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In western Europe – thus runs one of the standard narratives of medieval history – it is only after *c.*1200 that we really find the beginnings of administrative bureaucracies, which allowed for the growth of centralised governments, and were fed by the rise of professional law, enabled by growing literacy at various levels of society, and were one of the key elements in what John Watts has characterised as ‘the making of polities’ in later medieval Europe.⁽¹⁾ These changes also, one might add, relate to the growing bureaucratisation and formalisation of the Roman Church in the wake of the reforms of the tenth and especially the 11th centuries. Also in this same period, the manorial system finally gave way (on the continent), the ‘commercial revolution’ took off, and the motors of the economy shifted to the rapidly expanding urban centres.

This view of the period has, of course, been disputed, but it is nevertheless well-entrenched, in one form or another, in the scholarship.⁽²⁾ But what of the Carolingians? Did they not also create an administrative bureaucracy and bring into being a centralised government? Did they not, in addition, introduce an array of reforms to the Church? Isn’t the high level of Carolingian literacy now standard fare even among undergraduates? And do we not now witness economic growth, increasing trade, and even the beginnings of the monetisation of the rural economy?⁽³⁾ Well, yes, to all of these questions; but something funny happened on the way to the 13th century. According to some influential interpretations, after *c.*900, not just

the Carolingians, but also their 'Renaissance' vanished. In its wake came the 'feudal revolution' of the late tenth and 11th centuries: a decline of literacy and written administration, an increase in the importance of patronage and (patrilineal, locally-based) kin groups, privatised, decentralised jurisdiction in the hands of local lords based in the castles that were now being built everywhere, all coupled with a significant growth in incidences of unregulated, non-state violence. It is, at least in part, in reaction to these developments, culminating in the 'the crisis of the twelfth century', that the later transformations described in the previous paragraph are thought to have their origins.⁽⁴⁾ While the importance, in various respects, of the central middle ages would not seem to be in dispute, how, or even whether, this period (c.900–c.1200) relates to the Carolingians remains a thorny issue. Narratives regarding the growth of government, the 'feudal revolution', or ecclesiastical reform in the 11th and 12th centuries, depend in turn on other narratives: of post-Carolingian decline, or, for some, of the 'Carolingian Renaissance' as an illusion that only happened in the minds and writings of isolated intellectuals, with little bearing on facts on the ground.

In these two meticulously researched and wonderfully stimulating books, John Eldevik and (in particularly pointed fashion) Charles West provide compelling new ways of understanding both transformation and continuity from the Carolingian period through to the 12th century, Eldevik by means of an innovative and careful analysis of one quite specific topic with broad implications, West through a more wide-ranging study of the exercise, articulation and representation of aristocratic and royal power in this period. While the place of various strata of the lay aristocracy has often been studied in some detail, bishops as lords have received rather less attention, and Eldevik examines, with the example of detailed studies of the dioceses of Lucca, Salzburg and Mainz, how bishops functioned within the changing structures of power and landownership, and how this was reflected in the use and control of tithes. West provides a study of changes in power structures, relations of domination, and forms of symbolic communication, within the regions of what became Upper Lotharingia and Champagne, the heartlands of royal power during the Carolingian period – though at the peripheries of both the Empire and the Kingdom of France from the tenth century. While West sticks quite closely to his stated period, Eldevik's title is slightly misleading in this respect: he hardly enters the 12th century at all, with only a few pages towards the end of the book covering the period after 1100; conversely, the first two chapters and significant portions of the third are devoted to the period before 950, stretching as far back as the sixth century and the early tithing practices in the Frankish kingdoms. Although the stress in both works on the continuities with the Carolingian period – which both West and Eldevik rightly argue need to be understood alongside the transformations – is not entirely new ⁽⁵⁾, it seems to me that both authors go rather further than most earlier scholarship in making these continuities explicit, and bringing out the ways in which they relate to the changes that also undoubtedly took place. Both books are sure to become essential reading for anyone with a serious interest in understanding how the middle of the middle ages came into being and functioned.

The tithe – a tenth of the produce of agricultural land, or a tenth of one's income from other sources, owed as a religious obligation to the Church – had not always been a religious duty: it derived both from a secular tax, and from an (initially voluntary) obligation to support the Church. Only in the fifth century did ecclesiastical tithes begin to be formalised, along with the principle of quadripartition (for the church fabric, the parish clergy, the bishop, and the poor); only in the later sixth century does a council say that non-payment of tithes should lead to excommunication. Fiscal tithes, paid to the crown, continued to be exacted in the Carolingian period, and indeed in this period ecclesiastical incomes could derive from these tithes, since Charlemagne donated many such tithes to the support of churches in the regions from where they were paid. This already underlines the significance of the Carolingian period in the later development of the tithe: it is during the ninth century that tithes became universal obligations, enforced by the secular authority, and used as a tool for the expansion of political power and economic control in newly conquered territories. How exactly tithes were to function, however, remained a matter of some debate, since the Carolingian Church, while asserting the importance and authority of the bishops, simultaneously stressed the significance of local

churches and parish priests managing affairs at the local level. Given that the tithe was literally a tenth of the produce, it was an important economic resource, and the collection of the tithes at parish churches thus made these churches into attractive places to control for secular lords as well. This is one of the reasons why bishops sometimes had trouble regulating and controlling tithes in their dioceses; a further problem they faced came from the immunities from episcopal control granted to monasteries that allowed the latter effective control over parish churches, thus not only depriving the bishops of economic resources, but also potentially undermining their ability to administer parish churches in their diocese. Furthermore, tithes, like other payments, could also be viewed as part of a system of gift-giving and patronage, with tithe-payers potentially expecting favours in return beyond God's grace. Eldevik stresses here that normative texts did not actually function terribly well in determining tithe payments: it was through disputes and efforts to resolve them that practices of tithing were actually settled, and networks of patronage and kinship were often rather more effective than the letter of the law, even in the Carolingian period.

