

# I

## INTRODUCTION<sup>1</sup>

Because this book will be read by people representing a broad spectrum of interest and background, I will begin with a brief review of the textual problem.

That there is a problem concerning the identity of the Greek text of the New Testament is made clear by the existence of a number of competing editions in print. By competing I mean that they do not agree with one another as to the precise wording of the text. Such disagreement is possible because no two of the ancient Greek manuscripts (handwritten copies) known to us are identical in wording, and we are dependent upon those copies because the Apostles' Autographs, or original documents, are no longer in existence. (They were probably worn out well before A.D. 200.)

In short, we are faced with the challenge of recovering the original wording of the text from the surviving manuscripts, no two of which entirely agree. In this task we may also appeal to copies of the ancient Versions (translations into Syriac, Latin, Coptic, etc.) and to the surviving writings of the early church Fathers where they quote or refer to New Testament passages.

There are over 5,000 extant (known) Greek manuscripts (hereafter MSS, or MS when singular) of the New Testament. They range in size from a scrap with parts of two verses to complete New Testaments. They range in date from the second century to the sixteenth.<sup>2</sup> They come from all over the Mediterranean world. They contain several hundred thousand variant readings (differences in the text). The vast majority of these are misspellings or other obvious errors due to carelessness or ignorance on the part of the copyists. However, many thousands of variants remain which need to be evaluated as we seek to identify the precise original wording of the text. How best to go about such a project? This book seeks to provide an answer.

Of course, I am not the first to attempt an answer. Numerous answers have been advanced over the years. They tend to form two clusters, or camps, and these camps differ substantially from each other. In very broad and over-simplified terms, one camp generally follows the large majority of the MSS (seldom less than 80 and usually over 95 percent) which are in essential agreement among themselves but which do not date from before the fifth century A.D., while the other generally follows a small handful (often less than ten) of earlier MSS (from the third, fourth and fifth centuries) which not only disagree with the majority, but also disagree among themselves. The second camp has been in general control of the scholarly world for the last 110 years.

The most visible consequence and proof of that control may be seen in the translations of the New Testament into English done during these 110 years. Virtually every one of them reflects a form of the text based upon the few earlier MSS. In contrast to them, the King James Version (AV) and the New King James Version (NKJV) reflect a form of the text based upon the many later MSS. Thus, the fundamental difference between the New Testament in the American Standard Version, Revised Standard Version, New English Bible, Today's English Version, New American Standard Bible, New International Version, etc., on the one hand, and in the AV and NKJV on the other is that they are based on different forms of the Greek text. (There are over 5,500 differences between those two forms.)<sup>3</sup>

To the extent that you may be aware of these matters you may well have accepted as reasonable the statements usually made to the effect that the very considerable improvement in our stock of available materials (Greek manuscripts and other witnesses) and in our understanding of what to do with them

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<sup>1</sup>A good deal of the research underlying this book was done in connection with the master's thesis I submitted to the Dallas Theological Seminary in 1968 entitled "An Evaluation of the Contribution of John William Burgon to New Testament Textual Criticism." My thesis was subsequently published in edited form in *True or False?*, ed. D. Otis Fuller, (Grand Rapids: Grand Rapids International Publishers, 1972)—the full text of the thesis appears in the 2nd edition, 1975. I have re-used some of the material in the thesis by permission of both entities.

<sup>2</sup>There are over a hundred from the seventeenth and another forty from the eighteenth (and even nineteenth), but since several printed editions of the Greek New Testament appeared during the sixteenth, the manuscripts produced subsequently are presumed to be of little interest.

<sup>3</sup>F.H.A. Scrivener, ed., *The New Testament in the Original Greek, together with the variations adopted in the Revised Version* (Cambridge: Cambridge University Press, 1880). In spite of the differences between the printed editions of the Greek text in general use, they are all agreed as to the identity of about 90 percent of the Text.

(principles of textual criticism) has made possible a closer approximation to the original text in our day than was achieved several hundred years ago. The statements to be found in the prefaces of some versions give the reader the impression that this improvement is reflected in their translations. For example, the preface to the Revised Standard Version, p. ix, says:

The King James Version of the New Testament was based upon a Greek text that was marred by mistakes, containing the accumulated errors of fourteen centuries of manuscript copying [not true; almost all TR readings are ancient]. . . . We now possess many more ancient manuscripts of the New Testament, and are far better equipped to seek to recover the original wording of the Greek text.

And the preface to the New International Version, p. viii, says:

The Greek text used in the work of translation was an eclectic one. No other piece of ancient literature has so much manuscript support as does the New Testament. Where existing texts differ, the translators made their choice of readings in accord with sound principles of textual criticism. Footnotes call attention to places where there is uncertainty about what constitutes the original text.

But if you have used a number of the modern versions you may have noticed some things that perhaps intrigued, bewildered, or even distressed you. I am thinking of the degree to which they differ among themselves, the uncertainty as to the identity of the text reflected in the many footnotes regarding textual variants, and the nature and extent of their common divergence from the King James Version.

The bulk of the differences between the modern versions is presumably due to differences in style and translation technique. However, although they are in essential agreement as to the Greek text used, as opposed to that underlying the AV, no two of them are based on an identical Greek text. Nor have the translators been entirely sure as to the precise wording of the text—while some versions have few notes about textual variation, others have many, and even in these cases by no means all the doubts have been recorded.<sup>4</sup> In short, no one in the world today really knows the precise original wording of the Greek text of the New Testament.

Such a realization may beget an incipient uneasiness in the recesses of your mind. Why isn't anyone sure, if we have so many materials and so much wisdom? Well, because the present "wisdom," the "sound principles of textual criticism" currently in vogue, may be summed up in two maxims: choose the reading that best explains the origin of the competing variants, and choose the variant that the author is more/most likely to have written.

No wonder Bruce Metzger<sup>5</sup> says, "It is understandable that in some cases different scholars will come to different evaluations of the significance of the evidence."<sup>6</sup> A cursory review of the writings of textual scholars suggests that Metzger's "in some cases" is decidedly an understatement. In fact, even the same scholars will vacillate, as demonstrated by the "more than five hundred changes" introduced into the third edition of the Greek text produced by the United Bible Societies as compared with the second edition (the same committee of five editors prepared both).<sup>7</sup> Further, it is evident that the maxims above cannot be applied with certainty. No one living today knows or can know what actually happened. It follows that so long

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<sup>4</sup>For instance, Tasker says of the NEB translators, "Every member of the Panel was conscious that some of its decisions were in no sense final or certain, but at best tentative conclusions, . . ." *The Greek New Testament* (being the text translated in the New English Bible) ed. R.V.G. Tasker (Oxford: Oxford University Press, 1964), p. viii. See also B.M. Metzger, *Historical and Literary Studies*, NTS, VIII (Grand Rapids: Wm. B. Eerdmans, 1968), pp. 160-61.

<sup>5</sup>Bruce M. Metzger is one of the senior New Testament scholars of North America. He has been a Professor at Princeton University for many years and has authored many scholarly works including the standard textbook, *The Text of the New Testament*.

<sup>6</sup>B.M. Metzger, *The Text of the New Testament* (London: Oxford University Press, 1964), p. 210.

<sup>7</sup>K. Aland, M. Black, C.M. Martini, B.M. Metzger, and A. Wikgren, eds., *The Greek New Testament*, third edition (New York: United Bible Societies, 1975), p. viii. Although this edition is dated 1975, Metzger's *Commentary* upon it appeared in 1971. The second edition is dated 1968. It thus appears that in the space of three years ('68-'71), with no significant accretion of new evidence, the same group of five scholars changed their mind in over five hundred places. It is hard to resist the suspicion that they are guessing.

as the textual materials are handled in this way **we will never be sure** about the precise wording of the Greek text.<sup>8</sup>

It is not surprising that scholars working within such a framework say as much. For example, Robert M. Grant, a well-known biblical scholar, says:

The primary goal of New Testament textual study remains the recovery of what the New Testament writers wrote. We have already suggested that to achieve this goal is well-nigh impossible. Therefore we must be content with what Reinhold Niebuhr and others have called, in other contexts, an "impossible possibility."<sup>9</sup>

And Kenneth W. Clark, another well-known textual scholar and professor, commenting on P<sup>75</sup>:

. . . the papyrus vividly portrays a fluid state of the text at about A.D. 200. Such a scribal freedom suggests that the gospel text was little more stable than the oral tradition, and that we may be pursuing the retreating mirage of the "original text."<sup>10</sup>

Fifty years ago Grant had said, "it is generally recognized that the original text of the Bible cannot be recovered."<sup>11</sup>

At this point I get uncomfortable. If the original wording is lost and gone forever, whatever are we using? The consequences of such an admission are so far-reaching, to my mind, that a thorough review of the evidence is called for. Do the facts really force an honest mind to the conclusion expressed by Grant? In seeking an answer to this question I will begin with the present situation in New Testament textual criticism and work back. The procedure which dominates the scene today is called "eclecticism."<sup>12</sup>

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<sup>8</sup>Even where there is unanimous testimony for the wording of the text, the canons of internal evidence do not preclude the possibility that that unanimous testimony might be wrong. Once internal evidence is accepted as **the** way to determine the text there is no basis **in principle** for objecting to conjectural emendation. Hence no part of the Text is safe. (Even if it is required that a proposed reading be attested by at least one manuscript, a new Papyrus may come to light tomorrow with new variants to challenge the unanimous witness of the rest, and so on.)

<sup>9</sup>R.M. Grant, *A Historical Introduction to the New Testament* (New York: Harper and Row, 1963), p. 51.

<sup>10</sup>K.W. Clark, "The Theological Relevance of Textual Variation in Current Criticism of the Greek New Testament," *Journal of Biblical Literature*, LXXXV (1966), p. 15.

<sup>11</sup>Grant, "The Bible of Theophilus of Antioch," *Journal of Biblical Literature*, LXVI (1947), 173. For a most pessimistic statement see E.C. Colwell, "Biblical Criticism: Lower and Higher," *Journal of Biblical Literature*, LXVII (1948), 10-11. See also G. Zuntz, *The Text of the Epistles*, 1953, p. 9; K. and S. Lake, *Family 13 (The Ferrar Group)*, 1941, p. vii; F. C. Conybeare, *History of New Testament Criticism*, 1910, p. 129.

<sup>12</sup>In ordinary usage the term "eclecticism" refers to the practice of selecting from various sources. In textual criticism there is the added implication that the sources are disparate. Just what this means in practice is spelled out in the section "What is it?" in Chapter 2.

## 2

# ECLECTICISM

In 1974, Eldon Jay Epp, a respected contemporary textual scholar, wrote: "The 'eclectic' method is, in fact, **the** 20th century method of NT textual criticism, and anyone who criticizes it immediately becomes a self-critic, for we all use it, some of us with a certain measure of reluctance and restraint, others with complete abandon."<sup>1</sup>

Thus, the RSV (Revised Standard Version), NEB (New English Bible) and NIV (New International Version) are confessedly based upon an eclectic text.

The two great translation efforts of these years—RSV and NEB—each chose the Greek text to translate on the basis of the internal evidence of readings. F C. Grant's chapter in the expository pamphlet on the RSV made this clear. The translators, he says, followed two rules: (1) Choose the reading that best fits the context; (2) Choose the reading which explains the origin of the other readings. Professor C. H. Dodd informed me that the British translators also used these two principles—Hort's Intrinsic Probability and Transcriptional Probability. One of the RSV translators while lecturing to the New Testament Club at the University of Chicago replied to a question concerning the Greek text he used by saying that it depended on where he was working: he used Souter at the office and Nestle at home. One of the British translators in admitting the unevenness of the textual quality of the NEB translation explained that the quality depended on the ability of the man who made the first draft-translation of a book.

Whether in early Christian times or today, translators have so often treated the text cavalierly that textual critics should be hardened to it. But much more serious is the prevalence of this same dependence on the internal evidence of readings in learned articles on textual criticism, and in the popularity of manual editions of the Greek New Testament. These latter with their limited citations of variants and witnesses actually reduce the user to reliance upon the internal evidence of readings. The documents which these rigorously abbreviated apparatuses cite cannot lead the user to dependence upon external evidence of documents. These editions use documents (to quote Housman) "as drunkards use lampposts—, not to light them on their way but to dissimulate their instability."<sup>2</sup>

The statement in the preface to the NIV has already been noted: "The Greek text used in the work of translation was an eclectic one."

The introduction to the Greek text put out by the United Bible Societies, pp. x-xi (1966), says:

By means of the letters A, B, C, and D, enclosed within "braces" { } at the beginning of each set of textual variants, the Committee has sought to indicate the relative degree of certainty, arrived at on the basis of internal considerations as well as of external evidence, for the reading adopted as the text. The letter A signifies that the text is virtually certain, while B indicates that there is some degree of doubt. The letter C means that there is a considerable degree of doubt whether the text or the apparatus contains the superior reading, while D shows that there is a very high degree of doubt concerning the reading selected for the text.

A review of their apparatus and its lack of pattern in the correlation between degree of certainty assigned and external evidence makes clear that it is eclectic. In Acts 16:12 they have even incorporated a conjecture! It will be remembered that this text was prepared specifically for the use of Bible translators. The TEV (Today's English Version) is translated directly from it, as is the *Version Populaire*, etc. The text-critical conclusions of G.D. Kilpatrick, a thoroughgoing eclecticist, were finding expression in *A Greek-English*

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<sup>1</sup>E.J. Epp, "The Twentieth Century Interlude in New Testament Textual Criticism," *Journal of Biblical Literature*, XCIII (1974), p. 403.

<sup>2</sup>E.C. Colwell, "Hort Redivivus: A Plea and a Program," *Studies in Methodology in Textual Criticism of the New Testament*, E.C. Colwell (Leiden: E.J. Brill, 1969), pp. 152-53. Tasker records the principles followed by the NEB translators: "The Text to be translated will of necessity be eclectic, . . ." (p. vii).

*Diglot for the Use of Translators*, issued by the British and Foreign Bible Society. And so on. Enough evidence has been given to show that eclecticism is a major, if not controlling, factor on the textual scene today.

## What Is It?

Wherein does "eclecticism" consist? Metzger explains that an eclectic editor "follows now one and now another set of witnesses in accord with what is deemed to be the author's style or the exigencies of transcriptional hazards."<sup>3</sup>

E. C. Colwell<sup>4</sup> spells it out:

Today textual criticism turns for its final validation to the appraisal of individual readings, in a way that involves subjective judgment. The trend has been to emphasize fewer and fewer canons of criticism. Many moderns emphasize only two. These are: 1) that reading is to be preferred which best suits the context, and 2) that reading is to be preferred which best explains the origin of all others.

These two rules are nothing less than concentrated formulas of all that the textual critic must know and bring to bear upon the solution of his problem. The first rule about choosing what suits the context exhorts the student to know the document he is working on so thoroughly that its idioms are his idioms, its ideas as well known as a familiar room. The second rule about choosing what could have caused the other readings requires that the student know everything in Christian history which could lead to the creation of a variant reading. This involves knowledge of institutions, doctrines, and events. . . . This is knowledge of complicated and often conflicting forces and movements.<sup>5</sup>

(What living person really possesses these qualifications? And how can such rules be applied when neither the identity nor circumstances of the originator of a given variant is known?)

More recently Colwell seems to be less enchanted with the method.

The scholars who profess to follow "the Eclectic Method" frequently so define the term as to restrict evidence to the Internal Evidence of Readings. By "eclectic" they mean in fact free choice among readings. This choice in many cases is made solely on the basis of intrinsic probability. The editor chooses that reading which commends itself to him as fitting the context, whether in style, or idea, or contextual reference. Such an editor relegates the manuscripts to the role of supplier of readings. The weight of the manuscript is ignored. Its place in the manuscript tradition is not considered. Thus Kilpatrick argues that certain readings found only in one late Vulgate manuscript should be given the most serious consideration because they are good readings.<sup>6</sup>

J.K. Elliott, a thorough-going eclecticist like Kilpatrick, says of transcriptional probabilities:

By using criteria such as the above the critic may reach a conclusion in discussing textual variants and be able to say which variant is the original reading. However, it is legitimate to ask: can a reading be accepted as genuine if it is supported by only one ms.? There is no reason why an original reading should not have been preserved in only one ms. but obviously a reading can be accepted with greater confidence, when it has stronger support. . . .

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<sup>3</sup>Metzger, *The Text*, pp. 175-76.

<sup>4</sup>The late Ernest Cadman Colwell might well have been described as the dean of New Testament textual criticism in North America during the 1950s and 1960s. He was associated with the University of Chicago for many years as Professor and President. Some of his important articles have been collected and reprinted in *Studies in Methodology in Textual Criticism of the New Testament*.

<sup>5</sup>Colwell, "Biblical Criticism," pp. 4-5. For words to the same effect see also K. Lake, *The Text of the New Testament*, sixth edition revised by Silva New (London: Rivingtons, 1959), p. 10 and Metzger, *The Text*, pp. 216-17.

<sup>6</sup>Colwell, "Hort Redivivus," p. 154. Cf. pp. 149-54.

Even Aland with his reservation about eclecticism says: "Theoretically the original readings can be hidden in a single ms. thus standing alone against the rest of tradition," and Tasker has a similar comment: "The possibility must be left open that in some cases the true reading may have been preserved in only a few witnesses or even in a single relatively late witness."<sup>7</sup>

Among what Elliott calls "positive advantages of the eclectic method" is the following:

An attempt is made to reach the true or original text. This is, of course, the ultimate aim of any textual critic, but the eclectic method, by using different criteria and by working from a different standpoint, tries to arrive at the true reading, untrammelled by discussion about the weight of ms. support. . . .<sup>8</sup>

No wonder Epp complains:

This kind of "eclecticism" becomes the great leveller—all variants are equals and equally candidates for the original text, regardless of date, residence, lineage, or textual context. In this case, would it not be appropriate to suggest, further, that a few more conjectural readings be added to the available supply of variants on the assumption that they must have existed but have been lost at some point in the history of the textual transmission?<sup>9</sup>

What shall we say of such a method; is it a good thing?

## What About It?

An eclecticism based solely on internal considerations is unacceptable for several reasons. It is unreasonable. It ignores the over 5,000 Greek MSS now extant, to say nothing of patristic and versional evidence, except to cull variant readings from them. In Elliott's words, it "tries to arrive at the true reading untrammelled by discussion about the weight of ms. support." It follows that it has no principled basis for rejecting conjectural emendations. It has no history of the transmission of the text. Therefore the choice between variants ultimately depends upon guesswork. This has been recognized by Colwell.

In the last generation we have depreciated external evidence of documents and have appreciated the internal evidence of readings; but we have blithely assumed that we were rejecting "conjectural emendation" if our conjectures were supported by some manuscripts. We need to recognize that the editing of an eclectic text rests upon conjectures.<sup>10</sup>

F.G. Kenyon<sup>11</sup> called conjectural emendation "a process precarious in the extreme and seldom allowing anyone but the guesser to feel confidence in the truth of its results."<sup>12</sup> Although enthusiasts like Elliott think they can restore the original wording of the text in this way, it is clear that the result can have no more authority than that of the scholar(s) involved. Textual criticism ceases to be a science and one is left wondering what is meant by "sound principles" in the NIV preface.

Clark and Epp are right in calling eclecticism a secondary, tentative, and temporary method.<sup>13</sup> As A.F.J. Klijn<sup>14</sup> says, "This method arrives at such varying results that we wonder whether editors of Greek

<sup>7</sup>J.K. Elliott, *The Greek Text of the Epistles to Timothy and Titus*, ed., Jacob Geerlings, *Studies and Documents*, XXXVI (Salt Lake City: University of Utah Press, 1968), pp. 10-11. Cf. K. Aland, "The Significance of the Papyri for Progress in New Testament Research," *The Bible in Modern Scholarship*, ed. J.P. Hyatt (New York: Abingdon Press, 1965), p. 340, and Tasker, p. viii.

<sup>8</sup>Elliott, p. 11.

<sup>9</sup>Epp, p. 404.

<sup>10</sup>Colwell, "Scribal Habits in Early Papyri: A Study in the Corruption of the Text," *The Bible in Modern Scholarship*, ed. J.P. Hyatt (New York: Abingdon Press, 1965), pp. 371-72.

<sup>11</sup>Frederick G. Kenyon was an outstanding British scholar during the first half of this century. He was Director and Principal Librarian of the British Museum and his *Handbook to the Textual Criticism of the New Testament* is still a standard textbook.

<sup>12</sup>F.G. Kenyon, *Handbook to the Textual Criticism of the New Testament*, 2nd ed., 1926, p. 3.

<sup>13</sup>Epp, pp. 403-4. Cf. K.W. Clark, "The Effect of Recent Textual Criticism upon New Testament Studies," *The Background of the New Testament and its Eschatology*, ed. W.D. Davies and D. Daube (Cambridge: The Cambridge University Press, 1956), p. 37. In a paper presented at the forty-sixth annual meeting of the Evangelical Theological Society (Nov., 1994), Maurice A. Robinson reinforces the serious deficiency that "neither 'reasoned' nor 'rigorous' eclecticism offers a consistent history of textual transmission. . . ." (p. 30). The seriousness

texts and translations can safely follow this road."<sup>15</sup> This procedure seems so unsatisfactory, in fact, that we may reasonably wonder what gave rise to it.

## What Is Its Source?

Eclecticism grew out of the Westcott and Hort (hereafter W-H) theory of textual criticism. Epp gives a useful summary statement of that theory, for our immediate purpose:

. . . the grouping of manuscripts led to the separation of the relatively few early manuscripts from the mass of later ones, and eventually the process reached its climactic point of development and its classical statement in the work of Westcott and Hort (1881-1882), and particularly in their (actually, Hort's) clear and firm view of the early history of the NT text. This clear picture was formed from Hort's isolation of essentially three (though he said four) basic textual groups or text-types. On the basis largely of Greek manuscript evidence from the middle of the 4th century and later and from the early versional and patristic evidence, two of these, the so-called Neutral and Western text-types, were regarded as competing texts from about the middle of the 2nd century, while the third, now designated Byzantine, was a later, conflate and polished ecclesiastical text. . . . This left essentially two basic text-types competing in the earliest traceable period of textual transmission, the Western and the Neutral, but this historical reconstruction could not be carried farther so as to reveal—on historical grounds—which of the two was closer to and therefore more likely to represent the original NT text.<sup>16</sup>

. . . the question which faced Westcott-Hort remains for us: Is the original text something nearer to the Neutral or to the Western kind of text? . . . Hort resolved the issue, not on the basis of the *history of the text*, but in terms of the presumed *inner quality* of the texts and on grounds of largely subjective judgments of that quality.<sup>17</sup>

Hort, following the "ring of genuineness," preferred the readings of the "Neutral" text-type (today's Alexandrian) and especially those of Codex B, while some subsequent scholars have preferred the readings of the "Western" text-type and of Codex D, on the same basis. Although Hort professed to follow external evidence—and he did in fact follow his "Neutral" text-type, by and large—his prior choice of that text-type was based on internal (subjective) considerations.<sup>18</sup> Still, the general impression was given that the W-H theory was based on external (manuscript and historical) evidence.

But various facets of the theory came under attack soon after it appeared in 1881, and with the conflicting voices came confusion. It is this confusion that has given rise to eclecticism. Thus, Elliott frankly states: "In view of the present dilemma and discussion about the relative merits of individual mss., and of ms. tradition, it is reasonable to depart from a documentary study and to examine the N.T. text from a purely eclectic standpoint."<sup>19</sup> In R.V.G. Tasker's words, "The fluid state of textual criticism today makes the adoption of the eclectic method not only desirable but all but inevitable."<sup>20</sup> Metzger cites dissatisfaction "with the results achieved by weighing the external evidence for variant readings" as the cause.<sup>21</sup> Epp blames "the lack of a definitive theory and history of the early text" and the resultant "chaotic situation in the

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of this deficiency may be seen from the results. UBS<sup>3</sup>, a confessedly eclectic text, repeatedly serves up a patchwork quilt. For example, in Matthew there are at least 34 places where its precise rendering is not to be found, as such, in any single extant Greek MS (cf. R.J. Swanson, *The Horizontal Line Synopsis of the Gospels, Greek Edition, Volume I. The Gospel of Matthew* [Dillsboro, NC: Western North Carolina Press, 1982]).

<sup>14</sup>A.F.J. Klijn is a well-known textual scholar who has specialized in the study of the "Western text-type."

<sup>15</sup>A.F.J. Klijn, *A Survey of the Researches into the Western Text of the Gospels and Acts; part two 1949-1969* (Leiden: E.J. Brill, 1969), p. 65.

<sup>16</sup>Epp, pp. 391-92.

<sup>17</sup>*Ibid.*, pp. 398-99.

<sup>18</sup>Metzger states that "Westcott and Hort's criticism is subjective." *The Text*, p. 138. See also Colwell, *Studies in Methodology in Textual Criticism of the New Testament* (Leiden: E.J. Brill, 1969), pp. 1-2.

<sup>19</sup>Elliott, pp. 5-6.

<sup>20</sup>Tasker, p. vii.

<sup>21</sup>Metzger, *The Text*, p. 175.

evaluation of variant readings in the NT text."<sup>22</sup> Colwell also blames "manuscript study without a history."<sup>23</sup> The practice of pure eclecticism seems to imply either despair that the original wording can be recovered on the basis of external evidence, or an unwillingness to undertake the hard work of reconstructing the history of the text, or both.

But most scholars do not practice **pure** eclecticism—they still work essentially within the W-H framework. Thus, the two most popular manual editions of the Greek text today, Nestle-Aland and UBS (United Bible Societies), really vary little from the W-H text.<sup>24</sup> The recent versions—RSV, NEB, etc.—also vary little from the W-H text.

Why is this? Epp answers:

One response to the fact that our popular critical texts are still so close to that of Westcott-Hort might be that the kind of text arrived at by them and supported so widely by subsequent criticism is in fact and without question the best attainable NT text; yet every textual critic knows that this similarity of text indicates, rather, that we have made little progress in textual *theory* since Westcott-Hort; that we simply do not know how to make a definitive determination as to what the best text is; that we do not have a clear picture of the transmission and alteration of the text in the first few centuries; and, accordingly, that the Westcott-Hort kind of text has maintained its dominant position largely by default. Gunther Zuntz enforces the point in a slightly different way when he says that "the agreement between our modern editions does not mean that we have recovered the original text. It is due to the simple fact that their editors . . . follow one narrow section of the evidence, namely, the non-Western Old Uncials."<sup>25</sup>

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<sup>22</sup>Epp, p. 403.

<sup>23</sup>Colwell, "Hort Redivivus," p. 149.

<sup>24</sup>See K.W. Clark, "Today's Problems with the Critical Text of the New Testament," *Transitions in Biblical Scholarship*, ed. J.C.R. Rylaarsdam (Chicago: The University of Chicago Press, 1968), pp. 159-60, for facts and figures. Also see Epp, pp. 388-90. G.D. Fee has charged that my treatment of eclecticism is "hopelessly confused" ("A Critique of W. N. Pickering's *The Identity of the New Testament Text: A Review Article*," *The Westminster Theological Journal*, XLI [Spring, 1979], p. 400). He feels that I have not adequately distinguished between "rigorous" (my "pure") and "reasoned" eclecticism and have thereby given a distorted view of the latter. Well, he himself says of the reasoned eclecticism which he espouses, "Such eclecticism recognizes that W-H's view of things was essentially correct, . . . (*Ibid.*, p. 402)." My statement is, "But most scholars do not practice **pure** eclecticism—they still work essentially within the W-H framework (p. 28)." Are the two statements really that different?

The fairness of this assessment may be illustrated from the works of both Fee and Metzger (whom Fee considers to be a practitioner of reasoned eclecticism). In his "Rigorous or Reasoned Eclecticism—Which?" (*Studies in New Testament Language and Text*, ed. J.K. Elliott [Leiden: Brill, 1976]), Fee says: "Rational eclecticism agrees in principle that no MS or group of MSS has a *prima facie* priority to the original text (p. 179)." But on the next page he says of Hort: "if his evaluation of B as 'neutral' was too high a regard for that MS, it does not alter his judgment that compared to all other MSS B is a superior witness." Metzger says on the one hand, "the only proper methodology is to examine the evidence for each variant impartially, with no predilections for or against any one type of text (*Chapters*, p. 39)," but on the other hand, "readings which are supported by only Koine, or Byzantine witnesses (Hort's Syrian group) may be set aside as almost certainly secondary (*The Text*, p. 212)."

But Fee has more to say. "An even greater error [than my 'distortion' discussed above] is for him to argue that Elliott's method is under 'the psychological grip of W-H' (p. 29) ("A Critique," p. 401)." He goes on to explain that Elliott and W-H are on opposite ends of the internal/external evidence spectrum because "it is well known that W-H gave an extraordinary amount of weight to external evidence, just as do Pickering and Hodges (*Ibid.*)."

And yet, on another occasion Fee himself wrote: "it must be remembered that Hort did **not** use genealogy in order to discover the original NT text. Whether justified or not, Hort used genealogy solely to dispense with the Syrian (Byzantine) text. Once he has [*sic*] eliminated the Byzantines from serious consideration, his preference for the Neutral (Egyptian) MSS was based **strictly** on intrinsic and transcriptional probability [emphasis Fee's] ("Rigorous," p. 177)." And again: "In fact the very internal considerations for which Kilpatrick and Elliott argue as a basis for the recovery of the original text, Hort used **first** [emphasis Fee's] for the evaluation of the existing witnesses (*Ibid.*, p. 179)."

It seems to me that these latter statements by Fee are clearly correct. Since Hort's preference for B and the "Neutral" text-type was based "strictly" on internal considerations, his subsequent use of that text-type cannot reasonably be called an appeal to external evidence. In sum, I see no essential difference between "rigorous" and "reasoned" eclecticism since the preference given to certain MSS and types by the "reasoned" eclecticians is itself derived from internal evidence, the same considerations employed by the "rigorous" eclecticians. I deny the validity of "eclectic method" in whatever guise as a means for determining the identity of the NT Text. (I do agree with Z.C. Hodges, however, that any and all Traditional Text readings can be defended in terms of internal considerations, should one wish to.)

<sup>25</sup>Epp, 390-91. Cf. G. Zuntz, p. 8. Epp reinforces an earlier statement by Aland: "It is clear that the situation with which our present day method of establishing the New Testament text confronts us is most unsatisfactory. It is not at all the case that, as some seem to think, everything has been done in this field and we can for practical purposes rest satisfied with the text in use. On the contrary, the decisive task still lies ahead." "The Present Position of New Testament Textual Criticism," *Studia Evangelica*, ed. F.L. Cross and others (Berlin: Akademie-Verlag, 1959), p. 731.



Clark agrees with Zuntz: "All are founded on the same Egyptian recension, and generally reflect the same assumptions of transmission."<sup>26</sup> Clark also gives a sharper focus to one aspect of Epp's answer.

. . . the Westcott-Hort text has become today our *textus receptus*. We have been freed from the one only to become captivated by the other. . . . The psychological chains so recently broken from our fathers have again been forged upon us, even more strongly. . .

Even the textual specialist finds it difficult to break the habit of evaluating every witness by the norm of this current *textus receptus*. His mind may have rejected the Westcott-Hort term "neutral," but his technical procedure still reflects the general acceptance of the text. A basic problem today is the technical and psychological factor that the Westcott-Hort text has become our *textus receptus*. . . .

Psychologically it is now difficult to approach the textual problem with free and independent mind. However great the attainment in the Westcott-Hort text, the further progress we desiderate can be accomplished only when our psychological bonds are broken. Herein lies today's foremost problem with the critical text of the New Testament.<sup>27</sup>

In spite of the prevailing uncertainty and dissatisfaction, when it comes right down to it most textual critics fall back on W-H—when in doubt the safe thing to do is stay with the party line.<sup>28</sup>

Elliott, mentioned earlier, deliberately tried to set the party line aside, and the result is interesting—his reconstruction of the text of the Pastoral Epistles differs from the *Textus Receptus* 160 times, differs from W-H 80 times, and contains 65 readings that have not appeared in any other printed edition. A review of his reasoning suggests that he did not altogether escape the psychological grip of W-H, but the result is still significantly different from anything else that has been done.<sup>29</sup>

Elliott's effort underscores, by contrast, the extent to which UBS, NEB, etc. still hew to the W-H line. To really understand what is going on today we must have a clear perception of the W-H critical theory and its implications. Its importance is universally recognized.<sup>30</sup> J.H. Greenlee's statement is representative. "The textual theory of W-H underlies virtually all subsequent work in NT textual criticism."<sup>31</sup>

So, to a discussion of that theory I now turn.

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<sup>26</sup>Clark, "Today's Problems," p. 159.

<sup>27</sup>*Ibid.*, pp. 158-60. Cf. M.M. Parvis, "Text, NT.," *The Interpreter's Dictionary of the Bible* (4 Vols.; New York: Abingdon Press, 1962), IV, 602, and D.W. Riddle, "Fifty Years of New Testament Scholarship," *The Journal of Bible and Religion*, X (1942), 139.

<sup>28</sup>Cf. Clark, "Today's Problems," p. 166, and especially Colwell, "Scribal Habits," pp. 170-71.

<sup>29</sup>Elliott's results are interesting in a further way. He does his reconstruction "untrammelled" by considerations of manuscript support and then traces the performance of the principal manuscripts. Summarizing his statement of the results, considering only those places where there was variation, Codex Aleph was right 38% of the time, A was right 38% of the time, C right 41%, D right 35%, F,G right 31%, and the bulk of the minuscules (Byzantine) was right 35% of the time (pp. 241-43). He claims that doing a reconstruction his way then enables one to trace the behavior of individual MSS and to show their "illogical fluctuations." Such a tracing is based upon his own subjective evaluation of readings but the illogical fluctuations can be seen empirically by comparing the collations of a variety of MSS.

<sup>30</sup>See, for example, K. Aland, "The Significance of the Papyri," p. 325; Colwell, "Scribal Habits," p. 370; Metzger, *The Text*, p. 137; V. Taylor, *The Text of the New Testament* (New York: St. Martin's Press Inc., 1961), p. 49; K. Lake, p. 67; F.G. Kenyon, *Handbook to the Textual Criticism of the New Testament* (2nd ed.; Grand Rapids: Wm. B. Eerdmans Publishing Co., 1951), p. 294; Epp, "Interlude," p. 386, and Riddle, Parvis and Clark, noted above (fn. 27).

<sup>31</sup>J.H. Greenlee, *Introduction to New Testament Textual Criticism* (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1964), p. 78.

## THE WESTCOTT-HORT CRITICAL THEORY

Although Brooke Foss Westcott identified himself fully with the project and the results, it is generally understood that it was mainly Fenton John Anthony Hort<sup>1</sup> who developed the theory and composed the Introduction in their two-volume work.<sup>2</sup> In the following discussion I consider the W-H theory to be Hort's creation.

At the age of 23, in late 1851, Hort wrote to a friend: "I had no idea till the last few weeks of the importance of texts, having read so little Greek Testament, and dragged on with the villainous *Textus Receptus*. . . . Think of that vile *Textus Receptus* leaning entirely on late MSS.; it is a blessing there are such early ones."<sup>3</sup>

Scarcely more than a year later, "the plan of a joint [with B.F. Westcott] revision of the text of the Greek Testament was first definitely agreed upon."<sup>4</sup> And within that year (1853) Hort wrote to a friend that he hoped to have the new text out "in little more than a year."<sup>5</sup> That it actually took twenty-eight years does not obscure the circumstance that though uninformed, by his own admission, Hort conceived a personal animosity for the *Textus Receptus*,<sup>6</sup> and only because it was based entirely, so he thought, on late manuscripts. It appears that Hort did not arrive at his theory through unprejudiced intercourse with the facts. Rather, he deliberately set out to construct a theory that would vindicate his preconceived animosity for the Received Text.

Colwell has made the same observation: "Hort organized his entire argument to depose the *Textus Receptus*."<sup>7</sup> And again, "Westcott and Hort wrote with two things constantly in mind; the *Textus Receptus* and the *Codex Vaticanus*. But they did not hold them in mind with that passive objectivity which romanticists ascribe to the scientific mind."<sup>8</sup>

As the years went by, Hort must have seen that to achieve his end he had to have a convincing history of the text—he had to be able to explain why essentially only one type of text was to be found in the mass of later manuscripts and show how this explanation justified the rejection of this type of text.

### The Basic Approach

Hort started by taking the position that the New Testament is to be treated like any other book.<sup>9</sup> "The principles of criticism explained in the foregoing section hold good for all ancient texts preserved in a

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<sup>1</sup>F.J.A. Hort and B.F. Westcott were highly respected and influential Anglican churchmen of the past century—especially during the 70s and 80s. Westcott was Bishop of Durham and Hort a Professor at Cambridge. The Greek text of the N.T. prepared by them was adopted (essentially) by the committee that produced the English Revised Version of 1881. Westcott wrote a number of commentaries on N.T. books which are still considered to be standard works. His prestige and influence were important to the success of their (W-H) undertaking.

<sup>2</sup>B.F. Westcott and F.J.A. Hort, *The New Testament in the Original Greek* (2 Vols.; London: Macmillan and Co., 1881).

<sup>3</sup>A.F. Hort, *Life and Letters of Fenton John Anthony Hort* (2 Vols.; London: Macmillan and Co. Ltd., 1896), I, 211.

<sup>4</sup>*Ibid.*, p. 240.

<sup>5</sup>*Ibid.*, p. 264.

<sup>6</sup>The expression "*Textus Receptus*" properly refers to some one of the printed editions of the Greek text of the N.T. related in character to the text prepared by Erasmus in the sixteenth century. (Of over thirty such editions, few are identical.) It is not identical to the text reflected in the AV (though it is quite close) nor yet to the so-called "Syrian" or "Byzantine" text (these terms will be introduced presently). The critical edition of the "Byzantine" text prepared by Zane C. Hodges, former Professor of New Testament Literature and Exegesis at the Dallas Theological Seminary, Arthur L. Farstad, and others, and published by Thomas Nelson in 1982, differs from the *Textus Receptus* in over 1,500 places.

<sup>7</sup>Colwell, "Hort Redivivus," p. 158.

<sup>8</sup>Colwell, "Genealogical Method: Its Achievements and its Limitations," *Journal of Biblical Literature*, LXVI (1947), 111.

<sup>9</sup>In fact, Hort did not hold to a high view of inspiration. Cf. A.F. Hort, I, 419-21 and Westcott and Hort, II, "Introduction," 280-81.

plurality of documents. In dealing with the text of the New Testament no new principle whatever is needed or legitimate."<sup>10</sup>

This stance required the declared presupposition that no malice touched the text. "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."<sup>11</sup>

Such a position allowed him to bring over into the textual criticism of the New Testament the family-tree method, or genealogy, as developed by students of the classics.

## Genealogy

Here is Hort's classic definition of genealogical method:

The proper method of Genealogy consists . . . in the more or less complete recovery of the texts of successive ancestors by analysis and comparison of the varying texts of their respective descendants, each ancestral text so recovered being in its turn used, in conjunction with other similar texts, for the recovery of the text of a yet earlier common ancestor.<sup>12</sup>

Colwell says of Hort's use of this method:

As the justification of their rejection of the majority, Westcott and Hort found the possibilities of genealogical method invaluable. Suppose that there are only ten copies of a document and that nine are all copied from one; then the majority can be safely rejected. Or suppose that the nine are copied from a lost manuscript and that this lost manuscript and the other one were both copied from the original; then the vote of the majority would not outweigh that of the minority. These are the arguments with which W. and H. opened their discussion of genealogical method. . . . They show clearly that a majority of manuscripts is not **necessarily** to be preferred as correct. It is this *a priori* possibility which Westcott and Hort used to demolish the argument based on the numerical superiority of the adherents of the Textus Receptus.<sup>13</sup>

It is clear that the notion of genealogy is crucial to Hort's theory and purpose. He felt that the genealogical method enabled him to reduce the mass of manuscript testimony to four voices—"Neutral," "Alexandrian," "Western," and "Syrian."

## Text-types and Recensions

To sum up what has been said on the results of genealogical evidence proper, as affecting the text of the New Testament, we regard the following propositions as absolutely certain. (I) The great ancient texts did actually exist as we have described them in Sections II and III. . . . (III) The extant documents contain no readings (unless the peculiar Western non-interpolations noticed above are counted as exceptions), which suggest the existence of important textual events unknown to us, a knowledge of which could materially alter the interpretation of evidence as determined by the above history.<sup>14</sup>

The "great ancient texts" are the four named above. Although Hort's "Neutral" and "Alexandrian" are now generally lumped together and called "Alexandrian", and Hort's "Syrian" is now usually named "Byzantine", and the literature refers to an added text-type, "Caesarean", the notion of at least three major text-types or recensions dominates the field to this day. Here is another basic tenet of Hort's theory.

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<sup>10</sup>Westcott and Hort, p. 73.

<sup>11</sup>*Ibid.*, p. 282.

<sup>12</sup>*Ibid.*, p. 57.

<sup>13</sup>Colwell, "Genealogical Method," p. 111.

<sup>14</sup>Westcott and Hort, pp. 178-9. Note that Hort made use of only a small fraction of the manuscripts extant in his day. Cf. K. Aland, "The Significance of the Papyri," pp. 327-28. A check of W-H's "Notes on Select Readings" in volume 2 of their *The New Testament in the Original Greek* suggests that Aland is probably generous.

Having, ostensibly, justified the handling of the mass of later manuscripts as one witness or text, Hort now moved to demonstrate that this supposed text was an inferior, even inconsequential, witness. The first proof put forward was "conflation".

## Conflation

Once manuscripts are assigned to different text-types on the basis of characteristic variants shared in common, almost any early manuscript that one chances to pick up is observed to exhibit variants thought to be diagnostic or characteristic of alien text-types. Such a situation has been called "mixture". "Conflation" is a special kind of mixture. In Hort's words,

The clearest evidence for tracing the antecedent factors of mixture in texts is afforded by readings which are themselves mixed or, as they are sometimes called, 'conflate,' that is, not simple substitutions of the reading of one document for that of another, but combinations of the readings of both documents into a composite whole, sometimes by mere addition with or without a conjunction, sometimes with more or less of fusion.<sup>15</sup>

Hort urged the conclusion that a text containing conflate readings must be posterior in date to the texts containing the various components from which the conflations were constructed.<sup>16</sup> Then he produced eight examples<sup>17</sup> where, by his interpretation, the "Syrian" (Byzantine) text had combined "Neutral" and "Western" elements. He went on to say:

To the best of our belief the relations thus provisionally traced are never inverted. We do not know of any places where the  $\alpha$  group of documents supports readings apparently conflate from the readings of the  $\beta$  and  $\delta$  groups respectively, or where the  $\beta$  group of documents supports readings apparently conflate from the readings of the  $\alpha$  and  $\delta$  groups respectively.<sup>18</sup>

It was essential to Hort's purpose of demonstrating the "Syrian" text to be posterior that he not find any inversion of the relationships between the three "texts." (An "inversion" would be either the "Neutral" or the "Western" text containing a conflation from the other plus the "Syrian.") So he claimed that inversions do not exist.

Hort's statement and interpretation have been generally accepted.<sup>19</sup> Vincent Taylor calls the argument "very cogent indeed."<sup>20</sup> Kirsopp Lake calls it "the keystone of their theory."<sup>21</sup> Here is another tenet crucial to Hort's theory and purpose. For a second and independent proof of the posteriority of the "Syrian" text he turned to the ante-Nicene Fathers.

## "Syrian" Readings Before Chrysostom

After a lengthy discussion, Hort concluded:

Before the middle of the third century, at the very earliest, we have no historical signs of the existence of readings, conflate or other, that are marked as distinctively Syrian by the want of attestation from groups of documents which have preserved the other ancient forms of text. This is a fact of great significance, ascertained as it is exclusively by external evidence, and therefore

<sup>15</sup>Westcott and Hort, p. 49.

<sup>16</sup>*Ibid.*, p. 106. This seems obvious enough, since the materials used to manufacture something must of necessity exist before the resulting product. A clear putative example occurs in Luke 24:53. The "Western" text has "praising God," the "Neutral" text has "blessing God" and the "Syrian" text has "praising and blessing God." According to Hort's hypothesis the longest reading was constructed out of the two shorter ones. Note that the use of the word "conflation" embodies the rejection of the possibility that the longer reading is original and that the shorter ones are independent simplifications of that original longer reading.

<sup>17</sup>Mark 6:33; 8:26; 9:38; 9:49; Luke 9:10; 11:54; 12:18; 24:53.

<sup>18</sup>Westcott and Hort, p. 106. By " $\alpha$  group" Hort means his "Neutral" text, by " $\beta$  group" he means his "Western" text, and by " $\delta$  group" he means his "Syrian" text.

<sup>19</sup>Cf. Kenyon, p. 302; E.F. Harrison, *Introduction to the New Testament* (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1964), p. 73; and Metzger, *The Text*, pp. 135-36.

<sup>20</sup>Taylor, p. 53.

<sup>21</sup>Lake, p. 68.

supplying an absolutely independent verification and extension of the result already obtained by comparison of the internal character of readings as classified by conflation.<sup>22</sup>

Elsewhere he considered that Chrysostom (who died in 407) was the first Father to characteristically use the "Syrian" text.<sup>23</sup>

The importance of this argument to Hort's theory has been recognized by Kenyon.

Hort's contention, which was the corner-stone of his theory, was that readings characteristic of the Received Text are never found in the quotations of Christian writers prior to about A.D. 350. Before that date we find characteristically "Neutral" and "Western" readings, but never "Syrian". This argument is in fact decisive; . . .<sup>24</sup>

Lake, also, considered it to be decisive.<sup>25</sup>

Hort's purpose would appear to have been achieved, but for good measure he advanced a third argument against the "Syrian" text, one based on internal evidence.

