

CHAPTER I

A HISTORICAL SURVEY OF SCHOLARSHIP OF Cx2, ESPECIALLY FOCUSED ON THE PROBLEM OF ITS SOURCE

This chapter offers a survey of the scholarship related to Cx2, focusing on the different conclusions critics have reached during the twentieth century. Especial attention has been dedicated to studies which concentrate on the text of Cx2 and on ω . Remarks which are not strictly related to textual matters made by analytical bibliographers have also been included, since these might reveal evidence that could help to clarify the nature of this lost manuscript. A section of this chapter has been dedicated to Caxton as an editor, since this could give some insight on his treatment of ω . A synthesis of the conclusions reached by different scholars can be found at the end of this chapter. Each of them is addressed in the conclusions of this work.

1. THE STORY AND THE HISTORY OF THE PRODUCTION OF Cx2

The story behind Cx2 is widely known: in the preface to this book, Caxton wrote that a "gentylman" came to him and said that the text of Cx1 was not accurate, that it was not what Chaucer had written, and that his father had a better manuscript which he could lend to Caxton.¹ This preface, with all its implications, has drawn

¹ See the introduction for the quotation from Caxton's prologue.

critics' attention to Cx2, because several issues emerge from it. Some of these issues are obvious, some others less so:

1. The textual differences between Cx1 and Cx2. This is important because the differences between both editions might help us to understand how Caxton worked and what were his aims.²
2. The textual affiliations of both editions. The affiliation of Cx1 with Manly and Rickert's **b** group was established by them as part of their edition (Manly and Rickert 1940, 2: 57 and ff.). However, the affiliations of Cx2 are not so clear and need further study.
3. The process through which Caxton arrived at the text of Cx2. This has implications concerning Caxton's idea of what a good text should be and about his knowledge of and interest in textual matters (See Dunn, 1939).

These and other problems have preoccupied generations of critics and stimulated learned discussions and scholarly articles during the past hundred years.

2. APPROACHES TO CAXTON AND HIS WORK

The most comprehensive single set of studies about Caxton and his editions was carried out by William Blades in the second half of the nineteenth-century.³ No later scholar has attempted, in a single book, what Blades did in *The Life and Typography of William Caxton* (1861-3). Both this and his other book, *The Biography and Typography of William Caxton* (1877), are, as the names suggest, heavily centred

² Dunn is the only scholar to have undertaken a complete collation between Cx1 and Cx2 (1939). Greg only collated lines at the beginning of KT (1924), while Kilgour collated PD (1929). Each of these scholars offered his or her own perspective about the possible affiliations of the manuscript source of Cx2. See Kilgour 1929 and Greg 1924 and 1929.

³ A biography of Caxton by John Lewis was published in London in 1737 and is quoted by Blake (1969, 207). For information about studies about Caxton in the eighteenth century see Hellinga (1982, 25-35).

on biographical and bibliographical aspects. A point directly related to textual issues concerns Blades' opinion of Caxton's editorial practices, commenting directly on his Prologue to Cx2:

The Prologue of his [Caxton's] second edition of the "Canterbury Tales" proves how anxious he was to be correct, and, at the same time, the difficulty he had in obtaining manuscripts free from corruption. The poetical reverence with which Caxton speaks of Chaucer, "the first founder of *ornate* eloquence in our English," and the pains he took to reprint the "Canterbury Tales," when a purer text than that of his first edition was offered him, shows his high appreciation of England's first great Poet. (Blades 1861, 80)

In this quotation, it appears that Blades trusted Caxton's prologues and epilogues as historical, unbiased sources, and that these lead Blades to conclude that he was a careful printer with deep concerns about the accuracy of the texts he was publishing. However, recent critics, such as Blake (1969, 103) and Boyd (1984, 13), have a very different understanding of Caxton that makes him only a businessman, trying to maximise the financial income rendered by his business.⁴ These critics are reluctant to assume that Caxton was writing the unvarnished truth when he wrote the prologues and epilogues, and do not, necessarily, assume that he was telling the truth about his reasons to print a second edition of the *Canterbury Tales*. Even if we read Cx2's prologue literally, some other questions would arise, formulated here by Blake:

He [Caxton] agreed to print a second edition from another manuscript before he had seen it. He cannot have formed for himself any reasonable

idea as to its quality; he merely accepted the word of his gentleman-client that it contained a better text. It is doubtful, therefore, whether his primary motive in printing the second edition was to produce a good text; his motive may have been a desire to oblige a noble customer, or simply a publisher's realization that a new, revised edition might sell well. (1969, 103)

Blake's views differ radically from those of Blades, because he focuses on the interpretation of what is being said without necessarily believing it literally. While Blades makes an effort to present Caxton as a reliable source, Blake is much more sceptical about the methods employed by the printer. This scepticism is characteristic of 20th century scholars, and Beverly Boyd, for example, seems to agree with Blake's assessment:

His [Caxton's] knowledge of the texts of Chaucer's works was not that of an exegete of his own time, much less that of a present-day editor of Chaucer, though the printer himself never claimed such skill. In no case do we have copies or precise records of Caxton's sources for these books. Within these limitations the evidence is strong that he followed his texts closely except in the second edition of *The Canterbury Tales*, where he can be faulted for a serious mistake in judgement when he tried to correct the first edition from a manuscript obviously of a different textual tradition, though the circumstances were not altogether his fault. Having followed his sources elsewhere may not, however, be entirely to his credit, for the evidence is that in most of the Chaucers he did little or

⁴ See also Needham's introduction to the facsimile of Caxton's edition of *Le Morte Darthur* (Needham

nothing in the way of editing but turned over the exemplars to his staff for copy editing and printing. In that case his staff is mainly responsible for what actually appears on the printed pages, all of which contain routine modernization of the grammar, syntax, vocabulary, and orthography. Definite evidence of his own editing resides only in his second edition of *The Canterbury Tales* and in his *House of Fame* (1984, 33-34)

At the beginning of this quotation, Boyd seems to be defending Caxton from accusations of lack of knowledge of the text, and she seems to relieve him of any blame when she points out that Caxton never claimed to be a scholar. However, when she refers to Cx2 she states that "the evidence is strong that he [Caxton] followed his texts closely except in the second edition of *The Canterbury Tales*, where he can be faulted for a serious mistake in judgement when he tried to correct the first edition from a manuscript obviously of a different textual tradition." Boyd is not correct in saying that Caxton can be 'faulted' for conflating texts from different recensions, and much less for not recognising this fact, since genetic groups for the witnesses of the text have been widely accepted as accurate only after Manly and Rickert's edition. It is rather unfair to suggest that Caxton should have had such knowledge.

