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London, Victoria and Albert Museum, the Basilewski ivory *titula*, c. 980
(photo © Victoria and Albert Museum)

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INTRODUCTION: READING THE TENTH CENTURY

Timothy Reuter

THE PRESENT volume covers a period in European history best described as the ‘long tenth century’, stretching from the 890s through to around 1020/30. Though this volume covers Byzantine history of the period and also Islamic history so far as it impinges on European territory, the emphasis in this introduction will be largely on what was or would become the Latin west. I shall try to sketch what currently seem the main concerns of historians working on the period and what are generally seen as its salient features, though any such attempt will probably date far faster than the substantive chapters which follow. The ways in which historians make and have made sense of the period as a whole have been determined by a range of inputs. Before we can look at the general trends which are currently held to characterise the period (and the extent to which they actually do) we need to examine these inputs. The most important of them is the nature, real and perceived, of the available source-materials. But two others are almost as important. The first comes from the traditional and non-traditional interpretative schemata and periodisations which the community of professional scholars has brought to bear. The second, perhaps even more important, is the fact that the members of this community for the most part work and have worked within specific historiographical traditions.

It is widely held that the long tenth century is a period more lacking in sources and reliable and precise information on ‘what actually happened’ than any other period of post-Roman European history, with the exception perhaps of the seventh century. It is not just the very evident brutality of much of the period that has caused it to be termed a ‘dark century’ (*dunkles Jahrhundert*) or an ‘obscure age’ (*secolo oscuro*), or an ‘iron age’ (with the overtone, so chilling for modern professional scholars, that words and thoughts are silenced in the face of armed force).¹ It is also the difficulty historians often encounter, for

¹ See Zimmermann (1971), pp. 15–21, on the history of these terms; Lestocquoy (1947), White (1955) and Lopez (1962) are early attempts at re-evaluating the period as a conscious reaction against them.

example, when trying to establish precise sequences of events or office-holders. At least in parts of the post-Carolingian core of Europe there seems to have been a decline in pragmatic literacy and a reversion to oral and symbolic means of communication. As we shall see, this was by no means a universal feature of the long tenth century; but to the extent that it did really exist it meant that human interaction often took forms which have inevitably left relatively fewer traces in the written record, and those often indirect and difficult to interpret.

Nevertheless, notions of a dark or obscure or 'iron' age are problematic. Though they go back a long way, they exercised their most formative influence during the period when a Rankean primacy of political history still dominated medievalists' consciousnesses. When there is at most one substantial narrative dealing with the high politics of a region, writing about 'what actually happened' seems even more difficult and uncertain than it is in any case, and the results thus dark or obscure. Many regions of Europe are in this position for most of the long tenth century: east Frankish/German history is unusual in having the accounts of Widukind of Corvey, Liudprand of Cremona and Adalbert of St Maximin running in parallel for much of the middle third of the tenth century.

Even this dearth of narratives is a difficulty found mainly in the west, Latin and Islamic, rather than the east, where the tenth century is no more obscure than any other period of Byzantine history and rather less than some. Outside the Mediterranean world there are indeed regions for which we have virtually no contemporary narratives at all. The emergent realms of Rus', Hungary, Bohemia and Poland, naturally, as well as the Scandinavian kingdoms, have no contemporary indigenous accounts, only later, mythologising origin histories: the *Tale of Bygone Years* or *Russian Primary Chronicle* for Rus'; the late twelfth-century Anonymus and later derivatives like Simon de Kéza and the *Chronicon pictum* for Hungarian history; the early twelfth-century court writers, Cosmas of Prague and Gallus Anonymus, for Bohemian and Polish history; Saxo Grammaticus, *Heimskringla* and its precursors for Scandinavian history. The savage positivist source-criticism of the late nineteenth and early twentieth centuries has left few historians willing to use such works as 'primary sources' except in a state of cautious desperation or for the citation of an occasional phrase to add rhetorical colour. Even when it is evident that their authors must have drawn on earlier works now lost to us, it is normally impossible to tell precisely where they are doing this, while the analysis of these works as later representations of an earlier past has in many cases barely begun. Once the information offered by these high-medieval versions of earlier pasts is seen as the product of later construction rather than the echo of past reality, the political history of these regions has to be written in a much more tentative and

uncertain fashion, drawing mainly on casual and largely decontextualised fragments of information found in narratives from the Frankish, Anglo-Saxon and Byzantine world and in Arabic and Jewish travellers' tales. Some parts of western Europe are almost as badly placed, most notably the kingdom of Burgundy and the principalities of Catalonia and Toulouse, at least as far as any reconstruction of *histoire événementielle* is concerned: few European rulers of any period can have left as little trace in the record after reigning for nearly sixty years as has Conrad the Pacific of Burgundy.

Yet the long tenth century is also an age of great historians, writers who offer rich and juicy texts with a wide narrative sweep and much significant detail: Widukind of Corvey, Adalbert of Magdeburg and Thietmar of Merseburg working in Saxony; Flodoard and Richer in Rheims; Dudo of Saint-Quentin in Normandy; Adhémar of Chabannes and Radulf Glaber in central France; Liudprand of Cremona in Italy (and north of the Alps); Benedict of Soracte in Rome; Sampiro in León. Some sections and some versions of the enigmatic complex known collectively as the *Anglo-Saxon Chronicle*, notably the strange compilation by the ealdorman Æthelwold written around 980, would also qualify. There are also impressive works of more local compass, such as the Lotharingian episcopal *gesta*, or Flodoard's lengthy and archivally based history of the church of Rheims. Most important of all, and not only for the sheer bulk of what survives, is the large corpus of saints' lives and miracle-collections from this period: it was a golden age of hagiographic production.

