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Topographies of power: towns and elites in Merovingian northern Gaul, 450-650

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Conclusion

The question asked at the beginning of this book was to what extent the rural inhabitants of northern Gaul had their surplus extracted by the elite, be it king, bishop or aristocrat. The hypothesis formulated by the *Rural Riches* project had been that the peasantry of northern Gaul had been relatively free of elite control, allowing them to keep most of their economic surplus for themselves. This would have enabled them to save wealth for what anthropologist Eric Wolf has called the ‘ceremonial fund’, a reserve needed for important life cycle rituals such as birth, wedding and funeral.¹ The project assumes that the thousands of well-furnished graves from the fifth to seventh centuries found by archaeologists all over northern Gaul are evidence of this ceremonial fund, and thus that the average rural dweller had access to wide-ranging economic networks in which they could partake without overbearing elite control or extraction.

For various reasons, it has been hard to answer the research question directly. We lack a substantial body of evidence on issues such as estate management and tax registers that could help us provide more straightforward answers. Instead, I have tried to answer the question in a more roundabout way by considering in detail what I have called the social and geographical topographies of power. That is, where was power, broadly conceived, located in society and the physical landscape? Here, too, the evidence has been sparse and ambiguous, but at least sufficient to map the possible and the probable, to sketch various models, or at the very least, to reveal the limits of the unknown.

The first part of this monograph focused on the social topography of power, that is to say, to what extent and in what way society was hierarchically configured. Chapter two has looked at what limited evidence there is for elites in the graveyard and the village. After all, the status quo idea of early medieval elites has it that they were highly conspicuous in the burial record. Similarly, one expects elites to have conspicuous dwellings, or at least to have vast estates from which they could finance a comfortable aristocratic lifestyle. This chapter has called these hypotheses into question. First of all, I have argued that lavish burials cannot be easily equated to high-status persons graves, following theoretical issues against the traditional paradigm raised among others by Härke, Halsall, Brather and Theuws. That is to say, a grave need not be lavishly furnished because the person interred was powerful and wealthy, if (s)he was at all. My colleague Femke Lippok has shown in her research that the Merovingian burial ritual is highly heterogeneous and too diverse to be explained by monocausal explanations and metanarratives derived from the written texts.² The point here is that the equation of (burial) wealth with power is one underwritten by

¹ Wolf 1996, 4-10.

² Lippok 2020, Lippok *forthcoming*.

modern assumptions about value, personhood and the burial rite, which cannot and should not in a straightforward way be transposed onto the early medieval funeral. As a consequence, the archaeological dataset that has hitherto been assumed to offer unproblematic evidence of elite presence can now no longer be regarded in this way. This would not be a problem, were it not that the Merovingian elites are similarly hard to find 'above ground'. Their dwellings, palaces, villas or otherwise hitherto escape archaeological notice. I have covered a few possible exceptions, but even these are often unconvincing and in any case, hardly as magnificent as the *palatia* described by the written sources. Finally, the evidence for landownership is extremely limited. The one case of Bertram le Mans, a true Merovingian millionaire, does not prove the ubiquity of huge landed estates on which toiled a tied peasantry, as is sometimes assumed. Rather, I have seen the limited portfolios of Remigius of Reims in the sixth century and Adalgisl Grimo in the seventh as more representative of elite landed property. Wealthy, but humble compared to wealthy elites in other historical periods, including our own. The 'typical' Merovingian-period settlement, on the other hand, seems to lack clear signs of elite habitation. Thus, as a first conclusion, it can be said that the material and written record paints no clear picture of an omnipresent and omnipotent aristocracy. Instead, the Merovingian elite is conspicuously absent in our evidence. This may be an artefact of the quality of our datasets, but equally, it forces the scholar to reconsider how exactly we ought to conceptualise Merovingian social structure.