Boyd also brings up the contrast in treatment which different texts received in Caxton's workshop. Caxton's influence on the texts he edited has also been the focus of the critics' discussions.⁵ For example Paul Needham, in his introduction to the facsimile edition of Caxton's *Le Morte Darthur*, explains that its text, as we read it today, owes a great deal to Caxton's editing (Needham 1976). Needham suggests that Caxton took extreme liberties with this text in the form of adding or deleting sections

1976).

⁵ See section 8, later in this chapter.

and passages or completing the text from the French Arthurian tradition, even censoring it when he felt that it did not accommodate his expectations of what an Arthurian romance should be (Needham 1976, no pagination). According to Boyd, because of Caxton's attitude towards the text of Malory, his editions of Chaucer have been approached with suspicion (1984, 16). One could suspect that changes similar to those found in *Le Morte Darthur* could also be present in the *Canterbury Tales*. However, Manly and Rickert have shown that Caxton followed closely a **b** manuscript for this first edition of the *Tales* (1940). The question of the process of composition of Cx2 has been studied by several scholars, each of whom appears to have reached different conclusions.

3. THE COMPOSITION OF CX2

In 1924 in an article concerning the *Canterbury Tales* incunabula, Greg stated, for the first time, what will become another well-known fact about Cx2: that it was not set directly from the new manuscript, that Caxton probably took a copy of his first edition and made the corrections directly on it:

...although the treasured manuscript was offered him [Caxton] 'for a cotype', all Caxton claims to have done is to have 'corrected my book' by it, which, of course, is just what we should expect a printer to do, but which is not the same as setting up a new edition afresh from a manuscript copy. (740)

This conclusion is the result of the analysis of Caxton's prologue and of the first 116 lines of KT as they appear in the first six printed editions.⁶ It might seem that the analysis of 116 lines is not sufficient to draw any long-lasting conclusions. However, Dunn, who analysed the complete text of Cx2, agreed with Greg and refined his conclusions about the composition process in Cx2. According to Dunn, what Caxton probably did was to write the corrections from the second manuscript in an unbound copy of his first edition; as a consequence of this, besides the fact that we have a conflated text, it seems that in many circumstances the typesetters misunderstood Caxton's instructions and made the wrong corrections (Dunn, 1939, 74). On some occasions the compositors added a word that was meant to replace another; on some others they added the correction at the beginning of the line, where Caxton had written the new word, instead of in its proper place somewhere else in the line. This can be seen, for example, in MI 113 and NU 301:⁷

MI 113

BASE⁸ A clerc hadde lutherly / bisset his while
Cx1 A clerk hadde lowdly beset his whyle
Cx2 Lythyrly a clerk had^θ beset hys whyle
E1 A clerck hadde lutherly / bisset his whyle
Hg A clerck hadde lutherly / bisset his while

A clerc hadde lutherly]
Lytherly a clerk hadde] Cx2 Wy
Lytherly a clerk hath] Pn

⁶ These six editions are both of Caxton's editions, Pynson (1492), de Worde (1498), Pynson (1526), and Godfray (1532).

⁷ Another example of this can be found in FK 905. The format for the collation is as follows: a lineated collation which includes a base text --Base--, Cx1, Cx2, Hg and E1 is given followed by a more traditional *apparatus criticus* which includes all the collated witnesses. The base text is included as a point of reference and it is as defined by the Canterbury Tales Project "a lightly edited version of Hg" in which all passages not present in this manuscript have been included. This is especially useful as a reference for lines, links --L8, L15, L31-- and CY, which are not present in Hg but are included in Cx2. For the lineation system see Blake (1996).

⁸ The base text, as used at the Canterbury Tales Project is a lightly edited version of Hg from which special characters have been removed and to which lines found in other manuscripts have been added (see Robinson 1996b and Solopova 2000).

lutherly] Ad1 Ad3 Bo2 Ch Cn Cp Dd Dl Ds1 El En1 En2 En3 Fi Gg Gl
 Ha2 Ha3 Ha4 Ha5 Hg Hk La Lc Ld1 Ld2 Ln Ma Mg Ps Pw Py Ra3
 Ry1 Ry2 Se Sl1 Sl2 Tc1 To1
 lyghtly] Ad2 Bw li Ra1
 litle] Bo1
 lowdly] Cx1 He Mm Ne Tc2
 ful evel] Ht Nl
 simply] Ph2

NU 301

Base The Aungeles face , of which thy brother tolde
 Cx1 That angelis face whiche thy brothir tolde
 Cx2 The aungelis face whiche thy brother of tolde
 El The Angeles face , of which thy brother tolde
 Hg The Aungeles face , of which thy brother tolde

of which thy brother] Ad3 Bo2 Ch Cp Dd Ds El En1 Gg Ha4 Hg Ht
 La Ra3
 which thy brother] Cx1
 whiche thy brother of] Cx2

These examples show that although the corrections here given as introduced in Cx2 agree with the readings found in the vast majority of the witnesses, they have been introduced in the wrong place. The evidence supports Dunn's suggestion about Caxton's marginal corrections in Cx1 being misinterpreted by the compositors of Cx2. Dunn's conclusions about the composition process of Cx2 have been accepted by later scholars such as Blake.⁹ After Dunn's work, no other study as detailed as his has been

⁹ In *Caxton: England's First Publisher*, Blake offers a couple of examples: "It has been proved that he took a copy of his own first edition and emended that against the new manuscript. The changes were haphazardly and irregularly made. The following types of mistake arose. In the first edition a line in 'The Miller's Tale' reads 'A clerk had lowdly biset his whyle'. But in the second edition the reading of this line is 'Lytherly a clerk had biset his whyle'. The reading arose through Caxton crossing our 'lowdly' and putting the correction for it, 'litherly' was to replace 'lowdly' and simply placed at the front of the line because it was in the left-hand margin. In other passages there has been conflation. In a line in 'The Pardoner's Tale' the first edition reads 'Thou my bel amy John Pardoner, he sayde', whereas most manuscripts read 'Thou beel amy thou pardoner, he sayde'. One may assume that 'John' was deleted and 'thou' added either above or in the margin. But in this case the compositor included both words so that the line became 'Thou beel amy, thou John Pardoner, he sayde'. The effect is disastrous in poetry." (1976, 99)

carried out concerning the source of Cx2. But even if no further effort was dedicated to the study of ω , scholars such as Blake, Needham and Hellinga have remained interested in studying Caxton's printhouse, his methods, and the dating of his editions.¹⁰

The way in which the text was handled by Caxton does not make it easy to determine the textual affiliations of ω . What we have is a collection of variants of unclear origin among which we can potentially find some that came from that second manuscript. These issues have troubled textual critics for a long time, since it is a difficult task to identify the variants that were in ω . It requires us to isolate variants that could potentially have weight in tracing the affiliations of this manuscript.