Eldevik demonstrates effectively both that Carolingian regulations did actually work, and also that local networks of patronage and kinship often functioned quite independently of the central administration. The period cannot therefore be seen either as some sort of medieval anarchy of the sort some scholars think they find in the later centuries, nor as characterised by a completely effective, centralised administration. Rather, it was a bit of both. This mixed characterisation is brought out more fully by West. More explicitly than Eldevik, he shows that both perspectives on the Carolingian period are well-supported by the documentation from his region. Moving beyond the capitulary evidence, he argues that, for example, counts did actually hold public meetings to issue judgements as acts of public, royal justice; and royal authority was not just theoretical, but actually exercised through the control of the fisc and of royal monasteries, through the itineracy of the kings whose presence functioned as an effective reminder of power, and by means of the *missi*, who travelled widely to enforce royal writ. Like Eldevik, West stresses the importance of Carolingian bishops within the kingdom, for which reason their appointments were largely matters settled by the royal court; but apart from providing financial resources, military support and administrative advice to the court, bishops also effectively administered their dioceses, examining and appointing parish priests, visiting parishes and ensuring that priests attained the level of Latinity and moral and intellectual probity that was decreed necessary. While a good part of the evidence cited is anecdotal and perhaps not quite as reliable as West suggests (hagiographers would have had an interest in showing that the holy water of a church was effective; this need not say much about what villagers actually thought), his main point seems plausible enough: the Church was a real factor in people's lives at the lowest levels of rural society, and the efforts made by bishops to control and reform the Church did have some real effect. Despite all this, central power had limits. As Eldevik also shows with regard to bishops, appointments to office took place through a system of patronage and many offices were passed on within networks of kin. Lords exercised power through violence, and while most of it was in the context of large-scale conflict, lords did also beat up their peasants on and off, the sort of behaviour supposedly belonging to the later 'feudal revolution'. Furthermore, counts and *missi* were not solely royal agents, but also had local interests that they pursued for the private profit of themselves and their families. West rightly argues that royal initiative and local power were not opposing forces: wherever the Carolingian institutions had any effect, it was as a result of a compromise with local interests.

Somewhat less clear are West's efforts to distinguish between owning property and exercising power. He argues that compulsory labour services (*corvée*) were a form of tribute, and that therefore 'ownership was a form of tribute-taking' (p. 71); thus '*villae* were in reality not so much owned as dominated' (p. 72). But that *people* were dominated does not necessarily tell us very much about the conception of property attached to the *land* on which these people worked and lived. Owning land and dominating people who work the land need not be seen as analogous, and the fact that the latter is a dominant social relation seems to me no reason to call into question the reality of the former. Moreover, to call enforced labour services a form of tribute,

while perhaps following the rhetoric of the sources, might go a bit too far: there is a huge difference between a tribute received by an armed person from another armed person with the ability to resist, and from an unarmed person without such an ability. It is also not clear how changes in the manner of domination relate to economic change: whatever happened in the tenth and 11th century in the former sphere, I think there is little disputing that these centuries were a time of growing economic complexity and arguably production as well. Furthermore, as West himself concedes, there is no reason to believe that the function of the state was to diminish lords' ability to coerce, through violence, their dependents. Although it is certainly true that there was always a rhetoric of the royal maintenance of peace, surely the major concern of kings in this respect, throughout the middle ages, was less a matter of preventing lords from violently coercing their peasants to render 'tribute', but rather a matter of preventing them from exercising violence against *other* lords (and the latter's peasants). Relative levels of domination of the peasantry say little about the effectiveness of government because no medieval government was much concerned with restraining such domination, as long as that domination did not come into competition with others of the dominating class.

West's explicit aim is to synthesise the conflicting views of the effectiveness of the Carolingian state, which he believes can be done by turning to the insights gained from the study of symbolic communication and the gift. I am a little sceptical of the argument that peasant renders were really thought of (by whom?) as 'gifts': perhaps this is what the texts sometimes call them, but if the giving of gifts is enforced by the violent exercise of power, it becomes an obligation, and, I would think, an extortion. Although it may be true that gift-exchange and violence 'were mechanisms of communication, ways of articulating status and relative social positions' (p. 84), he may have made more central to his argument the fact that the violence was primarily a means of ensuring that the gifts were given, making them in effect compulsory rents or renders. The purpose of the 'gift' was, for the giver, to avoid violence upon her or his body, and for the receiver to provide material enrichment; when one recalls – as one must – that the fundamental issue here is about a coercive extraction of and control over resources, which are precisely what shored up status and social position, thinking of what was happening as 'mechanisms of communication' in order to 'articulate status' seems a bit euphemistic. It seems to me that sometimes in the language of communication and gift the central fact of the economy of this period (and indeed later ones as well) – that it functioned by means of at least the threat, and often the reality, of actual physical violence – gets blurred. Doubtless those who produced the texts that are our evidence wouldn't particularly mind, and would have been happy to emphasise the symbolic more than the material aspects; I am sure the peasants who had to face the possibility of physical harm would have seen things somewhat differently, for which reason they might not have been so keen to think of 'gifts' in these terms.

More compelling are West's arguments regarding the importance of ceremony and symbolic communication at the higher levels of society, which he suggests became more prominent as means of organising social interaction. West believes that there is no need to think of a dichotomy between 'state' and 'lordship' if one approaches the issues of power and control from the perspective of gift-giving. He makes the further important point that – contrary to a good deal of the scholarship on this period – there is no necessary dichotomy between symbolic communication and the written word: bureaucracy and ritual were two sides of the same coin. Government, property, written and symbolic communication, aristocratic domination and institutions, were all categories that were related to each other. Both written and gestural or symbolic forms of communication were means of greater control, methods by which, without the apparatus of the Roman state system and its tax apparatus, an empire could nevertheless be managed. The state and lordship were both means of managing resources through the exercise of power, and there was no necessary opposition between public and private power – indeed, central public power depended on the resources of the localities, necessarily at least partly private, and we know that many public officials were equally engaged in estate management at the local level.

These arguments raise some interesting questions: why did all this happen *now*? Is it the case that there was *more* control than in earlier periods, and these ‘symbolic’ aspects were expressions of this greater control? Or was there more *competition* for control, making it necessary to have more symbolic expressions of control as one means of trying to work through that competition before a resort to outright violence? Ultimately, power was based on the control of resources: on production. Why did it seem necessary at this point to control resources more closely – was it because there was a growing non-producing class requiring more production to enable its social reproduction; or increasing competition among that class; or both? How do the developments of the ninth century relate to what happened in the tenth, and especially the 11th century and beyond: demographic expansion, greater monetisation, urbanisation, and probably growth in production as well? These later developments suggest that regardless of how we assess the ways in which lordship, the state, and the nature of symbolic communication might have changed, resource extraction did not diminish; so, given that other methods of extracting resources that enabled growth could be found later, why were the methods West finds in the Carolingian period necessary at that time? Here it might be worth recalling Timothy Reuter’s arguments regarding the importance of plunder and tribute in the Carolingian world: these were the means by which the wheels of power were kept lubricated, but after a point, without enemies on the border, there was no one to plunder.⁽⁶⁾ Hence, surely, the need to increase control over the peasants in order to get more ‘tribute’ from them; hence, I suspect, the growing importance of projecting power in relation to others who had power, as competition for resources increased; hence also, perhaps, the fact that symbols and forms of power might have been somewhat simpler in Ottonian Germany – there was still room to find plunder further east.

