

CONCLUSIONS

1. A SYNTHESIS OF THE VARIANT DISTRIBUTION IN Cx2

The aim of this work was to trace the textual affiliations of ω --the manuscript source of the variants in Cx2-- , and in order to do that a complete collation between Cx1 and Cx2 had to be produced and variants between the two editions isolated. Around three thousand variants which fitted the preliminary criteria and could have had their origin in ω were isolated in Cx2. Their distribution by groups is as follows:

77.2% Cx2-O variants

11.6% Cx2-Unique variants

4.5% Cx2-Hg/El variants

6.7% Cx2-not-Hg/El variants

The vast majority of these variants were Cx2-O, which means that they are likely to be improvements on the text of Cx1 and are in agreement with the majority of the witnesses. Some of these are very small changes, while others are more obviously meaningful and impress the reader as producing dramatic changes of meaning. Although this group of variants does not need --for this research-- a thorough analysis, and most of them have been confined to an appendix, they are determinant in order to establish the quality of ω . In fact, because these good-quality variants represent approximately 77% of the total one can say that the text of ω contained a

good text of the *Tales*. In order to determine how good a text ω had, we need to assess the rest of the isolated variants.

It might seem that there is a relatively high number of Cx2-Unique variants; it is important, however, to take into account the fact that not all the witnesses have been fully transcribed. For this reason, variants which might be found in some other witnesses --including Wy and Pn-- might appear as singletons.¹ This could explain the seemingly large number of this particular kind of variants. In theory, there should be only two kinds of singleton variants: those which were present in ω and those produced either by Caxton or his compositors. The second case, that of compositorial mistakes, could have been considered just as mistakes and not taken into consideration. Occasionally, it might be relatively easy to distinguish compositorial mistakes, for example in the case of inverted letters --'u' and 'n'. However, the difficulty in distinguishing the two kinds suggested that even those variants which were suspected of being the result of compositorial mistakes had to be retained. In doing this, material was added to the bulk, perhaps making it appear larger than it really is.

The Cx2-Hg/EI variants represent 4.5% of the total, and they have been shown to be useful to establish some of the most important relationships of ω . Moreover, because, these variants represent points in which Hg and EI disagree, they are helpful in supporting relationships established by the Cx2-not-Hg/EI variants. For example, if in the Cx2-Hg/EI variants there were agreements with ω below the archetype, and these agreements were to occur with the same witnesses that are grouped with Cx2 in the Cx2-not-Hg/EI variants, one could then expect to be in the presence of a genetic

¹ If a variant only appeared in Cx2, Pn and Wy, this would remain a singleton since both Pn and Wy are dependent on Cx2.

group. If this were to occur often it would substantiate the existence of genetic relationships between ω and other witnesses. In fact, this research has found consistency of agreements below the archetype with Ad3 Ch and Ha4. In a different set of agreements, the variants from ω agree with **E** group manuscripts such as El and Gg in what are clearly non-archetypal readings. The agreements with **E** are not found throughout the text. Instead, in the current collation, these are localised to particular sections of the text --SQ and KT, for example.

The Cx2-not-Hg/El variants can be divided into those in which ω agrees with Hg and those in which it agrees with El. The division is more or less even in these agreements, with some 51% agreeing with Hg and 49% with El. Although this could probably be explained, it is the nature of these variants which is apparently puzzling. On the one hand, when Cx2 agrees with Hg the variant is usually archetypal: a very good example of this is WBP 484, where Cx2 and Hg share the reading 'troce' against El's 'croce.' On the other hand, variants in which Cx2 agrees with El appear to be of greater importance to determine the nature and affiliations of ω , since, in general, they are non-archetypal variants, e.g. those found in SQ 194 and 491. Some of Robinson's conclusions concerning WBP are related to El's apparent change of exemplar around line 400 of WBP. This together with the evidence of non-archetypal agreements with ω indicates that these manuscripts share a common ancestor below the archetype for at least part of the text of the *Tales*.

