Published on *Reviews in History* (http://www.history.ac.uk/reviews)

Noble Power in Scotland from the Reformation to the Revolution

Review Number:
1214
Publish date:
Wednesday, 1 February, 2012
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ISBN:
9780748612987
Date of Publication:
2011
Price:
£75.00
Pages:
344pp.
Publisher:
Edinburgh University Press
Place of Publication:
Edinburgh
Reviewer:
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In seminar rooms from Dornoch to St Andrews, in Dublin, and around Europe, early modern history informed by social network theory is promoted with an almost religious fervour. Those of us who continue to take a deep interest in ?bureaucratic structures and administrative measures? (p. 179), here ridiculed by Keith Brown, can find its popularity as baffling as that of Twitter. At best, a history rooted in ?relational networks? can serve as a principled, conservative, usually British response to an older, usually French historiography based around impersonal forces and quasi-automatic processes. At worst, it can read as a Babel, a pile of thousands of sound-bites and hundreds of micro-summaries of individuals? careers that can be selected and arranged to mean almost anything the author pleases. In that sense, the saving grace of Brown?s richly detailed *Noble Power in Scotland from the Reformation to the Revolution* is its honesty. Drawing on an impressive array of sources for the theory, as well as the practice, of lordship throughout the period 1560?1637, Brown is clearly not racing to a single explanation or a simplistic conclusion; the book is refreshingly even-handed in regard both to Scotland?s regions and its religious denominations, and the author shows a welcome awareness of the Irish dimension in Scottish and British politics. Most of the chapters are themed around a particular societal niche that nobles held, and within most of these broadly defined roles, Brown finds that Scottish noble power was stable (?Lords?), or declining only slightly (?Magistrates?). In the case of ?Courtiers?, noble power would seem to have spiked toward the middle of the period; and so forth. As one might expect, a book with hundreds of voices yields dozens of conclusions, not

all of them mutually supportive. This level of nuance renders the book no less useful, however, and the chapter ?Magistrates? in particular provides long-overdue explanation of a complex and easily misunderstood area: the Scottish nobility?s control of ?powerful private and hereditary feudal jurisdictions?, a control which was, by European standards, ?extreme? (p. 89). Along the way, Brown takes swipes at ?the myth that nobles thrived on lawlessness? (p. 90), and questions the widespread assumption that royal courts of justice were ?fairer or more efficient? than their numerous noble-run counterparts (p. 89).

The twin Achilles heels of *Noble Power in Scotland* are definition and periodisation. The key questions of what power is, and who nobles are, are treated cursorily. From the result, it is very difficult to tell whether it is Brown, or early modern Scottish society itself, that sees various fellow-travellers of noblemen as also basically noble, and in the end comes to regard nobility and power as so closely intertwined that they could barely be distinguished one from another, as the actual titled aristocracy shades off through ?lesser nobles and men of local significance? (p. 75) to younger sons in the professions, as well as non-nobles who became ennobled as a result of their commercial or professional efforts (p. 175). The firm definition of ?baron? as ?lowest rank of the nobility?, given in the glossary on p. viii, is not borne out thereafter. The wide-net, we-are-his-sisters-and-his-cousins-and-his-aunts approach to noble status employed in the main text is reminiscent of Paul Monod?s approach to English Jacobitism. Here, instead of wondering whether the author perceives any serious difference between having an uncle with anti-Whig views and actually being a Jacobite subversive oneself, we find ourselves wondering if there is any real gap between the nobility as deployed by Brown, and the whole politically active population. Noble power was characterised above all by its ?interconnectedness throughout every corner of society and government? (p. 237). If early modern Scotland was mysteriously quiescent in the face of all this noble power ? it ?had no tradition of anti-noble sentiment? and ?did not experience popular revolts or protest directed at noble society? (p. 244) ? wags will no doubt wonder if it was because the peasants were heavily outnumbered. And to discover that the entire political class of an entire nation had quantitatively the same amount of power at one point in time as at another, even if any such quantification were practicable, is to discover nothing in particular. The infiltration of the professions by the nobility and *vice versa* can be seen as evidence of noble dynamism or noble decline or neither, very much as a matter of personal taste. Yet, seemingly small changes in the *composition* of the political class, changes which Brown makes it his special mission to downplay, are always significant ? assuming only that one?s historical interest is in something less vague than ?power? *tout court*. The numerical expansion of the Scots peerage from 58 persons in 1600 to 106 in 1633 also deserves more time here than it receives (pp. 156, 159). On the nobility front, this book begs the question: is it logically possible to reject the historiography of ?class? in and through a history of *a* class?

