for some

types of artefact and not others with the same distribution and, frequently, similar elements of decoration. Close inspection of the artefacts too suggests that they were being manufactured in Gaul, exported to *Germania*, and copied there, as had long been the case with Roman jewellery. It is clear to me that only the intrusion of a pre-determined historical narrative has led to this evidence being read as evidence of barbarian immigration into Gaul. There are many, many other illogical arguments and self-contradictions in the traditional argument which I have discussed at length elsewhere (HALSALL 2010:131-67).

Similar cases from other areas can be adduced. For example, recent work has suggested that some of the brooches used as evidence for the presence of eastern *Germani* in the west in the fifth century are not imports from the east at all (GAUSS 2009). When one looks at distribution maps one can indeed join the dots to produce a 'migration' from the Danube to Gaul or Spain (KAZANSKI/MASTIKOVA/PÉRIN 2008), but one need only do that if one has decided in advance (on the basis of non-archaeological sources) that that has to be the direction of movement. Why not *from* Spain to the Danube? Or, more plausibly and as has apparently been argued recently, from a Mediterranean production centre and then distributed in two directions, east and west, to Gaul or Spain and to the Danube and further east?

Sometimes a historical narrative is adduced, without worrying about the fact that actually it is not attested in any actual written sources! Like Widimer's army, such is the case, with the argument that certain brooch types found in northern Gaul, which have some general similarities with others found on the Danube, represent the presence of East Germanic soldiers in the region. These brooches are items of female apparel, so it is argued that these women are the wives of the (archaeologically invisible) soldiers. I have already drawn attention to the problem with assuming an east-west movement behind the distribution map. No matter that no written source mentions the presence of East Germanic soldiers (let alone their wives) in northern Gaul. A story is com-

posed on the basis of the political history of the period, which has East Germanic federates arriving in northern Gaul to fight in the armies of the Roman king Syagrius. Note too that the very nature of Syagrius – even the reality of his existence as a king of the Romans, in opposition to invading Franks – cannot be securely proven on the basis of the written evidence! Therefore, the written sources are not being subjected to close scrutiny, either. At every turn, whenever one looks into the details of the approach and the arguments deployed, we could not be further from a methodology which relied upon the strict, rigorous, 'pure' analysis of separate bodies of evidence on their own terms before the comparison of conclusions at a higher level. In actual fact, this is *Mischargumentation* at its *most mixed!* Indeed, *mixta confusaque*, to adapt a phrase of Gregory of Tours.

In fact, in an appendix to the 2008 article, Kazanski criticises R. Hachmann, one of the pioneers of Eggers' methodology, praised at the start (KAZANSKI/PÉRIN 2008:185-6), for not making Scandinavian archaeology fit the story provided by Jordanes' *Getica* (KAZANSKI/PÉRIN 2008:212-3). Never mind that just about every scholarly analysis of the *Getica* has rejected its account of the Goths' Scandinavian origins (HEATHER 1991; GOFFART 1988:20-111; 1995)... This does not seem to demonstrate a very deep commitment to the principles of 'regressive purification'!

Similar incoherence emerges when the argument's other premises are examined. Let us start with the idea of burial ritual as a marker of ethnic identity. Of course, in theory, the way one disposes of the dead, bound up as it is with ideas of cosmology and religion, might be expected to be an area where traditions were strictly guarded. It is therefore frequently said that burial is a very conservative element of social practice. And yet, in terms of its archaeological traces, it is anything but conservative. Changes in burial practice come thick and fast in antiquity. At least *ten* changes in methods of placing the dead took place in lowland Britain during the first seven and a half centuries of the Christian era (HALSALL 2000b:261). Between the time when unaccompanied inhumation, wrapped in a shroud or in simple costume, became

normal in Europe sometime in the latter half of the first millennium (ZADORA RIO 2003) and the revival of cremation in the early twentieth century, burial *does* look very conservative across most of Mediterranean and western Europe, in terms of its archaeological remains, but one need only consult other records, about mourning, funerals, commemoration or even the above-ground markers or gravestones to see that burial in fact continued to be a dynamic area of social expression.

