

A Reply

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One of the great joys in writing a book is the opportunity to engage with quality readings of it, including critical ones, such as those offered by the various contributors in the previous pages. In this regard, already when writing *Il mutamento signorile* – an English version of which was published a few years later (with some changes and additions) as *The Seigneurial Transformation* – the idea of translating the book was one of my priorities, precisely in order to broaden my potential readership beyond the all-too-narrow confines of the Italian language. I like to think that this effort was not in vain.

Before responding to the observations and criticisms formulated by the reviewers, I believe it is worth briefly discussing the volume's genesis, which may help one to better grasp the analytical perspective I have adopted, and therefore to more accurately frame my responses. A book, after all, is only the more or less final outcome of a research path made up also of doubts, shifts of perspective, second thoughts, and new hypotheses. In my specific case, this volume concludes the first major stage on a research path that I have been pursuing for over ten years now, and which has yet to reach its end. Its underlying ideas, particularly the key topic on which my research is centred, namely the major transformation in socio-political practices in the Italian countryside at the turn of the 1100s, have chiefly been developed as a response to a series of

problems related to the evolution of political languages over time, whose analysis constituted my starting point. Before focusing on these decades, I analysed certain political languages, which I then newly dealt with in the book (respectively pacts, customs, and violence) with a different chronology, ranging from the late eleventh century to the early thirteenth.¹ The analysis of sources pertaining to the earliest phase enabled me to grasp a more or less marked – yet recurrent – discontinuity with respect to previous political discourses.² In the light of this, I became aware of the need to understand whether the discontinuities I was detecting at the level of discourses (and related written sources) was a break simply associated with a transformation in the interpretation of society at the time, based on the emergence of new analytical categories, or whether it instead constituted the documentary reflection of a more concrete and deeper transformation of the very balances shaping that society. The development of my research, which was crucially systematised and reorganised through the drafting of the book, led me to definitely favour this latter hypothesis.

I feel this is a necessary premise in order to address the criticism of my book advanced by Haack and Smith. I will be discussing it before shifting my attention to the observations formulated by Vollono and Santos Salazar, and will then finally respond to Kohl and West's contributions – although in the concrete development of my argument, some juxtapositions will inevitably occur.

I will therefore begin by addressing Christoph Haack and Isaac Smith's contribution, which is by far the most critical of the five, and which essentially suggests that I have reproduced the conceptual framework of the "mutationism" of the 1980s and early 1990s, shifting it from early 11th-century southern France to central-northern Italy in the 1100s. Given this picture, in their view the criticism formulated by Barthélemy, Reuter, and White (among others) in the

¹ Fiore, 2010b; Fiore, 2012; Fiore, 2015.

² Fiore, 2018. Although it was only published much later, I had already completed this essay in 2015, which is to say before the publication of the Italian book.

1990s is still applicable: these scholars saw the transformation of the sources and their new emphasis on violence as reflecting not socio-political transformations, but rather a new way of interpreting (and transcribing) processes and practices that had already characterised earlier society – not as a transformation, but as a revelation. These critical positions, which emerged in the debate in *Past&Present* in the mid-1990s, did indeed constitute a starting point for me, a conceptual framework within which I began to develop my analysis of political languages connected to the rural seigneurial world. As already noted, I progressively overcame this framework as I shifted my analysis outside the discursive dimension to focus on concrete power relations and forms of social control – particularly (yet not exclusively) territorial lordship, understood as a form of patrimonial control exercised over a territory by an individual or religious institution, through the exercise of judicial, military, and fiscal prerogatives (what Wickham has called “a simple and local version of the state”).³ Indeed, the seigneurial (and territorial) transformation of aristocratic power is an integral part of that complex process of the (relative) standardisation of European socio-political structures characterising the central centuries of the Middle Ages: what Robert Bartlett has labelled the “Europeanization of Europe.”⁴ If the staunch criticism of the “mutationism” of the second half of the 1990s provides a valuable contribution to the debate on the transformation of the socio-political picture of the countryside in the years roughly between 850 and 1100 (two periods which, as we all agree, differ sharply), it is by highlighting the divergences between local chronologies, the different rhythms in this process, and particularly the need to engage with the sources in a non-naïve and demystifying way. After a fruitful initial stage, the “mutationist” model gradually grew rigid and sclerotic. The fresh reading of the sources proposed by the “anti-mutationists” (which owed much to the linguistic turn) is precisely what revealed the aporias and problems associated with this

³ Wickham, 2003.