4. THE DIFFERENT TALE-ORDERS

In his analysis of the relationships between the early printed editions of the *Tales*, Greg emphasised that differences in tale order between them could be useful in establishing the relationships among these books. Caxton's two editions, which Greg refers to as C7 and C8, are central in the essay. Greg suggests that the manuscript used by Caxton as the source of his corrections was a very good manuscript, and also that the order of the tales in Cx2 is not necessarily that of the manuscript, and that it might have been the result of Caxton's interpretation of what the right order could have been originally: "The order of C8 [Cx2] does not appear to be that of any known

¹⁰ The case of the source Caxton's *Le Morte Darthur* is very different from that of the one for Cx2. The Caxton Malory has been the focus of multiple essays and polemic discussions (See section 7 of this chapter).

manuscript, and most likely Caxton merely took a hint from this source toward an improved order" (1924, 760).¹¹

It seems possible that Caxton did not completely revise the order of the tales in Cx2, but instead followed ω for some changes he felt he needed to introduce. There is no extant manuscript with the same order as that of Cx2, thus supporting the idea of an editorial order. Blake has written extensively about the order of the tales and he has suggested that indeed all the orders are editorial rather than Chaucerian. For example, in "The Debate on the Order of the *Canterbury Tales*" he states:

Most, probably all, the orders are the result of some consideration and they cannot just be dismissed out of hand as aberrant or wrong. If, as it is widely accepted, all orders are scribal, then the order proposed by one scribe has as much validity as that followed by any other. (1985a: 36)

Because there is no authoritative order, any possible order is as good as any other, at least theoretically. In this way all the manuscripts, no matter how late they were produced, have to be placed at the same level. If we accept this idea as true, we have also to take into account that probably some scribes and editors were more careful and worried about what they were doing than others, and that should have some weight when we consider the different possible orders of the tales. This contradicts Blake's statement: not all scribal orders have the same validity. It is a question of focus: all scribal orders have a theoretically little value in the sense that these are not Chaucerian, but once one has accepted that there is no Chaucerian order then scribal orders acquire new interest and have to be differentiated from one another. Some scribes and their supervisors are likely to have been more interested and careful than

¹¹ For a thorough analysis of the order of the tales in Cx1 and Cx2 see chapter 3.

others, and thus one has to weigh the tale-orders since some might be of more interest than others.

In any case, Blake's view differs from the ideas about Chaucer embedded in many earlier essays. For example, Eleanor Hammond, writing at the beginning of the twentieth-century states:

It has long been recognized that the original form in which the *Canterbury Tales* were circulated, perhaps that in which they were worked upon by Chaucer himself, was fascicular, booklike, and in several or many parts. Only in this way can we explain the systematic confusion which we find in the manuscripts, and only in this way can we imagine Chaucer as working over an unfinished poem of such character and scope. (1905-6, 162)

Undoubtedly, this generalised assumption influenced the way in which Hammond interprets the textual differences in the order of the tales. On one hand, when Blake suggests that the unfinished work was put together by the Hengwrt scribe, we could assume that there could have been some hints in Chaucer's working copy as to how he wanted his text to be ordered. On the other hand, if we accept the theory of fascicular circulation then the problem of the order of the tales becomes much more related to chance and to the good (or bad) sense of the different scribes and their editors. Even though the evident differences in tale-order between Cx1 and Cx2 are a good place to start comparing these books, as Hammond's work suggests, my own research shows that these differences --in isolation-- are unlikely to present enough evidence to sustain long-lasting conclusions about the textual status of ω .

5. TRACING THE AFFILIATIONS OF ω

Besides attempts to approach the differences between Cx1 and Cx2 using tale-order variation, previous attempts at collating the texts of both editions have been carried out by other scholars such as Koch (1902), who produced a critical edition of PD.¹² For his edition of PD, Koch collated 63 witnesses of the text (1902, XXX-XXXIII), including both of Caxton's editions:

*Cax.*² is, on the whole, a revised and corrected edition of *Cax.*¹, with which, however, it shares a good many mistakes, mostly together with the other MSS. of this subdivision, and only few that do not occur anywhere else... For his corrections, however, Caxton evidently made use of a MS. of the A-Type, in which alterations he is frequently followed by Thynne. (1902, LII)

Evidently, Koch was aware of the fact that the text of ω had a very different affiliation from that of Cx1, but his dual division of the witnesses of the *Canterbury Tales* gave him only one alternative group in which to place the variants of Cx2.

In the case of Greg's 1924 essay, he produced a detailed comparison of 116 lines of the beginning of KT in the six earliest editions. His general conclusion about the early printed editions of the *Canterbury Tales* makes evident the frustration he felt when unable to identify with certainty the sources for Caxton's corrections:

¹² Koch also produced a book called *A Detailed Comparison of the Eight Manuscripts of Chaucer's Canterbury Tales as Completely Printed in the publications of the Chaucer Society*. Koch did not only collate the manuscripts which the Chaucer Society had printed in full --E1 Hg Gg Cp Pw La (Furnivall 1868-77) Dd (Furnivall 1902), Ha4 (Tatlock 1909)-- but also included manuscripts that have been partially published --Ha2 Ha3 Ha5 Ad2 Ht Ii Ld1 Lc Ry1 Ry2 Se S11 and Bw. Among Koch's conclusions we find that he separated two types of manuscripts, type A and type B, that he thought of E1 as the manuscript with the best text and that there is no evidence in these witnesses to support the theory of independent circulation of the tales (Koch 1967, 418-9).