## Internal Evidence of Readings

Such "evidence" is based on two kinds of probability, intrinsic and transcriptional. Intrinsic probability is author oriented—what reading makes the best sense, best fits the context, and conforms to the author's style and purpose? Transcriptional probability is scribe or copyist oriented—what reading can be attributed to carelessness or officiousness on the part of the copyist? Aside from inadvertent mistakes, presumed deliberate changes have given rise to two important canons of criticism—*brevior lectio potior*, the shorter reading is to be preferred (on the assumed propensity of scribes to add material to the text), and *proclivi lectioni praestat ardua*, the harder reading is to be preferred (on the assumed propensity of scribes to attempt to simplify the text when confronted with a supposed difficulty).

On the basis of such considerations, Hort declared the "Syrian" text to be characterized by "lucidity and completeness," "apparent simplicity," "harmonistic assimilation," and as being "conspicuously a full text."<sup>26</sup> He said further:

In themselves Syrian readings hardly ever offend at first. With rare exceptions they run smoothly and easily in form, and yield at once to even a careless reader a passable sense, free from surprises and seemingly transparent. But when distinctively Syrian readings are minutely compared one after the other with the rival variants, their claim to be regarded as the original readings is found gradually to diminish, and at last to disappear.<sup>27</sup>

Hort's characterization of the "Syrian" text has been generally accepted by subsequent scholars.<sup>28</sup>

Even after demonstrating, so he thought, the "Syrian" text to be eclectic and late, Hort had a major obstacle to hurdle. He had to explain how this "text" came into being, and above all how it came to dominate the field from the fifth century on. An organized revision of the text, executed and imposed upon the churches by ecclesiastical authority, was his solution to the problem.

## The "Lucianic Recension" and the Peshitta

"The Syrian text," Hort said, "must in fact be the result of a 'recension' in the proper sense of the word, a work of attempted criticism, performed deliberately by editors and not merely by scribes."<sup>29</sup>

<sup>22</sup>Westcott and Hort, p. 115.

<sup>23</sup>*Ibid.*, p. 91.

<sup>24</sup>F.G. Kenyon, *Recent Developments in the Textual Criticism of the Greek Bible* (London: Oxford University Press, 1933), pp. 7-8.

<sup>25</sup>Lake, p. 72.

<sup>26</sup>Westcott and Hort, pp. 134-35.

<sup>27</sup>*Ibid.*, pp. 115-16.

<sup>28</sup>See, for example, Kenyon, *Recent Developments*, p. 66, Metzger, *The Text*, p. 131, and Greenlee, p. 91.

An authoritative Revision at Antioch . . . was itself subjected to a second authoritative Revision carrying out more completely the purposes of the first. At what date between A.D. 250 and 350 the first process took place, it is impossible to say with confidence. The final process was apparently completed by A.D. 350 or thereabouts.<sup>30</sup>

Hort tentatively suggested Lucian (who died in 311) as perhaps the leader in the movement and some scholars subsequently became dogmatic on the subject.

The matter of the Syriac Peshitta version is often treated in connection with the "Lucianic recension" of the Greek because of a supposed connection between them. Because the Peshitta does witness to the "Byzantine" text Hort had to get it out of the second and third centuries. Accordingly, he posited a late recension to account for it. F.C. Burkitt went further than Hort and specified Rabbula, Bishop of Edessa from A.D. 411-435, as the author of the revision.<sup>31</sup>

Both ideas have had a wide acceptance. H.C. Thiessen's statement is typical, both in content and dogmatism.

This [Peshitta] was formerly regarded as the oldest of the Syrian versions; but Burkitt has shown that it is in reality a revision of the Old Syriac made by Rabbula, Bishop of Edessa, about the year 425. This view is now held by nearly all Syriac scholars. . . . The text of the Peshitta is now identified as the Byzantine text, which almost certainly goes back to the revision made by Lucian of Antioch about A.D. 300.<sup>32</sup>

## Summary and Consequences

And there you have the essence of the W-H critical theory. I have read every word of Hort's "Introduction," all 324 difficult pages of it, and I believe the description offered above is a reasonable one. Suffice it to say that Hort achieved his purpose, even if it took him twenty-eight years. Although such men as Tischendorf, Tregelles, and Alford had done much to undermine the position of the TR (*Textus Receptus*), Westcott and Hort are generally credited with having furnished the deathblow, beginning a new era. Many scholars have written to this effect,<sup>33</sup> but Colwell expresses it as well as anyone.

The dead hand of Fenton John Anthony Hort lies heavy upon us. In the early years of this century Kirsopp Lake described Hort's work as a failure, though a glorious one. But Hort did **not** fail to reach his major goal. He dethroned the *Textus Receptus*. After Hort, the late medieval Greek Vulgate was not used by serious students, and the text supported by earlier witnesses became the standard text. This was a sensational achievement, an impressive success. Hort's success in this task and the cogency of his tightly reasoned theory shaped—and still shapes—the thinking of those who approach the textual criticism of the NT through the English language.<sup>34</sup>

And that explains the nature and extent of the common divergence of the modern versions from the AV (King James Version)—they are all based essentially on the W-H theory and text whereas the AV is essentially based on the *Textus Receptus*.

But the question remains: Has the apparent potential for improving the text (arising from increased materials and "wisdom") been realized? Did the translators of the RSV, for instance, make better use of the manuscripts and employ superior principles of textual criticism than did the translators of

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<sup>29</sup>Westcott and Hort, p. 133.

<sup>30</sup>*Ibid.*, p. 137.

<sup>31</sup>F.C. Burkitt, *Evangelion da-Mepharreshe* (2 vols.; Cambridge: Cambridge University Press, 1904), II, 161.

<sup>32</sup>H.C. Thiessen, *Introduction to the New Testament* (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1955), pp. 54-55.

<sup>33</sup>See footnote 30, Chapter 2.

<sup>34</sup>Colwell, "Scribal Habits," p. 370.

the AV? Well, the principles they used led them to adopt the W-H text with very little variation, and that text is based essentially on just two manuscripts, Codices B and Aleph.<sup>35</sup>

Hort declared: "It is our belief (1) that the readings of  $\aleph$  B should be accepted as the true readings until strong internal evidence is found to the contrary, and (2) that no readings of  $\aleph$  B can safely be rejected absolutely. . . ."<sup>36</sup>

Again, Hort said of B and Aleph, "The fullest comparison does but increase the conviction that their preeminent relative purity is likewise approximately absolute, a true approximate reproduction of the text of the autographs."<sup>37</sup> One wonders whether the W-H theory and text would ever have seen the light of day had Codex B not been extant. Hort gave himself away while discussing genealogy.

In the Apocalypse the difficulty of recognizing the ancient texts is still greater, owing to the great relative paucity of documents, and especially the absence or loss of this book from the Vatican MS (B) which is available for nearly all the rest of the New Testament; and thus the power of using a directly genealogical method is much limited.<sup>38</sup>

The practical effect of the W-H theory was a complete rejection of the "Syrian" text and an almost exclusive preference for the "Neutral" text (equals B and Aleph). Subsequent scholarship has generally rejected the notion of a "Neutral" text but sustained the rejection of the "Syrian" text.

Curiously, there seems to be a determination not to reconsider the status of the "Syrian" text even though each of the arguments Hort used in relegating it to oblivion has been challenged. Thus J.N. Birdsall, after referring to the work of Lake, Lagrange, Colwell and Streeter, as well as his own, declares: "It is evident that all presuppositions concerning the Byzantine text—or texts—except its inferiority to other types, must be doubted and investigated *de novo*."<sup>39</sup> (But doesn't the supposed inferiority depend on those presuppositions?)

Recalling what has already been said above in the discussion of eclecticism, it seems evident that Clark is quite right when he says that "textual theory appears to have reached an impasse in our time."<sup>40</sup>

Since Hort's purpose was to get rid of the "Syrian" text and that is the one point of his theory that subsequent scholars have generally not questioned, perhaps it is time to ask whether that circumstance may not have something to do with the present confusion and impasse, and to wonder whether Hort was really right. I proceed to work through Hort's theory again, point by point, to inquire to what extent it corresponds to the evidence.

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<sup>35</sup>Cf. Colwell, "External Evidence and New Testament Criticism," *Studies in the History and Text of the New Testament*, eds. B.L. Daniels and M.J. Suggs (Salt Lake City: University of Utah Press, 1967), p. 3; Colwell, "Hort Redivivus," p. 162; Clark, "Today's Problems," pp. 159-60; Epp, p. 390.

<sup>36</sup>Westcott and Hort, p. 225. Cf. pp. 212-13.

<sup>37</sup>*Ibid.*, p. 276. And, "B very far exceeds all other documents in neutrality of text," p. 171.

<sup>38</sup>*Ibid.*, pp. 109-10.

<sup>39</sup>J.N. Birdsall, "The Text of the Gospels in Photius," *Journal of Theological Studies*, VII (1956), p. 43. Some scholars seem even to reflect the emotion of the twenty-three-year-old Hort--not long ago Epp spoke of "the tyrannical *textus receptus*" (p. 386).

<sup>40</sup>Clark, "The Effect of Recent Textual Criticism," p. 50.

## AN EVALUATION OF THE W-H THEORY

### The Basic Approach

Should the New Testament be treated just like any other book? Will the procedures used on the works of Homer or Aristotle suffice? If both God and Satan had an intense interest in the fate of the New Testament text, presumably not. But how can we test the fact or extent of supernatural intervention? Happily we have eyewitness accounts to provide at least a partial answer. Hort said that "there are no signs of deliberate falsification of the text for dogmatic purposes," but the early Church Fathers disagree. Metzger states:

Irenaeus, Clement of Alexandria, Tertullian, Eusebius, and many other Church Fathers accused the heretics of corrupting the Scriptures in order to have support for their special views. In the mid-second century, Marcion expunged his copies of the Gospel according to Luke of all references to the Jewish background of Jesus. Tatian's Harmony of the Gospels contains several textual alterations which lent support to ascetic or encratite views.<sup>1</sup>

Gaius, an orthodox Father who wrote between A.D. 175 and 200, names Asclepiades, Theodotus, Hermophilus, and Apollonides as heretics who prepared corrupted copies of the Scriptures and who had disciples who multiplied copies of their fabrications.<sup>2</sup>

Surely Hort knew the words of Origen.

Nowadays, as is evident, there is a great diversity between the various manuscripts, either through the negligence of certain copyists, or the perverse audacity shown by some in correcting the text, or through the fault of those, who, playing the part of correctors, lengthen or shorten it as they please (*In Matth. tom. XV, 14; P. G. XIII, 1293*).<sup>3</sup>

Even the orthodox were capable of changing a reading for dogmatic reasons. Epiphanius states (ii.3b) that the orthodox deleted "he wept" from Luke 19:41 out of jealousy for the Lord's divinity.<sup>4</sup>

Subsequent scholarship has tended to recognize Hort's mistake. Colwell has done an instructive about-face.

The majority of the variant readings in the New Testament were created for theological or dogmatic reasons.

Most of the manuals and handbooks now in print (including mine!) will tell you that these variations were the fruit of careless treatment which was possible because the books of the New Testament had not yet attained a strong position as "Bible." The reverse is the case. It was because they were the religious treasure of the church that they were changed.<sup>5</sup>

<sup>1</sup>Metzger, *The Text*, p. 201. For actual examples from Irenaeus, Clement, Tertullian, and Eusebius, please see Sturz (pp. 116-19), who also has a good discussion of their significance. As he says, "While scribal blunders were recognized by them as one cause of variation, the strongest and most positive statements, by the Fathers, are in connection with the changes introduced by heretics" (p. 120). H.A. Sturz, *The Byzantine Text-Type and New Testament Textual Criticism* (Nashville: Thomas Nelson Publishers, 1984).

<sup>2</sup>J.W. Burgon, *The Revision Revised* (London: John Murray, 1883), p. 323.

<sup>3</sup>Colwell, "The Origin of Texttypes of New Testament Manuscripts." *Early Christian Origins*, ed. Allen Wikgren (Chicago: Quadrangle Books, 1961), p. 130.

<sup>4</sup>J.W. Burgon, *The Causes of the Corruption of the Traditional Text of the Holy Gospels*, arranged, completed and edited by Edward Miller (London: George Bell and Sons, 1896), pp. 211-12. Cf. Martin Rist, "Pseudepigraphy and the Early Christians," *Studies in New Testament and Early Christian Literature*, ed. D.E. Aune (Leiden: E.J. Brill, 1972), pp. 78-79.

<sup>5</sup>Colwell, *What is the Best New Testament?* (Chicago: The University of Chicago Press, 1952), p. 53. Observe that Colwell flatly contradicts Hort. Hort said there were no theologically motivated variants; Colwell says they are in the majority. But, in the next quote, Colwell uses the term "deliberately," without referring to theology (both quotes come from the same work, five pages apart). What is Colwell's real meaning? We may no longer ask him personally, but I will hazard the following interpretation on my own.

The MSS contain several hundred thousand variant readings. The vast majority of these are misspellings or other obvious errors due to carelessness or ignorance on the part of the copyists. As a sheer guess I would say there are between ten thousand and fifteen



The New Testament copies differ widely in nature of errors from copies of the classics. The percentage of variations due to error in copies of the classics is large. In the manuscripts of the New Testament most variations, I believe, were made deliberately.<sup>6</sup>

Matthew Black says flatly:

The difference between sacred writings in constant popular and ecclesiastical use and the work of a classical author has never been sufficiently emphasized in the textual criticism of the New Testament. Principles valid for the textual restoration of Plato or Aristotle cannot be applied to sacred texts such as the Gospels (or the Pauline Epistles). We cannot assume that it is possible by a sifting of 'scribal errors' to arrive at the prototype or autograph text of the Biblical writer.<sup>7</sup>

H.H. Oliver gives a good summary of the shift of recent scholarship away from Hort's position in this matter.<sup>8</sup>

The fact of deliberate, and apparently numerous, alterations in the early years of textual history is a considerable inconvenience to Hort's theory for two reasons: it introduces an unpredictable variable which the canons of internal evidence cannot handle, and it puts the recovery of the Original beyond reach of the genealogical method.<sup>9</sup>

thousand that cannot be so easily dismissed—i.e., a maximum of five percent of the variants are "significant". It is to this five percent that Colwell (and Kilpatrick, Scrivener, Zuntz, etc.) refers when he speaks of the "creation" of variant readings. A fair number of these are probably the result of accident also, but Colwell affirms, and I agree, that most of them were created deliberately.

But why would anyone bother to make deliberate changes in the text? Colwell answers, "because they were the religious treasure of the church." Some changes would be "well intentioned"—many harmonizations presumably came about because a zealous copyist felt that a supposed discrepancy was an embarrassment to his high view of Scripture. The same is probably true of many philological changes. For instance, the plain Koine style of the New Testament writings was ridiculed by the pagan Celsus, among others. Although Origen defended the simplicity of the New Testament style, the space that he gave to the question indicates that it was a matter of some concern (*Against Celsus*, Book VI, chapters 1 and 2), so much so that there were probably those who altered the text to "improve" the style. Again, their motive would be embarrassment, deriving from a high view of Scripture. Surely Colwell is justified in saying that the motivation for such variants was theological even though no obvious doctrinal axe is being ground.

To judge by the emphatic statements of the early Fathers, there were many other changes that were not "well intentioned." It seems clear that numerous variants existed in the second century that have not survived in any extant MS. Metzger refers to Gwilliam's detailed study of chapters 1-14 of Matthew in the Syriac Peshitta as reported in "The Place of the Peshitta Version in the Apparatus Criticus of the Greek N.T.," *Studia Biblica et Ecclesiastica* V, 1903, 187-237. From the fact that in thirty-one instances the Peshitta stands alone (in those chapters), Gwilliam concluded that its unknown author "revised an ancient work by Greek MSS which have no representative now extant (p. 237)" (*The Early Versions of the New Testament*, Oxford, 1977, p. 61). In a personal communication, Peter J. Johnston, a member of the IGNT editorial panel working specifically with the Syriac Versions and Fathers, says of the Harklean Version: "Readings confidently referred to in the Harklean margin as in 'well-approved MSS at Alexandria' have sometimes not come down to us at all, or if they have, they are found only in medieval minuscule MSS." In commenting upon the discrepancies between Jerome's statements of MS evidence and that extant today, Metzger concludes by saying, "the disquieting possibility remains that the evidence available to us today may, in certain cases, be totally unrepresentative of the distribution of readings in the early church" ("St. Jerome's explicit references to variant readings in manuscripts of the New Testament," *Text and Interpretation: Studies in the New Testament presented to Matthew Black*, edited by Best and McL. Wilson, Cambridge: University Press, 1979, p. 188).

Some of my critics seem to feel that the extant evidence from the early centuries is representative (cf. Fee, "A Critique," p. 405). However, there is good reason for believing that it is not, and in that event the extant MSS may preserve some random survivors from sets of alterations designed to grind one doctrinal axe or another. The motivation for such a reading in isolation would not necessarily be apparent to us today.

I would go beyond Colwell and say that the disposition to alter the text, even with "good motives," itself bespeaks a mentality which has theological implications.

(Those who are prepared to take the Sacred Text seriously would do well to ponder the implications of Ephesians 2:2, "the spirit [Satan] presently at work in the sons of disobedience," not only during the first 200 years of the Church but also during the last 200.)

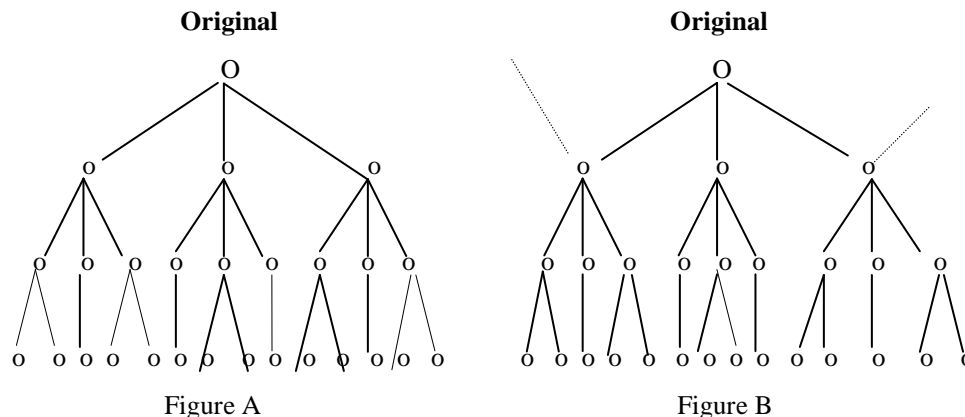
<sup>6</sup>Colwell, *What is the Best New Testament?*, p. 58.

<sup>7</sup>M. Black, *An Aramaic Approach to the Gospels and Acts* (Oxford: Oxford University Press, 1946), p. 214.

<sup>8</sup>H.H. Oliver, "Present Trends in the Textual Criticism of the New Testament," *The Journal of Bible and Religion*, XXX (1962), 311-12. Cf. C.S.C. Williams, *Alterations to the Text of the Synoptic Gospels and Acts* (Oxford: Basil Blackwell, 1951), pp. 14-17.

<sup>9</sup>The "inconvenience" referred to is virtually fatal to the W-H theory, at least as formulated in their "Introduction." The W-H theory is much like a multistoried building—each level depends on the one below it. Thus, Hort's simplistic notion of "genealogy" absolutely depends upon the allegation that there was no deliberate alteration of the Text, and his notion of "text-types" absolutely depends upon "genealogy," and his arguments concerning "conflation" and "Syrian" readings before Chrysostom absolutely depend upon those "text-types." The foundation for the whole edifice is Hort's position that the New Testament was an ordinary book that enjoyed an ordinary transmission. With its foundation removed, the edifice collapses.

To illustrate the second point, Hort's view of early textual history may be represented by figure A whereas the view suggested by the Church Fathers may be represented by figure B. The dotted lines in figure B represent the fabrications introduced by different heretics (as the early Fathers called them).



Genealogy cannot arbitrate the conflicting claims posed by the first line of descendants in Figure B.<sup>10</sup> Further, in Colwell's words, this method (genealogy)

rested on identity in *error* as the clue to common ancestry. These errors were unintentional changes which can be identified objectively as error. Agreement in readings of this kind seldom occurs by chance or coincidence. The New Testament copies differ widely from copies of the classics at this point. The percentage of variations due to error in copies of the classics is large. In the manuscripts of the New Testament, on the other hand, scholars now believe that most variations were made deliberately.<sup>11</sup>

The reconstruction of family trees is seriously complicated by the presence of deliberate alterations. And those are not the only difficulties under which genealogy labors.

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Fee seems to miss the point when he says, "if the 'foundation' is found to be secure, then the superstructure may only need some reinforcing, not demolition" ("A Critique," p. 404). The removal of any of the intervening floors as well will 'destroy the building,' that is, invalidate Hort's conclusions. It seems to me that the first three floors of Hort's building, at least, are beyond restoration.

Fee claims that I confuse "deliberate" and "dogmatic" changes and in consequence my critique of Hort's foundation fails ("A Critique," pp. 404-8). In his own words, "The vast majority of textual corruptions, though deliberate, are **not** malicious, nor are they theologically motivated. And **since** they are not, Pickering's view of 'normal' transmission (which is the crucial matter in his theory) simply disintegrates" (p. 408).

Fee fastens upon my use of the term "malicious," which I use only in discussing the **abnormal** transmission. I nowhere say that a majority of variants are malicious. The clear testimony of the early Fathers indicates that some must be, and I continue to insist that Hort's theory cannot handle such variants. (Fee seriously distorts my position by ignoring my discussion of the **abnormal** transmission. It would appear that the distortion was deliberate since he cites my pp. 104-110 for the "normal" transmission, whereas pp. 107-110 contain my treatment of the abnormal transmission.) But what are the implications of Fee's admission that the vast majority of textual corruptions are "deliberate"? Setting aside the question of theological motivation, can the canons of internal evidence really handle "deliberate" variants?

In Appendix E van Bruggen shows that supposed harmonizations may reasonably have other explanations. Fee himself recognizes this possibility ("Modern Text Criticism and the Synoptic Problem," *J.J. Griesbach: Synoptic and Text-Critical Studies 1776-1976*, ed. B. Orchard and T.R.W. Longstaff, Cambridge: University Press, 1976, p. 162). On the next page Fee recognizes another problem.

It should candidly be admitted that our predilections toward a given solution of the Synoptic Problem will sometimes affect textual decisions. Integrity should cause us also to admit to a certain amount of inevitable circular reasoning at times. A classic example of this point is the well-known 'minor agreement' between Matt. 26:67-8 and Luke 22:64 (//Mark 14:65) of the 'addition' *τις εστιν ο παισας σε*. B.H. Streeter, G.D. Kilpatrick, and W.R. Farmer each resolve the textual problem of Mark in a different way. In each case, a given solution of the Synoptic Problem has affected the textual decision. At this point one could offer copious illustrations.

Fee's ("Rigorous") debate with Kilpatrick ("Atticism") demonstrates that possible philological changes are capable of contradictory interpretations on the part of scholars who both use internal evidence. In sum, I reiterate that the canons of internal evidence **cannot** give us dependable interpretations with reference to deliberate variants. Those who use such canons are awash in a sea of speculation.

<sup>10</sup>Further, if a genealogical reconstruction ends up with only two immediate descendants of the Original, as in Hort's own reconstruction, then the genealogical method ceases to be applicable, as Hort himself recognized. Westcott and Hort, p. 42.

<sup>11</sup>Colwell, *What is the Best New Testament?*, p. 49.

## Genealogy

We have already noted Hort's definition and supposed use of genealogy. However, scholars have so far isolated only a few parent-child sets among all 5,000 plus manuscripts.<sup>12</sup> How then did Hort go about plotting the genealogical descent of the extant MSS? M.M. Parvis answers: "Westcott and Hort never applied the genealogical method to the NT MSS, . . ."<sup>13</sup> Colwell agrees.

That Westcott and Hort did not apply this method to the manuscripts of the New Testament is obvious. Where are the charts which start with the majority of late manuscripts and climb back through diminishing generations of ancestors to the Neutral and Western texts? The answer is that they are nowhere. Look again at the first diagram, and you will see that a, b, c, etc. are not actual manuscripts of the New Testament, but hypothetical manuscripts. The demonstrations or illustrations of the genealogical method as applied to New Testament manuscripts by the followers of Hort, the "Horticuli" as Lake called them, likewise use hypothetical manuscripts, not actual codices. Note, for example, the diagrams and discussions in Kenyon's most popular work on textual criticism, including the most recent edition. All the manuscripts referred to are imaginary manuscripts, and the later of these charts was printed sixty years after Hort.<sup>14</sup>

How then could Hort speak of only "occasional ambiguities in the evidence for the genealogical relations,"<sup>15</sup> or say: "So far as genealogical relations are discovered with perfect certainty, the textual results which follow from them are perfectly certain, too, being directly involved in historical facts; and any apparent presumptions against them suggested by other methods are mere guesses against knowledge"<sup>16</sup> when he had not demonstrated the existence of **any** such relations, much less with "perfect certainty"?

Another challenge to genealogy is "mixture."

The second limitation upon the application of the genealogical method to the manuscripts of the New Testament springs from the almost universal presence of mixture in these manuscripts. . . .

The genealogical diagram printed above (p. 110) from Westcott and Hort shows what happens *when there is no mixture*. When there *is* mixture, and Westcott and Hort state that it is common, in fact almost universal in some degree, then the genealogical method *as applied to manuscripts* is useless.

*Without* mixture a family tree is an ordinary tree-trunk with its branches—standing on the branches with the single trunk—the original text—at the top. The higher up—or the further back—you go from the mass of late manuscripts, the fewer ancestors you have!

*With* mixture you reverse this in any series of generations. The number of possible combinations defies computation, let alone the drawing of diagrams.<sup>17</sup>

Other scholars have agreed that the genealogical method has never been applied to the New Testament, and they state further that it *cannot* be applied. Thus, Zuntz says it is "inapplicable,"<sup>18</sup> Vaganay

<sup>12</sup>Codex Claromontanus apparently has a "child" three centuries younger than it (also, minuscule 205 may have been copied from 208). Codices F and G containing Paul's Epistles appear to be almost twin brothers, and groups like family 1 and family 13 are clearly closely related. Also, in the Apocalypse Hoskier has identified a number of related groups, which include a few sets.

<sup>13</sup>Parvis, p. 611. Fee says much the same. "Properly speaking, genealogy must deal with the descent of manuscripts and must reconstruct stemmata for that descent. This Hort never did; rather he applied the method to text-types, and he did so **not** to find the original text, but to eliminate the Byzantine manuscripts from further consideration" ("Modern Text Criticism," pp. 155-56).

<sup>14</sup>Colwell, "Genealogical Method," pp. 111-12.

<sup>15</sup>Westcott and Hort, p. 63.

<sup>16</sup>*Ibid.*

<sup>17</sup>Colwell, "Genealogical Method," p. 114. The sort of genealogical diagram that one always sees is like a family tree that shows only male parents. Because of mixture the diagrams should be like a family tree that shows **both** parents, at every level—the farther back you go the more hopelessly complicated it gets.

<sup>18</sup>Zuntz, p. 155.

that it is "useless,"<sup>19</sup> and Aland that it "cannot be applied to the NT."<sup>20</sup> Colwell also declares emphatically "that it *cannot be* so applied."<sup>21</sup> In the light of all this, what are we to think of Hort when he asserts:

For skepticism as to the possibility of obtaining a trustworthy genealogical interpretation of documentary phenomena in the New Testament there is, we are persuaded, no justification either in antecedent probability or in experience. . . . Whatever may be the ambiguity of the whole evidence in particular passages, the general course of future criticism must be shaped by the happy circumstance that the fourth century has bequeathed to us two MSS of which even the less incorrupt must have been of exceptional purity among its own contemporaries.<sup>22?</sup>

After demolishing the genealogical method, Colwell concludes his article by saying, "yet Westcott and Hort's genealogical method slew the Textus Receptus. The *a priori* demonstration is logically irrefutable."<sup>23</sup> However, the *a priori* demonstration cannot stand in the face of an *a posteriori* demonstration to the contrary. Colwell himself, some twelve years prior to this statement, recognized that the "*a priori* demonstration" to which he here refers has been refuted.

The universal and ruthless dominance of the middle ages by one texttype is now recognized as a myth. . . .

The complexities and perplexities of the medieval text have been brought forcibly to our attention by the work of two great scholars: Hermann von Soden and Kirsopp Lake. . . .

This invaluable pioneer work of von Soden greatly weakened the dogma of the dominance of a homogenous Syrian text. But the fallacy received its death blow at the hands of Professor Lake. In an excursus published with his study of the Caesarean text of Mark, he annihilated the theory that the middle ages were ruled by a single recension which attained a high degree of uniformity.<sup>24</sup>

Actually, Hort produced no "demonstration" at all—just assumptions. Since the genealogical method has not been applied to the MSS of the New Testament it may not be used as an integral part of a theory of NT textual criticism. If it was Hort's genealogical method that "slew the Textus Receptus" then the TR must still be alive and well—the weapon was never used. But Hort claimed to have used it, and the weapon was so fearsome, and he spoke of the "results" with such confidence, that he won the day.

Since Westcott and Hort, the genealogical method has been the canonical method of restoring the original text of the books of the New Testament. It dominates the handbooks. Sir Frederic Kenyon, C.R. Gregory, Alexander Souter, and A.T Robertson are a few of the many who declare its excellence.<sup>25</sup>

The situation is essentially the same today, and the warning Colwell gave in 1965 is still valid.

Many years ago I joined others in pointing out the limitations in Hort's use of genealogy, and the inapplicability of genealogical method—strictly defined—to the textual criticism of the NT. Since then many others have assented to this criticism, and the building of family trees is only rarely attempted. Therefore we might assume that the influence of Hort's emphasis upon genealogical method is no longer a threat. But this assumption is false.

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<sup>19</sup>L. Vaganay, *An Introduction to the Textual Criticism of the New Testament*, translated by B.V. Miller (London: Sands and Company, 1937), p. 71.

<sup>20</sup>Aland, "The Significance of the Papyri," p. 341.

<sup>21</sup>Colwell, "External Evidence," p. 4.

<sup>22</sup>Westcott and Hort, p. 287.

<sup>23</sup>Colwell, "Genealogical Method," p. 124.

<sup>24</sup>Colwell, "The Complex Character of the Late Byzantine Text of the Gospels," *Journal of Biblical Literature*, LIV (1935), 212-

13.

<sup>25</sup>Colwell, "Genealogical Method," p. 109.

Hort's brilliant work still captivates our minds. So when confronted by a reading whose support is minimal and widely divorced in time and place, we think first and only of genealogical relationships. Hort has put genealogical blinders on our eyes. . . .<sup>26</sup>

Present-day scholars, exegetes, and translators continue to act as though the genealogical method not only can be, but has been, applied to the NT MSS, and to base their work on the supposed results. But what about those "results"?

## Text-types and Recensions

Although Hort claimed absolute certainty for the results of genealogical evidence as described by him, it is clear that the "results" were a fabrication. How could there be results if the method was never applied to the MSS? A contemporary of W-H protested that such claims would only be allowable if the textual critic had first indexed every principal Church Father and reduced MSS to families by a laborious process of induction.<sup>27</sup>

Still, Hort's "results" became accepted as fact by many—George Salmon spoke of "the servility with which his [Hort] history of the text has been accepted, and even his nomenclature adopted, as if now the last word had been said on the subject of New Testament criticism. . . ."<sup>28</sup>

### Subsequent scholarship

Subsequent scholars have been obliged to reconsider the matter by the discovery of the Papyri and closer looks at MSS previously extant. Parvis complains:

We have reconstructed text-types and families and sub families and in so doing have created things that never before existed on earth or in heaven. We have assumed that manuscripts reproduced themselves according to the Mendelian law. But when we have found that a particular manuscript would not fit into any of our nicely constructed schemes, we have thrown up our hands and said that it contained a mixed text.<sup>29</sup>

Allen Wikgren shows that sweeping generalizations about text-types in general and the "Byzantine" text and Lectionaries in particular, should not be made.<sup>30</sup> Colwell affirms:

The major mistake is made in thinking of the "old text-types" as frozen blocks, even after admitting that no one manuscript is a perfect witness to any text-type. *If* no one MS is a perfect witness to any type, then all witnesses are mixed in ancestry (or individually corrupted, and thus parents of mixture).<sup>31</sup>

After careful study of P<sup>46</sup>, Zuntz makes certain observations and concludes:

One would like to think that observations like these must put an end to time-honoured doctrines such as that the text of B is the 'Neutral' text or that the 'Western' text is 'the' text of the second century. If the factors of each of these equations are meant to be anything but synonyms, they are wrong; if they are synonyms, they mean nothing.<sup>32</sup>

Klijn doubts "whether any grouping of manuscripts gives satisfactory results,"<sup>33</sup> and goes on to say:

<sup>26</sup>Colwell, "Scribal Habits," pp. 370-71.

<sup>27</sup>Burgon, *The Revision Revised*, p. 358. Burgon's own index of the Fathers is no doubt still the most extensive in existence—it contains 86,489 quotations.

<sup>28</sup>G. Salmon, *Some Thoughts on the Textual Criticism of the New Testament* (London, 1897), p. 33.

<sup>29</sup>M.M. Parvis, "The Nature and Task of New Testament Textual Criticism," *The Journal of Religion*, XXXII (1952), 173.

<sup>30</sup>A. Wikgren, "Chicago Studies in the Greek Lectionary of the New Testament," *Biblical and Patristic Studies in Memory of Robert Pierce Casey*, ed. J.N. Birdsall and R.W. Thomson (New York: Herder, 1963), pp. 96-121.

<sup>31</sup>Colwell, "The Origin of Texttypes," p. 135.

<sup>32</sup>Zuntz, p. 240.

<sup>33</sup>Klijn, p. 36.

It is still customary to divide manuscripts into the four well-known families: the Alexandrian, the Caesarean, the Western and the Byzantine.

This classical division can no longer be maintained. . . .

If any progress is to be expected in textual criticism we have to get rid of the division into local texts. New manuscripts must not be allotted to a geographically limited area but to their place in the history of the text.<sup>34</sup>

After a long discussion of the "Caesarean" text, Metzger says by way of summary that "it must be acknowledged that at present the Caesarean text is disintegrating."<sup>35</sup> Two pages later, referring to the impact of P<sup>45</sup>, he asks, "Was there a fundamental flaw in the previous investigation which tolerated so erroneous a grouping?" Evidently there was. Could it be the mentality that insists upon thinking in terms of text-types and recensions as recognized and recognizable entities?<sup>36</sup> Those few men who have done extensive collations of manuscripts, or paid attention to those done by others, as a rule have not accepted such erroneous groupings.<sup>37</sup>

H. C. Hoskier, whose collations of NT MSS are unsurpassed in quality and perhaps in quantity, commented as follows after collating Codex 604 (today's 700) and comparing it with other MSS:

I defy anyone, after having carefully perused the foregoing lists, and after having noted the almost incomprehensible combinations and permutations of both the uncial and cursive manuscripts, to go back to the teaching of Dr. Hort with any degree of confidence. How useless and superfluous to talk of Evan. 604 having a large "Western element," or of its siding in many places with the "neutral text." The whole question of families and recensions is thus brought prominently before the eye, and with space one could largely comment upon the deeply interesting combinations which thus present themselves to the critic. But **do** let us realize that we are in the infancy of this part of the science, and not imagine that we have successfully laid certain immutable foundation stones, and can safely continue to build thereon. It is not so, and much, if not all, of these foundations must be demolished.<sup>38</sup>

## The "text-types" themselves

To take the "text-types" one by one, Kenyon says of the "Western" text:

What we have called the  $\delta$ -text, indeed, is not so much a text as a congeries of various readings, not descending from any one archetype, but possessing an infinitely complicated and

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<sup>34</sup>*Ibid.*, p. 66.

<sup>35</sup>Metzger, *Chapters in the History of New Testament Textual Criticism* (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1963), p. 67.

<sup>36</sup>Klijn seems to be of this opinion (pp. 33-34). Not so D.A. Carson. He refers to my position here as "a basic flaw in Pickering's overarching argument" (*The King James Version Debate*, Grand Rapids: Baker Book House, 1979, p. 108). After a confused discussion wherein he misrepresents my position (one of at least ten misrepresentations) Carson concludes by saying: "On the face of it, because one manuscript was copied from another or from several others, genealogical relationships **must** exist. The only question is whether or not we have identified such relationships, or can identify them" (p. 109). Exactly. Of course genealogical relationships must exist, or must have existed, but the **whole question** is "whether or not we have identified" them. I take it that Aland, Colwell, Klijn, Parvis, Vaganay, Wikgren, Zuntz, etc. are saying that such relationships have in fact **not** been identified. That is my point! And I insist that until such relationships are empirically demonstrated they may not legitimately be used in the practice of NT textual criticism. (Some of the above named scholars go on to affirm that we **cannot** identify such relationships, at least by direct genealogy—almost all the links are missing.)

The concepts of "text-type" and "recension", **as used by Hort** and his followers, are demonstrably erroneous. It follows that the conclusions based upon them are invalidated. But it remains true that community of reading implies a common origin, and agreement in error convicts the participants of dependence. Carson wishes to retain the term "text-type" to refer to "types of text as indexed by several remarkable extremes" (p. 109). That is fine with me, just so it is made clear to all that the term is not being used in the Hortian sense. For statements of evidence, however, I believe the editors of the UBS editions have set the correct example—no cover symbols for "text-types" are used except for "Byz", which refers to the Byzantine manuscript tradition.

<sup>37</sup>Cf. Burgon, *The Revision Revised*, p. 380.

<sup>38</sup>H.C. Hoskier, *A Full Account and Collation of the Greek Cursive Codex Evangelium 604* (London: David Nutt, 1890), Introduction, pp. cxv-cxvi.

intricate parentage. No one manuscript can be taken as even approximately representing the  $\delta$  text, if by "text" we mean a form of the Gospel which once existed in a single manuscript.<sup>39</sup>

Colwell observes that the Nestle text (25th edition) denies the existence of the "Western" text as an identifiable group, saying it is "a denial with which I agree."<sup>40</sup> Speaking of von Soden's classification of the "Western" text, Metzger says: "so diverse are the textual phenomena that von Soden was compelled to posit seventeen sub-groups of witnesses which are more or less closely related to this text."<sup>41</sup> And Klijn, speaking of "a 'pure' or 'original' Western Text" affirms that "such a text did not exist."<sup>42</sup> K. and B. Aland speak of "the phantom 'Western text'" and replace it with "D text", referring to Codex Bezae.<sup>43</sup> In fact, it has been many decades since any critical apparatus used a cover symbol for the so-called "Western" text.

As for today's "Alexandrian" text, which seems essentially to include Hort's "Neutral" and "Alexandrian," Colwell offers the results of an interesting experiment.

After a careful study of all alleged Beta Text-type witnesses in the first chapter of Mark, six Greek manuscripts emerged as primary witnesses:  $\aleph$  B L 33 892 2427. Therefore, the weaker Beta manuscripts C  $\Delta$  157 517 579 1241 and 1342 were set aside. Then on the basis of the six primary witnesses an 'average' or mean text was reconstructed including all the readings supported by the majority of the primary witnesses. Even on this restricted basis the amount of variation recorded in the apparatus was dismaying. In this first chapter, each of the six witnesses differed from the 'average' Beta Text-type as follows: L, nineteen times (Westcott and Hort, twenty-one times); Aleph, twenty-six times; 2427, thirty-two times; 33, thirty-three times; B, thirty-four times; and 892, forty-one times. These results show convincingly that any attempt to reconstruct an archetype of the Beta Text-type on a quantitative basis is doomed to failure. The text thus reconstructed is not reconstructed but constructed; it is an artificial entity that never existed.<sup>44</sup>

Hoskier, after filling 450 pages with a detailed and careful discussion of the errors in Codex B and another 400 on the idiosyncrasies of Codex  $\aleph$ , affirms that **in the Gospels alone** these two MSS differ well over 3,000 times, which number does not include minor errors such as spelling, nor variants between certain synonyms which might be due to "provincial exchange."<sup>45</sup> In fact, on the basis of Colwell's suggestion that a 70% agreement be required so as to assign two MSS to the same text-type, Aleph and B do not qualify. The UBS and Nestle texts no longer use a cover symbol for the "Alexandrian" text-type.

Of the "Byzantine" text, Zuntz says that "the great bulk of Byzantine manuscripts defies all attempts to group them."<sup>46</sup> Clark says much the same.

The main conclusion regarding the Byzantine text is that it was extremely fluid. Any single manuscript may be expected to show a score of shifting affinities. Yet within the variety and confusion, a few textual types have been distinguished. . . . These types are not closely grouped like the families, but are like the broad Milky Way including many members within a general affinity.<sup>47</sup>

<sup>39</sup>Kenyon, *Handbook*, p. 356. Whereas Hort used " $\delta$  group" to refer to his "Syrian" text, Kenyon uses " $\delta$  text" to refer to the "Western" text.

<sup>40</sup>Colwell, "The Greek New Testament with a Limited Critical Apparatus: its Nature and Uses," *Studies in New Testament and Early Christian Literature*, ed. D.E. Aune (Leiden: E.J. Brill, 1972), p. 33.

<sup>41</sup>Metzger, *The Text*, p. 141.

<sup>42</sup>Klijn, p. 64.

<sup>43</sup>K. and B. Aland, *The Text of the New Testament* (Grand Rapids: Eerdmans, 1987), pp. 55, 64.

<sup>44</sup>Colwell, "The Significance of Grouping of New Testament Manuscripts," *New Testament Studies*, IV (1957-1958), 86-87. Cf. also Colwell, "Genealogical Method," pp. 119-123. Colwell follows Kenyon and uses "Beta text-type" to refer to today's "Alexandrian" text, whereas Hort used " $\beta$  group" to refer to his "Western" text.

<sup>45</sup>H.C. Hoskier, *Codex B and its Allies* (2 vols.; London: Bernard Quaritch, 1914), II, 1.

<sup>46</sup>Zuntz, "The Byzantine Text in New Testament Criticism," *The Journal of Theological Studies*, XLIII (1942), 25.

<sup>47</sup>Clark, "The Manuscripts of the Greek New Testament," *New Testament Manuscript Studies*, ed. M.M. Parvis and A.P. Wikgren (Chicago: The University of Chicago Press, 1950), p. 12.

Colwell's emphatic statement to the same effect has been given above. The work of Lake referred to by Colwell was a collation of Mark, chapter eleven, in all the MSS of Mt. Sinai, Patmos, and the Patriarchal Library and collection of St. Saba at Jerusalem. Lake, with R. P. Blake and Silva New, found that the "Byzantine" text was not homogeneous, that there was an absence of close relationship between MSS, but that there was less variation "within the family" than would be found in a similar treatment of "Neutral" or "Caesarean" texts. In their own words:

This collation covers three of the great ancient collections of MSS; and these are not modern conglomerations, brought together from all directions. Many of the MSS, now at Sinai, Patmos, and Jerusalem, must be copies written in the scriptoria of these monasteries. We expected to find that a collation covering all the MSS in each library would show many cases of direct copying. But there are practically no such cases. . . . Moreover, the amount of direct genealogy which has been detected in extant codices is almost negligible. Nor are many known MSS sister codices. The Ferrar group and family 1 are the only reported cases of the repeated copying of a single archetype, and even for the Ferrar group there were probably two archetypes rather than one. . . .

There are cognate groups—families of distant cousins—but the manuscripts which we have are almost all orphan children without brothers or sisters.

Taking this fact into consideration along with the negative result of our collation of MSS at Sinai, Patmos, and Jerusalem, it is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred books.<sup>48</sup>

J.W. Burgon,<sup>49</sup> because he had himself collated numerous minuscule MSS, had remarked the same thing years before Lake.

Now those many MSS were executed demonstrably at different times in different countries. They bear signs in their many hundreds of representing the entire area of the Church, except where versions were used instead of copies in the original Greek. . . . And yet, of multitudes of them that survive, hardly any have been copied from any of the rest. On the contrary, they are discovered to differ among themselves in countless unimportant particulars; and every here and there single copies exhibit idiosyncrasies which are altogether startling and extraordinary. There has therefore demonstrably been no collusion—no assimilation to an arbitrary standard—no wholesale fraud. It is certain that every one of them represents a MS, or a pedigree of MSS, older than itself; and it is but fair to suppose that it exercises such representation with tolerable accuracy.<sup>50</sup>

Kurt Aland<sup>51</sup> sums it up:

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<sup>48</sup>K. Lake, R.P. Blake and Silva New, "The Caesarean Text of the Gospel of Mark," *Harvard Theological Review*, XXI (1928), 348-49. The more recent work of Frederick Wisse furnishes a strong objective demonstration of the diversity within the "Byzantine" textform. *The Profile Method for Classifying and Evaluating Manuscript Evidence* (Grand Rapids: Eerdmans, 1982), is an application of the "Claremont Profile Method" to 1,386 MSS in Luke 1, 10 and 20. He isolated 15 major groupings of MSS (which sub-divide into at least 70 subgroups), plus 22 smaller groups, plus 89 "mavericks" (MSS so mixed that they neither fit into any of the above groupings nor form groupings among themselves). One of the 15 "major" groups is the "Egyptian" ("Alexandrian")—it is made up of precisely four (04) uncials and four (04) cursives, plus two more of each that were "Egyptian" in one of the three chapters. If I understand him correctly he considers that virtually all the remaining MSS fall into the broad "Byzantine" stream. In other words, when we talk of examining the "Byzantine" text there are at least 36 strands of transmission that need to be considered!

