

**THE PHYLOGENY OF THE ORDER
IN THE *CANTERBURY TALES***

BY

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A BEGO

Confiaste en mí cuando otros dudaban
Te aseguraste de que mis sueños no fueran sueños rotos
Me diste siempre consejo generoso
No eres sólo amiga: hay cosas que pueden más que la sangre

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ABSTRACT

Since Furnivall's *Six-Text Edition*, the order of the *Canterbury Tales* has been a matter of concern for many scholars interested in Chaucer's work. Moreover, this issue is faced by all editors of the text, since the tales need to be arranged in a specific sequence or in a series of sequences, as suggested by Pearsall when he proposed an edition in booklets.

This work is an interdisciplinary approach that uses computer technologies in combination with traditional codicological analysis to study the relationships between the different tale orders in the 58 more or less complete manuscripts and fifteenth century editions of the *Tales*. This work is organized in seven chapters. Chapter 1 focuses on scholarly work relating to the order of the *Canterbury Tales*; chapter 2 presents a brief history of the stemmatic approach to the criticism of texts; chapter 3 explores the possibility of studying the order of the *Tales* from a stemmatic perspective; chapter 4 presents the results of the use of phylogenetic software applied to the study of the order of the tales and analyses these results; chapter 5 analyses the relationships between the tale-order and the word-variant stemmata; chapter 6 presents codicological analyses of Ad3 Ch Cp Dd Ha4 and Hg. The findings of this work are presented in the conclusion (chapter 7), where the implications of the different orders are considered.

It is a known fact that some scribes accidentally altered the order of the *Tales* (as the Hg scribe did), but this work shows that some scribes altered the order intentionally. Codicological evidence suggests that both Ch and Ha4 added the Tale of Gamelyn to their texts when this tale was not part of their copy-texts. The conclusion of this work also suggests that further codicological analyses of the witnesses of the *Canterbury Tales* could still cast some light on the developments of manuscripts with **b**, **c** or **d** tale-orders as well as elucidate or suggest the places in the textual tradition in which these orders might have originated.

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ABBREVIATIONS

Witness Sigils

Manuscripts

- Ad1 London, British Library, MS. Add. 5140
Ad2 London, British Library, MS. Add. 25718
Ad3 London, British Library, MS. Add. 35286
Ad4 London, British Library, MS. Add. 10340
Bo1 Oxford, Bodleian Library, MS. Bodl. 414
Bo2 Oxford, Bodleian Library, MS. Bodl. 686
Bw Oxford, Bodleian Library, MS. Barlow 20
Ch Oxford, Christ Church College, MS. 152
Cn Austin, University of Texas, Humanities Research Center, MS. 143 (ex Cardigan)
Cp Oxford, Corpus Christi College, MS. 198
Ct Manchester, Chetham's Library, MS. 6709
Dd Cambridge, University Library, MS. Dd.4.24
Dl Tokyo, Takamiya MS 24 (ex Delamere)
Ds1 Tokyo, Takamiya MS 32 (ex Devonshire)
El California, San Marino, Huntington Library, MS. El. 26 C 9 (Ellesmere)
En1 London, British Library, MS. Eg. 2726
En2 London, British Library, MS. Eg. 2863
En3 London, British Library, MS. Eg. 2864
Fi Cambridge, Fitzwilliam Museum, MS. McClean 181
Gg Cambridge, University Library, MS. Gg.4.27
Gl Glasgow, Hunterian Museum, MS. U.1.1 (197)
Ha1 London, British Library, MS. Harley 1239
Ha2 London, British Library, MS. Harley 1758
Ha3 London, British Library, MS. Harley 7333
Ha4 London, British Library, MS. Harley 7334
Ha5 London, British Library, MS. Harley 7335

He	New Jersey, Princeton University Library, MS. 100 (Helmingham)
Hg	Aberystwyth, National Library of Wales, MS. Peniarth 392 D (Hengwrt)
Hk	Norfolk, Holkham Hall, MS. 667
Hl1	London, British Library, MS. Harley 1704
Hl2	London, British Library, MS. Harley 2551
Hl3	London, British Library, MS. Harley 2382
Hl4	London, British Library, MS. Harley 5908
Hn	California, San Marino, Huntington Library, MS. HM 144
Ht	Oxford, Bodleian Library, MS. Hatton Donat.1
Ii	Cambridge, University Library, MS. Ii.3.26
La	London, British Library, MS. Lansdowne 851
Lc	Lichfield Cathedral, MS. 29
Ld1	Oxford, Bodleian Library, MS. Laud Misc. 600
Ld2	Oxford, Bodleian Library, MS. Laud Misc.739
Ll1	Wiltshire, Longleat House, MS. Longleat 257
Ll2	Wiltshire, Longleat House, MS. Longleat 29
Ln	Lincoln Cathedral Library, MS. 110
Ma	University of Manchester, John Rylands Library, MS. English 113
Mc	Chicago, University of Chicago Library, MS. 564 (McCormick)
Me	Aberystwyth, National Library of Wales, MS. 21972 D (Merthyr)
Mg	New York, Pierpont Morgan Library, MS. 249
Mm	Cambridge, University Library, MS. Mm.2.5
Ne	Oxford, New College, D.314
Nl	Northumberland, Alnwick Castle, MS. 455
Np	Naples, Royal Library, MS. XIII.B.29
Ox1	University of Manchester, John Rylands Library, MS. English 63 (Oxford)
Ox2	Philadelphia, Rosenbach Museum and Library, MS. 1084/2 (Oxford)
Ph1	Austin, University of Texas, Humanities Research Center MS. 46 (Phillipps 6570)
Ph2	Geneva, Bodmer Library, MS. 48 (Phillipps 8136)
Ph3	Philadelphia, Rosenbach Museum and Library, MS. 1084/1 (Phillipps 8137)
Ph4	California, San Marino, Huntington Library, MS. HM 140 (Phillipps 8299)

- Pl New York, Columbia University Library, MS. Plimpton 253 (Phillipps 9970)
- Pp Cambridge, Magdalene College, MS. Pepys 2006
- Ps Paris, Bibliothèque Nationale, MS. Fonds Anglais 39
- Pw Sussex, Petworth House, MS. 7
- Py London, Royal College of Physicians, MS. 388
- Ra1 Oxford, Bodleian Library, MS. Rawl. poet.141
- Ra2 Oxford, Bodleian Library, MS. Rawl. poet.149
- Ra3 Oxford, Bodleian Library, MS. Rawl. poet.223
- Ra4 Oxford, Bodleian Library, MS. Rawl. poet C.86
- Ry1 London, British Library, MS. Royal 17 D.XV
- Ry2 London, British Library, MS. Royal 18 C.II
- Se Oxford, Bodleian Library, MS. Arch. Selden B.14
- Si Tokyo, Takamiya 22 (Sion College)
- Sl1 London, British Library, MS. Sloane 1685
- Sl2 London, British Library, MS. Sloane 1686
- Tc1 Cambridge, Trinity College, MS. R.3.3
- Tc2 Cambridge, Trinity College, MS. R.3.15
- Tc3 Cambridge, Trinity College, MS. R.3.19
- To1 Oxford, Trinity College, MS. 49
- To2 Oxford, Trinity College, MS. 29

Pre-1500 Printed Editions

- Cx1 William Caxton, first edition (c. 1476)
- Cx2 William Caxton, second edition (c. 1482)
- Pn Richard Pynson (1492)
- Wy Wynkyn de Worde (1498)

Abbreviations for Tales and Links¹

General Prologue	GP
The Knight's Tale	KT ²
Link 1 (The Miller's Prologue)	L1
The Miller's Tale	MI
Link 2 (The Reeve's Prologue)	L2
The Reeve's Tale	RE
Link 3 (The Cook's Prologue)	L3
The Cook's Tale	CO
Link 7 (The Man of Law's Prologue)	L7
The Man of Law's Tale	ML
Link 15 (The Merchant's Prologue)	L15
The Merchant's Tale	ME
Link 8 (The Man of Law's Endlink)	L8
The Squire's Tale	SQ
Link 20 (The Squire-Franklin Link)	L20
The Franklin's Tale	FK
The Wife of Bath's Prologue	WBP
The Wife of Bath's Tale	WBT ³
Link 10 (The Friar's Prologue)	L10
The Friar's Tale	FR
Link 11 (The Summoner's Prologue)	L11
The Summoner's Tale	SU
The Clerk's Tale	CL
Link 13 (Lenvoy de Chaucer)	L13
Link 14 (The Host Stanza)	L14
The Nun's Tale	NU

¹ For a detailed account of the Canterbury Tales Project's sigils and lineation system see Blake, "Lineation."

² KT is the only abbreviation in which I do not follow Blake's lineation system ("Lineation" 5-14). In this case I follow the abbreviation used in Stubbs, *The Hengwrt Chaucer Digital Facsimile*.

³ WBP and WBT correspond to Blake's WB.

Link 33 (The Canon's Yeoman's Prologue)	L33
The Canon's Yeoman's Tale	CY
The Physician's Tale	PH
Link 21 (The Physician/Pardoner Link)	L21
The Pardoner's Prologue and Tale	PD
The Shipman's Tale	SH
Link 24 (The Shipman/Prioress Link)	L13
The Prioress' Tale	PR
Link 25 (Prologue to Sir Thopas)	L25
The Tale of Sir Thopas	TT
Link 28 (Thopas/ Melibee Link)	L28
The Tale of Melibee	TM
Link 29 (The Monk's Prologue)	L29
The Monk's Tale	MO
Link 30 (The Nun's Priest's Prologue)	L30
The Nun's Priest's Tale	NP
Link 31 (The Nun's Priest's Endlink)	L31
Link 36 (The Manciple's Prologue)	L36
The Manciple's Tale	MA
Link 37 (The Parson's Prologue)	L37
The Parson's Tale	PA
Chaucer's Retraction	RT

INTRODUCTION

This research developed from studies carried out by members of the STEMMA Project,¹ with funding from the Leverhulme Trust. The preliminary research suggested that further investigation into the relationships between the early manuscripts of the *Canterbury Tales* could help our understanding of this very complex textual tradition. The present work indicates that the study of the textual tradition of the *Tales* is likely to have consequences for scholars other than textual critics and editors. Indeed, the investigation and interpretation of the codicological aspects of the manuscripts analysed here might be helpful for scholars interested in material culture, scribal behaviour and other such matters. The use of phylogenetic software and the analysis of its methods might be of help in understanding the reasons why these methods work and might serve as an example of their use.

1. CONCEPTS AND IDEAS USED THROUGHOUT THIS WORK

1.1 The Concept of O

It might be useful here to define some concepts and ideas used throughout this work to avoid confusion in the following chapters. A fundamental concept in my research is that there was a single archetype for the textual tradition of the *Canterbury Tales*. This archetype² is referred to as O. Research by the Canterbury Tales Project team shows that O is likely to have been a pile of papers,³ more or less loosely bound, and that this single set of papers was the origin of the textual tradition that is extant today. This concept

differs from Manly and Rickert's notation, which distinguishes an original by Chaucer from the archetype:

The processes of the genealogical method (recension, as they are called) can result only in the establishment of the archetype where an archetype existed and in displaying the rival variants where copies existed separate from the main archetype. Comparison of the MS readings affords no means of passing beyond the archetype to the author's original except where there is reason to believe that certain variants transmitted by extant MSS have been preserved by direct derivation from the author's original. (2:40)

Clearly, Manly and Rickert reject the idea of attempting to reclaim Chaucer's original (O^1), while they leave open the possibility of reconstructing the origin of the tradition (O^2). It is difficult, however, to understand what exactly they refer to as 'Chaucer's original.' It appears that they are not referring necessarily to a physical text, since they consider that the archetype of the tradition can potentially be recovered, but they discard the idea that 'Chaucer's original' could have been this archetype. It is more likely that O^1 was understood as an ideal text and not any of its physical manifestations.⁴ Manly and Rickert consider 'Chaucer's original' (O^1), irrecoverable. Instead, O^2 can be reconstructed. The implication is that the head of the textual tradition, the archetype, is not 'Chaucer's original.' This raises a question concerning the definition of this original. For example, if the archetype was copied by a scribe from wax tablets which contained the text as written by Chaucer, then surely the tablets, not the copy, would have to be accepted as the archetype of the tradition. If this were the case, from a theoretical

perspective, both O¹ and O² would be equally easy to reconstruct. Manly and Rickert's caution is unnecessary. It is interesting to note that Manly and Rickert's prudence appears to have its root in the fact that they seem to have foreseen that the 'reconstruction' of an original was likely to be impossible. They failed to note, however, that the reconstruction of the archetype of a tradition (independently of who generated it) is not borne out by the evidence. The data as they exist today, do not allow us to determine with certainty that the manuscripts we have represent all the branches of the tradition that ever existed. It is conceivable that a complete branch might have disappeared leaving no trace. If such branch had uniquely preserved archetypal readings, these would also have been lost, thus making it impossible to reconstruct the archetype. And because of this, they continued to assert that it is possible to 'recover' it.⁵ My own interpretation of O¹ and O² in Manly and Rickert is that they conceive Chaucer's original as a flawless text (O¹) and therefore find it difficult to explain variants that are clearly archetypal, but are nonsensical (such as the case of 'troce'/'croce' in WBP 484). Manly and Rickert also have difficulty with variants which are not clearly archetypal or non-archetypal (as is the case of 'certres'/'sterres' in KT 1179). The undeniable fact that the archetype of the tradition (O²) contained mistakes, decided Manly and Rickert to keep it separate from what they conceive as Chaucer's original, which, in their eyes, could not contain any errors.

In this work there is no assumption as to who wrote the archetype of the tradition (O) and there is no attempt to recover its order. In order to distinguish the references to O from those of the manuscripts directly descended from it, known as the **O** manuscripts,⁶ bold type is used for the latter. In the case of intermediate nodes in the tree where the

manuscript is no longer extant (hyparchetype), the same rule applies: the hyparchetype is represented using normal type and, when the reference is to the group itself, bold type is used.

1.2 Variants

1.2.1 Variants and Readings

In this work, there is no real distinction between the term ‘variant’ and the term ‘reading.’ For example, a variant may be referred to as the ‘archetypal variant.’ This concept of archetypal variant might seem a contradiction in terms for many textual critics. In the context of traditional stemmatics, intentionalism and other schools which have as their goal the ‘recovery’ of a text, the notion of an archetypal variant might even be considered nonsensical. It seems clear, however, that to say that one can isolate an ‘archetypal reading’ would be to admit that one can be certain of being in the presence of the text that originated the textual tradition. The idea of the reconstruction of the archetype is, of course, possible up to a certain degree, but it could enter into conflict with the concept and formulation of the New Stemmatics.⁷

1.2.2 Word Variants vs. Tale-Order Variants

For the purposes of this research, a distinction has been drawn between different kinds of variation. Usually, textual critics refer only to ‘variants’ (or, sometimes, ‘textual variants’), but, for the purposes of this research there was a strong need to differentiate between two different kinds of variants. For this reason, the terms ‘word variant’ and ‘tale-order variant’ are used to make this difference clear. The use of these terms allows

the explicit contrast and separation of two different aspects of the tradition, both of which are textual (which is why the term ‘textual variant’ is clearly inadequate).

2. BRIEF NOTES ON TEXTUAL CRITICISM

The results of this research would have a more fitting context if they were analyzed in the context of current perspectives on textual criticism. It seems clear that different editors of the *Canterbury Tales* have presented very different tale-orders in their editions. In large measure, this is because different editors have worked to different editorial theories.

2.1 The Intentionalist School⁸

When Greg published “The Rationale of Copy-Text,” he could not have predicted the enormous influence this article was going to have in Anglo-American scholarly editing (Greg “Rationale”). Fredson Bowers and G. Thomas Tanselle, in work based on Greg’s ideas, have produced editions that attempt to uncover the intentions of the author and to reconstruct these intentions. The prestige of these scholars led to the widespread practice of editing from this perspective. In fact, the language used by Tanselle to describe the different kinds of editions does not seem objective:

Whereas noncritical editions aim at preserving the texts of particular documents, critical editions aim at constructing, by means of the editor’s critical judgement, texts that come closer to attaining some desired standard than any of the surviving documentary texts happen to do. . . . Because no preserved document may contain a text that fully reflects its author’s

intentions, the critical editor undertakes the task of deciding, after an assessment of all available evidence, which preserved text is most authoritative and what alterations are required in it so that it will conform still more closely with the author's wishes. (37)

Although Tanselle leaves open the possibility that other approaches might be used in critical editions, he privileges the one of recovering authorial intention. It seems that the task of the 'critical editor' is to make the text conform to the wishes of the author. This perspective was a common approach for many years, so much so, that D. C. Greetham explains:

I was fully part of this ideology. The editions produced by my doctoral students were all eclectic, intentionalist editions, and my editorial work on John Trevisa, while not formally eclecticist, was in practice precisely that -- the construction of an ideal text approximating to the intentions of the absent author and dependent on the paradox of both a rejection of the evidence of scribally corrupt manuscripts and the employment of this corruption in constructing an idealist 'text that never was.' (373)

Greetham refers to his 'intentionalist' period in the past tense, thus suggesting that this is an outdated mode of editing. The situation he describes in *Theories of the Text* was generated by Jerome McGann's controversial 1983 book (*A Critique of Modern Textual Criticism*) in which he proposes an alternative approach to the editing of texts. The intentionalist approach has dominated the landscape for editions of Chaucer's *Canterbury*

Tales. This is especially true of editions which seek to ‘complete’ or ‘fulfil’ what Chaucer did not finish himself, for example by establishing what tale order Chaucer (in the editor’s view) ‘really’ intended.⁹

2.2 McGann and the Sociological Approach

After the publication of *A Critique of Modern Textual Criticism*, a debate started as to what was the best editorial method. McGann proposed that texts were not just produced by their author, but that they were the product of a collaborative effort of copy-editors, proofreaders and publishers with the author. McGann turned out not to be alone in proposing such a sociological approach, and soon his book was joined by D. F. McKenzie’s *Bibliography and the Sociology of Texts* (1986). Even though these texts were published in such a relatively brief period of time, their theories were unrelated:

[T]he dual existence of McGann and McKenzie’s work does itself have sociological import. It is striking that, until McGann’s 1988 review of McKenzie’s Panizzi Lectures (‘Theory of Texts’), the two social critics seemed to operate independently of each other, within their own textual societies. (Greetham 407)

But even with this real or supposed independence, McGann and McKenzie were perceived as part of a new editorial ‘movement’ (Tanselle “Sociology” 84). Tanselle had his say on the debate in 1998 when he published what may be considered to be an answer to McGann’s *A Critique*. To the best of my knowledge, there has been no attempt to produce a sociological edition of the *Canterbury Tales*. Indeed, if one were to bend the

concept of ‘collaboration’ to include any kind of scribal accretion, then any facsimile or documentary edition could be presented as a sociological edition. However, it seems clear that these are not what McGann would consider to be sociological, since these editions do not seem to accept the scribe as an ‘equal’ to Chaucer, and they tend to ‘correct’ the mistakes that might have been introduced into the text.

2.3 The Editing of Medieval Texts

There have been editions of medieval texts which are eclectic,¹⁰ and some that are not.¹¹ In eclectic editions, the intentionalist approach is clearly at work. Sometimes, as is the case of the Kane and Donaldson’s *Piers Plowman*, the authorial intention is recovered by the use of editorial judgement alone. In other cases, such as Manly and Rickert’s edition, there is an attempt to recover the archetype of the tradition rather than the author’s text. Other editors, such as Ruggiers in his edition of Hg with variants from El and Blake (*Canterbury Tales*), have prepared best-text editions into which they have intervened to a greater or lesser degree.

In chapter 2, I describe a method by which it is possible to construct a text that explains the texts as they are extant today. The New Stemmatics is an alternative editorial method which could represent a better approach to the study of the *Canterbury Tales* and perhaps of other medieval texts.

3. THE ORGANIZATION OF THIS WORK

This work is organized in seven chapters. Chapter 1 focuses on scholarly work relating to the order of the *Canterbury Tales*; chapter 2 presents a brief history of the stemmatic approach to the criticism of texts; chapter 3 explores the possibility of studying the order of the *Tales* from a stemmatic perspective; chapter 4 presents the results of the use of phylogenetic software applied to the study of the order of the tales and analyses these results; chapter 5 analyses the relationships between the tale-order and the word-variant stemmata; chapter 6 presents codicological analyses of Ad3 Ch Cp Dd Ha4 and Hg. The conclusion considers the implications of the different orders and presents the outcomes of this work.

¹ STEMMA stands for Studies of Textual Evolution of Manuscripts by Mathematical Analysis. The Project is lead by Christopher Howe from the University of Cambridge, and has been funded by the Leverhulme Trust. I will discuss the main goals of STEMMA in chapter 2.

² The term archetype is used to refer to the text from which all other texts in the tradition descend. The term hyparchetype refers to the text from which a genetic group descends.

³ This has been suggested before by Derek Pearsall, who has even proposed that the *Tales* should be published in this way and that the reader should be allowed to re-arrange them at will (*Life* 233).

⁴ Manly and Rickert are not completely precise about the nature of the archetype and do not offer an explicit statement about its nature.

⁵ This is not to diminish the importance of Manly and Rickert's work. By separating the 'author's original' from the archetype, Manly and Rickert present an important advancement in the theoretical aspects of the application of stemmatic analysis to the study of texts.

⁶ The **O** manuscripts have been described by Robinson as follows: "if the manuscripts in O are indeed only related by common descent from the archetype, then the six groupings in (four pairs and the two singletons Ch and Hg) represent a further six independent lines of descent. For convenience, the witnesses in this group are referred to as 'O,' but they should not be seen as constituting a genetic group in the same sense as do the other groups. . . ." For WBP, Robinson gives the **O** manuscripts as follows: Ad1/En3 Ad3/Ha5 Ra3/Tc1 Bo2/Ht Ch and Hg ("Stemmatic" 80). In spite of the clarity of this quotation concerning the nature of the **O** manuscripts, they have often been referred to incorrectly as if they were a genetic group (Cf. Blake, "Caxton's").

⁷ See below where I briefly describe some contemporary textual critical views.

⁸ For more details on intentionalism and especially on stemmatics see chapter 2.

⁹ See, for example, Benson's *The Riverside Chaucer*, Furnivall's *A Six-Text Print of Chaucer's Canterbury Tales in Parallel Columns* and Pratt's "The Order of the Canterbury Tales."

¹⁰ Lachmann's editions fall into this category, but so also do Kane and Donaldson's *Piers Plowman*, Manly and Rickert's *The Text of the Canterbury Tales* and the *Riverside Chaucer*. Although all of these editions are eclectic, the procedure followed by their editors was different. See chapter 2 for more details.

¹¹ Basically, all facsimile editions would be considered by Tanselle documentary editions.

CHAPTER I: A HISTORICAL SURVEY OF THE SCHOLARSHIP CONCERNING THE TALE-ORDER PROBLEM

In this chapter I make a chronological synthesis of the scholarship produced regarding the order of the *Canterbury Tales* in its different witnesses. I also explain the changes introduced by some of the scholars (for example Furnivall-Bradshaw and Skeat) who have edited this poem. For the purposes of this research, I have focused only on scholars who deal explicitly with the problem of the order of the tales and from a theoretical perspective. I briefly explain that even though the editors and scribes responsible for preparing manuscripts and incunabula seem to have faced the same issues when ordering their text, they did not offer a theoretical explanation of their approach to these issues.

Modern scholarship on the problems generated by the different orders appears to have started when F. J. Furnivall put together his “Six-Text” edition, which thus becomes the starting point of the discussion. After this study, many other scholars have dedicated a great effort to try to clarify the reasons for the differences in the order of the tales and which one of these orders, if any, is Chaucerian. When possible, I follow a chronological order for the discussion of the diverse positions regarding tale-order.

The very early manuscripts of the *Canterbury Tales* show different orders, and some of them appear to be the result of purposeful and sharply directed ideas. For example, the position of CL and ME after WB in Hk seems to anticipate the twentieth-

century birth of the so-called “marriage group,” and manuscripts belonging to Manly and Rickert’s genetic *d* group have TG directly after the Cook’s incomplete tale, as if the Cook had changed his mind and decided to tell the Gamelyn story instead of the one he had originally started. Similarly, both of William Caxton’s editions, and those printed later by Wynkyn de Worde and Richard Pynson, exhibit different orders. All of these, however, lack any comment on why and how their respective orders were conceived.

In the case of the manuscripts, there are at least three likely alternatives. The first one is that the scribe copied the tales in the order in which they were in his exemplar. The second is that either the scribe or his supervisor decided or was forced (for whatever reason) to change the order of the exemplar being copied. The third possibility is that the scribe might have been copying not from another book from the beginning to the end, but from a series of booklets that came in a casual order and that this, to some extent, determined the running order of the final product. The case of the printed editions could be seen as more straight forward: their orders probably originate, in one way or another, from the manuscript used to modify each of them, although, of course, there is still the chance of an editorial decision made by the publisher. The only fifteenth century edition directly and completely set from a manuscript is Caxton’s 1476 edition (Cx1). When he decided to print a second edition of the work, he used an offprint of Cx1 in which he made changes based on a different manuscript (Dunn 74). According to Greg, all fifteenth century editions, excluding Cx1, of the *Tales* were produced using the same method, i.e. a manuscript was used to correct them, but they used as a copy-text the text of the previous edition.¹ Probably, the tale-orders of the incunabula originated either

directly from the second exemplar, i.e. from the manuscript against which the previous text was being corrected, or from a combination of this text and the printed edition they were using as a base. Later editions, such as Thynne's, also took their tale-orders from manuscripts or previous editions but did not offer any explanations as to why they had chosen a particular order over the others (Blodgett 35-52).

Blake has pointed out that even if the text of the *Tales* has been printed regularly since the fifteenth century, only during the nineteenth century did discussions about textual (including tale-order) problems surrounding the text commence (Blake "Approach"). This does not mean that some of the pre-nineteenth century editors of the *Canterbury Tales* did not have a highly sophisticated agenda and very particular reasons to change the tale-order in their editions. It means simply that for the purposes of this research, I have focused only on texts that deal with the tale-order problem from some kind of theoretical perspective: i.e. that they not only have the intention of modifying the order of the tales but also of investigating and explaining the differences between the diverse tale orders.

When Furnivall assumed the task of producing his "Six-Text" edition of the *Canterbury Tales*, he could not have imagined he would be the first person who would really have to think about tale-order from a theoretical perspective. The reason Furnivall became the first to write about the order of the *Tales* is quite simple: the text of the six manuscripts chosen for publication was to be printed in parallel. Since the manuscripts have different orders, he found himself faced with the need of deciding which one of them to follow.² For the first time, this decision was unrelated to the manuscript being

used as a base, the opposite situation from the one presented by the incunabula, as explained above.

Furnivall corresponded with Henry Bradshaw,³ who suggested that fragment VII (SH, PR, TT, TM, MO, NP) should be moved immediately after ML.⁴ This idea originates in the fact that several manuscripts have a link after ML (L8, also known as the Man of Law's Endlink) in which the next speaker is mentioned. The manuscripts disagree as to who the next speaker is and they present three variant readings: Squire, Summoner, and Shipman⁵. In fact, no matter whether a manuscript has the reading 'Squire' or 'Summoner,' L8 is never followed by SU (Blake, "Links"). This means that even when some manuscripts (Ln Py Ra3 etc.) have the reading 'Summoner,' L8 is immediately followed by SQ. There is only one manuscript that has the reading 'Shipman' in L8 and in which this link precedes SH: the Selden manuscript. Se is generally considered an unreliable manuscript, but is the only manuscript that has the sequence ML-L8-SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP, as suggested by Bradshaw.⁶ Bradshaw's idea was not to use the running order in Se, since this manuscript has ML much later than it appears in El, for example. Instead, he suggested putting the sequence ML-L8-SH etc, immediately after fragment I (group A). The result of this is that we have an altered version of the El order in which we start with fragment I (group A), follow with ML and L8, and then we have SH and the rest of fragment VII (group B2). Furnivall went beyond merely following this and also changed the position of PH and PD (group C) to put them between fragment VII (group B2) and fragment III (group D). Furnivall's movement of

group C had a basis in what he considered the internal evidence of the text. About this tale-order, Donald Baker has said:

Furnivall's own contribution to the order was the shift of fragment C (*The Physician's Tale* and *The Pardoner's Tale*) to place no. 4 largely on the strength of the Pardoner's indication that he was hungry. Although the Chaucer Society order and numbering has had much influence in the tradition of the text of *The Canterbury Tales*, this particular shift of fragment C has been largely ignored since it was enshrined in Skeat's edition. Furnivall's speculations on the number of days that the pilgrimage required, and so forth, are of no particular importance for the history of the text of *The Canterbury Tales*. But his adoption of the "Bradshaw shift," . . . has been quite important and continues to be debated. (Baker 161)

The resulting order for the "Six-Text" edition was GP KT MI RE CO ML SH PR TT TM MO NP PH PD WB FR SU CL ME SQ FK NU CY MA PA RT, but as Baker pointed out, Furnivall's own contribution to this order was set aside, and it is Bradshaw's that continues to be debated and analysed. The questioning of what is known as the Chaucer Society order followed soon after.

When Walter Skeat edited his book, *The Works of Geoffrey Chaucer*, which was originally printed in 1894, he carefully acknowledged his debt to Furnivall:

[The book] owes everything to the labours of Dr. Furnivall for the Chaucer Society, but for which no satisfactory results could have been obtained, except at the cost of more time and toil than I could well devote to the

subject. In other words, my work is entirely founded upon the splendid ‘Six-text’ Edition published by that Society, supplemented by the very valuable reprint of the celebrated ‘Harleian’ manuscript in the same series. (Skeat vii)

It seemed clear, at that point, that the Chaucer Society’s tale-order was authoritative and had to be followed. However, slowly but surely, Skeat developed his own ideas about how the text should be ordered. Robert Kase suggested that Skeat’s order choice (he followed Furnivall’s in his 1894 edition) was based only on the fact that it had been approved by the Chaucer Society (Kase, “Observations”). This view about the authority of Furnivall’s edition, which continued to be held for many years, has been the result of the critics’ belief that they are more knowledgeable than the scribes were and, therefore, better suited to rearrange the *Canterbury Tales*.⁷ Baker describes Skeat’s change of ideas as follows:

Skeat rejected the Bradshaw arrangement and Furnivall’s own contribution of the arrangement of fragment C [*sic*]; he argued lengthily in *The Evolution of the Canterbury Tales* and in his *The Eight-Text Edition* that there was no “correct” or “final” order but only a “last” order, that we could not go on rearranging tales by what seemed more logical references, place-names, and so on... (167)

Skeat finally argued, both in *The Evolution of the Canterbury Tales* and in his 1909 essay about the Chaucer Society edition,⁸ that the order of Ha4 was Chaucer’s working order:

I shall proceed to show that the chronological order of [composition of] the types of the seven chief MSS., . . . is as follows: –Hengwrt, an archetype; Petworth, showing the first scheme of arrangement; Corpus and Landsowne, the second, Harleian, the third; Ellesmere and Cambridge, the fourth and last. In the first three schemes, we find Chaucer himself, at work, making various experiments. In the last scheme, we find the work of a careful editor. It follows that the authoritative type, the only one which arranges the Tales as Chaucer at last left them, is the Harleian. (9-10)

Skeat's argument for the use of Ha4 shows a clear tendency towards the recovery of authorial intention, although he carefully specifies that the order in Ha4 is, by no means, Chaucer's final order (Skeat 10).

In 1905, four years before Skeat's *The Eight-text Edition*, it was already an old assumption that the reason why we have diverse orders is because the text of the *Canterbury Tales* circulated in booklets. Eleanor Hammond pointed out that "the original form in which the *Canterbury Tales* circulated . . . was fascicular, booklike in several or many parts (Hammond 162)."⁹ From her perspective, only this can explain why there are so many orders in the different manuscripts of the *Tales*. Hammond also stated that the Bradshaw shift required a series of assumptions that she thought were difficult to sustain. The first of them was that Selden could have authority in reference to the order of the tales, but not in any other respect. Another counter-argument against Furnivall's order that Hammond puts forward is that the time and place allusions of the links are being interpreted by him as final. She points out that there is very clear evidence in the text,

such as the feminine pronouns that the Shipman uses to refer to himself or the indication of the Man of Law that he would tell a tale in prose when in fact he tells one in verse, which makes evident that the *Canterbury Tales* still required a vast amount of revision and adjustment. From Hammond's point of view, although the idea of the Bradshaw shift is indeed possible, other views should not be discarded:

[T]he counter-assumption is equally defensible, that the *Canterbury Tales* are not a torso, that the fragments contain contradictions that do not permit their organic union; and that the 'Chaucerian' order of the tales exists more clearly in our imaginations than it did in Chaucer's mind. (165)

Here Hammond seems more in tune with what would be the critical tendency of the late 20th century than with many who wrote immediately after her. The suggestion that the so-called Chaucerian order might be just a figment of the critics' imaginations could prove to be right, if it is true that Chaucer died leaving no fair copy behind. A strong part of Hammond's argument is her insistence on the idea of the tales circulating in an independent manner. At the time, this was, a commonly accepted idea, and the manuscripts were probably more difficult to reach than they might be today, creating problems if one had to rely on codicological evidence.

Skeat's ideas about the text continued to evolve and his disagreement with Furnivall became more and more evident. Especially important is the idea that the Bradshaw shift has a doubtful origin:

My notion is, that instead of deleting the whole Prologue [L8], the sole precaution taken was to erase the name of *squier*¹⁰ in the phrase "Sayde the

squier,” and to strike out the headline that described the Prologue. Hence the scribe of Hl. [Ha4] gives us no headline, and (seeing perhaps the *s* of the erased *squier*) wrote *sompnour* at a venture, though the following Tale was that of the Wife of Bath! And finally, finding that he was on a wrong track, never completed the Prologue, but left off in the middle of a sentence, and simply added -- “Here endeth the man of lawe his tale”; and so cut the tale off from all that followed. . . . the assignment to the Shipman is against all the evidence; but I have allowed myself (as Tyrwhitt did) to make believe that it is right, merely for the purpose of preserving the 28 lines, which are certainly all genuine. (Skeat, *Eight-text* 47-8)

Although it appears that Skeat’s main goal is to vindicate Ha4 and to change the editorial perspective towards this manuscript, what becomes very clear in the above quotation is his belief that there is not enough manuscript evidence to support the Bradshaw shift. However, even after such a strong series of statements about the shift and so many doubts cast on the evidence provided by L8, still in 1932, Kase classified the manuscripts in two distinct groups (class A and class B) and based this classification on the sequence ML-SQ. Manuscripts that follow this order belong to class A; those that break it belong to class B.¹¹ Kase describes his class A manuscripts as follows:

[T]he common maintenance of the Man of Law-Squire sequence, the violation of the Squire-Franklin sequence, and the insertion at this point of the disarranged parts of Groups DE, are in brief chief characteristics of Class A manuscripts. (17)

There is no need to go further than this to see that Kase is referring to orders such as that of Hg. It is easy to see where Kase is going: manuscripts that belong to class B are those that have the EI order.

[Class B] violates the Man of Law-Squire sequence and drops the passage which determines it, inserting in this place the parts of DE, now in their proper order. It further maintains intact the Squire-Franklin sequence. In both cases the order following the Franklin is substantially the same, except that a large number of Class B manuscripts, to maintain the consistency of topographical allusions, shift Group G further to the end of the framework.

(19)

From this we can tell not only that EI and the manuscripts that have its same order belong to class B, but also that there is another important characteristic of class A that was not absolutely clear before: the Man of Law's Endlink (L8) is present and serves to link ML and SQ. In contrast, manuscripts belonging to class B drop "the passage that determines the ML-SQ sequence." Kase does not offer a convincing explanation of why the manuscripts should have these two different orders. Moreover, his argument about L8 is inconsistent, since in many instances this link does not have the name 'Squier' in it, and has instead 'Sompnour,' as Hammond had already pointed out several years before. In this sense, L8 does not determine any particular sequence.

Manly and Rickert, as part of their eight-volume work printed in 1940, produced a detailed essay on the *Canterbury Tales'* order. They classified the manuscripts according to the differences that they presented in their tale-orders. As a result of this, four groups

became apparent. Some, however, manuscripts remained difficult to classify and Manly and Rickert put them together in a group labelled 'anomalous manuscripts.'¹² It is interesting that although their tale-order classification is closely related to their textual groupings, some manuscripts present different textual and tale-order affiliations. An example of this is El, which is the head of the tale-order **a** group, but has an independent textual status.¹³ The importance of Manly and Rickert's work is due to the fact that they were the first ones to take on the enterprise of analysing the complete corpus of fifteenth century manuscripts of the *Canterbury Tales* and, although their work is far from flawless, it has marked all subsequent textual studies. Manly and Rickert were the first scholars to produce an edition of the *Tales* for which all the manuscripts in existence were taken into account. They were also the first to attempt a classification of the manuscripts of the *Canterbury Tales* using the order of the tales as the main classificatory element.

