Chapter 2
Seriation of the DB booklets

From the enterprise known to contemporaries (if they could read Latin) as the *Descriptio totius Angliae*, 'the survey of the whole of England', three large batches of documentation survive in the original. As Galbraith (1942) was the first to realize, they represent successive stages in the process of compilation. Here I am concerned with just one of these batches, typologically the latest of the three – a collection of two dozen booklets, written (so it seems safe to say) in 1086–7, containing abbreviated versions of the survey reports for thirty counties. At some uncertain date, these booklets were bound together. The book thus brought into existence (rebound from time to time) has never been out of official custody. It is now in the Public Record Office, with call-number E 31/2. I propose to refer to it here as the *Descriptio brevis*, DB for short.[1]

DB comprises 379 leaves, measuring about 370 mm long by 260 mm wide, plus four inserted slips. All the way through, the text is arranged in two columns, but the number of written lines per column varies greatly, from fewer than forty to more than sixty. The leaves were numbered in January 1660 by one of the Exchequer officials, Edward Fauconberge:[2] his numbering included the inserted slips, but did not include the first leaf, originally blank.[3] Modern commentators, not wanting to disturb Fauconberge's foliation, which is otherwise perfectly accurate, have chosen to refer to the first leaf as folio 0.

As long as DB remains in its binding, the only obvious division is the division into counties.[4] Every new county begins a new page; most counties are separated from the preceding county by one or more blank pages – usually only one or two, twice as many as seven. Removed from its binding, DB reveals more structure. Apart from the inserted slips already mentioned, it consists of 360 paired leaves (i.e. 180 folded sheets) and 19 single leaves, organized into 47 gatherings. Not counting the singletons, most of the gatherings (29 of them) are regular quires comprising eight paired leaves; but gatherings of six leaves (10), ten leaves (6) and four leaves (2) also occur. By and large, the division into gatherings correlates closely with the division into counties. Almost every county begins at the beginning of a gathering; almost every county ends near the end of a gathering, without much space being wasted. It is clear, up to a point, that this result was brought about deliberately, by adjusting the size of the gatherings to the expected quantity of text.

These facts became knowable whenever the book was rebound, but nobody was obliged to take any notice of them, except for the binder himself. They could have been recorded in the 1860s; in fact the opportunity went untaken until the 1950s, when DB was repaired and rebound once more, at the instigation of the Deputy Keeper, Sir Hilary Jenkinson.[5] A diagram published in 1954, showing exactly how DB is constructed (Jenkinson 1954, app. I), can fairly be said to have put discussion of the book on a solid basis for the first time. The credit for producing this diagram belongs to A. W. Mabbs.[6]

Given the facts recorded in Mabbs's diagram, DB can be seen to divide itself objectively into 27 booklets (Table 1, cf. Rumble 1985, pp. 34–5). There are 16 booklets which comprise a single county contained in a single gathering; six booklets which comprise a single county but extend into more than one gathering; and four booklets which comprise two counties, the second county beginning in the same gathering which contains the end of the first. The largest booklet is the one for Yorkshire and Lincolnshire, which runs to nine gatherings. Finally there is a single gathering contain-

[1] It is sometimes called 'Domesday Book' ('Domesday Book volume 1', to be more precise). That name is a twelfth-century joke. The name was never very apt, and the joke was never very funny; I suggest we might cease repeating it.

[2] Edward Fauconberge (d. 1679) was chamberlain of the Receipt 1655–60, deputy chamberlain 1660–79 (Sainty 1983, pp. 19, 176).

[3] This is where Fauconberge wrote a note recording what he had done and giving the date as 3 January 1659 (Jenkinson 1954, p. 20; Hallam 1987, p. 146). There is, I take it, no doubt but that he was starting the year in March.

[4] Some parts of the country fell outside the basic scheme. Rutland (293va–4ra) is appended to and indexed with Nottinghamshire (280vb); the New Forest (51ra–vb + 50vb) and the Isle of Wight (52rb–4ra) are appended to and indexed with Hampshire (37vb); six hundreds north of the Mersey (269va–70rb) are appended to and indexed with Cheshire (262vb). (In this last case there is no proper index, just a note explaining why in Cheshire no index is needed.)

[5] The facts discovered in 1952–3 were put on record in a pamphlet published by the Public Record Office in April 1954. No author's name appears on the title-page, but the text is obviously an individual (not to say egotistical) production, and I cite the pamphlet as Jenkinson (1954). As the preface acknowledges, however, all the preparatory work had been done by Jenkinson's staff. (The copy which I have used is a second impression, 'with corrections', dated 1960.)

[6] The diagram showing the construction of DB (there is also a similar diagram for E 31/1, rebound at the same time) is credited to Mabbs in the preface (Jenkinson 1954, p. viii). This is Alfred Walter Mabbs, who joined the PRO as an assistant keeper in 1950 and retired as Keeper in 1982. His diagram is reproduced, with some alteration in the labelling, as Williams and Erskine 1987, app. III. In both versions, it has one defect: it fails to distinguish between singletons and slips.

Fauconberge's foliation	number of gatherings	paired leaves	single leaves	inserted slips		counties
[0]–15	2	14	2		Ke	Kent
16–29	2	12	2		Sx	Sussex
30–36	1	6		1	Sy	Surrey
37–55	3	18		1	Ha	Hampshire
56–63	1	8			Be	Berkshire
64–74	1	10	1		Wi	Wiltshire
75–85	1	8	1	2	Do	Dorset
86–99	2	14			So	Somerset
100–125	3	24	2		DnCo	Devon–Cornwall
126–131	1	6			Mx	Middlesex
132–142	1	8	3		Ht	Hertfordshire
143–153	1	10	1		Bu	Buckinghamshire
154–161	1	8			Ox	Oxfordshire
162–178	2	16	1		GlWo	Gloucestershire–Worcestershire
179–188	1	10			He	Herefordshire
189–202	2	14			Ca	Cambridgeshire
203–208	1	6			Hu	Huntingdonshire
209–218	1	8	2		Bd	Bedfordshire
219–229	1	10	1		Nn	Northamptonshire
230–237	1	8			Le	Leicestershire
238–245	1	8			Wa	Warwickshire
246–251	1	6			St	Staffordshire
252–271	3	20			ShCh	Shropshire–Cheshire
272–279	1	8			Dy	Derbyshire
280–296	2	16	1		Nm	Nottinghamshire
297–372	9	74	2		YoLi	Yorkshire–Lincolnshire
373–382	1	10				clamores etc.
	47	360	19	4		

Table 1. Division of DB into booklets.

ing a collection of supplementary material relating to these two counties. This gathering is a puzzle by itself, and I do not discuss it further here.[7]

The pamphlet which included Mabbs's diagram was momentous in another sense too. At Jenkinson's invitation, the disbound leaves were examined closely by A. J. Fairbank – a civil servant in the Admiralty, but also, more to the point, a distinguished calligrapher and teacher of calligraphy.[8] Having taught himself to imitate the script of DB, Fairbank participated in timed experiments designed to estimate how many man-days would be required to write the whole book. (The answer was: in the order of 240 man-days.) He also decided, from his study of the script, that DB was the work of a single scribe throughout.

