

The Historiography of Elites in Gaul

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This cannot be a comprehensive survey of the historiography on elites in Gaul. The subject is too vast. Furthermore, there already exist a host of historiographical surveys and summings up of scholarship on the aristocracy (to identify, for the moment, elites solely with the aristocracy) from the time of Guilhermoz to our own time (e.g., Le Jan 1995, pp.9-15; K. F. Werner, 1998; Reuter 1997 and Reuter 1979). There seems little point here in retracing the differing views in older debates such as that of H. Grahn-Hoek (1976) and F. Irsigler (1969), studies that themselves discuss previous historiography (brief summary in Geary 1988, pp.110-11). This is not to dismiss the relevance of older questions, which are real questions about power, nor to denigrate the value of older work. Who can study the Frankish aristocracy of the Carolingian era without still turning to, for example, the work of L. Levillain? So this is not a comprehensive survey. It is both selective and programmatic. I will concentrate on some recent work on the topic and outline problems and opportunities for future research. This seems appropriate for what is after all a new initiative, i.e. this project on the study of elites. I take the term 'elites' to signal a call for new approaches. It is a more inclusive term than 'aristocracy' or 'nobility'. It includes kings (and queens) as well as aristocrats. It includes clergy as well as laity. It also calls us to look below the level of the high aristocracy: how broad is an elite? Can it include what historians of Anglo-Saxon England would call 'the gentry'? How many elites are there? It is a 'relational' term: for there to be an elite, there have to be non-elites (*potens* and *pauper*), and the actual exercise of power and authority over the non-elite is a subject that the late Tim Reuter thought was insufficiently studied by historians of elites. (Reuter 2000)

Nor, in its limited space, can this survey do justice to the regional variation of Gaul, however we define that territory. I will take it to be, roughly, the Roman province that became the Merovingian and Carolingian kingdoms west of the Rhine. Regional studies have of course played a key role in shaping the historiographical landscape here; one need only mention Duby, Fossier, Lauranson-Rosaz to be reminded of the variety of achievement in this field. Nor is this tradition exhausted. The recent collection of papers, *La royauté et les élites dans l'Europe carolingienne* (1995) contained studies of elites in firm geographical contexts of Gaul such as in northern Neustria, Aquitaine, Burgundy etc.

In fact, important regional studies that are highly relevant to our topic continue to appear and I simply signal here two that add to the tradition of English-language scholarship's interest in this topic (for such interest, see, e.g., Smith 1992 on Brittany, Geary 1985 on Provence and Gerberding 1987 on Neustria, with Halsall 1995 on the Metz region with welcome emphasis on the archaeological evidence). First, John Nightingale has written *Monasteries and Patrons in the Gorze Reform: Lotharingia c.850-1000* (2001) This is a very detailed study of aristocratic patronage of monasteries in a specific landscape and, although its focus remains tightly confined to Lotharingia, it demonstrates clearly that the aristocracy pursued 'access to land and holiness' with equal vigour. It is thus an instructive study of the social history of monasticism and of what one might call the spiritual history of the aristocracy (a theme to which we shall return at the end of this paper) Further east, the Rhineland has been the subject of a more theoretically sophisticated study by Matthew Innes

(2000), a study that deserves careful attention for its integrating of a detailed study of a locality into a framework of larger questions of power and authority.

Both these books come from an English-language historiographical context while drawing on the work of continental historians such as M. Parisse and F. Staab. This does not mean that it is a simple matter to integrate differing historiographical approaches and traditions. Of course, national historiographical traditions remain important as the late Tim Reuter reminded us with some wittily thought-provoking titles of non-existent books (*Contestations et légitimations du pouvoir au bassin de la Tamise, VIIIe-XIe siècle*, and *Adelsherrschaft und Reformmönchtum in Aquitanien des 10. Jahrhunderts*). But international co-operation and the crossing of boundaries seem to be the order of the day. One need only think here of the project on the Transformation of the Roman World (though French contributors do not seem to be as visible in this as one might have hoped) or of the 1999 conference on Les transferts patrimoniaux en Europe occidentale. While the critical reception of Jean Durliat's work in Britain might appear to be Anglo-Saxon scepticism confronting French system-building, one should recall that Chris Wickham's review ('La chute de Rome n'aura pas lieu') was written in French for *Le Moyen Age* and that there have been other French-language reservations towards Durliat's ideas (Devroey 1985). Similarly, Susan Reynolds' robust attack on 'feudalism' has received nuanced notice in France. (Barthélemy 1997).

