Migration

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No serious scholar believes that migration of various kinds did not play a significant role in events of the first millennium AD, but the extent and importance of any large-group migration is particularly controversial. This paper seeks to think again about this highly controversial dimension of the subject area. Given that neither revisionist accounts of the operation of group identities, nor archaeological materials offer any sure guidance on the matter, it suggests that some the available historical materials are worth taking more seriously, and explores the kind of picture that emerges from them.[1]

STATE OF THE QUESTION

In the standard views of European history in the first millennium AD, which prevailed up to about 1960, migration played a critical role in the unfolding meta-narrative of socio-political development. Different versions of the same type of story were told in various academies, but they all basically envisaged their own particular group of migrant ancestors arriving in their own corner of Europe to kickstart a process of continuous political development which led directly to many of the modern polities of nineteenth- and twentieth-century Europe. In Britain, the Anglo-Saxons arrived in the fifth century to sweep surviving Celts westwards into Wales, Cornwall, and Brittany and establish the foundations of an English polity which then developed slowly but surely into the triumphant

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[1] NB, this is meant to be more a piece of thinking-out-loud than a fully worked-up academic treatment, so the footnotes will only cover the bare essentials rather than providing anything like a complete coverage of the available literature.

England of the eighteenth century and beyond. But similar visions of different first-millennium founding fathers prevailed across very large parts of Europe, whether we are talking about Croats, Bulgars, Magyars, or Poles.[2]

These narratives rested on two key intellectual foundations. First, they envisaged each set of founding fathers as a 'people'. By this, they meant a closed, endogamous population-group, mixed in age and gender, which had its own entirely distinct non-material (particularly linguistic) and material cultures. Such a picture of entirely distinct population groups roving through Europe, unsullied by outside contact, had its origins in the developing field of linguistics from the later eighteenth century, but it was quickly reinforced by the emergence of scientific archaeology. As data sets built up over the next hundred years or so, it became clear that similar finds tended to cluster in particular times and places. These clusters were quickly interpreted as the physical remains of particular peoples ('culture history' as it has come to be labelled), adding an intense physical reality to the groups first imagined through linguistic analysis.

Second, the narratives took a very particular view of the migration process such 'peoples' customarily engaged in. Now sometimes labelled the 'invasion hypothesis', it was generally imagined that when such a population group moved into a new territory, it drove out the existing population to take complete control of the chosen destination. Early European history – up to, but not much beyond, the year 1000 – was envisaged as a series of periodically complete population replacements. Intellectual justification for this view was found in a handful of narratives in historical sources (such as Jordanes and Paul the Deacon), and the first attempts to theorise what it meant when, in any particular locale, one set of archaeological remains – a 'culture' - was replaced by another. If 'cultures' were the remains of 'peoples', the appearance of a new culture could surely only mean that a new people had replaced the sitting tenants.

Since the 1960s, this meta-narrative has been effectively undermined (even if elements of it retain a hold, particularly in totalitarian contexts where regimes like to bolster national solidarity by rooting it in the depths of time), dismantled by devastating assaults from multiple, overlapping directions:

a)   A new social scientific – largely sociological and anthropological – literature on identity has emphasised that an individual's group affiliations can be multiple and indeed change over time. Individuals can belong to several groups in the course of their lifetimes, swapping between them according to convenience and maximum utility. It is now impossible to believe in a European past littered with closed 'peoples' utterly distinct from one another.

b)   Equally important, successive generations of archaeologists have revolutionised the range of models available for explaining material cultural change. Areas of distinct similarity sometimes exist,

[2] There were some parts of Europe where it was difficult or impossible to tell such stories – French nationalism was marked, for instance, by a tension between its supposed Celtic/Gallic and Frankish heritages – but these were the exceptions.

but they are not the unique material cultural footprints of closed peoples. Material cultural patterns can and do change for a host of reasons – whether technological, ideological, or other – and very often do so while the same population group continues to occupy the same geographical space. It has also been – very fairly – pointed out that the old invasion hypothesis didn't really explain anything anyway. Why should these supposedly closed population groups have periodically relocated themselves in the way that the old narrative envisaged?

c)   These doubts have been reinforced by some engagement with the developing discipline of comparative migration studies, from which it quickly became apparent that complete population replacements are rare to non-existent in better-documented eras of human history.

As a result of these solid intellectual advances, the old orthodoxy has been swept away, but no new consensus has yet emerged.[3] No one believes any more in 'peoples' and complete population replacements and almost nobody thinks there was NO migration in the first millennium. That much agreement establishes only the widest of parameters, however, within which huge differences of opinion are visible in recent secondary literature. At one end of the spectrum, whatever the ancient source material apparently implies, some scholars don't believe that primary migratory activity ever involved larger groups of humanity than what we might term 'warbands': all- or primarily-male units numbering usually hundreds – not thousands (and certainly not several thousand) – of individuals. This view seems to be based on the conviction (sometimes the argument is not fully articulated) that the new understandings of group identity make it inconceivable that much larger, fully-mixed population groups could ever have managed to keep themselves together on the road. And in practice, as this suggests, migration and identity have in effect become two sides of the same intellectual coin in first-millennium studies. The weaker your view of the potential of group identities to combine individuals into a larger unit, the more you will minimise migration and produce alternative narratives, where relatively few people move, around whom then gather large numbers of new, indigenous recruits at the point of destination. In this view, neither the group identities of the migrants, nor those of the indigenous population pose any substantial barrier to the formation of a new group.[4]

I would myself argue, however, that taking such a minimalist position *a priori* on the possibilities of first-millennium group identities actually represents only a partial reading of the total social scientific literature on group identity. It certainly reflects what was immediately new and exciting about that literature in the 1960s, when it was realised for the first time that that nationalist-era conceptions of human identity – that it was 'normally' single and unchanging – were often mistaken. But if you read widely in the full body of post-1960s literature, the overall picture which emerges is that

[3] P. J. Heather, *Empires and Barbarians: Migration, Development and the Birth of Europe* (London, 2009), c. 1, or G. Halsall, *Barbarian Migrations and the Roman West 376-568* (Cambridge, 2007), c. 14 (both with full refs.) give remarkably similar accounts of the intellectual evolution of identity debate given the extent of their disagreement elsewhere. For an overview of developing archaeological theory, see, e.g., P. Bahn, *The History of Archaeology: An Introduction* (London, 2014).
[4] Warbands: e.g. J. Drinkwater, *The Alamanni and Rome 213-496* (Oxford, 2007), where the case is explicitly argued, or R. Miles and A. Merrills, *The Vandals* (Oxford, 2010), esp. cc. 1-2, where it is just assumed.

