

## Domesday: the Inquest and the Book

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It is now forty years since Galbraith published the *Making of Domesday Book*. Since then his thesis has been refined in various ways, but there has been no serious challenge to his central propositions: that the object of the Domesday survey was to produce Domesday Book, and that the purpose of the whole enterprise must be inferred from Domesday Book itself. Dr Roffe's book develops a radical alternative thesis.

His main propositions are these. William the Conqueror did not commission Domesday Book. The threat of invasion and the strain on resources created by the need to billet a large mercenary army in England caused him to commission the Domesday inquest at Christmas in 1085. This inquest took place the following year and produced a survey of royal resources and a geld survey, and also a survey of the tenurial resources of tenants-in-chief and their tenants. Prior to 1086, the land which tenants-in-chief held in demesne had been exempt from the geld, and the purpose of the inquest was to identify this land with a view to taxing it. All the records from the inquest were brought to the king. There followed some hard bargaining between the king and his barons: in return for the loss of geld exemption on their demesne, the tenants-in-chief received certain concessions concerning the service they owed to the king, and their requirement to billet mercenaries was also lifted. The production of Domesday Book was an entirely separate and later exercise. Domesday Book was 'unrelated to the concerns which launched the inquest in 1085. It seems to have been compiled, probably under the supervision of Rannulf Flambard, from the records of the inquest after 1089 and is best interpreted as a response to the revolt, and consequent tenurial chaos, of 1088' (p. ix).

The book is structured as follows. Chapter 1 describes how Domesday Book acquired the awesome reputation which accounts for its name. It also surveys Domesday historiography since Galbraith. A fixation with Domesday Book - 'the mystique of the book' - has caused most scholars to ignore the possibility that the Domesday inquest and production of Domesday Book might have been separate exercises. Chapter 2 turns to land tenure, title, and the problem of tenurial continuity between 1066 and 1086. Roffe rejects the

proposition that the Domesday survey was intended to determine or resolve disputes concerning title to land. The 'Norman Settlement' was virtually a *fait accompli* by 1085. The process of transferring land from English antecessores to the incoming aristocracy was comprehensive and efficient, mainly because one of the structures of pre-Conquest lordship - parcels of bookland held by 'king's thegns' with 'sake and soke' - had been preserved in the process. Domesday Book suggests that only a small proportion of the estates in England were the subject of disputes about title in 1086 and the inconsistent, almost casual, manner in which those disputes were recorded proves that 'title was not an issue in the Domesday inquest' (p. 46).

Chapter 3 compares the Domesday inquest with certain later enterprises including in particular the inquests which generated the 'Hundred Rolls' of Edward I's reign. It argues that inquests did not as a rule have 'executive' power: the function of the inquest as an instrument of government was 'more modestly to inform' (p. 50). By analogy, the Domesday inquest was merely a data collection exercise: 'its business was to collect evidence for further action, whether it be fiscal, administrative, or judicial' (p. 67). Chapter 4 contains detailed descriptions and analyses of the principal extant Domesday texts: Great Domesday Book (GDB), Little Domesday Book (LDB), Liber Exoniensis (Exon Domesday), Inquisitio Comitatus Cantabrigiensis (ICC), Inquisitio Eliensis (IE), and a number of other Domesday-related texts which scholars have hitherto referred to as 'satellites' but which Dr Roffe prefers to call 'schedules'.

The remainder of the book attempts to reconstruct how the Domesday inquest proceeded and how Domesday Book was written. Roffe rejects Galbraith's premise that all of the extant documentation had a role in an integrated process which ultimately led to the production of Domesday Book, and argues instead that different categories of documentation were generated during the Domesday inquest for specific and discrete purposes. Chapter 5 deals with the process of data collection. First, there was a survey of royal lands and resources: the information collected at this stage eventually supplied raw material for the descriptions of towns, customs, and for the 'terra regis' sections in Domesday Book. Separately, there was also a geld inquest which took place in county courts. This inquest may have drawn on a certain amount of existing data and documentation, but it also involved input from tenants-in-chief who tendered certain basic details with respect to their estates. The result of this exercise 'was effectively a fully annotated geld list in which explicitly fiscal data were combined with presentments on title and tenure' (p. 138). Landholders were then called before the Domesday commissioners a second time: a date was appointed for each landholder to make his 'presentment', and it was during these sessions that the more detailed record of individual estates took shape.