Perhaps the most interesting group of all are the Cx2-not-Hg/El variants. These, with the support of the Cx2-Hg/El variants, have shown that the closest affiliations of ω are Ad3 Ch and Ha4. Ht and Hk are also very close but, because transcriptions of them are incomplete, it is not possible to make a definite statement about these

manuscripts at the moment. The text present in Cx2 and not present in Hg and El consistently seems to be a very early and very good text.² The fact that Cx2 shares variants that are widely distributed in the tradition seems to point in this direction, but variants such as 'sterres' in KN 1179 witness a common origin --an opinion supported by Blake (1985: 56) for the origin of the text of ω , Ad3, Ch and Ha4 in what could be an ancestor below the archetype.

2. DIFFERENCES BETWEEN PARTIAL AND OVERALL ANALYSES

Clearly, conclusions drawn from analysis of part of the text may differ from the overall analysis. In different sections, tales and links, analysis shows that their textual affiliations differ. This does not mean that the tales circulated independently before Chaucer's death. Instead it might be the result of differences in interest on the part of the scribes and supervisors who might have known certain tales better than others.³ If a special interest was developed for certain sections of the *Tales*, this might reflect on the changes or accuracy of the copied text. These can be, occasionally, explained because not all the manuscripts have been transcribed for the whole of the *Canterbury Tales*. Even manuscripts which have been completely transcribed could sometimes have lost leaves which makes it impossible to be sure what the affiliations for the missing parts could have been.⁴ However, even in the cases in which we have

² See, for example, the ambiguous variants in L8, L31 --12. Other good examples can be found in the variants in WBP in the first of the so-called added passages.

³ An example of this can be found in the amount of glosses and commentary found in WBP and ML, in contrast with those found in MI. The interest of the scribes is also reflected in deliberate alterations made to the text.

⁴ A good example of this is given by Ad3, quire 17, where the first folio is missing. This folio would have contained ME 61 and 62 or some variant of these lines, a potentially determinant factor to explain

complete transcriptions one can find that the affiliations they show vary from one section of the text to another. For example, in SQ, when Cx2 and El agree, they do so in what seem to be non-archetypal readings, which indicates that they share a common ancestor below the archetype. This is consistent with the change of exemplar suggested by Robinson for El in the WBP, where El seems to be in agreement with manuscripts of the **E** group. This could explain the agreements in non-archetypal variants shared by ω El and Gg, by suggesting that these witnesses share a common ancestor. However, the difference between analyses by sets and the overall analysis has nothing to do with independent circulation of tales, and it is more likely to be due to the fact that only a partial collation has been carried out for this work, or it might concern the different variation rates from text to text or from scribe to scribe. If, for example, El had more than one change of exemplar, then the results of the collation would become clearer if we had a complete collation, although this does not imply that such changes of affiliation could not appear in a collation with a limited number of witnesses. But the problem will remain and, with incomplete transcriptions, we might find a significant amount of singleton readings which could be erroneously interpreted as unique. In addition, if when we are able to discover, by collating all or nearly all available witnesses, that some of these witnesses share the same characteristics, then the overall collations for this work would appear to yield inconsistent results --since the result of a partial and that of a complete, or virtually complete, collation might appear as if these results were pointing in different directions. An example of this can be seen in the printed editions after Cx2: when Wy and Pn have been transcribed, these incunabula often agree with Cx2. It is

the affiliations of this manuscript. In the same way, Ha4 lacks L20. If this had been available, the collation of the other witnesses could have been greatly enriched.

conceivable that readings which have been classified as Cx2-Unique variants might be supported by other witnesses which have yet to be transcribed. Admittedly, this has no importance for the purposes of this research but shows only that both Pn and Wy were based on Cx2, as suggested by Greg (1924). It is not possible to find other examples, at the moment, because of the current state of the transcriptions. But let us consider what would have happened if Ad3 had not been completed for FK. It would then have been impossible to observe that only Ad3 Cx2 and E1 share FK 746-1 to 746-2 and FK 782-1 to 782-6. These passages, which represent such a strong piece of evidence of genetic relationship between these witnesses, in the absence of the transcript of Ad3 would appear to be less significant than they really are. Only when all the witnesses have been collated will we be able to draw a more accurate picture of the fifteenth-century witnesses of the *Canterbury Tales*.