That was, at the time, a quite astonishing conclusion. Till then, it seems to have been taken for granted (as far as anybody had ever thought to ask) that DB was the work of a team of scribes, all trained to write the same type of script. The fact that nobody had ever managed to distinguish the contributions of individual scribes was not allowed to undermine that assumption: on the contrary, it tended to reinforce the notion that DB was produced by a strictly disciplined scriptorium. The credit for realizing that the question needed to be asked – was DB the work of one scribe or many? – seems to belong to Jenkinson; the credit for getting the right answer belongs to Fairbank. There were, it seemed to him, 'certain weaknesses' in the script – individual foibles occurring throughout the book – which indicated 'that a single hand was responsible for the whole' (Jenkinson 1954, p. 34).

Unlike Mabbs's diagram, Fairbank's suggestion did not command instant assent. It was a statement of opinion, not of fact; and Jenkinson made a point of assuring his readers that they were free to disbelieve it, if they chose.[9] His expectation was that most readers would do just that –

[7] ((The ruling is similar to that used for aspect 3, but not exactly the same. To judge from its spelling of French words (below, p. 141), this quire belongs with aspect 1 or the earlier part of aspect 2.))

[8] This is Alfred John Fairbank (1895–1982). A festschrift presented to him on his seventieth birthday (Osley 1965) includes a biographical chapter; there is an obituary in *The Times*, 20 March 1982, p. 8.

[9] Fairbank's opinion drew its weight from his experience (not mentioned by Jenkinson) as a teacher of calligraphy, accustomed to scrutinizing every detail of his pupils' work. But perhaps it would not be unjust to say that there were a few palaeographers active at the time whose opinion, had they been asked for it, might have carried greater conviction than Fairbank's.

would prefer to continue thinking that the work had been 'distributed', shared out among several indistinguishable scribes. After 1954, though Fairbank's suggestion was not forgotten, it is hard to detect any definite progress until the 1980s, when a spate of important publications (Rumble 1985, 1987, Gullick 1987, Chaplais 1987) carried the discussion very much further forwards.[10] Whether to rely on Fairbank's opinion or not is no longer a pertinent question; but for thirty years it was.

For historians, and for anybody else who wants to make use of this evidence, more is at issue than merely deciding whether to say 'scribe' or 'scribes'. Interpretation may vary, perhaps to a large extent, depending on whether DB was a collaborative effort or the work of just one man. It is important to know how far we can feel sure of our ground.

Suppose it is true that DB was written, entirely or almost entirely, by one scribe. Then it follows (subject to certain conditions) that we ought to be able to arrange the constituent booklets into a single sequence corresponding with the order in which they were written. Conversely, if we can arrange the booklets into such a sequence, that will tend to confirm the single-scribe theory. To put the question in archaeological terms, is it possible to seriate the booklets?

Nobody would have thought of asking that question before 1954. It arose for the first time when Fairbank's suggestion was juxtaposed with Mabbs's diagram. Once the question is asked, however, the answer is clear enough. As this chapter aims to show, it is possible – mostly quite easy – to sort the booklets into a single sequence; and the single-scribe theory is, to some degree, corroborated independently by that fact. Furthermore, the sequence turns out to be very different from the sequence in which the booklets were eventually bound; and that may be, in the long run, the more important result.

1

Seriation would not be possible unless the scribe had varied his procedure, to some significant extent, as he went along. If he had settled on a plan in advance and stuck with it from start to finish, we would have no hope of seriating the booklets. On the whole, the scribe had a definite idea what format he wanted to use and what facts he wanted to include, and he followed this format and reported these facts consistently throughout. But that did not prevent him from altering his plan, in many small and some quite large respects, as the work progressed. Examples are quoted below. For purposes of seriation, the most useful changes are those which occur gradually, over the space of several columns, in the middle of a county. Before our eyes, one formula fades away. A new formula appears, alternating with the old one at first and then replacing it. (An example of this is shown in Fig. 1.) If a change of this kind can be found, it is safe to assume – as safe as we can hope for it to be – that the change occurred during the writing of this particular manuscript. It makes no difference where the new formula came from, nor whether the scribe made the change consciously or not. All that matters is the solid fact that the change did occur. Any booklet which uses the old formula is sure to be earlier than this one; any booklet which uses the new formula is sure to be later than this one.

There are 26 booklets to be dealt with. A preliminary classification – which is also a partial seriation – is given in Table 2. This classification is based, first, on variations in the ruling – the grid of vertical and horizontal guidelines scored onto each sheet before writing began.[11] These variations seem to have gone unnoticed until the 1950s: once again it was Jenkinson's pamphlet which first drew attention to them. While the sheets were disbound, the number of rulings on each was counted and recorded;[12] and the facts discovered were published – not in full detail, leaf by leaf, but in summary, gathering by gathering (Jenkinson 1954, table I).[13] I reproduce these data here, simplifying them even further.[14]

Variation in the ruling is not enough by itself to form the basis for an adequate classification. We need more evidence; and for this we have to turn to the text itself. As even a cursory inspection will prove, there is wide variation, between one group of booklets and another, in the organization and wording of a typical entry. Thus the booklets which I classify as aspect 1 resemble one other and disresemble all the rest in some rather obvious features: here and only here, for instance, the first personal name to appear in each entry is the name of the man who held this manor *T.R.E.* (mean-

[10] Around the same time, the Phillimore edition of DB was brought to completion, and the Alecto facsimile was published. DB itself, disbound in order to be photographed, was rebound once again, as two volumes, in 1985–6.

[11] In several booklets, however, the scribe disobeyed the horizontal ruling, more or less consistently. Where this happens, the result is always that the written lines are closer together than the ruled lines.

[12] As far as I can gather, all or most of this work was done by another of the assistant keepers, D. H. Gifford, whose contribution is acknowledged in the preface (Jenkinson 1954, p. viii), but only in general terms. This is Daphne Heloise Gifford (d. 1990).