All this means that the historiography is currently eclectic and pluralistic in approach. There is no master method. No master thinker, not even Pierre Bourdieu, dominates the field. It is therefore difficult to isolate key themes and trends. Of course, it is possible to do so. One could, for example, identify and welcome recent work on gender which has brought genuinely new insights into the study of familial, social and political structures (e.g. Nelson; Le Jan; Gradowicz-Pancer; Stafford; NB also the forthcoming Transformation of the Roman World volume on gender, edited by L. Brubaker and J. Smith). Or one could welcome Guy Halsall's new book on early medieval warfare while lamenting the general lack of study of war. (Halsall 2003; Bachrach 2002 is idiosyncratic) Instead, this paper itself opts for variety and eclecticism. It is a highly selective view of some recent work across the Merovingian and Carolingian/early Capetian periods. Over this chronological span, it will look at, broadly speaking, three aspects or themes of current work: sources, structures and ideas/values. Obviously, all three are inter-related. In highlighting achievements and problems in recent scholarly work, it attempts to identify some pathways and agendas for our subject to move forward.

We begin, of course, with sources. In turning to sources, I refer to written sources, though we should bear in mind the relevance of archaeology and the study of material culture for the understanding of the nature of elites in Gaul. The grave of Childeric is a well-known example but recent surveys of palaces and work by Bonnie Effros on graves and social status are also highly relevant here. (Renoux and see, e.g. the catalogue, *Die Franken, Wegbereiter Europas*) Texts may be traditionally more central to the historian's vision and the picture here is a very rich one. We seem to be living in a particularly fertile age of textual scholarship and activity. In 2001 alone *Francia* reviewed A. Freeman's edition of the *Libri Carolini*, new editions of Ademar of Chabanne's Chronicle, of the *Life* of Odilo of Cluny, of a text by Hincmar as well as the facsimile of the Cartulary of Redon. Key texts such as Flodoard's History of the Church of Reims, Richer's Histories and Ansegisus' collection of capitularies have all flowed from the MGH. (Stratmann; Hoffmann; Schmitz) A new edition of Gregory of Tours' Histories is in preparation (Heinzelmann) and a new

edition of the Le Mans material has just appeared. (Weidemann) Major studies of authors and narrative texts have appeared (Heinzelmann; Sot) while study of Frankish capitularies has been put on a new footing by H. Mordek's magnificent work. On a slightly smaller scale, editions and translations of the *Gesta Abbatum Fontanellensium*, of the letters of Frothar of Toul and work by Jonas of Orleans now line up on the bookshelves along with English translations of Avitus of Vienne (Wood and Shanzer) and of some major sources for the later Merovingian period (Fouracre and Gerberding 1996). The annotation and introduction to these translations are of such good quality as almost to give them the status of editions.

What is the precise relevance of all this material to the study of elites? Two points about translation need to be made at the outset. First, we ourselves, as professional historians need to ensure that source material is made available to our students who are, at least in Great Britain, increasingly short of linguistic skills, i.e. Latin. We must avoid elitism on our own part if we are to attract future researchers (and the availability of Fouracre and Gerberding and Nelson's *Annals of St-Bertin* on CD-Rom is worth noting here). Secondly, we must remember that translations are necessarily partial, tendentious and far from neutral. Paul Fouracre, for example, has noted that the new *Chronique des abbés de Fontenelle* is flawed by its editor's 'unquestioning assumption of the fiscalist view of landholding, which sees in the term *villa* a fiscal unit'. (Fouracre 2001) Editions as well as translations reflect editorial choices, a point to which we shall return.