3. CRITICAL PERSPECTIVES ON THE SOURCE OF Cx2 REVISITED

In the first chapter of this work I produced a synthesis of the scholarly opinions on the manuscript source of Caxton's second edition of the *Canterbury Tales*. Here I address each one of these opinions in order to make clear if the collation data answers, refines, confirms or denies them.

1. No extant manuscript can be identified with ω (Greg, Dunn).

Nothing has changed since Greg and Dunn pursued their respective enquiries on the subject. In the textual tradition of the *Canterbury Tales*, as it exists today, no manuscript can be said to have been the source for the corrections in Cx2.

2. The affiliation of ω is clearly different from that of Cx1 (Greg).

Manly and Rickert have shown that Cx1 belongs to the **b** group. The affiliations of ω are very different from this. The manuscript source for Cx2 has a marked tendency to agree with Ad3 Ch and Ha4, but Ht and Hk also seem to share a significant proportion of variants with ω .

3. It is possible that more than one manuscript was used to correct Cx1 (Greg).

The possibility that more than one manuscript was used to make the corrections for Cx2 should be taken into consideration. However, this research has shown consistency in the variation throughout the text, that is, the agreements found in the different sets, if occasionally slightly different, do not appear to contradict each other. On the contrary, the variation in Cx2 points in a single direction. I have also shown that in the places in which the variants appear to differ from those in the greater part of the text this may be due to factors other than a change of exemplar, e.g. agreement by coincidence, contamination, or compositorial intervention.

4. It is impossible to determine the precise affiliations of ω (Greg).

The problem with this statement is that it depends on how one defines 'precise.' It is a fact that it would be very difficult to determine exactly the affiliations of ω , but this is also true concerning the affiliations of the vast majority of the witnesses of the *Canterbury Tales*. However, this could also be said about the vast majority of the witnesses of the *Tales*, that their affiliations can be established with a relative degree of certainty, such as those of **a** group, Cn Ds En1 and Ma, or of the pair Ad1 and En3 -- **α** manuscripts. The same is true --and this research has shown it clearly-- concerning the relationships of ω with the manuscripts and early printed editions of the *Tales*.

5. Ad3 is the closest manuscript to ω (Kilgour).

Kilgour was probably right, since Ad3 is clearly the manuscript that shares the highest number of stemmatically significant variants with ω . Her statement was based on the data of KN only, and it is lucky that in this particular tale the affiliations of ω are clearer than in other parts of the *Canterbury Tales*. Had Kilgour analysed MO, she might have reached different conclusions. To establish with certainty the textual affiliations of any witness, a complete collation of the whole of the text should be carried out.

6. Variants from Cx2 are of no textual authority (Manly and Rickert).

Manly and Rickert did not themselves carry out any detailed textual analysis of the variants found in Cx2. Instead, they gave the task of tracing the affiliations of ω to Dunn. One has to assume that when Manly and Rickert reached this conclusion they were thinking about Cx2 as a conflated text only, and this inclined them to regard the variants in this book as unimportant.

However, my research shows that variants from ω are of the very best quality. Some of these can help support the variants of Hg or El when these manuscripts are not in agreement. Occasionally, the variants from ω can help to make evident the cases in which Hg and El agree in error --as seems to be that of KN 1179 and CL 1067. In the worst case scenario, variants found in Cx2 are very useful to understand a part of the development of the textual tradition of the *Tales*.

7. Of the extant manuscripts, Ad3 Ch Dd El En1 and En3 are the closest manuscripts to ω (Dunn).

Basically, Dunn concludes that six manuscripts are very close to ω . This research has shown that the manuscript that is consistently closest to ω is Ad3, followed very

closely by Ch. Ha4 is frequently in agreement with ω , but not as often as Ad3 and Ch. The fourth closest manuscript seems to be Ht.

8. ω is a conflated text (Dunn).

Once more, this conclusion depends on how one might interpret the data. It is true that at some point in Cx2, its manuscript source seems to change affiliation. This is especially obvious after TM where the most frequent agreements seem to be with manuscripts of the **a** group. This, however, does not mean that there are no shared variants with Ad3 Ch or Ha4.