⁴ Bartlett, 1993.

model. What has emerged, then, is a perspective that is not confined to any single point of view (*querimoniae*, accounts of acts of violence in *miracula*, or the disappearance of *placita*), but which rather intertwines with a range of different sources. This also includes material data, so as to eliminate the risk of a skewed perspective due to the exclusive use of written sources. In this respect, my “return” to Duby, Bonnassie, and Bisson did not involve finding a way around the anti-mutationists’ critical blockade, but rather crossing it and incorporating its analytical tools (as West actually acknowledges in his contribution).

In my view, recognising the importance of archaeological data – wholly ignored by Haack and Smith’s considerations, as well as by the debate in the 1990s – is crucial in order to fully appreciate the concreteness of the transformation of socio-political balances, and its relative swiftness, as emphasised by Giulia Vollono, who in her contribution has focused precisely on this specific point. The construction of the mutationist model entailed only a weak contribution from archaeology, and even the criticism of this model was essentially developed at the level of written sources. Besides, excavations were far less numerous than today, and the tools to establish (relative and absolute) dates were far less accurate than present-day ones. I would further add that historians were less inclined to engage with archaeological data: a situation that has changed significantly over the past few years. As far as the research on “seigneurial transformation” is concerned, the specific advantage when it comes to Italy is that research on castles – that is, the buildings arguably most closely connected to local power dynamics – has represented one of the main strands of archaeological enquiry since the 1970s (and the pioneering investigations carried out by Riccardo Francovich).⁵ As Vollono has emphasised, up until the early 2000s researchers only distinguished between a first stage of *incastellamento* in the tenth–eleventh centuries and a second one in the late twelfth century. By

⁵ For an overview of archaeological research on *incastellamento* in Italy over the past forty years, see Augenti, 2018.

contrast, more recent research has highlighted the existence of an intermediate phase in the decades at the turn of the 1100s, and it is this that is now described as the second stage of *incastellamento*.⁶ This period, while relatively short, marks a crucial transition with respect to tenth- and early eleventh-century castles. What we witness are far sturdier and more complex shapes, the use of stone and bricks instead of wood (previously widespread) and, in parallel, a careful selection of sites through the abandonment (or destruction) of a great many castles incapable of being developed in these new material directions.⁷ This shift of perspective in the archaeological research crucially contributed to orienting my research towards a strongly discontinuistic interpretation of the period after 1080. If this change in the material aspects of castles is viewed in relation to the birth of stronger seigneurial authorities in the countryside, it becomes clear that the new material structures do not simply reflect their owners' wealth, but stand dynamically at the centre of a radical increase in the ruling classes' capacity to extract surplus value from farmers, the importance of which can hardly be overestimated. In particular, a constant dialogue with archaeological findings – in particular with Giovanna Bianchi – has proven essential for me (probably even more so than a reading of my book might suggest). In this respect, as Vollono emphasises, it is important for my research to become a point of reference for archaeologists themselves, in view of a further refining of dating methods in relation to castle sites and of a better understanding of the material dynamics associated with this phase, including diagnostic problems pertaining to the emergence of a new militarised elite in the countryside (*milites*). Vollono emphasises the need to carry out more systematic sample surveys of the *milites'* skeletal remains (by identifying physical markers of military life) and of animal remains connected to the food consumption patterns of lords and their retinues in order to better grasp the chronologies and characteristics of the differentiation process. One aspect to be

⁶ Carocci, 2018b.

⁷ Bianchi, 2014.

drawn upon and systematically explored is the emergence of stone (and brick) houses as a marker of the new elites, who, through the early use of the same materials adopted by lords for their towers and donjons, associated themselves with the latter – not least symbolically. In doing so, they distinguished themselves from mere farmers, who for decades to come would continue to use wood for their homes. These are only some ideas that show the potential for dialogue between historians focusing on written sources and archaeologists, from a perspective that assigns the two disciplines equal dignity while respecting their different research agendas.⁸

The investing of material resources in castles, however, does not concern only lords, but extends to other political actors active at a local level, particularly the royal/imperial authority. In the previous stage, the king's local presence was displayed through royal palaces and large, specialised, rural *villae*, which Giovanna Bianchi has defined as “out of scale sites”. But then, around 1100, castles became a new marker of the imperial presence, a presence that – compared to the past – was increasingly indistinguishable from the rural aristocracy from a material perspective.⁹

