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The 'Feudal Revolution' After All? A Discussion on Four Recent Books

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The article discusses four books published during the 2010s, which address — directly or indirectly — the problem of the 'feudal revolution'; that is, the break-up of the Carolingian structures of political power that is said to have taken place all across western Europe at some point between the early and the central medieval period, leading to the creation of localised lordships. This interpretative framework was seriously challenged in the early 1990s, when Dominique Barthélemy and other historians argued that the sharp distinction between Carolingian 'public order' and 'feudal' lordships was ill-conceived. The article shows how the books by Charles West, Alessio Fiore, Nicolas Schroeder and Maria Elena Cortese have contributed — and might further contribute — to this debate.

A short historiographical background

The aim of this article[1] is not to provide an overall picture of the studies on the 'feudal revolution', not even in this introductory section. Doing so would require too much space, and some historians have written excellent syntheses of the *status quaestionis* also in very recent years;

1 I thank Simone M. Collavini, Giovanni Isabella and Chris Wickham for their careful reading of this tex.t

this is indeed the case, too, with the books that I shall examine in the following pages. The main questions that animate the discussions on the 'feudal revolution', however, do need some framing here. We must understand what their meaning is, in order to flesh out how the four volumes that will retain our attention have contributed – and might further contribute – to nourishing the debate on a long-lasting historical and historiographical problem, which has fascinated many generations of medievalists.

Up to the early 1990s, most scholars would have agreed that the 'feudal revolution' could be described as the break-up of Carolingian or Carolingian-style hierarchies of power, which took place across most of western Europe – albeit with significant geographical and chronological variations – at some point between the tenth and the eleventh centuries. According to this interpretative line, the main outcome of the 'feudal revolution' was to be identified in the birth, the stabilisation and/or the formalisation of local lordships, usually – but not exclusively – centred on new-built castles, which took over the rights to impose justice, taxation and military obligations from a by-then collapsed royal authority. The relatively peaceful times of Charlemagne and his successors were thus said to have been replaced by a world dominated by violence, which resulted from the absence of any effective overarching power and the subsequent fragmentation of public prerogatives. Only the slow emergence of fairly strong monarchies and coherent polities over the twelfth and (above all) thirteenth centuries supposedly brought to an end the 'feudal age' of western European history.
This narrative has its roots in Georges Duby's landmark book, *La société aux XI^e^ et XII^e^ siècles dans la région mâconnaise* (Duby 1953), in which the French historian described the increasing autonomy gained by castellan lords in the region of Mâcon, in Burgundy, at the expense of the count – i.e. the Carolingian local officer – during the second half of the tenth century, when the crisis of public authority was reaching

its peak. Duby's model strongly influenced the research of many medievalists – mostly French ones, but by no means just them – of the second half of the twentieth century, who endeavoured to test its validity through the study of other European regions. Without a doubt, those studies nuanced some of Duby's conclusions, and Duby himself appears to have reformulated some parts of his model during the 1980s (see the overview in Carocci 1997); but one can well say that the model held.

It faced, however, serious criticisms in the early 1990s. More exactly, we owe to Dominique Barthélemy some of the major attacks on the foundations of the theory of the 'feudal revolution'. In a series of highly polemical contributions (most of them gathered in Barthélemy 1997), Barthélemy argued that the very notion of Carolingian or Carolingian-style 'public order' should be contested and – ultimately – dismissed. It is true, 'one must not underestimate the efforts put in place by the Carolingians in order to oppose the machinations of the "powerful ones" [*puissants*, the landed aristocracy] through capitularies [legislative and administrative acts] and *missi* [royal representatives]'; but 'the Carolingian capitularies… constantly report the oppressions of the "miserable ones" [*pauvres*] at the hands of the "powerful ones". And in fact, did the so-called public institutions ever prevent this?' (Barthélemy 1992, p. 774)[2]. In other words, Carolingian society was supposedly as violent as 'feudal' society; the relative peacefulness of Charlemagne's times would be in essence a *trompe-l'oeil* – an impression engendered by the highly-formalised charters of the Carolingian period, which were meant to represent (artificially and indeed misleadingly) the wide social consensus that the royal court aimed to create. If the violence-based argument falls, the whole theory of the 'revolution' turns out to be equally inconsistent.

This critique is undoubtedly important, in that it highlights the latently positivistic attitude of some studies on the 'feudal revolution', which

[2] From here on, translations of quotations originally in French and Italian are my own.

have not paid enough attention to the *intentions* driving the people who dressed the relatively irenic documents of the Carolingian age. As a result, it identifies the conflicts between *potentes* and *pauperes* – one may well say, between lords and peasants – as the real structural feature of medieval society, whether 'Carolingian' or 'feudal'. Moreover, it questions the chronology of social and political change in western Europe, arguing for the absence of any significant rupture in the decades following the 950s. Indeed, it should be noted that Duby's timeline for the 'revolution' is in sharp contrast with the periodisation proposed by Marc Bloch in *La société féodale* (Bloch 1939-1940), who characterised the period stretching from the 870s (when Carolingian kingship suffered its first major crisis) to the late twelfth century as the two 'feudal ages' (with the first ending towards 1050) of western European history. To all this, Barthélemy added well-grounded critiques against other tenets of the 'mutationist' theory, as he called it: first, the disappearance of free peasant landowners – something that the sources seldom allow us to prove; second, the militarisation of the higher strata of rural society, which would give birth to the cavalry as a specific social class – for the importance of this military activity, and the social distinction it implied, was by no means exclusive to 'feudalism'; third, the appearance of the central-medieval serfdom – Barthélemy preferred the notion of 'case-specific uses of the servile argument' (Barthélemy 1992, p. 771-2), which he thought of as a better explanation for the different types of servitude that central-medieval documents reveal[3]. Finally, even the use of the adjective 'feudal' seems inappropriate. The quite highly technical juridical tool of the *feudum* hardly accounts for the development of local lordships, let alone defines a whole era of western European history (the classic point of reference is here Reynolds 1994). To sum up, there was apparently no 'revolution', nor was this 'feudal'.