The role of the Domesday commissioners in all this was limited. They were not expected to confirm title or to resolve disputes, but were simply required to collect data for fiscal purposes (chapter 6). All demesne land held by tenants-in-chief was exempt to the geld. The information collected during the course of the Domesday inquest - in particular ploughland data - would enable royal officials to identify the exempt portion of the demesne of tenants-in-chief so that it could be taxed. 'The ultimate aim was to increase the yield of the geld. The inquisitio geldi had identified hides that had been hidden and individuals who had not paid. The calculation of ploughlands went further by investigating how much more the tax might yield to the king' (p. 162). Domesday commissioners produced their findings in the form of 'circuit reports' (chapter 7). There was no standard format. Exon Domesday is a circuit report which lists estates in a 'seigneurial' order, under the heading of each tenant-in-chief and within these headings by county. ICC is a fragment of the report produced for the East Midlands circuit (circuit III) and lists estates in a 'geographical' order, hundred by hundred rather than fee by fee. Since GDB preserves clear traces of a 'hundredal order' in most of the counties it covers, Roffe concludes that most of the circuit reports followed the ICC arrangement or something approximating to it.

Chapter 8 considers the writing of Domesday Book itself and reconstructs the order in which it was written. LDB was not, as Galbraith argued, a circuit report at all. It was based on 'a fully formulated, geographically arranged recension of the data' (p. 177), and was the first part of Domesday Book to be written: 'what is now volume II of Domesday Book should be more properly volume I' (p. 223). LDB was a prototype written by seven different scribes and it provided a model for the writing of GDB, but GDB was written in its entirety by a single scribe (with a small quantity of matter written by a second 'correcting' scribe). The scribe of GDB

began by writing up a fair copy of circuit VI (the northern circuit), devising methods for distilling the matter before him into a comprehensible format as he went. As work progressed, he became more assured in the way he handled his source material. The visible signs of this growing confidence - in particular, the number of rulings to a page which increased until abandoned altogether - make it possible to reconstruct his progress writing up the various circuit reports: circuit VI was followed by circuits III, I, II, V and IV in that order. Chapter 9 summarises the argument and considers the dating evidence. It concludes that both LDB and GDB were written during the reign of William Rufus: Rannulf Flambard was " 'the man behind the Domesday Book' (as opposed to 'the man behind the survey')" (p. 246).

This is by any standards a remarkable thesis. To read it is to feel as if the landscape of Domesday scholarship has been struck by a tidal wave: one feels compelled to return to that landscape to see how many of the familiar structures of Domesday historiography have been swept away, and whether some of them will remain standing when the water eventually recedes. What follows can be no more than a sketch of some initial reactions: some of Roffe's propositions are forceful and compelling, but others are much less persuasive.

Some of the most convincing matter is that which relates to the structure of the Domesday inquest. The suggestion that the inquest went through several discrete stages corresponds neatly with the near contemporary descriptions of the process supplied by recension 'E' of the Anglo-Saxon Chronicle and by Robert de Losinga. The inquest clearly did result in the production of a wide range of written materials, and it is by no means clear that all of them were subordinate to the objective of producing Domesday Book: some of them may have been useful outputs in their own right. This suggestion has an important bearing on the question as to why Domesday Book was made. Since the arrangement of the text would have made it difficult for royal officials to elicit fiscal information from Domesday Book, many scholars have found it difficult to accept Maitland's famous assertion that Domesday Book was 'a geld book'; conversely, explanations which have stressed the essentially 'feudal' arrangement and purpose of Domesday Book have always struggled to explain why it contains so much fiscal information. Roffe reminds us that, in addition to circuit 'reports', the inquest also produced documents which would have been eminently serviceable in a fiscal context including geld lists and 'summaries' of the tenurial and financial resources of tenants-in-chief. It may be that texts of this kind are not prominent in the extant corpus of Domesday texts precisely because they were immediately and heavily used by agents of royal government. It is perfectly plausible that King William had more than one objective when he commissioned the Domesday inquest in 1085 and, as often happens as large-scale surveys or research projects develop their own momentum, those objectives may have become refined as the inquest progressed. With these considerations in mind, it is no surprise that the inquest generated a wide range of written outputs as well as the raw material for Domesday Book itself.