The issue of the empire – and of the transformation of the ways in which it manifests itself at the local level – lies at the centre of Igor Santos Salazar's contribution, and I can only agree with his conclusions. Santos Salazar clearly highlights the fact that in the case of Italy the problem is not so much the crisis of royal power as the transformation of the ways in which it was exercised. The transition from a central, land-based form of power to one based on the exercise of jurisdiction (albeit chiefly within the empire's own domains) is one of the major acquisitions in the study of medieval taxation in recent years.¹⁰ But Santos Salazar also stresses that, regardless of the transformation of the nature of the fiscal network (from land-based to seigneurial), the latter remained a crucial means of control and

⁸ Molinari, 2010.

⁹ On “out of scale sites”, see Bianchi, Collavini, 2018.

¹⁰ Carocci, Collavini, 2014.

the linchpin of the emperors' actions throughout the twelfth century. Its flexibility and changeability over time represented an opportunity. Once a centre had entered into the fisc – for whatever reason – even if it later returned into the hands of other actors, it could never completely fall outside the empire's sphere of interest: the imperial authorities' memory, supported by an increasing use of writing, could stretch back for centuries, leading them to reclaim rural sites.

Thomas Kohl has focused his critical remarks on the religious aspect of the transformation – an aspect also highlighted, albeit less emphatically, by Charles West. According to Kohl, the main limitation of my book lies in its failure to integrate the interpretation of social dynamics with that of religious dynamics, which occurred on a different level. This separation, he argues, does not take into account the permeability of the two areas at this stage, which instead lies at the centre of Florian Mazel's interpretation.¹¹ Kohl himself brilliantly developed it in his monograph on conflicts in north-western France and southern Germany.¹² This is certainly a reasonable observation. I have not adequately emphasised the ideological and religious dimension of the conflict between the pro-Gregory and the pro-Henry parties, favouring the more neutral label of "civil wars". The latter, in my view, has the advantage of not limiting the conflict(s) to the "investiture struggle", but of opening the perspective up to the plethora of local conflicts that were grafted onto the conflict at the highest level and subsequently prolonged it. However, I too believe that the ideological dimension of the conflict was very important for local socio-political developments. The reciprocal delegitimisation of the highest authorities, the crisis of the episcopal network, and the removal of most public high officials ultimately undermined the legitimacy of the whole traditional power structure supporting the *regnum*; and it was in the context of this systemic crisis (which was also a crisis of political society's faith in the system itself) that seigneurie, urban proto-communes, and autonomous

¹¹ Mazel, 2010, pp. 233-98, 447-91.

¹² Kohl, 2019.

rural communities emerged. We can see these realities as a range of responses to the simultaneously ideological and material crisis of the old order.

Kohl's observations about the overlaps between the two levels can also open up fruitful new research avenues. For example, another element that ought to be explored and emphasised in this respect is the disappearance of private churches in aristocratic hands, which before 1050 were widespread in the Italian countryside. This phenomenon is connected to the Gregorian reform, which effectively promoted the return of private churches under episcopal or monastic control.¹³ As private churches disappeared, at most being replaced by forms of patronage, the aristocracies – for whom churches constituted an important means for displaying social superiority (as well as, presumably, a source of significant assets through the levying of tithes) – found themselves in the position of having to find new avenues to express their ambition to control society. This process may therefore have fostered the seigneurial transformation of the lay aristocracy through an investment in the “secular” sphere of power, which fell outside the ecclesiastical authorities' sphere of intervention.

Finally, Charles West's contribution identifies three points worthy of further development: the role of religion (a point which I have just addressed), the economy, and the scale of the enquiry and role of central power.