[3] This is a point which inspired Alice Rio's excellent analysis of unfreedom in Europe from *c*. 500 to *c*. 1100 (Rio 2017).

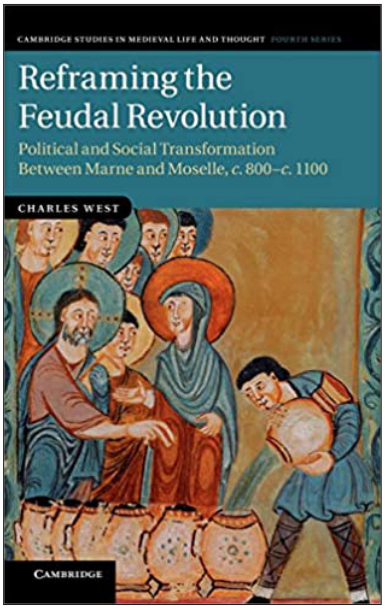
Barthélemy was followed by other historians, namely from the USA (some of them, like Stephen White, had in fact inspired Barthélemy's critiques; cf. for instance White 1978), who joined him in the creation of the 'anti-mutationist' school. This position has gained many followers, particularly in France, even though it never managed to construct anything like an unanimous consensus. Suffice it to recall, here, the contributions of the mid-1990s on *Past and Present* (Bisson 1994; Barthélemy 1996; White 1996; Reuter 1997; Wickham 1997; Bisson 1997. See also Wickham 1995 and Barbero 1995), which brought to light major divisions within the scholarly community. It is essential to recognise, all the same, that the 'anti-mutationist' approach has engaged even the most resistant 'mutationist' historians in the effort to refine the definitions of the social and political transformations they endeavoured to describe. This contributes to explaining, for instance, the preference now generally given to the more generic adjectives *seigneurial* (in France) or *signorile* (in Italy) over 'feudal', or else the replacement of 'revolution' with 'mutation' – in order to stress the gradual nature of change. I shall not delve into these particular matters, since this would take us too far from the content of the four books that I intend to discuss; but it is important be aware of their existence (and I will myself use hereafter the adjective 'signorial', the adaptation into English of the Italian *signorile*).

The first decade of the new millennium witnessed the publication of many excellent contributions which touched upon some of the main themes that had been at the core of the 'feudal' *querelle*. But one could not avoid noticing that the effort to confront directly the very nature of the 'feudal revolution' as a historiographical problem, as well as the polemical vigour of the 1990s, had got lost. The 2010s, instead, have been characterised by a renewed interest in the debate; and at least two

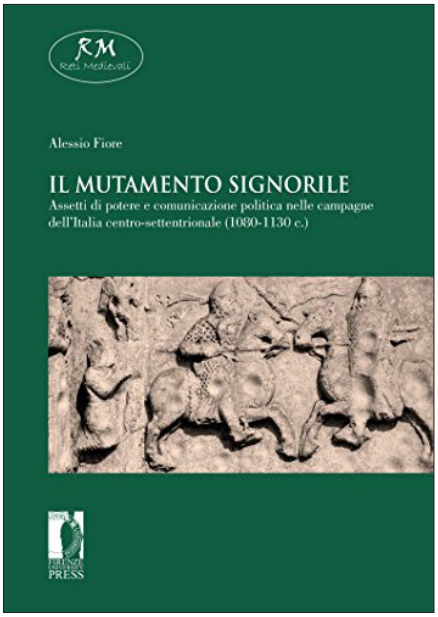
of the four books that I will examine provide clear evidence of this[4]. The other two, instead, do not discuss at length the 'mutationist' and 'anti-mutationist' positions, but they do contribute – or can contribute – to reassessing the whole subject in their own ways, and these contributions I shall try to highlight.

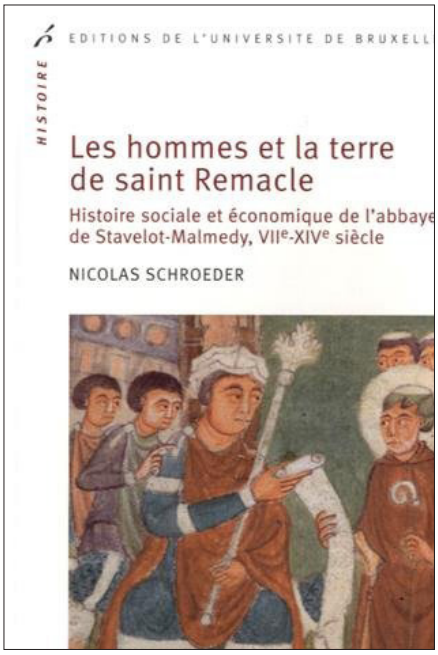
Four books to renovate the debate

The two volumes that engage explicitly in the 'feudal' *querelle* are Charles West's *Reframing the Feudal Revolution* (2013) and Alessio Fiore's *Il mutamento signorile* (2017), which has been recently translated into English (Fiore 2020; I will refer, however, to the Italian edition). West, who is today a reader in medieval history at the University of Sheffield, analyses the socio-political (and, to a lesser extent, the economic) history of the regions between the rivers Marne and Moselle between *c.* 800 and *c.* 1100. He makes a bold and far-reaching attempt to reassess the nature of the 'revolution' between the early and the central Middle Ages by taking into account a vast portion of the northern heartland of the Frankish



[4] Moreover, they are not the only ones: cf. in particular Carocci 2014 and an article by Chris Wickham on the 'feudal revolution' in France and the Italian communes (Wickham 2014). It is worth citing also Bisson 2009, a distinctly 'mutationist' account; Bisson strongly believes that 'Old order was public order' and that 'in this age [i.e. that of the 'feudal revolution'] the concept of order became illusory' (cf. ibid., p. 29 and p. 31). His book, however, is concerned more with the effects of the 'feudal revolution' than with the problem of its very existence.



