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**Problems in the New Testament:**  
Old Manuscripts and Papyri, the New  
Genealogical Method (CBGM) and the Editio  
Critica Maior (ECM)

by  
Bengt Alexanderson



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Kungl. Vetenskaps- och Vitterhets-Samhället

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## ABSTRACT

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Chapter 1 discusses readings found in the four oldest documents of the Gospel of John: P66, P75, codex Sinaiticus and codex Vaticanus. The conclusion is that even these old documents have been rather heavily interfered with.

Chapter 2 deals with the way a prominent specialist looks upon early papyri containing text of the New Testament. The conclusion is that the texts of these papyri are far from being as firmly established as they are thought to be.

Chapter 3 is a fairly thorough discussion of the Coherence-Based Genealogical Method (CBGM) which has been fundamental to establishing the text of the Editio Critica Maior of the New Testament. The method is found to be of no or little value. This is serious, considering the fact that this very ambitious new edition will in all probability have an impact on future work on the New Testament and generally on editing classical and medieval texts.

Chapter 4 is a critical review of the Editio Critica Maior, trying to bring to the fore both strengths and weaknesses.

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## Introduction

How can we arrive at the best text of the New Testament? Asking that question and trying to do something about it is like enjoining upon oneself a tall order indeed, but at the same time we should not be ashamed of doing our part, however small, considering the fact that the text is the most important in the history of Western culture.

We all know that the tradition is extraordinarily rich and thus difficult, not to say impossible, to grasp. Anyhow, it is of course necessary to have an idea about the oldest witnesses. The oldest papyri are briefly treated in chapters 1 and 2 and the oldest manuscripts in chapter 1. Since one can prove almost anything by picking an example here and another there, I think that a fairly thorough study of a longer text like the Gospel of John (chapter 1) may give us a better idea of what is original and what is not. Concerning the papyri, I have been following in the footsteps of a most distinguished scholar whose views on these documents must be taken into account (chapter 2), since I think that they are essential for the *Editio Critica Maior* (ECM). I have concluded that the texts are more interrelated and the ground more slippery than she did.

Chapter 3 concerns the Coherence-Based Genealogical Method (CBGM), which underlies the second edition of the *Editio Critica Maior* (ECM), published by the Institute for New Testament Textual Research at Münster and presenting so far the Catholic Letters. For the same general reason as mentioned above it is worthy of a lengthy study that goes into much detail, but there are other reasons as well: One has already been mentioned, the simple fact that the New Testament is the most important text of Western culture; another that the method is highly ambitious and promises much; another that its impact on editing texts will in all probability be strong. The last consideration makes me think that there is *periculum in mora*. Generally speaking, I find the method of little or no value. One reason is that it is footed on a faulty use of statistics, giving the same weight to accidental changes as to deliberate ones, another that the ideas of how one reading develops into another are often highly improbable, a third that the method consistently undervalues interpolation.

Chapter 4 is a critical examination of the *Editio Critica Maior* (ECM), attempting to show both its strengths and its weaknesses. The strong points are the choice of witnesses to the Greek text among what looks like overwhelmingly many, the both thorough and clear presentation of what they offer and, most important, the good text it presents. It is in fact astonishing that using a poor method, the result is as good as it is, the reason being that ‘reasoned eclecticism’ is after all not abandoned.



Perhaps we should not speak about weaknesses but rather about what remains to be done. Concerning the Church Fathers and traditions other than the Greek one, there is much to do, but this means an enormous amount of work, and we cannot ask for this now or in a near future.

Generally speaking, I think that in the work that underlies the edition and which is essential for it, there is a strong wish to find stability: The oldest papyri are on the whole reliable; there is a reliable method. To me this is wishful thinking: The oldest witnesses are unreliable, the method is faulty. We are on a slippery ground indeed. Our knowledge of the Greek language is unsatisfactory; so is our knowledge of the life and traditions of early Christian congregations; we do not know much about copying in antiquity and in the Middle Ages. What we have is unsatisfactory knowledge and a highly fallible judgement. This is of course no reason for despondency. We should try to enhance our knowledge and make the best use of what we have.

## Chapter 1. The Gospel of John and the Venerable Four: P66, P75, S, B<sup>1</sup>

### The four documents

No doubt, the four witnesses P66, P75, S (codex Sinaiticus, 01) and B (codex Vaticanus, 03) are extremely important. The two papyri are the most ancient documents to have preserved a large part of a Gospel, and the two manuscripts are the only ones to be ascribed to the 4th century, except some manuscripts which do not comprise more than small fragments.

The text is well preserved in these ancient documents: almost completely in P66<sup>2</sup>, in P75 up to 15:8, which is where this study stops. I follow the apparatus of Nestle-Aland (NA), except when The Greek New Testament (GNT) gives more information.

### Some general reflexions

It is common knowledge that the tradition of the New Testament is very complicated, not to say inextricable. There is no hope that we could arrive at some kind of an original text of John. We must suppose that tales about the life, acts and words of Jesus were soon transmitted to writing in an environment where the written word had a long history. But we could imagine that a first outline had been drawn and elaborated step by step until arriving at a text well established and accepted in wide circles. If so, where is the original? And what if one text was accepted in Alexandria and another in Antiochia? It is an important idea of the *Editio Critica Maior*<sup>3</sup> that all preserved documents go back to an 'initial' text. I consider that a wise decision, but not uncomplicated.

Let us take 1:49  $\sigma\upsilon\ \epsilon\acute{\iota}\ \acute{\omicron}\ \upsilon\acute{\iota}\omicron\varsigma\ \tau\omicron\upsilon\ \theta\epsilon\omicron\upsilon$  P66c P75 S B  $\sigma\upsilon\ \epsilon\acute{\iota}\ \acute{\alpha}\lambda\eta\theta\acute{\omega}\varsigma\ \acute{\omicron}\ \upsilon\acute{\iota}\omicron\varsigma\ \tau\omicron\upsilon\ \theta\epsilon\omicron\upsilon$  P66\* as an example. Most of us would think that the author, whoever he was, wrote without  $\acute{\alpha}\lambda\eta\theta\acute{\omega}\varsigma$  but that someone, whoever he was, added that word to strengthen the profession of faith. Now all documents which do not have the addition may go back to a

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1 This chapter was published in an earlier version as *L'Évangile de S. Jean dans les « quatre grands » : une comparaison entre les leçons de Jean 1–15, 8 dans P66, P75, S et B*. <http://hdl.handle.net/2077/32601>. I have been much helped by Metzger. The task of Metzger was to explain why the Editorial Committee of the GNT had chosen a special variant, not to proclaim his own opinion. I quite often disagree with the decision of the Committee, but that is not a criticism of Metzger.

2 In quotations from chapters 1–14, P66 means the text taken from P66 (1956), sometimes commented on after a comparison with P66 (1962). The readings in 15:4 and 15:8 (p. 41) are taken from P66 (1962).

3 I use the 2nd revised edition, ECM2.

document X without it and all documents with the addition may go back to a document Y with it. But that does not mean that Y goes back to X; in fact both X and Y may go back to an earlier stage of the text, Z, which in this place is kept in X but changed in Y. The history of another reading may be different. I think that we have a tradition of readings which in many places can be made clear. The whole texts are interwoven to an extent that makes it difficult to speak about an original or initial *text*.

It all boils down to initial *readings*, and I do not think that we can get behind that. The initial, hereafter also called original reading, is the one that explains the other or the others. Whether it is also the reading of the author we do not know, but it is well known that there is not much need of conjectures for establishing the text of the New Testament; the tradition mostly offers a text which gives a good sense. When speaking about an original or initial reading, I am of course only expressing an opinion.

### **Problem**

The problem can be stated as follows: comparing these four very old witnesses, can we show that the text has already been changed intentionally, and if so, how and why? It boils down to the question: Which reading explains the others? I shall try to discuss all the passages which seem to say something about this problem, or which, although they have different readings, cannot tell us anything about intentional changes. This way may seem verbose and redundant, but by selecting passages here and there you can prove anything or almost anything. Of course my selection is personal, and I can only hope that it is not too arbitrary. I shall try to state which reading ought to be considered initial and able to explain the others; the next step is to attribute variations from that reading to some category of errors. Often no evaluation is possible and the passage will be left without a conclusion about what is initial or original.

### **Changes generally**

We find in the documents changes, omissions and additions.<sup>4</sup> They are made intentionally or unintentionally. It used to be a common opinion that the shorter text is more original and that additions are the sign of influences brought in later.<sup>5</sup> However, there are statistical studies<sup>6</sup> which

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4 In what follows, omission simply means that one reading is shorter than another, addition that it is longer, whereas change means some other kind of variation. Thus, an omission can be a more original reading where nothing is lost, an addition may mean a more initial text where nothing is added.

5 The old and dear *lectio brevior potior*, so often correct!

6 Cf. for instance Roysse, especially chap. 10. *The Shorter Reading?*

indicate that there are more omissions than additions in the Gospels, and that should teach us to be generally rather cautious about the *lectio brevior*, but as to the special case, we must judge it as independent of other cases. Statistics are here not much use. Generally speaking, I assume that an omission may occur unintentionally, whereas there is often, but not always, some thought behind an addition. In fact an addition of αἰώνιος after ζῶή may appear almost automatically, cf. below under *Unintentional changes*. To arrive at an original or initial reading, we must ask the question why a change, an omission or an addition was made. We must be aware that a distinction between intentional and unintentional is crucial but often impossible.

An opinion expressed by Hort has often been repeated, almost *ad nauseam*<sup>7</sup>: “Knowledge of documents should precede final judgement upon readings.” This is more often said than it is true. We want to arrive at an original text, and when there is a variation, there is only one way to choose: If possible, we must try to find how and why one or more among these readings have been elaborated or influenced and which reading explains the other or the others. The reading that explains is original. It may be found in an old and venerable document or in one that is generally of less importance. Add to ‘explains’ also ‘strikes’; what is striking does not appear by chance, it is original. The ‘knowledge of documents’ cannot have the last word. The ‘final judgement’ rests with the person who gives his opinion on the text. Let us hope that the person in question is well aware of the fact that our knowledge is insufficient and our judgement always frail and never sure.

### **Intentional variations**

Intentional variations are made for different reasons: The scribe or reader may be influenced by another passage of the same or another Gospel; he might have wished to improve the text, making it grammatically correct or easier to understand; he might have wished to adjust the text to a theological conception; he might have consulted other exemplars.

It is reasonable to think that adding a word or substituting one for another is always made intentionally, but I think it is quite possible that the scribe adds ἄγιον if he finds the word πνεῦμα, that he adds θεοῦ if he finds υἱός, that he adds or omits the article without thinking about it or that he changes the order of words. Some of these alterations could be caused by an opinion about correct theology, but since it is often hard to know whether they are intentional or unintentional, they must

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7 *The New Testament in the Original Greek* (1881–82), *Introduction*, p. 31.

often be disregarded.<sup>8</sup> An omission is intentional or unintentional, but we do not know which is which; an addition/insertion or a change is either intentional or unintentional, but is at least possible to explain. Trying to state whether a text has been subject to influences, explanations, ‘improvements’, we are more helped by additions and changes than by omissions. It is necessary to disregard everything which is or could be unintentional.

A correction in the original text made after another tradition can only be verified if the text is changed by the same hand, which is often the case in P66, or if there is a conflation of readings, as in 2:13, see below. It is certainly possible that a change is nothing but a correction of a mistake which the scribe has become aware of afterwards. Accordingly, I do not register corrections if they are not confirmed by another tradition found in the other witnesses which are dealt with here (which is more often the case), or in another strong tradition (rarely).

Of course many of the omissions are made by mistake, probably most of them. But there are also intentional omissions: A copyist or a reader may think that a word, maybe even a whole phrase, is superfluous and omits it on purpose. He may also find that a passage is contrary to his theological ideas and suppress it. If you are an adoptionist and think that Jesus is only man, υἱὸς ἀνθρώπου, you would like to suppress the words υἱὸς θεοῦ after Ἰησοῦ Χριστοῦ in Mark 1:10.<sup>9</sup>

The changes which I regard as intentional or explicable are as follows:

- influence from a passage nearby or from the context
- influence from a passage further away in the same Gospel
- influence from another Gospel
- text influenced by two traditions
- ‘theological’ change
- clarifying or supplementary insertion or change
- stylistic or grammatical ‘improvement’

It goes without saying that it is difficult, maybe impossible, to distinguish between several of the types, especially between influence from another Gospel and ‘theological’ change. See below about customary Christian language.

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8 I think that Ehrman in his important book sometimes pays too much attention to variations which may well be unintentional, see below p. 35 about 10:33.

9 Cf. Ehrman, p. 72–75.

### **‘Theological’ changes**

I am not at all sure that it is possible to distinguish between an influence from customary Christian language and a ‘theological’ change. By customary I mean a variation between for instance πνεῦμα and ἅγιον πνεῦμα, πατήρ and θεός, σωτήρ and Ἰησοῦς. Such a variation may occur everywhere. It has not much if anything to say about our problem and should not be taken into consideration. On the other hand, ‘theological’ change is certainly intentional. If an opinion of Hort’s is not totally false but rather makes us think, see above, another has been proved wrong<sup>10</sup>: “It will not be out of place to add here a distinct expression of our belief that even among the numerous unquestionably spurious readings of the New Testament there are no signs of deliberate falsification of the text for dogmatic purposes.” Ehrman<sup>11</sup> has made important studies which show that in the first Christian times, heresies, as they were called afterwards, have influenced the text of the New Testament. If, in some places, I raise objections to Ehrman, that is because another explanation is possible. Such an explanation could be founded on paleography, or it may be possible that for instance someone has added Christ to Jesus almost automatically; that would not mean that he wanted to confirm with the orthodox that Jesus is also the divine Christ, against heretics who had other ideas about the nature of the Son.

### **Unintentional changes**

I find it necessary that we pay no attention to all those variants which may be produced unintentionally:

Variations between synonyms or near-synonyms like θεός and κύριος, υἱός and παῖς. It is possible that such words are chosen for subtle theological reasons, but they may also be unintentional deviation from the original. We should, however, except cases in which there is a variant that lacks the word. In such a passage, the reading without that word is probably the ancient one.

Variations in the word order. They are very common, but attention should be paid to cases where words are added.<sup>12</sup> A scribe, writing down

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10 *The New Testament in the Original Greek* (1881–82), *Introduction*, p. 282.

11 Ehrman has predecessors who have treated more specialized problems, e.g. Epp. See also Roysse, p. 738, for “a few examples in connection with P72 of scribal change for (as it seems) theological reasons.”

12 Transposition of words is a very common variation in the material used by Roysse. Just an example from our texts: In Jn 21:1 ἐφανέρωσεν ἐαυτὸν πάλιν ὁ Ἰησοῦς, πάλιν is placed as the first, the second or the third of the first three words, also as the fifth word after ὁ Ἰησοῦς, or is omitted. In Basil the Great’s (Basile de Césarée) *Adversus Eunomium* transposition of words is extremely common in the manuscripts.

a text which he has established out of *scriptio continua* and repeated to himself, or which he had heard dictated, may have changed the word order without noticing it or without caring very much about the exact word order.

Nothing or almost nothing is more common than unintentional faults caused by homoeoteleuton.<sup>13</sup>

Variations with or without pronouns like αὐτῷ or αὐτόν where it is difficult to see any difference between readings with or without those words. If the words are placed in different positions or omitted in some documents, they should probably not be there.

Omission or addition of small words like particles and articles. Even negations sometimes fall out.<sup>14</sup>

Omission or addition of other words of no great importance. Examples are 1:21 and 1:35 where the witnesses read or do not read πάλιν. The variant without πάλιν may be older, πάλιν being introduced later because it is clear from the context that something happens again, in this case that someone speaks again. It is also possible that the copyist has skipped the word by sheer negligence or found it redundant. Another example is 1:26 where λέγων occurs or does not occur before a direct quotation, likewise 1:49 with or without καὶ εἶπεν. Without thinking about it, someone may have added σύ in 1:21, also in 1:22.

Also passages where only one of these four has a reading with no or little support in other traditions do not say very much, since such a reading may be a singular and unintentional fault. However, if such a variant is supported by the context, more attention should be paid to it. There is little interest in readings like κείμενοι in 2:6, missing in S but not in the other three, or ἐπέθηκεν in 9:6 which only in B replaces ἐπέχρισεν. More important is 3:34 δίδωσι without τὸ πνεῦμα in B\*, because the context gives a certain support to this variant.

There is a good explanation for these commonplace variations: One sees what one expects to see and one does not see what one does not expect to see. If there is no doubt that the subject is θεός or κύριος, the word may be omitted even if it is in the exemplar. The text may very well have an αὐτῷ or an αὐτόν which are not absolutely necessary, and the copyist does not observe it. On the other hand, if the scribe expects θεός or αὐτόν, he may very well put it into the text, even if it is not in the exemplar.

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13 Dain, p. 48, counts omissions caused by homoeoteleuton as one of the two most common errors, the other being the loss of small words. Many of the omissions noted by Roysse are caused by homoeoteleuta (leaps).

14 Cf. n. 13 for the loss of small words.

We have already, cf. above *Intentional variations*, mentioned the case where an addition may be made unintentionally, like ἅγιον added to πνεῦμα.

It is hardly possible to make a list of all the commonplace variations which may occur unintentionally. The list below does not present all the cases, and in what follows I shall take up and dismiss some passages of another type, if it is evident that they are of no importance to our problem. The list is certainly of a personal character and incomplete, but I hope it will have something to say about the method used in this study. It presents what is in my opinion the most frequent cases of unintentional variations, caused by human negligence. Concerning the passages to follow, I have tried to indicate if possible, but only if it is possible, what kind of change we have before us, but often a variant can be explained in more than one way. The list is as follows:

omission or addition of the definite article or of a particle  
omission or addition of some word of small importance<sup>15</sup>  
variation occasioned by customary Christian language of the type  
πατήρ/θεός, σωτήρ/Ἰησοῦς, πνεῦμα/ἅγιον πνεῦμα, υἱός/υἱός θεοῦ,  
ἀπεκρίθη/ἀπεκρίθη καὶ εἶπεν<sup>16</sup>  
variation of the type εἶπον/λέγουσιν, ζήσει/ζήσεται  
grammatical variation of the type παραδώσων/μέλλει παραδιδόναι,  
γέγραπται/γεγραμμένον ἐστίν  
omission or addition of ὅτι before direct discourse  
singular variant or fault in one of the four  
variants caused by homoeoteleuton  
diverging word order, especially if there are no other changes.

### **Is one witness better than another?**

If the evolution of variants can help us to explain a difficult passage, it is evident that the age of the witness says nothing in the particular case. We could at most say that if a witness is old, there are probably fewer links between it and the initial text, but that does not take us very far. Our four witnesses are the oldest existent, but they are far from infallible. In Hebr. 2:9, the reading χωρὶς θεοῦ instead of χάριτι θεοῦ is

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15 For instance omission or addition of αὐτῶν and other forms of this pronoun where not clearly necessary.

16 In my original article, there was an unfortunate distinction between a vague and general influence from Christian language and changes of a more 'automatic' type like πατήρ/θεός, πνεῦμα/ἅγιον πνεῦμα. I do not think now that such a distinction can be maintained.



original according to Ehrman<sup>17</sup>, and I for one agree. Of the manuscripts which present  $\chi\omega\rho\iota\varsigma$   $\theta\epsilon\omicron\upsilon$  none is earlier than the 10th or 11th century (0243, 424, 1739). In this case, it is important that Origen and Fathers of the Church quote the initial text, but what if there is no such witness? In fact, the list of Fathers who know the reading  $\chi\acute{\alpha}\rho\iota\tau\iota$   $\theta\epsilon\omicron\upsilon$  is very impressive; already Origen knew this less original variant, and with him Greek Fathers like Athanasius and Didymus the Blind.

Also the general quality of the text, it may be good, it may be bad, has nothing to say about the particular passage. The texts of our witnesses are generally good, there is nothing else to say.

The wide dispersion of a reading tells us that this reading is either initial or that it became very popular. The interesting thing is to understand why it was popular, that is to say: to explain.<sup>18</sup>

An old translation can start from an exemplar that contains many false readings, a more recent one from a much better.

All this does not mean that the study of the tradition of the text of the New Testament is of no interest. On the contrary, the development of this text and the reasons to be found for the alterations which occur in the course of time are certainly worthy of the strictest attention. The text has been changed unintentionally, that is by negligence of the scribes, but also by intentional interference from scribes or readers. In this latter case, parallels and influences from theological thinking are of special importance.

Very often, too often, the context gives us no clear answer, and in such cases we must accept that we do not know how to edit the text. Keeping to the oldest witnesses or to a reading which is well documented in Greek and in other traditions is practical and often absolutely necessary, but it is always a last resort.

### **Scribes and readers**

There are careful and less careful scribes. Probably most professional scribes just copied what they had before them. But what they had before them could be rather different from time to time. The text was important, and someone wanted it copied exactly as it was, whereas someone else was more inclined to make changes. He might have been concerned about passages in other Gospels which may be more to the point or regard the grammar as faulty and unworthy of its purpose or found that the

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17 P. 146 ff.

18 An example: In Lk 22:17–20 a solid majority of manuscripts have a long text which together with the bread speaks about wine or cup. Ehrman, pp. 197–209, and others have, in my opinion rightly, preferred the shorter text. The addition is easily explained.

text was not in accord with his idea about true Christian doctrine. This someone could be the scribe, but more probably a reader. It is also possible that the reader did not regard the text as very sacred. It goes without saying that to the first Christians, the sacred text was the Old Testament. It is clear that to Justin Martyr, the prophets are inspired by God, they are θεόπνευστοι, whereas the Gospels, mostly called ἀπομνημονεύματα, are more like reports, stating that the prophetic, inspired message has come true. Their purpose is practical and down to earth. I do not think that Justin ever says that they are inspired by God.

### **Conclusion**

We must disregard everything which is or may be unintentional. In order to establish whether a text has been subject to influences, clarifying or ‘improvements’, we start from changes, additions being more important than omissions. Since our knowledge of the evolution of the text is scarce, it is only for practical reasons that I speak about an original or initial text, which is the starting-point of the tradition we know and behind which we cannot get. This way of handling the text, which I am very far from calling a method, has the consequence that so-called inner criteria become more important, or even remain as the sole survivors on the battlefield. This means that we must rely on our modest knowledge and on our feeble judgement. Let us accept this position with humility.<sup>19</sup>

### **Passages of interest in the Gospel of John**

It is necessary not only to consider the passages which are illuminating, but also to explain, if possible, why others in fact say nothing, although they may at first sight seem to be of some importance. Please observe that in P66 the corrections (P66c) are made by the same hand that wrote the original text (P66\*). The corrections in the other witnesses are usually made by other hands and are left aside. If it is said that a text is influenced, that does not mean that the copyist himself has consulted another copy; it is more probable that he had before him a text which had already been subject to changes. I would also like to underline once more that we must try to find all the passages of some importance; it is not difficult to pick a few cases and draw conclusions from them. The inevitable consequence is that quite a number of passages are taken up

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<sup>19</sup> Cf. Metzger, p. 191: “the majority of the Committee was impressed by the age, range and diversity of evidence.” This means that the reading was highly valued, not that it was true.

just to be dismissed and that many passages are left without a conclusion about what is initial or original.

1:4 (1) (ἡ ζωῆ) ἦν P66 P75 B ἐστὶν S. The reading of S could be secondary; the present tense is a way to emphasize that the life is here now. We cannot say whether the first ἦν of the verse supports the second, because a certain irregularity would speak in favour of the present ἐστὶν. Also, the variant is quite commonplace and says nothing.

1:4 (2) (καὶ ἡ ζωῆ ἦν) τὸ φῶς τῶν ἀνθρώπων P66 P75 S τὸ φῶς B\*. It is very possible that B\* has the initial reading, but the omission could also be a singular error. The prologue of the Gospel is about the divinity of the Word and its position with God. The relationship between Word and man does not enter until v. 9, the Word coming into the world, ἐρχόμενον εἰς τὸν κόσμον. Christ as the light of the world (τοῦ κόσμου) or the light coming to the world is found several times in John, τὸ φῶς τῶν ἀνθρώπων never again. The longer text is perhaps influenced by φῶς τοῦ κόσμου of other passages in the Gospel, but that is far from certain.

1:13 οὐδὲ ἐκ (ἐκ om. S\*) θελήματος ἀνδρὸς P66 P75 S om. B\*. The omission is probably due to a homoeoteleuton (σαρκὸς/ἀνδρὸς) and is of no relevance.

1:15 οὗτος ἦν ὃν εἶπον (ὁ ὀπίσω μου ἐρχόμενος) P66 P75 οὗτος ἦν ὁ εἰπὼν B\* οὗτος ἦν S\*. The reading of B\* is both commonplace and absurd. The reading of S\* is in itself good, but could also be a singular error.

1:18 μονογενῆς θεὸς P66 S\* B ὁ μονογενῆς θεὸς P75. Another reading, ὁ μονογενῆς υἱὸς is well attested, especially in the versions and in the Fathers of the Church. Our four witnesses all have the same, or almost the same reading, omission or addition of an article being of no consequence. So the discussion is about θεὸς or υἱὸς. If υἱὸς is the good variant, our four witnesses have been influenced. Ehrman<sup>20</sup> is sure that υἱὸς is original and that θεὸς is an orthodox change, made in order to make Jesus possess the full divinity. I am not sure that he is right. It is important to him that μονογενῆς means ‘unique’, and Jesus cannot be the unique and only God, because there is of course also the Father. I think that the meaning of μονογενῆς θεὸς could well be what the words say, ‘only born

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<sup>20</sup> Ehrman, pp. 78–82, 265–266.

God’ or ‘only born as God’, there being no one in that position but the Son. We must dismiss the passage, since μονογενῆς θεὸς could very well be correct, and nothing has happened to our four witnesses.

1:19 (ὅτε ἀπέστειλαν) πρὸς αὐτὸν (οἱ Ἰουδαῖοι ἐξ Ἱεροσολύμων ἱερεῖς καὶ Λευίτας ἵνα ἐρωτήσωσιν αὐτόν) B πρὸς αὐτὸν om. P66\* P75 S. In P66, there is a sign after Λευίτας, but it is not possible to see what it means. It could be an addition, but if so, it is not preserved, because the margin does not exist any more. It is thus not at all impossible that a corrector has read πρὸς αὐτὸν after Λευίτας. The words can be an explicative addition, but they may also have been skipped as superfluous.

1:20 (καὶ ὠμολόγησεν καὶ οὐκ ἠρνήσατο,) καὶ ὠμολόγησεν (ὅτι ἐγὼ οὐκ εἰμὶ ὁ χριστός) P66 P75 B καὶ ὠμολόγησεν<sup>2</sup> om. S. The reading of S can be a singular fault, but in my opinion, the long variant is a clarifying insertion in the other witnesses and the initial reading is that of S. After ἠρνήσατο, someone has inserted καὶ ὠμολόγησεν, because there was a need to make the context clearer: A phrase like “he did not deny that he was not there” is a bit difficult: Was he there, or was he not?<sup>21</sup>

1:25 καὶ ἐρώτησεν αὐτόν P66 P75 B om. S. The passage does not say anything. The three witnesses were perhaps influenced by v. 19, but it is also possible that S has a singular fault.

1:27 ἄξιός S B ἱκανός P66 P75. The papyri are influenced by the parallel passages of the other Gospels, which all have ἱκανός.

1:33. I cannot see in the fac-simile that after correction, P75 has the addition καὶ πυρί after ἐν πνεύματι ἁγίῳ, as NA indicates. It seems that all our four documents have the same reading without καὶ πυρί.

1:34 (μεμαρτύρηκα ὅτι οὗτός ἐστι) ὁ υἱὸς τοῦ θεοῦ P66 P75 B ὁ ἐκλεκτός τοῦ θεοῦ S\*. Nothing is clear. There are quite evident additions in some versions: ὁ ἐκλεκτός υἱὸς τοῦ θεοῦ in Latin and Syriac translations, ὁ μονογενῆς υἱὸς τοῦ θεοῦ, after v. 14 and v. 18, in the Coptic version. Ehrman<sup>22</sup> thinks that ἐκλεκτός is the initial reading, altered by someone who found the word too adoptionist. Cf. p. 52 on the same passage.

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21 Cf. Galen, *De captionibus* IV πέμπτη (sc. ἀμφιβολία) δὲ παρὰ τὸν πλεονασμόν, ὥσπερ ἡ τοιαύτη ‘ἀπηγόρευσεν αὐτῷ μὴ πλεῖν’· τὸ γὰρ ‘μὴ’ προσκειμένον ἀμφίδοξον ποιεῖ τὸ πᾶν, εἴτε τὸ πλεῖν ἀπηγόρευσεν εἴτε τὸ μὴ πλεῖν.

22 P. 69 f.

1:36 ἴδε ὁ ἄμνός τοῦ θεοῦ P66c P75 S B ἴδε ὁ ἄμνός τοῦ θεοῦ ὁ αἴρων τὴν ἁμαρτίαν τοῦ κόσμου P66\*. In P66 there is an influence from v. 29, also attested in other traditions.

1:39 ἔρχεσθε καὶ ὄψεσθε P66 P75 B ἔρχεσθε καὶ ἴδετε S. S probably has a grammatical or stylistic correction after the imperative ἔρχεσθε.

1:49 (1) ἀπεκρίθη αὐτῷ Ναταναήλ P66 P75 B ἀπεκρίθη αὐτῷ Ναταναήλ καὶ εἶπεν S. S seems to be influenced by customary Christian language.

1:49 (2) σὺ εἶ ὁ υἱὸς τοῦ θεοῦ P66c P75 S B σὺ εἶ ἀληθῶς ὁ υἱὸς τοῦ θεοῦ P66\*. ἀληθῶς is cancelled by the scribe of P66, who evidently had access to two traditions. The word is found also in 1241 (12th c.). It is of some interest that the same reading is found in two witnesses, the distance between them being towards 1,000 years. Ehrman<sup>23</sup> thinks that ἀληθῶς is an addition made in order to emphasize that Jesus is really the Christ, the son of God, against the so-called ‘separatists’, who stressed the difference between Jesus man and Jesus God. That could very well be true, but it is also possible that ἀληθῶς is just a clarifying addition.

2:3 (1) ὑστερήσαντος οἴνου P66 P75 B οἶνον οὐκ εἶχον ὅτι συνετελέσθη ὁ οἶνος τοῦ γάμου· εἶτα S\*. A supplementary reading is introduced into the text of S\*.

2:3 (2) οἶνον οὐκ ἔχουσιν P66 P75 B οἶνος οὐκ ἔστιν S\*. A passage of no interest.

2:11 Ταύτην ἐποίησεν ἀρχὴν (τῶν σημείων) P66c P75<sup>vid</sup> B ταύτην πρώτην ἀρχὴν ἐποίησεν P66\*, ταύτην ἀρχὴν ἐποίησεν, add. πρώτην post Γαλιλαίας (in eo quod sequitur) S\*. πρώτην is evidently a clarifying or supplementary addition, and it is clear that both P66 and S have been influenced by two traditions; P66 has cancelled the word, S\* has placed it in the wrong position. There is some interest in the fact that 1241 has a reading without πρώτην, that is to say one which is probably more original than that of P66, cf. above the remark on 1:49 (2).

2:13 (1) Καὶ ἐγγὺς ἦν τὸ πάσχα P75 B καὶ ἐγγὺς δὲ ἦν τὸ πάσχα P66\* ἐγγὺς δὲ ἦν τὸ πάσχα S. P66\*, adding one particle to another, may have conflated two traditions, but the other witnesses may have dropped one of them.

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23 P. 160.

2:13 (2) (ἀνέβη) εἰς Ἱεροσόλυμα ὁ Ἰησοῦς S B ὁ Ἰησοῦς εἰς Ἱεροσόλυμα P66 P75. A third tradition, that of the family *f*<sup>13</sup>, omits ὁ Ἰησοῦς. In such cases, it is very probable that the subject has been added afterwards. However, such cases say little or nothing.

2:15 (ποιήσας) φραγέλλιον S B ὡς φραγέλλιον P66 P75. The papyri have a commonplace, you may say pedantic, addition: the tool is not a real whip, it is something like a whip.

3:5 (1) εἰσελθεῖν εἰς (τὴν βασιλείαν τοῦ θεοῦ) P66 P75 B ἰδεῖν S\*. S is influenced by v. 3.

3:5 (2) (οὐ δύναται εἰσελθεῖν εἰς τὴν βασιλείαν) τοῦ θεοῦ P66 P75 B τῶν οὐρανῶν S\*. Matthew is the only evangelist to write βασιλεία τῶν οὐρανῶν, whereas βασιλεία τοῦ θεοῦ is well represented in all the Gospels and in Paul. Shortly before, v. 3, there is τὴν βασιλείαν τοῦ θεοῦ. τῶν οὐρανῶν could be a *variatio sermonis*, but that does not seem very necessary after two verses. We probably have an influence from another Gospel.

3:8 ἐκ τοῦ πνεύματος P66 P75 B ἐκ τοῦ ὕδατος καὶ ἐκ τοῦ πνεύματος S. S is influenced by v. 5.

3:19 ἠγάπησαν οἱ ἄνθρωποι μᾶλλον τὸ σκότος (ἢ τὸ φῶς) P75 B ἠγάπησαν μᾶλλον οἱ ἄνθρωποι τὸ σκότος P66 οἱ ἄνθρωποι ἠγάπησαν τὸ σκότος μᾶλλον S. If we disregard the words which are ‘mobile’, only ἠγάπησαν τὸ σκότος is left, and a shorter reading without them could well be initial. There is need of a subject, and οἱ ἄνθρωποι is supplied, because the context speaks only about κόσμος; before the comparison ἢ τὸ φῶς, one would like to have μᾶλλον, although a comparison could very well be made without that word if the verb expresses a wish, like ἠγάπησαν. But who dares propose a reading which has no support in the documents? The initial reading remains uncertain.

3:20 (ἵνα μὴ ἐλεγχθῆ) τὰ ἔργα αὐτοῦ S B τὰ ἔργα αὐτοῦ ὅτι πονηρὰ ἐστὶν P66 αὐτοῦ τὰ ἔργα P75. P66 is influenced by v. 19 ἦν γὰρ αὐτῶν πονηρὰ τὰ ἔργα. Even in other passages, πονηρὰ is often added to ἔργα, but here the variant of P66 probably derives from the nearby verse.

3:31 (ὁ ἐκ τοῦ οὐρανοῦ) ἐρχόμενος ἐπάνω πάντων ἐστὶν P66 B ἐρχόμενος (without ἐπάνω πάντων ἐστὶν) P75 S\*. ἐπάνω πάντων ἐστὶν is probably

an insertion that comes from the beginning of the verse where the words are found. We could say that the longer text alludes to the beginning of the verse and want to keep them, but in fact the extra words rather confuse the context. Without them, there is a point; we attach ἐκ τοῦ οὐρανοῦ directly to what follows and thus there is a parallel between the one who is from the earth and speaks earthly things, and the one who comes from heaven and bears witness to what he has seen and heard. Thus, P66 and B have been influenced by a nearby passage.

3:34 (οὐ γὰρ ἐκ μέτρου) δίδωσιν τὸ πνεῦμα P66 P75 S δίδωσι (without τὸ πνεῦμα) B\*. No doubt the reading of B\* could be a singular fault, but since it is well suited to the context, it seems acceptable to me. It is not a question about τὸ πνεῦμα but of the one who comes from heaven, to whom the Father does not give partially (ἐκ μέτρου) but everything (v. 35: πάντα). The addition of τὸ πνεῦμα is a case of customary Christian language, maybe with a shade of theological thinking.

4:9 οὐ γὰρ συγχρῶνται Ἰουδαῖοι Σαμαρίταις P66 P75 B om. S\*. The addition looks like a clarifying insertion. Why should anyone have omitted this phrase, which is not without interest? Metzger is of the opinion that such explanations are characteristic of the Gospel of John, but I am not sure that he is right. We find in John simple explanations of names like Siloam, Ephraim, Golgotha, but the only explanation concerning religion is, as far as I have seen, 2:6, where John explains why there were water jars at the wedding at Cana.

4:20 (ἐν Ἱεροσολύμοις ἐστὶν) ὁ τόπος ὄπου (προσκυνεῖν δεῖ) P66 P75 B ὄπου S. This may be a question of stylistic ‘improvement’, but if so, we do not know in what direction it goes. Is ὁ τόπος added to explain, or is it omitted as superfluous?

4:24 ἐν πνεύματι καὶ ἀληθείᾳ (δεῖ προσκυνεῖν) P66 P75 B ἐν πνεύματι ἀληθείας S\*. This is no doubt an accommodation in S\* to other passages in John<sup>24</sup> presenting τὸ πνεῦμα τῆς ἀληθείας.

4:25 οἶδα P66\* P75 S\* B οἶδαμεν P66c. It is clear that P66 agrees with another well attested tradition, but endings do not say much.

4:37. Due to homoeoteleuton a whole verse has disappeared in P75.

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24 14:17 ; 15:26 ; 16:13. Also 1 Jn 4:6.

4:42 (οὐκέτι διὰ) τὴν σὴν λαλιάν (πιστεύομεν) P66 τὴν λαλιάν σου P75 B τὴν σὴν μαρτυρίαν S\*. Jn 8:43 shows that *λαλιά* is not pejorative, because the word has the same meaning as *λόγος* soon after. I think that S\* wanted to reinforce the value of what the ignorant woman said, qualifying it as ‘testimony’. Why should a ‘testimony’ be made less important, being changed into mere ‘words’? S\* has a clarifying change, maybe with a tint of theology.

4:51 (ὑπήντησαν αὐτῶ) λέγοντες (ὅτι) P75 B καὶ ἠγγειλαν S καὶ ἀπήγγειλαν λέγοντες P66. It is difficult to judge between P75 and B on the one hand and S on the other. καὶ ἠγγειλαν/ἀπήγγειλαν can be an ‘improvement’ on a commonplace and trivial word, but λέγοντες could be an intentional short cut. However that may be, the reading of P66 is elaborated and shows influences from two traditions.

5:2 (Ἔστιν δὲ ἐν τοῖς Ἱεροσολύμοις) ἐπὶ τῇ προβατικῇ κολυμβήθρα ἢ ἐπιλεγομένη (Ἐβραϊστὶ Βηθζατά) P66c P75 B ἐπὶ τῇ προβατικῇ κολυμβήθρα ἣ ἐστὶν λεγομένη P66\* προβατικὴ κολυμβήθρα τὸ λεγόμενον S\*. The text of S\* does not look correct; it is only just understandable. It is no doubt better to read with the other witnesses: “near the Gate of the Sheep, there is a pond called B.”<sup>25</sup> The reading of S\* stands by itself and says nothing about relationship between that document and the other three. It may be a singular fault.

5:6 πολὺν ἤδη χρόνον ἔχει B πολὺν ἔχει χρόνον P66\* πολὺν ἔχει ἤδη χρόνον P66c<sup>26</sup> πολὺν χρόνον ἤδη ἔχει P75 πολὺν χρόνον ἔχει S. When there are readings with and without a certain word, one should suspect that initially it was not there, especially if it is placed differently as here ἤδη. Thus, P66\* and S probably have the original readings, P75 and B a stylistic ‘improvement’ and P66c is influenced by two traditions.

5:15 ἀνήγγειλεν P66 P75 B εἶπεν S. There is also a tradition which reads ἀπήγγειλεν. Does S present an unintentional simplification? That is possible, which means that the passage does not say anything.

5:19 Ἀπεκρίνατο οὖν ὁ Ἰησοῦς καὶ ἔλεγεν αὐτοῖς P66 Ἀπεκρίνατο οὖν καὶ ἔλεγεν αὐτοῖς P75 B ἔλεγεν οὖν αὐτοῖς ὁ Ἰησοῦς S\*. The variant with ὁ Ἰησοῦς is very suspect, but it is an addition, or an omission,

25 We must understand *πύλη* with *προβατικὴ*, which does not seem at all impossible.

26 The reading of P66c is not πολὺν ἤδη ἔχει χρόνον (so NA), but πολὺν ἔχει ἤδη χρόνον.



which could be made anywhere, and that also goes for the variation ἀπεκρίνατο with ἔλεγεν and only ἔλεγεν. Thus, the passage does not indicate anything.

5:25 ἔρχεται ὥρα (ὥρα om. P75)<sup>27</sup> καὶ νῦν ἐστὶν P66 P75 B ἔρχεται ὥρα (without καὶ νῦν ἐστὶν) S\*. It is possible that S\* has preserved a more original reading and that the other witnesses are influenced by 4:23.

5:32 οἶδα P66 P75 B οἶδατε S\*. We could with Metzger consider the plural number a change in order to signal that the Jews already knew the divinity of Jesus. However, Jesus himself speaks against this in v. 37, stating that the Jews had never heard about nor seen the Father. It could be a ‘theological’ change, but we should not believe too much in endings.

5:44 (τὴν δόξαν τὴν παρὰ) τοῦ μόνου θεοῦ (οὐ ζητεῖτε) S τοῦ μόνου (without θεοῦ) P66 P75 B. The reading without θεοῦ is abrupt, but it may very well be initial. Jesus says that the others look for honour from one another (δόξαν παρὰ ἀλλήλων λαμβάνοντες), but there is only one where you can find honour. θεοῦ could be a ‘theological’ change, but it is also possible that the word has fallen out because of a homoeoteleuton.