More generally, are the written (Latin) sources of Gaul in this period the products of an elite, for an elite? How can we best approach them as sources for the representation and reproduction of elites? We should not assume that all our sources were written by members of an undifferentiated elite. Richer of Reims was of a very different social status from, say, Avitus of Vienne. But while we need to distinguish texts and authors from each other, we should not erect artificial barriers of genre that inhibit our analysis. F. Lifshitz's attack on the scholarly construction of the category of hagiography is relevant here, as is the presence of narrative texts together with legal texts in manuscript transmission. (Mordek; Tremp; Tischler)

Here questions of literacy, of textual communities and of communication are important. (Mostert) Some examples may be illuminating here. The great struggles between Louis the Pious and his sons in 833-34 led a donor and an abbey in Brittany to hedge their bets in the dating of charters in December 833. (Smith 1992, pp.81-2) A stream of eighth-century charters from the abbey of Weissenburg in Alsace, in their relentless re-iteration of formulae of Carolingian authority, both reflected and created the aura of royalty for the new dynasty. (Airlie, forthcoming (a)) Literacy, access to documents, brought scribes, donors and their 'audience' into the presence of higher authority and its discourse as well as bringing them to the need to decide on the details of its political quarrels. We can think of this as the elites reaching down to local level or we can think of the literary activity of local communities as part of the activity of a local elite and the study of charters is particularly significant here, as exemplified in the important work on charters and dispute settlement and on charters and property and power by the Bucknell group (Davies and Fouracre 1986 and 1995; see also *Transfert des patrimoines*; see also McKitterick 1989 for a non-Gaulish example). Elites were differentiated and literacy, broadly defined, was a tool in differentiation and communication. Even royal charters did not stand untouchably outside textual networks, as the textual practices of chroniclers demonstrates (Morrelle); the production, reception and preservation of charters tells us much about conceptions of authority. (Heidecker, 2000)

While such recent work on literacy and communication relates to and builds on the traditional classic work of an older generation of scholarship (e.g. Bautier 1984 on Carolingian chancery), other recent developments in the study of texts present a more threatening aspect to traditional scholarship, indeed to historical studies generally. I refer here to 'post-modernism' or the 'linguistic turn'. How far ought we, as historians, to deploy the new strategies of literary criticism, strategies that may have far-reaching implications for our discipline as they insist on the text's status as linguistic object rather than its status as a source for a vanished past? The posing of such questions to historians often generates more heat than light and it would be a bold Scotsman indeed who dared to pose such questions in the city of Paris. All I intend to do here is to draw your attention to the brief but balanced discussion of this problem, and how it relates to the study of Merovingian hagiography, in Fouracre and Gerberding (1996) and to a slightly more extended discussion by Walter Pohl (2001, on Italian material but his methodological remarks are generally relevant; see also Spiegel).

Fouracre and Gerberding (1996, pp.39-41; see also Fouracre 1990) clearly outline the benefits of a linguistic approach as 'the technique of 'deconstruction' aims to lay bare a 'sub-text' which reveals a more realistic level of social thought'. But with equal clarity they point out the inappropriateness of such an approach for many of our sources, not least because 'we are still generally at the stage where we are thinking about [Merovingian texts] provenance rather than their literary construction'. The question of context also remains of primary importance for the historian here. Even scholars who are sympathetic to literary and social theory have insisted that such theories cannot simply undercut traditional concerns of historians. Walter Pohl has reminded early medievalists of the validity of Michel Foucault's views that 'language does not simply reflect the world as it is; it is a medium of its construction... the old model of the text as a simple mirror of objective reality has been superseded' (Pohl 2003, p.2, and p.13 on power and knowledge). This, however, should be seen as offering us new ways to study the representation and reproduction of elites, rather than as a threat to the study of history.

Paradoxically, such new approaches to texts include the very traditional skills of manuscript scholarship, a 'return to the manuscripts' as Walter Pohl has put it. Here, older editorial principles that enshrined a 'fixed' edition have been abandoned or refined as scholars become ever more aware that variant readings and differing recensions of texts have their own meaning. Detailed study of the manuscript versions of, for example, the *Histories* of Gregory of Tours has revealed the popularity of the six-book edition and how this is to be associated with the 'new northern cultural and political centres of the [Frankish] realm' of c.700. Studies in the manuscript tradition of Gregory's text become studies in the appropriating and refashioning of that text to meet the changing needs of new elites. Such changes also cast light on what is distinctive about Gregory's own text in his own time and thus give us a much sharper sense of Gregory's individuality and the social meaning of his text in the sixth century. (Pohl 2001, p.349; Reimitz, 2003, p.259-60; Heinzelmann, 1994; and cf Reimitz 2000)