The ‘long tenth century’ is characterised by an ‘ebbing of royal power’ (something equally evident in Lotharingia, Tuscany, and Salzburg, though perhaps less so in Mainz). It also seems to be the case that local families became more local: while earlier they had had power bases elsewhere in the Carolingian empire as well, now they were more firmly rooted within their individual regions, and shored up their power through the building of castles and the consolidation of holdings within the locality. West is aware of the problem of using the developments in his region to characterise the period more generally: the kings both in the east and the west were no longer based in Lotharingia, so it is hardly surprising that their interest in maintaining a presence here receded as well. He explains the development by suggesting that Carolingian policies had been so successful that the forms of local governance no longer needed the royal court as a ‘clearing house’ (p. 136). It was not so much that kings were no longer able to coerce, but rather that regions and aristocrats ‘disengaged from courts which no longer had anything substantial to offer’, and thus the elites ‘opted out’ (*ibid.*). But surely the fact that elites were able to disengage suggests either that they could do so because the kings could not coerce – which would suggest a genuine weakening of kingship, along the lines of the ‘feudal revolution’ argument, but apparently rejected by West; or because the kings were no longer interested, because the region was now marginal to their interests – an argument also rejected by West.

One way in which local elites managed to become more entrenched in their local power base is revealed by the evidence regarding the slow building up of territorial lordships in the form of genuine counties.⁽⁷⁾ While earlier, the *pagi* had been largely descriptive markers of a region, not linked specifically with a single count or political centre, now the term *comitatus* began to be used, which did map quite clearly onto a particular, bounded space, and might not have been precisely coterminous in a geographical sense with the *pagus* of the same name; and the *comitatus* tend to be linked to fixed abodes of counts, not recorded in the Carolingian period. It is less clear how the growth of territorial lordship related to changes in the conception and practice of landlordship. This period saw the beginnings of the breakdown of the bipartite estate, but what this means is a matter of some debate, since the same period witnessed a growth in commercialisation and – particularly

around this region – urbanisation, which suggests that production and productivity certainly did not retreat; West's exposition does not really address how these developments relate to changing patterns of power.⁽⁸⁾ He makes much of the fact that charters now record dues owed by those working the land, whereas earlier they had recorded dues owed by lessees; this is interpreted as a greater level of interest in documenting landlordship. But the earlier polyptychs had also recorded dues owed by those working the land, and I do not see how these earlier sources are evidence only of estate management, whereas the later sources betray a greater interest in landownership: the estates recorded in the polyptychs were thought of as belonging to the monasteries that produced those documents, and the management of those estates was predicated on a belief that they belonged to the monasteries that managed them. I find unconvincing the argument that that while property ownership might not have been 'the best way of conceptualising Carolingian relations of production, even though that was clearly the aspiration of those who made formal records of it, it would seem to be rather more appropriate by the end of the tenth century' (p. 157). The distinction between domination of people and the ownership of property is not, to my mind, really convincing in showing that the property was in fact not owned; the fact that the social relations that determined how production took place are relations of coercive domination of people does not mean that the land on which that production takes place is not thought of as property.

How all of this relates to West's arguments regarding what he calls 'symbolic impoverishment' in the tenth century – meanings of rituals and ceremonies became less polyvalent and more fixed – is also, to my mind, quite unclear. I am not sure, moreover, that he can prove there was a greater level of fascination with ritual and ceremony in this period than in the preceding century. While he is certainly right about the importance of ritual scenes in the narratives of, for example, Thietmar of Merseburg or Dudo of St Quentin, he himself acknowledges that the emphasis on 'the rhetoric of the scene' was a feature of much earlier narrative texts as well.⁽⁹⁾ I am not so sure that in the tenth-century narratives scenes had more of a real meaning in and of themselves, whereas earlier they were 'just' means of moving the story forward: this seems like a not very fair characterisation at least of Paul the Deacon and Gregory of Tours, and probably also of Notker. West's suggestion that in the Carolingian period, there was a 'pervasive uncertainty' about the meanings of symbolic communication (p. 92), whereas in the Ottonian period things became less ambiguous, seems fair enough. But in trying to understand the causes of this, we should recall one key aspect of the later Carolingian period (whence almost all of West's examples are drawn): there was a surfeit of Carolingians, and thus a constant contestation of power. Under the Ottonians, there was less of such competition perhaps simply because there were fewer legitimate rivals; perhaps meaning appears more stable because we have only one point of view in that textual production is more hegemonic – a hardly surprising effect of the stability of the Ottonians when viewed against the dispute-ridden succession issues of the multifarious Carolingians after Louis the Pious.⁽¹⁰⁾ Therefore, it seems to me far from proven (and to be fair, West does concede that his arguments are inconclusive) that this shift, insofar as there is one, is necessarily to be understood as a change in the historical imagination rather than a reflection of the fact that there was genuinely less political competition.

The tenth and early 11th centuries might present ambiguous evidence regarding the nature of lordship and communication, but they clearly were a time when relations of power were changing and becoming more formalised at more local levels; both the books under review agree on this point. Among the things that changed were the ways in which bishops related to their dioceses and attempted to control their tithes, developments related to both the rise of lay territorial lordship, and the bishops' own increasing sense of themselves as territorial lords. This is evident from all three examples Eldevik examines, though Lucca, Salzburg, and Mainz belong to quite different social and political contexts. In Lucca, landownership in the ninth century was far more fragmented than in the other two dioceses, leading to more diffuse structures of power; there was not very much in terms of a royal or fiscal presence here with regard to landownership, though the kings did, throughout this period, have an interest in local politics and patronage. In the ninth

century, donations made by landowners to the diocese dried up, but leases of parishes – *pievi* – (including their tithes) by the bishop quickened, and *pievi* continued to be leased out through the tenth and into the early 11th century, by which point bishops were leasing out whole *pievi* to individual aristocrats instead of just fragments, as they had done earlier.⁽¹¹⁾ By the late 11th century, however, writers within the Church were beginning to excoriate the earlier alienations of Church properties; modern scholarship has often followed these sources and identified the earlier period as one of moral decline, or at least a too-close connection between secular and ecclesiastical interests. However, Eldevik argues convincingly that the later rhetoric should not be accepted at face value. (John Nightingale presented a similar argument with regard to Lotharingia.⁽¹²⁾) There is a difference between a lease made for the purpose of patronage, creating ties of fidelity between the diocese and local elites, thus increasing the influence of the Church, and one that has become a permanent lay fief. Although the shift in perception of the later writers is certainly in itself important, we need to judge the practices of the ninth and tenth centuries on their own terms.