different group identities exercise weaker and stronger holds on their memberships, that different members of the same group will often feel different strengths of affiliation, and, again, that the hold on individual allegiance exercised by notionally the same group can vary over time. Sometimes, groups whose members enjoy considerable material and other advantages in a mixed context will not allow just anyone to join the group, and sometimes individuals feel sufficiently strong emotional attachments to their original group identity that they have no desire to change. Some of the more recent synthetic theoretical contributions to the debate have also pointed out that there was a strong political agenda in newly-emergent nation states in Asia and above all Africa, in which much of the original research was done in the 1950s and 1960s, to dismiss any older inherited identities as recent colonial inventions, so as to deny the legitimacy of any claims based upon them, an agenda that was beautifully in tune with the prevailing Marxist interpretations that anything other than class had to be 'false consciousness', in the sense of being artificially created to confuse true identities. In my view, the body of theory, as a whole, is now more accepting of the view that group identities – while certainly all created and artificial in the sense that they are not primordial – don't all have to be viewed as utterly insignificant. Read carefully, this was already there in the inherent logic of Barth's famous statement that group identity is a 'situational construct'. This has often been adopted as the slogan of the revisionist 1960s position that all group identities are multiple and fluid, exerting essentially no constraining force on individual human behaviour. But since not all historical situations are the same, the statement in fact implies that the likely strength and importance of group identities will vary according to different situations: sometimes stronger, sometimes weaker, according to the details of the precise context.[5]

The overall effect of the developing literature on identity, therefore, is to downplay the amount of traction that it provides for understandings of first-millennium migration. If it were the case that group identities are always weak and highly fluid, then the kind of sustained, large-group migratory activity seemingly described in some of our sources could be ruled out as impossible *a priori*. But this is not the case. Group identities do sometimes effectively constrain the behaviour and choices of their members. As a result, the post 1960s identity literature does not, and cannot dictate one monolithic vision of first-millennium migration. It does not justify, in other words, replacing one massive over-simplification – 'peoples' and the 'invasion hypothesis' – with another: that only warbands ever moved, and any larger groups were always assembled by large-scale recruitment at the point of destination. And as the lines of thought generated by the new thinking on identity run gently out of steam, there is a case to be made that it is time to invert the process. Instead of using identity to dictate a view of likely first-millennium migration, perhaps further serious thought about the evidence

[5] F. Barth, *Ethnic Groups and Boundaries: The Social Organisation of Ethnic Difference* (Boston, 1969), quotation at p. 9. My own views are laid out more fully in P.J. Heather, 'Ethnicity, Group Identity, and Social Status in the Migration Period', in I.H. Garipzanov, P.J. Geary and P. Urbańczyk (eds.), *Franks, Northmen, and Slavs: Identities and State Formation in Early Medieval Europe* (Turnhout, 2008), pp. 17-50. Since then L. Vail (ed.), *The Creation of Tribalism in Southern Africa* (Berkeley, 1989) introduction, and T. Spear, 'Neo-Traditionalism and the Limits of Invention in British Colonial Africa', *Journal of African History* 44 (2003): 3-27 have been drawn to my intention, illustrating how the social scientific discussion has moved on in important ways from Barth and his immediate supporters.

for migration can help us develop prevailing understandings of the nature of first-millennium group identities?

SOURCES OF DISAGREEMENT

Any attempt to understand the nature of first-millennium migratory phenomena using historical sources will be hampered by the same basic limitations that affect our ability to use them to understand first-millennium group identities. Looked at in the round, the written source material either tends to be written by contemporaries who were complete outsiders - Roman commentators overwhelmingly in the first half of the millennium (or Carolingian/Ottonian and Anglo-Saxon counterparts later on) – or by non-contemporary semi-outsiders. Jordanes and Paul the Deacon have some kind of Gothic and Lombard credentials (if not very direct ones), but were describing migratory events which often happened centuries before their own time. Neither remotely matches the profile of an ideal witness.

This much is well-understood, but perhaps less-securely grasped (although it is hardly a secret) is the body of archaeological evidence currently available to us is actually no more authoritative: whether you're looking to prove or disprove that large-scale migration was a significant phenomenon. The old certainties of culture history and the invasion hypothesis – thankfully – are gone forever. Population groups do not have material cultural profiles, defined once and for all time, and material culture can change for many reasons other than migration. But this observation has a straightforward, logical extension which seems to be rather less well-understood. Once you've decided that groups do not have particular material cultures and that an individual's identity cannot be measured by material cultural attributes, then what you've actually done is admit that archaeological investigation can only ever produce *ambiguous* evidence either for identity or migration. A set of dress items and funerary practices, say, *might* shift from point A to B because the indigenous population at point B changed their habits; that much is clear. But it is equally possible, *a priori*, that the change was caused by actual migration (and throughout human history, people have indeed moved on a regular basis). Adoption/adaptation is no more than one possible explanation of an observed material cultural transformation, and needs to be kept in play alongside other possible explanations, such as migration, which are no more but certainly no less likely.

I stress this point because such has been the ferocity of the response among particularly processually-influenced archaeologists to the past overuse of very simple migration models, that some participants in the debates clearly have the attitude that if you can think of an alternative explanation for some observable phenomenon that does not involve migration, this makes it necessarily superior to one that does.[6] *A priori*, this is incorrect. The fact that you can think of another possible explanation

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[6] See e.g. G. Halsall, *Early Medieval Cemeteries: An Introduction to Burial Archaeology in the Post-Roman West* (Skelmorlie, 1995), pp. 57, 61 or J. Hines. (ed.), *The Anglo-Saxons from the Migration Period to the Eighth Century:*

does not – *ever* – mean that migration has been disproved; you have just added another reasonable possibility to the list. Ambiguity is precisely that; it does not amount to proof that migration had no important role to play in any observable series of changes. The overall effect of realising that identity is located first and foremost in the head, and not in material cultural artefacts, is thus substantially to downplay the potential contribution of archaeology to many migration and identity debates, since it effectively makes archaeological material no kind of guide to either. Just to give one example: it is reasonable to think that shifting material cultural profiles in south-eastern Britain and north-eastern Gaul in the fifth century AD had something to do both with the arrival of immigrants and with indigenous populations adopting new habits. But the balance between these two very different types of historical phenomena - intrusion on the one hand, imitation on the other – is absolutely impossible to tell on the basis of surviving material evidence. Both migration and adaptation are equally possible explanations, and, with existing methods, it is absolutely impossible to tell one from the other, and hence which type of behaviour may have been the predominant cause of change. It is time to be methodologically more resolute on this point and not allow the determination of some archaeologists not to be tarred with the brush of the old invasion hypothesis era to generate a series of false negative conclusions. The fundamental ambiguity of the available archaeological material must be recognised for what it is. It should never be taken to mean that the importance of migration has been disproved.

It is certainly possible that innovative methods may provide entirely new archaeological data-sets in the future to form the backbone to entirely new understandings of first-millennium migration, but we are a long way from that yet. Solid-state isotope analysis of teeth samples, for instance, provides brilliant insights into the histories of particular individuals who moved in adulthood from one geological region to another. But some of these regions cover very broad areas of Europe, which means that you might well miss what was still a substantial act of migration that did not cross a geological boundary. There is also the more fundamental problem that isotope analysis allows you to identify only first-generation immigrants. In the Anglo-Saxon case, for instance, the children of two echt Jutlanders born after a move would have impeccably East Anglian teeth, but that does not mean they did not belong to an immigrant community. Of much broader potential are the new DNA sequencing techniques, which offer for the first time an opportunity to generate useful data from ancient remains (previously analysing modern DNA patterns was a more useful if still limited approach). The problem here, however, is that it will take several decades of extremely expensive research to build up a sufficient background picture of European DNA patterns for any really indicative information about first-millennium migrants to emerge from all the background interference.