This is exactly what my goal was in chapter two. I have argued that scholars tend to view the Merovingian period through their own glasses (as is, by the way, inevitable), colouring the way the archaeology and written sources are read. I have tried to simplify the variety of scholarly perspectives into four models of conceptualising the early medieval world, which I have called the feudal, heroic, bureaucratic and communal concepts of society. The first sees Merovingian society through the lens of the later Middle Ages or Early Modern period, as one dominated by an aristocracy of birth as typical of the *ancien regime*. The second sees a "Dark Age" society of warriors and feasts as lifted straight out of the pages of Beowulf, often with a pan-Germanicising approach that combines written evidence from across a millennium to create the mirage of a "Germanic" way of life. The opposite can be found in the "bureaucratic" concept, which tends to look for continuities with the bureaucratic, administrative realities of the Late Roman world. The fourth model is that of the communal concept of society, which is partially based on nineteenth-century romantic and/or anarchist perspectives on early medieval society as a free society. I have argued that this last concept need not be as far-fetched as is often thought, since there are various examples of medieval and non-medieval societies with relatively flat societal structures which may be compared to Merovingian societies.

In the end, none of these concepts provide the one true "lense" through which Merovingian Gaul can or should be viewed. Rather, a variety of scholarly perspectives may

reveal that Merovingian society was diverse and composed of a mosaic of interlocking heterarchic social realities. Local village life, especially far away from centres of power, may have been organised in a *relatively* horizontal manner, with status differences (between adult males, in any case) being small, informal and/or fluid, aligning with the communal concept. I have used Angelbeck's and Grier's metaphor of the upside-down pear; the bottom rungs of society may have been filled with undesirables, criminals and slaves, but the majority of local society could have consisted of "good people"; free men³ who participated in public village life. Simultaneously, the remnants of a Late Roman bureaucratic machinery may have co-existed at a state level and only interfaced loosely with local village life. The "good people" of the village served as the recruiting base for court and military officials, and free men could thus step over from the local pear to the state pyramid (figure 7.1), but the pyramid did not loom large over local life, by and large, at least for northern Gaul in the fifth and sixth centuries. The heroic concept can throw some light on how, especially within a military context, men could gain prestige, wealth and influence with leaders such as *duces* and *reges*,⁴ and the feudal concept helps discuss landownership patterns, especially of the old Gallo-Roman "aristocracy", but overall I have deemed the communal and bureaucratic lenses to offer the most promising fresh insights for future research.

³ Although whether only men were able to stake out a position in public life deserves its own study. I assume here that age and gender were important markers in social hierarchy, perhaps more relevant in daily life than those based on wealth or birth. To what extent and in what ways local life was patriarchal, and what *spielraum* was afforded to women should be subject of future research. Certainly, the cases of powerful queens such as Brunhild and Fredegund reveal that Merovingian could and did have public agency at times and the lives of peasant women was certainly better than of their predecessors in antiquity; Wemple 1981 189-198.

⁴ Sarti 2020, 266.