[13] All this evidence was looked at again by Gullick (1987, pp. 94–7). At some points, the results which he reports are at variance with those reported previously by Jenkinson, and I cannot feel sure that Gullick's results are always closer to the truth. Here I have to speak carefully, because what is say is based only on a study of the facsimile – and the facsimile, of course, did not aim to optimize the visibility of the ruling. It is generally possible to see some trace of the lines, or of the prickings for them, but only rarely possible to see the whole pattern. In the footnotes, I have indicated some of the points which seem to be in need of further investigation. But the seriation does not depend entirely (nor even mainly) on this evidence, and the areas of uncertainty are not alarmingly large. ((Caroline Thorn, who has scrutinized the evidence more closely than ever before, has been kind enough to clarify some details for me. The reader may be sure that all doubts will be dispelled when her findings are published in full; but I have not thought it right to anticipate them here.))

[14] In Jenkinson's table, 'unruled' should be taken to mean that there are two horizontal lines, one each at the top and bottom of the frame (Gullick 1987, p. 95).

aspect	ruling		booklets
1	8	44	Hu, Dy, Nm, YoLi
2	8	44	Mx, Ht, Bu, Ca, Bd
3	7	50	Ke, Sx, Sy, Ha, Be
4	4	2	GlWo, He, Wa, St, ShCh
5	4	50–59	Wi, Do, Ox, Nn, Le
6	4	2	So, DnCo

Table 2. A preliminary classification of the DB booklets.

ing *tempore regis Eduuardi*, 'in the time of king Edward'). When the evidence supplied by the text is combined with the evidence of the ruling, the classification falls out quite easily.[15]

The seriation is worked out, link by link, in the following paragraphs. Some of the relevant transitions are illustrated by diagrams; their format will, I hope, be easy to understand.[16] The argument is, by design, as sparse as I can make it. Because it can be proved that Sx is later than Ke, it does not need to be proved that Ke is earlier than Sx. Because it can be proved that Sy is both later than Sx and earlier than Ha, it follows immediately (since Sy is the only county for which both statements hold true) that there is only one possible place in the sequence for it. The reader will have no trouble finding evidence which tends to confirm the sequence given here. At least some of the relevant evidence was known to Jenkinson (1954, pp. 30–2); but he disabled himself from making sense of it, by presuming the order of binding to be original.

Aspect 1 In aspects 1–2, the sheets are ruled with 8 vertical lines (a pair of lines on either side of either column) and 44 horizontal lines.[17] In aspect 1, by and large, the written lines conform to the ruling; in aspect 2, by and large, they do not. The distinction appears more sharply in the text – in the value clause, for instance, which has two parameters in aspect 1, three in aspect 2 (see below). The usual formula here is: *T.R.E. ual' .. sol', mº .. sol'*.

YoLi Yo is earlier than Li.[18] The formula *T'ra ad .. car'*, which evolves near the end of Yo, persists halfway through Li. Then it contracts to *T'ra .. car'* (Fig. 1), and in that simplified form persists throughout the rest of aspect 1.[19]

Nm Later than Li, earlier than Dy.

Dy The formula *Ibi n'c in d'nio .. car'* evolves early in Dy, replacing an unstable formula – *Ibi mº in d'nio .. car'* or *Ibi in d'nio .. car'* – which occurs sporadically in Nm. Evolving here, the new formula persists throughout most of Hu, the last booklet in aspect 1, with only slight variation (but consistency seems to be breaking down near the end).

Hu Later than Dy.

Aspect 2 The transition to aspect 2 is marked by (among other changes) a drastic alteration in the value clause. In aspect 2 the clause looks basically like this: *Val' .. sol', Quando recep' .. sol', T.R.E. .. sol'*. Three values are given, not two; and the T.R.E. value is given last, not first.

Mx The *Quando recep'* formula can be seen evolving in Mx: *et quando*, then *quando*, then *Quando*. Having evolved, it persists throughout the rest of aspect 2, and (with qualifications) further still.

Bd In cases where the *Quando recep'* value is the same as the current value, Bd tries three formulas which avoid repeating the numeral (Fig. 2). It starts with *Quando recep' similit'*, which is the formula found in Mx; then it shifts towards *et tntd' quando recep'*; and then it makes a more drastic shift, telescoping the first two terms to make the formula *Val' et ualuit .. sol', T.R.E. .. sol'*. (This is an echo from the formula *Val' et ualuit semper .. sol'*, employed

[15] ((A similar approach was taken by Roffe (1990, 2000), whose findings, so far as they anticipated mine, are cited in footnotes below.))

[16] Gatherings are shown by square brackets. Paired leaves are numbered, single leaves marked 'S', inserted slips omitted. Frequencies are counted column by column: the recto columns appear above the line, the verso columns below it. Each instance is represented by a unit square; for aesthetic reasons, the individual histograms are centred (as in the classic paper by Dethlefsen and Deetz (1966), to which I hope some readers will recognize my indebtedness). All the counting was done from the printed text, the errors in which are very few, and also (more importantly) unsystematic: a small number of sporadic errors, including any made by me, will not affect the pattern. For one diagram (Fig. 3), I have checked each item against the facsimile, without finding a single mistake. Anything visibly added as an afterthought – in the margins, between the lines – is disregarded; thus in this figure I ignore the *ualb'* which appears in an entry written across the foot of a page (17v) and count *T.R.E. `et post´ ualeb'* (18ra) as an instance of *T.R.E. ualeb'*.

[17] The first two YoLi gatherings are anomalous: they were ruled before the scribe had settled on a plan. The distinction made by Gullick (1987, p. 96) between 'pattern 1a' and 'pattern 1b' seems too slight to be significant.

[18] The Yorkshire text deserves very close analysis, because this is where we can see the scribe coming to terms with his task – deciding, for instance, what degree of abbreviation is appropriate. That explains why (but only at first) he shows 'a surprising taste' – as Jenkinson (1954, p. 32) put it – 'for writing *carucata* in full'. The scribe has reached 303va before he decides that *caruca* 'plough' can normally be written *car'*; he has reached 315rb before he decides that *carucata* 'ploughland' can normally be written *car' t're*, and that his readers can be trusted to tell the difference. (Here and everywhere, it has to be remembered that the scribe is thinking in French and assuming that his readers will do the same: the words he is trying to communicate are *carue* and *caruede*.) Many more such examples can be found. For present purposes, however, the only point which needs to be established is that Yo is earlier than Li; and that is obvious enough.

[19] ((The sequence for aspect 1 was worked out by Roffe (1990, pp. 320–1 = 2000, pp. 202–3), except that he did not prove that Hu is later than Dy.))