5:45 κατηγορῶν ὑμῶν Μωϋσῆς P66 P75 S κατηγορῶν ὑμῶν πρὸς τὸν πατέρα Μωϋσῆς B. B is influenced by πρὸς τὸν πατέρα before in the same verse.

6:1 τῆς θαλάσσης τῆς Γαλιλαίας τῆς Τιβεριάδος P66c P75<sup>vid</sup> S B τῆς θαλάσσης τῆς Γαλιλαίας P66\*. There are several examples of τῆς θαλάσσης τῆς Γαλιλαίας in Matthew and Mark, only one of τῆς θαλάσσης τῆς Τιβεριάδος, in Jn 21:1. Since the wording of P66c P75<sup>vid</sup> S B is very clumsy, it seems more probable that it is a conflated reading than that it is initial. We should read either τῆς Γαλιλαίας or τῆς Τιβεριάδος, but which? Did someone read τῆς Γαλιλαίας in his exemplar but remembered that John elsewhere says τῆς Τιβεριάδος and added it? Or did he read τῆς Τιβεριάδος but added the better known reading, being influenced by another Gospel? This is, I think, the better explanation. Be that as it may, all the four are influenced, either by two traditions or by another Gospel. Metzger thinks that the original could be an awkward phrase with the two names of the lake; afterwards, the text has

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27 NA does not indicate this reading.

been ‘improved’, deprived of one or the other synonym. He could be right, but is such a clumsy phrase really typical of John?

6:5 πολὺς ὄχλος P66c P75 B ὄχλος πολὺς P66\* S. P66 originally has the order of S but changed to the order of P75 B. We do not know whether the copyist corrected an error he had committed or if he was influenced by another tradition.

6:15 (1) ἀρπάζειν αὐτὸν ἵνα ποιήσωσιν βασιλέα P75 B ἀρπάζειν αὐτὸν καὶ ἀναδεικνύουσι βασιλέα S\* lacuna P66. ἀναδεικνύουσι is perfectly normal in this context and is a more specialized word than ποιεῖν. A reader or a scribe careful about style may have changed ποιεῖν into ἀναδεικνύουσι, or did the commonplace word replace the more ‘elegant’? We do not know. Cf. below 6:17 (2); 6:25; 12:30.

6:15 (2) ἀνεχώρησεν (πάλιν εἰς τὸ ὄρος) P75 B φεύγει S\* lacuna P66. In Mt 24:16 and Lk 21:21, Jesus predicts that Judaea will be destroyed and that people will flee to the mountains: φευγέτωσαν εἰς τὰ ὄρη. That is another situation, but it is possible that S\* has been influenced by that passage.<sup>28</sup>

6:17 (1) καὶ σκοτία ἤδη ἐγγόνει P75 B κατέλαβεν δὲ αὐτοὺς ἡ σκοτία S lacuna P66. S has been influenced by 12:35 ἵνα μὴ σκοτία ὑμᾶς καταλάβῃ.

6:17 (2) οὐπω ἐληλύθει πρὸς αὐτοὺς ὁ Ἰησοῦς is the text of NA, supported by among others L (8th c.) and W (4/5th c.). Our four documents have: οὐπω πρὸς αὐτοὺς ἐγγόνει ὁ Ἰησοῦς P75 οὐπω ἐληλύθει Ἰησοῦς πρὸς αὐτοὺς S οὐπω πρὸς αὐτοὺς ἐληλύθει ὁ Ἰησοῦς B lacuna P66. (ὁ) Ἰησοῦς is suspect, since it has no definite place and could have been introduced and placed here or there, but we must leave it; much more suspect are cases where a word is absent in some witnesses and placed differently in others. Should we accept ἐγγόνει of P75 against ἐληλύθει of the others? γίνεσθαι with a preposition is perfectly normal, at least in later Greek, indicating that someone arrives or has arrived somewhere. In the New Testament, such phrases are found especially in the Acts, but also for instance in 2 Jn 12 ἐλπίζω γενέσθαι πρὸς ὑμᾶς, but there is the variant ἐλθεῖν. Cf. also v. 21 εὐθέως ἐγένετο τὸ πλοῖον ἐπὶ τῆς γῆς (v. 1.

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<sup>28</sup> Metzger, p. 181, thinks that φεύγει is original but rejected as not appropriate to Jesus and replaced by ἀνεχώρησεν.

τὴν γῆν) and v. 25 πότε ὧδε γέγονας; ἐληλύθει is more ‘classic’, which could speak for ἐγεγόνει as initial, replaced by a more ‘refined’ word. But we could also regard ἐγεγόνει as an unintentional choice of a word familiar to the copyist.<sup>29</sup> Cf. remarks on 6:15 (1) ; 6:25 ; 12:30.

6:22 εἰ μὴ ἐν (sc. πλοιάριον) P75 B εἰ μὴ ἐν ἐκεῖνο εἰς ὃ ἐνέβησαν οἱ μαθηταὶ τοῦ Ἰησοῦ S\* lacuna P66. S\* is influenced by the context, particularly by v. 16 f.

6:23 ἄλλα ἤλθεν πλοῖα P75 B ἐπελθόντων οὖν τῶν πλοίων S lacuna P66. S has ‘improved’ the style. Cf. v. 24.

6:24 ὅτε οὖν εἶδεν ὁ ὄχλος ὅτι Ἰησοῦς οὐκ ἔστιν ἐκεῖ P75 B καὶ ἰδόντες ὅτι οὐκ ἦν ἐκεῖ ὁ Ἰησοῦς S\* lacuna P66. As at v. 23, S has ‘improved’ the style.

6:25 πότε ὧδε γέγονας; P75 B πότε ὧδε ἤλθεις; S lacuna P66. Cf. the observations on 6:15 (1) ; 6:17 (2) ; 12:30.

6:27 τὴν βρῶσιν (τὴν μένουσαν) P75 B om. S lacuna P66. The words are not necessary after τὴν βρῶσιν almost immediately before. It is difficult to say which reading is original.

6:36 (ἐωράκατέ) με P66 P75<sup>vid</sup> B om. S. The text without με refers to the signs which Christ had done (cf. v. 26). The addition can be a simple *lectio faciliior*, but a small word like με often falls out.

6:39 τοῦτο δέ (δέ om. P75<sup>30</sup>) ἐστὶν τὸ θέλημα τοῦ πέμψαντός με P66 P75 B om. S\*. The phrase is not necessary, but could have fallen out because of a homoeoteleuton.

6:40 τοῦτο γάρ (γάρ om. P75<sup>31</sup>) ἐστὶν τὸ θέλημα P66c P75 S B om. P66\*. P66 omitted the phrase but added it in the upper margin. Probably the copyist has corrected his own mistake, since these words must stand if there is to be any sense in the passage.

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29 γίνεσθαι in this sense is not low style. There are many examples in Justin Martyr, Clement of Alexandria, Origen and also in Didymus the Blind, who is, however, not much of a stylist.

30 This omission is not indicated in NA.

31 This omission is not indicated in NA.

6:42 (1) καὶ τὴν (τὴν in marg. P66) μητέρα P66 P75 B om. S\*. In Mt 13:55, one knows the father, whose name is not given, but whose profession (τέκτων) is indicated, and the mother Mary. The words may be an insertion, but they could also have fallen out through homoeoteleuton. Ehrman<sup>32</sup> considers the omission intentional: the point is to underline the difference between the crowd who thought that they knew the father, and Jesus, indicating who his real Father is. But if someone wants to stress the mistake of the crowd, who thought that Jesus was just an ordinary man, why not let people say that they knew both father and mother? That would make him the more human.

6:42 (2) νῦν P75 B οὖν P66 S. νῦν is more striking, since it indicates an antithesis: once he was one of us, now he says that he comes from heaven. However, in P66 and S it could just be a simple error by a scribe.

6, 46 οὐχ ὅτι τὸν πατέρα ἐώρακέν τις εἰ μὴ ὁ ὢν παρὰ τοῦ (τοῦ om. B) θεοῦ, οὗτος ἐώρακεν τὸν πατέρα. This is the text of P66 and B. P75 says, like P66, τοῦ θεοῦ but is lacunose and does not show the second πατέρα (or θεόν, see below). S reads τοῦ πατρός instead of τοῦ θεοῦ and τὸν θεόν instead of τὸν πατέρα. The reading of S could be influenced by 8:38 ἃ ἐγὼ ἐώρακα παρὰ τῷ πατρὶ λαλῶ. It is impossible to say which of the readings is initial and if there is an influence from another passage.

6:51 (ὁ ἄρτος δὲ (δὲ om. S) ὃν ἐγὼ δώσω) ἢ σὰρξ μου ἐστὶν ὑπὲρ τῆς τοῦ κόσμου ζωῆς P66 P75 B ὑπὲρ τῆς τοῦ κόσμου ζωῆς ἢ σὰρξ μου ἐστὶν S. It is not easy to judge, but the reading of S is as good as the other, perhaps original. The whole context places the bread in the centre: I am the bread of life, this bread comes down from heaven, I am the living bread; what follows, the bread given for the life of the world, may be more in line with what precedes. However, the wording of S ὃν ἐγὼ δώσω ὑπὲρ τῆς τοῦ κόσμου ζωῆς may be considered a *lectio faciliior*, bringing together δώσω ὑπὲρ τῆς τοῦ κόσμου ζωῆς, or one of these readings may just have occurred out of negligence. Nothing can be said with any certainty.

6:58 (ὁ ἐξ οὐρανοῦ) καταβάς P75 B καταβαίνων P66\* S\* καταβάς P66c. The variation is very trivial but indicates that P66 has been influenced by two traditions.

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32 P. 57.

6:64 (1) οὐ πιστεύουσιν P66c S B μὴ πιστεύουσιν P66\*<sup>33</sup>. P75 has οὐ, but the ending of the verb cannot be seen. μὴ de P66\* seems to derive from μὴ πιστεύοντες shortly after. The variant is singular and has not influenced the later tradition.

6:64 (2) τίνες εἰσὶν οἱ μὴ πιστεύοντες καὶ P66c B τίνες εἰσὶν οἱ πιστεύοντες καὶ S om. P66\*. P75 is lacunose: it is clear that the text is there, but it is impossible to see if it had the negation or not. The scribe of P66 originally omits the text but adds it, with μὴ, in the upper margin. The omission can be a singular fault committed by the scribe and corrected by himself. The words are not necessary and may have been added after εἰσὶν ἐξ ὑμῶν τινες οἱ οὐ πιστεύουσιν shortly before. They stand, however, in our four witnesses, and we must leave them there. It is almost impossible to say whether the negation should be accepted or not. With the negation, Jesus takes up the question from before, without it, there is a contrast to ὁ παραδώσων later in the same verse.

6:69 ὁ ἅγιος τοῦ θεοῦ P75 S B ὁ χριστὸς ὁ ἅγιος τοῦ θεοῦ P66. Royle<sup>34</sup> thinks that P66 may be influenced by Mt 16:16 and Jn 11:27. That is not convincing, since we read ὁ χριστὸς ὁ υἱὸς τοῦ θεοῦ there. It is probably an influence from many passages which have ὁ χριστὸς in a customary Christian way of speaking.

6:71 Ἰσκαριώτου P66 P75 B ἀπὸ Καρυώτου S\*. The reading of S\* comes from another tradition. Codex Bezae (05) has ἀπὸ Καρυώτου in 12:4; 13:2; 14:22.

7:37 ἐάν τις διψᾷ ἐρχέσθω πρὸς με (ἐμὲ) καὶ πινέτω P66c P75 B πρὸς με (ἐμὲ) om. P66\* S\*. Maybe there is an addition in P75 and B, but it could also be an unintentional omission in P66\* S\*. Afterwards, P66 has corrected this omission, consulting his exemplar or an exemplar from another tradition.

7:39 (οὐπω γὰρ ἦν) πνεῦμα P66c P75 S πνεῦμα ἅγιον P66\* πνεῦμα ἅγιον δεδομένον B. Clarifying additions in P66\* and B. The additions may derive from Jn 20:22 or from Acts 5:32, but they are rather an influence from customary Christian language. Two traditions meet in P66.

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33 NA reads μὴ πιστεύουσιν in P66\*. In the fac-simile, it is clear that in P66c the second sigma of the verb has been erased and the original μὴ changed into οὐ.

34 P. 509.

7:40 Ἐκ τοῦ ὄχλου οὖν ἀκούσαντες P66c P75 S\* B πολλοὶ ἐκ τοῦ ὄχλου οἱ ἀκούσαντες P66\*. A clarifying insertion in P66\*, and an influence from another tradition in P66c.

7:41 (ἄλλοι ἔλεγον ...) οἱ δὲ (ἔλεγον) P66c P75 B ἄλλοι P66\* S. P66\* and S are influenced by the preceding ἄλλοι. Traditions meet in P66.

7:46 οὐδέποτε ἐλάλησεν οὕτως ἄνθρωπος P66c P75 B οὐδέποτε οὕτως ἄνθρωπος ἐλάλησεν ὡς οὗτος λαλεῖ ὁ ἄνθρωπος P66\* S\*. We cannot explain the shorter reading as caused by a homoeoteleuton. Rather P66\* and S\* have an inserted explanation which is somewhat verbose and says the same thing as the shorter version. Ehrman<sup>35</sup> thinks that the addition is antidocetic, emphasizing that Jesus is really a man. I think this is reading too much into the wording.

7:52 ἐκ τῆς Γαλιλαίας προφήτης οὐκ ἐγείρεται P75<sup>vid</sup> B προφήτης ἐκ τῆς Γαλιλαίας οὐκ ἐγείρεται P66c S. The interesting reading is that of P66\*: ἐκ τῆς Γαλιλαίας ὁ προφήτης οὐκ ἐγείρεται. Afterwards, the copyist has changed the order of words and omitted the article.<sup>36</sup> P75 presents the same order of words as B, but we do not know if the article ὁ is there or not. The text of P66\* alludes to the prophet who according to the Jews ought to appear towards the last days. There has been much discussion around the text with or without the article<sup>37</sup>, but so much is clear that the text with ὁ προφήτης is much more striking and connects this passage with Jn 1:21; 6:14; 7:40. It ought to be preferred, because what is striking does not appear by chance, even if we should concede that a small word like the article could be put into the text or omitted without much ado. The reading without the article is probably just a trivial fault.

8:16 ὁ πέμψας με πατήρ P66 P75 B ὁ πέμψας με S\*. πατήρ is probably an addition taken from v. 18. Jesus has not so far pronounced the word ‘father’, and it was thought necessary to add it.

8:25 τὴν ἀρχὴν (ὅ τι ὅτι καὶ λαλῶ ὑμῖν). P66\* P75 S B εἶπον ὑμῖν τὴν ἀρχὴν P66c. The interesting thing is that the scribe of P66 or a reader has introduced a text which is found nowhere else and probably is a

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35 P. 237 f.

36 These changes are clearly shown in the fac-simile.

37 Cf. e.g. Thyen, pp. 416–418. For a new translation of the Bible into Swedish, Riesenfeld argued for the reading with the article in *Nyöversättning av Nya testamentet*, p. 234, but unfortunately this interpretation was abandoned in the final new version.

conjecture. This text of P66c gives quite another meaning to this difficult passage.

8:27 (οὐκ ἔγνωσαν ὅτι τὸν πατέρα αὐτοῖς) ἔλεγεν P66 P75 B ἔλεγεν τὸν θεόν S\*. In this context, Jesus does not mention God, but “the one who has sent me” and “his father”. A clarifying addition could be felt necessary, and S\* supplies it.

8:35 ὁ (δὲ add. P66) υἱὸς μένει εἰς τὸν αἰῶνα P66 P75 B om. S. The omission is of no importance, since it could very well be caused by a homoeoteleuton.

8:38 ἃ ἠκούσατε παρὰ τοῦ πατρὸς ποιεῖτε B ἃ ἠκούσατε παρὰ τοῦ πατρὸς λαλεῖτε P75 ἃ ἐωράκατε παρὰ τοῦ πατρὸς ποιεῖτε P66 ἃ ἐωράκατε παρὰ τοῦ πατρὸς ὑμῶν ποιεῖτε S\*. In my opinion, the text of B is original. P66, P75 and S\* are more or less influenced by the preceding words in the same verse, ἃ ἐγὼ ἐωράκα παρὰ τῷ πατρὶ λαλῶ. P75 has taken λαλεῖτε after λαλῶ, P66 and S\* have taken ἐωράκατε after ἐωράκα. The text makes a difference between seeing what is with the Father, which distinguishes the Son, and hearing about what should be done, which is characteristic of the Jews. This point disappears with the replacements. The readings of P66 and S\* stand in some relationship.

8:39 (εἰ τέκνα τοῦ Ἀβραάμ ἐστε, τὰ ἔργα τοῦ Ἀβραάμ) ἐποιεῖτε P75 S\* ποιεῖτε P66 B\*. The variant of P75 S\* ought to be initial. Someone has introduced ποιεῖτε after ἐστε and at the same time changed the sense of the phrase: instead of “you should have done”, there is the imperative “do”, a stylistic ‘improvement’.

8:51 (θάνατον οὐ μὴ) θεωρήση/-ει P75 S B ἴδη P66. One synonym may supplant another, and mostly such a replacement does not say very much. But it is possible that ἰδεῖν θάνατον was a well-known expression in the Church. It is found in Lk 2:26 and Hebr. 11:5. Thus, P66 could be influenced by another Gospel or by customary Christian language.

8:57 Ἀβραάμ ἐώρακας (ἐώρακες B\*) P66 B ἐώρακέν σε P75 S\*. The context is that the Jews said: “you are not 50 years old yet and you have seen Abraham”, πενήτηκοντα ἔτη οὐπω ἔχεις καὶ Ἀβραάμ ἐώρακας; Probably, there has been a mistake concerning the endings leading to ἐώρακεν, and then it was necessary to add σε, a sort of grammatical ‘improvement’. There is a relationship between P75 and S\*.

9:4 ἡμᾶς δεῖ ἐργάζεσθαι τὰ ἔργα τοῦ πέμψαντός με B ἡμᾶς δεῖ ἐργάζεσθαι τὰ ἔργα τοῦ πέμψαντος ἡμᾶς P66 P75 S\*. The reading of P66 P75 S\* has hardly any sense and must be secondary. The best reading is probably that presented by codex Alexandrinus (A, 02) and codex Ephraemi (C, 04), which have ἐμέ instead of ἡμᾶς and then με (like B). Jesus says that he should work as long as the day lasts, further that he is the light of the world as long as he remains on earth. The reading of A and C is strongly supported by the later tradition, by versions and by Fathers.

9:18 (ἐφώνησαν τοὺς γονεῖς αὐτοῦ) τοῦ ἀναβλέψαντος P75 S B om. P66\*, add. in marg. P66c. τοῦ ἀναβλέψαντος could be a clarifying insertion, but the words could have been disregarded intentionally or omitted unintentionally. If intentionally, they were considered redundant after ὅτι ἦν τυφλὸς καὶ ἀνέβλεψεν shortly before.

9:21 αὐτὸν ἐρωτήσατε, ἡλικίαν ἔχει, αὐτὸς περὶ ἑαυτοῦ λαλήσει P66 B αὐτὸς ἡλικίαν ἔχει, αὐτὸς περὶ ἑαυτοῦ λαλήσει P75 αὐτὸς ἡλικίαν ἔχει, περὶ ἑαυτοῦ λαλήσει S\*. There is one wording with and one without αὐτὸν ἐρωτήσατε. We find in v. 23 οἱ γονεῖς αὐτοῦ εἶπαν ὅτι ἡλικίαν ἔχει, αὐτὸν ἐπερωτήσατε. The variation in v. 21 can be explained in two ways: Either the longer version is original, because it is clear from v. 23 that the parents really did say αὐτὸν ἐρωτήσατε, or these words are added because starting from v. 23, someone thought that the words should really be there and added them. It is hard to see why anyone would have omitted αὐτὸν ἐρωτήσατε from v. 21. It seems more probable that the words are supplementary and originate from nearby and once were not there. We must also take into the account that there are documents where αὐτὸν ἐρωτήσατε is placed after ἡλικίαν ἔχει, among others codex Alexandrinus (02); this makes the words more suspected of being an addition.

9:26 εἶπον οὖν αὐτῷ· τί P75 S\* B εἶπον οὖν αὐτῷ πάλιν· τί P66<sup>38</sup>. The reading εἶπον οὖν αὐτῷ πάλιν· τί is well represented in the traditions, for instance in S<sup>2</sup> and codex Alexandrinus (02). P66 has obviously followed another tradition than the other three.

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38 πάλιν τί is the word order in P66, as the fac-simile shows and NA indicates. The edition of the papyrus reads erroneously τί πάλιν.



9:27 οὐκ ἠκούσατε P75 S\* B ἠκούσατε P66<sup>39</sup>. Both readings are possible: You had not paid attention (οὐκ ἠκούσατε), you had already heard (ἠκούσατε).

9:33 (εἰ μὴ ἦν) οὗτος παρὰ θεοῦ P75<sup>40</sup> S B οὗτος παρὰ θεοῦ ὁ ἄνθρωπος P66. A few manuscripts, Θ (038), N (022) and 1241 (with a different word order), add ὁ ἄνθρωπος with P66 in v. 33, but in v. 35, Θ and 1241<sup>41</sup> read τὸν υἱὸν τοῦ θεοῦ instead of τὸν υἱὸν τοῦ ἀνθρώπου, which is the text of P66 and the other three, accepted by NA. Ehrman<sup>42</sup> wants to see in the reading τὸν υἱὸν τοῦ θεοῦ an attempt at introducing an orthodox text against the adoptionists. He may be right, but we should note that the addition of ἄνθρωπος is of a trivial type and does not necessarily show a theological tendency.

9:36. The text of NA is established after among others codex Bezae (D, 05): ἀπεκρίθη ἐκεῖνος καὶ εἶπεν· καὶ τίς ἐστιν, κύριε. Our documents read: ἀπεκρίθη ἐκεῖνος καὶ τίς ἐστιν ἔφη (ἔφη del. P66c) κύριε P66\* καὶ τίς ἐστιν ἔφη κύριε P75 B ἀπεκρίθη ἐκεῖνος καὶ εἶπεν κύριε τίς ἐστιν S\*. I would say that the text of P75 and B are initial, and that this text has been completed in P66 and S\* by the addition of ἀπεκρίθη ἐκεῖνος and further changed in S\* and codex Bezae by the addition of καὶ εἶπεν. These are all trivial additions, influenced by customary Christian language. We do not arrive at the shorter text by a homoeoteleuton, which speaks in favour of this shorter text.

9:38/39 (ὁ λαλῶν μετὰ σοῦ ἐκεῖνός (αὐτός P66) ἐστιν.) ὁ δὲ ἔφη· πιστεύω, κύριε· καὶ προσεκύνησεν αὐτῷ. Καὶ εἶπεν ὁ Ἰησοῦς· (εἰς κρίμα ἐγὼ εἰς τὸν κόσμον τοῦτον ἦλθον, ἵνα οἱ μὴ βλέποντες βλέπωσιν ...) P66 B ὁ δὲ ἔφη ... Ἰησοῦς om. P75 S\*. The man who used to be blind had asked Jesus who the Son of man is (v. 36), and continued: ἵνα πιστεύω εἰς αὐτόν. For what follows, the simplest explanation seems to be that the text did not say explicitly that the man has come to believe in the Son of man. Accordingly, it was felt necessary to emphasize through an addition that the man really became a believer: ὁ δὲ ἔφη· πιστεύω, κύριε· καὶ προσεκύνησεν αὐτῷ. After that, it became imperative to add: Καὶ εἶπεν ὁ Ἰησοῦς. The text runs well without what is found outside the

39 The edition and NA has P66<sup>vid</sup>, but there is no space for οὐκ.

40 For P75, the edition gives αῠος with a dot under alpha. In the fac-simile, this letter seems to be totally destroyed.

41 Also N, according to Ehrman, p. 114, n. 186, but GNT does not indicate N.

42 P. 114, n. 186.

shorter text. Jesus answers: You have seen him, it is me. Then the blind man drops out of the context and Jesus continues, alluding in a general way to the blind and the seeing of this world and so to speak raising the level, cf. remark below on 13:10. The long text should be regarded as clarifying and supplementary, with a shade of educational theology: Follow this man's example!

10:4 (τὰ ἴδια) πάντα ἐκβάλῃ P66c P75 B ἐκβάλῃ πάντα P66\* ἐκβάλῃ (without πάντα) S\*. There is a strong tradition that reads πρόβατα instead of πάντα. Consequently, πάντα is under suspicion; probably S\* is right. We expect a noun after ἴδια, and a complement was added.

10:7 ἡ θύρα P66 S B ὁ ποιμὴν P75. A change in P75, caused by the context, especially v. 2 and v. 16, and the general Christian idea of Christ as the good shepherd.

10:8 (πάντες ὅσοι) ἦλθον πρὸ ἐμοῦ (κλέπται εἰσὶν) P66 B ἦλθον P75 S\*. There is also another tradition that places πρὸ ἐμοῦ before ἦλθον. Ehrman<sup>43</sup> thinks that the words were omitted in order to avoid an interpretation by gnostics and docetists, who rejected the Old Testament and did not regard its characters as the forerunners of Christ. Omitting πρὸ ἐμοῦ a severe judgement on them was avoided, and what Jesus said became vaguer, no more a blanket condemnation of the saints of the Old Testament. It could rather be interpreted as Jesus speaking about more or less contemporary men, like the agitators mentioned by Gamaliel (Acts 5:36–37) or about false Messiahs. An addition is very suspect if it is missing from some traditions and if it is placed differently where it occurs. ἦλθον alone is very vague, and it is possible that somebody wanted to complete by supplying a grammatical 'improvement'. Accordingly, I think that the shorter text is original; Ehrman's interpretation is certainly possible.

10:11 (τὴν ψυχὴν αὐτοῦ) τίθησιν (ὕπερ τῶν προβάτων) P66 P75 B δίδωσιν S\*. It is difficult to know which variant is the original. We could consider δίδωσιν more 'normal', that is to say a reading which has supplanted a more special reading, found however several times in John, for example in v. 17 and v. 18. But on the other hand, δίδωσιν could have been replaced by the more 'johannine' τίθησιν.

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43 P. 240.

10:15 (τὴν ψυχὴν μου) τίθημι B δίδωμι P66 S\* lacuna P75. Cf. remark above on 10:11.

10:18 (οὐδεις) αἶρει (αὐτήν, sc. τὴν ψυχὴν) P66 ἤρην S\* B lacuna P75. αἶρει could be interpreted as αἰρεῖ, but αἶρω is well represented in the New Testament, also in John, whereas αἰρέω is employed only in the middle voice, meaning ‘to choose’. Even though ἤρην is a *lectio difficilior*, αἶρει is probably the original reading, forming an antithesis to τίθημι shortly after. There seems to be a relationship between S\* and B.

10:26 (ἐκ τῶν προβάτων) τῶν ἐμῶν. (τὰ πρόβατα τὰ ἐμὰ τῆς φωνῆς μου ἀκούουσιν) P66c P75 S B τῶν ἐμῶν καθὼς εἶπον ὑμῖν ὅτι P66\*. There is evidently an insertion in P66\*, taken from v. 3 and v. 4, and afterwards rejected. The scribe has followed two traditions, the addition being well known from elsewhere.

10:29 (ὁ πατήρ μου) ὃ δέδωκέν μοι πάντων μεῖζόν ἐστιν B\* ὃς ἔδωκεν (μοι add. P66c) μεῖζων πάντων ἐστίν P66\* ὃ δέδωκέν μοι πάντων μεῖζων ἐστίν S. P75 has lacunas, but reads ὃς and ἔδωκεν. The original reading is in my opinion ὃς and μεῖζων. Jesus shows his power and from where it comes: What I have no one can tear from my hand; my Father, who has given me that, is greater than anyone else, no one tears anything from his hand; I and the Father, we are one. Metzger thinks that ὃ after πατήρ is striking and may explain a false ὃς, a *lectio facilior*; he could very well be right, but we could also think that ὃς has been changed to ὃ in order to give a complement to δέδωκέν μοι. However, the variants are trivial and do not say very much, if anything.

10:33 (σὺ ἄνθρωπος ὃν ποιεῖς σεαυτὸν) θεόν P66 P75<sup>44</sup> S B τὸν θεόν P66\*. Ehrman<sup>45</sup> thinks that the article is introduced in order to confirm the idea that Jesus is really God, that he and the Father are one. This is not impossible but not very convincing, since an article can be lost or added in a rather easy-going way, and in our passage, the difference between the readings may be due to a dittography or a haplography; there is the sequence -τὸν τὸν θεόν. In 5:18, θεός has the article twice in a similar context. I do not think that 10:33 indicates anything.

44 The space is not sufficient for τὸν in a lacuna in P75.

45 P. 84.

10:34 ἀπεκρίθη αὐτοῖς ὁ (ὁ om. B) Ἰησοῦς P75 S B ἀπεκρίθη Ἰησοῦς καὶ εἶπεν αὐτοῖς P66. The addition in P66 is trivial, belonging to the customary Christian language, but on the other hand, the shorter text can result from an omission of a not very necessary phrase.

10:38 (ἴνα γινῶτε) καὶ γινώσκητε P66 P75 B καὶ πιστεύητε S. There are two forms of the same verb γινώσκω, which may have caused a replacement in S. But I think that it is better to regard πιστεύητε as a repetition of πιστεύετε/-ητε shortly before in the text.

10:40 (πέραν τοῦ Ἰορδάνου) εἰς τὸν τόπον ὅπου P75 B εἰς τὸν τόπον οὗ P66 ὅπου S\*. Without εἰς τὸν τόπον, the passage corresponds verbatim to 1:28. The idea might have been to conform the wording to 1:28, but εἰς τὸν τόπον could also have been omitted as redundant.

11:3 ἀπέστειλαν οὖν αἱ ἀδελφαὶ πρὸς αὐτὸν λέγουσαι P75 S B ἀπέστειλεν οὖν Μαρία πρὸς αὐτὸν λέγουσα P66\*<sup>vid46</sup>. Starting from the fac-simile, it is difficult for me to arrive at the reading which NA attributes to P66\*, but let us suppose that NA is right. We may then ask whether the papyrus is influenced by Jn 12:2–3 or by Lk 10:38–42, where Mary stands out. In our pericope, Jn 11, it is rather Martha who has the more important role. Is the reading supposed to be that of P66\* maybe a change in order to make Mary more prominent? Impossible to decide.

11:12 σωθήσεται P66 S B ἐγερθήσεται P75. It is evident that P75 is influenced by the context where Jesus talks about Lazarus as if he were asleep.

11:27 ναὶ κύριε, ἐγὼ πεπίστευκα (ὅτι σὺ εἶ ὁ χριστὸς) P75 S B ναὶ κύριε πιστεύω· ἐγὼ πεπίστευκα P66. πιστεύω in P66 is not necessary after πιστεύεις in v. 26. It could be an addition in P66, but also an omission in the other documents, occasioned by a homoeoteleuton.

11:31 δόξαντες S B δοξάζοντες P75 λέγοντες P66. It is impossible to say which reading, δόξαντες or λέγοντες, is initial. δοξάζοντες has no sense.

11:33 ἐνεβριμήσατο τῷ πνεύματι καὶ ἐτάραξεν ἑαυτὸν P75<sup>47</sup> S B. P66c reads: παραχθη[[σατο]] τῷ πνεύματι ὡς εμβριμωμένος [[τον]]. It is evi-

46 The edition of P66 has the same text as P75 S B.

47 I think that in P75 we cannot read more than εβρεμ(η?)σατ not preceded by εν, τ in τω, ξ in εταραξεν, τον in εαυτον.

dent that the copyist of P66 has changed his text after 13:21. In the facsimile, it is hardly possible to see what was there originally.

11:54 (1) εἰς τὴν χώραν ἐγγὺς τῆς ἐρήμου, εἰς Ἐφραῖμ λεγομένην πόλιν S B εἰς τὴν χώραν ἐγγὺς τῆς ἐρήμου Ἐφραῖμ λεγομένην (πόλιν add. P66c) P66\*. P75 is lacunose, but it is possible to establish εἰς τὴν χώραν ἐγγὺς τῆς ἐρήμου εἰς Ἐφραῖμ, and the space of a lacuna shows that it reads πόλιν. It seems that P66\* attests an original text and that the longer text is a clarifying addition. One does not see why one should reject both εἰς and πόλιν. P66c might have corrected its own error by adding πόλιν, or it had access to another tradition.

11:54 (2) ἔμεινεν P66\* P75 S B διέτριβεν P66c. P66 has been corrected according to another tradition, already existing in the very old papyrus P45 (3rd c.) and later in manuscripts and versions.

12:1 Λάζαρος S B Λάζαρος ὁ τεθνηκώς P66 lacuna P75. P66 might have a clarifying addition, but the word could also have been omitted as unnecessary.

12:3 (λίτραν) μύρου νάρδου πιστικῆς πολυτίμου S B μύρου πιστικῆς πολυτίμου (without νάρδου) P66\* νάρδου ss. P66c lacuna P75. Quite a few words are used in order to describe a perfume of great value, among them πιστικῆς, the meaning of which is not well known. We do not know whether it is an adjective indicating a quality or a noun denoting a special kind of perfume. Possibly this word was considered difficult to understand and explained by the addition πολυτίμου, but since the tradition always presents πολυτίμου, we should accept it.

On the other hand, νάρδου is absent from P66\*. This perfume was well known, and consequently, it could be an insertion. There is a relationship between our passage and Mk 14:3 μύρου νάρδου πιστικῆς πολυτελοῦς, where codex Bezae (D, 05) omits νάρδου πιστικῆς πολυτελοῦς. It looks probable to me that for our passage P66\* and for Mark, codex Bezae have original readings.

12:8 (τοὺς πτωχοὺς γὰρ πάντοτε ἔχετε) μεθ' ἑαυτῶν, ἐμὲ δὲ οὐ πάντοτε ἔχετε P66 S B om. P75. The omission is caused by a homoeoteleuton.

12:11 πολλοὶ δι' αὐτὸν (sc. Λάζαρον) ὑπῆγον τῶν Ἰουδαίων καὶ ἐπίστευον P75 S B πολλοὶ δι' αὐτὸν τῶν Ἰουδαίων ἐπίστευσαν P66. The Jews, or more exactly the Pharisees, feared that they would be abandoned, cf. v.

19 ἴδε ὁ κόσμος ὀπίσω αὐτοῦ ἀπῆλθεν. The context shows that ὑπῆγον is not an empty word which could be disregarded. Probably we find here an isolated unintentional fault in P66.

12:17 (ἐμαρτύρει οὖν ὁ ὄχλος ...) ὅτε (τὸν Λάζαρον ἐφώνησεν ἐκ τοῦ μνημείου) S B ὅτι P66 σ[ P75. As to the meaning of the passage, P66 has a reading which is perfectly possible. One cannot decide which text is initial.

12:22 ἔρχεται Ἀνδρέας καὶ Φίλιππος καὶ λέγουσιν P75<sup>vid48</sup> B καὶ πάλιν ὁ Ἀνδρέας δὲ καὶ ὁ Φίλιππος λέγουσιν P66\* Ἀνδρέας δὲ καὶ Φίλιππος λέγουσιν P66c καὶ πάλιν ἔρχεται Ἀνδρέας καὶ Φίλιππος καὶ λέγουσιν S. What we find and do not find in this text is ἔρχεται and πάλιν. It seems that P66 intentionally has left out ἔρχεται, annoying after an ἔρχεται occurring shortly before, and that P66\* and S have added καὶ πάλιν to make the text proceed smoothly. P66\* and S have been influenced and ‘improved’, but in different ways. P66c omits καὶ πάλιν, thereby following another tradition.

12:28 (δόξασόν) σου τὸ ὄνομα P66 P75 S μου τὸ ὄνομα B. The unique reading of B is due to a false interpretation of the context, as is also σου τὸν υἱὸν in a later tradition.

12:30 (ἡ φωνὴ αὐτῆ) γέγονεν P75 B S ἦλθεν P66. Cf. above 6:15 (1); 6:17 (2); 6:25. It is hardly possible to decide which reading is original.

12:40 (1) ἐπώρωσεν B\* ἐπήρωσεν P66 P75 S. ἐπήρωσεν is an error introduced early in the text without forming a strong tradition. There must be a relationship between the witnesses of this variant.

12:40 (2) νοήσωσιν (τῆ καρδία) P75<sup>49</sup> S B μὴ νοήσωσιν P66\*. P66\* has inserted a negation, influenced by the preceding ἵνα μὴ ἴδωσιν τοῖς ὀφθαλμοῖς καὶ. This could be a grammatical ‘improvement’ or help to understand the text, but should rather be considered an influence from the preceding negation.

13:5 νιπτῆρα S B ποδονιπτῆρα P66 lacuna P75. An clarifying addition in P66.

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48 It seems to me that the reading of P75 is obvious. What we do not see is the second καὶ, but there is room for the word.

49 P75 has a lacuna where there is hardly space enough for the negation.

13:10 (ὁ λελουμένος) οὐκ ἔχει χρείαν εἰ μὴ τοὺς πόδας νίψασθαι B οὐκ ἔχει χρείαν εἰ μὴ τοὺς πόδας μόνον νίψασθαι P66 οὐκ ἔχει χρείαν ἢ τοὺς πόδας νίψασθαι P75<sup>50</sup> οὐκ ἔχει χρείαν νίψασθαι S. In my opinion, the short text of S is the initial one. εἰ μὴ τοὺς πόδας is an insertion, clarifying in a rather pedantic way. It comes from the text that follows, v. 12 ff. The addition emphasizes that it is all about washing the feet, but this is in my opinion a false interpretation. Peter talks about washing in a practical sense, Jesus raises the level, speaking generally about purity.<sup>51</sup> There is also another addition, that of μόνον, in P66.

13:18 (ὁ τρώγων) μου (τὸν ἄρτον) B μετ' ἐμοῦ P66 S lacuna P75. There is an allusion to Ps. 40 (41):10 ὁ ἐσθίων ἄρτους μου, but there, two very old papyri (4th c.) have μου μετ' ἐμοῦ.<sup>52</sup> The reading μετ' ἐμοῦ may be influenced by Mk 14:18. It is impossible to have an opinion about the priority of these variants.

13:24 (νεύει οὖν τούτῳ Σίμων Πέτρος) πυθέσθαι τίς ἂν εἴη P66c (P66\* is illegible) πυθέσθαι τίς ἂν εἴη περι οὗ ἔλεγεν, καὶ λέγει αὐτῷ· εἰπέ τίς ἐστίν S καὶ λέγει αὐτῷ· εἰπέ τίς ἐστίν B lacuna P75. Evidently, there is a conflation in S. We have to choose between καὶ λέγει αὐτῷ· εἰπέ τίς ἐστίν and πυθέσθαι τίς ἂν εἴη. It is reasonable to say to the disciple whom Jesus loved: “Ask him who it is!” It is not very reasonable to say: “Tell us who it is!”, because of course the disciple did not know and could not tell. Exactly because of that, I think that the reading of B is the more original: καὶ λέγει αὐτῷ· εἰπέ τίς ἐστίν. In P66c and S, this reading has been manipulated in order to make it more logical and suitable to the context.

13:26 (1) βάψω τὸ ψωμίον καὶ δώσω αὐτῷ B βάψας τὸ ψωμίον ἐπιδώσω P66 S lacuna P75. P66 and S have an ‘improvement’ of style.

13:26 (2) βάψας οὖν τὸ (τὸ om. B) ψωμίον λαμβάνει καὶ δίδωσιν S<sup>1</sup> B βάψας οὖν τὸ ψωμίον δίδωσιν S\* καὶ ψας (sic) τὸ ψωμίον δίδωσιν P66\* καὶ ἐμβάψας τὸ ψωμίον δίδωσιν P66c lacuna P75. λαμβάνει καὶ is probably a later addition, but it could be original and disregarded as redundant in S\* and P66; we do not know which reading is initial.

50 According to NA and GNT, P75 has the text quoted above. The edition and the fac-simile show a lacuna for the whole passage.

51 Cf. above remark on 9:38/39.

52 2013 and 2050, cf. *Psalmi cum Odis*.

ἐμβάψας of P66c could be influenced by Mt 26:23 ἐμβάψας, but Mk 14:20 ἐμβαψόμενος is also close to our text.

13:33 μικρὸν P66 B μικρὸν χρόνον S lacuna P75. S may have an clarifying addition; there could be an influence from 7:33 and 12:35, but cf. 14:19 and 16:16–19, where we find μικρὸν without χρόνον. The omission could also be due to a homoeoteleuton.

13:33/34 (ὅπου ἐγὼ ὑπάγω ὑμεῖς οὐ δύνασθε ἐλθεῖν, καὶ ὑμῖν) λέγω ἄρτι. Ἐντολὴν (καινὴν δίδωμι ὑμῖν) S B λέγω· πλὴν ἄρτι ἐντολὴν P66 lacuna P75. P66 has πλὴν, maybe in order to avoid an asyndeton and ‘improve’ the style. S and B seem to have the original text, because there is a contrast between εἶπον τοῖς Ἰουδαίοις shortly before and ὑμῖν λέγω ἄρτι. It is, however, difficult to evaluate the variants.