There are fifty-four manuscripts and four fifteenth-century printed editions of the *Canterbury Tales* which are complete enough to allow comparisons of tale order. If we only take into account the table produced by Manly and Rickert as part of their edition as our base, we find that there are some 40 tale-orders.¹⁴ However, this table lacks some information that is of capital importance. Manly and Rickert included the links between the tales, but they failed to distinguish them individually. Their only distinction is a single link (represented as L) or a succession of links between, before or after the tales.¹⁵ During the course of my research, it became evident that more detail was necessary if this table was to be of any use. It was then that I decided to change the nomenclature of Manly and

Rickert's table to the one used by the Canterbury Tales Project, and to add the links as they were distinguished by Blake ("Lineation"). I also decided to add color to it, in order to make the traditional groups or fragments easier to identify.¹⁶ Based on this modification of the Manly and Rickert table (table 2), we find that there are at least 48 distinct tale-orders in the extant witnesses of the *Canterbury Tales*.

Faced with these data, it is not surprising to discover that the problem of tale-order in the *Canterbury Tales* is a very complex one. It also becomes evident that, if we choose to believe that Hg is the oldest manuscript of the *Tales* now extant, as has been suggested by Manly and Rickert and later supported by other scholars,¹⁷ we must understand that the issue of the order was present in the textual tradition from the very beginning. Codicological evidence shows that there is hesitation on the part of the Hg scribe when arranging the document, and also that there are changes of ink at crucial moments in the text. Anyone who is familiar with the *Canterbury Tales* might find it difficult to identify Hg's order as a well-known one. Indeed, even Manly and Rickert, who thought that the Hg text was superior to that of the El decided to change the order of the former in favour of the latter. Perhaps, as a consequence of this, but also due to the preference editors give to El, the order that most scholars would immediately recognize as "the order" of the *Canterbury Tales* is none other than that of El.¹⁸ This fact owes its origin to the idea (common among scholars during the first half of the twentieth century) that El was the best manuscript with the best text and the best tale order. One of the consequences of this idea was that all editions adopted El as their base text and also its tale-order, since this was supposed to be, by definition, the best.¹⁹ In any case, the

differences between Hg and El indicate that the tale order issue was important from the very beginning of the history of the text. Other early manuscripts also confirm this idea and, in fact, the scribes of most early manuscripts appear to have been searching for a better order in one way or another. But because there are so many different tale orders, it has been very difficult to decide which one (if any) has any precedence over the others.

Manly and Rickert's work had a great influence at the time of its publication, and awakened a renewed interest in textual matters relating to the *Canterbury Tales*, including tale order. In the years following the publication of their work, Germaine Dempster wrote a series of articles relating the manuscript history and the origin of the different orders. She based many of her ideas on the geographical references in the tales, but some of the conclusions she reached seem to have been a matter of common sense. For example, Dempster points out that the **a** order is likely to be editorial, as all are the others. Moreover, she concludes that the **a** order is not independent of that in Hg: "it should be noted that the one unquestionably un-Chaucerian feature of the a-El order, the position of CB² in the second half of the work and after the reference to Sittingbourne, was already in Hg (1139)." Although her conclusion about Hg being the origin of the **a** order seems reasonable, the means that helped her to reach it are dubious. Dempster is obviously working under the assumption that the geographical references are final and that Chaucer would not have changed his mind about them. This position can be questioned, however, since the *Canterbury Tales* was never finished, and therefore it is likely that, even if there was an order for the tales, this was a working order that needed to be revised, and not a final order. As Blake has put it: "The places mentioned are best

understood as provisional and without significance as far as final tale-order is concerned (“Critics” 218).”²⁰ This seems the most reasonable view as far as geographical references are concerned since we cannot be sure what Chaucer might have done if had he continued working on the *Tales*.

Another scholar, Pratt, suggested that L8 is one of the key riddles in the tale-order problem: if we could determine the name of the next speaker based on L8 then we should know if the Bradshaw shift should be accepted. Pratt also realized that there is something else wrong with the Hg order:

The order, ML Endlink-Squire’s Tale, found in at least thirty-one MSS., is also scribal in origin and unauthoritative, being derived from the error of the scribe of Hg, who failed to obtain the Merchant-Squire and Squire-Franklin Links until after he had placed the tales as follows: ML, Sq, Me, Fk. (1148)

In noticing this, Pratt is in good company, but he did not explain why a mistake in Hg (perhaps the earliest extant manuscript but not the origin of the textual tradition²¹) was reproduced by so many other manuscripts that also show the same order. Pratt’s final solution is as follows:

[T]he internal evidence suggests that Chaucer finally intended the Fragments of the *Canterbury Tales* to be arranged in the following order:

I	II	VII	III	IV-V	VI	VIII	IX	X
(A	B1	B2	D	E-F	C	G	H	I)

However, the continuity of the genealogical groups of MSS. through VI and the first half of VII, suggests that soon after Chaucer's death, before copying began, Fragment VII was accidentally misplaced from its "Chaucerian" position in such a manner as to yield the incorrect "1400" order. . . . (1166)

In the above quotation we can see the need of recovering the "authorial intention," and how scholars have searched for any clues as to what this could be. Pratt's argument is mainly based on internal evidence. One might argue that if the indications provided in the *Canterbury Tales* were so clear, then we probably would not have a problem with the order of the tales, but again, we must remember that this was a work in progress and therefore all internal reference might have been revised before the work could be completed.

A very different approach is the one put forward by Larry Benson in 1981, in a lengthy article published in *Studies in the Age of Chaucer*. He surveys some of the problems presented by previous interpretations of the order in Chaucer's poem. The most important aspect discussed by Benson is the idea of a Chaucerian order, which of course, would not be sustainable if we believe that Chaucer did not finish his work in the *Tales*. He argues that:

[W]e have Chaucer's own word, in the *Retraction*, that unfinished as *The Canterbury Tales* obviously is, he was finished with it. We have, in short, not a work in progress to which Chaucer intended to return and would have, had not death or illness prevented this, but rather Chaucer's final version, as it was when he decided his work on it was ended. (Benson, "Order" 80)

Here, Benson clearly does not acknowledge that the authenticity of the *Retraction*, not present in Hg Gg Dd or Cp, among other manuscripts, has been doubted by some scholars. In fact, a few years later, Blake observed:

Benson's view that Chaucer had finished with the poem [the *Canterbury Tales*], though it is apparently incomplete, runs counter to the accepted scholarly position and it is supported simply by his interpretation of Rt. In his opinion the inclusion of Rt indicates that Chaucer had finished with the poem. However, the status of Rt is dubious. It is not found in Hg or Corpus, perhaps the two earliest manuscripts, because of the lack of the final folios. It occurs for the first time probably in Ha7334, but that is an early manuscript which contains material which is probably not authorial. Its inclusion in Ha7334 cannot therefore guarantee its genuineness. . . . Furthermore, one may question whether Rt does indeed imply that Chaucer had finished with the poem. ("Debate" 187-8)

Indeed, there is no positive proof that RT is authentic, but even if there were, it could still be argued that the order the poem had at the time the RT was written might not be final, since Chaucer had lost interest in this work and had left it as it was. The simple fact that someone has decided not to do any more work on a book does not mean that by default one should take whatever text happens to exist at that point as what the author intended. If we leave aside the question of intentionality, we can still find some other arguable statements in Benson's article:

The scribes, as we have seen, were willing to tamper with the order, but the mss show no instance of a scribe changing the order of the tales on the basis of anything other than the most obvious clues in the links –'seyde the Squyer' and such. Most scribes were apparently interested only in smooth transitions from one tale to the another, and they were not above making such transitions by adding spurious links or changing readings when this seemed necessary. No scribe was ever influenced by internal evidence within the tales– allusions by one speaker to another tale, or geographical allusions to the Canterbury Road, which apparently no one ever noticed until the nineteenth century. ("Order" 111)

As I have mentioned before, the breaking up of group D in Hk is a clear example of purposeful alteration of the tale order to fit the allusions of one speaker to another. In this light, the phrase "no scribe was ever influenced by internal evidence within the tales" appears to be at least questionable. Not only had the scribe, or his supervisor, organized the tales in reference to the allusions made in the prologues, but apparently he went farther to create a group of tales with a common theme. However, what is probably Benson's most interesting sequence of statements is in his conclusion, where he makes a series of firm remarks about the authority of tale-order and his Type a:

The mss show that from the very beginning the work circulated in but two orders, both of which can be attributed to Chaucer; one may be an earlier version, in which case the Type *a*-Ellesmere order is the final arrangement, or it might be derived from the Type *a* by scribal error, the accidental

misplacement of the leaves containing G, in which case Type *a* is the only order attested by the mss. . . . (“Order”117)

Here, Benson seems to conclude, at the same time, that his type *a* order is the original and all others derive from it; that it is a later version and therefore Chaucer’s final arrangement; and finally that there is only one Chaucerian order and that is the type *a*. The arguments concerning Chaucer writing RT or not are irrelevant to determine the order of the *Tales*; even if he had decided he was done with the book, this does not mean that the tale-order of his working copy was his final intended order. In fact, the order in which Chaucer’s papers were left at the time he died, or at the time these were found by someone and copied by scribes, was the latest order rather than the ‘final’ one. This is not to say that the order in which the tales were left by Chaucer, and that in which these were copied by the first scribe to produce a unified manuscript, are the same. It is conceivable, if the *Canterbury Tales* were left in a pile of papers, that these could have been mixed up by anyone when putting them together. This seems to make even more doubtful Benson’s statement about the *a* order being Chaucer’s final intention, since it would imply that somehow there were instructions imbedded in the papers left after his death and there is no indication in the manuscripts of such instructions.

Although many critics question the intentionalist focus of previous research,²² in one way or another they seem to follow the same path and end up discussing what Chaucer wanted or what he might have done with the *Canterbury Tales* given the chance to finish the book. Donaldson starts his article “The Ordering of the Canterbury Tales,” by stating that it would be impossible for us to be certain which is the correct tale-order.

Then, as other critics before and after him, he goes on to explain that scholars should make decisions about tale-order in the same way they make decisions about variants: by carefully analyzing the data and then making appropriate choices in each case. Donaldson concludes as follows:

In a critical edition, I suppose the endlink [L8] should be printed either in an appendix as representing the passage genuinely Chaucerian but not placeable, or else, within square brackets, in its usual place after the Man of Law's Tale, with the speaker of the next tale identified as the Summoner (according to Manly and Rickert the reading of O), or, less conservatively, left blank on the grounds that the three names preserved in the MSS are equally scribal. In a less austere edition, I should do what I already have done: adopt Jones' conjecture and read *Wif of Bathe* as probably the character whom Chaucer once had in mind as the speaker of the next tale. But it seems to me, on the basis of the MS evidence, that all treatment of the Man of Law's endlink must be conjectural, and that its status is too uncertain to affect the matter of order.

("Ordering" 202-3)

Even if Donaldson had not provided strong arguments or if these had not been well constructed, he would still have had the merit of bringing editorial concern back into the picture, a subject that was later discussed by Blake in the introduction to his edition of the *Canterbury Tales*. However, the evidence linking L8 to WBP and WBT does not seem sufficiently strong to suggest that these have to follow it.

Blake has discussed the problem of the tale-order at length. When he edited the *Tales*, using Hg as his copy-text, he raised the question of the discrepancy between the acknowledgement of the excellence of its text and the complete rejection of its tale-order. In other words, he attempted to explain why the manuscript having what is considered the “best” text of the *Canterbury Tales* does not have the “best” tale-order. In order to maintain the status of Hg as the most important manuscript, Blake tried to show that if the tale order in it is not Chaucerian, no other order is either:

Readers will, however, appreciate that the order in Hg, which is the order followed in this edition, is a scribal one in so far as the arrangement of the sections is concerned, since Chaucer had not determined a final order by the time he died. (*Canterbury Tales* 6)

The idea that Chaucer did not have a final order for the poem at the moment of his death is a reasonable one. It seems supported by the lack of internal coherence of the text, which could have been either abandoned by Chaucer or awaiting more revision at some point. This of course, implies that all other orders are also scribal and that any choice of order for a modern edition would have to be a decision of the editor.

In his book, *The Textual Tradition of the Canterbury Tales*, Blake gives a thorough analysis and describes how different scribes faced the task of making sense of Chaucer’s text. Blake argues that Chaucer could not have finished the poem, and once more raises doubt as to the authenticity of parts of the text, such as RT. He insists on the fact that Chaucer had not released the poem at the time of his death, even though he

accepts that parts of it might have been read in court. The description of how the *Canterbury Tales* was first compiled is as follows:

Chaucer had been composing his poem for some time. He did so by writing individual tales which were gradually amalgamated with other tales through the provision of links... When he died a friend or some friends decided to publish the poem. To do this they collected the various sections of the draft from Chaucer's house and put them into some order. In the course of this exercise they realized that some linking passages were missing and that their first attempt at organizing the tales had certain flaws. They were therefore obliged to provide certain links to give the poem a veneer of completeness, and they also found it necessary to experiment with the order of some sections, although the bulk of the poem remained in the order in which was first devised for it after Chaucer's death (Blake, *Textual* 174-5).

This description seems to imply that some of the links, even in the first manuscripts produced, are by definition non-authorial, and although this point is certainly possible, there is no sure way to prove that the links that are present in the earlier manuscripts were or were not written by Chaucer. It seems a more likely possibility that the links were written by Chaucer, who must have had a working order for the tales. Robinson, while analysing the Hg sequence SQ-L20-ME-L17-FK, shows that the differences in meter between Hg and El in L17 and L20 are the result of changes introduced by the Hg scribe ("Can We Trust" 204 and ff.). Robinson, as others had done before him (Cooper 245-62), argues that the Hg scribe received the tales without the links and copied them in the order

SQ-ME-FK, leaving the appropriate space for the links that would follow. When the scribe got the links he realized that he had copied the tales in the wrong order and altered the readings of L17 and L20 so they would link the tales in the order he had copied them. The result of this is that Hg (otherwise metrically more regular than El) has a series of lines that are metrically inferior to those in El and all of these lines are the ones in which the names of the pilgrims have been altered to conform to the Hg tale-order. In this view, Robinson accepts that the original order of this section is that of El, i.e., ME-L17-SQ-L20-FK. The problem for him is to explain how the Hg tale-order appears in the Manly and Rickert *d* manuscripts:

The only possible explanation is that the text of the links was not altered just in Hengwrt. It was altered, probably by the scribe's supervisor, in the exemplar, that is, in *O* itself. The three tales were then placed in the exemplar in the same order as they are copied in Hengwrt, with the now-altered text of the links connecting them. This newly reshuffled *O*, then, in turn became the exemplar not only of the type *d* copies but also of Manly and Rickert's *c* group, and the additional group I label *f*. ("Can We Trust" 207)

I accept this interpretation as a likely one, which explains why we can find the altered sequence and the altered variants in a group that does not descend from Hg. This implies that Robinson's α^{23} was copied before Hg, and if this is true, then we can also assume that *O* was a pile of papers that were not bound together. In fact, this last assumption is supported by evidence put forward by Stubbs in *The Hengwrt Chaucer Digital Facsimile*, where she points out the odd quiring and the change of color of the ink in MI:

Quire six, fols. 42-43, consists of only two folios and quire seven, fols. 44-49, is a gathering of six leaves. This is the first irregularity in the assembly of the manuscript. The vellum of both quires is distinctly different from that of the quires before and after. All leaves are extremely dirty and the vellum has a different feel. Doyle and Parkes indicate the difference in ink and size of writing frame compared with the quires on either side. Because of their shared characteristics, the two quires have the appearance of a unit despite the division of folios. A two-leaf quire is an oddity though there are a number of possible explanations... The division into two uneven quires might be indicative of an earlier stage of text adaptation. A single outside bifolium would have contained the two disparate quires and created a prologue and tale unit of ten folios in a flexible format. As the Hg scribe came to the end of his copying of the Knight's Tale he was told to adapt the 'booklet' which he had copied previously and incorporate it into the larger block of tales which was to become Section I in Hg. In order for this to be an effective incorporation, the original outside bifolium of the older booklet would have to be discarded. The text contained on its original opening leaf would be recopied onto the final leaf of quire 5, fol. 41, to provide a smooth continuation from Knight to Miller.

Stubbs explanation, although possible, attempts to show that the Hg scribe was extremely careful and precise. The change in the ink color that goes almost to the end of MI suggests that the so called fragments might not have existed at all, and that, instead, some

of the tales were assigned their places because of the references in the links. In any case, if this hypothesis does not hold for the whole of the *Canterbury Tales*, it could at least be useful to explain what seems to have happened with “section I” in Hg, since scholars agree about the internal consistency of this part of the text (GP-KT-L1-MI-L2-RE-L3-CO), broken only by Ad3, which places the L3-CO at the end, between the Manciple’s Tale and the Canon’s Yeoman’s Prologue and Tale.²⁴ The digital facsimile also allows us to see the problems presented by L17 and L20, which were also written in a different ink color that Stubbs calls the “yellow” ink, implying that is the lightest found in Hg. About L17 Stubbs says that:

After copying the newly acquired ending for the Merchant’s Tale in the gray ink, the Hg scribe left half a page blank and then began copying the thirteenth line of the Franklin’s Tale... Presumably this was on the instruction that a link would need to be accommodated between Merchant and Franklin. The link was finally copied on to an inserted leaf making an irregular quire of nine leaves. It is unclear at what stage this extra folio was added but it may have been when the scribe attempted to ‘finish’ the manuscript in the yellow ink, copying as well as the link, the missing twelve lines of the Franklin’s Tale.

Stubbs also points out that L20 was copied in the same ink as L17; she also mentions that this is the ink used for what is believed to have been copied at the very end of the production process: the Nun’s Priest’s Tale and the Manciple’s Tale and the title of the book on folio 2r. This might indicate that both links were probably later additions to the manuscript.

At least in appearance, the problem of the order of the *Canterbury Tales* seems to be extremely difficult to study. The traditional approaches to it have left many questions unanswered, and scholars continue to debate which one is the order that Chaucer intended or which is the one that we should use when editing the *Tales*. It was required that someone propose a new approach to study the order of the tales. For these reasons, and as part of the work of the STEMMA Project it was decided to use phylogenetic programs, designed to meet the needs of evolutionary biologists researching relationships between different species of animals, to try to cast new light on the problem of tale-order in the *Canterbury Tales*.

Programs such as PAUP (Phylogenetic Analysis Using Parsimony, Swofford) and SplitsTree (Hudson) had been used successfully by Robinson to construct word-variant based stemmata, but they had never been tried with problems such as tale-order. In fact, there were questions about whether or not there might be a stemmatic relationship between the different tale-orders. The data that we used for this research is the result of the conversion of table 2 into computer-readable form.²⁵ In this sense, the work of STEMMA would not have been possible if it had not been for the new lineation system devised by Blake ("Lineation"). This system treats each link and tale as a separate item (the reason why table 2 offers richer data than the one produced by Manly and Rickert) and has allowed us to go beyond the groups and fragments into a more complex, but more precise, set of relationships.

STEMMA's initial research indicates²⁶ that there is evidence of a genetic relation between the different tale-orders. Moreover, we can also show that there are equivalent

relationships in a word-variant stemma and in one produced using tale-order data. However, the aim of the STEMMA Project is to give us a clearer idea of how and why the programs work with manuscript data, and there is no time for more detailed research on the order of the *Tales*. For this reason that I decided to take our research forward, as my PhD dissertation, and attempt to make more explicit those problems that members of the STEMMA project did not have time to pursue further.

There are several points that become evident after years of debate on the order of the *Canterbury Tales*. For example, it seems very clear that some of the manuscripts might have had “edited” orders, i.e., orders that arose because the scribes or their supervisors tried to make sense of the text. However, it is also clear that these manuscripts with edited orders might have been copied from, which means that the later manuscripts (and their tale-order) descend from them.

A second point is that there is clear evidence that the text, as Chaucer left it, was a work in progress,²⁷ and that for this reason it is not possible to discuss authorial intention in a traditional way. Which of Chaucer’s intentions should we discuss? His intention on the day he died, on the day he started the work, or on the day he wrote (if he did) the Retraction? If we accept that the *Tales* is a work in progress, none of the references within the tales can be taken as final, whether these are geographical or to the time of the day. Any of these references could have been changed at any point, and perhaps some of the incoherences that bother the scholars so are the result of changes in the order that Chaucer did not revise in more detail. Chaucer probably did not have a final order for the

tales completely planned, but he must have had a working order that he might have changed over and over as his work on the *Tales* progressed.

There are more interesting problems relating to tale-order than those of authorial intention or realistic accuracy. For example, the general relationship between all the tale-orders in the manuscripts of the *Canterbury Tales*. The comparison between these might lead us to the most important question on this research from an editorial perspective: is there any way to decide which is the most appropriate order to present to a reading audience in an edition of the *Canterbury Tales*? The answer to this question would probably generate others. If one tale-order is more appropriate than others, one should wonder if it exists already in a manuscript or has to be the result of editorial intervention. If editorial intervention is required, then one should ask what the justification of it would be.

In this work I use evolutionary biology programs to build stemmata based on tale-order and on word variants. I compare the results of these stemmata to see if it is possible to establish a clear genetic relationship among the tale-orders in different manuscripts, and finally, I use codicological information to try to clarify the origins of the tale-orders in some of the fifteenth century witnesses of the *Canterbury Tales*. With this approach I expect to draw a clearer picture of the variation in tale-order in the manuscripts, and also to outline a new perspective for editors of the poem.

¹ “While Caxton’s first edition was the only one set up from a manuscript, the printers of the next five editions all had recourse more or less extensively to manuscript sources in the hope of improving their texts.” (Greg, “The Early Printed Editions of The Canterbury Tales” 737-61).

² Furnivall chose to include El Hg Gg Cp Pw La (*Six-Text*), and later the Chaucer Society would print Ha4 (*The Harleian MS 7334*) and Dd (*The Cambridge MS. Dd. 4.24*) in order to complement the group.

³ For a brief account of the Bradshaw-shift see Pearsall, *Life* 234 and ff.

⁴ For a comparative table with all the different fragments, groups and sections see table 1 in the appendix.

⁵ Elizabeth Hammond was the first to suggest that perhaps the reading had been partially erased from the archetype and that the only visible part was the initial 's.' See Hammond 159-78. The variants do not generate a problem with the meter and, for this reason, they are difficult to assess from an editorial point of view.

⁶ The running order in Se is : GP-KT-L1-MI-L2-RE-CO CL-L14 WB-L10-FR-L11-SU L15-ME-L17a -SQ-L02 L7-ML-L8c-SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP NU-L33-CY-L34-PH-L21-PD FK L36-MA L37-PA RT

⁷ For example, M. L. S. Lossing states: "... no MS of the *Canterbury Tales* has the authority of the order adopted by the Chaucer Society. That is, modern editors have better resources at their command to aid them in determining the order of the *CT*, and consequently their arrangements have more authority than those of scribal editors." (153-63). Of course, statements like these have not been uncontested. Robert Pratt has pointed out the "undeserved respect" that scholars have granted to the Chaucer Society order. He suggested to emphasise the internal evidence to try to arrange fragment VI in a better position (1141-67).

⁸ *The Eight-text Edition* is not an edition; instead, it is an essay mainly focused on the eight texts which had been published by the Chaucer Society (see note 4 in this chapter).

⁹ This assumption is still accepted by some scholars. Pearsall, for example, states: "Chaucer had no copy of *The Canterbury Tales* made, and did not prepare the work for publication. Parts of it circulated in written copies, but it was not until after his death that the work began to be copied as a whole" (190).

¹⁰ Just as a name was erased in Hn. in Group I, line 1.

¹¹ It seems important to observe that Kase's groups are unrelated to Manly and Rickert's, since when he was writing their work was far from finished.

¹² The groups that Manly and Rickert produced based on tale-order are distributed as follows:

Group a: El Gg Dd En¹ Ds Cn Ma En³ Ad¹ Bo² Ad³ Ha⁵ Ad² Bo¹ Ph²

Group b: He Ne Cx¹ Tc² Ha³ Ln Py Ra³ Tc¹ Ma Ra1

Group c: Cp Sl² La

Group d: Lc Mg Ha² Sl¹ En² Bw Ry² Ld² Dl Ry¹ Fi Ii Ht Ra² Pw Mm Gl Ph³

Anomalous: Hg Ha⁴ Ch Ld¹ To Hk Ps Se Ni Cx²

¹³ "The MSS of group a have essentially the same arrangement as El, but it is textually clear that the group is not derived from the same immediate ancestor as El" (Manly and Rickert 2:480).

¹⁴ This figure depends on whether we choose to believe Manly and Rickert when they presume that certain leaves now missing in some manuscripts contained particular tales or links.

¹⁵ See table 1.

¹⁶ The groups were devised by Skeat, for his edition of the *Canterbury Tales*. The fragments are those used in the *Riverside Chaucer*.

¹⁷ Cf. Blake (*Canterbury Tales; Textual*), Pratt (1141-67) and the *Chaucer Variourum*, for example.

¹⁸ Among the editors who have followed the El order we find F. N. Robinson (*The Complete Works of Geoffrey Chaucer*) and Benson (*The Riverside Chaucer*). For more details about the use of El as copy-text see also Pearsall (234).

¹⁹ For example, Cp has a tale-order arrangement in chapters rather than as tale-link-tale. See Blake, *Textual*.

²⁰ Sandy Feinstein also supports this view: "I would take Blake's point a step further and argue that the effect of our narrative expectations, our need for verisimilitude in plot and structure, may blind us to different but nevertheless conventional, even common, forms of reading the past: reading aloud, reading to an audience, performance, recitation." (137).

²¹ It has been suggested by several scholars, including Manly and Rickert (*The Text of the Canterbury Tales*), Blake (*Textual, Canterbury Tales*) and Robinson ("Commentary," "Analysis"), that Hg is, at least, one stage removed from the origin of the textual tradition. In other words, not all the extant manuscripts

descend from Hg. Most of them descend, in one way or another, from a hypothetical manuscript which has been called O.

²² See, for example, Norman F. Blake, “Critics” and “Debate.” E. T. Donaldson, “Ordering.”

²³ Robinson has hypothesised that the α exemplar originated both E1 and the **a** and **b** groups. He has also suggested that this manuscript was very close to the one used by Caxton to correct his second edition of the *Canterbury Tales*. See Robinson, “A Stemmatic Analysis of the Fifteenth-Century Witnesses to the Wife of Bath's Prologue” and “Can we Trust the Hengwrt Manuscript?”

²⁴ The running order of Ad³: GP-KT-L1-MI-L2-RE L7-ML WB-L10-FR-L1-SU NU CL-L13-L15-ME-L17-SQ-L20-FK PH-L21-PD SH.....PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA L3-CO L-33-CY L37-PA.

²⁵ The encoding of the data was carried out by Matthew Spencer. This has been described in an unpublished article, “‘Gene Order’ Analysis Reveals the History of *The Canterbury Tales* manuscripts.”

²⁶ Currently, STEMMA has two articles on tale-order under that are being considered for publication –one in a scientific journal, the other in a computers and the humanities book. A third is being produced for a humanities journal.

²⁷ This is independent of the fact that he might or might not have been working on the book until the moment of his death.

CHAPTER II: A HISTORY OF THE STEMMATIC

APPROACH TO THE CRITICISM OF TEXTS

This chapter aims to present a brief chronological account of the development of the stemmatic method. It focuses on the traditional applications of the stemmatic method and the criticism put forward by its detractors, including Joseph Bédier. I try to explain how the dissemination of the stemmatic method through the Maas book, *Textual Criticism*, generated a series of misconceptions which seem to permeate criticism up to the 21st century. This chapter also attempts to show the flaws of traditional stemmatology, and put forward the solutions proposed by the New Stemmatology. Although the paths opened by this approach solve some problems presented by the stemmatic method, they also present some new challenges. Part of my discussion focuses on the current application of computer technology to the construction of stemmata and its benefits and shortcomings.

1. CLASSICAL STEMMATICS: THE LACHMANN METHOD

Although the stemmatic approach, sometimes called ‘historical editing,’ has traditionally been attributed to the German scholar Karl Lachmann, he was not the first one to suggest that there are genealogical relationships between manuscripts. In fact, Erasmus in 1508 and Scaliger in 1552, had argued that manuscripts could be shown to be genetically related (Cameron 231). The roots of this critical approach can be traced back

to Alexandria, where the classics were already being revised and stripped of passages that were considered spurious (Reynolds and Wilson 9 and ff.).

Lachmann presented his ideas about editing in several of his works, notably in his 1850 edition of Lucretius.¹ What he proposed was to group the manuscripts belonging to a particular tradition according to their errors. In this way, ‘families’ of manuscripts could be established and their relationships made clearer:

Lachmann’s crucial change was the separation of two phases in the preparation of the critical text: *recensio* and *emendatio*. Before establishing his text, he subjected all variant readings obtained through collation to a critical analysis and attempted to establish through a calculus of common and individual errors the different groups or families of manuscripts into which the tradition was divided, and the place which each manuscript occupied within the family to which it belonged, summing up his analysis with a so-called *stemma* that derived all extant manuscripts and families from a single archetype. (Kristeller 14)

Independently of the implementation of the two-phased schema, Lachmann’s ideas made such an impact that the genealogical approach he used came to be known as the Lachmann method. However, Lachmann did not write independent theoretical books about textual criticism or scholarly editing, a fact that has been pointed out by Ben Salemans:

Speaking of the method of Lachmann, we may think that this is a concrete text critical, stemma generating method which was clearly formulated by Lachmann in one of his books or articles. This interpretation is incorrect: Lachmann himself did not formulate a concrete ‘method of Lachmann’. So, THE method of Lachmann does not exist. Several text genealogists have worked out some fundamental ideas and they all call their results (extensions of) the method of Lachmann. Paul Maas may be considered as the one who ultimately formulated the method of Lachmann. (434)

What Salemans affirms seems reasonable. It is true that part of the traditional terminology related to the Lachmann method does not come directly from his ideas or of his work, and instead comes from Maas’ interpretation of them. Independently of who formulated these ideas, the method which uses common errors in manuscripts to determine if they are or are not related and that assumes that the process of understanding the textual tradition must be carried out before one can decide which variants are to enter the emended text is usually called the Lachmann method, and I refer to it by this name as scholars have done traditionally.

2. RE-THINKING THE LACHMANN METHOD

2.1 Henri Quentin: Considering All Variants

When Dom Henri Quentin started to think about an edition of the Vulgate, he discovered what seemed to be insurmountable defects in the Lachmann method. One of

Quentin's objections was very much a theoretical one. He was uneasy at the idea that the scholars who followed the Lachmann method claimed to be attempting to produce a text which was a reconstruction of the author's original rather than simply an archetype:

He (Quentin) insisted [...] that the processes of recension could result, if properly applied, only in the establishment of the archetype, and that to lose sight even for a moment of this fact was a grave editorial error. (1:16)

Quentin's criticism goes further to say that even the author's original had to have errors and, for this reason, errors could not be used to group manuscripts, since the errors could be archetypal.² What he did was to propose what he thought was a more scientific approach to textual problems:

Il renonce à la méthode des fautes communes. Dans le text de la Vulgate, les fautes sont sans cesse corrigées par le recours à d'autres exemplaires latins, au grec ou à l'hébreu elles ne se propagent pas de façon régulière de l'ancêtre au descendant; les fautes communes ne pouvaient donc être d'aucun secours. Dom Quentin renounce, dit-il, à la notion de faute, pour lui substituer celle de variante; à vrai dire, il ne fait que la repousser jusqu'à une étape ultérieure. (Froger 14-5)

The innovation of looking at 'variants' instead of 'errors' is one of the most important contributions that Quentin made, and this contribution has even been implemented in modified versions of the Lachmann method, in which all variants are taken into account.³

Quentin's observations are, in fact, related to the issues that have divided most textual critics during the last century, that is, the question of whether it is possible to recover the original text.⁴ The Lachmann method did not attempt to recover an authorial original, but instead proposed to reconstruct the archetype of the tradition.⁵ Quentin pointed out this unattainable goal (the recovery of an original text) as a basic error in the construction of the Lachmann method and from this point of view his contribution was of great value for the later development of stemmatology.

2.2 Joseph Bédier: 'Best Text' Editing

Probably the most famous detractor of the Lachmann method was Joseph Bédier, who produced an edition of *Lai de l'Ombre* for which he used this method. But at a later point, Bédier became suspicious of it:

C'est pour avoir remarqué ces choses que je rouvris un jour mon antique édition du *Lai de l'Ombre*: n'était-elle pas fondée elle aussi sur un classement de ce type? Le bel arbre bifide qui s'y dresse, je le regardai à nouveau, mais on imagine de quel regard désabusé, hostile. Il fallait reprendre le travail en sosoevre, préparer une autre édition. (14)

Once Bédier had discovered that many stemmata produced using the Lachmann method were initially bifid, i.e that most of them had their first division into two branches, he surveyed stemmata and decided to re-edit *Lai de l'Ombre*, and, in the process of doing

this he produced the now famous essay: “La tradition manuscrite du Lai de l’Ombre.” In this essay, Bédier explains how he came to be suspicious of the method and how, after confirming his suspicions he decided to revise his own ideas about how textual editing. About his new edition he states:

[L]e texte qui se lit en cette édition est celui d’un bon manuscrit, le manuscrit A, imprimé presque sans retouches et accompagné de notes qui marquent un retour vers la technique des anciens humanistes. (17)

It seemed that Bédier had come up with a different way of approaching scholarly editing, at least, it seemed so in the context of Lachmann’s influence. His proposal was that, instead of attempting to restore the text to its original form, editors should find the best witness to the text and use this, with the minimum amount of retouches, as the text of their editions. This came to be known as best-text editing, but in fact, Bédier does not refer to the ‘best’ text, he just says that the manuscript he has chosen is a ‘good’ one. Of course, the statement about the good manuscript begs the question: how does the editor get to know which one is a ‘good’ manuscript? Bédier explains that one has to take into consideration the variants of the text, he also insists on using neutral terms and referring to the ‘diverse forms of the tradition.’ But even if Bédier’s theories seem to disagree completely with the Lachmann method, he still attempts to define the genetic relationships among the manuscripts. The fact that the two methods, which differ in so many other fundamental aspects, appear to have a common goal appears puzzling at first, but this is what allows for the differences between the two approaches. ‘Best-text’ editing relies on the identification of the best extant witness of a given text, but in order to tell

with any degree of certainty which one is likely to be the best manuscript, one has first to evaluate the textual tradition and the relationships between the witnesses. Yet the difference in approach seems to be deeper than what it would seem at first sight. The questions that Bédier's ideas generate touch on many more general aspects of textual criticism, such as which is the text an editor should aim for and how far he should go when attempting to establish the text. Once more, the choice is not about what method is used to study the history of the text; instead, the debate is about how much the edited text should be altered in relation to the documents that preserve its versions. If the text is based on the best manuscript available and altered as little as possible, we are more likely to be in the presence of a 'Bédierist' approach. If the text is altered, by emending it (usually in order to recover a previous state of the text) and the relationships between the witnesses are greatly emphasized and are one of the prime pieces of data used to make decisions about how the text should be emended, we are probably facing a 'Lachmannian' edition.