The regional aristocracy dominated the Lucchese episcopacy, and from the late ninth century, the royal presence was increasingly distant. The leasing of *pievi* was therefore in that period a useful ‘tool that allowed bishops to create ties of loyalty and fidelity between themselves and important laymen’, from which, because both resources and ‘symbolic capital’ accrued to the lessee, the bishops attained the status of patrons (pp. 154; 155). It is perhaps also not a coincidence that in many cases beneficiaries were related to the bishop. However, during the course of the 11th century, the Lucchese bishops became increasingly disconnected from the local aristocracy, and more closely tied to the Canossan and thus pro-reform party; thus their growing disinclination to lease church property should be seen not simply as a matter of religious ethics, but also as a political move. Furthermore, it needs to be understood in the context of developments similar to what West finds in his regions: the increasingly localised control of land by local lords by means of fortifying settlements and building up more dependent military retinues. As lay landowners became more independent, their claims to their leases also made the latter seem more like their own property; it is also in part a reaction to this that bishops became more reluctant to lease *pievi* and tithes, and eventually began to build their own castles and retinues of *milites*. It is arguably also at least in part because of the growing loss of control over tithe rights resulting from their being perceived as hereditary that later ecclesiastical writers took such a dim view of earlier practices. The changes in practices of tithe control in Lucca can therefore usefully be understood in the context of the sort of increasing formalisation of regional power that both these books suggest was common in many regions of post-Carolingian Europe.

Similar developments are found in Salzburg, since here as well descendants of lessees of ecclesiastical properties in the ninth century began later to view their leases as heritable property, thus by the 11th and 12th century depriving the Church of lands and income. As in Lucca, earlier practices of leasing tithes had been part of an effort to increase the influence of the archdiocese by expanding its networks of patronage, but as lay tithe-holders – and archbishops themselves – began to strengthen their claims to territorial lordship, tithes were, by the late 11th century, largely leased to people who appear to have belonged to the class of *ministeriales* and were thus more directly dependents of the bishops. (The development of this class here and its relation to the Salzburg Church has been examined in some detail in a number of publications of John Freed.⁽¹³⁾) The changes in tithing policy, here as in Lucca, might well have been influenced by the fact that the bishops in question were in both places now no longer so closely embedded within the local aristocratic kin-groups; much of the ‘reform’ at Salzburg occurred under Archbishop Gebhard I as a means of exercising and establishing ecclesiastical authority, as a counterbalance to an increasingly independent and powerful lay aristocratic class. Gebhard did not belong to the local aristocracy himself, and he seems to have been the last archbishop to make efforts to tap into the local aristocratic networks. As in Lotharingia and Lucca, local aristocrats from the late 11th century increasingly concentrated on patrilineal kin-groups centred on particular castles ⁽¹⁴⁾, and the archbishops’ patronage efforts were focussed increasingly on the episcopal *familia*, here consisting of *ministeriales*. Eldevik’s argument is therefore that since in the tenth

century the diffusion of ecclesiastical property had not been the result of 'feudalisation', but rather part of a set of adjustments to slowly-changing relationships between kin-groups and various institutions, granting tithes as a means of cementing relationships could still be seen as 'augmenting the church' in the early 11th century (pp. 214; 265). From the later 11th century, however, in response to similar trends among the lay aristocracy, the bishops began increasingly to cultivate 'relations of lordship based more on hierarchical authority than bilateral attempts to augment the church through exchange and reciprocity' (p. 214).

Mainz presents a somewhat different case, since here, the archbishops derived their power from effective control of the economic resources of the town, its tolls, and its mint, rather than from rural landholding and tithes as was the case in Lucca and Salzburg. The archdiocese of Mainz, at the heart of the old East Frankish kingdom, was also populated with old and powerful monasteries, notably Fulda and Hersfeld, both of which clung stubbornly to their immunities, including the privileges of tithes from parish churches. The efforts made by the archbishops of Mainz in the 11th century – and Eldevik focuses on Siegfried of Mainz in the 1070s – were thus more attempts to enforce episcopal authority even over other ecclesiastical landowners, rather than primarily (as was the case in Lucca and Salzburg) to secure power relative to lay landowners and increase revenues. While other German sees also made efforts to reclaim tithes that had earlier been leased, and to assert their presence through tithe inquests, the financial need at Mainz was rather less pressing than elsewhere. The example of Mainz thus demonstrates, perhaps more clearly than the others, the importance given by bishops at this time to establishing clear lines of authority: just as lay territorial lordship was becoming more clearly defined and hierarchies were becoming more fixed, so the ecclesiastical system was increasingly being viewed in terms of stable vertical structures. The reform of tithing practices was thus a matter of demonstrating the seriousness of purpose, presence, and power of the bishop; reforming was a sign of authority and established (if successful) the bishop firmly at the top of the ecclesiastical pyramid. These reforms not only brought the morals and liturgical practices under closer scrutiny, but also the ways in which church property was disposed of. Furthermore, here, as at Lucca and Salzburg, the bishops increasingly cultivated their own armed retinue, and claimed, in return for granting rights to tithes, hospitality from the recipients, behaving in this respect exactly like lay lords.