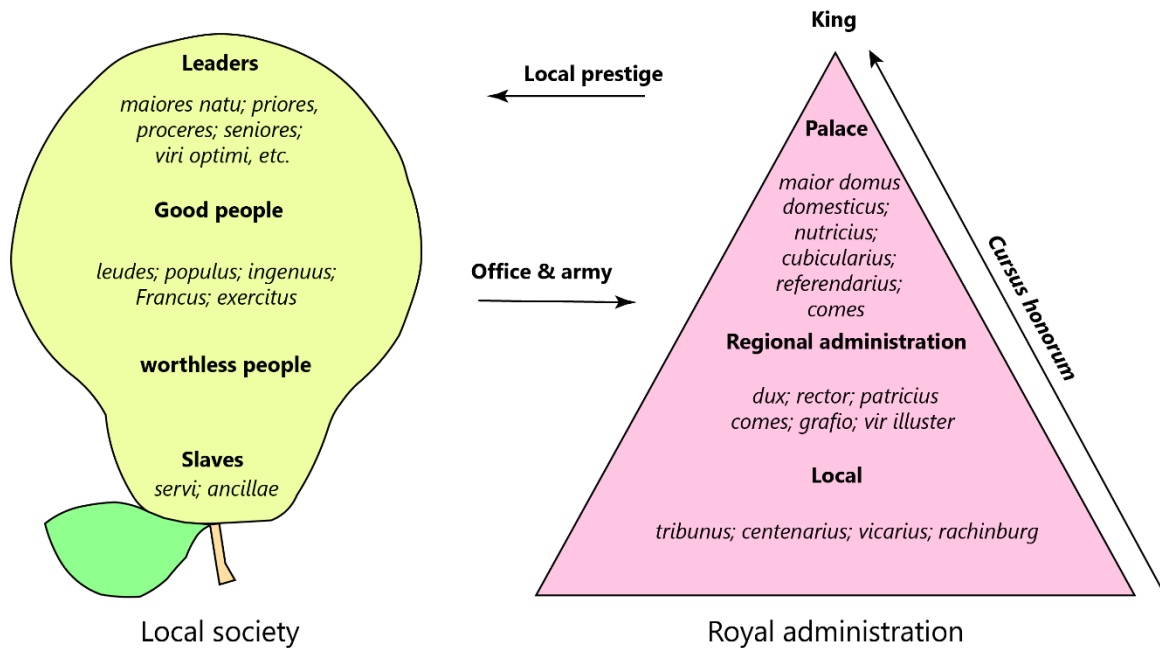


Figure 7.1. A conceptual diagram of what a Merovingian heterarchic society could look like; an intersection of a bottom-up pear on the local scale with the traditional hierarchical pyramidal structure of social organisation at the state level is conceivable. Archaeologists will, by virtue of their evidence, encounter the pear-shaped society in their research more than they will the pyramid; vice versa historians reading the written sources produced by the most powerful “one percent” will primarily find a pyramidal society.

The next part of this book looked at the physical topography of power, first by asking where the Merovingian kings and court moved through the Gallic realm. It was first hypothesised that for northern Gaul kings and courtiers were relatively absent, therefore allowing the peasantry to prosper. This hypothesis has been partially disproven, in the sense that the *espace royale*, the area in which the Merovingian kings routinely itinerated from (capital) city to (capital) city, clearly covered the southern half of the research area. Roughly, a line can be drawn from Cologne to the coast at Boulogne-sur-Mer, also known in modern scholarship as the “Via Belgica”, south of which the Merovingian can be found with great frequency at central places such as Reims, Soissons, Metz, and Cologne. In effect, this zone continues from Paris northwards and continues, I have hypothesised, at least to some extent the “urban” culture of central Gaul as highlighted by the “bureaucratic” perspective.

Nevertheless, care must be taken in reconstructing the spatial topography of the early medieval world based on written sources. As I have argued, written sources are written with their specific audience in mind and with authorial intent, eloquently termed the ‘social logic of the text’ by Gabriel Spiegel.⁵ The knowledge the author had of the wider world and their relation to it, therefore also shaped how they spatially circumscribed their

⁵ Spiegel 1999, 24.

narrative, what I call the ‘topographic logic of the text’. Gregory of Tours, our main source for royal whereabouts in the sixth century, writes from the vantage point of central France, namely Clermont and later Tours, and with a specific idealised image in mind of a Catholic, Frankish Gaul. Much of what happens on the periphery of Gregory’s Gaul therefore escapes our attention or is seldom, or inaccurately, described. Nevertheless, when combining Gregory’s picture of royal whereabouts with that of other sources, a similar trend emerges, only perhaps with more royal presence in Gaul’s northeast around the Metz-Cologne axis. Still, above the Cologne-Maastricht-Boulogne line royal presence was scarcely present, and it seems therefore that much of the Low Countries was a peripheral Merovingian “backyard”. It can be assumed that the Merovingians exercised some form of (soft) hegemony over this area, which was otherwise left to its own devices.