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. It follows that Caxton's first edition alone ranks with the manuscripts as a textual authority. In no case can the readings of the manuscripts used in later editions be recovered with anything approaching completeness; the editions themselves are merely reprints of the first more or less seriously conflated, and their only textual value lies in the fact that they may possibly preserve individual readings derived from manuscripts but not found in any now extant. Lastly, the utter failure to identify the affinities of the manuscripts used in Caxton's second edition and in Thynne's, unless it be due to a plurality of sources, raises some doubt as to whether conflation may not be so wide spread as seriously to interfere with any useful classification of the manuscripts. This, however, is clearly a problem requiring more extensive investigation before any considered opinion can be expressed. (761)

The quotation from Greg is extensive because it delineates succinctly the challenge of identifying the affiliations of ω . All of Greg's conclusions remain mostly true. Even if the texts were conflated it is possible to isolate the variants as Greg did. After this process he was still unable to trace the affiliations of ω . The only way to isolate such variants is by comparing each line in Cx1 and Cx2 and deciding about which of the differences can be considered significant, in this concept therefore what is significant is where the origin of Greg's results is explained.¹³

¹³ Greg's concept of variant can be found in *The Calculus of Variants* (Greg 1927) and "The Rationale of Copy-Text" (Greg 1966).

According to Greg only Cx1 "ranks with the manuscripts as a textual authority" but it has no more textual authority than any other text belonging to the **b** group, which does not say very much since the text of the **b** recension is believed to be distant from the origin of the tradition. In fact, Manly and Rickert state: "The MS from which Caxton printed was a very corrupt text" (1940, 57).

Although it is true that conflation could make it very difficult to classify the different texts into families, this task seems less daunting after Manly and Rickert's publication of their work. Manly and Rickert, recorded by hand, the variants they found in collation cards. This system has been described by Ramsey:

[T]he very process of registering the variants in all of the manuscripts by means of the very efficient collation cards became in effect the whole of the collation procedure because it performed what is ordinarily the second step after the registration of the variant readings, namely the discovery of the "variational groups" in the various loci. Still, if the cards changed two steps into one and made collation a mechanical enough procedure for students to perform, only Manly and Rickert could then use the cards for the far from mechanical process of classifying the groupings within the "variational groups" in terms of whether their relations were genetic or coincidental. (Ramsey 1994, 154)

The collation cards were probably the best system available at the time. As Dunn pointed out, before the 'Chicago collations' were available the task of assessing the relationships between manuscripts was much more arduous.¹⁴ They have the advantage of being easily corrected if a mistake is found in one of them. However,

they are not very effective to retrieve information --Manly and Rickert produced some 60,000 cards with more than 600,000 entries (Ramsey 1994, 81)--, no matter how systematically arranged.

Once it has been established that the starting point of research about ω is the collation of Cx1 and Cx2, Boyd's observation that the source for Cx2 belongs to a different textual group from that of the source of Cx1 becomes truly interesting (See Boyd 1984, 33-34). The fact that Cx1 and ω belong to different textual groups should facilitate the separation and classification of their variants.

In the past, textual critics have produced inconclusive results when trying to pinpoint the affiliations of ω . They relied on visual comparison of Cx1 and Cx2, as Greg and Hammond did, or on collation cards --it is likely that Dunn used Manly and Rickert's collation cards. Greg concluded that Cx2's manuscript source was not clearly defined (1924, 761). At least for the opening of KT, he thought that Koch was wrong in his assertion that the manuscript probably belonged to the Ellesmere-Dd group (Koch 1902). Later, in 1929, Margaret Kilgour attempted to articulate an answer to the problem identified by Greg. Kilgour limits her analysis to a comparison of the variants between Cx1 and Cx2 in PD, as Koch had done. She wrote that "Dr. Greg finds that none of the MSS of this group is consistently more successful than others in the Petworth or Corpus groups..." (Kilgour 1929, 186). Kilgour obtained results that are in conflict with those of Greg. She stated that Ad3 is closely related to Cx2 in PD and in GP. Greg, on the other hand, had concluded that in KT, Cx2 was related to a different manuscript, namely Ha3 --although he emphasised the fact that the affiliations of Cx2 for the opening of KT could not be determined (Greg 1924, 761).

¹⁴ "The handicap under which all of these previous scholars labored was the inadequacy of the evidence accessible to them" (Dunn 1939, 6).

The contradictory results of the two analyses could point towards a possible shift of exemplar in ω and not necessarily to the fact that Caxton might have used several different manuscripts to correct his first edition. It seems that there is no real point of disagreement between Greg and Kilgour, since they are focusing on different parts of the text. That fact alone should be enough to justify the different conclusions that they reached. Kilgour's own analysis points towards Ad3, which she considers very close to the manuscript source of Cx2. However, later she states that Ad3 is identical to the mysterious manuscript. Greg was invited to answer Kilgour's statements and he published an essay, "The MS Source of Caxton's Second Edition of the Canterbury Tales." In this essay he affirms:

When...she (Kilgour) writes in respect to A³[Ad3] that "the evidence strongly suggests that Caxton used either this very manuscript or one remarkably like it," she is stating no more than the fact. But later she argues that it was indeed A³ and not a closely similar manuscript that was the source. (Greg 1929, 1251)

The implication is, of course, that Greg did not consider the latter a fact, but mere speculation on Kilgour's part. The criticism seems valid, since it shows that she is inconsistent in her arguments. However, the conceptual problems embedded in Kilgour's essay are deeper than that, and are problems that tend to invalidate this early and apparent solution to the enigma of Cx2, in contrast to Greg's careful reservations. They are present at very basic levels of the text. She states: "In my anxiety not to overlook any variant which might have possible significance I may have included some which Dr. Greg would regard as of no consequence; these, however, will not affect the result" (Kilgour 1929, 187). In the first place Kilgour is including some

variants that Greg himself would have dismissed, which is brave. But then, she positively affirms that those same variants will not affect the result, which is simply misguided. If we acknowledge *a priori* that the variants are not going to affect the result, it seems unwise to include them in the first place. If the statement is some kind of conclusion, obtained after collating the variants, it is misleading to present it in the way Kilgour does. This problem with the data affects the solidity of Kilgour's general argument because it makes her appear as unreliable. However, the reason for the different conclusions reached by Kilgour and Greg might be due to the choice of analysed variants, which might unwittingly have led one of them to the wrong conclusion. The alternative explanation is that both Kilgour and Greg are right in the interpretation of their data, and that ω shifted its exemplar at some point, which would produce different results if we analyse isolated parts of the *Tales*. There is yet a third possibility, that in the data analysed by Kilgour and Greg the variants being analysed are archetypal and so cannot help place ω in a group with witnesses below the archetype. This would mean that, although both of them might have been right in their conclusions about the sections of the text they analysed, if the variants they isolated were not below the archetype of the tradition, all that was found was a series of archetypal readings which tell us nothing about the affiliations of ω below the archetype.