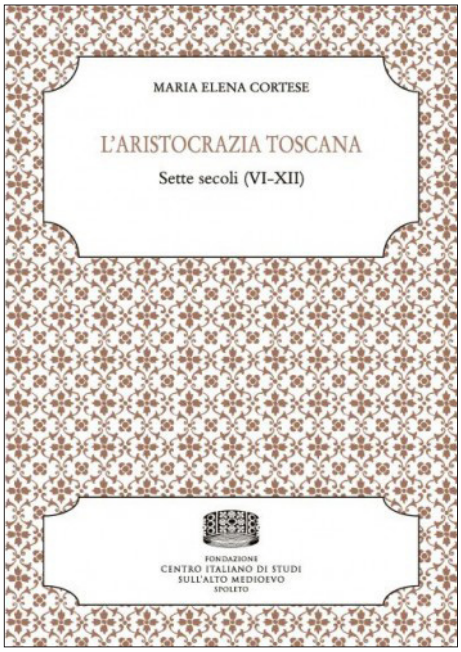


Empire. Indeed, West's research has to be praised, among other reasons, for the remarkable effort to study in great detail both 'Carolingian' and 'feudal' society. One can easily see that this is the only way to discuss properly the problem of change *vs* continuity.

West deliberately excluded Italy from his comparative remarks, for he suspected 'the timing of developments there to have been sufficiently distinct as to require separate treatment' (West 2013, p. 13). This impression has been confirmed by Alessio Fiore's findings. *Il mutamento signorile* is focused on the decades straddling the eleventh and the twelfth centuries, which the author identifies as the period that witnessed a radical change in the structures of local power across the territories of the *regnum Italiae*, the polity created by Charlemagne in northern-central Italy after the takeover of the Lombard kingdom in 774. Fiore, now a *ricercatore* at the University of Turin, has produced a long-awaited synthesis of the many local studies on the Italian *signoria*. One does not have to go too far to suggest that his book will spark much debate in Italy (it has already done so, actually; cf. Collavini

2017 and Santos Salazar 2017) and abroad; it does represent an essential point of comparison for future studies on the 'feudal revolution'.

Let us now turn our attention to the two books that examine the subject of this paper only tangentially – that is, as part of another topic. The first one is *Les hommes et la terre de saint Remacle* by Nicolas Schroeder (2015), *chargé de recherches* at the Université Libre de Bruxelles. The volume is drawn from Schroeder's doctoral thesis, which was written under the supervision of Jean-Pierre Devroey. It analyses the history of Stavelot and Malmedy, two monasteries located in the Ardennes region and placed under the authority of one abbot, between the seventh and the fourteenth centuries. The book is divided into two sections, one outlining the most important events in the history of the monastic communities, and one discussing monastic lordship (*seigneurie monastique*).



The latter section is in turn twofold: it first explores the notion of *seigneurie* as a social structure, then as an economic one. Despite simply claiming to be 'a classic exercise to which many medievalists have been committed' (Schroeder 2015, p. 9), Schroeder's monastic monograph is in fact one of the most detailed and thoughtful accounts of the powers exerted by religious people from the early to the late Middle Ages. It needs to be dealt with, in that the history of Stavelot-Malmedy encapsulates some of the major themes that any historian with an interest in the 'feudal revolution' needs to discuss.

Finally, I shall take into consideration Maria Elena Cortese's *L'aristocrazia toscana: Sette secoli* (2017). The book covers the history of medieval

Tuscany from the sixth to the twelfth century, with a focus on the networks of aristocratic power and wealth. Like her previous volume on Florentine landed aristocracies during the central medieval period (Cortese 2007), *L'aristocrazia toscana* testifies to the extensive use of both written and archaeological sources. Cortese – who is now a *ricercatrice* at the Università Telematica Internazionale Uninettuno – has thereby carried out a remarkably thorough and thought-provoking investigation revolving around two main questions: what made an aristocrat an aristocrat? And how did this change over time? Thus, even though it is not explicitly focused on the 'feudal revolution', Cortese's book makes significant inroads into the problematic of the changing nature of royal power – in other words, into the passage from 'Carolingian' to 'feudal' society. It is definitely worth looking closely at, and this I will do in the following pages.

The documentation of Stavelot-Malmedy constitutes one of the sets of sources examined by Charles West, in that the two monasteries were located on the northern edge of the region examined by the British historian. Similarly, Tuscany was part of the *regnum Italiae* and is therefore discussed in Fiore's study. As a consequence, the geographical scopes of West's and Fiore's books overlap, to an extent, with – respectively – Schroeder's and Cortese's ones. This can make the comparison particularly fruitful. Shifting from a more general to a more local scale of analysis will allow us to get a sense of the different types of arguments that can be made. Indeed, that the four authors have studied the same areas do not imply that they have always used the same evidence, or that this evidence was analysed with the same level of detail. Obvious as it may appear, it is nonetheless important to stress this point, for it seems to me that the 'feudal' debate has originated, in essence, from the difficulties implied by the interpretation of sources – sources that are often erratic, unclear, if not simply absent in many areas and for long periods. Two sections of this article are dedicated to the main questions

that stand at the core of the 'feudal' *querelle*: the (alleged) existence of the Carolingian order, and its (alleged) change. The final section, by contrast, is less descriptive and more personal; it discusses briefly how political and economic factors have been, or should be, taken into account in order to reframe the 'feudal-revolution' debate. The books by West, Fiore, Schroeder and Cortese will serve as guides to find provisional responses to all this.

Debate (1): Was there anything like a Carolingian (or Carolingian-style) public order?