Traditional attitudes, however, are slow to change. Modern medievalists' relationship with 'hagiography' is revealed by the fact that whereas almost all the major 'historiographical' works of the period are available in good modern editions, most 'hagiography' still has to be consulted in old and often very inadequate editions. A nineteenth-century distinction between historians, who deal in facts, and hagiographers, who deal in fictions, was perhaps appropriate to an era of scholarship in which it was important to begin by establishing the who, the what, the where and the when, all matters on which 'hagiographic' texts are often imprecise or inaccurate. But it now needs to be transcended: it is by no means clear that the distinction reflects anything significant about the intentions and practices of tenth-century authors: many 'historians' also wrote 'hagiography'.²

Yet few even of those conventionally thought of as historians rather than hagiographers have left us straightforward and unproblematic texts. The acid-bath of positivist source-criticism may have dissolved the later mythologising histories of the European periphery almost completely, but it has also left the smooth surfaces of writers like Widukind, Richer and Dudo deeply pitted, so

² Lifshitz (1994).

much so that Martin Lintzel could write about the ‘problem of truth in the tenth century’ (meaning the problem of having any confidence at all in the relation between our surviving accounts and the course of an increasingly inaccessible past reality ‘out there’), and more recently Carlrichard Brühl has felt able to dismiss both Widukind and Richer as *romanciers*.³ Few historians at the end of the twentieth century are still willing to offer this kind of robust empiricism without qualms; but though the aspects of these sources problematised by Lintzel and Brühl are not the only ones, they are real enough, for elements of saga, of epic, of the preacher’s *exemplum*, of folk-tale, seem to greet us on many pages of these works, and they will rarely submit to a straightforward positivist unpacking of their meaning.⁴

Historians of a positivist frame of mind have traditionally contrasted the uncertain and subjective information derived from narratives with the firmer data to be won from record evidence, which in this period means from charters. Many series of royal diplomata from this period now exist in complete and satisfactory modern editions: those issued by or in the name of the rulers of east Francia/Germany, of Burgundy, of Hungary and of Italy are available complete, and those of the west Frankish rulers almost so, while as far as surviving papal letters and privileges are concerned it is for this period alone that we possess a comprehensive edition of everything surviving.⁵ Even for those regions where the picture is still incomplete – Anglo-Saxon England, the Spanish peninsula, Byzantium – the gaps are being filled. Below that level the picture is less favourable. Although the period is characterised by the exercise of ‘quasi-regal’ power by figures with less than royal status – archbishops, bishops, dukes, margraves – the charters they issued were not numerous, and in most regions have hardly begun to be collected in modern editions;⁶ an exception is the collection of the *placita* of the kingdom of Italy, accounts of judicial decisions given by a court president acting (or ostensibly acting) in the ruler’s name.⁷

The bulk of non-royal charter material surviving from this period consists of what we would nowadays think of as either conveyancing records or accounts of dispute settlement. Normally such documents offer a miniature narrative of a conveyance or settlement with a list of those present at the transaction; in many areas of northern Europe they were treated, so far as we can tell, as a mere record of the transaction with no inherent legal force, though both England and Italy show that this did not have to be the case. It is precisely during the period covered by this volume that the narratives in many parts of

³ Lintzel (1956); Brühl (1990), pp. 465–7, 589–93. ⁴ Reuter (1994).

⁵ Zimmermann, H. (ed.), *Papsturkunden 896–1046*.

⁶ Kienast (1968) provides a convenient guide to the charters produced for secular princes; there is a complete edition for Normandy in *Recueil des actes des ducs de Normandie*.

⁷ Manaresi, C. (ed.), *I placiti del ‘Regnum Italiae’*.

Europe, especially in France, become less miniature and more detailed, and it has indeed been argued that such loquacity has misled historians into thinking that the things they describe in such detail were really new around the millennium rather than simply coming to be recorded for the first time.⁸ Both their geographical distribution and the quality of the editions they have received are very uneven. The archives of the Mediterranean regions – Italy, both north and south, and parts of Spain (especially Catalonia and Castile) – are very full, if not always very fully known or exploited. In northern Europe such collections of material as have survived have normally done so in the form of cartularies put together by religious institutions, often in the century and a half after the period covered by this volume, when such institutions were taking steps to put their property ownership and administration on a more ordered and rational basis, and so to arrange selected and edited versions of their archives in book form. Large and unmediated archival deposits are rare, the large tenth- and eleventh-century archives of Cluny being an unusual exception.⁹ In particular, many of the north European centres active in producing archival material in the eighth and ninth centuries, from Redon to St Gallen, either ceased to do so altogether in the tenth century or else did so at a greatly reduced rate.

Little of this material has been edited both comprehensively and recently. Nor has its nature always been properly appreciated by historians. The history of diplomatic has been one of a preoccupation with distinguishing the genuine from the false. The question of authenticity is an appropriate and important point from which to start when dealing with royal and papal charters, because such documents, at least in theory, were in themselves adequate to guarantee the claims contained in them, and this made them worth forging, both at the time and later. But it does not go far enough, even for them. Every charter tells a story, and even if we can establish that the charter is indeed what it purports to be, the authenticity of the charter in a formal legal sense is in itself no guarantee of the authenticity or completeness or meaningfulness in a historical sense of the story which it tells. Most such stories are indeed manifestly incomplete, and historians have barely begun to study the narrative strategies of charter-writers and of those who commissioned their activities. This is all the more significant with the advent, already noted, of a much more garrulous style of charter-writing, including complaints (*querimoniae*) and concords (*convenientiae*) which set out the whole history of a dispute. The fact that these miniature histories are found embedded in what look like legal documents does not make them any less subjective or their interpretation any less problematic.

In some, though not all parts of Latin Europe there was a temporary downturn in charter production in the early part of this period, though the view of the

⁸ Barthélemy (1992a).