As a possible hypothesis for future research, and as an alternative to the traditional paradigm stating that this coastal area was inhabited by rivalling ‘Frisian’ polities in much the same (but smaller) form as that of the Merovingian kingdoms, I have raised the possibility that the Low Countries were instead a ‘shatter zone’,⁶ a marshy “no-man’s land” where fishermen, traders and herdsmen could retreat behind the difficult terrain at the sight of a royal army. Such a zone was a perfect space to live in opposition to the state and a logical place where we would expect to find a ‘communal’ type of society. This would fit with new insights into the early medieval Low Countries as consisting of autonomous coastal and riverine communities benefitting from water-based networks but being more-or-less ‘ungrappleable’ by the land-based Merovingian state.⁷ A precursor to the much romanticised “Frisian freedom” of the later Middle Ages, self-organising local communities who had no need for a Frisian king.⁸ Here, a comparison can be made to Medieval Iceland, officially subject to the king of Norway, yet, in reality, an overseas refuge where men fled to in order to escape royal grasp and founded a ‘free state’ in opposition to the royal and feudal hierarchy of the “old world”.⁹

What life was like on exactly the other side of the spectrum, at the royal court in the heart of state space, was the subject of the next chapter. The aim here was, by combining a close reading of Venantius Fortunatus’ friendship poetry with a quantitative social network, to reconstruct political culture and elite networks at the Austrasian court of Sigibert and Childebert II (561-596). This analysis reveals much about the workings of the Austrasian court in northeastern Gaul, which remained otherwise relatively invisible in the narrative of Gregory of Tours. I have argued that it shows the ideals of Late Roman *paideia* and literary praxis to be very much alive on the very northeastern periphery of the Late

⁶ cf. Scott 2009.

⁷ Loveluck/Tys, 2006; Theuws/de Bruin/Bult 2021, 453-462.

⁸ I discuss the notion of “Frisian freedom” and its applicability to the Early Medieval world in section 3.6

⁹ Byock 2001.

Roman world, along the Moselle and Middle Rhine rivers in former Roman centres. Probably only a small elite of educated courtiers participated in this culture, but this would not have been that different from the Mediterranean world further south, or the Late Roman world for that matter. Furthermore, not just ‘noble-born’ individuals participated in this literary tradition, as the examples of Gogo (possibly of modest background) and more clearly, the courtier Conda and the slave Andarchius show us. The whereabouts of Gogo, revealed by a charming poem directed by Fortunatus to his friend, should not be taken too literally, but even so, helps us understand the Austrasian space in which literate officials of the Merovingian government could wander (the topographical logic of the text). Thus, as indirectly revealed through Gogo’s whereabouts, to Fortunatus it was very much conceivable that the Austrasian court of Sigibert and Childebert II could be found residing in a broad zone around the Moselle, middle Rhine and Maas rivers. In any case, it proves that to at least some extent, the literate world of Gregory’s Tours stretched out all the way north to Metz, Andernach and Cologne. As the social network analysis has shown, magnates such as Gregory himself, the *comes* Gogo in Metz, the *dux* Lupus in Reims and the *patricius* Dynamius in the Provence were all connected by the same literary network; by extension, the northeastern realm of Francia on the Rhine, menacingly called *Germania* by Fortunatus, was culturally connected to the rest of Gaul and the *Romanitas* of the Mediterranean world.