Eldevik concludes that changes in tithing practises are an element of the clarification and expansion of concepts of property – a result similar to what West finds in relation to jurisdiction. Tithes had become compulsory under the Carolingians, and had also been at that point tools in the efforts to cultivate client–patron relations; these two aspects of the tithe remained in force, but over time, they came to be understood as a form of alienable property. The growing efforts to control tithes more closely must thus be understood within a broader context of a more exclusive conception of property attaching to more things than land, and the growth of 'more territorialized conceptions of power and a shift away from the kinds of multilateral, horizontal relationships that had defined the episcopal church and its tithe regime in the early middle ages' (p. 258). Given the context of increasingly independent-minded lay territorial lords, who were more and more inclined to treat tithes as heritable property rather than as gifts from the patron (and here once more there is a similarity with West's arguments regarding property and gift), it seems to me that these efforts are also to be understood as a reaction to a growing difficulty in controlling clients; hence the turn to *ministeriales* rather than maintaining a broader web of patronage. In this context I feel Eldevik's arguments would have profited from consideration of the problem of advocacy. (This was a system by which lay lords performed, in return for various perquisites, jurisdictional functions of ecclesiastical lords that the latter were unable to fulfil because of religious restrictions on members of the Church.) Lay advocates were becoming more of a thorn in the side of ecclesiastical landlords from the 11th century onwards, treating their rights arising from advocacies more like property, and it seems obvious that this would have been a further – and important – factor influencing the bishops' turn to making the grant of tithes more limited, as 'a private grant from the bishop to a favoured vassal' (p. 265).[\(15\)](#)

Like Eldevik, West presents a compelling argument that the 11th century witnessed a growing territorialisation of power, coupled with notions of property being attached to things other than land. This has been seen as the period of the rise of the 'seigneurie banale' or 'Bannherrschaft', and West, while sensibly pointing out that private violence as a means of domination was hardly new and jurisdictional rights had even earlier not been solely royal perquisites, also stresses that now these rights had become things that could be bought and sold, quite independent of any land with which they might be associated. This development cannot be understood just in terms of a usurpation of royal power, since it occurs more than a century after the fading of Carolingian power in this region. Political power in the 11th century became reified into a form of property, which could now also be transferred, like other property. Associated with this change is the appearance of what seems to be a very locally-based lordship one level down from territorial lordship, with the rise of lords of individual villages (coupled with the increasing use of surnames, normally toponyms), and knights asserting what West calls 'micro-dominance' (p. 189). What emerges from the work of West and Eldevik together is that by the late 11th century, there is an increasingly clearly articulated distinction between three kinds of alienable property or quasi-property rights: rights over land, rights to income arising from jurisdiction (which had previously been primarily a non-alienable royal perquisite), and tithes. All of these could be viewed equally under the rubric of surplus extraction mechanisms, and it seems to me that a fruitful way of thinking about this set of issues might be asking why and whether the elites felt a need to extract more surplus, and the extent to which they did so as a reaction to the possibility of more surplus, or whether rather the latter arose out of elite demands. In either case, these issues cannot, surely, be taken separately from the changes in agricultural production, including land clearance, specialisation, urbanisation, and trade. West certainly does begin his discussion of bannal lordship with a survey of the economic changes that took place, but he doesn't really show how its rise is connected by anything more than chronological coincidence to the fragmentation of the *mansus*, the expansion of the population, and the commutation of labour services for rents. He suggests that perhaps the castle-building activity that was so vibrant in this period, and led to the build-up of local centres of military power that were the foci of jurisdictional lordship, was funded more by jurisdictional revenues than rents from land, but even he must admit that the sources do not allow anything beyond very tentative speculation on this point. In any case, this argument would not explain why *rents* (as distinct from the perquisites of bannal lordship) should have been converted to coin, nor why labour services should have been converted to natural rents. Perhaps it is the case – however bizarre it might sound – that despite the violence of the period, and the castles and growing private retinues of armed men, there was some effective passive peasant resistance to labour services, and given a growing population and the availability of markets that allowed peasants to raise cash rents, lords simply found it easier to take money rather than coerce their peasants. Such an explanation would suggest, however, that at least some peasants also benefited in some manner from the changes taking place, not just the lords; perhaps this is one reason why there might not have been any need for the sort of increase in lords' violence at the very local level that West believes – contrary to much earlier scholarship – did not take place.

I find West's argument that the new form of domination was now 'primarily a matter of owning rights over people' rather than land (p. 190) a bit confusing. Land ownership was still as much a form of domination as it had been earlier, and it is not clear to me that jurisdictional rights are necessarily 'rights over people': surely they are, in most cases at least, rights to collect revenues arising from taxing the actions of people. Here some further discussion on people's status might have been helpful: in some regions at least, it is thought that while by the ninth century there was a blurring of boundaries between free and unfree (as indeed West also suggests), there was by the 12th century also (at least in the German lands) a separation of three aspects of domination, over land ('Grundherrschaft'), jurisdictional rights ('Gerichtsherrschaft' and/or 'Bannherrschaft'), and persons ('Leibherrschaft'). West might have been more clear in distinguishing these kinds of domination – or establishing that the boundaries were not so clear. It is only in the last instance – fees or services owing from people who are of unfree status – that we can really speak of domination now

being a matter of owning rights over people ('Leibherrschaft' is literally lordship over the body – the 'Leib' – of the dependent person); in the other cases it is still a matter of the land on which people live and work, or the things people do or do not do. And jurisdictional rights pertaining to an individual's actions are not synonymous with ownership of that person. Nor is it really clear from West's arguments how changing forms of domination actually contributed to increasing 'the efficiency of production' (p. 197). Perhaps Reuter's arguments are once again of interest here: if, at least in this region, no further 'plunder and tribute' was possible, bannal lordship with the control of its revenues, *and* increasing commercialisation, could both be seen as forms of expansion of material resources without requiring expansion of the controlled land surface. There is a further issue here, namely a distinction between lords' methods of increasing their revenues, some involving competition with other lords over sources of income, and others rather a matter of increasing incomes from already existing sources. What lords did as forms of competition (building castles, maintaining military retinues, ravaging the countryside of disputed territories, disputing tithes) might have augmented their revenues, without increasing the overall surplus and the efficiency of production, which developments might, however, have been aided by changing the forms of surplus extraction (including introducing or allowing new forms of tenure and rents) and by squeezing more surplus out of (new) jurisdictional perquisites. Approaching the question in this manner might be helpful in understanding both the 'feudal revolution'-problem of more powerful local lordship, and how this related to the continuing growth of economic complexity.