For the foreseeable future, therefore, there is not going to be anything more available on the archaeological front than the more traditional stock of possible indicators (the transfer of items and habits from one area to another) for thinking more generally about migration (as opposed to identifying the odd individual). And since this type of data is inherently ambiguous, it is worth looking ra-

An Ethnographic Perspective (Woodbridge, 1997), p. 179.

ther more closely, I would argue, at the body of available historical evidence than some scholars currently appear willing to do. In particular, I would make two preliminary observations. First, not quite all of the historical evidence is either outside or late. Court-oriented sources from Ostrogothic Italy in the time of Theoderic, reflecting back on the migratory phenomena at the heart of the kingdom's creation, were written within a political generation of the move, for an audience that included some of the actual movers. It is hard to think that these reflections could have got away with fundamentally misrepresenting such a recent and crucial experience to those who had actually participated in it. Some of the 'outside' Roman material, likewise, is absolutely contemporary with the migratory events it purports to describe, and – occasionally – highly detailed. This remains of course far from perfect as a portfolio of available materials, but we are dealing with the mid-first millennium, and, as a body of material, it would on the face of it pass muster in general terms as worthwhile, serious evidence according to the limited kinds of rules that are normally available to the first-millennium historian. Rather than (conveniently) dismissing all the available historical evidence *a priori*, the ambiguity of the archaeological record and the fact that neither the identity debate nor DNA sequencing is currently offering us a silver bullet, it is worth taking the historical materials more seriously, at least as a thought experiment, to see where argument might lead. I propose to do this via two case studies, where, in my view, the case can be made that more historical evidence, and of a better quality, is available than is now generally recognised.

VISIGOTHS: FROM THE BLACK SEA TO AQUITAINE

The various outsiders who eventually coalesced into the Visigoths settled in the Garonne valley by agreement with the Roman state in 416/18 have seen much recent interest. If you are thinking about the reality (or not) of large-group migration, and its possible nature, the key sub-issues raised presented by their case history are:

a)   Size, nature, and coherence of the groups who crossed the Danube in 376.
b)   Their motivations.
c)   What was agreed between the Goths and the Emperor Theodosius I in 382?
d)   The nature of any continuity between Balkan Gothic settlers of 382 and the personnel behind Alaric's revolt in 395.
e)   The size & nature of the force led to Gaul by Alaric's brother-in-law Athaulf, and eventually settled in Aquitaine.

In each instance, the available historical source material is certainly less than ideal, but, considered without preconceptions, it does nonetheless offer some reasonably convincing outline answers.
a)   Ammianus and others don't give exact figures, but they do offer pretty clear guidance that it was a very large body of humanity which came to the Danube in 376. The point is confirmed by the ability of the united Tervingi and Greuthungi – with additional Hun and Alan allies – to win the battle of Hadrianople two years later. This was clearly some kind of fluke (all other set-piece Goth-

Roman conflicts in the following years ended in some kind of draw), but Valens and two-thirds of his field army cannot have been wiped out by less than a proper army. Ammianus' comments on why Valens didn't wait for Gratian might well suggest that just the Tervingi of Fritigern alone fielded around 10,000 men.[7]

On the coherence front, both of the main Gothic groups who turned up on the Danube to negotiate had just lost their existing political leaderships (Ermenaric etc/Greuthungi; Athanaric/Tervingi), but were able to undertake coherent and chronologically extensive negotiations with the Roman state, during which Gothic ambassadors went all the way from the Danube to Antioch, and maintain their identities, too, after crossing the Danube. The sources also make it plain that warriors were accompanied by families. Thus, for all the lack of exact figures, I don't think there is anyone who thinks less than several tens of thousands of individuals in total – men, women, and children – were on the move in 376, and substantial cores of the two largest groups at least, were able to maintain a political/military cohesion – despite some evidence of fringe fragmentation – throughout the move towards and then over the Roman frontier, and on up to the battle of Hadrianople.[8]

b)   The unanimous (and in Ammianus' case, again, more circumstantially-detailed) view of a bundle of contemporary Roman commentators is that the Goths of 376 were on the move because they had been displaced by the rise of Hunnic power. One recent strand of scholarly opinion has attempted to undercut this, arguing, instead, that it was Valens' victory over Athanaric in 369/72 which undermined the Goths' position and drew the Huns in to take advantage of the situation. This would put the onus on the emperor in explaining what was going on, and has the distinct advantage for those influenced by processual archaeology of making 376 look much less like the dreaded invasion hypothesis in action. The (sometimes not properly acknowledged) problem with this line of argument is that it necessitates 'correcting' the account of Ammianus – by far and away the best informed of our contemporary sources – on the basis of a demonstrably partly garbled passage in a stray chapter of the Church historian Socrates, written over 50 years later.[9]

It is not an absolutely impossible argument, but nineteen times out of twenty (or more) you would not follow this methodological procedure, which inverts all the normal rules of good practice. I also think that if you take away an initial presumption that a quasi-invasion hypothesis scenario is *a priori* impossible, you would not do it here. In other words, it is overwhelming likely that the Goths were on the move because Hunnic power was on the rise, and this had little or nothing to do with Valens'

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[7] Ammianus, *Res Gestae*, 31.12, 3 reports that Valens advanced from Hadrianople because he thought that only about 10,000 Goths were nearby, and the presence of the Greuthungi at the battle seems to have been the surprise factor. My own is that Valens brought around 15,000 men to the battle of whom two-thirds died, but others place the numbers much higher. For further discussion and full refs. see Heather, *Empires and Barbarians*, c. 4.

[8] Ammianus 31. 3-4, with commentary in P.J. Heather, *Goths and Romans 332-489* (Oxford, 1991), c. 4.