2.3 The Influence of the Anglo-American School

Best-text editing became popular in France and Spain where it was used for many medieval texts, but the Italians and the Germans kept analysing and rethinking Lachmann's ideas and the derivations of his method.⁶ It is difficult to pinpoint the exact reason why best-text editing in France and Spain became so popular for so long and perhaps a more appropriate question would be for the editing of which texts. For

example, the case of Spain is interesting, because many medieval texts survive in a few copies only. Some of them, such as the *Poem of the Cid* survive in a single manuscript, a fact that has not prevented the text from being edited very differently by different scholars.⁷ The Anglo-American school, on the other hand, was pursuing authorial intention as its final goal and therefore could not make very much use of the 'best-text' approach which would have limited the scope of the emendation in an edition (at least in theory). When a scholar of the Anglo-American school edited a text without altering it greatly, this was referred to as 'documentary editing.'⁸ It seems important to point out that although there are many documentary editions, notoriously many of the ones produced by the Early English Text Society, these are not necessarily considered critical editions. Tanselle strictly differentiates both kinds of editions:

An edition is either a *noncritical edition*, which aims to reproducing a given earlier text as exactly as possible, or a *critical edition*, which contains a text resulting from the editor's informed judgement at points where questionable readings occur--a text that may therefore incorporate readings drawn from various other texts or supplied by the editor. (Tanselle, "Scholarship" 32-3)⁹

Following the pattern of the above quotation, a Lachmannian edition is a kind of critical edition in the sense that this permits the emendation of the text based on editorial judgement. In the light of the above quotation, a Lachmannian edition differs from other kinds of critical editions in which editors rely solely on their judgement to decide which variants will go into the reading text.¹⁰ The intentionalist school (to a greater or lesser degree derived from Greg's work) asserts that the intentions of the author are recoverable

through a process that requires knowledge of the genesis of the text and all its relevant witnesses as well as familiarity with the author in question. According to Tanselle, the work of an author is only possible in a non-material environment (the author's mind) since material objects have accidents:

My phrase "the intractability of the physical," which McGann takes as a sign of my "romantic" position, has--in its context in *A Rationale of Textual Criticism*--a less exalted meaning. It simply refers to the difficulty of getting words transferred accurately to a physical surface. Authors do formulate texts, not just ideas for texts, at the moment of composition; but they may make mistakes in writing down the words. An authorially intended text is a text that once existed, though it may not have existed in physical form. Such a situation can occur because language is intangible, and a verbal text can therefore exist apart from being made physical. ("Scholarship" 12)

Tanselle supports the idea that even the author of a text can have "slips of the pen" introduced by the author himself (a position that cannot be denied in a general way). The intentionalist school (of which Tanselle is a recognized follower) proposes that if a text contains such authorial "slips of the pen," these ought to be corrected by the editor. Documentary editions, on the other hand, are concerned with the preservation of the document and not with recovering authorial intention.¹¹ The influence of Greg, Bowers and Tanselle on Anglo-American editing was so widespread during the 20th century that other kinds of edition have been somewhat overshadowed.

2.4 Paul Maas: Describing the Lachmann Method

As stated before, Lachmann did not produce any theoretical description of his method. Instead, this was comprehensively described by Paul Maas in his essay *Textual Criticism*. Maas was interested in the ‘scientific’ aspect of the method, or at least its apparent objectivity when compared with other approaches:

Previously the principle was to follow the vulgate (*textus receptus*) without troubling about the quality of the witnesses; or to follow the text of the majority of the witnesses, in spite of the fact that 100 manuscripts which derive from a single manuscript and have no more authority than one manuscript which does not go back to that single manuscript; or to follow the oldest, the most complete, the best witness, just as if *every* scribe were not liable to error. This was all completely arbitrary, and there was never any attempt made at a methodical justification. The mistake of treating the *codex optimus* as if it were the *codex unicus* has not been completely overcome even today; it is often set right by the *codex optimus* finally revealing itself as the *codex unicus*. (19)

There is a clear point about the fact that editors need to make choices and that this might be helped by the use of a method that allows us to learn the origins of the variants. In this case, the variants can be selected according to the likelihood of their presence in the archetype.

Maas questions the lack of objectivity shown by other scholars when choosing a single manuscript as a base for an edition. His criticism has to do with the fact that there are mistakes in every witness of the text that should be corrected. Moreover, Maas thought that the errors found in the witnesses of a tradition were the key to discovery of the relationships between them. There is a very subtle point here, since if we accept that errors, and only errors, can be used to group the witnesses of a text, by definition, the archetype we presuppose should be free of them. If the archetype contained errors, then these could not be used to group the witnesses together since some errors could be indeed archetypal readings (in which case they would not be able to tell us anything about the genetic relationships among the witnesses). Another implication is that the original state of the text, which could not have been physical since material things are accidental, is, at least, approximable. But whatever underlies Maas' book, his description of the Lachmann method is thorough and minute. Step by step he guides the reader into the most elaborate descriptions and examples of problems generated while dealing with a textual tradition. Maas' influence can be seen in the extremely high number of references to his work made by other scholars working in the area. The other side of the coin is that someone who can produce such an influential text also produces visceral counter reactions.

2.5 Reactions against Maas' Lachmann Method

The strongest argument against Maas' approach was put forward by Giorgio Pasquali in his book *Storia della tradizione e critica del testo*. It seems that Pasquali

disagreed with Maas from the very beginning, and mostly he thought that Maas had not been very accurate in attributing the invention of terms and techniques. When one faces Pasquali's book there is no mistake: one immediately realizes that he has studied in detail the totality of Lachmann's work and of that of his followers. Pasquali describes each of the steps of the creation of the Lachmann method, and makes clear which terms have to be attributed to whom. He points out that we owe the notion of 'lectio difficilior' to J. J. Griesbach, but that it was Quentin who noticed the importance of this idea (See Pasquali). In this way, Pasquali demystifies the idea of a unitary methodology developed by Lachmann for his edition. Once he has done this, he moves on to point out what seem to him the weaker aspects of Lachmann's own ideas:

Il Lachmann fondava il suo metodo sul presupposto che la tradizione di ogni autore risalisse sempre e in ogni caso a un unico esemplare già sfigurato di errori e lacune, quello ch'egli chiamava archetipo. (15)

As many others after him, Pasquali has doubts about those who postulate a single archetype. It is not clear, though, how it is that you could have more than one origin to the tradition. In fact, when, for example, a scribe re-writes a text and interpolates passages from a different one, he creates what is known as a 'new recension' of the text. This modified text usually becomes the origin of its own stemma. By definition, this new recension would be the result of the conflation of two or more texts, and its origin would be, at least in part, in all of the conflated texts. In theory, this supposes a textual tradition with more than one archetype. But if we take this further and suggest that the whole of the textual tradition descended from the scribal conflated version, we would have the

choice of where to place the beginning of it.¹² This is an interesting theoretical problem, because one would have to decide if the tradition indeed starts with the two authorial versions or if the texts descended from the conflated scribal text represent a new recension. In theory, given enough data, it should be possible to build a stemma of more than one recension of a text. However, a further problem arises with reference to the different authorial texts, where these are independently produced and bear no relationship to one another or where one is a revision of the other text. In the case in which a revised version of a text has been released, the ultimate origin of the textual tradition would be the first authorial text and not its revision (as it seems to be the case of L31, which has two different versions one of which must be earlier than the other). If the author re-writes the text in such a way as to make it almost impossible to see the relationship between the two versions, these versions, of a text could be transmitted independently of each other, therefore having distinct textual traditions (an example of this is Langland's *Piers Plowman*, which appears to have been released in three or more authorial versions).¹³

Although Maas has to be credited with establishing some of the language that stemmatologists still use today, the method he exposed is still the target of the criticism of literary critics who are not aware of the developments in this field. An example of this is the statement by Ralph Hanna:

To construct a stemma in order to carry on "scientific editing," the researcher must be able to recognize at least some range of "palpable errors," for in stemmatic theory only agreement in such corruption can demonstrate that any two manuscripts share a common corrupt exemplar. However, the term

palpable here represents an equivocation, since it relies upon a judgement: wise persons may well disagree on the “palpability” of any single corruption, and consequently, the scientific factuality of the stemmatic evidence is engendered by having identified specific readings as erroneous. But this necessary move simultaneously indicates that, however contentiously, one might believe oneself able to identify certain readings as “errors.” (*Pursuing* 85)

Hanna’s criticism is valid if applied to the Lachmann method as described by Maas, but it does not apply to the latest developments in the field. In fact, the use of ‘errors’ as the sole element to establish the genetic relationships of a text is a characteristic of the Lachmann method. The New Stemmatics differs from it (among other things) in the fact that it takes into account variant distribution, i.e. the agreements between manuscripts independently of the fact that these might or might not be mistakes. The argument against stemmatics developed by Kane and Donaldson suggests that there is no use for the methods since this might require knowledge *a priori* of the nature of the variants in the text. Both Kane and Donaldson have stated their criticism together and independently. For example, in his assessment of Manly and Rickert’s work, Kane states:

[N]ot all agreements in unoriginal readings are necessarily evidence of a genetic relation. The editor is again thrown back upon his judgement: to classify the manuscripts he must somehow distinguish between genetic and random variational groups, identify the evidentially valid agreements. (“John” 209)

This argument suggests that the reliance on editorial judgement invalidates the use of stemmatic analysis (since this becomes unnecessary). Donaldson takes the point further while discussing variant wight/ wrighte in WBP 117 (D 117), where he suggests that manuscripts should not be considered wrong in all readings just because most of them are non-archetypal:

[B]ut I will argue for the right and the responsibility of an editor who is trying to reconstruct Chaucer's text --not merely O¹-- to let all MSS help him, not just the respectable ones. (*Speaking* 128)

Donaldson's point is unconvincing because it mainly relies on authorial intention. He presupposes that Chaucer originally intended the reading 'wrighte,' the only one that makes sense, but to justify his intervention in the text he relies on the fact that the reading is in three manuscripts: Ld2 Ry2 and Ln. Interestingly enough, it does not seem that this procedure is the standard he would follow; that is, even if what he believes to be the correct reading was not present in any manuscript, he might still decide to introduce an emendation into his edited text.¹⁴

2.6 Steps towards a Revision of the Genetic Method

For their eight-volume work *The Text of the Canterbury Tales*, Manly and Rickert used a revised version of the Lachmann method, a genetic approach that radically differed from the one described by Maas. Manly and Rickert referred to the fact that

manuscripts could be grouped “according to their readings without reference to whether the readings are correct or incorrect” (1:20). This truly represents a step in the evolution of stemmatology; in a quite evident fashion Manly and Rickert implied that the text that originated the tradition might have contained mistakes. Errors were not the only way to determine the affiliations of a manuscript. Based on this idea, Manly and Rickert conceived the concepts of genetic group, accidental group (agreement by coincidence) and group-variant. Another of the characteristics of their work is that they attempted to establish first the smaller genetic groups and go from there to the larger ones. Clearly they propose that Chaucer’s original (O^1) was impossible to recover, but that the archetype of the textual tradition (O^2) could be reconstructed (Manly and Rickert 1:40-1). But even after the enormous work carried out by Manly and Rickert, few critics realized that what they had actually suggested might change the way scholars view stemmatics.¹⁵

In fact, despite the developments in the field, still in 1973, Martin West insisted on reinforcing the idea that the purpose of stemmatic analysis was to restore the text as the author originally conceived it:

When the evidence of the various sources for the text has been collected and organized, apographa eliminated, hyparchetypes and archetypes reconstructed where possible, and so on, the time has come to try to establish what the author originally wrote. Sometimes this is a matter of choosing between transmitted variants, sometimes it is a matter of going beyond them and emending the text by conjecture, or adopting an emendation already

proposed. We will consider these alternatives separately; but the requirements which a satisfactory solution must fulfil are the same in both cases. (47-48)

This description, in fact, goes back to the Maas conception of the method, in which by using the errors in the witnesses one could, somehow, make deductions as to the text as originally written by its author. Once more, Quentin's criticism becomes valid, since it is inconceivable that the author could have produced a perfect text completely free of mistakes (since the work would have to be held in a material document liable to the accidents of the physical world).¹⁶ Therefore, any emendation based on errors may, in fact, change what the author wrote rather than restoring what was in the original text. This takes us to another theoretical problem: if the archetype of a textual tradition had mistakes and these are corrected by an editor, the re-constructed text would not be the origin of such tradition; instead, it would be some other text. From this we must assume that West's pursuit is not really to re-construct the archetype of a textual tradition, but instead, is to produce a text as it might have been conceived by the mind of the author with no relation to any of the physical copies of the text which could have been produced.¹⁷

At the same time that critics such as West were reinforcing a perception of stemmatics drawn from concepts presented by Maas, other scholars had already been influenced by what Manly and Rickert had suggested as a new perspective for the use of stemmatic analysis. Peter Robinson was one of them.

2.7 Modern Genetic Methods: The New Stemmatology

At the end of the 20th century, the advances in computer technology made it possible to use it to analyse texts. Software especially designed for studying texts was developed, and soon other programs (written for other disciplines) were incorporated into the equation.

In his 1991 DPhil thesis, Robinson criticizes the inflexibility of the Lachmann method as exposed by Maas and suggests that the way forward is to follow Manly and Rickert's example and use all variants and not only errors "as potential evidence of MS relations" (Robinson 153). This was a first step in the next big change for the New Stemmatology, and on July 31st 1991, Robinson launched the "Textual Criticism Challenge" which was published in Humanist Discussion Group. The challenge consisted in constructing "by Housman's 'mathematical principles' alone, and not using any external evidence, a table of relationships of the manuscripts (a 'stemma') like that I [Robinson] have already made [for the *Svigragsmál* data]." Scholars could use "any method, any computer, any software" that would help to build a stemma comparable to the one that Robinson had produced for the Norse tradition.

2.7.1 Using Phylogenetic Software with Manuscript Traditions

Later, Robinson, with Robert O'Hara, presented a series of papers based on the Challenge.¹⁸ A version of the "Report on Textual Criticism Challenge 1991" has been

published on the internet. In it we find a more or less detailed account of what happened after the posting of the “Textual Criticism Challenge.” We read that “nine scholars requested the data” and that there were “three submitted entries.” Two of those were based on statistical clustering techniques, the third was Robert O’Hara’s attempt, for which he used PAUP. O’Hara’s result is described as achieving the following successes:

- (1) It placed directly adjacent to one another (usually as descendants from the same node) sixteen manuscripts known from external evidence to be directly related to one another.
- (2) It successfully defined the seven manuscript groups deduced by Robinson within the tradition.
- (3) It successfully defined two of these groups as subgroups of another, larger group.
- (4) It suggested, accurately, that the two largest groups were each descendants of single manuscripts, and that a third group also descended from one of these two manuscripts.
- (5) It provided lists of just what variants were introduced at what point in the tradition. These agreed reasonably closely with Robinson’s own lists of variants, derived by database analysis of the collation output, characteristic of particular groups of manuscripts. (“Report”)

The phylogenetic proposal was, by far, the closest to what a hand-made stemmata had produced. It seems that the way in which programs such as PAUP work has many things

in common with the stemmatic approach. In fact, there are clear parallels between the phylogenetic approach and stemmatics.

Cladistic analysis . . . is an explicitly historical approach that aims at reconstructing sequences of events, and fundamental to the cladistic approach is the identification of ancestral readings and their elimination at every point. . . . A further reason for the success of cladistics is that it works explicitly on the tree model. It assumes that a varied group of objects (whether manuscripts or species) is the result of a sequence of branching descents over time. Cladistics simply finds the shortest (or “most parsimonious”) tree of descent which explains the agreements and disagreements within this group. The overall similarity or dissimilarity of the objects under study, so important in statistical clustering, is unimportant in cladistics. (“Report”)

O’Hara’s idea of using PAUP to build stemmata was quite successful; however, the program showed some problems when dealing with contamination and coincident variation. In this sense, PAUP has similar limitations to those that have affected scholars in the past. The advantage that a computerized method for dealing with manuscript descent presents is that once all the variants have been isolated it takes a relatively short amount of time to get the results.

2.7.2 Ben Salemans: The Neo-Lachmannian Approach

Another contributor to the New Stemmatics is Ben Salemans,¹⁹ whose doctoral thesis, *Building Stemmas with the Computer in a Cladistic, Neo-Lachmannian, Way*, is an example of the application of computer software to the making of stemmata:

Bédier found out that the history of the text deliverance of the Lai de l'Ombre can be sketched [*sic*] in eleven differently shaped stemmas ("schémas"). It was for Bédier impossible to pick out the one correct stemma. Therefore he concluded that the value of text genealogy, resulting into eleven different models of textual deliverance of the Lai de l'Ombre, is rather poor. (454)

When faced with many stemmata, one might feel puzzled at the fact that all of them can be arrived at using the same data. Salemans, by abstracting information from the stemmata produced by Bédier has been able to show that he was incorrect in being concerned about the differences between them:

Bédier's problem is not as complex and disastrous [*sic*] for text genealogical methods as he surmises: the eleven stemmas with different shapes go back to one and the same chain. The only problem left is the orientation of the Lai de l'Ombre chain. . . . (*Stemmas* 456)

The problem left (the orientation of the chain) is not an easy one to solve, but at least Salemans has shown that all of Bédier's stemmata show the same relationships as those that appear in Salemans' chain. Salemans, as others before him, has also attacked the Lachmann method on the premise that it relies on errors in a way in which they cannot be

verified and validated. The other great criticism agrees with that of Quentin: there is no clear way to determine which variants are errors and, therefore, the approach is ‘unscientific’ and subjective (Salemans, “Cladistics” 5). Salemans is not just criticising the approach; he is also offering a solution:

Most twentieth-century [stemmatic] methods have in common that they first determine the shape or deep-structure of the stemma (the chain) and that they then raise (orient) a stemma from this chain. For the construction of the chain no knowledge about the originality of variants is necessary. (“Cladistics” 22)

He offers the alternative to the stemma (rooted tree): the chain or unrooted tree. Unrooted trees had also been put forward by Robinson in *The General Prologue in CD-ROM*, but he did not elaborate as much as Salemans on the idea of the chain and its advantages. Salemans also makes a good point (often forgotten by critics of the stemmatic method) about the role and meaning of stemmata:

One should bear in mind that a stemma is a *minimal* picture relating only to the text versions that still exist. Thus, a stemma can only be considered as a hypothesis about (a part of) the historic reality. On and around the lines of descent, we can imagine lost manuscripts whose contents are unknown. (“Cladistics” 14)

Although this statement seems to be relatively straight forward, it is quite clear that many scholars have taken stemmata to be ‘reality’ rather than a representation of it. Stemmata are constructs that have the function of helping to explain the status of a particular textual tradition; they do not have to be taken as a synthesis of actual history or a parallel of the

real world. In an ideal situation a stemma aims to produce a representation that parallels a textual tradition. In reality, a textual tradition is incomplete because historically we have lost some of the witnesses of the text; thus a stemma (whether hand or computer made) can only represent relationships of witnesses which we still have. It is important to remember that, at least in this sense, the stemma can only present a partial representation of a historical phenomenon.

Salemans makes detailed studies of the way in which stemmata can be built using phylogenetic software, but his work does not go beyond this. He concentrates on the *recensio* of the text, not on the *emendatio*. The reason for this is that while the construction of stemmata can be carried out, with the help of computers, in a quite objective way, the emendation of the text still requires the use of editorial judgement. The subjectivity of editorial judgement has deterred scholars from following this path, which might be, at least in part, one of the reasons why best-text editing has been so popular for so long.

2.7.3 Robinson and the STEMMA Project

O'Hara and Robinson had shown that phylogenetic software could help with the construction of stemmata and that it provided an accurate way of dealing with these, so the next natural step was for someone to take this further and study why and how the programs work: this is the role of STEMMA. The STEMMA Project is a collaborative project in which textual scholars are working with molecular biologists to clarify and test

issues that have arisen from the application of phylogenetic programs to the study of transmission of texts. The group of scholars in STEMMA have set themselves to test five considerations:

- 1- Phylogenetic analysis should be applicable to manuscripts other than those already tested by Robinson, i.e. the Norse *Svigragsmál* tradition and the *Canterbury Tales* manuscripts for the Wife of Bath's Prologue.
- 2- "Different methods of evolutionary inference should give consistent results", i.e. even if the approach used by the programs is different the results produced by them should not be in conflict.²⁰
- 3- "The tree structure should be independent of the characters used."²¹ Different types of variants should have the same 'weight' when a tree is produced.
- 4- "There might be useful information in the order of sections in the manuscripts."
- 5- "New programmes may allow us to carry out more sophisticated analysis."

If these goals are achieved, the phylogenetic methods could become more widely accepted and used. In fact, point 4 was specifically created with the *Canterbury Tales* in mind. The idea was to test if genetic reconstruction could be carried out using the order of the sections as data. The STEMMA Project has been running for almost three years and, up to this date, it has explored most of the aspects outlined above. As part of the work that has been produced, the tale-order data has been coded by Matthew Spencer²² and has been fed into the programs. It is part of my work in this research, to try to explain how well it has worked and why, as well as to make clear when the codicological evidence supports the stemmata that we now have.

It seems that there have been some remarkable changes in the way in which scholars now approach genetic studies of texts. The New Stemmatics uses sophisticated software which has either been originally designed for evolutionary biology (such as *PAUP* and *SplitsTree*) or which has been specifically written for use with texts (such as Wattel's programs²³). Besides the use of computers, which has widely spread in most areas of knowledge, one of the main changes in stemmatology is the fact that now all variants are taken into account, as opposed to errors being the only significant type of variant.²⁴ Also, thanks to the role of the computer there is no need to make a supposition *a priori* about the place at which to root the stemma. The New Stemmatics has also changed the focus of the edited texts: these are not attempts to reconstruct lost archetypes or texts that reflect authorial intentions. The New Stemmatics proposes to 'construct' a text which would help the reader to understand better the textual tradition.

With all the advantages brought by this new method, there are, however, issues that remain difficult and require further study. An example of this is the problem of contamination, which has not been satisfactorily solved with the computer programs we use. When dealing with contaminated traditions, we still have the need of manual analysis to clarify relationships between the witnesses of a text. Another problem arises when we have to make the decision about which variants are likely to be archetypal, but this is partially solved by the fact that we do not need to establish the nature of the variants before we actually produce a tree. It seems that the advantages presented by the New Stemmatics are considerable and that it has overcome most of the problems that scholars have pointed out when referring to the Lachmann method. If some of these have

not yet been completely solved, it is just because the methods that the New Stemmatics has implemented are still being tried and developed.

¹ For details of the theories presented by Lachmann, see *Kleinere Schriften*.

² The consideration that there are errors that could have been present in the archetype could suggest that these might have been authorial (if the archetype was a holograph copy) or scribal (if the copy which originated all the others was copied by a scribe). In any case, what is important is not who generated the errors, but the consideration that there can be errors in the archetype of the tradition, in which case, a reconstructed text might have to account for them.

³ However, probably Griesbach was the first one to suggest this. See my discussion later in this chapter.

⁴ What is meant by 'original' text varies from one approach to another. See below for a description of the different methods proposed by various editors.

⁵ It could be argued that it is conceivable that the archetype of the tradition was the latest version of a text by its author. Because the archetype could be one with the author's version, at least in theory, the Lachmann method could show similarities with the intentionalist school of editing.

⁶ For a history of the Lachmann method see Timpanaro, *La Genesi del Metodo del Lachmann*.

⁷ See Ramón Menéndez Pidal (*Cantar de mio Cid*) and Colin Smith (*Poema de mio Cid*).

⁸ It seems important to point out that although there are many documentary editions, notoriously many of the ones produced by the Early English Text Society, these are not necessarily considered critical editions. "An edition is either a *noncritical edition*, which aims to reproducing a given earlier text as exactly as possible, or a *critical edition*, which contains a text resulting from the editor's informed judgement at points where questionable readings occur--a text that may therefore incorporate readings drawn from various other texts or supplied by the editor" (Tanselle, "Scholarship" 32-3).

⁹ For more details about the differences between critical editions and non-critical editions see Tanselle "Scholarship."

¹⁰ An example of such editorial position can be found in Kane and Donaldson's edition of *Piers Plowman*.

¹¹ 'Best-text' editing (described below, in this chapter) often offers a text that is as close as possible to its manuscript source, frequently advocating minimal emendation. See the introduction to Mitchell and Robinson's edition of *Beowulf*.

¹² The theoretical possibility exists of a scribe (or even the author himself) merging two or more recensions of the same text, creating a point of union between them and a new start for a different recension. Each of the recensions has its own archetype, at least from a theoretical perspective, an independent tree for each of the recensions can be built.

¹³ It is also conceivable that two different recensions of a text might be conflated by a scribe, generating a third recension.

¹⁴ See Kane and Donaldson, *Piers Plowman: The B Version* and Kane, *Piers Plowman: the A version*.

¹⁵ Kane heavily criticizes Manly and Rickert's method in the essay in which he reviews their work ("John" 207-29).

¹⁶ A fact also accepted by Tanselle. See above quotations.

¹⁷ "Detecting erroneous readings at points where there are no variants is one of the primary responsibilities of the critical editor, but it has not been written about as much as other editorial duties because it is not amenable to theorizing or systematizing. Some corrections of this kind are obvious, as when they rectify mere slips of the pen or typographical errors. Others, however, come only after repeated close readings of the text and long familiarity with the author, for the errors they correct are by no means immediately apparent. An error that makes a kind of sense and that requires concentrated attention for its discovery may remain undetected for years or through a succession of editions. Once it is pointed out, readers often wonder why they had not noticed it before; similarly, the correct reading, when the editor hits upon it,

seems so natural and so clearly right that it scarcely leaves room for doubt. An emendation of this sort is the product of imagination and insight and therefore does not occur with great frequency; locating subtle errors, difficult as it is, often is easier than knowing what to replace the errors with." (Tanselle 37-8).

¹⁸ Robinson and O'Hara were not the first scholars to try computer assisted stemmatic analysis. Clustering techniques have been used since John Griffith applied them to variants in Juvenal manuscripts (101-38), and other scholars have analysed and assessed phylogenetic methods Cameron ("227-42), Platnick and Cameron (380-5).

¹⁹ Although some of Salemans ideas are compatible with those of the New Stemmatics, he stops at the application of computerized tools to the study of textual traditions and the theoretical classification of variants, that is, he does not go further to produce an edited text (see below).

²⁰ In chapter IV I will discuss the different approaches used in the phylogenetic software.

²¹ Here the word 'character' is used in reference to the data utilized after it has been coded. The term does not refer to letters, but to the coded version of a place of variation, that is, a set of parallel variants occurring in various witnesses.

²² Details on the coding of the data can be found in Spencer et al., "'Gene Order' Analysis Reveals the History of *The Canterbury Tales* manuscripts"

²³ See E. Wattel, 311.

²⁴ There are still scholars who sustain views which could be related to Maas' interpretation of the Lachmann method: "Zoologists really are most interested in the end-points of their trees, the individual taxa. They are interested in how real animals are related and the course of evolution that got them there. The more end-points, the more successful the analysis. They are excited about apomorphies. The textual critic is not really interested in the end-points of the tree, that is, the specific manuscripts. Indeed, he tries to eliminate as many as possible. His only interest is in reconstructing the archetypes as a step in reaching the author's original text" (Cameron 239). Clearly, Cameron's views are based on traditional perceptions. Now, his ideas present an interesting contrast with the proposals of the New Stemmatics, since this advocates, not the reconstruction of a lost 'original,' but the construction of a text that can help explain the extant witnesses.

CHAPTER III: STEMMATIC ANALYSIS AND TALE ORDER

This chapter explores the application of stemmatics to the differences in tale-order in the manuscripts of the *Canterbury Tales*, while at the same time examining the validity of this approach and evaluating the possible advantages of its application to an analysis of tale order. Manly and Rickert base one of their manuscript groupings on tale-order, but this approach has been questioned since their genetic groups and tale-order groups are not in complete agreement. This inconsistency, however, could be interpreted as an the result of changes purposely introduced into the order of some manuscripts. In this chapter, I first discuss some of the issues related to Manly and Rickert's groups in order to show the fundamental problem of their work: the inconsistency between their acceptance of the idea of prior circulation of the *Tales* and their attempt to present a single stemma of the textual tradition of the poem. Later, I compare the groups proposed by Manly and Rickert with those suggested by Robinson based on data from WBP and GP.

1. MANLY AND RICKERT'S ASSESSMENT OF THE TALE-ORDER PROBLEM

The work carried out by Manly and Rickert on the textual tradition of the *Canterbury Tales* raises two points which are critical for my analysis: the question of prior circulation of the *Tales* and the lack of coincidence between the tale-order and the word-variant groups. A close examination of these issues would help to determine if the

stemmatic approach is or is not likely to be useful to an analysis of the tale-order problems.

1.1 The Word-Variant Manuscript Groups

After the completion of the manual collation of the witnesses of the text of the *Canterbury Tales*, Manly and Rickert grouped the manuscripts according to the variants they had isolated in their research. The result is the now well-known groups that are commonly accepted in studies of the *Tales*. These word-variant groups are as follows:

Group a: Cn Dd En1 Ds Ma

Group b: He Ne Cx1 Tc2

Group c: Cp La Sl2

Group d: En2 Ll1 Lc Mg Pw Mm Ph3 Ry2 Ld2 Dl Ha2 Sl1

Manly and Rickert state that, textually, some manuscripts do not belong to any of these groups, and instead form independent pairs. These are Ad3 and Ha5, Bo1 and Ph2, En3 and Ad1, Mc and Ra1, Ps and Ha1, and Ra2 and Ht. They also suggest, referring to their analysis of GP, that:

Of the 49 MSS [in which GP is included], all but six—Hg, Ch, El, Gg, Do, To—are derived from the same common ancestor. Their exact relationships are obscured by the loss of intervening exemplars, by supply of lost leaves, and by much independent editing and contamination. (2:78)

Manly and Rickert's conception of the development of the text of the *Canterbury Tales* is clearly defined, at least for GP. They postulate a single common ancestor from which two copies were made. In this conception, some manuscripts descend from O^1 , while the six mentioned in the above quotation descend from O^2 .¹ In this case, it is less clear how Manly and Rickert conceived the relationships among the six manuscripts descended from O^2 . They also suggest that the **a** group has a single common hyparchetype, and that manuscripts belonging to the **b** group descend from another hyparchetype. But a stemma based on Manly and Rickert's conception of the textual tradition of the *Tales* is so complex to draw that they present only partial stemmata, which explain only part of their conception of the development of the text. For example, for GP they propose the following stemma, which shows some of the relationships between some manuscripts and the **b** group (2:79):

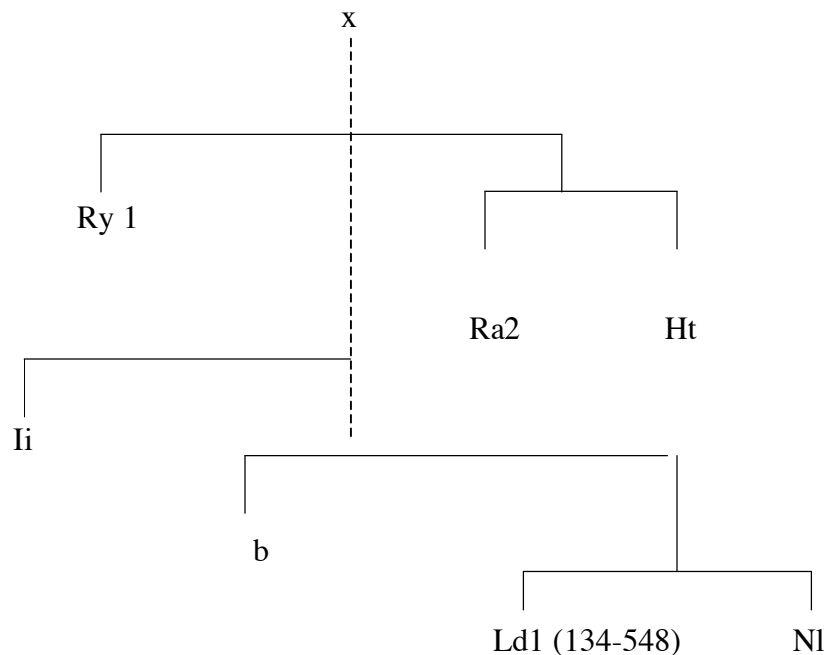


Figure 1. A partial stemma of the **b** group, from Manly and Rickert

It is interesting to notice that partial stemmata are drawn, while very complex ones are absent. One might think that by putting together a series of partial stemmata a full stemma would result, at least for part of the text of the *Canterbury Tales*. However, the problems of contamination and conflation do not allow the easy production of stemmata. In addition to these problems, one of the most serious questions stemmatologists face is to make it clear that there is a difference between reality and a stemma that attempts to explain it. The importance of emphasizing this point could be underestimated, but any such statement is absent from Manly and Rickert's writing. The difficulty they experienced in producing a single stemma of the textual tradition of the *Canterbury Tales* may have been that they thought they were drawing a tree which was very close to the historical reality of the *Tales*.

In general, stemmata may possibly blur or even erase details of the relationships among different texts. There are several reasons for this: for example, contamination and changes of exemplar (Manly and Rickert 2:24 and ff). On the one hand, an attempt to represent all the complexities of a textual tradition in a single stemma does not seem wise or even possible. On the other, the alternative is to produce a single comprehensive stemma but without losing sight that this might be useful in showing where the problems are and which areas of a tradition need to be explored further. Less complex stemmata can be produced to describe specific parts of the textual tradition. In this way we ensure that we have the best of both worlds: a general indicator of the position of the different

manuscripts in the textual tradition and a more detailed diagram that shows the relationships between closely related texts.

1.2 The Independent Circulation Hypothesis

The idea of the independent circulation of some of the tales cannot be attributed to Manly and Rickert, even though they seem to support it at times.² When Hammond wrote her article about the order of Caxton's editions of the *Tales* it appears that scholars commonly accepted the idea of the tales having circulated independently before the *Canterbury Tales* was put together (Hammond 159-78). Subsequently, scholars have also accepted this hypothesis. Owen, for example, writes: "What happened at Chaucer's death, if not the postulated copying of exemplars? Some of the *Canterbury Tales* were already in limited circulation" (Owen 106). Manly and Rickert are slightly more reserved about the idea, suggesting that their research might finally provide positive evidence of the tales having circulated either independently or only as part of the work as a whole.³ This cautious attitude did not give rise to a clear answer. Manly and Rickert do not seem to have ever reached any firm conclusions on the subject; or, if they did, this is not clearly stated in their work. If the tales circulated independently before they became part of the *Canterbury Tales*, then each tale would have at least two different textual traditions: one where the archetype would be the first version of a tale, circulating on its own; and the second where the archetype is the copy containing all the *Tales* collectively (O).

Conceivably, Manly and Rickert's ideas about how the *Canterbury Tales* developed might have been influenced by their attitude concerning the early circulation of the text. Manly and Rickert provide the more localized kind of stemma (relating to a single group or the relationship to two different groups), but do not produce these for the whole text; instead, they give partial stemmata based on data from a single tale. An examination of Manly and Rickert's ideas suggests that their decision not to offer a complete stemma may have been due to an undeclared conviction that some tales circulated independently before Chaucer's death:

That at Chaucer's death more than one copy of some of the tales –copies differing slightly in wording and in contents– may have been in the hands of some of his friends seems not improbable. It will generally be admitted that the story of "Palamon and Arcite" would not have been mentioned in the Prologue to the "Legend of Good Women" unless the poem were known to the readers to whom the Prologue was addressed. The exact nature of this early version may admit of doubt, but that it was an early draft of KtT can hardly be questioned. For similar reasons the existence and circulation of SNT under the title of the "Life of St. Cecilia" also admits of no doubt. But the tales in circulation can certainly not be limited to these. Chaucer himself in his "Envoy to Bukton" assumes Bukton's familiarity with WBP, and we can hardly go wrong if we maintain that Bukton possessed a copy of this bit, if not the whole fragment D. (2:36)

From a stemmatic perspective, the argument of prior circulation of individual tales has two main problems.⁴ The first is that if individual copies of particular tales circulated before the publication of the *Canterbury Tales*, then the tales would present at least two distinct and independent lines of descent: one that would be the one originated by the prior circulation of a tale, and the second that started when the *Tales* were put together and published as a book. The prior circulation argument gives rise to complications since it could also imply the possibility of authorial revision of the texts: if Chaucer released some of the tales independently, he could have adapted them to make them more suitable for their assigned teller once he had decided who this should be.⁵ If, however, the *Canterbury Tales* was published only after Chaucer's death, it would be possible that he should have had more than one version of more than one tale or link, this becomes more unlikely. The real conflict arises when we examine both Manly and Rickert's ideas at the same time. Although it is possible to reconcile the concept of a single archetype for the whole of the textual tradition with the argument of prior circulation (this presupposes some of the tales could display different textual relationships), it would not be possible to suggest that at the same time there is a single stemma that could explain the totality of the textual tradition of the *Tales* (as Manly and Rickert did). In other words, when Manly and Rickert accepted the prior circulation hypothesis, they should have understood that this required more than one stemma: one for the tradition of the book circulating as a whole, and a new one for each of the tales that had been released previously and were genetically related. If a tale had been released independently, the release copy would be the archetype of its tradition, which would have been independent of the tradition of the same

tale as part of the *Canterbury Tales*. In theory, it is possible to accept that if the version released before inclusion in the *Tales* were exactly the same as the one included in the complete work, then these two would virtually share the same archetype. This is much more difficult in practice, however, since the versions included in the complete poem are likely to have been adapted to fit them with the teller and the context of the story.

The conflict between the arguments for prior circulation and for a single origin to the textual tradition might explain why Manly and Rickert did not produce a stemma for the whole of the *Canterbury Tales*. The way in which they had to handle their data could not have made it easy to analyse sets of variants for each of the tales and then put all of this together to produce a complete stemma of the book. Another obvious problem is the difficulty of handling the data without the help of computers. The difficulty of remembering all the variants and grouping them mentally, makes the use of computers very important. The use of computer technology makes it possible to group variants as required to develop ideas concerning the genetic relationships between the witnesses of the text.

