Chapter 1
Introduction

The chapters which follow were written over a period of about six years, in exactly the sequence in which they are printed here, except that this introduction was written last. Each chapter was completed before the next was begun; often some interval elapsed before I was sure what the next chapter ought to be about, let alone what it ought to say. At the time when I was writing chapter 2, I had no clear idea what chapter 3 would look like, no idea at all what the final chapter would look like, if I ever got so far. In drawing the schematic map which sums up the conclusions reached in chapter 2 (Fig. 7), it did not occur to me that this map would prove to have the deeper significance which accrues to it later on (Fig. 12). It was a slow, sometimes painful process, which at any moment might have ended in failure; but in the event I managed to keep moving forward, and I think that I have now reached the end.

In revising the text for publication, I have been able to streamline it to some extent. I have consolidated the bibliography; I have cut out some repetitious passages, replacing them with cross-references. But I have not attempted to give the impression that these chapters were all written at a single stretch. By and large I have left them as I wrote them, even though there are passages here and there which I would have worded rather differently had I known at the time what I only discovered later. Occasionally I have allowed a passage to stand which I no longer think to be right, adding a footnote to explain to the reader why I have changed my mind.[1] This is, I hope, not mere laziness or self-indulgence. It seems to me that the reader will be able to understand the interpretation more readily if he or she can reenact something of the process of exploration by which it was arrived at.

Though I have tried to deal fairly with the reader, not trying his or her patience further than is necessary, not demanding assent where the evidence does not require it, there are some idiosyncrasies of mine which may cause irritation.[2] I recognize this; I cannot help it. For a start, I seem to have an obsession with putting things in the right order. This is, for me, the font of all wisdom, as far as the survey is concerned; but the reader may think it pathological. Then again, I have a deeply-rooted distrust of the power of words, which, though it seems perfectly justified to me, may seem perverse to the reader. Two words in particular – words which anyone who knows the literature will be expecting to find on almost every page – are largely absent here. I do not voluntarily use the word 'Domesday' (except when I am speaking of the use to which the records of the survey were put, in the late twelfth century and later), because it seems to me to beg too many questions.[3] I do not use the word 'circuit', because, in the current discourse, it implies belief in a silly conjecture which has done a great deal of damage. Words of this sort are idols. We need not waste time subverting them; it is enough just to ignore them.

On the other hand, I ask the reader to tolerate a certain amount of algebra. It seems to me indispensable to develop some notation which can mean exactly what we want it to mean. Without any further preamble, I will introduce the notation which I have been using for the last few years, and have found to work well enough (though I do not say that it cannot be improved on). For the four versions of the survey text – the versions defined and sequenced by Galbraith (see below) – I use the following symbols:

B = the version represented by a late twelfth-century copy from Ely (BL, Cotton Tib. A. vi, fos. 71–98)

C = the version represented by Exeter Cathedral Library 3500, fos. 25–62, 83–494, 530–1

D = the version represented by PRO, E 31/1

DB = the version represented by PRO, E 31/2, fos. 0–372

The notation is arbitrary (there is no version A), but the reader who wants some mnemonic might consider making use of the following words: *breves*, *codicelli*, *Descriptio*, *Descriptio brevis*.

To identify each county I use a two-letter code, which generally consists of the first and second or first and last letter

[1] Additions of one kind or another, most numerous in chapters 2–3, are enclosed in double brackets: I hope that this makes them obvious enough but not excessively conspicuous.

[2] It annoys some people, I know (because they have told me so), that I have taken to spelling medieval names – Willelm, Rotbert, Goisfrid, and so on – in a contemporary manner. But I see no need to apologize for this: it is a matter of simple courtesy to try to spell a person's name correctly, all the more so if the person is dead.

[3] The names 'Great Domesday' and 'Little Domesday' have only recently become fashionable, but are not of recent creation. Through Pollock (1895), I trace them back to a popularizing book by Morgan (1858). (Apart from his name, I know nothing about the author, and I have not seen the book. There is a review of it, however, in *Gentleman's Magazine*, 205 (1858), 120–7.)

of the name (Table 1).[4] Thus I write D-Ex to denote the D text for Essex. By extension, D-Ex can mean the booklet containing this text, and D-ExNkSk can mean the volume containing the D booklets for Essex, Norfolk and Suffolk.

Especially in dealing with B, it is useful to have some easy way of warning the reader – or oneself – that what is being said, though true for some copy that survives, is not necessarily true for the original. For this purpose I have got into the habit of using a slash, and again I find that the trick seems to work well enough. Thus B-Ca / V means the B text for Cambridgeshire as it is represented in the copy that I call V (the only copy, as it happens).[5]

The same notation may be found helpful in dealing with the various derivative texts which survive as copies from the archives of the monasteries which commissioned them. I have deliberately avoided discussing most of these texts; but there are two which I have cited often enough that some algebra seems to be justified, and this is what I have adopted:

xEl = edited excerpts from some version of the survey text for six counties (Ca, Ht, Ex, Nk, Sk, Hu) made for the monks of Ely, surviving as a mid twelfth-century copy (Cambridge, Trinity College O. 2. 41, fos. 92r–143r)

xAug = edited excerpts from the B text for Kent made for the monks of Saint Augustine's, surviving as an early fourteenth-century copy (PRO, E 164/27, fos. 17r–25r)

Derivative texts like these, surviving only as copies, are difficult material to handle, and historians who have looked at them have generally misunderstood them.[6]

As far as matters of methodology are concerned, I have mostly preferred to let any relevant issues emerge at the appropriate moment, in the course of the argument; but there is one point which should perhaps be alluded to briefly here. Because we are dealing with several successive versions, we have to make it a rule to work backwards, one step at a time, from the latest towards the earliest. If we find something in DB that seems to need explaining, we start by trying to explain it on the assumption that it originated in DB, through some decision (or some mistake) on the part of the DB scribe. If that fails, we go back to D; if that fails, we go back to C; if that fails, finally, we are allowed to go back to B. The rule is, in a word, that no explanation should be deeper than it needs to be. In this respect, life has become much harder since Galbraith. For Eyton, for Round, for Maitland, it was easy to jump from DB to B, almost as easy as to jump from D to B. Take any feature of the DB text: if it seemed sufficiently improbable that this feature originated in DB, one could assume straight away that it originated in B. There was no other possibility. But now we know, thanks to Galbraith, that there are other possibilities which have to be considered, and which (if we remember the rule) have to be considered first. Except in those counties for which part of B or part of C survives, we are extrapolating into the unknown, with nothing to get a grip on. We cannot expect it to be an easy matter to identify textual features which originated in B.

With these points in mind, readers should be able to navigate the following chapters without much difficulty. They are, of course, at liberty to read the chapters in any sequence, or to read just some of them and ignore the rest. But I hope that they will read them all, and read them in their proper order. The analytical chapters (2–9) work backwards from the completion of the survey towards its obscure beginnings. The last two chapters (10–11), which aim towards some synthesis, start at the beginning and work forwards, ending with the compilation of DB itself.