Amidst these new developments in textual studies, traditional skills thus have a key role. In fact, I would like to end this section on texts by drawing your attention to a very traditional study of a specific text. Hartmut Hoffmann's study of the *Histories* of Richer of Reims accompanies his new edition of that text for the MGH. (Hoffmann 1998 and 2000) Hoffmann takes a robustly sceptical view of postmodern literary critics' intrusion into medieval textual studies. His first footnote is a snort of

disapproval directed at Hayden White ('fehlerhaft und scheid'). But Hoffmann's views of Richer are not simply the views of an old-fashioned positivist (for such views on Richer, one can turn to the late C.R. Brühl, also rebuked by Hoffmann). Hoffmann takes great care to situate Richer in a precise historical context, arguing, for example, that Richer's views of the west Frankish kings as rulers over Lotharingia stem from the fact that contemporaries did not know the future destiny of that region and so Richer cannot simply be branded as a fantasist. But Hoffmann does not merely establish a context, he interrogates it. We should not assume that Richer's views on, say, the political contests of Carolingians, Capetians and Ottonians matched Gerbert's simply because the text was dedicated to Gerbert and Hoffmann reminds us that Richer himself was no grand seigneur. In other words, a context for a text is not in itself an adequate explanation or analysis of that text. There remains no substitute for careful reading of the text itself and Hoffmann provides an exemplary reading of the text. It is exemplary because it pays full attention to literary genre and conventions (e.g., the set speeches that Richer, in true classical style, gives to the actors in that text) while refusing to assume that the text is simply always a reflection of Richer's own views. It thus becomes difficult to see Richer's text as being written to legitimate the Capetians. Hoffmann stresses the inconsistencies to be found within the text and stresses the sheer complexity for contemporaries of the issue of Capetian legitimacy in the face of Carolingian claims (Charles of Lorraine). There was thus no unproblematic notion of 'legitimacy' that Richer could simply reflect and broadcast (the fact that Richer may not have given his text a final revision could also be relevant here). All this does not just pay Richer the compliment of taking him seriously as an author. It raises rather disturbing issues concerning the autonomy of texts as Hoffmann concludes that Richer was not a representative author of his time though he can only be understood in the context of his time. In some ways this echoes Ian Wood's recent provocative comment on Gregory of Tours: 'He is too much of an individual to be a reliable guide to the norms of the sixth century.' (Wood 2002) Hoffmann's fine reading of Richer casts light on that text but raises more questions than answers about the meaning of such narrative texts; it is difficult for historians to get the balance between context and text right. To repeat, establishing a context of a text is not to explain that text. Historians' tendency to find (impose?) patterns in texts can be a *déformation professionnelle*.

We turn now from texts to chronology. First, the Merovingian period. As for the old question of the survival of the Gallo-Roman world in the face of Germanic invasions, here consensus has been reached. The views of Fustel de Coulanges and his less well-known predecessor, J.-N. Dubos, have triumphed. The Gallo-Roman elites survived and flourished. Further, there were no 'great invasions'. Roman and 'barbarian' elites came together to form a new elite that became Frankish. To speak of a Frankish conquest of Gaul was therefore to make an error and K.F. Werner has traced its historiographical progress. As he has put it, Clovis was 'un roi, ou plutôt une dynastie, qui a pris le pouvoir dans un royaume qui existait déjà'. (Werner 1996, p.15) These views should not be seen as simply assuming that Roman institutions continued to exist without changing or that the establishing of Gothic, Frankish or Burgundian kings in Gaul took place without conflict. For example, Ian Wood's 1994 survey of the Merovingian kingdoms does not take the Roman background as a static or unproblematic given. It sensitively surveys important political changes (e.g. the disappearance of a 'Roman' army in the fifth century) and charts the emergence of new attitudes among Romans to central authority (e.g. Arvandus). Even where Wood stresses cultural continuity, he remains alert to what is new in the resulting culture as

seen, for example, in his ability to show the continuities between the literary culture of Sidonius and of Gregory of Tours while simultaneously highlighting what was new and distinctive about Gregory, namely his activity as a historian. Such nuanced views can be seen in other areas. B. Rosenwein's 1999 study of immunities pays due heed to Roman legal concepts of immunity but goes on to show, with help of insights derived from anthropology, that the development of immunities in the Merovingian period is due to the political creativity of Merovingian kings. In studies of towns as centres of political, cultural and political power, awareness of the survival of a town-based episcopate is balanced by awareness of the fate of urban fabric and scholars remain sensitive to the importance of changing chronological and geographical contexts. (Loseby 1998; Gauthier 2002; Wood 2001) In fact, awareness of the variables of geography and chronology may be sharper in studies of the elites of the Merovingian world than in studies of its Carolingian successor. It is in such studies of Merovingian Gaul that we see a transformation of the Roman world.