The problem of 'feudalisation' has been linked to the issue of whether, in this period, we see the rise of 'fiefs and vassals', to cite the title of Susan Reynolds's complex and controversial book, published two decades ago, in which Reynolds argued that in fact these terms are not useful at all for this period.⁽¹⁶⁾ While agreeing broadly with Reynolds's premises, West also points out that the term *feudum* in fact now begins to be used in a variety of sources, and cannot therefore just be a matter of changing habits of diplomatic; and increasingly, tenurial dependence did indeed also come to be more closely related to social and political dependence. Political power was now being formalised in terms of control over landed property as well; thus, paradoxically perhaps, while there were clearer and better articulated notions of property extending even beyond land, there was also now a greater sense of multiple layers of ownership of land, with people lower down the rung requiring explicit permission to alienate their lands. This development is equally clear in Eldevik's findings regarding the changing nature of the patronage of bishops, who now tended to grant rights to tithes only to their vassals; the relationship of lord and vassal was, however, arguably rather more clear in Germany insofar that many vassals were *ministeriales*, who were not just in some manner dependent, but also formally unfree. But even outside Germany, West argues that while there probably was no close link between the grant of fiefs and the specific terminology of vassalage (and here he agrees with Reynolds), insofar as landownership was more closely linked to patronage, and ownership of land was also the holding of a fief, the latter aspect of landownership was established through relations of homage. There appears not to be very much in the way of evidence to show, in this period, what precisely this homage might have meant in concrete terms, however; and West's turn to the 'investiture conflict' is not, to my mind, a terribly compelling means of demonstrating the importance of homage at other levels of society.

The broader point made by both these books is that there was a growing formalisation of the ways in which property and power were conceived and articulated, and a strengthening of vertical hierarchies of (now more explicitly defined) client-patron relationships at the expense of more fluid, horizontal relations of *inter*dependence; these developments are not, however, to be seen in any way as in contradiction to a state that could and did exercise a form of centralised authority. Social relations were increasingly reified, and this reification included within its scope matter such as homage, tithe, and social status, which had earlier been less subject to alienation and exchange; these new forms of social relations were also bounded by more clear means of symbolic communication. Here, the regional differences are also important. While bishops and lay lords alike, within and without the German Empire, built up territorial lordships based in increasingly

closely-controlled military retinues and funded by both rents and jurisdictional perquisites which were now equally alienable (and need not have been tied to the same blocks of land), there were differences, within the regions West examines, between the west and the east. The counts of Champagne, West argues, were able to create a more effective regional hegemony than the dukes of Upper Lotharingia; this was, he believes, because of the significance of ecclesiastical advocacy in the east. While monasteries (and bishops, as we have seen) sought to build up their own *familiae* not least in order to escape the problems posed by advocacy, it nevertheless became the case that the Lotharingian principalities derived a good portion of their revenues from the jurisdictional rights arising from advocacy. The consequence of the importance of advocacy in the east was that while the duke had strong rights in his heartlands, his rivals had equally strong rights in the regions where they were advocates; in Champagne, however, since there were less close links at the local level, the counts were able to gain more control over local resources by the 12th century. The reason why advocacy – like the *ministeriales* – was so prominent within the Empire but less so outside it awaits a compelling explanation; West does not attempt one, but speculates that it might have been because of the survival of older monasteries in the east, while in the west the older monasteries lost their power and influence. He also suggests that advocacy might have been more important in the east because from the late 11th and especially in the 12th century, emperors had begun to pay attention to and eventually legislate about it – something West believes was a sign of ‘the coherence of the Empire’ (p. 250), a view of the medieval Empire that one does not often encounter (which is not to say there is no validity to it)! Nevertheless, while with regard to advocacy and its effects in terms of allowing for the growth of a more powerful principality in Upper Lotharingia versus Champagne, there was a difference between east and west, in other respects the developments were very similar. (And in other parts of the Empire, we should note, the significance of advocacy notwithstanding, very large principalities could and did arise, suggesting that perhaps West’s arguments regarding the differences between Upper Lotharingia and Champagne might need further refinement; the relative economic positions of both regions in this respect might deserve more attention that West is willing to grant.)

Eldevik and West agree, in broad terms, about how to characterise the period between c.800 and c.1150: innovations of the Carolingian period in terms of landownership, resource extraction, and symbolic communication, continued to have an effect in the tenth century and beyond, and – following the trends of the Carolingian period – they were increasingly formalised, removed from an explicit link with a particular royal court, and made into perhaps more static forms of hierarchical control over resources. The differences between the beginning and end of this long period do not have to do with more or less violence, greater or lesser dilution of the Church by involvement in secular affairs, or a collapse of public order. At one level, the differences were, both authors argue, a matter of representation: the ways in which power relations were presented changed. At another level, there was indeed a greater level of explicit reification of various kinds of power relations, not all having to do with landownership. Nevertheless, there was no real caesura either with the end of the Carolingians or the ‘feudal revolution’ of the tenth century (West), nor with the Gregorian reforms of the 11th century and a ‘crisis of feudalism’ in the 12th (Eldevik). West goes further than Eldevik, however, arguing that the 12th century arose out of a path-dependency arising from Carolingian innovations, and should be understood as ‘the consummation of the Carolingian moment’ (p. 260). It is precisely a result of Carolingian success that we have what might be called Carolingian failure: because of the increasing stability – striven towards by the Carolingians – of symbolic and written means of communication, and because of the increasingly accepted authority of local figures, it was no longer necessary either for this authority or the meaning of various forms of communication to be mediated by the court. Thus success bred irrelevance.

Choosing a point where path-dependency can be said to begin is difficult. If the ‘long tenth century’ was a period of transition between the Carolingians and the more formal, ‘feudalised’ world of the 11th and 12th, the (also ‘long’) ninth century, one could argue, was part of the development from the eighth century (less