14:4 ὅπου ἐγὼ (ἐγὼ om. P66) ὑπάγω οἴδατε τὴν ὁδὸν P66c S B οἴδατε καὶ τὴν ὁδὸν οἴδατε P66\* lacuna P75. The original reading could be ὅπου ἐγὼ ὑπάγω οἴδατε, completed from v. 5 πῶς δυνάμεθα τὴν ὁδὸν εἶδέναι; However, Thomas talks in what follows both about the destination (ποῦ ὑπάγεις) and about the ‘road’, and consequently, we may suppose that there he alludes to a parallel phrase in v. 4 with those two members, like the reading in P66\*. In P66, there are signs of two traditions.

14:5 (πῶς) δυνάμεθα τὴν ὁδὸν εἶδέναι; P66 τὴν ὁδὸν εἶδέναι δυνάμεθα S οἴδαμεν τὴν ὁδὸν B lacuna P75. Since the preceding οἴδαμεν may have influenced B, the shorter reading should not be taken for granted but is very tempting, δυνάμεθα being placed differently.

14:7 (εἰ ἐγνώκατέ με, καὶ τὸν πατέρα μου) γνώσεσθε P66 S ἂν ἤδειτε B lacuna P75. B is influenced by 8:19. Also a future tense after εἰ ἐγνώκατέ με may have been felt awkward and unexpected.

14:10 (τὰ ῥήματα ἃ ἐγὼ) λέγω (ὑμῖν ἀπ’ ἐμαυτοῦ οὐ λαλῶ) P75 λαλῶ P66 S om. B\*. P66 and S may be influenced by the following λαλῶ or by 8:28. It could, however, be that B\* has something of an original reading; if we reject ἃ, which could come from a dittography, the proposition of B\* runs well. We must of course stick to the reading of the other documents, but whether we should prefer λέγω or λαλῶ is unclear.



14:14 ἐγὼ ποιήσω P66\* S τοῦτο ποιήσω P75 B τοῦτο ἐγὼ ποιήσω P66c. P66 seems to be influenced by two traditions, but adding ἐγὼ could also be an unintentional change which comes about almost automatically.

14:16 μεθ' ὑμῶν εἰς τὸν αἰῶνα ἧ̃ P75 B μεθ' ὑμῶν ἧ̃ εἰς τὸν αἰῶνα S μένη μεθ' ὑμῶν εἰς τὸν αἰῶνα P66. μένη μεθ' makes a homoeoarcton and may bring about the disappearance of μένη, later replaced with ἧ̃, which is placed differently. The reading of P66 ought to be original, that of the others is a grammatical 'improvement' after the loss of μένη.

14:21 ἀγαπηθήσεται (ὑπὸ τοῦ πατρός μου) P66 S B τηρηθήσεται P75. This is a singular fault in P75. The verb τηρεῖν goes well with ἐντολάς, cf. shortly before, less well when it comes to the relationship between God and man.

14:31 ἐνετείλατό μοι ὁ πατήρ S ἐντολὴν ἔδωκέ μοι ὁ πατήρ P75<sup>vid</sup> B lacuna P66. Impossible to say which reading is original.

15:4 (οὕτως) οὐδὲ ὑμεῖς ἐὰν μὴ ἐν ἐμοὶ μένητε S B καὶ ὁ ἐν ἐμοὶ μένων P66 lacuna P75. It is possible that P66 originally had καὶ ὁ ἐν ἐμοὶ μὴ μένων, where μὴ has disappeared in the sequence μοι μη με. When Jesus hereafter speaks about those who remain or do not remain in him, he says "you" or addresses one generalized person. It could be that P66 is influenced by the preceding (τὸ κλῆμα) ἐὰν μὴ μένη ἐν τῇ ἀμπέλῳ or by the following ὁ μένων ἐν ἐμοὶ. A reading like that of P66 is found in some manuscripts of the Itala, but always with the negation.

15:8 καρπὸν πολλὸν S B καρπὸν πλείονα P66 lacuna P75. P66 could be influenced by v. 2.

### **Overview of the readings**

It goes without saying that the passages presented do not have the same weight as witnesses of one or the other type of alteration. I refer once and for all to the discussion above, but I really think that it is at least possible that the changes can be classed as follows. There is a question-mark at the passages which I regard as less certain than the others. It is especially difficult or downright impossible to know whether a reading should be attributed to one or the other of the adjacent classes *Influences from another Gospel* and *'Theological' change*. It is also difficult to attribute variants correctly to the class of unintentional variants called

above *Variation occasioned by customary Christian language*. Also a distinction between *Clarifying or supplementary insertion or change* and *Stylistic or grammatical 'improvement'* is certainly difficult.

*Influence from a passage nearby or from the context*

1:36 P66\*; 3:5 (1) S\*; 3:8 S; 3:20 P66; 3:31 P66 B; 5:45 B; 6:22 S\*; 7:41 P66\* S; 8:16 P66 P75 B; 8:38 P66 P75 S\*; 9:21 P66 B; 10:7 P75; 10:26 P66\*; 10:38 S; 11:12 P75; 12:28 B; 12:40 (2) P66\*; 13:10 P66 P75 B; 15:4 P66; 15:8 P66.

*Influence from a passage of the same Gospel*

1:4 (2)? P66 P75 S; 4:24 S\*; 5:25 P66 P75 B; 6:17 (1) S; 11:33 P66c; 14:7 B.

*Influence from another Gospel*

1:27 P66 P75; 3:5 (2) S\*; 6:1? P66c P75<sup>vid</sup> S B; 6:15 (2)? S\*; 8:51? P66; 12:3 P66c S B; 13:18 P66 S; 13:26 (2) P66c.

*Text influenced by two traditions*

1:36 P66c; 1:49 (2) P66c; 2:11 P66c S\*; 4:51 P66; 5:6 P66c; 6:1? P66c P75<sup>vid</sup> S B; 6:58 P66c; 7:39 P66c; 7:40 P66c; 7:41 P66c; 7:46 P66c; 10:26 P66c; 11:54 (1)? P66c; 11:54 (2) P66c; 12:22 P66c; 13:24 S; 14:4 P66c.

*'Theological' change*

1, 49 (2)? P66\*; 3:34? P66 P75 S; 4:42? S\*; 9:38/39 P66 B; 10:8? P66 B.

*Clarifying or supplementary insertion or change*

1:20 P66 P75 B; 1:49 (2)? P66\*; 2:3 (1) S\*; 2:11 P66\* S\*; 2:15 P66 P75; 4:9 P66 P75 B; 4:42 S\*; 7:40 P66\*; 7:46 P66\* S\*; 8:27 S\*; 9:21 P66 B; 9:36 P66 B; 9:38/39 P66 B; 11:54 (1) S B; 13:5 P66; 13:24 P66c (P66\* is illegible) S.

*Stylistic or grammatical 'improvement'*

1:39 S; 5:6 P66c P75 B; 6:23 S; 6:24 S\*; 8:39 P66 B\*; 8:57 P75 S\*; 10:4 P66c P75 B; 10:8? P66 B; 12:22 P66\* S; 13:26 (1) P66 S; 14:7? B; 14:16 P75 S B.

## Conclusions

### *Relationship between the four witnesses*

The aim has not been to establish real and close relationships between the four documents. It is well known from elsewhere that P75 and B are close to each other, but that is not confirmed by the variants shown above. Feeble and different as they are, the best examples of relationship are the following: 8:38 P66 S\*; 8:57 P75 S\*; 9:4 P66 P75 S\*; 10:18 S\* B; 12:40 (1) P66 P75 S.

### *Character of the variants*

It is of course not possible to establish statistics, not even very approximate ones, concerning these witnesses. The places of variations are too few, the uncertain cases too many. But we can make some rough statements:

1. Influences from a passage nearby or from the context are comparatively many. P66 has been much more interfered with (towards a dozen times) than the others (each one around half a dozen times). Passages from the same Gospel but from further away do not have much influence.<sup>53</sup>

2. Influences from another Gospel are less evident. There are 5–6 of them in P66 and S, only solitary cases in the others.

3. Passages influenced by two traditions are numerous in P66, almost non-existent in P75 and B, and there are a few cases in S.

4. ‘Theological’ change is rare and not very certain. There too P66 is represented more.

5. The clarifying or supplementary insertions or changes are numerous, like the influences under 1. above. They are especially frequent in P66 (about a dozen times), S and B present about half that number, P75 only a few.

6. The examples of stylistic or grammatical ‘improvement’ are not many. P75 has few cases, the others around half a dozen.

It is thus evident that the variants of the types 1 and 5 are the most numerous. It is also evident that all these four witnesses have been interfered with, even as early as during the first centuries of Christianity. P66, the oldest of them, has been subjected to a more thorough elaboration than the others.

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<sup>53</sup> Cf. Royse, p. 737 about harmonizing: “it seems evident that the major influence on our scribes was the immediate context.”

### *Old and new witnesses*

Comparing the older and younger documents, it is easy to see that there are false readings very early which have not influenced the tradition much. Both P66 and P75 read in 1:27 *ικανός*, a variant not much represented elsewhere. The readings of P75 in 11:12 and 14:21 are unique, and 10:7 is not well known outside P75; even B, very close to P75, does not present those readings. In those cases, B is superior to P75. Variants unique to B are 5:45 *πρὸς τὸν πατέρα* and 12:28 *μου τὸ ὄνομα*. Other variants rarely represented in the later traditions are found in S: 3:5 (2) S\*; 3:8 S; 4:24 S\*; 4:42 S\* ; 8:27 S\*. In these passages, the influence of the tradition of S appears outside the Greek tradition.

In 9:4, P66 P75 S\* have the false reading *ἡμᾶς δεῖ ἐργάζεσθαι τὰ ἔργα τοῦ πέμψαντός με*. This variant appears in witnesses of a venerable age. Nevertheless, this wording has in my opinion no sense, whereas other witnesses, among them the Byzantine tradition, have the better variant with *με* and *ἐμέ* instead of the two *ἡμᾶς* of P66 P75 S\* and *ἡμᾶς ... με* of B. In 12:40 (1) P66 P75 S read *ἐπήρωσεν*, but the dominating tradition, including the Byzantine, writes correctly *ἐπόρωσεν* or *πεπόρωκεν*.

Both bad and good variants are preserved in the following passages. In 2:15 there is *ὡς φραγέλλιον* in P66 P75 and only *φραγέλλιον* in S B; the two readings are well, almost exceedingly well, represented in later traditions, the preferable text without *ὡς* for instance in the Byzantine tradition against the two papyri. In 6:22, the variant of S\* with an addition that is certainly not original and the reading without it are both well represented in early and late documents. As for 7:39, where P66\* and B have the addition *ἅγιον* to *πνεῦμα*, both readings are very well represented, but the initial reading is generally found in the uncials and the addition in the minuscules. In 10:26, the better reading without the addition and the less good with it are both well represented, the good one especially by the Fathers, the other by a cloud of witnesses, among them the Byzantine tradition.

### **Consequence**

The consequence to draw from this study is not original, or should not be: We should not put too much trust in old documents. Age and dispersion of a reading ought to be of less importance, or, to put it a bit more strongly: It should be of no importance at all. To base one's choice on such criteria is often necessary but always the last resort. It is necessary to try and find a reading which explains the others. This is the *βασιλικὴ ἄτροπος*, to follow as best we can.

## Chapter 2. Papyri and the New Testament: Barbara Aland on Important Papyri

### Three articles

Three articles on papyri by Barbara Aland have a bearing on how we regard the early tradition of the New Testament and, consequently, how we may judge ECM2. Consequently, these articles are of great importance. In my opinion, they partly explain why nothing is said about papyri in the introduction to ECM2. A quotation from ECM2 seems to indicate that the text was comparatively fixed early and that the variants of the early papyri do not contribute much to establishing the text<sup>54</sup>; after a statement that minor differences arise as a result of manual copying, we read: “However, we have not found evidence indicating that significant changes must have been introduced between the authorial texts and the archetype of the tradition<sup>55</sup>. Following the simplest assumption we claim that the present reconstruction is a hypothesis about the text of the authors.” The idea is evidently that not much of importance happened between the text of the author and the text of the earliest documents and that it is possible to reconstruct the text of the authors with great certainty. The question is now: What importance should we attach to these early papyri?

Before entering on a critique of them, I would like to make it clear that I have a different opinion on some issues of importance for editing the New Testament, but that I have the greatest respect for the work done by Barbara Aland and her collaborators at the Institute for New Testament Textual Research (Institut für Neutestamentliche Textforschung) at Münster. I hope, like all interested in such studies, that it will proceed steadily and successfully.

The articles are Aland (2003), Aland (2002) and Aland (2006). I think that Aland (2003) is the best starting-point.<sup>56</sup>

### Aland (2003)

Aland examines the oldest papyri of the Gospel of John, except P66 and P72, which are almost complete and could be compared to manuscripts. You may wonder why they are not taken up. They are from the same early time and the fragmentary texts which Aland studies are probably

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54 EMC2, p. 30\*.

55 I take it that the ‘archetype of the tradition’ is what in the context of the CBGM and in ECM2 is usually called ‘initial text’.

56 As to spiritus and accent, I follow Aland.

what remains of more complete texts. So there is no fundamental difference between longer texts and shorter ones. These documents have been treated in ch. 1.

The papyri discussed are from the second or third century, except P6 (4th c.). In order to point out clearly where our opinions differ, I would like to resume important points as they are presented by Aland: There are few important variants in these papyri; they all go back to an initial, hypothetical text called *A*; this text can be reconstructed and is identical with the *Novum Testamentum Graece*<sup>57</sup>; these papyri present a good text which is close to the origin; the scribes are not interested in changing the text, they just copy, but through negligence they commit many errors; their readings are of special value only if they are supported by other reliable witnesses; the number of such witnesses may increase with a better knowledge of minuscules of equal value to the old and reliable manuscripts.

I would like to ask immediately what ‘reliable’ means. You may speak of a manuscript or another witness as reliable in a broad sense, but we cannot speak of ‘reliable’ in a special case. The well-known uncials present on the whole a good text, but they show many deviations from what we consider an initial text.

Aland likes to think that the scribes were professionals and generally speaking did their job quite well, even if they were human and sometimes negligent. She does not like the idea that they compared exemplars. But it must be acceptable to think that they had in front of them texts where some interested reader had introduced corrections and additions, maybe indicated what ought to be omitted, and that the scribe followed suit. We all know that such changes are to be found in papyri and in manuscripts, and we shall see many examples later. A papyrus with many changes is P66, but the others too present occasional changes and additions. We can imagine that documents with many corrections were thrown away, as happened later to manuscripts which had been corrected and used as exemplars in the early days of printing.<sup>58</sup>

Before entering upon the papyri, I would like to state some opinions of mine. I prefer not to pay attention to what I regard as such variants as show no relationships because they may appear anywhere unintentionally. There is a list of such readings in chap. 1<sup>59</sup>. My point is that what may be unintentional should be considered unintentional. Otherwise we may see relationship where there is none. Such variants as I regard as

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57 I take this to be the 27th edition.

58 It seems that many uncials have been lost after the transcription of texts into minuscules.

59 P. 16.

of slight or no interest are often considered to be of some importance by Aland, e.g. on p. 52 concerning P45. She discusses ‘Verwandschaft’ between documents, for instance concerning P28<sup>60</sup>, using a word which may mean, I think, both ‘kinship’ and ‘relationship’. I prefer ‘relationship’, a word with a broader meaning which does not necessarily make us think of genealogy. Taking a reading as intentional or unintentional is of course a matter of personal judgement and personal experience of texts. At the Institute for New Testament Textual Research (INTF), likeness between documents is very important, in fact fundamental for building a genealogical relationship between documents, even though the usual philological criteria are used when judging disagreements between texts.<sup>61</sup> Singular readings I regard as usually caused by negligence and thus uninteresting. Nevertheless, we cannot exclude that they might have existed in other documents, unknown to us. They may even be the original reading, corrupted everywhere else.

Below follows a list of the most interesting passages from the Gospel of John in these papyri. When the readings are compared with those of other documents, I only give examples, mainly from the old witnesses such as B, S, P66, P75.

P22. Readings from about a dozen verses. Two of them are interesting:

16:21 θλιψεως: λυπης D 579

16:28 εξηλθον παρα του πατρος: missing in among others D W

Aland is of the opinion that almost always the text of P22 is close to the hypothetical initial text (*A*), but this is not clear. θλιψεως may seem more original, because λυπης may be influenced by λυπηθησεσθε, λυπη, λυπην nearby. On the other hand, there is a contrast between sorrow and joy, between λυπη and χαρα; χαρα and χαρησεται occur time after time in the same way as corresponding expressions for sorrow. In v. 16:28, the longer text may be an addition that emphasizes the fatherhood of God and the shorter text which runs well enough may be original. It is not evident that P22 has a more original text.

P28. The text presents only the ends of lines belonging to several verses. There are two telling passages:

6:11 εδωκεν τοις ανακειμενοις. After εδωκεν<sup>62</sup> there is a well attested addition, found in D among others; τοις μαθηταις οι δε μαθηται. The

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60 P. 21.

61 See chap. 3 for a fairly thorough discussion of the Coherence-Based Genealogical Method (CBGM), applied at the Institute for New Testament Textual Research.

62 εδωκεν P28 διεδωκεν NA. δεδωκεν is also well attested.

longer text looks like an explanation of a fussy kind: Jesus did not go around alone handing out food to thousands of people but was helped by the disciples!

6:17 και σκοτια ηδη εγεγονει. S D have the variant κατελαβεν δε αυτους η σκοτια. Both texts seem perfectly normal and it is difficult to have an opinion on which one is more original. You may think that καταλαμβανειν, a perfectly normal word, is a bit more literary than the omnipresent γινεσθαι, but what did the scribe do? Did he ‘improve’ the text putting κατελαβεν into it, or did he, perhaps without much thinking, write a more familiar phrase? Aland thinks that the text of P28 is close to *A*, but I think that the true reading of 6:17 is not clear. She sees no relationship (Verwandtschaft) between P28 and other witnesses, but we may say that branches of the later tradition are already present in P28.

P39. This papyrus contains only a few verses, and the text can be well established. It agrees with the hypothetical *A*-text, except for one passage, 8:14, where P39 has a different word order, in common with P75 B W.

P52. This very old document (first half of the second century) contains parts of a few verses. The text is not easy to establish. There is a reading of a certain interest, 18:33 (εισηλθεν ουν) παλιν εις το πραιτωριον ο πιλατος. This reading with παλιν is well known from early witnesses, but so is a variant that places παλιν after πραιτωριον. A couple of minuscules, 33 and 1424, from the 9th and 9/10th c., omit the word. It is not possible to say with Aland that P52 depends on *A*. The hypothetical *A* may have had παλιν, but we cannot be sure where. When a word is placed in different positions and omitted in a part of the tradition, it is very suspect.

P80. Has only the text of 3:34, but presents two interesting readings: εκ μερους<sup>63</sup> instead of εκ μετρου (διδωσιν) ο θεος (videtur) το πνευμα

The reading εκ μερους is attested by few witnesses (P6\* P80 U). Both ο θεος το πνευμα (A D) and το πνευμα without ο θεος (P66 P75 S) are old readings. In B\*, διδωσιν is the last word, there is neither ο θεος nor το πνευμα. NA informs us that το πνευμα is added by B<sup>2</sup>, but according to Metzger<sup>64</sup> the addition is made by the same hand.

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63 NA 27 and NA 28 do not inform us about the reading εκ μερους.

64 P. 176.



ου ... εκ μερους may be easier to understand than ου ... εκ μετρου and can hardly be regarded as the initial text. The longer reading ο θεος το πνευμα is through P80 older than the shorter one without ο θεος. An important fact is that B\* omits both ο θεος and το πνευμα. We are permitted to ask if these words were added later, sometimes with conflation. ο θεος can be added from the previous text, but το πνευμα is not easily understood. The context is not about the Spirit, it is all about Father and Son. το πνευμα may well be the result of theological thinking and is in my opinion suspect.

Aland's conclusion is that P80 already deviates much from *A*. I agree, but I would like to emphasize that ο θεος το πνευμα, looking like a conflation, hints at an early interference in the text and that το πνευμα is not self-evident as a choice.

P90. Text from a dozen verses. Some variants agree with Aland's initial text *A*; in other places P90 has the same text as many other witnesses, but there is no real difference as to the sense. Many of these readings are such as may appear independently anywhere. The most interesting variants are as follows:

18:36 (οι υπηρεται) οι εμοι ηγωνιζοντο αν. There are three variants in the tradition: αν is placed as above, but also at the end; B\* omits it. It is impossible to decide which reading is original. The variant of B\* may be original or a slip of the pen. Cf. above about P52, the reading παλιν.

18:38 ουδεμιαν ευρισκω εν αυτω αιτιαν. There are four different word orders, but most of the witnesses have an order where ουδεμιαν ευρισκω precedes εν αυτω. The exceptions are D<sup>s65</sup> and *pc* (*pauci*) which have the order ουδεμιαν αιτιαν εν αυτω ευρισκω. αιτιαν is placed after ουδεμιαν or after ευρισκω or last. I would like to think that αιτιαν has fallen out and been replaced differently. Most probably it was placed after ουδεμιαν, as in 19:4, and fell out because of homoeoteleuton.

19:1 λαβων ο πιλατος τον ιησουν P90<sup>vid</sup>. Common to the different readings is that they have ελαβεν/λαβων τον ιησουν and after that και if the text has ελαβεν, coordinated with εμαστιγωσεν. All the variants also have ο πιλατος, in different places. Probably the subject ο πιλατος was not in an original text, or it has fallen out. In either case, it has been introduced and placed differently.

19:4. P90 P66<sup>vid</sup> W have αιτιαν εν αυτω ουχ ευρισκω, NA has ουδεμιαν αιτιαν ευρισκω εν αυτω and notes different word orders, sometimes, as in P90, with the negation ουχ instead of ουδεμιαν. In all variants we

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65 D<sup>s</sup> means a supplement in D.

find that *ατιαν* precedes *ευρισκω* and that *εν αυτω* is placed differently or is missing. It is possible that the original text was short: *ατιαν ουχ ευρισκω*, but that it was expanded and made clearer under the influence of 18:38. It is not evident what the original text was like.

To Aland, there is no doubt that the text of P90 is based on the initial text *A*.<sup>66</sup> But at the same time, she does not stand aloof from the idea that it may be influenced from other traditions, the reason being that many witnesses have the same variants as P90. In my opinion, this should make us think that scribes or readers have interfered with the text. The main idea of Aland, that the scribe of P90 starts from the original text but commits quite a number of errors of his own, is not well founded.

P95. This short text is of no interest in this context.

P5. The papyrus contains text from many verses of the first, the sixteenth and the twentieth chapters, but it is badly preserved. There are many omissions which are found almost exclusively in P5, probably caused by negligent copying. P5 has some readings in common with many other witnesses, but most often these variants are not very interesting, like omission of pronouns or articles. In such cases it is hardly possible to state which variant is the more original. The following readings are of interest:

1:38 *ερμηνευομενον* P5<sup>vid</sup> (very uncertain reading) together with among others S\*, against *μεθρημενευομενον* in for instance P66 P75 B.

16:18. P5, together with P66 and S\*, omits *ο λεγει*, which is found in B.

16:22. *αρει* P5 with B D\*, against *αρει* S.

16:23 *δωσει υμιν εν τω ονοματι μου* P5<sup>vid</sup> together with S B. The text of NA is *εν τω ονοματι μου δωσει υμιν*, supported by P22<sup>vid</sup> A D. However, a different word order is of no great importance.

Aland thinks that P5 is a witness to an earlier and more original text, but that errors have crept into the text and that the scribe has taken liberties making small changes that do not affect the sense. I do not agree, because P5 seems to follow different branches of the tradition in the few cases where there are telling agreements with other documents.

P6 contains text from around 15 verses of the tenth and eleventh chapters. Apart from a very commonplace erroneous ending of a type common to other documents, it almost exclusively presents singular readings. It cannot be ascribed to any tradition, nor is it a follower of a hypothetical original text.

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66 P. 23: "basiert eindeutig auf dem Ausgangstext."

P45. This papyrus, the most extensive of those commented on, contains text from the fourth, fifth, tenth and eleventh chapters. Aland indicates seven places where P45 has the same reading as other witnesses but does not comment on them, because she thinks that we cannot know whether the variant has come from some existent tradition or has appeared on its own in P45. In my opinion, we should rather consider a connection with other documents, the very reason being that these variants are found elsewhere. Concerning P90, Aland thinks that the scribe might have followed variants existing in other witnesses. When it comes to some passages in P45, Aland<sup>67</sup> states that P45 with other documents deviates from a supposed initial text, but thinks that these variants did already exist and were copied in P45. It is hard to see why these passages are judged in another way than those in P90. We find that in three cases out of four, P45 has the same readings as A and Θ.

According to Aland, the scribe shows both judgement and what we may perhaps call an ability to see the big picture (*Grosszügigkeit* in the original text) in his work: He has a good view of what the text says and keeps to the meaning but may do away with what is superfluous. This does not fit in quite well with another opinion of Aland's, that the scribe may use omissions to make a complicated sentence clearer, which is a more advanced way of handling a text, and that he also adapts the text to the style and usage of John. To me, this means that the scribe works consciously on the text, or perhaps rather that he has before him a text where someone has already introduced changes. Passages where we can suspect intentional changes are:

10:8 παντες οσοι ηλθον κλεπται εισιν. Thus P45 like most of the witnesses has παντες, omitted by D and a Latin manuscript, but omits, with many witnesses, προ εμου. These words are in some documents placed before, in some others after, ηλθον. Aland thinks that the words are omitted because of the cursory way (*Flüchtigkeit*) of the scribe, but we must reckon with an intentional change. The words are puzzling. They might have been omitted in order not to condemn with one stroke of the pen all venerable characters of the Old Testament.<sup>68</sup>

10:34–35.<sup>69</sup> Before v. 34 εν τω νομω there is the addition εν τη γραφη, in v. 35 προς ους ο λογος του θεου εγενετο and η γραφη are omitted. This looks like the result of strong and decided interference, but both omis-

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67 P. 29.

68 Metzger, p. 195, Ehrman, p. 240. Cf. also above, p. 34.

69 For comments on this passage, Aland (p. 28) refers to another article which I cannot find; the comments above are my own.

sions are also found in Cyprian, which makes it very possible that P45 preserves an old text.

11:25 εγω ειμι η αναστασις, omitting και η ζωη. The reason why the words are omitted may be that since the text before only speaks about resurrection, the text of P45 has been adapted accordingly. But on the other hand, they were perhaps added in the main tradition, because immediately afterwards Jesus speaks about eternal life, and that is a central idea. So maybe P45 is right, together with Cyprian and the Sinaitic Syriac translation, against P66 P75 S B. Aland thinks that the text has been adapted to the context, which would mean an intentional change that can hardly be ascribed to the scribe's focus on the big picture (Grosszügigkeit).

In some cases<sup>70</sup> Aland accepts P45 as a text which has been subject to change, but I do not think that these passages say very much. In 10:38 πιστευσατε or πιστευσετε or πιστευετε are examples of readings which may emerge anywhere, the historical present in 11:29 εγειρεται and ερχεται contrasting to ηγερθη and ηρχετο is hardly of great importance, even if it is a fact that the story in this context has past tenses, and that the change to the present tense is rather striking. Likewise we do not know which reading is original in 11:54 διετριβεν or εμεινεν. Did someone prefer a less frequent word, that is διετριβεν, or did someone change the more unusual word into the more familiar εμεινεν? Who can tell?

Summarizing, I think that it is clear that P45 has a text which has been interfered with. Aland too concedes<sup>71</sup> that the papyrus does not present a pure *A*-text.

P106. This document has text from 14 verses of Jn 1. The most important disagreement with the supposed initial text is 1:34 ο εκλεκτος του θεου instead of the common ο υιος του θεου. There are different opinions about which reading to prefer. This is not the place to enter into this discussion, but it may be permitted to present as an example of the different opinions that Aland<sup>72</sup> speaks for υιος, Ehrman<sup>73</sup> for εκλεκτος. Cf. for the same passage above p. 20.

P107. Very little text. The interesting reading is 17:11. The usual text is ουκει ειμι εν τω κοσμω και αυτοι (variants: ουτοι or omission) εν τω κοσμω εισιν καγω προς σε ερχομαι. After ερχομαι P107, with D and a

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70 P. 29.

71 P. 32.

72 P. 33 f.

73 P. 69 f.

few Latin manuscripts, has ουκετι ειμι εν τω κοσμω και εν τω κοσμω ειμι. I do not think it necessary to have an opinion on the possible theological intention of the longer text. It is probably just a dittography, the context making it necessary to change εισιν into ειμι, the original text being one without αυτοι or ουτοι. It is important that this papyrus has an example of a deviation with an impact on D and the Latin tradition.

P108. Text from seven verses of Jn 17 and 18, nothing of special interest.

P109. Text from six verses in Jn 21. The variant 21:18 is about endings; plural instead of the usual singular in αλλοι [ζωσουσιν και οισ]ουσιν σε. This is not much; it is enough that αλλος becomes αλλοι, then the endings will automatically be changed. Besides, the reconstruction is not certain.

### **Aland (2002)**

Aland (2002) takes up 15 smaller fragments. Some of them contain texts of the Gospel of John and are the same as those treated in Aland (2003). Here too she starts from an initial text which she thinks can be established. In this context, the important conclusion<sup>74</sup> is that if the fragments diverge from this hypothetical text, it is a case of unintentional mistakes (Versehen) in the papyri. It may be astonishing that Aland thinks this to be true, not only about singular readings but also if there are passages harmonizing with other Gospels and if there are readings diverging from the initial text also in other witnesses. My objection is as above that harmonizing could well point to somebody changing the text on purpose, although it must be said that well-known passages may have influenced the scribe unintentionally. Further, parallels in other documents may appear independently but may also point to the text being influenced from these other documents.

In her General Conclusion (Gesamtfazit)<sup>75</sup>, Aland divides these 15 fragments into three groups: One consists of P110 and P112, which represent a 'free' tradition with many readings diverging from the hypothetical initial text, another of P101 and P107, representing a more 'normal' tradition with a reasonable number of deviations, and the third group includes the remaining fragments. They represent a 'firm' tradition, one that is close to the initial text. These eleven seem to be a secure enough majority supporting the opinion that there was early on a firm tradition

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<sup>74</sup> P. 12

<sup>75</sup> P. 12.

passed on to the oldest manuscripts. Studying these fragments as they are treated by Aland, we shall see if her opinion of them stands the test. P110 and P112 do not enter, since I agree with Aland that they represent a text that has been interfered with. In the following, I do not consider passages where the reading of the papyrus may well be unintentional.

P77. Text from 10 verses of Mt 23. In my opinion, the only passage of interest is Mt 23:38, where we read in NA ἴδου αφιεται υμιν ο οικος υμων ερημος. Aland prefers to read ερημος in P77, but it is very uncertain if the word is there or not.<sup>76</sup> It is generally well attested, but missing in B among others. It is possible that it is a later insertion, brought on by passages in the Septuagint about the destruction of Jerusalem: Isa. 64:9 (Vulgata 64:10), Tob. 14:4, and above all Jer. 22:5 εις ερημωσιν εσται ο οικος ουτος, note ο οικος ουτος. We can draw no conclusion from P77.

P108. See p. 53. No variants of interest.

P106. See p. 52. In Jn 1:34, P106 might diverge from the supposed initial text.

P104. Text from 5–6 verses of Mt 21. The interesting reading would be the omission of v. 44, but in the fragmentary papyrus it is far from certain that the verse really was missing.

P107. See p. 52. Evidently there is an insertion in Jn 17:11, which is probably the result of a dittography. Aland ascribes this papyrus to the ‘normal’ type of tradition, not diverging too much from the hypothetical initial text, but I think that such an important deviation within a very short text rather shows that variants occur early.

P105. Text from seven verses in Mt 27 and 28. The only important reading in this context is the variants 28:2 απεκύλισεν τον λιθον with or without the additions απὸ τῆς θύρας or απὸ τῆς θύρας τοῦ μνημείου, but here the papyrus has a lacuna.

P109. See p. 53.

P111. Text from five verses of Lk 17. In Lk 17:22, the dominant reading is οτε επιθυμησετε. P111, together with D and *f*<sup>3</sup>, has του επιθυμησαι

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<sup>76</sup> Cf. papyri 2683 and 4405 in *The Oxyrhynchus Papyri*, vols. 34 and 64. The only sign of ερημος is the last letter, which is not read with certainty, and there is not space enough for οίκος ὑμῶν ερημος.

υμας. Aland thinks that this reading is coincidental where it is attested, which does not seem very probable to me. We should rather consider it a deviation from the firm tradition which Aland presupposes. In 17:12 απηνητησαν instead of υπηνητησαν of many witnesses does not say very much, since a confusion between the words is frequent.<sup>77</sup> On the whole, one cannot agree with Aland that P111 offers a firm tradition with its origin in a B-text.

P102, P103, P113, P114 have too little text to be of interest in this context.

P101. Text from eight verses in Mt 3 and 4. There is one reading of interest: In Mt 3:11 the papyrus does not read οπισω μου in front of ερχομενος. This could be an adaptation after Lk 3:16, but it could also be a rare variant found in a couple of Latin manuscripts, in Sahidic manuscripts and in Cyprian. Another rare reading is 3:16 ως περιστ[εραν instead of ωσει, which could be a harmonization after Lk 3:22 and Mk 1:10. Aland thinks that the papyrus does not belong to a special type of text, and that the copyist has rather tried to follow the hypothetical initial text. I think that such a text, with many diverging readings of a common type, probably caused by negligence, and readings which may be the result of harmonization, stands alone, and that we can say nothing more about it.

### **Aland (2006)**

Aland (2006) demonstrates in a convincing way that Papyrus Bodmer Vff, containing among other texts the Letter of Jude and the two Letters of Peter (in the context of the New Testament these texts are called P72) has been put together for the purpose of rejecting heresies, vindicating Christ as God and Redeemer, consoling suffering Christians. She takes up the following passages of P72 where the text seems to confirm the idea of Jesus as God.

2 Pt 1:20. The text of P72 is πασα προφητεια και γραφη (ιδιας επιλυσεως ου γινεται) where the other witnesses have προφητεια γραφης or γραφη προφητειας. Aland's interpretation is that the passage places the prophecies of the Old Testament and the text of New Testament (γραφη) on the same level, both inspired by God. This is, I think, not at all certain. Even

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77 Cf. Bauer.

though γραφή at this time may refer to the New Testament, it is by no means evident that it does here.

Jd. 5. P72 gives the text θεὸς χριστὸς λαὸν ἐκ γῆς αἰγύπτου σώσας, where the other traditions have (ὁ) θεός or (ὁ) κύριος or (ὁ) ἰησοῦς or κύριος ἰησοῦς instead of θεὸς χριστὸς of P72. We may observe that A and B have ἰησοῦς, which indicates that the tendency to emphasize the divine power of Jesus has been at work early.

1 Pt 5:1. P72 has τῶν τοῦ θεοῦ παθημάτων instead of τῶν τοῦ χριστοῦ παθημάτων of all the other traditions.

2 Pt 1:2. P72 reads εἰρήνη πληθυνθείη ἐν ἐπιγνώσει τοῦ θεοῦ ἰησοῦ, elsewhere always καὶ ἰησοῦ. This may be of less importance, since small words often go amiss.

Although Aland is somewhat vague and thinks that there could also be other explanations of these variants, she is rather inclined to think that these passages proclaim the divine nature of Jesus Christ. Also the composition of the whole document of which P72 is a part speaks for this theory. I think that she is right, and I would like to emphasize that theological thinking has been at work early and interfered with the text intentionally.

### Summary

Aland (2003). Aland thinks that the papyri she comments on give a good idea of a text that on the whole is firmly established in the third and fourth centuries and can be reconstructed. This initial text, called *A*, is the text transferred to the great uncials. According to her, we can only find a small number of important variants, in fact seven<sup>78</sup>. Usually she wants to explain readings diverging from the supposed initial text *A* as due to negligence on the part of the copyist or as corrections of a simple kind made in the process of copying by a copyist who is intent on presenting the content but does not care very much about reproducing the text literally. Copyist copy, says Aland, they do not interpret, they do not invent readings. This is very probable, but what kind of text did they have before their eyes? Were there already corrections, additions, omissions? In my opinion, it is very probable that a reading was already in the exemplar and was not caused by negligence if we find that same

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78 P. 36.



reading elsewhere. Aland does not deny this<sup>79</sup> but emphasizes that the copyist's job is to copy. I find that intentional interference appears in more than twice Aland's number of such readings. Are Aland's seven variants a good enough support for her opinion that there has been little interference in a stable text, or is it overthrown by perhaps twice that number? It is a matter of judgement. My opinion is that we must feel much doubt about the idea that on the whole these documents present an old text which can be reconstructed.

It also seems to me that Aland is not very sure about how scribes work. She would like to think that they copy meticulously, but can also admit that they change the text in order to make it easier to read, but if they do, it usually concerns small matters. Aland also imagines that copyists may make changes in a semi-conscious way (*halbbewusst*, see p. 89). I think we must keep to an idea about the copyist either just copying what he sees before him, meticulously or negligently, or that he makes changes intentionally. The notion of 'semi-consciously' makes it too easy to explain deviations as accidental or unintentional. Aland does not at all mention that an interested and knowledgeable reader may have influenced the text, comparing texts with similar passages, adding or omitting in the text to make it run better. Such changes may later have been introduced by a scribe into the text, which afterwards shows no outer sign of interference.

Aland (2002) is in the same vein. Aland is a champion of a firm text, but in my opinion, the support is much less than she thinks. Aland & Aland, *The Text of the New Testament*<sup>80</sup> emphasizes that the 'strict' text has a strong position among the papyri, but the passages which Aland takes up in her articles give no support to that standpoint.

Aland (2006) shows an early text subjected to intentional interference indicating theological thinking about the nature of Jesus Christ.

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79 P. 36.

80 P. 93 ff.

### **Chapter 3. Editing the New Testament: The Coherence-Based Genealogical Method (CBGM) and the Editio Critica Maior, 2nd Revised Edition (ECM2)**

#### **The CBGM**

Gerd Mink has presented a method, the Coherence-Based Genealogical Method (CBGM), which aims at illuminating the way the text of the New Testament in Greek has come to us and making it possible to present a better text. The method is based on passages with variants where it seems possible to show a relationship between these variants; some of them are taken to be original, others to derive from the original ones. Thus, local stemmata can be built and from them more comprehensive stemmata. So far nothing new, but we shall see that the method is different from a traditional way of handling a tradition. The CBGM is in fact very important, because it has been accepted by the Institute for New Testament Textual Research (Institut für Neutestamentliche Textforschung) at Münster as fundamental for editing the New Testament in the very ambitious *Editio Critica Maior* in the second, revised edition, ECM2. I quote the editors<sup>81</sup>: “Based on preliminary studies on method and related tests for which the 98 test passage collations for “Text und Textwert” of the Catholic Letters were used, the Coherence-Based Genealogical Method (CBGM) was developed once the entire material for the Letter of James became available. On the basis of full collations of all Greek manuscript texts used in the edition and a philological assessment of their variants, this method aims at developing an overview of the relationships between all witnesses<sup>82</sup> involved; this overview yields a set of genealogical statements that are plausible at each variant passage. A comprehensive picture emerges which is based on all text-critical decisions made by the editors. This picture enables the editors to challenge their own approach.” I also take the liberty to translate Barbara Aland freely<sup>83</sup>: “We do not at all abandon the so-called inner criteria<sup>84</sup>, but these are subjective and can often be turned the other way around. Accordingly, we at the Institute for New Testament Textual Research work at strengthening the outer criteria, establishing the knowledge of

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81 ECM2, p. 31\*.

82 Mink and ECM2 concentrate on the text and speak about ‘textual flow’ between ‘witnesses’, the ‘witness’ being the text, not the document. This is in my opinion a mistaken idea, cf. p. 69.

83 Aland (2006), p. 304.

84 By ‘inner criteria’ I understand traditional text-critical methods using knowledge of language and making an effort to understand the passage in its context. I certainly do not think that Aland, Mink and followers are deficient in these respects, even if we may sometimes come to different results.

the value of witnesses *per se* and to take all witnesses into account in an objective and rational way. My successor Holger Strutwolf continues this work, which is based on the coherence-based genealogical method of Gerd Mink. The aim is to come as close as possible to the initial text or rather to the text which is at the beginning of the tradition.” When the editors speak about the value of witnesses, I take this to mean that the general value of a witness is of importance for establishing the best possible reading<sup>85</sup> of a special passage. In my opinion, this is not the case. A reading is better than another reading, but the fact that a witness generally speaking is better than another does not mean that it is better in the special case. I hope this will be made clear in the following.

Considering the enormous importance of the New Testament text, it may be worthwhile to discuss the principles of the CBGM and how it is handled in examples presented. Exactly because it is important, I am sure that it will have an impact on editorial work in the future.<sup>86</sup> Therefore I find it necessary to look into the method at some length, not picking some questionable points here and there but trying to discuss both the fundamentals of the method and the way singular passages are treated.