The term ?power?, meanwhile, lends itself easily to over-broadness and vagueness, and Brown is to be commended for declining to adopt atomic-age, American-comic-book notions of power as limitlessly self-renewing, non-contingent, and unopposable except by a greater power. I share wholeheartedly his view that government without the consent of the governed in the early modern period was, in effect, physically impossible. Absolutism in early 17th-century Britain was, *pace* J. P. Sommerville and others, a pious hope or a spectre, according to one?s politics, but in no sense a settled state of affairs. It is surprising that Brown, whose book tends to support this broad and important point better than it does any other, gives so little space to it, preferring to position himself in opposition to the notion of a *longue-durée* ?crisis of the aristocracy? ? a topic to which we will return. But even in the case of absolutism, the argument can be shaky: on p. 180, the arrival of the Scottish nobility transforms post-1603 English court culture almost beyond recognition, apparently through sheer force of numbers, but on p. 186, their numbers are so low that the political messages of ?Ben Johnson [*sic*], Inigo Jones and Peter Paul Rubens? are unable to penetrate the Scottish nobility as a group. Power is defined as ?primarily ... political power? (p. 239), but otherwise not defined at all, except perhaps as the sum of its parts. Though the economic, legal, and political spheres are given nearly equal time, the book?s framing chapters (?Noble power and politics, 1560?1603? and ?Noble power and politics, 1603?37?) seem to portray ?power? as a zero-sum game between just two powerful entities, the nobility and the crown, with any decline in the power of the one creating a power vacuum into which the other rushed ineluctably. In fairness, Brown expressly disclaims the zero-sum model, stating instead that the crown could and did increase its power without diminishing that of the nobles (p. 209), pointing to ?the

massive reservoir of loyalty to the crown in noble society? (p. 12), and making it clear that nobles ?colluded in and prospered under? such limited growth in state power as occurred (p. 245). At its most sensible, *Noble Power* argues that while state power grew, a little, and noble power changed, a little, these things are not necessarily cause and effect. Be that as it may, the book leaves almost unexplored the possibilities that the crown and the nobility both lost power to some other group or groups, either within Scottish society or outside it; that the power of the state, taken as a whole, might experience phases of marked decline; and that noble attempts to strangle the crown?s effectiveness, to the extent they were successful, might have been experienced not as a ?swing? from one party to another, but a destructive diminution of Scotland?s power in general. Could noble arrogance, like royal absolutism, be a symptom not of strength but of weakness? What if the outbreak of noble-led revolution was not the ultimate manifestation of the nobles? ?immense power? (p. 209), but a defensive response to that power ebbing away ? either because it *was* ebbing away, or because nobles thought it was? (Brown comments on p. 215 that a ?general attack? on the nobility by the crown was perceived by contemporaries, but did not actually occur, which raises more questions than it answers.) Immense power or no, can the noble-led revolution be considered a ?success? (p. 245), given that the ?noble-dominated covenanter government? (p. 147) was economically and militarily emasculated by the (also noble-led) royalist guerrilla campaign of 1644?5? And that less than a decade later, Scotland had been forced into an incorporating union with an England no longer dominated by its own nobility? In the medium term, was the covenanting Scottish nobles? total victory over the crown anything other than a disaster, both for themselves and for the Scottish nation as a whole?

My difficulties with the definitions of ?nobility? and ?power? are mere quibbles, however, in comparison to the galling mismatch between Brown?s chosen period and his theoretical aims. I am not merely referring to the above-mentioned effects of choosing to end the story in the late 1630s, and the book?s infrequent, brief and vague admissions that major societal change occurred shortly after the end of the period covered by it (pp. 60, 80). The central argument made by Brown is that the nobility was not in decline. This leads him into a series of needlessly controversial arguments to the effect that changes perceived by contemporaries were not actually occurring. I found it particularly interesting that, over the relatively brief period covered by the book, exemplars of the ?ideal of lordship circulating within noble society? shifted from living persons to the dead; but even this important finding is twisted into an argument that there was ?no change? to the ideal itself (pp. 62?3). Two of several ghosts in the machine here are Lawrence Stone?s discredited *The Crisis of the Aristocracy 1558?1641* (1), and J. C. D. Clark?s various critiques of Stone. In his most Clark-esque moments, Brown seems to be trying to demonstrate that basically, nothing happened (pp. 86?7). Yet *Noble Power* can easily be read against this grain, and contains within itself an alternative narrative, in which the Scottish nobility failed to colonise Nova Scotia, failed to colonise Lewis, failed to maintain themselves as hereditary justices-general, failed to prevent the crown clawing back sheriffships and regalities, failed to block the rise of the Court of Session and the JPs, and failed to retain control of the Scots Parliament; and in which ?[t]he military dimensions of lordship did recede? (p. 87), and Highland clans did become ?vulnerable to crown policing? (p. 126). There is, of course, a difference between ?military dimensions of lordship? in a domestic/administrative sense and the participation of nobles in warfare elsewhere, and the positions Brown takes on military affairs are not always easy to comprehend. He cites Roger Manning?s excellent *Swordsmen: The Martial Ethos in the Three Kingdoms* (2) in support of his own comment that military participation by the English peerage rose under Elizabeth but fell under James (p. 124). Manning seems to me to say the opposite: that the English peerage was comprehensively, and steadily, re-militarised over the 50-year period between the Armada and the outbreak of the Bishops? Wars. Military peers ? if defined as those who held military rank, or who fought (with or without formal rank) in at least one battle or rebellion ? increased from 36 per cent of all English peers in 1595, to 45 per cent in 1605, 49 per cent in 1615, 57 per cent in 1625, and 65 per cent in 1635.(3) Brown claims further, not citing any particular source, that the proportion of the Scottish nobility to experience military service between *c*.1585 and *c*.1610 fell from ?around half? to ?around a third? (p. 124); this seems also to be contradicted by Manning, for whom it remained relatively steady, with the mean, median and mode all hovering around 47 per cent from 1585 to 1630. This discrepancy may be a result of the conceptual slippage between ?nobility? and ?peerage? about which I have already complained, but if so it is an interesting finding in itself that deserved more exploration. The routine misspelling of ?ordnance? as ?ordinance? (pp. 128?9) in an otherwise well copy-