<sup>49</sup>John William Burgon was Dean of Chichester from 1876 until his death in 1888. His biographer declared him to be "the leading religious teacher of his time" in England (E.M. Goulburn, *Life of Dean Burgon*, 2 Vols.; London: John Murray, 1892, I, vii). Clark lists Burgon along with Tregelles and Scrivener as "great contemporaries" of Tischendorf, whom he calls "the colossus among textual critics" ("The Manuscripts of the Greek New Testament," p. 9). As a contemporary of Westcott and Hort, Burgon strenuously opposed their text and theory and is generally acknowledged to have been the leading voice in the "opposition" (cf. A.F. Hort, II, 239).

<sup>50</sup>J.W. Burgon, *The Traditional Text of the Holy Gospels Vindicated and Established*, arranged, completed, and edited by Edward Miller (London: George Bell and Sons, 1896), pp. 46-47.

<sup>51</sup>Kurt Aland, former Director of the *Institut für neutestamentliche Textforschung* at Munster, was perhaps the leading textual critic in Europe until his death (1995). He was a co-editor of both the most popular editions of the Greek N.T.—Nestle and U.B.S. He was the one who cataloged each new MS that was discovered.



P<sup>66</sup> confirmed the observations already made in connection with the Chester Beatty papyri. With P<sup>75</sup> new ground has been opened to us. Earlier, we all shared the opinion, in agreement with our professors and in accord with NT scholarship, before and since Westcott and Hort, that, in various places, during the fourth century, recensions of the NT text had been made, from which the main text-types then developed. . . . We spoke of recensions and text-types, and if this was not enough, we referred to pre-Caesarean and other text-types, to mixed texts, and so on.

I, too, have spoken of mixed texts, in connection with the form of the NT text in the second and third centuries, but I have always done so with a guilty conscience. For, according to the rules of linguistic philology it is impossible to speak of mixed texts before recensions have been made (they only can follow them), whereas, the NT manuscripts of the second and third centuries which have a "mixed text" clearly existed before recensions were made. . . . The simple fact that all these papyri, with their various distinctive characteristics, did exist side by side, in the same ecclesiastical province, that is, in Egypt, where they were found, is the best argument against the existence of any text-types, including the Alexandrian and the Antiochian. We still live in the world of Westcott and Hort with our conception of different recensions and text-types, although this conception has lost its *raison d'être*, or, it needs at least to be newly and convincingly demonstrated. For, the increase of the documentary evidence and the entirely new areas of research which were opened to us on the discovery of the papyri, mean the end of Westcott and Hort's conception.<sup>52</sup>

(I have quoted men like Zuntz, Clark and Colwell on the "Byzantine" text to show that modern scholars are prepared to reject the notion of a "Byzantine" recension, but the main lesson to be drawn from the variation among "Byzantine" MSS is the one noted by Lake and Burgon—they are orphans, independent witnesses; at least in their generation. The variation between two "Byzantine" MSS will be found to differ both in number and severity from that between two "Western" MSS or two "Alexandrian" MSS—the number and nature of the disagreements between two "Byzantine" MSS throughout the Gospels will seem trivial compared to the number (over 3,000) and nature (many serious) of the disagreements between Aleph and B, the chief "Alexandrian" MSS, in the same space.)

### A recent return

Both Colwell<sup>53</sup> and Epp<sup>54</sup> take issue with Aland, claiming that the papyri fit right in with Hort's reconstruction of textual history. But the existence of an affinity between B and P<sup>75</sup> does not demonstrate the existence of a text-type or recension. We have just seen Colwell's demonstration and declaration that an "Alexandrian" archetype never existed. Epp himself, after going on to plot the early MSS on three trajectories ("Neutral," "Western," and "midway"), says:

Naturally, this rough sketch should not be understood to mean that the manuscripts mentioned under each of the three categories above necessarily had any **direct** connections one with another; rather, they stand as randomly surviving members of these three broad streams of textual tradition.<sup>55</sup>

The point is, although different manuscripts exhibit varying affinities, share certain peculiarities, they each differ substantially from all the others (especially the earlier ones) and therefore should not be lumped together. There is no such thing as the testimony of a "Western" or "Alexandrian" text-type (as an entity)—there is only the testimony of individual MSS, Fathers, Versions (or MSS of versions).

In disagreeing with Aland (see notes 52 and 54), Epp declared that our extant materials reveal "only two clear textual streams or trajectories" in the first four centuries of textual transmission, namely

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<sup>52</sup>Aland, "The Significance of the Papyri," pp. 334-37.

<sup>53</sup>Colwell, "Hort Redivivus," pp. 156-57.

<sup>54</sup>Epp, pp. 396-97.

<sup>55</sup>*Ibid.*, p. 398.

the "Neutral" and "Western" text-types.<sup>56</sup> He also suggested that P<sup>75</sup> may be considered as an early ancestor for Hort's "Neutral" text, P<sup>66</sup> for Hort's "Alexandrian" text, and P<sup>45</sup> for Hort's "Western" text.

But he himself had just finished furnishing counter evidence. Thus, with reference to 103 variation units in Mark 6-9 (where P<sup>45</sup> is extant), Epp records that P<sup>45</sup> shows a 38 percent agreement with Codex D, 40 percent with the *Textus Receptus*, 42 percent with B, 59 percent with f<sup>13</sup>, and 68 percent with W.<sup>57</sup> How can Epp say that P<sup>45</sup> is a "Western" ancestor when it is closer to chief representatives of every other "text-type" than it is to D? In Mark 5-16, Epp records that Codex W shows a 34 percent agreement with B, 36 percent with D, 38 percent with the *Textus Receptus*, and 40 percent with ℵ.<sup>58</sup> To which "textual stream" should W be assigned?

Both P<sup>66</sup> and P<sup>75</sup> have been generally affirmed to belong to the "Alexandrian text-type."<sup>59</sup> Klijn offers the results of a comparison of ℵ, B, P<sup>45</sup>, P<sup>66</sup> and P<sup>75</sup> in the passages where they are all extant (John 10:7-25, 10:32-11:10, 11:19-33 and 11:43-56). He considered only those places where ℵ and B disagree *and* where at least one of the papyri joins either ℵ or B. He found eight such places *plus* 43 where all three of the papyri line up with ℵ or B. He stated the result for the 43 places as follows (to which I have added figures for the *Textus Receptus*, BFBS 1946):

P<sup>45</sup> agrees with ℵ 19 times, with B 24 times, with TR 32 times,  
 P<sup>66</sup> agrees with ℵ 14 times, with B 29 times, with TR 33 times,  
 P<sup>75</sup> agrees with ℵ 9 times, with B 33 times, with TR 29 times,  
 P<sup>45,66,75</sup> agree with ℵ 4 times, with B 18 times, with TR 20 times,  
 P<sup>45,66</sup> agree with ℵ 7 times, with B 3 times, with TR 8 times,  
 P<sup>45,75</sup> agree with ℵ 1 time, with B 2 times, with TR 2 times,  
 P<sup>66,75</sup> agree with ℵ 0 times, with B 8 times, with TR 5 times.<sup>60</sup>

As for the eight other places,

P<sup>45</sup> agrees with ℵ 2 times, with B 1 time, with TR 1 time,  
 P<sup>66</sup> agrees with ℵ 2 times, with B 3 times, with TR 5 times,  
 P<sup>75</sup> agrees with ℵ 2 times, with B 3 times, with TR 4 times.<sup>61</sup>

(Each of the three papyri has other readings as well.)

Is the summary assignment of P<sup>66</sup> and P<sup>75</sup> to the "Alexandrian text-type" altogether reasonable?

G.D. Fee goes to considerable lengths to interpret the evidence in such a way as to support his conclusion that "P<sup>66</sup> is basically a member of the Neutral tradition,"<sup>62</sup> but the evidence itself as he records

<sup>56</sup> *Ibid.*, p. 397.

<sup>57</sup> *Ibid.*, pp. 394-96.

<sup>58</sup> *Ibid.*

<sup>59</sup> Cf. Metzger, *A Textual Commentary on the Greek New Testament* (London: United Bible Societies, 1971), p. xviii.

<sup>60</sup> Klijn, pp. 45-48.

<sup>61</sup> *Ibid.* I have used Klijn's study with reference to the existence of texttypes, but his material also furnishes evidence for the antiquity of the "Byzantine" text. Summing up the evidence for the 51 instances Klijn discusses, P<sup>45</sup> agrees with Aleph 21 times, with B 25 times, with TR 33 times, P<sup>66</sup> agrees with Aleph 16 times, with B 32 times, with TR 38 times, P<sup>75</sup> agrees with Aleph 11 times, with B 36 times, with TR 33 times; or to put it another way, all three papyri agree with Aleph 4 times, with B 18 times, with TR 20 times, any two of them agree with Aleph 8 times, with B 13 times, with TR 15 times, just one of them agrees with Aleph 36 times, with B 62 times, with TR 69 times, for a total of 48 times, 93 times, 104 times.

In other words, in the area covered by Klijn's study the TR has more early attestation than B and twice as much as Aleph—evidently the TR reflects an earlier text than either B or Aleph!

it, for John 1-14, is as follows: P<sup>66</sup> agrees with the TR 315 times out of 663 (47.5%), with P<sup>75</sup> 280 out of 547 (51.2%), with B 334 out of 663 (50.4%), with ⋈ 295 out of 662 (44.6%), with A 245 out of 537 (45.6%), with C 150 out of 309 (48.5%), with D 235 out of 604 (38.9%), with W 298 out of 662 (45.0%).<sup>63</sup>

Does this evidence really suggest "two clear textual streams"?

In these third-century manuscripts, whose evidence takes us back into the mid-second century at least, we find no pristine purity, no unsullied ancestors of Vaticanus, but marred and fallen representatives of the original text. Features of all the main texts isolated by Hort or von Soden are here found—very differently 'mingled' in P<sup>66</sup> and P<sup>45</sup>.<sup>64</sup>

## The classifying of MSS

A serious part of the problem is the manner in which MSS have been assigned to one "text-type" or another. For example, the editors of P<sup>1</sup> (Oxyrh. 2), Grenfell and Hunt, stated that "the papyrus clearly belongs to the same class as the Sinaitic and Vatican codices, and has no Western or Syrian proclivities." The papyrus contains only Matt.1:1-9a,12b-20 (not all of it legible) but C.H. Turner declared that it agrees closely with the text of B and "may be fairly held to carry back the whole B text of the Gospels into the third century."<sup>65</sup> To this day P<sup>1</sup> is assigned to the "Alexandrian text-type."<sup>66</sup> It evidently agrees with B seven times, against the TR, but four of those variants have some "Western" support; however it **disagrees** with B ten times, albeit supporting the TR in only two of those.<sup>67</sup> Is it really reasonable to lump P<sup>1</sup> and B together?

For a clear demonstration of the folly of characterizing a manuscript on the basis of just one chapter (or even less!) the reader is referred to the study of P<sup>66</sup> by Fee. He plots the percentage of agreement between P<sup>66</sup> and the T.R., P<sup>75</sup>, B, ⋈, A, C, D, and W respectively, chapter by chapter, throughout the first 14 chapters of John.<sup>68</sup> For each of the documents the graph bounces up and down from chapter to chapter in an erratic fashion. All of them show a range of variation in excess of 30%—e.g. Codex B goes from 71.1% agreement with P<sup>66</sup> in chapter 5 to 32.3% agreement in chapter 7.

It has already been noted that B and Aleph disagree well over 3,000 times just in the Gospels. (Their agreements are fewer.)<sup>69</sup> Should they be lumped together? It is not enough to notice only the shared peculiarities between two MSS; the extent of disagreement is equally germane to any effort at classification.<sup>70</sup>

Rather than lining up in "clear streams" or "text-types" (as objectively defined entities) the earliest manuscripts are dotted helter-skelter over a wide spectrum of variation. Although varying degrees of affinity exist between and among them, they should be treated as individuals in the practice of textual criticism. Until such time as the relationships among the later manuscripts are empirically plotted, they also should be treated as individuals. To dump them into a "Byzantine" basket is untenable.

<sup>62</sup>G.D. Fee, *Papyrus Bodmer II (P<sup>66</sup>): Its Textual Relationships and Scribal Characteristics* (Salt Lake City: University of Utah Press, 1968), p. 56.

<sup>63</sup>*Ibid.*, p. 14.

<sup>64</sup>J.N. Birdsall, *The Bodmer Papyrus of the Gospel of John* (London, 1960), p. 17.

<sup>65</sup>C.H. Turner, "Historical Introduction to the Textual Criticism of the New Testament," *Journal of Theological Studies*, Jan. 1910, p. 185.

<sup>66</sup>Metzger, *The Text*, p. 247; Epp, "Interlude," p. 397.

<sup>67</sup>Hoskier, *Codex B*, p. xi.

<sup>68</sup>Fee, *Bodmer II*, pp. 12-13.

<sup>69</sup>A hurried count using Nestle's (24th) critical apparatus (I assume that any agreement of ⋈ and B will infallibly be recorded) shows them agreeing 3,007 times, where there is variation. Of these roughly 1,100 are against the "Byzantine" text, with or without other attestation, while the rest are against a small minority of MSS (several hundreds being singular readings of Codex D, one of the papyri, etc.). It appears that B and Aleph do not meet Colwell's requirement of 70 percent agreement in order to be classified in the same text-type.

<sup>70</sup>This is one of the central features in the method proposed by Colwell and E.W. Tune in "The Quantitative Relationships between MS Text-Types," *Biblical and Patristic Studies in Memory of Robert Pierce Casey*, eds. J.N. Birdsall and R.W. Thomson (Frieberg: Herder, 1963).

Since genealogy as not been (and cannot be?) applied to the MSS, the witnesses must be counted, after all—including many of the later minuscules, which evidently had independent lines of transmission (cf. quotes 47 and 49). It will immediately be protested that "witnesses are to be weighed, not counted." Because of the importance of this question I will discuss it in some detail, in its turn.<sup>71</sup> But first, we must continue our evaluation of the W-H theory and for that purpose I will still speak of "text-types" in Hort's terms.

## Conflation

Hort's whole case against the *Textus Receptus*, under this heading was based upon just *eight* examples, taken from two Gospels (Mark and Luke). To characterize a whole text for the whole New Testament on the basis of eight examples is foolish. Colwell states the problem well.

No text or document is homogeneous enough to justify judgment on the basis of *part* of its readings for the rest of its readings. This was Hort's Achilles' heel. He is saying here that since these eight conflate readings occur in the Syrian text that text as a whole is a mixed text; if a manuscript or text lacks these readings, it is in its other readings a witness to a text antecedent to mixture. . . .

Westcott and Hort state this fallacy very clearly in their argument for the importance of the evidence of a document as over against readings:

"Where then one of the documents is found habitually to contain these morally certain or at least strongly preferred readings, and the other habitually to contain their rejected rivals, we can have no doubt, first, that the text of the first has been transmitted in comparative purity, and that the text of the second has suffered comparatively large corruption; and, next, that the *superiority of the first must be as great in the variations in which Internal Evidence of Readings has furnished no decisive criterion as in those which have enabled us to form a comparative appreciation of the two texts.*" [Emphasis his.]

This would be true if we knew that there was no mixture involved and that manuscripts and texts were rigorously homogeneous. Everything we have learned since Hort confirms the opposite position.<sup>72</sup>

It has been generally supposed and stated that there are many other examples. Thus Harrison says, "Another objection was the paucity of examples of conflation. Hort cited only eight, but he could have given others."<sup>73</sup> Kenyon and Lake made the same claim,<sup>74</sup> but where are the "other" examples?

<sup>71</sup>See the section with that heading in Chapter 6.

<sup>72</sup>Colwell, "Genealogical Method," p. 118. In spite of this demonstrably correct statement by Colwell, Bart Ehrman, in his M.Div. thesis at Princeton, 1981, virtually repeats Hort's words:

. . . two points must constantly be kept in mind. First, if a reading were proved to be a conflation, then the documents containing it—to a greater or lesser extent—would preserve a text that is mixed (by definition). This is true, that is to say, if only one proved instance of conflation should be found in these documents. And since most mixing would have resulted in non-conflated readings, i.e. in the arbitrary or intentional choice by a transcriber of one manuscript's reading over another's, then the solitary proven case of mixture would indicate that more numerous instances exist which cannot be so readily demonstrated. Second, the textual character of groups of documents can be fairly assessed by ascertaining the degree to which they contain conflations. If, for example, there are two groups of documents that never contain conflated readings, and one that sometimes does, then clearly the latter group must represent a mixed text. Whether the other groups do or not is indeterminable by this criterion. But the point is that even isolated instances of mixture do show that a text is mixed, and hence both late and secondary in its witness to the true text. Hort's contention was that the Syrian text, and the Syrian alone, contained conflations. Whether it contained eight or eight hundred would be immaterial on this score. The simple presence of conflations of any number prove the text to be mixed ("New Testament Textual Criticism: Quest for Methodology," pp. 55-56).

It has been demonstrated repeatedly that the textual quality of a MS may change significantly from chapter to chapter, let alone from book to book. A proved conflation does indeed convict its MS of mixture at that point, but only at that point. Ehrman's statement about "eight or eight hundred" is simply stupid. Even the eight examples that Hort adduced have all been challenged, and by scholars with differing presuppositions.

<sup>73</sup>Harrison, p. 73.

<sup>74</sup>Kenyon, *Handbook*, p. 302; Lake, p. 68. Ehrman states that "it is significant that other examples can be found with little difficulty. Hort provided four examples of conflation from Mark and four from Luke; the following examples complement his list, four being from Matthew and four from John" (*Ibid.*, p. 56). He gives examples from Matt. 10:3, 22:13, 27:23, 27:41 and John 5:37, 9:25, 10:31,

Why does not Harrison, or Kenyon, or Lake produce them? Because there are very few that have the required phenomena. Kenyon does refer in passing to *An Atlas of Textual Criticism* by E. A. Hutton (London: Cambridge University Press, 1911) which he says contains added examples of conflation.

Upon inspection, the central feature of the 125-page work proves to be a purportedly complete list of triple variant readings in the New Testament where the "Alexandrian," "Western," and "Byzantine" texts are pitted against each other. Hutton adduces 821 instances exhibiting the required phenomena. Out of all that, a few cases of possible "Syrian conflation," aside from Hort's eight, may be culled—such as in Matt. 27:41, John 18:40, Acts 20:28 or Rom. 6:12. Fifty years ago a Hortian might have insisted that John 10:31 also has a "Syrian conflation," but now that P<sup>66</sup> moves the "Syrian" reading back to 200 A.D. a different interpretation is demanded.

Hutton's list may well be open to considerable question, but if we may take it at face value for the moment it appears that the ratio of "Alexandrian-Western-Byzantine" triple variants to possible "Syrian conflations" is about 100:1. In other words, for every instance where the "Syrian" text is possibly built on the "Neutral" and "Western" texts there are a hundred where it is *not*.

That raises another problem. If the "Syrian" text is eclectic, where did it get the material that is its private property? As Burgon observed at the time: "It is impossible to 'conflate' in places where B,  $\aleph$  and their associates furnish no materials for the supposed conflation. Bricks cannot be made without clay. The materials actually existing are those of the Traditional Text itself."<sup>75</sup>

But there is another consideration which is fatal to Hort's purpose. He claimed that inversions do not exist; but they do. He himself cited one of each kind; D conflates in John 5:37 and B conflates in Col. 1:12 and 2 Thess. 3:4.<sup>76</sup> Further, there are a number of other conflations, not only on the part of D, B, and Aleph, but also the "Western" and "Alexandrian" text-types. Please see Appendix D for examples and evidence. Marcion (2nd century) conflates the "Byzantine" and "Neutral-Western" readings in 1 Corinthians 14:19!

Bodmer II shows some "Syrian" readings to be *anterior* to corresponding "Neutral" readings around 200 A.D.

17:23. All these may be found in Appendix D except for John 9:25, because the "Western" reading has no Greek attestation and is therefore not valid for the present purpose. Ehrman misstates the evidence for John 5:37, giving a false impression. In Appendix D I speak to all these examples, plus all of Hort's eight.

<sup>75</sup>Burgon, *The Traditional Text*, p. 229.

<sup>76</sup>Westcott and Hort, p. 94 and pp. 240-41. (Since Hort regarded D and B as adequate to represent the "Western" and "Neutral" texts elsewhere, he should not object here.) But Ehrman favors us with the following:

What is most noteworthy is that the significance of such 'inversions' is rarely explained by advocates of the Majority text. Pickering, for instance, is content to list the inverted conflations, apparently assuming that this alone negates Hort's contention. But there are two considerations that obviate any appeal to these inversions for the purpose of critiquing Hort's basic position on the late and secondary nature of the Syrian text. In the first place, most of the instances that have been granted as genuine inversions occur in isolated members of a text-type, but not throughout the larger grouping itself. [He had finished his thesis before he saw my Appendix D, which was not in the first edition.] In other words, the three cases of conflation in Codex B do not indicate that the Alexandrian text-type is mixed, but only that B is. And the fact that B was transcribed in the 4th century would suggest that in some cases it might be expected to contain evidence of mixture from prior texts. [An interesting admission.] This can hardly vitiate Hort's proof, since he himself acknowledged the presence of conflations in both D and B, in the latter case, especially in the Pauline epistles.[]

Secondly, by adducing this kind of argument against Westcott and Hort, the advocates of the Majority text have placed themselves on the horns of a dilemma. On the one hand, if they choose to deny the validity of Hort's assertion—that a text containing conflations is secondary, and that the more conflations it contains the less it is a trustworthy witness to the original text—then an appeal to inverted conflations is no argument at all. If conflations do not show that a text is secondary, then why point to them? In such a case, contrary examples would only show Hort's error in assuming that Syrian texts alone contain conflations, but would indicate absolutely nothing about the character of the respective text-types. Thus, clearly, the argument is viable only if Hort's premise is accepted.

But, on the other hand, by accepting this premise, the advocates of the Majority text are faced with a serious problem. If the Alexandrian and Western text-types contain conflations, then all three texts are late and secondary (*Ibid.*, pp. 60-61).

Either Ehrman misses the point or he is being duplicitous. Of course we advocates of the Majority text recognize that a conflation is a secondary reading, of necessity. If all three text-types contain conflations, "then all three texts are late and secondary". Just so! And that invalidates Hort's use of "conflation" to disqualify the "Syrian" text. Since the "Alexandrian" and "Western" texts both contain evident conflations, they are both secondary. If Hort had only admitted that at the outset, a great deal of needless debate would have been spared. However, I have yet to see any putative "Byzantine" conflation that impresses me as really being one—Appendix D gives numerous examples with 2nd or 3rd century attestation; if any is a conflation it is an early one. (Of course, a genuine conflation is by definition secondary even if created in A.D. 100!)

The Bodmer John (P<sup>66</sup>) is also a witness to the early existence of many of the readings found in the Alpha text-type (Hort's "Syrian"). Strangely enough to our previous ideas, the contemporary corrections in that papyrus frequently change an Alpha-type reading to a Beta-type reading (Hort's "Neutral"). This indicates that at this early period readings of both kinds were known, and the Beta-type were supplanting the Alpha-type—at least as far as this witness is concerned.<sup>77</sup>

Hoskier, after his thorough (450 pages) study of Codex B, offered this verdict: "the maligned Textus Receptus served in large measure as the base which B tampered with and changed."<sup>78</sup>

It is clear that Hort's characterization of the "Syrian" text as eclectic and secondary, as posterior to and building upon the "Western" and "Neutral" texts, does not square with the evidence. But while we are on the subject, what of Hort's eight examples; do they lend themselves to his interpretation? We must ask whether they really qualify as possible confections and then consider the reverse explanation, namely that the shorter forms are independent simplifications of the original long form.

Burgon examined the eight at length and observed that most of them simply do not exhibit the required phenomena.<sup>79</sup> The reader may see for himself by consulting any reasonably complete *apparatus criticus* (all are included in Appendix D). Whatever explanation may be given of the origin of the "Byzantine" readings in Mark 8:25, Luke 11:54, and Luke 12:18, they are not "confections" of the "Neutral" and "Western" readings. The same thing may be said, though not so emphatically, about Mark 6:33 and Luke 9:10.

In almost every case the witnesses within the "Neutral" and "Western" camps are divided among themselves, so that a somewhat arbitrary choice has to be made in order to give *the* "Neutral" or "Western" reading. Hort approached his discussion of the eight examples of conflation he adduced "premising that we do not attempt to notice every petty variant in the passages cited, for fear of confusing the substantial evidence."<sup>80</sup>

But in a question of this sort the confusion must be accounted for. If the "Neutral" witnesses disagree among themselves, what credence can we give to the "Neutral" testimony as a whole?

Given an instance, such as Luke 24:53, where the required phenomena for a conflation are present, it must be demonstrated that the two shorter readings did not arise through independent omissions of different parts of the longer reading before it can be asserted that conflation took place. Apart from such demonstration it is not fair to assume a conflation and then build a theory upon it. Hort's total demonstration relative to Luke 24:53 is, "This simple instance needs no explanation."<sup>81</sup>

Burgon (who personally collated D) observed that in the last chapter of Luke the Received Text has 837 words—of these D omits 121, or one word in seven.<sup>82</sup> To someone using Nestle's Text (24th) D omits 66 out of 782, or one in twelve (Nestle has omitted thirty-eight words from the Greek text of Luke 24 on the sole Greek authority of D, and another five on D and  $\aleph$  alone).

In the face of such an inveterate propensity for omission, it is not unreasonable to suspect that in verse 53 D has omitted "and blessing" from the original "praising and blessing" rather than that the reading of all but six of the extant Greek MSS is a conflation. Furthermore, the reading of D may easily have arisen from the "Byzantine" by homoioteleuton (*OYNTEC--OYNTEC*). Kilpatrick is among the most recent of a number of scholars who have argued that at least some of Hort's "Syrian confections" are the original reading.<sup>83</sup>

<sup>77</sup>Colwell, "The Origin of Texttypes," pp. 130-31.

<sup>78</sup>Hoskier, *Codex B*, I, 465.

<sup>79</sup>Burgon, *The Revision Revised*, pp. 257-65.

<sup>80</sup>Westcott and Hort, p. 95.

<sup>81</sup>*Ibid.*, p. 104.

<sup>82</sup>Burgon, *The Revision Revised*, p. 264.

<sup>83</sup>G.D. Kilpatrick, "The Greek New Testament Text of Today and the *Textus Receptus*," *The New Testament in Historical and Contemporary Perspective*, H. Anderson and W. Barclay, eds. (Oxford: Basil Blackwell, 1965), pp. 190-92. Cf. Bousset, *TU*, vol. 11 (1894), pp. 97-101, who agreed with Hort on only one of the eight.

K. Lake spoke of the problem of deciding which interpretation to take.

The keystone of their [W-H] theory is in the passages where we get this triple variation, and the point of the argument lies in the assumption that the longer reading is made by uniting the two shorter ones—not the two shorter by different dealings with the longer. This point can be tested only by an appeal to Patristic evidence and general probability.

The latter argument is precarious because subjective, so that the ultimate and decisive criterion is Patristic evidence.<sup>84</sup>

It appears, according to Lake, that patristic evidence is to decide the issue. But neither Lake nor anyone else has produced any Patristic citations of these passages in the first three centuries. The few citations available after that time all support the Byzantine readings.<sup>85</sup>

Actually, the whole matter of "conflation" is a pseudo-issue, a tempest in a teapot. There simply are not enough putative examples to support generalizations. Such evidence as there is, however, is certainly not unfavorable to the "Syrian" text. As Zuntz says, the idea that the late text was derived from the two earlier "recensions" combined is erroneous.<sup>86</sup>

## "Syrian" Readings Before Chrysostom

Hort's statements concerning the nature of the ante-Nicene patristic testimony are still widely believed. Thus, Chrysostom is widely affirmed to have used the "Byzantine" text.<sup>87</sup> But, Lake has stated:

Writers on the text of the New Testament usually copy from one another the statement that Chrysostom used the Byzantine, or Antiochian, text. But directly any investigation is made it appears evident, even from the printed text of his works, that there are many important variations in the text he quotes, which was evidently not identical with that found in the MSS of the Byzantine text.<sup>88</sup>

Metzger calls attention to the work of Geerlings and New.

It has often been stated by textual scholars that Chrysostom was one of the first Fathers to use the Antiochian text. This opinion was examined by Jacob Geerlings and Silva New in a study based on evidence which, in default of a critical edition; was taken from Migne's edition of Chrysostom's *opera*. Their conclusions are that "Chrysostom's text of Mark is not that of any group of manuscripts so far discovered and classified. . . . His text of Mark, or rather the text which can faintly be perceived through his quotations, is a 'mixed text' combining some of the elements of each of the types which had flourished before the end of the fourth century."<sup>89</sup>

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<sup>84</sup>Lake, p. 68.

<sup>85</sup>Victor of Antioch for Mark 8:26, 9:38 and 9:49; Basil for Mark 9:38 and Luke 12:18; Cyril of Alexandria for Luke 12:18; Augustine for Mark 9:38.

<sup>86</sup>Zuntz, *The Text*, p. 12. Sturz (pp. 70-76) has a chapter entitled, "Byzantine-Western Alignments Go Back Into The Second Century Independently And Originate In The East—Not In The West." He makes heavy use of Zuntz' work and concludes that . . . his findings deal a devastating blow to WH's basic theory of the history of the text, i.e. they destroy the supposed partial dependence of the K-text on Western sources.

If this dependence in K-Western alignments must be reversed as Zuntz demonstrates, then one half of the support for Hort's basic theory of conflation collapses immediately! But, not only does the WH theory fail at this point, it is changed into the opposite! This is more than the "general consensus of scholarship" can concede. It is an intolerable thought and too revolutionary to acknowledge that the Antiochian text may have been the source rather than the recipient of the common material in such Byzantine-Western alignments (p. 76).

I have not knowingly misrepresented Zuntz, or Colwell, Metzger, Aland, etc., in quoting from their works. I take it that Colwell does reject Hort's notion of genealogy, that Aland does reject Hort's notion of recensions, that Zuntz does reject Hort's notion of "Syrian" conflation, and so on. However, I do not mean to imply and it should not be assumed that any of these scholars would entirely agree with my statement of the situation at any point, and they certainly do not agree (so far as I know) with my total position.

<sup>87</sup>Westcott and Hort, p. 91.

<sup>88</sup>Lake, p. 53.

<sup>89</sup>Metzger, *Chapters*, p. 21.

They say further: "No known manuscript of Mark has the text found in Chrysostom's homilies, or anything approaching it. And probably no text which existed in the fourth century came much nearer to it."<sup>90</sup> They did a collation of Chrysostom's text and observe concerning it: "The number of variants from the Textus Receptus is not appreciably smaller than the number of variants from Westcott and Hort's text. This proves that it is no more a typical representative of the late text (von Soden's **K**) than it is of the Neutral text."<sup>91</sup>

What about Origen; does he really represent the "Neutral" text?

It is impossible to reproduce or restore the text of Origen. Origen had no settled text. A reference to the innumerable places where he is upon *both* sides of the question, as set forth in detail herein, will show this clearly. Add the places where he is in direct opposition to  $\aleph$  and B, and we must reconsider the whole position.<sup>92</sup>

Zuntz agrees.

The insuperable difficulties opposing the establishment of 'the' New Testament text of Origen and Eusebius are well known to all who have attempted it. . . . Leaving aside the common difficulties imposed by the uncertainties of the transmission, the incompleteness of the material, and the frequent freedom of quotation, there is the incontestable fact that these two Fathers are frequently at variance; that each of them quotes the same passage differently in different writings; and that sometimes they do so even within the compass of one and the same work. . . . Wherever one and the same passage is extant in more than one quotation by Origen or Eusebius, variation between them is the rule rather than the exception.<sup>93</sup>

Metzger affirms: "Origen knows of the existence of variant readings which represent each of the main families of manuscripts that modern scholars have isolated."<sup>94</sup> (That includes the "Byzantine.") Edward Miller, in his exhaustive study of the Fathers, found that Origen sided with the Traditional Text 460 times while siding with the "Neologian" text 491 times.<sup>95</sup> (The "Neologian"<sup>96</sup> text, as Miller used the term, includes both "Neutral" and "Western" readings; while "Traditional Text" is his term for Hort's "Syrian" text.) How then could Hort say of Origen, "On the other hand his quotations to the best of our belief exhibit no clear and tangible traces of the Syrian text"?<sup>97</sup>

What about Irenaeus; does he really represent the "Western" text? Miller found that Irenaeus sided with the Traditional Text 63 times and with the "Neologian" text 41 times.<sup>98</sup> He said further:

Hilary of Poitiers is far from being against the Traditional Text, as has been frequently said: though in his commentaries he did not use so Traditional a text as in his *De Trinitate* and his other works. The texts of Hippolytus, Methodius, Irenaeus, and even of Justin, are not of that exclusively Western character which Dr. Hort ascribes to them. Traditional readings occur almost equally with others in Justin's works, and predominate in the works of the other three.<sup>99</sup>

Hoskier adds a word concerning Hippolytus.

<sup>90</sup>J. Geerlings and S. New, "Chrysostom's Text of The Gospel of Mark," *Harvard Theological Review*, XXIV (1931), 135.

<sup>91</sup>*Ibid.*, p. 141.

<sup>92</sup>Hoskier, *Codex B*, I, ii-iii.

<sup>93</sup>Zuntz, *The Text*, p. 152.

<sup>94</sup>Metzger, "Explicit References in the Works of Origen to Variant Readings in N.T. MSS.," *Biblical and Patristic Studies in Memory of Robert Pierce Casey*, ed. J.N. Birdsall and R.W. Thomson (New York: Herder, 1963), p. 94.

<sup>95</sup>Burton, *The Traditional Text*, pp. 100, 121.

<sup>96</sup>To be precise, the Greek text used by the English Revisers in 1881 is meant here, or rather those places where it differs from the TR.

<sup>97</sup>Westcott and Hort, p. 114.

<sup>98</sup>Burton, *The Traditional Text*, p. 99.

<sup>99</sup>*Ibid.*, p. 117.



Let us take another most interesting witness, viz. *Hippolytus*, who, like *Lucifer*, frequently quotes at such length from both Old and New Testaments that it is absolutely beyond question that he was **copying** from his exemplar of the Scriptures.

Hippolytus cites 1 Thess. iv.13-17, 2 Thess. ii.1-12, in full. In the face of these quotations it is seen how loosely Turner argues when he says "Hort was the last and perhaps the ablest of a long line of editors of the Greek Testament, commencing in the eighteenth century, who very tentatively at first, but quite ruthlessly in the end, *threw over the LATER in favor of the EARLIER Greek MSS*, and that issue will never have to be tried again."

But permit me to ask what Mr. Turner means by this lighthearted sentence. What does he mean by earlier and later Manuscripts? He cannot mean that Hippolytus' manuscript was later than that of B? Yet, allow me to state that in these long passages, comprising twelve consecutive verses from one epistle and four from the other, Hippolytus' early third-century MS is found generally on the side of what Turner would call the "later" MSS.<sup>100</sup>

According to Miller's study, the advantage of the Traditional Text over the "Neologian" before Origen was actually 2:1, setting aside Justin Martyr, Heracleon, Clement of Alexandria and Tertullian. If these four are included, the advantage of the Traditional Text drops to 1.33:1 since the confusion which is most obvious in Origen is already observable in these men. From Origen to Macarius Magnus the advantage of the Traditional Text drops to 1.24:1 while from Macarius to 400 A.D. it is back up to 2:1.<sup>101</sup> Please note that the Traditional Text was always ahead, even in the worst of times.

### Miller vs. Kenyon

Because of the importance of Miller's study, already cited, I will now consider it more in detail along with Kenyon's answer. Miller saw clearly the crucial nature of Hort's proposition.

It is evident that the turning point of the controversy between ourselves and the Neologian school must lie in the centuries before St. Chrysostom. If, as Dr. Hort maintains, the Traditional Text not only gained supremacy at that era but did not exist in the early ages, then our contention is vain. . . . On the other hand if it is proved to reach back in unbroken line to the time of the Evangelists, or to a period as near to them as surviving testimony can prove, then Dr. Hort's theory of a 'Syrian' text formed by recension or otherwise just as evidently falls to the ground.<sup>102</sup>

Miller, posthumous editor to Burgon, probed the question of ante-Nicene testimony exhaustively, making full use of Burgon's massive index of patristic citations (86,489 of them) from the New Testament. He deserves to be heard, in detail.

As to the alleged absence of readings of the Traditional Text from the writings of the Ante-Nicene Fathers, Dr. Hort draws largely upon his imagination and his wishes. The persecution of Diocletian is here also the parent of much want of information. But is there really such a dearth of these readings in the works of the Early Fathers as is supposed?<sup>103</sup>

I made a toilsome examination for myself of the quotations occurring in the writings of the Fathers before St. Chrysostom, or as I defined them in order to draw a self-acting line, of those who died before 400 A.D., with the result that the Traditional Text is found to stand in the general proportion of 3:2 [this is 60%, precisely as Peter Johnston verified—see footnote 101]

<sup>100</sup>Hoskier, *Codex B*, I, 426-27.

<sup>101</sup>Burgon, *The Traditional Text*, pp. 99-101. Fee calls my use of Miller's figures "absurd" and rejects them in sweeping terms ("A Critique," pp. 419 and 422). However, Peter J. Johnston (personal communication) gives the following report on an independent check of early Fathers, using critical editions. Checking six from the 3rd century (Irenaeus, Clement Alex., Tertullian, Hippolytus, Origen, Cyprian), five from the 4th century (Aphraates, Ephraem Arm., Ephraem Syr., Gregory Naz., Gregory Nys.) and seven from the 5th century (Chrysostom, Pelagius, Niceta, Theodore Mop., Augustine, Cyril Alex., Faustus), in the Gospels, he found them siding with the Majority Text "approximately 60%" of the time, where there is variation. This is very close to the results stated by Miller!

<sup>102</sup>Burgon, *The Causes of the Corruption*, pp. 2-3.

<sup>103</sup>E. Miller, *A Guide to the Textual Criticism of the New Testament* (London: George Bell and Sons, 1886), p. 53.

against other variations, and in a much higher proportion upon thirty test passages. Afterwards, not being satisfied with resting the basis of my argument upon one scrutiny, I went again through the writings of the seventy-six Fathers concerned (with limitations explained in this book), besides others who yielded no evidence, and I found that although several more instances were consequently entered in my notebook, the general results remained the same. I do not flatter myself that even now I have recorded all the instances that could be adduced:—any one who is really acquainted with this work will know that such a feat is absolutely impossible, because such perfection cannot be obtained except after many repeated efforts. But I claim, not only that my attempts have been honest and fair even to self-abnegation, but that the general results which are much more than is required by my argument, as is explained in the body of this work, abundantly establish the antiquity of the Traditional Text, by proving the superior acceptance of it during the period at stake to that of any other.<sup>104</sup>

Kenyon acknowledged Miller's work and stated the results correctly.

Here is a plain issue. If it can be shown that the readings which Hort calls "Syrian" existed before the end of the fourth century, the keystone would be knocked out of the fabric of his theory; and since he produced no statistics in proof of his assertion [!], his opponents were perfectly at liberty to challenge it. It must be admitted that Mr. Miller did not shirk the test. A considerable part of his work as editor of Dean Burgon's papers took the form of a classification of patristic quotations, based upon the great indices which the Dean left behind him, according as they testify for or against the Traditional Text of the Gospels.

The results of his examination are stated by him as follows. Taking the Greek and Latin (not the Syriac) Fathers who died before A.D. 400, their quotations are found to support the Traditional Text in 2,630 instances, the "neologian" in 1753. Nor is this majority due solely to the writers who belong to the end of this period. On the contrary, if only the earliest writers be taken, from Clement of Rome to Irenaeus and Hippolytus, the majority in favour of the Traditional Text is proportionately even greater, 151 to 84. Only in the Western and Alexandrian writers do we find approximate equality of votes on either side. Further, if a select list of thirty important passages be taken for detailed examination, the preponderance of early patristic evidence in favour of the Traditional Text is seen to be no less than 530 to 170, a quite overwhelming majority.

Now it is clear that if these figures were trustworthy, there would be an end to Hort's theory, for its premises would be shown to be thoroughly unsound.<sup>105</sup>

Before proceeding to Kenyon's rebuttal it will be well to pause and review the implications of this exchange. Hort, and the many like Kenyon who have repeated his words after him, have asserted that not a single "strictly Byzantine" reading is to be found in the extant works of any Church Father who dates before Chrysostom (d. 407). To disprove Hort's assertion, it is only necessary to find *some* "strictly Byzantine" readings before the specified time, since the question immediately in focus is the existence of the "Byzantine" readings, not necessarily their dominance. Miller affirms that the Byzantine text not only is to be found in the writings of the early Fathers, but that in fact it *predominates*.

As far as the Fathers who died before 400 A.D. are concerned, the question may now be put and answered. Do they witness to the Traditional Text as existing from the first, or do they not? The results of the evidence, both as regards the quantity and the quality of the testimony, enable us to reply, not only that the Traditional Text was in existence, but that it was

<sup>104</sup>Burgon, *The Traditional Text*, pp. ix-x. Miller's experiment pitted the Received Text against the Greek text pieced together by the body of revisers who produced the English Revised Version of 1881, which Miller aptly styles the "Neologian." He used Scrivener's *Cambridge Greek Testament* of 1887 which gives the precise Greek text represented by the E.R.V. but prints in black type the places that differ from the Received Text. Miller limited the investigation to the Gospels. He said that he discarded doubtful quotations and mere matters of spelling, that in doubtful cases he decided against the *Textus Receptus*, and that in the final tabulation he omitted many smaller instances favorable to the *Textus Receptus* (*Ibid.*, pp. 94-122).

<sup>105</sup>Kenyon, *Handbook*, pp. 321-22. Both Hort and Kenyon clearly stated that no strictly "Syrian" readings existed before the end of the 4th century. It is encouraging to see that both Carson (p. 111) and Fee ("A Critique," p. 416) have retreated to the weaker statement that it is all such readings together or the whole "text-type" that had no early existence.

predominant, during the period under review. Let any one who disputes this conclusion make out for the Western Text, or the Alexandrian, or for the Text of B and S, a case from the evidence of the Fathers which can equal or surpass that which has been now placed before the reader.<sup>106</sup>

### **No one has ever taken up Miller's challenge.**

As quoted above, Kenyon recognized that if Miller's figures are right then Hort's theory is at an end. But Kenyon continued:

An examination of them however, shows that they cannot be accepted as representing in any way the true state of the case. In the first place, it is fairly certain that critical editions of the several Fathers, if such existed, would show that in many cases the quotations have been assimilated in later MSS to the Traditional Text, whereas in the earlier they agree rather with the "Neutral" or "Western" witnesses. For this defect, however, Mr. Miller cannot be held responsible. The critical editions of the Greek and Latin Fathers, now in course of production by the Academies of Berlin and Vienna, had covered very little of the ground at the time when his materials were compiled, and meanwhile he might legitimately use the materials accessible to him; and the errors arising from this source would hardly affect the general result to any very serious extent.<sup>107</sup>

After raising the "quibble" about critical editions he admitted that "the errors arising from this source would hardly affect the general result." However, Kenyon's suggestion that "in many cases the quotations have been assimilated in later MSS to the Traditional Text" gives the essence of a contention widely used today to parry the thrusts of the mounting evidence in favor of an early "Byzantine" text. To this we must presently return.

Kenyon proceeded:

The real fallacy in his statistics is different, and is revealed in the detailed examination of the thirty select passages. From these it is clear that he wholly misunderstood Hort's contention. The thirty "traditional" readings, which he shows to be so overwhelmingly vindicated by the Fathers, are not what Hort would call pure "Syrian" readings at all. In nearly every case they have Western or Neutral attestation in addition to that of the later authorities.<sup>108</sup>

He then referred briefly to specific instances in Matt. 17:21, Matt. 18:11, Matt. 19:16, Matt. 23:38, Mark 16:9-20, Luke 24:40, and John 21:25 and continued:

In short, Mr. Miller evidently reckoned on his side every reading which occurs in the Traditional Text, regardless of whether, on Hort's principles, they are old readings which kept their place in the Syrian revision, or secondary readings which were then introduced for the first time. According to Hort, the Traditional Text is the result of a revision in which old elements were incorporated; and Mr. Miller merely points to some of these old elements, and argues therefrom that the whole is old. It is clear that by such argumentation Hort's theory is untouched.<sup>109</sup>

It is hard to believe that Kenyon was precisely fair here. He had obviously read Miller's work with care. Why did he not say anything about "to repentance" in Matt. 9:13 and Mark 2:17,<sup>110</sup> or "vinegar" in

<sup>106</sup>Burgon, *The Traditional Text*, p. 116.

<sup>107</sup>Kenyon, *Handbook*, pp. 322-23.

<sup>108</sup>*Ibid.*, p. 323.

<sup>109</sup>*Ibid.*

<sup>110</sup>Supported by Barnabas (5), Justin M. (*Apol.* i.15), Irenaeus (III. v. 2), Origen (Comment. in Joh. xxviii. 16), Eusebius (Comment. in Ps. cxlvi), Hilary (Comment. in Matt. *ad loc.*), Basil (*De Poenitent.* 3; Hom. in Ps. xlvi. 1; *Epist. Class.* i. xlv. 6). The evidence cited in footnotes 110-117 was taken from Burgon, *The Traditional Text*.