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The interpretation summed up in those last two chapters is (to the best of my knowledge) original in some respects; but it would not upset me at all if anyone wished to call it a neo-Galbraithian interpretation. (From time to time I have called it that myself.) In consequence the reader may be surprised to see that Galbraith's name is seldom mentioned in the following chapters. The reason is that to find the Galbraithian basis on which I have built one has to go back a long way – not just beyond Galbraith's (1961) book, but also beyond the (1942) article which was his first published statement on the subject. At some very early stage in the evolution of his thinking, Galbraith was on the right track; but he was losing the thread by the time that he completed this article, and had lost it completely by the time that he completed his book.[7]

There are two Galbraithian ideas which seem to me to form a solid foundation: this whole book is built upon them. Before Galbraith, no one had been willing to see that there are four versions of the survey text, each distinctly different from each of the others. Galbraith saw that – and had the courage not to flinch away from it. He saw, furthermore, that these four versions can be arranged typologically to

[4] In most cases the meaning will be obvious at a glance; only two codes are at much risk of being misread. 'He' is Herefordshire (not Hertfordshire, which is 'Ht'); 'Be' is Berkshire (not Bedfordshire, which is 'Bd').

[5] In principle I see no objection to the use of multiple slashes. Thus one might write B-Ca / V / Hamilton / Round to mean a passage from B-Ca as it is reproduced in the surviving manuscript, as it was edited from this manuscript by Hamilton, as it was quoted from this edition by Round. In practice, however, I seldom use more than one slash, and would be reluctant to use more than two.

[6] The word 'satellite' is another idol which had better be forgotten. Used casually by Maitland, it took on a life of its own; but it never had and cannot be given any useful meaning.

[7] This is Vivian Hunter Galbraith (1889–1976). The reader who wishes to know more about the man might start with his 'lapse into autobiography' (Galbraith 1970); there is a memoir by Southern (1978), who also wrote a shorter piece for the *Dictionary of National Biography*. (Southern was one of two people – the other was J(ohn) G(oronwy) Edwards – whose help was acknowledged by Galbraith (1942, p. 161).)

make a single sequence, which in my notation is B > C > D > DB. Let it be assumed (we can always discard the assumption if it proves to lead nowhere) that a single sequence does indeed exist: then it becomes a simple exercise to piece the sequence together. The B text has to represent one extremity of it: in this version, unlike the other three, the text is organized cadastrally (hundred by hundred, village by village), not feodally (barony by barony). We can be sure straight away that this extremity is the earlier one, i.e. the beginning of the sequence: without going into detail, it is easy to see how something like D or DB could be derived from something like B, impossible to see how the sequence could work in reverse. The DB text must represent the opposite extremity from B, i.e. the end of the sequence: in this version, unlike the other three, some categories of data (such as livestock statistics) are absent. C and D fall into place between these extremities, and it is clear that C is the earlier of the two. In D, as in DB, the text is written out continuously: except occasionally by chance, the beginning of a new chapter does not coincide with the beginning of a new quire. In C, by contrast (to simplify but not to misrepresent the facts), each chapter occupies a separate booklet. Since D and DB have a property in common which C does not possess, the sequence, for these three versions, has to be C > D > DB.

By constructing this typological sequence, we have arrived at a theory as to how the compilation process worked. (To the extent that this theory makes sense, we have justified the assumption that we made to begin with.) For each county in turn, we start with B, where the entries are cadastrally organized. Extracting from B the entries for each baron in turn, we produce the collection of booklets which constitutes C. Arranging these booklets in a suitable order and turning them into a continuous text, we produce D. Making numerous additional changes – omitting some categories of information, rewriting the entries, altering the format – we end up by producing DB. That is the core of Galbraith's interpretation.

However plausible we think this looks, it does not hold together unless we are willing to suppose that every version of the text (with the possible exception of DB) was originally more comprehensive than it is now. As things stand, two versions are the most that exist for any county, and two successive versions do not exist anywhere. We may be inclined to assume that the surviving portion of the C text was the source for a lost portion of the D text, or that the surviving portion of the D text was derived from a lost portion of the C text, but we are not in a position to prove it. Given that we have to start making assumptions, there is no virtue in making them piecemeal. To gain as much leverage as possible, we have to be ready to make it our working hypothesis that the compilation process was everywhere the same. Under this hypothesis, what is known to be true for one county can (in the absence of proof to the contrary) be taken to be true for every county. This is what I propose to call the uniformity assumption: it is the second element which I borrow from Galbraith. With it, we can make some useful progress, as I hope that the following chapters will go to show.[8] Without it, Galbraith's interpretation does not have any purchase on the facts.

These ideas of Galbraith's are so familiar by now that it takes some effort to realize how novel they were. Some sense of Galbraith's originality is most easily got by comparing his theory with Round's. For Round (1895), the uniformity assumption covered only the B text: because it could be proved that the survey text originally took this form for Cambridgeshire, he saw no difficulty in assuming that the same was true for every county. The facts had to be written down in some form: what could be more likely than that they were written down – hundred by hundred, village by village – in a manner framed by the conduct of the meeting at which the hundred juries were brought before the commissioners? Here in B one could see the survey in action. His views on the rest of the compilation phase were more diffidently expressed. The C text was and remained a mystery for him: he took the risk of ignoring it. For D and DB he had only a tentative explanation to propose (Round 1895, pp. 140–2). Two attempts had been made, he thought, to produce a feodalized version of the survey text. The first attempt produced the D text, a rearranged but unabridged copy of the B text. But then, after only three counties had been dealt with, that attempt was given up. Sooner or later a second attempt was made; and now the policy was for the text to be abridged as well as rearranged. This is what produced the DB text. (For consistency, the clerks ought to have gone back and dealt with the first three counties again on this new plan, completing the DB text and discarding the aborted D text; but they did not feel obliged to do that.) On this view, in short, both D and DB are directly derived from B; the form in which they exist is the form in which they originally came into existence; the uniformity assumption does not apply.

As Round pointed out, it then became allowable to think that D and DB – especially DB – might be, by some significant margin, later than the survey itself. Enthused by that possibility, some historians began competing to push DB further and further forward in point of time;[9] and Douglas (1936, p. 255) summed up this tendency by concluding that it would, at the least, be 'unwise' to suppose that DB was completed before 1100. This article of Douglas's seems to have been the final provocation which stirred Galbraith into action. Nothing enraged him more than any hint of a suggestion that DB was an afterthought.

There were weaknesses in Round's interpretation – most obviously his failure to come to grips with the Exeter manuscript – which Galbraith was able to exploit.[10] Es-

[8] But there is, I think, something to be said in its favour a priori. A job as complicated as this could hardly have been done at all unless it was done systematically.

[9] The winners were Johnson and Jenkinson (1915), who thought that DB might perhaps be as late as circa 1130.

[10] There is a footnote of Round's – 'It will be observed that I do not touch

sentially what he did was to extend the uniformity assumption so that it covered not only B but also C and D. He had hardly made the suggestion before he started changing his mind; but it seems to me that he was right in the first place. The C text, though by accident it only survives for five counties (and is complete only for one) did originally exist for every county; the D text, though by accident it only survives for three counties, did originally exist for every county.

