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In theory, 'ecclesiastical history' is just a polysyllabic synonym for 'church history'. In practice, however, it connotes something more precise: the history of the church institutional. Like other forms of institutional history, it has become something of a historiographical backwater - very respectable and much loved backwater, I hasten to note - in the last generation or so. Ian Forrest, who has been in recent years among the most interesting of ecclesiastical historians of England in the later Middle Ages, here proposes a new kind of ecclesiastical history, what he (and Sethina Watson) call a history of the 'social church,' in which church institutions are seen as socio-economic and cultural entities. Although this kind of ecclesiastical history may not be absolutely new (I think some of the work by the late Robert Brentano might qualify), there has not been enough of it. Forrest's efforts in this endeavor bear fruit in this brilliant book.

In it, Forrest asks a neglected and deceptively simple question: how did English bishops c.1200-c.1500 know about conditions in the parishes of their dioceses? The answer takes one to the heart of both peasant life and the rising power of medieval bishops.

Forrest answers that bishops called on the opinions of *fidedigni*: panels of 'trustworthy men' of the parish assembled by episcopal authority. What did it mean to be a *fidedignus*? That question leads Forrest to a very extended discussion of *fides*, that is faith or trust, both in terms of high intellectual history and in terms of relationships among very ordinary laypeople. The meaning of *fides* turns out to have been highly multiform. A sense of the term which applies to Forrest's concern in this book is, as he argues, a willingness to believe (or have faith or trust) in what is unseen, in what is otherwise unknown. Among medieval intellectuals, this applied to some of the truths of the faith, such as the Trinity or even the existence of God himself. But this could also apply to what bishops needed to know about the parishes. It must also be said that, given the flexibility of the term *fides*, it might be hard to know how exactly bishops or parishioners understood it in any given case. I suspect anyone interested in the subject will need to consult Forrest's impressive discussion of the meanings of *fides*, although I also think that in the end the book's larger argument can stand without it.

Who these trustworthy men were in the end says more about them than the meanings of *fides*. They were typically the greater people of their parish, what Forrest felicitously calls a 'very local elite'. Here Forrest is careful to navigate the various senses in which one might be such a greater person: wealth (and he is likewise careful in sifting different kinds of evidence of wealth, from tax assessments - with some sharp observations about the partial picture such assessments supply - to payments of rents, to lands owned); multiple holding of manorial office; coming from a family marked by such characteristics; and not least simply being a man. That patriarchy and other forms of power operated at the parish level should not come as a surprise, but it is good to see it demonstrated so clearly and with statistical finesse. Moreover, as Forrest usefully observes, his discussion cuts against the conclusions of some historians, who argue that villages or parishes operated according to

consensus. Such consensus did not, Forrest warns, imply a latent democracy, even among the male part of the population. Consensus was quite compatible with significant socio-economic inequality and the exercise of power by the few. And it reflected the exercise of that power. Forrest shows that his period was not all of a piece when it came to who acted as trustworthy men. The forces set in motion by the Black Death further narrowed who might serve in that capacity: very local elites got smaller and more elite, even qualifying as lords 'in all but name' (p. 208). A reversal of this development in the 15th century drove bishops to start bypassing peasants altogether for a more clearly defined elite: local gentry, sometimes even members of the nobility. A renewed rise of substantial peasants, often displacing peasant competitors, toward the end of the century, led bishops to return to such men, the yeomanry, to fulfil the function of trustworthy men.

So when bishops needed to know a range of things – such as the facts of a lawsuit, or whether a church was truly vacant, or the local report regarding the clergyman who had been presented to it, or what repairs a church needed, or what sins were to be discovered during a visitation of a parish – they asked panels of trustworthy men, for most of this period peasants drawn from a very local parish elite. In this sense, as Forrest notes, bishops acted rather like the king, whose courts relied on local juries – made up of a more exalted elite – to report local facts.