[9] N. Lenski, 'The Gothic Civil War and the Date of the Gothic Conversion', *Greek Roman and Byzantine Studies*, 36 (1995), 51-87 attempted to restore the credibility of Socrates'; he has been followed e.g. by Halsall, *Barbarian Migrations*, 170 ff.

earlier war (which had anyway ended – from the emperor's point of view – in an unsatisfactory draw).[10]

I would also add one further point to the mix. Ammianus' account also indicates that, having originally decided to move because of Hunnic pressure, different Gothic groups went in different directions in 375/6, according to their various different judgments of the best likely outcome available to them. There was, in other words, a distinctly predatory element to the decision of large groups of Tervingi and Greuthungi to move in a Roman direction, with the wealth and relative safety of Roman territories figuring in their decision-making processes. The sources are not suggesting some kind of simple domino effect here, therefore, but a more complex process involving the assessment of various options.[11]

c)   Misguided recent attempts have also tried to cast doubt on what has been traditionally understood about the agreements of 382 which eventually brought the Gothic war to a close, four years after Hadrianople. It is entirely reasonable to argue, I hasten to add, that much of the detail can only be guessed at. We do not know where exactly the Goths were settled, nor the precise terms of the services they agreed to provide to the Roman state, nor what had happened to all the known Gothic leaders of 376-8 (Fritigern, Alatheus, Saphrax, and Vithimer: all of whom just 'disappear'). But it is impossible to deny, as some have recently tried to do, that there even was a treaty. The *Consularia Constantinopolitana* gives us an exact date for it, and Themistius, just four months later, spent many words attempting to justify its terms to the Senate of Constantinople. Even more important, whatever its details, contemporary commentators - both those sympathetic to the emperor (Themistius and Pacatus) and hostile (Synesius) - are in complete and utter agreement that its basic effect was to grant the Goths and others settled under its terms an unprecedented degree of political and cultural autonomy for such a large migrant group entering Roman soil. Issues of detail must not be allowed to obscure this much more fundamental point.[12]

d)   Two contemporary commentators (Claudian and Synesius: one eastern, one western) report that in 395 Alaric was essentially leading the Goths of 382 in revolt against the terms of their existing treaty. Again, a body of recent opinion has argued that this was not the case, basing itself on a passage in Zosimus which reports that Alaric originally revolted because he was denied a Roman command, taken to suggest that he only slowly evolved into a Gothic leader. This is deeply uncon-

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[10] As Themistius *Orations*, 8 & 10 and Ammianus 27.5 make clear. Valens ended it via a compromise peace because the much more important Persian front had fallen into crisis: see further P.J. Heather and J.F. Matthews, *The Goths in the Fourth Century*, Translated Texts for Historians (Liverpool, 1991), c. 2.
[11] Ammianus 31. 3. 8.
[12] *Consularia Constantinopolitana* s.a. 383 (October 3rd). Our main information on the peace deal is provided by Themistius *Orr.* 16 & 34; Pacatus *Panegyrici Latini.* 12 & Synesius *De Regno.* The attempt to overturn the collective overview that emerges from both sympathetic and hostile sources of M. Kulikowski, 'Nation versus army: a necessary contrast?', in A. Gillet (ed.), *On Barbarian Identity: Critical Approaches to Ethnicity in the Early Middle Ages* (Turnhout, 2002), pp. 69-84 – followed e.g. by Halsall, *Barbarian Migrations* (2007), p. 180 ff. – is completely unconvincing.

vincing, and mustn't be allowed to stand just because it sounds interestingly 'complex' on the identity front. First, Zosimus was writing over a century later, and, precisely in the same passage, can be shown to garble the join between his two main sources for the events of c.400: Eunapius and Olympiodorus. This has the direct effect of turning his account of Alaric into nonsense. Rather like the Ammianus vs Socrates case in b) above, you would never prefer post-dated confusion to contemporary precision unless you have other axes to grind. It is also worth pointing out that, in better-documented negotiations, Alaric often requested a Roman generalship at points when he was undoubtedly leading a large group of Goths, since it granted him both political recognition and justified a large flow of funds to his followers. The passage from Zosimus thus provides only the weakest of reasons for not following Claudian and Synesius. They were also writing for different audiences, and no one has yet offered a good reason why they would both have simultaneously come up with the same fabrication. The only reasonable conclusion is that Alaric was leading a large chunk of the Goths of 382 in revolt.[13]

Obfuscatory recent accounts of the period between 382 and 395 notwithstanding, this conclusion also makes perfectly good sense in the light of two further points. First, as already noted, contemporary sources hostile and favourable to Theodosius' treaty with the Goths *both* report that it granted the Goths an unprecedented degree of autonomy, making their capacity to mount a revolt only just over decade after the treaty utterly unproblematic (despite our ignorance of exact details). Second, every time opportunity offered, there had been substantial Gothic revolts in the intervening years: Theodosius recruited Gothic contingents for both his wars against western usurpers (Maximus and Eugenius), and both recruiting drives prompted revolts. This both underlines the fact that Gothic autonomy was a reality after 382, and also helps explain Alaric's revolt, since it came in the aftermath of heavy Gothic losses suffered at the Frigidus on the Eugenius campaign, which could only have reinforced the already established tendency to rebellion. All this has to be pieced together from fragmentary accounts (Ammianus having ceased in the aftermath of Hadrianople), but a) it is really not that difficult to do, and b) it makes perfectly good historical sense. The 382 treaty allowed unprecedented Gothic autonomy which intervening issues, particularly the problem of military service for the Roman state, both kept alive over the next thirteen years and provided good reason to want to revise its terms when opportunity offered on the death of Theodosius I.[14]

e)   To an initial body of supporters composed of many of the settlers of 382 in revolt, Alaric added two further major bodies of recruits in 408/10. First, the fall of Stilicho led large numbers of non-Roman soldiers in the army of Italy to join Alaric, when their families, quartered in various Italian cities, were massacred in pogrom. Second, large numbers of slaves joined Alaric while he was en-

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[13] The argument was started by J.H.W.G. Liebeschuetz, 'Alaric's Goths: Nation or Army?', in J. Drinkwater and H. Elton (eds.), *Fifth-Century Gaul: A Crisis of Identity?* (Cambridge, 1992), pp. 75-83 on the basis of Zosimus 5. 5. 4, on whose confusions see Heather, *Goths and Romans*, 193 ff. The much more contemporary sources are Claudian *De Bell. Get.* 166 ff., 610 ff. (402) and Synesius *De Regno* 19-21 (399).

[14] For fuller discussion of the issue, see Heather, *Empires and Barbarians*, 191 ff. contra Kulikowski, 'Nation versus Army'.

camped outside Rome in 409/10. There is a good probability that many of the first set of recruits derived from the elite followers of Radagaisus whom Stilicho recruited into the army of Italy as recently as 406, and I suspect – but certainly couldn't prove – that many of the second derived from the less fortunate of Radagaisus' followers, so many of whom were sold into slavery in the aftermath of his defeat that the bottom fell out of the Italian slave market.[15] But that is a side issue. While Alaric's following, particularly when it faced harder times in Gaul and northern Spain under his brother-in-law Athaulf after August 410, surely will have suffered both some military losses and political desertions too, no major diminution of numbers is reported before the agreements his successors eventually made with Fl. Constantius in 416/18. These agreements curbed excessive Gothic ambition (Athaulf had married the – childless – emperor's sister and called their son Theodosius), and required substantial military service of the Goths in Spain, but it also involved further major concessions. The Goths not only received substantial, fertile territories in the Garonne valley, but this was the moment when the Roman state definitively recognised (and negotiated with) a Gothic king. These concessions are best explained by, and make perfect sense in the light of, the extra recruiting of 408/10. In those years, Alaric added many extra followers to the now unified Tervingi, Greuthungi, and others of 376/8, to create the new Visigothic alliance which was large enough to extract definitive concessions from the imperial state.