To the extent that the C text survives, it can be compared with the DB text for the same counties; and the comparison shows that DB was derived (perhaps immediately, perhaps not immediately) from C. The point had been proved by Baring (1912), and Galbraith contented himself with citing that paper. But Baring himself referred back to an article by Whale (1905), which, despite its chaotic appearance, has the merit of including the first report of one crucial fact. By and large, the order of the entries in each chapter of DB is the same as in the corresponding booklet of C. Nothing can be argued from that, because the same might be true if C and DB were (as Eyton and others had supposed) derived independently from B. But there is one chapter in DB-Dn which seems to be an exception to the rule, and the order here, as Whale (1905, p. 266) discovered, was produced by a temporary transposition of two quires of C. That evidence is conclusive: DB derives from C.[11] Given that, what Baring envisaged was a single version of the text, intermediate between B and DB, variably C-like or D-like. The recognition that C and D are typologically so different that they have to be taken to represent two separate stages of the compilation process is, I think, original with Galbraith. In every county (or group of counties), a 'rough draft' in the form of C was compiled from B; this was superseded by a 'fair copy' in the form of D; and this was the source text used by the compilers of DB.[12]

Galbraith could cite some evidence which tended to confirm the idea that C was not the immediate source for DB. When facsimiles of two pages from the Exeter manuscript were published by the Palaeographical Society, the editors (or one of the editors) mentioned the existence of two marginal notes elsewhere in the manuscript which appeared to be 'the memoranda of persons engaged on a fair copy'.[13] Even if that is the right interpretation, this evidence does not necessarily mean very much: it might mean that a fair copy was made only of C-Dn (or only of some portion of C-Dn). Even if we took this evidence to mean that a fair copy was made of the entire C text, there would be nothing to prove that this fair copy was the source text used by the compilers of DB. Galbraith, throwing caution to the winds, pounced on this remark.[14] Explicit proof that C gave birth to D, not just here but everywhere, was hardly to be expected, but here was one good hint – all the better because its authors had not been trying to prove a theory, just noting a fact which might turn out to be of interest.

I take the discussion of these points no further here, because the rest of this book is an extended commentary on them. The thrust of it is Galbraithian. In a sense it is more Galbraithian than Galbraith, because he, in this sense, was only a Galbraithian at the very beginning. The typological sequence, B > C > D > DB, is his; his too is the willingness to drive the uniformity assumption as far as it will go. These are the points which seem to me to give this article what permanent value it has. Having made them, however, Galbraith started to back away from them. He was in retreat before he had even finished writing this paper; the retreat became a rout later on.

Some of the reasons why things went wrong are obvious. One striking feature of this article is its failure to make use of a crucial piece of evidence – a strictly contemporary account of the survey, written by no less a person than the bishop of Hereford – which had been discovered and published by Stevenson (1907). This evidence proves (as Stevenson saw) that the survey was conducted in two separate stages; and that had never been known or even suspected. Round, writing in 1895, did not have any way of realizing this; nor did Maitland, or Eyton, or Ellis, or anyone else. Since 1907, historians who drew their inspiration from Round had failed to exploit this new evidence; Galbraith had the chance to do better – had the chance but wasted it. In this article, bishop Rotbert is barely even mentioned.[15] It was only at a later stage that Galbraith (1950) began try-

the *Liber Exoniensis*' (1895, p. 146) – which Galbraith delighted in quoting.

[11] Strictly speaking, the conclusion is valid only for this one chapter; how far to go in generalizing from it is another question.

[12] Some excerpts survive (in the Ely text which I call xEl) from a version of the survey text for Huntingdonshire more primitive than DB-Hu. Round (1895, p. 135) had left this evidence unexplained; Galbraith (1942, pp. 168–9) saw it as proof of the existence of C-Hu or D-Hu. In fact, it is not decidable where these excerpts came from; on balance they are (in my opinion) more likely to have come from B, rather than from C or D. (In Galbraith's view of the case, there was a good chance that they might have come from C. In mine there is hardly any chance of that: the only likely sources are B and D.)

[13] 'As both these notes are in different hands from those of the text, it is evident that they cannot refer to the compilation of the present MS., but are probably the memoranda of persons engaged on a fair copy' (Bond, Thompson and Warner 1884–94, vol. 1, letterpress to plates 70–1). This remark is quoted by Galbraith in an exaggerated and inaccurate form: 'The marginalia, the editors say, "are in different hands from those of the text, from which it is evident that they cannot refer to the compilation of the present manuscript, but are probably the memoranda of those engaged on a fair copy" ' (Galbraith 1942, p. 165, note 2).

[14] As far as I can see, he had no warrant for assuming that these 'marginalia' – all two of them – were in 'non-curial' script (see below), though as a matter of fact they are. (If they were 'curial', they would mean something different.) There is, moreover, a price attached to this conclusion (below, note 24) which Galbraith evaded paying.

[15] He is referred to by name just once, and only incidentally (Galbraith 1942, p. 175). (Here and later, Galbraith tended to call him 'Robert of Hereford', as if he were some local chronicler.) There is also one footnote reference (p. 171), which takes the cryptic form 'Cf. *Select charters* (1913), p. 95': an extract from the text printed by Stevenson (1907) had been included in that edition of Stubbs's book.

ing to fit this evidence into the picture; and he found that there was no room for it. By 1961 he had come to the conclusion that bishop Rotbert was … mistaken. There is, we are told, ‘certainly no question of two successive panels of Domesday *legati*’. Rotbert must have been confusing two different operations. ‘Perhaps the first body of inquisitors were the Domesday commissioners and the second a special Treasury panel sent to collect the arrears of the tax’, i.e. the current geld (1961, pp. 94–5).[16] In effect, we are asked to believe that Galbraith understands what was happening better than it was understood by a well-placed contemporary observer. Galbraith was, to a surprising degree, confident of this himself; but I do not see how we can feel the same.

Apart from ignoring this evidence, Galbraith committed two disastrous errors. First, he took it for granted, seemingly without hesitation, that DB was written by a plurality of scribes. As everyone agrees by now, this was a mistake. It was, at the time, an assumption which had never been questioned; but no one had given it as much importance as Galbraith was about to do. DB, it seemed to him, was the product of a very tightly disciplined scriptorium: the scribes employed here had all been trained to write a distinctive style of script. No one had ever been able to decide how many different scribes were involved – but that just went to show how thoroughly the scribes had been trained.[17] Within narrow limits, they all used the same sort of script, the same abbreviations, the same technical terms, the same turns of phrase.[18] In short, it seemed obvious that DB was produced by a group of government scribes – ‘curial’ scribes, as Galbraith preferred to call them.

In any number of respects, C and D are very different from DB. Each is manifestly the work of a group of scribes, some of them quite good, some others barely competent. Not only does the script vary greatly from scribe to scribe: none of it bears much resemblance to the ‘curial’ script exemplified by DB.[19] So C and D must each be the work of a group of ‘non-curial’ scribes. Who these scribes were, where they came from, exactly how many of them there were, are questions which Galbraith does not seem to have bothered to ask himself; it was enough for him to know that they had not undergone the same ‘curial’ training as the scribes of DB. The interpretation which he developed depends absolutely on this distinction between ‘curial’ and ‘non-curial’ scribes.

Second, he gambled on the rightness of a conjecture of Eyton’s. Thinking (as he did at the time) that the entire survey, including the compilation of DB, was completed within three months or so, Eyton had had to find some way of explaining how this might have been achievable; and he came up with the idea that the work was divided among nine ‘Corps of Commissioners’, each of which dealt with a group of neighbouring counties (Eyton 1877, pp. 107–8). Galbraith adopted this suggestion, to the extent that it suited his purpose. There is no discussion: it is, he says, ‘generally agreed that distinct commissions visited separate groups of counties’ (1942, p. 162).