Among the numerous dubious affirmations with which Fee favors us, none is more startling than his charge that "Burgon's and Miller's data are simply replete with useless supporting evidence" ("A Critique," p. 417). Anyone who studies their works with care (as I have) will come away convinced that they were unusually thorough, careful and scrupulous in their treatment of Patristic evidence. Not so Fee. Of the reading "vinegar" in Matt. 27:34 he says, "I took the trouble to check over three-quarters of Burgon's seventeen supporting

Matt. 27:34,<sup>111</sup> or "from the door" in Matt, 28:2,<sup>112</sup> or "the prophets" in Mark 1:2,<sup>113</sup> or "good will" in Luke 2:14,<sup>114</sup> or the Lord's prayer for His murderers in Luke 23:34,<sup>115</sup> or "an honeycomb" in Luke 24:42,<sup>116</sup> or "whom" in John 17:24?<sup>117</sup>

These instances are also among "the thirty." They would appear to be "strictly Syrian" readings, if there really is such a thing. Why did Kenyon ignore them? The cases Kenyon cited fell within the scope of Miller's inquiry because they are Traditional readings; whatever other attestation they may also have, and because the English Revisers of 1881 rejected them. (Please note that since Hort *et al.* rejected the non-Byzantine witnesses that agree with the Byzantine text, in those places, they must be viewed as having departed from the "norm" that he chose. If they assimilated to the Byzantine text they may not reasonably be adduced as evidence against that text.) Kenyon asserted that Miller's figures "cannot be accepted as representing in any way the true state of the case," but he has not shown us why. Kenyon said nothing about the alleged "secondary readings" that have early Patristic support.

Miller's figures represent precisely what he claimed that they represent "the true state of the case" is that the Traditional Text ("Byzantine") receives *more support* from the early Church Fathers than does the critical text (essentially W-H) used by the English Revisers. It should be noted that there are doubtless numerous so-called "Western" and "Alexandrian" readings<sup>118</sup> to be found in the early Fathers which are not included in Miller's figures because the Revisers rejected them. If they were all tabulated the "Byzantine" readings would perhaps lose the absolute majority of early patristic attestation but they would still be present and attested, from the very first, and that is the question just now in focus.

## Pure "Syrian" readings

Kenyon's statement contains another problem. He referred to "pure 'Syrian' readings" and in effect denied to the "Syrian" text any reading that chances to have any "Western" or "Alexandrian" attestation (which attestation has been arbitrarily pigeon-holed according to the presuppositions of the theory). But just which are those late or "pure Syrian" elements?

Fathers and **not one of them** [emphasis Fee's] can be shown to be citing Matthew!" (pp. 417-18). Since he affirms that he did check the Fathers himself, the most charitable construction that can be placed on Fee's words is that the check was hasty and careless. (Please turn to footnote 3 in chapter 7 for a refutation of Fee's statement.) With reference to the Patristic evidence for "to repentance" in Matt. 9:13 and Mark 2:17, the concerned reader will be well advised to check the sources for himself.

<sup>111</sup>Supported by Gospel of Peter (5), *Acta Philippi* (26), Barnabas (7), Irenaeus (pp. 526, 681), Tertullian, Celsus, Origen, Eusebius of Emesa, ps-Tatian, Theodore of Heraclea, Ephraem, Athanasius, *Acta Pilati*.

<sup>112</sup>Supported by Gospel of Nicodemus, *Acta Philippi*, Apocryphal Acts of the Apostles, Eusebius (*ad Marinum*, ii. 4), Gregory Nyss. (*De Christ. Resurr.* I. 390, 398), Gospel of Peter.

<sup>113</sup>Supported by Irenaeus (III. xvi. 3), Origen, Porphyry, Eusebius, Titus of Bostra.

<sup>114</sup>Supported by Irenaeus (III. x. 4), Origen (*c. Celsum* i. 60; *Selecta* in Ps. xlv.; Comment. in Matt. xvii.; Comment. in Joh. i. 13), Gregory Thaumaturgus (*De Fid.* Cap. 12), Methodius (*Serm de Simeon. et Anna*), Apostolic Constitutions (vii. 47; viii. 12), *Diatessaron*, Eusebius (*Dem. Ev.* pp. 163, 342), Aphraates (i. 180, 385), Jacob-Nisibis, Titus of Bostra, Cyril of Jerusalem (p. 180), Athanasius, Ephraem (Gr. iii. 434).

<sup>115</sup>Supported by Hegesippus (*Eus. H.E.* ii. 23), Marcion, Justin, Irenaeus (*c. Haer.* III. xviii. 5), Archelaus (xliv), Hippolytus (*c. Noet.* 18), Origen (ii. 188), Apostolic Constitutions (ii. 16; v. 14), Clementine Homilies (*Recogn.* vi. 5; Hom. xi. 20), ps-Tatian (E. C. 275), Eusebius (canon x), Hilary (*De Trin.* 1. 32), *Acta Pilati* (x. 5), Theodore of Heraclea, Athanasius (i. 1120), Titus of Bostra, Ephraem (ii. 321).

<sup>116</sup>Supported by Marcion (*ad loc.*), Justin M. (ii. 240, 762), Clement Alex. (p. 174), Tertullian (i. 455), *Diatessaron*, Athanasius (i. 644), Cyril of Jerusalem (iv. 1108), Gregory Nyss. (i. 624).

<sup>117</sup>Supported by Irenaeus (*c. Haeres.* IV. xiv. 1), Clement Alex. (*Paed.* i. 8), Cyprian (pp. 235, 321), *Diatessaron*, Eusebius (*De Eccles. Theol.* iii. 17--bis; *c. Marcell.* p. 292), Hilary (pp. 1017, 1033), Basil (*Eth.* ii, 297), Caelestinus (*Concilia* iii. 356).

<sup>118</sup>Again we are faced with the question-begging of Hort and many subsequent writers. Irenaeus, for instance, is arbitrarily declared to be a witness to the "Western text-type" and then any reading he has is thereupon declared to be "Western." Even if we granted the existence of such entities as the "Western" and "Alexandrian" text-types (for the sake of the argument), if the requirement were imposed that only those readings which are supported by a **majority** of the witnesses assigned to a text-type may be claimed for that text-type then the number of "Western," "Alexandrian," and "Caesarean" readings would shrink **drastically**. By contrast, the number of "Byzantine" readings would remain about the same.

There is a further detail that, I think, has not received sufficient attention. Miller pitted the Traditional Text against the "Neologian" (W-H) because it represented the Revisers' judgment as to what was the original text. It follows that any "Western" and, especially, "Alexandrian" witnesses that attested something else were rejected, at each point. So presumably any rejected "Alexandrian" witnesses would no longer be "Alexandrian", at that point—or were there several "Alexandrian" text-types? On what basis can those rejected "Alexandrian" witnesses (rejected by Hort and the Revisers) be used to invalidate "Byzantine" readings?

E. F. Hills evidently conducted a search for them. He observes:

The second accusation commonly urged against the Byzantine text is that it contains so many late readings. A text with all these late readings, it is said, must be a late text. But it is remarkable how few actually were the Byzantine readings which Westcott and Hort designated as late. In his *Notes on Select Readings* Hort discussed about 240 instances of variation among the manuscripts of the Gospels, and in only about twenty of these instances was he willing to characterize the Byzantine reading as a late reading. Thus it would seem that even on Hort's own admission only about ten percent of the readings of the Byzantine text are late readings, and since Hort's day the number of these allegedly late Byzantine readings has been gradually dwindling.<sup>119</sup>

(And yet Hort wrote off the whole "Syrian" witness as late.)

It seems clear that the "Byzantine" text cannot win in a court presided over by a judge of Kenyon's bent. Whenever an early witness surfaces it is declared to be "Alexandrian" or "Western" or "Caesarean" and thereupon those "Syrian" readings which it contains cease to be "pure Syrian" and are no longer allowed as evidence. Such a procedure is evidently useful to defenders of Hort's theory, but is it right?

It is commonplace among the many who are determined to despise the "Byzantine" text to dodge the issue, as Kenyon did above. The postulates of Hort's theory are assumed to be true and the evidence is interpreted on the basis of these presuppositions. Apart from the imaginary nature of the "Alexandrian" and "Western" texts, as strictly definable entities, their priority to the "Byzantine" text is the very point to be proved and may not be assumed. Kirsopp Lake's statement is representative. Taking Origen, Irenaeus, and Chrysostom as representatives of the "Neutral," "Western," and "Byzantine" texts respectively, he asserted:

Though Chrysostom and Origen often unite in differing from Irenaeus, and Chrysostom and Irenaeus in differing from Origen, yet Chrysostom does not differ from them both at once. And this is almost demonstrative proof that his text, characteristically representative of the later Fathers, versions and MSS, is an eclectic one.<sup>120</sup>

Even if Lake's description of the phenomena were true (but remember what he himself said about scholars copying from each other, regarding Chrysostom), there is another perfectly adequate interpretation of such phenomena. In Hill's words,

There is surely a much more reasonable way of explaining why each non-Byzantine text (including Papyrus Bodmer II) contains Byzantine readings not found in other non-Byzantine texts. If we regard the Byzantine text as the original text, then it is perfectly natural that each non-Byzantine text should agree with the Byzantine text in places in which the other non-Byzantine texts have departed from it.<sup>121</sup>

Also, given the priority of the "Byzantine" text, the places where all the divergent texts happened to abandon the "Byzantine" at the same time would be few. To arbitrarily assign Fathers and manuscripts and versions to the "Alexandrian" and "Western" families and then to deny to the "Byzantine" text readings which one or more of these arbitrarily assigned witnesses happen also to support seems neither honest nor scholarly.

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<sup>119</sup>E.F. Hills, *The King James Version Defended!* (Des Moines: The Christian Research Press, 1956), p. 73. Carson continues to beg the question (p. 111). If the present trend continues until all "purely Byzantine" readings have early attestation he will not be disturbed since he will continue to arbitrarily declare such readings to be "Western" or "Alexandrian". May I respectfully submit that the generally accepted norms of scholarship do not permit the continued begging of this particular question.

<sup>120</sup>Lake, p. 72. On the contrary: such a situation reflects three independent lines of transmission. If Chrysostom is never alone then his is clearly the best line.

<sup>121</sup>J.W. Burgon, *The Last Twelve Verses of the Gospel According to Saint Mark* (Ann Arbor, Mich.: The Sovereign Grace Book Club, 1959), p. 55. This reprint of Burgon's 1871 work contains an Introduction by E.F. Hills occupying pages 17-72.

## A biased expedient

Before closing this section, it remains to take up the expedient, alluded to earlier, whereby many seek to evade the ante-Nicene patristic evidence for the "Byzantine" text. Vincent Taylor states the expedient as baldly as anyone. "In judging between two alternative readings [of a given Father in a given place] the principle to be adopted is that the one which *diverges* from the later ecclesiastical text (the TR) is more likely to be original."<sup>122</sup>

This expedient is extended even to cases where there is no alternative. The allegation is that copyists altered the Fathers' wording to conform to the "Byzantine," which the copyists regarded as "correct."<sup>123</sup> It is obvious that the effect of such a proceeding is to place the "Byzantine" text at a disadvantage. An investigation based on this principle is "rigged" against the TR.<sup>124</sup>

<sup>122</sup>Taylor, p. 39. Fee continues to vigorously propound this expedient. "My experience is that in every instance a critical edition of the Father moves his New Testament text in some degree **away from** the Byzantine tradition" ("Modern Text Criticism," p. 160). He has recently observed that "all of Burgon's data . . . is suspect because of his use of uncritical editions" ("A Critique," p. 417).

But there is reason to ask whether editors with an anti-Byzantine bias can be trusted to report the evidence in an impartial manner. Certainly a critical edition of Irenaeus prepared by Fee could not be trusted. In discussing the evidence for "in the prophets" versus "in Isaiah the prophet" in Mark 1:2 ("A Critique," pp. 410-11) Fee does not mention Irenaeus under the Majority Text reading, where he belongs, but says "except for one citation in Irenaeus" under the other reading. He then offers the following comment in a footnote: "Since this one citation stands alone in all of the early Greek and Latin evidence, and since Irenaeus himself knows clearly the other text, this 'citation' is especially suspect of later corruption." He goes on to conclude his discussion of this passage by affirming that the longer reading is "the only reading known to every church Father who cites the text." By the end of his discussion Fee has completely suppressed the unwelcome testimony from Irenaeus.

But is the testimony of Irenaeus here really suspect? In *Adv. Haer.* III.10.5 we read: "Mark . . . does thus commence his Gospel narrative: 'The beginning of the Gospel of Jesus Christ, the Son of God, as it is written in the prophets, Behold, . . . [the quotations follow].' Plainly does the commencement of the Gospel quote the words of the holy prophets, and point out Him . . . whom they confessed as God and Lord." Note that Irenaeus not only quotes Mark 1:2 but comments upon it, and in both quote and comment he supports the "Byzantine" reading. But the comment is a little ways removed from the quote and it is entirely improbable that a scribe should have molested the comment even if he felt called upon to change the quote. Fair play requires that this instance be loyally recorded as 2nd century support for the "Byzantine" reading.

Another, almost as unambiguous, instance occurs in *Adv. Haer.* III.16.3 where we read: "Wherefore Mark also says: 'The beginning of the Gospel of Jesus Christ, the Son of God; as it is written in the prophets.' Knowing one and the same Son of God, Jesus Christ, who was announced by the prophets . . ." Note that again Irenaeus not only quotes Mark 1:2 but comments upon it, and in both quote and comment he supports the "Byzantine" reading.

There is also a clear allusion to Mark 1:2 in *Adv. Haer.* III.11.4 where we read: "By what God, then, was John, the forerunner . . . sent? Truly it was by Him . . . who also had promised by the prophets that He would send His messenger before the face of His Son, who should prepare His way . . ." May we not reasonably claim this as a **third** citation in support of the "Byzantine" reading? In any case, it is clear that Fee's handling of the evidence from Irenaeus is disappointing at best, if not reprehensible.

While on the subject of Fee's reliability, I offer the evaluation given by W.F. Wisselink [cf. footnote 167, below] after a thorough investigation of some of his work.

While studying Fee's account ["P<sup>75</sup>, p<sup>66</sup>, and Origen: The Myth of Early Textual Recension in Alexandria," *New Dimensions in New Testament Study*, ed. R.N. Longenecker and M.C. Tenney (Grand Rapids: Zondervan, 1974), pp. 42-44] it became apparent to me that it is incomplete and indistinct, and that it contains mistakes. Fee gives account of his investigation in a little more than one page. He introduces this account as follows: "The full justification of this conclusion will require a volume of considerable size filled with lists of data. Here we can offer only a sample illustration with the further note that the complete data will vary little from the sampling" (Fee, 1974, 42).

Therefore I called upon Fee for the complete data. I received six partly filled pages containing the rough data about the assimilations in Luke 10 and 11. After studying these rough data I came to the conclusion that the rough data as well are incomplete and indistinct, and contain mistakes. So question marks can be placed at the reliability of the investigation which those rough data and that account have reference to. [Wisselink, p. 69.]

Wisselink then proceeds to document his charges on the next three pages.

I repeat that a critical edition of Irenaeus prepared by Fee could not be trusted, and I begin to wonder if any edition prepared by someone with an anti-Byzantine bias is to be trusted. This quite apart from their fallacious starting point, namely that the "Byzantine" text is late.

The three quotations from Irenaeus are taken from A. Roberts and J. Donaldson, eds. *The Ante-Nicene Fathers*, 1973, Vol. I, pp. 425-26 and 441, and were checked for accuracy against W. W. Harvey's critical edition (*Sancti Irenaei: Episcopi Lugdunensis: Libros Quinque Adversus Haereses*, Cambridge: University Press, 1857). I owe this material on Irenaeus to Maurice A. Robinson.

<sup>123</sup>Of course this principle is also applied to the Greek MSS, with serious consequences. A recent statement by Metzger gives a clear example.

It should be observed that, in accord with the theory that members of f<sup>1</sup> and f<sup>13</sup> were subject to progressive accommodation to the later Byzantine text, scholars have established the text of these families by adopting readings of family witnesses that differ from the Textus Receptus. Therefore the citation of the siglum f<sup>1</sup> and f<sup>13</sup> may, in any given instance, signify a minority of manuscripts (or even only one) that belong to the family. (*A Textual Commentary on the Greek New Testament* [companion to UBS<sup>3</sup>], p. xii.)

Such a procedure misleads the user of the apparatus, who has every right to expect that the siglum will only be used when all (or nearly all) the members agree. A distorted view of the evidence is created—the divergence of f<sup>1</sup> and f<sup>13</sup> from the "Byzantine" text is made to appear

Even if there appear to be certain instances where this has demonstrably happened, such instances do not justify a widespread generalization. The generalization is based on the pre-supposition that the "Byzantine" text is late—but this is the very point to be proved and may not be assumed.

If the "Byzantine" text is early there is no reason to suppose that a "Byzantine" reading in an early Father is due to a later copyist unless a clear demonstration to that effect is possible. Miller shows clearly that he was fully aware of this problem and alert to exclude any suspicious instances from his tabulation.

An objection may perhaps be made, that the texts of the books of the Fathers are sure to have been altered in order to coincide more accurately with the Received Text. This is true of the *Ethica*, or *Moralia*, of Basil, and of the *Regulae brevius Tractatae*, which seem to have been read constantly at meals, or were otherwise in continual use in Religious Houses. The monks of a later age would not be content to hear every day familiar passages of Holy Scripture couched in other terms than those to which they were accustomed and which they regarded as correct. This fact was perfectly evident upon examination, because these treatises were found to give evidence for the *Textus Receptus* in the proportion of about 6:1, whereas the other books of St. Basil yielded according to a ratio of about 8:3. [But might it possibly be the case that, precisely because of the "continual use in Religious Houses" (the more so if that use began early on), the 6:1 ratio reflects a pure/faithful transmission while "the other books" suffered some adulterations?]

For the same reason I have not included Marcion's edition of St. Luke's Gospel, or Tatian's *Diatessaron*, in the list of books and authors, because such representations of the Gospels having been in public use were sure to have been revised from time to time, in order to accord with the judgment of those who read or heard them. Our readers will observe that these were self-denying ordinances, because by the inclusion of the works mentioned the list on the Traditional side would have been greatly increased. Yet our foundations have been strengthened, and really the position of the Traditional Text rests so firmly upon what is undoubted, that it can afford to dispense with services which may be open to some suspicion. (Yet Marcion and Tatian may fairly be adduced as witnesses upon individual readings.) And the natural inference remains, that the difference between the witness of the *Ethica* and *Regulae brevius Tractatae* on the one hand, and that of the other works of Basil on the other, suggests that too much variation, and too much which is evidently characteristic variation, of readings meets us in the works of the several Fathers, for the existence of any doubt that in most cases we have the words, though perhaps not the spelling, as they issued originally from the author's pen. Variant readings of quotations occurring in different editions of the Fathers are found, according to my experience, much less frequently than might have been supposed. Where I saw a difference between MSS noted in the Benedictine or other editions or in copies from the Benedictine or other prints, of course I regarded the passage as doubtful and did not enter it. Acquaintance with this kind of testimony cannot but render its general trustworthiness the more evident.<sup>125</sup>

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greater than it really is, and the extent of variation among the members is obscured. Greenlee's study of Cyril of Jerusalem (p. 30, see next footnote) affords another example. Among other things, he appeals to "the well-known fact that all the Caesarean witnesses are more or less corrected to the Byzantine standard, but in different places, so that the groups must be considered as a whole, not by its [*sic*] individual members, to give the true picture." Would not the behavior of the individual MSS make better sense if viewed as departing from the Byzantine standard?

<sup>124</sup>I believe J.H. Greenlee's study of Cyril of Jerusalem is an example. *The Gospel Text of Cyril of Jerusalem* (Copenhagen: Ejnar Munksgaard, 1955).

<sup>125</sup>Burgon, *The Traditional Text*, pp. 97-98. I believe that Suggs tends to agree with Miller that the assimilating proclivity of medieval scribes can easily be overestimated ("The Use of Patristic Evidence," p. 140). The Lectionaries give eloquent testimony against the supposed assimilating proclivity. After discussing at some length their lack of textual consistency, Colwell observes: "Figuratively speaking, the Lectionary is a preservative into which from time to time portions of the living text were dropped. Once submerged in the Lectionary, each portion was solidified or fixed" (Colwell and Riddle, *Prolegomena to the Study of the Lectionary Text of the Gospels*, p. 17). Similarly, Riddle cites with favor Gregory's estimate: "He saw that as a product of the liturgical system they were guarded by a strongly conservative force, and he was right in his inference that the conservatism of the liturgy would tend frequently to make them media for the preservation of an early text. His analogy of the Psalter of the Anglican church was a good one" (*Ibid.*, pp. 40-41). Many of the lessons in the Anglican Prayer Book are much older than the AV but have never been assimilated to the AV. In short, we have good reason to doubt that medieval copyists were as addicted to assimilating the text as scholars such as Taylor would have us believe.

After this careful screening Miller still came up with 2,630 citations, from 76 Fathers or sources, ranging over a span of 300 years (100-400 A.D.), supporting readings of the "Byzantine" text as opposed to those of the critical text of the English Revisers (which received 1,753 citations). Will anyone seriously propose that all or most of those citations had been altered? What objective grounds are there for doing so?

Hills discusses the case of Origen as follows:

In the first fourteen chapters of the Gospel of John (that is, in the area covered by Papyrus Bodmer II) out of 52 instances in which the Byzantine text stands alone Origen agrees with the Byzantine text 20 times and disagrees with it 32 times. Thus the assertion of the critics that Origen knew nothing of the Byzantine text becomes difficult indeed to maintain. On the contrary, these statistics suggest that Origen was familiar with the Byzantine text and frequently adopted its readings in preference to those of the Western and Alexandrian texts.

Naturalistic critics, it is true, have made a determined effort to explain away the "distinctively" Byzantine readings which appear in the New Testament quotations of Origen (and other ante-Nicene Fathers). It is argued that these Byzantine readings are not really Origen's but represent alterations made by scribes who copied Origen's works. These scribes, it is maintained, revised the original quotations of Origen and made them conform to the Byzantine text. The evidence of Papyrus Bodmer II, however, indicates that this is not an adequate explanation of the facts. Certainly it seems a very unsatisfactory way to account for the phenomena which appear in the first fourteen chapters of John. In these chapters, 5 out of the 20 "distinctively" Byzantine readings which occur in Origen occur also in Papyrus Bodmer II. These 5 readings at least must have been Origen's readings, not those of scribes who copied Origen's works, and what is true of these 5 readings is probably true of the other 15, or at least of most of them.<sup>126</sup>

This demonstration makes it clear that the expedient deprecated above is in fact untenable.

### **The testimony of the early Fathers**

To recapitulate, "Byzantine" readings are recognized (most notably) by the *Didache*, Diognetus, and Justin Martyr in the first half of the second century; by the Gospel of Peter, Athenagorus, Hegesippus, and Irenaeus (heavily) in the second half; by Clement of Alexandria, Tertullian, Clementines, Hippolytus, and Origen (all heavily) in the first half of the third century; by Gregory of Thaumaturgus, Novatian, Cyprian (heavily), Dionysius of Alexandria, and Archelaus in the second half; by Eusebius, Athanasius, Macarius Magnus, Hilary, Didymus, Basil, Titus of Bostra, Cyril of Jerusalem, Gregory of Nyssa, Apostolic Canons and Constitutions, Epiphanius, and Ambrose (all heavily) in the fourth century. To which may be added the testimony of the early Papyri.

### **The testimony of the early Papyri**

In Hort's day and even in Miller's the early Papyri were not extant—had they been the W-H theory could scarcely have appeared in the form that it did. Each of the early Papyri (300 A.D. or earlier) vindicates some "Byzantine" readings. G. Zuntz did a thorough study of P<sup>46</sup> and concluded:

To sum up. A number of Byzantine readings, most of them genuine, which previously were discarded as 'late', are anticipated by P<sup>46</sup>. . . . How then—so one is tempted to go on asking—where no Chester Beatty papyrus happens to vouch for the early existence of a Byzantine reading? Are all Byzantine readings ancient? In the cognate case of the Homeric tradition G. Pasquali answers the same question in the affirmative.<sup>127</sup>

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<sup>126</sup>Burton, *The Last Twelve Verses*, p. 58. Sturz lists a number of further "Byzantine" readings that have had early Patristic support (Clement, Tertullian, Marcion, Methodius) and which now also have early Papyrus support (pp. 55-56). Here again it will no longer do to claim that the Fathers' MSS have been altered to conform to the "Byzantine" text.

<sup>127</sup>Zuntz, *The Text*, p. 55.



Colwell takes note of Zuntz's statement and concurs.<sup>128</sup> He had said of the "Byzantine New Testament" some years previous, "Most of its readings existed in the second century."<sup>129</sup>

Hills claims that the Beatty papyri vindicate 26 "Byzantine" readings in the Gospels, 8 in Acts and 31 in Paul's epistles.<sup>130</sup> He says concerning P<sup>66</sup>:

To be precise, Papyrus Bodmer II contains thirteen percent of all the alleged late readings of the Byzantine text in the area which it covers (18 out of 138). Thirteen percent of the Byzantine readings which most critics have regarded as late have now been proved by Papyrus Bodmer II to be early readings.<sup>131</sup>

Colwell's statement on P<sup>66</sup> has already been given.

Many other studies are available, but that of H. A. Sturz sums it up.<sup>132</sup> He surveyed "all the available papyri" to discover how many papyrus-supported "Byzantine" readings exist. In trying to decide which were "distinctively Byzantine" readings he made a conscious effort to "err on the conservative side" so that the list is shorter than it might be (p. 144).

He found, and lists the evidence for, more than 150 "distinctively Byzantine" readings that have early (before 300 A.D.) papyrus support (pp. 145-59). He found 170 "Byzantine-Western" readings with early papyrus support (pp. 160-74). He found 170 "Byzantine-Alexandrian" readings with early papyrus support (pp. 175-87). He gives evidence for 175 further "Byzantine" readings but which have scattered "Western" or "Alexandrian" support, with early papyrus support.<sup>133</sup> He refers to still another 195 readings where the "Byzantine" reading has papyrus support, but he doesn't bother to list them (apparently he considered these variants to be of lesser consequence).<sup>134</sup>

The magnitude of this vindication can be more fully appreciated by recalling that only about 30 percent of the New Testament has early papyrus attestation, and much of that 30 percent has only one papyrus. Where more than one covers a stretch of text, each new MS discovered vindicates added Byzantine readings. Extrapolating from the behavior of those in hand, if we had at least 3 papyri covering all parts of the New Testament, almost all the 6000+ Byzantine readings rejected by the critical (eclectic) texts would be vindicated by an early papyrus.

It appears that Hort's statement or treatment of external evidence has no basis in fact. What about his statement of internal evidence?

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<sup>128</sup>Colwell, "The Origin of Texttypes," p. 132.

<sup>129</sup>Colwell, *What is the Best New Testament?*, p. 70.

<sup>130</sup>Burgon, *The Last Twelve Verses*, p. 50. (Hills wrote the Introduction.)

<sup>131</sup>*Ibid.*, p. 54.

<sup>132</sup>H.A. Sturz, *The Byzantine Text-Type and New Testament Textual Criticism*.

<sup>133</sup>Pp. 188-208. Sturz remarks that a number of readings (15 from this list) really should be considered as "distinctively Byzantine" but one or another so-called "Western" or "Alexandrian" witness also has them and so . . .

Sturz draws the following conclusions from the evidence he presents: 1) "Distinctively Byzantine" readings are found in early papyri (p. 55). 2) Such readings are therefore early (p. 62). 3) Such readings cannot be the result of editing in the 4th century (p. 62). 4) The old uncials have not preserved a complete picture of the textual situation in the 2nd century (p. 62). 5) The "Byzantine" texttype has preserved some of the 2nd century tradition **not** found in the others (p. 64). 6) The lateness of other "Byzantine" readings, for which early papyrus attestation has not yet surfaced, is now questionable (p. 64). 7) "Byzantine-Western" alignments go back into the 2nd century; they **must** be old (p. 70).

(Fee speaks of my "misrepresentations of the papyrus evidence" and says with reference to it that I have "grossly misinterpreted the data" ("A Critique," p. 422). I invite the reader to check the evidence presented by Sturz and then to decide for himself whether or not there has been misrepresentation and misinterpretation.)

<sup>134</sup>P. 189. This means that the early Papyri vindicate "Byzantine" readings in 660 (or 885) places where there is significant variation. One might wish that Sturz had also given us the figures for "distinctively Western" and "distinctively Alexandrian" readings, but how are such expressions to be defined? Where is an objective definition for "Western reading," for example?

## Internal Evidence of Readings

We have already noted something of the use Hort made of internal evidence, but he himself recognized its weaknesses. He said: "In dealing with this kind of evidence [Intrinsic Evidence of Readings] equally competent critics often arrive at contradictory conclusions as to the same variations."<sup>135</sup>

And again, four pages later: "Not only are mental impulses unsatisfactory subjects for estimates of comparative force; but a plurality of impulses recognized by ourselves as possible in any given case by no means implies a plurality of impulses as having been actually in operation."<sup>136</sup>

Exactly! No twentieth century person confronting a set of variant readings can know or prove what actually took place to produce the variants.

Again Hort's preaching is better than his practice:

The summary decisions inspired by an unhesitating instinct as to what an author must needs have written, or dictated by the supposed authority of "canons of criticism" as to what transcribers must needs have introduced, are in reality in a large proportion of cases attempts to dispense with the solution of problems that depend on genealogical data.<sup>137</sup>

If we but change the words "genealogical data" to "external evidence" we may agree with him. Unfortunately, however, the fine sentiments quoted above were but a smoke screen. As Fee says:

The internal evidence of readings was also the predominant factor in the choice of his "Neutral" text over the "Western" and "Alexandrian" texts . . . and his choice of B. . . .

The point is that Hort did not come to his conclusion about the Byzantines and B by the genealogical method, . . .<sup>138</sup>

The precarious and unsatisfactory nature of internal evidence has already received some attention in the discussion of eclecticism. Colwell says specifically of the use of intrinsic and transcriptional probability: "Unfortunately these two criteria frequently clash in a head-on collision, because ancient scribes as well as modern editors often preferred the reading which best fits the context."<sup>139</sup> "If we choose the reading that best explains the origin of the other reading, we are usually choosing the reading that does not fit the context. The two criteria cancel each other out."<sup>140</sup> And that leaves the scholar "free to choose in terms of his own prejudices."<sup>141</sup>

Burgon said of internal considerations: "Often they are the product of personal bias, or limited observation: and where one scholar approves, another dogmatically condemns. Circumstantial evidence is deservedly rated low in the courts of justice: and lawyers always produce witnesses when they can."<sup>142</sup>

We venture to declare that inasmuch as one expert's notions of what is 'transcriptionally probable' prove to be the diametrical reverse of another expert's notions, the supposed evidence to be derived from this source may, with advantage, be neglected altogether. Let the study of **Documentary Evidence** be allowed to take its place. Notions of 'Probability' are the very pest of those departments of Science which admit of an appeal to **Fact**.<sup>143</sup>

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<sup>135</sup>Westcott and Hort, p. 21.

<sup>136</sup>*Ibid.*, p. 25. Fee criticizes me rather severely for my "agnosticism" ("A Critique," p. 409), but my statement is scarcely stronger than Hort's.

<sup>137</sup>*Ibid.*, p. 286.

<sup>138</sup>Fee, "Modern Text Criticism and the Synoptic Problem," *J.J. Griesbach: Synoptic and Text-Critical Studies 1776-1976*, ed. B. Orchard and T.R.W. Longstaff (Cambridge: University Press, 1978), p. 156.

<sup>139</sup>Colwell, "The Greek New Testament," p. 37.

<sup>140</sup>Colwell, "External Evidence," p. 4.

<sup>141</sup>*Ibid.*, p. 3.

<sup>142</sup>Burgon, *The Traditional Text*, p. 67.

<sup>143</sup>Burgon, *The Revision Revised*, p. 251.

He also called attention to a danger involved in the use of a system of strict canons. "People are ordinarily so constituted, that when they have once constructed a system of Canons they place no limits to their operation, and become slaves to them."<sup>144</sup> (Gordon Fee's use of *ardua lectio potior* seems to me to be a case in point.)<sup>145</sup>

### The shorter reading

Perhaps the canon most widely used against the "Byzantine" text is *brevior lectio potior*—the shorter reading is to be preferred. As Hort stated the alleged basis for the canon, "In the New Testament, as in almost all prose writings which have been much copied, corruptions by interpolation are many times more numerous than corruptions by omission."<sup>146</sup> Accordingly it has been customary since Hort to tax the Received Text as being full and interpolated and to regard B and Aleph as prime examples of non-interpolated texts.<sup>147</sup>

But is it really true that interpolations are "many times more numerous" than omissions in the transmission of the New Testament? B.H. Streeter thought not.

Hort speaks of "the almost universal tendency of transcribers to make their text as full as possible, and to eschew omissions"; and infers that copyists would tend to prefer an interpolated to an uninterpolated text. This may be true of some of the local texts of the second century; it is the very opposite of the truth where scribes or editors trained in the tradition of Alexandrian textual criticism are concerned. The Alexandrian editors of Homer were as eagle-eyed to detect and obelise "interpolations" in Homer as a modern critic. . . .

That Christian scholars and scribes were capable of the same critical attitude we have irrefragable evidence. . . . The notion is completely refuted that the regular tendency of scribes was to choose the longer reading, and that therefore the modern editor is quite safe so long as he steadily rejects. . . .

Now, whoever was responsible for it, the B text has been edited on the Alexandrian principle.<sup>148</sup>

The whole question of interpolations in ancient MSS has been set in an entirely new light by the researches of Mr. A. C. Clark, Corpus Professor of Latin at Oxford. . . . In *The Descent of Manuscripts*, an investigation of the manuscript tradition of the Greek and Latin Classics, he proves conclusively that the error to which scribes were most prone was not interpolation but accidental omission. . . . Hitherto the maxim *brevior lectio potior* . . . has been assumed as a postulate of scientific criticism. Clark has shown that, so far as classical texts are concerned, the facts point entirely the other way.<sup>149</sup>

Burgon had objected long before.

How indeed can it possibly be more true to the infirmities of copyists, to the verdict of evidence on the several passages and to the origin of the New Testament in the infancy of the Church and amidst associations which were not literary, to suppose that a terse production was first produced and afterwards was amplified in a later age with a view to 'lucidity and completeness,'

<sup>144</sup>Burgon, *The Traditional Text*, p. 66.

<sup>145</sup>Fee, *Papyrus Bodmer II*.

<sup>146</sup>Westcott and Hort, p. 235.

<sup>147</sup>Actually, a look at a good apparatus or at collations of MSS reveals that the "Byzantine" text-type is frequently shorter than its rivals. Sturz offers charts which show that where the "Byzantine" text with early papyrus support stands against both the "Western" and "Alexandrian" it adds 42 words and omits 36 words in comparison to them. The "Byzantine" comes out somewhat longer but the picture is not lopsided. Among the added words are 9 conjunctions and 5 articles but among the omitted are 11 conjunctions and 6 articles, which would make the "Byzantine" **less** smooth than its rivals. (Sturz, p. 229.)

<sup>148</sup>B.H. Streeter, *The Four Gospels: A Study of Origins* (London: Macmillan and Co., 1930), pp. 122-24. For a more recent discussion of critical activity at Alexandria, see W.R. Farmer, *The Last Twelve Verses of Mark* (Cambridge: Cambridge University Press, 1974), pp. 13-22.

<sup>149</sup>*Ibid.*, p. 131. I am aware that Kenyon and others have criticized Clark's treatment of this maxim, but I believe that it has sufficient validity to be worth taking into account.

rather than that words and clauses and sentences were omitted upon definitely understood principles in a small class of documents by careless or ignorant or prejudiced scribes.<sup>150</sup>

Leo Vaganay also had reservations concerning this canon.

As a rule the copyist, especially when at the work of revision, is inclined to amplify the text. . . . But the rule suffers many exceptions. . . . Distraction of the copyist, . . . intentional corrections. . . . And finally, . . . the fundamental tendency of some recension, of which a good example is the Egyptian recension. . . . And also we must not forget that the writers of the New Testament were Orientals, who are more given to length than to brevity.<sup>151</sup>

Kilpatrick actually suggests that a substitute canon, "the longer reading is preferable," would be no worse. He concludes:

On reflection we do not seem able to find any reason for thinking that the maxim *lectio brevior potior* really holds good. We can only hope that a fuller acquaintance with the problems concerned will enable us increasingly to discern reasons in each instance why the longer or the shorter reading seems more probable.<sup>152</sup>

Colwell has published a most significant study of scribal habits as illustrated by the three early papyri P<sup>45</sup>, P<sup>66</sup>, and P<sup>75</sup>. It demonstrates that broad generalizations about scribal habits should never have been made and it follows that ideas about variant readings and text-types based on such generalizations should be reconsidered. It will be well to quote Colwell at some length.

The characterization of these singular readings can go on further until the individual scribes have been characterized. Their peculiar readings are due to their peculiarities. This has been well said by Dain. He reminds us that although all scribes make mistakes and mistakes of the same kind, yet each scribe has a personal coefficient of the frequency of his mistakes. Each has his own pattern of errors. One scribe is liable to dittography, another to the omission of lines of text; one reads well, another remembers poorly; one is a good speller; etc., etc. In these differences must be included the seriousness of intention of the scribe and the peculiarities of his own basic method of copying.<sup>153</sup>

In general, P<sup>75</sup> copies letters one by one; P<sup>66</sup> copies syllables, usually two letters in length. P<sup>45</sup> copies phrases and clauses.

The accuracy of these assertions can be demonstrated. That P<sup>75</sup> copied letters one by one is shown in the pattern of the errors. He has more than sixty readings that involve a single letter, *and* not more than ten careless readings that involve a syllable. But P<sup>66</sup> drops sixty-one syllables (twenty-three of them in "leaps") and omits as well a dozen articles and thirty short words. In P<sup>45</sup> there is not one omission of a syllable in a "leap" nor is there any list of "careless" omissions of syllables. P<sup>45</sup> omits words and phrases.<sup>154</sup>

As an editor the scribe of P<sup>45</sup> wielded a sharp axe. The most striking aspect of his style is its conciseness. The dispensable word is dispensed with. He omits adverbs, adjectives, nouns, participles, verbs, personal pronouns—without any compensating habit of addition. He frequently omits phrases and clauses. He prefers the simple to the compound word. In short, he favors brevity. He shortens the text in at least fifty places in *singular readings alone*. But he does *not* drop syllables or letters. His shortened text is readable.<sup>155</sup>

Enough of these have been cited to make the point that P<sup>66</sup> editorializes as he does everything else—in a sloppy fashion. He is not guided in his changes by some clearly defined

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<sup>150</sup>Burton, *The Causes of the Corruption*, p. 156.

<sup>151</sup>Vaganay, pp. 84-85.

<sup>152</sup>Kilpatrick, p. 196.

<sup>153</sup>Colwell, "Scribal Habits," p. 378.

<sup>154</sup>*Ibid.*, p. 380.

<sup>155</sup>*Ibid.*, p. 383.

goal which is always kept in view. If he has an inclination toward omission, it is not "according to knowledge," but is whimsical and careless, often leading to nothing but nonsense.<sup>156</sup>

P<sup>66</sup> has 54 leaps forward, and 22 backward; 18 of the forward leaps are haplography.

P<sup>75</sup> has 27 leaps forward, and 10 backward.

P<sup>45</sup> has 16 leaps forward, and 2 backward.

From this it is clear that the scribe looking for his lost place looked ahead three times as often as he looked back. In other words, the loss of position usually resulted in a loss of text, an omission.<sup>157</sup>

The tables have been turned. Here is a clear statistical demonstration that interpolations are **not** "many times more numerous" than omissions. Omission is more common as an unintentional error than addition, and P<sup>45</sup> shows that with some scribes omissions were *deliberate* and extensive. Is it mere coincidence that Aleph and B were probably made in the same area as P<sup>45</sup> and exhibit similar characteristics? In any case, the "fullness" of the Traditional Text, rather than a proof of inferiority, emerges as a point in its favor.

### The harder reading

Another canon used against the "Byzantine" text is *proclivi lectioni praestat ardua*—the harder reading is to be preferred. The basis for this is an alleged propensity of scribes or copyists to simplify or change the text when they found a supposed difficulty or something they didn't understand. But where is the statistical demonstration that warrants such a generalization? Probably, as in the case of the canon just discussed, when such a demonstration is forthcoming it will prove the opposite.

Vaganay said of this canon:

But the more difficult reading is not always the more probably authentic. The rule does not apply, for instance, in the case of some accidental errors. . . . But, what is worse, we sometimes find difficult or intricate readings that are the outcome of intentional corrections. A copyist, through misunderstanding some passage, or through not taking the context into account, may in all sincerity make something obscure that he means to make plain.<sup>158</sup>

Have we not all heard preachers do this very thing?

Metzger notes Jerome's complaint: "Jerome complained of the copyists who 'write down not what they find but what they think is the meaning; and while they attempt to rectify the errors of others, they merely expose their own.'"<sup>159</sup> (Just so, producing what would appear to us to be "harder readings" but which readings are spurious.)

After recounting an incident at an assembly of Cypriot bishops in 350 A.D. Metzger concludes:

Despite the vigilance of ecclesiastics of Bishop Spyridon's temperament, it is apparent from even a casual examination of a critical apparatus that scribes, offended by real or imagined errors of spelling, grammar, and historical fact, deliberately introduced changes into what they were transcribing.<sup>160</sup>

Would not many of these changes appear to us to be "harder readings"?

In any case, the amply documented fact that numerous people in the second century made deliberate changes in the text, whether for doctrinal or other reasons, introduces an unpredictable variable which invalidates this canon. Once a person arrogates to himself the authority to alter the text there is

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<sup>156</sup> *Ibid.*, p. 387.

<sup>157</sup> *Ibid.*, pp. 376-77.

<sup>158</sup> Vaganay, p. 86.

<sup>159</sup> Metzger, *The Text*, p. 195.

<sup>160</sup> *Ibid.*, p. 196.

nothing in principle to keep individual caprice from intruding or taking over—we have no way of knowing what factors influenced the originator of a variant (whoever he was) or whether the result would appear to us to be "harder" or "easier." This canon is simply inapplicable.<sup>161</sup>

Another problem with this canon is its vulnerability to the manipulation of a skillful and determined imagination. With sufficient ingenuity, virtually any reading can be made to look "convincing." Hort is a prime example of this sort of imagination and ingenuity. Zuntz has stated:

Dr. Hort's dealing with this and the other patristic evidence for this passage [1 Cor.13:3] requires a word of comment. No one could feel greater respect, nay reverence, for him than the present writer; but his treatment of this variant, in making every piece of the evidence say the opposite of its true meaning, shows to what distortions even a great scholar may be driven by the urge to square the facts with an erroneous, or at least imperfect theory. Souter, Plummer, and many others show the aftereffect of Dr. Hort's tenacity.<sup>162</sup>

Salmon has noted the same thing: "That which gained Hort so many adherents had some adverse influence with myself—I mean his extreme cleverness as an advocate; for I have felt as if there were no reading so improbable that he could not give good reasons for thinking it to be the only genuine."<sup>163</sup>

Samuel Hemphill wrote of Hort's role in the New Testament Committee that produced the Revised Version of 1881:

Nor is it difficult to understand that many of their less resolute and decided colleagues must often have been completely carried off their feet by the persuasiveness and resourcefulness, and zeal of Hort, . . . In fact, it can hardly be doubted that Hort's was the strongest will of the whole Company, and his adroitness in debate was only equaled by his pertinacity.<sup>164</sup>

(It would appear that the composition of the Greek text used by the English Revisers—and consequently for the RSV, NASB, etc.—was determined in large measure by Hort's cleverness and pertinacity, inspired by his devotion to a single Greek manuscript.)

Hort's performance shows the reasonableness of Colwell's warning against "the distortion of judgment which so easily manipulates the criteria of internal evidence."<sup>165</sup>

### Harmonization<sup>166</sup>

It is widely asserted that the "Byzantine" text is characterized by harmonizations, e.g. Metzger: "The framers of this text sought . . . to harmonize divergent parallel passages."<sup>167</sup> By the choice of this terminology it is assumed that the diverse readings found in the minority of MSS are original and that copyists felt impelled to make parallel accounts agree. Perhaps it is time to ask whether it ever has been or can be proved that such an interpretation is correct. Jakob Van Bruggen says of Metzger's statement, "this judgment has not been proven, and can not be proven."<sup>168</sup>

<sup>161</sup>To anyone who feels that we are obligated to explain the origin of any or every peculiar variant reading, even if found in only one or two copies—especially if the copies happen to be B, Aleph or one of the Papyri—Burgon calls attention to the far greater correlative obligation. "It frequently happens that the one remaining plea of many critics for adopting readings of a certain kind, is the inexplicable nature of the phenomena which these readings exhibit. 'How will you possibly account for such a reading as the present,' (say they,) 'if it be not authentic?' . . . They lose sight of the correlative difficulty:—How comes it to pass that the rest of the copies read the place otherwise?" (*The Causes of the Corruption*, p. 17.)

<sup>162</sup>Zuntz, *The Text*, p. 36.

<sup>163</sup>Salmon, pp. 33-34.

<sup>164</sup>S. Hemphill, *A History of the Revised Version* (London: Elliot Stock, 1906), pp. 49-50.

<sup>165</sup>Colwell, "External Evidence," p. 2. The application is mine. Colwell would perhaps not have agreed with it.

<sup>166</sup>My critics graciously called attention to some genuine weaknesses in my treatment of this topic in the first edition. For the second edition the section was rewritten and considerably enlarged. For this considerable revision we now have access to W.F. Wisselink's massive four-volume evaluation of this question. His work deprives the opponents of the Byzantine text of their last argument.