The available historical sources do not provide definitive answers to every question you want to ask of the forty years or so from 376 to 418. Employing all the normal rules for mid-first millennium historical evidence, however, I would argue that the material does establish a good probability that large-scale group migration (involving 10,000 warriors plus dependents) was a crucial feature of the Visigoth story, and that a central core group was able to maintain enough political coherence over the period to generate a pattern of staged, repeat migration.

OSTROGOTHS: FROM PANNONIA TO ITALY

In this instance, the issues are essentially two-fold. First, what was the scale and nature of the group led by Theoderic the Amal and his father Thiudimer from Pannonia into the Roman Balkans in 472/3, towards its initial settlement site near Thessalonica? The same question then also needs to be asked of the Amal's second great documented move from the Balkans into Italy in 488/9. In between, this force went on an extended Balkan tour which took it first east towards Marcianople, then back west again to Epidamnus, and engaged in various political manoeuvres involving not just Constantinople but a second group of Goths established in Thrace (led initially – for added clarity of exposition - by a second Theoderic). The complex political narrative which ensued thus raises a second major issue. How much continuity was there between the initial Amal force of 472/3 and the group that went to Italy in 488/9. Is this another case of staged, repeat migration?

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[15] Zosimus 5. 35. 5-6 (foreign auxiliaries from the Roman army of Italy; Zosimus 5. 45. 3 (slaves); cf. Orosius 7. 37. 13 ff. on the collapse of the slave market following Radagaisus' defeat.

a)   We have good evidence – in terms of being contemporary and circumstantially detailed - for the size of the armed forces that Amal father and son led into the Roman Balkans. In 479, Malchus of Philadelphia reports, Theoderic could contemplate offering Constantinople the use of a picked force of 6000 men while leaving enough to keep the group's non-combatants safe at Epidamnus, which required a garrison of 2000 men. This followed the drift of some manpower from the Amals to the Thracian Goths, but clearly puts the total of Theoderic's soldiery in the region of 10,000 men. Malchus also tells us that, as part of an agreement in 478, the Emperor Zeno had to provide the other Theoderic, leader of the Thracian Goths, with pay and supplies for 13,000 men, putting the size of the military component of the Thracian Goths in the same broad ballpark. This picture is broadly confirmed by the complex twists and turns of the narrative, which strongly imply that the two separate Gothic forces were so similar in size that one couldn't easily swallow up the other. Malchus (and other East Roman sources, particularly John of Antioch) report that the two Gothic groups were not just armies, all-male or overwhelming male-dominated, but came with very large numbers of dependents attached. Both groups therefore apparently mustered 10,000+ warriors, and total populations, with dependents, of several tens of thousands of individuals.[16]

b)   The key to understanding the force which eventually left the Balkans for Italy in 488/9 is pro-vided by the political narrative which unfolded in between. Some population exchanges initially went from Theoderic the Amal to his Thracian namesake in 477/8, but we have good – if certainly implicit rather than explicit – evidence that the bulk of the Thracian Goths then attached themselves to Amal leadership in the early 480s. At this point, the Thracian Theoderic had died, and Theoderic the Amal organised the assassination of his son and successor Recitach. The reason for thinking that the bulk of the Thracian Goths attached themselves to Amal leadership at this point is that the Thracian Goths disappear as a group from East Roman narrative accounts of the Balkans the post-480s. These are very detailed at points (particularly during the revolt of Vitalianus in the 510s) but the Thracian Goths are nowhere to be seen, and what indications we have from Ostrogothic Italy imply that Theoderic then had a substantially larger force at his disposal than the c.10,000 warriors he had led out of Pannonia in 472/3.[17] Group numbers were also inflated for the move to Italy by recruiting a substantial force of refugee Rugi, who joined Theoderic on the march after Odovacar had destroyed their hold on the middle Danube. Confirmation that Theoderic had led to Italy not only a very large military force, but one which was accompanied by many women and children besides, as well as of the participation of the Rugi, again with many non-military dependents, is provided by authoritative court-linked sources from Ostrogothic Italy, which, given their audience, cannot have substantially misrepresented the phenomena they describe.[18]

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[16] Warrior numbers: Amal-led Goths: Malchus fr. 20; Thracian Goths: Malchus fr. 18. 4. Fuller discussion of the evidence for non-combatants as integral to the group: Heather, *Goths and Romans*, c. 7.

[17] Fuller discussion: Heather, *Goths and Romans*, c. 9.

[18] Procopius *Wars* 5. 1. 6 ff. but also Ennodius *Panegryic on Theoderic* 26-7 (produced for Theoderic's court) & *Life of Epiphanius* 118-19 (cf. 111-12). The point needs emphasis because P. Amory, *People and Identity in Ostrogothic Italy, 489-554* (Cambridge, 1997), claimed that it was only Byzantine sources which described Theoderic's following as both large and accompanied by women and children. Rugi: John of Antioch fr. 214. 7 cf. Pro-

Again, the sources don't tell us everything that we would ideally want to know. In this case, the fragmentary survival of both Malchus of Philadelphia and John of Antioch means that the crucial events following the assassination of Recitach are not fully recoverable. But contemporary and circumstantially detailed Roman sources, in part this time confirmed by court-linked sources from Ostrogothic Italy, indicate firmly that large-group migration, involving many women and children, provides a thread of continuity in the story of the relocation of the power of the Amal line from Pannonia, first of all, into the East Roman Balkans, and then on to Italy in twenty years after 472, even if again, as in the career of Alaric, substantial further recruitment to the original core clearly also occurred.

INTERIM CONCLUSIONS

Looking carefully at what the better (i.e. more contemporary and circumstantially detailed) historical evidence has to report about the migration flows which became the Visigoths and Ostrogoths suggests a number of interim conclusions. First, it absolutely does not indicate that all (by any means) first-millennium migration was undertaken by large groups. There are many cases suggesting exactly the opposite. But it does provide at least two case studies of large-group migration, where the documentation satisfies all the rules of evidence that one would normally apply to mid-first millennium phenomena. The overall migratory pattern which emerges does not, however, conform to the assumptions of the old invasion hypothesis, where one unchanging large group was envisaged as moving untouched from point A to B. What we observe is something altogether more interesting. In both cases, we are faced with a substantial core (the Tervingi and Greuthungi in 376, the Pannonian Goths in 473) that was already large in and of itself, but which then added to itself in several bouts of subsequent recruitment. This was clearly not an accidental process, since the recruitment allowed the leadership to build up a following of sufficient size to allow the groups' members to achieve an ambitious set of aims they either had in mind from the beginning or evolved on the march. In particular, to succeed both Visigoths and Ostrogoths had to become large enough entities to force the Roman state to concede more attractive terms than it would give voluntarily, and/or replace existing political structures (Odovacar's Kingdom for instance) with new ones based around and primarily benefitting themselves. To that extent, these phenomena strongly resemble patterns seen later among ninth-century Vikings, where, in the third generation, great army coalitions eventually evolved via alliances formed among previously separate raiding forces, which were then large enough to destroy the political coherence of Anglo-Saxon kingdoms and allow large-scale land redistributions to members of the force.[19]

It is obviously important, however, not to over-generalise the applicability of this large-group migration model, in the way that the old invasion hypothesis was universally applied to ancient and pre-

copius *Wars* 7. 2. 1 ff. on the fact that they were still identifiable in Italy in 541 (Ennodius *Panegyric on Theoderic 55* records that they swapped sides twice during the war against Odovacar).

