Introduction

What is the justification for yet another book, and a fairly fat one at that, on the subject of the late Roman Empire in Europe, specifically western Europe, a field already jostling with other publications? After all, in anglophone scholarship the last decade alone has seen a series of major publications that chronologically or geographically cover this period and area, indeed usually ranging more widely. On one bookshelf alone in my office I can find a selection of these. There are general and illuminating textbook-style introductions to the period such as Roger Collins's (1991) *Early Medieval Europe 300–1000*, Stephen Mitchell's (2007) *A History of the Later Roman Empire AD 284–641*, John Moorhead's (2001) *The Roman Empire Divided 400–700*, and the 'terrible twins' of 2005, Peter Heather's *The Fall of the Roman Empire: A New History* and Bryan Ward-Perkins's *The Fall of Rome and the End of Civilization*, two books that in their sizes, their approaches, their types of evidence and their intellectual frameworks show how much divergence there can be in studying this period. Other recent, wide-ranging works include another 2005 publication, Chris Wickham's massive and already massively influential *Framing the Early Middle Ages: Europe and the Mediterranean, 400–800*. More immediately concerned with the Western Empire are Guy Halsall's (2007) *Barbarian Migrations and the Roman West 376–568* and Matthew Innes's (2007) *Introduction to Early Medieval Western Europe 300–900: The Sword, the Plough and the Book*. On a more sanguinary note there are two recent works that bear upon the area under consideration in this book, Michael Kulikowski's (2007) *Rome's Gothic Wars from the Third Century to Alaric*, and Chris Kelly's (2008) *The End of Empire: Attila the Hun and the Fall of Rome*. These are just some of the publications of the last decade or so. Behind them lie such seminal works as A. H. M. Jones's (1964) *The Later Roman Empire 284–602* or Peter Brown's (1971) *The World of Late Antiquity: From Marcus Aurelius to Mohammed*, the works that for the anglophone world opened up the study of the period, made it into a thriving field of intellectual endeavour and conditioned much of its growth over the better part of half a century. Apart from such single-author works there are of course the great multi-author works such as Volumes XII (2005), XIII (1997) and XIV (2000) of

The Cambridge Ancient History covering respectively the periods 193–337, 337–425 and 425–600. And this is just the English-speaking world – indeed just the British part of that world. Similar developments have taken place in the modern countries that lie within or overlap with the territories of the Western Roman Empire, some of which will appear in the course of this book, as well as in the English-speaking world elsewhere, especially North America (one thinks, for instance, of scholars such as F. X. Gearey or R. Mathiesen).

So why then another book in an already crowded field? The simple answer is that all the books above are about history and this book is about archaeology. The books above take as their evidence base the written sources, be they narrative history or chronicles or hagiography or laws or the acts of Church councils or other classes of written object such as inscriptions or coins. This opens up huge resources for interrogation but also constrains that interrogation partly because of the inbuilt biases of literary form or the personal biases of authors, but more importantly because of what the written sources that survive do not deal with. There are enormous areas of life and death in this ancient world that simply cannot be approached from the written evidence that has come down to us. And because of this the questions to be asked and the analyses to be undertaken are circumscribed by what that evidence is concerned with and can reasonably be asked to tell us about. Of course, many of the authors listed above are well aware of archaeological evidence and what it can offer; in ways that, for instance, A. H. M. Jones famously was not. The most sustained use of archaeological evidence is that by Chris Wickham, who uses it throughout his major work and shows a sophisticated understanding of its strengths and weaknesses. Nevertheless, it is largely there to supplement textual evidence, and the questions posed and the approaches used in the book remain ones that are essentially text-driven. Moreover, he employs only certain parts of the archaeological record, principally those to do with economic structures over time, and to an extent social structures also, rather than using the full range of archaeological evidence, since that is not his purpose. Guy Halsall and Bryan Ward-Perkins also use elements of the archaeological record in the context of what are also still text-driven approaches. Though the latter ranges more widely through the archaeological repertoire, this is still using parts of it rather than a systematic consideration of all the domains of archaeological evidence available. Ward-Perkins's book has attracted much discussion for its moralising title. On the whole I avoid offering such judgements, but, equally, I have to say that I do not see that more than a handful of people (some kings and bishops)