Judging from the previous chapters, given the fact that royal presence can be observed in (parts of) northern Gaul in the written sources, and the continuity of Late Roman political and literary culture (into the sixth century), it would seem to follow that northern Gaul showed a strong continuity in Late Roman urbanism. Certainly, the *civitas*-centres played important roles in the administrative division of the kingdom and played an important symbolic role as political and religious centres. This gives rise to the paradox that I aimed to resolve in the last chapter, namely how can it be that these “urban” places, so present in the textual evidence, are so absent in the archaeology? Looking at the cases of Soissons, Reims, Maastricht, Andernach and Cologne has repeated the same observations, namely that archaeological indications of urban development are almost completely absent between the fifth to seventh centuries, or at the very least very limited. Cologne and Maastricht offered the most archaeological evidence in terms of settlement excavations, but even here we are limited to the occasional timber-frame house, crafts workshop and, fortunately, a few early churches. Any indication of townhouses, let alone comital, ducal or royal palaces, is sorely missing. If the Merovingian king and court, itinerating around northern Gallic centres, liked to “play Roman”, then where was the “Roman décor”?

Here, part of the answer may lie in archaeological preservation, excavation methods and techniques. The simple, ephemeral structures of the post-Roman world may easily escape archaeological detection, especially in dense urban centres and given the traditional disinterest in medieval times in the early years of the archaeological discipline. However,

this cannot be the full answer, as by now we would surely expect to have excavated more examples of “urbanism” than we currently have, however how residual. The answer, therefore, must equally lie in the fact that post-Roman urbanism was simply different from what we expect “urbanism” to be, namely one of densely and continuously settled places. Rather, the post-Roman town may have been a temporary phenomenon, a busy place at only specific times of the year, for instance on the feast day of the saint, or during the visit of the king, days charged with symbolic value that attracted people from all over the district to visit, meet and exchange. The infrastructure and built environment left over from the Roman period continued to exist as a décor, to mark the space as a “place of distinction”,¹⁰ separate from ordinary day-to-day rural life and enhancing the prestige of the bishop, the king or governor. Thus, to understand these spaces more thoroughly, we are in urgent need of an “archaeology of visitors”¹¹ as a methodology to research temporary meeting places. I want to stress that I do see continuity in many of the former Roman towns; there seems to have been an enduring emotional and ideological attachment to Roman urbanism, infrastructure and urban lifestyle, kept alive perhaps by remnants of the original populations as well as new immigrants. But overall, the forces of transformation were as strong as those of continuity, changing not just the urban fabric but the very essence of urbanism.

In summary, the answer to the original question of elite control and extraction must be a cautious and nuanced response. On the one hand, it cannot be said that northern Gaul in the fifth and sixth centuries knew no governing class, that the royal family was absent or that there was no semblance of a state apparatus whatsoever. By contrast, the royal family was very much present, and the ideology of a working state apparatus is supported by the written sources, although it is hard to gauge to what extent the Merovingians were able to put it into practice, especially in the peripheral zones of northern Gaul. On the other hand, and in line with modern historiography, it must be concluded that a strong, entrenched elite akin to that of the Late Roman Empire or the Late Middle Ages was also absent. The Merovingian kings seemed to have depended on fluid, mobile hierarchies composed of a broad range of agents with a variety of social backgrounds. Insofar as a state extraction continued to function, it must have been rather limited, as evidenced by the lack of observable stone monumentality.¹² The exception, here, must be noted, is in the ecclesiastical sphere. Insofar as any monumentality is detected by archaeologists, it concerns church archaeology. Were bishops more able to extract and redirect surplus wealth to construction than secular elites were? This seems unclear and depends

¹⁰ Fafinski 2001, 42.

¹¹ Theuws 2017a, 388.

¹² The alternative possibility is that extracted surplus was redistributed in a prestige-gift economy and put into funerary “monumentality” or grave goods, on which see above section 3.4.

furthermore also on the wealth of the unobservable timber architecture of elites, which remains a black box for historical analysis. I would hesitate, in any case, to designate the church as proxy for an extractive economy, given their important role in redistributing wealth to the (urban) poor. That said, it seems that in the sixth century, society's overall "ceremonial fund" was spent more on grave goods than on churches, a trend that would reverse from the seventh century onwards.