Studying the CBGM, my starting-point has been a recent work by Gerd Mink, Mink (2011), which gives a good idea and overview of the theory and presents many examples: *Contamination, Coherence, and Coincidence in Textual Transmission: The Coherence-Based Genealogical Method (CBGM) as a Complement and Corrective to Existing Approaches*. Since I do not see that the main features of the method have changed over the years, I shall also refer to earlier studies by Mink, especially where I find that a problem is presented in a more instructive way.<sup>87</sup> Aland (2011) is a short and instructive overview of the method where some points are very clearly expressed. Barbara Aland’s position as to papyri and early manuscripts is also of interest for the tradition of the text; she takes a strong view on copyists trying to be exact and changing little.<sup>88</sup>

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85 To me, ‘reading’ and ‘variant’ mean the same thing, the different forms which a passage has. See also p. 64 f. about ‘place of variation’.

86 It has started. Recently Parker, p. 84 ff., gives an overview of the method, which is accepted without any criticism of its basic assumptions.

87 Mink (1993) gives a short and clear overview of the CBGM. Mink (2004) is, like Mink (2011), a detailed exposition, often with the same chapter-headings as Mink (2011). There is an extensive and pedagogical *Introductory Presentation* online: [http://www.uni-muenster.de/INTF/cbgi\\_presentation/download.html](http://www.uni-muenster.de/INTF/cbgi_presentation/download.html), but I think that the presentations on paper are instructive enough.

88 Cf. chap. 2.

If the following exposition is meandering and sometimes repetitive, the fault does not lie with Mink (2011), which I follow roughly; added to human weaknesses the reason is that the problems are closely connected with one another and that one question gives rise to a discussion about another.

### **A traditional view**

#### *Coherence, genealogy, relationship*

Genealogical<sup>89</sup> relationship or genealogical coherence, terms very often used by Mink, are traditionally thought to be shown by readings which are different and where one reading can be shown to be inferior to the other, being for instance a *lectio facilior*, in any case a change from the initial reading<sup>90</sup>. There is of course a sort of a genealogical relationship between all documents, because usually they all go back to a text produced by an author, but mostly the term genealogical is used about a rather close relationship, not for instance about the relationship between 03<sup>91</sup> and a Byzantine manuscript. On one hand, likenesses between readings play a minor role, because identical readings may exist between documents which are generally very different and do not seem to be closely related; one reason is that such readings have never changed from the initial text, another is that they can appear coincidentally in different documents, being mostly rather unimportant changes such as omission or insertion of particles or a change of word order or the result of a homoeoteleuton. On the other hand, however, some identical readings supposed not to be initial and not to appear by coincidence are considered very important, because they are supposed to be the result of contamination or interpolation<sup>92</sup> and thus show a relationship between the documents. I adhere to these notions, which among other things means that I find a relationship, although not a close one, between a very early document and a very late one, if they present the same non-initial reading or readings, even if they are generally rather different. In fact, the relationship exists between readings, usually not between the whole texts of the documents, because one and the same document may carry readings which show relationship to various other documents. We shall see that to the CBGM, likenesses play an important role as a starting-

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89 About the notion 'genealogy', see p. 72.

90 About 'initial text', see p. 68.

91 ECM2 and Mink always use the numbers of the manuscripts, 01, 02 etc.

92 I use 'contamination' and 'interpolation' in the same sense.

point for stating relationship, and that whole texts, or witnesses as they are called, are supposed to show a flow between them.

Generally speaking, according to a traditional view agreements give a hint at genealogical relationship, but only a hint. They do not really show or prove such relationship, since the agreements may result from causes mentioned above. On the other hand, poor general agreement does not necessarily mean lacking relationship between documents.

### *Prior and posterior readings*

If readings are different, evidently one of them must be original and right, the other secondary and wrong, in the terminology of the CBGM prior and posterior. Trying to decide which reading is posterior and making stemmata accordingly is a method which we associate with the names of Karl Lachmann and Paul Maas.<sup>93</sup> The method has its shortcomings. All is well and good if a scribe used only one exemplar<sup>94</sup> and if the same readings never occurred coincidentally in various documents. But the scribe does not always keep to one document, and identical readings may occur coincidentally. Accordingly, editors find themselves in difficulties.

### *Contamination*

It is difficult or impossible to follow interpolated variants backwards to where they come from. Therefore we must listen to Maas's concluding remark in his famous *Textkritik*: "Gegen die Kontamination ist noch kein Kraut gewachsen", there is yet no remedy against contamination.

Contamination occurs, especially in a rich tradition, and no tradition is richer than that of the New Testament. Mink is of course well aware of this and strongly stresses the point.<sup>95</sup> We shall see that at the same time he works on circumscribing its role. If contamination potentially is omnipresent, the right or prior and the wrong or posterior variant may be found everywhere in the tradition. The prior variant may be found in documents because it has never been changed, or it may be interpolated into documents where it has replaced another reading. The posterior variant must of course always be the result of a change, which may have been interpolated into one document, or into many, maybe into all. If the same posterior reading is found in documents which are otherwise rather

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93 Timpanaro discusses similar ideas of scholars, earlier than Lachmann or his contemporaries, among them Johann Albrecht Bengel and Johan Nicolai Madvig.

94 Elliott & Moir, p. 23: There are few *codices descripti* in the tradition of the New Testament.

95 See titles and introductions in Mink (2004) and Mink (2011).

different, the question is whether these readings are coincidental or the result of contamination. We shall see many times that the CBGM prefers coincidence (multiple emergence), in my opinion too much so.

*Unintentional (coincidental) and intentional readings*

It may be necessary to give some thought to the term ‘coincidental’, used by Mink. I take ‘coincidental’, to mean a reading appearing in more than one document which are not influenced by another, at least not in this passage. He also uses the term ‘multiple emergence’ or ‘multiple genesis’, which I take to mean the same thing. We may often wonder whether a change is coincidental or not. If for instance a phrase is initially an asyndeton, different scribes may, each on his own account, add the same connecting particle. Perhaps we should not call that coincidental, since there is an intention behind it, but the main thing is that there is no connection between such changes. Also, if a passage is corrupted or difficult to understand, different readers or scribes may change the text the same way, making it easier to follow. Is that coincidental or intentional? The change is intentional, the result is coincidental. The main thing is that such changes have nothing to do with one another. I reckon them as coincidental.

I have in an article<sup>96</sup> tried to show relationship or lack of relationship between the four oldest documents of the text of the Gospel of John. There I took for granted that 1) some variants were coincidental and did not show any relationship, 2) that if they *could* be coincidental, they *must* be counted as such, otherwise the result from clear cases of interdependence, few indeed among the four documents, would be obscured.

I shall more than once accuse Mink of taking for granted what should be proved. Do I fall myself into the same trap? Do I assume that some readings are coincidental, and when I find them in the text, I state that they are? Not quite so, I hope. I say that if they *can* be coincidental, they *must* be counted as such. Otherwise, we shall have a row of likenesses, not knowing whether they are coincidental or not, and all kinds of statistics based upon those agreements will break down. I hope that the lists presented above<sup>97</sup> are not based on mere assumptions but come close to facts. They do not come from statistics, which would be impossible, but from looking into rather many apparatuses, finding that such variants are very common. It may also help to reflect upon what mistakes are com-

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96 Alexanderson (2013). This article is now revised and occurs as chap. 1 in this work.

97 For unintentional or coincidental or accidental readings, see p. 16, for intentional changes, see p. 13.

mitted by us human beings. This is very vague, deplorably so compared with statistics, which have an air of reliability around them, at least at first sight.

### *Summary*

To resume a traditional view shortly: Likeness between supposed initial readings is not important, difference is important but only as far as it is not coincidental.

### **Discussing some basic notions of the CBGM**

I think that it is appropriate to start with statistics, since they are fundamental to the CBGM and omnipresent. Some other basic notions<sup>98</sup> of the CBGM will be discussed afterwards.

### *Statistics*

The CBGM is based on comparisons between documents which are alike to some percentage, maybe 95%, maybe 80%. Every agreement has the value 1, every disagreement also the value 1. Can this be a reasonable way of judging? When passages are alike, they may be so for different reasons: They may originate in the initial text, never being changed, they may be identical interpolations, because someone once had an idea of the text and made a change which had some success, or they may be alike coincidentally, because a change seemed natural and reasonable to more than one scribe or reader, e.g. the insertion of a καί. Passages which disagree cannot have the same value when it comes to pointing out the fate of the text, since some are coincidental, others are not, some have bearing on the meaning, others not or very little. We do not know which is which, and therefore we must pay little heed to such readings as may be coincidental and also have little bearing on the meaning. It is an important idea of Mink's that so-called 'minor agreements'<sup>99</sup> are as important as any other readings<sup>100</sup>: "... in the context of the CBGM, each variant shared by highly similar witnesses - regardless of whether the variant appears significant - counts as an instant supporting their affinity, their genealogical coherence. In such witnesses, minor agreements are not considered coincidental."

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98 See Mink (2011), chapter 2. *A Brief Explanation of Basic Terms*, chapter 5, *The Need for Novel Methodological Considerations*, and chapter 6, *Basic Assumptions*.

99 For 'minor agreements', cf. p. 79.

100 Mink (2011), p. 152.

Mink states that the genealogical correlation of a manuscript should be explored on the basis of *all* its variants.<sup>101</sup> He continues: “For a scribe, there were no “significant” or “insignificant” variants in terms of the textual history as studied by modern text critics.” This is no doubt true, but we know that scribes were very prone to making some mistakes, those which we see all the time in the apparatuses. That is exactly what makes the great difference between different readings. Let us look at Jas 1:23/32–36. The variants are τῆς γενέσεως αὐτοῦ, αὐτοῦ τῆς γενέσεως, τῆς γεννέσεως αὐτοῦ, τῆς γνώσεως αὐτοῦ. The disagreement between γενέσεως and γεννέσεως<sup>102</sup> and the change in word order are extremely frequent variants and do not, at least in my opinion, connect manuscripts, whereas the difference between γενέσεως and γνώσεως is a variant which by no means appears almost automatically, as the others do, but is significant and connects those manuscripts which have it. It is therefore a much more important variant, but in the statistics of the CBGM all four variants have the same value.

But since we cannot put different values to every single variant, let us instead count them. That is what the CBGM does. How then are they counted? What is a ‘reading’, a ‘variant’, a ‘passage’, a ‘place of variation’? From a practical point of view, I think that these terms all mean the same thing, and I think they do so to the CBGM. There is no need to go into definitions, the problem is how they are counted. *Text und Textwert* takes up 98 passages (‘Teststellen’) concerning the Catholic Letters, chosen because they are supposed not to have emerged coincidentally but to show a relationship to the exemplar (Vorlage) they derive from and to belong to different types of the tradition.<sup>103</sup> The CBGM lists 3,046<sup>104</sup> variant passages which are compared and which are the basis of the statistics. The Letter of James contains 761 variants<sup>105</sup>; obviously

101 Mink (2011), p. 145. Italics by Mink. Cf. also *Introductory Presentation*, p. 305: “The pre-genealogical and genealogical data used rest upon the entire corpus of the Catholic Letters (not upon single writings).”

102 Cf. p. 111 with n. 294.

103 *Text und Textwert* 2/1, p. VII. I do not accept these 98 passages as telling examples: there are too many cases of passages that do not tell anything. There is for instance a variation of synonyms or near-synonyms, e.g. no. 15 (Jas 3:3/22–26) εἰς το πειθεσθαι/προς τ. π.; of article or no article, e.g. no. 51 (2 Pt 3:16/10–12) επιστολαις/ταις επιστολαις, where επιστολαις is preferred in *Text und Textwert*, ταις επιστολαις in ECM2; of different word order, e.g. no. 87 (3 Jn 14/8–10) σε ιδειν/ιδειν σε; of addition or no addition of αμην at the end of a Letter, e.g. no. 52 (2 Pt 3:18/45), no. 75 (1 Jn 5:21/13), no. 82 (2 Jn 13/19).

104 Mink (2011), p. 147. Of course comparisons do not start from 3,046, because there are always lacunas. The more comprehensive manuscripts are compared at around 3,000 passages.

105 ECM2, p. 31\*.



all the variants registered in the apparatus are counted for the statistics, nothing is omitted.<sup>106</sup>

There is in fact a kind of definition of ‘places of variation’ in Mink (2004)<sup>107</sup>: “Places of variation are places in the text where variants appear (*sic*). At least two different variants occur in a place of variation; the maximum in James is 24 variants. A place of variation may comprise more than one word, but it can also be the space between words.<sup>108</sup> Ideally, it covers a logical unit of variation. This means that mutually interdependent changes to a text should belong to one unit of variation (e.g. if a subject and correspondingly the predicate are put in the singular). A unit of variation can also be postulated when a group of words presumably belonged together in a copyist’s view (e.g. if a word group consisting of article/particle/noun shows changes in different combinations for the article/noun and for the particle). Sometimes, very pragmatic considerations might be adduced to determine a unit of variation, so as to enable the comparison of all texts at a certain place.” We are thus supposed to know how a copyist considered the text, but of that we know nothing. Did he laboriously work out one word after another in a difficult *scriptio continua*, did he put together some words into a context before writing them down, or did he hear someone else dictate the text to him, combining the words more or less correctly? We are also supposed to establish ‘logical units’ and to accept a ‘very pragmatic’ way of handling the text. In short: This is all arbitrary, a ‘place of variation’, a reading, a variant, a passage can be anything.

Looking at the apparatus of ECM2 we observe that of course different words in the same place count as a variant, for instance φέροντα instead of φοροῦντα or one particle instead of another or the omission of the particle. Article or the omission of the article is counted as a variant, as is also negation or omission of negation. If two words change places, it is also evident that we have a variant to count, for instance ἀνέμων σκληρῶν or σκληρῶν ἀνέμων. It becomes much more difficult when we find longer sequences where it is not possible to compare word by word. Let us look at 2 Pt 1:4/8–18 τίμια καὶ μέγιστα ἡμῖν ἐπαγγέλματα δεδώρηται, with 15 variants. This is regarded as one passage or variant between documents and is counted as one disagreement. There are in

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106 I did not arrive by manual counting at exactly 761, but close to it! We should believe in the computer.

107 P. 27.

108 Which means that for instance in Jas 1:2 between word no. 2 and word no. 4 there is in the tradition also a word no. 3, i.e. γὰρ. Such additions between the accepted readings in the primary line have uneven numbers.

fact several disagreements within this sequence, which we may well call a logical unit. There is a variation of word order between τίμια καὶ μέγιστα and μέγιστα καὶ τίμια, there is a variation between pronouns, appearing as ἡμῖν, ὑμῖν, ἡμῶν, ὑμῶν and differently placed, and the pronoun is also omitted in one witness. Thus, between for instance 01 and 03, the method counts one variant, but in fact these two manuscripts differ as to the order of the words τίμια καὶ μέγιστα and as to the place of the pronoun ἡμῖν. If we compare 01 and 02, we add the difference ἡμῖν/ὑμῖν.<sup>109</sup> Exactly such disagreements as those mentioned are in other places considered variants of their own, but in longer sequences they are ‘concealed’<sup>110</sup> and do not count. It is thus evident that variants compared are very different. But not only a sequence in the primary line<sup>111</sup> but also one word in the primary line may ‘conceal’ disagreements. In Jas 1:8/2 the primary line has ἀνὴρ, the variants are ἀνὴρ γὰρ, ὁ ἀνὴρ γὰρ, ἀνὴρ δέ. Shouldn’t we say that ἀνὴρ differs from ὁ ἀνὴρ γὰρ not in one place but in two, the longer reading presenting both the article and a particle? And if we put ὁ ἀνὴρ γὰρ in the primary line, we get a sequence, Jas 1:8/2–6.

The situation also becomes difficult if another word is inserted, perhaps in different places. Let us look at Jas 2:3/26–30: The readings are κάθου ὧδε καλῶς, κάθου καλῶς ὧδε, ὧδε κάθου καλῶς, κάθου ὧδε λαμπρῶς. If the reading κάθου καλῶς ὧδε did not exist, we would count two disagreements, one being a difference of word order, κάθου ὧδε versus ὧδε κάθου, the other καλῶς versus λαμπρῶς. But now the reading κάθου καλῶς ὧδε exists, and the place of ὧδε forces the CBGM to work with all three words and establish one variant, although there are really two. It happens time after time that the word order or a deviating or ‘extra’ word forces the method to establish long passages as variants, thus ‘concealing’ disagreements within the passage. Take for instance 1 Pt 5:13/2–12, a long passage counted as one variant. Within it we find interesting variants such as Βαβυλώνη, Ρώμη, Σίων.

Let us look at 2 Pt 3:10/48–50. The variant is οὐχ εὑρεθήσεται and εὑρεθήσεται without the negation and as a third possibility κατακαήσεται<sup>112</sup>. But we could also think that the negation and the ab-

109 2 Pt 1:4/8–18 is by far not the worst example. In Jd 5/12–20, counted as one passage, there are the differences in the place of ἄπαξ, there is τοῦτο or omission of τοῦτο, ὅτι in different places or omitted, ἡμᾶς or ὑμᾶς placed differently, we find ὁ κύριος, κύριος, ὁ ἰησοῦς, ἰησοῦς. See also below, Jas 1:19/10–12.

110 Of course I do not mean by ‘concealing’ that the method is handled with a view to mislead.

111 The primary line presents the text preferred by the editors. In the apparatus, it is indicated by *a*, the variants being *b*, *c*, etc.

112 Disregarding a few other variants meaning ‘being burned, destroyed’.

sence of negation form one important variation, and the disagreement εὔρεθήσεται versus κατακαθήσεται another, and thus count two disagreements. A negation and the absence of it are elsewhere counted as a variant, but here it is not.

If we look at 2 Jn 3/2–8, there are 11 variants. If we compare reading *a*: ἔσται μεθ' ἡμῶν χάρις with reading *b*: ἔσται δὲ μεθ' ὑμῶν χάρις we shall find two disagreements rather than the only one counted, one being the particle δὲ or the omission of it, the other the disagreement between ἡμῶν and ὑμῶν. It is also difficult to see how we should judge the reading *j*: χάρις ὑμῖν instead of the whole phrase labeled *a*. How many disagreements are there in this reading *j*, compared with *a*?

The last example shows the difficulty which arises if one reading is quite different from the others. Another case is Jas 1:27/40–44 ἄσπιλον ἑαυτὸν τηρεῖν. There are the variants ἄσπιλον, καὶ ἄσπιλον, ἀσπίλους, further ἑαυτὸν, σεαυτὸν, αὐτον, those pronouns placed in different places, also the disagreement τηρεῖν and τηρεῖτε which are placed differently. Add to this that there is a variant, ὑπερασπίζειν αὐτούς, which is not only different in form but also has another meaning. To the CBGM, these variants have the same value.

Now someone may argue that the cases of ‘concealing’ are rare. I do not think that it is possible to present reliable statistics, just because it is so difficult to judge the number of variants. As shown above, the method can handle cases where there is a word *a* instead of a word *b*, also if there are the words *ab* instead of *ba*, or if the word *a* corresponds to *xa* or *ax*, *x* standing for e.g. an article or a particle. If the variations run over a string of words, it is difficult or impossible to state how many differences there really are. Anyone can see that, just looking into the apparatus of ECM2. It is clear that the number of variants must be considerably higher than 3,046. How much higher? I hope that it has been made clear that there is no reliable way of counting. However, counting the places of variation in 10 verses at Jas 1:1–10, there are 50 of them, but in reality these places are around 57. I am being rather kind, counting for instance 1:3/10–14 as one place of variation: A manuscript may have ὑμῶν τῆς πίστεως, it may have ἡμῶν instead of ὑμῶν, it may omit ἡμῶν/ὑμῶν, it may omit τῆς πίστεως; starting from τῆς πίστεως, these words can have an addition, they can stand alone, they can be omitted, and all in one place. Counting that way, there is no ‘concealment’. Examples of types of ‘concealment’ are given above. In 2 Pt 2:5–14, also 10 verses, there are 57 places of variation, but by my counting around 69.

‘Concealing’ disagreements means that all statistics break down, and thereby the method, which builds on statistics. I thought at first that the

method could at least show similarities between documents, but in fact it cannot do so but roughly. It is not enough to count: It is fundamental that we know what we count, and that the phenomena we count are comparable.

But could we not regard these 3,046 cases as a sample and take guidance from it? I do not think that it is possible. For one thing, it is against the whole idea of the CBGM, which aims at and claims completeness. Secondly, it is not a good sample. Counting the ‘places of variation’ in 2 Jn, we find that there are around 55 such places with two different readings, somewhat more than twenty with three variants and about as many with more than three variants. As I have tried to show, the places with several variants are much more difficult to handle, and they are too many to be left aside.

*Text, initial text, witness*

I do not think that a discussion about what is an archetype and what is an initial text is essential when it comes to the practical handling of ECM2 and the CBGM. We may keep to their terminology, which speaks about the ‘initial text of the tradition’.<sup>113</sup> This text is perhaps not the very text of the author, but it is supposed to be the one from which the entire tradition originates. As far as I know, the bulk of the texts is the same wherever we find a document containing a large part of a book.

However, we may ask how far an initial text was the same everywhere. Certainly it was not exactly the same. The text or texts we are talking about were read and copied in different Christian congregations. Did they receive additions, maybe eliminations, in the course of time? We cannot believe that the texts were identical in e.g. Alexandria and in Ephesus, considering the differences we can identify even in the earliest documents.<sup>114</sup> One initial text may have existed, but it certainly underwent changes. Some of them may have remained in most or all documents, others may have disappeared. The ending of the Gospel of Mark must have been felt unsatisfactory, and other endings were added, but they did not reach all documents, far from it. The pericope in John 7:53–8:11 about the adulteress does not appear in the oldest documents. Other additions and changes, greater or lesser, may have been more lucky and remain unexposed because they are harder to reveal. However, concerning not the whole text but the different passages we are to some extent better equipped, because there are tools which may help us to state

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<sup>113</sup> See e.g. ECM2, p. 30\*.

<sup>114</sup> See chap. 1 and chap. 2.

which one ought to be, if not original, then at least more original than the others.<sup>115</sup> Speaking in a general way about the New Testament, we ought to be sceptical about the text as it is presented, even if elaborated with the greatest possible care, and be prepared to have less confidence in that text and greater interest in the *apparatus criticus*.

Mink and ECM2 concentrate on the text and speak about ‘textual flow’ between ‘witnesses’, the ‘witness’ being the text, not the document. I do not understand this way of reasoning. A text is an existing phenomenon. Maybe there was what we can call an initial text, and there are the different existing texts of different existing documents. There is no flow of a theoretical text between documents; there cannot be, because the texts of the documents usually point in different directions.<sup>116</sup> Some readings of document A are initial and earlier than those of document B, some are changed and later. I shall often use the word ‘witness’; it means to me the text of a document. The word is very handy, since it can be used of all kinds of documents, papyri, manuscripts, translations, etc. In fact, both Aland and Mink often use ‘witness’ in the sense of ‘document’, e.g. in Mink’s many tables showing agreements and disagreements between ‘witnesses’, i.e. documents.<sup>117</sup> Witnesses in the sense of the CBGM, whatever that sense is, do not exist.

The fundamental idea of the CBGM concerning ancestors and descendants is on the whole misleading. To simplify somewhat: Manuscript A may to 55% have a more original text than manuscript B and to 45 % a less original one. To the CBGM this means that A is the ancestor of B, that there is a flow from A to B, but the same manuscript is in fact both ancestor and descendant, both father and son.

I would not compare the development of texts to a mighty flow but would like to start from the document, comparing it in a more prosaic way to a pizza. This pizza is copied from another pizza but sometimes the cook has also put into it ingredients observed in some other pizza. As to the ‘rule of parsimony’, more than once evoked by Mink<sup>118</sup>, it is more convenient to buy all the ingredients of the pizza in one store, but it may well be necessary to turn to others, some of them perhaps situated far away. Unfortunately, we do not know how the cook works. Mink prefers to think that most ingredients are close at hand, as we shall

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115 See n. 84.

116 Cf. n. 94 about *codices descripti*.

117 Mink (2011), p. 146 makes it clear that a ‘witness’ is ‘specified’ by a Gregory-Aland number, that is the number of a document. Still this witness is not a document but a text. This is most confusing.

118 See p. 85.

see. It is in fact a fundamental idea of the CBGM that scribes use one or few exemplars, and if more than one, usually one which is like the main exemplar. A very dangerous simile is that of the sequences of variants being compared to DNA chains.<sup>119</sup> This sounds very scientific, but all mammals have two biological parents, no more, no less, whereas the document may have many parents, none of them biological.

### *The two bases of the CBGM*

As stated above<sup>120</sup> under *The CBGM*, the method is based on two assumptions. One is that of inner criteria<sup>121</sup>, the other that of the genealogy of documents. One should not gainsay the other. Aland is very clear on this point.<sup>122</sup> I translate freely: “decisions about texts can never be stated if they are contrary to the genealogical position of the witnesses which present them.” See also *ibid.* p. 60: “Decisions about texts have a frame which can be tested; they must move within that frame, which will be the result of the totality of the local stemmata which are all the time revised.” The consequence of this is: If a document which is supposed to be strongly related to other documents and consequently to be within the same frame has an unexpected reading disagreeing with the other closely related documents, then this reading is supposed to have its origin in these related documents, even if that reading does not exist in them at all; it is not supposed to come from some other document outside the frame, even if such an ‘outside’ document may present that very reading. This means reducing the role of contamination, as we shall see<sup>123</sup>, and it comes very handy when establishing a stemma, but is it true? Contradictory to this idea of keeping everything within one frame we shall find a notion of non-ancestors becoming ancestors.<sup>124</sup>

We have some knowledge of inner criteria. We have some knowledge of the language of the New Testament and of the language of documents outside these texts. We also have a good knowledge of the context of the passages and of other comparable passages and of ideas expressed in the New Testament. There is always the danger of vicious circles; we build our knowledge of the language from what we read, then we tend

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119 Mink (2011), p. 146. Another dangerous idea is that on an average (im Durchschnitt) the tradition has developed in an organic manner, so Mink (1993), p. 482. This way of thinking might in fact be behind the method. If so, it has proved fatal, cf. p. 117.

120 P. 58.

121 See n. 84.

122 Aland (2011), p. 59.

123 For examples, see e.g. pp. 98–112, *Textual flow diagrams, A practical example: Jude 15/14–16, Some examples of substemmata and textual flow.*

124 See p. 107.

to correct what we read from what we have constructed from the written texts. The same goes for comparable passages and ideas found in the New Testament; we think that they may influence each other, and then we tend to state that they do so. But our position in this respect also has some factual basis. There is a real knowledge of the language; we do not know what is 'right' or what an author ought to say, but we know what is usual and what is not. In the best of cases, it is possible to explain why we do not find the usual expression. We also have much experience of apparatuses and of the confusion between readings they often present.

The weak point of the other assumption, also called outer criteria, is of course that genealogy is very uncertain and that contamination from not very much related documents can never be ruled out. The assumption of the CBGM is not only that usually one exemplar was available to a scribe but that similar texts were also close at hand, from which they could pick other readings. Of this availability, nothing less than a pillar of the CBGM, we know very little, in fact next to nothing. Studying the method, I have not seen any attempt at placing similar documents in the same environment or originating in the same *scriptorium*. The reason is simple: It cannot be done. We know very little about where our manuscripts were before they ended up where they are now, and we know very little or nothing about where they were produced or how manuscripts were distributed, maybe over vast areas from some *scriptorium* or religious centre which was held in high regard.<sup>125</sup> If it is not evident from changes made by the same hand, we do not know whether a copyist had at his disposal one manuscript or more than one, and we do not know how he<sup>126</sup> used them.

I think that Lowden has presented the problem of manuscript production in a very clear way<sup>127</sup>: "The position is most straightforward with an 'in-house' monastic product: the abbot instructs one of his monks to produce a new copy to supply a need. The evidence of manuscripts still in the monasteries on Mt Athos in and for which they were made, as for example at Vatopedi, exemplifies this process. But the range of possible alternatives to the in-house scenario is very large: a scribe might be a lay 'professional' (possibly female) rather than a monk or priest (or nun); the commission may very well come from a layman or laywoman;

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125 There is very little or no information about where our manuscripts were produced, even where you may expect to find some if it was available, e.g. in Aland & Aland and, as far as I can see, in catalogues of manuscripts. About scribes of the papyri, see p. 87, *Scribes generally*.

126 I apologize for writing 'he', which in my text really means 'he or she', since there is evidence for female scribes. See below the quotation from Lowden.

127 Lowden, p. 465 f.

the model(s) might need to be borrowed, possibly from a distance; the book might be commissioned in one location for use in another, and so forth. Without specific information these unpredictable circumstances, vital to the act of production, remain completely opaque. It is important, therefore, that the example of the Stoudios Monastery as a centre of production in the ninth-eleventh centuries, or the well-documented (by colophon) activities of the monk Ioasaph of the Hodegon Monastery in Constantinople in the fourteenth century, is not assumed to be normative.”

The ‘concerned reader’, who tried to better his text, hardly exists to the CBGM. By ‘concerned reader’ I mean someone who reads the text with great attention and also introduces in his copy variants and explanatory readings, which later on are transmitted to other documents. He is supposed to be more interested in important passages than in small matters. Do we not all, reading the New Testament, gladly skip the question whether there should be a ὄτι or not before direct speech and instead reflect on difficult passages? Aland does not neglect the ‘concerned reader’ but tries to reduce his influence by showing that more important changes which had an impact on meaning did not become very widespread. This opinion does not stand the test very well.<sup>128</sup>

So I think that whereas the inner criteria are weak and unreliable, outer criteria based on availability do not exist. If we add to our weak knowledge the use of non-existent knowledge and think that the latter has an impact, we will make our already weak knowledge still weaker.

### *Pre-genealogical coherence, relationship*

When speaking about the tradition of a text it is usual to speak about genealogy. This gives a false impression of a strong connection, and I shall try personally to avoid it, using ‘relationship’.<sup>129</sup> It is, however, quite impossible to get around the word, since Mink uses it and it occurs all the time in the texts we discuss. We shall remember that texts are not living beings, they are stone-dead. They get life when we read them, within the reader. The result may be that the reader changes the text, intentionally or unintentionally, and so one stone-dead artefact becomes another stone-dead artefact. All documents are related to each other; they, or rather their scribes, are givers and takers of readings, but usually we do not see a direct relationship between giver and taker.

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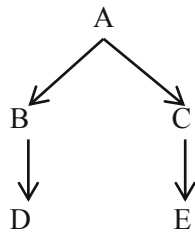
128 See p. 89 and chapters 1 and 4, especially p. 128 ff., 03, 1739 and changes in 1 Jn.

129 It would be still better to use ‘giver’ and ‘taker’.



Mink<sup>130</sup> introduces what he calls ‘pre-genealogical coherence’: “Strong pre-genealogical coherence (= a high degree of agreement) indicates a close relationship.” To my mind, pre-genealogical coherence means that documents are alike, that there may be a relationship between them, but that a relationship is not established. They are just alike. Genealogy depends on how they interact. Mink<sup>131</sup>: “Strong genealogical coherence arises between witnesses with strong pre-genealogical coherence if the text of one witness can be explained as deriving from the other at points where they differ.” We should then start from witnesses or texts that are much alike and study where they differ. We are never told how much alike they should be in order to be of interest. The average agreement of all pairs of witnesses, or ‘documents’, to the Catholic Letters is 87.6%, the minimum is 77.9%, the maximum 99.1%.<sup>132</sup> EMC2<sup>133</sup> states that an agreement of 92% or more between a document and the supposed initial text *A*<sup>134</sup> “can be seen as good or very good”. We shall mostly speak about documents that are alike to 80% or much more, which means that they are generally much the same.

We shall see, or so I hope, that the whole idea of starting from likenesses is mistaken. Let us for a start consider an artificial stemma which Mink (2003)<sup>135</sup> presents in order to explain the relations between witnesses. He starts from an initial text called *A*. In this stemma, witness 5, which has only three other witnesses between itself and the initial text *A*, is already very different from *A* (one identical reading out of five) and differs already so much from the two immediate descendants from *A* (two identical readings for one and three for the other out of five) that we may wonder whether we should presuppose a close relationship between them. We should remember that it only takes one careless scribe to change the text considerably. Let us build another, quite possible stemma (which has nothing to do with Mink’s example, discussed above):



<sup>130</sup> Mink (2011), p. 144.

<sup>131</sup> Mink (2011), p. 144, under ‘Genealogical coherence’.

<sup>132</sup> Mink (2011), p. 157, n. 25.

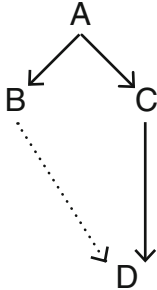
<sup>133</sup> P. 33\*.

<sup>134</sup> *A*, the supposed initial text, is not to be confused with the manuscript 02, also called *A*, the codex Alexandrinus.

<sup>135</sup> Mink (2003), p. 41 ff.

There is no interpolation in this stemma. A has the initial text, B and D are very close transcriptions of A and of one another. C is a very careless transcription of A and E is a very careless transcription of C. C and E both have a lot of so-called nonsense readings and a lot of more or less acceptable efforts to reconstitute an understandable text. As in Mink’s example, these documents are very close as to relationship (‘genealogy’) but their coherence is small.

Let us look at another example:



D is copied from C but has been interpolated from B. D and B may be more like each other than C and D. In fact D can have more prior readings than B and C and thus, automatically and in conformity with the method, be regarded as an ancestor of B and C. See below, *Counting prior and posterior readings*<sup>136</sup> and *Nodes and circular edges*<sup>137</sup>. This simple stemma is in fact a stumbling-block for the method.

Similarity and genealogical relationship may of course very often, I guess mostly, coincide, but there is no rule that this is always the case.

### Coincidence

A fundamental problem is that of ruling out those identical readings which to my understanding have nothing to do with genealogy, because they may occur anywhere. The CBGM views the problem in another way<sup>138</sup>: “A distinction between contamination, which can occur only in connection with genealogical dependence, and multiple emergence<sup>139</sup> of variants is mandatory for CBGM procedures.” As we shall see later<sup>140</sup>, it is clearly stated that to the CBGM, contamination is generally speaking

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136 P. 95.

137 P. 113.

138 Mink (2011), p. 149.

139 I take ‘multiple emergence’ to mean the same thing as ‘coincidental’, see p. 62, *Unintentional (coincidental) and intentional readings*.

140 Pp. 75, 92 f.

only possible between fairly closely related witnesses. We shall never find this statement confirmed by evidence.

Let us look at the constructed example mentioned above, taken from Mink (2003). Mink lets the original reading  $x$  be changed into  $y$  by witness 1, an immediate descendant of  $A$ , but then witness 3, a direct descendant of witness 1, changes back  $y$  into  $x$ . This  $x$  is preserved by the other witnesses. Now Mink thinks that the reading  $x$  has two origins. But in real life, why should it? Why has not witness 3 taken over a reading found in one of the other witnesses? How can we prove that it has not? If we somehow knew that for geographical or historical or religious reasons a direct relationship between them was impossible, then we could talk about an independent, coincidental origin of the same reading. But such reasons hardly exist or can hardly be proved to exist. Of course we should not deny that the same variant may emerge more than once, but we should not try to avoid the idea of contamination, but for the CBGM, it is downright stated: “Low pre-genealogical coherence within an attestation implies multiple coincidental emergence of the variant.”<sup>141</sup> I think that we can see here a tendency which I shall comment on later<sup>142</sup>: To ‘keep it in the family’, that is to avoid the idea that contamination from afar may occur. That would disturb and complicate the stemmata, which Mink likes to keep as uncomplicated as possible, following a most questionable ‘rule of parsimony’, which too will be discussed later.<sup>143</sup>

The stemmata in Mink (2003) were designed for our better understanding, but I think that they make us question the very foundations of the method. Genealogy and agreement are two unrelated things, and we cannot start from one to arrive at the other. One manuscript can derive from another without being very similar, two manuscripts may be very much alike but have nothing to do with one another directly. Byzantine manuscripts may offer very much the same text, a ‘textus receptus’, without being directly connected genealogically with each other. The work of Colwell<sup>144</sup> is illuminating: Each of the three papyri he discusses shows that a great number of disagreements have arisen early, although they are the result of a rather short tradition.

### *Prior and posterior readings generally*

The idea of genealogical coherence builds on the presupposition that it is possible to state, if not always, at least in sufficiently many cases,

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141 *Introductory Presentation*, p. 146.

142 P. 82, *Reassessments*.

143 P. 85, *The ‘rule of parsimony’*.

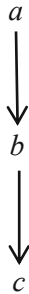
144 Cf. p. 87.

that a reading (the posterior), can be explained by another reading (the prior). I think we all agree, but anyone knows the enormous difficulties which meet us trying to judge prior and posterior readings. In the discussion we shall continually come back to this problem. Following à la Maas only very clear errors in the transmission, you will find clearer lines of tradition, but in a much contaminated text like that of the New Testament, this path will lead into an impenetrable jungle. Mink tries to take a firm grip on the whole tradition, which is in itself most laudable. Assessing prior and posterior readings is fundamental to the method.<sup>145</sup>

*Some practical examples of prior and posterior readings*

We shall have a look at Mink’s examples presented in chapter 9, *Divergence between Variants as Indicator of Genealogical Relatedness of Witnesses*<sup>146</sup>. It will of course prove nothing strictly, but may give food for some thought. It will, or so I hope, show how difficult it is to state which reading precedes the other or the others. The method depends upon correct decisions in this respect.

The first example is taken from Jas 3:6/17 (καὶ ἡ γλῶσσα πῦρ. ὁ κόσμος τῆς ἀδικίας) ἡ γλῶσσα (καθίσταται ἐν τοῖς μέλεσιν ἡμῶν). The variants for the second ἡ γλῶσσα are: *a*: ἡ γλῶσσα, without addition, *b*: οὕτως ἡ γλῶσσα, *c*: οὕτως καὶ ἡ γλῶσσα. Mink thinks that *a* is the prior reading and designs a local stemma where *b* develops out of *a* and *c* out of *b*:



This may well be correct, but it is not at all certain, one reason being that the right punctuation is far from evident. We could also punctuate strongly after ἀδικίας and start another sentence from οὕτως καὶ; however, a scribe or a reader may think that the sentence starts with ὁ κόσμος and reject οὕτως καὶ. Further, it is not clear that the reading οὕτως is more original than οὕτως καὶ: a καὶ, rather superfluous to us, is

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<sup>145</sup> Mink (2011), p. 142.

<sup>146</sup> Mink (2011), p. 159.

often added in comparisons, and this is a sort of comparison: The tongue is a dangerous thing and is compared to fire.

The next example is Jas 4:2/30–32 (ἐπιθυμεῖτε καὶ οὐκ ἔχετε, φονεύετε καὶ ζηλοῦτε καὶ οὐ δύνασθε ἐπιτυχεῖν, μάχεσθε καὶ πολεμεῖτε,) οὐκ ἔχετε (διὰ τὸ μὴ αἰτεῖσθαι ὑμᾶς.) The variants before διὰ are *a*: οὐκ ἔχετε, *b*: καὶ οὐκ ἔχετε, *c*: οὐκ ἔχετε δέ, *d*: καὶ οὐ δύνασθε. Mink presents a local stemma, below left:



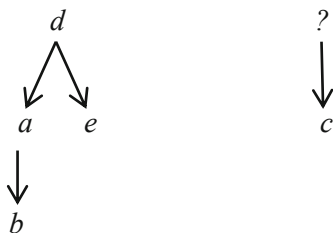
Mink leaves it open whether *a* or *b* is the initial text, which is therefore represented by ?. I think that another stemma is equally possible, above right. It is perhaps even more possible, because the asyndetic οὐκ ἔχετε may give rise to the two variants with καὶ (*b*) or δέ (*c*).

The third example is Jas 2:4/2–4, a difficult passage. The context is: If a rich man and a poor man enter the synagogue, and if you speak despondently to the rich man offering him a good seat and brusquely to the poor giving him a bad one; it continues<sup>147</sup>: καὶ οὐ διεκρίθητε (ἐν ἑαυτοῖς καὶ ἐγένεσθε κριταὶ διαλογισμῶν πονηρῶν;). The variants are *a*: οὐ διεκρίθητε, *b*: οὐχὶ διεκρίθητε, *c*: διεκρίθητε, *d*: καὶ οὐ διεκρίθητε, *e*: καὶ διεκρίθητε. They boil down to two alternatives: We regard the sentence either as a rhetorical question with a negation, or as a direct statement. Mink thinks that *d* is the initial reading, because the καὶ is an example of the so-called καὶ *apodoseos* and thus a *lectio difficilior*. He considers this a semitism and “completely non-Greek”<sup>148</sup>. I do not think that this is correct. A καὶ often slips into the text when a reader or a scribe loses the thread, but perhaps even the author himself may be responsible. There are some examples of such uncertain readings in Alexanderson (2012)<sup>149</sup>, and many others can be found. The reading *d* is no more initial than *a* or *b*, but Mink designs a local stemma with an addition for variant *c*:

<sup>147</sup> I quote the text of ECM2, where καὶ οὐ διεκρίθητε is the reading called *a* and the other variants are named consecutively. In the discussion, I follow the different denotation of Mink.

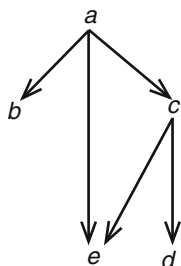
<sup>148</sup> Mink (2004), p. 61.

<sup>149</sup> See Index des mots grecs, καί.