Was it ‘generally agreed’? Though Galbraith was much better placed than I am to judge, I am not persuaded that this statement was true at the time. Neither Round nor Maitland had expressed support for the idea.[20] So far as they gave it any thought, they would have regarded the suggestion as a guess of Eyton’s; and Eyton’s guesses had almost invariably turned out to be wrong. It is true that the suggestion had been taken up in a popularizing book by Ballard (1906) – this seems to be where Galbraith came across it – but that endorsement can hardly have counted for much.[21] The article by Douglas (1936) mentioned above says not one word about Eyton’s suggestion; it does not even mention Eyton’s name.[22] Stenton (1943) is silent on the subject.

And yet, whether or not the statement was true when Galbraith made it, it certainly did become true. As far as I can see, it went entirely unchallenged.[23] Without bothering to look at the evidence for himself, without being made to justify the assertion, he was allowed to assume that Eyton’s conjecture was an established fact. The country was divided up into groups of contiguous counties; each group of coun-

[16] By 1974, he had changed his mind (without explaining why, without saying that he had done so): now he preferred to reverse the sequence of events. Bishop Rotbert is quoted as saying that there were ‘two separate Inquests in 1086, of which the first was a geld inquest and the second the Domesday survey’ (1974, p. 23). The bishop says no such thing: if that is what really happened, the bishop got it wrong. Yet we are also told, with much more emphasis here than previously, that bishop Rotbert is an excellent witness: it is ‘difficult to imagine a man better fitted to testify’ (Galbraith 1974, pp. 22–3). (This statement is true in a way, but exaggerated to the point of becoming untrue. In fact it is easy to think of a man who would be ‘better fitted’: given the choice, we should have preferred to hear from the bishop of Lincoln – the only bishop who is known for certain to have served on one of the commissions of inquiry.)

[17] Somewhere in my reading, I met with an anecdote about a nineteenth-century government department whose clerks all wrote so similarly that only they could tell which of them had written what. Foolishly I failed to make a note of it. Can anyone tell me where to find it again?

[18] And, one might add, as Sawyer (1956) did, they all spelt English place-names in the same way. And, one might also add, they all spelt French words alike.

[19] There are, in fact, two or three stretches of text in the Exeter manuscript for which one might make an exception; but they are all additions, not part of the original C text.

[20] In a passing remark (‘It should be added, however, …’), Round (1895, p. 134) mentions it as an alternative possibility that the bishop of Coutances and bishop Walkelin ‘may have been, respectively, the heads of two distinct commissions for adjoining groups of counties’. To cite this in paraphrase as ‘Round’s opinion’ (Galbraith 1942, p. 162) is, to put it politely, a careless piece of wording – not as careless, however, as a passage elsewhere (1948, p. 94), which seems to attribute the entire conjecture to Round.

[21] Tait (1908) reviewed the book at some length without referring to this point. Galbraith at that time was a student in Manchester, and Tait was one of his teachers.

[22] Ballard’s book (in its second edition) is listed in the ‘bibliographical note’ (Douglas 1936, p. 249) but never cited specifically.

[23] I do not challenge it here; in the absence of any evidence for it, I take no notice of it.

ties was dealt with by a separate group of commissioners; and – this is where the argument engages with the palaeographical evidence – each group of commissioners was accompanied by a separate group of 'non-curial' scribes.[24]

For Galbraith, furthermore, each version of the survey text (other than DB) existed for only one purpose: to serve as a source for the next version. Once the data had been put into a feodal frame, B was no longer useful. The various portions of the B text were discarded wherever they happened to be at the time; if one portion survived, that was because it was rescued and taken home by the abbot of Ely. Once a fair copy had been made of it, C was no longer useful. The various portions of the C text were discarded wherever they happened to be at the time; if one portion survived, that was because someone in Exeter was able to get hold of it and thought it worth preserving. Officially B and C had both ceased to exist; so far as they survived at all, they were scattered around the country. Only the fair copy was delivered to the treasury. And finally, once DB had been compiled, D was no longer useful: if one portion survived, that was due to some accidental cause.[25]

This is just speculation, though Galbraith presents it in the sort of peremptory language which suggests that only an idiot will disagree with it. I disagree with most of it. As far as C is concerned, I agree that it was made solely for the purpose of allowing D to be made. (I do not agree, however, that any part of either C or D was made in Exeter.) But D contains a large amount of information which was not transferred into DB. Unless one thinks that this information was never really wanted and had only been collected by mistake – Galbraith was reduced to precisely that absurdity – D does not become redundant as soon as DB exists. On the contrary, D is the full record of the survey, and DB is just an epitome, condensed into one volume. Something similar is true (though I was slow to realize it) with regard to B. There is information in B which was not transferred, via C, into D. Unless one thinks that this information was worthless, it seems to follow that B would also have been worth keeping, at least for a while. There is nothing here which conflicts in any significant way with Galbraith's interpretation; he had no logical reason for denying any of this. Instead, for reasons of his own, he preferred to insist on two points which are rather obviously wrong: that the B text never came anywhere near the treasury,[26] and that the DB text superseded all earlier versions.

Having reached this point, Galbraith summed up the argument so far by restating his previous conclusion, B > C > D > DB, but now he had to preface it with an 'if' clause: 'if any uniform system governed the actions of the various commissions', the conclusion would still be valid (1942, p. 169). That is a fatal concession. If the compilation process was decentralized to the extent that Galbraith is suggesting, what justification can there be for assuming that the procedure was uniform? How can we suppose that uniformity prevailed when we cannot see that any means existed for enforcing it? The obvious agents to use would be the 'curial' scribes: let them be trained in the proper procedure and then sent out, one here, one there, to make sure that this procedure is understood and fully complied with by each group of 'non-curial' scribes. But the 'curial' scribes, in Galbraith's interpretation, are not permitted to leave Winchester: they sit there, twiddling their thumbs, while B and C and D are being compiled.[27] There is nothing whatever for the 'curial' scribes to do until portions of the D text start arriving. If anything has gone wrong (if the hundred headings, for example, have been omitted), it is, by now, too late to put it right. The 'curial' scribes can only grumble. The 'non-curial' scribes, for their part, have no contact with the treasury while they are actually at work: the first and only contact occurs at the moment when they deliver the finished D text. (If someone else makes the delivery, there is no contact at all.) Their job is over and done with before that of the 'curial' scribes has even begun.

Galbraith had a choice to make, and this 'if' clause proves that he was aware of the fact. If he continued stressing the decentralized nature of the compilation process, he would have to back away from the uniformity assumption. If he wanted to hold on to this assumption, he would have to find some way of explaining how uniformity might have been maintained, in spite of decentralization. Perhaps more by drift than by conscious decision,[28] he eventually made his choice: he preferred the first alternative. That was a miscalculation from which he never recovered.

The reception of Galbraith's article is hard to gauge. It is not to be forgotten, of course, that the article was written and published in the middle of a war – at a time, that

[24] But this is not a fair conclusion. The only reason for thinking that certain marginal notes in the Exeter manuscript (above, note 13) are 'probably the memoranda of persons engaged on a fair copy' is the fact that they are 'in different hands from those of the text'. If we think that a 'fair copy' was made, we must also think that the commissioners were accompanied by two groups of scribes: the first group was responsible for a 'rough draft' (the surviving C text), but this second group took over when the time arrived to make a 'fair copy' (the lost D text). At a stroke, we seem to have doubled the number of 'non-curial' scribes who would have to be employed. Are we comfortable with that? Do we see any rationale for such a division of labour? Galbraith disposes of these difficulties by ignoring them.