Indeed, one might note that such activity was part of a larger pattern. The era's centralizing authorities, be they ecclesiastical or lay, relied on volunteer local elites to function. This could be a matter of information gathering, as in royal juries or episcopally nominated trustworthy men. This could be a matter of local royal office holding – what at what one time was called 'self-government at the king's command'. This could be a matter of delegated episcopal authority, such as the monastic tax collectors appointed by bishops discussed by Forrest, or the various local clergy, or local monastic heads, commissioned by bishops to, for example, reconsecrate polluted churches. This could be a matter of the operation of papal authority, such as the local ecclesiastical *potentes* appointed by the pope to hear local disputes as papal judges delegate. Everywhere, centralizing power relied on local elites to make that power operative. It was not a time of centralizing authority simply overcoming local power. What is remarkable is that this book shows *peasants* regularly taking on the role of local notables who enabled central authority. Perhaps this should surprise one a little less when one thinks about lords relying on peasants as manorial officials. But surprise it does.

Did bishops get the truth from trustworthy men? It is usually impossible to answer the question. While bishops wrote of trustworthy men as stable men of discernment who could reveal the truth of matters, Forrest argues that for bishops the accuracy of what trustworthy men said, or even the actual trustworthiness of trustworthy men themselves, was beside the point. Bishops were after closure, not truth. And trustworthy men, being the local elite, had the social weight to make their version of the facts stick. Indeed, in situations where bishops communicated the result they wanted, a trustworthy man 'knew what was required of

him' (p. 127). Such conclusions are not mere cynicism on Forrest's part; he produces evidence of trustworthy men reporting what they and the bishop must have known to be nonsense, and bishops accepting that nonsense. As Forrest argues, bishops' willingness to rely on trustworthy men is especially noteworthy given a general clerical distrust of the laity as a source of knowledge. Bishops had to get something they really needed or wanted to go to laymen, and peasant laymen at that. They must have gotten it.

What impact did such reliance have on localities, and local elites, themselves? Forrest concludes that while bishops took advantage of existing inequality in peasant society, they also deepened it. The latter conclusion is plausible, but hard to prove. On the one hand, as Forrest argues, activity as a trustworthy man gave members of the parish elite another opportunity to lord it over others, to increase their standing as men whose views would inform official action. The same can be said of the jurors and unpaid royal local officials called into being by royal power. On the other hand, the evidence that allows Forrest both to identify trustworthy men *and* recover their local status is already spotty. Forrest must rely on the happy conjunction of say, a list of the trustworthy men used to determine a lawsuit preserved in a bishop's register and, say, the survival of manor court rolls from about that same time for that same place in order to identify which trustworthy men were also of the parish elite. He is able to find such conjunctions; his argument that trustworthy men were largely drawn from the local elite depends on them. But although Forrest is able to draw some fairly rounded portraits of a few trustworthy men, such evidence is too haphazard to reveal a 'before and after', to show becoming a trustworthy man actually increasing that man's local standing.

Indeed, the argument that bishops' use of trustworthy men deepened inequality stands in tension with Forrest's analysis of the geography of trustworthiness. In a beautifully argued section, Forrest shows that English bishops' use of trustworthy men was not universal. It was largely to be found in lowland areas: in denser, nucleated settlements that focused on arable farming. Such places were ones of great inequality, economic and social. In the uplands, people, more dependent on herding, were more spread out and more equal. Bishops needing information from such places found trustworthy men - as in very local elites whose position would make their version of events accepted enough to have staying power - in short supply. And so such bishops found themselves resorting to other methods, such as simply calling for witnesses, examining accused persons personally, even casting lots. If this discussion of geography is true - and Forrest persuades me on the point - then it highlights the implication of Occam's razor for Forrest's argument about episcopal action fostering peasant inequality. Where inequality gave bishops a purchase, bishops used trustworthy men. Where it did not, bishops did not. The evidence does not demand the conclusion that bishops' reliance on trustworthy men increased inequality. Again, such a conclusion remains very plausible, but not proven - and not likely to be provable, given the sources.