Since the publication of Round's *Feudal England* in 1895, it has been axiomatic that one must understand how Domesday Book was made in order to appreciate why it was made. However, Roffe has demonstrated that this is by no means the only justification for the study of the making of Domesday Book. The present book (and Roffe's work taken as a whole) has shown that it is vital to understand how Domesday Book was made before interpreting the matter it contains. Roffe's analysis of the northern circuit first printed in the *English Historical Review* and reworked for inclusion in chapter 8 of the book is brilliant and fundamental. He shows, for example, that some of the peculiarities of Yorkshire Domesday may not have been a function of the harrying of the north by the Conqueror's army, but may instead have been caused by the rather more mundane activities of the GDB scribe who began his work with material relating to Yorkshire and was still developing his technique for abbreviating his source material as he wrote. The wider implications of this proposition are considerable. Domesday evidence supplies us with an image of eleventh-century England which has been refracted through a complex sequence of lenses, and it is necessary to reconstruct this apparatus of distortion in order to bring the original image back into focus.

It would be possible to list the virtues of the book at greater length. Some of Roffe's propositions are of such great moment that they merit very careful consideration; his difficult but plainly important thesis on demesne geld exemption is a case in point. For these reasons, one might go so far as to describe the book as a gold mine of Domesday scholarship; but the corollary is that one must mine for the nuggets of gold that it

contains. There are also some striking omissions and some very doubtful propositions.

The book is the product of very considerable learning. Not the least of its virtues is its extensive bibliography. It is therefore all the more surprising that two of the most important contributions to Domesday studies since 1986 have not been taken up. Roffe's discussion of Exon Domesday is seriously vitiated by its failure to take account of work by Dr Teresa Webber, who has established beyond reasonable doubt that Exon Domesday was at least partly written by Salisbury scribes. It is also surprising that Roffe does not engage with the researches of Dr George Garnett, who has shown that a substantial proportion of the documentation of the Conqueror's reign is concerned with questions of legitimacy associated with and arising from William's supposed claim to the throne of England, and that Domesday Book itself, with its unremitting insistence that the tenants of 1086 were the legitimate successors to those who held land *Tempore Regis Edwardi*, is a logical and integral part of the Conqueror's propaganda. This is a significant omission in a book which attempts to divorce Domesday Book from the Conqueror's regime.

Roffe has many innovative suggestions and hypotheses, but there is a tendency at crucial moments for the interesting possibility to harden into established fact, and for this to form the basis for further, increasingly flimsy speculation. Roffe's thesis on bookland, sake and soke and tenurial continuity after 1086 is a case in point. It may be epitomised as follows. A lord who had both jurisdictional rights and the right to collect certain other renders due from an estate held it with 'sake and soke'. This was a privilege which only the king could grant in the form of royal diplomas or writs, and thus 'sake and soke was synonymous with tenure by book' (p. 33); bookland was therefore land held by a king's thegn with sake and soke. This form of lordship was distinct from personal or commendatory lordship. Old English legislative texts made the distinction between a hlaford, a lord by commendation, and a landhlaford, a lord of bookland; and the same distinction can also be observed in or inferred from the formulae which describe pre-Conquest lordship and land tenure in Domesday Book. The structure of manors and their dependent sokelands are only consistently identified in circuit VI, but elsewhere it is possible to detect bookland manors and their dependent estates. For example, in circuit III, the 'notice of freedom to go with land and the like is in effect a record of subordination to a booklord' (p. 34). Those TRE landholders whose tenure is described without reference to a freedom of alienation formula must therefore have been king's thegns and the holders of bookland, so the '[x] tenuit' formula in Domesday Book identifies king's thegns holding bookland with sake and soke. The 'valuation' or 'valet' of each manor in Domesday Book was not the value of the land to its tenant: rather, it was 'a sum that went out of the estate to an overlord in recognition of a soke relationship' (p. 42). Units of extended lordship held together by sake and soke were frequently transferred wholesale from pre-Conquest thegns to barons after 1066: in the north 'over three quarters of honourial courts met in the later Middle Ages in manors which were held with sake and soke, toll and team in 1066' (p. 36); 'baronies represent pre-Conquest estates both in form and composition', and 'in its essential the honour was a pre-Conquest institution' (p. 45).