There may be, however, some problems with this sort of view. Problems of evidence are paramount. Paul Fouracre (2000) has recently reminded us that our generalisations about a Frankish aristocracy coming from a 'synthesis' of Gallo-Romans and Germanic incomers apply to the upper elite, but are harder to prove for the 'lesser nobility' simply because we know so little about the latter. Indeed the problem of sources, e.g. the lack of substantial amounts of charter material for the sixth and seventh centuries, means that there are sharp limits to our knowledge of the broader elites of this period. But there may also be problems at the conceptual level. New thinking on ethnicity seems to have resolved some problems by simply abolishing them. The 'Germanic' presence is disappearing from the historiographical landscape. This is partly due to the work of Walter Goffart who, in a series of studies marked by intellectual elegance and great erudition, has 'deconstructed' the concepts of Germanic invasions and of a common 'Germanic' culture and scaled the barbarians down to size. As P. Geary's striking formulation has it: 'The Germanic world was perhaps the greatest and most enduring creation of Roman political and military genius.' (Geary 1988, p.vi) It is also due to new thinking about ethnicity in social and cultural terms which holds, in another striking phrase of Geary's, that 'ethnicity is a situational construct'.

Such thinking is characteristic of the contributors to the *Transformation of the Roman World* volumes, though it has not penetrated all corners of the scholarly world. Frans Theuvs has noted disapprovingly that outmoded conceptions of the end of the empire and of a fixed Germanic culture still exert a baneful influence in German and French archaeology. (Theuvs 2000, p.2) It is surprising to see that in a book published as recently as 1999, Georg Scheibelreiter can still assume that a division between firm and homogeneously Roman and Germanic attitudes is a helpful scholarly tool for the understanding of the history of office-holding: 'Ein kennzeichnender Unterschied zwischen antiker Staatsauffassung und germanischer Gesellschaftsordnung lässt sich im Bereich zur Tätigkeit feststellen, die über das Einzelinteresse hinaus für viele Bedeutung hat...: das Amt'. (Scheibelreiter 1999, p.84)

But the new scholarship's reaction to such older certainties may go too far if the result is a denial that there was any 'barbarian' culture at all. There is a paradox at the heart of the new thinking, one summed up in Walter Pohl's view that, as the empire dissolved, 'ethnic discourse became the key to political power' but that ethnic identity was itself fluid, with no fixed markers. (Pohl 1998) Merovingian Gaul in this perspective looks a lot less Frankish than it used to do. In a previous generation, feud

played a central part in Merovingian studies. (Wallace-Hadrill 1962) In stark contrast, Ian Wood's *Merovingian Kingdoms* has no index entry for 'feud' and indeed Wood reclassifies some conflicts traditionally seen as feud as essentially political conflicts. (e.g., p. 127; contrast S.White 1996) But recent work on law has re-iterated the traditional position of a Wallace-Hadrill: barbarians did have 'identifiable customs and the 'Germanic origin of feud-centred law' cannot be denied. (Wormald 1999, p.39) In this context, Wormald's work on law can be related to a recent study of violence by Nira Gradowicz-Pancer whose starting-point is the *Pactus legis salicae* because it 'reflects a ... type of society in which social relations are embedded in violence and retaliation cycles'. All this does not mean that Merovingian society was uniquely violent (though Gradowicz-Panzer claims that there was something distinctive about the violence she has studied, namely female violence in a very specific context) nor that scholars of the stature of Wood have denied that feud existed. (see Wood 1998, and discussion) Nor am I advocating that we should return to the older conceptualising of barbarian identity as fixed and universal (but see now Heather 1996 on Goths; and see Pohl 2002, for subtle overview). But the drive to 'de-Germanize' cultures such as those of Merovingian Gaul may come to hinder our understanding of some of the institutions of that society.