Let us start this section with Charles West's book, which describes what he calls 'the parameters of Carolingian society' (West 2013, part 1, chapter 1). According to West, the attempts put in place by Carolingian rulers to frame local societies were real – sometimes unsuccessful, it is true, but often effective. The evidence from the regions between the Marne and the Moselle suggests that the Carolingians endeavoured to integrate the localities with the centre – one may say, to centralise power – thanks to new impositions and new institutions. In this sense, West stresses the importance of public exactions and the formalisation of the *mallum*, a word which used to indicate the traditional meetings of free Frankish people and then came to acquire the quasi-technical meaning of court presided over by a local officer, the count, between the late eighth and the ninth centuries. But one should look also at the ecclesiastical hierarchies, both secular and regular, in order to understand the great ambitions of the Carolingian political project. Bishops were central to this. They generally were highly-educated people, often more powerful than counts, and their appointment was a result of direct royal initiative; they prayed for the king and organised the army; ecclesiastical lands were used to reward soldiers – which means that the royal domain could come to encompass both 'State' and 'Church' lan-

ds, the two of them being sometimes not clearly distinguishable. Abbeys were very prominent, too, endowed as they often were with royal estates, and connected to the lay world through mediating figures, the ecclesiastical advocates. Indeed, much-trusted abbots could be used by the royal authority to limit the excessive powers of bishops. Moreover, that same authority appears to have extended its control, sometimes, even over local priests, who could be deposed if they were unfit for the role. All this points to a genuine effort aimed at organising society down to the level of small villages and parishes.

West is well aware that this description is partial; it only characterises the formal networks of power of Carolingian society; it reflects, to an extent, the image of public order that the Carolingians wanted to convey. That is why he devotes the following chapter of his book to the limits of the Carolingian rule – that is, the informal patterns of domination (ibid., chapter 2). With regard to this, it may be useful to recall what West writes about the 'dark side' of Carolingian counts, who seem in fact to resemble closely the 'feudal lords' of later periods: counts forced peasants to build unnecessary fortifications; they raised war taxes from them even in times of peace; they fined them for specious reasons. Violence was, if not structural, certainly widespread – which explains both the capitularies, relentlessly trying to regulate it, and the concerns expressed by major Carolingian intellectuals, like Bishop Hincmar of Rheims (845-882), who inveighed against the oppression of the peasantry. Royal agents were sometimes responsible for the illicit acquisition of lands, which means that fiscal estates could be expanded quite arbitrarily at the expenses of 'private' owners.

Should we conclude that the Carolingian order was in fact a superstructure, hiding the underlying, structural struggles between lords and peasants? In the end, West would not agree with this. He believes, it is true, that we should dismiss the rigid distinctions between 'Carolingian State' and 'private lordship', for public offices and aristocratic powers often overlapped. According to him, however, Charlemagne

and his successors did try – in many cases successfully – to regulate and tame the *potentes*, to define and circumscribe their role in the Christian society so that the *pauperes* could be protected, 'to attain a greater and clearer definition of social status, in which everyone's political position was expressed and aligned, and the status of certain members of this elite raised and defined by means of formal title' (ibid., p. 100). Hence, the importance of the symbolic communication of social consensus in formalised meetings, with which the Carolingians were deeply concerned, for they were meant to represent the synergy between local aristocracies and the 'State'.

Let us now consider Alessio Fiore's book. As a first point, it needs stressing that Fiore does not explore the inner structures of Carolingian society, simply because Carolingian Italy is excluded from his account. He does stress, however, that up to the 1050s the political landscape of the *regnum Italiae* still had visible Carolingian traits. It can be described as a mosaic of relatively large polities held by marquises and counts, to which we need to add smaller-scale ecclesiastical principalities, sometimes centred on cities. The model for the administration of justice remained, in general, the *placitum*, the Carolingian-style assembly gathering lay and ecclesiastical aristocrats (Fiore 2017, part 1, chapter 1.1). This state of affairs started to change after the unexpected death of Emperor Henry III in 1056. His son, Henry IV, was only six years old at that time; he and his mother Agnes, moreover, had to face the fierce opposition from Bishop Anno II of Cologne and other princes of the Empire. According to Fiore, the power vacuum resulting therefrom unleashed the latent rivalries among the aristocrats of the *regnum* – which had been controlled, up to then, thanks to the effectiveness of royal power – thus leading to an escalation of local military struggles. Political instability and growing warfare reached their zenith during the Investiture Controversy (conventionally 1075-1122), the conflict between royal and papal authority over episcopal appointments, which was extremely violent and undermined the king's authority in both his

German and Italian lands. As a consequence, the pre-existing aristocratic networks of allegiance centred around the public courts underwent major – indeed, irreversible – disruptions. The Carolingian-style order came to an end, making room for a new set of political and social relations.

Let us now have a look at the two books that do not engage directly with the 'feudal-revolution' debate. As to Nicolas Schroeder's volume, we need to start with a preliminary quotation. 'Like Jean-Pierre Devroey and Laurent Feller, we think that the word *seigneurie* can well define the monastic domains of the early Middle Ages… The protection, the mediation, the command or the domination are included, in different degrees, in these relationships [those between monastic lords and dependants]. Finally, the transfer of "regalian" rights to, and their integration with, the monastic *seigneuries* are attested already in this period. As a result, it would be difficult to try to distinguish between the "public" and the "private" origin of the components of the *seigneurie*' (Schroeder 2015, pp. 136-137). Two observations should be made here. There has to be stressed, in the first place, a difference that separates this notion of *seigneurie* from the concept of *signoria*, as is generally used by Italian historians: the latter is the outcome of a specific process, i.e. the formation of localised patterns of political power that followed the break-up of the Carolingian order; the former is instead a broader label for medieval local domination, whether 'Carolingian' or 'feudal', political or economic. Both these definitions are perfectly acceptable, but one has to keep the difference in mind when it comes to comparisons (on this cf. in particular Wickham 2004). If it is clear to an Italian scholar what it means to debate the *mutamento signorile*, it is more problematic for a Francophone to make use of the expression *mutation seigneuriale* – and indeed, if we welcome Schroeder's wide definition, such a choice of words is ultimately meaningless.