At first sight, this is an exciting argument which seems to offer a new method for decoding Domesday evidence and for reconstructing the tenurial landscape of both pre- and post-Conquest England. However, the argument presents many difficulties. There is not the slightest doubt that some Anglo-Saxon royal diplomas and many late Anglo-Saxon writs conferred rights of sake and soke to beneficiaries including some thegns, but this does not prove that the acquisition of bookland was the only way to acquire this form of lordship. There are good reasons for supposing that bookland did not convey peculiar rights of lordship at all: the special value of a bookland estate was simply that its owner could grant or dispose of it as he wished in ways which could preclude the hereditary expectations of kin (in this respect it was distinct from folkland). Grants of bookland were not the only source of royal patronage in late Anglo-Saxon England. For example, the king could also grant estates to officials such as earls for the duration of their period in office without ever issuing a royal diploma, and it is probable that many of the great manors to which soke was rendered in Domesday England were comital manors of this kind. There was certainly an important distinction between personal lordship and lordship over land in pre-Conquest England, but there is no clear evidence that the word landhlaford meant anything more than 'landlord' in the most general sense. Everything we know about soke renders is that they were relatively light and occasional, and it therefore

seems highly improbable that the Domesday 'valet' represents the sum rendered by a sokeman to an overlord who had his soke: there remain good reasons for accepting Reginald Lennard's argument that it was an estimate of the sum of money which an estate would yield if it were farmed out on lease. There are also reasons for doubting whether the '[x] tenuit' formula did anything more than establish the identity of pre-Conquest landholders. When formulae with power of alienation clauses are collected from circuit III and elsewhere in Domesday Book, it seems plain that when jurors testified that a man could go where he would with his land, they were not recording 'subordination to a booklord' (p. 34), but were simply distinguishing between laenland (leased land) held in dependent tenure (in which case the TRE tenant could not go where he would) and allodial or demesne land which was not held in dependent tenure (in which case the holder could go where he would). For these reasons, soke lordships are only clearly visible in circuit VI and (to a much lesser extent) circuits III and VII; and it is therefore impossible to quantify how significant these lordships were either before or after 1066.

Of course, these objections are not statements of established fact, but propositions which must (and will) be argued through at greater length; but it is not necessary to accept all of the objections listed here to register a wider methodological point, that Roffe's technique for identifying bookland and soke lordship from the Domesday evidence is a long and precarious chain of mutually dependent arguments which cannot hold the weight placed upon it.

Roffe's suggestion that work did not commence on Domesday Book until 1089 is also built on insufficiently secure foundations. It hinges on just two entries in Domesday Book which refer to William de Warenne as earl. These are significant because two apparently independent sources suggest that William was not appointed to the earldom of Surrey until the first few months of the reign of William Rufus, and because one of the entries occurs in the Huntingdonshire Domesday in circuit VI, apparently the first circuit written up by the GDB scribe. If this evidence is accepted, it follows that work on Great Domesday could not have progressed far by the date of William's appointment. This much was established by Dr Chris Lewis writing in 1990. Roffe pushes the argument further. He cites a passage from Orderic which is concerned with events in 1089 and which says that William Rufus caused Rannulf Flambard to 'revise the survey of all England' and to measure all the hides in England 'with a rope'. Roffe also argues that the treatment of Rannulf's estates in Domesday Book were in certain respects 'anomalous' (p. 247). On this basis he concludes that work on Domesday Book did not commence until after 1089, and that Rannulf was the man behind the book.