In 1940, Manly and Rickert had already dismissed any possible textual authority that Cx2 could have had:

Photostats of the Grenville copy of Caxton's second edition at the British Museum were collated in full and recorded in our collation cards, but the results are not included in our Corpus of Variants, as it became clear that

they could not aid in establishing Chaucer's text. Caxton collated his first edition with a MS lent him by a patron, but his collation was so hasty and imperfect that the readings of Cx², though interesting, are of no textual authority. (1940, 1: 81)

This statement is surprising given that they suggested the investigation of the source for the corrections in Cx2 as the dissertation topic for their student Thomas Dunn. If Manly and Rickert were so convinced of the lack of authority and importance of Cx2, there must have been another reason for them to suggest that Dunn should carry out research on the manuscript source of Cx2. The explanation for this might be that they thought this inquiry might offer an insight into Caxton's printing methods. However, the possibility remains open that they were doubtful about their own assessment of Cx2.

Dunn's dissertation was supposed to be centred on the editorial methodology used by Caxton in his second edition and it had to explain the textual affiliations of Cx2. Because Dunn was carrying out such a detailed study of Cx2, Manly and Rickert not only gave no details about textual matters referring to this edition, but also decided to suppress its bibliographical description. As a result, it is Dunn who has written most extensively on the text of Cx2. He even seems to offer a possible answer to the Greg-Kilgour dilemma when he states:

No existing manuscript could have been the source of the Knight's Tale, but E1 seems to be the closest. Ad3 would probably appear equally close had we the more than 600 lines which are lost from it. But even with the lost portions of Dd, that manuscript would probably not appear close. The

evidence is only that Y [ω] was close to the best extant texts." (Dunn, 1939, 50)

The statement is of extreme importance because Dunn's analysis is the most thorough that we have on the text of Cx2, and it seems to contradict Greg's conclusion that "...it does not appear possible to determine the affinities of Caxton's second manuscript for the opening of the Knight's Tale" (1924, 754). Greg had thought that no affiliation could be determined for the source of Cx2, but the data he analysed was different from that studied by Dunn.¹⁵ The different approach to the concept of textual variation is enough to explain the apparently different results achieved by Greg and Dunn. Although Dunn did not succeed in presenting a firm hypothesis about the relationships of ω , he was able to show some of these relationships for parts of the text. Using Manly and Rickert's concept of a variant, Dunn concluded that no extant manuscript could be the source for Caxton's corrections, but he failed to determine the affiliations of ω .

One of the problems with Dunn's work has to do with the fact that he offers detailed statistics of only a very limited number of manuscripts (Ad3, Ch, Dd, El, En1, En3), in which the absence of Hg is noteworthy. Although he states that he has used the collations of all manuscripts and of both of Caxton's editions (1939, 2), his detailed analysis covers only the manuscripts mentioned before. Dunn justifies this as follows:

I shall... take only representative manuscripts of the sub-groups that are nearest to Y. I shall list Ad3 Ch Dd El En1 En3 and not concern myself

¹⁵ Manly and Rickert revised the concept of variant to include more than just 'errors' as in the traditional Lachmann method. Instead they focused on the agreements and disagreements between the

with the multitudinous and shifting agreements that are to be found among all the manuscripts of every group from time to time. (1939, 43)

Choosing certain manuscripts because they appear to be closest to ω is not an issue in itself, but the process employed might allow doubts: Dunn decided about the closeness of these manuscripts to ω based on shared lines among those added in Cx2. Dunn's work presents this decision as a choice *a priori*, made before analysing the complete corpus of variants. Dunn explains that he based the decision of the closeness of certain manuscripts to ω on the presence or absence of major variants, that is, he assumes that if a manuscript lacks lines which were added in Cx2 it cannot be the source for its corrections. As a first approach, this must be deemed valid, especially if we take into account that when Dunn carried out his research it had not been confirmed yet that no extant manuscript could have been the source for the corrections found in Cx2. It is interesting to observe, however, that although Dunn took into account the complete corpus of variants for his collation of lines, he presented only a partial corpus for the word by word collation. It is conceivable that the word by word collation could shed light on the affiliations of ω and, for this reason, the present work includes a word by word collation which takes into account all available transcribed witnesses.

A problem, highlighted out by Dunn, is the difficulty of deciding which changes in Cx2 are editorial, i.e. did not come from ω but were introduced by Caxton himself. Although studying Caxton's editorial practices was one of the main objectives of Dunn's thesis he found this difficult to solve (1939, 6). In his conclusion Dunn states:

witnesses (1940, 20). Greg, on the other hand, divided and classified variants according to his own method. For a discussion of the concept of textual variant see chapter 4.

Of editing there appears to be very little in Cx². Five instances of it are mentioned in the conclusion to Chapter III; yet it is barely possible that Y contained these readings. But the possibility is remote in these instances. There is perhaps one other instance of it in B 4652 where the word man is inserted to adapt the line to introduce the narrator [the Manciple] of the following tale. Here are just enough instances to enable one to say the text is edited. (1939, 75)

Dunn does not make completely clear how these five instances mentioned in his chapter 3 and the example given in the conclusion provide evidence that Caxton edited the text of Cx². If anything, it would seem that the evidence is thin and that Dunn seems to be relying on unstated criteria to reach conclusions which do not appear to be justified in his work. Some of what will become Dunn's conclusions about the quality of ω 's text appear very early in his work:

From the study of the unique readings of Cx² in their relationship to the readings of Cx¹ and to those in the manuscripts one can postulate fairly safely the readings that Caxton found in Y. These readings are uniformly superior to those in Ne and Cx¹. In general, they are the readings of the best manuscripts. The source of corrections in Cx² was, therefore, a good manuscript. (1939, 29)

This is, perhaps, the most important outcome of Dunn's research. Indeed, his conclusion contradicts Manly and Rickert's statement about the lack of any authority in the readings present in Cx², since variants shared by 'the best manuscripts' should, by definition, have some textual interest.