[25] Why this portion survives 'we do not know. … This is only one of many questions … that we cannot answer' (Galbraith 1948, pp. 97–8). Such candour, in my view, is much to be preferred to gratuitous remarks – inspired by Baring (1912, p. 310) – about the 'extreme complexity of the free tenures in East Anglia' (Galbraith 1961, p. 8).

[26] The point being that, in that case, the compilation process must have been started immediately, wherever each portion of the B text was available; otherwise it could hardly have been started at all. This is specious; but a rather more plausible argument became available to him later, once he had proved (Galbraith 1950) that the processing of the survey text was coordinated with that of the current geld account.

[27] Though Galbraith did not know it at the time, two passages in the Exeter manuscript were written by a 'curial' scribe (below, p. 7). But they are later insertions, not properly part of the C text; so they hardly affect the issue.

[28] The same sort of drift which caused 'Essex and East Anglia' (1942, p. 166) to degenerate into 'East Anglia' (p. 168). The same sort of drift to which everyone is prone who teaches about the same subject year after year.

is, when it took an abnormal effort for any semblance of normality to be maintained. Douglas, writing an introduction for his facsimile edition of a manuscript from Canterbury, used the opportunity to make a riposte to Galbraith (Douglas 1944). Another historian, reviewing that edition, used the opportunity to declare himself 'a convinced "Galbraithian" in most respects' (Lennard 1946). At some stage (I do not know quite when), it would have become known that Galbraith was working on a book; and people aware of that would naturally prefer to wait for the book to appear. Excuses for doing nothing just yet are always welcome.[29]

At the time when he wrote his 1942 article, Galbraith was still assuming – like everybody else – that the geld accounts surviving at Exeter (below, pp. 60–9) were accounts of the six-shilling geld of 1083–4, referred to by a disgruntled English chronicler (Swanton 1996, p. 215). The question which seemed to need answering, therefore, was how a batch of two-year-old accounts might have become connected with the record of the survey (1942, p. 171). But some time later he saw that this was the wrong question. The dating proposed by Eyton – accepted even by Round, agreeing this once with Eyton – was in error: in fact it was possible to prove that these accounts are contemporary with the survey (Galbraith 1950). (There is also a remark, instantly forgotten, to the effect that the geld accounts appear to have been written in the treasury (p. 3). So indeed they do – because they were.) It has seemed to almost everyone, it seems to me, that Galbraith was perfectly right. Though no chronicler mentions the fact, it is clear that another six-shilling geld was being collected in 1085–6, and that the business of getting hold of the money and writing up the accounts was in progress concurrently with the business of the survey. Galbraith could produce enough evidence to prove the point; some further evidence (the significance of which is only to be seen in light of the subsequent history of the holdings concerned) was added soon afterwards by Mason (1954). When Darlington (1955), editing the Wiltshire accounts for the Victoria County History, professed to be still uncertain as to their date, Galbraith (1957), reviewing the volume in question, seems to have been more amused than annoyed by this gesture of recalcitrance.[30]

By establishing the date of the geld accounts, Galbraith had made an important contribution; but that, as I regret to say, was the last one. While he was working on that problem, the Exeter manuscript was loaned to the Bodleian by the dean and chapter, so that Galbraith could have the use of it; but nothing much came of that. There is a rather confused footnote regarding the various hands that appear in the geld accounts (Galbraith 1950, p. 6); but mostly he seems to have been working from the printed text. Time passed, the manuscript went back to Exeter, and Galbraith's hope of writing about it 'at greater length in the future' (p. 1) remained unfulfilled.[31] It was Finn (1951), not Galbraith, who noticed that there were two passages, added on blank pages of the manuscript, which were written by a 'curial' scribe – a discovery which obviously had to mean something important, though neither Finn nor Galbraith was ever able to make up his mind quite what that something might be. It was Finn, too, who made the first serious attempt to identify the numerous individual hands which participated in the writing of the main text. After looking at only part of the manuscript, he had already found a dozen different hands; but his results were greeted with so much incredulity – on Galbraith's part, I assume – that Finn became discouraged and gave up. The article that he published on this subject includes a rather pathetic paragraph which amounts to an admission that he has probably got it all wrong (Finn 1959, p. 363); and when he wrote a whole book about this manuscript – a book which says so little to the point that I have not had to cite it elsewhere (Finn 1964) – he avoided the subject altogether.

Galbraith's book was finally published in 1961. From the preface, and from internal evidence (quite frequently a statement in one chapter is contradicted by a statement in another), it is clear that the book took a long time to write; apparently Galbraith had to wait till after his retirement in 1957 before he could concentrate on getting the book completed. Compared with what might have been hoped for, it is a meagre piece of work. Nineteen years on, it is still little more than a sketch. Much of the time, it gives the impression that Galbraith was writing from memory, without going back to look at the evidence again; and often his memory deceived him, not just on points of detail.

Step by step, I have come to think that there is almost nothing of value in this book – nothing worth saying that had not been said before. By the time that Galbraith finished it, he had entirely lost his way: if he was right about anything, by now it was only by accident. This conclusion, so to speak, crept up on me. I did not invite it; I derive no pleasure from it. All the way through, I assumed that I would, after working through the evidence for myself, find out that I was merely rediscovering what Galbraith had already discovered. Looking back, however, I see that this never happened. Not once. Every time it turned out that Galbraith had got things wrong – had misstated the facts, had posed the wrong question or come up with the wrong answer, had waved away some serious objection with a facetious remark. As the reader will notice (below, p. 134), one of Galbraith's ideas – an idea which I was hoping would be right – was given the benefit of the doubt until almost the final moment; but here again, when I examined the evidence

[29] I apply this to myself. It is my excuse for writing nothing more on the subject of pseudo-Lanfranc (Flight 1997, pp. 187–90) that I am waiting for the critical edition of Bernard of Cluny, still said to be forthcoming one day.

[30] But he accepted Darlington's mistaken conclusion as to the sequencing of the different versions of the Wiltshire account (below, pp. 69–70).

[31] One has only to look at Ker's (1977) description of this manuscript to see how much needed to be done that Galbraith failed to do.

more closely, I saw that his interpretation had to be rejected. Though I still wish that it had been right, I am sure now that it was not.

The basic problem had still not been (because it could not be) resolved. By stressing the decentralized nature of the compilation process, he was undermining the assumption that the process was largely the same for every county (or group of counties). Even in 1942, he had been aware that his argument was taking a turn which weakened that initial assumption; by now he had largely abandoned it. It is, we are told, 'unlikely' that any two commissions 'proceeded entirely alike' (1961, p. 35); to dare to assume that they did would be 'unscholarly' of us (p. 59). Instead we are invited to suppose that each group of 'non-curial' scribes was left to work out its own procedure, without guidance from the centre. One group of scribes submitted a fair copy of the survey text in a feodalized but unabridged form; but that does not mean that fair copies were submitted by every group of scribes. One group of scribes produced the collection of booklets which (mostly) survives at Exeter; but that does not mean that similar collections were being produced by other groups of scribes elsewhere. Nineteen years earlier, his intuition had told him, quite rightly, that the version of the text represented by the Exeter booklets was the vehicle used for transforming the 'original returns' into a feodally organized version of the text. On that view the existence of D implies the existence of C (or something like it), and the existence of C implies the existence of D (or at least the intention to bring it into existence). Similarly the existence of C implies the existence of B. But that insight had been lost.