<sup>167</sup>Metzger, *A Textual Commentary*, p. xx.

<sup>168</sup>Jakob Van Bruggen, *The Ancient Text of the New Testament* (Winnipeg: Premier, 1976), p. 30. Cf. W.F. Wisselink, *Assimilation as a Criterion for the Establishment of the Text*, 4 vols. (Kampen: Uitgeversmaatschappij J.H. Kok, 1989). Wisselink

### 1) Van Bruggen

Because Van Bruggen's valuable work may not be available to many readers, I will quote from his treatment of the subject in hand at some length. His reaction to Metzger's statement continues:

Often illustrative examples are given to support this negative characterization of the Byzantine text. But it would not be difficult to "prove", with the aid of specially chosen examples from other text-types, that those types are also guilty of harmonizing, conflating readings and smoothing the diction.<sup>169</sup>

Kilpatrick, using strictly internal evidence, concludes that, "though the Syrian text has its share of harmonizations, other texts including the Egyptian have suffered in this way. We cannot condemn the Syrian text for harmonization. If we do, we must condemn the other texts too on the same grounds."<sup>170</sup>

Van Bruggen continues:

Here illustrations do not prove anything. After all, one could without much difficulty give a large number of examples from the Byzantine text to support the proposition that this text does *not* harmonize and does *not* smooth away. In commentaries the exegete is often satisfied with the incidental example without comparing it to the textual data as a whole. Yet a proposition about the Byzantine *type* should not be based on illustrations, but on arguments from the text as a *whole*. Whoever wishes to find such arguments will meet a number of methodical problems and obstacles which obstruct the way to the proof. Here we can mention the following points:

1. Methodically we must first ask how a "type" is determined. This can not be done on the basis of selected readings, because then the selection will soon be determined by what one is trying to prove. You can only speak of a text-type if the characteristics which must distinguish the type are not incidental but are found all along, and if they do not appear in other types from which the type must be distinguished. The criteria must be distinctive and general. As far as this is concerned, suspicion is roused when Hort remarks that the harmonizing and assimilating interpolations in the Byzantine text are "fortunately capricious and incomplete" (*Introduction*, p. 135). Did Hort then indeed generalize and make characteristics of some readings into characteristics of the text-type? This suspicion becomes certainty when Metzger in his *Textual Commentary* has to observe more than once that non-Byzantine readings, for example, in the Codex Vaticanus, can be explained from the tendencies of scribes to assimilate and to simplify the text.<sup>171</sup>

In a footnote, Van Bruggen cites Metzger's discussion of Matthew 19:3 and 19:9, John 6:14, James 2:3, 4:14, 5:16, and 5:20, where harmonization and other smoothing efforts are ascribed to Codex B and its fellow-travelers. His discussion proceeds:

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concludes: "Assimilations occur in all manuscripts. Even in manuscript B there is a question of assimilation in 31 percent of the 1489 variations that have been investigated. In P<sup>75</sup> the number of assimilations is: 39 percent of the 165 variations that have been investigated" (p. 87). Maurice A. Robinson contributes the following relevant questions:

- 1) Why did not the Byzantine Textform develop as it should have [by the Hortian hypothesis], and move more consistently toward harmonization of all passages?
- 2) Why do we instead find as many or more possible harmonizations among the minority texttypes as is alleged to have occurred in regard to the Byzantine Textform?
- 3) Further, why did the keepers and guardians of the Byzantine tradition correctly reject the vast bulk of such harmonizations? Most harmonizations never gained more than a slight foothold which could not and did not endure.
- 4) Why also—if harmonization were so common, as well as a popular tendency within a growing and continuing process—did not the plain and clear "early harmonizations" among representatives of the Alexandrian and Western texttypes endure as the text progressed into the Byzantine era?
- 5) Why, especially, were pre-existing harmonizations as found in the Western and Alexandrian traditions *de*-harmonized by the scribes of the Byzantine era, since this was precisely the opposite of what should have occurred?

Robinson, "Two Passages in Mark: A Critical Test for the Byzantine-Priority Hypothesis," presented to the forty-sixth annual meeting of the E.T.S., Nov., 1994, p. 25. The interested reader would do well to read pp. 24-34 of this paper—Robinson makes a number of telling points.

<sup>169</sup>*Ibid.* Cf. E.F. Hills, "Harmonizations in the Caesarean Text of Mark," *Journal of Biblical Literature*, 66 (1947), 135-152.

<sup>170</sup>Kilpatrick, p. 193.

<sup>171</sup>Van Bruggen, pp. 30-31.

What is typical for the Byzantine text is apparently not so exclusive for this text-type! But if certain phenomena seem to appear in all types of text, then it is not right to condemn a type categorically and regard it as secondary on the ground of such phenomena.

2. Moreover, it is methodically difficult to speak of harmonizing and assimilating deviations in a text, when the original is not known. Or is it an axiom that the original text in any case was so inharmonious, that every harmonious reading is directly suspect? Hort lets us sense that he personally does not prefer a New Testament "more fitted for cursory perusal or recitation than for repeated and diligent study" (*Introduction*, p. 153). Yet who, without the original at his disposal, can prove that this original had those characteristics which a philologist and a textual critic considers to be most recommendable?<sup>172</sup>

P. Walters comments upon Hort's sense of style as follows:

Hort's sense of style, his idea of what was correct and preferable in every alternative, was acquired from a close acquaintance with his "neutral" text. It did not occur to him that most of its formal aspects tallied with his standards just because these were taken from his model. So far his decisions are in the nature of a vicious circle: We today who live outside this magic circle, which kept a generation spellbound, are able to see through Hort's illusion.<sup>173</sup>

Van Bruggen continues:

4. If editors of the Byzantine text would have been out to harmonize the text and to fit parallel passages of the Gospels into each other, then we must observe that they let nearly all their opportunities go by. . . . In addition, what seems to be harmonization is in a different direction often no harmonization. A reading may seem adjusted to the parallel passage in an other Gospel, but then often deviates again from the reading in the third Gospel. A reading may seem borrowed from the parallel story, yet at the same time fall out of tune in the context of the Gospel itself. Here the examples are innumerable as long as one does not limit himself to a few texts and pays attention to the context and the Gospels as a whole.<sup>174</sup>

With reference to giving due attention to the context, Van Bruggen reports on a study wherein he compared the TR with Nestle<sup>25</sup> in fourteen extended passages to see if either one could be characterized as harmonizing or assimilating.

The comparison of the edition Stephanus (1550) with Nestle-Aland (25th edition) led to the result that the dilemma "harmonizing/not harmonizing" is unsuited to distinguish both of these text-editions. We examined Matthew 5:1-12; 6:9-13; 13:1-20; 19:1-12; Mark 2:18-3:6; Luke 9:52-62; 24:1-12; John 6:22-71; Acts 18:18-19:7; 22:6-21; 1 Corinthians 7; James 3:1-10; 5:10-20; Revelation 5. In the comparative examination not only the context, but also all the parallel passages were taken into account. Since the Stephanus-text is closely related to the Byzantine text and the edition Nestle-Aland is clearly non-Byzantine, the result of this investigation may also apply to the relation between the Byzantine text and other text-types: the dilemma "harmonizing/not harmonizing" or "assimilating/not assimilating" is unsound to distinguish *types* in the textual tradition of the New Testament.<sup>175</sup>

One is reminded of Burgon's observation that decisions based on internal considerations are often "the product of personal bias, or limited observation."<sup>176</sup> In this connection it will be well to consider some examples.

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<sup>172</sup>*Ibid.*, pp. 31-32.

<sup>173</sup>P. Walters, *The Text of the Septuagint. Its Corruptions and their Emendation*, ed. D.W. Gooding (Cambridge: University Press, 1973), p. 21. (Cited by van Bruggen.)

<sup>174</sup>Van Bruggen, pp. 32-33.

<sup>175</sup>*Ibid.*, p. 33.

<sup>176</sup>Burgon, *The Traditional Text*, p. 67.



## 2) Examples

Mark 1:2—shall we read "in Isaiah the prophet" with the "Alexandrian-Western" texts or "in the prophets" with the "Byzantine" text? All critical editions follow the first reading and Fee affirms that it is "a clear example of 'the most difficult reading being preferred as the original.'"<sup>177</sup> I would say that Fee's superficial discussion is a "clear example" of personal bias (toward the "harder reading" canon) and of limited observation. The only other places that Isaiah 40:3 is quoted in the New Testament are Matthew 3:3, Luke 3:4, and John 1:23. The first two are in passages parallel to Mark 1:2 and all three are identical to the LXX. The quote in John differs from the LXX in one word and is also used in connection with John the Baptist. The crucial consideration, for our present purpose, is that Matthew, Luke, and John all identify the quote as being from Isaiah (without MS variation). It seems clear that the "Alexandrian-Western" reading in Mark 1:2 is simply an assimilation to the other three Gospels. It should also be noted that the material from Malachi looks more like an allusion than a direct quote. Further, although Malachi is quoted (or alluded to) a number of times in the New Testament, he is never named. Mark's own habits may also be germane to this discussion. Mark quotes Isaiah in 4:12, 11:17, and 12:32 and alludes to him in about ten other places, all without naming his source. The one time he does use Isaiah's name is when quoting Jesus in 7:6.<sup>178</sup> It is the "Byzantine" text that has escaped harmonization and preserves the original reading.

Mark 10:47 -- *Ναζαρηνος* B L W Δ Θ Ψ 1 lat cop  
*Ναζορηνος* D  
*Ναζωραιος* Byz ℵ A C (K) X Π 13 pl it<sup>pt</sup> syr

//Luke 18:37 -- *Ναζαρηνος* D 1 pc  
*Ναζωραιος* rell

Mark 1:24 -- *Ναζαρηνε* all agree

Mark 14:67 -- *Ναζαρηνον* all agree

Mark 16:6 -- *Ναζαρηνον* all agree except that ℵ and D omit.

All critical editions follow the first reading in Mark 10:47 and interpret the "Byzantine" reading as an assimilation to Luke 18:37 (where they reject the reading of D). It should be observed, however, that everywhere else that Mark uses the word the *-αρην-* form occurs. Is it not just as possible that Codex B and company have assimilated to the prevailing Markan form?<sup>179</sup>

Mark 8:31 -- *μετα τρεις ημερας* all agree

//Matt 16:21 -- *μετα τρεις ημερας* D al  
*τη τριτη ημερα* rell

//Luke 9:22 -- *μεθ ημερας τρεις* D it  
*τη τριτη ημερα* rell

Mark 9:31 -- *μετα τρεις ημερας* ℵ B C D L Δ  
*τη τριτη ημερα* Byz Θ pl

//Matt 17:23 -- *μετα τρεις ημερας* D it

<sup>177</sup>Fee, "A Critique of W.N. Pickering's *The Identity of the New Testament Text: A Review Article*," *The Westminster Theological Journal*, XLI (Spring, 1979), p. 411.

<sup>178</sup>I owe the material in the above discussion to Maurice A. Robinson.

<sup>179</sup>This discussion is adapted from Van Bruggen, pp. 33-34.

τη τριτη ημερα rell

Mark 10:34 -- μετα τρεις ημερας Ἕ B C D L Δ Ψ it cop

τη τριτη ημερα Byz A<sup>C</sup> K W X Θ Π 1 13 p/ syr

//Matt 10:19 -- τη τριτη ημερα all agree

//Luke 18:33 -- τη ημερα τη τριτη all agree

All critical editions follow the first reading in Mark 9:31 and 10:34 and interpret the "Byzantine" reading as an assimilation to Matthew, in both cases. But why, then, did the "Byzantines" not also assimilate in Mark 8:31 where there was the pressure of both Matthew **and** Luke? Is it not more likely that the "Alexandrians" made Mark consistent (note that Matthew is consistent) by assimilating the latter two instances to the first one? Note that in this example and the preceding one it is Codex D that engages in the most flagrant assimilating activity.<sup>180</sup>

Mark 13:14—shall we read "spoken of through Daniel the prophet" with the "Byzantine" text or follow the "Alexandrian-Western" text wherein this phrase is missing? All critical editions take the second option and Fee assures us that the "Byzantine" text has assimilated to Matthew 24:15 where all witnesses have the phrase in question.<sup>181</sup> But let us consider the actual evidence:

Matt 24:15 -- το ρηθεν δια Δαβιηλ του προφητου

Mark 13:14 -- το ρηθεν υπο Δαβιηλ του προφητου

If the "Byzantines" were intent on copying from Matthew, why did they alter the wording? If their purpose was to harmonize, why did they disharmonize, to use Fee's expression? Furthermore, if we compare the full pericope in both Gospels, Matthew 24:15-22 and Mark 13:14-20, using the "Byzantine" text, although the two accounts are of virtually equal length, fully one third of the words are different between them. The claim that the "Byzantines" were given to harmonizing becomes silly. Still further, there appear to be three clear assimilations to Mark on the part of the "Alexandrian-Western" witnesses, and one to Matthew—*επι* to *εις* in Matthew 24:15, *καταβαινετω* to *καταβατω* in Matthew 24:17, *τα ιματια* to *το ιματιον* in Matthew 24:18, and the omission of *ων* in Mark 13:16—plus three other "Western" assimilations—*τα* to *τι* in Matthew 24:17, *και* to *ουδ* in Mark 13:19, and *δε* added to Matthew 24:17. But, returning to the first variant, why would the "Alexandrians" have omitted the phrase in question? A comparison of the LXX of Daniel with the immediate context suggests an answer. Mark's phrase, "where he ought not," is not to be found in Daniel. That some people felt Mark's integrity needed protecting is clear from the remedial actions attempted by a few Greek and version MSS. The Alexandrian omission may well be such an attempt.<sup>182</sup>

To conclude, it is demonstrable that all "text-types" have many possible harmonizations. It has not been demonstrated that the "Byzantine" text has more possible or actual harmonizations than the others. It follows that "harmonization" may not reasonably or responsibly be used to argue for an inferior "Byzantine" text type.

<sup>180</sup>This discussion is adapted from Van Bruggen, p. 34. I suspect that a thorough check will reveal that it is the "Western" text that leads all others in harmonization, not the "Byzantine". Wisselink confirms this, "D especially has been assimilated" (p. 87). Here is his conclusion.

With rather great certainty we can come to this conclusion: Assimilation is not restricted to a single group of manuscripts, neither to a single gospel; assimilation has not taken place to any one gospel to a strikingly high degree.

So if an assimilation is signaled, nothing can be concluded from that regarding the age of any variant or the value of any text-type. (Wisselink, p. 92.)

<sup>181</sup>Fee, "A Critique," pp. 411-12.

<sup>182</sup>I owe the material used in the above discussion to Robinson.

## Inferiority

Hort did not offer a statistical demonstration in support of his characterization of the "Byzantine" text.<sup>183</sup> Metzger refers to von Soden as supplying adequate evidence for the characterization. Upon inspection of the designated pages,<sup>184</sup> we discover there is no listing of manuscript evidence and no discussion. His limited lists of references purportedly illustrating addition or omission or assimilation, etc., may be viewed differently by a different mind. In fact, Kilpatrick has argued for the originality of a considerable number of Byzantine readings of the sort von Soden listed.<sup>185</sup>

The length of the lists, in any case, is scarcely prepossessing. No one has done for the "Byzantine" text anything even remotely approximating what Hoskier did for Codex B, filling 450 pages with a careful discussion, one by one, of many of its errors and idiosyncrasies.<sup>186</sup> As we have already noted, Hort declared the *Textus Receptus* to be "villainous" and "vile" when he was only twenty-three years old—before he had studied the evidence, before he had worked through the text to evaluate variant readings one by one. Do you suppose he brought an open mind to that study and evaluation?

Elliott and Kilpatrick profess to do their evaluating with an open mind, with no predilections as to text-types, yet inescapably use the ambiguous canons of internal evidence. What do they conclude? Elliott decided the "Byzantine" text was right about as often as Aleph and D, the chief representatives of the "Alexandrian" and "Western" texts (in the Pastorals).<sup>187</sup> Kilpatrick affirms:

Our principal conclusion is that the Syrian text is frequently right. It has avoided at many points mistakes and deliberate changes found in other witnesses. This means that at each variation we must look at the readings of the Byzantine manuscripts with the possibility in mind that they may be right. We cannot dismiss their characteristic variants as being in principle secondary.<sup>188</sup>

The basic deficiency, both fundamental and serious, of any characterization based upon subjective criteria is that the result is only opinion; it is not objectively verifiable. Is there no better way to identify the original wording of the New Testament? I believe there is, but first there is one more tenet of Hort's theory to scrutinize.

## The "Lucianic Recension" and the Peshitta

Burgon gave the sufficient answer to this invention.

Apart however from the gross intrinsic improbability of the supposed Recension,—the utter absence of one particle of evidence, traditional or otherwise, that it ever did take place,

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<sup>183</sup>Hort's characterization is similar to contemporary descriptions of Koine Greek in New Testament times.

Non-biblical sources attest that there was such a simple and plain style of Greek writing and speaking stemming from the earliest New Testament times. Such sources as the non-biblical papyri and the Discourses of Epictetus, the Stoic philosopher, attest this style. In addition, there is a formal delineation of what the plain style ought to be, which has been dated at approximately the same time in which the New Testament was being written. *Demetrius, On Style*, names "the plain style" . . . as one of four which he describes and discusses. . . . parts of his treatment of this subject tend to remind one of descriptions of the Koine of the Hellenistic period and the kind of Greek supposed to characterize the New Testament. . . .

In spite of the known existence of such a plain style as set forth by Demetrius and found in Epictetus, there were those in the early period of the Church and its writings who scoffed at the plain style and spoke contemptuously of it as it is found in the Scriptures. One of these was the pagan Celsus, who sought to refute the Christian faith in a literary attack penned sometime between 161-180 A.D. Origen indicates that Celsus ridiculed the Scriptures by holding them up to unfavorable comparison with the writings of the philosophers in places where there seemed to be some parallel (Sturz, pp. 112-13).

<sup>184</sup>H.F. von Soden, *Die Schriften des Neuen Testaments* (2 Vols.; Gottingen: Vandenhoeck und Ruprecht, 1911), Vol. 1, part ii, pp. 1456-1459 (cf. 1361-1400), 1784-1878.

<sup>185</sup>Kilpatrick, *Op. Cit.*

<sup>186</sup>Hoskier, *Codex B*, Vol. I. I fail to see how anyone can read this work of Hoskier's with attention and still retain a high opinion of Codices B and Aleph.

<sup>187</sup>Elliott, pp. 241-43.

<sup>188</sup>Kilpatrick, p. 205.

must be held to be fatal to the hypothesis that it *did*. It is simply incredible that an incident of such magnitude and interest would leave no trace of itself in history.<sup>189</sup>

It will not do for someone to say that the argument from silence proves nothing. In a matter of this "magnitude and interest" it is conclusive. Kenyon, also, found this part of Hort's theory to be gratuitous.

The absence of evidence points the other way; for it would be very strange, if Lucian had really edited both Testaments, that only his work on the Old Testament should be mentioned in after times. The same argument tells against any theory of a deliberate revision at any definite moment. We know the names of several revisers of the Septuagint and the Vulgate, and it would be strange if historians and Church writers had all omitted to record or mention such an event as the deliberate revision of the New Testament in its original Greek.<sup>190</sup>

Colwell is blunt: "The Greek Vulgate—the Byzantine or Alpha text-type—had in its origin no such single focus as the Latin had in Jerome."<sup>191</sup> F.C. Grant is prepared to look into the second century for the origin of the "Byzantine" text-type.<sup>192</sup> Jacob Geerlings, who has done extensive work on certain branches of the "Byzantine" text-type, affirms concerning it: "Its origins as well as those of other so-called text-types probably go back to the autographs."<sup>193</sup>

In an effort to save Hort's conclusions, seemingly, Kenyon sought to attribute the "Byzantine" text to a "tendency."

It seems probable, therefore, that the Syrian revision was rather the result of a tendency spread over a considerable period of time than of a definite and authoritative revision or revisions, such as produced our English Authorised and Revised Versions. We have only to suppose the principle to be established in Christian circles in and about Antioch that in the case of divergent readings being found in the texts copied, it was better to combine both than to omit either, and that obscurities and roughnesses of diction should be smoothed away as much as possible.<sup>194</sup>

But what if we choose not "to suppose" anything, but rather to insist upon evidence? We have already seen from Hutton's *Atlas* that for every instance that the "Syrian" text possibly combines divergent readings there are a hundred where it does not. What sort of a "tendency" is that? To insist that a variety of scribes separated by time and space and working independently, but all feeling a responsibility to apply their critical faculties to the text, should produce a uniformity of text such as is exhibited within the "Byzantine" text seems to be asking a bit much, both of them and of us. Hodges agrees.

It will be noted in this discussion that in place of the former idea of a specific revision as the source-point for the Majority text, some critics now wish to posit the idea of a "process" drawn out over a long period of time. It may be confidently predicted, however, that this explanation of the Majority text must likewise eventually collapse. The Majority text, it must be remembered, is relatively uniform in its general character with comparatively low amounts of variation between its major representatives. No one has yet explained how a long, slow process spread out over many centuries as well as over a wide geographical area, and involving a multitude of copyists, who often knew nothing of the state of the text outside of their own monasteries or scriptoria, could achieve this widespread uniformity out of the diversity presented by the earlier forms of text. Even an official edition of the New Testament—promoted with ecclesiastical sanction throughout the known world—would have had great difficulty achieving this result as the history of Jerome's Vulgate amply demonstrates. But an unguided process achieving relative stability and uniformity in the diversified textual, historical,

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<sup>189</sup>Burgon, *The Revision Revised*, p. 293.

<sup>190</sup>Kenyon, *Handbook*, pp. 324-25.

<sup>191</sup>Colwell, "The Origin of the Texttypes," p. 137.

<sup>192</sup>F.C. Grant, "The Citation of Greek Manuscript Evidence in an Apparatus Criticus," *New Testament Manuscript Studies*, ed. M.M. Parvis and A.P. Wikgren (Chicago: The University of Chicago Press, 1950), pp. 90-91.

<sup>193</sup>J. Geerlings, *Family E and Its Allies in Mark* (Salt Lake City: University of Utah Press, 1967), p. 1.

<sup>194</sup>Kenyon, *Handbook*, p. 325.

and cultural circumstances in which the New Testament was copied, imposes impossible strains on our imagination.<sup>195</sup>

An ordinary process of textual transmission results in divergence, not convergence. Uniformity of text is usually greatest near the source and diminishes in transmission.

The accumulating evidence seems not to bother Metzger. He still affirmed in 1968 that the "Byzantine" text is based on a recension prepared by Lucian.<sup>196</sup> There is an added problem with that view.

Lucian was an Arian, a vocal one. Does Metzger seriously invite us to believe that the victorious Athanasians embraced an Arian revision of the Greek New Testament?

As to the Syriac Peshitta, again Burgon protested the complete lack of evidence for Hort's assertions.<sup>197</sup> A. Vööbus says of Burkitt's effort:

Burkitt has tried to picture the lifespan of Bishop Rabbula as a decisive period in the development of the New Testament text in the Syrian church.

Regardless of the general acceptance of the axiom, established by him, that "the authority of Rabbula secured an instant success for the new revised version . . ." and that "copies of the Peshitta were rapidly multiplied, it soon became the only text in ecclesiastical use"—this kind of reconstruction of textual history is pure fiction without a shred of evidence to support it.<sup>198</sup>

Vööbus finds that Rabbula himself used the Old Syriac type of text. His researches show clearly that the Peshitta goes back at least to the mid-fourth century and that it was not the result of an authoritative revision.<sup>199</sup>

Here again there is an added historical difficulty.

The Peshitta is regarded as authoritative Scripture by both the Nestorians and the Monophysites. It is hard to see how this could have come to pass on the hypothesis that Rabbula was the author and chief promoter of the Peshitta. For Rabbula was a decided Monophysite and a determined opponent of the Nestorians. It is almost contrary to reason, therefore, to suppose that the Nestorian Christians would adopt so quickly and so unanimously the handiwork of their greatest adversary.<sup>200</sup>

It is hard to understand how men like F.F. Bruce, E.C. Colwell, F.G. Kenyon, etc. could allow themselves to state dogmatically that Rabbula produced the Peshitta.

## Conclusion

And that completes our review of the W-H critical theory. It is evidently erroneous at every point. Our conclusions concerning the theory of necessity apply also to any Greek text constructed on the basis of it, as well as to those versions based upon such texts (and to commentaries based upon them).

<sup>195</sup>Hodges, "A Defense of the Majority Text," p. 42. For a further discussion of the problems confronting the "process" view see the section headed "**Objections**" in Appendix C.

<sup>196</sup>Metzger, *The Text*, (2nd ed., 1968), p. 212. In 1972 he wrote "Whether it really was Lucian . . .," so he may now be retreating from that position. "Patristic Evidence and the Textual Criticism of the New Testament," *New Testament Studies*, XVIII (1972), p. 385.

<sup>197</sup>Burgon, *The Revision Revised*, pp. 276-77.

<sup>198</sup>A. Vööbus, *Early Versions of the New Testament* (Stockholm: Estonian Theological Society in Exile, 1954), p. 100.

<sup>199</sup>*Ibid.*, pp. 100-102. Carson chides me for failing to mention "Matthew Black's decisive critique of Vööbus" (p. 112). Well, Metzger evidently does not regard it to be "decisive". "The question who it was that produced the Peshitta version of the N. T. will perhaps never be answered. That it was not Rabbula has been proved by Vööbus's researches" (*Early Versions of the New Testament* [Oxford: Clarendon Press, 1977], pp. 57-61).

<sup>200</sup>Burgon, *The Last Twelve Verses*, p. 56. Metzger recognizes the force of this circumstance (*Loc. Cit.*).

K.W. Clark says of the W-H text: "The textual history postulated for the *textus receptus* which we now trust has been exploded."<sup>201</sup> Epp confesses that "we simply do not have a theory of the text."<sup>202</sup> The point is that "the establishment of the NT text can be achieved only by a reconstruction of the history of that early text. . . ."<sup>203</sup> Colwell agrees: "Without a knowledge of the history of the text, the original reading cannot be established."<sup>204</sup>

In Aland's words, "Now as in the past, textual criticism without a history of the text is not possible."<sup>205</sup> Or as Hort himself put it, "ALL TRUSTWORTHY RESTORATION OF CORRUPTED TEXTS IS FOUNDED ON THE STUDY OF THEIR HISTORY."<sup>206</sup>

As already noted, one of the fundamental deficiencies of the eclectic method is that it ignores the history of the text. Hort did not ignore it, but what are we to say of his "clear and firm view"<sup>207</sup> of it? What Clark says is:

The textual history that the Westcott-Hort text represents is no longer tenable in the light of newer discoveries and fuller textual analysis. In the effort to construct a congruent history, our failure suggests that we have lost the way, that we have reached a dead end, and that only a new and different insight will enable us to break through.<sup>208</sup>

(The evidence before us indicates that Hort's history never was tenable.)

The crucial question remains--*what sort of a history does the evidence reflect?* The identity of the New Testament text, our recognition of it, hinges upon our answer!

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<sup>201</sup>Clark, "Today's Problems," p. 162.

<sup>202</sup>Epp, p. 403.

<sup>203</sup>*Ibid.*, p. 401.

<sup>204</sup>Colwell, "The Greek New Testament with a Limited Apparatus," p. 37. This theme pervades his "Hort Redivivus."

<sup>205</sup>Aland, "The Present Position," p. 731.

<sup>206</sup>Westcott and Hort, p. 40.

<sup>207</sup>Epp, "Interlude," pp. 391-92.

<sup>208</sup>Clark, "Today's Problems," p. 161.

## 5

# THE HISTORY OF THE TEXT

The logical place to start is with the possibility that the process of transmission of the text was normal.

Under normal circumstances the older a text is than its rivals, the greater are its chances to survive in a plurality or a majority of the texts extant at any subsequent period. But the **oldest** text of all is the autograph. Thus it ought to be taken for granted that, barring some radical dislocation in the history of transmission, a majority of texts will be far more likely to represent correctly the character of the original than a small minority of texts. This is especially true when the ratio is an overwhelming 8:2. Under any reasonably normal transmissional conditions, it would be . . . quite impossible for a later text-form to secure so one-sided a preponderance of extant witnesses.<sup>1</sup>

But were the transmissional conditions reasonably normal?

## Were the N.T. Writings Recognized?

Naturalistic critics like to assume that the New Testament writings were not recognized as Scripture when they first appeared and thus through the consequent carelessness in transcription the text was confused and the original wording "lost" (in the sense that no one knew for sure what it was) at the very start. Thus Colwell says: "Most of the manuals and handbooks now in print (including mine!) will tell you that these variations were the fruit of careless treatment which was possible because the books of the New Testament had not yet attained a strong position as 'Bible'."<sup>2</sup>

And Hort had said:

Textual purity, as far as can be judged from the extant literature, attracted hardly any interest. There is no evidence to show that care was generally taken to choose out for transcription the exemplars having the highest claims to be regarded as authentic, if indeed the requisite knowledge and skill were forthcoming.<sup>3</sup>

Rather than take Hort's word for it, prudence calls for a review of the premises. The place to start is at the beginning, when the apostles were still penning the Autographs.

## The apostolic period

It is clear that the apostle Paul, at least, considered his writings to be authoritative—see 1 Cor. 14:37, Gal. 1:6-12, Col. 1:25-6, 1 Thess. 2:13, 2 Thess. 2:15 and 3:6-14. And it is reasonable to infer from Col. 4:16 and 1 Thess. 5:27 that he expected his writings to have a wider audience than just the particular church addressed. In fact, in Galatians 1:2 he addresses "the churches of Galatia." John also is plain enough—Rev. 1:1-3 and 21:5. Both Paul (Rom. 16:25-6, Eph. 3:4-5) and Peter (1 Pet. 1:12, 25; 2 Pet. 3:2) declare that a number of people are writing Scripture in their day, presumably including themselves. I take it that in 1:3 Luke claims divine authority—"having faithfully followed all things from above."<sup>4</sup>

In 1 Tim. 5:18 Paul puts the Gospel of Luke (10:7) on the same level as Deuteronomy (25:4), calling them both "Scripture." Taking the traditional and conservative point of view, 1 Timothy is generally

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<sup>1</sup>Z.C. Hodges, "A Defense of the Majority Text" (unpublished course notes, Dallas Theological Seminary, 1975), p. 4.

<sup>2</sup>Colwell, *What is the Best New Testament?*, p. 53.

<sup>3</sup>Westcott and Hort, p. 9. Cf. p. 7. It is clear that Hort regarded the "extant literature" as representative of the textual picture in the early centuries. This gratuitous and misleading idea continues to be an important factor in the thinking of some scholars today.

<sup>4</sup>The normal, basic meaning of an ω qen is "from up/above"; since that meaning fits here perfectly well I see no reason to posit a different meaning

thought to have been written within five years after Luke.<sup>5</sup> Luke was recognized and declared by apostolic authority to be Scripture as soon as it came off the press, so to speak.

In 2 Pet. 3:15-16, Peter puts the Epistles of Paul on the same level as "the other Scriptures." Although some had been out for perhaps fifteen years, the ink was scarcely dry on others, and perhaps 2 Timothy had not yet been penned when Peter wrote. Paul's writings were recognized and declared by apostolic authority to be Scripture as soon as they appeared.

Clement of Rome, whose first letter to the Corinthians is usually dated about A.D. 96, made liberal use of Scripture, appealing to its authority, and used New Testament material right alongside Old Testament material. Clement quoted Ps. 118:18 and Heb. 12:6 side by side as "the holy word" (56:3-4).<sup>6</sup> He ascribes 1 Corinthians to "the blessed Paul the apostle" and says of it, "with true inspiration he wrote to you" (47:1-3). He clearly quotes from Hebrews, 1 Corinthians and Romans and possibly from Matthew, Acts, Titus, James and 1 Peter. Here is the bishop of Rome, before the close of the first century, writing an official letter to the church at Corinth wherein a selection of New Testament books are recognized and declared by episcopal authority to be Scripture, including Hebrews.

The Epistle of Barnabas, variously dated from A.D. 70 to 135, says in 4:14, "let us be careful lest, as it is written, it should be found with us that 'many are called but few chosen.'" The reference seems to be to Matt. 22:14 (or 20:16) and the phrase "as it is written" may fairly be taken as a technical expression referring to Scripture. In 5:9 there is a quote from Matt. 9:13 (or Mark 2:17 or Luke 5:32). In 13:7 there is a loose quote from Rom. 4:11-12, which words are put in God's mouth. Similarly, in 15:4 we find: "Note, children, what 'he ended in six days' means. It means this: that the Lord will make an end of everything in six thousand years, for a day with Him means a thousand years. And He Himself is my witness, saying: 'Behold, the day of the Lord shall be as a thousand years'."<sup>7</sup>

The author, whoever he was, is clearly claiming divine authorship for this quote which appears to be from 2 Pet. 3:8.<sup>8</sup> In other words, 2 Peter is here regarded to be Scripture, as well as Matthew and Romans. Barnabas also has possible allusions to 1 and 2 Corinthians, Ephesians, Colossians, 1 and 2 Timothy, Titus, Hebrews, and 1 Peter.

## The second century

The seven letters of Ignatius (c. A.D. 110) contain probable allusions to Matthew, John, Romans, 1 Corinthians and Ephesians (in his own letter to the Ephesians Ignatius says they are mentioned in "all the epistles of Paul"—a bit of hyperbole, but he was clearly aware of a Pauline corpus), and possible allusions to Luke, Acts, Galatians, Philippians, Colossians, 1 Thessalonians, 1 and 2 Timothy, and Titus, but very few are clear quotations and even they are not identified as such.

Polycarp, writing to the Philippian church (c. 115 A.D.?) weaves an almost continuous string of clear quotations and allusions to New Testament writings. His heavy use of Scripture is reminiscent of Clement of Rome; however, Clement used mostly the Old Testament while Polycarp usually used the New. There are perhaps fifty clear quotations taken from Matthew, Luke, Acts, Romans, 1 and 2 Corinthians, Galatians, Ephesians, Philippians, Colossians, 1 and 2 Thessalonians, 1 and 2 Timothy, 1 and 2 Peter, and 1 John, and many allusions including to Mark, Hebrews, James, and 2 and 3 John. (The only NT writer not included is Jude!)

<sup>5</sup>For a statement of my presuppositions see Appendix A.

<sup>6</sup>I am aware that it could be Prov. 3:12 (LXX) rather than Heb 12:6. Clement quotes from both books repeatedly throughout the letter, so they are equal candidates on that score. But, Clement agrees verbatim with Hebrews while Proverbs (LXX) differs in one important word. Further, the main point of Clement's chapter 56 is that correction is to be received graciously and as from the Lord, which is also the point of Heb. 12:3-11. Since Clement evidently had both books in front of him (in the next chapter he quotes nine consecutive verses, Prov. 1:23-31) the verbatim agreement with Hebrews is significant. If he deliberately chose the wording of Hebrews over that of Proverbs, what might that imply about their rank?

<sup>7</sup>I have used the translation done by Francis Glimm in *The Apostolic Fathers* (New York: Cima Publishing Co., Inc., 1947), belonging to the set, *The Fathers of the Church*, ed. Ludwig Schopp.

<sup>8</sup>J.V. Bartlet says of the formulae of citation used in Barnabas to introduce quotations from Scripture, "the general result is an absolute doctrine of inspiration," but he is unwilling to consider that II Peter is being used. Oxford Society of Historical Research, *The New Testament in the Apostolic Fathers* (Oxford: Clarendon Press, 1905), pp. 2, 15.



His attitude toward the New Testament writings is clear from 12:1: "I am sure that you are well trained in the sacred Scriptures, . . . Now, as it is said in these Scriptures: 'Be angry and sin not,' and 'Let not the sun go down upon your wrath.' Blessed is he who remembers this."<sup>9</sup>

Both parts of the quotation could come from Eph. 4:26 but since Polycarp split it up he may have been referring to Ps. 4:5 (LXX) in the first half. In either case he is declaring Ephesians to be "sacred Scripture." A further insight into his attitude is found in 3:1-2.

Brethren, I write you this concerning righteousness, not on my own initiative, but because you first invited me. For neither I, nor anyone like me, is able to rival the wisdom of the blessed and glorious Paul, who, when living among you, carefully and steadfastly taught the word of truth face to face with his contemporaries and, when he was absent, wrote you letters. By the careful perusal of his letters you will be able to strengthen yourselves in the faith given to you, "which is the mother of us all," . . .<sup>10</sup>

(This from one who was perhaps the most respected bishop in Asia Minor, in his day. He was martyred in A.D. 156.)

The so-called second letter of Clement of Rome is usually dated before A.D. 150 and seems clearly to quote from Matthew, Mark, Luke, Acts, 1 Corinthians, Ephesians, 1 Timothy, Hebrews, James, and 1 Peter, with possible allusions to 2 Peter, Jude, and Revelation. After quoting and discussing a passage from the Old Testament, the author goes on to say in 2:4, "Another Scripture says: 'I came not to call the just, but sinners'" (Matt. 9:13; Mark 2:17; Luke 5:32). Here is another author who recognized the New Testament writings to be Scripture.

Two other early works, the *Didache* and the letter to Diognetus, employ New Testament writings as being authoritative but without expressly calling them Scripture.

The *Didache* apparently quotes from Matthew, Luke, 1 Corinthians, Hebrews, and 1 Peter and has possible allusions to Acts, Romans, Ephesians, 1 and 2 Thessalonians and Revelation.

The letter to Diognetus quotes from Acts, 1 and 2 Corinthians while alluding to Mark, John, Romans, Ephesians, Philippians, 1 Timothy, Titus, 1 Peter and 1 John.

Another early work—the Shepherd of Hermas—widely used in the second and third centuries, has fairly clear allusions to Matthew, Mark, 1 Corinthians, Ephesians, Hebrews, and especially James.

From around the middle of the second century fairly extensive works by Justin Martyr (martyred in 165) have come down to us. His "Dialogue with Trypho" shows a masterful knowledge of the Old Testament to which he assigns the highest possible authority, evidently holding to a dictation view of inspiration—in *Trypho* 34 he says, "to persuade you that you have not understood anything of the Scriptures, I will remind you of another psalm, dictated to David by the Holy Spirit."<sup>11</sup> The whole point of *Trypho* is to prove that Jesus is Christ and God and therefore what He said and commanded was of highest authority.

In *Apol.* i.66 Justin says, "For the apostles in the memoirs composed by them, which are called Gospels, thus handed down what was commanded them. . . ." <sup>12</sup> And in *Trypho* 119 he says that just as Abraham believed the voice of God, "in like manner we, having believed God's voice spoken by the apostles of Christ. . . ."

It also seems clear from *Trypho* 120 that Justin considered New Testament writings to be Scripture. Of considerable interest is an unequivocal reference to the book of Revelation in *Trypho* 81. "And further, there was a certain man with us whose name was John, one of the apostles of Christ, who

<sup>9</sup>See footnote 6.

<sup>10</sup>See footnote 6.

<sup>11</sup>I have used the translation in Vol. I of *The Ante-Nicene Fathers*, ed., A. Roberts and J. Donaldson (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1956).

<sup>12</sup>I have used the translation by E.R. Hardy in *Early Christian Fathers*, ed., C.C. Richardson (Philadelphia: The Westminster Press, 1953).

prophesied, by a revelation that was made to him, that those who believe in our Christ would dwell a thousand years in Jerusalem."<sup>13</sup>

Justin goes right on to say, "Just as our Lord also said," and quotes Luke 20:35, so evidently he considered Revelation to be authoritative. (While on the subject of Revelation, in 165 Melito, Bishop of Sardis, wrote a commentary on the book.)

A most instructive passage occurs in *Apol.* i.67.

And on the day called Sunday there is a meeting in one place of those who live in cities or the country, and the memoirs of the apostles or the writings of the prophets are read as long as time permits. When the reader has finished, the president in a discourse urges and invites us to the imitation of these noble things.<sup>14</sup>

Whether or not the order suggests that the Gospels were preferred to the Prophets, it is clear that they both were considered to be authoritative and equally enjoined upon the hearers. Notice further that each assembly must have had its own copy of the apostles' writings to read from and that such reading took place every week.

Athenagorus, in his "Plea," written in early 177, quotes Matt. 5:28 as Scripture: ". . . we are not even allowed to indulge in a lustful glance. For, says the Scripture, 'He who looks at a woman lustfully, has already committed adultery in his heart'" (32).<sup>15</sup> He similarly treats Matt. 19:9, or Mark 10:11, in 33.

Theophilus, bishop of Antioch, in his treatise to Autolytus, quotes 1 Tim. 2:1 and Rom. 13:7 as "the Divine Word" (iii.14), quotes from the fourth Gospel, saying that John was "inspired by the Spirit" (ii.22); Isaiah and "the Gospel" are mentioned in one paragraph as Scripture (iii.14), and he insists in several passages that the writers never contradicted each other: "The statements of the Prophets and of the Gospels are found to be consistent, because all were inspired by the one Spirit of God" (ii.9; ii.35; iii.17).<sup>16</sup>

The surviving writings of Irenaeus (died in 202), his major work *Against Heretics* being written about 185, are about equal in volume to those of all the preceding Fathers put together.

His testimony to the authority and inspiration of Holy Scripture is clear and unequivocal. It pervades the whole of his writings; and this testimony is more than ordinarily valuable because it must be regarded as directly representing three churches at least, those of Lyons, Asia Minor, and Rome. The authoritative use of both Testaments is clearly laid down.<sup>17</sup>

Irenaeus stated that the apostles taught that God is the Author of both Testaments (*Against Heretics* IV. 32.2) and evidently considered the New Testament writings to form a second Canon. He quoted from every chapter of Matthew, 1 Corinthians, Galatians, Ephesians, Colossians and Philipians, from all but one or two chapters of Luke, John, Romans, 2 Thessalonians, 1 and 2 Timothy, and Titus, from most chapters of Mark (including the last twelve verses), Acts, 2 Corinthians, and Revelation, and from every other book except Philemon and 3 John. These two books are so short that Irenaeus may not have had occasion to refer to them in his extant works—it does not necessarily follow that he was ignorant of them or rejected them. Evidently the dimensions of the New Testament Canon recognized by Irenaeus are very close to what we hold today.

From the time of Irenaeus on there can be no doubt concerning the attitude of the Church toward the New Testament writings—they are Scripture. Tertullian (in 208) said of the church at Rome, "the law

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<sup>13</sup>See footnote 10.

<sup>14</sup>See footnote 11. His careful study of the early Christian literary papyri has led C.H. Roberts to conclude: "This points to the careful and regular use of the scriptures by the local communities" (*Manuscript, Society and Belief in Early Christian Egypt* [London: Oxford Univ. Press, 1979], p. 25). He also infers from P. Oxy. iii. 405 that a copy of Irenaeus' *Adversus Haereses*, written in Lyons, was brought to Oxyrhynchus within a very few years after it was written (*Ibid.*, pp. 23, 53), eloquent testimony to the extent of the traffic among the early churches.

<sup>15</sup>See footnote 11, except that Richardson is the translator here.

<sup>16</sup>Taken from G.D. Barry, *The Inspiration and Authority of Holy Scripture* (New York: The McMillan Company, 1919), p. 52.

<sup>17</sup>*Ibid.*, p. 53.

and the prophets she unites in one volume with the writings of evangelists and apostles" (*Prescription against Heretics*, 36).

## Were Early Christians Careful?

It has been widely affirmed that the early Christians were either unconcerned or unable to watch over the purity of the text. (Recall Hort's words given above.) Again a review of the premises is called for. Many of the first believers had been devout Jews who had an ingrained reverence and care for the Old Testament Scriptures which extended to the very jots and tittles. This reverence and care would naturally be extended to the New Testament Scriptures.

Why should modern critics assume that the early Christians, in particular the spiritual leaders among them, were inferior in integrity or intelligence? A Father's quoting from memory or tailoring a passage to suit his purpose in sermon or letter by no means implies that he would take similar liberties when transcribing a book or corpus. Ordinary honesty would require him to produce a faithful copy. Are we to assume that everyone who made copies of New Testament books in those early years was a knave, or a fool? Paul was certainly as intelligent a man as any of us. If Hebrews was written by someone else, here was another man of high spiritual insight and intellectual power. There was Barnabas and Apollos and Clement and Polycarp, etc., etc. The Church has had men of reason and intelligence all down through the years. Starting out with what they **knew** to be the pure text, the earliest Fathers did not need to be textual critics. They had only to be reasonably honest and careful. But is there not good reason to believe they would be **especially** watchful and careful?

### The apostles

Not only did the apostles themselves declare the New Testament writings to be Scripture, which would elicit reverence and care in their treatment, they expressly warned the believers to be on their guard against false teachers—see Acts 20:27-32, Gal. 1:6-12, 2 Tim. 3:1-4:4, 2 Pet. 2:1-2, 1 John 2:18-19, 2 John 7-11, Jude 3-4, 16-19. Peter's statement concerning the "twisting" Paul's words were receiving (2 Pet. 3:16) suggests there was awareness and concern as to the text and the way it was being handled. I recognize that the Apostles were focusing on the interpretation rather than the copying of the text, and yet, since any alteration of the text may result in a different interpretation we may reasonably infer that their concern for the truth would include the faithful transmission of the text. Indeed, we could scarcely ask for a clearer expression of this concern than that given in Rev. 22:18-19. 2 Thess. 2:2 is evidently concerned with authenticity.