As I have indicated here and there, one of this book's strengths is its use of evidence. Forrest deploys an exceptional number of sources; it sometimes feels as though he has read everything and forgotten nothing and is able to pluck from sources an apposite incident at will. Moreover, he uses those sources in a persuasive and nuanced way. That said, the argument is also very much informed by wide reading in the theory of trusting and other social theory. Indeed, Forrest makes what appear to me valuable contributions to those discussions, ones that go beyond the practices-and-conditions-which-theorists-have-thought-to-be-strictly-modern-are-not-so-because-they-are-to-be-found-in-the-Middle-Ages-too variety of argument often deployed by medievalists (although he does, properly, make such arguments). But while theory informs and inspires the discussion, it does not replace evidence from medieval sources. Old-fashioned empiricists should be pleased.

Some points may be worth reconsideration. One is the matter of how, on the ground, trustworthy men were chosen. Forrest notes that they could not often have been really identified by bishops themselves; bishops would rarely have known local micro-politics well enough to come up with the right list of names. So who did choose them? As Forrest observes, even a rural dean – representing the lowest jurisdictional level of a diocese – would have had trouble doing the job; such a figure might have charge of a couple of dozen parishes, and would not likely have known peasant society intimately in all of them. At one point, Forrest introduces the idea that trustworthy men might have been 'self-selecting'. That possibility eventually, and so far as I can see, silently, becomes a flat assertion that they were, at least in part, self-selecting, without any particular evidence for the point. It maybe that the problem here is that I don't know what Forrest means by 'self-selecting'. This description does have the effect of bolstering trustworthy men as autonomous partners of bishops, and so Forrest's point that this book is about the relationship *between* bishop and parish. But presumably *some* episcopal subordinate must have done the selecting; Forrest does find evidence of a lowly vicar and a lowly summoner charged with finding local trustworthy men. Forrest also suspects that on some occasions local lords played a role in selection. Yet he also finds that such figures were sometimes avoided, as when a bishop ordered a judge to choose the trustworthy men himself. My suspicion – unprovable – is that episcopal deputies typically did the choosing, and generally did so by simply consulting whatever local clergy with local knowledge seemed appropriate. Even if a parish rector or vicar were unavailable – or unsuitable – for this role, there would have been his clerical assistants, or perhaps a wandering friar or nearby monastic head. Such informal consultation is likely to have left little trace in the sources. And, of course, on some occasion an episcopal deputy would just have happened to have some lay connection of his own in the parish to talk to. I confess I do not see self-selection coming into it. Leaving out self-selection also, I should stress, leaves the larger argument undisturbed.

I also wonder whether Forrest takes the discussion of episcopal authority too far at one point. Surely he is correct that bishops' use of trustworthy

men is how they built their power in this period. It has long been clear that episcopal *fora* for resolving disputes, not to mention prosecuting morals crimes and heresy, was an element of that power. Forrest shows how critical trustworthy men were to the operations of such courts. And that is the essential argument. Forrest, however, goes further, arguing that people experienced a bishop's power as part of the physical environment, in that people saw objects, be they buildings or books, that evoked for them some earlier exercise of episcopal authority involving those objects. This is a very attractive conclusion, but is it true? Forrest considers that it requires only a 'small leap of the imagination' (p. 293) to think it is. Certainly people remembered earlier episcopal decisions, and could recall them when triggered to do so - as in some later inquest touching those objects prompting a consideration of earlier decisions made about them. But was such memory people's daily or frequent experience? Forrest's reference to a small leap of imagination indicates that his conclusion is tentative, and I think it should very much should be. I teach at a university in Alabama founded during the governorship of the infamous segregationist George Wallace (strictly speaking, under his wife, Lurleen Wallace, widely regarded as a placeholder for her husband). The campus is thus arguably redolent of that tortured time generally and of Wallace specifically. One might say the same of the books the campus library acquired at the time, even if they concern, say, the Middle Ages rather than 20th-century race relations. Yet it is quite possible to work on campus or read those books without thinking about either Wallace or the civil rights struggle on a daily or even regular basis: I do, and so do my students of all races, or so an unscientific poll suggests. Now it may be that illiterates in a smaller community - say, a medieval country parish - were much, much more likely to have memory evoked by merely seeing familiar objects. But I am not sure this was the case, at least to the extent it would have to have been for Forrest's suggestion to hold up. While Forrest refers to Tom Johnson's argument that what physical objects were could be determined by the law (e.g., the law made a rector's house a rectory) - which is perfectly sensible, as is Maureen Miller's argument also referred to here, that bishops used their palaces to communicate power - Forrest goes rather further. It may be a material turn too far.