By this time, in fact, he was making a positive effort to reduce the scope of the uniformity assumption (Galbraith 1961, pp. 64–6). The proven existence of a cadastrally organized version of the text for Cambridgeshire was no longer allowed to imply that a similar text existed for every county. Galbraith made the point that a feodalized version of the text (C-like in overall shape) could have been compiled during the survey itself: if the scribe who was servicing the meeting had a stack of booklets in front of him, one for each baron, he could write the facts recorded for each manor into the appropriate booklet.[32] Would that not be an improvement on the procedure which – as admittedly Round had proved – was followed in Cambridgeshire? Instead of the whole text having to be copied out twice (first in a B-like and then in a C-like form), it is only copied out once (in a C-like form straight away). This argument is just a quibble: it would have been perfectly possible for things to be done in this way, but there is not the slightest reason for thinking that they were. (As for the suggestion that this procedure would be more efficient, that involves the assumption that the text was being dictated to the scribe, while the meeting was in progress; and that is highly inefficient.) As Galbraith got older, his antipathy for Round became increasingly overt,[33] and this thought-experiment seems to originate in that. He could not deny that Round was right about Cambridgeshire; but he could deny that Round was right to generalize. To do that, however, he had to jettison the uniformity assumption.[34]

Preoccupied with the difficulties which he had created for himself, Galbraith failed to take full advantage of the new evidence which became available in 1952–3, when D and DB were rebound. That evidence was reported in a pamphlet published by the PRO (Jenkinson 1954). Galbraith's annoyance with this pamphlet is only thinly disguised: he thought (with some reason) that he had not been given the prominence that he deserved.[35] But there was information here which he could make use of, most of which would fit quite comfortably with his interpretation.

With hindsight one can see which point was of greatest consequence. Citing the opinion of Alfred Fairbank, Jenkinson made the provocative suggestion – absolutely new, it seems – that DB might have been written by just one man. Most people seem to have ignored the suggestion; Galbraith was willing to consider it. He did not change his mind at once. It is one of the symptoms which go to show that his book had a long gestation that in some chapters he speaks of 'the scribes', in others of 'the scribe'. Apparently through nothing more strenuous than introspection,[36] he became increasingly convinced that Fairbank was right; and from there he went on to wonder (vainly) whether it might be possible to put a name to this man.[37] It never occurred to him, as far as

[32] Within each booklet, the order of the entries would reflect the order in which the hundred juries had made their appearance; so this order would be consistent from booklet to booklet, and eventually from chapter to chapter of D or DB. Hence proof of consistency in D or DB was not acceptable as proof of the existence of B, and one young historian – who had thought that he was doing something helpful by finding as much consistency as possible (Sawyer 1955) – was informed that he had wasted his time.

[33] Without having ever met him (1974, p. 9), Galbraith 'had imbibed some morally well justified hostility to Round in the PRO' (Southern 1978, p. 416).

[34] The assumption is allowed to apply within a group of counties, to the extent that 'the inherent differences between counties' are overridden by 'the tendency to uniformity imposed by the legates' (Galbraith 1961, p. 167). In other words, it applies at best only within whatever limits are set by whatever version of Eyton's conjecture one chooses to believe.

[35] He was made to share a paragraph with Douglas, because 'both have reviewed in some detail the conclusions of Maitland and others' (Jenkinson 1954, p. 16). That is cruel; Galbraith was entitled to feel cross. For Jenkinson, as this comment shows, Maitland was still the standard authority. For Galbraith, Maitland was always a minor figure, just one more historian who had succumbed to 'the dominating force of Round's personality' (1948, p. 99): adopting Round's views, he carried them 'to such extreme lengths' that 'even Round was rather embarrassed by the zeal of his distinguished disciple' – or so 'it may be surmised' (1961, p. 15). This Maitland, the author of a book called *Domesday and Beyond*, published in 1907 (Galbraith 1948, p. 90), is an imaginary character, and his book will not be found in any catalogue. (Jenkinson's Maitland is, of course, the real F. W. Maitland, the author of *Domesday Book and beyond*, published in 1897.)

[36] 'The more one broods on the script . . . ' (Galbraith 1961, p. 202). He did not have the courtesy, here or anywhere, to mention Fairbank by name.

[37] The candidate proposed by Galbraith (1967) has attracted no support, as far as I am aware. An alternative suggestion by Chaplais (1987) has been more favourably received, but does not seem convincing to me.

I can see, that instead of trying to advance he ought to be retreating. Fairbank's suggestion, if it is right, annihilates the distinction which Galbraith had made between 'curial' and 'non-curial' scribes. If DB was written by one scribe, the style of it has to be assumed to be an individual style: it cannot be taken to represent a collective 'curial' style.[38] Conversely, a scribe cannot be said to be a 'non-curial' scribe just because he writes in a different manner from the DB scribe. (At this point, if he had reached it, Galbraith might perhaps have remembered what he had said about the geld accounts. That opening too was lost.) Yet this distinction was fundamental to Galbraith's interpretation. If one cannot draw a line between 'curial' and 'non-curial' scribes, the interpretation has lost one of its principal supports. Sooner or later it was bound to fall flat – but not until people realized the implications of Fairbank's suggestion, and that was slow to happen.

Because in the end he had rather little to say about the principal manuscripts, Galbraith's book is padded out with some feeble discussion of various derivative texts – the same texts which had seemed so promising to Douglas, twenty-five years before. For some historians at least, Galbraith's book seemed to imply that this was the way ahead: Galbraith himself had already said as much as needed to be said about the principal manuscripts, and the next step forward would be achieved by closer study of these derivative texts. There was briefly a time, during the 1970s, when this tendency appeared to be gaining ground. A few papers were published which were thought, not just by their authors, to be on the point of inaugurating a post-Galbraithian, neo-Douglasian era. But the impetus soon died away.

Galbraith had not intended to start such a trend; when it started, he disapproved of it. It seemed to him a regrettable fact that these derivative texts 'have of recent years attracted more attention than Domesday itself' (1974, p. 76). The same miscalculation was involved which had carried Douglas off course in the 1930s – a failure to appreciate the difference in value 'between the evidence of strictly contemporary manuscripts and of [even] slightly later copies'.[39] No one can doubt that he was right about that. Though much of the documentation resulting from the survey has been lost, the amazing fact is that a good-sized fraction survives in the original – nothing of B, but roughly one-eighth of C, roughly one-sixth of D, and at least five-sixths (perhaps the whole) of DB. To make any genuine progress in understanding the survey, we have to grasp this fact and take full advantage of it. That was the message which Galbraith had been trying to get across in the 1940s, and he never forgot it himself. 'These three' – 'three *absolutely* contemporary manuscripts' (C, D, DB) – 'taken in conjunction, and used properly, cannot mislead us' (1970, p. 15).[40] In the 1980s, around the time of the manuscripts' nine hundredth anniversary, historians heard it again. To that extent at least, we are all convinced Galbraithians by now.[41]

2

Over the last four hundred years, the survey of 1086 has generated a volume of literature vastly in excess of this original documentation. I have not tried to read it all; I should doubt the sanity of anyone who did. Much of it was ephemeral, forgotten and deservedly forgotten almost as soon as it appeared.[42] Much of it, good in its day, was, in the normal course of events, subsumed and superseded until it became of merely historical interest.