6. THE ALPHA EXEMPLAR

Dunn did not carry on further research on the textual affiliations of ω . However, his suggestion about the quality of this manuscript source of Cx2 was further supported years later by Peter Robinson's conclusions in his essay "A Stemmatic Analysis of the Fifteenth-Century Witnesses to the Wife of Bath's Prologue," (1997, 108-110) that there was a manuscript -- α --and that this manuscript, or one very similar to it, was the one used by Caxton to correct Cx1. Robinson's assessment of α being a very good text leads to precise conclusions about its textual status when he affirms that "...it is likely that α is a direct copy of O, Chaucer's original." (1997, 124)

However, this contradicts one of Dunn's most interesting conclusions, that "... the shifting family relationships in the tales, is strong evidence that Y was a conflated text" (1939, 55). It is difficult to reconcile the idea of ω being a very good manuscript with the fact that it seems to have been a conflated text itself. One has to wonder if, in Dunn's view, this might mean that the ω was, somehow, a conflation of the very best manuscripts. But this would present a bigger problem when we try to explain how this might have occurred. Perhaps a scribe had access to several good but fragmentary manuscripts and decided to put them together, which might be an indication of independent circulation of the tales. The explanation, however, could be much simpler. Perhaps ω was the origin of several traditions or was close to this hyparchetype. In this case, Dunn's interpretation might have the same flaw as that of Kilgour and Greg, that is, because ω was so near the origin of the tradition, many of the variants which Dunn interpreted to be indicative of a genetic relationship might be archetypal readings. This error, if it is indeed one, might not have been entirely

Dunn's fault, it could have had its origin in a problem that Manly and Rickert had while grouping certain basic manuscripts. Their fundamental method made no attempt to distinguish archetypal readings, relying instead purely on 'persistent' and 'consistent' agreement to indicate groupings. As a result, there is a danger that they will see manuscripts as related when in fact they only share variants descended from the archetype. These readings are not useful for the classification because they indicate only that the manuscripts are descended from the archetype rather than from a copy below the archetype, which they must be if they are related as members of a distinct family.¹⁶

As explained above, Dunn also points out that there are no major changes in the prose passages of Cx2, and that the changes in the prose could be the result of Caxton's own editing or simply typographical errors. The main problem here would be to establish why Caxton decided not to correct the prose passages, and whether it was, in fact, his decision and not the direct result of a gap in his copy-text. Some critics believe that Wynkyn de Worde, who inherited Caxton's workshop, used the same manuscript to correct the prose of his edition of the *Canterbury Tales*.¹⁷ If this were true we should have to ask about Caxton's criterion to decide which parts of the text needed to be improved and which did not. These are complicated issues because they imply the necessity of careful analysis of Caxton's printing practices: his treatment of the texts, his degree of care, and his idea of what a good text should be. Blake has pointed out that the manuscript used by Wynkyn de Worde for section ten could have been the same as the one used for Caxton's second edition: "The manuscript was a good one, closely related to Hg, and it is interesting to speculate

¹⁶ See my discussion of Kane's critique of the method employed by Manly and Rickert in the introduction.

whether this manuscript was the same one used by Caxton to revise Cx2" (Blake 1985b: 5). Indeed, these are important observations which, if confirmed, could not only shed light on the nature of ω , but also on the printing practices of early printers.

Daniel Ransom, in his 'Critical Commentary' to volume 2, part 1A of the *Chaucer Variorum*, suggests that, for GP, the closest manuscripts to the corrections in Cx2 are Ch Dd and El. He states:

And though Dd shows many divergences from Cx2's alterations of Cx1, since those divergences are for the most part trivial, it is not impossible that a twin of Dd served as Caxton's correction text.

The mss closest to the correction text are El and Ch. El could not have supplied CX²'s corrections of CX¹ at lines 57, 70, 217, 252b-c, 430 (2), 604; Ch could not at lines 179 and 408. If a twin of El were used by Caxton, it must have had lines 252b-c; this difference from El is possible given that the presence of these lines is not always consistent with other evidence for manuscript affiliation (see MR 2.78-96). The extraordinary correlation of Ch and CX²'s alterations of CX¹ and the high quality of Ch's text generally (see MR 1.87-88) suggest that the relationship of Ch and CX² deserves further attention. (Andrew et al. 1993, 84)

Indeed, although Dunn studied Ch in the group of manuscripts he deemed closest to ω , his research did not show an especially significant number of agreements in reference to the other witnesses. Ch is a manuscript that was classified as anomalous

¹⁷ Cf. Garbáty (1978) and Blake (2000).

by Manly and Rickert and that Robinson has called an **O** manuscript,¹⁸ and it remains to be seen if a more tuned classification can be offered.

One of the most interesting textual problems in Cx2 is the fact that, even if ω appears to be related to Manly and Rickert's **a** group,¹⁹ it lacks some significant features that usually occur in this group:

...all known manuscripts of group *a* contain other links between tales and other characteristic features of order *not* found in Caxton's second edition.

If the second manuscript belonged to group *a* and contained these additional links, why did he not take all the links to be found in the manuscript he was using? The answer is that he worked too quickly.

(Blake 1969, 104-5)

But if what Garbáty says (and Blake seems to support) is true, and the manuscript used by Caxton to correct his first edition was kept in the workshop, we would have to find an explanation, other than that he had to return the manuscript, for the speed with which he finished his second edition.²⁰ It is possible that he was trying to keep his presses busy so the business would be profitable. To have the presses stopped would have been very costly, which explains why, even if Caxton had been the owner of the manuscript, he had decided to work as fast as possible. However, there is yet an alternative explanation which might explain the fact that the prose was not altered in Cx2. On the one hand, to calculate the new pagination of the verse tales and links in Cx2 would have been a relatively easy task because it would have required merely to

¹⁸ See the introduction and chapter four (especially note 4) about the definition of the **O** witnesses.