⁹ *Recueil des chartes de l'abbaye de Cluny*.

period as an 'obscure age' has itself obscured the fact that this downturn was reversed almost everywhere by the later tenth century, to be followed by steady growth. But there was a quite genuine and long-lasting downturn in legislative activity almost everywhere in Latin Europe; it was one of the most evident contrasts between the Latin west on the one hand and Byzantine or Islamic political culture on the other, for those few contemporaries who were familiar with both.¹⁰ For most of the west during this period little or no legislation survives, even in those regions where rulers appear to have been powerful and impressive figures, and this is not to be attributed to large-scale losses of what once existed. The Carolingian capitulary tradition had virtually died out by the end of the ninth century (after 884 in west Francia, after 898 in Italy, after 852 in east Francia). The Ottonians and their entourages knew what capitularies were, but confined themselves to very occasional *ad hoc* edicts.¹¹ Collections of Carolingian capitularies, notably that of Ansegis, continued to be copied in the tenth and early eleventh centuries, both in west and in east Francia in particular, but it is far from clear what use might have been made of such manuscripts in practical life.¹² Anglo-Saxon England is the great western European exception to the tenth-century legislative drought; here, collections of Carolingian capitularies transmitted from the continent provided some of the inspiration which enabled the kingdom to catch up with, absorb and develop the lessons of Carolingian government in a long series of law-codes, notably those of Æthelstan, Æthelred and Cnut.¹³ Paler forms of imitation of the Carolingians can be seen in the laws of Stephen of Hungary from the early eleventh century.¹⁴ The Byzantine development was, as one might expect, smoother and more continuous: the tenth-century rulers continued to legislate as a matter of course, without break or decline.¹⁵

The church also legislated less: councils, where they did meet, were more likely to leave only protocols of judicial decisions or charters solemnised by the fortuitous presence of numerous imposing witnesses than they were to produce legislation in the form of canons.¹⁶ Equally, the great Carolingian tradition of episcopal capitularies had comparatively weak echoes in the practice of tenth-century bishops.¹⁷ This picture of inactivity is particularly true of the

¹⁰ See Nelson's analysis of John of Gorze's account of his visit to the Cordovan court, below, pp. 126–8.

¹¹ *MGH Const* 1, no. 8, p. 17; D H II 370.

¹² Mordek (1995); Ansegis, *Collectio capitularium*, ed. Schmitz, pp. 189–90.

¹³ Edited in *Die Gesetze der Angelsachsen*, ed. Liebermann; on the Carolingian sources for such legislation see Wormald (1978), pp. 71–4. ¹⁴ Stephen, King of Hungary, *Laws*.

¹⁵ See Shepard, below, pp. 553–4; on the contrast with the west in this respect see Leyser (1994b), pp. 160–1.

¹⁶ This is the conclusion of Schröder (1980) for west Francia; the situation elsewhere was similar if less extreme.

¹⁷ *Capitula episcoporum* III contains a few tenth-century specimens; the overall distribution of texts and manuscripts is to be surveyed in vol IV, which has not yet appeared.

early tenth century; from around 950 onwards there was something of a recovery. Although this recovery was hardly a rapid one anywhere, the great sequence of reforming councils initiated by Leo IX's councils at Rheims and Mainz in 1049 was not preceded by a long legislative drought in the way that the otherwise comparable revival of conciliar activity in the early Carolingian period had been.¹⁸ Our picture is still an imperfect one, for though such secular laws as have survived, in Byzantium and in the west, have generally been well edited, conciliar legislation is only now receiving the attention it deserves.¹⁹ In particular, we lack a comprehensive edition of the texts produced by those councils at which the 'legislation' of the Peace and Truce of God movements was promulgated.²⁰ But we also lack a modern edition of almost any of the collections of canon law regularly used in the long tenth century, or of the great collection produced at the end of it by Burchard of Worms, which largely superseded these earlier collections.²¹

Almost all of the surviving letter-collections of the period (and not many tenth-century letters have been preserved outside collections) can be seen in a context of canon law. It is not an accident that the most important ones are associated with important reforming clerics – Rather of Verona and Liège, Gerbert of Rheims, Fulbert of Chartres, Dunstan of Canterbury – and that they contain many letters dealing with practical matters of church law.²² Letters should not be seen in this context alone, however. The impulse to preserve them in collections, which would become stronger and more widespread in the course of the eleventh and twelfth centuries, was not simply a product of the period's concern with *memoria* and of a desire to preserve the memory of the people with whom they were associated. It also stemmed from the need for models to be used in the training of clerics: significantly, Dunstan, Gerbert and Fulbert were teachers as well as lawyers. The Latin poetry of the period was also located in this rhetorical-didactic tradition: an art of the schools rather than of the court, which it had been at least to some extent in the preceding period.²³ Here again we have a contrast between the Latin west and the court-centred cultures of Byzantium and Islam.

As with the earlier medieval centuries, one feels that the material remains of

¹⁸ Hartmann (1989) pp. 47–50.

¹⁹ *Concilia aevi Saxonici 916–1001*, I: 916–961; for commentary see Schröder (1980), Vollrath (1985), Wolter (1988), and the chapters in the forthcoming *History of Medieval Canon Law* edited by Wilfried Hartmann and Kenneth Pennington.

²⁰ See Hoffmann (1964) for details of the printed sources; much of the manuscript work remains to be done.

²¹ Hoffmann and Pokorny (1991) is now the starting point for any work on Burchard's collection.

²² Rather of Verona, *Epistolae*; Gerbert of Aurillac, *Epistolae*; Fulbert of Chartres, *The Letters and Poems; Memorials of Saint Dunstan*, pp. 354–438. The connection is most evident in the case of Fulbert: see, e.g., *opp.* 28, 36, 56, 71. ²³ Godman (1987).

the long tenth century ought to have made more impact on historians' consciousnesses and interpretations than in practice they have done. Excavation has played a major part in reshaping post-Carolingian urban history, not least through the very detailed investigation of Viking York and Dublin; Peter Johanek's chapter shows how this has affected our view of the period. Our view of post-Carolingian settlement patterns owes in general much less to archaeology: this is certainly true of villages, which, as Robert Fossier argues below (in common with many other though by no means all scholars), first start to take on definitive form and permanent location in this period. It is perhaps less true of the dwellings of the dominant aristocratic strata of post-Carolingian society, also seen as 'settling down' in the course of the long tenth century, but although the development of the aristocratic dwelling, often a fortified site, has been extensively studied and has been linked to shifts in family structure in this period, we are still far from having a clear view of where and how the non-urban aristocracies of northern Europe lived.²⁴ Historians of the tenth century should undoubtedly pay more attention to archaeology than they have, though the absence of substantial syntheses and the gaps in the publication of excavations as well as the divergences between national archaeological traditions (even more marked than the historiographical divergences to be examined shortly) will continue to make this difficult in the foreseeable future.