### The early Fathers

The early Fathers furnish a few helpful clues as to the state of affairs. The letters of Ignatius contain several references to a considerable traffic between the churches (of Asia Minor, Greece, Rome) by way of messengers (often official), which seems to indicate a deep sense of solidarity binding them together, and a wide circulation of news and attitudes—a problem with a heretic in one place would soon be known all over, etc. That there was strong feeling about the integrity of the Scriptures is made clear by Polycarp (7:1), "Whoever perverts the sayings of the Lord . . . that one is the firstborn of Satan." Present-day critics may not like Polycarp's terminology, but for him to use such strong language makes clear that he was not merely aware and concerned; he was exercised.

Similarly, Justin Martyr says (*Apol.* i.58), "the wicked demons have also put forward Marcion of Pontus." And in *Trypho* xxxv he says of heretics teaching doctrines of the spirits of error, that fact "causes us who are disciples of the true and pure doctrine of Jesus Christ to be more faithful and steadfast in the hope announced by Him."

It seems obvious that heretical activity would have precisely the effect of putting the faithful on their guard and forcing them to define in their own minds what they were going to defend. Thus Marcion's truncated canon evidently stirred the faithful to define the true canon. But Marcion also altered the wording of Luke and Paul's Epistles, and by their bitter complaints it is clear that the faithful were both aware and concerned. We may note in passing that the heretical activity also furnishes backhanded

evidence that the New Testament writings were regarded as Scripture—why bother falsifying them if they had no authority?

Dionysius, Bishop of Corinth (168-176), complained that his own letters had been tampered with, and worse yet the Holy Scriptures also.

And they insisted that they had received a pure tradition. Thus Irenaeus said that the doctrine of the apostles had been handed down by the succession of bishops, being guarded and preserved, without any forging of the Scriptures, allowing neither addition nor curtailment, involving public reading without falsification (*Against Heretics* IV. 32:8).

Tertullian, also, says of his right to the New Testament Scriptures, "I hold sure title-deeds from the original owners themselves . . . I am the heir of the apostles. Just as they carefully prepared their will and testament, and committed it to a trust . . . even so I hold it."<sup>18</sup>

## Irenaeus

In order to ensure accuracy in transcription, authors would sometimes add at the close of their literary works an adjuration directed to future copyists. So, for example, Irenaeus attached to the close of his treatise *On the Ogdoad* the following note: "I adjure you who shall copy out this book, by our Lord Jesus Christ and by his glorious advent when he comes to judge the living and the dead, that you compare what you transcribe, and correct it carefully against this manuscript from which you copy; and also that you transcribe this adjuration and insert it in the copy."<sup>19</sup>

If Irenaeus took such extreme precautions for the accurate transmission of his own work, how much more would he be concerned for the accurate copying of the Word of God? In fact, he demonstrates his concern for the accuracy of the text by defending the traditional reading of a **single letter**. The question is whether John the Apostle wrote  $\chi\zeta\zeta'$  (666) or  $\chi\iota\zeta'$  (616) in Rev. 13:18. Irenaeus asserts that 666 is found "in all the most approved and ancient copies" and that "those men who saw John face to face" bear witness to it. And he warns those who made the change (of a single letter) that "there shall be no light punishment upon him who either adds or subtracts anything from the Scripture" (xxx.1). Presumably Irenaeus is applying Rev. 22:18-19.

Considering Polycarp's intimacy with John, his personal copy of Revelation would most probably have been taken from the Autograph. And considering Irenaeus' veneration for Polycarp his personal copy of Revelation was probably taken from Polycarp's. Although Irenaeus evidently was no longer able to refer to the Autograph (not ninety years after it was written!) he was clearly in a position to identify a faithful copy and to declare with certainty the original reading—this in 186 A.D. Which brings us to Tertullian.

## Tertullian

Around the year 208 he urged the heretics to

run over the apostolic churches, in which the very thrones of the apostles are still pre-eminent in their places, in which their own authentic writings (*authenticae*) are read, uttering the voice and representing the face of each of them severally. Achaia is very near you, (in which) you find Corinth. Since you are not far from Macedonia, you have Philippi; (and there too) you have the Thessalonians. Since you are able to cross to Asia, you get Ephesus. Since, moreover, you are close upon Italy, you have Rome, from which there comes even into our own hands the very authority (of the apostles themselves).<sup>20</sup>

<sup>18</sup>*Prescription against Heretics*, 37. I have used the translation done by Peter Holmes in Vol. III of *The Ante-Nicene Fathers*.

<sup>19</sup>Metzger, *The Text*, p. 21.

<sup>20</sup>*Prescription against Heretics*, 36, using Holmes' translation.

Some have thought that Tertullian was claiming that Paul's Autographs were still being read in his day (208), but at the very least he must mean they were using faithful copies. Was anything else to be expected? For example, when the Ephesian Christians saw the Autograph of Paul's letter to them getting tattered, would they not carefully execute an identical copy for their continued use? Would they let the Autograph perish without making such a copy? (There must have been a constant stream of people coming either to make copies of their letter or to verify the correct reading.) I believe we are obliged to conclude that in the year 200 the Ephesian Church was still in a position to attest the original wording of her letter (and so for the others)—but this is coeval with P<sup>46</sup>, P<sup>66</sup> and P<sup>75</sup>!

Both Justin Martyr and Irenaeus claimed that the Church was spread throughout the whole earth, in their day—remember that Irenaeus, in 177, became bishop of Lyons, in **Gaul**, and he was not the first bishop in that area. Coupling this information with Justin's statement that the memoirs of the apostles were read each Sunday in the assemblies, it becomes clear that there must have been thousands of copies of the New Testament writings in use by 200 A.D. Each assembly would need a copy to read from, and there must have been private copies among those who could afford them.

We have objective historical evidence in support of the following propositions:

- The true text was never "lost".
- In A.D. 200 the exact original wording of the several books could still be verified and attested.
- There was therefore no need to practice textual criticism and any such effort would be spurious.

However, presumably some areas would be in a better position to protect and transmit the true text than others.

## Who Was Best Qualified?

What factors would be important for guaranteeing, or at least facilitating, a faithful transmission of the text of the N.T. writings? I submit that there are four controlling factors: access to the Autographs, proficiency in the source language, the strength of the Church and an appropriate attitude toward the Text.

### Access to the Autographs

This criterion probably applied for less than a hundred years (the Autographs were presumably worn to a frazzle in that space of time) but it is highly significant to a proper understanding of the history of the transmission of the Text. Already by the year 100 there must have been many copies of the various books (some more than others) while it was certainly still possible to check a copy against the original, should a question arise. The point is that there was a swelling stream of faithfully executed copies emanating from the holders of the Autographs to the rest of the Christian world. In those early years the producers of copies would know that the true wording could be verified, which would discourage them from taking liberties with the text.

However, distance would presumably be a factor—for someone in north Africa to consult the Autograph of Ephesians would be an expensive proposition, in both time and money. I believe we may reasonably conclude that in general the quality of copies would be highest in the area surrounding the Autograph and would gradually deteriorate as the distance increased. Important geographical barriers would accentuate the tendency.

So who held the Autographs? Speaking in terms of regions, Asia Minor may be safely said to have had twelve (John, Galatians, Ephesians, Colossians, 1 and 2 Timothy, Philemon, 1 Peter, 1 and 2 and 3 John, and Revelation), Greece may be safely said to have had six (1 and 2 Corinthians, Philippians, 1 and 2 Thessalonians, and Titus in Crete), Rome may be safely said to have had two (Mark and Romans)—as to the rest, Luke, Acts, and 2 Peter were probably held by either Asia Minor or Rome; Matthew and James by either Asia Minor or Palestine; Hebrews by Rome or Palestine; while it is hard to state even a probability for Jude it was quite possibly held by Asia Minor. Taking Asia Minor and Greece together, the Aegean area held the Autographs of at least eighteen (two-thirds of the total) and possibly as many as twenty-four of the twenty-seven New Testament books; Rome held at least two and possibly up

to seven; Palestine may have held up to three (but in A.D. 70 they would have been sent away for safe keeping, quite possibly to Antioch); Alexandria (Egypt) held **none**. The Aegean region clearly had the best start, and Alexandria the worst—the text in Egypt could only be second hand, at best. On the face of it, we may reasonably assume that in the earliest period of the transmission of the N.T. Text the most reliable copies would be circulating in the region that held the Autographs. Recalling the discussion of Tertullian above, I believe we may reasonably extend this conclusion to A.D. 200 and beyond. So, in the year 200 someone looking for the best text of the N.T. would presumably go to the Aegean area; certainly not to Egypt.

### Proficiency in the source language

As a linguist (PhD) and one who has dabbled in the Bible translation process for some years, I affirm that a 'perfect' translation is impossible. (Indeed, a tolerably reasonable approximation is often difficult enough to achieve.) It follows that any divine solicitude for the precise form of the NT Text would have to be mediated through the language of the Autographs—Greek. Evidently ancient Versions (Syriac, Latin, Coptic) may cast a clear vote with reference to major variants, but precision is possible only in Greek (in the case of the N.T.). That by way of background, but our main concern here is with the copyists.

To copy a text by hand in a language you do not understand is a tedious exercise—it is almost impossible to produce a perfect copy (try it and see!). You virtually have to copy letter by letter and constantly check your place. (It is even more difficult if there is no space between words and no punctuation, as was the case with the N.T. Text in the early centuries.) But if you cannot understand the text it is very difficult to remain alert. Consider the case of P<sup>66</sup>. This papyrus manuscript is perhaps the oldest (c. 200) extant N.T. manuscript of any size (it contains most of John). It is one of the worst copies we have. It has an average of roughly two mistakes per verse—many being obvious mistakes, stupid mistakes, nonsensical mistakes. From the pattern of mistakes it is clear that the scribe copied syllable by syllable. I have no qualms in affirming that the person who produced P<sup>66</sup> did not know Greek. Had he understood the text he would not have made the number and sort of mistakes that he did.

Now consider the problem from God's point of view. To whom should He entrust the primary responsibility for the faithful transmission of the N.T. Text? If the Holy Spirit is going to take an active part in the process, where should He concentrate His efforts? Presumably fluent speakers of Greek would have the inside track, and areas where Greek would continue in active use would be preferred. For a faithful transmission to occur the copyists had to be proficient in Greek, and over the long haul. So where was Greek predominant? Evidently in Greece and Asia Minor; Greek is the mother tongue of Greece to this day (having changed considerably during the intervening centuries, as any living language must). The dominance of Greek in the Aegean area was guaranteed by the Byzantine Empire for many centuries; in fact, until the invention of printing. Constantinople fell to the Ottoman Turks in 1453; the Gutenberg Bible (Latin) was printed just three years later, while the first printed Greek New Testament appeared in 1516. (For those who believe in Providence, I would suggest that here we have a powerful case in point.)

How about Egypt? The use of Greek in Egypt was already declining by the beginning of the Christian era. Bruce Metzger observes that the Hellenized section of the population in Egypt "was only a fraction in comparison with the number of native inhabitants who used only the Egyptian languages."<sup>21</sup> By the third century the decline was evidently well advanced. I have already argued that the copyist who did P<sup>66</sup> (c. 200) did not know Greek. Now consider the case of P<sup>75</sup> (c. 220). E.C. Colwell analyzed P<sup>75</sup> and found about 145 itacisms plus 257 other singular readings, 25% of which are nonsensical. From the pattern of mistakes it is clear that the copyist who did P<sup>75</sup> copied letter by letter!<sup>22</sup> This means that he did not know Greek—when transcribing in a language you know you copy phrase by phrase, or at least word by word. K. Aland argues that before 200 the tide had begun to turn against the use of Greek in the areas

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<sup>21</sup>Metzger, *Early Versions*, p. 104.

<sup>22</sup>Colwell, "Scribal Habits," pp. 374-76, 380.

that spoke Latin, Syriac or Coptic, and fifty years later the changeover to the local languages was well advanced.<sup>23</sup>

Again the Aegean Area is far and away the best qualified to transmit the Text with confidence and integrity. Note that even if Egypt had started out with a good text, already by the end of the 2nd century its competence to transmit the text was steadily deteriorating. In fact the early papyri (they come from Egypt) are demonstrably inferior in quality, taken individually, as well as exhibiting rather different types of text (they disagree among themselves).

### The strength of the Church

This question is relevant to our discussion for two reasons. First, the law of supply and demand operates in the Church as well as elsewhere. Where there are many congregations and believers there will be an increased demand for copies of the Scriptures. Second, a strong, well established church will normally have a confident, experienced leadership—just the sort that would take an interest in the quality of their Scriptures and also be able to do something about it. So in what areas was the early Church strongest?

Although the Church evidently began in Jerusalem, the early persecutions and apostolic activity caused it to spread. The main line of advance seems to have been north into Asia Minor and west into Europe. If the selection of churches to receive the glorified Christ's "letters" (Rev. 2 and 3) is any guide, the center of gravity of the Church seems to have shifted from Palestine to Asia Minor by the end of the first century. (The destruction of Jerusalem by Rome's armies in A.D. 70 would presumably be a contributing factor.) Kurt Aland agrees with Adolf Harnack that "about 180 the greatest concentration of churches was in Asia Minor and along the Aegean coast of Greece." He continues: "The overall impression is that the concentration of Christianity was in the East. . . . Even around A.D. 325 the scene was still largely unchanged. Asia Minor continued to be the heartland of the Church."<sup>24</sup> "The heartland of the Church"—so who else would be in a better position to certify the correct text of the New Testament?

What about Egypt? C.H. Roberts, in a scholarly treatment of the Christian literary papyri of the first three centuries, seems to favor the conclusion that the Alexandrian church was weak and insignificant to the Greek Christian world in the second century.<sup>25</sup> Aland states: "Egypt was distinguished from other provinces of the Church, so far as we can judge, by the early dominance of gnosticism."<sup>26</sup> He further informs us that "at the close of the 2nd century" the Egyptian church was "dominantly gnostic" and then goes on to say: "The copies existing in the gnostic communities could not be used, because they were under suspicion of being corrupt."<sup>27</sup> Now this is all very instructive—what Aland is telling us, in other words, is that up to A.D. 200 the textual tradition in Egypt **could not be trusted**. Aland's assessment here is most probably correct. Notice what Bruce Metzger says about the early church in Egypt:

Among the Christian documents which during the second century either originated in Egypt or circulated there among both the orthodox and the Gnostics are numerous apocryphal gospels, acts, epistles, and apocalypses. . . . There are also fragments of exegetical and dogmatic works composed by Alexandrian Christians, chiefly Gnostics, during the second century. . . . In fact, to judge by the comments made by Clement of Alexandria, almost every deviant Christian sect was represented in Egypt during the second century; Clement mentions the Valentinians, the Basilidians, the Marcionites, the Peratae, the Encratites, the Docetists, the Haimetites, the Cainites, the Ophites, the Simonians, and the Eutychites. What proportion of Christians in Egypt during the second century were orthodox is not known.<sup>28</sup>

<sup>23</sup>K. and B. Aland, *The Text of the New Testament* (Grand Rapids: Eerdmans, 1981), pp. 52-53.

<sup>24</sup>*Ibid.*, p. 53.

<sup>25</sup>Roberts, pp. 42-43, 54-58.

<sup>26</sup>K. and B. Aland, p. 59.

<sup>27</sup>K. Aland, "The Text of the Church?", *Trinity Journal*, 1987, 8NS:138.

<sup>28</sup>Metzger, *Early Versions*, p. 101.

It is almost enough to make one wonder whether Isaiah 30:1-3 might not be a prophecy about N.T. textual criticism!

But we need to pause to reflect on the implications of Aland's statements. He is a champion of the Egyptian ("Alexandrian") text-type, and yet he himself informs us that up to A.D. 200 the textual tradition in Egypt could not be trusted and that by 200 the use of Greek had virtually died out there. So on what basis can he argue that the Egyptian text subsequently became the best? Aland also states that in the 2nd century, 3rd century, and into the 4th century Asia Minor continued to be "the heartland of the Church." This means that the superior qualifications of the Aegean area to protect, transmit and attest the N.T. Text carry over into the 4th century! It happens that Hort, Metzger and Aland (along with many others) have linked the "Byzantine" text-type to Lucian of Antioch, who died in 311. Now really, wouldn't a text produced by a leader in "the heartland of the Church" be better than whatever evolved in Egypt?

### **Attitude toward the Text**

Where careful work is required, the attitude of those to whom the task is entrusted is of the essence. Are they aware? Do they agree? If they do not understand the nature of the task, the quality will probably do down. If they understand but do not agree, they might even resort to sabotage—a damaging eventuality. In the case of the N.T. books we may begin with the question: "Why would copies be made?"

We have seen that the faithful recognized the authority of the N.T. writings from the start, so the making of copies would have begun at once. The authors clearly intended their writings to be circulated, and the quality of the writings was so obvious that the word would get around and each assembly would want a copy. That Clement and Barnabas quote and allude to a variety of N.T. books by the turn of the 1st century makes clear that copies were in circulation. A Pauline corpus was known to Peter before A.D. 70. Polycarp (XIII) c. 115, in answer to a request from the Philippian church, sent a collection of Ignatius' letters to them, possibly within five years after Ignatius wrote them. Evidently it was normal procedure to make copies and collections (of worthy writings) so each assembly could have a set. Ignatius referred to the free travel and exchange between the churches and Justin to the weekly practice of reading the Scriptures in the assemblies (they had to have copies).

A second question would be: "What was the attitude of the copyists toward their work?" We already have the essence of the answer. Being followers of Christ, and believing that they were dealing with Scripture, to a basic honesty would be added reverence in their handling of the Text, from the start. And to these would be added vigilance, since the Apostles had repeatedly and emphatically warned them against false teachers. As the years went by, assuming that the faithful were persons of at least average integrity and intelligence, they would produce careful copies of the manuscripts they had received from the previous generation, persons whom they trusted, being assured that they were transmitting the true text. There would be accidental copying mistakes in their work, but no deliberate changes. It is important to note that the earliest Christians did not need to be textual critics. Starting out with what they knew to be the pure text, they had only to be reasonably honest and careful. I submit that we have good reason for understanding that they were especially watchful and careful—this especially in the early decades.

As time went on regional attitudes developed, not to mention regional politics. The rise of the so-called "school of Antioch" is a relevant consideration. Beginning with Theophilus, a bishop of Antioch who died around 185, the Antiochians began insisting upon the literal interpretation of Scripture. The point is that a literalist is obliged to be concerned about the precise wording of the text since his interpretation or exegesis hinges upon it.

It is reasonable to assume that this "literalist" mentality would have influenced the churches of Asia Minor and Greece and encouraged them in the careful and faithful transmission of the pure text that they had received. For example, the 1,000 MSS of the Syriac Peshitta are unparalleled for their consistency. (By way of contrast, the 8,000 MSS of the Latin Vulgate are remarkable for their extensive discrepancies, and in this they follow the example of the Old Latin MSS.) It is not unreasonable to suppose that the Antiochian antipathy toward the Alexandrian allegorical interpretation of Scripture would rather indispose them to view with favor any competing forms of the text coming out of Egypt. Similarly



the Quarto-deciman controversy with Rome would scarcely enhance the appeal of any innovations coming from the West.

To the extent that the roots of the allegorical approach that flourished in Alexandria during the third century were already present, they would also be a negative factor. Since Philo of Alexandria was at the height of his influence when the first Christians arrived there, it may be that his allegorical interpretation of the O.T. began to rub off on the young church already in the first century. Since an allegorist is going to impose his own ideas on the text anyway, he would presumably have fewer inhibitions about altering it—precise wording would not be a high priority.

The school of literary criticism that existed at Alexandria would also be a negative factor, if it influenced the Church at all, and W.R. Farmer argues that it did. "But there is ample evidence that by the time of Eusebius the Alexandrian text-critical practices were being followed in at least some of the scriptoria where New Testament manuscripts were being produced. Exactly when Alexandrian text-critical principles were first used . . . is not known."<sup>29</sup> He goes on to suggest that the Christian school founded in Alexandria by Pantaenus, around 180, was bound to be influenced by the scholars of the great library of that city. The point is, the principles used in attempting to "restore" the works of Homer would not be appropriate for the NT writings when appeal to the Autographs, or exact copies made from them, was still possible.

## Conclusion

What answer do the "four controlling factors" give to our question? The four speak with united voice: "The Aegean area was the best qualified to protect, transmit and attest the true text of the N.T. writings." This was true in the 2nd century; it was true in the 3rd century; it continued to be true in the 4th century. And now we are ready to answer the question, "Was the transmission normal?", and to attempt to trace the history of the text.

## Was the Transmission Normal?

Was the transmission normal? Yes and no. Assuming the faithful were persons of at least average integrity and intelligence they would produce reasonable copies of the manuscripts they had received from the previous generation, persons whom they trusted, being assured that they were transmitting the true text. There would be accidental copying mistakes in their work, but no deliberate changes. But there were others who expressed an interest in the New Testament writings, persons lacking in integrity, who made their own copies with malicious intent. There would be accidental mistakes in their work too, but also deliberate alteration of the text. I will trace first the normal transmission.

### The normal transmission

We have seen that the faithful recognized the authority of the New Testament writings from the start—had they not they would have been rejecting the authority of the Apostles, and hence not been among the faithful. To a basic honesty would be added reverence in their handling of the text, from the start. And to these would be added vigilance, since the Apostles had repeatedly and emphatically warned them against false teachers.

With an ever-increasing demand and consequent proliferation of copies throughout the Graeco-Roman world and with the potential for verifying copies by having recourse to the centers still possessing the Autographs, the early textual situation was presumably highly favorable to the wide dissemination of MSS in close agreement with the original text. By the early years of the second century the dissemination of such copies can reasonably be expected to have been very widespread, with the logical consequence that the form of text they embodied would early become entrenched throughout the area of their influence.

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<sup>29</sup>W.R. Farmer, *The Last Twelve Verses of Mark* (Cambridge: University Press, 1974), pp. 14-15. He cites B.H. Streeter, *The Four Gospels*, 1924, pp. 111, 122-23.

The considerations just cited are crucial to an adequate understanding of the history of the transmission of the text because they indicate that a basic trend was established at the very beginning—a trend that would continue inexorably until the advent of a printed N.T. text. I say "inexorably" because, given a normal process of transmission, the science of statistical probability demonstrates that a text form in such circumstances could scarcely be dislodged from its dominant position—the probabilities against a competing text form ever achieving a majority attestation would be prohibitive no matter how many generations of MSS there might be. (The demonstration vindicating my assertion is in Appendix C.) It would take an extraordinary upheaval in the transmissional history to give currency to an aberrant text form. We know of no place in history that will accommodate such an upheaval.

The argument from probability would apply to secular writings as well as the New Testament and does not take into account any unusual concern for purity of text. I have argued, however, that the early Christians did have a special concern for their Scriptures and that this concern accompanied the spread of Christianity. Thus Irenaeus clearly took his concern for textual purity (which extended to a single letter) to Gaul and undoubtedly influenced the Christians in that area. The point is that the text form of the N.T. Autographs had a big advantage over that of any secular literature, so that its commanding position would become even greater than the argument from probability would suggest. The rapid multiplication and spread of good copies would raise to absolutely prohibitive levels the chances against an opportunity for aberrant text forms to gain any kind of widespread acceptance or use.<sup>30</sup>

It follows that within a relatively few years after the writing of the N.T. books there came rapidly into existence a "Majority" text whose form was essentially that of the Autographs themselves. This text form would, in the natural course of things, continue to multiply itself and in each succeeding generation of copying would continue to be exhibited in the mass of extant manuscripts. In short, it would have a "normal" transmission.

The use of such designations as "Syrian," "Antiochian," and "Byzantine" for the Majority Text reflects its general association with that region. I know of no reason to doubt that the "Byzantine" text is in fact the form of the text that was known and transmitted in the Aegean area from the beginning.

In sum, I believe that the evidence clearly favors that interpretation of the history of the text which sees the normal transmission of the text as centered in the Aegean region, the area that was best qualified, from every point of view, to transmit the text, from the very first. The result of that normal transmission is the "Byzantine" text-type. In every age, including the second and third centuries, it has been the traditional text.

So then, I claim that the N.T. text had a normal transmission, namely the fully predictable spread and reproduction of reliable copies of the Autographs from the earliest period down through the history of transmission until the availability of printed texts brought copying by hand to an end.

### **The abnormal transmission<sup>31</sup>**

Turning now to the abnormal transmission, it no doubt commenced right along with the normal. The apostolic writings themselves contain strong complaints and warning against heretical and malicious activity. As Christianity spread and began to make an impact on the world, not everyone accepted it as

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<sup>30</sup>I have avoided introducing any argument based on the providence of God because not all accept such argumentation and because the superiority of the Traditional Text can be demonstrated without recourse to it. Thus, I believe the argument from statistical probability given above is valid as it stands. However, while I have not argued on the basis of Providence, I wish the reader to understand that I personally do not think that the preservation of the true text was so mechanistic as the discussion above might suggest. From the evidence previously adduced, it seems clear that a great many variant readings (perhaps most of the malicious ones) that existed in the second century simply have not survived—we have no extant witness to them. We may reasonably conclude that the early Christians were concerned and able watchdogs of the true text. I would like to believe that they were aided and abetted by the Holy Spirit. In that event, the security of the text is considerably greater than that suggested by probability alone, including the proposition that none of the original wording has been lost.

<sup>31</sup>I have been accused of inconsistency in that I criticize W-H for treating the NT like any other book and yet myself claim a "normal transmission" for the Majority Text. The crucial point is that I also recognize an "abnormal transmission," whereas W-H did not. Fee seriously distorts my position by ignoring my discussion of the abnormal transmission ("A Critique," pp. 404-08) and mis-stating my view of the normal transmission (*Ibid.*, p. 399). I hold that 95% of the variants, the obvious transcriptional errors, belong (for the most part) to the normal transmission, whereas most of the remaining 5%, the "significant" variants, belong to the abnormal transmission.

"good news". Opposition of various sorts arose. Also, there came to be divisions within the larger Christian community—in the N.T. itself notice is taken of the beginnings of some of these tangents. In some cases faithfulness to an ideological (theological) position evidently became more important than faithfulness to the N.T. Text. Certain it is that Church Fathers who wrote during the second century complained bitterly about the deliberate alterations to the Text perpetrated by "heretics". Large sections of the extant writings of the early Fathers are precisely and exclusively concerned with combating the heretics. It is clear that during the second century, and possibly already in the first, such persons produced many copies of N.T. writings incorporating their alterations.<sup>32</sup> Some apparently were quite widely circulated, for a time. The result was a welter of variant readings, to confuse the uninformed and mislead the unwary. Such a scenario was totally predictable. If the N.T. is in fact God's Word then both God and Satan must have a lively interest in its fortunes. To approach the textual criticism of the N.T. without taking due account of that interest is to act irresponsibly.

### 1) *Most damage done by 200 A.D.*

It is generally agreed that most significant variants existed by the end of the second century. "The overwhelming majority of readings were created before the year 200," affirms Colwell.<sup>33</sup> "It is no less true to fact than paradoxical in sound that the worst corruptions to which the New Testament has ever been subjected, originated within a hundred years after it was composed," said Scrivener decades before.<sup>34</sup> Kilpatrick comments on the evidence of the earliest Papyri.

Let us take our two manuscripts of about this date [A.D. 200] which contain parts of John, the Chester Beatty Papyrus and the Bodmer Papyrus. They are together extant for about seventy verses. Over these seventy verses they differ some seventy-three times apart from mistakes.

Further in the Bodmer Papyrus the original scribe has frequently corrected what he first wrote. At some places he is correcting his own mistakes but at others he substitutes one form of phrasing for another. At about seventy-five of these substitutions both alternatives are known from other manuscripts independently. The scribe is in fact replacing one variant reading by another at some seventy places so that we may conclude that already in his day there was variation at these points.<sup>35</sup>

Zuntz also recognized all of this. "Modern criticism stops before the barrier of the second century; the age, so it seems, of unbounded liberties with the text."<sup>36</sup>

Kilpatrick goes on to argue that the creation of new variants ceased by about 200 A.D. because it became impossible to "sell" them. He discusses some of Origen's attempts at introducing a change into the text, and proceeds:

Origen's treatment of Matt. 19:19 is significant in two other ways. First he was probably the most influential commentator of the Ancient Church and yet his conjecture at this point seems to have influenced only one manuscript of a local version of the New Testament. The Greek tradition is apparently quite unaffected by it. From the third century onward even an Origen could not effectively alter the text.

This brings us to the second significant point—his date. From the early third century onward the freedom to alter the text which had obtained earlier can no longer be practiced. Tatian is the last author to make deliberate changes in the text of whom we have explicit

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<sup>32</sup>Burgon, *The Revision Revised*, pp. 323-24.

<sup>33</sup>Colwell, "The Origin of Texttypes," p. 138.

<sup>34</sup>F.H.A. Scrivener, *A Plain Introduction to the Criticism of the New Testament*, fourth edition edited by E. Miller (2 Vols.; London: George Bell and Sons, 1894), II, 264.

<sup>35</sup>G.D. Kilpatrick, "The Transmission of the New Testament and its Reliability," *The Bible Translator*, IX (July, 1958), 128-29.

<sup>36</sup>Zuntz, *The Text*, p. 11.

information. Between Tatian and Origen Christian opinion had so changed that it was no longer possible to make changes in the text whether they were harmless or not.<sup>37</sup>

He feels this attitude was a reaction against the rehandling of the text by the second-century heretics. Certainly there had been a great hue and cry, and whatever the reason it does appear that little further damage was done after A.D. 200.

## 2) *The aberrant text forms*

The extent of the textual difficulties of the 2nd century can easily be exaggerated. Nevertheless, the evidence cited does prove that aberrant forms of the N.T. text were produced. Naturally, some of those text forms may have acquired a local and temporary currency, but they could scarcely become more than eddies along the edge of the "majority" river. Recall that the possibility of checking against the Autographs must have served to inhibit the spread of such text forms.

For example, Gaius, an orthodox Father who wrote near the end of the second century, named four heretics who not only altered the text but had disciples who multiplied copies of their efforts. Of special interest here is his charge that they could not deny their guilt because they could not produce the originals from which they made their copies.<sup>38</sup> This would be a hollow accusation from Gaius if he could not produce the Originals either. I have already argued that the churches in Asia Minor, for instance, did still have either the Autographs or exact copies that they themselves had made—thus they **knew**, absolutely, what the true wording was and could repel the aberrant forms with confidence. A man like Polycarp would still be able to affirm in 150 A.D., letter by letter if need be, the original wording of the text for most of the New Testament books. And presumably his MSS were not burned when he was.

Not only would there have been pressure from the Autographs, but also the pressure exerted by the already-established momentum of transmission enjoyed by the majority text form. As already discussed, the statistical probabilities militating against any aberrant text forms would be overwhelming. In short, although a bewildering array of variants came into existence, judging from extant witnesses, and they were indeed a perturbing influence in the stream of transmission, they would not succeed in thwarting the progress of the normal transmission.

## The Stream of Transmission

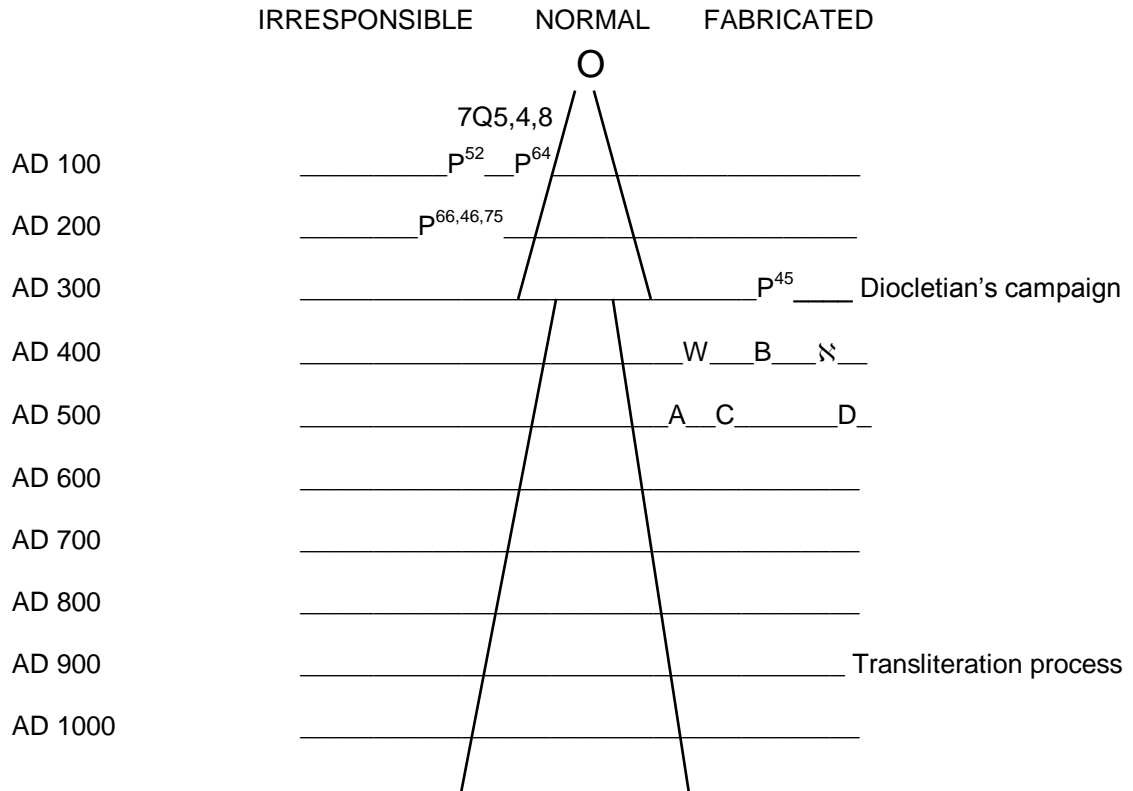
Now then, what sort of a picture may we expect to find in the surviving witnesses on the assumption that the history of the transmission of the New Testament Text was normal? We may expect a broad spectrum of copies, showing minor differences due to copying mistakes but all reflecting one common tradition. The simultaneous existence of abnormal transmission in the earliest centuries would result in a sprinkling of copies, helter-skelter, outside of that main stream. The picture would look something like *Figure C*.

The MSS within the cone represent the "normal" transmission. To the left I have plotted some possible representatives of what we might style the "irresponsible" transmission of the text—the copyists produced poor copies through incompetence or carelessness but did not make deliberate changes. To the right I have plotted some possible representatives of what we might style the "fabricated" transmission of the text—the scribes made deliberate changes in the text (for whatever reasons), producing fabricated copies, not true copies. I am well aware that the MSS plotted on the figure above contain both careless and deliberate errors, in different proportions (7Q5,4,8 and P<sup>52</sup> are too fragmentary to permit the classification of their errors as deliberate rather than careless), so that any classification such as I attempt here must be relative and gives a distorted picture. Still, I venture to insist that ignorance, carelessness, officiousness and malice all left their mark upon the transmission of the New Testament text, and we must take account of them in any attempt to reconstruct the history of that transmission.

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<sup>37</sup>Kilpatrick, "Atticism and the Text of the Greek New Testament," *Neutestamentliche Aufsätze* (Regensburg: Verlag Friedrich Pustet, 1963), pp. 129-30.

<sup>38</sup>Cf. Burgon, *The Revision Revised*, p. 323.



**Figure C**

As the figure suggests, I argue that Diocletian's campaign had a purifying effect upon the stream of transmission. In order to withstand torture rather than give up your MS(S), you would have to be a truly committed believer, the sort of person who would want good copies of the Scriptures. Thus it was probably the more contaminated MSS that were destroyed, in the main, leaving the purer MSS to replenish the earth (please see the section, "Imperial repression of the N.T." in Chapter six).

Another consideration suggests itself—if, as reported, the Diocletian campaign was most fierce and effective in the Byzantine area, the numerical advantage of the "Byzantine" text-type over the "Western" and "Alexandrian" would have been reduced, giving the latter a chance to forge ahead. But it did not happen. The Church, in the main, refused to propagate those forms of the Greek text.

What we find upon consulting the witnesses is just such a picture. We have the Majority Text (Aland), or the Traditional Text (Burgon), dominating the stream of transmission with a few individual witnesses going their idiosyncratic ways. We have already seen that the notion of "text-types" and recensions, as defined and used by Hort and his followers, is gratuitous. Epp's notion of "streams" fares no better. There is just one stream, with a number of small eddies along the edges.<sup>39</sup> When I say the Majority Text dominates the stream, I mean it is represented in about 95% of the MSS.<sup>40</sup>

Actually, such a statement is not altogether satisfactory because it does not allow for the mixture or shifting affinities encountered within individual MSS. A better, though more cumbersome, way to describe the situation would be something like this: 100% of the MSS agree as to, say, 50% of the Text; 99% agree as to another 40%; over 95% agree as to another 4%; over 90% agree as to another 2%; over

<sup>39</sup>One might speak of a P<sup>45</sup>,W eddy or a P<sup>75</sup>,B eddy, for example.

<sup>40</sup>Although I have used, of necessity, the term "text-type" throughout the book, I view the Majority Text as being much broader. It is a textual tradition which might be said to include a number of related "text-types," such as von Soden's K<sup>a</sup>, K<sup>l</sup>, and K<sup>l</sup>. I wish to emphasize again that it is only agreement in error that determines genealogical relationships. It follows that the concepts of "genealogy" and "text-type" are irrelevant with reference to original readings—they are only useful (when employed properly) for identifying spurious readings. Well, if there is a family that very nearly reflects the original its "profile" or mosaic of readings will distinguish it from other families, but most of those readings will not be errors (the competing variants distinctive of other families **will** be errors).

80% agree as to another 2%; only for 2% or so of the Text do less than 80% of the MSS agree, and most of those cases occur in Revelation.<sup>41</sup> And the membership of the dissenting group varies from reading to reading. (I will of course be reminded that witnesses are to be weighed, not counted; I will come to that presently, so please bear with me.) Still, with the above reservation, one may reasonably speak of up to 95% of the extant MSS belonging to the Majority text-type.

I see no way of accounting for a 95% (or 90%) domination unless that text goes back to the Autographs. Hort saw the problem and invented a revision. Sturz seems not to have seen the problem. He demonstrates that the "Byzantine text-type" is early and independent of the "Western" and "Alexandrian text-types," and like von Soden, wishes to treat them as three equal witnesses.<sup>42</sup> But if the three "text-types" were equal, how ever could the so-called "Byzantine" gain a 90-95% preponderance?

The argument from statistical probability enters here with a vengeance. Not only do the extant MSS present us with one text form enjoying a 95% majority, but the remaining 5% do not represent a single competing text form. The minority MSS disagree as much (or more) among themselves as they do with the majority. For any two of them to agree so closely as do P<sup>75</sup> and B is an oddity. We are not judging, therefore, between two text forms, one representing 95% of the MSS and the other 5%. Rather, we have to judge between 95% and a fraction of 1% (comparing the Majority Text with the P<sup>75</sup>, B text form for example). Or to take a specific case, in 1 Tim. 3:16 some 600 Greek MSS (besides the Lectionaries) read "God" while only seven read something else. Of those seven, three have private readings and four agree in reading "who."<sup>43</sup> So we have to judge between 99% and 0.6%, "God" versus "who." It is hard to

<sup>41</sup>I am not prepared to defend the precise figures used, they are **guesses**, but I believe they represent a reasonable approximation to reality. I heartily agree with Colwell when he insists that we must "rigorously eliminate the singular reading" ("External Evidence," p. 8) on the altogether reasonable assumption (it seems to me) that a solitary witness against the world cannot possibly be right.

<sup>42</sup>Sturz, *Op. Cit.* A text produced by taking two "text-types" against one would move the UBS text about 80% of the distance toward the Majority text.

<sup>43</sup>The readings, with their supporting MSS, are as follows:

o - D  
 ω - 061  
 ος Θεος - one cursive (and one Lectionary)  
 ος - Ν, 33, 442, 2127 (three Lectionaries)  
 Θεος - A, C<sup>vid</sup>, F/G<sup>vid</sup>, K, L, P, Ψ, some 600 cursives (besides Lectionaries) (including four cursives that read ο Θεος and one Lectionary that reads Θεου).

It will be observed that my statement differs from that of the UBS text, for example. I offer the following explanation.

Young, Huish, Pearson, Fell, and Mill in the seventeenth century, Creyk, Bentley, Wotton, Wetstein, Bengel, Berriman, and Woide in the eighteenth, and Scrivener as late as 1881 all affirmed, upon careful inspection, that Codex A reads "God." For a thorough discussion please see Burgon, who says concerning Woide, "The learned and conscientious editor of the Codex declares that so late as 1765 he had seen traces of the  $\theta$  which twenty years later (viz. in 1785) were visible to him no longer" (*The Revision Revised*, p. 434. Cf. pp. 431-36). It was only after 1765 that scholars started to question the reading of A (through fading and wear the middle line of the *theta* is no longer discernible).

Hoskier devotes Appendix J of *A Full Account* (the appendix being a reprint of part of an article that appeared in the *Clergyman's Magazine* for February 1887) to a careful discussion of the reading of Codex C. He spent three hours examining the passage in question in this MS (the MS itself) and adduces evidence that shows clearly, I believe, that the original reading of C is "God." He examined the surrounding context and observes, "The **contracting-bar** has often vanished completely (I believe, from a cursory examination, more often than not), but at other times it is plain and imposed in the same way as at 1 Tim. iii.16" (Appendix J, p. 2). See also Burgon, *Ibid.*, pp. 437-38.

Codices F/G read OC wherein the contracting-bar is a slanting stroke. It has been argued that the stroke represents the aspirate of ος, but Burgon demonstrates that the stroke in question never represents breathing but is invariably the sign of contraction and affirms that "ος is **nowhere** else written OC in either codex" (*Ibid.*, p. 442. Cf. pp. 438-42). Presumably the cross-line in the common parent had become too faint to see. As for cursive 365, Burgon conducted an exhaustive search for it. He not only failed to find it but could find no evidence that it had ever existed (*Ibid.*, pp. 444-45).

(I took up the case of 1 Tim. 3:16, in the first edition of this book, solely to illustrate the argument from probability, not as an example of "how to do textual criticism" [cf. Fee, "A Critique," p. 423]. Since the question has been raised, I will add a few words on that subject.)

The three significant variants involved are represented in the ancient uncial MSS as follows: O, OC, and  $\theta$ C, meaning "which," "who," and "God" respectively. In writing "God" a scribe's omitting of the two lines (through haste or momentary distraction) would result in "who." Codices A, C, F, and G have numerous instances where either the cross-line or the contracting-bar is no longer discernible (either the original line has faded to the point of being invisible or the scribe may have failed to write it in the first place). For both lines to fade away, as in Codex A here, is presumably an infrequent event. For a scribe to inadvertently omit both lines would presumably also be an infrequent event, but it must have happened at least once, probably early in the second century and in circumstances that produced a wide ranging effect.

The collocation "the mystery . . . who" is even more pathologic in Greek than it is in English. It was thus inevitable, once such a reading came into existence and became known, that remedial action would be attempted. Accordingly, the first reading above, "the

imagine any possible set of circumstances in the transmissional history sufficient to produce the cataclysmic overthrow in statistical probability required by the claim that "who" is the original reading.

It really does seem that those scholars who reject the Majority Text are faced with a serious problem. How is it to be explained if it does not reflect the Original? Hort's notion of a Lucianic revision has been abandoned by most scholars because of the total lack of historical evidence. The eclecticists are not even trying. The "process" view has not been articulated in sufficient detail to permit refutation, but on the face of it that view is flatly contradicted by the argument from statistical probability.<sup>44</sup> How could any amount of "process" bridge the gap between B or Aleph and the TR?

But there is a more basic problem with the process view. Hort saw clearly, and correctly, that the Majority Text must have a common archetype. Recall that Hort's genealogical method was based on community of **error**. On the hypothesis that the Majority Text is a late and inferior text form, the large mass of common readings which distinguish it from the so-called "Western" or "Alexandrian text-types" must be **errors** (which was precisely Hort's contention) and such an agreement in error would have to have a common source. The process view fails completely to account for such an agreement in error (on that hypothesis).

Hort saw the need for a common source and posited a Lucianic revision. Scholars now generally recognize that the "Byzantine text-type" must date back at least into the second century. But what chance would the original "Byzantine" document, the archetype, have of gaining currency when appeal to the Autographs was still possible?

Candidly, there is only one reasonable explanation for the Majority Text that has so far been advanced—it is the result of an essentially normal process of transmission and the common source for its consensus is the Autographs. Down through the centuries of copying, the original text has always been reflected with a high degree of accuracy in the manuscript tradition as a whole. The history of the text presented in this chapter not only accounts nicely for the Majority Text, it also accounts for the inconsistent minority of MSS. They are remnants of the abnormal transmission of the text, reflecting ancient aberrant forms. It is a dependence upon such aberrant forms that distinguishes contemporary critical/eclectic editions of the Greek New Testament, and the modern translations based upon them.

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mystery . . . which," is generally regarded as an attempt to make the difficult reading intelligible. But it must have been an early development, for it completely dominates the Latin tradition, both version and Fathers, as well as being the probable reading of the Syr<sup>P</sup> and Coptic versions. It is found in only one Greek MS, Codex D, and in no Greek Father before the fifth century.