It seems to me more probable that a reading with the negation is initial, but which one is impossible to say. The negation may be dropped, because someone has not understood that it is a rhetorical question, or it has, as so often, fallen out because it is a small word, and such words often go amiss.<sup>150</sup>

The fourth example is Jas 2:16/2–4, about unkind behaviour against poor people: (ἐὰν ἀδελφὸς ἢ ἀδελφὴ γυμνοὶ ὑπάρχωσιν ..., ) εἶπη δέ (τις αὐτοῖς ἐξ ὑμῶν· ὑπάγετε ἐν εἰρήνῃ, θερμαίνεσθε). The variants are *a*: εἶπη δέ, *b*: εἶποι δέ, *c*: καὶ εἶπη, *d*: καὶ εἶποι, *e*: καὶ εἶπη δέ. We may state that the readings *a* and *b* as well as *c* and *d* could be regarded as the same readings with itacism.<sup>151</sup> This is in my opinion a clear case where we must accept that variants can be the result of mere chance and consequently of no avail to a stemma. I consider the reading *a/b* and *c/d* as perfectly equal and candidates for the topmost position. But *e* cannot be ruled out, because the combination of καὶ and δέ is by no means rare, meaning ‘and also’, ‘but also’.<sup>152</sup> Thus I do not find Mink’s stemma shown below well founded:



150 Editors of our texts are busy adding or rejecting καὶ, other particles and the definite article, sometimes even negations, also changing a δέ into a δὴ, if δέ occurs in a principal sentence after a subordinate one; doing all this they may be right, they may be wrong. Cf. p. 90 f. with n. 210.

151 ECM2, p. 26\* f., is rightly meticulous and indicates most orthographic deviations, as it does here.

152 There is an example in 1 Jn 1:3/30–36.

### *'Minor agreements'*

It is an important idea of Mink's that so-called 'minor agreements' are as important as any other readings<sup>153</sup>: "... in the context of the CBGM, each variant shared by highly similar witnesses - regardless of whether the variant appears significant - counts as an instant supporting their affinity, their genealogical coherence. In such witnesses, minor agreements are not considered coincidental." I am not sure that there is a definition of what Mink calls 'minor agreements'. One may ask, for instance, how we shall regard 1Jn 3:20/6, where we observe four variants in ECM2: καταγινώσκη, καταγινώσκη, καταγινώσκει, καταγινώσκει.

In a way I think that Mink is right; a general agreement all over the text in variants of small importance, for instance particles, points to a general relationship. The difficulty is that most texts are alike in small matters and that coincidental readings may create a false connection.<sup>154</sup> If we put cases of small importance into statistics, we will get as a result that witnesses are on the whole alike. Witness A will be like witness B to 90%, most of the agreements being unimportant readings and the disagreements are maybe important readings; on the other hand, compared with witness C, the likeness between A and C will also be 90%, the important readings being identical in the two witnesses, the disagreements consisting of a large number of omissions and additions of particles. In this case, I regard C as much closer to A than is B. Similarities in small matters generally encumber the statistics and make the great differences more or less disappear.<sup>155</sup>

But let us think that there are two documents which are very much alike in small matters but disagree concerning important readings. This could only, or so I think, mean one thing: The 'concerned reader'<sup>156</sup> followed the text closely in small matters which did not interest him but sought other readings or made changes of his own in passages where he had difficulties understanding the text of his main exemplar. The result would be a document where 'minor agreements' really mean something, and I for one would be quite willing to accept them as important. But I do not know if there are any such documents at all, and if they exist, the CBGM would not be able to find them.

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153 See p. 63.

154 Considering the text of James, about half the text is subject to variation (Mink (2004), p. 18 f.), but if a manuscript is compared with another manuscript or with the supposed initial text *A*, the degree of agreement is high.

155 See p. 127 about 468, very like the initial text *A* in a small detail.

156 For 'concerned reader', see p. 72.

*Contamination, access to manuscripts, multiple emergence*

The traditional way of judging readings is by applying knowledge of language and context. The CBGM adds to this another way which I take the liberty of calling 'judging by environment'.<sup>157</sup> Mink has, as we shall see, difficulties combining two contrary opinions about scribes: They are supposed to use one exemplar, but they may also sometimes use more than one. If they use more than one, that other or the others are supposed to be close at hand. This idea is fundamental, expressed many times, very clearly in Mink (2004), from where I quote<sup>158</sup>: "The combination in the tradition is viewed as a process. The assumption is that, if contamination occurs, it emerges from those texts which were at the disposal of the scribe, i.e. texts in his direct environment, i.e. texts which are, for the most part, closely related with each other."

Let it first be said that Mink may be right in many cases, perhaps in most cases. The difficulty is that we do not know when. The traditional way has the advantage that we have some knowledge of language and context, however insufficient. We may well think that some religious centres were in high esteem for having good manuscripts, and that such manuscripts were often copied.<sup>159</sup> A problem, never mentioned by Mink, is that such centres may well have had an impact far away. Thus, manuscripts which are very much alike may have existed far away from each other, not being available to the scribe. On the other hand, it cannot be excluded that manuscripts of different types were available. In fact, we know very little about networks among religious centres and very little about how the scribes worked practically when copying.<sup>160</sup> Mink works with assumptions<sup>161</sup> about how they performed their task and presents no evidence for these assumptions. As we shall see later, the assumption that the environment is of great importance leads to two conclusions which are questionable<sup>162</sup>, 1) that multiple emergence plays a comparatively great role and contamination a comparatively small one, and 2), that readings different from those of the closest ancestor are supposed to be fetched from other close ancestors.

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157 Cf. p. 70, *The two bases of the CBGM*.

158 Mink (2004), p. 14, see also *ibid.*, p. 22, and Mink (2011), p. 154.

159 Cf. Mink (2004), p. 49, where a high degree of agreement in a substemma is said to be "a reflection of the circumstance that in a region with a dense tradition there exists a stable idea about which manuscripts are worth copying."

160 See further p. 71 f.

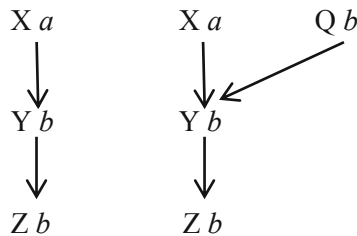
161 There are four assumptions about scribes, see Mink (2011), p. 151 ff., discussed on pp. 82 ff. and 87 ff.

162 See discussion at p. 82, *Reassessments*.



### Optimal substemma

Mink<sup>163</sup>: “A substemma consists of a descendant and the ancestors from which its text can be derived at all the variant passages it contains. It is optimal if the number of the ancestors is reduced to the minimum.” If we wish to design a so-called global stemma, it is of course highly desirable that the substemmata are as simple as possible and so allow us a good overview of the tradition. But we can absolutely not know whether the less complicated stemma is the one that comes closer to the truth. “The rule of parsimony demands”, says Mink<sup>164</sup>, “that the number of stemmatic ancestors be as small as possible.” This rule of parsimony<sup>165</sup> is wishful thinking. Above<sup>166</sup> I have tried to show that it is often possible to design different stemmata and that it is difficult or impossible to say which one is the best. A stemma which supposes that some readings common to two or more witnesses have appeared as a development of a prior reading will be less complicated than one which supposes contamination between witnesses, but it will probably not always be more credible. A stemma where the witness X has the reading *a* and the witnesses Y and Z have the reading *b* may be designed simply as the stemma to the left below, supposing that *b* is a change which appeared in Y and was transmitted to Z. On the other hand, if *b* is found elsewhere, e.g. in the witness Q, the stemma to the right is somewhat more complicated but perhaps not less true.



In Mink's chapter 14, *Constructing Optimal Substemmata*<sup>167</sup>, this constructing is said to be complex because it requires the frequent interaction of computerized procedures with philological assessments of intermediary results. I thought that the philological judgement was

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163 Mink (2011), p. 144.

164 Mink (2011), p. 162. See also Mink (2003), p. 59, where we are recommended to prefer one reading and not to ignore “den hier sehr erwünschten vereinfachenden Effekt”, the very desirable simplifying effect.

165 Cf. p. 85.

166 P. 77 ff.

167 Mink (2011), p. 189 ff.

fundamental for assessing the relationship, but now it is evident that the philological assessment changes according to results forthcoming from the studies of coherence and so-called genealogical relationship. Is this procedure not an example of a vicious circle<sup>168</sup>?

For some examples of designing real substemmata, I refer to the section below, called *Some examples of substemmata and textual flow*.<sup>169</sup>

### *Reassessments*

Reassessment and revision are fundamental notions in the CBGM. More than once Mink speaks about reassessments of the judgements on passages and about an iterative procedure which may correct the earlier judgement about priority and posteriority of readings and of the substemmata. Of course anyone may change his opinion. But how does this procedure of reassessment work? A couple of examples showing the practical handling are found below, Jas 4:17/6–8 and Jas 2:23/14–40. Let us listen to Mink about revision of text-critical decisions<sup>170</sup>: “Through such revision (i.e. subsequent revision) it can be determined whether extrapolation from “safe” cases throws light on problematic ones.” In this same context Mink refers to the Conclusion at the end of the article<sup>171</sup>: “many initial local stemmata have a preliminary status. On the other hand, most of the passages can be assessed with reasonable certainty. From the latter genealogical data are derived that can be used for a revision of the first results. Some genealogical statements made in the first phase will need correction, some previously unclear relations between variants will now become assessable. Many cases that had to be left pending altogether in the first phase will now be settled.”<sup>172</sup> As far as I can see from these quotations, genealogical relations are built up from the local stemmata, then the local stemmata are corrected according to genealogical relations. As far as I understand, this is a vicious circle.

A first consequence of the reassessment is that once a manuscript has been placed in a group of similar manuscripts, it is taken for granted that readings which differ from those of the most closely related potential ancestor are taken from some other manuscript in the same group. This is generally speaking the way Mink accepts contamination, and if there

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168 Cf. below under *Reassessments*.

169 P. 105.

170 Mink (2011), p. 165.

171 Mink (2011), p. 202.

172 See also Mink (2003), p. 66 f., freely translated: “if a reading connects two manuscripts, this is no longer a question about the character of the reading, but of the coherence between the witnesses.” Thus first coherence is stated according to readings, then readings are connected to each other according to coherence.

is no close relation between documents with the same reading, this reading is supposed to appear through multiple emergence.<sup>173</sup> A very important assumption of Mink's says<sup>174</sup>: "The sources feature closely related texts rather than less related ones." Let us think that the manuscript Y is very like the manuscript X, its closest ancestor, but differs in a certain place, where it has the same reading as Z, also a close ancestor. Then Mink is ready to suppose that Y has its reading from that other ancestor, Z, and also that the difference observed does not disturb the close relationship between X and Y. Of course, Mink may very well be right in supposing that the reading comes from Z, but there is no certainty at all. I should say that Y could very well have its reading from some other manuscript. In fact, we do not know at all where these XYZ once existed and if the scribe of Y had access to Z or to some other manuscript, maybe less related, maybe one of the innumerable ones lost to us. In reality, even manuscripts which are much alike must not necessarily have been in close contact with each other, and manuscripts which are unlike may have existed close to each other. Documents do not interact because they ultimately descend from the same *scriptorium*, but because they are physically and geographically available, and of that we know very little. We must keep in mind that it is a fundamental idea of Mink's that copying is strongly influenced from the neighbourhood.

Following the assumption mentioned above is a second consequence and a second way of explaining a difference between X and Y, otherwise much alike. This is an idea of deriving and at the same time changing, presented clearly in Mink (1993)<sup>175</sup>. Mink emphasizes the importance of coherence and continues, in my free translation: "We should not suppose that a reading has emerged independently if it is attested in a coherent way, that is if the witnesses often show the same reading in other cases."<sup>176</sup> This means that the different reading in Y may be explained as deriving from X. The reading is not independent of X, it is unlike, i.e. changed, but it not fetched from far away. The same idea is expressed in a more practical way in the same article<sup>177</sup>: "The descendant manuscript is one of the closest relatives of the ancestor; it has taken almost

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173 See p. 92, *Contamination and unique or multiple emergence of variants*. Mink accepts that in special cases there can be contamination between documents which are far from each other, see discussion below, p. 107.

174 Mink (2011), p. 154. This is one of Mink's four fundamental assumptions about how a scribe works.

175 Mink (1993), p. 483.

176 "Eine unabhängige Entstehung wird man nicht annehmen, wenn die Bezeugung kohärent ist, d.h. die Zeugen auch bei anderen Lesarten häufig zusammen erscheinen."

177 Mink (1993), p. 492.

every reading from the ancestor and developed only a few further.” Here we should observe the words ‘developed further’, i.e. changed. This is another way of ‘keeping it in the family’, if the expression is allowed, deriving one reading from the more or less disagreeing reading of an ancestor. The other way is commented upon above, that is the tendency to take over by interpolation variants from close ancestors, if there is a disagreement with the closest ancestor, not allowing these readings to come from further away. We shall see several examples of these two proceedings, taking over directly or taking over and changing, see below about Jas 4:17/6–8 and Jas 2:23/14–40, and especially the section *Some examples of substemmata and textual flow*<sup>178</sup>. It is rather strange that a method which sets store by likenesses and coherence is also quite ready to declare different readings to be not very different but such as can be explained as one being derived from the other. But I think that if the different reading is well known from outside the group of similar documents, it can very well be taken from there. The consequences of the CBGM are 1) underestimating contamination, which is only supposed to take place within the same coherent and related group, 2) overestimating coincidental appearance, because if the same posterior reading occurs in groups or manuscripts which do not show a close relationship, it is supposed to have emerged more than once. These consequences are inherent in the method, not to say that they *are* the method.

Proceeding according to the CBGM means that relationship is strengthened; we get rid of the awkward idea that a scribe may have got his reading from far away, and an optimal, that is a fairly uncomplicated stemma is created. A clear case of reassessment and thus arriving at an uncomplicated stemma is the judgement on the variant of 323 in Jas 4:17/6–8<sup>179</sup>, where the reading *a* is καλὸν ποιεῖν, the reading of 323 being *d*: καλὸν: “Variant *d* (καλὸν) could derive from *b* (καλὸν ποιῆσαι) or *c* (ποιεῖν καλὸν), if we just look at linguistic probability, but if genealogical coherence is taken into account, variant *a* is the more likely source.” In fact there are several documents which have exactly the variant καλὸν, and it is not at all necessary to regard this reading as related to other, unlike readings. This is a clear example of deriving the reading from other, unlike readings, just because there is a general agreement between documents, and of adjusting the stemma according to a preconceived idea.

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178 P. 105.

179 Mink (2011), p. 195, n. 93.

Another idea of how Mink’s theory works in practice is Jas 2:23/14–40 ἐπίστευσεν δὲ Ἀβραάμ τῷ θεῷ ... καὶ φίλος θεοῦ ἐκλήθη.<sup>180</sup> Mink presents a chain of ancestors and descendants consisting from below and upwards of 1799, 206, 429, 2200, 1611; they all omit δὲ and write δοῦλος instead of φίλος. But Mink also presents a chain of 614, which has the same reading as 1799 and its chain of ancestors. However, 1292, the most closely related potential ancestor of 614, likewise omits δὲ but reads φίλος, whereas the second most closely related potential ancestor, 1611, supports δοῦλος, omitting δὲ. Here we are supposed to find 614 influenced by its own chain, omitting 1292 and fetching δοῦλος from 1611, also a member of the chain mentioned above. In this chain, documents are clearly said to be connected by the common omission of δὲ as much as by the reading δοῦλος. In the chain 614-1292-1611, even the important disagreement δοῦλος/φίλος does not upset the close relationship. The reason is that the documents in these series are generally alike and show relationship; therefore even an unimportant agreement shows relationship and a rather important disagreement does not indicate lack of connection. But why should 614 not get its δοῦλος from some other source than its own supposed ancestors? There are quite a large number of manuscripts featuring δοῦλος.

### *The ‘rule of parsimony’*

Closely connected with the idea of reassessment is the ‘rule of parsimony’, often evoked by Mink, who, as shown above, likes to keep stemmata as uncomplicated as possible. The rule demands that the stemmatic ancestors be as few as possible. But there is often more than one way to design a stemma, and it is difficult or impossible to say which one is the best. The assumption quoted above<sup>181</sup>, “The sources feature closely related texts rather than less related ones” says that a different reading in one of the documents in a group should preferably be considered to have been taken from some document in the same group, if it does not come from the closest ancestor; it may also be considered as a development of the reading in the ancestors. Such a stemma will be less complicated than one which supposes interpolation from further away, but it will probably not always be more credible. This way contamination is ruled out as far as possible. I would like to stress that a ‘concerned reader’ may insert a reading from far away, but of course he may also have access to a document in the neighbourhood which comes from the same

<sup>180</sup> Mink (2011), p. 152–155.

<sup>181</sup> P. 83, from Mink (2011), p. 154.

*scriptorium* as the document he has before his eyes. In practice, there is a risk that manuscripts are supposed to be closer to each other than they are, and with the ‘rule of parsimony’, it is hard to withstand such a temptation. It is built into the method.

*Global stemma*<sup>182</sup>

I believe that the ‘rule of parsimony’ is closely linked to the idea of designing a global stemma. In order to master the relationship between the texts of around 160 documents for the Catholic Letters, with innumerable passages pointing in different directions, simplicity in the relations between the documents is highly desirable.<sup>183</sup> I suppose that with the help of computers, local stemmata, if simple and reliable, could be put together in such a stemma. But in the local stemmata, the relationship between readings and the direction of the influences between them are far from certain and would create, I suppose, such a number of possible global stemmata that it would have no sense to design them.

*Textual flow, ancestors, descendants, potential ancestors*

Mink<sup>184</sup> gives a definition of textual flow which I will try to summarize: There is a flow from prior to posterior variants, and the document X can have both prior and posterior variants as compared to document Y; predominant textual flow means a flow from the witness with the larger share of priority variants; the relationships between witnesses can be expressed by arrows in a textual flow diagram. So far Mink. But to my mind a document is a copy of another document or other documents, so we can talk about a textual flow from one or more documents towards others. But can we talk of the flow of a text? Now some readings are prior and flow from X to Y, but some of them flow in the other direction. There is a flow between readings, there is a flow between the texts of special documents, but these documents are conglomerations of readings pointing in different directions. We may indicate the relation of a father to a son by an arrow, but here the son is also the father of the father. There is, or so we think, an initial text and there are special texts, one in each document and all different from one another. I cannot see that there is a third kind of text. Mink<sup>185</sup> defines the relation between ancestor and descendant as a “hypothetical relation between witnesses

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182 On global stemma, see Mink (1993), p. 491 ff., Mink (2004), p. 29 f., Mink (2011), p. 204.

183 See p. 81, n. 164.

184 Mink (2011), p. 144.

185 Mink (2011), p. 143.

(= texts), not between manuscripts”. However, Mink’s ancestor is only partly an ancestor.

Consequently, I do not share Mink’s view of a ‘potential ancestor’ as a witness which has a high percentage of agreement and a higher number of prior variants when compared with another witness.<sup>186</sup> We shall also see more than once<sup>187</sup> that only minimal and often uncertain differences between prior and posterior variants are supposed to indicate which witness is the ancestor and which one the descendant.

### *Scribes generally*

One of Mink’s and Aland’s basic assumptions is<sup>188</sup>: “A scribe wants to copy the *Vorlage* with fidelity.” They think, of course quite rightly, that most variants do not result from intentional tampering with the text but only reflect the human factor in copying. Mink never, as far as I can see, takes the reader into account. Important variants exist. How did they come into the text? Could they not have been put there by the ‘concerned reader’? This ‘concerned reader’ could be the scribe, even if we think that he was mostly interested in doing his job, which is just copying. In the new, clean copy the traces of changes mostly disappeared.<sup>189</sup> How often did readers interfere with the text? We certainly do not know, but must we not think that in the course of the centuries, many scribes and readers tried to have access to the best possible text and did not shrink from making changes? If so, the number of those ‘concerned readers’ must finally be quite high. Strangely enough, Mink also talks about “a large pool of variants” thought to exist at a time when there is a “higher frequency of copying”, i.e., I suppose, in Byzantine times.<sup>190</sup>

The work of Colwell on P45, P66 and P75<sup>191</sup> is well known. Some of the results are highly interesting to us. Colwell concentrates on singular readings, of which there are hundreds in each papyrus. He claims that some of these are what he calls ‘nonsense reading’, but he gives no definition nor any examples of such variants. It is of course a matter of judgement whether we should classify a reading as a nonsense reading

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186 CBGM also speaks about first, second etc. potential ancestors, meaning those with the highest degree of agreement, second highest, etc. This means just likeness, not place in a stemma.

187 Examples for instance under *Textual flow diagrams*, p. 98 ff., and *Some examples of substemmata and textual flow*, p. 105.

188 Mink (2011), p. 151, Aland (2011), p. 57. I quote Mink.

189 Sometimes a double reading reveals conflation. In 1 Pt 4:16/22–28 the reading *e*: ἐν τῷ μέρει τούτῳ ἢ τῷ ὀνόματι τούτῳ, and in 2 Pt 2:18/22 the reading *d*: ὀλίγον ὄντως conflate two variants.

190 Mink (2011), p. 151.

191 P45 and P66 contain about 800 verses each, P 75 about 1400.

or not, but I take it for granted that there are serious difficulties understanding these passages. Doing some calculations based on Colwell's numbers<sup>192</sup> we will find that P45 has 25 such readings, P66 193, P75 67. Must we not think that many readers of these documents will eagerly try to understand what they read and even propose emendations?

The number 25 for P45 means that out of the many singular readings in P45, which are 275, less than one in ten are nonsense readings. The two other documents show a much higher proportion of such variants. We must conclude with Colwell that P45 shows much greater density of intentional changes than the other two. Someone has interfered comparatively much with P45. Among these singular readings Colwell presents examples from all three papyri of what he calls 'Harmonization to Remote Parallels', 'Harmonization to the Immediate Context', 'Editorial Changes'. That is what I call intentional changes and they agree quite well with some of the points in my list above of such changes<sup>193</sup>.

Interesting in this context is how Barbara Aland views the work of the scribe. There are contradictions. On one hand she thinks that his intentional changes are few, see below, but on the other, she allows him to be rather active interfering with the text. Cf. Aland (2006)<sup>194</sup> translated freely: "Do we really think that scribes who read or dictated the text to themselves should take no heed of its content but only think of reproducing it? The variants of our documents tell us that this is not the case." Barbara Aland too has commented on P45. I have elsewhere<sup>195</sup> tried to show that her intention seems to be to reduce the number of intentional changes in that papyrus, but that she does not succeed very well. I quote freely<sup>196</sup>: "... there is no intention to change. The scribe wants to keep the sense of the exemplar (*Vorlage*). His work is that of a professional scribe presenting no nonsense readings. He grasps the sense in a rapid and reliable way, the singular readings making the text easier to read, what is superfluous is left out, what is pedantic and long-winded (*umständlich Ausgedrücktes*) is simplified." To my mind, this description fits exactly with what I would call interfering or tampering with the text. Concerning other papyri, Aland concedes that P90 might show influences from other witnesses which have the same readings<sup>197</sup>, but generally speaking she is unwilling to think about intentional

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192 Colwell, p. 111–112.

193 P. 13.

194 P. 305.

195 See chap. 2.

196 Aland (2003), p. 32.

197 Aland (2003), p. 23.



changes and even introduces the notion ‘semi-conscious’<sup>198</sup>: The scribe of P5 is thought to reduce what is longwinded or superfluous in a semi-conscious (*halbbewusst*)<sup>199</sup> way. In another paper Aland thinks that P112 undertakes ‘editorial changes’ and she accepts a couple of readings in P101 as possibly caused by influences from other Gospels.<sup>200</sup>

A very thorough and ambitious work on scribes is that of Royse. He takes up well-known papyri which contain New Testament text: P45, P46, P47, P66, P72, P75. I think that it is enough to read the table of contents to see that quite a lot has happened to these documents. The table features corrections by the scribe, corrections by a second, a third, a fourth hand, significant corrections, theological changes, corrections to another *Vorlage*.

It is a basic assumption of Aland’s<sup>201</sup> that ‘theologians’ or ‘laymen interested in theology’ may have made corrections but that such interference has not had much impact on the text, scribes keeping to reliable exemplars without such additions. It is, however, quite clear that important changes have been made and that inferior readings have often been taken over by a great many documents. Many of these changes must have been made by what I call ‘concerned readers’. Certainly some of them were interested in theology, but I guess that to most of them it was more important to have a more readable text. We know that even 03 and other generally good witnesses have been influenced and present some inferior readings. Generally speaking, we can see correctors being at work everywhere, see for instance the list of correctors in the great majuscles. Some corrections led to nothing in the tradition, but others obviously did.<sup>202</sup>

Mink<sup>203</sup> gives an example of how he regards the work of a scribe, starting from a declaration: “The first basic assumption<sup>204</sup> means that in the context of the CBGM each variant shared by a highly similar witness - regardless of whether the variant appears significant - counts as an instant supporting their affinity, their genealogical coherence. In such witnesses minor agreements are not considered coincidental.” Here a high

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198 Aland (2003), p. 26.

199 ‘Halbbewusst’ also in Aland (2011), p. 63.

200 Aland (2002), p. 11, cf. above Colwell.

201 Aland (2011), p. 64 f.

202 Cf. the section 03, 1739 and changes in 1 Jn, p. 128 ff. The quotations in Origen’s *Contra Celsum* are interesting. A papyrus from Tura (or Toura, Papyrus Cairo 88747, 7th c.) has readings which go back to Origen, but in the course of the tradition, they have been replaced by others better known and they never arrived at the important Vaticanus Graecus 386 (13th c.) which, however, belongs to the same line of tradition. See Origène, *Contre Celse*, p. 42.

203 Mink (2011), p. 152.

204 Quoted under *Scribes generally*, p. 87.

degree of similarity means genealogical coherence, but elsewhere<sup>205</sup> a high degree is said to mean pre-genealogical relationship, the genealogical coherence or genealogical relationship being demonstrated by the direction of the influences between the witnesses. But this ‘minor agreement’ can be an initial reading appearing in most documents and does not indicate relationship. And we never, as far as I can see, find a definition of ‘minor agreement’, which may mean many kinds of variants. I suppose that particles like δέ and καί and omission of them may be counted as minor agreements and in fact be as important to the CBGM as any other reading. The examples presented show that the method works like that.

But what about those same readings in other, not similar manuscripts? They must, according to the CBGM, result from multiple emergence, unless they show an unusual character, never described nor exemplified.<sup>206</sup>

#### *Scribes and the source of variants*

Another basic assumption is<sup>207</sup>: “If a scribe introduces diverging variants, they come from another source, (i.e. they are not “invented”).”

Of course Mink knows as well as the rest of us that scribes make many mistakes of their own, but he obviously thinks of readings which are common to other documents. I have nothing against the idea that scribes usually do not invent readings; my objection is that diverging variants may come from other documents than those which show close coherence, cf. above *Reassessments*<sup>208</sup>.

However, this way of thinking raises questions as to the value of Mink’s statistics, certainly not as to statistics in a broader sense. Let us suppose that in many cases three manuscripts agree where they so to speak must agree, having the particle δέ close to the beginning of a sentence. In one case, however, they disagree, two of them having δοῦλος, one φίλος.<sup>209</sup> We would think that the two manuscripts which have δοῦλος are somehow more closely related to each other than to the third with φίλος. Let us complicate somewhat: Suppose that one of the documents with δοῦλος in one passage leaves the particle out and in this respect agrees with a fourth document. Is this disagreement as important as the disagreement δοῦλος/φίλος? If I understand Mink correctly, it is,

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205 Mink (2011), pp. 144, 157.

206 See *Contamination and unique or multiple emergence of variants*, p. 92.

207 Mink (2011), p. 153.

208 P. 82.

209 Cf. p. 85 about Jas 2:23/14–40.

and it is certainly equally important to statistics. We know from many apparatuses that the omissions and additions of small words are among the most common variants.<sup>210</sup>

*Scribes using few rather than many sources*

Another basic assumption<sup>211</sup> is that the scribe uses few rather than many sources. It is said to follow from a realistic view of the copying process and from the rule of parsimony. I have tried to show that realism has to do with availability, of which we know little<sup>212</sup>, and that the rule of parsimony does not apply<sup>213</sup>. I would like to stress that a ‘concerned reader’ may insert a reading from afar, an inventive reader may present a variant of his own. The copy the scribe produces would usually show no outward sign of such interference. After generations of copies, the number of sources influencing the text would be high indeed.

*General comments on Mink’s basic assumptions*

Generally speaking, my conclusion is that Mink as well as Aland aims at making the tradition as little complicated as possible. This is not a true aim: The aim is to find the truth, and the truth may well be complicated and difficult, even impossible to come to terms with.

*Summarizing the CBGM*

A short and I hope correct summary of the CBGM would be:

The method starts from coherence, that is, it establishes which documents are most like other documents, meaning that they have the greatest number of identical readings. In this phase, all readings are equally important. Between documents with a high percentage of identical readings, a pre-genealogical relationship is supposed to exist.

Readings which are not identical are examined in order to state which are original and which are secondary, in the terminology of the CBGM prior and posterior. Posterior readings are developments from the prior ones.

A genealogical relationship is supposed to exist between documents which have many identical readings and where the posterior readings can be explained as developments from the prior ones.

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210 Dain, p. 48.

211 Mink (2011), p. 153.

212 P. 71.

213 P. 85.

If texts have a high percentage of identical readings, the texts where prior readings dominate are supposed to be ancestors of those where posterior readings prevail. A textual flow is supposed to exist from a document with more prior readings towards another with fewer such readings.

Readings which are posterior to the readings of the closest ancestor are usually supposed to be taken over from another close ancestor, or to be a development of readings in close ancestors.

Scribes are thought usually to follow one exemplar fairly strictly.

The ‘concerned reader’, ready to interfere with the text, has no great part in the CBGM.

### **The CBGM in practice, with examples**

#### *Contamination and unique or multiple emergence of variants*

One of the vital points and one of the aims of the CBGM is to distinguish between contamination and multiple or coincidental emergence of variants. The very title of Mink (2011) shows it. According to the CBGM, it is only by making this distinction that it is possible to find a genealogical relationship between witnesses.

I do not quite see the intention behind Mink’s chapter 7<sup>214</sup>, *Contamination*. A table is presented which shows the agreements between certain manuscripts, ordered in pairs. What we can see is that some pairs of manuscripts reach a high percentage of general agreement, 98% or even 99%, whereas other manuscripts only arrive at a general agreement of around 90% at most. Maybe the idea is to show that old documents show comparatively little agreement (87%–90%) and younger ones a much higher (98%–99%). However, even manuscript that are rather far from each other in age, e.g. 307 (10th c.) and 453 (14th c.) show very strong coherence (98.55%). Also a really old manuscript, 02 (also known as A, codex Alexandrinus, 5th c.) may be as close to the much younger 1735 (10th c.) as 81 is to 2344, both being dated to the 11th c.; both these pairs show an agreement of around 90%. We may wonder whether 04 should be compared at all, since it is fragmentary and has only about 2000 comparable readings, whereas the others have around 3000.

Mink takes up the question of unique or multiple emergence of variants in his chapter 8<sup>215</sup>, called *Agreements of Variants as Indicator of Relatedness*. According to him, if witnesses which have strong coherence show a certain reading and this reading appears elsewhere in docu-

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214 Mink (2011), p. 155.

215 Mink (2011), p. 157.

ments which have weak coherence with those documents, then the reading has probably arisen more than once. If we follow Mink, it is not very probable that a common ancestor of those strongly coherent documents has taken the reading from that or those documents which are outside the coherent group, or vice versa. That is, we should not suppose contamination between groups. But why not? Further, according to Mink, if there are two groups of coherent documents which have some identical reading but which stand away from each other generally, then, even in this case, the reading has emerged twice, once in each group. But we must once again ask why we must suppose a twofold emergence. Why did not the reading appear once and was afterwards transmitted by interpolation to other manuscripts? However, finally Mink<sup>216</sup> makes something of a concession to a sceptical observer: “Yet in spite of weak pre-genealogical coherence the unusual character of variants may argue in favor of relatedness.” Thus, if the reading is peculiar enough, it has probably emerged only once and been taken over by other documents by contamination; this means that there is a link between giver and taker, that is to say a relationship. This case is, however, considered to be extremely rare.<sup>217</sup> How do we know? How special must a reading be, how do we arrive at a decision, how can we put that into statistics? We must ask that, because for Mink all relationship between documents starts from statistics. Of course we should not ask for a rigid rule, but at least for some examples.

A result of Mink’s way of reasoning is that contamination is underestimated and multiple emergence overestimated. An example of this is 1 Pt 4:16/24–28 ἐν τῷ ὀνόματι τοῦτῳ. We shall see below<sup>218</sup> that this reading is supposed to have emerged coincidentally 10 times. Evidently, ἐν τῷ ὀνόματι τοῦτῳ is not special enough to be taken over by interpolation. I should say that it is a rather interesting reading, well worth taking over as a substitute for the other, more difficult reading.

In fact, contamination is only accepted by Mink in one situation: If a descendant document has a reading which is not transmitted from the closest ancestor but occurs in another or in other close ancestors, then the descendant is supposed to have got its reading by contamination from this/these ancestor(s).

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216 Mink (2011), p. 158.

217 Mink (2003), p. 63 f.: ‘äusserst selten’, it is extremely rare.

218 Cf. p. 99.

*Summarizing the CBGM view on contamination and multiple emergence*

If a reading of a document which belongs to a closely related group shows a reading which diverges from that of the majority of the group, then it may very well have got that reading from some member of the group with the same reading, which, however, diverges from the reading of the majority. So far the CBGM may very well be right; We may call this ‘internal contamination’. However, this can hardly be proved, especially not if that peculiar reading also occurs outside the group. In such a case, there may be ‘external contamination’. The CBGM is generally averse to ‘external contamination’, preferring to suppose multiple emergence.

If a reading is found in a group of manuscripts which are closely related to each other, and also in one or more documents not so closely related, it is according to the CBGM improbable that these documents have got the reading through ‘external contamination’. In my opinion, ‘external contamination’ is very probable, the direction of which may be unknown. Multiple emergence, preferred by the CBGM, is in my opinion less probable.

The CBGM willingly accepts that a divergent reading in a closely related group is a conflation of other readings found in the same group of documents. We shall find examples below. These combinations are often improbable. They are still more improbable if an identical reading is found in other documents outside the group. If that is so, it is very possible that ‘external contamination’ has been at work, the direction of which may be unknown. Also in this case, the CBGM prefers to suppose multiple emergence.

*Potential ancestors*

In his chapter 10<sup>219</sup>, *Potential Ancestors*, Mink presents some lists showing potential ancestors of some documents. The first of them is 025. It is compared with *A*<sup>220</sup>, the hypothetical initial text, i.e. a sort of ideal text, the best possible. Now this *A*-text is constructed from a comparison of documents, and then *A* is compared with these documents. This is a sort of a circle. 025 is in fact one of the documents which show the greatest agreement with *A* and might have contributed substantially to the establishment of this initial text. There are only four manuscripts which show

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219 Mink (2011), p. 162. I have already commented on Mink’s chapter 9, *Divergence between Variants as Indicator of Genealogical Relatedness of Witnesses*, see p. 76.

220 See p. 46 and p. 73, n. 134.

more agreement with the hypothetical *A*.<sup>221</sup> And how about still more ‘venerable’ documents like 03 and the oldest papyri? Four manuscripts are compared with 025 in those passages which can be compared. These are around 2600, except for 04 (also known as C) which is fragmentary and only presents around 1900 comparable passages. The percentage of agreements is given, being for those manuscripts between 87.6% and 91.1%, *A* presenting 92.4%. We learn<sup>222</sup> that the minimum percentage of agreement between witnesses and *A* is 84.89%, which means that texts are mostly much alike. We may wonder what the bearing is of these agreements and disagreements, since we know that there is no difference between ‘important’ and ‘unimportant’ readings, and that the basic statistics are completely unreliable.<sup>223</sup>

### *Counting prior and posterior readings*

Where the texts disagree, Mink counts some readings as prior and some as posterior. This is fundamental to the method. Compared with 1739, 025 has 93 prior readings and 123 posterior ones. However, 63 cases rest as ‘uncertain’, and 18 as having ‘no relation’. ‘Uncertain’ always means that the readings are very close to each other.<sup>224</sup> It is not clear what ‘no relation’ really means. You may ask if it is possible that there is no relation at all between two readings, since all readings go back to an initial text.<sup>225</sup> The method says that such a proportion as that between 025 and 1739 shows the direction of the textual flow, which in this case means that the direction is from 1739 to 025.

The idea of a textual flow is in my opinion misleading, as stated before.<sup>226</sup> The uncertain cases, 63 in number, are in fact prior or posterior, only we cannot make out which is which. If 48 cases of these 63 are in fact prior readings in 025 and 15 readings in fact prior readings in 1739, then the documents are equal, both having 141 prior readings. How probable is such a distribution of the uncertain cases? Mink prob-

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221 Cf. ECM2, p. 33\*

222 Mink (2011), p. 166.

223 See *Statistics*, p. 63.

224 Mink (1993), p. 490.

225 There is an enigmatic explanation in *Introductory Presentation*, p. 251: “At these passages (i. e. those showing ‘no relation’), in a local stemma there is no relation between the variants in the witnesses compared because they are in different branches of the stemma.” Cf. the guide to *Genealogical queries*, 1. c.: “Column NOREL (= no relation) displays the number of variant passages where [the manuscripts] W1 and W2 read different variants which, however, have no direct relation to each other (e.g. if W1 reads variant *b* and W2 variant *c*, but the prior variant for both is variant *a*).” We shall, however, see that the method has no difficulty finding relations between different readings, cf. below under *Textual flow diagrams*, p. 98.

226 P. 69 f.

ably thinks that the probability is roughly the same for the reading to be prior as it is for it to be posterior, since he does not ascribe it to one or the other category. If so, statisticians can sort this out neatly. However, they had better not try to, because in reality the passages certainly differ from each other, the probabilities being unequal from case to case, which makes calculation impossible. Leaving speculations, it is much more probable that the direction is unclear or turned around if the supposed prior and posterior readings are more equal than 126 and 93. In table 1<sup>227</sup> the cases are as follows: 04 has 88 prior readings, 025 has 78, the uncertain cases being 45. If we bear in mind that 04 is fragmentary, it is clear that the so-called textual flow between these two documents is very uncertain. Mink does not hesitate: He indicates direction of textual flow in Catholic Letters between two manuscripts even if the difference between prior and posterior readings is sometimes only one point for separate letters, for the whole corpus 49/57 in one case and 84/99 in another.<sup>228</sup> This firm stand of Mink is astonishing, because he is well aware of the fact that the direction may easily change.<sup>229</sup> In Mink (2004)<sup>230</sup> we find a specification of textual flow, presenting an amount of ‘stability’ in the flow from ancestors to 1243. In fact this stability does not exist, because the differences between prior and posterior readings in the three existing ancestors (the constructed ancestor *A* is a fourth ancestor) are only 29 to 28, 29 to 26 and 35 to 32.<sup>231</sup>

### *Fragmentary texts*<sup>232</sup>

The fragmentary texts create difficulties. Anyone understands that if a document is very fragmentary and presents only 100 readings in common with another document, agreeing in 85 cases and disagreeing in 15, then the percentage 85% says much less than the same percentage in 1,000 common readings.<sup>233</sup> Less, but how much less? Sorting this out takes, I suppose, some complicated calculations. In some of the lists presented by Mink, manuscripts are compared in around 2600 places, other more complete documents in about 3000. However difficult, something should be done about this problem, especially because the papyri are extremely valuable and always fragmentary. Being valuable

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227 Mink (2011), p. 163.

228 Tables 3 and 4, Mink (2011), p. 167 f.

229 Mink (2011), p. 164.

230 Mink (2004), p. 57.

231 Mink (2004), p. 53.

232 See also *Potential ancestors*, p. 94.

233 Mink is of course well aware of this, see Mink (2011), p. 163.



does not mean that they necessarily have the better variants, but since they are old, their texts have been copied relatively few times, probably less often than the texts of younger documents.

Mink is not unaware of the difficulty and it seems that usually he does not intend to rely on documents with too little text.<sup>234</sup> But if they have some length and enough many common readings they may enter. P74 has a place in one of the tables as a potential ancestor, showing 24 prior and 23 posterior readings in comparison with 468.<sup>235</sup> The passages which are comparable are only about 300, whereas most of the other documents compared in this table feature around 3,000 common passages. When does a fragmentary text become comparable? It seems that we may go far down. I do not see the point of entering P74 into the comparison.<sup>236</sup>

### *Limits of agreement*

I have already quoted and discussed statements of Mink's concerning relationship and reassessments, warning against vicious circles.<sup>237</sup> An observation of Mink's<sup>238</sup> concerning agreements is in keeping with the way the CBGM reasons: "A minor difference (between prior and posterior readings, my remark) demands caution, because the direction of the textual flow can easily change in the course of a subsequent revision of local stemmata." Cf. however n. 41: "Very large differences, however, are unfavorable as well, because they point to small (pre-)genealogical coherence." When do differences really count? What proportions between prior and posterior readings are interesting? Is the percentage of agreement only telling in a middle field between strong and weak agreement? If so, which are the limits? Anyone understands that if we compare for instance 03 with a Byzantine manuscript, the percentage of agreement would be rather low, whereas the percentage would be high if we compare two Byzantine manuscripts.