If we turn to the Carolingian period, recent developments give us the chance to review the role of prosopography in the historiography of elites in Gaul. Traditionally, prosopography has been an important tool in the study of elites and Gaul is well served here across the Late Roman, Merovingian and Carolingian periods (e.g., Stroehker, Ebling, Hennebicque-Le Jan 1989; there is a prosopography of the Merovingians themselves in Wood 1994, and there is some valuable information in Settapani 1993; see also Heinzelmann 1976). But the most distinctive prosopographical study is that by P. Depreux on the entourage of Louis the Pious. Its distinctiveness lies in its tight chronological focus (781-840), which is shorter than the other works cited, in its fullness of coverage, thanks to the rich sources of the period, and above all in its explicit discussion of criteria for inclusion, a discussion that gives it great methodological and historiographical importance. It is more (and less) than a who's who for the reign of Louis. Although Depreux has expanded it and built on it, e.g., to produce an expanded entry on Matfrid of Orleans (Depreux 1994) and, like other scholars of the period, has drawn on it as a source of information on individuals and on office-holders as a group, the peculiar value of his work lies in the ensemble, not merely the individual entries.

Depreux's book appeared at almost the same time as R. Le Jan's *Famille et Parenté* but it renounces any concern with family and would thus seem to offer a view and approach diametrically opposed to Le Jan's. Both works, however, can be seen as re-acting against the explanatory force of a rather undifferentiated view of the importance of broad aristocratic kins, a view that one can characterise as that of the Freiburg school. (Airlie 1995) Further, both works are very concerned with power and above all with the interaction of the aristocracy with royal power. The importance of Le Jan's work lies in its study of this in terms of structures, above all in the structures of kinship. The importance of Depreux's work lies in its screening out of such structures from its picture. What may seem to be a weakness in Depreux's approach is actually a strength.

The distinctiveness of his approach should be emphasised. The book is not a comprehensive list of all holders of high office in Louis' reign; neither Agobard of Lyon nor Jesse of Amiens appear in it. It pays little or no heed to the family connections of such magnates as Bego, connections that played a part in his becoming

count of Paris. In concentrating only on figures who can be seen to be active in the emperor's entourage and only on a limited part of the activities of such men (and it is overwhelmingly a survey of men), Depreux explicitly omits much of importance. But it is precisely here that the importance of his work lies. The relentless accumulation of evidence on these men's activities as royal and imperial agents, their journeys on imperial business, their transmitting of messages, their attendance at court, their services in wars, in short, their performing of their office, all this provides an exceptionally clear picture of a state elite. It has proved possible, for example, to follow the careers of members of Louis' entourage from his years as ruler of Aquitaine in the light of questions on the nature, duration, location and representation of their activities (see Airlie, forthcoming (b)). One might say of Depreux's book what Adorno said of Wagner's music: orchestration creates form.

It is here that Depreux's work makes an instructive contrast with Matthew Innes' regional survey of the Rhineland. Innes' focus on regional elites' inter-action with kings leads him to conclude that 'at the level of local power, royal and aristocratic interests were not distinct, but inter-related in a complex and evolving relationship'(p.259) while his careful consideration of concepts such as 'the public' and 'the private' leads him to note the absence of a 'dedicated governmental infrastructure' (p.258) But Depreux's survey of men in royal service shows that the fact of that service was itself an infrastructure, or at least an institution in perpetual process through performance. Such agents of Louis the Pious were not a bureaucracy in the modern sense of the term and both Depreux and Innes would surely agree on that point. Further, the reign of Louis the Pious may be a special case, a privileged moment in the articulation of the *res publica*. But Depreux's survey of men regularly engaged in royal and imperial service, together with his view of the institutional role of the palace, show something of the distinctive nature of royal authority and power in Louis' reign. In Innes' book royal authority tends to dissolve in negotiations with local networks and elites, but that royal authority acquires a sharper profile in Depreux's study. Depreux's entries on individuals are not isolated units, they add up to a greater whole. His book should not simply be used as a resource for information on individuals; rather it should be seen as presenting, in its totality, a particular view of royal government of the Carolingian era, at least for the first part of the ninth century.

Of course, no work of prosopography is entirely neutral or fully objective. All such works ultimately rest on assumptions about criteria for inclusion and identification, as well as being subject to the constraints imposed by the nature of available source material. We might therefore pause before responding to Janet Nelson's call (in a review welcoming Depreux's book) for prosopographical treatment of other Carolingian reigns. Of course, such studies would be welcome, but Depreux himself has said that a single reign provides a very limited chronological frame for prosopographical studies. Questions on the importance of family claims for office-holding and on continuity and change in the nature of the functions of offices require a longer time span. (Depreux, p.5, p.12) This does not mean that we should revive the project for a *Prosopographia Regnorum Orbis Latini* (Werner 1977). But rather than simply setting out to repeat Depreux's work for other Carolingian reigns, we need to reflect again on questions on the nature and usefulness of prosopographical studies.