Most modern scholars regard "God" as a separate therapeutic response to the difficult reading. Although it dominates the Greek MSS (over 98 percent), it is certainly attested by only two versions, the Georgian and Slavonic (both late). But it also dominates the Greek Fathers. Around A.D. 100 there are possible allusions in Barnabas, "Ἰησοῦς . . . ὁ υἱὸς τοῦ Θεοῦ τυπῶ καὶ ἐν σαρκὶ φανερωθεὶς" (Cap. xii), and in Ignatius, "Θεοῦ ἀνθρωπίνως φανερούμενου" (*Ad Ephes.* c. 19) and "ἐν σαρκὶ γενομένου Θεοῦ" (*Ibid.*, c. 7). In the third century there seem to be clear references in Hippolytus, "Θεὸς ἐν σωματί εφανερωθῆ" (*Contra Haeresim Noeti*, c. xvii), Dionysius, "Θεὸς γὰρ ἐφανέρωθη ἐν σαρκὶ" (*Concilia*, i. 853a) and Gregory Thaumaturgus, "καὶ ἐστὶν Θεὸς ἀληθινὸς ὁ ἀσαρκὸς ἐν σαρκὶ φανερωθεὶς" (quoted by Photius). In the 4th century there are clear quotes or references in Gregory of Nyssa (22 times), Gregory of Nazianzus, Didymus of Alexandria, Diodorus, the Apostolic Constitutions, and Chrysostom, followed by Cyril of Alexandria, Theodoret, and Euthalius in the fifth century, and so on (Burgon, *Ibid.*, pp. 456-76, 486-90).

As for the grammatically aberrant reading, "who," aside from the MSS already cited, the earliest version that clearly supports it is the gothic (fourth century). To get a clear Greek Patristic witness to this reading pretty well requires the sequence *m u s t h r i o n o j e f a n e r w q h* since after any reference to Christ, Savior, Son of God, etc. in the prior context the use of a relative clause is predictable. Burgon affirmed that he was aware of no such testimony (and his knowledge of the subject has probably never been equaled) (*Ibid.*, p. 483).

It thus appears that the "Western" and "Byzantine" readings have earlier attestation than does the "Alexandrian." Yet if "which" was caused by "who", then the latter must be older. The reading "who" is admittedly the most difficult, so much so that to apply the "harder reading" canon in the face of an easy transcriptional explanation [the accidental omission of the two lines] for the difficult reading seems unreasonable. As Burgon so well put it:

I trust we are at least agreed that the maxim "*proclivi lectioni praestat ardua*," does not enunciate so foolish a proposition as that in choosing between two or more conflicting readings, we are to prefer **that** one which has the feeblest external attestation,—provided it be but in itself almost unintelligible? (*Ibid.*, p. 497).

<sup>44</sup>For further discussion see the final pages of Appendix C.

## What Is the Actual Evidence?

### The Uncials

In *The Text of the New Testament*<sup>45</sup> K. Aland offers a summary of the results of a "systematic test collation" for the more important uncials from centuries IV-IX. He uses four headings: "Byzantine", "original", "agreements" between the first two, and "independent or distinctive" readings. Since by "original" he seems to mean essentially "Egyptian" (or "Alexandrian") I will use the following headings: Egyptian, Majority ("Byzantine"), both ("agreements") and other ("independent"). I proceed to chart each MS from the IV through IX centuries for which Aland offers a summary:

By way of explanation: "cont." stands for content, **e** = Gospels (but Aland's figures cover only the Synoptics), **a** = Acts, **p** = Pauline Epistles (including Hebrews) and **c** = Catholic Epistles; "Cat." refers to Aland's five categories (*The Text*, pp. 105-6) and "class." stands for a classification devised by me wherein **E** = Egyptian, **M** = Majority and **O** = other. It has the following values, which are illustrated with M:

M+++++	=	100%		
M++++	=	over 95%	= 19:1	= very strong
M+++	=	over 90%	= 9:1	= strong
M++	=	over 80%	= 4:1	= good
M+	=	over 66%	= 2:1	= fair
M	=	over 50%	= 1:1	= weak
M-	=	plurality	=	= marginal
M/E	=	a tie		

I assume that Aland will agree with me that E + M is certainly original, so the "both" column needs to be disregarded as we try to evaluate the tendencies of the several MSS. Accordingly I considered only the "Egyptian", "Majority" and "other" columns in calculating percentages.

Codex	Date	cont.	Egyptian	both	Majority	other	total	class.	Cat.
B-03	IV	e	196	54	9	72	331	E+	I
		a	72	22	2	11	107	E++	I
		p	144	31	8	27	210	E++	I
		c	80	8	2	9	99	E++	I
N-01	IV	e	170	80	23	95	368	E	I
		a	67	24	9	17	117	E+	I
		p	174	38	76	52	340	E	I
		c	73	5	21	16	115	E	I
-----400									
W-032	V	e	54	70	118	88	330	M-	III
A-02	V	e	18	84	151	15	268	M++	III
		a	65	22	9	12	108	E+	I
		p	149	28	31	37	245	E+	I
		c	62	5	18	12	97	E+	I
C-04	V	e	66	66	87	50	269	M-	II
		a	37	12	12	11	72	E	II
		p	104	23	31	15	173	E+	II
		c	41	3	15	12	71	E	II
D-05	V	e	77	48	65	134	324	O-	IV

<sup>45</sup>K. and B. Aland (Grand Rapids: Eerdmans, 1987), pp. 106-125.



		a	16	7	21	33	77	O-	IV
I-016	V	p	15	1	2	6	24	E	II
Q-026	V	e	0	5	5	2	12	M+	V
048	V	p*	26	7	3	4	40	E+	II
0274	V	e	19	6	0	2	27	E+++	II
-----500									
D-06	VI	p	112	29	137	83	361	M-	II
E-08	VI	a	23	21	36	22	102	M-	II
H-015	VI	p	11	0	5	1	17	E	III
N-022	VI	e	8	48	89	15	160	M+	V
O-023	VI	e	0	4	9	3	16	M+	V
P-024	VI	e	3	16	24	0	43	M++	V
R-027	VI	e	0	4	11	5	20	M+	V
Z-035	VI	e	11	5	3	2	21	E+	III
Ξ-040	VI**	e	8	2	2	3	15	E	III
Σ-042	VI	e	15	83	140	25	263	M+	V
Φ-043	VI	e	11	83	131	18	243	M++	V
-----600									
0211	VII	e	10	101	189	23	323	M++	V
-----700									
E-07	VIII	e	1	107	209	9	326	M++++	V
L-019	VIII	e	125	75	52	64	316	E	II
047	VIII	e	6	96	175	21	298	M++	V
0233	VIII	e	3	23	47	5	78	M++	III
Ψ-044	VIII	e	52	21	40	19	132	E-	III
		a	22	25	43	15	105	M	III
		p	38	42	135	33	248	M	III
		c	54	8	21	14	97	E	II
-----800									
F-09	IX	e	0	78	156	11	245	M+++	V
F-010	IX	p	91	12	41	69	213	E-	III
G-011	IX	e	4	87	176	21	288	M++	V
G-012	IX	p	91	12	43	66	212	E-	III
H-013	IX	e	2	82	174	7	265	M++++	V

H-014	IX	a	2	22	48	1	73	M+++	V	
K-017	IX	e	8	107	197	15	327	M++	V	
K-018	IX	p	8	32	154	8	202	M+++	V	
			c	4	9	77	6	96	M++	V
L-020	IX	a	1	23	51	3	78	M+++	V	
			p	5	44	188	4	241	M++++	V
			c	5	9	78	3	95	M+++	V
M-021	IX	e	7	106	202	12	327	M+++	V	
P-025	IX	a	1	29	70	0	100	M++++	V	
			p	87	31	87	31	236	E/M	III
			c	26	6	46	9	87	M	III
U-030	IX	e	1	38	105	11	155	M++	V	
V-031	IX	e	8	101	192	17	318	M++	V	
Y-034	IX	e	4	95	192	6	297	M++++	V	
Δ-037	IX	e	69	88	120	47	324	M	III	
Θ-038	IX	e	75	59	89	95	318	O-	II	
Λ-039	IX	e	0	10	41	2	53	M++++	V	
Π-041	IX	e	11	104	190	18	323	M++	V	
Ω-045	IX	e	3	104	208	10	325	M+++	V	
049	IX	a	3	29	69	3	104	M+++	V	
			p	0	34	113	3	150	M++++	V
			c	1	9	82	4	96	M+++	V
063	IX	p	0	3	15	0	18	M++++	V	
0150	IX	p	65	34	101	23	223	M	III	
0151	IX	p	9	44	174	7	234	M+++	V	
33	IX	e	57	73	54	44	228	E-	II	
			a	34	19	21	11	85	E	I
			p	129	35	47	36	247	E	I
			c	45	3	21	14	83	E	I
461	835	e	3	102	219	5	329	M++++	V	

-----900

(\*Aland shows **ap**, but gives no figures for **a**. \*\*UBS<sup>3</sup> has VIII.)

So, what can we learn from this chart? Perhaps a good place to begin is with a correlation between "Cat." and "class." in terms of the values we have each given to specific MSS:

<u>I</u>	<u>II</u>	<u>III</u>	<u>IV</u>	<u>V</u>
E++	E+++ M- O-	E+ M++	O-	M+++++
E+	E+	E M		M++++
E	E	E- M-		M+++
	E-	E/M		M++
				M+

Categories I, IV and V are reasonably consistent, but how are we to interpret II and III? This is bothersome because in Aland's book (pp. 156-59) a very great many MSS are listed under III and not a few under II. It might be helpful to see how many MSS, or content segments, fall at the intersections of the two parameters:

	<u>I</u>	<u>II</u>	<u>III</u>	<u>IV</u>	<u>V</u>	<u>total</u>
E+++		1				1
E++	3					3
E+	5	2	1			8
E	6	5	2			13
E-		1	3			4
O-		1		2		3
E/M			1			1
M-		3	1			4
M			5			5
M+					5	5
M++			2		10	12
M+++					10	10
M++++					8	8
M+++++					1	1

0274 and 063 are fragmentary, which presumably accounts for their exceptional scores, E+++ and M+++++ respectively; if they were more complete they would probably each come down a level. Out of 45 M segments 31 score above 80%, while 9 are over 95% 'pure'. It should be possible to reconstruct a "Byzantine" archetype with tolerable confidence. But one has to wonder how Aland arrived at the "Egyptian" norm in the Gospels since the best Egyptian witness (except for the fragmentary 0274, which has less than 10% of the text but scores 90%), Codex B, barely passes 70%. (In *The Text*, p. 95, Aland gives a summary for P<sup>75</sup> in Luke—it scores 77%.) Further, besides B and 0274, P<sup>75</sup> and Z (both also fragmentary) are the only Greek MSS that score so much as an E+ in the Gospels. One is reminded of E.C. Colwell's conclusion after attempting to reconstruct an 'average' or mean Alexandrian text for the first chapter of Mark. "These results show convincingly that any attempt to reconstruct an archetype of the Beta [Alexandrian] Text-type on a quantitative basis is doomed to failure. The text thus reconstructed is not reconstructed but constructed; it is an artificial entity that never existed."<sup>46</sup>

For the other content areas the situation is not much better. Only P<sup>74</sup> (86%), B (85%) and 81 (80%) rate an E++ in **a**; apart from them only A and Aleph manage even an E+. Codex B is the only E++ (80%) in **p**, and only P<sup>46</sup>, A, C, 048 and 1739 manage an E+. Aside from B's 88% in **c**, only P<sup>74</sup>, A and 1739 manage even an E+. How did Aland arrive at his "Egyptian" norm in these areas? Might that "norm" be a fiction, as Colwell affirmed?

Codex A<sup>e</sup> is 82% Byzantine and must have been based on a Byzantine exemplar, which presumably would belong to the IV century. Codex W in Matthew is also clearly Byzantine and must have had a Byzantine exemplar. The sprinkling of Byzantine readings in B is sufficiently slight that it could be ascribed to chance, I suppose, but that explanation will hardly serve for Aleph. At least in **p**, if not throughout, Aleph's copyist must have had access to a Byzantine exemplar, which could have belonged to the III century. But Asterius offers much stronger evidence: he died in 341, so presumably did his writing somewhat earlier; it seems likely that his MSS would be from the III century—since he shows a 90% preference for Byzantine readings those MSS must have been **Byzantine**. (Using my classification,

<sup>46</sup>"The Significance of Grouping of New Testament Manuscripts," *New Testament Studies*, IV (1957-1958), 86-87.

Asterius would be M++, the Byzantine preference being 83%. On a percentage basis Asterius is as strongly Byzantine as B is Egyptian.) Adamantius died in 300, so he did his writing earlier. Might his MSS have been from the first half of the III century? Since he shows a 52% preference for Byzantine readings (or 39%, using my classification) at least some of his MSS were presumably Byzantine. For that matter P<sup>66</sup> has so many Byzantine readings that **its** copyist must have had access to a Byzantine exemplar, which would necessarily belong to the II century! The circumstance that some Byzantine readings in P<sup>66</sup> were corrected to Egyptian readings, while some Egyptian readings in P<sup>66</sup> were corrected to Byzantine readings, really seems to require that we posit exemplars of the two types—between them the two hands furnish clear evidence that the Byzantine text, as such, existed in their day. (For evidence from the early Fathers, Papyri and Versions please see the section, "But There Is No Evidence of the Byzantine Text in the Early Centuries", in Chapter six.)

Returning to the chart of the uncials above, in the IV century E leads in all four areas, although in Aleph E is weak and M is gaining. If W is IV century M has gained even more. I remind the reader that I am referring only to the information in the chart given above. In reality, I assume that the IV century, like all others, was dominated by Byzantine MSS. Being good copies they were used and worn out, thereby perishing. Copies like B and Aleph survived because they were "different", and therefore not used. By "used" I mean for ordinary purposes—I am well aware that Aleph exercised the ingenuity of a number of correctors over the centuries, but it left no descendants. In the V century M takes over the lead in **e** while E retains **apc** (it may come as a surprise to some that C<sup>e</sup> is more M than anything else). In the VI century M strengthens its hold on **e** and moves in on **a** (it may come as a surprise to some that D<sup>p</sup> is more M than anything else). After the V century, with the sole exception of the fragmentary Z, all the "Egyptian" witnesses are weak—even the "queen of the cursives," 33, does not get up to an E+. Of X century uncials for which Aland offers a summary, all are clearly Byzantine (028, 033, 036, 056, 075 and 0124) except for 0243, which scores an E.<sup>47</sup>

## The Cursives

When we turn to the cursives, Aland offers summaries for 150, chosen on the basis of their "independence" from the Byzantine norm. He lists 900 MSS only by number because "these minuscules exhibit a purely or predominantly Byzantine text," and therefore he considers that "they are all irrelevant for textual criticism" (*The Text*, p. 155). To do for the 150 "independent" cursives what I did for the uncials would take too much space, so I will summarize Aland's statistics in chart form, using my classification:

cont.	M+++++	M++++	M+++	M++	M+	M	M-	M/E	E-	E	E+	E++
e		10	23	12	6	16	1		2	1		
a		12	15	23	21	14	12	1	4	2		1
p	1	25	17	17	28	19	4		2	3	1	
c	1	9	18	6	30	21	10	1	5	10	1	
total	2	56	73	58	85	70	27	2	13	16	2	1

<sup>47</sup>In February, 1990 I debated Daniel Wallace at the Dallas Theological Seminary, where he was teaching. He used a graph purporting to show the distribution of the Greek MSS from the III to the IX centuries according to the three main "text-types" (a graph that he was using in the classroom). He has since used the same graph in a paper presented to the Evangelical Theological Society. The graph is very seriously misleading. I challenge Wallace to identify the MSS that the graph is supposed to represent and to demonstrate that each one belongs to the "text-type" that he alleged. It was stated that the extant MSS do not show the Byzantine text in the majority until the IX century, but according to Aland's statistics the Byzantine text took the lead in the Gospels in the V century, and kept it.

But let us consider the MSS from the IX century. Out of 27 Byzantine MSS or content segments (Gospels, Pauline corpus, etc.), eight are over 95% 'pure', ten are over 90% pure, and another six are over 80% pure. Where did these 24 MSS or segments get their Byzantine content? Since they are all distinct in content they were presumably copied from as many separate exemplars, exemplars of necessity earlier in date and also Byzantine. And what were those exemplars copied from? Evidently from still earlier Byzantine MSS, etc. Hopefully Wallace will not attempt to argue that all those IX century MSS were not copied from anything, but were independently created from nothing by each scribe! It follows that a massive majority in the IX century presupposes a massive majority in the VIII, and so on. Which is why scholars from Hort to Aland have recognized that the Byzantine text dominated the transmission from the IV century on.

Textual scholars of all persuasions, down through the years, have recognized that the extant witnesses from the early centuries are not necessarily representative of the actual state of affairs in their day. To insist that the extant witnesses are the whole story is unreasonable and begs the question.

Even among these "independent" cursives there are two content segments that actually score 100% Byzantine! (Just imagine how many more there must be among the 900 that are so Byzantine that Aland ignored them.) The best Egyptian representative is 81 in Acts, with an even 80%. 1739 scores 70% (E+) in **c** and 68% (E+) in **p**. These are the only three segments that I would call "clearly Egyptian". There are sixteen segments that score between 50 and 66% (E). Pitting M through M+++++ against E through E++ we get 344 to 19, and this from the "independent" minuscules. If we add the 900 "predominantly Byzantine" MSS, which will average over two content segments each, the actual ratio is well over 100 to one. I assume that almost all of these 900 will score at least M++, and most will doubtless score M+++ or higher. If we were to compute only segments that score at least 80%, the Byzantine:Egyptian ratio would be more like **1,000** to one—the MSS that have been classified by Aland's "test collation", as reported in his book, represent perhaps 40% of the total (excluding Lectionaries), but we may reasonably assume that most of the "independent" ones have already been identified and presented. It follows that the remaining MSS, at least 1,600, can only increase the Byzantine side of the ratio. If the Byzantine text is the "worst", then down through the centuries of manuscript copying the Church was massively mistaken!

The MSS discussed in Aland's book (first edition) reflect the collating done at his Institute as of 1981. Many more have doubtless been collated since, but the general proportions will probably not change significantly. Consider the study done by Frederik Wisse. He collated and compared **1,386** MSS in Luke 1, 10 and 20, and found only four uncials (out of 34) and four cursives (out of 1,352) that displayed the Egyptian text-type, plus another two of each that were Egyptian in one of the three chapters.<sup>48</sup>

## Concluding Remarks

In his book Aland's discussion of the transmission of the NT text is permeated with the assumption that the Byzantine text was a secondary development that progressively contaminated the pure Egyptian ("Alexandrian") text. But the chief "Alexandrian" witnesses, B, A (except **e**) and **Ⲛ** (*The Text*, p. 107), are in constant and significant disagreement among themselves; so much so that there is no objective way of reconstructing an archetype. 150 years earlier the picture is the same; P<sup>45</sup>, P<sup>66</sup> and P<sup>75</sup> are quite dissimilar and do not reflect a single tradition. In A.D. 200 "there was no king in [Egypt]; everyone did what was right in his own eyes," or so it would seem. But what if we were to entertain the hypothesis that the Byzantine tradition is the oldest and that the "Western" and "Alexandrian" MSS represent varying perturbations on the fringes of the main transmissional stream? Would this not make better sense of the surviving evidence? Then there would have been no "Western" or "Egyptian" archetypes, just various sources of contamination that acted in such a random fashion that each extant "Western" or "Egyptian" MS has a different 'mosaic'. In contrast, there would indeed be a "Byzantine" archetype, which would reflect the original. In fact, virtually perfect exemplars exist in our day, as illustrated by 1841 for the pauline corpus and 424 for the general epistles.

Aland seems to grant that down through the centuries of church history the Byzantine text was regarded as "the text of the church", and he traces the beginning of this state of affairs to Lucian.<sup>49</sup> He makes repeated mention of a "school of/at Antioch" and of Asia Minor. All of this is very interesting, because in his book he agrees with Adolf Harnack that "about 180 the greatest concentration of churches was in Asia Minor and along the Aegean coast of Greece".<sup>50</sup> This is the area where Greek was the mother tongue and where Greek continued to be used. It is also the area that started out with most of the Autographs. But Aland continues: "Even around A.D. 325 the scene was still largely unchanged. Asia Minor continued to be the heartland of the Church." "The heartland of the Church"—so who else would be in a better position to identify the correct text of the New Testament? Who could 'sell' a fabricated text in Asia Minor in the early fourth century? I submit that the Byzantine text dominated the transmissional history because the churches in Asia Minor vouched for it. And they did so, from the very beginning, because they knew it was the true text, having received it from the Apostles. The Majority Text is what it is just because it has always been **the Text of the Church**.

<sup>48</sup>*The Profile Method for the Classification and Evaluation of Manuscript Evidence* (Grand Rapids: Eerdmans, 1982).

<sup>49</sup>K. Aland, "The Text of the Church?", *Trinity Journal*, 1987, 8NS:131-144 [actually published in 1989], pp. 142-43.

<sup>50</sup>*The Text of the New Testament*, p. 53.

## SOME POSSIBLE OBJECTIONS

### Are Not the Oldest MSS the Best?

Burton recognized the "antecedent probability" with these words:

The more ancient testimony is probably the better testimony. That it is not by any means always so is a familiar fact. . . . But it remains true, notwithstanding, that until evidence has been produced to the contrary in any particular instance, the more ancient of two witnesses may reasonably be presumed to be the better informed witness.<sup>1</sup>

This **a priori** expectation seems to have been elevated to a virtual certainty in the minds of many textual critics of the past century. The basic ingredient in the work of men like Tregelles, Tischendorf and Hort was a deference to the oldest MSS, and in this they followed Lachmann.

The 'best' attestation, so Lachmann maintained, is given by the oldest witnesses. Taking his stand rigorously with the oldest, and disregarding the whole of the recent evidence, he drew the consequences of Bengel's observations. The material which Lachmann used could with advantage have been increased; but the principle that the text of the New Testament, like that of every other critical edition, must throughout be based upon the best available evidence, was once and for all established by him.<sup>2</sup>

Note that Zuntz here clearly equates "oldest" with "best." He evidently exemplifies what Oliver has called "the growing belief that the oldest manuscripts contain the most nearly original text." Oliver proceeds:

Some recent critics have returned to the earlier pattern of Tischendorf and Westcott and Hort: to seek for the original text in the oldest MSS. Critics earlier in the 20th century were highly critical of this 19th century practice. The return has been motivated largely by the discovery of papyri which are separated from the autographs by less than two centuries.<sup>3</sup>

But, the "contrary evidence" is in hand. We have already seen that most significant variants had come into being by the year 200, before the time of the earliest extant MSS, therefore. The **a priori** presumption in favor of age is nullified by the known existence of a variety of deliberately altered texts in the second century: Each witness must be evaluated on its own. As Colwell has so well put it, "the crucial question for early as for late witnesses is still, 'WHERE DO THEY FIT INTO A PLAUSIBLE RECONSTRUCTION OF THE HISTORY OF THE MANUSCRIPT TRADITION?'"<sup>4</sup>

It is generally agreed that all the earliest MSS, the ones upon which our critical texts are based, come from Egypt.

When the textual critic looks more closely at his oldest manuscript materials, the paucity of his resources is more fully realized. All the earliest witnesses, papyrus or parchment, come from Egypt alone. Manuscripts produced in Egypt, ranging between the third and fifth centuries, provide only a half-dozen extensive witnesses (the Beatty Papyri, and the well-known uncials, Vaticanus, Sinaiticus, Alexandrinus, Ephraem Syrus, and Freer Washington).<sup>5</sup> [To these the Bodmer Papyri must now be added.]

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<sup>1</sup>Burton, *The Traditional Text*, p. 40.

<sup>2</sup>Zuntz, *The Text*, pp. 6-7.

<sup>3</sup>Oliver, pp. 312-13.

<sup>4</sup>Colwell, "Hort Redivivus," p. 157.

<sup>5</sup>Clark, "The Manuscripts of the Greek New Testament," p. 3.

But what are Egypt's claims upon our confidence? And how wise is it to follow the witness of only one locale? Anyone who finds the history of the text presented herein to be convincing will place little confidence in the earliest MSS.

### Their quality judged by themselves

Quite apart from the history of the transmission of the text, the earliest MSS bear their own condemnation on their faces. P<sup>66</sup> is widely considered to be the earliest extensive manuscript. What of its quality? Again I borrow from Colwell's study of P<sup>45</sup>, P<sup>66</sup>, and P<sup>75</sup>. Speaking of "the seriousness of intention of the scribe and the peculiarities of his own basic method of copying," he continues:

On these last and most important matters, our three scribes are widely divided. P<sup>75</sup> and P<sup>45</sup> seriously intend to produce a good copy, but it is hard to believe that this was the intention of P<sup>66</sup>. The nearly 200 nonsense readings and 400 itacistic spellings in P<sup>66</sup> are evidence of something less than disciplined attention to the basic task. To this evidence of carelessness must be added those singular readings whose origin baffles speculation, readings that can be given no more exact label than carelessness leading to assorted variant readings. A hurried count shows P<sup>45</sup> with 20, P<sup>75</sup> with 57, and P<sup>66</sup> with 216 purely careless readings. As we have seen, P<sup>66</sup> has, in addition, more than twice as many "leaps" from the same to the same as either of the others.<sup>6</sup>

Colwell's study took into account only singular readings—readings with no other MS support. He found P<sup>66</sup> to have 400 itacisms plus 482 other singular readings, 40 percent of which are nonsensical.<sup>7</sup> "P<sup>66</sup> editorializes as he does everything else—in a sloppy fashion."<sup>8</sup> In short, P<sup>66</sup> is a very poor copy and yet it is one of the earliest!

P<sup>75</sup> is placed close to P<sup>66</sup> in date. Though not as bad as P<sup>66</sup>, it is scarcely a good copy. Colwell found P<sup>75</sup> to have about 145 itacisms plus 257 other singular readings, 25 percent of which are nonsensical.<sup>9</sup> Although Colwell gives the scribe of P<sup>75</sup> credit for having tried to produce a good copy, P<sup>75</sup> looks good only by comparison with P<sup>66</sup>. (If you were asked to write out the Gospel of John by hand, would you make over 400 mistakes?<sup>10</sup> Try it and see!) It should be kept in mind that the figures offered by Colwell deal only with errors which are the exclusive property of the respective MSS. They doubtless contain many other errors which happen to be found in some other witness(es) as well. In other words, they are actually worse even than Colwell's figures indicate.

P<sup>45</sup>, though a little later in date, will be considered next because it is the third member in Colwell's study. He found P<sup>45</sup> to have approximately 90 itacisms plus 275 other singular readings, 10 percent of which are nonsensical.<sup>11</sup> However P<sup>45</sup> is shorter than P<sup>66</sup> (P<sup>75</sup> is longer) and so is not comparatively so much better as the figures might suggest at first glance. Colwell comments upon P<sup>45</sup> as follows:

Another way of saying this is that when the scribe of P<sup>45</sup> creates a singular reading, it almost always makes sense; when the scribes of P<sup>66</sup> and P<sup>75</sup> create singular readings, they frequently do not make sense and are obvious errors. Thus P<sup>45</sup> must be given credit for a much greater density of intentional changes than the other two.<sup>12</sup>

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<sup>6</sup>Colwell, "Scribal Habits," pp. 378-79.

<sup>7</sup>*Ibid.*, pp. 374-76.

<sup>8</sup>*Ibid.*, p. 387.

<sup>9</sup>*Ibid.*, pp. 374-76.

<sup>10</sup>I am probably being unfair to the scribe who produced P<sup>75</sup>—some or many of those errors may have been in his exemplar. The fact remains that whatever their origin P<sup>75</sup> contains over 400 clear errors and I am trying by the suggested experiment to help the reader visualize how poor these early copies really are. Carson takes a different view. "If P<sup>75</sup>, a second-century papyrus [?], is not recensional, then it must be either extremely close to the original or extremely corrupt. The latter possibility appears to be eliminated by the witness of B" (p. 117). How so? If P<sup>75</sup> is "extremely corrupt" and B was copied from it, or something similar, then B must also be extremely corrupt. (Hoskier supplies objective evidence to that effect in *Codex B and its Allies*.)

<sup>11</sup>Colwell, "Scribal Habits," pp. 374-76.

<sup>12</sup>*Ibid.*, p. 376.

As an editor the scribe of P<sup>45</sup> wielded a sharp axe. The most striking aspect of his style is its conciseness. The dispensable word is dispensed with. He omits adverbs, adjectives, nouns, participles, verbs, personal pronouns—without any compensating habit of addition. He frequently omits phrases and clauses. He prefers the simple to the compound word. In short, he favors brevity. He shortens the text in at least fifty places in **singular readings alone**. But he does **not** drop syllables or letters. His shortened text is readable.<sup>13</sup>

Of special significance is the possibility of affirming with certainty that the scribe of P<sup>45</sup> deliberately and extensively shortened the text. Colwell credits him with having tried to produce a good copy. If by "good" he means "readable," fine, but if by "good" we mean a faithful reproduction of the original, then P<sup>45</sup> is bad. Since P<sup>45</sup> contains many deliberate alterations it can only be called a "copy" with certain reservations.

P<sup>46</sup> is thought by some to be as early as P<sup>66</sup>. Zuntz's study of this manuscript is well-known. "In spite of its neat appearance (it was written by a professional scribe and corrected—but very imperfectly—by an expert), P<sup>46</sup> is by no means a good manuscript. The scribe committed very many blunders . . . My impression is that he was liable to fits of exhaustion."<sup>14</sup>

It should be remarked in passing that Codex B is noted for its "neat appearance" also, but it should not be assumed that therefore it must be a good copy. Zuntz says further: "P<sup>46</sup> abounds with scribal blunders, omissions, and also additions."<sup>15</sup>

. . . the scribe who wrote the papyrus did his work very badly. Of his innumerable faults, only a fraction (less than one in ten) have been corrected and even that fraction—as often happens in manuscripts—grows smaller and smaller towards the end of the book. Whole pages have been left without any correction, however greatly they were in need of it.<sup>16</sup>

Hoskier, also, has discussed the "large number of omissions" which disfigure P<sup>46</sup>.<sup>17</sup> Again Zuntz says: "We have observed that, for example, the scribe of P<sup>46</sup> was careless and dull and produced a poor representation of an excellent tradition. Nor can we ascribe the basic excellence of this tradition to the manuscript from which P<sup>46</sup> was copied (we shall see that it, too, was faulty)."<sup>18</sup>

It is interesting to note that Zuntz feels able to declare the **parent** of P<sup>46</sup> to be faulty also. But, that P<sup>46</sup> represents an "excellent tradition" is a gratuitous assertion, based on Hort's theory. What is incontrovertible is that P<sup>46</sup> as it stands is a very poor copy—as Zuntz himself has emphatically stated.

Aland says concerning P<sup>47</sup>: "We need not mention the fact that the oldest manuscript does not necessarily have the best text. P<sup>47</sup> is, for example, by far the oldest of the manuscripts containing the full or almost full text of the Apocalypse, but it is certainly not the best."<sup>19</sup>

### Their quality judged between themselves

As to B and Aleph, we have already noted Hoskier's statement that these two MSS disagree over 3,000 times in the space of the four Gospels. Simple logic imposes the conclusion that one or the other must be wrong over 3,000 times—that is, they have over 3,000 mistakes between them. (If you were to write out the four Gospels by hand do you suppose you could manage to make 3,000 mistakes, or 1,500?) Aleph and B disagree, on the average, in almost every verse of the Gospels. Such a showing seriously undermines their credibility.

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<sup>13</sup>*Ibid.*, p. 383.

<sup>14</sup>Zuntz, *The Text*, p. 18.

<sup>15</sup>*Ibid.*, p. 212.

<sup>16</sup>*Ibid.*, p. 252.

<sup>17</sup>H.C. Hoskier, "A Study of the Chester-Beatty Codex of the Pauline Epistles," *The Journal of Theological Studies*, XXXVIII (1937), 162.

<sup>18</sup>Zuntz, *The Text*, p. 157.

<sup>19</sup>Aland, "The Significance of the Papyri," p. 333.



Burgon personally collated what in his day were "the five old uncials" (Ⲛ, A, B, C, D). Throughout his works he repeatedly calls attention to the *concordia discors*, the prevailing confusion and disagreement, which the early uncials display between themselves. Luke 11:2-4 offers one example.

"The five Old Uncials" (ⲚABCD) falsify the Lord's Prayer as given by St. Luke in no less than forty-five words. But so little do they agree among themselves, that they throw themselves into six different combinations in their departures from the Traditional Text; and yet they are never able to agree among themselves as to one single various reading: while only once are more than two of them observed to stand together, and their grand point of union is no less than an omission of the article. Such is their eccentric tendency, that in respect of thirty-two out of the whole forty-five words they bear in turn solitary evidence.<sup>20</sup>

Mark 2:1-12 offers another example.

In the course of those 12 verses . . . there will be found to be 60 variations of reading. . . . Now, in the present instance, the 'five old uncials' **cannot be** the depositories of a tradition—whether Western or Eastern—because they render inconsistent testimony **in every verse**. It must further be admitted, (for this is really not a question of opinion, but a plain matter of fact,) that it is unreasonable to place confidence in such documents. What would be thought in a Court of Law of five witnesses, called up 47 times for examination, who should be observed to bear contradictory testimony **every time**?<sup>21</sup>

Hort, also, had occasion to notice an instance of this *concordia discors*. Commenting on the four places in Mark's Gospel (14:30, 68, 72a,b) where the cock's crowing is mentioned he said: "The confusion of attestation introduced by these several cross currents of change is so great that of the seven principal MSS Ⲛ A B C D L Δ no two have the same text in all four places."<sup>22</sup> He might also have said that in these four places the seven uncials present themselves in **twelve** different combinations (and only A and Δ agree together three times out of the four). If we add W and Θ the confusion remains the same except that now there are thirteen combinations. Are such witnesses worthy of credence?

Recalling Colwell's effort to reconstruct an "Alexandrian" archetype for chapter one of Mark, either Codex B is wrong 34 times in that one chapter or else a majority of the remaining primary "Alexandrian" witnesses is wrong, and so for Aleph and L, etc. Further, Kenyon admitted that B is "disfigured by many blunders in transcription."<sup>23</sup> Scrivener said of B:

One marked feature, characteristic of this copy, is the great number of its omissions. . . . That no small portion of these are mere oversights of the scribe seems evident from the circumstance that this same scribe has repeatedly written words and clauses **twice over**, a class of mistakes which Mai and the collators have seldom thought fit to notice, . . . but which by no means enhances our estimate of the care employed in copying this venerable record of primitive Christianity.<sup>24</sup>

Even Hort conceded that the scribe of B "reached by no means a high standard of accuracy."<sup>25</sup> Aleph is acknowledged on every side to be worse than B in every way.

Codex D is in a class by itself. Said Scrivener:

The internal character of the Codex Bezae is a most difficult and indeed an almost inexhaustible theme. No known manuscript contains so many bold and extensive interpolations (six hundred, it is said, in the Acts alone). . . . Mr. Harris from curious internal evidence, such as the existence in the text of a vitiated rendering of a verse of Homer which bears signs of

<sup>20</sup>Burgon, *The Traditional Text*, p. 84.

<sup>21</sup>Burgon, *The Revision Revised*, pp. 30-31.

<sup>22</sup>Westcott and Hort, p. 243.

<sup>23</sup>Kenyon, *Handbook*, p. 308.

<sup>24</sup>Scrivener, *A Plain Introduction*, I, 120.

<sup>25</sup>Westcott and Hort, p. 233.

having been retranslated from a Latin translation, infers that the Greek has been made up from the Latin.<sup>26</sup>

Hort spoke of "the prodigious amount of error which D contains."<sup>27</sup> Burgon concluded that D resembles a Targum more than a transcription.<sup>28</sup>

### **Their quality judged by the ancient Church**

If these are our best MSS we may as well agree with those who insist that the recovery of the original wording is impossible, and turn our minds to other pursuits. But the evidence indicates that the earliest MSS are the worst. It is clear that the Church in general did not propagate the sort of text found in the earliest MSS, which demonstrates that they were not held in high esteem in their day.

Consider the so-called "Western" text-type. In the Gospels it is represented by essentially one Greek MS, Codex Bezae (D, 05), plus the Latin versions (sort of). So much so that for many years no critical text has used a cover symbol for "Western". In fact, K. and B. Aland now refer to it simply as the "D" text (their designation is objective, at least). The Church universal simply refused to copy or otherwise propagate that type of text. Nor can the Latin Vulgate legitimately be claimed for the "Western" text—it is more "Byzantine" than anything else (recall that it was translated in the 4th century).

Consider the so-called "Alexandrian" text-type. In more recent times neither the UBS nor the Nestle texts use a cover symbol for this "text" either (only for the "Byzantine"). F. Wisse collated and analyzed 1,386 MSS for chapters 1, 10 and 20 of Luke.<sup>29</sup> On the basis of shared mosaics of readings he was able to group the MSS into families, 15 "major" groups and 22 lesser ones. One of the major ones he calls "Egyptian" ("Alexandrian")—it is made up of precisely four uncials and four cursives, plus another two of each that are "Egyptian" in one of the three chapters. Rounding up to ten, that makes ten out of 1,386—less than 1%!

Again, the Church universal simply refused to copy or otherwise propagate that type of text. Codex B has no "children". Codex Aleph has no "children"—in fact, it is so bad that across the centuries something like 14 different people worked on it, trying to fix it up (but no one copied it). Recall Colwell's study wherein he tried to arrive at the archetype of the "Alexandrian" text in chapter one of Mark on the basis of the 13 MSS presumed to represent that type of text. They were so disparate that he discarded the seven "worst" ones and then tried his experiment using the remaining six. Even then the results were so bad—Codex B diverged from the mean text 34 times (just in one chapter)—that Colwell threw up his hands and declared that such an archetype never existed. If Colwell is correct then the "Alexandrian" text-type *cannot represent the Autograph*. The Autograph is the ultimate archetype, and it did indeed exist.

Consider one more detail. Zuntz says of the scribe of P<sup>46</sup>: "Of his innumerable faults, only a fraction (less than one in ten) have been corrected and even that fraction—as often happens in manuscripts—grows smaller and smaller towards the end of the book. Whole pages have been left without any correction, however greatly they were in need of it."<sup>30</sup>

A similar thing happens in P<sup>66</sup>. Why? Probably because the corrector lost heart, gave up. Perhaps he saw that the transcription was so hopelessly bad that no one would want to use it, even if he could patch it up. It should also be noted that although many collations and discussions of MSS ignore errors of spelling, to a person in the year 250 wishing to **use** a copy, for devotional study or whatever, errors in spelling would be just as annoying and distracting as more serious ones. A copy like P<sup>66</sup>, with roughly two mistakes per verse, would be set aside in disgust.

Further, how could the early MSS survive for 1,500 years if they had been used? (I have worn out several Bibles in my short life.) Considering the relative difficulty of acquiring copies in those days

<sup>26</sup>Scrivener, *A Plain Introduction*, I, 130. Cf. Rendel Harris, *A Study of the Codex Bezae* (1891).

<sup>27</sup>Westcott and Hort, p. 149.

<sup>28</sup>Burgon, *The Traditional Text*, pp. 185-90.

<sup>29</sup>F. Wisse, *The Profile Method for Classifying and Evaluating Manuscript Evidence* (Grand Rapids: Eerdmans, 1982).

<sup>30</sup>Zuntz, *The Text*, p. 252.

(expensive, done by hand) any worthy copy would have been used until it wore out. Which brings us to the next possible objection.

## Why Are There No Early "Byzantine" MSS?

Why would or should there be? To demand that a MS survive for 1,500 years is in effect to require both that it have remained unused and that it have been stored in Egypt (or Qumran). Even an unused MS would require an arid climate to last so long.

But is either requirement reasonable? Unless there were persons so rich as to be able to proliferate copies of the Scriptures for their health or amusement, copies would be made on demand, in order to be **used**. As the use of Greek died out in Egypt the demand for Greek Scriptures would die out too, so we should not expect to find many Greek MSS in Egypt.

It should not be assumed, however, that the "Byzantine" text was not used in Egypt. Although none of the early Papyri can reasonably be called "Byzantine", they each contain "Byzantine" readings. The case of P<sup>66</sup> is dramatic. The first hand was extensively corrected, and both hands are dated around A.D. 200. The 1st hand is almost half "Byzantine" (a. 47%), but the 2nd hand regularly changed "Byzantine" readings to "Alexandrian" and *vice versa*, i.e. he changed "Alexandrian" to "Byzantine", repeatedly. This means that they must have had two exemplars, one "Alexandrian" and one "Byzantine"—between the two hands the "Byzantine" text receives considerable attestation.

Consider the case of Codex B and P<sup>75</sup>; they are said to agree 82% of the time (unprecedented for "Alexandrian" MSS, but rather poor for "Byzantine"). But what about the 18% discrepancy? Most of the time, when P<sup>75</sup> and B disagree one or the other agrees with the "Byzantine" reading. If they come from a common source, that source would have been more "Byzantine" than either descendant. Even the Coptic versions agree with the "Byzantine" text as often as not.

### "Orphan children"

The study and conclusions of Lake, Blake, and New, already discussed in a prior section, are of special interest here. They looked for evidence of direct genealogy and found virtually none. I repeat their conclusion.

. . . the manuscripts which we have are almost all orphan children without brothers or sisters.

Taking this fact into consideration along with the negative result of our collation of MSS at Sinai, Patmos, and Jerusalem, it is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred books.<sup>31</sup>

Is it unreasonable to suppose that once an old MS became tattered and almost illegible in spots the faithful would make an exact copy of it and then destroy it, rather than allowing it to suffer the indignity of

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<sup>31</sup>Lake, Blake and New, p. 349. D.A. Carson offers the following response to this suggestion: "The answers to this ingenious theory are obvious: (1) If only one copy were made before the exemplar was destroyed, there would never be more than one extant copy of the Greek New Testament! (2) If several copies were made from one exemplar, then either (a) they were not all made at the same time, and therefore the destruction of the exemplar was not a common practice after all; or (b) they were all made at the same time. (3) If the latter obtains, then it should be possible to identify their sibling relationship; yet in fact such identification is as difficult and as precarious as the identification of direct exemplar/copy manuscripts. This probably means we have lost a lot of manuscripts; and/or it means that the divergences between copy and exemplar, as between copy and sibling copy, are frequently difficult to detect. (4) Why are there **no** copies of the Byzantine text before about A.D. 350, and **so many** [emphasis Carson's] from there on? This anomaly, it might be argued, demonstrates that the practice of destroying the exemplar died out during the fourth century" (*The King James Version Debate*, Grand Rapids: Baker Book House, 1979, pp. 47-48).

Perhaps it is fortunate that Lake is no longer available for comment upon this extraordinary statement. If I may presume to answer for him, it seems to me apparent that what Lake found was the end of the line, the last generation of copies. Neither Lake nor anyone else has suggested that only one copy would be made of any exemplar, but after a life of use and being copied a worn and tattered MS would be destroyed. Carson's point (4) is hard to believe. Lake, Blake, and New were looking at minuscule MSS, probably none earlier than the tenth century—they had to be copied from something, and it is a fact that Lake and company found no "parents." Carson offers no explanation for this *fact*. And what are we to understand from his strange remark about "Byzantine" MSS before and after A.D. 350? There are none from the fourth century, unless W (Matthew) be placed there, two partially so from the fifth, and a slowly expanding stream as one moves up through the succeeding centuries. It is only when we come to the minuscule era that we find "so many." Please see the next section, "the ninth century transliteration process," to find out why.

literally rotting away? What would such a practice do to our chances of finding an early "Byzantine" MS? Anyone who objects to this conclusion must still account for the fact that in three ancient monastic libraries equipped with scriptoria (rooms designed to facilitate the faithful copying of MSS), there are only "orphan children." Why are there no parents?!

Van Bruggen addresses the problem from a slightly different direction. He says of the "Byzantine" text:

The fact that this text-form is known to us via later manuscripts is as such no proof for a late text-type, but it does seem to become a proof when at the same time a different text is found in all older manuscripts. The combination of these two things seems to offer decisive proof for the late origin of the traditional text.<sup>32</sup>

He answers the "seeming proof" in the following way:

Let us make ourselves aware of **what** we have presupposed with this seemingly convincing argumentation. What conditions must be satisfied if we wish to award the prize to the older majuscules? While asking this question we assumed wittingly or unwittingly that we were capable of making a fair comparison between manuscripts in an earlier period and those in a later period. After all, we can only arrive at positive statements if that is the case. Imagine that someone said: in the Middle Ages mainly cathedrals were built, but in modern times many small and plainer churches are being built. This statement seems completely true when we today look around in the cities and villages. Yet we are mistaken. An understandable mistake: many small churches of the Middle Ages have disappeared, and usually only the cathedrals were restored. Thus, a great historical falsification of perspective with regard to the history of church-building arises. We are not able to make a general assertion about church-building in the Middle Ages on the basis of the surviving materials. If we would still dare to make such an assertion, then we wrongly assumed that the surviving materials enabled us to make a fair comparison. But how is the situation in the field of New Testament manuscripts? Do we have a **representative** number of manuscripts from the first centuries? Only if that is the case, do we have the right to make conclusions and positive statements. Yet it is just at this point that difficulties arise. The situation is even such that we know with certainty that we **do not** possess a representative number of manuscripts from the first centuries.<sup>33</sup>

The conclusion of Lake, Blake, and New reflects another consideration. The age of a manuscript must not be confused with the age of the text it exhibits. Any copy, by definition, contains a text that is older than it is. In Burgon's words, it "represents a MS, or a pedigree of MSS, older than itself; and it is but fair to suppose that it exercises such representation with tolerable accuracy."<sup>34</sup>

## The ninth century transliteration process

Van Bruggen discusses yet another relevant consideration.

In the codicology the great value of the transliteration process in the 9th century and thereafter is recognized. At that time the most important New Testament manuscripts written in majuscule script were carefully transcribed into minuscule script. It is assumed that after this transliteration-process the majuscule was taken out of circulation. . . . The import of this datum has not been taken into account enough in the present New Testament textual criticism. For it implies, that just the oldest, best and most customary manuscripts come to us in the new uniform of the minuscule script, does it not? This is even more cogent since it appears that various archetypes can be detected in this transliteration-process for the New Testament. Therefore we do not receive one mother-manuscript through the flood-gates of the transliteration, but several. The originals have, however, disappeared! This throws a totally different light on the situation that we are confronted with regarding the manuscripts. Why do

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<sup>32</sup>Van Bruggen, p. 24.