The second observation is more directly related to this section. The acknowledgment that royal ('regalian') rights were part of the mona-

stic *seigneurie* already in the early medieval period, and that 'public' and 'private' prerogatives were hardly distinguishable, seems to place Schroeder's book in the field of 'anti-mutationism'. In fact, the fine-grained investigation undertaken by the Belgian historian is more complex than that. The exercise of local lordship, it is true, was a long-standing feature of the monastic communities of Stavelot-Malmedy; but the way this lordship was related to royal power did change over time. The two monasteries were endowed with fiscal lands on the initiative of Merovingian kings and, in particular, of the majors of the palace (the *de facto* rulers of late-Merovingian *Francia*) towards the end of the seventh century. They were then strictly connected to, and controlled by, the Carolingians. Pippin III and Charlemagne, for instance, seem to have prevented the abbeys from receiving too many donations of rural estates, probably in order to limit the power that the abbots would otherwise have wielded locally by means of their excessive landed wealth. Louis the Pious, on the contrary, issued three royal diplomas in favour of Stavelot-Malmedy, possibly in the attempt to tighten the alliance with the then-abbot Wirundus, and to turn the monastery into a stronghold of the monastic reform promoted by Benedict of Aniane (Schroeder 2015, part 1, chapter 1). So the influence of royal power surfaces as a major feature in the history of Stavelot-Malmedy up to the mid-ninth century, even though it took up very different shapes – from Pippin's 'austerity measures' to the rather generous attitude of Louis the Pious. It is only in the early tenth century that we have clear evidence of the increased importance of local political figures at the expenses of the traditional public powers. To this, however, I shall come back in the next section.

We need now to take into account Maria Elena Cortese's book on medieval Tuscan aristocracies. To start with, it should be noted that her analysis investigates both political and economic aspects at the same time. Politics is not the quasi-exclusive focus here, as is the case with West's book; not the main one, like in Fiore's account; nor are the two

aspects kept separated, as in Schroeder's volume. This choice reflects the way in which Cortese characterises aristocratic power in Tuscany from the late eighth to the late eleventh century, a period in which 'a cultural and political model that was curial and, one may say, essentially "Carolingian" ' (Cortese 2017, p. 171) prevailed. Starting from the takeover of the Lombard kingdom on the part of Charlemagne in 774, and with distinct vigour after the creation of the March of *Tuscia* (the ancient name of Tuscany) in the mid-ninth century, the Frankish aristocracy managed to create a court-centred political and economic system. This was made possible by the integration of the religious hierarchies with the lay political and military apparatus, both represented in the *placitum* assemblies up to the 1070s; and by the expansion of ecclesiastical and, above all, fiscal estates. Even though it is not easy to quantify the extent of the two (and even though we should not underestimate the importance of free peasant landowners in *Tuscia*), there could be very little doubt that both of them were substantial and widespread, covering large parts of the region. Anyone aspiring to a stable aristocratic status had thus to be part of the clienteles of bishops and marquises, who conceded landed estates in exchange for administrative service and military support. Indeed, this system was strong enough to survive the political turmoil of the tenth century; only the 'Investiture Controversy' would destroy it. In conclusion, a court-centred politics and a 'redistribution system' of wealth and social prestige constituted the bedrock on which the long-lived March of *Tuscia* was built (on all this cf. ibid., chapters III-V).

It may be useful to end this section with a general remark, which introduces the theme of the following pages. The reader may sense that the system described by Cortese, although being presented as 'Carolingian', could in fact look 'feudal' to an extent – with aristocrats ceding out land in return for allegiance, which is the basis of the classic 'feudal' relationship and, in broader terms, one of the elements that constitute the standard (perhaps popular?) image of the Middle Ages as a whole.

One might be tempted, as a consequence, to privilege the 'anti-mutationist' view as a more effective framework to interpret the Tuscan social, political and economic history over the period from the late eighth to the late eleventh century. It is, however, the combination of political and economic analysis that ultimately allows Cortese to dismiss the 'continuitist' approach, and to argue for a veritable rupture around the 1070s – the same break that Fiore extends to the entire *regnum Italiae*. So, since the four books that we are examining recognise, albeit from different perspectives and in different degrees, the existence of a Carolingian or Carolingian-style order, it is now time to discuss its change.

Debate (2): What changed, and when?

Charles West begins to discuss change with a detailed and compelling analysis of the political history of the regions between the Marne and the Moselle (West 2013, part II). For a start, two key-dates have to be kept in mind: 869, when King Lothar II died without heirs, leaving his kingdom (named Lotharingia) to his two uncles and the lands between Meuse and Moselle to one of them, Charles the Bald, whose political action was focused elsewhere; and 887, when Emperor Charles III was deposed, making it possible for external figures – that is, people whose interests lay far from the traditional Carolingian heartlands – to become major political players. One has to consider, moreover, the destruction brought about by Viking raids in the 880s, although these did not last long, in fact, and their effects should not be overestimated. At any rate, the effectiveness of royal power was deeply affected; any Carolingian-style order was slowly becoming more an aspiration than a reality. This is true for the tenth century, too. To make an example, West stresses how the Ottonians, whose bases were the Rhineland, Bavaria and Saxony, struggled to keep control of Lotharingian bishoprics. Indeed they failed, sometimes at least, to impose their candidates – that is

to say, they barely managed to integrate ecclesiastical hierarchies with lay powers, a major feature of Carolingian rule (ibid., pp. 119–120). Royal domains were progressively alienated to aristocrats, who never gave them back to the legitimate owners. The families of the local elite, increasingly centred on castles, posed threats to political stability, but none of them was powerful enough to prevail over the others and reconstitute, thereby, anything like a coherent polity. A series of small-scale wars ravaged the region in the early eleventh century; kings were in fact able to intervene, but only intermittently. In conclusion, West argues that the failure of the Carolingian kingship resulted from the very nature of the Carolingian political project, 'the logical culmination of the steadily crystallising power of an elite, a process initiated by Carolingian kings' (ibid., p. 137). That project had gone too far; and this was the essence of the 'feudal revolution'. Landed aristocracies that had been made part of the civil, military and ecclesiastical framework created by Charlemagne 'no longer considered that their local and regional dominance needed to be underpinned by royal authority' (ibid., p. 136), for such an authority was discredited and contested as a result of contingent dynastic failures, fiscal profligacy and – to a lesser extent – external invasions.