If we were able to build a single stemma based on the word-variant data of *The Text of the Canterbury Tales* would show that, even though Manly and Rickert believed the tales had indeed circulated prior to the publication of the book, the explanation of the textual tradition they offer implies it had had a single origin.⁶ Indeed, some critics have found this contradiction unacceptable.⁷ This is not intended to undermine Manly and Rickert's enterprise; on the contrary, it seems that they tried to remain open to an alternative that was partially disproved by their own work. The groups they proposed

show a high degree of consistency throughout and, although there are some inconsistencies, most manuscripts show the same overall affiliations throughout. Manly and Rickert defend their position as follows:

The question was whether we should treat all the variants as if derived from a single archetype or should attempt, before classifying, to distinguish the separate sources and deal with them separately. Unfortunately it was impossible to separate the different sources except by the readings, and this involved the completion of the work of classification. We have therefore proceeded as if all MSS were from the same archetype, being on the watch, however, for indications of separate origin and separate lines of descent. It is doubtless true, as we are told by all writers on the subject of text-criticism, that the recension –that is, the establishment of the text solely on the basis of the readings in the MSS– can be carried out only when the MSS are all derived from a single archetype. But it seemed to us quite certain that the ordinary processes of classification would call attention to readings and MSS not derived from such a source and would enable the textual critic to distinguish such varied sources as had not become entirely unrecognizable by the spread of the vulgate readings. (2:39-40)

The question Manly and Rickert posed at the beginning of their work; whether some tales circulated independently before the publication of the book as a whole, was not answered. They finished their work without finding any proof of the circulation of independent tales; they seemed to have omitted a clear statement that their research had

not produced any evidence of the prior circulation of some of the tales. They did make very clear, however, they thought the archetype of the textual tradition of the *Canterbury Tales* was not a complete manuscript, neatly arranged, but rather “a body of incomplete material, in different stages of composition and only in part put in order and corrected.” (2:41)

In general, and referring to the ‘post-publication’ period in which the text of the *Tales* started to circulate as a whole, Manly and Rickert point out that groups **a** and **b** are genetically related, while **c** and **d** are independent of **a** and **b**.⁸ Similarly, they indicate that there are pairs of manuscripts, and that each of these pairs represents an independent line of descent from the archetype of the tradition.⁹ Manly and Rickert expected that both aspects would become evident when all the witnesses of the text had been analyzed, but, as stated above, no explicit reference to this can be found in their work. The conflict between the idea of independent circulation and post-publication development would become evident in the construction of stemmata: it would not be possible at the same time, to have a single archetype for the textual tradition and individual ones for each tale from the pre-publication state.

1.3 The Tale-Order Manuscript Groups

The theoretical inconsistency between single and multiple origins for the textual tradition, however, is not the only one confronting the reader of *The Text of the Canterbury Tales*. A further inconsistency appears when we analyze the word-variant groups together with the tale-order groups. In the second volume of *The Text of the*

Canterbury Tales, Manly and Rickert discuss issues raised by the different tale orders found in the manuscripts. They analyze the different tale orders and draw conclusions based on the manuscript evidence. Their tale-order table shows that the manuscripts are divided into the following groups:

Group a: El Gg Dd En1 Ds Cn Ma En3 Ad1 Bo2 Ad3 Ha5 Ad2 Bo1 Ph2

Group b: He Ne Cx1 Tc2 Ha3 Ln Py Ra3 Tc1 Ma Ra1

Group c: Cp Sl2 La

Group d: Lc Mg Ha2 Sl1 En2 BwRy2Ld2 Dl Ry1 Fi Ii Ht Ra2 Pw Mm Gl Ph3

Anomalous manuscripts: Hg Ha4 Ch Ld1 To Hk Ps Se Nl Cx2

The tale-order groups appear to be the result of careful work, but a few details, such as the inclusion of El in the **a** group, are in disagreement with the groups that Manly and Rickert suggested were based only on word-variant affiliations. Other fundamental differences are, for example, the size of the **a** and **b** groups, and the explicit separation of pairs and individual manuscripts in the analysis of word-variants. By not explaining them further, Manly and Rickert weaken the differences between the word-variant and tale-order groups. The groups partly overlap and, the reason for this should be found. Should the differences between the groups derived from word-variant and tale-order data be confirmed, it would be essential to attempt to explain why they occur.¹⁰

1.4 Comparing Tale-Order and Word-Variant Groups

Manly and Rickert's word-variant groups are based on the agreements and disagreements between the different witnesses of the text. The tale-order groups appear to result from a study of a process of development of the different orders.¹¹ Word-variants are clearly a result of the copying process, during which words might be miscopied, misread, intentionally or unintentionally changed.¹² Changes in the order of the tales clearly have a different nature. Even though it may be argued that some changes in tale order occurred to a mistake of some kind in the copying process,¹³ the type of error that would result in an alteration of tale order would be fundamentally different from those changes that produce word-variants. There are three different alternatives concerning the different orders in the manuscripts: the first is that the tale order in the diverse manuscripts is the result of chance or scribal intervention; the second is that it is the result of genetic transmission; the third that there is a mixture of both. If the first alternative were correct, there would be no correspondence between the tale-order groups and the word-variant groups or if there were such correspondence, this would be the result of chance. If there were a genetic relationship between the different tale orders, it should be possible to show that the evolution of the tale order is related to the evolution of the text. However, because the tale order may have been subjected to alteration by an editor or scribe for various reasons, an open mind should be kept in this respect or, at least, intervention of this kind should be expected.

A closer look at both sets of groups conceived by Manly and Rickert, shows that El and Gg are in the tale-order group **a**, but these are shown as having different lines of descent in the word-variant classification. Manly and Rickert point out that:

El, Hg, **a**, and Gg are for the most part derived from a better text. They appear as of independent descent or in varying combinations (e.g. a-El, El-Gg, infrequently Hg-El, more rarely Hg-Gg), or, as in the Knight's Tale, the Monk's Tale, the Nun's Priest's Tale, Block G, and the Manciple's Tale, as from the same source. (2:44)

It is important to remember that when Manly and Rickert suggest that the different lines of descent can occur in varying combinations they are likely to be referring to the slight changes in affiliation found in different parts of the text.¹⁴ The connection between El and Gg has also been picked up by Robinson in "A Stemmatic Analysis of the Fifteenth-Century Witnesses to the Wife of Bath's Prologue," where he discusses the possibility of El and Gg belonging to what he calls the E group, which could explain the frequent agreement between these two manuscripts.¹⁵ Other manuscripts belonging to the tale-order **a** group are classified as independent pairs, the case of En3 and Ad1, Bo1 and Ph2, and Ad3 and Ha5. Both Bo2 and Ad2 belong to the tale-order **a** group, although Manly and Rickert find that they do not have clear affiliations according to their word-variants. The **b** group presents a similar problem. There are witnesses (He Ne Cx1 and Tc2, for example) that overlap between the cells corresponding to Manly and Rickert's tale-order and word-variant groups:¹⁶ whereas, Ha3 Ln Py Ra3 Tc1 are of unclear word-variant affiliation, but present the tale order that can be found in the **b** manuscripts. Mc and Ra1,

both of which belong to the **b** tale-order group, form a pair of independent descent according to the word-variant classification. Group **c** includes Cp Sl2 and La in both cases, for word-variants and tale-order. The **d** group overlaps in the following witnesses: Lc Mg Ha2 Sl1 En2 Ry2 Ld2 Dl Pw Mm Ph3. The word-variant group **d** has an extra-manuscript, Ll1. On the other hand Ra2 and Ht form a pair of independent descent. The manuscripts left in the tale-order **d** group are Bw Fi Ii Ry1 and Gl, which Manly and Rickert thought were of unclear affiliation.

The tale-order of anomalous witnesses (such as Ha4 Ld1 Hk Se Nl and Cx2) was omitted from the word-variant groups since Manly and Rickert thought these presented no textual connections with any other group. For example, Ps forms one of the independent pairs together with Ha1. Se is a manuscript that Manly and Rickert considered highly conflated and, therefore, unclassifiable; and Cx2 was omitted from all their analyzes because it “became clear that they could not aid in establishing Chaucer's text” (Manly and Rickert 1:81); later Cx2 became the subject of the dissertation of their doctoral student, Thomas Dunn. Manly and Rickert thought that Ha4 had been copied after the hyparchetypes of each of the groups, and they agreed with Tatlock in deeming this manuscript as “lacking authority” (2:44).¹⁷ Manly and Rickert's explanation concerning the origin of the anomalous tale-order manuscripts is that they were possibly made by special order for particular owners, while manuscripts belonging to specific groups were the products of the work of professional scribes (Cf. 2: 490).

A clear overlap appears between the word-variant groups and the tale-order groups. This suggests that further research is needed to explain why some witnesses have

different affiliations in the two analyses. The witnesses in which word-variants and tale-order present unexplained discrepancies could be the points at which scribal or editorial intervention changed the shape of the order of the tales. If this were so, we should find that there are places at which the tale-order stemmata and the word-variant stemmata present different relationships among the same group of manuscripts. Manly and Rickert show that there are enough common points between word-variants and tale-order to make it worthwhile carrying out further research on the subject.

2. ROBINSON'S REVISION OF THE WORD-VARIANT GROUPS

The research carried out by Manly and Rickert was of such scope that it took until the last decade of the twentieth century for anyone to try to analyze the same data. The Canterbury Tales Project's main goal is to explain how the textual tradition of the *Tales* developed. One might say that the work carried out by Manly and Rickert is being done anew, this time with better tools and higher standards of accuracy. Robinson was the first member of the Project to carry out stemmatic analyses of different sections of the *Canterbury Tales*. He has pioneered the use of computer technology for textual, and especially stemmatic analysis, he was one of the first scholars to experiment with the application of phylogenetic software to the study of manuscript traditions,¹⁸ and the first to apply them to the textual tradition of the *Canterbury Tales*. Although Robinson has not carried out any tale-order analysis, his successful refinement of the word-variant groups using phylogenetic software and programs such as *Collate* and V-Base, suggest that there

is a possibility of analyzing tale-order using similar methods. Robinson's groups based on GP data are as follows:¹⁹

Alpha group: Ad1 Ad3 En3 Tc1

Group **a**: Cn Dd Ds1 En1 Ma

Group **b**: Cx1 Cx2 Ii Ld1 Ne Nl Pn Tc2 Wy

Group **ab**: Ht Py Ra2 Ry1

Group **cd**: Bw Cp Dl Fi Gl Ha2 Ha3 La Lc Ld1 Ld2 Mg Mm Pw Ry2 Se Sl1 Sl2

E Group: Bo1 Ph2

Non affiliation or uncertain: Bo2 Ch Do El Gg Ha4 Hg Ln Ra3 To1

The most obvious differences between these and Manly and Rickert's groups are the inclusion of **α**, **ab**, and **E** groups. Robinson has also merged **c** and **d** into a single large group and in this agrees with Manly and Rickert when they say that the two groups were indistinguishable in GP. But Robinson's groupings are slightly different for WBP, for which the manuscripts are classified as follows:

a Cn/Ma Ds/En1 Dd

b Ii He Ne Cx1/Tc2

cd Cp La Mm Ld1/Ry1 Ph3 Pw Sl2 To Dl Fi Nl Sl1 Lc/Mg

E Bo1/Ph2 Gg Si

F Bw/Ln Ld2/ Ry2

O Ad1/En3 Ad3/Ha5 Ra3/Tc1 Ch Bo2/Ht Hg

Each of Robinson's groups is thought to descend from a hyparchetype although the **b** hyparchetype, for example, is likely to have descended from a manuscript from the **a** group, that is, ultimately from the **a** hyparchetype. The last group of manuscripts, the so-called **O** manuscripts, are not a genetic group, but rather descend from the archetype of the tradition. Robinson explains it as follows:

Each of the groups AB CD E F appears to represent descent from a single hyparchetype. Therefore, the thirty-three witnesses in these four groups represent just four independent lines of descent (or only three, if E and F descend from a single hyparchetype.) However, if the manuscripts in **O** are indeed only related by common descent from the archetype, then the six groupings in **O** (four pairs and the two singletons Ch and Hg) represent a further six independent lines of descent. For convenience, the witnesses in this group are referred to as 'O,' but they should not be seen as constituting a genetic group in the same sense as do the other groups. . . . ("Stemmatic" 80)

Although Robinson's explanation of the difference between the **O** manuscripts and the genetic groups seems very clear, there has been some misunderstanding about the status of the **O** manuscripts.²⁰ Some of the **O** manuscripts appear as unclassified in GP, and this discrepancy in Robinson's groupings is probably due to the fact that his analyses have been based on the data of two sections of the *Canterbury Tales*. Although they offer a good indicator of the relationships among the manuscripts, they are by no means a

synthesis of what happened overall. A complete set of groups based on research carried out at the Canterbury Tales Project is part of a work in progress.

An important difference between Manly and Rickert's groups and those of Robinson is that in WBP he found that El (in parts) and Gg belong to his **E** group.²¹ This occurs, according to Robinson, because El changes its affiliation around line 400 of the WBP. He suggests that this is due to a change of exemplar at that point in the text. However, the agreement of El and Gg in non-archetypal variants is also found in SQ, which suggests that something else might be causing this coherence between the manuscripts (Bordalejo 206 and ff).²² The variants that El and Gg share possibly indicate that they are more closely related than scholars have thought up to this point. Robinson's classification is not final in the sense that it takes into account only a small part of the data.

Robinson's **α** group includes some of the manuscripts that Manly and Rickert consider to be pairs of independent descent (Ad3 Ad1 En3 and Tc1). Various other manuscripts they thought were of independent descent have been classified by Robinson as having either no, or an uncertain affiliation. To say that these manuscripts have no affiliation means that they do not share any common ancestors with any other manuscript extant in the tradition, but are directly and independently descended from O. In other words, Robinson agrees with Manly and Rickert in pointing out that some manuscripts are unrelated to any other manuscript extant today. Robinson's **a** group exactly coincides with Manly and Rickert's word-variant **a** group. His **b** group includes the same witnesses as Manly and Rickert's: Ne Cx1 Tc2. It also includes all the printed editions after the first

(Cx2 Pn Wy) since all of these were based on Cx1. They share the majority of its variants, which makes them part of the **b** group. Robinson also includes Ii and Ld1 in this group, two manuscripts that had not been classified by the previous analysis.

3. RECONCILING THE DIFFERENCES BETWEEN ROBINSON'S AND MANLY AND RICKERT'S GROUPS

A comparison between the groups formulated by Manly and Rickert and those refined by Robinson may help us to understand how the computerized tools work and whether or not their application to the tale-order problem might be helpful. Many of the groups formulated hitherto are consistent,²³ but the places in which the groups differ are those which are more likely to offer grounds for further research. The best example of a contrasting difference between all groups is the shift of El and Gg from Manly and Rickert's tale-order **a** group to independent descent (unclassified) in Robinson's GP word-variant groups.²⁴ This seemingly strange double affiliation could be explained if one of the manuscripts derived from the **a** group were the hyparchetype of the **E** group. This could account for El and Gg having the **a** order but very different textual affiliations in the groups devised by Manly and Rickert. It might also imply that El could be a derivative manuscript farther away from the origin of the tradition than it has been thought up to this point. In fact, when explaining the ancestor of the **a** group (Dd En1 Ds Cn Ma) and its tale order, Manly and Rickert observe:

The MSS of Group **a** have essentially the same arrangement as El, but it is textually clear that the group is not derived from the same immediate ancestor as El. It is, however, practically certain that the one arrangement was imitated from the other. There are some slight but notable differences between the contents of these MSS and the contents of El. In WBP it is clear that the five additional passages were not originally in the ancestor of El, but four of them were borrowed by El from the ancestor of **a** or some member of the group, **a** itself having doubtless obtained them from a special copy of WBP in which Chaucer had added them. (2:480)

The interest of this quotation lies in the awkward solution Manly and Rickert offer for the differences presented by El and the **a** group: that passages not present in El's hyparchetype, as well as the order of the tales, were borrowed from a manuscript which was different from the exemplar from which El was copied. The fact that El has been considered the best witness of the *Tales* since the end of the 19th and most of the 20th century makes it very difficult for scholars to keep an unbiased attitude towards it. Even Manly and Rickert, who provided strong arguments for the text of Hg as the most accurate witness of Chaucer's work, had to justify the El-**a** relationship at any cost. If we take into account for our analysis both the data produced by the Canterbury Tales Project for WBP and some of Manly and Rickert's observations, we could hypothesize the following stemma:

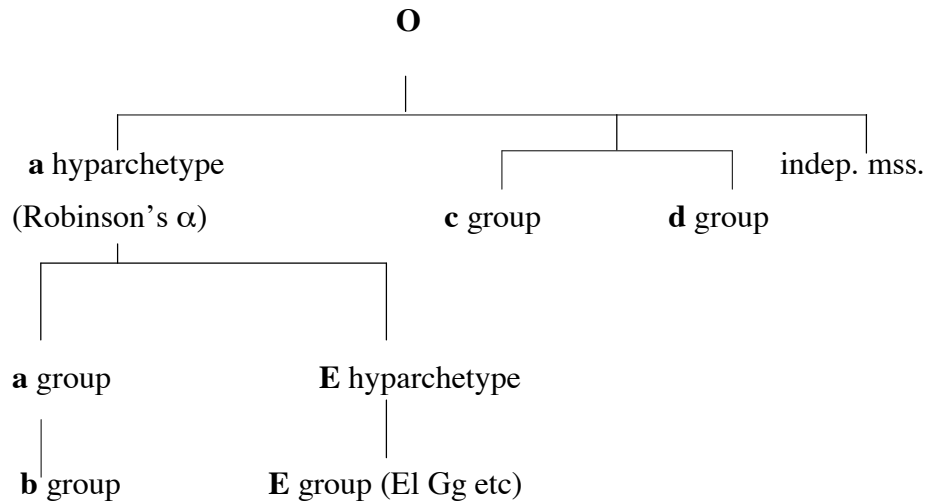


Figure 2. A possible stemma based on my research on Cx2

This stemma is not necessarily correct, but serves to illustrate one of the possible scenarios that can be constructed with the data we possess and their interpretation. I am not proposing to use any stemmata produced with the help of computers, or by manual means, as the definitive solution to the problems presented by the textual tradition of the *Canterbury Tales*; instead, they should serve as a guide to help us to understand the complex relationships between the extant witnesses of the text. Stemmata for tale-order have to be taken into account with the same reservations as those required for word-variant stemmata: it is important to keep an open mind and to be aware that no alternative should be discarded or confirmed *a priori*. It is difficult to date the manuscripts themselves, and one must also consider that the text they hold might be earlier than the date in the manuscript. For example, it is conceivable that a very early manuscript could have been separated by many copies from the origin of the tradition; it is also possible that a very late manuscript could have been copied directly from one separated only by one or two copies from the origin of the tradition. The only certainty is that no extant

witness can explain the textual tradition as it is today. But even this may not be true, since those parts of the tradition that cannot be explained may be the results of later additions to the text.

In order to understand better the textual history and the development of the text of the *Canterbury Tales* we need to explore as many of its aspects as possible, including the text, its order, and the making of the manuscripts, to help us understand whether there is a genetic relationship between the different texts and tale orders. This approach is important inasmuch as that it uses word-variant and tale-order relationships to explain each other, instead of seeing them as separate entities. The results of Manly and Rickert's work show that there are enough common points between tale-order and word-variants in the *Canterbury Tales* to make the exploration of these relationships worthwhile. The new approaches proposed by Robinson show that we can now take a new look at old problems, and offer alternative explanations about them that might help us better to understand not only the text that originated the *Tales* textual tradition, but also the process of transmission in medieval manuscript culture. This research attempts to discover whether the tale-order was transmitted in the same way as the word-variants, or whether the different tale orders were merely random or capricious. If the tale-orders are related we should see some similarities between tale-order and word-variant stemmata, at least for those manuscripts in table 4 we know overlap (such as core **a**). Once this is established, we could move forward and attempt to re-classify the 'anomalous' witnesses of the *Tales*.

¹ See the introduction for a definition of Manly and Rickert's O, O¹ and O² and the differences between their conception of the beginnings of the textual tradition and the ideas developed by the Canterbury Tales Project. O¹ and O² are used only while discussing Manly and Rickert's ideas. My own conception of the textual tradition distinguishes only between O (the archetype of the tradition) and its descendants. For my research, I do not deem it relevant to make any further distinction or clarification about the nature of O, other than that it is likely to have been a series of booklets. See the introduction for more details on O¹ and O² and O.

² Manly and Rickert, *The Text of the Canterbury Tales*.

³ This is how Manly and Rickert describe procedure for their work: "The question was whether we should treat all the variants as if derived from a single archetype or should attempt, before classifying, to distinguish the separate sources and deal with them separately. . . . We have therefore proceeded as if all MSS were from the same archetype, being on the watch, however, for indications of separate origin and separate lines of descent." (2:39)

⁴ The arguments about prior circulation discussed in this chapter are related to the possible theoretical inconsistency from a stemmatic perspective. However, a case can be made to explain the references to some of the texts included in the *Canterbury Tales*. It is conceivable that the tales were read by Chaucer's friends or in court, without necessarily assuming that there were offered to scribes to copy. In such a case, anyone who might have heard one of the tales, could have referred to it. Some of the stories did, in fact, 'circulate' before Chaucer took them over, a fact that does not alter our perception of these having a separated textual history.

⁵ Although this is not the only possible kind of revision, the Canterbury Tales Project research suggests that Chaucer did not revise word by word, but rather, re-wrote and deleted whole passages (Solopova "Authorial" 139). The number of internal inconsistencies in the *Tales* shows that Chaucer did not want or could not give a final form to his work.

⁶ To the best of my knowledge, no one has attempted this task. Manly and Rickert do not provide such 'overall' stemma, and the data presented in their edition is too complex for a manual analysis. In theory, it would be possible to code the information of their edition and use it in conjunction with phylogenetic software; however, the benefits of such task are unlikely to justify the amount of effort required.

⁷ See George Kane ("John" and *Piers A*) and Kane and Donaldson (*Piers B*).

⁸ Manly and Rickert also point out that these two groups are indistinguishable (2:77).

⁹ Some of these pairs have also been identified by Robinson, who also suggests that they descend independently from the archetype ("Commentary").

¹⁰ See chapter 4 for the discussion of tale-order groups based on newly built stemmata.

¹¹ See groups **a** and **b** in table 2.

¹² For a classification of word-variants based on the copying process see Vinaver, "Principles of Textual Emendation."

¹³ An example of tale-order miscopying is that of Hg, where the sequence SQ-L20-ME-L17 -FK is likely to have been the product of the scribe having copied the tales without their corresponding links.

¹⁴ This is by no means proof of independent circulation, since the variants show a certain degree of consistency; instead it might be a sign of scribal habits (some tales might have been more popular and well-known and, therefore more liable to memorial contamination) or it might be due to the nature of the data, which varies from witness to witness (some manuscripts have been badly damaged and, occasionally whole tales are missing). There is proof of independent circulation after publication (some tales were very popular and where included in anthologies), but although this might alter the textual history of a particular tale (which might have been copied more than the others) it should not greatly distort the overall shape of a stemma.

¹⁵ I discuss Robinson's revision of Manly and Rickert's classification later in this chapter.

¹⁶ See table 4.

¹⁷ My research on the manuscript source of Cx2 showed that some witnesses of the *Tales*, particularly Ad3 and Ha4, are likely to preserve archetypal readings. See "The Manuscript Source."

¹⁸ Other scholars who have experimented with these methods are Griffith, “A Taxonomic Study of the Manuscript Tradition of Juvenal” Platnick and Cameron, “Cladistic Methods in Textual, Linguistic and Phylogenetic Analysis” and Cameron, “The Upside-down Cladogram, Problems in Manuscript Affiliation.”

¹⁹ Since Robinson has carried out only word-variant analysis, I do not specify this each time I refer to his groups.

²⁰ See, for example, Blake’s article on Cx2 (“Caxton’s”), where he refers to the “O Group” as though these witnesses were genetically related.

²¹ El does not appear in table 4 as an **E** group manuscript because Robinson argues that its affiliation with **E** is the result of a change of exemplar and not of an overall genetic relationship (Robinson, “Stemmatic”).

²² See also Manly and Rickert 2: 294 and ff.

²³ Examples of this are the core **a** group (Dd En1 Ds Cn and Ma) and **b** (He Ne Cx1 and Tc2).

²⁴ The manuscripts Robinson presents as unclassified are likely to represent independent lines of descent, and therefore do not belong to a specific group (Robinson, “Commentary”).

CHAPTER IV: ANALYSIS OF THE TREES

PRODUCED USING PHYLOGENETIC SOFTWARE

This chapter discusses the validity of the results produced using phylogenetic programs and attempts to explain these results based on a manual analysis of particular aspects of the different tale-orders. First, I briefly survey some of the different phylogenetic approaches and point out the basic differences between them.¹ Later I explain why the programs represent the data as they do and compare the results obtained with two different encoding systems (breakpoint distance and IEBD or Inverse of Expected BreakPoint Distance).² I particularly focus on the cases in which the traditional Manly and Rickert tale-order and word-variant groups do not overlap and attempt to explain the reasons for this divergence.

1. METHODS OF PHYLOGENETIC ANALYSIS

Evolutionary biology, like any other discipline, has various ways to approach and solve a given problem, some of which are more successful than others. In this way, we find that there are many different approaches that could be used for the reconstruction of phylogenies. There are systems that produce tree-like representations of a given phylogeny while others represent the same relationships by using cycles. The latter are referred to as networks.³ My choice is to concentrate on tree-building methods. This choice is related to two main issues: in the first place, tree-building methods have similar

goals to those of the stemmatic analysis of manuscript traditions; moreover, they display their results in a similar way to the one employed by stemmatologists. In the second place, some tree-building methods, as I have explained in previous chapters, have been tried before with word-variant manuscript data with successful results, which makes them suitable to be tested with a different aspect of a textual tradition.⁴

Phylogenetic tree-building methods can be divided according to the kind of data they use (distance, discrete) and by the actual method employed by the programs for the construction of trees (clustering algorithm, search). Phylogenetic software was designed to work with deoxyribonucleic acid (DNA), but different methods approach the same problems differently. In order to understand how these programs work with manuscripts, it is necessary to understand how they work with DNA.⁵

1.1 How does Phylogenetic Software Work?

DNA is built by a sequence of nucleotides, and each nucleotide contains one of the four bases: adenine, cytosine, guanine and thymine.⁶ A DNA sequence contains the equivalent of words, but each word is written using three letters (See example below). Each of these three-letter sequences of nucleotides forms a specific amino acid. When a mutation occurs, one or more nucleotides are substituted by others, so changing the amino acid referred to by the sequence of three-letter words. If the change is successful it will be copied, individuals will inherit it, and it will become a feature of those individuals; otherwise it is just a random mutation which does not survive into further generations of copying. Similar processes can be observed in the scribal copying of

manuscripts: sometimes a change persists and is inherited by other witnesses; other times it is just random variation that gets corrected, and the variant is not inherited. Because of the similarity in these processes, there are many points of contact between the problems presented in evolutionary biology and those embedded in the study of manuscript traditions. For example, a single nucleotide could have been replaced on two successive occasions and the final result would be a single difference. Page offers the following example:

AAA

CAA

TAA (Page and Holmes 145)

The first nucleotide (A) is substituted by a second one (C), which is, in turn, replaced by a third (T). In this case, if we only knew about the first and last states, it would be impossible to infer the second state (unless the second step was necessary to reach the third one⁷). This problem is central for evolutionary biologists; and it is also relevant for the study of manuscript traditions. In evolutionary biology, most approaches deal with these data in either of two ways: distance methods and discrete methods.

1.2 Different Tree-Building Methods

1.2.1 Distance vs. Discrete Methods

Distance methods have their basis on the assumption that if the number of changes between the elements being compared were known to us, we would be able to calculate

the total number of these and reconstruct the complete sequence of changes (Page and Holmes 1979). In the proposed case, where two substitutions result in a single difference, we would need to know how many changes occurred between the first state and the third state. Although there are many difficulties involved in calculating this, several good methods have been developed.⁸ Another problem which is usually mentioned while referring to distance methods is that they have as their first step the conversion of the data into a distance matrix and as their second step they build trees based on such matrixes. Of course, there are occasions in which distance methods are the most appropriate ones, especially when the data one is working with have not been produced by an automated system. Minimum evolution and neighbour joining are methods which employ distance measurement before processing the data.

Discrete methods consider the data directly in each nucleotide site instead of attempting to calculate the number of changes in a sequence as distance methods do. The main difference between these two approaches is that the discrete methods “endeavour to avoid the loss of information that occurs when sequences are converted into distances” (Page and Holmes 1987). Indeed, it would seem that using directly the data without further processing should be a more straightforward approach and should yield undistorted results. Maximum likelihood and maximum parsimony are methods which consider the data directly, without any further processing. About these methods Page and Holmes observe:

The two major discrete methods are maximum parsimony (MP) and maximum likelihood (ML). Maximum parsimony chooses the tree (or trees)

that require the fewest evolutionary changes. Maximum likelihood chooses the tree (or trees) that of all trees is the one that is most likely to have produced the observed data. (187)

Of course, there are some disadvantages in both methods. For example, because maximum parsimony gives preference to the tree with the least number of changes, it is liable to leave out more complex alternatives which might have occurred in the real evolutionary path. In theory at least, because maximum likelihood produces trees that explain the extant data in the most satisfactory way, stemmata built using this method should be very useful to map the relationships between the witnesses which have reached us.⁹

1.2.2 Clustering vs. Search Methods

The second way in which phylogenetic methods can be classified is in regard to the procedure employed to build the trees. As stated above, one of the tree-building methods is clustering analysis. This uses algorithms to construct the trees, that is, it uses a series of steps represented in mathematical formulae to reach its results. Page synthesises some of the positive aspects of clustering analysis by pointing out that:

Clustering methods have the advantage of being easy to implement, resulting in very fast computer programs. Furthermore, they almost always produce a single tree. This combination of speed and an apparently unambiguous answer is naturally very appealing, and accounts for much of the sustained popularity of clustering methods. However, they have some severe limitation

as analytical tools. The result obtained from simple clustering algorithms often depends on the order in which we add the sequences to the growing tree. (Page and Holmes 174)

It seems clear from the above quotation that there are also serious concerns about the use of clustering methods. In fact, when one has used search methods and has seen the software evaluate more than 400,000 possible branch rearrangements before arriving at the best solutions, it is difficult to accept that any method could produce a single tree (although it is conceivable that one could arrive at a best tree). On the other hand, search methods (minimum evolution, maximum likelihood and maximum parsimony) may yield many equally good trees from a single data set. However, because so many trees can be produced, each of these methods has to determine which of the trees are the best ones. Minimum evolution searches for the tree with the shortest overall length, that is the one that requires the minimum number of changes to be produced. In a way, minimum evolution is related to maximum parsimony, but the former builds its trees by using pairwise distances, while the later is a discrete method.

1.2.2.1 Testing the Tree-building Methods

Scientists agree in saying that the true tree is very difficult to identify, so they have used models which attempt to show the accuracy of the diverse methods by reconstructing trees which are very similar to the proposed tree or to a known tree (Nei and Kumar 109). This means that a known phylogeny could be used to test the validity of

a given tree-building method. However, known phylogenies are as rare as known manuscript traditions and this poses a problem for the testing of software. One possible way to overcome this problem is to use phylogenies that have been produced in laboratories, but even with these the software has been shown not to be able to reconstruct the relationships perfectly and completely. It has been pointed out, especially, that neighbour joining and minimum evolution present problems while handling branches of length zero:

[Z]ero length interior branches in realized trees are the source of topological errors in reconstructed trees, particularly when there are many such branches. Unfortunately, we usually do not know such interior branches in real data, and therefore it is difficult to evaluate the effect of this factor, though parsimony methods are capable of identifying such branches under certain conditions. (Nei and Kumar 111)

Nei and Kumar suggest that factors such as the mentioned branches with length zero are likely to generate problems even in a known phylogeny.¹⁰ Known phylogenies, however, could be useful to test the degree of accuracy of the different methods. This represents a problem when dealing with manuscript traditions, since we could potentially have an internal branch of length 0 (a manuscript from which we know others were copied). This difficulty is not insurmountable, especially if a stemma is taken as a representation of reality. In fact, currently Matthew Spencer and Elizabeth A. Davidson are working on a pilot experiment of an artificial manuscript tradition. In their first attempts, Spencer and Davidson have included all the witnesses of the artificial tradition. The tree built using

these data is extremely accurate and reflected all the divisions and subdivisions where the researchers expected these, that is, where it was known that breaks and subdivision of the tradition had occurred. Further study of artificial textual traditions, in which parts of the tradition are suppressed, might be helpful to give a clearer idea of how accurate the programs are depending on how much data has been lost, since these factors could be monitored and controlled.¹¹

1.2.3 Split Decomposition

Another method which should also be considered is split decomposition (Dress, Huson and Moulton). Splits represent the relationships of a data set by arranging the different elements in two groups, therefore creating a partition. If the splits are compatible or suggest a derivation there would be no problem. However, when the data is conflicting, as it would be the case with highly contaminated manuscript traditions, then the scholar (or the software) has to decide which one of the splits is to be followed. Split decomposition expresses evolutionary relationships differently than other methods: it does not assume that evolution is a “*branching* or *tree-like* process” (Hudson). For this reason, split decomposition offers the advantage that the data does not have to be drawn as a tree at all. Hudson describes it as follows:

In contrast to methods such as maximum parsimony and maximum likelihood that reconstruct phylogenetic trees by optimizing certain parameters, split decomposition is a transformation-based approach. Essentially, evolutionary

data is transformed or, more precisely, "canonically decomposed", into a sum of "weakly compatible splits" and then represented by a so-called splits graph. For ideal data, this is a tree, whereas less ideal data will give rise to a tree-like network that can be interpreted as possible evidence for different and conflicting phylogenies. Further, as split decomposition does not attempt to force data onto a tree, it can provide a good indication of how *tree*-like given data is. ("Analyzing")

The above quotation explains how this method can be of help when the data is conflicting (such as in the cases of contamination or convergent variation). Given conflicting data, split decomposition can offer a different alternative for its representation. Because other methods assume that the evolutionary data is tree-like they try to impose this format upon them *SplitsTree* (Hudson), which implements split decomposition, when faced with ambiguous or contaminated data represents them as networks.

2. PHYLOGENETIC SOFTWARE AND THE ORDER OF THE *CANTERBURY TALES*

The fact that there are so many choices and different approaches to the reconstruction of phylogenies indicates that evolutionary biology is far from reaching a consensus about which method or software is the best.¹² The choice among all of these is difficult, but the main reason for the choices of software used in this research was the fact that these had already been tested by Robinson with the *Svigdagsmál* and the *Canterbury Tales* traditions. Both *SplitsTree* (split decomposition) and *PAUP* (originally designed as

maximum parsimony, but now also implementing maximum likelihood and distance methods) have been shown to be reliable with textual data, but in fact the latest version of *PAUP* allows the use of methods other than parsimony.

When using word-variant data, *Collate* (Robinson), one of the computer programs used at the Canterbury Tales Project, can encode them to be used with either *PAUP*, *SplitsTree* or any other phylogenetic software. In the case of the tale-order data, as was mentioned in chapter 2, the modified Manly and Rickert table had to be made computer readable. A special program was written by Spencer (Spencer et al.) to interpret the table and calculate the distance between the different tale orders (sequences); that is, the program can carry out calculations of how many changes have to occur in the tale-order of a particular manuscript so it can become that of another. The process originally used to encode the data is described as follows:

We calculated pairwise distances between manuscripts based on differences in item order. The natural choice for a distance measure is edit distance, the minimum number of editing operations (here insertions/deletions, adding/removing one or more items; and transpositions, moving one or more items to a new location) needed to convert one order into another. . . . We therefore used scaled breakpoint distance between the items common to each pair of manuscripts. . . . Breakpoint distance is the number of items common to both manuscripts and having different right-hand neighbours. (Spencer et al., “Gene Order”)

When there is missing data, it is not possible to know how many possible different right-hand neighbours might be in the data. For example, Gg has the sequence CL-L13-L14...ME...SQ...FK, due to loss of leaves. If someone tried to measure the difference between this manuscript and El, based in this data, he or she would have to speculate how many changes occurred between the El order to reach the Gg order (or vice versa). The method devised by Spencer had to cover two kinds of situations: when manuscripts have many common items which are now missing, or when they have fewer common items missing. This is what generated the lower and upper bound data. These two data sets differ in the fact that: “[t]he lower limit occurs when no common items were lost and the upper limit is approached if there are many lost common items” (Spencer et al., “Gene Order”). However, breakpoint distance “is only reliable when the number of transpositions is small” (Spencer et al., "Analizing" 102). In order to obtain more reliable data IEBP (Inverse of Expected BreakPoint Distance) was used.¹³ Both methods are described in “Analyzing the Order of Items in Manuscripts of *The Canterbury Tales*” (Spencer et al. 98-102).