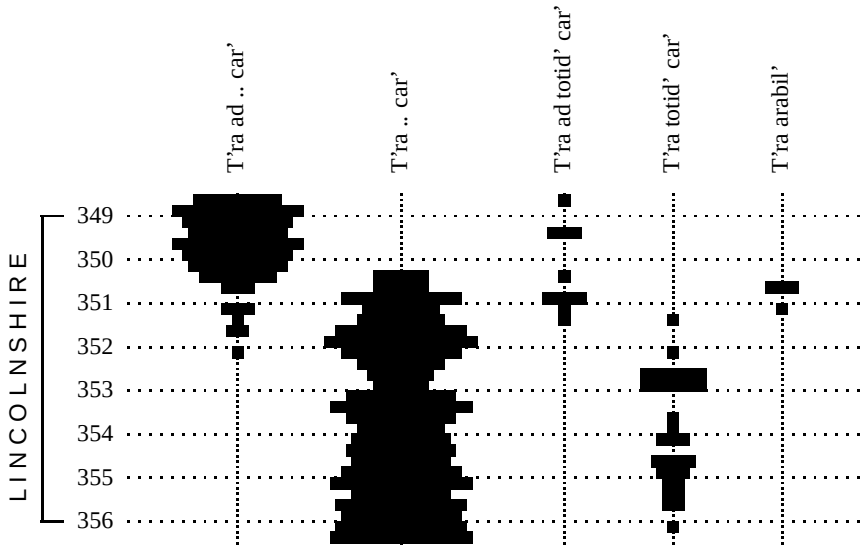

Figure 1. Part of booklet DB-YoLi. Transition from *T'ra ad .. car'* to *T'ra .. car'*. (Instances which replace the numeral with the word *totid'* are counted separately (channels 3–4); here the transition starts later but finishes sooner.)

when all three values are the same.) The telescoped formula is the one which is used, almost exclusively, in the rest of aspect 2.

Bu, Ht, Ca All later than Bd, but I have not been able to seriate them satisfactorily with respect to one another.[20]

Aspect 3 The transition is marked by a new pattern of ruling, with 7 vertical and 50 horizontal lines, and reflected also by changes in the wording. For example, the value clause is turned back to front, the T.R.E. value now being given first. In Ke the clause looks like this: *T.R.E. ualeb' .. sol', Quando recep' .. sol', Modo .. sol'*; here and later, *Quando recep'* alternates with the simpler phrase *et post*. With some variation in detail, that basic arrangement persists throughout aspect 3.[21]

Ke Earlier than Sx.[22]

Sx Early in Sx, the formula *T.R.E. ualeb' .. sol'*, normal in Ke, changes to *T.R.E. ualb' .. sol'* (Fig. 3), normal in the rest of aspect 3.

Sy Later than Sx, earlier than Ha.

Ha Late in Ha, the formula *T'c se defd' pro .. hid'* becomes unstable, the verb being frequently omitted. The oscillation continues into Be.

Be Halfway through Be, the formula *T.R.E. ualb' .. sol'* is replaced by *Valuit .. sol'*.

Aspect 4 With the transition to aspect 4, again the ruling changes: the number of vertical lines is reduced to four (that number persists throughout the rest of DB); and horizontal ruling is dispensed with.[23] The assessment clause changes to *Ibi .. hidę geld'*.[24]

GlWo This booklet and the next one are in need of detailed analysis. Gl is sure to be earlier than He, and that is the main point; but the textual evidence seems to suggest that Wo is later than He.

He Late in He, the formula *Val' et ualuit .. sol'*, normal in Gl and before, starts to alternate with *Valuit et ual' .. sol'*, normal in the rest of aspect 4.

ShCh Early in Sh, one category of information changes place. The number of slaves stops being put after the number of ploughs belonging to the peasants, and starts being put after the number of ploughs belonging to the lord. This new arrangement persists into Ch and throughout the rest of aspect 4.

St Later than ShCh, earlier than Wa.

[20] ((Having looked at these booklets again, I am now inclined to think that Ca should follow Bd (below, p. 141); but Bu and Ht continue to defeat me.))

[21] ((That Ke is the earliest aspect 3 booklet was recognized by Roffe (2000, p. 207).))

[22] The second Ke gathering (8–15) seems to have been made up from the stock of parchment left over from aspect 2. Gullick (1987, p. 96) points out that the sheets have been re-ruled to increase the number of lines. This seems sure to be right: the two systems of parallel lines are distinctly visible on the blank leaf at the end (15), and so are the two sets of prickings.

[23] The first GlWo gathering (162–9) retains the same vertical ruling as aspect 3. According to Jenkinson (1954, p. 26), the sheet at the centre (165 + 166) has only six ruled lines. This seems to be right; but the sheet was pricked for seven lines, like its companions, and there is no need to make a special case of it.

[24] ((The sequence for the first three aspect 4 booklets, GlWo He ShCh, was established by Roffe (2000, pp. 208–9).))

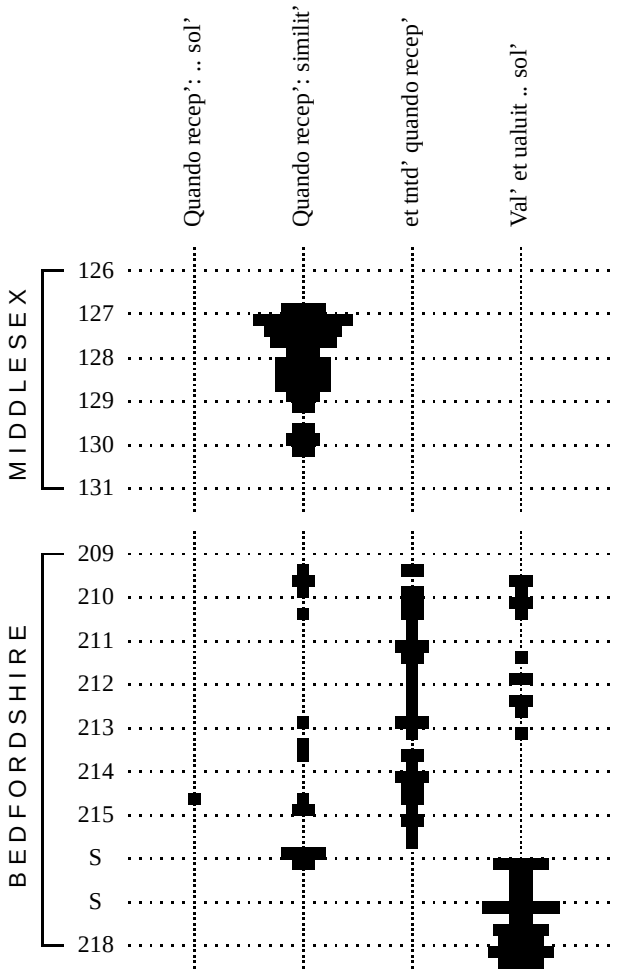

Figure 2. Booklets DB-Mx and DB-Bd. Oscillation between *Quando recep': similit'* and *et tntd' quando recep'*, followed by a transition to *Val' et ualuit .. sol'*.