When facing the evidence, one could assume --as Dunn did-- that ω was a conflated manuscript, which would explain the change in affiliation. Or one could think that Cn and Ma --the **a** manuscripts that seem to agree with ω after TM-- are the ones that have had a shift of exemplar. Another interpretation could be that ω was not a single complete manuscript but two or more pieces which were used to correct Cx1.

I tend to think of ω as a single manuscript, from which the **a** hyparchetype probably ultimately originated, and also from which Ad3 Ch and Ha4 might have descended. This is not to say that these are the only manuscripts descended from ω . It is also possible that the **E** hyparchetype and even E1 could be more distant descendants of this manuscript.

9. Caxton made marginal corrections that were occasionally misinterpreted by the compositors (Dunn).

This research has shown that now and again the compositors took literally a correction that Caxton had made in the margin but that was supposed to be inserted elsewhere in the line. The result is that the archetypal variant was reintroduced in

Cx2, but was put in the wrong position, therefore allowing for the creation of a new variant. Examples of this can be found in lines: MI 113, NU 301 and FK 905.

10. The α exemplar was very similar to ω . It would have been identical to it (Robinson).

In point 8, I have already stated what seems to be the most obvious result of this work: that ω was a very good manuscript, probably as good as the best manuscripts now in existence, perhaps only one step removed from the archetype, that is, likely to be a daughter of the archetype. It is also possible that ω was the ultimate parent of manuscripts that up to this point have been considered as representative of independent lines of descent in the textual tradition. These ideas seem to correspond to those of Robinson about the α exemplar. So in fact, ω and α could have been the same.

4. HYPOTHESISING STEMMATA OF THE RELATIONSHIPS OF ω

It is interesting to note that different interpretations depend, up to a certain degree, on which manuscript --hypothesised or actual-- is thought to be the archetype, or nearest to the archetype.

For example, one might explain the textual tradition as having O as the archetype --as Manly and Rickert proposed-- from which two manuscripts are descended: ω and Hg or Hg's mother --which we could call η . If one chooses to think that Hg is one step removed from the archetype, the sister of ω would be η . The tree would be as follows:

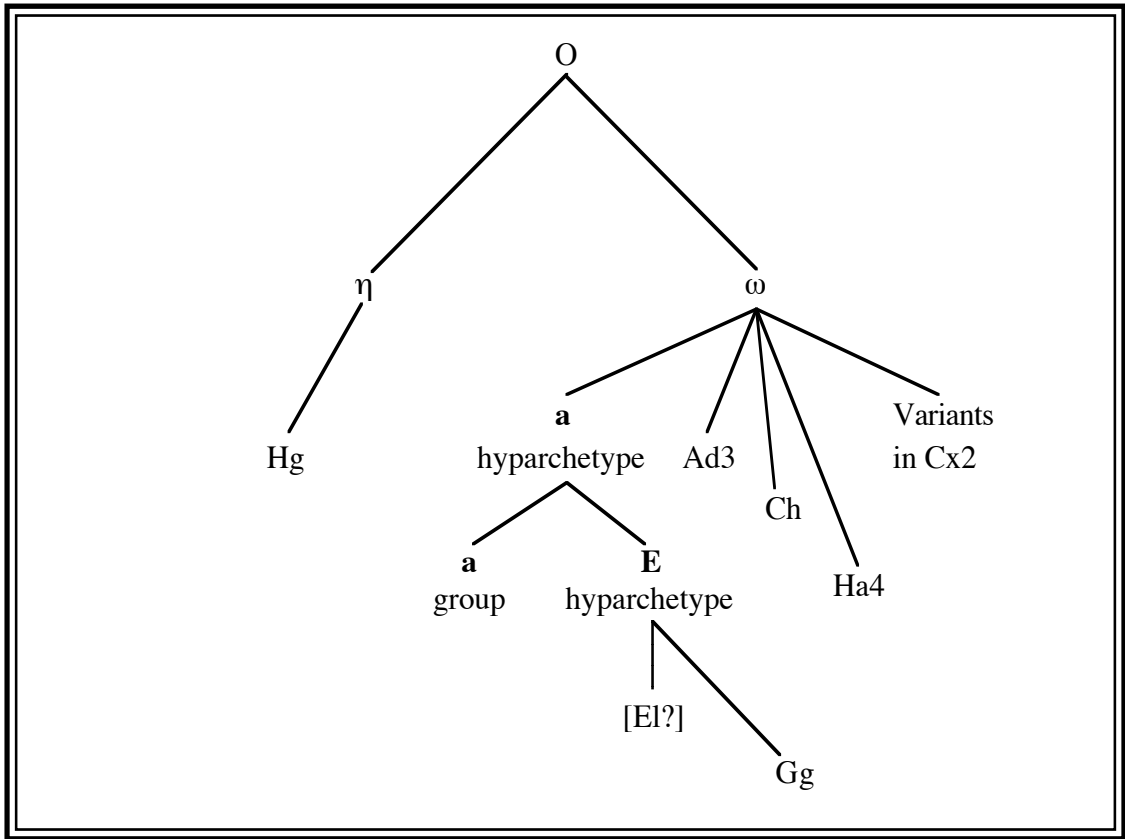


Figure 4

But one could also choose to shift the tree --as phylogenetic software allows and root it at η . This would mean that Hg is only one step removed from the archetype, that is, Hg would be a daughter of the archetype. In this case, a tree might look like this:

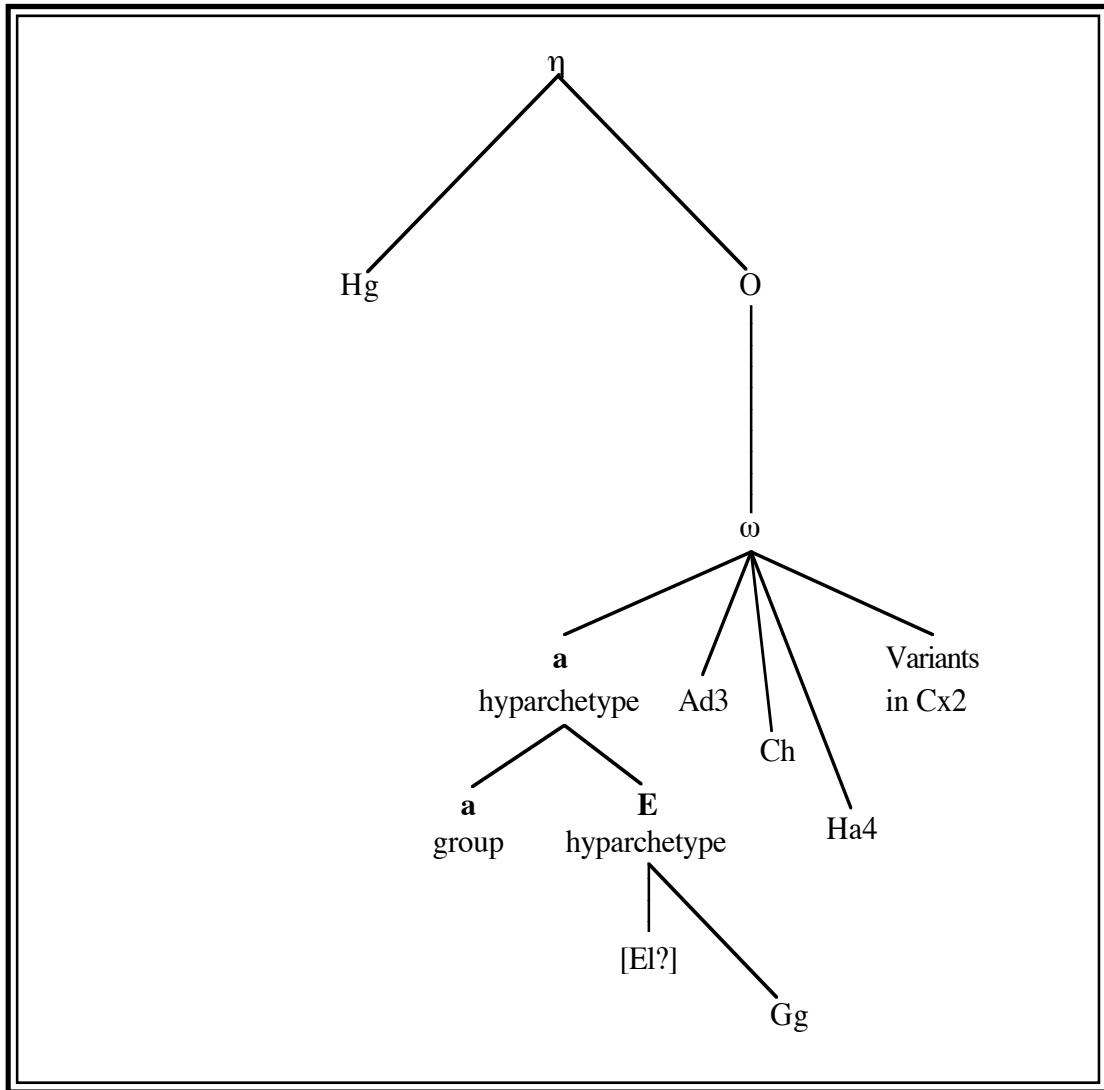


Figure 5

In this way, the same tree can be rooted at different points, but the relationships between the witnesses remain the same as shown by Robinson in the "Analysis Workshop" (Robinson 2000a). The problem with this proposed stemma is that the collations have shown that Hg is at least one step removed from the archetype. The second issue here is that there are two exemplars between Hg and ω , when the variants seem to indicate that there should be only one exemplar. In fact, ME 61 and 62, and their variant lines, are a good example of the possibility of Hg and ω sharing the same common source. This hypothesis, however, should not be understood as

positive statement about a deeper relationship between the two manuscripts: even if it could be shown that they had been copied from the same exemplar they would represent two distinct lines of descent.

ME 61 to 65 are not present in Cx1, and Cx2 has added ME 63 and 64. The likely reason for this kind of correction is that the lines were either not present in ω or that they were defective in such a way that Caxton thought it might be best to leave them out. The witnesses which lack the lines are those that belong to the **b** and **c** groups and, since we know that the textual affiliations of ω are not with this group, we can dismiss the idea that it lacks the lines. However, the α group --Ad1 En3 and Tc1-- also seems to lack the lines. We find, however, that the Hg scribe copied only half of line ME 61, and left the space for ME 62. The lines were later completed in Hg in a different hand. At this point of the text the rate of variation is very high: most early witnesses have improvised a solution, some of which have later been passed on in the copying process. An example of this are the variant lines in El and Gg, which show, once more, that there is more than a casual relationship between these manuscripts. In any case, if ω had had the lines and these had been clearly visible there would have been no reason for Caxton to leave them out --especially after he had added the lines that immediately follow the couplet. It would appear that ω might have had dubious readings at that point --as η probably had.

What is important in this research is that it has shown that it is possible that there is a genetic relationship, below the archetype, between manuscripts that had previously been unclassified or labelled as **O** manuscripts, that is, direct and independent descendants of the archetype. This last idea could be still sustained if we believed that the archetype could be equated with ω :