2.1 *SplitsTree*’s Tale-Order Stemmata

In the first instance the encoded tale-order data was fed into *SplitsTree*.¹⁴ The first results produced by the program can be seen in plates1 and 2.¹⁵ The graphs are not only uninformative, but also very different from the trees that one would expect. As stated above, when faced with data deemed non tree-like, *SplitsTree* would not force a tree shape on them; however, the nature of these trees indicates that there might be some other

reason for this. In fact, *SplitsTree* gives a warning ‘Non metric: Triangle inequalities are not satisfied.’ In order for distances to be metric, they have to fulfil four mathematical requirements: non-negativity, symmetry, triangle inequalities and distinctness.

The third property is the *triangle inequality*, which states that the dissimilarity between any two sequences cannot exceed the sum of the dissimilarities between each sequence and a third. (Page and Holmes 25)

What this means is that the data can be represented only if it can be built into triangles. See (Page and Holmes 24 and ff.). The command ‘Force triangle inequalities’ alters the distance matrix in order to make it satisfy the triangle inequality. This command allows the tree to be re-drawn by completing any distances which were lacking in the construction of the previous graph. The result of forcing the triangle inequalities can be seen in plates 3 and 4. There is a slight difference in the fit for these two plates, but if they are superimposed the differences are extremely difficult to detect. These differences are reflected by small changes in the length of some of the branches. The problems of *SplitsTree* attempting to handle the tale order data can be summed up as follows: on one hand, even when forcing the triangle inequalities, the trees are not very helpful in interpreting the relationships among the data. On the other hand, because forcing the triangle inequalities alters the data, trees built using this command might not be reliable in reflecting the true nature of the relationship between the manuscripts.

Even if *SplitsTree* were successful in the analysis of word-variant data, the way in which this program handles the tale-order data is not very efficient.¹⁶ For this reason, the trees built using this software are not helpful for the interpretation of the relationships

between the different tale orders, and an alternative method to reconstruct trees using the tale-order data was required.

2.2 *PAUP* and Tale-Order Stemmata

Since 1992, the time when *PAUP* was first used by Robinson to construct computer generated trees for the Norse text *Svigdagsmál*, the program has been altered to deal with several alternative phylogenetic approaches. In fact, *PAUP* 4.0b10 allows the construction of trees using maximum parsimony, maximum likelihood and distance methods as stated above. The modified Manly and Rickert table was made computer readable by calculating distances (breakpoint and IEBP) and, for this reason, this is the method used to build trees with *PAUP*. The criterion for the choice of trees is minimum evolution, which, as I have pointed out before, has some links in principle to maximum parsimony but is a distance method.

2.2.1 Stemmata Based on Data Encoded Using Breakpoint Distance

PAUP gives a single tree for each of the nexus files,¹⁷ one for upper bound and one for the lower bound data. The overall architecture of these trees is very similar, although we can find some alterations in some of the relationships in the branches of the trees. Manuscripts belonging to Manly and Rickert's tale-order groups with the exact same order, such as En1 and Ds or Cn Ma En3 and Ad1, are placed with E1 in both the upper

and lower bounds trees (See plates 5 and 6). This group of manuscripts is in the same position, relative to the rest of the analysed witnesses, in both trees (it appears at the top of the stemmata). The only exception in constant position is Gg, a manuscript that has many missing leaves, when the breakpoint method has been designed to take into account the loss of common items or their preservation. If one assumes that no common items were lost (lower bound tree, see plate 6) Ps appears nearer to the majority of the **a** manuscripts and apart from the group formed by To Gg Hk Se and Ch. Ps was classified as anomalous by Manly and Rickert, but its only differences from the **a** group are the position of CL, which is separated from ME and placed between ML and WPB, the lack of TM, the lack of L14 (as Ad3 and Ha5) and the loss of L37 PA and RT because of loss of leaves in the manuscript. This close inspection of the order of this manuscript shows that its order could easily be related to that of the **a** manuscripts. Moreover, on the assumption that common elements were lost in the witnesses, Ps still groups near **a**, but it appears closer to Gg and Hk. In the upper bound tree (plate 5) these three manuscripts separate from Se To and Ch which now form a small unit farther from the **a** group.

Another major difference between plates 5 and 6 is the position of S11 Py and Ld2. In the lower bound tree (plate 6), these manuscripts appear grouped with Pw Mm and Ph3, towards the middle left side of the stemma. However, in the upper bound, S11 Py and Ld2 move (as a group) form part of the **b d** cluster. Once more, this is due to the fact that some elements have been lost. Both groups of manuscripts still appear in the indistinct cluster formed by the **b** and **d** groups (a lack of distinction which had been

suspected by Dempster when she suggested that both of these orders were derived from a common exemplar [see Dempster 1123-42]).

A very small difference between the two stemmata is the relative position of Hg and Ra1, since both manuscripts appear closer in plate 5, separated by a single node, while plate 6 shows two nodes between them. Ra1 is an incomplete manuscript which has lost many leaves, and its main characteristic is the separation of NP from the rest of the fragment VII.

In both trees, Ch is separated from Ha4 by a single witness: Wy. The significance of this is that it is possible that the source for the order in Wy might have been the ancestor of Ha4.¹⁸

This summary of differences and similarities between the two trees suggests that they are equivalent in many features. However, I have not yet offered any explanations about the possible reasons for the relative positions of the manuscripts. If we take as an example the upper bound unrooted cladogram (plate 7), we can clearly see Manly and Rickert's **a** group at the top of the tree. Dd, one of the **a** group witnesses with the most missing items appears between the **a** group and the rest of the witnesses. The reason for this is the encoding of the data, which was done without presuming the text that might have existed in Dd's missing leaves. For this data set, even though one might think that the Dd order when the manuscript was intact was very similar to that of El, missing leaves have been encoded as items not present.

Gg, on the other hand, wants L10, L11, L15, L17, L20, L21, L24, L28, L30 and RT. One cannot be completely certain that these are indeed the only missing items in the

manuscript, but a combination of tale-order and codicological analysis might indeed suggest that the missing leaves might have contained these links.¹⁹ Gg appears between Hk and Ps (classified as anomalous by Manly and Rickert).²⁰ However, as explained above, the Ps order seems to be closely related to the **a** order. Agreeing with Manly and Rickert's classification, Ad3 Bo2 and Ha5 are nearer to Dd than to the manuscripts at the top of the tree (Ds En1 En3 Ad1 Ma El and Cn). The modified tale-order table shows that Ad3 and Bo2 have quite similar orders, even with some elements in different places.

In plate 5, Bo1 and Ph2, the two last manuscripts in Manly and Rickert's **a** group, are the only ones which appear far from the rest of **a** in the tree (they also appear separated from the **a** groups in plates 6, 7 and 8). In fact, according to this tree, they are more closely related to the **c** group and to the **b d** cluster. Both Bo1 and Ph2 have FK followed by NU-L33-CY, and all three **c** manuscripts have FK followed by NU. The number of changes required to move NU-L33-CY to the position they have in Bo1 and Ph2 is very significant for the overall placement of these manuscripts on the tree, but there is yet another element that displaces them from a position near the **a** group. In Bo1 and Ph2, CL is directly followed by FK, instead of the El sequence: CL-L13-L14-L15-ME-L17-SQ-L20-FK. Cp and La only have L13 and ME between CL and FK. Bo1 and Ph2 appear together in the tree, and this is not surprising since these two manuscripts present a very similar order. What seems strange is that Manly and Rickert decided to include these two manuscripts in the **a** group. The main difference in order between Bo1 and Ph2 and manuscripts of the **c** group is that the latter have L8-SQ and L13 ME which

are not present in Bo1 and Ph2. These two manuscripts have also included L34 between CY and PH and L22 between PD and SH (where La has L23).

The **b** and **d** group manuscripts appear mixed at the bottom of tree (plate 7). Dempster's ideas about the possible common origin for the **b** and **d** groups seem to be supported by the fact that the trees generated by *PAUP* represent them in this way. Ne and Cx1 have the same tale order, but it is not clear why Ra2 appears next to these. The tale order in Ra2 is clearly different, since it includes TG, L12, L22 and L34 and lacks L2, L3 and CO.

In plate 7, Ra1, classified as **b**, is the nearest manuscript to Hg. The Hg order is truly anomalous, in the sense that its links have been altered to cover for mistakes made by its scribe. It is likely that the scribe copied the SQ ME and FK tales before he copied the links. The scribe had left the space for the links to add them between the tales, but he had copied the tales in an order that was not supported by the links:

[T]he link between Squire and Merchant copied onto the verso of fol. 137 is copied in the hand of the Hg scribe but in the ink used for the last half of section III, suggesting a late addition. The link used may have been altered to fit this position though it could be an early version. The same applies to the link on the inserted fol. 153 which, along with decorative gaps, also contains the first twelve lines of the Franklin's Tale. It has been argued. . . . that the texts of the Nun's Priest and Manciple and the two linking passages in Section IV were probably the last work of the Hg scribe as he endeavoured to 'complete' the manuscript. It has also been noted that at this final stage the

supervisor in charge of the supply of texts and the ordering and organising of the material took no further part. None of the folios in yellow ink show any features of his work. It would seem then that the Hg scribe knew that he could not adequately link the tales in the order in which they had been copied. Nevertheless he used or adapted available material to make the manuscript appear more complete. If the Hg scribe acted on his own initiative, the supervisor may not have overseen the copying of material in the last half of Section III or advised on the placement of the two linking passages in Section IV (Inks).

In fact, Hg has two lines of SQ in f. 137v; the rest of the page probably was, as Stubbs suggests, written at a later time in a yellowish ink. The text following these two lines in f. 137v is L20, which goes to the end of the page. The textual variants between Hg and El at this point witness the scribe's alteration of the text to fit the order in which he had already copied the tales. The initial rubric in Hg reads: '☛ The prologe / of the Marchaūtes tale ~.' El, on the other hand has: '☛ Heere folwen the wordes of the Frankeleyn to the Squier ~ and the wordes of the hoost to the Frankeleyn ~ ~ ~.' Each manuscript follows the pattern established in the rubrics.

Quod the Marchant⁷ considerynge thy youthe

So feelyngly thow spekest⁷ sire I allow the (Hg ll. 3-4)

☛ Straw for youre gentillesse / quod oure hoost

What Marchaūt / pardee sire wel thow woost (Hg ll. 23-24)

¢ That knowe I wel sire / quod the Marchant c^oteyn
I prey yow / haueth me nat in desdeyn (Hg ll. 27-28)

Quod the Frankeleyn / considerynge thy yowthe
So feelyngly thou spekest^r sire I allowethe (El ll. 3-4)

¢ Straw for your^e gentillesse / quod oure hoost^r
What Frankeleyn / p^odee sire wel thou woost^r (El ll. 23-24)

¢ That knowe I wel sire / quod the Frankeleyn
I prey yow / haueth me nat in desdeyn (El ll. 27-28)

In these lines, the changes made in Hg by the scribe are quite evident. Sometimes, they have dramatic results on the meter of the line (l. 27). In L17 we also find that the meter of the line has been altered to such a degree that it would be difficult to accept that it is not the result of scribal intervention:

¢ Sire Frankeleyn / com neer / if it your^e wille be
And sey vs a tale / for certes ye (Hg ll. 23-24)

¢ Squier com neer / if it your^e wille be
And sey somewhat of loue / for c^otes ye ~ (El ll. 23-24)

It seems evident that after copying the tales, the scribe realized that the links suggested a different order; Stubbs' suggestion of the ink-color indicating a later addition seems correct, and the idea that the links arrived at a later date than the tales is acceptable.

Instead of correcting the mistake, the scribe attempted to cover it by altering the names in the links to make them look as if they should be linking the tales in the order in which he had copied them.²¹

The section from SQ to L14 has been completely modified in Hg and it includes NU. The Hg sequence FK NU CL is only found in two other manuscripts Ad3 and Ha5. There are not many points of coincidence between Hg and Ad3, but this link is important because Hg does not have (and never had) L33 and CY, while Ad3 has these two immediately before L37 and PA. There is only one other manuscript with the same sequence, L33-CY L37-PA, Ch. Another remarkable fact about Hg is that an alteration was made to the name of the pilgrim in L37 (the current reading in Hg in the Parson's Prologue is 'manciple'). This might indicate another change in the order in Hg, perhaps due to the intervention of the scribe or maybe due to the fact that part of the text never became available to him to be copied into Hg.

The previous discussion shows that the overall shape of the tree can be explained if we observe the closeness of certain witnesses to one another and compare it to the tale-order table. To do this by hand is a much more complex task, so much so that Manly and Rickert did not see the possible relationship between Bo1 and Ph2 and the c group. In other cases, such as the situation of Ch near To, we discover that the connection implied by the consecutive positions which Manly and Rickert assigned to these manuscripts is confirmed by the results yielded by the phylogenetic software.

It is important to stress that, not only the positions, but also the distances between the different items are informative. For example, in the case of Gg Ps and Hk, it is

possible to see that even if their positions remain the same the length of the branches of the tree indicates that there are not as many similarities between these as the unrooted cladogram seems to suggest. However, even with all the differences between Hk and Gg, *PAUP* still proposes that these have a closer relationship with each other than with any other witness.

2.2.2 Stemmata Based on Data Encoded Using IEBP

After the analysis of the trees built based on Spencer's method was carried out, STEMMA came across new ongoing research at the department of computer sciences at the University of Texas. There, Tandy Warnow and Li-San Wang have been working on new distance coding methods (Wang and Warnow 636-46). The exchange between STEMMA and Warnow and Wang resulted in a new coding of the tale-order data. This new method is the Inverse of Expected BreakPoint distance method (IEBP), which calculates the possible number of movements of each item in a sequence:

IEBP estimates the true evolutionary distance using an approximation to the relationship between evolutionary distance and expected breakpoint distance, under the assumption that all transpositions are equally likely. Simulations show that phylogenetic reconstructions based on IEBP distance are more accurate than those based on breakpoint distance. (Spencer, "Gene Order")

To produce the new coding, the tale-order table was revised and sent to Wang and Warnow. They coded it according to the principles of their method (IEBP). The nexus

file produced at Texas can be seen in the appendix. Although the Wang-Warnow method clearly offered better results than breakpoint distance when used with *SplitsTree*,²² the trees produced with these program were, once more, uninformative (See plates 11 and 12).

The new nexus file was fed to *PAUP* and yielded the results shown in plates 13 and 14. If we look at the unrooted cladogram (plate 13), we immediately discover that the groups appear in different positions on this tree. In order to establish any differences between the IEBP and the breakpoint stemmata, we have to rotate one of the trees until both of them are in the same relative position. By rotating the stemma in plate 13, 90° to the right, we have the **a** group at the top of the tree and **b** and **d** groups at the bottom left of the stemma.²³ If we now compare plates 7 (upper bound unrooted cladogram) and 13 (IEBP unrooted cladogram) we can see that the **a** group remains in the same relative position from the cluster where we find Gg. However, in this group, El has moved and it appears between Gg and the majority of the **a** group, while Ps and Hk pair with other witnesses. The cluster formed by Bo2 Dd Ad3 and Ha5, in plate 13, appears closer to the rest of **a**, instead of being separated by Ps Hk and Gg (as it is in plate 7). In fact, this is partly due to the new placement of Gg near El. When using IEBP encoding, we find that Hk pairs with To and that both of these spring from the same node as the Bo2 Dd Ad3 and Ha5 cluster. Analyzing this, the only difference at this point is that plate 7 shows an extra node between the Ad3 cluster and the Hk one. Ps, which in plate 7 shows in the Gg group, pairs with Se in plate 13. If we keep going down the tree, we find that Ha4 and Wy appear next, but in inverse positions to those of plate 7.

IEBP seems to suggest that Ha4 is closer to the **a** group than Wy, and it also places Cx2 one node removed from the latter and another from Ch. This could be further confirmation of a possible common source for the variants in both of these printed editions and Ha4 and Ch (Bordalejo 359 and ff.). Other similarities between IEBP and the breakpoint trees are the separation of Bo1 and Ph2 from **a** and their close relationship to one another, as well as the lack of a clear distinction between groups **b** and **d**, and the evident separation of **c** as a distinct and individual group.

However, even among so many common features in the stemmata produced using these two methods for the treatment of the data, in the IEBP unrooted cladogram it seems, once more, that Hg could be related to the **c** group. A peculiarity of this tree is that Hg appears closer to Fi and Ii than to any other witness. Once more, this can be explained by the sequence SQ ME FK which is unique to Fi Ii, and the **d** group. However, Fi and Ii, like Hg, separate CL with other items from this sequence.²⁴ Robinson's hypothesis that hand b (the Hg scribe) made changes to the Hg exemplar which were later transmitted to the **b** group appears supported by the association of Fi and Ii and Hg.

There is a striking similarity between the lower bound (plate 8) and the IEBP unrooted cladograms in the cluster which in the latter includes Ld2 S11 Tc2 Ph3 Mm and Pw. This cluster is comparable with the one in which in the lower bound unrooted cladogram includes Ld2 S11 Py Ph3 Mm and Pw. Py was classified as anomalous by Manly and Rickert, but the overall structure of the order in this manuscript could have fitted the **d** group pattern (although Ps does not have TG, a tale characteristically found in

the **d** group. In the unrooted phylogram, Py appears very close to Ra2 and Ln). In fact, it is closer to these than the other witnesses in this branch.

In plate 14, the IEBP unrooted phylogram, we can see the distances between the different witnesses. For example, in the case of Hk and To (two manuscripts that appear very close to the **a** group) the distance shown in plate 14 suggests that, although they are related, they are not as close to each other as some other witnesses. Once more the length of the branch indicates that it is likely that there were many permutations between one order and the other.

The relationship between Hg Fi and Ii appears to become more evident, since these manuscripts seem to derive from a same common origin in the branch. Another remarkable feature of this stemma is the closeness of Wy Ha4 and Cx2. Cx2 only differs from Cx1 in the addition of L31 and the movement of L8-SQ, but just these characteristics are enough to make Cx2 appear next to Wy and Ha4 instead of in the position of Cx1, which shows up together with the **b** and **d** groups. Cx2 is in the same branch with Ch and Ld1 (both of which were classified as anomalous by Manly and Rickert). The closeness of these five witnesses, all of them anomalous, suggests a genetic relationship between them. Not only do these share the sequence L15 ME...SQ L20 FK, but Cx2 and Wy also have L8 between ME and SQ (Ha4 has L8 after ML).

The changing position of Hg and Ch, for example, could suggest that the order in these manuscripts is not closely related to any other order or that the changes on them, rather than being genetic, are the result of scribal intervention. If a scribe or his supervisor, actively and radically changed the order of an exemplar, it becomes difficult

to sustain the notion that there should be a clear genetic relationship between the resulting manuscript and the one used as its exemplar (of course, this might depend on the number of introduced changes and their originality). However, even when changes have been purposely introduced (a fact which might appear as the unstable position of a manuscript on the trees) the consistency of certain groups in the stemmata points towards a genetic origin and subsequent transmission of many of the tale-orders found in manuscripts of the *Canterbury Tales*.

¹ This is by no means an attempt to survey all the different methods currently available. Instead, it is an attempt to clarify some differences and explain why some methods work or not.

² For further information about this method see Wang and Warnow (637-646).

³ STEMMA is currently exploring the applicability of networking programs to manuscript traditions. However, this research is just starting and it might be some time before it has yielded practical results. Networking might be useful to display the relationships of highly contaminated textual traditions, but this remains to be proven.

⁴ See also Griffith (101-38); Cameron (227-42); Platnick and Cameron (380-5); Robinson and O'Hara, ("Report" 331-37; "Computer-Assisted" 53-74; "Cladistic" 115-137); Robinson ("Best-Text" 71-103; "Stemmatic" 69-132); Robinson et al. ("Phylogeny" 839); Howe et al. ("Manuscript" 147-52).

⁵ See also Howe et al., ("Parallels").

⁶ In ribonucleic acid (RNA) thymine is replaced by another base called uracil (represented as U).

⁷ A good example of word variation in which the final state implies at least one previous state is SQ 491, where the Hg line reads Hg "¢ Ther I was bred / allas that ilke day" while El reads "¢ That I was bred / allas that harde day." Manly and Rickert's explanation states that there must have been an intermediate state in which a scribe miscopied 'ilke' as 'ille' and that this reading, in turn, was converted into 'harde.' (4:482-3)

⁸ See Page and Holmes; Spencer and Howe 467-84.

⁹ The key-words here are 'extant data,' while other methods might try the least amount of changes (minimum evolution) to build their trees, maximum likelihood attempts to explain the data that has been provided. In textual criticism terms, this philosophy is very close to that of the New Stemmatics, which presupposes that because the extant manuscripts are not the totality of the textual tradition a stemma built based on them would only map the relationships of these and should not attempt to express witnesses we do not have. If other witnesses were to be discovered in the future, these could be added to the analysis and the new results would be another step in the study rather than a unique solution to the problem.

¹⁰ The version of PAUP used for this research allows us to produce distance trees using options such as "Constrain branch lengths to be nonnegative" and "Collapse branches of effectively zero length when searching." The use of these options should help with the accuracy problems foreseen by Nei and Kumar, and greatly reduces the number of resulting trees, by excluding some branch re-combinations.

¹¹ Spencer and Davidson also present an interesting list of divergences between the aims of evolutionary biology and textual studies. Among these differences, the most interesting one is about the aims of both disciplines: "Another important difference is that biological evolution is continuous. After an evolutionary divergence, both species continue to change, so neither directly represents the ancestor. Phylogeneticists

therefore assume that contemporary species should always appear on the tips (terminal nodes) of the tree. In contrast, once a manuscript is produced, the text it contains does not change (except through corrections and damage). Some extant manuscripts may be the ancestors of others, and should therefore be represented by internal nodes of the stemma" (Spencer et al. "Artificial"). This observation also applies to the tale-order stemmata (since it is likely that some of the extant orders originated the other ones), but as this work does not presuppose a direct correlation between stemma and reality, the conceptual difference of the possibility of allowing internal nodes or presenting each witness as a terminal node should not present an insurmountable problem.

¹² In fact there are other methods (and even more programs which follow them) such as least squares and spectral analysis, which are not discussed here. A discussion of least squares can be found in New and Kumar, and spectral analysis in Page.

¹³ IEBP was devised by Wang and Warnow. For more details refer to their article "Estimating true evolutionary distances between genomes."

¹⁴ See the nexus files for the distance matrix of the tale-order data in the appendix.

¹⁵ There are two sets of trees here the upper and lower bound because of the way in which the data was processed. BreakPoint distance is defined as "the number of items common to both manuscripts and having different right-hand neighbours." The difference between upper and lower bound is that: "The lower limit occurs when no common items were lost and the upper limit is approached if there are many lost common items" (Spencer, "'Gene Order' Analysis"). When there is missing data, it is not possible to know the number of items which are missing, if the presupposition is that two witnesses have not lost corresponding parts of the text, this is best represented by the lower bound. When witnesses are so damaged that one can assume that many corresponding items are lost then the data is best expressed by the upper bound. Because it is impossible to decide *a priori* which one of these might offer better results, I have analyzed both the upper and the lower bounds.

¹⁶ The word-variant data is generated automatically using *Collate* while the tale-order data has been coded using different methods (Spencer et al., "Analyzing;" Wang and Warnow 637-46). It is possible that this difference might be the result of the difference in coding generated by the use of distance methods, while *Collate* offers a format which might be better for *SplitsTree*.

¹⁷ The nexus files contain both the data and a protocol for the software. These can be seen in the appendix.

¹⁸ See Feinstein, 45-60 and Bordalejo, "The Manuscript Source of Caxton's Second Edition of the *Canterbury Tales* and its Place in the Textual Tradition of the *Tales*."

¹⁹ At this point of the research, I want to present the data as it is in the manuscripts. After my codicological analysis of the chosen witnesses, the data will be revised to conform with the making of the data provided by a more detailed analysis of the manuscripts.

²⁰ But see plate 5 for an idea of the distance between the Gg and Hk orders, for example.

²¹ This and other aspects of the codicological analysis of the manuscripts are discussed in Chapter 6.

²² The fit of the tree is tree times as high when the encoding method used is IEBP than the one obtained with breakpoint distance. The fit of the tree is an indicator of how adequately *SplitsTree* has been at handling the data.

²³ The rotation does not affect the relationships between the branches of the trees. Because the tree is unrooted what really matters are the relative positions of the items in the tree (Robinson, "Analysis").

²⁴ Hg has NU between SQ L20 ME L17 FK and CL, while Fi and Ii have WB L10 FR L11 SU in that position. Ht has WB L10 FR L11 SU NU L33 CY L34 PH L21 and PD between SQ L20 ME L17 FK and CL.

CHAPTER V: RELATIONSHIP BETWEEN THE TALE-ORDER AND THE WORD-VARIANT STEMMATA

This chapter analyses the differences and similarities between tale-order stemmata and word-variant stemmata. This analysis suggests that the correspondences between the two kinds of stemmata are consistent with their variants having been inherited from one manuscript to another. Here I explore the strength and reliability of the relationship between tale-order and word-variant stemmata and try to explain the cases in which the relationship appears stronger and why. An important outcome of this chapter concerns the places in which the tale-order and word-variant stemmata present differences. These are likely to represent deliberate intervention in the ordering of the tales and might suggest where the origin of some of the orders (which were later inherited by other witnesses) can be found. For this chapter I compare the trees built based on tale-order data with all the available trees based on word-variants produced by Robinson for the Canterbury Tales Project and published in single-text CD-ROMs.

Manly and Rickert postulated manuscript groups based on tale-order, but these differed from their own word-variant groups, and in both cases they had unclassified witnesses which could not be fitted into any of the groups. Manly and Rickert were unable to explain why some manuscripts such as El seem to belong to a tale-order group (a) but cannot be easily classified according to their word variants; for this reason doubts

were cast on their tale-order groups. However, if tale-order can be transmitted from one manuscript to another, so too will be word-variants. That is, theoretically, the differences in word order should follow the same lines of descent as differences in tale order. In that case, what remains to be explained is how and why there are discrepancies between the two sets of groupings.

The General Prologue on CD-ROM includes several word-variant stemmata, each corresponding to a limited number of lines of GP. These stemmata were built using data from lines 1 to 250, 251 to 500 and 501 to the end; the methods used (especially the order in which they are used) are justified as follows:

Where the data is indeed 'tree-like', this method [parsimony] works very well indeed. However, such cladistic programs¹ can produce rather misleading results when the data is not tree-like, as they will find trees whether there are trees to be found or not. Accordingly, the Project uses PAUP only on sections of data in manuscripts where SplitsTree suggests that the cladistic method might be useful. ("Analysis")¹

At the time, this justification of the use of PAUP and parsimonious analysis, for areas in which the data had appeared to be tree-like, was valid. It would not have been a good idea to force a tree structure if the data had indicated a different kind of relationship (a network, for example).² However, the development of the software package PAUP has not stopped and changes have been implemented. In fact, as mentioned in chapter 4, the most recent versions of PAUP include not only parsimony, but also other methods, such

as maximum likelihood and distance.³ The SplitsTree stemma for the first 250 lines can be seen in plate 15.⁴ About this figure Robinson writes,

For these manuscripts, for this data, SplitsTree suggests that they do not appear to be related in a 'tree-like' manner. However, SplitsTree does suggest that for some seventeen manuscripts, for this data, there are 'tree-like' relationships. These are the four manuscripts Cn/Ds1 /En1 /Ma, in the bottom centre of the graph; the manuscripts Lc/Mg/Ha2, in the centre right; the group Cx1 /Cx2 /Pn/Wy/Ii/Ld1 /NI/Tc2 in the bottom right; and the pair En3 /Ad1 on the centre left. All these small groups appear to radiate from a single point separate from the centre; in some cases, radiation from separate points may suggest further archetypes within the group (as in the grouping Cx1 /Cx2 /Pn/Wy/Ld1 /NI/Tc2). ("Analysis")

Robinson also includes a PAUP rectangular cladogram using the same data, but he has also included the nexus file which can be executed in any version of the program and which was used to produce plates 16 and 17. Plate 16 has been produced using the data from *The General Prologue on CD-ROM*⁵ using parsimony (as Robinson does in his "Analysis") but the tree has been drawn as an unrooted phylogram. The main features which Robinson pointed out about his SplitsTree stemma also apply to plate 16. In fact, the **c** group and a couple of other manuscripts (Cp Sl2 La Pw and Se) form a cluster in the top left corner. The stemma also shows pairs which are known to be related such as En3 and Ad1 (alpha manuscripts) and Bo1 and Ph2 (**E** manuscripts). Plate 17, a stemma

built using PAUP using distance instead of parsimony, shows, once more, similar relationships; however, the relative positions of Hg and El have changed. In this plate, Hg and El appear very close to Ch, and all of these belong to a branch which is also shared with Ha4 Ps Bo1 and Ph2. This difference between the stemmata suggests that the relationships among these manuscripts should be explored further. However, it is also possible that some of the non-tree-like features mentioned by Robinson might be causing an interference with some aspects of the stemma. To solve the problem of possible interference from conflated texts, Robinson reduced the number of witnesses to be used on his analysis; then, for the first 250 lines of GP he presents a tree with 24 witnesses. Plates 21 and 22 show that, once the texts suspected of conflation have been removed, the different position of Hg and El in relationship to Ch is roughly the same. Of the three possible stemmata built using parsimony (plates 18 to 20), only the one in plate 20 shows a variation in the relationships between these manuscripts. In fact, if the stemmata are built as phylograms rooted at Hg, one of them shows a clear difference with Robinson's published rooted phylogram. The relationships in question are those of Hg El and Ch, mentioned above. Among the stemmata found in *The General Prologue on CD-ROM*, we find the following:

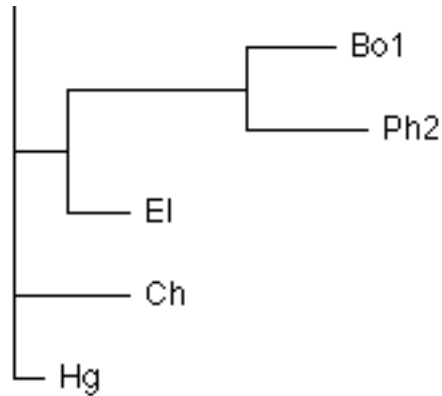


Figure 3. A partial view of the parsimony consensus tree based on the GP data

Although the text of Hg and El is close for GP, Robinson states that they belong to different lines of descent. In his analysis there are three main lines of descent (alpha, from which the **a** and **b** groups descend; **cd** and **E**) and there are manuscripts that appear to directly descend from O (Bo2 Ch El Gg Ha4 Hg Ln Ps Ra3 To1) about which Robinson writes:

Seven of these ten (all but Gg Ln To1) are among the seventeen classified as O group in The Wife of Bath's Prologue. Within this ten, it appears that El and the trio Ch/Ha4/Hg may represent two independent lines of descent.

("Analysis")

In this way, Robinson puts forward the idea that there might be a closer relationship between Ch Ha4 and Hg, while El could be further from Hg and other manuscripts related to it.⁶ In general, Robinson's stemmata (plates 18, 24 and 27⁷) suggest that there was a very early split in the tradition, where what he calls the alpha exemplar was copied introducing a series of variants that can be found in the descendants of this manuscript.

Two of the manuscripts which Robinson considers closely related to alpha are Ad1 and En3, and these appear on the same side of his stemmata (plates 18 and 27) with the **a** (Ma Cn En1 and Ds) and the **b** group manuscripts (Ii Tc2 Cx1 and Nl).

The relationships shown by the GP word-variant stemmata partly confirm Manly and Rickert's groupings; these point towards clearly distinguished **a** and **b** groups, and to less easily distinguishable **c** and **d**. That is, group **b** appears to descend from **a**, forming a single group, while **c** and **d** appear clustered together in a different branch of the stemma. The **cd** cluster appears in relative proximity to Hg and El, and the reasons for this are not completely clear. These stemmata also suggest that there might be connections in areas in which the manuscript relationships have not been established with clarity yet. As shown in chapter 4, the tale-order stemmata present some similarities with the word-variant ones. Some groups are clearly differentiated; for example, group **a** appears precisely separated and so does group **c**. Although **b** and **d** are drawn apart from the other groups, they are not distinguished from one another; that is, in the tale-order stemmata **b** and **d** appear as an undifferentiated cluster. In this way, a division which in the word-variant stemmata appears clear (that of the **b** and **d** groups) is not so clear in the tale-order ones.⁸

Another very significant difference between the tale-order and the word-variant stemmata lies with the positions of Hg and El. Both of these manuscripts are closely linked in the GP word-variant stemmata, while they are clearly separated in the tale-order ones. Although the mechanical reason for this is relatively evident (the manuscripts have different tale-orders) the factors which originated the differences are not so obvious.⁹ However, in the WBP stemmata a pattern emerges which links El to manuscripts other

than Hg.¹⁰ The first stemma (plate 34, figure 2 in Robinson, “Stemmatic”) corresponds to lines 301-400 of WBP and the second (plate 35, figure 3 in Robinson, “Stemmatic”) to lines 401-500. In the first one, El is grouped with Gg Si Bo1 and Ph2, that is, with Robinson's **E** group:

This suggests that El descended from an E exemplar up to about line 400 [of WBP]: up to that point, it has 44 of the 79 E variants. Thus, it will also be descended from an EF exemplar up to about line 400, as are all E witnesses: up to line 400, it has 12 of the 23 EF variants.

After line 400, El changes character dramatically. There are only two E and EF variants of a possible 93 from line 400 to the end of the [Wife of Bath's] Prologue, compared to 56 of a possible 102 up to line 400. From the increase in number of O variants (eight of twelve after 400 compared to one of sixteen before) El appears to move to an exemplar considerably closer to that of the O witnesses from line 400. (“Stemmatic” 110)

Robinson's hypothesis for this change of exemplar is not only related to the word-variants, but also to the so-called 'added passages' of WBP. According to Robinson, when these passages are present in an **E** group manuscript (as in the cases of Si and Gg) they are likely to be there due to contamination (“Stemmatic” 110). The El scribe might have changed his exemplar after line 400, to a manuscript of the alpha group, so he could include the 'added passages,' which were not present in the **E** exemplar he used for the first part of the prologue. Although Robinson does not make clear why the scribe chose

not to include the first passage (WBP 44-1 to 44-6) from his second exemplar, his hypothesis appears plausible.¹¹

The **E** group appears to be derivative and quite far from Hg and other early manuscripts (also separated by a larger number of copies from O). PAUP places it in a bigger group together with the **a** and **b** manuscripts and, if we do not accept Robinson's hypothesis,¹² it might be difficult to explain the shifting positions of El in the word-variant stemmata. These indicate a change from the **E** group to an alpha manuscript,¹³ while the tale-order stemmata clearly place El with the **a** group manuscripts.¹⁴ Although the previous chapter has shown that often the tale order was inherited when a manuscript was being copied, the points in which the tale-order and word-variant stemmata show disagreement suggest that, in all likelihood, a purposeful change was introduced into the copy.¹⁵ If this is true, it might be possible to find some codicological evidence to support the claim. For example, in the case of Hg, we know that the copying order and the manuscript final order are unlikely to be the same; that is, that the tales were copied in certain order and were later placed together to achieve a specific (and unique) tale-order.¹⁶

My own hypothesis about the apparent difference between the tale order and the word variants present in El is that this manuscript might ultimately derive from the **a** hyparchetype, but that there are a series of exemplars between them.