were better off at the end of the fifth century than had been their equivalents at its start; on the other hand, a great many people were certainly worse off in many ways, and not just the crudely financial. One book that might be thought to deal with much the same area and period as the present work is Ellen Swift's (2000) *The End of the Western Roman Empire: An Archaeological Investigation*. It is indeed based on archaeological evidence, but a very restricted compass of that evidence, the material culture relating to dress and thus to 'ethnicity' and mobility. It does this very well and its analyses will be very valuable later in this book, but it does leave much scope for treatment of other domains of archaeological evidence. The title of Richard Reece's (1999) *The Later Roman Empire: An Archaeology AD 150–600* also suggests something that has anticipated this book, but in fact it also has a restricted compass, concerned mainly with the material culture of the late Roman elite; it does this in his usual clear-sighted and stimulating fashion, but again leaves scope for other approaches. Likewise, Jeremy Knight's (1999) *The End of Antiquity: Archaeology, Society and Religion AD 235–700* covers much the same geographical area as this book and makes much use of archaeological evidence, but its core is to do with the creation of a Christian archaeology across the West and how to interpret the significance of the sites and monuments.

So the prospectus for this book is that it is a first attempt to move a wide spectrum of the archaeology of the late Roman West centre stage, and to try to discuss the area and period in terms of its archaeology and come to conclusions based on the archaeological evidence rather than the textual evidence, or the textual evidence augmented by some of the archaeological evidence. This is what some might term its unique selling point. The evidence used is mainly settlements, burials and artefacts; the evidence from areas such as osteology, palynology and other palaeo-environmental disciplines figures far less often. The broad outline of what this book sets out to do is given by its title; uninspiring but accurate. It is an attempt to provide an overview of developments in the western part of the Roman Empire from the end of the second century AD to the start of the sixth, an overview based on the archaeological evidence and its various possible interpretations. The reasons for these geographical and chronological limits are outlined below.

Of course, all that textual history has taught us cannot be 'unlearnt', nor should textual evidence be ignored, nor am I a 'rejectionist' who thinks that all historical evidence should be set aside and the purity of archaeology preserved, but the ways in which historical evidence and narrative will be used here and articulated with each other need to be set out. At a 'grand narrative' level, it is pretty much impossible to unthink questions such as

'decline and fall' or 'Germanic settlement' and the origins of the successor states, and they will make their appearance here, but considered from a starting point in the archaeology, which may well not take the same routes or get to the same destinations as the texts. At another level of discourse, there are things we know about through the textual record which we would be hard put to it to derive from the archaeology. Examples of this that play an important exegetic role in various chapters of this book are the *Notitia Dignitatum* (Chapter 2), Christian ideas about the dead (Chapter 4), the nature and role of *paideia* (Chapter 5), or the operations of the late Roman tax system with its consequences for the wider economy (Chapter 7). These again will all be taken into consideration, but as providing a series of models for the archaeology, rather than as preordaining structures into which the archaeology has to fit whether it wants to or not: history as archaeology's bed of Procrustes (or older Ugly Sister with the glass slipper). On occasion, particular items of textual evidence will be used, but only where they clarify what was happening in a way that archaeology of its nature cannot. I am all in favour of drawing on as wide a range of evidence as possible to study the past; the wider the range of evidence, the wider the range of perspectives and the richer the consequent analyses and discussions. But this can only be done if the proper nature of each discipline, its particular protocols of analysis and synthesis, and its strengths and weaknesses are understood and respected. Because the development of the disciplines of archaeology and of history accorded to history the chronological priority, in periods such as this with a relatively abundant historical record, archaeology has all too often been used as the handmaid of history. What this book seeks in part to do is to try to emancipate archaeology from the role of servant by establishing the sorts of evidence that archaeology can bring to debates about this area of the human past and also the parameters within which this evidence and its analyses can be interrogated, especially by practitioners of other disciplines. In this way archaeology and history (and all the subdisciplines within them from palynology to palaeography) will each be able to contribute its own perspectives. Sometimes these perspectives will be in harmony; sometimes they will disagree or even flatly contradict each other. This last possibility is perhaps where really interesting progress is to be made.