The alleged evidence of 'Germanic' migration into northern Gaul in the fourth century again stands as a useful lesson. Here, the custom employed in these supposedly intrusive burials is completely different from that used in the alleged incomers' homeland. This is explained as evidence of acculturation but, for this to carry any weight, archaeologically, one would need to see communities cremating their dead without accompanying goods and then gradually adopting the host population's rites. Instead, were we to assume that these *are* the graves of incomers, something for which I see no good evidence, what we would have here would be communities abandoning their ancestral funerary customs (those supposedly closely guarded, conservative markers of ethnic identity) immediately that they were over the frontier. Another of Kazanski and Périn's arguments in favour of acculturation is that burials with belt buckles alone are 'Roman' whereas those with the belt buckles *and* more grave-goods (weapons for men and suites of jewellery for women) are those of the immigrants, because the latter have included more of their traditional customs (KAZANSKI/PÉRIN 2008:191). Yet actually *none* of this custom of burial with grave-goods is traditionally 'Germanic' at all. The barbarians between the Rhine and the Baltic or the North Sea Coast cremated their dead, sometimes including dress-adjuncts or other objects, often not, sometimes in a cremation urn, sometimes with no container at all – indeed often without container *or* goods, making them archaeologically all but invisible.

This brings us on to the key support for Kazanski and Périn's position, which is the alleged conservatism of female dress. It is often

argued, against the empirical evidence for the northern Gallic production of the material in these burials, that the way it is used reveals that these burials are of immigrant *Germani* (SCHMAUDER 2003: 279-80, n.31). Allegedly, the brooches are used in the traditional fashion of West Germanic *Tracht* or costume. Two brooches are used at the shoulders, either to fasten a Peplos dress, or probably more plausibly, to pin a shawl over a dress. The problems with this argument are many. Most importantly, as I have just mentioned, the burial record of the areas whence these alleged immigrants are supposed to have come is overwhelmingly formed by cremation (the Frankish homeland famously being more or less blank on distribution maps). This means that we have very little evidence about how brooches were worn by the women of the *Germani*. Indeed most of it is furnished by the burials under discussion (e.g. BÖHME 1974:161), making the argument more logically problematic! A second problem is that, for all the supposed immutability and conservatism of 'sacralised' female costume, the archaeological record reveals great variability in the numbers and positioning of brooches, the presence and absence of other artefacts, and so on. It is often forgotten that Roman women also wore jewellery. Although the brooch had dropped out of use, temporarily at least, by the middle of the fourth century, it had been common and indeed sometimes used in exactly the same ways, up until the third century (FEHR 2008:89-97). One must ask why fashion only explains the Romans' discarding of the brooch, but not their readoption of it; why immigration only explains the brooch's reappearance and not its disappearance; and above all, why Roman female costume, in being subject to fashion like this, was less sacralised and conservative than 'Germanic' women's dress. In fact, though, when looking at late Roman Gallic burials the implicit assumption is that Roman *Tracht* was *more* immutable than Germanic because the archaeologically-revealed diversity of female graves supposedly shows variability and acculturation by 'Germanic' women; these cannot be Roman women because (it is implied) Roman women were not allowed to adopt new items or otherwise change their dress!

The third problem for the Kazanski-Périn hypothesis concerns the very nature of this supposedly North-West 'Germanic' costume with its pairs of brooches at the shoulders. It is actually a pair of problems. The first is that it runs completely against the argument that wearing two brooches at the shoulders is the traditional *East*, not *West*, Germanic female costume, so that burials with brooches at the shoulders can be argued to be of fifth-century 'East Germanic' immigrants (KAZANSKI/PÉRIN 2008:201). What the slightly earlier burials from northern Gaul show is that such a mode of employing brooches was already known in that region. Indeed the other half of the problem is that a rigorous examination of the data suggests that not only was it *not* exclusive to eastern *Germani*, it might have been a late Roman provincial Gallic fashion.