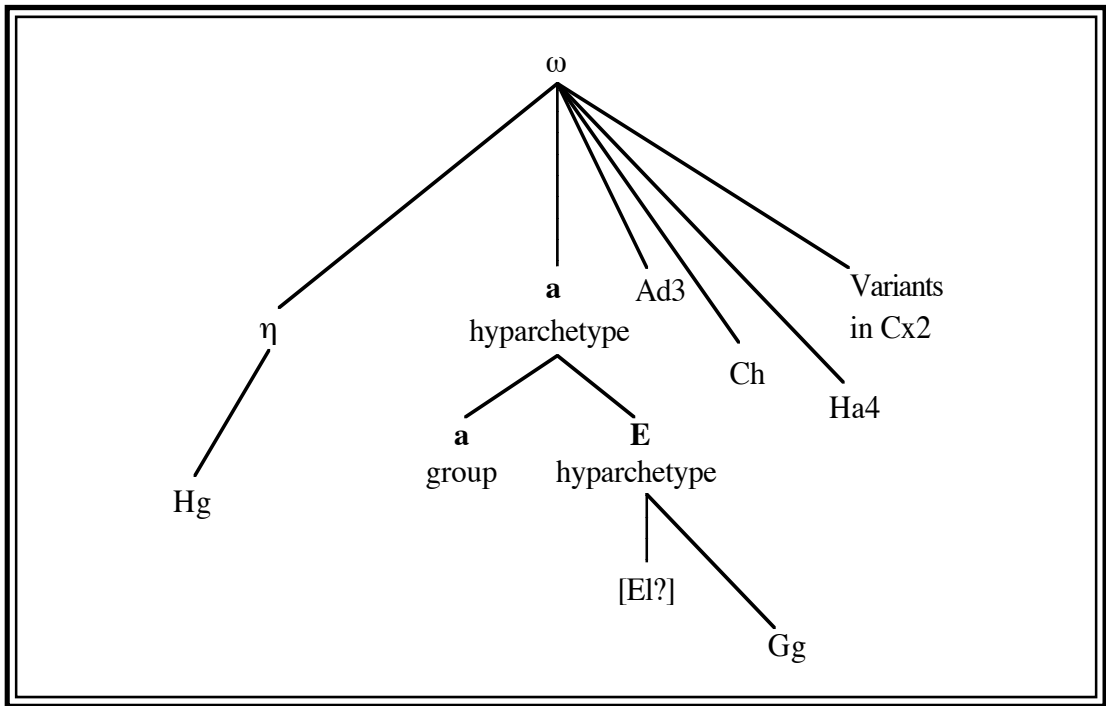


Figure 6

The difficulty with this is that all the evidence points towards the archetype --a working copy-- as being a pile of papers, some of which might have been bound and often came loose, while some sections were not fastened to anything else. It would be difficult to prove whether ω was a pile of papers, but it would be as difficult to prove the opposite. Some of the text found in Cx2, and which I interpret as coming directly from ω , has been the subject of controversy --the E1 passages in WBP or L31, the Nun's Priest's Endlink. It has been suggested, concerning the 'additional passages' in WBP, that these might have been marked for deletion in the archetype, or that they were added in the margins of the archetype. It seems conceivable, if we accept that parts of the text were marked for deletion in the archetype, to think that some parts were marked in a more obvious way than others. This could explain the case of L31 -- found only in eight witnesses-- which might have been clearly marked for deletion so that some of the scribes decided to leave it out, while, at the same time, these marks

might have been ignored by a single scribe --who produced a text that later originated, for example, the **a** hyparchetype.

5. THE POSITION OF ω IN THE TEXTUAL TRADITION AND ITS RELATIONSHIP TO OTHER MANUSCRIPTS

These three hypothesised stemmata are possible solutions --although, not the only possible solutions-- to the problem I set myself at the beginning of this research. One could also imagine that ω is a sister to Ad3 Ch and Ha4. If this had been the case, it would be much more difficult to explain why there are variants shared by ω Ad3 and Ha4, others shared by ω Ch and Ha4 and yet others shared by ω Ad3 and Ha4. In these groups, ω is the common element which suggests that this manuscript was probably higher than the others in the textual tradition. All of them show different interpretations of the same data. At present, I am more inclined towards the stemma in figure one. However, which one of these is the more accurate one is debatable, since the data can be understood in different ways. What seems much more important is that there are common elements in the stemmata. Even if the exact position of ω in the textual tradition of the *Canterbury Tales* cannot be pinpointed with exactitude, the fact still remains that it is likely that it was the origin of manuscripts which have remained unclassified up to now. The common elements concerning ω in all stemmata would then be represented as follows:

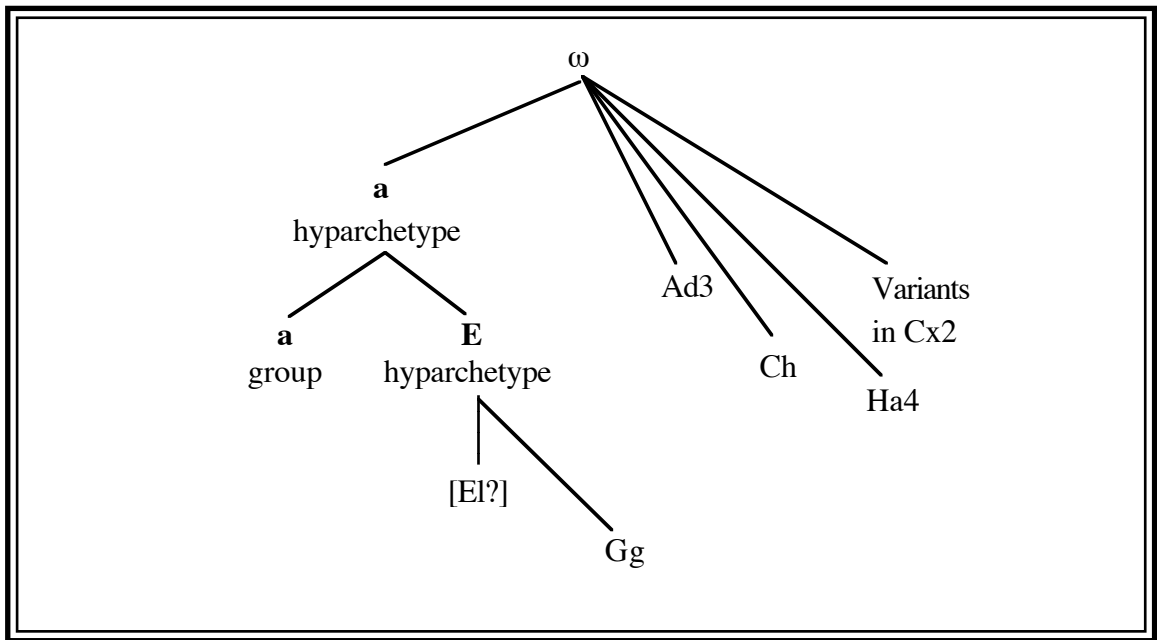


Figure 7

To summarise, since only around three thousand variants have been collated for this research, the stemmata I have proposed are not the only ones possible. For example, for GP Robinson has found that Hg Ch and Ha4 are very closely related, although he was taking into account the complete set of variants for this part of the text. For the same amount of text I have 150 variants only. Besides the matter of the number of variants, it is also possible that since the archetype of the tradition seems to have been unbound, there may have been some shifts of exemplar, if the part of the text the scribe was copying was unavailable for some reason. This would explain why in parts of the text some manuscripts may unexpectedly exhibit different affiliations which make them appear closer or farther from the archetype.

To identify correctly and without any doubts the position of any manuscript in the textual tradition of the *Canterbury Tales* is a very difficult task. This task becomes an intricate and perplexing experience when the manuscript one is trying to analyse

survives as a few variants in a printed edition probably modernised by its compositors. However, although laborious, the task is not impossible --as my research shows--, but it still presents the problems which I have explained above. If some determinant variants were to be found in those places where there is no preserved trace of the reading in ω , then, naturally, we should find that the results of this work are not completely accurate. Although it is very unlikely that ω will ever be found, only then would we know the exact position in which this manuscript should be placed in relationship to other witnesses.

6. UNANSWERED QUESTIONS AND FURTHER RESEARCH

As I have said, the results of this work are as close as I can get towards clarifying the nature of ω , and many other questions that need to be formulated and answered to gain a fuller comprehension about the textual tradition of the *Tales*. The main challenge arising from this research concerns the nature of the relationships between Ad3 Ch and Ha4 with each other. That is, although they seem to be grouped in reference to the variants which they share with ω , how are they related in the rest of the text? They have often been labelled as independent and one wonders if this judgement is correct. The Canterbury Tales Project provides tools which are ideal for a study of these relationships and this should surely be part of any future research.

Another interesting finding concerns some isolated variants in E1 which appears to be related to Robinson's **E** group. These should also be studied in detail. Comparisons between Hg and E1 have already been made, but only once --by Manly and Rickert-- with the benefit of the use of the text of all the other witnesses of the *Canterbury Tales*. Now, not only are we approaching the stage in which all the

transcriptions of the main witnesses of the text could be easily compared, but we are also nearer to having all the witnesses transcribed. The work that Manly and Rickert did in the twentieth century will soon be revised with the use of new and better tools than they ever dreamed. Computers have opened the doors to research that can now be taken to new levels, not because it can intrinsically be more accurate, but because it can be carried out over and over again, each time with better transcriptions.