The riches of this book far outweigh such uncertainties, more than this already long review does justice. Consider just one more of those riches. Historians have argued that the age saw a growing emphasis on the interior life, on what Martin Luther would later call the 'inner man' versus the 'outer man.' Historians have also seen this development as originating among clerical intellectuals and communicated to the laity by clerical encouragement of regular confession of sins: an occasion for priests to probe the intentions of parishioners and thus promote self-examination. Forrest points out that panels of trustworthy men were an unsuspected alternative forum for such issues. For, unlike (in theory) secular juries, trustworthy men made 'subtle judgments,' the subtlest perhaps being about the feelings or thoughts of other parishioners: what had been their intent? were they contrite? did they have heterodox

beliefs? and so on. Indeed, Forrest concludes that the frequency with which trustworthy men were asked to act made their use more important than confession in propagating a culture of interiority.

This book is original, well argued, and deeply penetrating. A further virtue is the range of subjects for which it will be important or even essential reading: high- and late-medieval governance, medieval ecclesiastical authority, as well as peasant society, the communication of information, and matters of trust in any age.

Author's Response

I would like to thank Michael Burger for his insightful and thoughtful review. He represents the central arguments of my book well, and highlights some important areas for debate, for which I am grateful. The key thing that his review identifies is my approach to understanding the logic within the system of episcopal inquests, visitations and 'trustworthy men'. This involves thinking about why repeated forms of action – such as inquests into the costs of repairs to churches – operated as they did, and what this tells us about *how* they worked and their likely effects on other areas of life. Thinking through the logical social and political implications of administrative actions allowed me to integrate ecclesiastical history with developments in social, economic and cultural life, as Burger generously describes. This approach is generally one that allows historians to say more than the evidence, at face value, chooses to reveal. It is here that the opportunities for disagreement, debate, and future research emerge, and Burger's review points to several areas where further thought ought to be productive. I will follow up four points that he raises, relating to the meanings of *fides*, the relationship of institutions to inequality, the selection of the trustworthy men, and materiality.

Burger raises a question as to whether the medieval meaning of *fides* was affected by the identity and behaviour of the trustworthy men, and the system within which they operated. This is a reasonable query, since there was a wide spectrum of meanings associated with *fides* and related terms. Aspects of the politics involved in inquests and visitations were indeed a long way from feelings affecting belief in the lives of those who wrote or read devotional literature. However, there was a continuum along this spectrum, with many feedback loops of meaning, some explicit, some implicit: those which seemed apparent to me are discussed in the book. It is hard to imagine meanings in one realm of experience not bleeding into others. Moreover, I think we should assume that the meaning of words is affected by the way they are used and their associations with particular actions and persons. Naturally there is huge scope for further debate and research into medieval meanings of *fides*!