This idea of fashion –only a description rather than an explanation, to be sure– nevertheless produces yet more reasons to question our authors' methodology. When one looks at the archaeological record one sees only huge variety, through time and place, such as does not accord with the assertion of deep conservatism in dress. Indeed the notion is more than slightly undermined by the invocation of acculturation. In Gaul the general lesson would indeed seem to be of the general acculturation of the Franks into the structures of fifth-century northern Gallic society at the same time as their political, ethnic identity began to be widely adopted. This nevertheless casts some doubt upon the degree to which female costume is either as conservative and regulated as is being proposed, or as directly, intimately linked to an ethnic identity.

When I have discussed these problems with Patrick Périn² he has explained that he sees the first generation of immigrants keeping closely to their traditional dress but subsequent generations adopting more and more items of the fashions of the host population. This is an interesting idea, but to explore it would require

close scrutiny of all aspects of the burial, not just the grave-goods and their date. It would require us to examine the deceased's *age*, for one would expect, were Périn's model correct, that old women would retain their traditional costume into the second generation of burials. One might expect children in the first generation to be buried in traditional dress by their parents, but what of adolescent and young adult women who died during the first generation, who would otherwise have become the more acculturated women of the second generation? How does this transition play out, and how is it reflected in the archaeological record? The hypothesis requires sophisticated thinking and modelling, not simply mapping onto different chronological phases. It should also be said that where the earliest intrusive graves on a site do not fit the traditional model, the argument is sometimes deployed that these women had partly acculturated before they arrived at the place where they died (KAZANSKI/PÉRIN 2008:198; 2009:157)! Of course, if one does not think that they are immigrants in the first place (I have already suggested that the empirical grounds for this assumption are weak) this is not a very convincing argument, even if it is convenient.

These points lead on to two further issues. One is that female costume, as revealed in the cemeteries, not just of Merovingian northern Gaul but in some parts of the Roman frontier provinces, in Anglo-Saxon England, southern Germany, and northern Italy, in fact varies significantly according to the age of the deceased (e.g. BARBIERA 2005; BRATHER 2004b; 2008; CLARK 2000). My study of the Frankish cemeteries of Lorraine reveals that children did not usually receive items related to gendered costume (HALSALL 1995:254; 1996). Most jewellery (the essential elements of 'traditional', 'ethnic' costume) is found with teenagers and young adults, and women older than their twenties are increasingly rarely interred with these artefacts. This alone must make a purely ethnic reading of the costume much too simplistic. It is not

2 I should underline that M. Périn has always been most friendly, supportive and willing to discuss these issues. I want to make it clear that, although we hold diametrically opposed interpretations, that opposition is founded in no personal animosity.

to deny that there might be ethnic significance in the nature of adolescent female costume. If, however, the nature of dress and its adornment changed through the female life-cycle, as I have just suggested, then this seriously questions the model of a progressive abandonment of traditional ethnic costume through time, generation by generation, according to idea of 'acculturation'.

This indeed raises one of the most serious problems of all with the traditional point of view: why the variability observable in the archaeological record need have anything at all to do with ethnicity. Kazanski and Périn acknowledge this point (KAZANSKI/PÉRIN 2008:191) but they do not allow it to obstruct their argument. All sorts of other dimensions of an individual's identity can come into play in the construction of the burial record. I have already mentioned, and discussed in detail elsewhere (HALSALL 2010:289-412), the role of gender and age. Kazanski and Périn mention religion and 'social factors' as being involved in the establishment of a burial rite and its archaeologically observable features (KAZANSKI/PÉRIN 2008:191) but they do not pause to consider how these different dimensions might work together as ultimately and primarily 'ethnic', rather than (as I would see it) cross-cutting each other and making the ethnic interpretation more incoherent.

This in turn leads me to the problem that underlies all such traditional 'ethnic' readings, and that is that they ignore the processes behind the creation of the archaeological record itself, seeing it simply (as mentioned) as a passive reflection of 'reality'. Whatever else one might say about British archaeological theory in its current state, in its post-processual phase in the 1980s it did bring to the foreground the idea that the formation of archaeological evidence is a deliberate and meaningful activity, founded upon active choices, designed to create information as well as conveying it to an audience. One must always, therefore, ask *why* people chose to bury their dead in this way. The problem with the ethnic reading is that for many of the ethnic groups known to us it is clearly the case that they did not generally bury their dead in the particular