[19] The parallel is argued more fully in Heather, *Empires and Barbarians*, c. 9.

historical contexts up to the 1960s. Nothing suggests that any migratory phenomena in the late-fourth and fifth-century British context, for instance, would have worked in this way. The historical evidence (such as it is) suggests that any Anglo-Saxon migration took the form of dynamic, small-group intrusion, and this makes total sense. Anglo-Saxon (and other) intruders into the British Isles did not have to establish their position in the face of a substantial and aggressive existing political structure, such as the Roman state (or even Odovacar's Italian kingdom), and hence faced nothing like the same systemic pressures towards large-group consolidation. Some of the migratory phenomena reported as part of fall-out from the collapse of the Hunnic Empire following the death of Attila in 453 also look pretty small-scale (these reports are to be found in a mix of more and less contemporary materials).[20]

That said, the historical evidence in these two Gothic cases is good enough to indicate that it would be wrong to dismiss out of hand the possibility migratory phenomena of similar in scale, nature, coherence, and motivation were also in operation in some other cases where the documentation is less good, particularly Radagaisus' intrusion into Italy in 405/6, and the Rhine crossing of the Vandals/Alans/ Sueves (amongst others) in 406. Both look like large-scale phenomena even if the available documentation is much less good, and the fact that we have well-documented cases of contemporary large-scale migration does, I think, make it easier to accept that a similar dynamic operated in some other cases too (without for a moment wanting to turn this into a monolithically applied model). I would myself argue, for both Radagaisus and the Rhine invaders, that not only were the groups large and mixed, but, as with the Goths of 376, a good case can be made that a quasi-invasion hypothesis type of motivation – fear of the Huns combined simultaneously with a predatory attitude towards Roman wealth – probably applied. And although the precise motivation was certainly different, some of the migratory fall-out from the collapse of Attila's Empire also looks large-scale, and again involved struggles for the political control of territory. In sum, the historical evidence suggests that, while we shouldn't apply such a model universally, large groups of migrants motivated by a mixture of losing out in competition for their existing territories and a predatory interest in those of their neighbours can reasonably be seen as a periodic part of the late-fourth and fifth-century narrative. But, if so, what was the nature of these groups? How exactly should we envisage them?

LARGE-GROUP IDENTITIES

The most obvious thing to say about them is that the larger groups met in the better fourth- and fifth-century sources were certainly not 'peoples' in the pre-1960s sense of the word. Most of the successor kingdom-forming groups to the western Empire, like Visigoths and Ostrogoths, are demonstrably new coalitions formed in the recent past, and often actually on the march. It is only the Burgundians for whom this cannot be demonstrated and that may be more due to the fact that we have

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[20] My own views on the likely course of events in post-Roman Britain are laid out in Heather, *Empires and Barbarians*, c. 6. Sources suggesting that some of the fallout from Hunnic collapse took the form of smaller-group migration include Jordanes *Getica* 50. 265-6; *Romana* 336; Sidonius *Panegyric on Anthemius* (*Carm.* II), 236-98.

almost no relevant detailed historical evidence. Otherwise, not only Visigoths and Ostrogoths, but also Vandals/Alans, and even Sueves and Merovingian Franks were new political formations whose creation is documented (if to differing degrees of detail) in the historical record. None were ancient, long-standing, endogamous population groups.

Even more, some of the groups were fundamentally multi-ethnic. This is most obviously true of the Hasding-led coalition which conquered North Africa, and which had at its heart both originally Germanic-speaking Vandals and Iranian-speaking Alans. And even where the evidence for fundamental ethnic multiplicity is less strong (both the major component parts of Theoderic the Amal's force, for instance, are labelled 'Goths' in contemporary sources independently of his leadership) a degree of ethnic variability is consistently documented. By 488/9, Theoderic had added significant numbers of Rugi to his following, and individuals with other labels turn up from time to time in the materials from Ostrogothic Italy. Some of these additions, at least, may well date back to the Pannonian period. In similar vein, the Tervingi and Greuthungi are both labelled Goths in contemporary sources well before anyone knew that they would be bound together in a new alliance under Alaric. But Huns and Alans were also part of the force by 408, quite possibly even from 377, and we have no idea, really, of the ethnic composition of Radagaisus' forces, who probably – one way or another – provided much of the new manpower which attached itself to Alaric's train outside Rome. Not only were the kingdom-founding migrant groups new, therefore, but also it does not look as though shared ethnic identity played a major role in gluing them together.

This does not mean, however, that all the newly-created group identities were utterly insubstantial. We can explore this by taking the case of Theoderic's Ostrogoths again. In essence, theirs was a new, composite identity formed in the Balkans to fend off dangerous and hostile East Roman state power. The Emperor Zeno clearly had it in mind to solve the problem presented by two large Gothic military forces loose on his territory by having them fight each other and then mop up the pieces. The logical response to this situation for the Goths as a whole (though not for the two leaderships, one of whom had to lose out) was to form a united block to extract maximum concessions, and this is broadly what happened after the assassination of Recitach. In passing, it is perhaps worth underlining that this is entirely in line with the drift of much of the revisionist social scientific literature on group identity, where outside hostility often plays a major role in generating cohesive larger-group identities.[21]

But different members participated in Ostrogothic group identity to different degrees. Narrative evidence relating to Ostrogothic Italy (matching legal evidence from other post-Roman kingdoms) indicates that not even all the group's warriors enjoyed the same status. For the Ostrogoths, and in fact all the other kingdom-forming groups as well, the sources provide strong indications that we have to envisage that group membership came in three graded and heritable ranks: free and freed,

[21] For full reconstruction, see Heather, *Goths and Romans*, c. 9. In contrast, the pre-1960s literature on identity imagined that separate identity was always a product of geographical distance rather than, as tends to be the case, of geographical proximity combined with competition.

both of whom carried weapons, and slaves who did not. This clearly indicates that an individual could not simply chose to occupy the prime social position of freeman warrior when joining a group, and that membership, even when these groups needed to gather in warriors, was in some senses controlled, since, I take it, no one would voluntarily chose a subordinate position if something better was on offer. The sources give few indications of the numbers enrolled in each status group, but what there is suggests – as you would expect – that there were many fewer freemen than freedman warriors, the former numbering no more than a fifth to a quarter of the group. It is an entirely reasonable expectation that the more an individual receives benefit from their group membership, the greater their commitment was likely to be (and, of course, vice versa), so free and freed should be expected to have had different degrees of loyalty, while Gothic slaves, presumably, had very little at all.[22]