commercialised, less overt control over peasantry and more independent owner-cultivators, less clear expression of ritual, less state) to the tenth (more control, more commerce and money, declining peasant independence, more clear ritual). To be sure, the centralised state fades in the tenth, but perhaps this should be seen less as the decease of empire and more as the birth of territorialisation, which might be understood as state formation on a smaller scale – certainly an approach that would seem appropriate for Germany. (Perhaps we should here recall the relative sizes of, for example, England vs Germany: if the centralised state faded in Germany, the developments of the period at hand surely contributed to the rise of principalities such as Bavaria, which was very large indeed.)⁽¹⁷⁾ All history is transition; so did the Carolingians really matter? And what is it about them that did? If the ninth century was so important for the 12th, we cannot really evade the question: whence the ninth? If we agree with a thesis that grants so much weight to the Carolingians, because it was only under them that ‘Francia was no longer scrabbling about amidst the ruins of the Roman Empire’ (West, p. 263), we are also inevitably dependent on some form of a ‘Fall of Rome and End of Civilisation’ narrative, to cite the title of Bryan Ward-Perkins’s deliberately provocative book.⁽¹⁸⁾ While a historian such as Chris Wickham might take exception to some aspects of this characterisation ⁽¹⁹⁾, both Ward-Perkins and Wickham agree that there was indeed some post-Roman contraction, whether in terms of the exercise of power over cultivators and the extraction of resources from them (Wickham), or the size of cattle, churches, and houses (Ward-Perkins). What happened in the Carolingian period was that increasingly complex (albeit different) forms of surplus extraction again came into being, based on increasingly coercive (albeit different) forms of power relations; this allowed for a renewed expansion of consumption; and these trends continued even after the the end of the Carolingian dynasty. Had the Carolingians endured, the western part of the European continent would arguably have evolved in a similar manner, into one in which power was itself so formalised that it could be thought of as almost property, and more importantly, into one in which there was more surplus extraction and more surplus consumption. Is it not possible that, at least in relation to the latter point, even without the Carolingians, things would have been quite similar? After all, many of the developments discussed here are to be found in England as well, and though it is certainly the case that England was far more a part of Europe than any medieval UKIP might have liked to believe, there is also surely a limit to how much of England’s history we can ascribe to the Carolingians. England had, however, been a part of the Roman Empire, so the narrative of Fall, contraction, and later growth in a different form, could certainly apply here as well – without the Carolingians to help it along. It is certainly the case that in England, we find from the eighth century economic growth and increasing monetisation, and from the early 12th century, the beginnings (or resurgence?) of centralised administrative government; and while Eldevik may be right that tithes were less important in ninth-century England than on the continent, Everett Crosby’s recent work suggests that Anglo-Norman bishops and their place within royal and aristocratic patronage networks were in many respects comparable to their continental counterparts.⁽²⁰⁾ The chronology of English history is not dissimilar to what we find on the continent (though one major difference is the fate of the manor). So if the Carolingian period was so important for the later development of regions on the continent, given the similarities between those regions and England, is the explanation to be found less by recourse to ‘Carolingians’ and more in terms of particular factors coinciding in a number of regions (with a starting point in the ‘Fall’) to produce similar forms of historical transformation? Perhaps as a dynasty and a set of aristocratic elites, the Carolingians did create certain innovations in the ways in which power and patronage were articulated; but we should recall – and if I have a truly substantive criticism of these two books, it is that they do not give enough weight to this fact – that power, whether over land, jurisdictional rights, or tithes (all sources of some form of revenue for non-producers!), was ultimately a matter of controlling resources that would have to be surplus to the basic needs of the producers of those resources. And perhaps the most significant aspect of the period c.800–c.1300 is that regardless of the quite varying forms of change taking place in the different regions of much of western Europe, we find a growth of this surplus, an expansion in the resources produced and extracted, and an expansion in the ways in which these resources were used and where they were used; these developments were connected inextricably to growing urbanisation and better-articulated, more stable networks of exchange that seem to have little to do with most of the discussion provided in Eldevik and West’s books. The central question – how did structures of power discussed here, lay or ecclesiastical, relate to the thing power was fundamentally most concerned with, the exploitation of surplus production? – remains in need of

answer.

Without wishing to appear too crudely materialist, it will be obvious that I feel that the questions regarding the communication and articulation of power and relations of patronage and status, and the amount of violence involved in these relations, are ultimately secondary. While accepting that economic and demographic growth would probably only have been possible given a baseline amount of stability and the interests of the elites in increasing their material wealth, it is also possible that both this stability and the elites' desire to get their hands on more resources might not have been dependent on a solely Carolingian (in the dynastic rather than epochal sense) regime; and one may note, perhaps, the signs of growing economic complexity already in some pre-Carolingian (again in the dynastic sense) regions. So perhaps many of the developments examined in these books were not rooted in anything specifically Carolingian, and were, rather, regional variations on something larger, a revival of economic complexity, coupled with the coming into being of forms of social and political relations that allowed for and enabled further growth. These developments were not necessarily particularly Carolingian – and there are parallels in some respects with other post-Roman regions, as well as elsewhere.⁽²¹⁾ Was this the beginnings of a Great Convergence – in which case the Carolingians can surely have little to do with it?⁽²²⁾ Path-dependency comes in various guises, and institutional path-dependency is only one of them; it is equally possible that, given certain geographical and geological considerations, increasing economic complexity is also a matter of path-dependency if institutions and socio-political structures exist to allow it, and such institutions and structures are likely to come into being simply because such complexity can benefit those at the top of these hierarchies. In other words: the institutions that the Carolingians brought into being, which are the roots of West's path-dependency, might themselves be path-dependent. The points I make are, I admit, more a matter of my own obsessions than the subjects of the books under review; but I think the forms of power and how it is articulated do, fundamentally, have to do with the things and people over which that power is exercised. To the extent that there are transformations with relation to those things and people (in this period primarily cultivators of land, who rendered tithes, rents, 'gifts', 'tribute', services), if such transformations take place in different regions even when some aspects of the relations between those who exercise domination over things and people are not identical, we need to think more closely about what causes changes at the bottom, and how these changes relate to the top.

I have posed a lot of questions, but this should not be seen as a criticism of the works under review. While it is certainly one of the tasks of history to attempt to establish 'facts', it seems to me that its higher purpose – like that of any other humanistic discipline – is to raise questions. It is a testament to the quality of Eldevik and West's works that they have provoked so many, and they will surely be equally useful and stimulating to other scholars and students in the future.

Notes

1. John Watts, *The Making of Polities: Europe, 1300–1500* (Cambridge, 2009).[Back to \(1\)](#)
2. For quite different, controversial, influential views that despite their divergences nevertheless would agree on many of the changes they locate as taking place from the early thirteenth century, see Robert Bartlett, *The Making of Europe: Crusade, Colonization and Cultural Change, 950–1350* (London, 1993); and R. I. Moore, *The First European Revolution, c.970–1215* (Oxford, 2000).[Back to \(2\)](#)
3. The literature on these various aspects of the Carolingian period is vast and multilingual, and was excellently synthesised in Marios Costambeys, Matthew Innes, and Simon MacLean, *The Carolingian World* (Cambridge, 2011). I reviewed this book in this forum in 2011: <<http://www.history.ac.uk/reviews/review/1122> [3]> [accessed 30 June 2014].[Back to \(3\)](#)
4. These arguments are, for the most part, based on studies of (some regions of) France, and articulated most clearly, though on the basis of a long and controversial tradition of French scholarship, by Thomas N. Bisson. See 'The feudal revolution', *Past and Present*, 142 (1994), 6–42; and the ensuing debate in *Past and Present*, 152 (1996), and 155 (1997), with Bisson's response: 'The feudal

revolution: reply', *Past and Present*, 155 (1997), 208–25. For the 12th century, see *idem*, *The Crisis of the Twelfth Century: Power, Lordship, and the Origins of European Government* (Princeton, NJ, 2009). [Back to \(4\)](#)