Coverage and approaches

The book's main area of study covers the region from the Strait of Gibraltar to the Rhine; that is its definition of the 'Roman West'. It thus encompasses

the whole of the Iberian peninsula (though not the small area of western North Africa, Mauretania Tingitana, that administratively formed part of the *diocesis Hispaniarum*); that peninsula will sometimes be referred to as the Iberian peninsula, sometimes as Spain, because the latter is in English a generic appellation for the peninsula, even if it does subsume modern Portugal into its larger neighbour. What are now France and much of the Benelux countries will appear under the name of Gaul, since that is also a common usage among anglophone scholarship, largely in order to avoid simply equating the Roman provinces north of the Pyrenees with modern France. 'Germany' or 'the Rhineland' will be used interchangeably for the Roman provinces of Germania. The work will also on occasion deal with Britain, where the archaeology of the island provides complements, comparisons or contrasts to the mainland areas of the West. It is not an attempt to rewrite the archaeology of Britain in the fourth and fifth centuries, but those whose main focus of study is insular may nonetheless find developments on and arguments relating to the Continent to have relevance to the study of Britain. This definition of the 'Roman West', of course, excludes the historic heartland of the empire, Italy. The exclusion of Italy is entirely pragmatic; to have dealt with it at the same level of detail and argument as the other areas would have enormously lengthened the book (to say nothing of the time needed for researching and writing it); it would also have posed the problem of how to deal with the giant incubus that was the city of Rome. This definition also means that the areas of central Europe lying outside the Rhine and upper Danube frontiers are only dealt with in passing. It might be objected that developments crucial to what happened west of the Rhine had their origins there, and this is true enough: indeed, this work makes use of the concept of a zone on both sides of the Rhine. But as well as the pragmatic reason again of not overextending an already very large field of study, it is the contention here that the central concern of this work is the areas that had for several hundred years formed part of the Roman Empire and thus had become different from areas that had remained outside the empire. The West as defined here is big enough already, if not too big.

The period AD 200–500, which does not respond to more traditional periodisations of the Roman Empire or of antiquity, needs some explanation. The year AD 200 was very purposefully chosen to break with the convention of starting works on the later Roman Empire or late antiquity in around AD 300, after the storms of the third century had been successfully navigated. This latter is an essentially historiographical tradition, justified in part by a change in the number and nature of the narrative sources at the end of the third century but more especially dictated by the contrast of the

relatively stable political and military situation from the accession of Diocletian in 284 with the political and military instability of the preceding half-century. The years either side of 300 really do seem to the historian to mark a *temporum renovatio* in a way that the years either side of 200 simply do not. But much more importantly, since this is a work based on archaeology and not text, AD 200 was chosen because it lies at the threshold of a period of major change in the archaeological record, one commencing well before any 'third-century crisis' and marking the end of an archaeological period stretching from at least the first century BC, with AD 200 marking (in round numbers) the transition to a period with important differences from what went before. AD 500 essentially marks the end of this archaeological period, not only with the loss of most of the Roman political, economic and cultural superstructure but also with the putting in place of important changes in the basic settlement forms, social and economic formations, and thus the nature of the archaeological record. So it will be a central argument of this book that the period AD 200–500 is in certain significant senses an archaeological 'period' with its own internal patterns that are in important respects (though not all respects, of course) different from what came before and after. This argument will be considered in much more detail in the Epilogue, Chapter 10, once all the evidence that forms the core of this book has been laid out. It is thus a period which, because it is defined by the archaeological record, does not accord well with the conventional historically derived periodisation, allowing us to re-evaluate perceptions of late antiquity in the West. This is largely why the term 'late Roman' has been preferred since the three centuries here do not encompass the whole of what is normally thought of as 'late antiquity', but instead are the period when the West either was still part of the Roman Empire or was still very strongly influenced by its continuing presence, politically, socially, culturally and economically.