Another interesting question is what happened to this group identity after 493, when the settlement process distributed them across the Italian landscape. Patrick Amory argued, nearly twenty years ago now, that this destroyed original Ostrogothic group identity, but that would appear incorrect; the terms of the settlement were carefully framed, in fact, to maintain it. The evidence indicates strongly that Theoderic's followers received land and not shares of tax revenues as their primary reward, but, either way, the financial arrangements marked them out as a highly privileged group in the Italian landscape, and those privileges were heritable. Theoderic's Gothic and other followers were also settled in distinct clusters, concentrated in strategically vital areas (the north-east and north-west, the Adriatic coast, and down the line of the Via Flaminia between Ravenna and Rome), and those clusters retained a degree of local cohesion, appointing their own local leaderships who had simultaneously to find recognition at Ravenna. Periodic assemblies of males of the appropriate ages for military service and the distribution to them of donatives were also part of the picture. Theoderic continued to need, and regularly employed his army after 493, and the settlement arrangements clearly addressed that need. In the process, they erected a set of structures and privileges which allowed the group identity created in one context (war against East Rome in the Balkans) to renegotiate its existence in the new conditions of the Italian kingdom.[23]

Which shows up, in my view, in what happened when Justinian and Belisarius applied heavy military

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[22] The Ostrogothic evidence comes in the form of anecdotal asides in Procopius' narrative of the Italian war, collected and analysed at P.J. Heather, *The Goths* (Oxford, 1996), App. 1. At *Wars* 3. 8. 12, the elite number 1/5th of the train which accompanied Theoderic's sister to her wedding with the Vandal Thrasamund. All the post-Roman legal codes of the main western successor states (Visigothic, Frankish, Anglo-Saxon, Lombard, Burgundian) are equally insistent that warriors came in two separate status groups, and Procopius, again, provides some narrative evidence of this for the Lombards at *Wars* 8. 26. 12 where the higher status warriors number 2500 and the lower 3000. This hugely significant point has not received sufficient recognition in subsequent treatments of identity.

[23] Amory, *People and Identity*. For the counterargument in more detail, see P.J. Heather, '*Gens* and *regnum* among the Ostrogoths', in H.-W. Goetz, J. Jarnut and W. Pohl (eds.), *Regna and Gentes: The Relationship between Late Antique and Early Medieval Peoples and Kingdoms in the Transformation of the Roman World* (Leiden, 2003), pp. 85-133. On the settlement/*hospitalitas* issue, the tide is now generally turning against the Goffart thesis: see most recently P. Porena & Y. Rivière (eds.), *Expropriations et confiscations dans les royaumes barbares: une approche régionale* (Rome, 2012).

pressure on this privileged group within the Italian landscape from 536. Not all of the descendants of Theoderic's army felt the same sense of group commitment; they probably never had, and certainly not two further generations down the line. The first phase of the war was marked by a series of surrenders by some of the local Gothic clusters to East Roman power: half of the Goths of Picenum and Samnium, persuaded by one of their local leaders, being the standout example. There was also a very interesting case where, once the free warrior caste among the Goths of Dalmatia had been eliminated in battle, the rest just surrendered: reinforcing the reasonable *a priori* expectation that an individual's likely emotional commitment to the group was closely correlated to their social standing within it. But, in the long run, a majority of the elite free warrior class within the group were willing to fight – indeed fight very hard - to hold on to their privileges. Ostrogothic group identity not only survived the total replacement of the Amal dynasty, but also was capable of sustaining a substantial military effort against Constantinople for over a decade. And, read from this perspective, Procopius' account of the war clearly underlines both the political and psychological effect of losses among the elite free class of warriors. Eventually, so many were eliminated by death or deportation that all chance of victory had gone.[24]

In total, therefore, the case of the Ostrogoths suggests a balanced picture. Group identities were neither totally fluid, despite the fact that such coalitions were new constructions, nor without a considerable hold on the loyalty of a significant percentage of their members, even if some others clearly felt only much more contingent affinities. Such a conclusion is unavoidable unless you are going to adopt the highly convenient and methodologically circular argument that all of Procopius' evidence should simply be rejected in order to preserve the contrary view. Which leaves us with one more 'big' question. Did the large warrior coalitions who carved out successor kingdoms to the Roman West come already pretty much fully equipped with wives and families, or did they acquire them later?

Cases of the latter kind are known. Towards the end of the Russian civil war, two hundred thousand White Russian soldiers eventually settled in Romania in the 1920s, having arrived there with only about one thousand women in tow. Most acquired their families subsequently. Was this the case with the Goths, Vandals, and others in the later fourth and fifth centuries? Possibly, but the contemporary Roman sources, while not providing a great deal of coverage on this point (not surprisingly, they are mostly interested in the political-military threat posed by outsiders), do consistently mention women and children, and just occasionally with substantial circumstantial detail. The Goths of 376, Stilicho's foreign soldiery who joined Alaric, the Vandals and Alans, Theoderic's Goths on the march to Italy: all are described – if sometimes very briefly - in terms not suggesting a few women in tow (as any pre-modern army always has), but with large numbers of non-combatant dependents as an integral part of the group. Attempts have been made to dismiss this as a 'migration topos' on the part of Roman sources, who expected barbarians found on Roman soil to be 'peoples' on the move, but that's a very unconvincing (as well as convenient) line of argument. Ammianus, for instance, describes a wide

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[24] Samnium and Picenum: Procopius *Wars* 5. 15. 1-2; Dalmatian incident: *Wars* 7. 7. 26-37. Fuller discussion: Heather, *The Goths*, 272-6.

variety of barbarian outsiders on the move on Roman soil, so his very particular description of the Goths of 376 accompanied by their families, is not so easy to dismiss. Similar accounts of Theoderic's following produced around the court of Ravenna are also difficult to discount.[25] To my mind, there are a couple of important additional reasons why reports of large numbers of women and children need to be taken seriously.

First, to build a successor kingdom in the face of Roman state power, you needed a warrior group numbering thousands, in fact over ten thousand men, not a few hundreds. Looked at in the round, the evidence raises serious doubts that non-Roman economies – where large groups such as the Goths of 376, Radagaisus, and the Rhine invaders originated - could have supported professional warriors on such a scale. Both archaeological evidence and historical sources suggest that fourth-century royal retinues tended to be only a few hundred men strong, so that even putting several retinues together would not generate a big enough force to face down Roman counterattack. For that, it would be necessary to involve a broader tranche of the male population (amongst whom our evidence does indicate that obligations to military service certainly existed) when recruiting for expeditions, at which point you would be bound to draw on men with families. And when that recruiting also had in mind a one-way expedition to pastures new on the other side of the Roman frontier, with Huns and others breathing down your neck, it is hardly surprising that participants would not leave their dependents behind. It does make sense in broader terms, therefore, that large numbers of women and children would have had to be involved in any large-group movements.[26] Second, while not supposing for a moment that all members of these groups shared the same folk-dances and folk-costumes, some of the groups do seem to have maintained some degree of linguistic and other cultural continuity over several generations (i.e. of the Gothic language among Theoderic's following). This again strongly suggests that we are dealing with migrant groups which were not purely male. While there must have been substantial variation, there is no obvious reason to disbelieve the basic picture offered to us by our Roman and some other sources. Especially when a quasi-invasion hypothesis motivation applied, and large groups were seeking new homes in the face of outside pressure on their old domains, the idea that the militarily-able element of a population would leave their dependents behind as they made a one-way move away from the disputed zone is extremely unconvincing.