Some kinds of material remains have escaped historians' general neglect of non-written sources, most notably those traditionally studied by art historians: painting, sculpture, goldsmithery and ivorywork, architecture. The study of manuscripts, both as material objects and as repositories of images, has received at least as much attention as the study of the written sources of the period. So have the surviving remains of metalwork and wood- and ivory-carvings, in the form of book-covers and other carved panels, of liturgical combs, and above all of reliquaries and items of regalia. Much of this record is lost, however, and some of its context is irrecoverable. Virtually no secular buildings and very few ecclesiastical ones have survived unchanged and intact from the tenth century. The wall-paintings and tapestries which once decorated them, and which would probably have told us even more about the culture and self-image of the period than do illuminated manuscripts, have vanished almost without trace, except for an occasional survival like the church of St George on the Reichenau with its almost intact cycle of wall-paintings. Ecclesiastical vestments have survived in quite substantial numbers, but the tapestries recording the deeds of kings and aristocrats are known only from a handful of casual written references. Many of these kinds of material survival have attracted the attention of cultural and political historians as well as of his-

²⁴ See below, pp. 18–19.

torians of art, because they fall or can be seen as falling into the category of 'signs of lordship and symbols of state', to use a phrase invented by the German medievalist Percy Ernst Schramm. Like their counterpart in written sources, the (often anecdotalised) record of symbolic action, they have seemed to offer a way in to the mindset of the period's elites which might otherwise be closed to us by the sheer inarticulacy of more direct evidence.²⁵

The source-materials available for the study of a period are far from defining the ways in which that period will be studied. Claudio Leonardi begins his chapter on intellectual life by remarking that the era between the late Carolingian scholars and *litterati* and the early scholastics of the later eleventh century is often thought of either as post-Carolingian or as pre-Gregorian, and is thus denied an identity of its own.²⁶ Analogous remarks could be made about the prevailing interpretation of other aspects of the period. There is, of course, some justification for such terminology and the interpretative schemata which lie behind it. Much of tenth-century Europe – though hardly the Byzantine and Islamic spheres – saw itself as in a sense post-Carolingian: it simultaneously perpetuated and looked back nostalgically to an order once glorious, now in decline. The heirs of the direct successor-states looked back to a supposedly golden age of Frankish unity, which seemed all the more golden for the absence of any clear and precise memories of it. Carolingian nostalgia was at its strongest in regions where the Carolingians had been largely absent, like the south of France, and it grew once real Carolingians were no longer around: it was Otto III, not Otto I, who took the first steps towards the canonisation of Charlemagne.²⁷ The post-Carolingian core of Europe retained a residual sense of pan-Frankishness long after kingdoms (not, as yet, nations), had started to develop their own sense of identity. In the large arc to the north and east of the former Frankish empire, from England through to Hungary, it was as much the written and unwritten myth of the Carolingian polity as experience of the contemporary hegemonial power, the Ottonians, that provided a model for development, whether in the form of imitation capitularies in the Wessex of Edgar and Æthelred or in the adaptation of *Lex Baiuvariorum* to serve as the basis for early Hungarian law. Equally, although the 'Gregorian' and 'pre-Gregorian' terminology may have been subjected to powerful attacks in recent years it can hardly be escaped altogether.²⁸ The apparent universality of the charges laid by the church reformers and historians of the mid- and late eleventh century and echoed by historians of the nineteenth and twentieth at least gives a degree of unification to our perceptions of tenth- and early eleventh-century Europe, united by sin, by

²⁵ For the work of Schramm see Bak (1973); for work on political ritual see Althoff (1990); Koziol (1992); Althoff (1997). ²⁶ Below, p. 187.

²⁷ Folz (1950), pp. 47–114; Remensnyder (1995). ²⁸ Tellenbach (1985, 1993).

ecclesiastical abuse, and by attempts by a small radical minority to overcome these failings.

Two other models currently offer broader versions of the divisions just mentioned. Much German-language historiography – and formerly French historiography as well, as witness Marc Bloch's distinction between the first and the second feudal age – sees the mid-eleventh century as having marked a crucial change from an 'archaic' society to that 'old European order' which prevailed from the late eleventh to the late eighteenth century.²⁹ This may be seen as a more secular and sociological rewriting of the schema 'pre-' and 'post-Gregorian': church reform was on this view merely symptomatic of more general changes in the eleventh century towards greater rationality and greater social differentiation.³⁰

An alternative view, which would stress political more than other kinds of development, is to see the period as initiating, as far as Latin-speaking western Europe is concerned, a very long era during which Europe would be shaped by competing dynastically oriented territories, many of them the ancestors of the modern nation-state, even though that term is hardly applicable to the tenth century. Geoffrey Barraclough defined the long tenth century as the 'crucible of Europe', the period in which large-scale supra-regional empires finally disappeared, to be replaced by the smaller kingdoms familiar from later European history.³¹ Certainly much of Europe's political geography can be seen to have begun in this period, a fact which was taken as the basis of a large international conference in 1968 on the 'origins of nation-states' in this period.³² Yet even as an interpretation of political history alone it fits some parts of Europe much better than it does others. It clearly works well for the northern and eastern parts of Europe, where present-day polities very evidently emerged from pre-history in a recognisable form in the course of the tenth century. German medieval historiography has also devoted much attention to the 'beginnings of German history', which are now generally placed in the course of the long tenth century rather than the ninth, even if they are no longer defined in terms of a significant date like 911 or 919 or 936.³³

Yet it is German medievalists who have sought to establish the 'beginnings of French history' and place them in the same period;³⁴ it is far less of a defining moment for French historians, for whom something recognisable as France had already been around for some time by the tenth century. Indeed it is in the French historiographical tradition that a quite opposite view has been developed. Rather than the 'birth of Europe' rhetoric, this offers the tenth

²⁹ Brunner (1968); Gerhard (1981) For Bloch's distinction see Bloch (1961), pp. 59–71.