West stresses the importance of grasping the economic dimension of the process of transformation: it is a matter of understanding not only if and how the economic pie was divided in a new way compared to the past, but also how its ingredients and the method of baking it changed. In my book I touch upon all these issues, although they remain in the background of my analysis, resurfacing in a more evident way only in the conclusions. The reason for this is not that I consider the economic dimension a secondary issue, but

¹³ I have outlined these ideas in Fiore, 2010a, pp. 353–357.

that the research on the topic is so scanty. For my part, I have recently continued to work on such problems, seeking to better understand the nature of economic transformations in Italy in the eleventh century. While I am far from having completed my research in this area (in fact, in many ways I am still at the beginning), I believe that in relation to this period it is possible to identify a discontinuity which is closely related to the increasing economic role played by cities within the framework of the break-up of the network of fiscal and aristocratic *villae*. As far as I can tell, from the late tenth century onwards, cities in central-northern Italy emerged as manufacturing and trading hubs within an economy where “market” transactions began to play an increasingly prominent role compared to those based on fiscal or aristocratic redistribution. Cities started to acquire increasing demographic weight compared to the countryside, and therefore the demand for agricultural products to feed the urban population increased.¹⁴ From this specific perspective, seigneurie can also be seen as a tool developed by the elites to take advantage of this transformation by directing rural economic developments in such a way as to maximise their own profits to the detriment of peasant village society (with the exception of the new group of *milites*, which was closer to the lords).¹⁵

Another important question that West raises is why “state” authorities failed to capture and co-opt the elites’ drive towards the localisation and seigneurial crystallisation of power. In my view, this is a crucial issue. To fully address it, basic research still needs to be conducted in many areas (a problem to which I will be returning shortly). But, as far as I understand it, this process of the local crystallisation of power was a way for emerging actors to challenge traditional power balances from below (along a chronological axis extending from the early eleventh to the mid-twelfth century), by making the most of those phases in which higher authorities experienced moments of crisis due to a number of factors. These actors

¹⁴ Hubert, 2004.

¹⁵ See also Collavini, 2021.

vary from context to context: high aristocrats in early twelfth-century Germany and (perhaps) in the eleventh-century Byzantine empire; low- and middle-ranking nobles, cities, and large rural communities in central-northern Italy around the year 1100; foreign former mercenaries in eleventh-century southern Italy, and so on, depending on the context.¹⁶ Only once the process of crystallisation had been accomplished (and only rarely when it was still under way) did higher (royal or princely) authorities find a way to exploit these experiences by turning them, in the long term, into one of the pivots of their polities.¹⁷ In my view, one of the challenges in this field in the coming years will be precisely to construct a robust explicative matrix for this process capable of taking regional differences into account.

On this note, I would like to end by discussing the need to broaden the field of enquiry, which has been emphasised, with various nuances, by both Kohl and West. Kohl chiefly stresses the need to overcome the bounds of national historiographies by fruitfully applying historiographical concepts developed within a specific tradition to the study of different regions in such a way as to transcend the conceptual as well as linguistic limits that continue to influence our work, in more or less conscious ways. West instead stresses the need for wide-ranging comparative enquiries in order to grasp the dynamics associated with the crystallisation of local power. These enquiries ought to move beyond Latin Europe to encompass the Byzantine world. Indeed, they ought to try to adopt a truly Eurasian perspective. I agree with all these suggestions. I too feel that, despite certain foreseeable difficulties, it is necessary to move beyond the comfort zones outlined by national historiographies, in relation both to our research topics and to the geographical framework of our investigations. What clearly emerges from my responses, I think, is

¹⁶ On the *regnum teutonicum* see West, 2013, pp. 228-254; on Byzantium (and the late Mark Whittow's views) see now Howard-Johnson, 2020; on southern Italy and the seigneurial turn associated with the Normans, see now Carocci, 2018a, pp. 69-114.

¹⁷ For a recent interpretation of lordship as one of the cornerstones of state order in the late Middle Ages and early Modern Age, see e.g. Van der Meulen, 2021.

that in my view the problem of “seigneurial transformation” or the “feudal revolution” in Italy and Europe remains open. I also believe that the only way to try to address this problem is by broadening our (geographical and conceptual) field of reference by trying to interpret and reinterpret from this perspective the socio-political processes that shaped several areas (thereby widening our set of questions). I believe we must strive to grasp the different configurations that characterise this process across different areas at different times, along with their chronologies, and build an increasingly solid explicative matrix. However, it is also my opinion that this enquiry will become even more fruitful if we succeed in the difficult and (at least from a heuristic perspective) risky task of setting these issues within an even broader analytical framework according to the ‘global Middle Ages’ perspective proposed by West, yet without losing sight of its peculiarities by adopting an excessively broad and all-encompassing perspective. While at the base of our studies we always glimpse the dark fascination exerted by the power of man over his fellow man – lordship being but one of the many possible expressions of such power – it is only by paying attention to the correct contexts in which certain questions can be addressed that we can avoid losing the thread of our research.

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