Michael Burger makes a very interesting point when he voices some scepticism about the evidence for church governance contributing to (as well as depending upon) inequalities within parishes, a key argument of my book. He is quite right to point to the scarcity of direct evidence for

increased social status flowing from acting as a trustworthy man – though it is not entirely absent, and there is a lot of indirect evidence – and it is also hard to isolate the social effects of one factor among many intersecting inequalities. He also suggests that the geographical differences I describe provide evidence against my thinking on the causes of inequality, saying that where bishops could not latch onto pre-existing inequalities they found it harder to act and did not exacerbate inequality; he then transposes this reasoning to the regions where bishops found it easier to identify ‘trustworthy men’, saying – I think – that neither therefore did they contribute to inequality here. Again, this is all reasonable scepticism, but I do disagree. For me the important thing is the way in which multiple inequalities interlock and contribute to one another, for example wealth and status (or ‘social capital’). This means I don’t see there being a separate measure of ‘standing’, independent of this intersectionality, to which the status arising from institutionally-attributed trustworthiness can be said to have contributed. In my understanding, possessing status as a ‘trustworthy man’ is not just something that created the potential for *actual* inequality via economic gain, it was itself a tangible facet of hierarchy. In regions where economic inequalities were less pronounced, such as mountain and moorland landscapes, bishops found it harder to latch their institutional social capital onto some pre-existing hierarchy, making it trickier for them to operate inquests. It is true, as Burger says, that they therefore did less to exacerbate the formation of hierarchies here than they did in the lowlands, but that is because the symbiosis between economic and cultural/institutional hierarchies of value was less likely to take hold. It isn’t proof that institutional office-holding had no impact on hierarchies. There is a set of connected questions here, to do with the evidence for local hierarchies and its interpretation, which deserves further study. Church records usually only give us a snapshot of a moment in time, making it hard to isolate the ‘before and after’ status of trustworthy men within parishes, but in England manorial documents and common law files would permit longer-range and more detailed studies of the intersection between economic position and institutional status within those jurisdictions. I hope historians will take up this challenge.

These issues are connected with that of how the trustworthy men were identified. As Burger accepts, there is no conclusive evidence of either my view (that self-selection predominated) or his (that clerics must have taken the lead). We have to fall back on the logic of the system, whose elements I take Burger to accept: bishops wanted ‘closure’ rather than truth, and to achieve this they needed informants who were locally incontrovertible, in the sense of being difficult to challenge. If we accept this, then even if clerics did play some part – and Burger is surely correct that they must have been involved – their preferences could not have departed much from the logic demanded by the system. Knowledge produced by men who didn’t otherwise fit the bill would not have stood the test of time. I suspect that there is actually much we would agree on

here, but the questions are (largely) new ones, and so we may be talking past one another.

Finally, I will make a brief comment on materiality. Burger doubts whether episcopal power was much considered when people looked at churches and thought about parish boundaries, using the contemporary example of his own university campus and its segregationist history: he reads in the university library without thinking about the history of segregation in Alabama. I can relate to this point (I am based at Oriel College, made infamous recently by the shadow of Cecil Rhodes), but I have a different perspective. I suspect there are students at Auburn who find it hard to forget the racial politics of the twentieth century when reading in its library, just as there are those at Oxford who can forget the role played by colonialism in our institution's history, and those who cannot. For the latter group historic and continuing power relations are very much expressed through buildings, spaces and imagery. This is related to the point I was making in the book, but there I focussed on facets of the material world whose meaning was not just connected with certain individuals and power relations, but was dependent upon them (by consecration, dispute settlement etc.). While I take Burger's point that episcopal power may not always have been *consciously* felt to be inherent in church buildings, for me it makes sense that people in the legally super-aware memory culture of the later Middle Ages would somehow feel power through objects and the physical spaces of religious life. As in the modern university, there were likely to have been different experiences of power and materiality in the medieval parish. Again, I recognise that there is room for disagreement and differences of emphasis in this field. I have my own scepticism about some aspects of the 'material turn' (for example that the word 'agency' can be applied to objects and people without major qualifications), and I suspect Michael Burger and I would agree on much about the medieval church, or if not then we would disagree productively.