style that archaeologists have pinned on them. There are no better examples than the Goths themselves. The followers of Theodoric, however minimalist a view one might want to take, must have numbered very many times more than the fifty or so archaeologically known 'Ostrogothic' graves in Italy and the Balkans (BIERBRAUER 1994). The Goths of Aquitaine and Spain – patently – did not bury all of their dead in a particular, Gothic style. Indeed they did not bury their dead in that way when they were in the Balkans, and even before 376, to judge from the Černjachov/Sintana-de-Mureš culture they did not have a single burial rite in any case, but a mix: cremations and inhumations of all sorts, found within the same cemeteries (for useful survey, see HEATHER/MATTHEWS 1991:59-69). So, even on the 'best-case scenario' (where one actually accepts the ethnic import of the rite), these 'ethnic groups' only buried *some* of their dead – a small minority – in a particular way. So one must ask, again, *why*? There must – clearly – have been some reason, other than simple ethnicity, that led some people to distinguish some of their dead from the great majority. In other words, even where ethnicity might be an acceptable *description* of the meaning of objects, it is rarely a satisfactory *explanation* for their deposition.

Here lies, in my view the solution to the problem –or *non*-problem, as I would prefer-of the archaeological invisibility of the Goths in Aquitaine. The explanation cannot simply lie in the Goths' lack of any Gothic metalwork. As the dominant force in the region surely they could simply have forced Roman craftsmen to make some. Or they could –as the Kazanski-Périn hypothesis suggests with regard to other situations- have worn local products in accordance with the Gothic *Tracht*. The simple absence of the right metalwork cannot explain the abandonment of a rite. At this point it must, however, be said that, as the 'Gothic' inhumation rite was actually only created in later generations (in Spain), Périn's theory about the Aquitanian Goths reverses time in arguing about the non-appearance of something that had in fact not yet been developed! For Kazanski and Périn a Goth is *always* a Goth and will (or should) *always* do what she or he

is attested as doing at some point in Gothic history (regardless of when or where). This is only one instance where, in spite of claiming to believe the opposite, they do in fact treat ethnic identities and cultures as unchanging and monolithic.

Where an ethnic or political identity is displayed in burial with grave-goods, the crucial thing is that it is displayed to an audience for a particular reason (HALSALL 2010:203-60). Therefore one must ask *why* the Goths would necessarily have buried their dead in a costume that proclaimed their Gothicism in Aquitaine in the fifth century. Migration is not something that automatically shows up in the excavated record; indeed it is very often archaeologically invisible. I have argued repeatedly that furnished inhumation (with grave-goods) is essentially a sign of social competition of some sort (HALSALL 2010:203-60). In earlier sixth-century northern Gaulish cemeteries, in a very fluid society with few or no rigid class distinctions and few means of securing local pre-eminence beyond royal service, whole communities seem to have participated in the competitive grave-goods ritual, as they did in lowland Britain (HALSALL 2010:278-84). In other areas, such as Ostrogothic Italy, lavishly furnished graves seem to be concentrated in the urban foci of the realm and may demonstrate a claimed Gothicism to an audience of other members of the aristocracy competing for royal favour (HALSALL 2007:336-8). There is not a blanket explanation for all burial rituals with grave-goods. One must look at what sorts of individual are being buried, in what numbers within what sorts of cemeteries and with what types and quantities of object (HALSALL 2008). But the display of grave-goods is transient by its very nature and therefore requires both the bringing together of an audience to see it and the existence of a symbolic language rendering the message intended by the ritually-deposited objects comprehensible to that audience. All this points, inexorably, towards political competition of some sort.

Indeed, the so-called Gothic cemeteries of Spain lie generally along the fringes of the kingdom: not just in the northern Meseta, as is well-known, but also in the south, around

the Byzantine enclaves, and on the Frankish border in Septimania. Furnished burials are also well attested on the Basque frontier where the presence of that political border must surely be part of the explanation. In other words, on the fringes of political authority, where claims to local power might be contested between individuals or groups asserting the backing of different political forces, and in situations like those of the sixth century, where political and military power were often based on ethnic ideas, we might expect the meaning of objects placed with the dead to have some ethnic import - but in a very different way from that envisaged in traditional readings.