Wa Early in Wa, the formula *X tenuit et liber homo fuit*, normal in St, is replaced by *X libere tenuit T.R.E.* (Fig. 4).[25]

Aspect 5 Horizontal ruling is reinstated in aspect 5.[26] The number of lines is variable, but 53 or 54 seems to be the norm.

Nn Earlier than Le.

Le Late in Le,[27] the formula *In d'nio s't .. car'*, normal in Nn and before, is replaced by *N'c in d'nio .. car'*, normal in Ox.

Ox Later than Le, earlier than Wi.

Wi Halfway through Wi, the formula *De hac t'ra s't in d'nio .. hidę*, normal in Ox, changes to *De ea s't in d'nio .. hidę* (Fig. 5). The new formula persists into Do and beyond.[28]

Do Later than Wi.

Aspect 6 Horizontal ruling disappears again.[29]

So Late in So, the formula *X tenuit T.R.E. et geldb' pro .. hid'*, normal in Wi and Do, changes to *X teneb' T.R.E. et geldb' pro .. hid'* (Fig. 6), normal in DnCo.

[25] ((That Wa follows St was recognized by Roffe (2000, p. 209–10).))

[26] This is where Gullick's results diverge the furthest from Jenkinson's. There are two areas of disagreement. (1) Three of the gatherings in aspect 5 (Le, Wi and Do) are said by Gullick to have six vertical rulings. Looking at the facsimile, I can see no proof of this. I think it possible that the scribe may sometimes have ruled an extra line where he thought he had made a column too narrow at first; but I am not convinced that he ever ruled six lines as a regular policy. (2) Two gatherings (Nn and Ox) are said by Gullick to lack horizontal ruling. It seems to me that some ruling can be discerned in both these gatherings; the last leaf in Nn (229) is quite certainly ruled on the verso, with 4 vertical and 52 horizontal lines.

[27] The sheet at the centre of this gathering (233 + 234) seems to have been left over from aspect 3 (Gullick 1987, p. 96).

[28] ((The sequence Wi Do So DnCo was worked out by Roffe (2000, pp. 207–8). It is too obvious, however, for anyone to claim much credit for discovering or rediscovering it.))

[29] In this negative respect, aspect 6 resembles aspect 4 more than it resembles aspect 5. But negative evidence does not count. According to Jenkinson's table, part of Do is also unruled; I cannot be sure, from the facsimile, whether that is right or not.

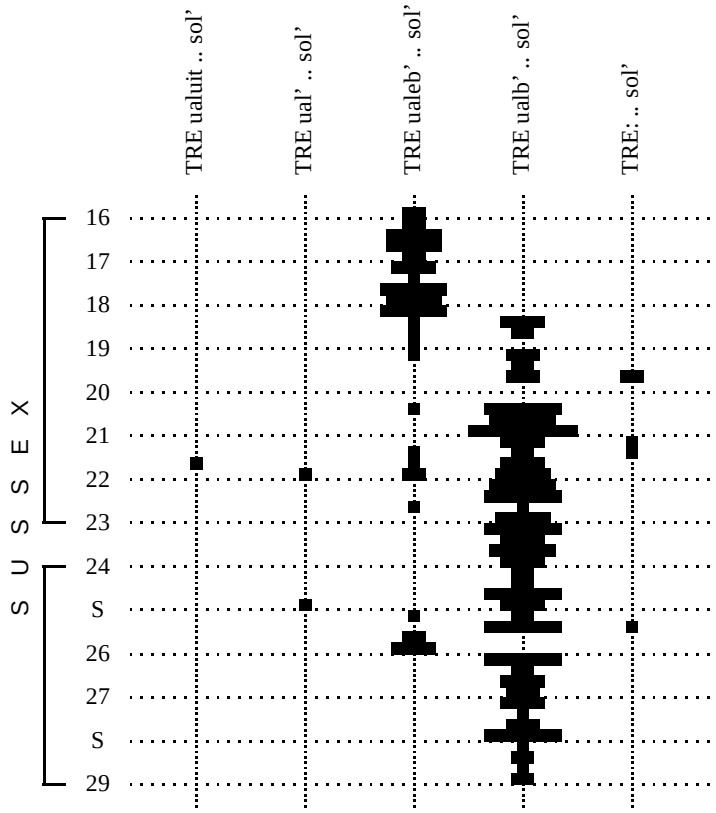

Figure 3. Booklet DB-Sx. Transition from *ualeb'* to *ualb'*.

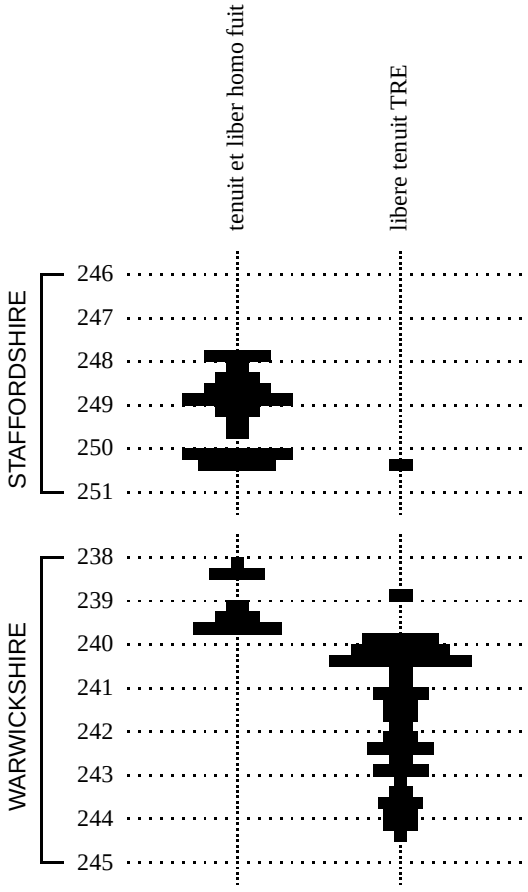

Figure 4. Booklets DB-St and DB-Wa. Disappearance of one formula coinciding with the appearance of another.