5. See for example John Nightingale, *Monasteries and Patrons in the Gorze Reform: Lotharingia c.850–1000* (Oxford, 2001) (on the region that is the subject of West's book); Chris Wickham, *The Mountains and the City: The Tuscan Apennines in the Early Middle Ages* (Oxford, 1988) (on one of the areas discussed by Eldevik); and specifically on the German regions (the areas I know best) and the similarities and differences between Carolingian and Ottonian Germany, see e.g. David S. Bachrach, *Warfare in Tenth-Century Germany* (Woodbridge, 2012); and the many of the papers of Timothy Reuter collected in his posthumously published *Medieval Politics and Modern Mentalities*, ed. Janet L. Nelson (Cambridge, 2006). [Back to \(5\)](#)
6. Timothy Reuter, 'Plunder and tribute in the Carolingian empire' (1985); and 'The end of Carolingian military expansion' (1990), both reprinted in his *Medieval Politics and Modern Mentalities* (2006), pp. 231–50; 251–67. [Back to \(6\)](#)
7. For analogous processes taking place in Germany, see Benjamin Arnold, *Princes and Territories in Medieval Germany* (Cambridge, 1991), esp. chapters 6 and 7. [Back to \(7\)](#)
8. A fundamental study raising important questions concerning the fate of the bipartite manor in this period – though mostly with evidence from further east – is Ludolf Kuchenbuch: 'Abschied von der "Grundherrschaft" – Ein Prüfungsang durch das ostfränkisch-deutsche Reich 950–1050', *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte, Germanische Abteilung*, 121 (2004), 1–99. On urbanisation in this region, see Adriaan E. Verhulst, *The Rise of Cities in North-West Europe* (Cambridge, 1999), pp. 68–155. [Back to \(8\)](#)
9. See here Joaquin Martínez Pizarro, *A Rhetoric of the Scene: Dramatic Narrative in the Early Middle Ages* (Toronto, 1989). [Back to \(9\)](#)
10. The difficulties of sustaining the Carolingian achievement under the pressure of dynastic politics after 840 are very well summarised in Costambeys, Innes, and MacLean, *Carolingian World*, pp. 379–427. [Back to \(10\)](#)
11. For Lucchese landholding structures, Chris Wickham's *The Mountains and the City* remains fundamental. [Back to \(11\)](#)
12. Nightingale, *Monasteries and Patrons*. [Back to \(12\)](#)
13. John B. Freed, 'The formation of the Salzburg ministerialage in the tenth and the eleventh centuries', *Viator*, 9 (1978), 67–102; *idem*, 'The ministerials and the Church in twelfth-century Salzburg', *Medieval Prosopography*, 3 (1982), 1–20; *idem*, 'Nobles, ministerials, and knights in the archdiocese of Salzburg', *Speculum*, 62 (1987), 575–611. The *ministeriales* were a peculiarly German class of unfree knights, whose status could, however, vary quite significantly; see Benjamin Arnold, *German Knighthood 1050–1300* (Oxford, 1985), for a discussion of *ministeriales*, their origins, and their function in the German-speaking lands. [Back to \(13\)](#)
14. On these developments in Germany between the eleventh and thirteenth centuries, see Arnold, *Princes and Territories*; specifically on Bavaria and Salzburg, see also John B. Freed, *The Counts of Falkenstein: Noble Self-Consciousness in Twelfth-Century Germany* (Philadelphia, 1984). [Back to \(14\)](#)
15. On the advocates in Germany, the benefits they accrued from this position, and the difficulties faced by ecclesiastical landlords as a result, see Arnold, *Princes and Territories*, pp. 82–6, *et passim*; specifically on Bavaria, see also Philippe Dollinger, *Der bayerische Bauernstand vom 9. bis zum 13. Jahrhundert*, trans. Ursula Irsigler (Munich, 1982). [Back to \(15\)](#)
16. Susan Reynolds, *Fiefs and Vassals: The Medieval Evidence Reinterpreted* (Oxford, 1994). Important responses to this book are presented in two recent essay collections: *Das Lehnswesen im Hochmittelalter*, ed. Jürgen Dendorfer and Roman Deutinger (Stuttgart, 2010); and *Feudalism: New Landscapes of Debate*, ed. Sverre Bagge, Michael H. Gelting, and Thomas Lindkvist (Turnhout, 2011); see also the earlier anthology of essays: Natalie Fryde, Pierre Monnet, and Otto Gerhard Oexle, *Die Gegenwart des Feudalismus* (Göttingen, 2002), in which the papers by Paul Hyams and Ludolf Kuchenbuch are particularly of note: 'Homage and feudalism: a judicious separation', pp. 13–49; and 'Feudalismus: Versuch über die Gebrauchsstrategien eines wissenschaftspolitischen Reizwortes', pp. 293–323. [Back to \(16\)](#)

17. For sage comments on the differences between the Empire, and France and England, stressing the importance of the size of territories in making comparisons, see Reuter, *Medieval Politics*, pp. 388–412; 432–58.[Back to \(17\)](#)
 18. Bryan Ward-Perkins, *The Fall of Rome and the End of Civilization* (Oxford, 2005).[Back to \(18\)](#)
 19. Chris Wickham, *Framing the Early Middle Ages: Europe and the Mediterranean, 400–800* (Oxford, 2005).[Back to \(19\)](#)
 20. Everett U. Crosby, *The King's Bishops: The Politics of Patronage in England and Normandy, 1066–1216* (New York, 2013).[Back to \(20\)](#)
 21. See the suggestive comments regarding China in Jean-Laurent Rosenthal and R. Bin Wong, *Before and Beyond Divergence: The Politics of Economic Change in China and Europe* (Cambridge, MA, 2011), p. 21, for a presentation of apparently very similar changes taking place in China between the eighth and 11th centuries.[Back to \(21\)](#)
 22. For a fascinating (if in some respects superficial) argument that there was a convergence of various kinds of developments – economic, political, social, and cultural – across at least the Eurasian landmass from around 1000, see David Northrup, 'Globalization and the Great Convergence: rethinking world history in the long term', *Journal of World History*, 16 (2005), 249–67.[Back to \(22\)](#)
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