edited volume suggests that military affairs may not be Brown?s forte, and indeed much of the chapter ?Soldiers? seems familiar. In any case, those of us whose responses to Geoffrey Parker?s ?Military Revolution? hypothesis range from scepticism to revulsion are unlikely to appreciate a chapter founded on Brown?s unelaborated view that the Military Revolution ?remains persuasive? (p. 121). A glancing blow struck by Brown against the concept of the fiscal-military state, while uncontroversial in itself (?Arguably, Charles I?s efforts to wage war wrecked his ability to govern?: p. 146), points up the greater absurdity of 1560?1637 as a stand-in for early modernity as a whole. Even enthusiasts like Steven Pincus would not, I think, date any aspect of the British fiscal-military state to any period prior to the Interregnum. If Brown is, as he claims to be and seems to be, fighting a war against the discourse of ?state formation? (p. 245 and throughout), why is he not writing about the *late* 17th century, when, by and large, those scholars who believe in ?state formation? believe it to have taken place? Why does he neither endorse nor refute Phil Withington?s model of 16th-century non-absolutist ?state formation?, as including

> a huge increase in the amount of centrally decreed governance from the 1560s [onward] that was implemented through ? rather than despite ? the connivance and discretion of local (and often quite humble) office holders.(4)

? a sentence which, by itself, calls into question the notion of rich nobles and poor nobles forming a common front against a ?state? made up principally of themselves and their nephews and sons? If Brown is, as he claims to be, fighting a war against the idea of a long, slow aristocratic decline that ended ?in the half century after 1914? (p. 238), what does he mean by locating his challenge to it in such a brief and early period as 1560?1637? As to what *did* eventually happen to noble power in Scotland, Brown provides no clues, except to say that the two centuries from *c*.1550 to *c*.1750 ?saw the consolidation of noble power across Europe? (p. 239). I would be grateful to hear if it was thereafter a brief, rapid decline, or perhaps an instantaneous evaporation of power, perhaps with the Heritable Jurisdictions (Scotland) Act 1746. If it turns out that there was, in fact, a long, slow erosion of Scottish noble power between, say, 1746 and 1945, perhaps at the hands of a ?meritocratic civil service and managerial class? (p. 238), Brown will have failed not merely to deny the importance of state bureaucracy in the wider process of noble decline; he will also, to some extent, have over-sold his ideas that Jacobean Scotland was developmentally normal (p. 238) and not ?under-governed? (p. 99).

It would be justifiable to write a biography of Dwight Eisenhower that ended with his inauguration as president in 1953. It would not be justifiable, however, to pretend that the content of such a book showed that Eisenhower never retired from politics, never grew old and never died. Placing events ?beyond the scope? is an authorial choice, not a final trump card. All of us study particular periods because we like something about them, and with time and effort, this liking may become a deeper understanding, and in rare cases like Brown?s, actual mastery. But a person?s mastery of an historical period, however complete, cannot and must not be inflated into that period?s mastery over other periods. Brown?s hope, expressed on p. 246, is that this ?case study? forms or will someday form part of a ?highly collective? intellectual enterprise. Such hopes also attended the production of county-by-county studies of the English Civil War, even as the notion of an *English* civil war steadily lost credibility; 80 years and one Conrad Russell later, the project is incomplete and incapable of completion. In the time it takes to write one book such as *Noble Power in Scotland*, the grounds and purposes of historical inquiry into Europe?s nobilities will profoundly change, in ways that no historian can now predict; and to justify a book?s limitations by reference to a future constellation of similar books about other countries and/or other time periods, is to justify in vain.

To enjoy reading this book, which I do, is not to reject its conclusions, but to doubt that they ever were conclusions *per se*, as opposed to the author?s opinions, strongly held and frankly expressed, bracketing a series of important facts that it behoves every early modern historian to know. One would not expect that a weak argument, weakly argued would make for a very useful book; this may be the exception that proves the rule.

Notes

1. Lawrence Stone, *The Crisis of the Aristocracy 1558?1641* (Oxford, 1965).Back to (1)
2. Roger Manning, *Swordsmen: The Martial Ethos in the Three Kingdoms* (Oxford, 2003).Back to (2)
3. Ibid, p. 18.Back to (3)
4. Phil Withington, *Society in Early Modern England: The Vernacular Origins of Some Powerful Ideas*, (Cambridge, 2010), pp. 155?6.Back to (4)

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