There is, in any case, only a small proportion of this literature which touches on the fundamental issues, as I understand them to be: the logistics of the fieldwork phase and the mechanics of the compilation phase. Until these issues have been adequately grasped, there is little point, so it seems to me, in discussing anything else. Within this narrower field, I have tried to read everything relevant; but even here, no doubt, I have fallen short. Perhaps I should say that I have read much more than is listed in the bibliography. It is not my practice to cite any publication solely for the purpose of proving that I am aware of its existence. If a book or article seems to me to have some positive value, I have made a point of citing it; if not, I have preferred to ignore it. Over the last few years, I have spent a share of my time rereading publications that I had read before, to make sure that I was understanding them correctly and giving credit where credit was due. Despite my efforts, it is possible that I may sometimes have failed to do this, through ignorance or inadvertence. If so, I can only say that I regret it and am ready to do what I can to set the record straight.

Meanwhile, for as long as I was pursuing my own train of thought, I deliberately refrained from reading anything new. If a book or article was published before 2000, I allowed myself (or, in some cases, compelled myself) to read it; if after that, I did not. When I first imposed this embargo on myself, I hardly realized how long it would have to stay in place; but I still think that it was a sensible decision.

[38] In some passages, Galbraith can be seen backing away from the assertion that DB is a specimen of contemporary 'curial' script. He says, for instance, that DB 'is written in a single distinctive set-hand … which is not found elsewhere in our surviving materials. One is tempted to see in this script the copy-book hand taught to the scribes of the royal *curia*, but the hypothesis cannot be verified for lack of comparable evidence of so early a date (Galbraith 1961, p. 4). (This sounds to me as if he had been consulting with Chaplais.) The ellipsis represents a parenthetical remark – 'possibly even by a single scribe' – which administers the kiss of death.

[39] This is one of the neo-Douglasians recalling her attempt to elicit some response from Galbraith (Harvey 1980, p. 125). The distinction that he was making seemed 'puritanical' to her.

[40] This echoes an earlier statement to the same effect (1948, p. 100).

[41] Or so I thought – until I read Roffe's (2000) book.

[42] Sifting through the dust, one may hope to find a few items which were undeservedly forgotten and ought to be reclaimed. As far as Kent is concerned, I can think of only one item which falls squarely into this category: an unfinished edition of DB-Ke (Larking 1869) which subsequent commentators have conspired to ignore.

As my thinking began to take an original turn, it seemed increasingly safe to assume that nobody else was on the same track as me. That assumption, as far as I can judge, was sound enough.

The world, however, did not stop turning in the year 2000, and I have recently been trying to catch up with it. Several important developments have taken place within the last few years, and it may be helpful for the reader if I comment on them briefly here.

A pair of books had been sitting on my shelf for a considerable length of time before I was ready to read them; but I have finally got round to doing so. The collection of essays edited by Hallam and Bates (2001) includes a number of useful pieces,[43] but only one which overlaps with the contents of my own book to any large extent. A long chapter by Frank Thorn and Caroline Thorn (2001) reports on the work that they had been doing, over the previous ten years or so, in collaboration with Michael Gullick. Caroline Thorn's familiarity with the manuscripts is vastly greater than mine, and I will only say that there is very little here with which I feel at all inclined to disagree.[44] Their seriation of the DB booklets (Thorn and Thorn 2001, p. 43) is (or was at the time) incomplete and tentative; but I am pleased to see that it is at least largely compatible with mine. I hope that they will work it out in full, in their forthcoming book, and will realize, as they do so, that Eyton's conjecture is becoming vacuous.[45] I also hope that they will stop stressing the suggestion – originally Baring's (above, p. 4) – that DB derives immediately from C. In the nature of the case, this proposition is not demonstrably true; to my way of thinking it is presumptively false. The evidence that they adduce (pp. 67–8) is not in the least 'compelling'; on the contrary, it is all ambiguous.[46]

Two of the pieces in Hallam and Bates (2001) are in the nature of book reviews. One (Holt 2001) is a review of the book in which it appears; the other is David Roffe's (2001) review of his own new book (Roffe 2000). Though it has attracted some favourable comment, not just from Roffe himself, I have to say that the book left me bemused. Roffe's sense of the priorities seems thoroughly wrong-headed to me. His valuation of the evidence differs from mine to an extent which I would not have imagined to be possible. To note only the most striking contrast, his remarks about the Exeter manuscript are brief and unoriginal (Roffe 2000, pp. 94–8);[47] they take up slightly less space than his remarks about the 'Crowland Domesday' (pp. 101–5). I was three-quarters of the way through the book before I found a passage which seemed on target to me (below, p. 104); and only one section of it is, to my mind, unquestionably an advance in the right direction. The idea that it might be possible to seriate the DB booklets had been in the air for some time, but Roffe was the person who knuckled down to the job and tried to get the seriation worked out (pp. 191–211). His analysis is not completely successful, but he is entitled to the credit for making the first attempt.[48] Apart from that, it is hard to see what common ground exists. On one point, however, I venture to think that Roffe will agree with me – that compromise is not the way forward. On matters of detail, no doubt, there is room for some give and take; beyond a certain point, it is no more possible than it is desirable to think of splitting the difference. One of us is on the right track, and one of us is not.

By my own reckoning, this book of mine will only be successful if it encourages some readers to look or look again at the primary sources. Though the C text is still hard to get at,[49] both D and DB have recently become much more accessible, but only in a qualified sense – more accessible for people who have deep pockets (or a first-rate library just around the corner), less so for the rest of us.

The translations of the DB text which appeared originally in the Phillimore edition (1975-86) have been consolidated and released on CD-ROM (Palmer, Palmer and Slater 2000). How far this is a welcome development I have to confess to feeling doubtful. I do not mean to belittle the Phillimore edition, which – above all because the individual volumes are cheap – has served a valuable purpose, and will continue to serve it for some years to come. Thanks to John Morris, the man who initiated this edition (he died in 1977), thanks also to those who continued what he had begun, D and DB were, almost for the first time,[50] made

[43] The chapter by Prescott (2001, pp. 180–5) includes some interesting information, new to me, about the Record Commission's 'Additamenta' volume (Ellis 1816). I have made some consequential corrections and additions in chapters 4 and 8.

[44] To mention just one point on which I feel competent to speak, I agree with their explanation of the marginal addition in DB-Ke-9rb (Thorn and Thorn 2001, p. 59, ill. 28). (But I would add that neither of the places in question has been satisfactorily identified.)

[45] It is disconcerting to see that they express their faith in this conjecture (Thorn and Thorn 2001, p. 42) in language which seems to be a deliberate echo of Galbraith's (1942, p. 162). For Galbraith it was 'generally agreed'; for them it is 'generally accepted'. After almost sixty years, does the conjecture still have no stronger claim on us than that? (By that sort of reasoning, the earth would still be flat and whales would still be fish.)

[46] As was indicated by Holt (2001, p. 23), the issue is a matter of logic, not of evidence. Because Baring's theory is (within the limits that they suppose to apply) not demonstrably inadequate, they say that it ought to be preferred to Galbraith's because it is simpler – more economical – than his. Of course it is simpler in a trivial sense; but is it simpler in an interesting sense, all things considered?

[47] And, if I may say so, not as well informed as they might be: there is no reference to Ker (1977) or Webber (1989).