Thus, overall – and especially in the north – rural dwellers seemed to prosper without too much elite interference. As a case-study I should mention here the site of Oegstgeest, studied in conjunction with the *Rural Riches* project. One of the large early medieval settlement excavations in the Netherlands, the archaeology shows a relatively humble riverine settlement of perhaps about 60 inhabitants, lacking any clear wealth differentiation in their housing. Yet, the finds horizon is quite astonishingly rich. Ordinary 'rural dwellers' could participate in wide-ranging "global" networks without elite interference.¹³ The research by my colleague Mette Langbroek confirms this latter conclusion in vivid ways. Her studies into beads show that rather ordinary rural women could be buried with "the world around their neck"; beads were imported from as far away as the Baltic, Egypt, the Near East and even India and ended up in rural cemeteries at places like Oegstgeest or Lent (nearby Nijmegen).¹⁴ Even if the texts mention the presence of elite agents such as kings, bishops, counts and dukes, rural exchange systems seemed to have been organised at least partially outside of their control and interference. It is best to think of these as 'self-organising networks' by rural dwellers and for the benefit of the "ninety-nine percent".¹⁵ We find self-organising networks, for instance, in the way riverine tradesmen (*ripuarii* / 'ribereinhos') were able to connect along far distances; in village mutual-aid systems, in elite literary networks perhaps, or in the organisation of craft centres.¹⁶ These self-organising networks, as those of the *ripuarii*, are scarcely noticed by our written sources and hence escape notice by historians. A new narrative of early medieval societies thus requires writing a history that includes those people ordinarily left outside of history.

In so far as hierarchies existed, the Merovingian society knew competing hierarchies; the royal and bureaucratic administration; the old Gallo-Roman aristocracy; the church; the village elite of clanheads, law-speakers (*rachimburgi*). In this sense, too, Merovingian society can be called heterarchic. It is not that hierarchies were absent, but that they were fluid, overlapping, fragile and unable to dominate society on the whole. It is this fluidity that to my mind best explains the complex nature of the archaeological

¹³ Theuws/de Bruin/Bult 2021.

¹⁴ Langbroek 2021; Langbroek et al., 2022.

¹⁵ cf. Angelbeck/Grier 2012, 551.

¹⁶ For an example of self-organising networks in early medieval craft centres, see Croix/Neið/Sindbæk 2019.

evidence while doing justice to the observable agents of power in the written sources. Within these interstices of power and extraction, ordinary rural dwellers were able to flourish well enough to establish sometimes lavish ceremonial funds for extraordinary funerary ceremonies now observable to archaeologists. I believe heterarchy, as a concept, allows a more nuanced perspective on these complex social relationships than an either/or paradigm of hierarchy versus anarchy.

The concept of heterarchy also helps explain the physical landscapes of power. Rather than a pyramidal scheme of central places, I have argued for a more complex and diffused understanding of the landscape. Former urban centres ceased to function as nodal points of economic, political, religious and social permanence. Rather, a dispersed population could use multiple meeting places throughout their district, of which the former Roman *civitas* was but one option at which to gather. Certainly, it was one highly charged with ideological value due to its infrastructural legacy leaving it a special aura of distinction. At the same time, new cult places could be founded far outside of urban centres, and the Merovingian kings seem to have preferred living in rural residences most of their time, even already in the sixth century; even bishops can be found residing in estates outside of cities.¹⁷ Further research is needed on ‘meeting places’, both the former Roman *civitas*-centres (also elsewhere in Gaul, and indeed, the post-Roman world on the whole) but also other possible meeting places, perhaps found in marked (natural) environments such as groves or notable trees, meadows, rock formations, cemeteries, and so on; places where members of the rural *pagus* districts could meet.¹⁸ This is where new approaches, such as Baker’s and Brooks’ phenomenological approach to meeting and landscapes,¹⁹ could book much progress and create an “archaeology of meeting” independent of and in addition to the written sources.