Once again, the argument hangs from a precarious chain of possibilities with weaknesses in each of its links. The date of William de Warenne's appointment to the earldom of Surrey is not as secure as one would like since it depends on the testimony of two early twelfth-century chronicles: Orderic, who attributed the appointment to both William I and William II in different parts of his Ecclesiastical History, and an anonymous text known erroneously as the 'Hyde Chronicle', which was probably written in the late 1120s or early 1130s at Lewes priory where William de Warenne was buried. It is also possible if not probable that the Domesday entries which appear to assign William a comital rank contain simple scribal errors. One of the entries in question must contain a mistake of some sort because, contrary to all the conventions of Domesday Book, it makes Hugh de Bolbec both the tenant-in-chief and the tenant of 'Earl' William in respect of the same estate. The suspicion of scribal error also hangs over the other entry, which occurs in William de Warenne's fee in the Sussex Domesday. This is immediately preceded by three large chapters describing the Sussex fees of Count Robert of Eu, Count Robert of Mortain and Earl Roger of Shrewsbury in which the GDB scribe wrote the words 'de comite' literally hundreds of times. The particular entry in question describes an estate which was attributed to Earl Godwine TRE. It is therefore legitimate to wonder whether the words 'de comite' slipped into William de Warenne's fee in error. Either way, one is bound to ask: if the scribe knew that William de Warenne had been appointed earl, why did he not apply that title to him consistently throughout the text?

The prologue to IE is usually, and with good reason, thought to preserve the 'terms of reference' which informed the Domesday survey. There is an important Carolingian parallel here since, as Patrick Wormald has observed, the written returns from the survey launched by Charlemagne's *Capitulare de Villis* are preserved in the very same unique manuscript. This creates major difficulties for Roffe's thesis since, if the

IE prologue does indeed preserve the 'terms of reference', it is powerful evidence that something very much like Domesday Book was envisaged at an early stage during the Domesday inquest. Roffe's attempt to explain it away as a 'template for the abbreviation of the Northern folios' (p. 116) is not persuasive. Nor is his handling of the colophon to LDB, which famously proclaims that *ista descriptio* was made in 1086 'not only though these counties but also through the others'. Roffe's argument that these words were written in 1089 referring to the inquest in 1086 is a remarkable exercise in intellectual yoga, but the common sense (and etymologically sound) alternative is simply to suppose that the word *descriptio* refers to the written output of the survey - that is LDB itself.

Further objections to the post-1088 dating of Great Domesday have been suggested by Caroline and Frank Thorn in a weighty article, imminently forthcoming, which distils some of the findings of their and Michael Gullick's researches on the writing of Great Domesday Book. They argue that work on Great Domesday began late in the summer of 1086 and was probably not completed until well into 1088. Work on Great Domesday began during the later stages of the inquest in 1086 while source material was still coming in: the scribe wrote in spurts, sometimes from more than one source, editing material as he wrote; he left spaces in his text apparently with the expectation that further information would be forthcoming; and in a substantial number of instances, he and another scribe went back over the text making corrections and additions as new information came to hand or as errors were identified. The Thorns also make the most obvious and perhaps the most powerful objection to the thesis that Domesday Book was commissioned by William Rufus. The text makes no attempt to distinguish King William the elder from King William the younger; indeed, William 'son of the king' is only specifically identified once in the entire text, and in this entry he is accused of despoiling land from the monks of Sherborne (GDB 77a (Dorset 3:6)). As for Rannulf being for the man behind the book, there is nothing particularly 'anomalous' about the treatment of his name or the representation of his estates in Domesday Book; indeed, his name is spelt in seven different ways throughout the text, a point which seriously weakens the suggestion that he was singled out for special treatment in the text. These considerations weigh heavily against an argument which turns on two possibly erroneous entries in Domesday Book combined with the dating evidence of two twelfth-century chronicles.

This is an imaginative and courageous book, and a valuable contribution to Domesday scholarship. The tidal wave it has generated may demolish some of the more vulnerable structures of Domesday historiography, but many of Roffe's arguments are deflected by the greater force of the structures they are directed against. It is one thing to argue that the Domesday inquest had more than one objective and consequently produced more than one output, but quite another to say that the production of Domesday Book itself was not one of the objectives envisaged from the outset. Again, it is one thing to argue that GDB was still being written early in the reign of William Rufus, but quite another to say that it was commissioned at that date. *Domesday: the Inquest and the Book* is another important refinement on Galbraith's thesis, but its attempt to refute that thesis is not ultimately persuasive.

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