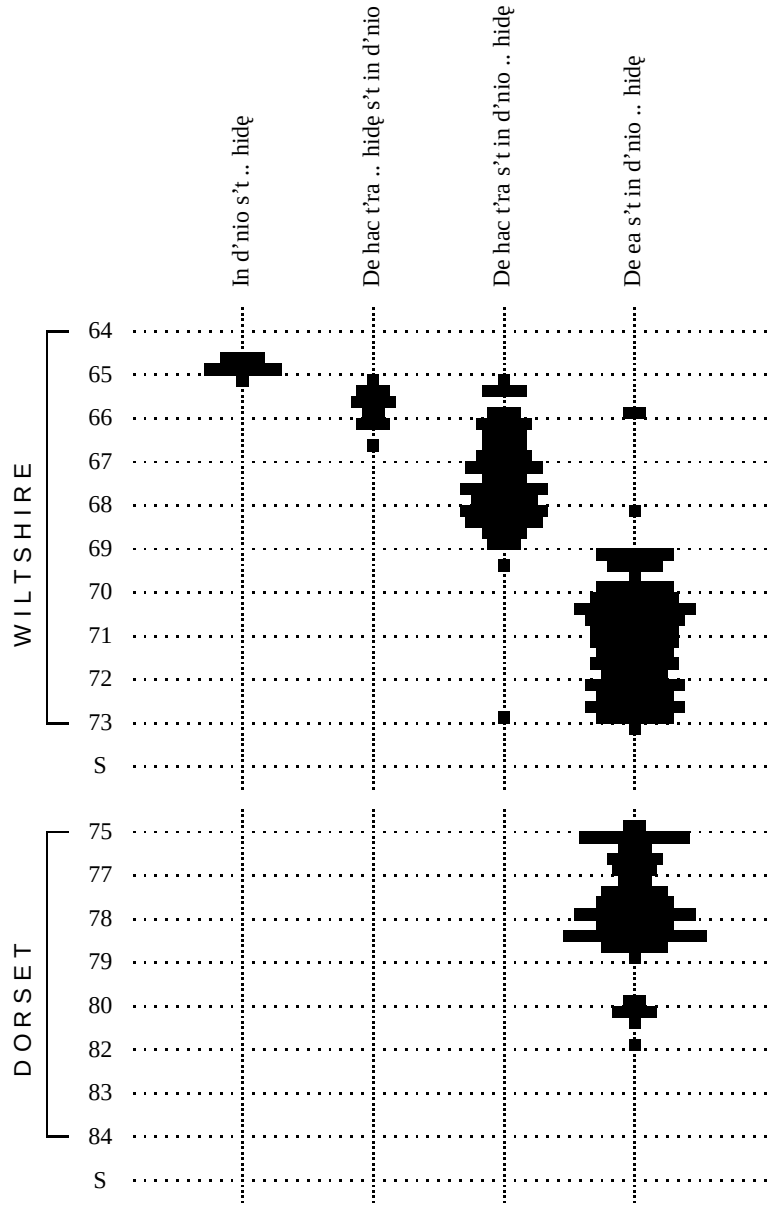

Figure 5. Booklets DB-Wi and DB-Do. Stabilization of word-order in the formula *De hac t'ra s't in d'nio .. hidę*, followed by a transition from *De hac t'ra* to *De ea*.

DnCo Later than So.

That is the end of the sequence. The conclusions arrived at are summed up in Table 3.[30].

2

It is clear that this table is not the last word on the subject. For a start, the latter part of aspect 2 has still to be sorted out: there are three booklets here which I have not been able to seriate satisfactorily.[31] (Presumably they were written in fairly rapid succession, without much pause for thought.) As far as it goes, however, the seriation is robust. I am sure that this sequence is right, and that anyone who thinks of repeating the experiment will arrive at the same result.[32]

[30] ((A modified version of this table will be found in chapter 11))

[31] In some respects, Ca seems to stand apart from the rest of aspect 2. (For example, in the formula *Hoc m' iacuit et iacet in d'nio ęccl'ę* . . . , Ca is the only booklet which, more often than not, replaces *iacuit et iacet* with *iacet et iacuit semper*, presumably through interference from the formula *Val' et ualuit semper* . . . .) So far as that is true, it suggests that Ca may be either the earliest or the latest of the batch; and since it cannot be the earliest (that place is already occupied by Mx), it may possibly be the latest. But the indications are slight, and I do not regard them as conclusive. ((Though I no longer agree with it, I allow this footnote to stand; at least I was right to say that this evidence is not conclusive. It seems rather to mean that the scribe became slack, while he was writing Ca, but then tightened up again.))

[32] Sceptical readers are welcome to try constructing a different seriation. If there are significant resemblances which cut across the sequence that I am proposing, it will be interesting to see how they can best be explained. (There are several possibilities.)

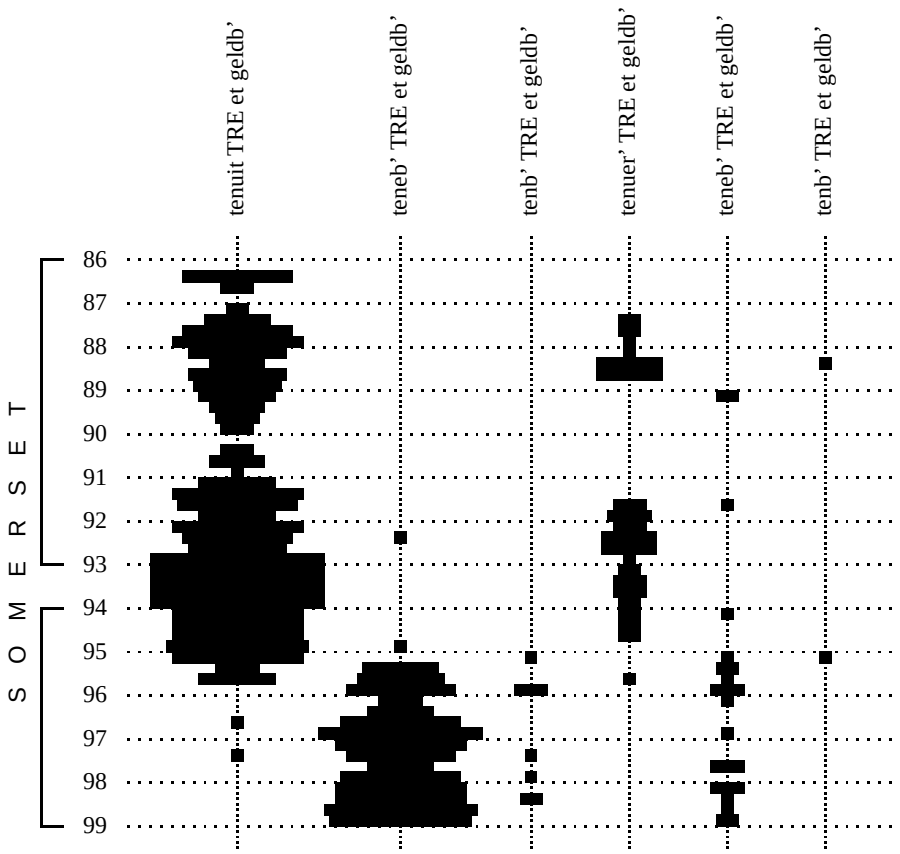

Figure 6. Booklet DB-So. Transition from *tenuit* to *teneb'*. (Plural instances are counted separately (channels 4–6); but the distinction becomes invisible when the verb is put into the imperfect tense.)