The long-term outcomes of the social and political process triggered by the Carolingian kings frame the interpretation of the late eleventh and twelfth centuries, too (ibid., part III). These were characterised by the definition and the formalisation of 'banal' rights – that is, the rights of command (*bannum*) concerning justice, taxation and military aids, which came to be attached to landed property, and controlled by local aristocratic families, from the 1060s onwards. West rejects the devolution of 'regalian' rights (one of the key elements of Duby's model) as the best explanation for it. First, the *banna* are first attested far later than the crisis of royal power in the late ninth and tenth centuries. Second, every type of medieval landed property was likely to imply some degree of political domination, even in the early Middle Ages

(this is Schroeder's notion of *seigneurie*). At the same time, West argues that the 'anti-mutationist', 'continuitist' view is ill-conceived too. The appearance of 'banal' lordship cannot be regarded as the simple revelation of the inner structures of the Carolingian world, which – like the 'feudal' one – could be said to be marked by the longstanding coercive aspects of aristocratic power over the peasantry. Suffice it to say that *banna* became the objects of legal disputes that did not exist earlier than the late eleventh and twelfth centuries; and that 'banal' and land-derived powers – the latter indicating the signorial obligations that aristocrats could impose only on their tenants – could be, and were, clearly distinguished. 'Banal' rights were a new thing, and a very real one: indeed, a form of property that could be purchased, sold and disputed over. 'Age-old informal powers of more or less ad hoc coercion which had long existed in practice were now put on a formal footing' (ibid., p. 185). Formalisation and clearer definitions of property rights are the key concepts with which West overcomes the rigidities of both the 'mutationist' position – which sees little connection between the 'Carolingian' and the 'feudal' period, if not in terms of structural diversity – and the 'anti-mutationist' one – which argues for the absence of any real change.

Alessio Fiore's book is instead clearly placed in the 'mutationist' field. His analysis revolves around the formation and generalisation of a new political model in northern-central Italy over the period from *c.* 1080 to *c.* 1130: the *dominatus loci*, what we might translate as territorial lordship (I opt here for 'territorial' in order to reflect the preference given by Italian scholarship to the adjective *territoriale*, which is generally employed to describe the type of local domination that Francophone scholars would instead qualify as *banale*). For a start, it has to be noted that the physical displaying of the documents issued by the royal authority in favour of lay and religious elites – the diplomas – during judicial hearings became rarer during the 1040s and disappeared around 1100 (Fiore 2017, p. 154), thus strongly pointing to the ebbing of public,

Carolingian-style order within the *regnum Italiae*. As a result, society and economy were transformed. Indeed, it might be useful to recall here Barthélemy's main critiques of the 'feudal-revolution' model, and observe how Fiore goes in the opposite direction; Barthélemy's superstructural changes become, in Fiore's analysis, substantial and real. As to violence, the Italian scholar remarks how the possibility of using it against the peasantry, and of displaying it publicly, was a way to reassert power, thus crystallising patterns of social and political submission. Here lies the main difference with Carolingian-style order. Violence by now was not something that rulers endeavoured to tame, however ineffectively; it was what legitimised someone as a ruler (cf. in general ibid., chapter 10). Another tenet of the 'anti-mutationist' position concerns the survival of free peasant landownership throughout the central medieval period. Fiore acknowledges – rightly – that in some areas of the *regnum Italiae* independent peasants never completely surrendered to lords during the eleventh and twelfth centuries, but he remarks how this resistance was not incompatible with *dominatus loci*. In fact, lords could and did extend their jurisdiction even on free owners (ibid., p. 104). The militarisation of some sectors of rural society – Barthélemy would say, the birth of the *chevalerie* as a recognisable social class, of those knights who were 'the lackeys of signorial terror' (Barthélemy 1992, p. 768) – surfaces clearly from the written sources. The decades straddling the eleventh and the twelfth centuries saw the semantic slip of the expression *boni homines* from a generic label for local elites to an equivalent of *milites*, soldiers; indeed, Fiore argues for a real bipartite division of local societies between armed men and peasants during the central Middle Ages (ibid., pp. 81–82). This originated, in turn, from a general tendency characterising the signorial mutation in central-northern Italy, which is, moreover, another core element of the 'feudal-revolution' model – that is to say, the disappearance of early medieval slaves (*servi*), taking place after *c.* 1050, and their replacement with a class of dependants whose freedom was strongly limited; these

were the subjects of the *dominatus loci* (ibid., pp. 103-104). Finally, it is worth pointing out that Fiore does not treat the 'signorial mutation' as a purely political phenomenon. He stresses that the period bridging the eleventh and the twelfth centuries brought a sharp increase in the number and the weight of signorial exactions, sometimes burdensome ones, which caused, in turn, a surge in the extraction of the agricultural surplus (ibid., p. 63). To this point I shall come back in the final section of the paper.

Let us here turn, more briefly, to Schroeder's and Cortese's books; they will help us to nuance and complicate the overall pictures of change drawn by West and Fiore. I have stressed earlier on that Schroeder identifies the first half of the tenth century as the period in which local political figures became particularly powerful. In more detail, it was from 915 that kings ceased to give backing to the documents issued by the abbot of Stavelot-Malmedy. At about the same time, the sources at our disposal testify to the importance of new political actors, the ecclesiastical advocate and the provost, both charged with the administration of the landed patrimony of the abbeys. Local aristocratic families managed to have their members nominated as provosts, thus making room for the seizure of monastic estates. The crisis of royal power, however, was only temporary. Under the Ottonians, parts of the ancient domains that had been alienated to aristocrats were retrieved; the successors of Otto I, moreover, prevented the monasteries from ceding out fiefs, in order to avoid the fragmentation, if not the outright loss, of those same domains. Indeed, the second half of the tenth century marked the beginning of a period in which Stavelot-Malmedy came to be (again) under the aegis of the Empire, becoming part of what the German historiography has called the *Reichskirche*, the imperial church; this state of things would change only in the late twelfth century, when the political crisis following the deaths of Frederick I (1190) and his son Henry VI (1197) paved the way for an unprecedentedly tight and

direct relation between Stavelot-Malmedy and the Papacy (on all this cf. Schroeder 2015, pp. 55-104).