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[25] Amory, *People and Identity* coined the term 'migration *topos*', but (cf. note xviii) his pretext for doing so was vitiated by his failure to realise that court-connected sources within the Ostrogothic kingdom also referred to the Goths on the move with women and children. The argument is also impossible to sustain in the case of Ammianus on the events of 376; this is the only instance where he describes large numbers of barbarians moving with their families in a text where he describes barbarians on the move on many occasions: see further Heather, *Empires and Barbarians*, 153 ff.; cf. 176-7 for full references in contemporary sources (such as they are) to women and children.

[26] Chnodomarius had a retinue of 200 men: Ammianus 16. 12. 60. This is about the size of the force whose destroyed weaponry was deposited at Ejsbol Mose: M. Orsnes, 'The Weapon Find in Ejsbol Mose at Haderlev. Preliminary Report', *Acta Archaeologica* 34 (1963): 232-48.

FINAL REFLECTIONS

If, in response to the totally ambiguous answer provided by other sources and approaches, you run the thought experiment of taking the historical evidence for large-scale migration more seriously, applying to it the normal rules of evidence for mid-first millennium materials, the following conclusions emerge. Large-group, armed migratory activity is well-documented in some contexts, in the case of groups moving from barbaricum and across the Roman frontier, and then, on occasion, in the subsequent history of those groups on Roman soil. Given the limited surpluses generated by mid-first millennium non-Roman economies, this has to suggest that the groups will have been not only large, but also mixed in age and gender, because professional, full-time warriors in the several thousands could not have been supported: a perspective supported by both what Roman historical evidence there is, and – in certain places – linguistic and other cultural continuities. These large groups were not 'peoples', but new coalitions. It is usually impossible to say anything much about ethnicity (which is not the same as saying there were no cultural commonalities; in reality, the evidence is again usually just ambiguous), but some of the groups possessed a pretty robust collective *political* identity, which many members were committed to, even in the face of considerable personal cost and danger. The fact that other members were much more ready to abandon ship for pastures new is neither here nor there. That is true of all groups, at all times. What I would argue, overall, is that the body of contemporary historical evidence is sufficiently detailed and coherent for us broadly to accept what it is telling us (especially when extra confirmation is provided by internal court-linked sources as in the case of the Ostrogoths) rather than rejecting this account in favour *a priori* modern views of what is 'likely', especially when those views are based on a selective reading of the modern social scientific literature on group identities and their operation. The past is a foreign country and if phenomena turn up that are not immediately recognisable, then so be it: whether it is a case of large-group, organised migration, or witch trials.

Having said that, however, as I have argued in more detail elsewhere, there are some striking ways in which key elements of this first-millennium migratory activity become entirely recognisable when compared to the now highly-developed field of comparative migration studies. In the modern world, the build up of information among potential migrants about routes and possible destinations plays a large role in dictating the speed and direction of migration flows. Thinking about Goths and other long-distance migrants from this perspective helps explain why their moves often came in stages with considerable gaps between. For both Gothic groups, for instance, information about the possibilities available in Italy was slowly acquired during a decade and more in the Balkans, or, in the case of the Vandals and Alans, information about North Africa during the best part of two decades spent in southern Spain. It is also undeniably the case that an established history (or memory: it can skip a generation) of movement – even if it was only previously small-scale local movement - generates an increased tendency for particular population groups to engage in migration. Having once moved across the Roman frontier, therefore, intrusive outside groups will have been more likely to move on again, and, indeed, for many of them even making that initial move was pre-prepared for by a recent history of relocation. Last but not least this list of potted highlights, while most migrants are actual-

ly moved by a complex mixture of political and economic motives, it is largely politically-driven migration which, as in our the first-millennium sources, sets really large numbers of people on the move more or less simultaneously.[27]

Such correspondences do not remove all the problems, and I would not pretend for a moment that they do. What stands out when the first-millennium material is compared to more modern examples is the implied degree of cohesion that some, politically-driven, large groups sometimes showed as they took to the road. In modern cases, such as Rwanda in the early 1990s, some kind of new political order eventually emerged in the refugee camps, but the mass flight to those camps was utterly unstructured. By contrast, the Tervingi, it is reported, decided to seek asylum in the Roman Empire in 376 only after 'long deliberations', and, even after jettisoning Athanaric, proceeded to negotiate as a block with the Emperor Valens. This overview certainly hides some important points. The evidence for varying social status strongly implies that the minority free warrior caste would have had much more of a say in the eventual decision than second-rank freed warriors, and slaves none at all. But the late fourth- and fifth-century narrative of imperial collapse makes no sense at all without coherent outsiders capable of defeating (as at Hadrianople) or consistently facing down (as in the career of Alaric) major imperial field armies, because there is no good evidence for the current historiographical myths that the Roman state changed its long-established policies to outsiders voluntarily, or that the end of the Western Empire was a largely peaceful process. The only large-scale admission by negotiation we know of is that of the Tervingi in 376, but Valens' hand was forced here by the fact that his army was fully engaged in Persia, leaving him with no choice but to admit *some* Goths, and, his policy was anyway demonstrably one of damage limitation. The Greuthungi asked for asylum at the same time, and Valens only admitted one of the two groups assembled on the Danube. Every other large-scale intrusion happened absolutely without imperial licence.[28] As this suggests, migrant groups of the late Roman era had thus regularly to stand up to Roman imperial field armies of 10,000 men and more, and those unable to do so were dismantled with heavy casualties (e.g. Radagaisus, the Siling Vandals, and many of the Alans from the Rhine crossing). For all its internal divisions and administrative limitations, the Roman state did not give up without a fight, and this bottom line broadly confirms the picture painted in detailed contemporary historical evidence of the scale and cohesion of some of the migrant groups with which it was faced.[29]

[27] About a year's worth of reading in the comparative literature on migration underlies the passim discussion of migration in Heather, *Empires and Barbarians*, pulled together in summary form in c. 11.

[28] Ammianus 31. 3-4 with Heather, *Goths and Romans*, 128-35. N. Lenski, *Failure of Empire: Valens and the Roman State in the Fourth Century AD* (Berkeley, 2002) c. 7 attempted to revive the idea that Valens was more than happy to see the Danube frontier go up in flames when his field forces were already fully engaged against Persia, but the patent absurdity of this contention is emphasised by Valens' highly cautious response to the offer of multiple Gothic allies. There are no other occasions where major intrusive outside forces (Radagaisus, the Rhine crossing, Uldin) were in any way invited to cross the frontier.

[29] Why mixed migrant forces on this scale were able to operate in such a coherent manner is a separate and secondary issue following on from accepting the basic view offered in the sources. For an initial discussion of this feature of the action, which, I suspect, reflects contemporary levels of agricultural development and hence the degree of attachment of non-Roman groups to any particular landscape, see Heather, *Empires and Barbarians*, c. 11.

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