Even if the sequence were fully resolved, it should still be regarded only as a first approximation. The sequence which I am seeking to establish here is the order of inception; I do not suggest that each booklet was completed – absolutely completed – before the next was started. From more detailed analysis it may appear that the scribe sometimes interrupted work on one booklet in order to start another, not going back to finish the first booklet till after he had finished the second.[33] Each booklet has a history of its own; and that history will need to be compared with the basic sequence, once that sequence has been adequately worked out.

In every booklet (though in some much more than in others) entries occur which are manifestly later than the main text – because they are written in the margins, because they lack rubrication, or because they display some other incongruity. In checking through what he had written, the scribe discovered that he had omitted an entry which ought to have been included (or, rarely, vice versa) and inserted it as best he could (or cancelled it). In some cases, he may have discovered his error almost immediately; in others, perhaps, not till very much later. To some extent it may be possible to seriate such added entries by matching them with the main sequence. In Ke, for example, one of the added entries (4va) uses a formula – *Olim xx sol', Modo ual' xxx sol'* – which is characteristic of aspect 6. (It starts appearing near the end of So and persists throughout DnCo.) Instances of this kind corroborate the basic sequence (by proving, as this entry does, that aspect 3 is earlier than aspect 6), and they also shed light on the history of the individual booklet (by proving, as this entry does, that Ke was being corrected, or still being corrected, only shortly before the whole project came to an end). The same *Olim . . . Modo* formula appears in some of the inserted entries in aspect 1 – most conspicuously in Nm. If the scribe had ceased work a few weeks sooner than he did, those additions (so it seems) would not have been made; if he had continued for a few weeks longer, there is no knowing what other additions he might have wanted to make. Because DB as a whole was left unfinished, it cannot be said for certain that any of the constituent booklets was completed, except in an adventitious sense (the scribe stopped making additions in a given booklet because he had stopped work altogether); and in that sense the booklets were all completed simultaneously.

There is, however, one action taken by the scribe which amounts to a declaration that in his view the booklet is finished, or very nearly so. At some point he decides that the booklet is ready to be rubricated and sets himself to work, highlighting some elements of the text and adding some

[33] There is one place (but only one place) where I suspect that this may have happened. If we were seriating counties rather than booklets, I think that He might fall between Gl and Wo.

aspect	counties	binding sequence	17th-century foliation
1	Yorkshire–Lincolnshire	26	297–372
1	Nottinghamshire	25	280–96
1	Derbyshire	24	272–9
1	Huntingdonshire	17	203–8
2	Middlesex	10	126–31
2	Bedfordshire	18	209–18
2	? Buckinghamshire	12	143–53
2	? Hertfordshire	11	132–42
2	? Cambridgeshire	16	189–202
3	Kent	1	0–15
3	Sussex	2	16–29
3	Surrey	3	30–6
3	Hampshire	4	37–55
3	Berkshire	5	56–63
4	Gloucestershire–Worcestershire	14	162–78
4	Herefordshire	15	179–88
4	Shropshire–Cheshire	23	252–71
4	Staffordshire	22	246–51
4	Warwickshire	21	238–45
5	Northamptonshire	19	219–29
5	Leicestershire	20	230–7
5	Oxfordshire	13	154–61
5	Wiltshire	6	64–74
5	Dorset	7	75–85
6	Somerset	8	86–99
6	Devon–Cornwall	9	100–25
	Not seriated:		
	clamores etc.	27	373–82

Table 3. Seriation of the DB booklets.

other elements in spaces which he has reserved for them. In the history of every booklet, there are, it seems, only two definite moments: the moment when the scribe starts writing, and the moment when he starts rubricating what he has written. (The ruling of the sheets is a definite moment too; but that precedes the formation of the booklet.) It may be possible to construct an independent seriation of the booklets based solely on variant features of the rubrication. If this can be done, even if only with partial success, the order which emerges ought to be approximately the same as the order of inception; but it need not be exactly the same.

3

Despite these uncertainties, we have a clear enough idea of the order in which the booklets were written to draw some conclusions from it. First, at one point the division into booklets needs to be reconsidered. It turns out that Nm and Dy were intended to form a single booklet, even though Dy consists of a separate gathering.[34] The preliminary material which appears in Nm relates to both counties: the survey of the town of Nottingham (280ra) is followed by the survey of the town of Derby (280rb), and then by an account of the customs of Nottinghamshire and Derbyshire, the two counties being treated together but named in that order (280va). The survey of Derbyshire, therefore, was certainly meant to follow the survey of Nottinghamshire (with its Rutland appendix), completing the order of business implied by the preliminaries; and the fact that Dy begins at the start of a new gathering (with five pages left blank at the end of Nm) does not mean that it forms a new booklet. That reduces the number of booklets to 26 (or to 25, if the gathering which I have ignored is treated as an appendix to YoLi, not as a separate booklet).

Second, there are some fairly definite discontinuities. In the early booklets large changes occur, but they do not coincide with the start of a new county. The beginning of Li resembles the end of Yo, the beginning of Nm resembles the end of Li, and so on. But eventually we come to a break: the beginning of Mx is obviously very different from the end of Hu. Subsequent discontinuities are not nearly as sharp as this one, but breaks occur fairly definitely at the beginning of Ke and GlWo, less definitely later on. From the fact that

[34] Uniquely, these two counties were coupled together during the fieldwork phase of the survey. One sheriff was in charge of both; one meeting at least was convened at which both counties were represented.