This political narrative is particularly helpful in that it reminds us that the 'feudal revolution' or 'signorial mutation' – if one accepts that it really took place – was not a linear process. Royal power could fade away and then reappear. It does seem, at any rate, that the late twelfth century witnessed the final crisis of imperial authority; this was matched, moreover, by other structural transformations which render that period a real turning-point in the history of Stavelot-Malmedy. This becomes clearly visible when one looks at the notion of *familia* – that is, the Carolingian all-encompassing label for monastic dependants of diverse origins, whose importance as a heuristic tool is strongly emphasised by Schroeder. Indeed, he argues that the *familia* framed all the personal bonds of allegiance between peasants and monks during the early and most of the central medieval period. It ceased to exist after 1200, however, when 'the nature of power and the framing [of rural dependants] was redefined under the pressure of neighbouring *seigneurs*, who transformed their spaces of domination into territorial principalities' (ibid., p. 185). According to Schroeder, the *familia* had worked against the localisation of power on a territorial basis: 'as long as marriage, land exchanges [*circulation foncière*] or justice were exerted within the frame of the *familia*, it was impossible to impose any "banal" or "upper" [*hautain*] power over large and homogeneous territories' (ibid., p. 193). The end of the *familia* does not imply that the Ardennes region became a mosaic of clearly-bounded territorial lordships between 1200 and 1400. It does mean, however, that localised jurisdictional remits became the space within which public prerogatives, sometimes pertaining to different lords, were enclosed; the region, the *pays*, was by then the main unit of reference that defined the political landscape. As a final point, it needs stressing that Schroeder argues that the passage from the twelfth to the thirteenth century was crucial as to the economic history of Stavelot-Malmedy, too. Up to then, the abbeys 'seem

to have slightly modified [*adapté*] models of management whose main traits had been inherited from the Carolingian age' (ibid., p. 288). From then on, change was instead quite radical. A series of elements – such as the decline of demesne farming, the fragmentation of the traditional, Carolingian units of agrarian exploitation, the increase of revenues derived from the exertion of justice, and the use of short-term leases of landed estates – suggests that Carolingian-style *seigneurie*, the old order encapsulated by the notion of *familia*, no longer existed.

In the case of Cortese, her chronology of social, political and economic change largely overlaps with Fiore's, in that the final decades of the eleventh century, and the huge disruptions caused by the 'Investiture Controversy', are regarded as a turning-point. In more detail, one of the key dates in the history of the March of *Tuscia* is 1081, when Emperor Henry IV deposed Marquess Mathilda, who subsequently had to flee Tuscany. Mathilda then managed to come back, but with a considerably lessened power. After her death in 1115, the old-style public order centred on the highly-formalised judicial assemblies known as *placita* disappeared completely, whereas the signs of an ever-growing localisation of power became more numerous and unequivocal. In the first place, rural and urban elites began to run justice by themselves, in ways that hardly resembled the *placitum*. But one has to look at archaeological data, too, in order to understand properly the significance and the impact of political change. The construction of fortified settlements, already on its way all throughout the eleventh century, accelerated during the 'Investiture Controversy'. Even though it is not always easy to determine what the effects of castle-building on power relations in the countryside were, it is beyond doubt that castles entailed the existence of some degree of aristocratic coercion – not least because the peasantry had to build them, and was then forced to perform castle-linked obligations, such as the maintenance of the walls or sentry-duties. Archaeology is equally useful to investigate the 'economic side' of the signorial mutation, and this Cortese does at length. According to her,

the twelfth century witnessed the take-off of the Tuscan medieval economy, and this is reflected by the evidence of aristocratic wealth over that period: after 1100, stone-buildings became far more common than before, in both the cities and the countryside; new villages were created, investments in mills are attested, and arable lands were extended.
This brings us to the final point that needs some discussion here, the end of the 'redistribution system', of the mechanism aimed at granting wealth and social prestige to a court-centred aristocracy, which had been the main trait of the March of *Tuscia* as a Carolingian-style, coherent polity. The implosion of the March implied that such a system could hardly survive, for the very authority that made it work was heavily weakened. To this, Cortese adds that aristocratic families, by then divided into quite numerous branches, had to distribute rural estates among all of them, thus leaving every branch with only a few lands. There were of course exceptions; the Guidi and the Aldobrandeschi, probably the two richest powers of Tuscany, could aspire to create relatively large and coherent dominions. All the other families, however, were forced to exploit local resources at the best of their possibilities, thus paving the way for an increase in the extraction of the agricultural surplus – which, as Fiore also says, can be regarded as the economic outcome of the fragmentation of political prerogatives (for what I have outlined hitherto cf. Cortese 2017, chapter VI). We may be tempted to infer that the 'redistribution system' was an effective framework to create social consensus, but a less effective one to intensify production at a local level. It is worth mentioning here the recent book by Paolo Tomei on the landed aristocracies in the territory of Lucca, in northern Tuscany, from the ninth to the twelfth centuries, as Tomei discusses some of the problems analysed in Cortese's volume. He shows effectively that Lucchese aristocrats based their wealth and social prestige on the possession of fiscal estates, which however was precarious and could be revoked. That the grants of those estates were precarious might explain why the Carolingian *élite* was, on a general level, less

motivated to invest in castle-building and relatively less inclined to exploit the local peasantry in order to increase the extraction of agricultural surplus in comparison to the 'feudal' aristocracy: there is no point in investing in something that can be taken away from you (cf. Tomei 2019, for instance pp. 425-426). In this sense, the 'feudal revolution', the political process that led to the localisation of power, would be an 'economic revolution', too. Around this nexus between the political and the economic nature of change my final remarks will revolve.