³⁰ Murray (1978), esp pp. 25–137.

³¹ Barraclough (1976); cf. also the titles of Calmette (1941), Fossier (1982) and Fried (1991).

³² Manteuffel (1968). ³³ Brühl (1990); Ehlers (1994). ³⁴ Ehlers (1985).

century as the last century of an old order, one which was not merely post-Carolingian, but post-Roman. The reasons which have been given for taking such a view have varied. Some scholars have wanted to stress a continuity of the late antique legal and political order through to the late tenth century.³⁵ Others, Marxisant or neo-Marxisant, have stressed an underlying shift in the mode of production and hence the dominant social formation from slavery to serfdom (and hence, in the Marxist sense of the terminology, from slaveholding to feudalism).³⁶ Others have seen the tenth century as ending in a new fragmentation (*encellulement*) of society, a world in which interaction at a distance had almost ceased to exist, in which the horizon did not extend much beyond the view from the castle wall.³⁷

With considerations like these we have already arrived at the third kind of input mentioned at the outset, and it is not only for the reasons just discussed that the interpretative schemata on offer for tenth-century history depend on the historiographical tradition in which a historian is working. There is a common European tradition, but its regional variations are very marked. In particular, the master narratives dominant in the various European countries and regions mean that there is no comprehensive European consensus on which aspects of the period are to be seen as significant. To some extent there is also a problem of language: both the technical terms and the underlying conceptual apparatus in use vary from national tradition to national tradition, and there are as yet few guides to these which will allow the historian to carry out reliable translation. It may well be that an increasing awareness of other traditions and of the work being done within them will create a more genuinely European view of tenth-century history within the coming generation; some of what we currently perceive as real differences in the past may turn out to be mere differences of perception, the products of divergent terminology and historiographical tradition.

It is noteworthy how many of the periodisations and implicit or explicit underlying models are drawn from French history, and in an English-language history it is worth stressing the point. Not only have French medievalists been given to offering such theories more than most; both the Anglo-Norman and Anglo-Angevin connections of English medieval history and the foreign-language teaching traditions dominant in the Anglolexic world have created a 'Francocentric' approach: French medieval history has

³⁵ Durliat (1990); Magnou-Nortier (1981, 1982, 1984); for a critique see Wickham (1993) The same periodisation is found, more impressionistically justified, in Sullivan (1989).

³⁶ Bois (1989); Bonnassie (1991).

³⁷ Fossier (1982), pp. 288–601, esp. pp. 288–90; also below, pp. 45–53. For the relationship between *encellulement* and *incastellamento*, its Italian relative, see the historiographical account in Wickham (1986), pp. xxiii–xxvi; for critiques of the concept see Leyser (1994c) and Campbell (1990).

often been taken metonymically in Britain and America for the whole of tenth- and early eleventh-century Europe. More important still is the way in which an impressive series of regional studies, beginning with and in many cases inspired by Duby's classic study of the Mâconnais, have fleshed out in often very substantial detail the transformation of various parts of France in the post-Carolingian era.³⁸ We have a better picture of the tenth century on the ground for west Francia than for any other part of Europe, not necessarily because the supply of sources is inherently superior, but because many of its regions have been systematically studied in a way in which tenth-century Bavaria or Umbria have not yet been (it would be possible to do so, and indeed French historians have themselves exported the approach beyond the boundaries of west Francia).³⁹ This is, arguably, accident: the original *Annales* idea of 'total history' has simply turned out to be more easy to realise by historians of the high middle ages than by historians of later periods in the time available for the production of theses. If this is so, it has been a very significant accident.

The positions and traditions of Italian and Spanish medievalists show great similarities. The tenth century is one of extreme localisation: meaningful generalisations about or general histories of the Italian or Spanish peninsulas are difficult, if not impossible. Moreover, the master narratives of Italian and Spanish historiography make the tenth century a period of marking time: waiting for the communes, or for the *reconquista*, and so looking for the antecedents of these things. The tenth century hardly works for either Italy or Spain as the end of an old or the beginning of a new era. Although it is possible to talk about the first half of the tenth century as one in which Italy was ruled by 'national' kings, this is only acceptable nowadays when accompanied by a heavy coating of inverted commas. Nor is the tenth century a significant one for Spanish self-perception. On the one hand, the crucial period for the survival of the kingdom of León-Asturias and its taking firm root was the ninth, not the tenth century. On the other hand, Spanish political geography was not definitively shaped until much later. Castile, which would ultimately play Wessex to most of the rest of the peninsula, was still an insecure border region in this period. There has also been much to do. Professional history-writing has not been so long established or so well funded as in the lands north of Alps and Pyrenees, and there is still an immense amount of positivist establish-the-facts spadework to be done for this period. It is significant, therefore, that Italian and Spanish historians have been heavily influenced in recent years by the concerns of French medievalists. Two large and highly influential studies, those of

³⁸ Duby (1952); most of the others are listed in Poly and Bournazel (1991), English translation, pp. 365–6.

³⁹ E.g. Toubert (1973a, 1973b); Bonnassie (1975, 1976); Taviani-Carozzi (1991); Menand (1993).

Pierre Toubert on Latium and of Pierre Bonnassie on Catalonia, have been particularly important in setting agendas.⁴⁰

As is explained in the preface, the present volume is ordered by reference to the tenth century's Carolingian past: the chapters on the 'post-Carolingian core' are grouped before those on what from this point of view was the periphery, though neither the Byzantines nor the Islamic rulers of Spain would have seen themselves in this light. But other groupings are possible: if the French, Italian and Spanish histories of this period appear highly regionalised and fragmented, German, English and eastern European histories appear much less so, though the reasons are different in each case. German medievalists have been little troubled by ideas of revolution, feudal or otherwise; for them the decisive break in European history comes in the second half of the eleventh century, with the end of Ottonian and Salian rule, church reform, crusades and the emergence of early scholasticism. Germany in the tenth century was as regionalised as France or Italy or England, but the master narrative for its history is still perceived as that of the history of kings. Although this has been rewritten in the last generation with considerable sophistication and surprising detail, it is still hardly linked at all to developments in social and economic history.⁴¹ The kinds of tenth-century developments which have impressed French, Italian and Spanish medievalists – fortified aristocratic residences, the growth of private jurisdiction, an increase in violence, the shift from slavery to serfdom – can also be registered in the German long tenth century, but they are not seen as having such significant consequences either for the course of events or for the development of the polity.