The sharper profile of royal authority and aristocratic service that one finds in Depreux's work is, perhaps surprisingly, also to be found in R. Le Jan's work. Current scholarship lays great stress on consensus and partnership between Carolingians and the aristocracy. As R. Le Jan herself has summed it up: 'L'idéologie des VIIIe et IXe

siècles est une idéologie du consensus qui associe la royauté et la noblesse dans un même exercice du pouvoir.’ (Le Jan 2001, p. 191; cf Airlie 1995; McKitterick in Hen and Innes) This scholarship does not lose sight of the real power and authority of Carolingian kings, above all in the period from Charlemagne to Charles the Bald, nor of how the partnership changed in the tenth century. (e.g. Nelson 1995; Nelson 1999) Nonetheless, one of the merits of Le Jan’s *Famille et pouvoir* is that it highlights the distinctiveness of the Carolingian royal family itself. It strove for ‘la hiérarchisation’ (which benefited favoured aristocratic families). But it strove to make itself special. The withdrawal of Carolingians from the network of marriage alliances that marked their early history; their cultivating of genealogies before the aristocracy did so; their use of names as badge of family identity in more precise ways than the aristocracy; all this emerges from Le Jan’s book and reveals some of the all-important distinctiveness of the royal family, which was not just an aristocratic family writ large. This point needs stressing as the current widespread emphasis on consensus and co-operation in the Carolingian world may be blunting our perception of the very real nature of the distinctive authority of the Carolingian royal family itself.

Of course, this family presided over distinctive structures and here other work by Le Jan is relevant. Her studies of justice in the Carolingian world and of the Lotharingian aristocracy around 900 highlight particular problems of violence and a crisis in ‘public’ structures in the late ninth and early tenth century. (Le Jan 2001, ch10, ch.13) (It should be noted that neither Le Jan nor other scholars who have looked at, say, law and justice in the Carolingian world are guilty of idealising it and there is general agreement on the importance of collective judgements and general scepticism as to the reality of ‘state power’, but there is also agreement on the importance of the rhetoric of public power as itself having historical force in this period. (In general, Davies and Fouracre vols; Settimane on justice; Wormald on law; Martindale 1995)) These later problems cast retrospective light back on the distinctiveness of Carolingian authority in the earlier period. In other words, the distinctiveness of the history of elites in the Carolingian period can emerge much more clearly if that period is studied by being compared and contrasted with what came after it.

Part of the distinctiveness of the Carolingian world was the exclusive claim to rule by members of a single family, the Carolingians. That family worked hard to make its exclusivity of rule appear to be a natural part of the political landscape. The Carolingian royal family was itself a key ‘public’ institution. Challenges to that family’s exclusiveness in 879 and 888 were thus part of a real crisis in legitimation. (Airlie 1995; Airlie 2000) The political problems of that family after 888 are thus not simply part of political history; they are part of the broader changes, in, e.g. justice and violence as seen above. Study of the system in crisis casts light on the distinctiveness of the system when it was operating at its height. Study of the Carolingian systems thus has to be undertaken in a fairly long perspective that should include the ninth century (and perhaps the eighth) as well as the tenth. One of the many problems about the debate on l’an Mil (see, eg, the Past and Present debate between Bisson, Barthelemy, White, Reuter, Wickham) has been that scholars looking at ‘anarchy’ or violence around 1000 contrast what they find then with a ‘generalized Carolingian past’ (Reuter 1997, p.193) while in fact ‘a full analysis of the local power of Carolingian aristocrats remains to be written’ (Wickham 1997, p.203, n.12).