Political change and economic take-off: A few concluding thoughts

Some changes did happen in the passage from the Carolingian or Carolingian-style society to the 'feudal', 'signorial' one. They took the shape of the formalisation of aristocratic power at the local level, and of a clearer definition of the rights of property, according to West; Fiore describes them as the generalisation of the *dominatus loci* in northern-central Italy; Schroeder as the end of the monastic *familia* and the redefinition of power at a localised and territorial level; Cortese, finally, identifies the end of the 'redistribution system' of the March of *Tuscia* as the real break-up of the old public order.

Despite these differences, that the 'Carolingian' and the 'feudal' order were not the same thing surfaces from all the four books that we have been looking at here. In this sense, it does look as if the 'mutationist' position has won over the 'anti-mutationist' one. But the latter has not only challenged – effectively – some of the theoretical tenets of the former, as the volumes by West and Schroeder show; it has greatly contributed, too, to reconsidering the problems of periodisation. It should be noted, in the first place, that the focus of the debate has shifted away from the years around 1000, which – in the wake of Barthélemy's critiques – are no longer considered as a turning-point of western Europe-

an history. Second, political and economic change cannot be regarded anymore as a linear process, going straight from a relatively centralised and powerful royal authority to localised lordships; there were fractures, moments of crisis that could be, and were, overcome. The early tenth century provides clear evidence of this. Once described in Italian handbooks as the period of 'feudal anarchy' (cf. for instance Villari 1978, p. 118), the phase from *c*. 880 to *c*. 960 seems now to be best understood in terms of profound political instability and recurrent struggles for power which, however, did not compromise definitively the possibility of (re)creating quite an effective Carolingian-style civil and military hierarchy, in some regions at least. Moreover, such a hierarchy could and did survive well after the *renovatio Imperii*, i.e. the reforms promoted by the powerful Ottonians from the 960s (indeed, up to roughly 1100 in the *regnum Italiae*). As a result, neither Duby's chronology of the 'feudal revolution' – around 1000 – nor Bloch's periodisation of the two 'feudal ages' – 870s-1050, 1050-1200 – can be used any longer as markers for European history *as a whole*.

This raises two further questions, which lead us to the final point that I would like to discuss here. What was the real basis of royal power? And what determined its change over time? Even if one accepts, with Barthélemy, that local lordship in some form was characteristic of both 'Carolingian' and 'feudal' society – and, to an extent, I personally believe that this is true – it still remains to be explained how the Carolingians appear to have tamed the aristocrats, the *potentes*, better than twelfth-century rulers. With regard to this, it might be fruitful to further investigate the effectiveness of the 'redistribution system', which Cortese discussed for Tuscany, by extending her research to the entirety of Carolingian Europe. Not only was it a source of wealth and social prestige for those who were granted royal or ecclesiastical estates; it was – perhaps first and foremost – the basis for securing military support, the same support that allowed Carolingian kings to be more powerful than local aristocrats; that allowed them, in other words, to be

effective rulers *despite* the existence of local lordships and the absence of a structured tax system.

The connections between political and economic history, and the ways one affected the other, probably constitute the path that researchers need to follow now in order to rethink the 'feudal-revolution' model. It seems notable that the two books that devote most space to economic transformations (that is, Schroeder's and Cortese's) are the two that do not address directly the 'feudal' *querelle*, which is still regarded as a political and social problematic above all. Charles West writes that 'arguments for twelfth-century economic take-off are convincing only when they take on board the now-overwhelming arguments for the origins of that growth in the earlier period' (West 2013, p. 256); and that 'it is inadequate to explain change through economic growth, or demographic shifts, or environmental altercation, because these are part of the picture, not its frame' (ibid., p. 257). Not all the authors whom we have taken into account so far would agree with this. Cortese, for instance, argues for a marked difference in economic exploitation in the ninth and the twelfth centuries, with little visible connection between the two; there was, at the most, a first moment of acceleration in the second half of the tenth century, followed by a long period of stability over the eleventh (cf. Cortese 2017, p. 346). Thus, we may wonder whether the formalisation of local power and property rights around and after 1100 cannot be explained, *too*, by the competition for local resources and the subsequent need to define precisely jurisdictional remits, rather than with the legacy of the Carolingian project; in broader terms, whether the 'political' did not derive from the 'economic'.

In my opinion, it would be extremely interesting for future research to examine the interplay between these aspects through the study of areas whose primary sources allow us to reconsider social, political and economic history at the same time, in order to avoid the risks implied by monocausal explanations of change. Let us think, for example, of the 'Investiture Controversy' in central and northern Italy. It is beyond

doubt that the 'Controversy' resulted in a profound crisis of Carolingian-style public order. All the same, dynastic failures and legitimacy crises were not a new thing in the final quarter of the eleventh century; they had happened before, for instance in the decades straddling the ninth and the tenth centuries and in the early eleventh century. As a consequence, one may wonder whether the 'Investiture Controversy' and the political disruptions it created can be really regarded as the main premise of the 'revolution' that followed, or whether we should consider them as part of a more complex picture (cf. on this Bianchi and Collavini 2017, esp. pp. 175-6).

I shall not deny that my own sensibility plays a role here. In fact, we have good reasons to believe that a clearer definition of local aristocracies was a primarily politically-driven phenomenon in some cases. The rather informal elites of some Italian cities turned into increasingly structured governments as a result of the collapse of traditional powers in the early twelfth century (even though one may observe that the break-up of the *regnum Italiae* was recent around 1100; cf. Wickham 2014). On a very general level, however, I tend to think that the spectrum of 'banal' and rent-based exactions, and the ways these could be imposed, is what needs to be looked at in order to reconsider properly the 'feudal revolution' and reconcile its political and economic aspects. It would be desirable, for instance, to distinguish between obligations that affected agricultural production, thus causing an increase in the extraction of the surplus, and obligations which are not directly connected to rural productivity, but only with social dependence – like castle-linked duties. This might help us to develop an ever more precise chronology of change and, subsequently, of its causes. We could thus further renovate the debate to which West, Fiore, Schroeder and Cortese have greatly contributed.

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