In Mink (2004)<sup>239</sup> we learn that a percentage higher than 89.5%, which is a comparatively low percentage, is sufficiently high to make a direct genealogical relationship probable because the document in ques-

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234 *Introductory Presentation*, p. 305: "Smaller fragments are those which share less than 50% of the variant places in the given witness."

235 Mink (2011), p. 164, n. 39 and tables 2 and 6, pp. 166 and 169.

236 There are worse examples. In Mink (2004), p. 38, figure 9, and p. 52–53, figures 21 and 23, fragmentary texts with in one case 8, in another 48 comparable passages are compared with other documents with towards 700 such passages.

237 See pp. 70, 82 *bis*, 94 105, 106.

238 Mink (2011), p. 165.

239 Mink (2004), p. 38 f.

tion, 1243, presents a comparatively old text form, whereas 92.5% is inadequate when there are a number of more closely related witnesses. So if there are not many documents which are like each other, a low percentage is satisfactory.

*Coherence in old and young documents, establishing a Byzantine text*

Tables 8 and 10<sup>240</sup> show some interesting but not at all surprising features: Old documents like 01 and 03 do not have a high percentage of identical readings, early documents compared with late ones also have a low percentage, whereas many young documents are very close to one another. There are, however, exceptions to this<sup>241</sup>, but on the whole, when Christianity was firmly established and some centres had a reputation for keeping good texts, of course manuscripts were heavily interpolated and often came to offer very much the same text. We do know that the texts were changed, but this is in itself of little interest when it comes to establishing a text of the New Testament. More interesting is why some readings from the old documents are kept, in what surroundings they live on. Equally interesting and a corollary is of course why other readings were rejected and in what kind of environment. Maybe something like the CBGM could help us to find answers to that, making it more feasible to follow the readings through the ages, but only after establishing reliable statistics, which do not exist at the moment. I would like to ask seriously if it is at all useful to place old documents like 01 and 03 in a list of potential ancestors, when there are probably many missing links. The only way of finding a link between an old document like 03 and younger ones is in my opinion to follow some peculiar readings which have come into existence intentionally.

*Textual flow diagrams*<sup>242</sup>

I would like to start from a chart of the local textual flow of 1 Pt 1:6/18<sup>243</sup>, where  $\lambda\pi\eta\theta\acute{\epsilon}\nu\tau\alpha\varsigma$  is supposed to be reading *a* and  $\lambda\pi\eta\theta\acute{\epsilon}\nu\tau\epsilon\varsigma$  reading *b*. All the manuscripts in the chart present  $\lambda\pi\eta\theta\acute{\epsilon}\nu\tau\alpha\varsigma$ , but it is striking that the descendants of 424 have this reading, although many of them, those farther away from 424, have a closest ancestor with the reading  $\lambda\pi\eta\theta\acute{\epsilon}\nu\tau\epsilon\varsigma$ . This is, as we know, no problem to the CBGM: Witnesses situated further away from 424 are supposed to fetch their reading, not

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240 Mink (2011), p. 172 f.

241 See p. 92.

242 See for textual flow also the section *Counting prior and posterior readings*, p. 95.

243 Mink (2004), p. 42, cf. below p. 137.

from the closest ancestor but from one higher up in the stemma.<sup>244</sup> But we shall look at the module *Coherence at Variant Passages in Genealogical Queries*, where we find that the reading *d*: λπηθῆναι in 1875 derives from λπηθέντας in 020 and λπηθέντες in 181, whereas the same reading in 1837 is supposed to be a combination deriving from 326 and 617. Why cannot there be a contamination between 1875 and 1837? As far as I understand, because there is not very much agreement between 1875 and 1837 (89.8%), whereas the agreement between 1875 and 181 is high (95.4%) and that between 1837 and 326 very high (98.7%)<sup>245</sup>. But of course the reading λπηθῆναι can derive from either λπηθέντας or λπηθέντες, so there is no need for a kind of combination of these two readings in order to arrive at λπηθῆναι. And why can there be no contamination between two manuscripts that are not very much alike? Was the geographical distance between them too great? Of this we know nothing. Did the scribe reject a manuscript that was not close enough to his main exemplar? Not very probable.

There is also a reading *c* in 048: ἡμᾶς λπηθέντας, and a reading *e* in 1845 where there is an omission. The variant of 048 is supposed to derive from the initial text *A*<sup>246</sup> λπηθέντας and from λπηθέντες of 81. We find ἡμᾶς nowhere. Why not regard the word as an independent attempt to link up with the rather distant φρουρουμένουσ? As for the omission of λπηθέντας/λπηθέντες in 1845, it is supposed to derive from λπηθέντας of 424 and λπηθέντες of 617. Regarding the variants *c* and *e*, it is difficult to see why these connections are made, because the agreements are nowhere very high<sup>247</sup> and do not so to speak force the method to accept a connection.<sup>248</sup>

In the same context, Mink also takes up 1 Pt 4:16/24–28, where the reading *a* is ἐν τῷ μέρει τούτῳ, *b* ἐν τῷ ὀνόματι τούτῳ.<sup>249</sup> What may seem somewhat strange is that the variant *b* seems to have appeared in quite a number of manuscripts coincidentally. This comes out more clearly if we look at the diagram in the modules *Coherence in Attestations* and *Coherence and Variant Passages in the Genealogical Queries*. Ten documents are supposed to derive their reading *b* from four documents with the text *a*, one of them the hypothetical *A*. Between these manuscripts featuring *b*, there is no contamination. It is true that

244 Cf. *Contamination, access to manuscripts, multiple emergence*, p. 80.

245 The percentages in this discussion concern 1 Pt.

246 *A* is the second potential ancestor of 048, 1751 being the first. See p. 87 with n. 186.

247 The agreement 048/81 is 89.8%, in the other cases between 93.0 and 93.6%.

248 If we look at the passage in *Local Stemmata* in the *Genealogical Queries*, we will find another picture, where it is not known from where the readings *d* and *e* derive.

249 Mink (2004), p. 43 ff. Cf. p. 137.

the supposed descendants are usually closer to their supposed ancestors than the descendants between them<sup>250</sup>, but there is one exception: 876, a descendant of 424 (agreement 90.0%), is very close (98.9%) to 1832, also a descendant of 424. Couldn't these two have interacted?

Another example of a strange result is Jd 5/12–20. ECM2 registers 31 variants. In 81, reading *b*: ἅπαξ πάντα ὅτι Ἰησοῦς is supposed to derive from readings and witnesses outside the frame<sup>251</sup> of close potential ancestors, i.e. *a* (witness *A*): ὑμᾶς ἅπαξ πάντα ὅτι Ἰησοῦς, *n* (witness 35): ὑμᾶς ἅπαξ τοῦτο ὅτι ὁ κύριος, *h* (witness 93): πάντα ὅτι Ἰησοῦς ἅπαξ, *x* (witness 307): ἅπαξ τοῦτο ὅτι ὁ κύριος, and *s* (witness 468): ὑμᾶς ἅπαξ τοῦτο ὅτι κύριος.<sup>252</sup> If we look at the so-called potential ancestors of 81<sup>253</sup>, we find that for all the Catholic Letters the textual flow is supposed to go from 35, 93, 307 and 468 towards 81, this flow being weakly attested by few prior readings, but if we only count the readings of Jd, the very Gospel where this reading occurs, the textual flow goes very decidedly in the other direction and the witnesses mentioned above have very few prior readings compared with 81. It is thus difficult to think of them as influencing 81 in Jd 1:5/12–20.

In chapter 12<sup>254</sup>, *Textual Flow Diagrams—How Coherent Are Attestations?*, Mink presents in figure 5 a perfect coherence between four documents in a row which have the reading 1 Pt 3:16/32–42 ὑμῶν τὴν καλὴν ἐν Χριστῷ ἀναστροφὴν. This reading is one of 18 variants of this passage, the primary text line, called reading *a*, of ECM2 being ὑμῶν τὴν ἀγαθὴν ἐν Χριστῷ ἀναστροφὴν. Each one of these four is the most closely related potential ancestor of the one below it. At the top is 400, of which the most closely related potential ancestor is 319. Now all the documents in the row have the reading shown above with καλὴν, but 319 does not. It presents reading *a*. Two other ancestors, also very close to 400, present other readings, one being *d*: ὑμῶν τὴν ἐν Χριστῷ ἀγαθὴν ἀναστροφὴν, the other *i*: ὑμῶν τῇ ἀγαθῇ ἐν Χριστῷ ἀναστροφῇ<sup>255</sup>. Most old-fashioned textual critics like myself would say that these readings are very much the same, especially the readings *a* and *i*, and would only be interested in the variants καλὴν and ἀγαθὴν. For Mink, however, these readings are found in the close ancestors of 400 and are therefore important to the reading of 400. He concludes that

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250 See *Comparison of Witnesses in Genealogical Queries*.

251 For 'frame', cf. p. 70.

252 Cf. the module *Coherence in Attestations* in *Genealogical Queries*.

253 Cf. the module *Potential Ancestors and Descendants* in *Genealogical Queries*.

254 Mink (2011), p. 173.

255 I take the liberty of making τῇ ἀγαθῇ ... ἀναστροφή into a dative after ἐπιπράζοντες.

the reading *a* is the source of the reading in 400. How can that be, when *καλὴν* is found instead of *ἀγαθὴν* in 400? There may be a real relationship between the documents which have *καλὴν*, but where did they get it from? Not from the so-called ancestors, but ECM2 tells us that Clement of Alexandria has the reading *καλὴν*, which means that it is very old and could have been preserved in documents unknown to us. It could in fact be the initial reading. Without Clement, we could think that its origin was coincidental, since words that are more or less synonymous are interchangeable and sometimes found to intrude upon each other.<sup>256</sup> I cannot follow Mink, who believes that the reading *a* has to do with the reading of 400. To him the same word order in 319 and 400 means much, the difference between *καλὴν* and *ἀγαθὴν* means nothing when it comes to relationship.

Another example<sup>257</sup>: Figure 6 is about Jas 2:25/2–4, where the readings are: *a*: ὁμοίως δέ, *b*: ὁμοίως, *c*: οὕτως<sup>258</sup>. Mink makes up a diagram showing some documents and their most closely related potential ancestors; the result is that the common reading ὁμοίως of these 11 witnesses seems to have emerged several times out of ὁμοίως δέ. But if you also include ancestors much farther away<sup>259</sup>, as in figure 7, all these 11 documents seem to be coherent, all being descendants of document 5. But then for instance document 1893 is only descendant number 33 from 5 and there are other high numbers. Further *A*, a close ancestor of 5 but a constructed and not really existing text, and 04, the other close ancestor, are supposed to take part in the textual flow towards 5, which reads ὁμοίως without δέ; I suppose that ὁμοίως would come from ὁμοίως δέ in *A*, the loss of δέ would come from 04 reading *c*: οὕτως without δέ. But 5 only ranks as number 32 in the coherence between 04 and other documents. There are thus a large number of other documents closer to 04 than 5. It does not make sense to me to go so far away from the starting-point in the reading *a*, nor does it make sense to suppose a connection like loss or addition of δέ where we may see only a coincidental change. But how far away from the starting-point are we prepared to

256 See chap. 1. Examples are Jn 6:17 (p. 26) ἐληλύθει/ἐγεγόνει, similarly 6:25 (p. 27) and 12:30 (p. 38); further 8:51 (p. 31) θεωρήση/ἴδῃ.

257 Mink (2011), p. 176.

258 The following text is καὶ Ρααβ ἡ πόρνη οὐκ ἐξ ἔργων ἐδικαιώθη ὑποδεξαμένη τοὺς ἀγγέλους καὶ ἐτέρᾳ ὁδοῖ ἐκβαλοῦσα;

259 In Mink's terminology we have in figure 6 'Average connectivity', which means that the ten closest potential ancestors are taken into consideration. 'Low connectivity' means the five closest. In figures 7 and 10 we find 'Absolute connectivity', with 499 ancestors, which is some small reduction from 522 complete Greek manuscripts and larger fragments used by ECM2, see ECM2, p. 22\*.

go? We are never informed about that. If we go sufficiently far away, all documents will of course be related, since they all descend from a supposed initial text.

First (see figures 6 and 7) Mink starts from the assumption that the reading *a*: ὁμοίως δέ is prior and reading *b*: ὁμοίως posterior, but he also changes the textual flow, figure 8, making *b* the initial reading. Now we arrive at multiple emergence of *b* in many documents. That it is possible to play like this is because we have no real knowledge about what is prior and what is posterior. The local stemma has no foundation. What counts are likenesses which bundle manuscripts together, some of which have kept the initial text (now *b*), whereas others for some unknown reason changed the now posterior text *a* into the now prior *b*.

But which was really the initial reading of Jas 2:25? We can only guess. Maybe a reading without δέ is to be preferred, because ὁμοίως or οὕτως are closely attached to the preceding words, being a sort of conclusion. Copyists or readers may have avoided an asyndeton by adding a connective particle.

We see from this example that the method makes different readings in ancestors somehow contribute to readings in descendants, and in combinations which are rather incredible. For more examples, see below Jas 1:12/31 and p. 107 ff.

Jas 1:12/31<sup>260</sup> has several variants, the most important being ἐπεγγείλατο with no subject, or with a subject κύριος or ὁ κύριος or ὁ θεός or even ὁ ἀψευδῆς θεός. First a diagram with ‘Average connectivity’ is supposed to indicate that the reading κύριος has emerged several times. It is difficult to see why κύριος should be derived from ὁ κύριος and not the other way around.<sup>261</sup> It is equally difficult to see how there can be a textual flow to 04, which has the reading ἐπεγγείλατο κύριος from the hypothetical *A* and from 1739. *A* is supposed to have the reading ἐπεγγείλατο without a subject, 1739 has ἐπεγγείλατο ὁ θεός. The result is multiple emergence, but if you present an ‘Absolute connectivity’, you will find a coherence between all the witnesses which have the reading κύριος, but then several of them are very far from their supposed ancestors; they may be numbers 61, 41, 31, and we must still suppose a most improbable relationship between ἐπεγγείλατο without a subject and ἐπεγγείλατο ὁ θεός, ending up in ἐπεγγείλατο κύριος. For Jas, the textual flow goes rather from 04 (20 prior readings) towards 1739 (17

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260 Mink (2011), p. 178.

261 Mink supposes that the omission of the article is a haplography depending on ἐπεγγείλατο ὁ κύριος. Why could the article not result from a dittography?

prior readings).<sup>262</sup> I do not think that this presentation of Jas 1:12/31 has much sense.

It has, I hope, already been made evident that the method tries hard to avoid contamination between manuscripts that are not very like each other, but doing so is forced to accept unprovable and sometimes highly improbable combined derivations between manuscripts.<sup>263</sup>

*A practical example: Jude 15/14–16*

Let us start from the beginning of Mink's chapter 13<sup>264</sup>, where he discusses the variant  $\pi\tilde{\alpha}\sigma\alpha\nu \psi\upsilon\chi\eta\nu$  in Jude 15: "Individual witnesses or groups of them within the same attestation sometimes are very different textually. In such cases coherence is weak, and one may ask whether agreement in these passages may be coincidental. The question refers to connectivity. A variant attested just by closely related witnesses can be regarded as connective, even if the variant as such allows one to suppose multiple coincidental emergence. If coherence is weak, then connectivity depends on the character of the variant. In such a case we would have to ponder whether the character of the variant suggests connectivity in spite of weak coherence." Let us say that a passage has a reading *a*, attested by witnesses which are rather different between them, that is: they show weak coherence. If this is the case, their reading *a* may be coincidental, which I take to mean that this reading does not come directly from an exemplar, be it directly or by interpolation. But how can some witnesses, otherwise unlike, have the same coincidental reading? They can, of course, if the reading is of a trivial type, for instance the addition or the omission of a particle. But if the reading is of a quite special type? In such cases, is the reading really coincidental, the result of multiple emergence? Must there not be a genealogic connection between them, caused by interpolation or by their adhering to an initial reading? Finally Mink, as quoted above, seems to land on an assumption that after all there can be connectivity in spite of a weak coherence, but then it depends on the character of the variant. What is this special character which allows us to suppose connectivity in spite of weak coherence? It seems that we are free to consider a reading connective or not connective just as we please.<sup>265</sup>

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262 Cf. the module *Coherence in Attestations* and the module *Comparison of Witnesses in Genealogical Queries*.

263 Cf. *Summarizing the CBGM view on contamination and multiple emergence*, p. 94.

264 Mink (2011), p. 181.

265 See also p. 93 about  $\acute{\epsilon}\nu \tau\tilde{\omega} \delta\acute{\nu}\omicron\mu\alpha\tau\iota \tau\acute{\omicron}\upsilon\tau\omicron$ , which is not special enough.

The passage discussed is Jd 15/14–16 (ἦλθεν ... ποιῆσαι κρίσιν κατὰ πάντων καὶ ἐλέγξει) πᾶσαν ψυχὴν (περὶ πάντων τῶν ἔργων ἀσεβείας αὐτῶν ὧν ἠσέβησαν). There are four variants, but for the sake of simplicity I think we may speak of only two, one being *a*: πᾶσαν ψυχὴν, the other being πάντα τοὺς ἀσεβεῖς with some differences but always presenting τοὺς ἀσεβεῖς. This is an example of common variants in witnesses with no very close coherence. Only P72 (3rd/4th c.), 01 (4th c.) and 1852 (13th c.) are the witnesses of πᾶσαν ψυχὴν but do not show close coherence.

Jude is here influenced by *I Enoch* 1:9 but is not an exact quotation from any known version. The known Greek text of the passage is ἔρχεται ... ποιῆσαι κρίσιν κατὰ πάντων, καὶ ἀπολέσαι πάντα τοὺς ἀσεβεῖς, καὶ ἐλέγξει πᾶσαν σάρκα περὶ πάντων ἔργων τῆς ἀσεβείας αὐτῶν ὧν ἠσέβησαν. There are also texts in Ethiopic and Latin which all come close to this Greek text, and a fragment in Aramaic which speaks about ‘flesh’.<sup>266</sup>

How can we explain the text of Jude? One idea would be that ἐλέγξει πᾶσαν σάρκα in *Enoch* is too strong: Why should the Judge criticize everyone severely, ἐλέγξει? It is more to the point to speak about the impious being treated that way, especially when the text speaks about impious actions. So someone rejected πᾶσαν ψυχὴν and introduced the milder πάντα τοὺς ἀσεβεῖς. But it is quite easy to think of a jump between similar words: καὶ ἀπολέσαι πάντα τοὺς ἀσεβεῖς easily falls out because of the two καὶ. I think that we must confess our ignorance: We do not know which text is prior and which is posterior.<sup>267</sup> I think that it is not improbable that the initial text had both readings, and that one or the other has fallen out in the course of the tradition.

Mink makes up several textual flow diagrams where we, as before, find that supposed ancestors may have quite other readings than the supposed descendants (see e.g. figure 13).<sup>268</sup> The supposed reading of *A*, that is *a*: πᾶσαν ψυχὴν and the reading of 03, that is *b*: πάντα τοὺς ἀσεβεῖς are for instance considered to be the origin of the reading *d*: πάντα ἀσεβεῖς in 1739. It is quite clear that Mink thinks that among readings which are different, one reading in combination with another may be regarded as the source of a third. So *x* plus *y* may give

266 The text from *Apocalypsis Henochi Graece*, p. 19. The Greek text is from a codex Panopolitana (Cairo Papyrus 10759, named also Codex Gizeh or Akhmim fragments) from the 6th c. See for an overview Wasserman, p. 301–304, who also presents the text in various languages.

267 ECM2 writes πᾶσαν ψυχὴν and Mink, see Mink (2011), p. 187 with n. 77, tends to regard that reading as the prior one, whereas Wasserman, p. 304, prefers πάντα τοὺς ἀσεβεῖς.

268 Mink (2011), p. 185.



z.<sup>269</sup> This is of course possible, but sometimes, as here, quite arbitrary. Mink<sup>270</sup> tells us that if *a* is not the initial text, it would be hypothesized to have emerged coincidentally three times, because the witnesses (P72, 01, 1852) are not genealogically closely related. Mink's reasoning looks to me very much like a *petitio principii*. If *a* is not the initial text, obviously the diagrams showing textual flow would be disturbed. But maybe these diagrams are wrong. It seems more probable that the initial text had both *πᾶσαν ψυχὴν* and *πάντας τοὺς ἄσεβεῖς*, and that different parts of the reading got lost in the tradition. P72 and 01 may have their reading from some common ancestor, perhaps from someone who consulted the text of Enoch, held in esteem in the early Church, 1852 perhaps from an unknown source among the many which have disappeared. A close relationship between the three witnesses which have *a* cannot be established, because there is a distance of about 1,000 years between on the one hand P72 and 01, on the other 1852, and on the whole, these witnesses do not have very much in common. Why should three witnesses which have the same reading show much agreement as to most of their readings? They could all keep the initial reading through the generations of witnesses, or they could all three be contaminated, or some could keep the initial reading, others have it through contamination.

*Some examples of substemmata and textual flow*<sup>271</sup>

In table 11<sup>272</sup> Mink presents as an example the ancestors of 323. The numbers of prior and posterior readings of the ten closest ancestors are shown. The readings of 35 compared with those of 323 are 111 prior readings in 35 and 111 in 323. 76 cases are declared uncertain, which means that it is impossible in these cases to see in what direction the so-called textual flow goes and 17 cases have what Mink calls 'no relation', which may mean that the readings are very far from one another.<sup>273</sup> With equality between prior and posterior readings, Mink decides that textual flow cannot be indicated for 35. But let us have a look at 307 and 424, where the numbers of prior and posterior readings are 123 to 120, and 121 to 116 respectively. The uncertain cases are 68 for 307 and 80 for 424. 307 and 424 are not the only manuscripts with a small difference

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269 This idea is clearly expressed by Mink (2004), p. 35: "Either a variant is designated as genealogically dependent on another variant (or on several, if they explain the variant together. e.g. by merging), or the origin of a variant is designated as uncertain."

270 Mink (2011), p. 187 with n. 77.

271 For a more general discussion, see *Optimal substemma*, p. 81.

272 Mink (2011), p. 190.

273 Cf. p. 95.

between prior and posterior readings. Seven or eight out of ten existing manuscripts in the table are roughly as equal as the two mentioned above, the exceptions being 1739 and 03, possibly also 04 (89 to 76). The uncertain passages are for most of the witnesses 62–88, exceptions being 1739 (only 26) and 04 (only 46). With such little preponderance for prior readings and so many uncertain passages, we simply cannot state the direction of the flow of most of these manuscripts. Of course one could say that we may expect the uncertain cases mostly to be equally or almost equally divided between prior and posterior readings, and so the relationship between prior and posterior would not be changed. This would be an average result, but the special case is often far from the average, and sometimes the tables would be turned.<sup>274</sup>

The clearest case of one manuscript depending on another is 323 depending on 03; 03 presents 195 prior readings to 75 posterior ones. Probably many 03-readings have landed in 323 through real genealogical relationship, whereas it is impossible to see a real relationship between 323 and most of the so-called ancestors, because the likenesses in them point in different directions. This indicates that in spite of rather weak coherence (87.81%), there may be a real relationship between 03 and 323.

According to Mink<sup>275</sup>, the possible combinations between 323 and these witnesses reveal four possibilities. The first of them is that 323 “agrees with at least one potential ancestor of the combination at a certain number of passages”. We shall soon see that if the combination of ancestors is 1739-04-617-93-307-025-03, we shall find almost all readings of 323 represented in at least one of these witnesses, to wit in 3,006 cases out of 3,040. Mink considers these readings in 323 as “explained by these agreements”, which means that the readings of 323 derive from the same readings in the presumed ancestors. We may, as always, ask: Why not the other way around?

Some readings of 323, numbering 26<sup>276</sup>, may be posterior readings deriving from but not agreeing with prior readings in the ancestors. Concerning eight readings, the decision is still pending, and what Mink<sup>277</sup> has to say about such cases makes me rather uneasy: “In this case it has to be reconsidered whether the variants in question can be derived from variants in the potential ancestors.” We are now close to a vicious circle; the temptation will be very strong indeed to consider a reading in the

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274 See p. 95 f.; it is in fact impossible to predict anything about the unclear cases.

275 Mink (2011), p. 191.

276 Mink (2011), p. 192.

277 Mink (2011), p. 191 (iii).

presumed descendant as derived from one of the presumed ancestors. If one believes in the ‘rule of parsimony’, it is hard to see that it can be avoided.

How shall we proceed in uncertain cases? Mink<sup>278</sup> says that if no combination of potential ancestors is able to explain all the variants of a witness, then the readings of a witness (in this case 323) can be derived from variants attested by non-ancestors only. To me a variant *b*, clearly derived from a variant *a*, means that the document containing *a* is an ancestor, not a non-ancestor. Mink tries to avoid this dilemma by supposing that there are lost ancestors behind the two texts, which otherwise do not seem to be related. This may very well be the case, but it means that the method breaks down at an important issue: Non-ancestors are suddenly ancestors. Practically speaking, in order to find all ancestors of 323 we must go further back until we have found them all. This may mean that we have to include among the ancestors witnesses that present a rather low percentage of agreements. However, in fact all witnesses are fairly like each other, the average agreement of all pairs of witnesses to the Catholic Letters being 87.6%, the minimum 77.9%<sup>279</sup>. Another way to get around a similar problem is presented in Mink (2004)<sup>280</sup>: 04 and 1175 show three identical readings but there is no ancestor which has them. Then Mink presents a so-called node<sup>281</sup>, carrying these three readings and preceding 04 and 1175. There are quite a lot of manuscripts with these three readings, but according to the method of ruling out interpolation as much as possible, they are not taken into account.

Let us take up some passages which are supposed to indicate why a witness should be included as an ancestor of 323, although it shows a low agreement rate and exclusively agrees with 323 in one passage only.<sup>282</sup> The row of ancestors is supposed to be 1739-04-617-93-307-025-03. The first case is 1 Pt 5:2 ποιμάνετε τὸ ἐν ὑμῖν ποιμνιον τοῦ θεοῦ ἐπισκοποῦντες μὴ ἀναγκαστῶς ἀλλ’ ἐκουσίως κατὰ θεόν, where 03 as the only document in the series of ancestors of 323 omits ἐπισκοποῦντες, also missing in 323. Mink considers this variant as connective, because there is no obvious reason for what he calls an omission. But we do not

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278 Mink (2011), p. 191, under (b).

279 Mink (2011), p. 157 with n. 25.

280 Mink (2004), p. 65 f.

281 It is not clear what a node is: Mink (2004), p. 64: “These intermediary nodes are not hyparchetypes in a traditional sense, but should better be understood as a special kind of connection.”

In Mink (1993), p. 487, Mink speaks about a mathematical model with ‘Knoten’ (‘nodes’) and ‘Kanten’ (‘edges’). ‘Edges’ are, as far as I understand, used by Mink in the same sense as elsewhere arrows showing a direction of the textual flow. See p. 113 *Nodes and circular edges*.

282 Mink (2011), p. 192 ff.

know if there is an omission. The participle may very well be an intrusion, and then it is connective to the rest of the tradition. It is lacking in 01\*, 03, 323, in the Sahidic and Ethiopic traditions and it is unknown to Didymus the Blind (4th c.) and Anastasius Sinaita (6th c.). This is no mean tradition, but of course we should as always rely on what is reasonable and not on what is old. What is reasonable? Perhaps someone wanted to explain the metaphorical ποιμάνετε by the more indicative and concrete word ἐπισκοποῦντες, perhaps someone thought the participle superfluous. We simply do not know. The passage does not indicate textual flow in any direction.<sup>283</sup>

Another example<sup>284</sup> is Jas 5:7 ὁ γεωργὸς ἐκδέχεται τὸν τίμιον καρπὸν τῆς γῆς μακροθυμῶν ἐπ’ αὐτῷ, ἕως λάβῃ πρόϊμον καὶ ὄψιμον. Here 323 presents a variant *b*: ἕως ἄν, together with only one of the presumed ancestors, i.e. 025. Mink remarks that “it is striking that for many witnesses of *b* the closest potential ancestor is not part of this attestation”, which is also true for 323; this witness, if counted as a descendant from 025, has the rank 9. Another conclusion than Mink’s would be that the documents presenting *b* have got it through contamination from various sources and that this reading does not indicate a close connection. Mink seems to leave the question open whether ἕως ἄν is connective or not. It is, he says, a matter of philological assessment. But as I have tried to show, the philological assessment should be made before, not after stating the relationship, since the philological assessment governs the relationship. It is not supposed to be the other way around. And there is no philological assessment; no one can tell which is initial, ἕως or ἕως ἄν. A stickler for grammar may have changed ἕως into ἕως ἄν, a careless scribe may have omitted ἄν, writing what came natural to him.

Also the judgement on Jas 2:13/8b is highly debatable.<sup>285</sup> In a context which is about ἔλεος, 93 and 323 have the reading *b*: ἀνίλεως, which is the only contribution of 93 to 323. The variant is the reading *a*: ἀνέλεος. The witnesses of *a* are usually well connected to their closest potential ancestors, whereas the *b*-readers much more often do not have this connection. A conclusion near at hand would be that the *b*-reading is introduced by contamination in many witnesses which have no strong connection between them, but Mink does not think so. He says: “Even

283 Another example of a dubious participle is 1 Pt 3:9/20–38 (μὴ ἀποδιδόντες κακὸν ἀντὶ κακοῦ ...) τοῦναντίον δὲ εὐλογοῦντες, ὅτι εἰς τοῦτο ἐκλήθητε, ἵνα εὐλογίαν κληρονομήσητε. A row of manuscripts, including the Byzantine tradition, add εἰδότες after εὐλογοῦντες. ECM2 does not accept the addition, I think rightly: εἰδότες was probably added in order to form a transition to ὅτι etc.

284 Mink (2011), p. 193.

285 Mink (2011), p. 193 f.

the closest potential ancestor does not necessarily qualify as a stemmatic ancestor because the optimal coverage of variants may be produced by a combination of witnesses without it.” I suppose that ‘without it’ means ‘without the variant in question’, and that the combination of two different variants can produce a third variant.<sup>286</sup> This interpretation seems to be confirmed by the study of figure 18<sup>287</sup> illustrating the tradition of 1 Pt 3:21/4–12, where e.g. the readings of 617 (*g*) and 93 (*a*) produce the reading of 020 (*d*). These readings are *a*: καὶ ὑμᾶς ἀντίτυπον νῦν σώζει, *g*: ἀντίτυπον νῦν καὶ ἡμᾶς σώζει, and *d*: καὶ ἡμᾶς ἀντίτυπον νῦν σώζει. It is arbitrary to draw conclusions from such readings, because they may emerge coincidentally anywhere. They may be caused by itacism and by the fact that the scribe repeats the word to himself but does not remember or does not care very much about the exact word order. If we follow figure 18, the witness 436, which has the reading *d* fetches its reading not only from witnesses with the readings *a* and *g* but also from a third ancestor with the reading *h*: ἀντίτυπον νῦν σώζει καὶ ἡμᾶς.

We may ask if there is such great agreement between 617 and 020 and between 93 and 020 that the reading of 020 is so to speak forced upon it by its potential ancestors. 617 and 93 cannot have influenced 020 at this very place, because none of them have the reading of 020, but maybe an ancestor which has disappeared<sup>288</sup> had the reading of 020? This is of course not impossible, but not very probable. Both 93 and 617 agree to a high degree with 020 (more than 95% for 1 Pt), but as to different readings, there are 13 prior ones in 617 versus 13 in 020 and 19 prior ones in 93 versus 9 in 020. So prior readings jump to and fro between these documents, although there is a general agreement.

Thus in my opinion, Mink sees a connection between witnesses where there is none, but there is in this context one example<sup>289</sup> of the opposite, concerning Jd 19 οὔτοι εἰσιν οἱ ἀποδιορίζοντες, ψυχικοί, πνεῦμα μὴ ἔχοντες. Quite a lot of witnesses, among them 04 and 323, have ἀποδιορίζοντες ἑαυτούς. The addition of ἑαυτούς after ἀποδιορίζοντες seems to me to be a connective, i.e. a false reading, connecting 04 and 323. It is perhaps influenced by κατὰ τὰς ἐπιθυμίας ἑαυτῶν or κατὰ τὰς ἑαυτῶν ἐπιθυμίας of vv. 16 and 18. In spite of such phrases, the author, like other authors of Letters in the New Testament, is concerned about those who actively cause separation and disorder in the congregation, the false teachers. That is why I think that the short version is the initial

<sup>286</sup> About such combinations, see also under *Textual flow diagrams*, p. 98.

<sup>287</sup> Mink (2011), p. 194.

<sup>288</sup> Cf. p. 113 ff. about ‘nodes’.

<sup>289</sup> Mink (2011), p. 194.

reading and the addition *ἑαυτούς* a false and connective one. The case is not quite clear, but I think that 04 may well be an ancestor of 323, but Mink thinks otherwise.

Discarding some of the presumed ancestors of 323 and keeping 1739-617-307-03, Mink<sup>290</sup> musters a row of 29 passages where 323 is considered to present posterior readings compared with at least one of these supposed ancestors. I think that some of them really show posterior readings in 323, as e.g. Jas 2:4/2–4<sup>291</sup>: the reading without negation is probably posterior. Most of the cases indicate in my opinion no direction, a couple of them speak against Mink's opinion. Those are:

1 Pt 5:10/38–44 *καταρτίσει, στηρίξει, σθενώσει, θεμελιώσει*. This reading *a* is supported by many witnesses, as is also the reading *b* with *-αι* as the ending all the way. The reading of 323 is *d*: *-αι, -ει, -ει, -ει*. This reading of 323 is supposed to derive from the ancestor 307, which has the reading *h*: *καταρτίσαι ὑμᾶς* and then the same reading as 323. I think that 323 is closer to the readings without *ὑμᾶς* and that the addition of *ὑμᾶς* forms a link between the documents which present it. This is another example of small phenomena like endings meaning much to the method, whereas a somewhat greater, but not very great, difference like adding or omitting the pronoun means nothing.

Jd 19/9 *ἀποδιορίζοντες; ἀποδιορίζοντες ἑαυτούς* 323. See above. 323 probably has a posterior reading, but not one that derives from Mink's supposed four ancestors; instead, there may be a relationship between 04, now discarded as an ancestor, and 323.

Starting from the four ancestors 1739-617-307-03 there is a remainder of nine questionable passages.<sup>292</sup> Following Mink's theories, in all these passages 323 must in fact present a posterior reading or at least a possible posterior reading, otherwise the theory of 323 as a descendant of these four will break down. Let us see if it stands the test.

Jas 2:18/42–52 (*κἀγὼ σοι δείξω*) *ἐκ τῶν ἔργων μου τὴν πίστιν*: Here 323 presents *τὰ ἔργα μου ἐκ τῆς πίστεως*. Mink thinks that the prior readings from which the variant of 323 descends is *a*, which is the reading above, and *d*: *ἐκ τῶν ἔργων μου τὴν πίστιν μου*. In fact, neither *a* nor *d* is very like the reading of 323, which gives quite another meaning to the phrase. I would say that 323 (and 322) have been subjected to theological thinking and as a consequence form a little group apart from the rest of the tradition. The reading of 323 speaks very clearly against Mink's theory.

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290 Mink (2011), p. 195 f.

291 P. 77.

292 Discussed in Mink (2011), p. 196 ff.

1 Pt 1:17/8. The variants are *αἰτεῖσθε* 323 and *ἐπικαλεῖσθε* in the supposed ancestors. *αἰτεῖσθε* may very well be a posterior reading, changing the original *ἐπικαλεῖσθε*, but *ἐπικαλεῖσθε* is found almost everywhere except in 323. A derivation from the supposed ancestors is possible but not proved. It could come from anywhere.

1 Pt 1:24/18. *δόξα αὐτῆς* (sc. *σαρκός*): 323 omits the word after *δόξα* which in the supposed ancestors is either *αὐτῆς* or *ἀνθρώπου*. Of course this does not prove that 323 has anything to do with these supposed ancestors, but it is equally self-evident that it does not speak against it. I simply cannot see why it is regarded as a questionable passage which has to be discussed.

1 Pt 3:21/4–12 has already been discussed above.<sup>293</sup> There is no special relationship between the supposed ancestors and 323.

1 Pt 5:9/32. *ἐπιμελεῖσθε*, found in 323 and 1241, is no doubt a posterior reading to the readings of all the other traditions: *ἐπιτελεῖσθαι*, *-σθε*, *-ται*. It is however not at all certain that the variant of 323 has anything to do with the supposed ancestors. It can derive from anywhere.

2 Pt 1:4/8–18. As so often, Mink starts from the textual flow diagram, which means taking for granted what should be proved. The variant of 323 is *k*: *μέγιστα ἡμῖν καὶ τίμια ἐπαγγέλματα δεδώρηται*. The diagram is supposed to show that this reading is derived from three readings, one being *a*: *τίμια καὶ μέγιστα ἡμῖν ἐ. δ.*, the second *b*: *τίμια ἡμῖν καὶ μέγιστα ἐ. δ.*, the third *i*: *μέγιστα καὶ τίμια ἡμῖν ἐ. δ.* The variant *a* is obviously chosen because it has the word order *μέγιστα ἡμῖν*, the variant *b* because it has *ἡμῖν* in the right place, the variant *i* because it starts by *μέγιστα*. The variant *b* is not represented by any of the supposed ancestors. I consider this as an example of arbitrariness.

2 Pt 2:12/10–14. To Mink the variation *γεγεννημένα/γεγεννημένα* plays some role, although he is aware of the fact that the orthography here is not at all reliable. However, starting from what should be proved, i.e. from the textual flow, he states that the reading *γεγεννημένα* in 323 derives from the reading *γεγεννημένα* in 03 and 1739. It may of course come from any manuscript presenting one or the other of these variants, which are in fact no variants. This is well known to anyone who has an experience of apparatuses.<sup>294</sup>

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293 P. 109.

294 Reading the *Contra Eunomium* of Basil the Great (Basile de Césarée) will cure anyone of bestowing much weight on *γενν-* and *γεν-*. I refer especially to a table in SCh 305 (Basile de Césarée, *Contre Eunome*, T. 2, p. 228), showing the anarchy of the spelling of such words, although the spelling is really important in the context. *γέννησις* is quite different from *γένεσις* (there is also *γέννεσις*), but these and related words are mixed up over and over again. See also *ibid.* T.1, p. 96 f. Another example of hopeless confusion: The preverb *προσ-* in a manuscript

3 Jn 9/4. 323 has ἔγραψα ἄν τι. Of course the reading of 323 could be a combination of ἔγραψα τι and ἔγραψα ἄν from the supposed ancestors, but it could also come from a combination of other documents or even belong to a direct tradition with ἔγραψα ἄν τι, represented by some documents.<sup>295</sup>

3 Jn 12/34–42 regards a change of word order and is in my opinion not relevant.

After studying these nine questionable passages, Mink concludes<sup>296</sup> that his theory of 1739-617-307-03 as the ancestors of 323 has stood the test. In my opinion, Jas 2:18 speaks clearly against Mink's conclusion and the other cases prove nothing.

Finally in this chapter<sup>297</sup> Mink makes a comparison between 323 and the other supposed ancestors concerning on one hand all the Catholic Letters, on the other only James. The ancestors are counted from number one to number ten, but in fact, for James 15 ancestors are counted, because some of them show strong agreement with 323 but the textual flow between them and 323 is not clear.<sup>298</sup> Interestingly, for all the Catholic Letters, 1739 has the first position, but for James, it is only number ten. Two of the supposed ancestors of 323 for all the Catholic Letters, to wit 03 and 04, do not figure at all when it comes to James, whereas no less than eight witnesses to James do not occur in the list for all the Catholic Letters. All higher ranking positions for James are held by clearly Byzantine witnesses. This indicates that for James we have a Byzantine text which has been thoroughly standardized and that consequently the agreements are omnipresent and textual flow often cannot be stated. Accordingly 1739, not considered a Byzantine text, loses its top position. This is really an interesting result of the CBGM and may lead to further research, but such work must be based on real statistics, not on those of the CBGM. The interesting question is why James may have a special position and was subjected to thorough standardization. Was this text considered more valuable?

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means προσ- or προ-, προ- likewise means προσ- or προ-; an editor is free to choose. Also, the endings -σθε and -σθαι must be interpreted according to the context, not according to the spelling, which is completely unreliable, like the spellings λειπ and λιπ.

295 The variants with ἄν may reflect some theological thinking (cf. comment on Jas 2:18, p. 110); someone did not like the idea that an apostolic Letter had been lost. So Metzger, p. 655.

296 Mink (2011), p. 199.

297 Mink (2011), p. 200 f.

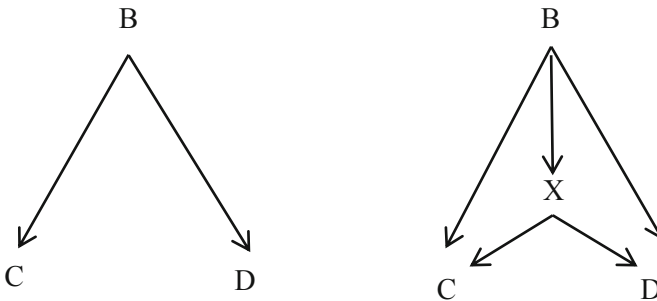
298 Which probably means, according to the usual way of counting, that there is exactly the same number of prior and posterior readings, plus a number of uncertain readings and readings showing no relation. Usually one prior reading more means that the textual flow is considered as proved, to which I object strongly, see p. 95, *Counting prior and posterior readings*.