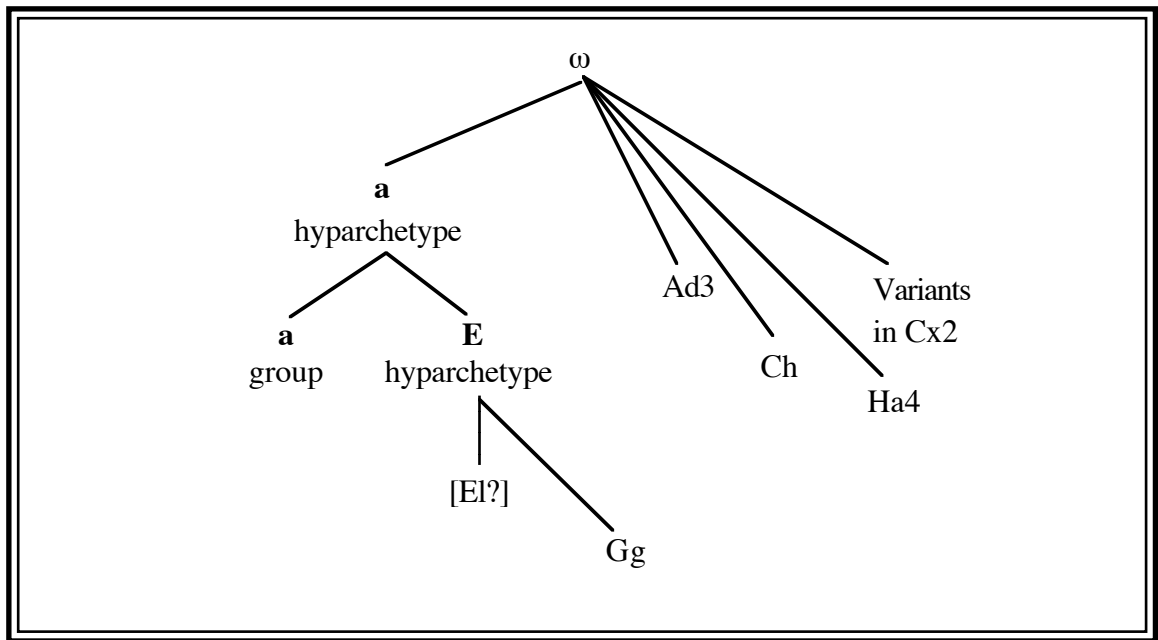


Figure 4. A hypothesized stemma with the position of El¹⁷

This stemma hypothesizes that El and the **a** group have the same common origin, presumably in a manuscript that had the order found in these manuscripts or one very similar to it. The **a** hyparchetype probably had all the 'added passages' in WBP and these were transmitted to the **a** and **b** manuscripts. It is possible that, in parts, the **a** hyparchetype was very close to O, and that some of these passages were transmitted to the **E** hyparchetype and subsequently to El. Parts of the text of the **E** hyparchetype were not as close to O and these were also transmitted to El. In this way, it would be possible to explain why the El order corresponds to that of the **a** group, while its word variants change in nature in different parts of the text. However, if we go back to Robinson's hypothesis (which refers only to WBP), we can attempt to apply it to the text of El as a whole. He thought that the shift of exemplar in El occurred because one of the

copy-texts employed by this scribe lacked 'added passages,' and the second copy-text was brought so the scribe could add the lines to the copy he was making. Although this is a possible explanation, it does not account for the fact that the scribe did not just add the lines, but directly copied from this second exemplar (the one with the **E** affiliations). I have found that this affiliation with the **E** group occurs in KT and SQ (Bordalejo 117 and ff.), and that it is also likely to be found in other parts of the text. The possibility remains open that this could be due to the scribe's familiarity with the text and his ability to recognize when his copy wanted some lines or passages. However, it is also possible that the changes has no relation to the familiarity of the scribe with the text, but rather to the fact that he was copying from a text preserved in a defective manuscript likely to have been closely related to O. If this had been the case, the scribe would have found that his exemplar lacked parts of the text, and he had the need to supply these, perhaps from a less reliable exemplar which introduced the **E** variants. The reason for El to have the **a** order group would be that the scribe decided to adopt the order of the more reliable manuscript (presumably the first one with the **a** or similar to **a** order) instead of the one of his other copy-text. There are a couple of problems with this theory. Firstly, El and some of the **E** manuscripts (Gg Bo1 and Ph2) were considered as belonging to the **a** group by Manly and Rickert,¹⁸ and are now being shown as differing from it by the tale-order stemmata.¹⁹ Secondly, it is problematic to explain the consistency of El and Gg in these **E** word-variants in parts of the text when El has been considered a very reliable manuscript of the *Tales*.²⁰ If it were shown that both manuscripts are consistent for the whole of the text (or major parts of it) then the theory of the shift of exemplar would immediately

become doubtful, since the statistical possibilities of two manuscripts following exactly the same patterns of shifting are very low.²¹ If a complete collation of Bo1 and Ph2 were to show that these two follow the same patterns, then we would have to rethink the idea of the shift of exemplar. However, one cannot leave out the fact that these manuscripts consistently group together in the word-variant and tale-order stemmata (see plates 14, 22, 25 and 28). A possible explanation is that the **E** hyparchetype was a conflated text and that this conflation was transmitted to its descendants. The main question to answer then would be when and how the **a** order originated and if any of the extant manuscripts, which follow that order, can actually be identified as its origin.

As pointed out before, a very striking difference between the tale-order and the word-variant stemmata has to do with groups **b** and **d**. These appear indistinguishable in the tale-order stemmata, while they show clearly apart in the word-variant ones. Manly and Rickert already pointed out that there are clear and specific **b** variants in the **b** recension of the text (Manly and Rickert 2:57); some of them are actually the result of trivialization,²² while some others are just the product of mistakes in the copying process. However, this is not to say that **b** and **d** did not share a common hyparchetype with **c** early in the tradition (Manly and Rickert 2:43-4). If we accept Dempster's idea about the common origin of the tale orders of groups **b** and **d** (Dempster), then we have to account for the fact that these are two distinct groups in relation to their texts.²³ Moreover, it seems quite clear that, textually, the **b** group is related to **a** (Robinson "Commentary"), while the **d** group is related to **c**, and that both clusters (**a/b** and **c/d**) belong to different branches of the textual tradition. In this case, a possible explanation is that the tale order

of the **b** hypearchetype was modified following the tale-order pattern of the **d** manuscripts. The latter include L20, TG and some spurious links such as L34 (linking CY and PH) and L22 (linking PD and SH). These seem minor additions to the text, since they do not affect the overall sequence of the tales.²⁴ In contrast, the changes between the tale orders of **a** and **b** witnesses are remarkable for manuscripts we know to be genetically related in their word-variation and are likely to have had their origin in the same hyparchetype. These changes (of **b** in reference to **a**) include the addition of L8 (the Man of Law's Endlink), which is attached to SQ, the position of (L15)-ME immediately after SQ, and the placement of CL-L13-L14- FK after PD and before NU. The cluster NU-L33-CY is immediately before PH-L21-PD in the **b** group, while it appears just before L36-MA in E1 and other manuscripts with **a** tale-order. These differences between the **a** and **b** orders seem to justify the idea that the **b** order came from one of the **d** manuscripts, and if this were true then Dempster would have been right about their common origin. This common origin, however, would not be due to an evolutionary transmission process. Instead, it would have its source in the conflation of a text derived from the **a** group and an order inspired by a **d** manuscript, elements which would have both been present in the **b** hypearchetype.

The positions of some of Manly and Rickert's 'anomalous' witnesses vary greatly from one word-variant stemma to another. For example, in plate 16, which is based on GP data, we find Ha4 clustered with Ch and Hg, suggesting that this manuscript (at least for GP) has a very early text. In plate 14 a similar relationship can be seen, with Ch and Cx2. Hg does not appear clustered with other manuscripts in the tale order stemmata, and

this is probably because its order was created by the scribe as the manuscript was copied (as opposed to being copied from the exemplar itself) and put together in an order which was not transmitted to any other extant witnesses. Therefore, it is not surprising to see Hg emerging from a long branch on its own in plate 14.²⁵

As it has pointed out in chapter 4, Ha4 is separated from Ch in the tale-order stemmata by two witnesses: Wy and Cx2. I argued there that, in all likelihood, the order of Wy comes from an ancestor of Ha4. Garbáty's study indicates that Wynkyn de Worde used a defective copy of Cx2 which he completed with a very good, unknown manuscript. The order of the tales in Wy is the same as that of Ha4, not including TG. However, it seems possible that the inclusion of TG in both Ha4 and Ch had to do more with the scribes' knowledge of the existence of the tale and its use and position in other manuscripts as the Cook's tale, rather than with its presence in the exemplar used to produce Ha4 or Ch.²⁶ In figure 4 above, the stemma shows a possible relationship between Ch Ha4 Ad3 and the variants in Cx2.²⁷ Such a hypothesis could explain the closeness in position of Cx2 Ch Ha4 and Wy in the tale-order stemmata, but it would fail to explain why Ad3 is clustered with the **a** group. This manuscript has a very peculiar order with NU directly after SU and before CL (an order shared only by Ha5). Especially interesting in Ad3 is the unique position of the clustered L3-CO L33-CY immediately after L36-MA and before L37-PA. There is evidence which suggests that a possible position for CY was immediately before L37-PA, and this evidence, in conjunction with the word-variant data, makes Ad3 a particularly interesting witness.²⁸

In the tale-order stemmata (see plate 14) some of the 'anomalous' witnesses (Ld1 Ch Cx2 Wy Ps Se) seem to emerge from relatively close nodes, and Hk and To come from the same long branch, but seem closer to **a** witnesses than to any other group. It is interesting to point out that in the WBP word-variant stemmata,²⁹ Se Ad3 and Ch all radically shift positions. Ad3, for example, after line 400 of WBP seems to change so much that its position, relative to Ha5, is completely different. Before line 400 in WBP, Ha5 and Ad3 are a close pair, both one node removed from Ch, but this relationship appears different after line 400 (the manuscripts are still relatively close in comparison with their proximity to others, but there are many intervening nodes between them).

As mentioned before, all of the word-variant stemmata agree in placing Ad1 and En3 in a cluster some nodes removed from Hg (this becomes even clearer in the rooted trees). These two manuscripts, together with Ad3 and Tc1, form Robinson's alpha group. In the tale-order stemmata all of them appear clustered (in diverse relative positions) with the **a** group. This is consistent with the existence of an alpha group, defined by Robinson as follows:

[I]t appears that there were two consecutive Dd/AB exemplars. The first, α , contained fifteen (or so) differences from O [in WBP] which were inherited by El, as well as the 'added passages.' This α witness was also copied again, into Dd/AB, with this copy introducing the additional readings shared by Dd and A (for example the renumbering of the husbands) but not shared by El. . . It is also likely, from the errors shared by El Gg Ad3 in the added passages, that all three of these took the text of these passages not direct from the α

exemplar but from an intermediate ancestor (unless, indeed, Gg and Ad3 took them from El itself.) (“Stemmatic” 123-4)

Robinson clearly thinks that alpha originated both groups **a** and **b** and other related manuscripts, but he also asserts that alpha was a direct copy of O (“Stemmatic” 124), which makes this hyparchetype much more difficult to trace. The evidence of the tale-order stemmata, however, suggests that his α manuscripts are indeed related. As mentioned above, Ad1 En3, as well as Ad3, cluster with the other **a** manuscripts. This might represent yet another clue as to where the **a** order originated, for, if it was indeed the order in α , then we should expect that this is how it was transmitted to El, the **a** and the **E** groups. If this were true, it would show that the tale order was transmitted from copy to copy in these groups and it would also show that there is a relationship between the **E** and the **a** orders. Although the possible existence of the α manuscript could shed some light on the nature of Ad3; the origin of the variants which Ad3 shares with Ha4 Ch and Cx2 becomes more obscure. One should not discard the possibility that these shared variants might be archetypal in origin and therefore cannot be said to indicate any other relationship between these witnesses than that of having had their origin in O.

In contrast with other manuscript groups, the **d** witnesses have been somewhat overlooked. The reason may be that their text is a late text and that they include links and tales which are considered spurious, and so their position in the tradition and relevance to the textual history of the text have not been considered as important as that of other witnesses. However, it is possible that the **d** group is not as straightforward as it has been thought and some of its peculiarities might be of interest for the understanding of the

development of the text. For example, although in the Manly and Rickert table the witnesses are clearly grouped according to their tale orders, Pw Mm Gl and Ph3 (all of which have an evidently different order from that of the other **d** witnesses are labelled as **d**³⁰). Naturally, because these manuscripts share a characteristic sequence, the tale-order stemmata (plate 14) shows Pw Mm and Ph3³¹ clustering apart from the **d** and **b** witnesses. In the WBP stemma of lines 301-400, these witnesses also appear remarkably close, clustering with the **c** manuscripts (La Cp Ld1 etc) rather than with the rest of the **d** group. This might suggest the presence of a further subgroup, unidentified up to this point.

Generally, it can be said that there are many areas of confluence between the tale-order and the word-variant stemmata: the consistency of the **c** and **a** groups, the relationships between witnesses of the **α** group, and the distinct presence of the **E** group. The idea behind this work (when it started) was to show that the tale order was inherited from one manuscript to another and that genetic relationships could indeed be built using tale order as a basis. It seemed important to confirm that tale-order, like any other textual feature, could be transmitted from an exemplar to its copy and that scribes indeed had the tendency to do this. This is true in many cases, as shown above; however, much more interesting are those cases in which the tale-order and the word-variant stemmata differ. A witness which contains word-variants that belong to a particular group and a tale order which reflects a different one might show the place in which the order of the tales changed. This indicates that, at such points, the scribes, their directors or their editors made deliberate decisions about the position of the different tales. One can imagine two reasons why the order of the tales in a manuscript was deliberately altered. In the one

hand, it is possible that the text, as received by a scribe or his supervisor, was incomplete or arrived in batches. In the other hand, it is conceivable that scribes and their supervisors, like editors today, might have felt the need to create a better arrangement of the tales than the one found in the exemplars they had available to copy. In this way, we can assume that these two kinds of intervention respond to different needs. One of them is more practical: to present a manuscript in a more or less coherent order without paying too much attention to the internal evidence provided by text or its sense (perhaps the case of Hg).³² The second kind of need is more ‘intellectual.’ Its aim is to make as much sense of the text as possible, and perhaps even to attempt to complete it (possibly, as seen in the **a** group, or in the insertion of TG as the Cook's tale). These alternatives are explored in chapter 6 where, with the help of codicological analysis, I attempt to establish whether the studied witnesses present evidence which might help us to understand the development of the different tale-orders.

¹ The program used by Robinson is *PAUP*. He refers to it as ‘cladistic’ software.

² See chapter 4, where SplitsTree is discussed.

³ All the tale-order stemmata are distance based because the data was coded following this method. See chapter 4.

⁴ This figure has been taken from Robinson's “Analysis.” Manuscripts excluded: BASE Ad4 Bw Dd D1 Do Gl Ln Ne Ra2 Ra3 SI1

⁵ This data is included as a nexus file in the CD-ROM in a folder called “docs.”

⁶ This coincides with some of the results of my research on the manuscript source of Cx2, where it was noted that a close relationship between El and Robinson's **E** group is likely for certain sections of the *Tales*, such as SQ. See the work I have carried out on word-variation in “The Manuscript Source of Caxton's Second Edition of the *Canterbury Tales* and its Place in the Textual Tradition of the *Tales*.”

⁷ In fact, plates 18 and 24 show this split quite clearly, it is less clear in plate 27, that is, it is less clear for lines 501 to the end of GP.

⁸ The possible common origin for groups **b** and **d** was first suggested by Dempster and is discussed in chapter 4.

⁹ The origins of the different tale orders are discussed in chapter 6.

¹⁰ These stemmata were published first in *Occasional Papers* 2 (“Stemmatic”). I have not built stemmata based on the WBP data.

¹¹ WBP 44-1 to 44-6 is the only added passage appearing before line 400.

¹² Robinson has also pointed out that the **E** and **F** manuscripts possibly had the same hyparchetype: “The significance of this is that it implies that the extant witnesses of groups E and F (and the manuscripts which are closely affiliated with them, notably El and Ha4) are actually removed by at least two stages of copying, both introducing significant error, from the archetype of the whole tradition.” (Robinson, “Stemmatic” 90).

¹³ Since alpha itself represents one line of descent from the archetype (O) technically, had it existed, it would have been an **O** manuscript. The added passages are found in witnesses which belong to groups **a**, **b** and **E**, as well as some **cd** manuscripts (Ry1 Se Ha2 and Ld1), Ch and Ad3.

¹⁴ My research on the manuscript source of Cx2 suggests the possibility of El being descended from the same hyparchetype as the **a** group but being further away from the archetype of the tradition. This is especially clear in WBP SQ and KT. See Bordalejo 350 and ff.

¹⁵ The codicological analysis of the manuscripts suggests that many scribes hesitated about the contents of their copy text and looked for sources to complete the *Tales*. See my discussion in chapter 6 about the inclusion of TG in Ha4 and Ch.

¹⁶ See Stubbs, *The Hengwrt Chaucer*. For a more detailed discussion of the codicological evidence in Hg see chapter 6.

¹⁷ This stemma has been taken from Bordalejo 374.

¹⁸ See Manly and Rickert’s table in volume II of their work.

¹⁹ These manuscripts appear consistently together, clustered apart from the **a** group to which Manly and Rickert assigned them.

²⁰ Bo1 Ph2 Si and other candidates for the **E** group have not been fully transcribed and, for this reason, it is impossible to tell if they are also consistent in their word-variation.

²¹ One possible exception to this would occur if it could be proven that both manuscripts were copied in the same workshop from the same exemplars (that is, if the manuscripts were sisters) by scribes who knew each other and were purposely imitating one another.

²² See, for example, CL 1067, where Bo2 El Gg and Hg have the reading ‘supposed’ in rhyme position, a likely eyeskip, since this is the rhyme word in CL 1065. In this same place, Ad3 Ch Cp Cx2 Ht La Ra3 have the reading ‘purposed,’ which is likely to have been present in the archetype of the tradition; and the **b** group (Cx1 Dd Ds En1) have ‘disposid,’ a clear non-archetypal substitution of the *lectio difficilior*.

²³ See Manly and Rickert and Robinson, “Stemmatic,” “Commentary.”

²⁴ See table 2 for the complete running orders.

²⁵ See chapter 6 for details of the making of Hg and the origins of its tale-order.

²⁶ See my codicological analysis of both Ch and Ha4 in chapter 6, especially with reference to the inclusion of TG.

²⁷ This relationship is based in the study of word variants and has yet to be confirmed by further analysis.

²⁸ The idea of CY immediately preceding L37-PA is discussed in chapter 6 in reference to Hg and Ad3.

²⁹ These have been mentioned earlier in reference to El (Robinson, “Stemmatic” 76-7).

³⁰ See table 2.

³¹ Gl has been removed from the nexus files for the tale-order stemmata because it presents repeated items (L24 and PR), and the methods used for coding the data are not able to adequately handle such situations.

³² It seems likely that the Hg scribe did as the best as he could to arrange the text; that is, he attempted to fix the problems with the ordering of the tales without copying again certain sections of the text.

CHAPTER VI: CODICOLOGICAL ANALYSIS AND ITS IMPLICATIONS FOR THE TALE-ORDER PROBLEM

This chapter focuses on the codicological aspects of the early manuscripts (Hg El Cp Ha4 Gg Dd Ad3) and of some later witnesses, such as Ch, which have been posited as of primary importance based on their texts. The aim of this part of the research is to try to cast some light on the results yielded by the phylogenetic software, especially with reference to the differences between the tale-order and word-variant stemmata, and to show the codicological features which might help us to understand the development of the tale-order in the studied manuscripts.

Some of the physical features of these manuscripts, such as rubrication, quiring and layout, are especially important because they may help decipher the early textual history of the *Canterbury Tales* and the development of the different tale-orders. One of the main questions to be answered is whether or not it is possible that the order in these manuscripts might be the result of editorial or scribal intervention rather than transmission from the original. If two or more manuscripts have been used as a source for another, then the order of the resulting manuscript is presumably the result of conflation. If the order of a manuscript is suspected of being conflated, there is little point in including this order in any phylogenetic analysis. However, there might be cases in which the order of the exemplar of a manuscript can be inferred using codicological

evidence. In these cases a re-assessment of the data used for the phylogenetic analysis might be necessary.

In this chapter the manuscripts are discussed separately, except for El Gg and Dd. These are discussed in a single section and analyzed consecutively because, according to the Manly and Rickert table, they present very similar tale orders.¹ The similarities between the orders of these manuscripts are likely to be the result of an ultimate common ancestor although the different affiliations of their word-variants show that they belong to different subgroups.

1. CODICOLOGICAL ANALYSIS

1.1 Hg: Peniarth 392D, National Library of Wales²

In their tale order table, Manly and Rickert give two different orders for Hg. The first one is the re-bound order while the second one is that which was originally intended for this manuscript.³ From the beginning of my research on tale-order it seemed that including both orders (especially one that is the result of misbinding) in the phylogenetic analysis might create problems with the data.⁴ After a careful codicological analysis of the manuscript, Stubbs points out that there are five structural sections in Hg (all of them based on the re-bound order).

Structural section 1 includes GP, KT, L1, MI, L2, RE, L3 and CO (quires 1 to 8. Stubbs points out that MI which was written in two irregular quires and has a different ink color from the rest of this structural section⁵) Structural section 2 includes WB, L10, FR, L11 and SU, written in quires 9 to 12 with some blank pages at the end of quire 12.

Stubbs observes that the ink used for this structural section is the same as is used for the gloss after CO, indicating that Chaucer never finished the tale. Structural section 3 includes L29, MO, L30, NP, L36 and part of MA, and is written in quires 13 to 15.⁶ Structural section 4 includes L7, ML, SQ, L20, ME, L17, FK, NU, CL, L13, L14, PH, L21, PD, SH, L24, PR, L25, TT, L28 and TM. It is written in quires 16 to 29 and it is by far the longest section in the manuscript. There are three irregular quires in this section (quire 21, 22, and 29) and one of them includes a singleton leaf (folio 154). Stubbs distinguishes three different inks used in this section, and one of them (the 'yellowish' ink) is used for L20, L17 and the beginning of FK. Structural section 5 includes L37 and PA, and is written in quires 30 and 31.

Although the structural sections, as presented by Stubbs, are useful for understanding the structure of the manuscript and its process of composition they are not very helpful from a tale-order perspective, since they try to explain the present state of the manuscript and do not deal with the original order which Manly and Rickert had suggested. In fact, based on the evidence provided by the current state of the manuscript Stubbs concludes that Hg:

came together over a period of time, the last tales composed for it were perhaps copied years after the first were completed. It is clear too that the order of tales in Hg is not the latest order devised for *the Book of the Tales of Caunterbury* but was arrived at in effect by default. The order of tales in the first part of Section IV (ML, SQ, ME, FK, NU, CL), suggests an early attempt to join tales together without linking passages in an order which may later have

been superseded. . . . The position of Section II (WB, FR, SU), is probably the result of being copied late in the assembly of the tales, and coincides with the point when, for some reason, supervision of the manuscript appears to have ceased.

Stubbs also suggests that because the ink used for the CO gloss is the same as the one used for WB, L10, FR, L11 and SU, this must have been written while the “scribe was copying or had just copied” that group of tales. She also suggests, although it is not clear how she reached this conclusion, that up to the end of SU, the manuscript was being supervised and that at that point supervision stopped.⁷

Stubbs’ structural section IV presents some of the most interesting tale-order features in Hg. The way in which L17 and L20 were added, out of place and modified to fit the order in which the scribe had already copied the tales, makes it evident that he did not receive the whole text at the same time. On folio 137v we can see the different ink color used for L20 after the two last lines of ME. The text of L20 fits in the space left by the scribe, but, it is apparent that there is insufficient space to include the blank lines which usually precede and follow the rubrication.⁸ Indeed, a comparison of the spacing in 137v and 138r shows that the scribe made an effort to include the whole text of L20 (including the rubrics) in the space he had available.

Even though the scribe managed to include L20 with relatively little impact on the layout of the manuscript, L17 presented more complex difficulties. At this point in the text, evidently, something did not go according to the original plan. The text of ME on

152v ends in the middle of the page, leaving sufficient space for the twelve lines of the beginning of FK and for the rubric to indicate the new tale. Apparently, the scribe was waiting for these lines only and started copying FK 13 in what was at that time the subsequent folio. However, not only did the twelve lines with the beginning of FK arrive, but also 30 lines of L17.⁹ This posed a problem since the scribe had not left enough space for the amount of text he later received. In these circumstances he decided to add the only singleton leaf in the manuscript. This singleton leaf (folio 153) has most of the text of L17 on the recto and four lines on the verso together with the rubrics and the twelve lines of the beginning of FK. The use of the space on this folio suggests that the scribe was attempting to cover the excess space caused by the insertion of the singleton (Manly and Rickert 1:272); that is, the scribe did not have enough text to fill the page so padded out what he had. Further confirmation of this is provided by the textual evidence in the links where the lines in Hg become hypermetrical.¹⁰ This evidence leads to the conclusion that the scribe had to modify the links in order to use them with the order which he had mistakenly created. Immediately after folio 153 a stub can be seen. The reason for this might be that folio 153 was originally inserted as a bifolium, and part of the second folio was left so that it could be bound correctly and securely. All the evidence appears to support the idea that the scribe adapted L17 and L20 after having copied the tales which these links were meant to unite in the wrong order. This alteration of tale order is the result of a simple mistake which the scribe attempted to cover in the best way he could, at the same time making the best possible use of the vellum on which he had already copied the tales.

There are other instances in which changes in the manuscript are so subtle that, at first sight, they seem impossible to explain, for example, in folio 235r, where in the first line of L37 the word ‘Manciple’ has been written over an erasure, and about which Manly and Rickert say:

What was written originally and erased in line 1 cannot be ascertained even by ultra-violet light, but both the photostat and the photograph made with ultra-violet light seem to show three facts about the erased name: 1) its initial letter extended below the line; 2) it was slightly longer than “Manciple”; 3) a letter near the end of the name extended as much above the line as the **I** of “Manciple”. The only Pilgrim name possible for the erased word is “Frankeleyn”, which might have been written in full or with contraction marks for the **n**’s. “Frankeleyn”, of course, could hardly have been intended by Chaucer if PsT was to close the outward series of tales, but could if PsT was to be the last tale of the homeward series. “Manciple” is the reading of all the MSS that contain PsP except four that are badly disarranged, but Chaucer cannot have intended this, for the brief tale of the Manciple (248 lines) was told in the morning (cf. H 16), whereas the tale preceding Ps has just finished at four o’clock (I 5), when the sun was fast sinking (I 70). (1:276-7)

Manly and Rickert found a very precise place where the word variants might be of great significance for the tale-order problem, but did not see this as determinant in any way. In fact, they dismiss the manuscripts which have variant readings at this point as ‘badly

disarranged.’ But the variant distribution indicates that the manuscripts present three alternative readings in place of ‘Maunciple.’

Maunciple] Ad3 Cp Cx1 Cx2 Ds El En1 Gg Ha4 Ht Hg La Ry1

Marchaunt] Gl Ra3

Frankleyn] Tc1

yeman] Ch

out Ad2 Bo2 Bw Dd Ha5 Hk Ld2 Mc Ph3 Ps Ra1 Sl1 Sl2¹¹

Manly and Rickert thought that the original reading in Hg was likely to have been ‘Frankleyn,’ and that this was later replaced by the word ‘Maunciple,’ but do not explain how the reading happened to occur in Hg (before the correction). Blake has suggested a different reading:

Attempts have been made to read under ultra-violet light what was erased in the first line of PsP [L37]. The attempts have not yielded much that is concrete, though what is visible has not usually led scholars to suggest that the reading was originally ‘Somnour’ or one of its other spelling variants. Despite this, no other teller from the material available in Hg could so readily have occupied this position originally. (*Textual* 62)

With the inclusion of ‘Somnour’ as a possible reading, we have at least two distinct alternative readings. What seems unclear is why Blake constrains himself to the ‘material

available in Hg,' when it is possible that the lack of material might have been the cause of such a correction.

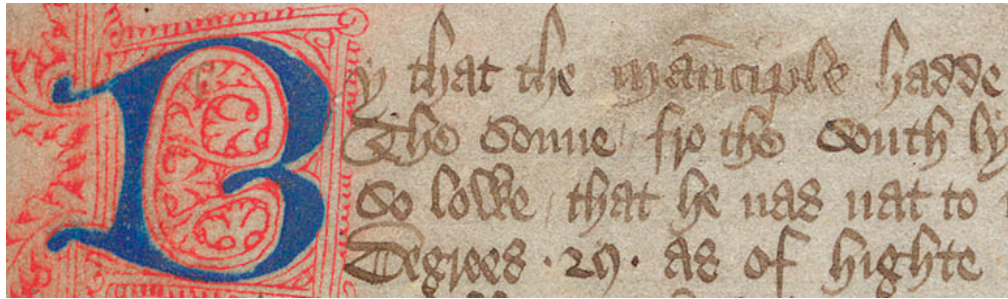


Figure 5. The first lines of L37 in Hg

The manuscript shows no clear signs of scraping where the word 'Manciple' now stands; instead an ink blotch can be seen in the space underneath the writing. If it were not for the ink blotch, it would be difficult to detect there had been an erasure. The quill that wrote the word 'Manciple,' however, is clearly different from that used on the rest of the page. In the letters 'm,' 'c,' and 'e' there is clear evidence that the quill was cut in a different way from that used for the rest of the page. In fact, both the ink color and the cut of the quill are similar to those used on L20, L17, and other texts in the 'yellowish' ink. These are the same as the ones used in part of quire 14 and the whole of quire 15 for the final rubric of MO (98v) L30, NP, L36 and MA. About the ink used for these passages, Stubbs writes:

The Nun's Priest's Prologue and Tale and the Manciple's Tale are both copied in an ink which is a distinctly different yellowish colour. This ink appears to be the same as that used to write the heading for the whole work, the Link and

first 12 lines of the Franklin's Prologue on the inserted leaf fol. 153, and the Link between the Squire and the Merchant. The final rubric to the Monk's Tale on fol. 98v was also written in the yellowish ink as the scribe began his copying of the Nun's Priest's Tale. The material in the yellow ink was the last part of the Hg manuscript to be copied, since it seems to include certain 'finishing' features. However there are other possibilities and a definitive order of copying is impossible to establish at the present time.

This suggestion that the ink used in the links, MA, the main title in 2r, and in the correction on L37 1 might have been the last ink used when the manuscript was being copied supports the idea that the change was a last minute one, and likely to have occurred because an alteration in the order of the tales was required.

It has been suggested (Blake, *Textual* 62; Manly and Rickert 1:276-7) that there might be traces of the word that was present before. For example, a very faded stroke above the letters 'ip' looks like a hook. Beyond the textual variants, the tale-order table 2 shows three manuscripts which have FK immediately before L37: Gl Ra3 and Tc1.¹² Ad3 and Ch have CY before L37.¹³ Mc has a sequence other than MA-L37, since this manuscript has MA immediately before NU-L33-CY. Although it is not possible to tell which was the order of Mc, since the manuscript is incomplete, either CY or FK could have been before L37, but so could have ME or PH-L21-PD, all of which are missing from it.

In Hg it is possible to see that the scribe had to rearrange L17 and L20 so these could serve as ME-SQ and SQ-FK as in El, so it is very unlikely that ‘Merchaunt’ or ‘Frankleyn’ was the reading in L37. Blake’s argument about the possibility of the reading ‘Somnour’ is based on the material available in Hg. However, a stronger case can be made that the original reading in Hg was ‘yeoman’, the reading that appears in Ch. Both Ch and Ad3, manuscripts which appear to be close to the archetype of the tradition, have the order CY-L37. If this had been the proposed order of the tales found in Hg’s exemplar, there would be a very good reason for the last-minute correction: the Hg scribe did not include CY (possibly because he never received it), so he decided to delete the reference to a character who was not named in the *Canterbury Tales* as he had copied it.¹⁴ This explains the need for a change in the reading in Hg, as well as the word-variant in Ch and the Ch Ad3 tale-orders, since these manuscripts have texts which are often seen in agreement with the archetype of the tradition (Bordalejo 364 and ff.).

In general, however, the tale order in Hg has been considered to be a very old one, which at least one scholar insists was supervised by Chaucer himself (See Stubbs). Skeat, on the other hand, suggested that the Hg order was a provisional one, closely derived from the archetype:

I propose to show, before hand, that we actually possess one MS. Which may be fairly regarded as approaching to the idea of an archetype; a MS., namely, in which the Groups of tales appear, at first sight, to take quite a casual order; a MS. in which they may have been committed to writing with a view to future re-arrangement. By such re-arrangement we must, of course, construct a

scheme that is necessarily the *oldest* of the four more orderly schemes, from which also, in turn, each of the later schemes can be naturally developed, in regular succession. The MS. to which I refer is the Hengwrt MS., which must, in any case, be considered, since it is generally agreed that it is, with one exception, the best we possess, at any rate as regards the grammatical forms.

(*Evolution 6-7*)

From Skeat's perspective, Hg preserves the oldest tale-order, perhaps even a Chaucerian one. To agree with Skeat, one would have to accept that Hg is indeed the oldest extant manuscript; although this is possible and, indeed, very likely to be true, it cannot be used to argue say that its order is authorial. It is very improbable that the changes in L17 and L20 were authorized by Chaucer since these imply an alteration of the meter which breaks the rhythm of the poem. Instead, it is more likely that this scribal misplacement might have originated the uncertainty about the relative position of CL FK ME and SQ in a large part of manuscript tradition (Manly and Rickert 1:275). It seems plausible, then, that the order of Hg was one of the first attempts to arrange the tales left by Chaucer; although it does not seem likely that the making of this manuscript was supervised by Chaucer as suggested by Skeat and Stubbs. Evidently, if Hg had been supervised by Chaucer the positions of L17 and L20 would not have been confused and the uncertainties about other parts of the text (such as the unfinished CO) would no have existed.

1.2 Cp: Oxford, Corpus Christi College, MS 198¹⁵

Cp is considered the earliest manuscript of the **c** group; although, its date in the overall consideration of the manuscripts of the *Canterbury Tales* remains a matter of disagreement. The debate is centred on the fact that the scribe of this manuscript, identified by Doyle and Parkes as ‘hand d,’ also copied Ha4. Blake has clearly stated that he thinks that Cp is earlier than Ha4:

[T]he evidence supports the contention that Ha7334 does follow Corpus in the hierarchy, for many of the features in Ha7334 are explicable only on the assumption that Corpus came earlier. Although the fact that both exhibit different methods of organization could be taken to suggest that either manuscript has precedence, the way in which the chapter arrangement gives way to the link-tale arrangement within Corpus confirms that it must have preceded Ha7334 in which the different method of organization is more fully carried out as though it was developing what had started in Corpus. (*Textual* 119)

Blake’s argument rests on his opinion that “many of the features in Ha7334 are explicable only on the assumption that Corpus came earlier.” However, he does not explain all these features. Instead, he reduces the whole problem to a single argument: because Cp changes from an order in chapters to one of tale-link-tale, and Ha4 is arranged from the beginning in tale-link-tale order, Cp must have been copied first. This argument is not substantial enough to offer a solid theory about which manuscript was

copied first.¹⁶ The opposite view, that Ha4 is earlier than Cp, has also been proposed. For example, Seymour explains: “[t]he priority of copy is not certain, but linguistic textual [*sic*], decorative and editorial comparisons suggest that MS. Harley 7334 is earlier” (13). Once more, there are no specific examples of the features which appear to be so relevant for the chronology of the manuscripts. Apparently it is left to the individual scholar to make up her mind and decide (if at all possible) which of these manuscripts should take chronological precedence over the other. Perhaps more telling than the uncertain date of copying are the characteristics shared by these manuscripts, as described by Seymour:

They [Cp and Ha4] have certain features in common (e.g. the inclusion of *Gamelyn*, the insertion of the Modern Instances in the middle of the Monk’s Tale, and the expansion of the Nun’s Priest’s Prologue) which argue some linking of their lost ancestors, but overall there is no close textual correspondence between them. (14)

In fact, Cp has the short version of L30 (the Nun’s Priest’s Prologue), while Ha4 has the long version. This inclusion of the long version of L30 depends on the assumption that a different copy text was used.¹⁷ The inclusion of TG and the Modern Instances also occurs in other manuscripts that are not necessarily related to Ha4 or Cp.

In Cp, TG starts four lines from the bottom in folio 62r in the second half of quire 8 (Plate 38). There is no discernable hesitation on the part of the scribe in copying this tale and no instructions from the supervisor can be found on the page. In fact, the only

rubric reads 'Incipit Fabula' with no further indication as to who the teller of the story might be, or how it related to the rest of the pilgrimage. Probably these have to do with the chapter arrangement. Blake interprets the appearance of TG in Cp as follows:

It is often suggested that it [TG] was found among Chaucer's papers and therefore included in the poem. This may be so, but since the number of possibilities is so large it is perhaps wiser not to speculate. It may have been included after CkT because a conclusion had been expected for that tale by the Hg scribe, and when one failed to materialize it was necessary to adopt a different solution to the problem. (*Textual* 104)

This hypothesis appears to imply that the Cp scribe (hand d) knew the Hg scribe (hand b) or that he knew Hg or about the gloss at the end of CO. In other manuscripts, for example Ha4, TG is clearly filling in for the incomplete tale by using running heads making it explicit that this is also the Cook's tale. It is not possible to discern whether this might be one of the Blake's "unnamed features," suggesting that Cp was produced earlier than Ha4. However, at a point at which the text of Ha4 required an indication, the text of Cp seems to flow without any apparent vacillation. In fact, of the three manuscripts analysed in this section that have TG, only Cp introduces it without any signs that might allow further analysis, so that, it would appear that Cp was being copied with the idea of having TG at this point. If this assumption is correct, it is an indication that very early in the textual tradition of the *Canterbury Tales* it was assumed that TG was part of the text and was assigned to the Cook.