¹⁹ In fact Robinson suggests that the "...[α] variants, shared by El Dd AB and Cx2, serve to mark their shared descent from α " (1997, 125)

²⁰ About this possibility, Blake wrote: "...Garbáty has shown that de Worde had access to a good manuscript closely related to Hg and it is possible that this is the same manuscript used to correct Cx1 for Cx2 which remained in the workshop from 1482 to 1496." (2000, 7?)

count the added or suppressed lines and allow more or less space for them. On the other hand, alterations to the prose would have been much more complicated to deal with as it would have been much more difficult to calculate the space the altered text would have occupied.²¹

7. THE ROLE OF ANALYTICAL BIBLIOGRAPHERS.

In 1975, Lotte Hellinga made an amazing discovery. She found traces of Caxton's types in printing ink on the Winchester manuscript of Malory's *Le Morte Darthur*. This discovery could offer further support for Blake's hypothesis about ω remaining in Caxton's workshop for a while, if we assume that the ink traces are an indication of how long the manuscript was there.²²

... there are traces that could indicate that the manuscript had been in Caxton's printing house: a fragment of an indulgence printed by Caxton was used to repair a leaf; and more intriguingly, there were smudges of printing ink, and some very faint offsets of printing types which only Caxton possessed. (Hellinga 1982, 90)

The marks made by the types in the Winchester manuscript suggest that it was at Caxton's workshop and that it had been left open and lying near the presses. Since we know that Caxton's edition of *Le Morte Darthur* was not set up directly from this manuscript, we could assume that it was there for some other reason and that perhaps the manuscript was in Caxton's workshop for some time. If he treated ω in the same

²¹ This hypothesis was suggested to me by Dr Peter Robinson, in private conversation.

²² Blades indicates that *Le Morte Darthur* was printed in type 4* (1882, 301 and ff.), while the traces found by Hellinga on the Winchester manuscript are types 2 and 4 (1982, 91).

way, there is a possibility that it remained in the workshop for a number of years, up to the time the workshop was inherited by de Worde.

Analytical bibliographers have tried to establish the date of publication of both editions of the *Canterbury Tales*, and have also studied the different types used in Caxton's workshop. The conventional wisdom can be summed up as follows:

The book [Cx1] is undated, but scholarly opinion has long placed this first printing of Chaucer in 1478. Caxton returned to England in 1476 and established his press in the precincts of Westminster Abbey, at a house with the sign of the "Red Pale" in the "almshouse." His first dated book issued in England is *Dictes or Sayengis of the Philosophres*, finished 18 of November of 1477. Paul Needham's recent research on watermarks in paper used by Caxton is showing that the *Canterbury Tales* was Caxton's first publication on his return to England in 1476. The book is consequently dated [1476]... (Anderson n.d., 9-10)

The implication is that the date of the second edition, if we accept 1476 for the first, would be 1482. This early date for the first edition would account for some of the features of Cx1: it lacks signatures and running titles, traits that could be attributed to Caxton's inexperience as a printer (Blake 2000). It would also point to a more adventurous, or more knowledgeable --depending on the point of view-- idea of Caxton as a printer. But mainly it would suggest that the printing of the *Canterbury Tales* was a risk since it was published towards the beginning of the venture and could not be guaranteed to be a success. On the other hand, the more polished layout of Cx2 suggests that Caxton might have been confident in the fact that the book would sell, and took care to make it more appealing by adding a preface and woodcuts.

8. CAXTON'S TREATMENT OF HIS COPY-TEXTS

Debates about Caxton's treatment of his copy-texts usually centre on his edition of Malory's *Le Morte Darthur*. Discussions about this book have continued since the Winchester manuscript was discovered in 1934.²³ In 1947 Eugene Vinaver published an edition based on the Winchester manuscript. Although for years it was considered the authoritative edition of Malory, later reception --that is, after 1975, when William Matthews questioned the authority of the text-- of this new text has been controversial. On the one hand, Vinaver claimed that he had edited the real Malory, and he did it under the name of *The Works of Sir Thomas Malory*, to reinforce the fact that he was rejecting Caxton's title *Le Morte Darthur*, and also rejecting any ideas about the unity of the text that might derive from it. And on the other hand, William Matthews, defender of the version printed by Caxton (1997, 129-30), appeared and with him a group of scholars, such as Moorman (1987, 1995) and Spisak (1982) who contend that it is a second authorial version and therefore has the authority of a text that carries the author's final intention. Other scholars such as P. J. C. Field (1995) and Shunichi Noguchi (1995), think that Caxton heavily edited Malory's text to produce his edition, and therefore the Winchester manuscript is the best copy-text for an edition. The editorial policies of William Caxton have been the subject of more than one controversy because the many divergences between the newly-discovered manuscript and the traditional text handed down by England's first printer called into question Caxton's role as an editor. The complex debates

²³ For a full account of the discovery of the manuscript see Oakenshott (1963).

surrounding Caxton's edition of the *Le Morte* were complicated further by Hellinga's discovery of offsets of Caxton's types on the Winchester manuscript:

...offsets of printing ink in many places in the Malory manuscript show that it has been used intensively in close contact with damp pages of books printed by William Caxton between 1480 and the end of 1483. The presence of a fragment of an indulgence printed by William Caxton agrees with the evidence for the presence of the manuscript in or near the workshop in Westminster, and forces us to assume that if it was there it remained there at least as late as 1489. (1981, 134)

This could have shifted the balance in favour of those who believe that Caxton heavily edited the text of Winchester since it shows that the manuscript was in Caxton's workshop, but Hellinga's own analysis suggests that the manuscript was not used as a copy-text in the workshop.²⁴ Blake has explained the offsets of Caxton's types in Winchester by suggesting that the manuscript was produced by Caxton's request and modified to produce a more moral text than that of the Winchester manuscript (2000).²⁵ It is clear that the problems generated by the differences between Winchester and Caxton's Malory present more difficulties than they can solve. The

²⁴ Hellinga offers the following explanation: "A first examination showed that the manuscript [Winchester] did not bear any trace of calculations made by a compositor to set his text by formes, or any other of the marks that compositors are known to make. These may merely consist of tiny dashes or dots, and could therefore hitherto have been easily overlooked. By the time of the first edition of *Le Morte Darthur*, which was completed on 31 July 1485, Caxton had his books set by forme (two pages in folio, or four pages in quarto at a time) as is proved, for example by the surviving printer's copy for the *Nova Rethorica* which was printed at Westminster, probably in 1479" (1982, 127)