*Nodes and circular edges*

Above<sup>299</sup> I shortly mentioned a problem with the manuscripts 04 and 1175, which Mink likes to solve by a so-called node. I think that it is appropriate to treat the problems of nodes and edges in one context. We meet nodes in Mink (2004), chap. 7–9.<sup>300</sup> These problems are very complex, and it is not always easy to follow how Mink arrives at a solution. I shall try to summarize it in a simplified way.

About nodes: Mink (2004) discusses a case where an ancestor B has two descendants, C and D (below left, a preliminary, faulty stemma).



Both C and D have posterior readings compared with B, but there are posterior readings in C which seem to be evolved from D. Now, cannot D simply be an ancestor of C? No, because if we compare with B, D has developed many more posterior readings than C has. Mink presents a solution, which is to posit a node (above right, more correct stemma) which I call X; this X has the posterior readings which are in C and which seemed to be developed from D.

This is of course quite possible, but there are difficulties, which Mink is well aware of, and he presents two: this X could be found on the way between B and C, or on the way between B and D; therefore, placing this intermediary witness as is done above is only a hypothesis. According to Mink, it helps to solve a problem of contamination and loss of intermediate witnesses. Obviously, this X has been lost. But if it has not been lost, if the unexpected readings in D also exist in some witness outside the stemma shown above? Mink would probably say that these readings do not exist in closely related witnesses but only in such as are far away from this group and thus without relationship to it, but I have tried to

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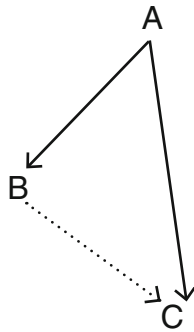
299 P. 107 with n. 281. I quote once more Mink (2004), p. 64: “These intermediary nodes are not hyparchetypes in a traditional sense, but should better be understood as a special kind of connection.”

300 Mink (2004), p. 59: 7. *Prior variants found only in non-ancestors*; p.63: 8. *Undirected genealogical coherencies*; p. 67: 9. *Circular edges*.

show that this way of reasoning is not acceptable.<sup>301</sup> Besides, there are the general problems of deciding which readings are prior and which posterior and of the sometimes small differences between prior and posterior readings. Are those readings in C decidedly posterior? I think that Mink's theoretical solution must be confirmed with evident examples. He adduces one example, Jas 2:4/2-4<sup>302</sup>, but as I have tried to show above<sup>303</sup>, the relationship between the variants there is far from evident.

Very much the same problem arises if C and D have an *Undirected genealogical coherence*<sup>304</sup>, that is to say that the numbers of prior and posterior readings are the same. We must then suppose an influence from another witness, and here too Mink prefers a lost witness which is closely related to known and closely related witnesses.

About circular edges: The problems are very complex and the solutions Mink presents are not easy to follow: The long and short of it is, as far as I can understand, that in a complicated tradition we can see prior readings in manuscripts that are younger than those with a posterior reading. Let us imagine that we know perfectly well that the following stemma, where C is younger than B, is true:



Now C is influenced by B but has kept so many prior readings from A that it looks like an ancestor of B. This is however impossible and would result in a totally false stemma. The problem can be solved in various ways. Mink posits nodes carrying readings common to both younger and older witnesses. Here, as elsewhere, I do not say that he is wrong, only that the possibility of contamination from documents further away is not considered at all, although it is perfectly conceivable.

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301 See p. 82, *Reassessments*.

302 Mink (2004), p. 62.

303 P. 77.

304 Mink (2004), p. 63 ff.

The problems about nodes and edges are discussed extensively in Mink (2004), where Mink acknowledges<sup>305</sup> that problems in this field have not been solved and that further research is necessary. He is very clear about the great difficulties and points among other things to the sometimes very small difference between prior and posterior readings. In Mink (2011), these problems are only hinted at in a note.<sup>306</sup> They are in fact fundamental. If they cannot be solved, the method breaks down. I suppose that if we accept nodes ‘in the family’, we can arrive at a kind of solution with comparatively (mark, please: comparatively) simple substemmata, but is it a true solution? How will it be possible to show that it is?

### Some general reflections

Mink has a chapter about conclusions<sup>307</sup>, but I think that most of the questions taken up there have already been discussed above, and in some cases more than once. There is much about ‘revision’ seen as an ‘iterative process’, and I have already expressed my doubts as to this procedure. I would, however, like to take up one statement which I find highly objectionable<sup>308</sup>: “In the beginning, a lack of pre-genealogical coherence of witnesses is the most reliable indicator of a correspondingly deficient genealogical coherence.” It is obvious that negligent copying, made once or several times in the course of the tradition, produces documents which are genealogically coherent but more or less unlike.<sup>309</sup>

We know that people have always been interested in having as good a text as possible of what they read, and consequently, a text has always been open to change. Besides, it has always suffered from human carelessness. Readers and scribes have contributed to these changes, inventing readings themselves, being influenced by other texts, making their own mistakes. Hence the starting-point when considering a text must be that the initial reading may be found in any witness and a non-initial reading also in any witness. Anything can happen, but will it always happen? Certainly not, but we must take into account that it may happen. About the New Testament we know that in the course of time, countless readers and countless scribes made innumerable changes, intentionally or unintentionally. Consequently, considering the fate of the initial text,

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305 Mink (2004), p. 67.

306 Mink (2011), p. 192, n. 85.

307 Mink (2011), p. 202 ff.: 15. *Conclusions: How the CBGM Copes with Contamination: The Character of the Iterative Process.*

308 Mink (2011), p. 202.

309 Cf. p. 73 f.

it may be useful to consider the well-known adage: If anything can go wrong, it will.

I think that when choosing what we think is the best reading, we must start from the context; what can the author possibly want to say now, in this context? Here our judgement is highly fallible. Second comes the linguistic question; what does the Greek language allow, what does it not allow? Here our knowledge is highly unsatisfactory. The third notion may be the general character of the manuscript, whether the text is transmitted in a careless way or not, whether the scribe or reader or some scribe or reader before them had a tendency to leave out, to add, to condense, to change. However, such tendencies may be shown to exist in singular documents but as far as I know, they generally have small impact on the tradition.

The serious mistakes of the CBGM are in my opinion:

- The method starts from statistics comparing passages, but those passages are far from comparable; consequently, the basic statistics are of no value, and the method breaks down at the start

- All variants are considered of equal value

- There is no clearly defined difference between coherence and relationship

- Local stemmata are constructed on weak or false foundations

- A flow is supposed to exist between texts, although the individual readings of these texts almost always point in various directions

- Very small differences in prior and posterior readings are supposed to indicate the so-called flow between texts

- General likeness to presumed ancestors has after reassessments an impact on the decision concerning individual passages

- Supposed environment, a factor almost totally unknown, is considered to have an impact on descendant manuscripts

- In an arbitrary way, different readings are supposed to contribute to forming later, posterior, readings

- Contamination is underestimated, differences in readings preferably being derived from more or less unlike readings in ancestors

- Coincidental emergence of identical readings is overestimated

- The ‘rule of parsimony’, reckoned as a guideline, is not applicable

- An active role of scribes and ‘concerned readers’ is underestimated

- There is no answer to crucial questions, like: When is agreement strong enough to hint at relationship, when is a variant different enough to be connective, how far away from the starting-point are we prepared to go in order to explain all variants and find relationship, what is the definition of passages showing ‘no relation’?

In my opinion, owing to highly unsatisfactory statistics, the CBGM can roughly show likeness between documents but not relationship between them. It may give an overview, especially concerning the Byzantine tradition, where documents agree both in essential and trivial readings. I do not think that the method is of any value for establishing the text of the New Testament.

Has the CBGM done much damage? I do not think so. Fortunately, ECM2 still sticks to ‘reasoned eclecticism’<sup>310</sup>. In chapter 4 below, there is a commentary on the textual changes in ECM2 and NA 28 as against NA 27. I think that EMC2 comes out quite well.

Why has the CBGM been worked out? One reason is, I guess and it is certainly only a guess, that a fundamental and totally mistaken idea is that of organic growth.<sup>311</sup> A living being is the ancestor of another living being, and the traces of the ancestor may be seen even in a remote descendant. If the evolution of a text is regarded in the same way, it may be tempting to search for the ancestor even if the likenesses in the supposed descendant are insignificant or non-existent, thereby discarding other explanations. A document is an artefact, its text is composed from other artefacts, it may be changed, augmented, shortened but not by itself but by human beings. The text is not a stream. A stream proceeds by itself from one point to another, the text of a document is fixed and does not change by itself. Neither is the text a being in a genealogical chain, it does not by itself carry the traces of an ancestor but does so according to the choice of a human being, and that human being may change the text intentionally or unintentionally.

Another reason could be love of statistics. Statistics are certainly fascinating, but the statistics of the CBGM constantly count what is important as equivalent to what is unimportant, and take account of some disagreements and disregard others that are equally important.

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310 P. 31\* f.

311 See p. 70, with n. 119.

## Chapter 4. Editio Critica Maior, 2nd Revised Edition (ECM2)

*Novum Testamentum Graecum*. Editio Critica Maior. Edited by the Institute for New Testament Textual Research. 4. Catholic Letters. Edited by Barbara Aland, Kurt Aland †, Gerd Mink, Holger Strutwolf, and Klaus Wachtel. P. 1. Text. T. 2. Supplementary Material. 2nd Revised Edition. Stuttgart, Deutsche Bibelgesellschaft 2013. Everything is presented in both German and English.

### Content and aims of ECM2

The first instalment of the Editio Critica Maior appeared in 1997 and was followed by three others, completing in 2005 the presentation of the Catholic Letters. The first volume of the revised edition contains introduction, text, and critical apparatus. The second volume is an additional apparatus which takes up information too voluminous for the first volume.

The reason for the new edition is, according to the preface, a more complete application of the Coherence-Based Genealogical Method (CBGM), which I have treated rather thoroughly in chapter 3. I have tried to show that the method is inadequate, but I think that it has not done much damage, see below *Comparing ECM2 and NA 27*. The great advantages of the Edition remain: It is thoroughly done, concerning the Greek tradition it is as complete as anyone can wish, and the choice of readings is done with ‘reasoned eclecticism’. After revision, the text was changed in 34 places. In my opinion, these changes are mostly for the better, as we shall see. I suppose that this is a result of the ‘reasoned eclecticism’, because I cannot see how the CBGM can help. Reading the introduction<sup>312</sup>, however, you may fear the worst: “Reconstructing the initial text basically is a matter of assessing the genealogical relationship between variants and thus the genealogical relationship between their witnesses.” It seems that the ‘reasoned eclecticism’ has suddenly been abandoned to the advantage of the CBGM, but in fact, I do not think that this is the case.

The goals of the edition are set forth as follows.<sup>313</sup> One goal is to present all the variants found in the selected Greek manuscripts and in citations by the Greek Fathers, another to give evidence of the three most important early versions, that is the Latin, Coptic and Syriac. Another

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312 P. 34\*.

313 P. 21\*.

aim of the edition is to present all material relevant to the textual history of the first millennium and to reconstruct the initial text.<sup>314</sup>

How then are the manuscripts and other documents selected? There are 522 complete manuscripts and larger fragments of the Catholic Letters. Most of them, 372 in number, present a text which is to more than 90% that of the 98 test passages in *Text und Textwert*.<sup>315</sup> Only a selection of them is represented in the apparatus, where they go under the denomination Byz. We are not informed how the selection is done, but for James they are close to a hundred and for the other letters at least more than 40. All documents showing more than 10% deviation from the Majority text have their readings registered, together with the early papyri; for the letter of James the readings of 183 documents are taken up. I think that this is quite enough. We can be reasonably sure that everything of interest is there. In vol. 2, *Additional Greek readings* are registered, taken from *Text und Textwert* and from the editions of Tischendorf and von Soden. Maybe this painstaking work could have been spared; there is always some odd reading to be found, but let those interested in them consult the works they are taken from. The lectionaries have been used in a restricted way, since it has been made clear that they only offer the Byzantine tradition.

Under 3. *The textual witnesses*<sup>316</sup> the introduction gives us a general idea of the manuscripts, but nothing is said about the papyri. The relevant ones are used for the edition, and in vol. 2 they are registered among the manuscripts. I think that the reader misses an overview of the papyri and their contribution to the text. Leaving out the papyri in the introduction looks like an attempt to reduce the value of them. It is an idea of Barbara Aland's that the text has been established firmly at an early age, and that opinion has, I think, had an influence on the ECM2. Cf. above the quotation<sup>317</sup> from the *Introduction* to ECM2 and the *Summary* of chap. 2<sup>318</sup>. Most of the papyri are fragmentary<sup>319</sup> and thus cannot be of much use to the CBGM, which may have contributed to their absence in the introduction. See below *Some remarks on readings in the papyri*.

Only the quotations from Greek Fathers are registered. Finding out what the readings of the Fathers actually are is far from easy. The difficulties are well known and much work remains to be done.<sup>320</sup> The

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314 P. 30\*.

315 Cf. pp. 58, 64 with n. 103.

316 P. 21\*.

317 P. 45.

318 P. 56.

319 In P72, 1 Pt, 2 Pt and Jd are preserved completely.

320 Cf. Aland & Aland, p. 171 ff.

lemma in a commentary may be influenced by another tradition and be changed; the same goes for a quotation anywhere, which may also be made from memory or be more like an allusion or a paraphrase, etc. However, ECM2 is not quite satisfactory regarding what knowledge we have<sup>321</sup>: “a true quotation is one where the wording of the Father’s text is identical with a reading found in the manuscript tradition.” I think that the most reliable text is the one we can conclude from the way the Father comments on the text, next to that a direct quotation which shows an unusual text which probably was not taken over from another, better known tradition.<sup>322</sup> A lemma or a direct quotation presenting a well-known text may or may not be reliable. GNT has a good summary of the problem<sup>323</sup>: “Citations from the commentary part are frequently more faithful than the lemma of a commentary manuscript in witnessing to a Father’s text, because later copyists would often replace the whole New Testament text of a manuscript with a text of a completely different type.”

But should we not consider the Latin, Syriac etc. Fathers of equal importance? EMC2 regards Augustine as an exception<sup>324</sup>; it registers what he has read, but only if the reading is peculiar to him or first attested by him. So we find<sup>325</sup> that he has read ῥοπηῆς in Jas 1:17/44, but not where<sup>326</sup>, whereas we get such information concerning the Greek Fathers. In 2 Pt 1:19/24–26, Augustine does not follow the very dominant reading λύχνῳ φαίνοντι but only quotes *lucerna*<sup>327</sup>, a singular reading not noted in ECM2. This may be a ‘Latin’ fault; perhaps *lucenti* fell out after *lucerna*, a case of homoeoarcton. From another field we can see that Julian of Eclanum has texts which are not known to the Septuagint.<sup>328</sup> Those are just a few examples, but what about all quotations in Augustine, and those of the other non-Greek Fathers? It would be a very good thing to know as much as possible about them, but that would belong to another project, demanding an immense amount of work.

Among the early versions, the Latin, Coptic and Syriac ones are consulted to the extent possible, whereas the Armenian, the Georgian, the Old Church Slavonic and the Ethiopian ones are taken into account only if they witness to variants in the Greek text and if they are available in

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321 ECM2, p. 23\*.

322 Alexanderson (2012), p. 72 f.

323 GNT, p. 38\*, n. 12.

324 Vol. 2, p. 63\* ff.

325 P. 2, p. 103.

326 E.g. *De Genesi ad litteram* V, XVIII, 36; *De civitate Dei* XI, 21; *De trinitate* I, 1, 2.

327 *De Genesi ad litteram* IV, XXIII, 40.

328 Cf. Alexanderson (2012), p. 61.



editions. We shall thus find Latin, Coptic and Syriac readings reported everywhere, those of the other versions only if they diverge from the reading called *a*, the one presented in the text, in the so-called primary line. I think that this is a wise and necessary decision.

### Some remarks on readings in the papyri

Many readings of the papyri, they may be right, they may be wrong, are of great interest for establishing the text, and I think that an overview of these documents, a summary of their importance, ought to have been presented in the introduction, where important reflections on the relationship between manuscripts are brought to the fore. Maybe relationships between the papyri cannot be stated, maybe we cannot say anything else than that they have sometimes good, sometimes bad readings; if so, that is valuable knowledge and would warn a reader against putting too much trust in old documents. But one thing is certain: Usually they have been copied fewer times than manuscripts. Below are some interesting readings. They throw some light on the fate of the text in an early period of its existence.

1 Pt 2:3/2–4 εἰ ἐγεύσασθε (ὅτι χρηστὸς ὁ κύριος) ECM2 εἰ ἐπιστεύσατε? Jerome εἰ ἐγεύσασθε ἐπιστεύσατε P72. Jerome has *si credidistis* in two quotations, a reading known otherwise only in the conflated reading of P72.

1 Pt 2:19/12–14 (εἰ διὰ) συνείδησιν θεοῦ (ὑποφέρει τις λύπας πάσχω ἀδίκως) ECM2 συνείδησιν ἀγαθὴν θεοῦ P72 81 συνείδησιν θεοῦ ἀγαθὴν 02\* 33 2344 συνείδησιν ἀγαθὴν *multi*. The bulk of the tradition is divided between συνείδησιν θεοῦ and συνείδησιν ἀγαθὴν. Must we not consider συνείδησιν θεοῦ a difficult reading and συνείδησιν ἀγαθὴν a simplification?<sup>329</sup> The interesting thing is that the inferior reading appears very early.

1 Pt 3:3/12–14 (ὁ ἔξωθεν) ἐμπλοκῆς τριχῶν (... κόσμος) ECM2 with most of the Greek tradition and some secondary tradition, ἐμπλοκῆς (without τριχῶν) P72, a few manuscripts, also with some secondary tradition: Clement of Alexandria, Latin and Coptic versions. For τριχῶν speaks that we get three parallels, ἐμπλοκῆς τριχῶν, περιθέσεως χρυσίων, ἐνδύσεως ἱματίων, against it that it could be an explication. Both readings are attested in the secondary tradition, but Clement may

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329 So Metzger, p. 619.

be a stronger name than Pseudo-Oecumenius whose date is unknown, sometimes given as 6th c.

1 Pt 3:7/34–36 χάριτος ζωῆς ECM2 χάριτος ζωῆς αἰωνίου P72 and a Syriac version (Peshitta). An addition near at hand, appearing early.

1 Pt 3:8/18 ταπεινόφρονες ECM2 with P72 and a large tradition, φιλόφρονες is also well attested. It is next to impossible to choose. The context is about living together in a peaceful and friendly way, which would speak for φιλόφρονες, but on the other hand, ταπεινόφρονες is a *lectio difficilior*, introducing something new.

1 Pt 3:14/28–30 (μὴ φογηθῆτε) μηδὲ παραχθῆτε ECM2 with most of the tradition, om. P72 with a few manuscripts, among them 03, and some secondary tradition. The longer text can be corrected after Is. 8:12, but may also have fallen out because of a homoeoteleuton.

1 Pt 4:16/26. The readings μέρει and ὄνομαι are fairly equally distributed in the tradition. P72 with ὄνομαι is no doubt wrong. This is an early example of an explanatory reading.<sup>330</sup>

2 Pt 1:21/28–32 ἀπὸ θεοῦ ἄνθρωποι ECM2 with P72 and a strong tradition, ἅγιοι θεοῦ ἄνθρωποι an equally strong tradition. There are also conflation of these two readings. Impossible to decide.

2 Pt 3:11/14–16 (ποταπὸς δεῖ) ὑπάρχειν ὑμᾶς (ἐν ἀγίαις ἀναστροφαῖς) ECM2 with most of the tradition; there are also ὑμᾶς ὑπάρχειν, ὑπάρχειν ἡμᾶς and ὑπάρχειν ἑαυτοῦς. ὑπάρχειν without ὑμᾶς is found in P72\* P74<sup>vid</sup> 03 1175. It is very possible that the shorter reading is correct and the others explanatory.

1 Jn 4:15/36–40 (ὁ θεὸς ἐν αὐτῷ μένει καὶ αὐτὸς) ἐν τῷ θεῷ ECM2, but there is also ἐν αὐτῷ in a dozen minuscules and in some Latin manuscripts. P9<sup>vid</sup> has ἐν αὐτῷ ἐστίν. ἐν τῷ θεῷ looks like an explanation and could very well be wrong.

Jd 15/14–16 πᾶσαν ψυχὴν. See p. 104. Whether πᾶσαν ψυχὴν is right or wrong, it indicates that a major reading may have a very tenuous tradition.

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<sup>330</sup> Cf. p. 137 for this passage.

These passages seem to me to indicate that an interesting reading may be old and be found only in a tenuous tradition, like 1 Pt 2:3/2–4; 2 Pt 3:11/14–16; 1 Jn 4:15/36–40; Jd 15/14–16. Other readings in papyri witness to early ‘improvements’ and explanations introduced into the text, like 1 Pt 2:19/12–14 ; 1 Pt 4:16/26 ; 1 Jn 4:15/36–40. We may also find an influence from traditional Christian language early, like 1 Pt 3:7/34–36.

### **What is ECM2, what is it not?**

Summing up, ECM2 is concentrated on the Greek tradition, and there it is as comprehensive and thorough and well-ordered as anyone can wish, with one exception: In the introduction, one misses an overview of the papyri, although they are of course fully represented in the edition. There are other things we would like to know, but we must go elsewhere to find them; I am thinking of other traditions than the Greek one and also of parallel passages in the Bible, which are not presented. We should be grateful for what it is; an indispensable tool made with the utmost care. Filling in what it is not means an enormous amount of work and must be, I suppose, a matter of vast projects.<sup>331</sup>

### **The presentation of text and tradition**

The layout of vol. 1 features 1) the primary text line, 2) the overview of variant readings, 3) the critical apparatus. In the primary line, every word has a number, which makes it easy to identify readings and variants, especially those extending over several words. Sometimes the primary line has an alternative, another reading being considered equal. I do not think this a very good idea. There are many, I should say innumerable, cases where one reading is about as good as another. I think that the split primary line shows that the editors are too keen on proclaiming one, if necessary two, readings as correct, the others as wrong. A high ambition, maybe laudable, but there is in many places so much uncertainty that such a selection becomes arbitrary.

The overview of variant readings comes very handy. You get rapidly an idea of existing variants before diving into the apparatus, which is by nature rather cumbersome since it presents all the witnesses of all the different readings. With the high ambition, the apparatus must be extensive and comprehensive. It may at first look rather difficult, but the explanations are good and after a while the reader will feel at home.

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331 Among other things, the Institute at Münster is working on the Syriac and Coptic tradition, see its home page.

The apparatus is extended into vol. 2, where there is a list of the Greek manuscripts and information about, among other things, the Byzantine manuscripts used for the different letters, which are not the same for all Letters, the selection of lectionaries, patristic quotations, readings of the versions. Vol. 2 is the result of incredibly painstaking work. As with the apparatus, it may take some time before one gets used to it, but it is presented in a clear way, and working with the text you will find it indispensable. Sometimes, you may get the impression that you learn much more than you ever wanted to know!

### **The ranking of manuscripts in ECM2**

A section of the *Introduction* is called *Notes on the text of the second edition of the Catholic Letters*.<sup>332</sup> It states the aim of the edition, to present all material relevant to the textual history of the first millennium and to reconstruct the *initial text*, called *A*.<sup>333</sup> Since the CBGM plays an important role, it is of course presented. A result of the method is that it is considered possible to see to what extent a document is close to the hypothetical initial text called *A*. This closeness builds upon the statistical method of the CBGM, which in my opinion is inadequate and gives poor if any guidance. Nevertheless, these tables give food for some reflections. We find a table of manuscripts which have *A* as their most closely related ancestor, and the percentage of agreement with *A* is given. We see for instance that 03 (codex Vaticanus) is very close to *A*, which does not surprise us. Compared with *A*, it agrees to 96.856% and is descendant 1. Next in rank is, surprisingly, 1739, a minuscule of the 10th century, which ranks considerably higher than for instance 01 (codex Sinaiticus). Also 468, for all Catholic Letters counted as part of the Byzantine tradition (Byz), and 025 with 1175, both for some Letters labelled Byz<sup>334</sup>, rank higher than 01. It might be of some interest to see how these and some other manuscripts fare if we compare them with the 98 test passages of *Text und Textwert*, which form a basis of the CBGM.<sup>335</sup> I compare 01 (not very high-ranking, descendant 44), 03 (descendant 1), 1739 (descendant 2), but also 025 (partly Byz, high-ranking, descendant 6), 468 (totally Byz, high-ranking, descendant 4), 1874 (totally Byz, not high-ranking, descendant 93). These positions are taken from the module *Potential Ancestors and Descendants* in

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332 P. 30\* ff.

333 Cf. p. 68 for the *initial text*. This text, *A*, is the same as the text presented in the primary line of the edition.

334 025 is labelled Byz for 1 Pt, 2–3 Jn and Jd, 1175 for 1–3 Jn and Jd.

335 Cf. pp. 58, 64 with n. 103.

*Genealogical Queries*. It is striking that a difference of only 1% agreement in the table of *A*'s descendants makes a manuscript drop more than 20 positions, for instance from 4 to 29 or from 29 to 52.

Be it said that I do not accept these 98 passages as giving good guidance. They were chosen because they are supposed not to have emerged coincidentally but show a relationship to the exemplar (Vorlage) they derive from and belong to different types of the tradition.<sup>336</sup> On the contrary, I think that many passages are exactly of the type that could have appeared coincidentally and say nothing as to relationship. Examples of this have been given above.<sup>337</sup> Certainly, many passages do give an indication, and below I have taken up more than twenty cases which I think are of that indicative type.

However, a comparison might be of interest because these 98 passages have played a part in developing the CBGM and because we can compare these passages with the text of ECM2. As the 'right' text I take the text of ECM2, which in most cases but not always is the same as the one preferred in *Text und Textwert*. The result of the comparison<sup>338</sup> is that 01 agrees around 60 times, 03 around 85, 1739 around 70, 025 (with 9 lacunas) around 40, 468 and 1874 16–17. The high-ranking 468 does not come off very well, no better than just any Byzantine manuscript like 1874.

Further, I have picked a row of passages which in my opinion distinguish between manuscripts to a higher degree than many of the other passages in *Text und Textwert*, and added them to those of the *Text und Textwert* which are indicative. Their total number is 39 passages. The documents compared are the same as above. The reading considered correct is to the left:

Jas 2:4/2–6 (καὶ) οὐ διεκρίθητε / διεκρίθητε (no. 9 in *Text und Textwert*)

Jas 2:18/30 χωρίς / ἐκ (11)

Jas 2:20/26 ἀργή / νεκρά (13)

Jas 2:25/26 ἀγγέλους / κατασκόπους

Jas 3:8/16 ἀκάστατον / ἀκατάσχετον (17)

Jas 3:12/22–30 om. / πηγῆ

Jas 4:4/1 μοιχαλίδες / μοιχοὶ καὶ μοιχαλίδες (18)

Jas. 5:7/44–45 λάβη / λάβη ὑετόν / λάβη καρπὸν (21)

Jas 5:10/10 κακοπαθείας / καλοκαγαθίας

Jas 5:12/62 κρίσιν / ὑπόκρισιν

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<sup>336</sup> *Text und Textwert* 2/1, p. VII.

<sup>337</sup> P. 36, n. 99.

<sup>338</sup> The result is expressed with caution. There might be some small errors in the hand-made calculation.

1 Pt 1:22/18 ἀληθείας / ἀληθείας διὰ πνεύματος (26)  
 1 Pt 1:23/8 ἐκ σποράς (φθαρτῆς) / ἐκ φθοράς (φθαρτῆς)  
 1 Pt 1:23/26 μένοντος / μένοντος εἰς (τὸν) αἰῶνα (28)  
 1Pt 1:24/18 αὐτῆς / αὐτοῦ / ἀνθρώπου  
 1 Pt 2:19/6–7 χάρις / χάρις παρὰ θεῶ  
 1 Pt 2:19/12–14 συνείδησιν θεοῦ / συνείδησιν ἀγαθῆν  
 1 Pt 3:16/24 καταλαλεῖσθε / καταλαλώσιν/-οῦσιν ὑμῶν ὡς κακοποιῶν (32)  
 1 Pt 4:1/6–8 παθόντος σαρκί / παθόντος ὑπὲρ ὑμῶν σαρκί (33)  
 1 Pt 4:14/20–21 δόξης / δόξης καὶ δυνάμεως et sim. (35)  
 1 Pt 4:14/36–37 ἀναπαύεται οἱ ἀναπέπαιται / ἅ. κατὰ μὲν αὐτοὺς βλασφημεῖται  
 etc.  
 1 Pt 4:16/26 μέρει / ὀνόματι  
 1 Pt 5:11/6 κράτος / addition of δόξα (38)  
 1 Pt 5:13/2–12 om. / ἐκκλησία  
 2 Pt 1:14/16 σκηνώματος / σώματος  
 2 Pt 2:13/2 ἀδικούμενοι / κομιούμενοι (44)  
 2 Pt 2:17/12 ὀμίχλαι οἱ ὀμίχλη / νεφέλαι (45)  
 2 Pt 3:10/12–13 κλέπτῃς / κλέπτῃς ἐν νυκτί (48)  
 1 Jn 2:20/20 πάντες / πάντα (59)  
 1 Jn 2:27/8 χρῖσμα / χάρισμα  
 1 Jn 2:27/48 χρῖσμα / πνεῦμα  
 1 Jn 3:14/34 ἀγαπῶν / ἀγαπῶν τὸν ἀδελφὸν (αὐτοῦ) (64)  
 1 Jn 4:3/16 Ἰησοῦν / Ἰησοῦν ἐν σαρκί ἐληλυθότα (67)  
 1 Jn 4:8/2–14 ὁ μὴ ἀγαπῶν οὐκ ἔγνω τὸν θεόν / om.  
 1 Jn 5:2/36 ποιῶμεν / τηρῶμεν  
 1 Jn 5:6/16 αἵματος / αἵματος καὶ πνεύματος / πνεύματος καὶ αἵματος (71)  
 1 Jn 5:13/8–36 manuscripts with both τοῖς πιστεύουσιν and ἵνα πιστεῦ(σ)ητε  
 are considered wrong (74)  
 2 Jn 9/6 προάγων / παραβαίνων (79)  
 Jd 15/14–16 πᾶσαν ψυχὴν / πάντας (τοὺς) ἀσεβεῖς (92)  
 Jd 24/12 ἀπταίστους / ἀπταίστους καὶ ἀσπίλους (95)  
 Jd 25/2–3 θεῶ / σοφῶ θεῶ

The result of this comparison<sup>339</sup> is that the readings considered correct occur in 01 23 times, in 03 35 times, in 025 (with three lacunas) 17 times, in 1739 29 times, in 468 12 times, in 1874 14 times. The remarkable thing is that 468 does not do well at all, no better than just any Byzantine manuscript.

If 468 is so high-ranking and so close to the hypothetical initial text *A*, where does it agree with this text? In many passages of some inter-

339 The result is expressed with caution. There might be some small but unimportant errors in the hand-made calculation.

est, it obviously does not, as shown above. I have looked at passages where there is variation concerning the word δέ in the Letter of James. There are between 50 and 60 variants comprising δέ or omitting it. I have only found one passage where there is disagreement between *A* and 468: Jas 1:19/10–12 ἔστω δέ *A* ἔστω 468 with the Byzantine tradition. It should be observed that for establishing percentages, the CBGM counts all variants, which in James are 761. So it does not matter whether in one reading *A* and 468 agree with almost all documents and only one or few documents show disagreement<sup>340</sup>, or the variants are so divided that *A* and 468 have one variant and as many or more documents the other<sup>341</sup>. In both cases, 468 agrees with *A*, and this agreement goes into the percentage. This is at least how I have understood the way the CBGM counts. But in one case we could speak about a scribal slip of no consequence in one or a few manuscripts, and we would not consider following this reading; in the other case there is a firmly rooted tradition in contrast to the tradition of *A* and 468, and we would have to think twice about it. So, as to rather unimportant readings, there is great agreement between *A* and 468, perhaps 95–99%, which contributes to the high ranking of 468. This lends some support to an idea which is close at hand: Scribes and readers take small things for granted but ask themselves about more interesting passages, and there they are prone to changes. Maybe they do exactly as we do: We ponder over what we think important and take what we think unimportant in our stride.

If we look at 01 and 1739 the same way as above for 468, the result is that they both mostly follow *A*, there are 2–4 divergencies in both of them. So both these manuscripts seem to be fairly close to *A* both in larger and in smaller matters, in contrast to 468.

Consequently, it seems to me that the percentages and the positions as close descendants of *A* are footed upon faulty statistics in the first place, the percentage of agreements being downright wrong.<sup>342</sup> Further, the agreements and disagreements are of a very different kind, many agreements concerning very small matters, many disagreements being of greater importance. These percentages only show a very superficial agreement and certainly cannot contribute to establishing a better text.

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340 An example: Jas 1:12/3 μακάριος δέ ἀνὴρ 631, μακάριος ἀνὴρ *A* 468 and all other tradition. This is nothing but a slip in 631.

341 An example: Jas 2:15/3 εἰν ἀδελφός *A* 468 with around 50 manuscripts, εἰν δέ ἀδελφός around 40 directly indicated manuscripts and most of the Byzantine tradition. There are two traditions, one with, one without the particle.

342 Cf. p. 63, *Statistics*.

### 03, 1739 and changes in 1 Jn

Aland thinks that texts are transmitted comparatively unchanged, that scribes have not interfered much with them.<sup>343</sup> She also regards the activity of ‘theologians’ as of small importance: Such interference is supposed to be rare and usually without much impact on the tradition as a whole.<sup>344</sup> We shall have a look into the First Letter of John. It is certainly true that theological interference plays no great part in this text, but there are a few cases, and such interference is intentional. To such cases I add below numerous cases where I think that we find interference of another kind, but still intentional: Changes made in order somehow to ‘improve’ the text. A comparison is made between the text of the ECM2 and the manuscripts 03 and 1739. The editors of ECM2 think that these manuscripts are closest to the initial text.<sup>345</sup> Can we find clear traces of such intentional interference in these manuscripts, and if they are there, have they played a role in the tradition, or did scribes and readers pay no attention to such readings? We shall look into passages where 03 or 1739 or both of them deviate from the text of ECM2, but also consider some passages where the primary line of ECM2 is attested by both 03 and 1739, but where nevertheless that very text may have been changed intentionally and, consequently, be inferior. We shall perhaps get a glimpse of the general value of 03 and 1739<sup>346</sup>, held in high regard, especially 03, by the editors. The tradition preserving one or the other reading is indicated in a summary way, full information being found in ECM2. By ‘manuscripts’ I mean manuscripts outside the Byzantine tradition (Byz in ECM2). I consider the passages of interest to be the following:

1 Jn 1:2/11 ἐωράκαμεν. So 1739 and a great majority of the tradition. 03, a small minority and a Coptic version have ὁ ἐώρακαμεν, which could be an adjustment to the preceding ὁ ἀκηκόαμεν, ὁ ἐώρακαμεν and ὁ ἐθεασάμεθα. 03’s reading is probably an intentional grammatical ‘improvement’.

1 Jn 1:4/2–20 καὶ ταῦτα γράφομεν ἡμεῖς, ἵνα ἢ χαρὰ ἡμῶν/ὕμῶν ἢ πληρωμένη. The variants are: ἡμεῖς 03 and a few manuscripts, ὕμῶν a large majority of manuscripts, among them 1739; ἡμῶν 03 and many manuscripts, also versions, ὕμῶν more numerous manuscripts, among

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<sup>343</sup> See chap. 2, also Aland (2011), p. 64 ff.

<sup>344</sup> Aland (2011), p. 64 f.

<sup>345</sup> ECM2, p. 33\*.

<sup>346</sup> I certainly do not mean that the general value of a manuscript is of interest for the special passage.



them 1739, also versions. ἡμῶν and ὑμῶν are regarded as equal and both put into the primary line. I think that ὑμῶν is right, because ἡμεῖς is a *lectio facillior*, adjusted to the preceding γράφομεν. Accordingly, ὑμῶν ought to be right. But on the other hand, ὑμῶν can be a *lectio facillior*: A scribe would find it natural that the intention of the writer is to make the addressees happy.<sup>347</sup> And are these changes, whatever they are, intentional? We cannot be sure of that, itacism and the general uncertainty about endings making the judgement difficult or impossible.

Let us, however, think that we have to do with intentional changes. If ὑμῶν and ὑμῶν are correct readings, most manuscripts and most versions keep the original reading ὑμῶν, and ἡμεῖς is an early intentional change, represented in 03 and a few other manuscripts, not much represented in versions. Also ὑμῶν is kept in a majority of manuscripts and in most versions, but ἡμῶν has a stronger Greek and non-Greek tradition than ἡμεῖς. If we prefer ἡμεῖς and ἡμῶν as original readings, most of the tradition has been influenced by an intentional interference. We cannot judge, but we must keep the possibility open that there has been important interference.

1 Jn 1:5/10 ἀγγελία 03 with many manuscripts and the Byzantine tradition. 1739 with the majority of the other manuscripts presents ἐπαγγελία. Also at 1 Jn 3:11/10 there is the same division between 03 and 1739, with less but still substantial support for ἐπαγγελία. This variant is probably a false, ‘theological’ correction. It does not fit very well into the context, which is not about a promise but rather states what God is like and, in 1 Jn 3:11/10, what is expected from a Christian.

1 Jn 1:7/3. Asyndeta are quite common in 1 Jn, but we often find that there is a variant with a particle in the more normal Greek way. Here 03 with an overwhelming support in the tradition has the particle δέ. We cannot decide whether this is a normalization and a posterior reading or if it is the prior reading, the particle having been dropped in a few manuscripts, among them 1739.

1 Jn 2:4/2–10 ὁ λέγων ὅτι ἔγνωκα αὐτὸν or ὁ λέγων ἔγνωκα αὐτὸν. ECM2 places both ὅτι and the omission of the word in the primary line. Both 03 and 1739 have the word, together with a vast majority, but it may also be a grammatical correction in order to avoid asyndeton.

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<sup>347</sup> So Metzger, p. 639.

1 Jn 2:5/16. A great majority of manuscripts, among them 03, have ἀληθῶς, a handful of them do not, among them 1739. Two manuscripts have ἀληθῶς at another place. ἀληθῶς could be an addition, influenced by the preceding ἀλήθεια and put into the text in order to convey a reinforcement. There is no reason why it should fall out, but after all, no reason is necessary; words sometimes do fall out. I think that there is no conclusion to be drawn.

1 Jn 2:6/24. οὕτως and the omission of the word are regarded as equal in ECM2. A vast majority with 1739 has the word, a smaller but still respectable number of manuscripts with 03 reject it. As above, concerning 1 Jn 2:5/16, there can be no conclusion.

1 Jn 2:7/2. There are two variants, ἀγαπητοί (03, 1739, most manuscripts except the Byzantine ones) and ἀδελφοί (some manuscripts, the Byzantine tradition). This must be an intentional change, but it is impossible to say in what direction it goes. The fact that ἀδελφοί is rare in 1 Jn does not indicate that it is a less probable reading, which it does to Metzger.<sup>348</sup> It can be, but certainly it does not have to be just the other way around, the rare reading being considered a *lectio difficilior*. It is, however, possible that ἀδελφοί comes from a reading known in lectionaries.<sup>349</sup>

1 Jn 2:12/6. An overwhelming majority, including 03, has τεκνία, 1739 and around 10 manuscripts have παιδία. Of course we do not know which is the initial reading. A strong influence has worked towards τεκνία or a much less strong one towards παιδία.

1 Jn 2:14/16–30 ἔγραψα ὑμῖν, πατέρες, ὅτι ἐγνώκατε τὸν ἀπ' ἀρχῆς. 03 and two manuscripts together with some versions have τὸ, all other tradition with 1739 presents τὸν. If it were not for v. 13/10–16 ἐγνώκατε τὸν ἀπ' ἀρχῆς, there would be no doubt in my mind that 03 is right. Reminding the addressees about what they know of old is an important theme of the Epistle and is brought to the fore immediately in 1 Jn 1:1. It brings home that the old message is valid, sometimes, as here, expressed in the neuter: cf. 1 Jn 1:1/2–26 ὃ ἦν ἀπ' ἀρχῆς, ὃ ἀκηκόαμεν, ὃ ἐώρακάμεν τοῖς ὀφθαλμοῖς ὑμῶν, ὃ ἐθεασάμεθα, 2:24/4–10 ὃ ἠκούσατε ἀπ' ἀρχῆς, *ibid.*/26–32 ὃ ἀπ' ἀρχῆς ἠκούσατε. ἀπ' ἀρχῆς is also combined with ἐντολή and ἀγγελία in 1 Jn 2:7/24–26 and 3:11/16–18. But v.

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348 Metzger, p. 640.

349 Metzger, *ibid.*

13 has τὸν ἀπ' ἀρχῆς, and only one manuscript, 044, Didymus the Blind and a part of the Latin tradition have τὸ, but after all, this means that τὸ is old. The τὸν of v. 13 has certainly influenced the choice of τὸν also in v. 14. It seems that the designation ὁ ἀπ' ἀρχῆς for God is not known from elsewhere in the New Testament. I think that τὸ is the right reading in both v. 13 and v. 14, but certainly an editor may hesitate.