As in some witnesses of the **b** and **d** groups, Cp uses L8 as the prologue to SQ. Once more, this differs from Ha4, which has L17 (as in El) to introduce SQ. L15, often used as a prologue to ME and not present in Hg, is also absent from Cp while present in Ha4.¹⁸ L20, as in all manuscripts of the **b** group, was not included in Cp (a peculiarity also shared by some anomalous witnesses, such as Ch Hk and To). The scribe did not feel the need to complete SQ as he had done with CO. Blake observes:

With respect to Gamelyn we may note that the Squire was never provided with another tale in the way that the Cook was. This we may attribute to the way the scribe of Hg set up the text. He allowed for a link to end the incomplete SqT and when that was written it effectively prevented any different solution to the incompleteness of SqT being proposed. (*Textual* 104)

Once more, we find that Blake links a feature in Cp to the copying of Hg and, although his idea remains possible, it is more likely that the different treatment of SQ in Cp from that of CO had more to do with length than with any of the characteristics of Hg. Other reasons might have concerned the availability of texts (perhaps TG was the only ‘floating tale’) or with pressure to finish the work.

An important group of tales, formed by NU L33 and CY, also appears in Cp in what today is considered to be a peculiar position. Following the order of Hg, Cp has the sequence FK-NU, but the latter is followed by L33 and CY as in El and other **a** manuscripts. Blake remarks that even in such an early manuscript there seems to be very little doubt as to which position this tale and link should occupy (*Textual* 98).¹⁹ The **a**

manuscripts include NU-L33-CY just before L36-MA, but here Cp follows the order of both Hg and Ha4, which might indicate a closer link to these manuscripts than to others.

Cp might be a very early manuscript, and, if Blake is correct in his assessment, it might be the earliest manuscript except for Hg. However, this is not proof that its order or its text is better than that of other witnesses. It remains possible that, when copying Cp, the scribe and his supervisor were trying to make sense of the poem as best as they could, but that the results were not as accurate or reliable as they might have been. Only when a complete collation of the text of Cp and the other witnesses has been made might we be able to reach a decision about the text in this manuscript.

1.3 Ha4: British Library, MS Harleian 7334²⁰

Originally, Ha4 had 38 quires (today quire 21 is missing). These are quires of 8, except for quires 9 and 19, with only six folios, and quire 38 with two.²¹ The order in this manuscript is very similar to the **a** order.²² The only exceptions are the presence of L8, which is not present in manuscripts with the **a** order, the inclusion of TG, which has a note in French to indicate the position it should occupy, and the position of NU-L33-CY, a group that appears before PH-L21-PD SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP, rather than after as in the **a** order. These similarities with the **a** order suggest a there might be a relationship between them and raise the possibility that a manuscript very close to Ha4 may have been the origin of the **a** order. Skeat suggested that Ha4 might contain the latest order as devised by Chaucer himself:

I shall proceed to show that the chronological order of the types of the seven chief MSS., with reference, that is to say, to their contents and arrangement, but without regard to the actual dates when these individual MSS. were written, is as follows: --Hengwrt, an archetype; Petworth, showing the first scheme of arrangement; Corpus and Landsowne, the second, Harleian, the third; Ellesmere and Cambridge, the fourth and last. In the first three schemes, we find Chaucer himself, at work, making various experiments. In the last scheme, we find the work of a careful editor. It follows that the authoritative type, the only one which arranges the Tales as Chaucer at last left them, is the Harleian. It is anything but final, and even some obvious mistakes remain. But we have *no authority* for proceeding further. (*Evolution* 9-10)

Although Skeat's suggestion of the order of development of these texts is plausible, the idea that Chaucer supervised the composition of their orders has been questioned. Blake has expressed his doubts about Skeat's hypothesis as follows:

If Ha7334 has Chaucer's final order, why does it have such a bad text? If Chaucer made these various orders at the end of his life when the poem as we have it was substantially complete, they must have been executed very quickly one after the other. If this is the case, it is not clear why Chaucer had these varying orders written out in manuscripts, since that would have been costly, or why these orders should have been 'published' so that further copies of them could be made. Skeat rejects Gamelyn from the Chaucer canon, although it occurs in Ha7334 which contains his most authoritative order. How did an

authoritative manuscript come to contain a tale which was not genuine?

(*Textual* 30)

Blake's observations on Skeat's position appear just since one cannot find answers to these objections in Skeat, but Blake does not acknowledge that some of Skeat's statements might still be sustained. It seems clear that Chaucer did not supervise Hg, or he might have found a better solution than the alteration of L17 and L20 to fit the new, incorrect, sequence SQ ME FK. This fact seems to weigh against the possibility of Chaucer having supervised the Hg order and, since this manuscript is considered to be a very early one, makes it much more unlikely that he supervised Ha4. Although Skeat's hypothesis that Chaucer supervised the production of some of the manuscripts of the *Canterbury Tales* has been defended on several occasions (see Stubbs), scholarly consensus still weighs heavily against this possibility. Skeat, however, is essentially right when he affirms that none of these orders is totally satisfactory, and that the one which has been considered the most authoritative by editors in the twentieth century (the one of El), is likely to have been the result of decisions by a scribe and his supervisor.

The main argument against Skeat's view that Ha4 was the result of Chaucer's latest order is Blake's important objection about the inclusion of TG. The inclusion of TG in Ha4 can be explained by analyzing the quiring in Ha4. TG begins in the first of the two quires of six in Ha4. This tale starts at the top of folio 59r, but at the bottom of 58v there is a note of the supervisor, "lcy comēcera le fable de Gamelyn."

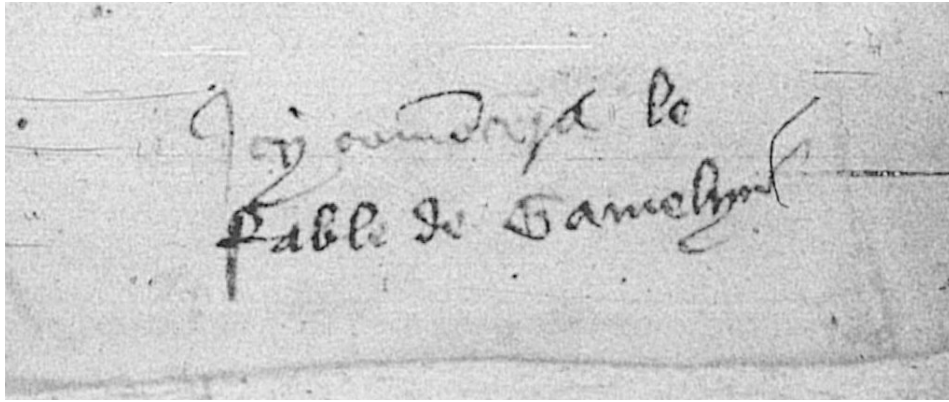


Figure 6. Gloss in folio 58v of Ha4

If we follow Mosser's collation (which appears to be correct if one takes into account the catchwords in the manuscript) 59r is in the middle of quire 8, which ends in f. 64. The ink color does not change and the *ordinatio* remains as at the beginning of previous tales. If it were not for the supervisor's indication, there would not be any evidence that TG was unavailable when the scribe was writing the section. Indeed, Blake thinks that the marginal note was prompted by motives other than the absence of the text:

When the manuscript [Ha4] was copied, Gamelyn was held back in order that a linking passage with CkT could be provided. It may be that it was going to be allocated to a different pilgrim or it may have been intended to link it in some way to the unfinished CkT as part of the Cook's story. The scribe simply arranged his quires in such way that he could include Gamelyn when the link was ready. (*Textual* 110)

There are two questions which remain unanswered. Firstly, if the scribe was told to wait for a link which was being prepared and had to be included before TG, why was this never included? Blake might have been thinking of what he now calls L6 (which links

CO and TG in La) or L5 (found in the **d** witnesses).²³ There is an inconsistency here since it appears that Ha4 has very different textual affiliations from those of the **c** group, while Cp and La are clearly related; why would the link have been prepared for Ha4 and not for Cp? Second (and Blake might not have been aware of this, since he did not analyze it), at least one other manuscript, Ch, presents the same uncertainty in quiring and a definite change of ink (among other characteristics), which suggest that TG was added after the rest of the text had been copied.²⁴ If we take a different view from that of Blake (that TG was held back when Ha4 was being copied), we might need to reconsider the dating of Cp and Ha4, and the possibility that the latter might be earlier than the former. It would be difficult to contest the paleographical or linguistic evidence in one way or another, and, for this reason, I cannot make a judgement about which of these two hypotheses might be correct. However, the textual evidence I gathered for my work on Cx2 indicates that the variants in Ha4 have the tendency to be archetypal; that is, independently of its date of production, the text of this manuscript seems to be older than that of Cp.²⁵

Another indication that TG might be a later addition is that the second half of the tale and its ending are in quire 9, which has six folios instead of the normal eight. If it is the later, then one must conclude that TG was not present in the exemplar from which hand d was copying. This would explain the need to add an indication of where to introduce the text. It would also account for the quire of six where TG ends, which would have been added after the rest of the manuscript was copied and when the tale had been found. If TG indeed comes from a different source from that of the rest of the manuscript, then it would be justified to suppress it from the tale-order analysis.

If TG was not in the manuscript from which Ha4 was copied, the order found in this manuscript would be virtually the same as that in Wy.²⁶ It seems plausible that there might be a relationship between Ha4 and the corrections in Wy. Although the textual affiliations of the base texts of the two witnesses are different, a collation of the corrections made in Wy would allow us to test this hypothesis.²⁷

Quire 19 is the other quire of six in Ha4. This quire contains the ending of ME, L17 and SQ. The text of ME ends on 148v, where the last line of the last couplet is placed. On the same page we have L17 and a rubric that indicates the end of L17 and the beginning of SQ. The text is continuous here, and, if it were not for the changes present in Hg in L17 and L20, there would be no other evident explanation for hesitation at this point of the text.

1.4 The Order of the a Manuscripts (Gg: Cambridge, University Library, Gg.4.27,²⁸ Dd: Cambridge, University Library, Dd.4.24,²⁹ El: San Marino, California, Huntington Library, MS 26.C.9)

The order of the manuscripts belonging to Manly and Rickert's **a** group is by far the best-known one. It is featured in all editions based on El, most notably in Robinson's edition and in the very commonly used *Riverside Chaucer*.³⁰ Manly and Rickert placed 15 manuscripts within their tale-order **a** group. Of these only one seems to have exactly the same order as El: the manuscript in question is Gg, which lacks many pages. Manly and Rickert's analysis led them to conclude that the order in Gg would have been the same as that in El, if the manuscript had not lost any pages. Seven other manuscripts (Dd

En1 Ds Cn Ma En3 and Ad1) appear to have an order that is very similar to that of El, but include minor additions such as L31 and L32.

Although there are similarities with other manuscripts which Manly and Rickert included in this group (Ad3 Ha5 Ad2 Bo1 and Ph2), these are not enough (or there is not enough evidence) to classify them as **a**. Bo2 is too incomplete and has a significant change in the position of MA and NU, perhaps suggesting that this manuscript had placed CY before L37. Ad3, discussed below, has placed L3-CO towards the end of the book, has L33-CY immediately before L37, and NU in the same position as does Hg. Ha5 is too incomplete to judge, but might have had the same or a very similar order to that of Ad3. Ad2 is too incomplete, and only a few fragments remain.³¹ Bo1 and Ph2 definitely represent a different edition of the *Tales*. They both lack CO, ME, SQ and the links which normally surround these and CL and FK. They also include L22 and L34 to link PD-SH and CY-PH respectively. Word-variant analysis of Bo1 and Ph2 places them with Robinson's E group.

1.4.1 Dd: Cambridge, University Library, Dd.4.24

Dd was an overlooked manuscript for many years. It was considered to have been copied by an amateurish scribe and to have an unreliable text with a great number of alterations (Manly and Rickert 1:101). Blake analysed it as part of his study in *The Textual Tradition of the Canterbury Tales*, where he points out:

After section 1 Dd 4.24 has section 3, as had now become standard. Instead of having ML endlink after MLT, Dd 4.24 has WBP. It thus exhibits the same

decision arrived at in Ha7334 but without any of the indecision found in that manuscript. Ha7334 had included the ML endlink in a truncated form and adapted it into a false SuP. This was clearly an impromptu decision which was quite unsatisfactory, and the scribe of Dd 4.24 adopted the sensible solution of simply omitting the ML endlink. (125)

According to Blake, the Dd scribe ‘omitted’ TG in a similar way to that described in the above quotation for the omission of L8. Blake also suggests that the Dd scribe is less hesitant than the scribe of Ha4.³² The reason for this could be that the Dd scribe might have been copying the order from a previously copied exemplar. However, such a hypothesis would enter in direct conflict with the conclusions of the most important study carried out about this manuscript. Recently, with the support of the Canterbury Tales Project and under the supervision of Blake and Robinson, Orietta Da Rold completed a doctoral dissertation on the manuscript.³³ The thesis focuses heavily on codicological matters, so that its raw data is particularly useful for the purposes of this research.

In general, Da Rold insists on two main points: the Dd scribe was a professional, and the manuscript was produced earlier than believed hitherto. The first point has very little impact on the current work, since professional scribes are as likely as amateurish ones to introduce changes in the tale order. The second point could be relevant if it were convincingly shown with the support of codicological evidence, in view of the importance of establishing which of the manuscripts with this order was copied before the other **a** manuscripts. Da Rold’s main argument about the date of the manuscript is based on paper evidence. She investigates the watermarks about which she states:

The watermarks of the paper in Dd offer a very interesting issue, which can be reiterated as follows:

1. The watermarks of the paper of quires 1-8 do not have exact matches, but the closest ones are dated 1392-4.
2. The watermark of the paper of quire 9 has an absolute parallel found only in Briquet Archive 1416.

The gap between 1392-4 and 1416 is too large to suggest that the manuscript was written more or less at the same time. There are two possible solutions to the situation:

1. Quires 1-8 paper-stock was around for more than twenty years or later absolute matches are found for quires 1-8. So far no evidence has been found to support this thesis.
2. The second paper-stock existed earlier than 1416. (Da Rold 68)

Although Da Rold's work is very thorough, it is difficult to understand the conclusions she reaches based on the paper data. She relies on close matches for the early dating (1392-4) of the first paper stock but rejects the evidence of exact matches for the later (1416). It is conceivable and, indeed, very likely that the second paper stock existed before 1416, as Da Rold points out, but it is unlikely that this paper was closer to the 1392-4 than to that of dated documents which use this paper and which are dated 1416. It seems much more probable that the date of the paper in Dd and, therefore, the date of the

copying of the manuscript, is more likely to be closer to that of the watermark-matched dated paper stock. However, Da Rold concludes:

Dd could be an earlier manuscript than previously considered and could have been produced at the very beginning of the fifteenth century. A later date could match with the second stock of paper, but it would be too late for the stock of paper used in quires 1-8. (70)

The problem with this statement is that it seems to be based on an absolute watermark match for the first paper stock, which Da Rold admits she could not find. For this reason, the conclusion would have been more true to the actual data had it been more focused on the accurately, but not definitively dated, firmly matched second paper stock. It might still be argued that the second paper stock in Dd could have been much earlier than the date assigned to it.³⁴ It is also possible that the dated document which Briquet used to assign the date used this paper many years after it was produced.

Early in her work, Da Rold appears to conclude that the manuscript was not copied from the beginning to the end (Da Rold 86), very much like Hg. Indeed, ink color is one of the factors she names as supporting evidence of discontinuous copying (Da Rold 93). The assumption is that the scribe did not receive the complete text of the *Canterbury Tales* and, like hand b, had to copy the sections of the text that were available at the time these arrived. Da Rold puts forward a very convincing piece of evidence which she describes as follows:

On the right top margin of f. 105v, just below the ruling frame next to the gutter, there is a transfer of paraph in red. There must have been an opposite page that had a paraph mark in red that had not dried completely when the original two sheets were pressed together. Folio 106r does not have the red paraph mark, which indicates that this leaf was replaced. No extant leaf could have produced the transfer though many leaves are missing. . . .

The transfer on f. 105v is probably a consequence of the scribe re-arranging the ordinatio either of L15 or ME or both. . . . (80)

She goes on to describe how L15 starts on a new folded leaf with no space left for the initial rubric. The rubric is peculiar in that it was written in two languages. Da Rold states that “[t]he rubric seems to have been written in two different stages; I suspect that the reference to the Merchant was added later, using a different pen, shade of ink and language” (Da Rold, 80). If Da Rold were right, the gathered evidence would support her hypothesis of the rearrangement of the tales. However, even if the Dd scribe rearranged parts of the manuscript, it is unlikely that the **a** order was first used in this manuscript. The fact that the tales are copied across quire boundaries (that is, they contain part of a tale which continues in the next quire [Da Rold 90]) suggests that the scribe was probably copying a known order. It also indicates, if we follow Da Rold’s argument, that the Dd scribe (or his supervisor) is likely to have been familiar with the text of the *Tales* since, while copying, he was able to leave the necessary amount of space for tales and links he had not yet received. Indeed, the text was copied in such a way as to leave gaps at the end of CO and SQ:

The gaps after CO, SU, SQ and between PH-L21 and PD-SH can also be found in Hg. Some of these gaps coincide with the fragments that traditionally scholars have thought may reflect stages in the circulation of the poem by Chaucer. A recent theory has been put forward, which suggests that the gap after CO and SQ may be Chaucerian. . . . [I]t seems that in Dd the gaps are related to the ordering of the different parts. (91)

Dd, together with Hg Ch Cp El and Ad3,³⁵ has left a gap at this stage of the text. Even if these manuscripts have a space after SQ, this does not mean that the space is ‘Chaucerian.’ To assume that it is so gives rise to several problems, such as the understanding that medieval scribes and readers might have had of such a gap, and the way in which it was transmitted (since there is no evidence to support that the space should have been kept as part of the text). In all likelihood, the scribes thought that it was possible that the last part of the tale would eventually appear, and they provided for such a possibility. In fact, Hg and Ch show that it was possible to add further text and in a seemingly fitting manner.

The evidence does not point towards Dd as being the first manuscript with the **a** order, nor does it imply, as suggested by Da Rold (although it is conceivable that this could have been the case) that the “scribe was in with other scribes, who were working towards the compilation and organisation of the *Canterbury Tales*” (95). This manuscript, however, includes L31 (not present in El) and has some corrections (see especially those found in the ‘additional passages’ of WBP) which indicate that a different manuscript was used to introduce some changes. The relationship between the manuscript used for

the corrections, which has affiliations that have not been identified, and the order of Dd remain to be investigated.

1.4.2 El: San Marino, California, Huntington Library, MS 26.C.9

Scholars commonly agree on three things about El: it is a luxurious object likely to have been produced for a wealthy patron, its construction is regular and its text is of very good quality probably close to the archetype of the tradition. Parkes describes the physical aspects of the manuscript as follows:

The construction of the Ellesmere manuscript is straightforward, and like that most commonly found in English fifteenth-century books. The manuscript consists of twenty-nine quires of eight leaves (thirty, with the original endleaves), and, apart from the originally blank leaf (fol. 48) that follows the incomplete Cook's Tale at the end of quire 6, the text was copied across the quire boundaries. ("Planning" 41)

El exhibits no hesitation in its copying process so that it seems likely that its order was to be found in its exemplar. The El order is the same as that of Dd, but without the hesitation found in that manuscript. The question here is not whether El is related to the **a** manuscripts, but how closely it is related to them. Hanna, for example, greets with skepticism a possible link between El and Dd (Hanna, "Editing" 232). Hanna's suggestion about the El copytext is also interesting:

It is nevertheless abundantly clear that the El team had full access to a range of archetypes before copying began: these would have led them to see Hg as inadequate, on the grossest possible scale, in its presentation of the whole *CT* as it was known in the contemporary London book trade: the team would have been put off by such features of Hg as missing and misplaced links (for example, MchtPro and Sq-Fk Link, respectively), the absence of CYT, and the "short form" of NPro. These features would have suggested to them that a fuller consecutive text could be assembled for copying. ("Editing" 235)

It is difficult to imagine, however, that hand b, responsible for copying Hg, had not realized before copying El the problems with the order of the former. In all likelihood, he was responsible for the changes in L17 and L20, and might have been compensating for the absence of CY when he changed the name of the pilgrim in L37. It seems unlikely that hand b might have been surprised by the irregularities in the Hg order (especially those which he is likely to have created). This need to attribute special qualities to the El order appears to be a reflection of the general interest which the manuscript has generated. After all, even Hanna has accepted that El is a "slightly less accurate" version than that of Hg ("Editing" 236). Thus, we cannot be sure whether the canonization of the El order has to do with the fact that it is better, or Chaucerian, or just the order of the commonly "preferred" manuscript.³⁶

Pamela Robinson's remarks about the tale-order of El are of little help, in the sense that they present a series of questions and give hardly any answers. For example, she poses the questions of whether Chaucer decided a final order (245), whether the El

scribe presents a correct (Chaucerian) and final order of the tales, whether the El order came from its exemplar or exemplars or if it was an invention of his scribe (P. Robinson, “Order” 246). Her concrete conclusions about El are two: that L17 and L20 in El represent unaltered versions in comparison with the same links in Hg (P. Robinson, “Order” 250-2) and that any analysis of the manuscript is unlikely to offer an answer to whether the order on it is Chaucerian.

As stated above, the lack of hesitation in the copying of this manuscript and the fact that the manuscript was mostly copied across quire boundaries seem to suggest that the scribe might have been working from a pre-established order. In contrast with the copying of Hg, he must have had most of (or all) the text.

1.4.3 Gg: Cambridge, University Library, Gg.4.27

In contrast with the other manuscripts studied for this work, Gg is an anthology. It contains not only the *Canterbury Tales*, but also *Troilus and Criseyde*, the *Legend of Good Women*, the *Parliament of Fowles* and Lydgate’s *Temple of Glass*, among other texts.³⁷

Some of the leaves containing the text of the *Canterbury Tales* have been removed; that is, the manuscript has been mutilated, in all likelihood, to steal miniatures, illuminated capitals and ornamented borders (Manly and Rickert 1:173). There are some changes in the ink, but these are progressive, as in Ch, not sudden, as in Hg. This suggests that the scribe was probably working from beginning to end, rather than by fragments. Manly and Rickert state that the vast number of corrections suggest that the

scribe was working under supervision. But corrections introduced by the scribe himself are also interesting:

It seems worthy of note that almost always omitted lines are supplied not in the side margin but at the foot of the page as part of the regular allowance for the page. This suggests that before completing his page the scribe proof-read what he had written. The place where the omitted line belongs is marked with a sign and a letter corresponding to those attached to the omitted line. In view of the care exercised in this matter we may perhaps infer that omitted lines not supplied at the foot of the page were lacking in Gg's exemplar. (Manly and Rickert 1:174)

It is remarkable that both the Gg scribe and his supervisor took so much care about the accuracy of the text (although collations including Gg show that its text has many non-archetypal readings (See Bordalejo). The order of this manuscript is almost impossible to interpret, since it lacks many of the links and the beginnings of the tales. As Blake has put it: "Gg 4.27 is a difficult manuscript to analyse because many of its pages have been torn out. . . . Hence it is not easy to calculate what may have been found on a leaf no longer extant" (*Textual* 136). There is very little point in speculating about the tale-order in this manuscript. However, Blake thinks that the fact that Gg has the additional passages in WBP suggests that it might be later than Dd (*Textual* 136). My analysis of some the variants in Gg points in the same direction (since they appear to have been introduced late in the tradition [Bordalejo 206 and ff.]), but the passages in WBP do not appear to be a firm proof for a manuscript chronology.

1.5 Ad3: British Library, MS Additional 35286

This manuscript has 34 quires and has lost a few leaves. Quires 3 and 8 are missing, along with four folios of quire 1, the second folio of quire 7, the first folio of quire 17, the last folio of quire 24, the seventh folio of quire 33, the fifth, sixth and eight folios of quire 34 (Manly and Rickert, 1:41; Mosser).³⁸ All the quires are of eight folios (although some of them are incomplete, as already pointed out), with only the exception of quire 20, which is a singleton.

The order of Ad3 is unique, and there are no signs of hesitation in the copying process.³⁹ A clear change in the color of the ink can be seen in quire 11 (WBP 624), and some corrections have been made in darker ink. Simon Horobin, in his doctoral thesis affirms:

With regard to the content of this manuscript [Ad3] sticks rigidly to what we now consider to be the accepted *Canterbury Tales* canon, and there are none of the spurious additions that occur in other manuscripts. The scribe does not attempt to conceal any incompleteness or inconsistency by adding extra tales, such as TG, or by composing new links. The material missing from this text is due to subsequent loss rather than to scribal incompetence or problems concerning the availability of exemplars. (63)

Although the scribe “does not attempt to conceal any incompleteness or inconsistency by adding extra tales,” he does modify aspects of the text as he sees fit (see, for example, the suppression of the first two lines of L33 which removes the reference to NU).

This manuscript has L3 and CO in a very peculiar position, after MA. Mosser explains this as follows: “this positioning is unique, but logical in that it connects the Host-Cook-Manciple exchange in MancPro with CkT” (Mosser).⁴⁰ Horobin observes:

However, this extremely regular and controlled organisation of this first group of tales [GP to RE] is disturbed by the removal of CO from this constant group. Yet the manuscript itself shows no signs of such a radical disturbance, and the change is implemented with cool conviction. (58)

What Horobin appears to suggest is that there is no indication of doubt in the scribe’s plan. In his opinion, this manuscript also follows the pattern prologue-tale rather than tale-link.⁴¹ The change in position of the incomplete CO, however, is not as interesting as the fact that this tale is followed by L33 and CY. In the first folio of quire 31 (folio 211r) we find the ending of L36 (the Manciple’s Prologue), from line 89 to 104 (*Riverside* 89 to 104), and the beginning of MA up to line 22. The last line of MA is in 24v, and L3 starts in the same folio. L3 ends in 215r, where CO starts. In 215v we find the ending of what we have of CO immediately followed by L33 and then CY, which starts in 218r (the last page of the quire). The fact that no tale or link in this section of Ad3 starts at some point within this or the previous quires suggests that the scribe was copying this order directly from the exemplar in front of him, rather than composing it while he copied.

The misplacement of CkP and T and CYP and T in Ad3 cannot have been due to misbinding as the heading of MLP follows ReT, the heading of CkP follows the ending of McT, and the heading of CYP follows A 4422 --all within the

pages. Since Ha5 also lacks these tales in their usual positions, the misplacement probably occurred in $\sqrt{\text{Ad3}}$. (Manly and Rickert 1:44)

Table 2 clearly shows that Ha5 is missing L3-CO from its normal position after RE, which suggests that the scribe of this manuscript probably had located it in some place in the later part of the tales (presumably the same as in Ad3). NU in both Ad3 and Ha5 appears before CL, which is the same position in that these tales have in Hg. However, Ha5 is wanting after PD, and therefore we cannot be sure if L3-CO and L33-CY had been immediately before L37-PA, but the evidence is strong that L3-CO appeared in a different place in the ancestor of these manuscripts, as suggested by Manly and Rickert. The common link in the position of NU, which they share with Hg, strongly suggests that the ancestor of Ha5 and Ad3 was one of the first attempts to put together the *Canterbury Tales* with the inclusion of L33-CY. We should note that the association of CO and CY in Ad3 has been suggested to be a “late attempt at conflation to recover a missing copy and add it at the last possible place.” (Seymour 103) The problem with Seymour’s interpretation of the data is that it fails to account for the textual variant which names the Yeoman as the speaker who precedes PA and for the erasure in Hg below the reading ‘Manciple.’ Horobin is guilty of a similar fault and that of mainly taking into account Hg and El as reference points. His conclusion, “[t]hus the evidence of the ordering of Ad³ presents a unique blend of features relating to both the already-established *a* tradition, with close associations with El, and also earlier influences, particularly that of Hg (Horobin 76),” shows that he could not escape the enormous influence of Hg and El to realize the possibility that the exemplar from which Ad3 was copied might have belonged

to an independent line of descent and might have been very close to the archetype of the tradition, as seems to be suggested by the order of L33-CY-L37-PA.

1.6 Ch: Oxford, Christ Church College, MS 152⁴²

Ch has been dated c. 1460, but its text is considered to be an early one and, according to Manly and Rickert, some of the spellings found in it show a relationship to manuscripts such as Hg or El (1:88). They also state that:

The order of the tales shows that the MS was made up from various sources. In particular, Gam occupies the last 3 folios of Q3 (originally left vacant for the continuing CkT) and a complete quire of 10 leaves of paper with a different WM, the whole written in a different ink (also marked by blue paragraph marks not used elsewhere in CT portion except twice). It is, therefore, probable that the insertion of Gam was made after the writing of WBP, which begins the first leaf of Q5. (Manly and Rickert 1:88-9)

Codicological analysis of the manuscript confirms all the statements Manly and Rickert made about it. Seymour (201) and Mosser have also made reference to the different color of the capitals and paraph marks in TG. Seymour interprets this fact as a sign of the text having been included at a later stage in the copying process: “[b]lue initials in the later insertions of *Gamelyn* and at the beginning of *Thebes* suggest that these items were added after the ms. had been rubricated” (Seymour 201). The other two occurrences of blue capitals are in PA (Mosser), and are clearly the result of corrections of position of the original red ones.⁴³ The explanation for the occurrence of these letters might be that after

the blue capitals had been finished in TG, two mistakes became evident in PA, both of which were corrected by using the same blue color found in the other tale. Seymour appears to be correct about the later insertion of TG into this manuscript. However, more precision about how much later TG was added would be useful. A hypothesis concerning this date can be drawn if we take into account all the features discussed above. The fact that the ink color changes progressively suggests that the manuscript was successively copied from the beginning to the end (a fact also suggested by the position of L36-MA).

The manuscript was originally organized in quires of 10 bifolia, so that quire 4 (which contains most of TG) is an oddity, and suggests, once more, that the tale was a later addition (as might have been the case with Ha4). The quality of the paper changes in quire 4, and the watermark found on it is Mont, Briquet 11845 (Manly and Rickert 1:85). This watermarked paper is the same as that found at the end of the *Canterbury Tales*, starting in quire 13, folio 232, where NU begins. From this point on, the paper is of a more flexible kind with the same watermark found in the quire containing TG (quire 4). This indicates that the last paper used in the manuscript might be the same as is used for TG, that the manuscript was copied sequentially, and that TG was at the end of the sequence.⁴⁴ Further proof of this is that TG is written in a dark grey ink, which is also found at the end of the text. There is a smooth transition between the ink used at the beginning (golden brown) and that used at the end (dark grey).⁴⁵ However, because TG has been placed within the first part of the *Canterbury Tales*, the color of the ink appears strikingly different. By establishing that TG was copied after the rest of the book had been finished, in the light of the gloss found in Ha4, we can infer that its text was not

present in the exemplar from which the scribe was originally copying. If TG had been present, there would have been no need to leave it until the end to copy and insert. It would have been sufficient just to include it as part of one of the normal quires of 20 and proceed with the continuous copying of the text. As with Ha4, the scribe or his supervisor apparently knew about the existence of TG and also that this tale had already been used to complete CO. For these reasons, someone actively looked for the text of TG so that it could be included.

Quire 13, in which the paper changes to be the same as that of TG, contains the sequence NU-L33-CY, but the group could not have been moved independently because it contains the beginning of L37, which has the reading “yeman.”

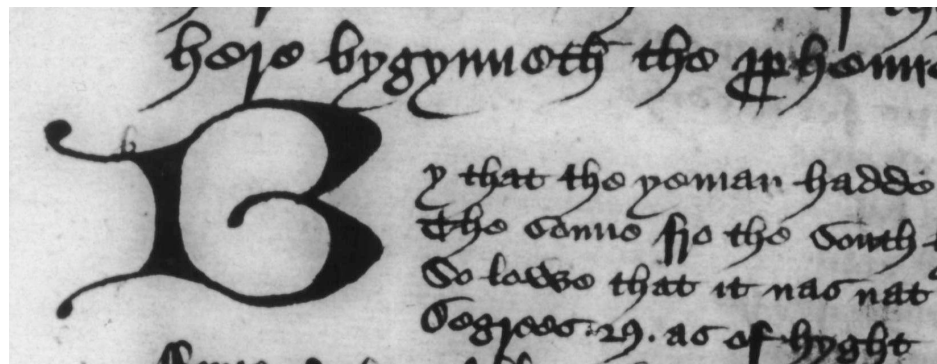


Figure 7. The first lines of L37 in Ch

Moreover, even if quire 13 had been considered an independent, movable unit to be placed in the most convenient (or in any random) position, L36 and MA were already locked into position in quire 10, a quire with a text starting at the end of NP which is then followed by L31, L36, MA and FK. In turn, FK is in a fixed place because it starts in quire 10 and finishes in quire 11, where it is followed by L7 and ML. The distribution of

these tales in the quires means that even if NU-L33-CY had been independent, MA could not have been restored to the place it usually occupies in manuscripts of the **a** group. This makes Seymour's remark about its order ("basically Group A, Ellesmere, with a unique displacement of tales of Franklin, Man of Law, Merchant, Squire" [Seymour 201]) unsatisfactory. Although Ch obviously shares some of the features of the **a** order, the evidence suggests that its order is likely to be previous to that of **a**. In other words, because of the positions of NU-L33-CY and L36-MA in Ch, it is difficult to justify its order as a derivative of **a**. This is especially true with relation to the absence of L20, a link which is present in Manly and Rickert's **a** manuscripts as the SQ-FK link. The only exceptions to this rule are manuscripts with lost leaves and Bo1 and Ph2, which have the sequence CL FK NU-L33-CY.⁴⁶ In Ch, the first twenty lines of FK are used as the prologue because of the absence of L20. Another feature that has been interpreted as relating Ch to the **a** group is L31, about which I have written:

This text [L31] is present in only 11 witnesses--Ad1 Ch Cn Cx2 Dd Ds1 En1 En3 Ma Ry1 and Wy. Wy was probably set from Cx2 and, for this reason, it is not surprising to find that their texts share many variants. Most of the witnesses that include L31 are manuscripts of the **a** group--En1 Cn Ma. However, the text is also present in Ch, a manuscript, as I have said before, whose text is likely to be genetically related to that of ω . Ry1 probably acquired the text through contamination. (330)

In this connection, we must also consider Ad1 and En3, both of which belong to Robinson's alpha group and might suggest a relationship between Ch and alpha.⁴⁷ Manly

and Rickert have suggested that L31 is genuine, but that it was cancelled by Chaucer after he had written a similar passage for the Monk. According to them "[o]nly the a ancestor failed to note the cancellation of the rejected lines" (Manly and Rickert 4:517). From this perspective, the only witnesses that had the link would have to have been a distant ancestor of the **a** group (perhaps the alpha exemplar) and the archetype of the tradition. This leaves open the possibility of Ch having descended directly from O, through a different branch from that of the **a** group, and might also explain other common features with the **a** group, such as the sequence L15-ME-L17-SQ (the same as that of the **a** order).

That Ch is the only witness of the *Canterbury Tales* to contain PL is a well known fact: "SqT breaks off (line 672) on f.228b, and the remainder of the quire, which was left blank to continue the tale, is now nearly filled with the spurious PLT, inserted later" (Manly and Rickert 1:89). The fact that the hand in PL is a later one (Manly and Rickert 1:86) indicates that PL is a later addition. This, together with the common scholarly opinion about the status of PL as non-Chaucerian, presents a problem for the analysis of tale-order. This tale is clearly a later addition by a different hand, and so it does not make much sense to include it as part of the data for phylogenetic analysis. The fact that this tale is not found in any other witness and, therefore, has the same stemmatic status as that of a singleton variant, i.e. it is not informative from a genealogical perspective, is another reason to omit PL from the analyses is that.

The codicological information from Ch gives rise to an interesting problem since a very late manuscript such as this still presents signs of uncertainty about the text of the *Canterbury Tales*. The fact that the scribe left space for the continuation of CO and SQ

suggests that he was not aware that the tales were incomplete or that he thought that the rest of the tales could still be found. Further research clearly shows that TG was the only piece which used after CO (at least by some scribes) and the blank pages after SQ were never filled by the original scribe. Only much later was the spurious PL added by a different hand seemingly to complete the *Tales*. Thus, as late as 1460, the text of the *Canterbury Tales* remained a puzzle to those who were dealing with its order and incomplete state.