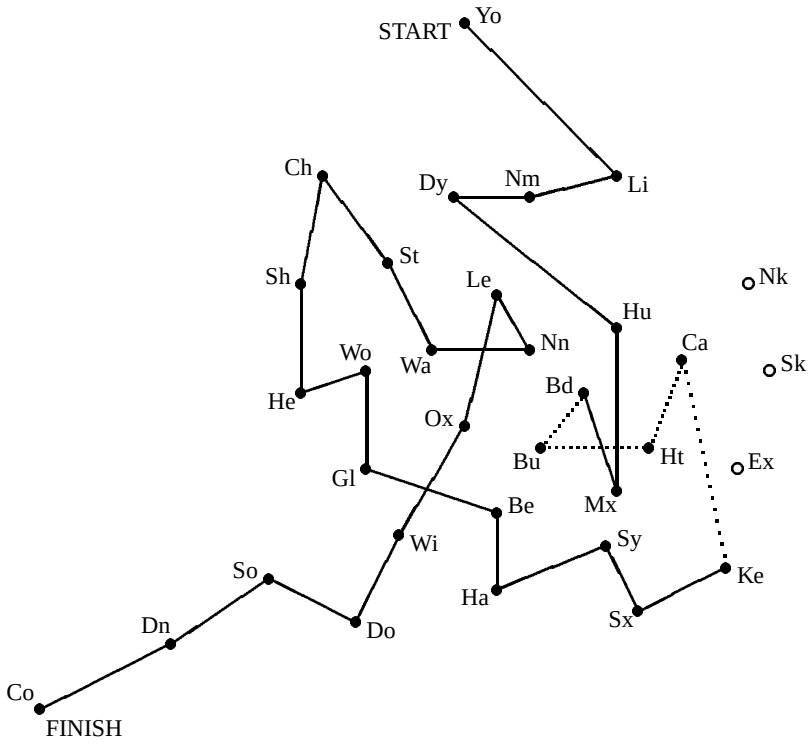

Figure 7. The sequence of DB booklets represented as a tour of the country.

these discontinuities fade away, it seems obvious that they originated during the writing of DB. The scribe, I suppose, set himself a target: he would complete a certain number of counties (five would be a good number), and then he would take a break. Having reached his target, he stopped work, and spent some time looking over what he had done. Perhaps he discussed it with his colleagues; perhaps he had to submit it to a supervisor. By the time he started work again, he had decided (or had been advised or instructed) to make certain changes in the treatment of the information. Early on, while he was still feeling his way, those changes might be very large ones; later, after he had settled into his task, the changes were relatively slight. As far as I can see, there is no need (and therefore no justification) for any deeper explanation than that.

Third, the order is not haphazard – though conceivably it might have been, if the scribe had let it be dictated by the order in which the source texts became available.[35] However the compilation process worked, it seems safe to assume that the source texts were not all completed simultaneously; and the scribe must surely have been constrained by that fact, to some degree. But he was not so tightly constrained that he could not think of imposing some order on the booklets; and the order that he imposed is – by and large – a geographical order. As far as he could, he arranged things so that each new county was contiguous with the preceding county. The sequence thus represents an imaginary journey around the country, starting in Yorkshire and finishing in Cornwall (Fig. 7).[36] There are some anomalies, however, and possibly they mean that the accidents of the compilation process constrained the scribe to vary the order which he would otherwise have preferred. His headlong descent from Derbyshire through Huntingdonshire into Middlesex seems out of keeping with the overall plan. The explanation may be that after the scribe had finished with Dy the source text that he wanted next was not yet ready; and since he could not afford to wait for it his only other option was to jump ahead. At this juncture, therefore, the order did become haphazard, but only momentarily. Perhaps the same explanation applies to the three counties – Norfolk, Suffolk, Essex – which were (apparently) omitted from his itinerary.[37] However we think of explaining them, these anomalies tend to prove that the order in which the booklets were written was not exactly the same as the order in which the scribe would have chosen to write them, if his choice had been unconstrained.

By extension, the order in which the booklets were written cannot be assumed to be the same as the order in which the scribe would have wanted to have them bound, when that moment arrived. As far as I can see, there are no indi-

[35] ((Though I have shortened this paragraph, which seems very primitive to me now, I have not rewritten it with the advantage of hindsight. At the time, I think, it was an honest attempt to face up to the evidence – without taking anything for granted, without postulating deep explanations for what might be merely surface phenomena.))

[36] ((The crux is the intersection of edges (Be, Gl) and (Ox, Wi), where the sequence loops back over itself: the moment when I understood that was my eureka moment. I am gratified to see that Thorn and Thorn (2001, p. 43) incline towards the same conclusion.))

[37] The scribe may have intended to complete his imaginary tour by writing booklets for Nk, Sk and Ex. Apparently he never did so: but the fact that no such booklets survive does not prove that they never existed.

cations as to what the ultimate ordering would have been, if the scribe had completed the manuscript and prepared it for the binder himself.[38] He had committed himself on some points – Li had to follow Yo, Wo had to follow Gl, Ch had to follow Sh, Co had to follow Dn – but in most respects he had kept his options open. This does not necessarily mean that he had any positive intention of shuffling the booklets into a different order: he may simply have found that it was generally more convenient, while the text was being written, and perhaps even more so while it was being checked, if each county occupied a booklet by itself. My guess is that he would have made a few transpositions, but not more than a few. There is no certainty, however. For all we know, he may have been intending to rearrange the booklets into some completely different order – or, alternatively, may never have made up his mind.

It is disappointing not to know how the components were supposed to be assembled: it means that we cannot form any distinct idea what the scribe would have wanted the finished book to look like. From one point of view, this uncertainty may be welcome. If we knew beyond doubt what ordering had been intended by the scribe, we should have to think of taking the book apart, rearranging the booklets, and renumbering every leaf. Because we do not, we are spared from doing that. The order in which the booklets are bound will have to be left unchanged. We may not like it, but we are not in a position to say that it is wrong (except as far as Dy is concerned).[39] On the other hand, anyone working with a facsimile (or with the Phillimore edition) will presumably want to reshuffle the booklets, so as to be able to deal with them in their original order.[40] Much will be gained by doing so, nothing lost. Anyone who thinks that it will make no difference has not thought hard enough. Perhaps it might be excessive to say that anything written about DB in ignorance of this seriation was written too soon. But there cannot be many questions which will not need to be reconsidered once the seriation has been thoroughly worked out.

[38] Ten gatherings are signed; but the signatures are not original (Gullick 1987, p. 104), and in any case they only serve to keep the YoLi booklet in order. Jenkinson pointed out that the red D in DOUERE (1ra) is, by some margin, the largest coloured initial in the book, and might be taken as a sign that the Ke booklet was meant to come first. This seemed to him 'a small point', worth mentioning only in a footnote (Jenkinson 1954, p. 32); I concur.

[39] It was certainly a mistake to put the third NmDy gathering in front of the other two: in this respect, without doubt, the order of binding is not the order originally intended.

[40] Sooner or later, I suppose, it will have to be decided whether to introduce an alternative foliation, to be used for some purposes in preference to Fauconberge's.