1 Jn 2:17/4–16 ὁ κόσμος παράγεται καὶ ἡ ἐπιθυμία αὐτοῦ or ἡ ἐπιθυμία without αὐτοῦ. The readings with αὐτοῦ (a vast majority with 01 and 03, including the Byzantine tradition and many versions) and that without it (some twenty manuscripts, among them 1739 with not much documentation beside the Greek manuscripts) are considered equal. αὐτοῦ goes back to v. 16/4–30 πᾶν τὸ ἐν τῷ κόσμῳ, ἡ ἐπιθυμία τῆς σαρκὸς καὶ ἡ ἐπιθυμία τῶν ὀφθαλμῶν. It is very possible that αὐτοῦ is a clarifying addition which has been successful very early.

1 Jn 2:20/2–20 καὶ ὑμεῖς χρῖσμα ἔχετε ἀπὸ τοῦ ἁγίου καὶ οἴδατε πάντες. As for πάντες, the support is not overwhelming (01, 03 and few manuscripts, some versions), the variant πάντα on the contrary is supported by the bulk of the tradition, including 1739 and the Byzantine tradition. This may be a case of unintentional variation of endings, an extremely frequent phenomenon. Maybe πάντα is a bit ahead, considering the omnipresent allusions to the knowledge and experience of the addressees: They already know everything. But *non liquet*.

1 Jn 2:23/2–34 πᾶς ὁ ἀρνούμενος τὸν υἱὸν οὐδὲ τὸν πατέρα ἔχει, ὁ ὁμολογῶν τὸν υἱὸν καὶ τὸν πατέρα ἔχει. The bulk of the tradition, including 03 and 1739, has ὁ ὁμολογῶν τὸν υἱὸν καὶ τὸν πατέρα ἔχει, around 15 Greek manuscripts and the Byzantine tradition omit. Of course the loss may have occurred as a result of a homoeoteleuton, but it is strange that the Byzantine tradition, inclined to adding and completing, has on the whole stubbornly refused to enter the phrase. Or did someone prune the sentence, considering the latter half superfluous? If that is the case, it is strange that this pruning had such a wide-spread effect. As to the content, the omitted phrase could not be offensive. Consequently, it seems that the Byzantines had some good reason for not writing the phrase, the reason being that they knew of a tradition considered to be valuable. Maybe they were right.

1 Jn 2:27/6–8 τὸ χρῖσμα. 03 has τὸ χάρισμα, 1739 τὸ χρῖσμα. 03 has an unintentional error which had very little following, owing to the fact that the χρῖσμα is important in the context, cf. v. 20/6 and v. 27/48.

1 Jn 3:7/2. It is difficult to see why ECM2 here has παιδία with 1739 against the bulk of the tradition, including 03, which has τεκνία, whereas in 1 Jn 2:12/6 we find τεκνία with the bulk of the tradition accepted and παιδία relegated to the apparatus.

1 Jn 3:11/10. See 1 Jn 1:5/10.

1 Jn 3:13/2. 03 with the majority of the tradition has an asyndeton, 1739 with a minority has καὶ at the beginning of the sentence. It is impossible to judge, asyndeta being quite frequent in 1 Jn. Maybe 1739 *et alii* have a normalization.

1 Jn 3:19/2. 03 with the minority of the tradition has an asyndeton, 1739 with a majority has καὶ at the beginning of the sentence. Cf. above 1 Jn 3:13/2. Here too it is impossible to judge, and it is possible that 1739 *et alii* have a normalization.

1 Jn 3:21/4–24 ἐὰν ἡ καρδία ἡμῶν μὴ καταγινώσκη, παρρησίαν ἔχομεν πρὸς τὸν θεόν. 03, 1739 and a few other manuscripts read ἔχει instead of ἔχομεν. It is in my opinion absolutely impossible to know which reading is correct. In the context, we find the plural many times, but the subject can be ἡ καρδία, as it is immediately before. Both ἔχομεν and ἔχει can be a correction of a previous reading. We do not know if the majority has kept the original reading or if a change has gained most of the ground.

1 Jn 4:3/2–26 καὶ πᾶν πνεῦμα ὃ μὴ ὁμολογεῖ τὸν Ἰησοῦν ἐκ τοῦ θεοῦ οὐκ ἔστιν. After Ἰησοῦν the bulk of the tradition, including the Byzantines, has ἐν σαρκὶ ἐληλυθότα. This is obviously taken from 4:2/28–32 and is an early addition, also existent in 01. 03, 1739, a few other manuscripts and some secondary tradition rightly omit.

1 Jn 4:8/2–4:9/42 ὁ μὴ ἀγαπῶν οὐκ ἔγνω τὸν θεόν, ὅτι ὁ θεὸς ἀγάπη ἐστίν. (9) ἐν τούτῳ ἐφανερώθη ἡ ἀγάπη τοῦ θεοῦ ἐν ἡμῖν, ὅτι τὸν υἱὸν τὸν μονογενῆ ἀπέσταλκεν ὁ θεὸς εἰς τὸν κόσμον etc. ECM2 chose with 03 and the majority of the tradition to let v. 9 start as an asyndeton. A lesser tradition, with 1739, lets the verse start with καὶ ὅτι. Probably a grammarian has been at work there, taking offence at the asyndeton and

coordinating the sentence with the preceding ὅτι-sentence. It is hard to see why the two words should have been dropped. The change was only moderately successful.

1 Jn 4:9/32–42 (τὸν υἱὸν αὐτοῦ τὸν μονογενῆ) ἀπέσταλκεν ὁ θεὸς εἰς τὸν κόσμον. Most of the tradition, including 03 and 1739, has this text, but ὁ θεὸς is missing in some manuscripts and in a couple of versions. I do not think that we should make much of the fact that ὁ θεὸς is placed differently in one manuscript and that another has αὐτὸς ἀπέσταλκεν instead of ἀπέσταλκεν ὁ θεὸς. But ὁ θεὸς could very well be an addition.

1 Jn 4:15/22–40 ὁ θεὸς ἐν αὐτῷ μένει καὶ αὐτὸς ἐν τῷ θεῷ. The overwhelming bulk of the tradition, with 03 and 1739, has this reading, but there are a few manuscripts presenting ἐν αὐτῷ instead of ἐν τῷ θεῷ, and P9<sup>vid</sup> has ἐν αὐτῷ ἐστίν. It is possible that most of the tradition has an explanation which comes naturally and was successful, without having much of a ‘theological’ ring about it.

1 Jn 4:16/58–66 ὁ θεὸς ἐν αὐτῷ μένει. About half of the documented tradition, including 03 and 1739, has μένει or μενεῖ, whereas the other half omits it. The Byzantine tradition is divided between the variants. ECM2 accepts the longer reading, but of course the shorter one may be initial.

1 Jn 4:19/2–14 ἡμεῖς ἀγαπῶμεν, ὅτι αὐτὸς πρῶτος ἠγάπησεν ἡμᾶς. This is the reading of a small part of the tradition, including 03 and 1739, whereas most of it presents either αὐτὸν or τὸν θεὸν after ἀγαπῶμεν. Also instead of αὐτὸς, there is the variant ὁ θεὸς in a few manuscripts and in a couple of versions. The addition αὐτὸν or τὸν θεὸν comes naturally and was successful, cf. above 1 Jn 4:15/22–40.

1 Jn 4:20/30–62 ὁ γὰρ μὴ ἀγαπῶν τὸν ἀδελφὸν αὐτοῦ ὃν ἐώρακεν, τὸν θεὸν ὃν οὐκ ἐώρακεν οὐ/πῶς δύναται ἀγαπᾶν. Both οὐ and πῶς are placed in the primary line. A minor part of the tradition, including 03 and 1739, has οὐ, the majority has πῶς. A way of reasoning like ‘if anyone is/does not, so this or that does not occur’ is common in the Epistle; examples are found in chap. 4, another one in 2:23. For a rhetorical question with πῶς, see 3:17/46, other examples occur in the Gospels and Paul. So there is no way of knowing which variant is the initial one. Personally I believe that πῶς is right, a rhetorical device being normalized into the flatter reading with the negation.

1 Jn 5:10/2–60. The context is that the witness of God is better than that of man, and the witness of God is about his Son. Then follows: ὁ πιστεύων εἰς υἱὸν τοῦ θεοῦ ἔχει τὴν μαρτυρίαν ἐν αὐτῷ, ὁ μὴ πιστεύων τῷ θεῷ ψεύστην πεποίηκεν αὐτόν, ὅτι οὐ πέπιστευκεν εἰς τὴν μαρτυρίαν ἣν μεμαρτύρηκεν ὁ θεός. We find in /32–34 τῷ θεῷ, which is the reading of ECM2, of 03, of around 60 recorded manuscripts and of the Byzantine tradition, but τῷ υἱῷ is found in about half that number, among them 1739 in its text. I think that it is very probable that 1739 and others are right. Who believes in the Son has God’s witness about him, who does not believe in the Son makes God a liar. Certainly the other reading is quite possible too, but less striking, since it is just stating the obvious. However this may be, we have here a theological change which has had a great impact if τῷ υἱῷ was changed into τῷ θεῷ, and a more modest one if it was the other way around. Cf. 2:23: “whoever denies the Son has not the Father; who confesses the Son has also the Father.” Believing in the Son means believing in the Father.

1 Jn 5:13/8–36 ἴνα εἰδῆτε ὅτι ζωὴν ἔχετε αἰώνιον, τοῖς πιστεύουσιν εἰς τὸ ὄνομα τοῦ υἱοῦ τοῦ θεοῦ. There are 26 variants of this phrase, ECM2 prefers that of 03, which is supported by only a few manuscripts<sup>350</sup> and by the Syriac translation called Harklensis. Another reading is ἴνα εἰδῆτε/ἴδητε ὅτι ζωὴν ἔχετε αἰώνιον καὶ ἴνα πιστεύητε εἰς τὸ ὄνομα τοῦ υἱοῦ τοῦ θεοῦ, which is the readings called f and h, presented by a handful of manuscripts, among them 1739. The reading which dominates the tradition and the Byzantine manuscripts is a conflation, more or less τοῖς πιστεύουσιν εἰς τὸ ὄνομα τοῦ υἱοῦ τοῦ θεοῦ ἴνα εἰδῆτε/ἴδητε ὅτι ζωὴν αἰώνιον ἔχετε καὶ ἴνα πιστεύητε εἰς τὸ ὄνομα τοῦ υἱοῦ τοῦ θεοῦ. I think that it is very probable that the reading of 03 is original, with its more difficult construction, τοῖς πιστεύουσιν going back to the preceding ὑμῖν. A sort of correction, near at hand if you do not see the connection to ὑμῖν, appears in 1739 and some others. To me, this means that the manuscripts 03 and 1739, supposed to be very close to the initial text, are in fact not at all or only slightly subjected to changes, whereas most of the tradition is highly contaminated, and in a rather clumsy way at that.<sup>351</sup>

1 Jn 5:18/24–36 ὁ γεννηθεὶς ἐκ τοῦ θεοῦ τηρεῖ ἑαυτόν. The majority of the tradition, with 1739, has ἑαυτόν, 03 and a minority presents αὐτόν.

<sup>350</sup> Five, among them 01\*, if we count the close variants called *ao* and *b*.

<sup>351</sup> I am not sure that Metzger, p. 649, is right, supposing that πιστεύουσιν ... ἴνα πιστεύητε is influenced by Jn 20:31 where the context is different.

This passage has been interpreted in different ways and there is no consensus. Is ὁ γεννηθεὶς ἐκ τοῦ θεοῦ Christ or the believer? In any case, theology has been at work, either in 03 *et alii* or in 1739 *et alii*.

1 Jn 5:20/2–42 οἶδαμεν δὲ ὅτι ὁ υἱὸς τοῦ θεοῦ ἦκει καὶ δέδωκεν ἡμῖν διάνοιαν, ἵνα γινώσκωμεν τὸν ἀληθινόν, καὶ ἐσμὲν ἐν τῷ ἀληθινῷ. 2–4 οἶδαμεν δὲ is the reading of 03, the Byzantine tradition and few more manuscripts. A majority of manuscripts, with 1739, have καὶ οἶδαμεν, around 15 have neither δὲ nor καὶ. All three variants are represented in versions, the last two also in Church Fathers. It is not at all impossible that the asyndeton is the initial reading, which has been changed in two different ways.

The reading of 30–32 τὸν ἀληθινόν is that of ECM2, in accordance with 03 and many other manuscripts, among them the Byzantine tradition. However, the addition of θεόν after τὸν ἀληθινόν is common, found in 1739, and outside the Byzantine tradition represented in many more manuscripts than the shorter reading. This very simple ‘theological’ addition, if we should speak about theology in such a natural addition which anyone could make, has been quite successful.

After considering these around 30 passages, a conclusion would be that in the passages treated above, there is much suspicion about the readings of 03 and 1739, however good, generally speaking, these manuscripts may be. In many cases, it is impossible to state which reading is initial, in many the readings of 03 and/or that of 1739 are probably later and inferior. We can only speak about probable or possible readings, which also goes for the examples referred to below, supposed to be comparatively clear cases. Some of the variants considered inferior are due to corrections of grammar (03: 1 Jn 1:2/11. 1739: 1 Jn 4:8/2–4:9/42) or to influences from a neighbouring phrase (only one case, both 03 and 1739: 1 Jn 4:3/2–26), some are explanatory, often indicating that the passage is about God (both 03 and 1739: 1 Jn 4:15/22–40. 03: 1 Jn 2:17/2–16. 1739: 1 Jn 2:14/16–30; 1 Jn 5:20/30–32), some have a more clear theological ring to them (03: 1 Jn 5:10/32–34. 1739: 1 Jn 1:5/10; 1 Jn 3:11/10;).

Some of these readings, considered inferior above, have had a strong impact (03 and 1739: 1 Jn 4:3/2–26; maybe 1 Jn 4:15/22–40. 03: 1 Jn 2:17/4–16; 1 Jn 5:10/2–20. 1739: 1 Jn 5:20/30–32), some a modest one (03: 1 Jn 1:2/11. 1739: 1 Jn 4:8/2–4:9/42).

The only conclusion one can draw must be that a judgement on tradition generally and on certain documents especially is of no relevance when it comes to considering a special passage.

### Comparing ECM2 and NA 27

Let us consider p. 34\*: “Reconstructing the initial text basically is a matter of assessing the genealogical relationship between variants and thus the genealogical relationship between their witnesses.” Obviously, we have here to do with the direct and heavy influence of the CBGM on the text. However, I have tried to show, in chap. 3, that the method of assessing the relationship is most unsatisfactory and misleading, the way the stemmata are established being arbitrary and in many cases highly unlikely. After all this work with such a method, should we not expect the text presented to be bad indeed? In fact it is not.

First I would like to point out that it is not at all necessary to present a different text: The mere fact that an edition presents the witnesses of the text in a comprehensive, clearly arranged and reliable way makes it useful, and ECM2 is such a text. It goes without saying that here and there we may prefer another reading than that of an editor.

In ECM2<sup>352</sup>, there is a list of 34 textual changes in ECM2 and NA 28 as against NA 27. In many, in fact in most cases, it is impossible to form an opinion about what is better, what is worse, but in some cases it may be possible to arrive at a judgement or it will at least be worth while to discuss the passage:

Jas 2:3/40–56 *σὺ στῆθι ἢ κάθου ἐκεῖ ὑπὸ τὸ ὑποπόδιόν μου* ECM2 *σὺ στῆθι ἐκεῖ ἢ κάθου ὑπὸ τὸ ὑποπόδιόν μου* NA 27. The context is that if a rich man and a poor man enter the synagogue, and if you speak despondently to the rich man offering him a good seat and brusquely to the poor giving him a bad one, then you make a bad distinction. *ἢ κάθου ἐκεῖ* (ECM2) is attested by few manuscripts, among them 03, and only poorly in the versions, *ἐκεῖ ἢ κάθου* (NA 27) has more witnesses, but most documents and versions present *σὺ στῆθι ἐκεῖ ἢ κάθου ὧδε*. If we follow NA 27, we get a parallel: The preceding *σὺ κάθου ὧδε καλῶς*, said to the rich man, and *σὺ στῆθι ἐκεῖ*, said to the poor man. Most of the tradition has preserved that reading but added *ὧδε*. I think, as always, that if one reading has a special point and the other does not, then the reading with a point is probably the right one. A point does not present itself accidentally. So here I should vote for *σὺ στῆθι ἐκεῖ*, which is also

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352 P. 35\* f.



supported by the overwhelming tradition, if we regard ὧδε as a later addition, taken from the preceding σὺ κάθου ὧδε καλῶς.

Jas 2:4/2–6 καὶ οὐ διεκρίθητε ECM2 οὐ διεκρίθητε NA 27. Both readings are well attested. The reading of 03, διεκρίθητε, is probably secondary. See for this passage p. 77. The καὶ is rather ungrammatical, and because of that it might be right to put it into the text, but it could also be a pseudo-correction by someone who has lost the thread.

1 Pt 1:6/18 λυπηθέντας ECM2 λυπηθέντες NA 27. Both readings are well attested, λυπηθέντες in 03 among other manuscripts. The author speaks about the hope that God bestows on the faithful: εἰς ἡμᾶς τοὺς ἐν δυνάμει θεοῦ φρουρουμένους διὰ πίστεως εἰς σωτηρίαν ἐτοιμὴν ἀποκαλυφθῆναι ἐν καιρῷ ἐσχάτῃ ἐν ᾧ ἀγαλλιᾶσθε ὀλίγον ἄρτι, εἰ δέον ἐστίν, λυπηθέντας ἐν ποικίλοις πειρασμοῖς. This is part of a long sentence, comprising seven verses (1:3–1:9). The question is whether we should connect the participle to φρουρουμένους or to ἀγαλλιᾶσθε. I think that we should let it refer to ἀγαλλιᾶσθε, reading λυπηθέντες: You rejoice, thinking of the last day (and its reward), although you must now suffer a little. The point is the opposition between the joy and the not too great suffering. As said above (Jas 2:3/40–56), we should not miss the point.

1 Pt 4:16/26 (δοξαζέτω δὲ τὸν θεὸν ἐν τῷ) μέρει (τούτῳ) ECM2 ὀνόματι NA 27. Both readings are well attested, μέρει in many manuscripts and also in the Byzantine tradition, ὀνόματι likewise in many manuscripts, among them 03, and more represented in versions than the other reading. The context is that one should not be ashamed of being a Christian, “but praise God in this way”, i.e. as a Christian. I take it that μέρει here should be compared with phrases like ἐν μέρει τινὸς τιθέναι, “consider something as so and so”, see Liddell-Scott-Jones.<sup>353</sup> I think that ECM2 here has made the right choice, preferring a clear *lectio difficilior*.

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353 Other examples of this construction are Theodore of Mopsuestia, *Le commentaire sur les Psalms*, p. 208, 19 τοῦτο ἐν μέρει τιθέμενος μακαρισμοῦ, Clement of Alexandria (Clemens Alexandrinus), *Paedagogus* II, II, 23, 1 τὸν οἶνον ... ἐν (the manuscript: ἐπ’) θεραπείας μέρει προσφέρεισθαι, *ibid.* III, VIII, 41, 3 ἐν ὑπογραφῆς μέρει παρασημιουμένοις, *ibid.* III, XI, 77, 3 ἐν παιδιᾶς μέρει παραλαμβάνεσθαι ... τὰς θέας; *ibid.* III, XII, 87, 1 ταῦτα δὲ ἡμῖν ἐν κεφαλαίου μέρει δι’ αὐτῶν ὑποτίθεται καὶ παρατίθεται τῶν γραφῶν. Also ἐν μοίρᾳ, see Liddell-Scott-Jones and Clement of Alexandria, *Paedagogus* I, IX, 80, 1 ἐν φαρμάκου μοίρᾳ. An interesting case is Basil the Great (Basile de Césarée), *Contra Eunomium* II, 20 (616 b/c) ἐν ἰδίῳ τόπῳ ... ἐν ᾧ καὶ τοῦτο τὸ μέρος, Θεοῦ διδόντος, διευθυνθήσεται: τοῦτο τὸ μέρος goes back to Prov. 8:22 Κύριος ἔκτισέν με and seems to mean something like “this phrase”.

2 Pt 2:6/14, 22 (πόλεις Σοδόμων καὶ Γομόρρας τεφρώσας) καταστροφῆ<sup>354</sup> (κατέκρινεν ὑπόδειγμα μελλόντων) ἀσεβεῖν (τετεικώς) ECM2. NA 27 has [καταστροφῆ] and ἀσεβέ[σ]iv. The bulk of the tradition, with versions, has καταστροφῆ which is missing in a few documents, among them P72 and 03. The reason that the shorter variant enjoys a certain support from editors is no doubt that it is represented in these very old documents. The longer text has not only the support of the majority, there is also a passage in Gen. 19:29 talking about the destruction of Sodom and Gomorrah, using both καταστροφή and καταστρέψαι. This could be counted as support, but in fact such an allusion is a double-edged sword. The author might have thought of the phrase in Genesis and put καταστροφῆ into his text, but a scribe or reader could have found the shorter text before him and added καταστροφῆ from Genesis. Also, the sequence καταστροφῆ κατέκρινεν might have resulted in a loss through homoeoarcton. An unsolvable case.

The bulk of the tradition has ἀσεβεῖν, but around 20 documents, among them P72 and 03, have ἀσεβέσiv. A translation of the first variant would be: “an example to those who are going to act impiously”, of the second: “an example to impious persons of things that will happen (to them)”. Also here P72 and 03 have no doubt given some prestige to the variant ἀσεβέσiv, but is it right? Maybe, because the whole context is about sinners, the righteous Noah and Lot contrasting to them. Consequently, I think that ἀσεβέσiv is a better reading, but of course there is no certainty. The other reading also gives good meaning.

2 Pt 2:18/22 (δελεάζουσιν ἐν ἐπιθυμίαις σαρκὸς ἀσελγείαις τοὺς) ὄντως (ἀποφεύγοντας τοὺς ἐν πλάνῃ ἀναστρεφομένους) ECM2 ὀλίγως NA 27. The vast majority have ὄντως, much fewer documents, but among them P72 and 03, read ὀλίγως. The text is about false teachers and heretics. There may seem to be a contradiction in ὄντως: If they have really escaped, how can they become a prey to false teachers and impious persons? But this is no real contradiction, only an indication that even the strong and faithful may be seduced. However, perhaps someone thought that this could only happen to those of a weaker faith, so ὄντως was changed to ὀλίγως: those who had barely escaped. I think ὄντως is the better reading.

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<sup>354</sup> καταστροφῆ is not in the list of textual changes.

2 Pt 3:5/2–4: 16 (λανθάνει γὰρ αὐτοὺς τοῦτο θέλοντας ὅτι οὐρανοὶ ἦσαν ἔκπλαλοι καὶ γῆ ἐξ ὕδατος καὶ δι’ ὕδατος συνεστῶσα τῷ τοῦ θεοῦ λόγῳ) δι’ ὃν (τότε κόσμος ὕδατι κατακλυσθεὶς ἀπόλετο) ECM2 δι’ ὧν NA 27. Only a few manuscripts present δι’ ὃν and it is weakly represented in versions, whereas δι’ ὧν is represented in the majority of the tradition, including P72 and 03. It is of course very easy to refer δι’ ὃν to τῷ τοῦ θεοῦ λόγῳ but the plural of the relative may also allude in a general way to what precedes. Here I think that δι’ ὧν may mean “by means of which”, “because of which”.<sup>355</sup> I think that this goes back to the general situation; an earth made of water, and so perishable and flooded by water. Its later form is more stable, heaven and earth being τῷ αὐτῷ λόγῳ τεθησασμένοι ... πυρὶ, as it is said in the following verse: This new world will stand until destroyed by fire. The author may have remembered God speaking to Noah about never more destroying all living beings and never more flooding the earth.<sup>356</sup> Another example of a “general” δι’ ὧν is 2 Pt 1:4/2–4, where I do not think that δι’ ὧν goes back to ἰδίᾳ δόξῃ καὶ ἀρετῇ but to the general situation: God’s power gives life, he calls us, that is how (δι’ ὧν) he gives his great promises. I think that δι’ ὧν may hint generally to the preceding phrase.<sup>357</sup>

This is all very vague and highly uncertain, but δι’ ὃν is such a *lectio facilior* that it arouses suspicion.

2 Pt 3:10/48–50 οὐχ εὐρετήσεται ECM2 εὐρετήσεται NA 27. The reading of ECM2 has very weak support and is found only in a couple of versions; εὐρετήσεται is found in few documents, among them 03, but cannot be said to have much support; most of the documents read κατακαήσεται. There are also quite a lot of emendations, see Metzger.<sup>358</sup> I think that there is no saving this phrase, which brings up the question what to do about ‘hopeless’ passages. It does not feel right to print a text without some kind of comment when you have in fact no idea at all about how to establish it.

1 Jn 5:10/22–24 ἐν αὐτῷ ECM2 ἐν ἑαυτῷ NA 27. Both readings are well documented, and the reading of 03 can be ἐν αὐτῷ or ἐν αὐτῷ. I think

355 δι’ ὧν may mean ‘because’, as perhaps more frequently ἀνθ’ ὧν. This cannot be the case here, but it shows the broader range of δι’ ὧν.

356 Gen. 8:21; 9:11.

357 Another case: Clement of Alexandria (Clemens Alexandrinus), *Protrepticus* XI, 117, 1 οὐ γὰρ μιμῆσθαι τις δυνήσεται τὸν θεὸν ἢ δι’ ὧν ὁσίων θεραπεύει, “except by means of serving him”.

358 P. 636 f.

that ECM2 is right here: It is about having confidence in him, God, who bears witness to his Son.

1 Jn 5:18/36 (οἶδαμεν ὅτι πᾶς ὁ γεγεννημένος ἐκ τοῦ θεοῦ οὐχ ἁμαρτάνει, ἀλλ' ὁ γεννηθεὶς ἐκ τοῦ θεοῦ τηρεῖ) ἐαυτὸν ECM2 αὐτὸν NA 27. Most of the tradition has ἐαυτὸν, 03 and few manuscripts have αὐτον according to the *apparatus*. The discussion is about who ὁ γεννηθεὶς ἐκ τοῦ θεοῦ is, Christ or the Christian believer. No one can doubt, I think, that πᾶς ὁ γεγεννημένος ἐκ τοῦ θεοῦ is the Christian, and it seems to me inadmissible to think that ὁ γεννηθεὶς ἐκ τοῦ θεοῦ could mean anything else, especially since we have immediately afterwards οἶδαμεν ὅτι ἐκ τοῦ θεοῦ ἐσμεν. The difference between ὁ γεγεννημένος and ὁ γεννηθεὶς is a question of the aspect of the verb, there is the believer and the one who has come to believe. Cf. 1 Pt 2:10 οὐκ ἠλεημένοι, νῦν δὲ ἐλεηθέντες. We have a figurative way of speaking about the Christian believer, made clearer by ἐκ τοῦ θεοῦ ἐσμεν. The reading of ECM2 is here clearly better.

Jd 5/12–20 (ὑπομνήσαι δὲ ὑμᾶς βούλομαι, εἰδότας) ὑμᾶς ἅπαξ πάντα ὅτι Ἰησοῦς (λαὸν ἐκ γῆς Αἰγύπτου σώσας τὸ δεύτερον τοὺς μὴ πιστεύσαντας ἀπώλεσεν) ECM2 [ὑμᾶς] πάντα ὅτι [ὁ] κύριος ἅπαξ NA 27. Here ECM2 records no less than 31 variants, most of them supported by one or few manuscripts. The reading chosen by ECM2 is that of 03 and of no other document. The reading of NA 27 is a combination of variants, one of which could be *y*: πάντα ὅτι ὁ κύριος ἅπαξ, another *m*: ὑμᾶς πάντα ὅτι κύριος ἅπαξ. *m* is found only in 01, *y* is one of the readings which have comparatively strong support. Other readings represented by a fair number of documents are *n*: ὑμᾶς ἅπαξ τοῦτο ὅτι ὁ κύριος (the Byzantine tradition and other manuscripts), and *x*: ἅπαξ τοῦτο ὅτι ὁ κύριος. The only papyrus present here is P72, reading *l*: ἅπαξ πάντας ὅτι θεὸς χριστὸς, πάντας having been changed into πάντα.

The passage is extremely chaotic. To summarize: The pronoun, mostly ὑμᾶς but also ἡμᾶς, mostly in the first place but also in the second, is not found in all the manuscripts; ἅπαξ is in all the manuscripts but one, placed first, second, third or last; some manuscripts have πάντα, others τοῦτο, some have none of them, no manuscript has them both; we find (ὁ) Ἰησοῦς, (ὁ) κύριος, ὁ θεὸς, θεὸς χριστὸς. We may think that ὑμᾶς was considered superfluous and could have been rejected, that ἅπαξ is suspect, being placed so differently but that it is nevertheless almost omnipresent and thus not negligible, that τοῦτο could have been added in order to make ὅτι etc. better connected to the preceding proposition.

We may ask if theology has entered the text, changing (ὁ) κύριος into Ἰησοῦς (03) and θεὸς χριστὸς (P72) in order to enforce the divinity of Jesus, the theological view being, as is well known, that the acting person in the Old Testament is the Son. Writing ὁ θεὸς (Latin, Syriac and Armenian versions) may also be a kind of theological change: Since (ὁ) κύριος makes a Christian think in the first place of the Son and Saviour, someone may have pointed out that in the Old Testament, the text speaks about God.

This passage once more raises the question about ‘hopeless’ passages. ECM2 sometimes places two versions in the primary line, considering them to be of equal value. But this could be done in the countless cases where we in fact cannot at all be sure of the best reading, and sometimes there are not only two possible readings. I think that the edition should not point out one special variant as an alternative reading, but just put one version in the primary line and record the others in the *apparatus*, as is usually done. We would all like guidance in such cases, but that must be left to a commentary.

On the whole, I think that EMC2 comes out quite well. There are four passages where I think that the new reading is clearly better, to wit I Pt 4:16/26, 2 Pt 2:18/22, 1 Jn 5:10/22–24 and 1 Jn 5:18/36. In some cases the old reading may be better, but generally speaking I regard those cases as less evident: Jas 2:3/40–56, 1 Pt 1:6/18, 2 Pt 2:6/22.

### **Establishing the text of the New Testament: Some wishes**

What remains to be done? I do not think that the CBGM can be rescued. Editing must be based on our courage to decide between what is important and what is not, instead of fleeing to a quasi-statistical method pretending to have a solution. I have no prescription, but I think that more could be done concerning the quotations by the Church Fathers and by other early authors. The way the ECM2 looks at the Fathers is not satisfactory.<sup>359</sup> It can almost be considered as a warning against paraphrases and allusions and a recommendation of readings which are identical with those found in the manuscript tradition. I think that the text which can be concluded from commentaries, more or less paraphrasing, are often the most reliable, whereas direct quotations are often changed in order to make them correspond to a wording which was better known to the scribe or the reader. Evidently, there is also still a large amount of

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359 EMC2, p. 22\* f.

work to be done on translations into other languages.<sup>360</sup> I do not know if we should wish for a database of published commentaries on variants. A too ambitious one, aiming at completeness, would probably lead into a jungle of opinions, often far-fetched ones, often such as only the author believes in. A more subjective and ruthless database, making short shrift of too hopeless ideas but discussing others at some length, would however be a great help.

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<sup>360</sup> EMC2, p. 23\*.

## Abbreviations and Literature

- Aland, Barbara (2002). "Kriterien zur Beurteilung kleinerer Papyrusfragmente des Neuen Testaments." In: *New Testament Textual Criticism and Exegesis. Festschrift J. Delobel*. Ed. by A. Denaux. Leuven, pp. 1–13. (Bibliotheca ephemeridum theologicarum Lovaniensium. 161.)
- (2003). "Der textkritische und textgeschichtliche Nutzen früher Papyri, demonstriert am Johannesevangelium." In: *Recent Developments in Textual Criticism. New Testament, Other Early Christian and Jewish Literature*. Ed. by Wim Weeren and Dietrich-Alex Koch. Assen, pp. 20–38. (Studies in Theology and Religion (STAR). 8.)
  - (2006). "Welche Rolle spielen Textkritik und Textgeschichte für das Verständnis des Neuen Testaments? Frühe Leserperspektiven." *New Testament Studies* 52, pp. 303–318.
  - (2011). "Was heisst Abschreiben?" In: *Mark and Matthew I. Comparative Readings: Understanding the Earliest Gospels in their First-Century Settings*. Ed. by Eve-Marie Becker and Anders Runesson. Tübingen, pp. 55–76.
- Aland, Kurt, & Aland, Barbara (1989). *The Text of the New Testament. An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism*. Transl. by Erroll F. Rhodes. 2nd ed., rev. and enl. Grand Rapids.
- Alexanderson, Bengt (2012). *Le texte du Psautier chez Théodore de Mopsueste et chez Julien d'Éclane: avec des notes de critique sur les commentaires de Théodore et de Julien*. Roma. (Studia ephemeridis "Augustinianum". 129.)
- (2013). *L'Évangile de S. Jean dans les "quatre grands": une comparaison entre les leçons de Jean 1–15, 8 dans P66, P75, S et B*. <http://hdl.handle.net/2077/32601>.
- Apocalypsis Henochi Graece* (1970). Ed. M. Black. Leiden. (Pseudepigrapha Veteris Testamenti Graece. 3.)
- Basile de Césarée (1982–1983). *Contre Eunome ...* Introd., trad. et notes de Bernard Sesboüé. T. 1–2. Paris. (Sources chrétiennes. 299, 305.)
- Bauer, Walter (1957–58). *Griechisch-deutsches Wörterbuch zu den Schriften des Neuen Testaments und der übrigen urchristlichen Literatur*. 5., verb. und stark vermehrte Aufl. Berlin.
- Clemens Alexandrinus (1972). Hrsg. von Otto Stählin. Bd 1. *Protrepticus und Paedagogus*. 3., durchges. Ausg. von Ursula Treu. Berlin. (Die griechischen christlichen Schriftsteller der ersten Jahrhunderte.)
- Colwell, Ernest C. (1969). "Method in Evaluating Scribal Habits. A Study of P<sup>45</sup>, P<sup>66</sup>, P<sup>75</sup>." In: Colwell, E.C., *Studies in Methodology in Textual Criticism of the New Testament*. Leiden, pp. 106–124. (New Testament Tools and Studies. 9.)

- Dain, A. (1975). *Les manuscrits*. 3. éd. rev. et augm. d'un index. Paris. (Collection d'études anciennes.)
- ECM2 (2013). *Novum Testamentum Graecum. Editio critica maior*. Hrsg. vom Institut für neutestamentliche Textforschung. 4. Die katholischen Briefe. Hrsg. von Barbara Aland ... T. 1. Text. T. 2. Begleitende Materialien. 2., rev. Aufl. Stuttgart.
- Ehrman, Bart D. (1993). *The Orthodox Corruption of Scripture. The Effect of Early Christological Controversies on the Text of the New Testament*. New York & Oxford.
- Elliott, Keith, & Moir, Ian (1995). *Manuscripts and the Text of the New Testament. An Introduction for English Readers*. Edinburgh.
- Epp, Eldon J. (1966). *The Theological Tendency of Codex Bezae Cantabrigiensis in Acts*. Cambridge. (Society for New Testament Studies Monograph Series. 3.)
- Galenos (1977). *Galen on Language and Ambiguity*. An English transl. of Galen's "De captionibus (On fallacies)". With introd., text, and commentary by Robert Blair Edlow. Leiden. (Philosophia antiqua. 31.)
- Genealogical Queries*. <http://intf.uni-muenster.de/cbqm/GenQ.html>.
- GNT (2001). *The Greek New Testament*. 4th rev. ed. Ed. by Barbara Aland ... Stuttgart.

### *Introductory Presentation.*

[http://www.uni-muenster.de/INTF/cbqm\\_presentation/download.html](http://www.uni-muenster.de/INTF/cbqm_presentation/download.html).

- Lowden, John (2008). "Book Production." In: *The Oxford Handbook of Byzantine Studies*. Ed. by Elizabeth Jeffries. Oxford, pp. 462–472.
- Maas, Paul (1950). *Textkritik*. 2., verb. und vermehrte Aufl. Leipzig.
- Metzger, Bruce M. (1994). *A Textual Commentary on the Greek New Testament. A Companion Volume to the United Bible Societies' Greek New Testament (4th rev. ed.)*. 2nd ed. Stuttgart.
- Mink, Gerd (1993). "Eine umfassende Genealogie der neutestamentlichen Überlieferung." *New Testament Studies* 39, pp. 481–499.
- (2003). "Was verändert sich in der Textkritik durch die Beachtung genealogischer Kohärenz?" In: *Recent Developments in Textual Criticism. New Testament, Other Early Christian and Jewish Literature*. Ed. by Wim Weren and Dietrich-Alex Koch. Assen, pp. 39–68. (Studies in Theology and Religion (STAR). 8.)
- (2004). "Problems of a Highly Contaminated Tradition: the New Testament. Stemmata of Variants as a Source of a Genealogy for Witnesses." In: *Studies in Stemmatology* 2. Ed. by Pieter van Reenen ... Amsterdam, pp. 13–85.



- (2011). "Contamination, Coherence, and Coincidence in Textual Transmission: The Coherence-Based Genealogical Method (CBGM) as a Complement and Corrective to Existing Approaches." In: *The Textual History of the Greek New Testament. Changing Views in Contemporary Research*. Ed. by Klaus Wachtel and Michael W. Holmes. Atlanta, pp. 141–216. (Text-Critical Studies. 8.)
- NA 27 (2001). *Novum Testamentum Graece*. Post Eberhard et Erwin Nestle ... communiter ed. Barbara et Kurt Aland ... 27. rev. Aufl. Stuttgart.
- NA 28 (2012). *Novum Testamentum Graece*. Begründet von Eberhard und Erwin Nestle. Hrsg. von Barbara und Kurt Aland ... 28. rev. Aufl., hrsg. vom Institut für Neutestamentliche Textforschung. Stuttgart.
- The New Testament in the Original Greek* (1881–82). The text rev. by Brooke Foss Westcott and Fenton John Anthony Hort. [1.] Text. [2.] Introduction, Appendix. Cambridge & London.
- Origène (1967). *Contre Celse*. T. 1. (Livres I et II). Introd., texte critique, trad. et notes par Marcel Borret. Paris. (Sources chrétiennes. 132.)
- The Oxyrhynchus Papyri* 34 (1968). Ed. with transl. and notes by L. Ingram ... London. (Graeco-Roman Memoirs. 49.)
- 64 (1997). Ed. with transl. and notes by E.W. Handley ... With Contributions by P.G. McC. Brown ... London. (Graeco-Roman Memoirs. 84.)
- P66 (1956). *Papyrus Bodmer II. Evangile de Jean chap. 1–14*. Publ. par Victor Martin. Cologny-Genève. (Bibliotheca Bodmeriana. 5.)
- (1962). *Papyrus Bodmer II. Supplément. Evangile de Jean chap. 14–21*. Publ. par Victor Martin et J.W.B. Barns. Nouv. éd. augm. et corr. avec reprodu. photographique complète du manuscrit (chap. 1–21). Cologny-Genève. (Bibliotheca Bodmeriana. 5: Supplément.)
- P75 (1961). *Papyrus Bodmer XV. Evangile de Jean chap. 1–15*. Publ. par Victor Martin et Rodolphe Kasser. Cologny-Genève.
- Parker, David C. (2012). *Textual Scholarship and the Making of the New Testament*. Oxford. (The Lyell Lectures in Bibliography.)
- Psalmi cum Odis* (1931). Ed. A. Rahlfs. Göttingen. (Septuaginta. Vetus Testamentum Graecum. 10.)
- Riesenfeld, Harald (1968). "Den textkritiska forskningens nuvarande läge." In: *Nyöversättning av Nya testamentet. Behov och principer*. Stockholm, pp. 197–245. (Statens offentliga utredningar. 1968:65.)
- Royse, James R. (2008). *Scribal Habits in Early Greek New Testament Papyri*. Leiden. (New Testament Tools, Studies and Documents. 36.)
- Septuaginta: id est Vetus Testamentum Graece iuxta LXX interpretes ...* (1979). Ed. Alfred Rahlfs. Stuttgart.

- Text und Textwert der griechischen Handschriften des Neuen Testaments* (1987).  
1. Die katholischen Briefe. Bd 1–3. Hrsg. von Kurt Aland. in Verbindung mit Annette Benduhn-Mertz und Gerd Mink. Berlin. (Arbeiten zur neutestamentlichen Textforschung. 9–11.)
- Theodore of Mopsuestia (1939). *Le commentaire de Théodore de Mopsueste sur les Psaumes (I-LXXX)*. [Éd. par] Robert Devreesse. Città del Vaticano. (Studi e testi. 93.)
- Thyen, Hartwig (2005). *Das Johannesevangelium*. Tübingen. (Handbuch zum Neuen Testament. 6.)
- Timpanaro, Sebastiano (2005). *The Genesis of Lachmann's Method*. Chicago & London.
- Wasserman, Tommy (2006). *The Epistle of Jude: its Text and Transmission*. Stockholm. (Coniectanea Biblica. New Testament Series. 43.)