²⁵ In 1969, before Hellinga's discovery, Blake had reached a different conclusion, that a scribe might have been responsible for the differences between Caxton's copy-text and the Winchester manuscript: "He [Caxton] used one manuscript (he mentions only one manuscript in his prologue), which included all the tales; and those tales appeared in his manuscript in the order in which he printed them... He did, on the other hand, frequently alter the language of his copy and add or delete episodes. As the Winchester manuscript is not the one that he used in setting up his text, it is not certain that the differences between the printed text and the manuscript should be attributed to Caxton rather to the scribe of the manuscript he was using; but this seems most probable and will be assumed here (1969, 108).

only conclusion that can be drawn from the Malory debate is that we do not have Caxton's copy-text for *Le Morte Darthur* and so are not in a position to judge exactly what changes he might have made as an editor.

Blake has also studied Caxton's edition of Trevisa's translation of Higden's *Polychronicon*, in the translation finished in 1387 (1969, 114):

Trevisa's book is then printed much as it appeared in the manuscript Caxton was using, except that he modernized the language. Trevisa's prologue and colophon are given as well as Higden's own prologues. After Trevisa's colophon, Caxton added the epilogue which introduces the final book, often referred to as the *Liber Ultimus*, that he had decided to include with the *Polychronicon*. Trevisa's translation had ended in 1357; Caxton's continuation carried it down to 1461. However, in the epilogue which precedes the *Liber Ultimus* Caxton mentions that his own addition is not to be compared with Trevisa's work and that he has therefore separated the two parts of the volume. (1969, 114-5)

Blake clearly states that although Caxton added the *Liber Ultimus* to Trevisa's translation he printed the text as it was in the manuscript, and that he went to great length to ensure that his own production was clearly distinguished from Trevisa's text. Blake makes no mention of possible copy-texts for this book. In an article entitled "William Caxton: His Choice of Texts," Blake mentions that Caxton used three different manuscripts for his edition of Gower's *Confessio Amantis* (1965, 304). Later however, Blake carried out his own analysis and concluded that:

Although one can easily accept that one manuscript may have got lost, it becomes less probable that one of two or three such manuscripts would

not have survived, when one considers that 49 manuscripts of *Confessio Amantis* are extant. (1965, 292)

Blake finishes the article by stating that there is nothing in Caxton's prologue to the book to indicate the use of more than one manuscript. Blake takes Caxton literally and concludes that the arguments he puts forward indicate that only one manuscript was used, even though no extant manuscript contains all the features of the text found in Caxton's edition of *Confessio Amantis* (1965, 289).

The main problem in establishing Caxton's attitude as an editor is that we do not have the copy-texts he used for his editions. As Boyd has put it: "Since the source-manuscripts from which Caxton prepared his editions are not available, his books not only stand at the head of the printed traditions in each case, but, as records of his sources, they are themselves part of manuscript tradition in the study of the texts" (Boyd 1978, xix). Although this fact grants Caxton's texts a more important role in textual studies, it also makes it very difficult to assess how accurate or free these editions are and allows for speculation about Caxton's editorial methods. There is one case, however, in which we have Caxton's copy-text and which could help us clarify his attitude towards the texts he edited. Dunn demonstrated that Caxton did not make changes in the prose texts of Cx2, which means that these were printed directly from Cx1:

In the preceding pages I have examined carefully every indication that Caxton might have had a manuscript for his second edition of the Canterbury Tales in the prose. Other than for one phrase in the 'Retractions' I find no evidence that he used one. Instead I find agreement with Cx¹ all the way through. Caxton (or his compositor) could certainly

follow an exemplar closely, as well, in fact, as a modern printer. The changes that he made, save for the last two possibly, were intended to improve the readability of Cx² over Cx¹. His only aim apparently was to produce a text that made sense. Cx¹ in general did that. Therefore he accepted Cx¹ for his text, and changed it deliberately where it was obviously incorrect. Only a small number of errors crept in. His criterion of a good text in prose, then, I surmise to be readableness. (Dunn 1939, 11-12)

Dunn concludes, based on the low rate of corrections found in the prose, that the only changes made to TM and PA are related to errors in Cx1 which were corrected in Cx2 without the use of any external sources. The collation of the texts of Cx1 and Cx2 shows that the rate of variation in the prose is very low and this supports Dunn's conclusions.²⁶ The closeness of Cx1 and Cx2 in the prose shows that Caxton could produce a printed text which followed its source extremely closely.

9. SYNTHESIS AND CURRENT ISSUES

As this chapter shows, few critical discussions focus on the textual problems of Cx2. However, the critics share some seemingly recurrent ideas. Most of them agree in saying that Cx2 is a conflated text resulting from the correction of an exemplar of Cx1, which itself was based on a poor manuscript, and that this manuscript belonged to a different textual group. Critics also seem to agree in

²⁶ See chapters 6 and 7 for the analysis of the data from the prose texts.

pointing out that the Cx2 manuscript source (ω) would have been among the best texts, and that it would be of the greatest importance to establish its affiliation.

In synthesis, what critics have said specifically about Cx2 is as follows:

1. No extant manuscript can be identified with ω (Greg, Dunn).
2. The affiliation of ω is clearly different from that of Cx1 (Greg).
3. It is possible that more than one manuscript was used to correct Cx1 (Greg).
4. It is impossible to determine the precise affiliations of ω (Greg).
5. Ad3 is the closest manuscript to ω (Kilgour).
6. Variants from Cx2 are of no textual authority (Manly and Rickert).
7. Of the extant manuscripts, Ad3 Ch Dd El En1 and En3 are the closest manuscripts to ω (Dunn).
8. ω is a conflated text (Dunn).
9. Caxton made marginal corrections that were, occasionally, misinterpreted by the compositors (Dunn).
10. The α exemplar was very similar to ω . It could have been identical to it (Robinson).

The present work addresses these issues in order to validate or deny them, or find evidence that could open new perspectives and generate different problems. Specific statements about each of these issues, based on the new collation of the variants in Cx2, can be found in the conclusions where they are individually discussed. In the conclusion, I shall return to these specific questions and present the answer to each question suggested by the research of this thesis.