2. A DIFFERENT PERSPECTIVE CONCERNING THE ORDER IN KEY MANUSCRIPTS

Phylogenetic software cannot predict the degree of scribal intervention found in manuscripts such as Ha4 and Ch, so that the software's interpretation of these data is unlikely to be completely accurate. This is also true of manual analyses of tale-order since a researcher would find the same obstacles for interpretation. To recreate a genetic relationship between the different tale-orders, both where an order comes from and where it leads should be considered. In the best interest of this specific work, to attempt to clarify the relationships between the order of the early manuscripts it is necessary to identify those aspects of a particular order which are likely to have been copied from its exemplar and those which were probably modified by scribes and supervisors.

To overcome problems generated by human intervention, late additions such as PL (clearly by a different hand and at a different time) should not be taken into account for the analysis of tale order.⁴⁸ Other additions such as TG, which I have shown is likely

to have been copied from a different exemplar from that of the rest of the text, might better be left out of the overall tale-order analysis, at least concerning early manuscripts that have indications of hesitation at this point of the text. In the next chapter, I offer an analysis of stemmata built by omitting TG from Ha4 and Ch. Stemmata based on altered data might should produce different results from those analyzed hitherto. If such results show more consistency with the word-variant stemmata then we could produce a new hypothesis about the order in these manuscripts and how they relate to other extant witnesses.

¹ It is possible that the only differences between these manuscripts might be due to accidents such as loss of leaves.

² This manuscript was dated 1400-10 by Manly and Rickert (1:47). Blake writes about it: "Hg represents an attempt to arrange Chaucer's fragments into a cohesive poem--an attempt which encouraged the compiler to complete those gaps in the poem which his arrangements created. While Hg was being prepared, certain details came to light which indicated that a different order was desirable. Hg has, therefore, what might be called a dynamic order: it was modified as the manuscript was written. This factor is important in assessing Hg's age and reliability. Paleographers are not agreed whether Hg is the oldest extant manuscript of the *Canterbury Tales*. As it contains the best text and as it embodies what can be understood only as a first attempt to arrange the tales in an intelligent order, modern editors may accept it was the earliest manuscript whose text and order they should follow" (Blake, *Canterbury Tales* 9).

³ Rebound order: GP-KT-L1-MI-L2-RE-L3-CO WB-L10-FR-L11-SU L29-MO-L30-NP L36-MA L7-ML SQ-L20-ME-L17-FK NU CL-L13-L14 PH-L21-PD SH-L24-PR-L25-TT-L28-TM L37-PA
Original order: GP-KT-L1-MI-L2-RE-L3-CO WB-L10-FR-L11-SU L7-ML SQ-L20-ME-L17-FK NU CL-L13-L14 PH-L21-PD SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA L37-PA
As I have pointed out before, for all the analyses the original order of Hg was the only one used.

⁴ In practice, when both of the Hg orders were included in the data to be processed by PAUP, these showed, not suprisingly, close together. The explanation of why two seemingly different tale-orders might appear so close probably has to do with the usual stability of fragment VII (which in the re-bound Hg order has been split in two sections) and L36-MA.

⁵ I have synthesized Stubbs structural sections here. See Stubbs' analysis in *The Hengwrt Chaucer Digital Facsimile* for a complete discussion of her conclusions about the making of the manuscript. I have collated the manuscript and can confirm that the difference in the color of the ink which can be seen in the digital facsimile is clearly visible in the manuscript itself.

⁶ This section is misplaced in the current order of Hg.

⁷ WBP-WBT-L10-FR-L11-SU is a separate structural section. There is a blank folio after SU. See Stubbs, ed., *The Hengwrt Chaucer Digital Facsimile*.

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- ⁸ It is also clear that the initial rubric has been crammed by the scribe, and that the explicit runs at least one line below the normal text layout.
- ⁹ L17 links SQ and ME in Hg, but it links ME and FK in El.
- ¹⁰ See the discussion of the variants in L17 and L20 in chapter IV.
- ¹¹ See the collation by Manly and Rickert (2:361).
- ¹² Of these manuscripts, G1 and Ra3 have the reading 'marchaunt,' while Tc1 reads 'Frankeleyn.'
- ¹³ In fact, Ch has also the variant "yeoman" in L37 as can be seen in the collation above. See the codicological analysis of Ch and Ad3 later in this chapter.
- ¹⁴ It is also conceivable that the scribe changed the reading more than once, so the possibility remains that at some point the reading was 'Frankeleyn' as suggested by Manly and Rickert and supported by Tc1, which has the sequence FK-L37-PA.
- ¹⁵ 1410-1420 (Manly and Rickert 2:46).
- ¹⁶ Mosser agrees with Blake in dating Cp earlier than Ha4, but he also points out that Smith (quoted by Owen, 1991) based on linguistic data, suggests that Ha4 might be earlier than Cp (Cf. Mosser, "Witness Descriptions"). In fact, although linguistic features have been suggested as crucial in dating these two manuscripts, it is possible that these were copied directly from the exemplars of each of these manuscripts.
- ¹⁷ In my previous research, an incomplete collation of the *Canterbury Tales*, I found few links between the texts of Cp and Ha4. (See especially the electronic appendix of "The Manuscript Source of Caxton's Second Edition of the *Canterbury Tales* and its Place in the Textual Tradition of the *Tales*").
- ¹⁸ This is another suggestion of a different copy text for both manuscripts.
- ¹⁹ Blake thinks that Cp is the first manuscript to contain L33-CY (Blake, *Textual* 98 and ff.). However, the fact that these links and tale, together with NU, do change positions in other manuscripts, shows it is possible they were interpreted as belonging to a different part of the text.
- ²⁰ c. 1410 (Manly and Rickert 2:47).
- ²¹ The manuscript also has a flyleaf at the beginning and two at the end.
- ²² Ha⁴ GP-KT-L1-MI-L2-RE-L3-CO TG L7-ML-L8 WB-L10-FR-L11-SU CL-L13-L15-ME-L17-SQ...FK NU-L33-CY PH-L21-PD SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA L37-PA RT
- ²³ The new lineation system was developed for the *Canterbury Tales* Project, for this reason, it was not available when Blake wrote *The Textual Tradition of the Canterbury Tales*. It is not possible to know whether Blake was referring to L5, L6 or some other link.
- ²⁴ See below the discussion about Ch.
- ²⁵ About the affiliations of Ha4 and its closeness to O, see Bordalejo.
- ²⁶ Wy does not include L8 and has L31 after NP. The latter probably came from Cx2, but only a detailed collation of these two witnesses would prove this. Miss Satoko Tokunaga, Keio University, in cooperation with the *Canterbury Tales* Project is currently carrying out a study of the affiliations of the source of Wy.
- ²⁷ Thomas Garbáty has conducted a partial study of the textual affiliations of Wy (See Garbáty 57-67).
- ²⁸ 1420-40 (Manly and Rickert 2:46).
- ²⁹ 1400-20 (Manly and Rickert 2:46).
- ³⁰ Both of these also include some passages not found in El.
- ³¹ I am not sure whether Manly and Rickert have based their guess of the position of these tales on codicological evidence (stints for example). It would be interesting to check the manuscript to clarify the reasons for this classification.
- ³² Blake also observes that both Dd and Ha4 arranged the text in a prologue-tale pattern rather than in a tale-link pattern. This created complications with links that could not be used as prologues (such as L8) and the scribes had to deal with this situation, perhaps by suppressing part of the material. (Blake, *Textual* 127).
- ³³ Because Da Rold's dissertation includes a very detailed codicological analysis of Dd, it was unnecessary for me to see this manuscript. I have relied on her analysis in conjunction with digital images of the manuscript.
- ³⁴ Briquet assigns dates based on those that appear in the studied manuscripts, which could have been copied relatively late in comparison with the original date of production of the paper. See Briquet.

³⁵ Hg and Ch have filled the space in different ways. Hg added the modified version of L20, with a compressed initial rubric and a final rubric that goes below the regular margins of the text. In a later hand, Ch has PL. Ha4 has lost the pages which originally contained the end of SQ and the beginning of FK, for this reason, it is impossible to know whether the scribe had left a space. Gg has also lost leaves at this point.

³⁶ As put by Parkes: “To the extent that Robinson’s editions of 1933 and 1957 were based on Skeat, and that the new *Riverside Chaucer* is based on Robinson, the Ellesmere text is *The Canterbury Tales*, since Robinson and now the *Riverside* have been and are the editions most extensively used for citation in critical books and articles” (Pearsall, *Life* 234).

³⁷ See Manly and Rickert 1:170.

³⁸ The quire numbers include the two missing quires.

³⁹ Ad³ GP-KT-L1-MI-L2-RE L7-ML WB-L10-FR-L11-SU NU CL-L13-L15-ME-L17-SQ-L20-FK PH-L21-PD SH.....PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA L3-CO L-33-CY L37-PA..

⁴⁰ In private conversation, Mr Jacob Thaisen has informed me that preliminary results of his research on the spelling patterns in Ad3 suggest that there is a link between L3-CO and the rest of fragment I (GP, KT, L1, MI, L2 and RE). Since there is no break in the copying process, for the time being, I have to assume that this consistency already existed in Ad3’s exemplar.

⁴¹ This is not suprising since Horobin’s thesis was supervised by Blake, who gives great importance to this distinction.

⁴² 1460-79 (Manly and Rickert 1:46).

⁴³ These can be found in 261v and 270v.

⁴⁴ When he started quire 13, the scribe had probably finished with all his other paper. Then he started a new batch which he used from this point to the end. When the scribe finished copying PA, he received (or acquired) a copy of TG, he started to copy it in quire 3, where he had left space for the rest of CO. It became clear that the whole of TG would require more space, and then is when he reached for the same batch of paper which he had used for the last quires of the *Canterbury Tales* and used it to make the extra quire of 5 bifolia (10 folios).

⁴⁵ At least, I have not been able to establish the place in which there is a definitive change in its composition.

⁴⁶ These two manuscripts belong to Robinson’s E group. Elsewhere, I have hypothesized a series of stemmata in which the E group and El are derivatives of a and α (Bordalejo 368-75).

⁴⁷ In the light of the variants analyzed in my other work (the differences between Cx1 and Cx2), I have not previously put forward this idea. It is possible that a complete analysis of the text of Ch might show that this manuscript had a close relationship with the alpha exemplar. The data from the three thousand variants analyzed in my De Montfort University PhD thesis suggests that the variants shared by Ad3 Ch Ha4 and Cx2 are likely to be archetypal (although some of these are difficult to classify), but this same data is too partial in reference to the manuscripts to present a reliable classification. Complete collations of these manuscripts against other witnesses of the text would be necessary to complete a classifying scheme based on both tale-order and word-variants.

⁴⁸ Because PL appears only in Ch it was left out from the tale-order analysis. However, TG was included in all analyses presented in chapters 4 and 5. For the analysis of the tale order without TG see chapter 7.

CONCLUSION

1. IMPLICATIONS OF THE DIFFERENT TALE-ORDERS IN THE MANUSCRIPTS OF THE *CANTERBURY TALES*

In the previous chapter I analyzed the individual orders of some important manuscripts of the *Canterbury Tales*. In this section of the conclusion, I would like to suggest paths to take this further and to attempt to explain the relationships between these witnesses of the text. Particularly, I discuss the importance of tale-order matters in relation to O and also to the codicology of the early manuscripts. In this section of the conclusion, I consider parts of the text that appear in some manuscripts and not in others, such as CY and the passage that links the Second Nun's tale with it (L33).

As shown in chapter 6, there are peculiarities in the manuscripts of the *Tales* that might have profound implications on the order in which each of them presents the text. Sometimes, the scribes made an effort to complete the text (by adding TG, for example). At other times, they showed that they had not realized (or did not care) that a specific part of the text was missing and would never be recovered (as happened with SQ).

1.1 When the Scribes Know What to Do

1.1.1 The Cook's Tale (CO)

The unfinished CO puzzles the modern reader and leads to speculation about the reasons Chaucer might have had to leave the tale in such a state. Is it that Chaucer died

and could not complete it? Did he decide that the tale might become excessive since it features a prostitute? Did Chaucer intend to make a modern statement about textuality by including a tale that was complete in its incompleteness? (Pearsall 241) Scholars who have taught the *Canterbury Tales* are likely to have come across students who proposed these and other alternatives, but most specialists, such as Pearsall, seem to believe that the tale was never finished. The reason for this is very simple: the Hg scribe left a note for us:

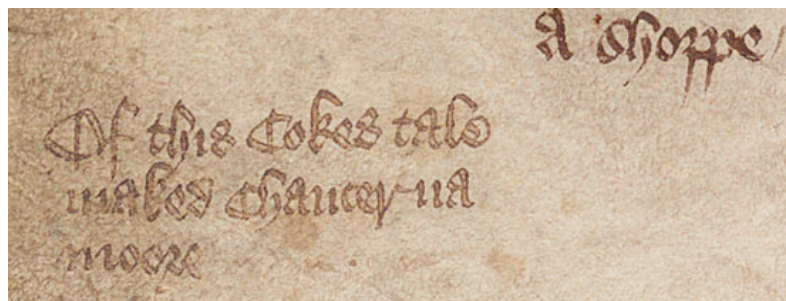


Figure 8. Gloss after CO in Hg

This comment can be interpreted in different ways. Stubbs, referring to this specific comment states: "The Hg/El scribe proved in his copying of Hg that he copied under instruction and was reluctant to include material without specific endorsement. His marginal comment affirming that there was no continuation of the Cook's Tale is simple but definite" (Stubbs). Although this is a possible explanation, it is not devoid of a great deal of speculation on her part (that is, that the scribe was "reluctant to include material without specific endorsement"). There is no need to confer upon the scribe such an amount of responsibility; indeed, Parkes and Doyle just tell us that this is a "well informed comment," which is more than enough. Stubbs insists on the privileged position

of this particular scribe because she has put forward this hypothesis about the textual tradition:

The scribe of Dd, who may have been known to the Hg/El scribe and whose manuscript shows close connections in varying ways in both text and tale order with Hg and El, also left a blank space after the Cook's Tale. El has no comment of any kind when the text of the Cook's Tale runs out, but this time two and a half folios are left blank to the end of the quire, again with no catchword. (Stubbs)

This observation about Dd leaving blank the rest of the folio as in Hg and El is correct (see plate 39). However, the Dd feature is weak (the space is not enough to insert anything else), while in other manuscripts, notably Ch (plate 37), we find a longer space and three more folios left on the quire. This means that several manuscripts could have had the option of adding TG at this point, but only a few did.

1.1.2 The Tale of Gamelyn (TG)

As pointed out in the previous chapter, some of the scribes 'completed' CO by adding TG immediately after. It is unclear how the Cp scribe decided to make the addition and, indeed, there is no sign of hesitation in this manuscript that might have indicated that TG was not part of its copy-text (plate 38). However, CO ends in mid-folio in Cp, just as in Hg, El and Dd, with the difference that in this manuscript a new tale is included in such way that it appears to be told by the Cook. However, the rubric used for TG is just 'Incipit Fabula,' without any specificity as to who is the teller of Gamelyn.

Clearly, the Ch scribe had left a space after CO and this space was later completed with TG (which also required the introduction of an extra quire.) Indeed, of the manuscripts analysed for this work, only Ha4 finishes the folio with the last lines of CO and starts TG in the next folio (plates 40 and 41). This means that even when the scribes included TG, they finished CO in mid-folio.¹ As suggested in chapter 6, the importance of the indication for the inclusion of TG in Ha4 and the different ink, color for rubrication and paper used in Ch indicate that the tale was a later addition to the original text of these manuscripts.

The most important feature about CO is the fact that the scribes found ways of dealing with its incompleteness, Hg with the note about its incompleteness and Cp, Ha4 and Ch with the introduction of TG as the 'real' Cook's Tale. This leaves Dd and El as the manuscripts, which, for one reason or another, did not need to include a rubric or an extra-tale. This fact, when taken into account together with other features in tale-order and with word-variants, suggests that there might be an interesting line of research in trying to discover if there are any other features that could act as links between Dd and El.

1.2 When the Scribes Do Not Know What to Do

1.2.1 The Squire's Tale (SQ)

The position of SQ (together with the other re-arranged tales, ME, CL and FK) is one of the most interesting features in the order of the witnesses of the *Canterbury Tales*, but this tale presented a problem for the scribes since it, like CO, was incomplete. There has

been some speculation as to the reason why scribes did not attempt to complete SQ as they did with CO (see Partridge's article, "Minding the Gaps"). This could be because SQ is a much more substantial piece of work. (No scribe in the analysed manuscripts makes reference to this; that is, we do not have a gloss indicating that its incompleteness was due to Chaucer himself). In fact, the Hg scribe left a blank page after SQ. Only the last two lines of the tale are present in folio 137v of Hg and the rest of the page has been completed with the adapted version of L20 in a yellowish ink. It is possible that the scribe had intended to leave the rest of the folio blank awaiting for the continuation of SQ and, when he received the extra links, he decided to include the adapted version of L20 which had to be crammed into the folio.

Ha4 has lost the last page of SQ, but consistently we find that the different scribes left a space, which often remained blank, at the end of it. It seems that the scribes, even in late manuscripts such as Ch, left some space for the continuation of the tale, which never appeared.² Ad3 also shares this same characteristic: in 131v, we find only the last three lines of SQ (plate 42), and at the beginning of 132r L20 starts. However, what is interesting about folio 131 in Ad3 is that it is a singleton leaf. The Dd scribe has left a similar kind of blank space (plate 45), and this also happens in El. Cp is not an exception in this matter, and its scribe has written the last 18 lines of SQ in folio 99v, leaving the rest of the folio blank (plate 44). This blank, however, is not at the end of the quire, as one would expect if the scribe had the intention of attempting to complete the text; rather, it is two folios before the end of the quire. WBP starts in 100r.

It seems very peculiar that when all scribes seem to be able to cope with the lack of completion of CO, these same scribes appear to be waiting for a conclusion of SQ to appear. One has to wonder how much of this attitude is actually related to the gloss in Hg for CO and the lack of it for SQ. Even if the Hg scribe had thought of giving SQ the same treatment as CO, that is to add an explicative note to let the reader know that the tale had reached him in its incomplete state, this never happened. Instead, the Hg scribe forced the text of L20 to fit in a reduced space and lost any opportunity of ever adding the explanatory gloss.

1.3 Could a Scribe Have Left Text Out?

1.3.1 The Canon's Yeoman's Tale (CY) and L33

As I have explained before, in reference to Hg, it is conceivable and also probable that the scribe altered the sequence of the tales and indeed part of the text to compensate for text that never reached his hands. In the previous chapter, I put forward the hypothesis that because the Hg scribe never received CY he altered the order of his text and the reading in the first line of L37. This question then arises: how did this reading spread to almost the whole textual tradition. The answer might be that the Hg scribe not only altered his copying order and the text of L37 in Hg, but also changed the text of his exemplar. It is even possible that when the Hg scribe eventually received L33 and CY he decided to change their place to before MA in O. This may not be the first time this scribe behaved in this manner, since Robinson has suggested that this particular scribe did exactly this in at least one other instance: L20 and L17.³

If it is true that the Hg scribe modified the text of the archetype, then we must assume that any copies bearing a different order or variants at these points must have had their origin in a pre-existing copy of such an archetype. If this were true, in turn, we would have to admit that manuscripts such as Ad3 and Ch (both of which have a different treatment of L33 and CY) might have as their ultimate ancestor an unaltered copy of O which was produced at some point before the alterations were introduced in to the archetype. In any case, the alteration of L37, along with the order L36-MA L37-PA, must have been introduced at a very early stage, before Cp was copied. However, at this point, the alteration of L17 and L20 had not been performed in O.

1.4 Altering the Order of the Tales

After carrying out a codicological analysis of the manuscripts, it has become increasingly evident that some of them were not copied from exemplars which had the same tale-order which is now extant; instead, either the scribes or their supervisors, by mistake (as in Hg) or on purpose (as in Ha4 and Ch), modified, in one way or another, the order of the tales. This means that even when a scribe had a specific order in front of him, he could have changed this in order to suit specific needs.

At this point, and just as a way of experimenting with this hypothesis, I have created a tale order containing what seem to me moderately conservative alterations to the original data drawn from the tables. TG has been suppressed from both manuscripts in which it seems to be an obvious addition: Ha4 and Ch. I give this created order the sigil BB. This item represents the order of the tales, not as it was in the archetype of the

tradition, or as it was intended by Chaucer, but as it seems to better explain the tale-order in the early manuscripts: GP-KT-L1-MI-L2-RE-L3-CO L7-ML WB-L10-FR-L11-SU CL-L13-L15-ME-L17-SQ-L20-FK PH-L21-PD SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA NU-L33-CY L37-PA RT. This order could be interpreted as a modified version of Manly and Rickert's **a** order, but instead it is intended to explain the variations in tale-order in the analysed witnesses of the *Canterbury Tales*.

The new tree (plate 30) including this created order and the modified Ch and Ha4 does not show dramatic differences from other trees, but it shows enough differences to make it interesting. For example, if we take two BreakPoint distance unrooted phylograms, we find that Ha4 appears in a similar position (plate 31) as the one it had before (plate 9). Ch has moved from a position near to Ha4 in plate 9, to one very close to Dd and the **a** group. The same occurs if we compare plates 9 and 31: Ch has moved from being with Ld1, in a branch in the central part of the tree, to being grouped with the **a** manuscripts. This new grouping of Ch and the **a** group (supported by both the BreakPoint distance and the IEBP trees) is interesting because these witnesses seem to share some characteristics with manuscripts of the **a** group, and these are often difficult to explain. For example, if we were to think that Ch is completely unrelated to **a**, then the only explanation for the presence of L31 in this manuscript would be contamination. However, analysis of word-variants in the manuscripts suggests that Ch might have some relationship with this group (as Ch tends to cluster with Hg and El, as can be seen in MI⁴). Because the elimination of TG from the data groups Ch and the **a** manuscripts, one

could suspect that this possible relationship between Ch and the **a** manuscripts requires further research.

Not surprisingly, the BB order also groups together with **a**. Although there is a crucial difference between the **a** order and that of BB (the position of NU-L33-CY) the general stability of the group seems to have made this item less determinant in the overall shape of the tree.

Although the inclusion of the item with the created order and the alterations in Ch and Ha4 in the phylogenetic analysis does not seem to have a dramatic effect on the overall shape of the tree, the changes are enough to make this an interesting starting point for future research. Few of the manuscripts of the *Canterbury Tales* have been analysed in detail. This is especially true of the later manuscripts (as is the case for Ch); however, after my brief approach to the codicological aspects of these texts, it seems that it would be of great interest to carry this research further. The results of such research might suggest that there are other points of the textual tradition in which new links were incorporated to the text and in which the tales were re-arranged for one or another reason. For example, we might be able to point out one manuscript (or more than one) that was closer to the moment at which certain parts of the text were added. Perhaps we might even be able to tell, when (and in which manuscripts or hyparchetypes) the order changed to become that of specific groups or individual manuscripts. If we are lucky, there might still be **d** manuscripts holding some clues as to the origin of this recension which, although textually less important, might hold key aspects of the textual tradition of the *Tales*.

2. SUMMARY OF HYPOTHESES ADVANCED BY THIS WORK

At the beginning of this work, the main task was to find out if there is a clear relationship between the word-variant stemmata and the tale-order stemmata; that is, if the tale-order could have been transmitted from one manuscript to another through the same family relations as those by which the word-variants were transmitted from one manuscript to another. This work hypothesized that if phylogenetic software can be used to show or clarify genetic relationships among manuscripts on the basis of word variants, it should also be able to show any existent relationships between the tale-orders when the appropriate data was used.

There are two differences between this and previous research carried out about the order of the *Canterbury Tales*. Firstly, computer technology is used to help us understand the problems of the relationships between the different tale-orders. Secondly, tale-order is not seen as an isolated feature, but is, instead, studied together with the evidence drawn from the word-variants. The assumption behind this procedure is that a break in the correlation between tale-order and word-variant stemmata might suggest that there was some degree of intervention in the order of the tales at that point of the tradition.

2.1 Manly and Rickert's Tale-Order Classification

One cannot pay sufficient tribute to the work carried out by Manly and Rickert on the subject of tale-order in the manuscripts of the *Canterbury Tales*. The path they opened has allowed many scholars to continue this research. With the help of sophisticated

computer tools, however, it is possible to broaden and deepen their initial research. The analyses produced for this work allow us to establish when Manly and Rickert made a correct assumption and when they fell short of establishing certain groups. For example, manuscripts such as Bo1 and Ph2, which were re-classified as **E** by Robinson according to their word-variants, present a particular tale-order. The phylogenetic software usually places these witnesses together on the basis of the order of their tales, and this seems consistent with Robinson's **E** group. Other manuscripts, which Manly and Rickert assigned to the tale-order **a** group, but which clearly had different affiliations in their word-variants, place consistently with **a**. This is the case of El, a manuscript for which Manly and Rickert were not able to establish clear word-variant affiliations throughout. Robinson has suggested, based on such variants,⁵ that El is likely to be an **O** manuscript. My own research on Caxton's first and second editions of the *Canterbury Tales* showed that for some variants in parts of the text, El (together with Gg) could be affiliated with the **E** group. Although this might appear inconsistent with the El and Gg tale-orders, which is that of **a**, it is possible that the E hyparchetype descended from a manuscript of the **a** group and that, in turn, Bo1 and Ph2 descended from an **E** manuscript with a modified order. This reasoning is a good example of how word-variants and tale-order can be used together to explain manuscript affiliations. In fact, this hypothesis about the formation and development of the **E** group shows that some areas of the textual tradition can become much clearer when the order of the tales in different manuscripts is analyzed. If, indeed, the **E** group is proven to be a derivation of **a**, then Manly and Rickert's

classification would have been partially correct about the inclusion of these two manuscripts in the **a** group.

Another imprecision in Manly and Rickert's **a** group for tale-order is the inclusion of Ad3, which exhibits changes which do not seem to be directly related to **a**. Moreover, word variation in Ad3 has suggested that this manuscript might be closer to the origin of the tradition than previously thought. It is likely that the tale-order in Ad3 was directly copied from its exemplar, and, for this reason, this manuscript must have been removed at least one step from the origin of the tradition.

It is fair to say that Manly and Rickert were right in the general way in which they classified manuscripts according to their tale-order, but occasionally, as in the cases of Bo1 Ph2 and Ad3, their classification lacks precision. These witnesses, roughly grouped with the **a** witnesses, appear to be related to subgroups (Bo1 and Ph2 are likely to be **E** manuscripts) or be closer to the archetype than most of **a** (as might be the case of Ad3). A similar situation is found with the so-called anomalous manuscripts, which the more powerful tools we now have might show as grouping with other manuscripts. Ch is a good example of this, since with the suppression of TG, the phylogenetic software groups it together with the **a** manuscripts for the new tale-order data.

2.2 Refining Manly and Rickert's Groupings

The weaknesses in Manly and Rickert's classification according to tale-order may derive from this being the only criterion taken into account for the groupings. From this

procedure also arises the problem of the lack of compatibility between their word-variant and their tale-order groupings. To combine both aspects into a single classificatory schema is undoubtedly more complex than was possible when they attempted their work, but, in the light of the present work, it appears that such an endeavour might be a fruitful one.

As stated above, Ch appears with the **a** group in plates 30 to 33. Because these plates are only based on tale-order, the information offered by them cannot be seen as a definitive link. However, Ch shares whole passages, such as L31, with **a** manuscripts. Further research on the manuscript affiliations of Ch might show more clearly if there is indeed a relationship between this manuscript and manuscripts of the **a** group.⁶

Ha4, analysed with or without TG, appears very close to Wy in the middle of the stemmata, as if serving as a link for all groups. The idea that the person supervising its copying was the one who thought of acquiring the 'missing' tale suggests that at least another manuscript already had added it to the sequence of the *Tales*. Although many scholars (see especially Blake, *Textual*) may feel inclined to state that this proves the precedence of Cp over Ha4, this does not seem so clear in the light of the present research. The possibility still remains open that the exemplar used for Cp was already in existence, but that Cp itself had not yet been copied when Ha4 was produced. The hypothesis of the existence of an exemplar previous to Cp (which has been considered one of the earliest extant manuscripts of the *Canterbury Tales*), is supported by the fact that the manuscript does not appear to exhibit hesitation in the order of the tales, which

suggests that its scribe might have been copying successively from the beginning to the end.

One of the main reasons why it is necessary to take into account both word-variants and tale-order to establish manuscript groupings is that at some points of the textual tradition scribes and their supervisors changed the order of the tales. Such a situation represents a break between the line of descent of tale-order and word-variants; that is, it is possible that the word-variants in a manuscript might be related to those of another while their tale-orders might differ. Whenever this occurs, it provides strong evidence that either one of the extant manuscripts or its exemplar had its tale-order modified. This opens another area of research with reference to textual reception and manuscript culture: we should attempt to deduce the underlying reason behind each change.

If both word-variant and tale-order are used to establish the relationships among the witnesses of the *Canterbury Tales*, one should be able to produce a much more refined version of the groups observed by Manly and Rickert, which might help to explain the chronology of the production of some manuscripts.

2.3 Tale-Order and the New Stemmatology

It seems clear that no order extant in the manuscripts of the *Tales* is Chaucerian, but even if there were a Chaucerian order, it would be impossible to distinguish from all the others. The question then is not how to find, approach or re-build a Chaucerian order, but which order is the best to use in an edition of the *Tales*. A solution which has been

commonly used by editors is to use the order of a specific manuscript (facsimiles and best-text editions are likely to use this procedure). The problem is that all tale-orders are unsatisfactory and all might need some emendation or not, depending on the perspective of the editor. For example, intentionalist editors are likely to search for the order that seems to them to be closer to Chaucer's intentions. Best-text editors might prefer to adopt the order of the manuscript being edited. *The Riverside Chaucer* uses the El order (the **a** order) as a base, but it adds L8 in brackets after ML. L14 (the Host Stanza) is also added in brackets, although it is present in El.⁷ L31 has been included, but the textual notes suggest that the editors think it was cancelled,⁸ a widely accepted scholarly opinion. *The Riverside Chaucer* contains what is probably the most widely read edition of the *Canterbury Tales*, but the text presented in it is a conflated one which has its origin in the comparison of manuscripts and early printed editions. The problem with the order in the *Riverside* is more a conceptual issue than anything else. It seems clear that the order in *Riverside* is exactly the same of that of F. N. Robinson's edition (on which *Riverside* was based). Robinson briefly argued that the order he retained for the second edition of *The Works of Geoffrey Chaucer* was that transmitted in the manuscript tradition (F. N. Robinson viii), but although some **a** order manuscripts have L31,⁹ not a single one of them has L8.¹⁰ In this way, we can see that, even though F. N. Robinson appears to have thought that his edition followed the order of the 'best manuscripts,' he did not follow this order as closely as he could have done. The presence of L8 is especially interesting because of Robinson's rejection of the Bradshaw shift (F. N. Robinson 2). This link has been the main justification for the shift (which requires SH to immediately follow ML-

L8); therefore, its inclusion in an edition which claims to follow the order found in the best manuscripts appears peculiar.

The Bradshaw shift has been accepted by many editors, apparently because they wish to find an order representing Chaucer's intention. This brings us to the question of the reason for preferring one particular order instead of another. A different editorial approach, the New Stemmatics,¹¹ proposes, not to reconstruct an authorial or archetypal text, but to 'construct' a text that better explains the textual tradition as it is extant.¹² In this way, if we adopt this approach, the order of the tales which an editor would aim for should be that which helps the reader understand all the orders extant in different manuscripts. In light of this approach I have proposed as the order of a possible edition of the *Canterbury Tales* that expressed in the created order included in the last set of stemmata (plates 30 to 33).¹³ This tale-order, as stated before, is basically the **a** order with a modification in the position of NU-L33-CY based on the Hg variant at the beginning of L37 and the position of the tales in Ch, Ad3 and Bo2, all of this taken into account together with the evidence of the word-variants.

2.4 Tale-order and Word-variants

This research dissipates some of the doubts about the transmission of the order of the tales from one manuscript to another. The fact that there are some instances in which tale-order and word-variants do not overlap indicates that scribes and their supervisors faced similar problems to those that modern editors face. The scribes also struggled to make

sense of an unfinished text which appeared to have no fixed final form. The changes they made do not prevent research on tale-order from being carried out or being a helpful tool for scholars and other readers to understand better the textual tradition of the *Tales*. Instead, they represent a challenge for the researcher who has to take into account several different aspects of the study of texts in order to obtain the maximum results for his or her work. The role of codicology, which appears to have been underestimated in studies of tale-order in the *Canterbury Tales*, has proven to be of primary relevance to understand the copying process of particular texts, as shown by previous studies (Stubbs) and particularly in chapter 6 of this work. Occasionally, this kind of analysis has been useful in showing that a specific order is likely to have been already present in a manuscript's exemplar (as seems to be the case of Ad3). Often, as in the cases of Hg, Ha4 and Ch, the structure of a manuscript might suggest that its order was being created as the manuscript was being copied.

At those points of the textual tradition in which scribal intervention can be seen, more research is necessary to try to explain the reasons why the changes are likely to have occurred and to revise the manuscript affiliations.

2.5 Further Research

The relative success of the codicological analysis of the studied manuscripts is encouraging because it offers new possibilities of further study. An interesting area of research would be, for example, the manuscripts classified as **d** in Manly and Rickert's

tale-order table. These manuscripts have some links, such as L5, L16, L18, L22 and L34, that are usually considered spurious, but it is possible that the presence of these links could offer help in investigating the relationships within **d** order witnesses. This investigation, in turn, might be helpful in dating some of these manuscripts more accurately. In any case, the addition of these links suggests more than intervention in tale-order, since they show clear attempts to complete the text.

Clearly, more research on the word-variants of some of the analysed witnesses is also necessary. The tools which are now available to collate the text have not yet been exploited to their full potential (partly because not all witnesses of the text have been transcribed), and only a complete collation of the most important witnesses of the *Tales* could potentially offer clearer answers to the questions of affiliation and development of the text. It is important, however, to combine the evidence from word-variation with that of the order of the tales. These two aspects of the textual tradition are better analysed together, in a deliberate manner, not just by accidental and obvious agreement, but by using all the tools available for this research. The use of evolutionary biology software has shown relations that we did not suspect existed, and has confirmed, in combination with codicological analysis, that some manuscripts (such as Ch and Ha4) require a much more detailed textual analysis to be used in conjunction with the conclusions presented in this work. The combination of codicological analysis and computer-assisted analytic techniques have already produced significant insights into the development and transmission of the *Canterbury Tales*. They have raised new questions and opened

alternative inquiry approaches, suggesting a long and fruitful future for this kind of research.

¹ See, for example, Ad3 (which has CO in a peculiar position, see plate 36), Ch (plate 37), and Cp.

² In Ch eventually, a different hand added PL at the end of 228v (plate 43).

³ “The only possible explanation is that the text of the links was not altered just in Hengwrt. It was altered, probably by the scribe’s supervisor, in the exemplar, that is, in *O* itself. The three tales were then placed in the exemplar in the same order as they are copied in Hengwrt, with the now-altered text of the links connecting them. This newly reshuffled *O*, then, in turn became the exemplar not only of the type *d* copies but also of Manly and Rickert’s *c* group, and the additional group I label *f*.” (Robinson, “Can We Trust” 207)

⁴ MI, edited by Peter Robinson, will be published by Scholarly Digital Editions later this year.

⁵ The data available was that of GP and WBP.

⁶ In light of the Canterbury Tales Project’s research which has already been published, it appears that Ch is also linked to Hg, which suggests that its copy-text might have been a good and early manuscript.

⁷ The reason for the editors of the *Riverside* to include this stanza in brackets is that they think that it might have been cancelled by Chaucer (Benson 1128).

⁸ See Benson 1133.

⁹ The witnesses which have L31 are Dd En1 Ds1 Cn Ma En3 Ad1 Ch Cx2 Wy Pn.

¹⁰ About the order of this edition, F. N. Robinson writes: “In the present edition the inconsistent arrangement of the best manuscripts is followed, and no attempt is made to correct discrepancies left standing by the author, or to reconstruct the stages of a pilgrimage which he seems never to have completely planned.” (2).

¹¹ Robinson points out about the New Stemmatics: “Like the stemmatics of the last century, its aim is to illuminate the history of the text. Unlike the stemmatics of the old century, its aim is not a well-made edition, but a well-informed reader” (“Analysis”).

¹² In this sense, such a text is recognized to be the last production of the textual tradition instead of an attempt to reproduce the first. This also allows the text to be superceded by a new edition if new evidence becomes available.

¹³ The created order, for which I have used the sigil BB, is as follows: GP-KT-L1-MI-L2-RE-L3-CO L7-ML WB-L10-FR-L11-SU CL-L13-L14-L15-ME-L17-SQ-L20-FK PH-L21-PD SH-L24-PR-L25-TT-L28-TM-L29-MO-L30-NP L36-MA NU-L33-CY L37-PA RT.