Such conservatism should not be taken to mean stasis. A generation ago the historiography of the German long tenth century did indeed not seem particularly lively. The sources were both well edited and of known limitations, and it was generally felt that, except perhaps for the ideology of rulership, where there was evidently still mileage in continuing the lines of investigation opened up by Schramm, Erdmann and Kantorowicz, there was little new to be said. If today that no longer seems true, then this is not because of major discoveries of source-material, or because the subject has received significant impulses from outside: the debates on periodisation and revolutions have hardly touched German historians at all. In retrospect, the shift can be seen to have been begun by Helmut Beumann's study of Widukind of Corvey;⁴² what this triggered off over the next forty years was an increased sense of the need to

⁴⁰ See note 39.

⁴¹ The largest recent survey, Fried (1994), goes further in attempting such an approach than any previous survey; see also Fried (1991). It may be a sign of change that Fried's neo-Lamprechtian approach was not challenged, though other aspects of his work were: see Althoff (1995) and Fried (1995).

⁴² Beumann (1950).

read the great works of Ottonian historiography in their own terms. An almost literary 'close reading' (though this owed little to literary scholarship and nothing at all to post-structuralist views of the world, which have affected German medievalists hardly at all) replaced what had become the increasingly desperate interwar attempts to unpack these texts in a purely positivist manner, to try to force them to reveal 'how it really was'. At the same time, our understanding of the nuts and bolts of the east Frankish/German kingdom was transformed by detailed prosopographic investigations and by meticulous reconstructions of the rulers' itineraries.⁴³

England in the long tenth century was clearly as regionalised a society as anything on the other side of the Channel. Indeed, it was in this period that England came into being as anything more than an aspiration and perhaps on occasions as a virtual community, and the process was not yet fully completed by the early eleventh century.⁴⁴ Yet its historiography firmly resists a regionalising perspective; it is not that no such perspective has been offered, but rather that there is no real place for it within the dominant discourse.⁴⁵ It might be thought that the main reason for this is the sheer paucity of source-material: the number of indisputably genuine tenth-century charters of all types from the whole of Anglo-Saxon England hardly exceeds the number of surviving genuine diplomata issued by Otto I alone, and is a mere fraction of the number surviving from the single if admittedly atypically rich archive of Cluny. The richly symbolic accounts of east or west Frankish politics found in contemporary narratives also have no surviving counterpart from Anglo-Saxon England. More significant, though, is the influence of a dominant master-narrative, one of English history as a success story made possible by the early development of a strong centralising state. Recent historiography has fought hard to push back the beginnings of this development beyond its traditional starting point in the generations following the Norman Conquest, and a plausible case can be (and has been) made for a 'Carolingian' phase of English history between Alfred and Edgar, one in which military success, unification, legislation and the development of what by early medieval standards was a fairly homogenous set of local institutions went hand in hand.⁴⁶ Yet where an older generation of historians saw England as first dragged kicking and screaming into Europe, and hence into modernity, as a result of the Norman Conquest, the new view has rewritten tenth- and eleventh-century English history at one level whilst preserving its isolation from continental developments at another. No kind of mutation or revolution, feudal or otherwise, troubles the island, nor apparently

⁴³ For the methodology and bibliography see Müller-Mertens, chapter 9 below; see also Fleckenstein (1966) and Leyser (1982b). ⁴⁴ Wormald (1994).

⁴⁵ For examples of regional studies see Stafford (1985), Gelling (1992), Yorke (1995).

⁴⁶ See Campbell (1994), for the fullest recent statement of the view.

do such things as the development of fortified residences or the freezing of previously fluid settlement patterns, which remain by and large the concern of archaeologists.⁴⁷

If the sources for English history in the long tenth century seem thin compared with the wealth of the Mediterranean regions or even the plenty of the former Frankish kingdoms north of the Alps, they are rich compared with those available for eastern and northern Europe. The histories of Rus' and of the eastern European proto-states, 'Poland', 'Bohemia' and 'Hungary', are probably the most contestable and contested of all those covered in this volume.⁴⁸ This is partly the inevitable product of fragmentary information, often late in date and highly ambiguous in its interpretation. But it is also, at least for eastern Europe, a product of twentieth-century uncertainties. The new states of the post-Versailles settlement have simply not enjoyed a continuous existence over the last eighty years, unthreatened from without and consensually accepted from within, and under such conditions it is not surprising that historians of these regions have been slow to take up the methodological novelties increasingly taken for granted further west. The histories of tenth-century Poland, Hungary or Russia are as difficult to 'read' as those of sixth-century Gaul or Britain – if anything, more so, since the written information we have is almost all external as well as being late. But they are not so distant in time and significance as are, for example, the sixth-century Saxon kingdoms in England; and interpretations of the fragmentary evidence are not as detached from present-day reality and significance as they are for western European historians, who inhabit societies whose sense of national identity does not require a consensual view of a very distant past.

There remain the anomalous (from a western European perspective) historiographical traditions of Byzantine history and European Islamic history.⁴⁹ Though Byzantine history has a particular significance for Greeks and Russians as the history of a 'virtual precursor', it is a more international discipline than any of the areas of 'national' history so far studied. At the same time, the high demands it makes on its scholars' linguistic and technical skills have a double effect: few of its specialists have had the time or energy to become genuinely familiar with the history of western Europe (or even a part of it) on the same level, while western medievalists have equally had to rely on others as guides (as has the author of this chapter). None of the trajectories which apply to the west really fit Byzantine history, for which the long tenth century between 886 and 1025 is as much a golden age as an age of iron, in recent interpretations not only politically and culturally, but also economically.

⁴⁷ Hodges (1991) offers an outsider's perspective on this.