Having laid out the geographical and chronological fields of this book, we now need to turn to the methodological and theoretical approaches that will inform the work. Since the area and period to be studied have a considerable chronological and geographical extent and saw the lives and deaths of millions of people, the archaeological record thus created exhibits enormous variability. In order to avoid the Scylla of overgeneralisation where the macrolevel of the big picture risks obscuring multiple variations and realities, and the Charybdis of overspecificity where concentration on the microlevel of those same variations and realities risks obscuring how they may interrelate at a macrolevel, the big picture, I propose to employ a number of exegetic categories that allow for variation but also hold those

variations in relation to larger-scale analyses that articulate the particular with the general. The categories are as follows: periodisation, regionality, integration/disintegration, identity and materiality. These have been chosen as they respond to recurring and important aspects of the archaeological record.

Periodisation

This aspect relates to how the period defined above in an essentially descriptive way may be used more dynamically to structure and explain the processes observable over the three centuries concerned. As was stated above, it is a contention of this book that the period from the late second/early third centuries to the late fifth/early sixth centuries is a time span which has a logic that makes it, to a significant extent, internally coherent and thus distinct from what went before and came after. It will be argued that it is a period where the archaeology shows episodes of relatively rapid change at beginning and end framing a time of relative stability. The evidence and justification for this position will be presented in the Prologue and Epilogue chapters, Chapters 1 and 10. But the idea of a period of some three centuries within a longer span evokes the ideas of Fernand Braudel (1972) and the *Annales* school in history and archaeology (Braudel 1972; cf. Bintliff 1991), especially since this book will have a certain amount to say about the Mediterranean. Archaeologists have tended to favour some of the concepts and analyses of Braudel's approaches, since archaeology is much better adapted to recognition of more drawn-out change in the *moyenne durée*, the medium term measured in centuries, than to that of the rapid happenings of *histoire événementielle*; to recognition of the cumulative processes of the collective than to the actions and motives of the individual. But these ideas need to be used more than descriptively. This book will draw on the *Annales* tradition by, in particular, using its insights in trying to relate the period which is its central concern and may be classed as partaking of the character of the *moyenne durée* to the longer term and the shorter term. The longer term will concern itself with fundamental constraints, principally agricultural resources, systems and productivity, along with features such as geography and communications, which time and again return to condition what humans can achieve in this pre-industrial area (see especially Chapters 5 and 6). The shorter term will be the preserve of *histoire événementielle* and the *conjonctures* this produced and which fed through into alterations in the *mentalités* conditioning human actions in the more medium term. This is the area where political,

military or other events altered people's perceptions of the state of the world and how to react to it, or where short-term cultural events fed through into overall cultural formations over a more extended time span. The changes at the opening and closing of our period here can be seen as *conjonctures*, where relatively rapid and short-term changes impacted strongly upon *mentalités*, resulting in and represented by defining changes in the archaeological record.

Finally, a Braudel-style approach makes it incumbent upon the writer to explain how a period of the *moyenne durée*, such as the one studied here, articulates with the preceding and succeeding periods within the structures and rhythms of the *longue durée*. Thus the *conjonctures* act upon and modify existing structures so as to create new ones, linking the period that is our focus here to longer-term developments, and contributing to these developments rather than being partitioned off from what went before and what came after.

How these *conjonctures* impacted on existing patterns and how this fed through into modifications in the creation of the archaeological record take us into a methodological area where Braudelian analyses have long been criticised for lack of clarity, with the articulation between *histoire événementielle* and the associated *conjonctures*, on the one hand, and their interaction with the *longue durée* and existing structures of the *moyenne durée*, on the other hand, not being fully worked through and characterised. In large measure, this can now be analysed through the application of some of the theories put forward by Anthony Giddens (1986 in particular) relating to the reflexive dialogue between 'structure' and 'agency'. Again this approach is one that archaeologists find fruitful because of the ways in which it responds to the nature of their evidence and the timescales at which it operates. This book opens and closes with two periods when the existing structures were subject to a great deal of 'feedback' from those agents that operated within them, resulting in profound modifications to those structures over a relatively short period of time (of the order of inside of a century in each case, fast in terms of archaeological change). These changes operated not only at the level of 'structures' in their role as conceptions of social or cultural order, but also at the level of the physical structures from which people/agents learnt their roles and within which they carried on their lives. Major changes in these structures, both as organising concepts and lived environment, characterise both the thresholds at the beginning and the end of the period discussed here. Ideas such as 'lived environment' and its role in constituting the individual and the group and their praxis recall the *habitus* of a Bourdieu-style analysis (Bourdieu 1977, 1984), a concept that will be