One of the central themes in this book has been the relationship between the evidence available to historians and archaeologists on the one hand, and the historical reality that we would like to be able to uncover on the other. There will always lie a chasm between the past artefacts and texts available to us in the present and the past that we wish to research. The question is to what extent currently extant evidence is a (reliable) guide to the past, in what ways it skews our perspectives, and how our own scholarly lenses determine how we interpret the evidence. A further problem is the old adage, that absence of evidence is not evidence of absence. This has been a core problem throughout this book.

¹⁷ Cf. Verhulst 1999, 24.

¹⁸ The *Vita Eligii* (2.16) mentions pagan or superstitious sacred places that a good Christian needs to avoid, such as temples, sources, trees, sacred forests, boundaries or intersections; *Nullus christianus ad fana vel ad petras aut ad fontes vel ad arbores aut ad cancellos vel per trivium luminaria faciat aut vota reddere praesumat*; Julia Smith notes about pre-Christian religion: ‘a natural feature such as an ancient tree might encapsulate the essence of a community; or a sacred site might be the ritual heart of a polity or a place of sacrifice’, Smith 2007, 234.

¹⁹ cf. Baker/Brookes 2015.

We simply cannot be sure whether the fact that we have not excavated townhouses, royal palaces or aristocratic mansions means that they were never there. Perhaps, future archaeologists will finally excavate a Merovingian palace, or perhaps they never will. Whether we have to work with the assumption that they were never there as long as one has not been excavated, *contra* the written sources, depends on how much faith one puts in the textual evidence, an issue I will return to below. Similarly, simply because Gregory does not mention royal presence in the northern half of the research area does not mean they were never there. This has been shown, in fact, by Childebert's II presence in Maastricht noted only in a chance survival of an edict signed there. How many other edicts were signed in northern towns but never survived, we will never know. Another example; whether the *civitas*-structure really declined in the seventh century as opposed to the sixth, as Loseby argues,²⁰ or whether this is an artefact of surviving narrative sources is equally hard to corroborate. With about one main narrative source for the sixth, Gregory, and one for the seventh, Fredegar, it is hard to gauge exactly how much our perception of the character of that century has been determined by our surviving witness.

Thus, in many ways, this book has not been about proving what was there, but rather, sketching the limits of the knowable and the unknowable. This leads me to the somewhat relativist stance that the Merovingian past is ultimately objectively unknowable, the most we can do is create narratives that explain the evidence to the best possible degree. This means sometimes various competing narratives can co-exist, as I have argued with the various concepts of society, but equally that it can be fruitful to create new scholarly narratives with new perspectives, as this book and the Rural Riches project have aimed to do.

One of the more enduring problems with the evidential base is the difficulty in gauging how much of the iceberg we are missing, and exactly how skewed the data is. This is particularly true for the written evidence, which often depends on chance fortitudes of later history. Thus, the Abbey of St. Denis has been particularly lucky and able to preserve its archive for 1500 years, dominating our record of Merovingian *diplomata*. It is hard to tell just how many other monastic archives have been lost over the years; how many epistolary exchanges have vanished forever; how many "Gregories" whose histories did not make it into copyists' hands we can never read. As I have argued, there probably is a large body of texts lost to us,²¹ although it is impossible to estimate how much exactly, and how different our perception of the Merovingian world would be had those texts not been lost. Would northern Gaul bask in the light of written history, or was written and urban culture always more confined to the south? I have hinted more towards the former than the latter;

²⁰ Loseby 2013; Loseby 2020.