⁴⁸ For organisational reasons, the history of the Scandinavian lands was covered in *NCMH* II; see the preface. ⁴⁹ See chapters 22–5 (Jonathan Shepard) and 27 (Hugh Kennedy) below.

Similar considerations apply to the histories of the Spanish caliphate and the Islamic amirs in Sicily in this period, except that here the problem is compounded by the fragmentary (and often late) nature of the source-material, and by the politico-cultural significance of these regions, peripheries of a larger culture whose metropolitan centre lay elsewhere. Nowhere in the area covered by this volume is cross-cultural comparison more needed or more difficult to carry out, from either side of the divide. In the present state of play, all that can be said is that few of the periodisations and interpretative schemata which have been applied to western Latin European history in the long tenth century seem to have much relevance to Byzantine or Islamic history in the same period, but that impression may nevertheless represent optical illusion rather than reality.

Some differences must have been real enough, however; the surviving sources and traditions of interpretation no doubt exaggerate the extent to which Byzantium (and its Bulgarian imitator) and Islamic Spain were societies centred on a capital with a fixed court and a ruler who was much more than *primus inter pares*, but no allowance one might make for this could reduce them to the organisational status of the societies shaped by western European itinerant rulership. Cultures which are urbanised and court-centred, whose rulers are normally to be found at a fixed point from which they habitually tax and legislate, are *inherently* different from those of the main area covered in this volume; in particular, the antithesis of core and periphery (or of metropolis and province) is a reality, not simply a metaphor.

The other anomalous historiographical tradition is that of American medievalists (as it happens, hardly represented in the present volume, though this is the result of chance rather than calculation). Their traditions have not always been clearly distinct from European ones; the first generations of American medievalists were largely trained in and inspired by European schools of historical writing, an intellectual dependency sustained in the mid-century era by the influence of a number of important émigrés and refugees, as elsewhere in the American academy. But although the European medieval past is also America's medieval past, it is not its past in the same way. The links with English history, and so, via the Anglo-Norman and Angevin empires, with French medieval history have continued to be important, but they are not the only possible ways of appropriating the past. For Americans whose secondary or primary ethnicity is eastern, central or southern European (there are very few African-American or Asian-American medieval historians), they are not even the most important ones. Moreover, the organisation of studies has favoured a holistic approach to this particular past culture, taking in literary and artistic remains as well as 'straight' history under the umbrella of Medieval Studies, and in consequence exposing medieval historians to the influences of

neighbouring disciplines in a way that is only beginning to happen in many parts of Europe. Although American medievalists have taken sides in European medievalists' debates – and they have shown themselves just as liable to Francocentrism as European historians – they have in many cases taken a more detached and also a more innovative approach to the medieval past, and a number of significant recent studies could probably only have been written from the distance provided by the Atlantic.⁵⁰

However fragmented the long tenth century may have been by the accidents of source preservation and divergent historiographical traditions, there are still generalisations which can be made about it, though, as we shall see, few are uncontested. Estimates of changes in the level of economic activity in the long tenth century have on the whole been moving upward in recent decades. Monetisation is perceived positively; the Viking, Saracen and Magyar incursors who caused Marc Bloch to depict the era in such gloomy terms are now thought by many to have given positive impulses by raiding centres of accumulated treasure and releasing it once more into economic circulation.⁵¹ Population is also thought to have risen, though hard evidence is almost impossible to come by. The beginnings of the urban renaissance which characterises the high middle ages have also been sought in this period.⁵² To the extent that there is or can be any 'pure' economic history of this period, there is probably more consensus about it at present than about any other aspect of the period.

Yet such developments are more easily described in a broad-brush sense than explained. When we move on to social and political history in search of explanations, consensus recedes. A number of other changes can apparently be identified as characteristic of this period, and historians have been tempted by the idea that many, perhaps even all of them can be linked in some way. There is, first of all, the idea (Marxian in origin, though less so in its exposition or its specific application to the tenth century) that the long tenth century saw a crucial shift away from slavery towards a serfdom which embraced not only slaves but also a good part of what had previously been a free peasantry.⁵³ Second, we have the view, already mentioned, that settlement patterns, previously fluid and shifting, solidified in this era. Linked with this we have, third, the spread of the 'private', small-scale and residential fortification, by contrast with the refuge fortifications of an earlier era, still being built and planned in the late ninth century.⁵⁴ Fourth, such centres of aristocratic

⁵⁰ Koziol (1992) and Geary (1994) are two examples; many more could be offered.

⁵¹ Duby (1974), pp. 118–19.

⁵² See Johaneck, chapter 3 below, and also Hodges and Hobley (1988) and Verhulst (1993, 1994).

⁵³ Bois (1989); Bonnassie (1991); see also, from rather different perspectives, Wickham (1984) and Müller-Mertens (1985).

⁵⁴ Fossier (1982), pp. 182–234; Toubert (1973a, 1973b); Böhme (1991a, 1991b).

domination were significant not only for the exercise of power but also for shifts in family consciousness. Noble families defined themselves less in terms of broad kindreds including relatives on both the male and the female side and more in terms of a male descent lineage; these lineages often took their names from the names of the fortifications which were the basis of their power.⁵⁵ Fifth, the lordship exercised from these centres was often of a new kind, based on pragmatic local dominance without much legitimation and certainly with little legitimation through 'public' office-holding. Rather, it came to replace an older 'public' order which had survived in many regions from the Carolingian era. This larger-scale public order was hollowed out to the point of extinction in many parts of Europe during the long tenth century; royal authority suffered earliest and worst, but it was followed into decline by the authority of intermediate powers (dukes, counts, earls, archbishops, bishops).⁵⁶ Sixth, what remained was in essence 'ties between man and man': legitimate authority had become privatised and personalised.⁵⁷ Linked with all these developments was a seventh: the emergence of a new and enlarged dominant class, a class which still had its own internal divisions but one in which lords and their warrior followers increasingly perceived themselves as members of a single group set apart from (and over) the rest of society; in the course of the eleventh century a separate ideology and initiation rites would be found for this class.⁵⁸