invoked in discussions of major aspects of the built environment such as towns and villas and their roles in constituting the individual and transmitting structures of thought and praxis, but that also clearly relates to other aspects of the generation and internalisation of elite behaviour in particular and is displayed here in the correlates of elite culture and 'taste' (Bourdieu 1984), a major exegetic category for this book. But as is also a major axis of discussion here, these forms and their expressions change markedly in this period, so the causative factors (e.g. Christianity, militarisation) which modified the behaviours of individuals and groups and fed back into and changed their existing structures also need to be identified and their impacts characterised.

Regionality

Given the large geographical area covered here and the fact that it encompassed a wide range of climatic zones, landforms, natural resources and communication patterns, it is only to be expected that there would be a range of regions identifiable. At this physiographic level, we are dealing with the factors of the *longue durée* such as climate and resources that conditioned the patterns of human activity since the last retreat of the ice. As we shall see, particularly in Chapter 5 and to an extent in Chapter 6, these factors continued to shape certain aspects of human activity right through the period under consideration here. Overlaid onto this was a whole array of human regionalities conditioned by aspects such as ethnicity, language, and social and political structures operating in the *moyenne durée* and then acted upon by shorter-term events, in particular the processes of the incorporation of different regions into the Roman Empire and their response (especially those of their elites) to the threats and opportunities thus opened up to them. At one level, regions will be used here in an essentially descriptive fashion, identifying areas where the archaeology appears to have a degree of coherence, particularly if that coherence sets the area apart from its neighbours. It will also become clear that there are a number of regions that recur with some frequency in the chapters that follow (for instance, the Paris Basin, the south-west of Gaul, the south-east of Gaul, Catalonia, the northern Meseta, the Guadalquivir valley), regions defined not only by a coherence in the past but also by the quantity and quality of archaeological work in the present. But over and above these largely descriptive and pragmatic uses of the idea of the region, there are some more dynamic aspects of the concept. It will be argued that though the Roman Empire was always an empire of regions (and indeed of smaller

subdivisions), regions become more visible and important from the later third century on than they had been in the first and second centuries (a trend perhaps most pronounced in Gaul, but perfectly visible in Iberia and Britain also). At one level this is visible in the archaeology of the regional elites, which becomes more differentiated, partly as a result of reactions to the events of the mid to later third century (e.g. northern Gaul), partly as a result, it will be argued, of the degree of relationship with the imperial court (e.g. south-western Gaul, the northern Meseta), and partly because of geographical factors such as the influence of the Mediterranean seaways (south-eastern Gaul, Catalonia) or the geography of the Iberian peninsula. So these regions are not merely descriptive, nor simply the ones that are most archaeologically visible; in the fourth century they were the ones that were becoming increasingly differentiated from their neighbours. This has important repercussions into the fifth century as the overarching political, military and fiscal structures of the Western Empire disintegrated, leaving the various regions to follow different trajectories fashioned by a combination of their fourth-century histories, short-term events through the fifth century and continuing cultural developments over that period with which the various regions interacted in different ways. So regions are not just descriptive conveniences; their inhabitants are central to driving the processes of change visible in the archaeology.

Integration and dis-integration

Leading on from the importance of regions is the complex of ways in which these regions were integrated into the larger system that was the Roman Empire through the third and fourth centuries and the ways in which that system promoted, consciously or unconsciously, the various forms of integration we can trace in the archaeology. Equally, with the collapse of the central political, military and fiscal formations of the Western Empire in the course of the fifth century, how did this dis-integration at the level of the empire itself have knock-on effects leading to other forms of dis-integration down the spatial and political hierarchies? Integration and dis-integration are not only synchronic measures of the ways in which societies were structured and the levels at which they operated, but they are also diachronic descriptors of processes that vitally affected the changing levels of complexity at which those societies could operate. These levels in their turn were major influences on the formation and patterning of the archaeological record, and thus on our interrogation of that record. For the late Roman West, and operating in the direction of integration, there was for two-thirds