[48] The added footnotes in chapter 2 will indicate how far my own results were anticipated by Roffe. I ought not to have overlooked an earlier publication of his which came close to establishing the sequence for the first five counties (Roffe 1990, pp. 320–1 = 2000, pp. 202–3).

[49] Caroline Thorn allows me to say that she is working on a new edition of the Exeter manuscript.

[50] The crude facsimile issued by the Ordnance Survey in the 1860s was not kept in print, and copies seem to have dropped out of circulation fairly soon. Whether it was worth resurrecting this facsimile and scanning it onto the Phillimore CD, I am not in a position to decide.

readily available to anyone who wanted to consult them.[51] Without the Phillimore edition, the task which I set myself would have been impossible; and I think it only fair to say so. Nevertheless, the edition has rather more than its share of faults. The translations are eccentric, and often somewhat inaccurate; the editorial contribution varies greatly, in quantity and quality, between one volume and another. Every page of my copy of the Kent volume (Morgan 1983) is thick with pencilled corrections and annotations. It is no longer the original edition: it is the record of a one-sided conversation between the editor and me, protracted over twenty years. For my part, I cannot imagine having any use for a copy of this translation in an uncorrected and (more to the point) uncorrectable form. The Phillimore edition, I fear, was not ready to be cast in bronze.

Back in 1985, while D and DB were being repaired and rebound, both manuscripts were photographed in their entirety, with a view to the publication of a new facsimile.[52] The photographer was Miki Slingsby; he was assisted, he tells me (and asks me to state for the record), by Kerstin Firth-Clarke. The facsimile edition of DB was first published in 1986 (Alecto Historical Editions 1986); a separate edition for each individual county was issued over the next few years (Alecto Historical Editions 1987–92).[53] A facsimile of the three D booklets came out at last in 2000 (Alecto Historical Editions 2000). I am not sure that this facsimile of D will become as widely accessible as that of DB; to judge from a few online catalogues which I have checked, not every library which purchased the DB edition has felt obliged to purchase this one too. (For that reason I have not yet had a chance to see it myself.)

The English translations originally made to accompany the Alecto facsimiles of DB and D have been extracted and collected together to make a separate book (Williams and Martin 2002), already out in paperback (2003). I do not know who is expected to buy this book and not lose interest after the first few pages. It would be a pity, in any case, if the availability of a translation diverted attention from the Latin text.[54] It would also be a pity if this book conveyed the impression that everything has been settled, and that nothing is left to be done. For some counties this may (for all I know) be true or nearly true; but it is far from being true for Kent, where many places have been identified wrongly, or not as rightly as they might be.[55]

The same photographs taken in 1985 were used for creating a digitized facsimile of both manuscripts, released in January 2003 (Alecto Historical Editions 2003). Again, I am not sure how widely this CD-ROM edition will become available. It does not (as yet) appear in any of the catalogues that I have checked. (For that reason, again, I have not yet seen it myself.) I should not be surprised if it had to overcome some degree of consumer resistance. If I were a librarian who had spent a large sum of money, not many years ago, to buy what was advertised then as the best-possible facsimile of DB, I might hesitate before spending another large sum of money to buy (almost) the same thing again.[56] In one respect, this new edition is less good: it cannot replicate the physical properties of the original manuscript. In the quality of the reproduction, however, it is (I have reason to expect) an astonishing improvement.[57] The transparencies made in 1985, reproduced by a method which does them justice, are superb; and the photographer ought to be thanked by name by anyone who uses this facsimile. The entire edition is beyond the budget of most private individuals; but the intention is for each county text to be issued separately, and these 'county editions' come at a more affordable price.[58]

These various attempts to make money out of DB – or, to

[51] Not in their original form, but in the form which was given to them by an eighteenth-century compositor. His name, as far as I can see, went unrecorded. We know who made the copy and checked the proofs. We know who designed the type; we know who manufactured it; but we do not know who did the work of setting it.

[52] The history of the Alecto facsimile is recounted by Pearson (2001). Almost twenty years on, I do not see why it should not be said that the DB facsimile was poorly reproduced, and that the apparatus could mostly have been done without. All that was wanted was a good facsimile, as cheap as it could be.

[53] A collection of essays which accompanied the DB facsimile (Williams and Erskine 1987) has recently been repackaged (Erskine and Williams 2003). The format is different, the title is new, but the contents appear to be (except for the acknowledgments) identical.

[54] With a little practice, the script of DB is quite easy to read, and the language is, most of the time, not difficult to understand. (This is business Latin, not the poems of Catullus.)

[55] In the Alecto edition of DB-Ke, Williams and Martin (1992, p. 67) give a list of the place-names which they identify differently from Morgan (1983). It seems an impressively short list. The fact is, however, that they are simply repeating almost all of Morgan's identifications, irrespective of whether they are right or wrong or something in between (and some of the changes which they have made are changes for the worse). I cite a few examples to illustrate the sort of inadequacies which occur. (i) *Stoches* (8va19) is Stoke, but that is not specific enough: the particular manor in question here is the one which came to be called Malmaynes (TQ 8175). (ii) *Berham* (9vb35) is Barham, but that name – as Ward (1933) pointed out – was being used in a very loose sense: the particular place in question here is Kingston (TR 1951). (iii) *Middeltune* (2va46) is Milton (TQ 9065), but that was a huge manor: a score of places which seem to be missing from DB are missing because they are silently included in this paragraph. (iv) Finally one example of an outright error: *Boltone* (4rb37) is not Boughton Malherbe, correctly identified elsewhere (8rb1); it is Boughton Monchelsea (TQ 7749).

[56] The CD-ROM edition includes the introductions commissioned for each volume of the 'county edition' (1987–92), which were previously hard to get hold of. (Few libraries which bought the 'library edition' saw any need to buy the 'county edition' as well.) When I put in a request through the inter-library loan system for a copy of the 'county edition' of DB-Ke, the copy which eventually arrived in Clemson came all the way from Boston Spa.

[57] Some years ago, I wrote to the people at Alecto to ask whether something had gone wrong with the facsimile of DB-Ke-10r, where one patch of text is illegible (10ra15–18). They were kind enough to send me a full-size reproduction of the original transparency. As soon as I recovered from my surprise, I wrote to ask whether they could let me have similar reproductions of all the other pages in DB-Ke. When they told me how much they would have to charge per page, I dropped the idea.

[58] The last time that I checked (November 2005), four of these county editions were available: D-Ex, DB-Yo (part of booklet DB-YoLi), DB-Dn (part of booklet DB-DnCo), DB-Wa. The Web site to watch is www.phillimore.co.uk.

put it bluntly, out of the ignorance of people who do not realize that DB means dreadfully boring – are not much to my taste;[59] but they are, I am sure, only a passing phase. It will not be long (if it has not happened already) before somebody starts developing an online edition of DB and D, freely available to everyone. The basis for it will be, I hope, not an English translation but the tightest possible transcription of the Latin text – by clicking on which it will be possible to summon up a translation into English (or some other metalanguage), if that is what one wants, or an extended version of the Latin, or an explanatory note. (I also hope that the DB booklets will be put into the right order.) To say nothing of my incompetence, I do not have the time to undertake this task myself; but I should be happy to contribute, so far as I am capable, so far as Kent is concerned.

[59] 'For anyone tracing the history of his or her family, homeland or village [this edition] is invaluable.' Though no one should expect a publisher's blurb to tell the plain truth, is this not a touch dishonest?