What all this adds up to is the totalising interpretation known as the 'feudal revolution' or 'feudal mutation'. It is a compelling view of the history of post-Carolingian Europe (or at least of the history of Europe's post-Carolingian core); and yet for all its attractions it is a highly problematic one. Even leaving aside those regions of northern and eastern Europe which were clearly following another developmental trajectory altogether (as were Byzantium and Islam, for quite different reasons), and in any case have not preserved the kind of evidence which would enable us to form a judgement, the model does not really seem to work for important parts of Europe: southern Italy, León, England, Germany. As suggested above, this may be in part the product of different historiographical traditions, though at least for England the model has been explicitly rejected as inappropriate.⁵⁹ It is in any case a gross oversimplification to call it 'the model': most historians working on this period would acknowledge the existence of at least some of the phenomena enumerated in the previous paragraph and feel tempted by the idea that these phenomena were in some way linked to one another, but, as already suggested, variations in empha-

⁵⁵ Reuter (1997a) provides a survey of the immense literature on this shift.

⁵⁶ The essence of the 'feudal mutation'; see Poly and Bournazel (1991).

⁵⁷ The phrase was placed at the centre of interpretation, if not actually invented, by Bloch (1961).

⁵⁸ Duby (1978); Flori (1979, 1983). ⁵⁹ Campbell (1990).

sis can produce considerable variations in the overarching interpretation which provides the explanation of how these links actually worked.

Moreover, many of the most significant elements of the model are currently under challenge, even for the core regions of west Francia (including Catalonia) and northern Italy, from which the model was derived. The challenges have intensified during the period between conception and publication of the present volume. The extent of slavery in the early middle ages, and the sense in which it was replaced in the long tenth century by serfdom, is highly contentious.⁶⁰ So too is what once seemed common ground, the replacement of public authority by personal ties, in other words 'feudalism'. It has been argued that feudalism, in the sense of a homogenous juridification of personal relationships amongst the European governing elites, was an invention of the twelfth century; fiefs and vassals, in this sense, were absent from the long tenth century, and there was in any case no necessary link between vassalage and benefice.⁶¹ It is still not clear whether we should think of a feudal revolution or mutation at all; though Europe in 1100 was clearly very different from the Europe of 800 or 900, not all would see the decades around the millennium as marking a clear period in which most of the transition took place.⁶² The consolidation of a small aristocracy and its warrior following into a single, wider class was a process which does seem to have occurred across most of Europe between the Carolingian era and the thirteenth century, but it was hardly a homogenous or simultaneous one.

There are difficulties of perception here: are we dealing with new phenomena, or merely with phenomena which began to be recorded more frequently towards the end of the long tenth century? As local complaints of violence and abuse increase, we are tempted to contrast them with an idealised Carolingian past which may well never have existed, and which would appear quite different to us were we to have as much information about its local look and feel as we do about much of the post-Carolingian core of Europe around the millennium.⁶³ Equally, the apparent fragmentation of large-scale political authority in many parts of Europe may indicate a new order, but at least at the regional level the polities of this period (notably the French, German and Italian principalities) were in most cases not arbitrary creations but had much older roots as vehicles of being and consciousness, often traceable back through the Carolingian era to the early middle ages. It is even conceivable that the smaller units of lordship which become clearly visible around the millennium had

⁶⁰ Verhulst (1991); Barthélemy (1993); see also the symposium of responses to Bois (1989) in *Médiévales* 21 (1991). ⁶¹ Reynolds (1994); for initial responses see Nortier (1996); Barthélemy (1997).

⁶² Bisson (1994), with responses by White (1996), Barthélemy (1996), Reuter (1997b), Wickham (1997) and a reply by Bisson (1997) See also the exchange between Barthélemy (1992a) and Poly and Bournazel (1994). ⁶³ White (1996), pp. 218–23; Reuter (1997b), pp. 178–87.

older roots, now lost to sight. Any attempt to offer a synthesis at this stage would be futile; as historians from different European traditions become more aware than they have been of each other's practices and findings, and as interest in the period around the first millennium finds at least a temporary increase in public and professional awareness from our contemplation of our own position in the decades around the second millennium, debate on these issues, which are also of central importance to historians of the periods preceding and following the long tenth century, is likely to intensify and to shift as it does so.

If we leave the awkward terrain of social and political history and turn to religious history, then we might at first think that the history of the church in this period would appear to be a good example of *encellulement*, at least at a purely institutional level. Ninth-century popes had commanded and occasionally threatened bishops; they had deposed or confirmed some of them in office; at least a few had been significant figures who could not easily be bypassed. But papal leadership of Christianity was far more muted in the period which followed. Ecclesiastics might journey to Rome on pilgrimage, but they mostly settled their own affairs. Neither the existence of a papal judgement nor the presence of a papal legate was necessarily bankable capital in the course of a dispute, and the privileges granted by popes were more than once in this period publicly repudiated. This was not so much a rejection of the pope *qua* pope as a reflection of a more general attitude which meant that the members of the higher ranks of the church hierarchy were largely insignificant except in their capacity as bishops. Councils were rare, and usually local affairs when they did meet: bishops were largely sovereign within their own dioceses, and were the crucial figures of the tenth-century church, as Rosamond McKitterick's chapter demonstrates.

Ecclesiastical *encellulement* was also visible, in a sense, in the history of monasticism in this period. Historians have been able to free themselves, slowly, from the notion that monastic reform in this period was spelled Cluny; but it has been more difficult to dispel ideas of monastic 'orders' projected back from the twelfth century and later. Yet even Cluny's collection of monasteries with varying ties of dependence on it was not an order in the later sense: the *ordo Cluniacensis* was, as Joachim Wollasch points out, Cluny's 'way of life', not a legally defined body. Other monastic groupings were still less institutionalised, depending as they generally did on the attentions of a reforming 'expert'. Yet the very existence of such 'experts', men like Gerard of Brogne or William of Volpiano, shows how *encellulement* was not all-determining. Even if such monastic families had a short-lived and tenuous existence, they could link and unite, however briefly, monasteries scattered over several dioceses, even kingdoms. The elite owners of monasteries, especially when these were bishops,