This brings me to my concluding points. I have taken the Kazanski-Périn argument on its own internal merits, to show that the thesis is not very satisfactory even by its own lights, using the sorts of empirical archaeological methodologies that it claims to espouse. From there, my argument has led us, bit by bit, to overall theoretical problems, which cast serious doubt on the whole project.

The first is that the relationship between material culture and ethnic identity is very problematic. A classic anthropological study from many years ago showed that one could quite easily compile a list of features, of language, of dress or hairstyles, or other features that people said distinguished their group from others, or which distinguished other groups from theirs. And yet, in practice one found that these features were either rarely if ever observed in use or, where they were, one found that they did not distinguish one group from another (MOERMAN 1969; POHL 1998 makes similar points about the early Middle Ages). Another study, from East Africa, showed that age-grades within one particular society adopted material culture associated with a neighbouring group to distinguish themselves from the age groups below and above them (LARRICK 1986). It is difficult to begin to imagine the havoc that this would play with any attempt to read ethnic identity from the distribution map of artefacts! Yet we can see similar things within our late antique evidence, where Roman soldiers and aristocrats adopted items of costume which

were held to be barbarian –what I have termed ‘barbarian chic’ (HALSALL 2007:110)– or where people within *barbaricum*, women as well as men, used imported Roman material culture to show their high status (HALSALL 2007:57-58). Indeed, around 400 some people in the north of *Germania* adopted the Roman inhumation rite in order to distinguish themselves from their fellows (BEMMANN 1999; KLEEMANN 1999). This did not make them Romans by birth, although for all we know some of these individuals, if they had served the Roman Empire, might well have styled themselves *Romani*. In Gothic Italy or on the margins of the Gothic kingdom of Spain, individuals might very well have styled themselves *Gothi* without being descended from people who had crossed the Danube in the 370s or 380s. It was a claim to power and status.

That, ultimately, is the point. Ethnicity is a state of mind, with no necessary correlation to things which are objectively measurable, whether material, biological or genetic. This will always make attempts to read off monolithic ethnic identities, or even the interplay between monolithic ethnic identities (which is what is at stake in ‘acculturation’ arguments), highly dubious. More pertinently, perhaps, ethnicity is itself a complex dimension of an individual’s identity, existing in several layers which can be adopted or highlighted, abandoned, played down or concealed. Early medieval people did not have to see themselves as *either* Romans or Franks, as *either* Goths or Sueves. An inhabitant of sixth-century Spain, who took up arms and attended the army using an assertion of Gothic identity as a means to acceptance within this military-political group, was not thereby precluded from having Roman, provincial or *civitas* identities *as well*, which he might have used at other times in other circumstances. None of these groups was monolithic in itself: Romans self-identified by their *civitas*, a very important and much-neglected level of post-imperial identity: there were different groups within the Franks, there were political regional groupings, by kingdom or by Roman province, which have most of the features of ethnicity (Neustrian, Austrasian, Aquitanian or Provençal).

In a sense we have come full circle, because it may be that, as with the military associations of barbarian ethnic identities, we can propose that some objects in graves – weapons – might have conveyed that identity to an audience. This archaeological reading would fall foul of most of the strictures set out at the beginning of this paper, being a reading of material culture entirely in the light of documentary sources. However, this reading of the documentary sources and its application to material culture is somewhat more subtle (and indeed more grounded in the written data). It might be the case that certain types of brooch, used in particular ways with particular types of people, in particular contexts, *did* have an ethnic connotation, so that a Jutish brooch in England might have implied that the wearer claimed a Jutish identity.

An important caveat for this point, though, is that it only remains a suggestion, which can only be made in a particular context. It cannot be taken as a general rule, such as that people with weapons are always Franks, wherever they are found and in whatever context: that is plainly untrue. It also implies nothing biological, genetic or exclusive about the claim being made. Indeed this suggestion has the fluidity of our modern understandings of ethnicity. Thus, although having the appearance of having come round in a circle, we end with a very different understanding of the relationship between material culture, and ethnicity from that with which we started. The argument moves forward, as in a spiral and in so doing I think that it opens up our cemetery evidence to much more interesting and less constricting readings.

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