More work is in fact needed, not on violence, ‘anarchy’ and new forms of lordship around 1000 but on such topics in the Carolingian period itself, above all from c.850 to, say, 987. This means that several historiographical boundaries have to be crossed

and there may need to be much more co-operation between individual historians. The study of east and west Francia can only profit from being undertaken as a common subject. Of course, this does not homogenization; the problem of 'la dislocation du pagus' is not a problem for historians of Germany. This point emerges clearly from C.R. Brühl's monumental 1990 survey, though its positivist approach is limiting. G. Koziol's work on ritual and pacts should be set beside G. Althoff's work on *Amicitia*, as well as his work on rituals. The study of principalities could benefit from acquaintance with M. Becher's work on the origins of the Saxon duchy. In a recent article on kings and the aristocracy in the Ottonian period, the late Tim Reuter stressed that what really counted for the shift/decline in royal power ('das Königtum') was not the political weakness of individual kings but the disappearance of structures of regnal collectivity: 'Vor allem Hoftage und sonstige Formen der regalen Selbstdarstellung und -findung fanden immer seltener statt'. This is, as Reuter pointed out, of more than Ottonian relevance. (Reuter 2001 (a), p. 128, itself a model of a historiographically wide-ranging study; see also Reuter 2001 (b)) Here, we do need to cross national historiographical boundaries. But we also need to cross chronological ones. Studies of the tenth century routinely focus on problems faced by 'public' authority, the importance of local lordship, castles, violence etc We can fruitfully 'problematise' the ninth century by asking 'tenth-century' questions of it. But this should help us grasp what is distinctive about the high era of Carolingian power and thus understand better its successes and ultimate change.

It is tempting to locate the study of Gerald of Aurillac in this context of changes in Carolingian structures, but this in fact fits better into our final section. This section is concerned with Christian values and the elite, and it will argue that there was more to the former than simply underpinning the status of the latter. There has been good work on the cults of saints and the aristocracy. One could crudely summarise its findings by charting a three-stage development: first, a Late Antique and Merovingian Gaul where saints' cults were local and their claims were asserted and contested within the elite and between the elite and local communities (P. Brown, 1977/1982); secondly, a Carolingian world where the Carolingians managed to bring cults under their own control and regulation (Fouracre 1999); and finally, the Gaul of the Peace of God, a pluralist world where some members of the elite sought to use the saints to discipline unruly members of the elite in the name of a wider community. (Head and Landes 1992) Such work shows the importance of placing 'the issue of sanctity in the broader perspective of social power'. (Fouracre 1999, p.165)

But we ought to consider more generally the issue of the Christian values of the elite. A host of studies have confirmed how the relentless calls of churchmen for Christian order and correction to be imposed on society strengthened kings and how such calls resounded often and for long. (Nelson; Sassier) It may now, however, be time to pay more attention to the tensions generated by such calls. This is not to suggest that there was a division between 'church' and 'state' in, for example, the Carolingian era. But did Christian values in this period simply legitimate power? Here, debates and divisions within the elite are instructive subjects of study. Such divisions could be simply political as in, for example, the series of trials or persecutions of bishops by kings for infidelity, a series that includes Praetextatus of Rouen, Hincmar of Laon (NB the forthcoming MGH volume by R. Schieffer) and Arnulf of Rheims (V. Huth). Examination of such cases across the period, as well as of the fate of political 'martyrs' such as Leudegar of Autun, could cast light on the problems in structures of Christian office-holding.

More broadly, we should remember, with G. Koziol, that ‘Beneath all these layers of [social and political] meaning, there was a simple fact of Christian soteriology: all of the faithful were Christians, even those who ruled Christians’. (Koziol, 1992, p.99) An important article by M. Heinzelmann (1997) conceptualises this problem in historical terms by looking at the implications of Augustine’s ideas on the *societas sanctorum* for the formation of a Christian aristocracy. Careful reading of Gregory of Tours leads him to conclude that such a formation was not straightforward and that the force of Augustinian tradition and developments in asceticism led to, not ‘der Adel’, but ‘der adlige, heilige Bischof’ becoming the leading figure of such a *societas*. (p.255 and cf pp217-18 on Three Orders) Henizelmann here opens broad perspectives. In this sort of perspective such later developments as the writing of ‘mirrors’ for aristocrats such as Wido and Matfrid and by an aristocrat such as Dhuoda acquire extra significance. Christian values did not always sit easily with the exercise of power and Janet Nelson has fruitfully explored some of the tensions generated ‘when a warrior aristocracy became a Christian elite’. (Nelson, 1999) It is in this perspective that the work by Anne-Marie Bultot-Verleysen (2000) on the ‘dossier’ of Gerald of Aurillac becomes very relevant for us as the figure of Gerald is particularly good for us to ‘think with’ here. With the editing of texts dealing with clashing values in a post-Carolingian world, this paper has now come full circle in its survey of sources, structures and values/ideas.