²¹ Cf. Gillet 2020.

although reality will more likely lie somewhere in the middle. The problem is that we have no real way to ever know for certain how much textuality has been lost. Archaeology has a similar problem. Merovingian archaeology is richly endowed with thousands of cemeteries, but the settlement, and especially urban archaeology is much impoverished by contrast. As I have argued, there is a high chance that much of the then-built environment will forever remain invisible to us, because of its ephemeral character (wood, wattle, daub, etc.) and because of later disturbances to the soil.²² Thus, with some imagination, one might conjure up a bustling and active mercantile town of Cologne or Maastricht, although there is little material evidence to back it up. The written sources point to some kind of continued urban life, but the cynic can dismiss these as a textual mirage, perhaps telling of sixth-century imaginations but not of everyday material life.

The paradoxical images sketched by the written sources and the archaeology is therefore one of the other main themes running through this book. The response to the discrepancy between archaeology and history can be solved in two ways, depending on the scholar's inclinations. A maximalist point of view assumes that the textual sources are more or less a reliable guide to the past and that contradictions with the archaeology indicate lacunae in our archaeological knowledge, for one reason or another, such as the ephemerality of early medieval material, problems with the methodology or simply lack of excavations. Thus, from a maximalist point of view, the problem lies with the archaeological dataset on the one hand; but at the same time in the future archaeological evidence may theoretically surface that corroborates the written sources. The minimalist, on the other hand, will emphasise a literary reading of the written sources as (mostly) fiction. In the minimalist's reading, written sources only offer a distorted mirror of reality, at best a guide to contemporary ideology and discourse, but archaeology offers a more reliable guide to real existing material conditions.²³

I have preferred to not take a clear stance privileging either body of evidence over the other or to choose the minimalist's or maximalist's side. I do believe that the written sources must be taken seriously as more than just fiction, at least as guides to the mental and social worlds of their times – which Spiegel calls the social logic of the text. The stories written by Gregory or the poems by Fortunatus needed to be believable to their listeners or readers and could not have completely deviated from what contemporaries thought possible. Although the caveat must be named here that, especially for poetry, author and audience alike may have shared a “fictional” or “nostalgic” look towards the Roman past

²² Much of the early medieval world was ‘first and foremost a world of wood’; Theuws/de Bruin/Bult 2021, 443.

²³ The maximalist versus minimalist debate originated from biblical archaeology, where similarly the question was how to resolve contradictions between the archaeological record of the Near East versus biblical narratives; Lending 2020 (2009).

and that audience may have had a specific perspective on their world (order) not shared by society at large. Thus, contemporaries may have easily recognised Nicetius' castle as a metaphor, whereas the modern archaeologist has been quick to assume a real structure. Textual sources are not directly representative of a material world, and can and do distort, mould and generate the reality they purport to describe. Thus, archaeologists need to avoid an either/or approach to written sources. One extreme is to disavow using the sources altogether, and pretend a "prehistorical" approach to the early Middle Ages is possible as if the written sources simply had not been there. This is impossible, and here I agree with Moreland's *Archaeology and text*, in the sense that any historical archaeology is inevitably shaped by perspectives derived from the written texts.²⁴ At the other extreme lies the pitfall of trying to squeeze the archaeology into a written metanarrative. Throughout this book, I have shown multiple examples of where this has led to circular inferences, with historians and archaeologists arguing in a feedback loop without fully engaging with the interpretative difficulties in each other's discipline. For instance, an archaeologist seeking to date a church in northern Gaul or identify a grave with the help of written sources should beware that there is only a limited and skewed surviving dataset, written by agents who for the most part were members of the same social circles; and thus that many events and people of the Merovingian period were never commemorated in written texts, or if they were, those texts never survived long enough for us to be able to read them.

I have thus consciously chosen to leave the tension between text and object partially unresolved, acknowledging that both types of evidence have their own truths, their own discourses and epistemes, and that the discrepancy between them can on its own be a fruitful way forward to asking new kinds of questions as scholars. To bridge the gap between history and archaeology requires not placing one above the other or aligning the two into a single easy (meta)narrative, but rather to interrogate both datasets on their own merits, exposing the contradictions between them and treating both text and object as discursive artefacts that show us past representations, never past realities.

²⁴ Moreland 2001.