<sup>33</sup>*Ibid.*, p. 25.

<sup>34</sup>Burgon, *The Traditional Text*, p. 47.

the surviving ancient manuscripts show another text-type? Because they are the only survivors of their generation, and because their survival is due to the fact that they were of a different kind. Even though one continues to maintain that the copyists at the time of the transliteration handed down the wrong text-type to the Middle Ages, one can still never prove this codicologically with the remark that older majuscules have a different text. This would be circular reasoning. There certainly were majuscules just as venerable and ancient as the surviving Vaticanus or Sinaiticus, which, like a section of the Alexandrinus, presented a Byzantine text. But they have been renewed into minuscule script and their majuscule appearance has vanished. Historically it **seems** as though the most ancient majuscule manuscripts exclusively contain a non-Byzantine text, but the perspective [sic] is falsified here just like it is regarding church-building in the Middle Ages and at present.<sup>35</sup>

The significance of the transliteration process was explained by A. Dain as follows: "The transliterated copy, carefully written and securely bound, became the reference point for the subsequent tradition. The old papyrus and parchment exemplars that had been copied, doubtless quite worn out, were of no further interest and were usually discarded or destroyed."<sup>36</sup> Apparently there was an organized movement to "transliterate" uncial MSS into minuscule form or script. Note that Dain agrees with Lake that the "worn out" exemplars were then destroyed (some may have been "recycled", becoming palimpsests). What if those exemplars were ancient "Byzantine" uncials? Come to think of it, they must have been since the cursives are "Byzantine".

C.H. Roberts comments upon a practice of early Christians that would have had a similar effect.

It was a Jewish habit both to preserve manuscripts by placing them in jars . . . and also to dispose of defective, worn-out, or heretical scriptures by burying them near a cemetery, not to preserve them but because anything that might contain the name of God might not be destroyed. . . . It certainly looks as if this institution of a morgue for sacred but unwanted manuscripts was taken over from Judaism by the early Church.<sup>37</sup>

Note that the effect of this practice in any but an arid climate would be the decomposition of the MSS. If "Byzantine" exemplars, worn out through use, were disposed of in this way (as seems likely), they would certainly perish. All of this reduces our chances of finding really ancient "Byzantine" MSS. Nor is that all.

### **Imperial repression of the N.T.**

There is a further consideration. "It is historically certain that the text of the New Testament endured a very hard time in the first centuries. Many good and official editions of the text were confiscated and destroyed by the authorities during the time of the persecutions."<sup>38</sup>

Roberts refers to "the regular requisition and destruction of books by the authorities at times of persecution, so often recorded in the martyr acts."<sup>39</sup> Such official activity seems to have come to a climax in Diocletian's campaign to destroy the New Testament manuscripts around A.D. 300.

If there was any trauma in the history of the normal transmission of the text, this was it; the more so since the campaign evidently centered upon the Aegean area. Many MSS were found, or betrayed, and burned, but others must have escaped. That many Christians would have spared no effort to hide and preserve their copies of the Scriptures is demonstrated by their attitude towards those who gave up their MSS—the Donatist schism that immediately followed Diocletian's campaign partly hinged on the question of punishment for those who had given up MSS. The Christians whose entire devotion to the Scriptures was thus demonstrated would also be just the ones that would be the most careful about the pedigree of their own MSS; just as they took pains to protect their MSS they presumably would have taken pains to ensure that their MSS preserved the true wording.

<sup>35</sup>Van Bruggen, pp. 26-27.

<sup>36</sup>A. Dain, *Les Manuscrits* (Paris, 1949), p. 115.

<sup>37</sup>C.H. Roberts, p. 7.

<sup>38</sup>Van Bruggen, p. 29. Cf. Eusebius, *Historia Ecclesiastica* VIII, II, 1.4 and F.H.A. Scrivener, *A Plain Introduction*, pp. 265-66.

<sup>39</sup>Roberts, p. 8.

In fact, the campaign of Diocletian may even have had a purifying effect upon the transmission of the text. If the laxity of attitude toward the text reflected in the willingness of some to give up their MSS also extended to the quality of text they were prepared to use, then it may have been the more contaminated MSS that were destroyed, in the main, leaving the purer MSS to replenish the earth.<sup>40</sup> But these surviving pure MSS would have been in unusually heavy demand for copying (to replace those that had been destroyed) and been worn out faster than normal.

In short, if the history of transmission presented herein is valid we should not necessarily expect to find any early "Byzantine" MSS. They would have been used and worn out. (But the text they contained would be preserved by their descendants.) An analogy is furnished by the fate of the *Biblia Pauperum* in the fifteenth century.

### **The *Biblia Pauperum***

Of all the *Xylographic* works, that is, such as are printed from wooden blocks, the *BIBLIA PAUPERUM* is perhaps the rarest, as well as the most ancient; it is a manual, or kind of catechism of the Bible, for the use of young persons, and of the common people, whence it derives its name,—*Biblia Pauperum—the Bible of the Poor*; who were thus enabled to acquire, at a comparatively low price, an imperfect knowledge of some of the events recorded in the Scriptures. Being much in use, the few copies of it which are at present to be found in the libraries of the curious are for the most part either mutilated or in bad condition. The extreme rarity of this book, and the circumstances under which it was produced, concur to impart a high degree of interest to it.<sup>41</sup>

Although it went through five editions, presumably totaling thousands of copies, it was so popular that the copies were worn out by use. I maintain that the same thing happened to the ancient "Byzantine" MSS.

Adding to all this the discussion of the quality of the earliest MSS, in the prior section, early age in a MS might well arouse our suspicions—why did it survive? And that brings us to a third possible objection.

### **"But There Is No Evidence of the Byzantine Text in the Early Centuries"**

Although Hort and Kenyon stated plainly that no "Syrian readings" existed before, say, A.D. 250, their present day followers have been obliged by the early papyri to retreat to the weaker statement that it is all the readings together, the "Byzantine" ("Syrian") text that had no early existence. Ehrman states the position as baldly as anyone: "No early Greek Father from anywhere in the early Christian world, no Latin nor Syriac Father, and no early version of the New Testament gives evidence of the existence of the Syrian text prior to the fourth century."<sup>42</sup>

### **Evidence from the early Fathers**

This question has already received some attention in Chapter 4, "'Syrian' Readings before Chrysostom," but K. Aland offers us some fascinating new evidence. In "The Text of the Church?" he offers a tabulation of patristic citations of the N.T.<sup>43</sup> The significance of the evidence is somewhat obscured by the presentation, which seems to be a bit tendentious. The turn of phrase is such as to lead the unwary reader to an exaggerated impression of the evidence against the Majority Text. E.g., Origen is said to be: "55% against the Majority text (30% of which show agreement with the 'Egyptian text'), 28% common to both texts, and 17% with the Majority text."  $55 + 28 + 17 = 100$ . The problem lies with the "of which". In normal English the "of which" refers to the 55% (not 100%); so we must calculate 30% of 55%, which gives

<sup>40</sup>Here was an excellent opportunity for the "Alexandrian" and "Western" texts to forge ahead and take "space" away from the "Byzantine", but it did not happen. The Church rejected those types of text. How can modern critics possibly be in a better position to identify the true text than was the Church universal in the early 4th century?

<sup>41</sup>T.H. Horne, *An Introduction to the Critical Study and Knowledge of the Holy Scriptures*, 4th American edition (4 vols.; Philadelphia: E. Little, 1831), vol. II, p. 217. I am indebted to Maurice Robinson for calling this material to my attention.

<sup>42</sup>Ehrman, p. 72.

<sup>43</sup>K. Aland, "The Text of the Church?", *Trinity Journal*, 1987, 8NS:131-144 [actually published in 1989], p. 139.

us 16.5% (of the total). 55 minus 16.5 leaves 38.5% which is neither Egyptian nor Majority, hence "other". I will chart the statistics unambiguously, following this interpretation.

<u>Father</u>	<u>date</u>	<u>Egyptian alone</u>	<u>both E&amp;M</u>	<u>Majority alone</u>	<u>other (-EM)</u>	<u># of pass.</u>
Marcion	(160?)	23%	10%	18%	49%	94
Irenaeus	(d.202)	16%	16.5%	16.5%	51%	181
Clement Alex.	(d.215)	13.5%	29%	15%	42.5%	161
Hippolytus	(d.235)	14.5%	31%	19%	46.5%	33**
		13.5%	18%	21%	43.5%	21
		14.5%	18%	21%	46.5%	33
Origen	(d.254)	16.5%	28%	17%	38.5%	459
Methodius	(280?)	12.5%	31%	19%	37.5%	32
Adamantius	(d.300)	11.5%	21%	31%	36.5%	29
Asterius	(d.341)	---	40%	50%	10%	30
Basil	(d.379)	2.5%	39%	40%	18.5%	249
Apost. Const.	(380?)	3%	33%	41%	23%	46
Epiphanius	(d.403)	11%	33%	41%	37%	114**
		11%	30%	22%	37%	114
Chrysostom	(d.407)	2%	38%	40.5%	19.5%	915
Severian	(d.408)	3%	37%	30%	30%	91
Theod. Mops.	(d.428)	4.5%	29%	39%	27.5%	28
Marcus Erem.	(d.430)	5.5%	35%	35%	24.5%	37
Theodotus	(d.445)	3%	37.5%	37.5%	22%	16
Hesychius	(d.450)	3.5%	37%	33%	26.5%	84
Theodoret	(d.466)	1%	41%	42%	16%	481
John Damascus	(d.749)	2%	40%	40%	18%	63

\*\* (With reference to Hippolytus and Epiphanius, the first line reflects the statistics as given in Aland's article, but they do not add up to 100%. The second line reflects the statistics as given in a pre-publication draft of the same article distributed by the American Bible Society. For Epiphanius the second line is probably correct, since it adds up to 100%—the 33 and 41 were presumably copied from the line above. For Hippolytus the second line doesn't add up either; so we are obliged to engage in a little textual criticism to see if we can recover the original. The third line gives my guess—the 31 and 19 were probably borrowed from the line below [in his article Methodius is placed before Origen—I put them in chronological order]. Six errors in the pre-publication draft were corrected, but another four were created.)

One thing becomes apparent at a glance. With the sole exception of Marcion, each of the Fathers used the Majority Text **more than** the Egyptian. Even in Clement and Origen (in Egypt, therefore) the Majority text is preferred over the Egyptian, and by the end of the third century the preference is unambiguous. This is startling, because it goes against almost everything that we have been taught during this entire century. Perhaps we have misconstrued Aland's statement. Returning to Origen, we are told that he is "55% against the Majority text (30% of which show agreement with the 'Egyptian text'), . . ." On second thought, the "of which" is probably supposed to refer to the total. In that event a less ambiguous way of presenting the statistics would be to say: "30% with the Egyptian text, 17% with the Majority text, 28% common to both and 25% differing from both." I will chart his statistics in this way, using "other" for the last category.

<u>Father</u>	<u>date</u>	<u>Egyptian alone</u>	<u>both E&amp;M</u>	<u>Majority alone</u>	<u>other (-EM)</u>	<u># of pass.</u>
Marcion	(160?)	32%	10%	18%	40%	94
Irenaeus	(d.202)	24%	16.5%	16.5%	43%	181
Clement Alex.	(d.215)	24%	29%	15%	32%	161
Hippolytus	(d.235)	24%	18%	21%	37%	33
Origen	(d.254)	30%	28%	17%	25%	459
Methodius	(280?)	25%	31%	19%	25%	32
Adamantius	(d.300)	24%	21%	31%	24%	29
Asterius	(d.341)	---	40%	50%	10%	30
Basil	(d.379)	11%	39%	40%	10%	249
Apost. Const.	(380?)	11%	33%	41%	15%	46
Epiphanius	(d.403)	23%	30%	22%	25%	114
Chrysostom	(d.407)	8.5%	38%	40.5%	13%	915
Severian	(d.408)	9%	37%	30%	24%	91
Theod. Mops.	(d.428)	14%	29%	39%	18%	28
Marcus Erem.	(d.430)	19%	35%	35%	11%	37
Theodotus	(d.445)	12.5%	37.5%	37.5%	12.5%	16
Hesychius	(d.450)	12%	37%	33%	18%	84
Theodoret	(d.466)	6%	41%	42%	11%	481
John Damascus	(d.749)	11%	40%	40%	9%	63

(I will assume that this second display is more probably what Aland intended, so any subsequent discussion of the evidence from these early Fathers will be based upon it.)

Something that Aland does not explain, but that absolutely demands attention, is the extent to which these early Fathers apparently cited neither the Egyptian nor the Majority texts—a plurality for the first four. Should this be interpreted as evidence against the authenticity of both the Majority and Egyptian texts? Probably not, and for the following reason: a careful distinction must be made between citation, quotation and transcription. A responsible person transcribing a copy will have the exemplar before him and will try to reproduce it exactly. A person quoting a verse or two from memory is liable to a variety of tricks of the mind and may create new readings which do not come from any textual tradition. A person citing a text in a sermon will predictably vary the turn of phrase for rhetorical effect. All Patristic citation needs to be evaluated with these distinctions in mind and must not be pushed beyond its limits.

### **Evidence from Clement of Alexandria**

I wish to explore this question a little further by evaluating a transcription of Mark 10:17-31 done by Clement of Alexandria. Clement's text is taken from *Clement of Alexandria*, ed. G.W. Butterworth (Harvard University Press, 1939 [The Loeb Classical Library]); *Clemens Alexandrinus*, ed. Otto Stahlin (Berlin: Akademie-Verlag, 1970); the Library of Greek Fathers (Athens, 1956, vol. 8). It is compared to UBS<sup>3</sup> as a representative of the Egyptian text, to the H-F Majority Text as a representative of the Byzantine text, and to Codex D as a representative of the "Western" text. The Greek text of these four sources has been arranged for ease of comparison and is given on the following pages. The four lines in each set are always given in the same order: Clement first [where the three editions are not in full agreement, I follow two



against one], Majority Text second, UBS<sup>3</sup> third and Codex D fourth. The result is interesting and, I think, instructive.

Clem. 17) --- εκπορευομενω αυτω εις οδον προσελθων τις --- εγονυπετει ---  
 MT και εκπορευομενου αυτου εις οδον προσδραμων εις και γονυπετησας αυτον επιρωτα αυτον  
 UBS “ “ “ “ “ “ “ “ “ “ “ “ “  
 Bezae “ “ “ “ “ “ “ “ “ γονυπετων “ ηρωτα “

λεγων διδασκαλε αγαθε τι αγαθον ποιησω ινα ζων αιωνιον κληρονομησω 18) ο δε Ιησους λεγει --- τι  
 --- “ “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ ειπεν αυτω τι  
 --- “ “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ “  
 λεγων “ “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ “

με αγαθον λεγεις ουδεις αγαθος ει μη --- εις ο Θεος 19) τας εντολας οιδας μη μοιχευσας μη φονευσης  
 με λεγεις αγαθον “ “ “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ “  
 “ “ “ “ “ “ “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ φονευσης μη μοιχευσης  
 “ “ “ “ “ “ “ “ “ μονος “ -- “ “ “ “ “ “ “ “ “ “ “ μοιχευσης μη πορνευσης

μη κλεψης μη ψευδομαρτυρησης -- --- τιμα τον πατερα -- και την μητερα 20) ο δε αποκρι-  
 “ “ “ “ “ μη αποστερησης “ “ “ σου “ “ “ “ “ “ “ “  
 “ ---  
 “ “ “ ψευδομαρτυρησεις “ αποστερησεις τειμα “ “ -- “ “ “ “ “ “ αποκρι-

θεις λεγει αυτω --- παντα ταυτα εφυλαξα -- --- -- 21) ο δε Ιησους εμβλεψας ---  
 “ ειπεν “ διδασκαλε ταυτα παντα εφυλαξαμην εκ νεωτητος μου “ “ “ “ αυτω  
 -- εφη “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “  
 θεις ειπεν “ “ παντα ταυτα εφυλαξα “ “ “ “ “ “ “ “ “

ηγαπησεν αυτον και ειπεν -- εν σοι υστερει ει θελεις τελειος ειναι --- πωλησον οσα εχεις και διαδος  
 “ “ “ “ αυτω “ “ “ --- --- --- υπαγε οσα εχεις πωλησον “ δος  
 “ “ “ “ “ “ σε “ --- --- --- “ “ “ “ “ “ “  
 “ “ “ “ “ “ σοι “ --- --- --- “ “ “ “ “ “ “

-- πτωχοις και εξεις θησαυρον εν ουρανω και δευρο ακολουθει μοι --- --- --- 22) ο δε στυ-  
 --- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ αρας τον σταυρον “ “ “  
 (τοις) “

τοις “ “ “ “ “ “ “ “ “ “ “ “ ----- “ “ εστν-

γνασας επι -- --τω λογω -- --απηλθεν λυπουμενος ην γαρ πλουσιος εχων κτηματα πολλα και αγρους 23)

“ “ ----- “ “ -- “ “ “ “ ----- “ “ “ -----

“ “ ----- “ “ -- “ “ “ “ ----- “ “ “ -----

γνασεν “ τουτω “ “ και “ “ “ “ ----- “ πολλα χρηματα -- --

-- περιβλεψαμενος δε ο Ιησους λεγει τοις μαθηταις αυτου πως δυσκολως οι τα χρηματα εχοντες εισε-  
και “ “ -- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ -----

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ -----

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ -----

λευσονται εις την βασιλειαν του Θεου -- -- 24) οι δε μαθηται -- -- εθαμβουντο επι τοις λογοις

----- “ “ “ “ “ “ “ “ “ “ “ “ “ ----- “ “ “ “ “

----- “ “ “ “ “ “ “ “ “ “ “ “ “ ----- “ “ “ “ “

----- “ “ “ “ “ “ “ “ “ “ “ “ “ ----- “ “ “ “ “

αυτου παλιν δε ο Ιησους αποκριθεις λεγει αυτοις τεκνια πως δυσκολον εστιν τους πεποιθοτας επι ----

“ ο δε Ιησους παλιν “ “ “ “ τεκνια “ “ “ “ “ “ “ “ -----

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ -----

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- τους πεποιθοτας επι τοις

χρημασιν εις την βασιλειαν του Θεου εισελθειν 25) ευκολωτερον -- -- δια της τρυμαλιας της βελονης

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- ευκοπωτερον εστι καμηλον δια της τρυμαλιας της

----- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- (“ “ (“

χρημασιν “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- ( fragmented ) καμηλος “ -- τρυμαλιδος --

καμηλος εισελευσεται η πλουσιος εις την βασιλειαν του Θεου -- -- 26) οι δε περισσως εξεπλησσαντο

ραφιδος εισελθειν η πλουσιον “ “ “ “ “ “ “ “ “ “ ----- εισελθειν “ “ “ “ “

“ διελθειν “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ -----

“ διελευσεται η πλουσιος “ “ “ “ “ “ “ “ “ “ ----- “ “ “ “ “

και ελεγον ---- ---- -- τις ουν δυναται σωθηναι 27) ο δε εμβλεψας -- αυτοις ---- ειπεν οτι

λεγοντες προς εαυτους και “ -- “ “ “ ----- “ δε “ ο Ιησους λεγει --

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- “ “ “ “ “ “

“ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ ----- ενβλεψας δε “ “ “ “ “

44 D has a lacuna.

45 D inverts vv. 24 and 25.

παρα ανθρωποις --- αδυνατον --- παρα --- Θεω --- δυνατον εστιν ---  
 “ “ --- “ --- αλλ ου “ --- “ παντα γαρ δυνατα “ παρα τω Θεω  
 “ “ --- “ --- “ “ “ “ “ “ “ “ “  
 “ “ τουτο “ εστιν --- “ δε τω “ --- δυνατον ---

28) -- ηρξατο ο Πετρος λεγειν αυτω ιδε ημεις αφηκαμεν παντα και ηκολουθησαμεν σοι 29) αποκριθεις  
 -- “ “ “ “ “ ιδου “ “ “ “ “ “ “ “  
 -- “ λεγειν ο Πετρος “ “ “ “ “ “ ηκολουθηκαμεν “ εφη  
 και “ --Πετρος λεγειν “ ειδ ου “ “ “ “ “ “ αποκριθεις

-- ο Ιησους λεγει αμην υμιν λεγω ος αν αγη τα ιδια και γονεις και αδελφους ---  
 δε<sup>46</sup> “ “ ειπεν “ λεγω υμιν ουδεις εστιν ος αφηκεν οικιαν η “ η αδελφας η πατερα η  
 --- “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ μητερα “  
 δε “ “ --- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “

--- και χρηματα ενεκεν εμου και ενεκεν του ευαγγελιου μου 30) αποληψεται  
 μητερα η γυναικα η τεκνα η αγρους “ “ “ “ “ “ “ -- εαν μη λαβη  
 πατερα--- “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “ “  
 --- “ η ενεκα “ “ -- ος αν μη λαβη

εκατομπλασιονα νυν εν τω καιρω τουτω αγρους και χρηματα και οικιας και αδελφους ---  
 εκατονταπλασιονα “ “ “ “ “ “ --- “ “ “ “ και αδελφας  
 “  
 “ --- “ “ “ “ “ “ ος δε αφηκεν οικειαν “ αδελφας “ αδελφους

--- μετα διωγων εν δε τω --- ερχομενω ζωη εστιν αιωνιος  
 και μητερας και τεκνα και αγρους “ “ και εν “ αιωνι τω “ ζων --- αιωνιον  
 “  
 “ μητερα “ “ “ “ “ “ διωγμου-- “ “ “ “ “ “ “ “ “

--- 31) πολλοι δε εσονται πρωτοι εσχατοι και οι εσχατοι πρωτοι.  
 --- “ “ “ “ “ “ “ “ “ “  
 --- “ “ “ “ “ “ “ (οι) “ “  
 λημψεται “ “ “ “ “ “ “ “ “ “

<sup>46</sup> The true MT probably agrees with UBS here.

The total number of variation units in this passage may vary slightly according to differing ways of defining such units (e.g., I treated each long omission as a single variant), but the same basic patterns will emerge. According to my calculation:

Clement has a total of 58 "singular" readings (within this comparison),

Codex D " " " " 40 " " ,

UBS<sup>3</sup> " " " " 10 " " ,

MT " " " " 4 " " .

Further, Clement and Codex D agree alone together 9 times,

" " MT " " " 5 " ,

" " UBS<sup>3</sup> " " " 1 " .

This does not necessarily mean that Clement is more closely related to D than to the others. Within the variation units:

the total agreements between Clement and Codex D are 14,

" " " " " " UBS<sup>3</sup> " 26,

" " " " " " MT " 33.

It thus appears that of the three most commonly mentioned "text-types"—Byzantine, Egyptian, and Western—Clement has least relationship to the "Western" (in this passage), although the 9 singular agreements suggest some common influence. It has been commonly stated that Clement is one of the most "Alexandrian" or "Egyptian" of the early Church Fathers, in terms of his textual preference. In this passage, at least, Clement is closer to the Byzantine than to the Egyptian text-type. 24 of the 26 UBS<sup>3</sup> agreements with Clement are in common with the MT.

Codex D has long been notorious for its "eccentricity", and this passage provides an eloquent example. But compared to Clement Codex D almost looks tame. I would say that Clement has over 60 mistakes (involving over 120 words) in these 15 verses, or an average of four mistakes per verse! How should we account for such a showing?

Conventional wisdom would argue that with a passage so extensive as this one, 15 verses, the father must have been copying an exemplar that was open in front of him. But it is hard to imagine that an exemplar could have been this bad, or that Clement would have used it if one did exist. I feel driven to conclude that Clement transcribed the passage from memory, but was not well served. I wonder if this doesn't give us a possible explanation for the statistics offered by Aland.

Comparing "other", "Egyptian" and "Majority" the four earliest fathers have "other" leading with a plurality. Among them is Clement, who sides with "other" 32%. However, Aland's statistics are based on a selection of variation units (variant sets) considered to be "significant". If we plot all of Clement's readings within the variation units in Mark 10:17-31 (as given above) on the same chart we get:

E = 2(2%)    E&M = 24(23.5%)    M = 9(9%)    O = 67(65.5%)    # 102

The value of "other" rose dramatically. This is because O does not represent a recognizable text-type. In this exercise E and M are discrete entities (UBS<sup>3</sup> and MT) while O is a wastebasket that includes singular readings and obvious errors. Perhaps we could agree that true singular readings should be excluded from such tabulations, but any limitation of variant sets beyond that will presumably be influenced by the bias of whoever conducts the exercise.

So what conclusions should we draw from this study of Clement? I submit that all statements about the testimony of the early Fathers need to be re-evaluated. Most NT citations were presumably from memory—in that case allowance must be made for capricious variation. If they would be likely to make stylistic alterations of the sort that are typical of the Egyptian text (such as moving toward classical Greek)

they could happen to make the same "improvement" independently. Such fortuitous agreements would not signal genealogical relationship. Also, anti-Byzantine bias needs to be set aside. For instance, faced with Clement's preference for Majority readings in Mark 10:17-31 it is predictable that some will try to argue that medieval copyists "corrected" Clement toward the Byzantine norm. But in that event, why didn't they also correct all the singular readings? Question begging tactics, such as assuming that the Byzantine text was a secondary development, need to be dropped.

Now I wish to return to the chart of the Fathers (the second one) and apply my classification (see Chapter 5) to those statistics. The result looks like this:

<u>II &amp; III</u>		<u>IV</u>		<u>V</u>	
Marcion	O- (45%)	Asterius	M++ (83%)	Theod. Mops.	M (55%)
Irenaeus	O (51.5)	Basil	M (66)	Marcus Erem.	M (54)
Clement Al.	O- (45)	Apost. Const.	M (61.5)	Theodotus	M (60)
Hippolytus	O- (44.5)	Epiphanius	O- (36)	Hesychius	M (53)
Origen	E- (41.5)	Chrysostom	M (65)	Theodoret	M+ (71%)
Methodius	E/O(36.5)	Severian	M- (47.5)		
Adamantius	M- (39%)				

(Epiphanius, Chrysostom and Severian presumably did most of their writing in the IV century, and their MSS would date well back into it.)

I imagine that almost everyone who has studied NT textual criticism, as generally taught in our day, will be surprised by this picture. Where is the Egyptian text? The II and III centuries are dominated by **O**—only in Origen does **E** manage a plurality while tying with **O** in Methodius. By the end of the III century (Adamantius), **M** has taken the lead, and is in clear control of the IV and V. The detractors of the Byzantine text have habitually argued that while Byzantine "readings" may be attested in the early centuries the earliest extant attestation for the Byzantine "text", as such, comes from the V. In contrast, say they, the Egyptian "text" is attested in the III and IV. Well, the tabulations of actual readings from the Fathers and uncials that Aland has furnished seem to tell a different story. In the first place, just what is the "Egyptian text"? How did Aland arrive at the "norm"? Could it be that there is no Egyptian "text" at all, just "readings"? Many of the readings that have fallen under "**O**" have frequently been called "Western". There are Western "readings", but is there a Western "text"? Many scholars would say no. If there is no Western "text", how can there be Western "readings"? On what basis is a reading to be identified as "Western"? How about the Byzantine "text", can it be objectively defined? Yes. That is why we can tell when we are looking at a Byzantine "reading"—it is characteristic of that objectively defined "text". If the Byzantine "readings" that occur in the II and III century Fathers and papyri do not constitute evidence for the existence of the "text", then neither do the Egyptian and Western "readings" constitute evidence for those "texts".

### Evidence from the early papyri

On page 140 Aland also appeals to the papyri: "There is not a trace to be found of the Majority text (as defined by Hodges and his colleagues) in any of the forty-plus papyri of the early period (prior to the period of Constantine), or of the fifty more to the end of the 8th century." He is referring to "text", not "readings", but what does he mean by "not a trace"? In normal usage a "trace" is not very much. After his tabulation of the citations in the earliest Fathers, Aland states: "At least one thing is clearly demonstrated: it is impossible to say that the existence outside Egypt in the early period of what Hodges calls the 'Egyptian text' is unproved" (p. 139). He then refers to the first five Fathers by name. Notice that he is claiming that the 24% preference for Egyptian "readings" in Irenaeus, for example, "proves" the **existence** of the Egyptian **text** outside Egypt in the II century. If 24% is enough to prove the existence of a "text", surely 18% would qualify as a "trace"? If Aland's argument here is valid then Marcion's 18% preference for Majority "readings" proves the existence of the Majority **text** in the middle of the II century! If Aland is unwilling to grant that the percentage of Byzantine "readings" to be found in these early Fathers constitutes a "trace", then presumably they contain no trace of the Egyptian text either. But what about the papyri?

Unfortunately Aland's book does not contain a summary of the "systematic test collation"<sup>47</sup> for the papyri, as it does for the uncials, so brief mention will be made of Eldon Epp's study of P<sup>45</sup> and Gordon Fee's study of P<sup>66</sup>. With reference to 103 variation units in Mark 6-9 (where P<sup>45</sup> is extant) Epp records that P<sup>45</sup> shows a 38% agreement with D, 40% with the TR, 42% with B, 59% with f<sup>13</sup>, and 68% with W.<sup>48</sup> Fee records that in John 1-14 P<sup>66</sup> shows a 38.9% agreement with D, 44.6% with Aleph, 45.0% with W, 45.6% with A, 47.5% with the TR, 48.5% with C, 50.4% with B, and 51.2% with P<sup>75</sup>.<sup>49</sup> Does 40% not constitute a "trace"? The picture is similar to that offered by the early Fathers. If we plotted these papyri on a chart with the same headings there would be a significant number of variants in each column—"Egyptian", "Majority" and "other" were all important players on the scene in Egypt at the end of the second century.

Mention should be made of the study done by Harry A. Sturz.<sup>50</sup> He himself collated P<sup>45,46,47,66,72</sup> and <sup>75</sup>, but took citations of P<sup>13</sup> and P<sup>37</sup> from apparatuses in Nestle texts (p. 140). He compared these papyri with the Byzantine, Alexandrian and Western texts throughout the NT. He charts the results as follows:

<u>Readings Compared</u>	<u>Number of Occurrences</u>	<u>Percentage of Total</u>
PB/A/W	31	6.3
PB/AW	121	24.7
PBW/A	169	34.4
PBA/W	<u>170</u>	<u>34.6</u>
Total:	491	100.0%

"PB = papyrus readings supporting the Byzantine text; A = the Alexandrian text; and W = the Western text. Thus PB/A/W means the Papyrus-Byzantine readings are being compared against the Alexandrian where it differs from the Western readings" (p. 228). It thus appears that Sturz identified 152 places where early papyri side with the Byzantine text against both the Alexandrian and Western texts. He gives evidence for 175 further papyrus-supported Byzantine readings but which have scattered Western or Alexandrian support as well, and thus are not "distinctively Byzantine" (pp. 189-212). He refers to still another 195 cases where the Byzantine reading has papyrus support, but he doesn't list them (p. 187). The 169 PBW/A instances remind us of the statement made by Gunther Zuntz. "Byzantine readings which recur in Western witnesses **must** [emphasis his] be ancient. They go back to the time before the Chester Beatty papyrus [P<sup>46</sup>] was written; the time before the emergence of separate Eastern and Western traditions; in short, they reach back deep into the second century."<sup>51</sup> One could wish that Sturz had also given us the PA/BW and PW/AB alignments, but he didn't. In any case, doesn't all that early papyrus attestation of Byzantine readings deserve to be called at least a "trace"?

## Evidence from the early Versions

It has been affirmed that the early versions, Latin, Syriac and Coptic, do not witness to the "Byzantine" text. This is part of the larger question-begging procedure, wherein these versions are assigned to the Alexandrian or Western "text-types" (whose own existence has not been demonstrated) and thus denied to the "Byzantine" text. But what would happen if we looked at the performance of these versions without any such preconceived ideas? I just did a rough check of the statements of evidence in the UBS<sup>3</sup> apparatus for John. 172 variant sets are listed (recall that they included only "significant" ones), but 13 of them are variant sets within disputed verses—these I disregarded since the prior question is whether or not

<sup>47</sup>Not only that, we are not given the criteria used in choosing the variant sets to be collated. Similarly, we are not given the criteria used in choosing Fathers and citations for his article, "The Text of the Church?". Considering Aland's anti-Byzantine bias, we are probably safe in assuming that no choices were made so as to favor the "Byzantine" text; in that event a wider sampling could well increase the Byzantine percentages.

<sup>48</sup>Eldon Epp, "The Twentieth Century Interlude in New Testament Textual Criticism," *Journal of Biblical Literature*, XCIII (1974), pp. 394-96.

<sup>49</sup>G.D. Fee, *Papyrus Bodmer II (P<sup>66</sup>): Its Textual Relationships and Scribal Characteristics* (Salt Lake City: U. of Utah Press, 1968), p. 14.

<sup>50</sup>H.A. Sturz, *The Byzantine Text-Type and New Testament Textual Criticism* (Nashville: Thomas Nelson, 1984).

<sup>51</sup>G. Zuntz, *The Text*, pp. 150-51.

to include the passage. That left 159, some three dozen of which were not very applicable (some differences are ambiguous in a translation). With reference to the Latin, Syriac and Coptic witness, I asked whether it was **with** the Byzantine text, **against** it, or if there was a significant split. Here is the result of that rough count:<sup>52</sup>

	With	Against	Split
Latin	60	32	27
Syriac	63	23	35
Coptic	49	45	27

Even the Coptic sides with the Byzantine more often than not, but the tendency of both the Latin and the Syriac is clearly toward the Byzantine. And there seems to be no predictable correlation between any of these versions and the important early uncials and papyri. The Old Latin frequently disagrees with D, for instance, or divides. I would say that the Old Latin gives clear testimony to the early existence of the Byzantine "text". If the Syriac and Coptic do not witness to the Byzantine "text" then presumably they may not be claimed for any other "text" either.

### Summary and conclusion

The distinction between "readings" and "text" is commonly made in a misleading way. For instance, it is not legitimate to speak of "Western" readings until one has defined a "Western" **text**, as such. To define a "text" one should reconstruct the presumed archetype. Having done so, then one can identify the readings that are peculiar to that archetype and therefore characteristic of it. No one has ever reconstructed a "Western" archetype, and there is general agreement among scholars that there never was one. That is why critical editions of the Greek NT do not include a cover symbol for the "Western" text. In their recent textbook the Alands now speak of the "D" text, referring to Codex Bezae. It follows that it is not legitimate to speak of "Western" readings. It is even less legitimate to assign MSS, Fathers or Versions to the phantom "Western" text. It is true that early MSS, Fathers and Versions certainly contain many readings that are neither "Alexandrian" nor "Byzantine", but they appear to be largely random, with a common influence discernible here and there. If the "Western" text has no archetype, it cannot represent the original.

Similarly, it is not legitimate to speak of "Alexandrian" readings until one has reconstructed the presumed archetype. Colwell tried and gave it up, declaring that it never existed. The UBS editions and N-A<sup>26</sup> no longer use a cover symbol for the "Alexandrian" text. By Aland's figures, the strongest "Alexandrian" witness, Codex B, is only 72% 'pure' in the Synoptics—where shall we go to find the other 28%? P<sup>75</sup> and B are said to have an 82% agreement—where shall we go for the other 18%? The witnesses commonly assigned to the "Alexandrian" text are in constant and significant disagreement between and among themselves. A common influence is indeed discernible, but there is a great deal of seemingly random variation as well. They all show significant agreements with the "Byzantine" text, in different places and in varying amounts. In fact, Codex C is more "Byzantine" than "Alexandrian" in the Synoptics. Since there is no "Alexandrian" archetype in hand, I challenge the legitimacy of speaking of "Alexandrian" readings and of claiming early MSS, Fathers and Versions for that supposed "text". If the "Alexandrian" text has no archetype, it cannot represent the original.

In contrast, a "Byzantine" or "Majority" archetype can indeed be reconstructed, with over 99% certainty. This is why modern critical editions of the Greek NT still use a cover symbol for this type of text. It follows that it is entirely legitimate to speak of "Byzantine" or "Majority" readings—they are defined by the archetype. Since the "Byzantine" archetype is the only one that has been demonstrated to exist, where did it come from if not the Autographs?

In any case, the considerations presented demonstrate that if the evidence from the II and III centuries does not attest the presence of the Byzantine **text**, then neither does it attest the presence of the Western or Alexandrian "texts". However, I affirm that the evidence is clear to the effect that the Byzantine "text", as such, must have existed in the II century.

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<sup>52</sup>Peter J. Johnston did an independent evaluation of this material and concluded that I was too cautious; especially in the case of the Syriac the attestation for the "Byzantine" text is stronger than my figures indicate (personal communication).

## Should Not Witnesses Be Weighed, Rather Than Counted?

The form of the question, which reflects that of the assertion usually made, is tendentious. It infers that weighing and counting are mutually exclusive. But why? In any investigation, legal or otherwise, witnesses should be **both** weighed and counted. First they should be weighed, to be sure, but then they must be counted—or else why bother weighing them, or why bother with witnesses at all? I will discuss the two activities in order, beginning with the weighing.

### Weighing first

Just how are MSS to be weighed? And who might be competent to do the weighing? As the reader is by now well aware Hort and most subsequent scholars have done their "weighing" on the basis of so-called "internal evidence"—the two standard criteria are, "choose the reading which fits the context" and "choose the reading which explains the origin of the other reading."

One problem with this has been well stated by Colwell. "As a matter of fact these two standard criteria for the appraisal of the internal evidence of readings can easily cancel each other out and leave the scholar free to choose in terms of his own prejudgments."<sup>53</sup> Further, "the more lore the scholar knows, the easier it is for him to produce a reasonable defense of both readings. . . ."<sup>54</sup>

The whole process is so subjective that it makes a mockery of the word "weigh." The basic meaning of the term involves an evaluation made by an objective instrument. If we wish our weighing of MSS to have objective validity we must find an objective procedure.

How do we evaluate the credibility of a witness in real life? We watch how he acts, listen to what he says and how he says it, and listen to the opinion of his neighbors and associates. If we can demonstrate that a witness is a habitual liar or that his critical faculties are impaired then we receive his testimony with skepticism. It is quite possible to evaluate MSS in a similar way, to a considerable extent, and it is hard to understand why scholars have generally neglected to do so.

Please refer back to the evidence given in the discussion of the oldest MSS. Can we objectively "weigh" P<sup>66</sup> as a witness? Well, in the space of John's Gospel it has over 900 clear, indubitable errors—a witness to the identity of the text of John it has misled us over 900 times. Is P<sup>66</sup> a credible witness? I would argue that neither of the scribes of P<sup>66</sup> and P<sup>75</sup> knew Greek; should we not say that as witnesses they were impaired?<sup>55</sup>

Recall from Colwell's study that the scribe of P<sup>45</sup> evidently made numerous **deliberate** changes in the text—should we not say that he was morally impaired? In any case, he has repeatedly misinformed us. Shall we still trust him?

Similarly, it has been shown by simple logic/arithmetic that Aleph and B have over 3,000 mistakes between them, just in the Gospels. Aleph is clearly worse than B, but probably not twice as bad—at least 1,000 of those mistakes are B's. Do Aleph and B fit your notion of a good witness?

Even when it is not possible to affirm objectively that a particular witness is misinformed, his credibility suffers if he keeps dubious company. Several references have already been given to the phenomenon Burgon called *concordia discors*. I will add one more. Burgon invites us to turn to Luke 8:35-44 and collate the five old uncials ℵ, A, B, C, D throughout these verses. Comparing them to each other against the background of the majority of MSS—A stands alone 2 times; B, 6 times; ℵ, 8 times; C, 15 times; D, 93 times—A and B stand together by themselves once; B and ℵ, 4 times; B and C, once; B and D, once; ℵ and C, once; C and D, once—A, ℵ and C conspire once; B, ℵ and C, once; B, ℵ and D, once;

<sup>53</sup>Colwell, "External Evidence," p. 3.

<sup>54</sup>*Ibid.*, p. 4.

<sup>55</sup>The fact that the transcriber of P<sup>75</sup> copied letter by letter and that of P<sup>66</sup> syllable by syllable (Colwell, "Scribal Habits," p. 380) suggests strongly that neither one knew Greek. When transcribing in a language you know you copy phrase by phrase, or at the very least word by word. P<sup>66</sup> has so many nonsensical readings that the transcriber could not have known the meaning of the text. Anyone who has ever tried to transcribe a text of any length by hand (not typewriter) in a language he does not understand will know that it is a taxing and dreary task. Purity of transmission is not to be expected under such circumstances.



A, B,  $\aleph$  and C, once; B,  $\aleph$ , C and D, once. Not once do all five agree against the majority. As Burgon observed, they "combine, and again stand apart, with singular impartiality," which led him to conclude:

Will any one, after a candid survey of the premises, deem us unreasonable, if we avow that such a specimen of the *concordia discors* which everywhere prevails between the oldest uncials, but which especially characterizes  $\aleph$  B D, indisposes us greatly to suffer their unsupported authority to determine for us the Text of Scripture?<sup>56</sup>

Must we not agree with him?

We need also to check out the opinion of a witness' contemporaries. Do they testify to his good character, or are there reservations? To judge by the circumstance that Codices like Aleph and B were not copied, to speak of, that the Church by and large rejected their form of the text, it seems they were not respected in their day. What objective evidence do we have to lead us to reverse the judgment of their contemporaries?

Scholars like Zuntz will protest that a MS may represent an excellent tradition in spite of the poor job done by the scribe.<sup>57</sup> Perhaps so, but how can we know? I see only two ways of reaching the conclusion that a certain tradition is excellent—either through the testimony of witnesses that commend themselves as dependable, or through the preference and imagination of the critic. In neither case does the conclusion depend upon the poor copy itself—in the one case it rests upon the authority of independent, dependable witnesses, and in the other it rests upon the authority of the critic. The poor copy itself has no claims on our confidence.

### Counting next<sup>58</sup>

Having weighed the witnesses, we must then count them. In the counting, preference must be given to those copies that are not demonstrably poor, or bad. Just as before the law a person is considered innocent until proven guilty, so a witness must be assumed to be truthful until it can be proved a liar. But before counting, we must try to determine if there has been any collusion among the witnesses. Any that appear to be mutually dependent should be lumped together. Then, each witness that appears to be both independent and trustworthy must be allowed to vote; such witnesses must indeed be counted. If several hundred such witnesses agree against three or four inveterate prevaricators, can there be any reasonable doubt as to the identity of the true reading? I will return to this matter in the following chapter.

Should anyone still care to raise the objection that "Byzantine readings repeatedly prove to be inferior," I reply: "Prove it!" Since all such characterizations have been based upon the demonstrably fallacious canons of "internal evidence" they have no validity. I consider the allegation to be vacuous. I would also require that he openly state his presuppositions. Differing presuppositions normally lead to differing conclusions.

I have demonstrated that the W-H critical theory and history of the text are erroneous. I have outlined the history of the transmission of the text which I believe best accords with the available evidence. It remains to give a coherent statement of the procedure by which we may assure ourselves of the precise identity of the original wording of the New Testament text.

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<sup>56</sup>Burgon, *The Revision Revised*, pp. 16-18.

<sup>57</sup>Cf. Zuntz, *The Text*, p. 157.

<sup>58</sup>Carson's representation of my position here calls for some comment. He says that I argue that "we must view most manuscripts as independent authorities that ought to be counted, not weighed" (p. 108). "Should not manuscripts be weighed, not counted? Pickering thinks counting is to be preferred because he has already dispensed with the genealogical principle—at least to his own satisfaction" (p. 107). "The only alternative [to eclecticism] is to resort to a method of counting manuscripts" (p. 105). Does not the reader of Carson's critique have the right to assume that he read my book with reasonable care? If Carson did so read my book he has deliberately misrepresented my position, as the reader can easily verify.

## DETERMINING THE IDENTITY OF THE TEXT

By way of a framework for the following discussion I shall use Burgon's seven "Notes of Truth." They are:

1. Antiquity, or Primitiveness;
2. Consent of Witnesses, or Number;
3. Variety of Evidence, or Catholicity;
4. Respectability of Witnesses, or Weight;
5. Continuity, or Unbroken Tradition;
6. Evidence of the Entire Passage, or Context;
7. Internal Considerations, or Reasonableness.<sup>1</sup>

### The "Notes of Truth"

#### Antiquity, or Primitiveness

A reading, to be a serious candidate for the original, should be old. If there is no attestation for a variant before the middle ages it is unlikely to be genuine. A word of caution is required here, however. Not only may age be demonstrated by a single early witness, but also by the agreement of a number of later independent witnesses—their common source would have to be a good deal older. Sturz has a good discussion of this point.<sup>2</sup> But any reading that has wide late attestation almost always has explicit early attestation as well.

To give a concrete definition to the idea of "antiquity" I will take the year 400 A.D. as an arbitrary cut-off point. Allowing only those witnesses who "spoke" before that date, "antiquity" would include over seventy Fathers, Codices Aleph and B and a number of fragmentary uncials, the early Papyri and the earliest Versions. By way of specific illustration, ever since 1881 the word "vinegar" in Matt. 27:34 has been despised as a "late, Byzantine" reading—but what is the verdict of "antiquity"? Against it are Codices Aleph and B, the Latin and Coptic versions, the Apocryphal Acts of the Apostles, the Gospel of Nicodemus, and Macarius Magnes—seven witnesses. In favor of it are the Gospel of Peter, *Acta Philippi*, Barnabas, Irenaeus, Tertullian, Celsus, Origen, pseudo-Tatian, Athanasius, Eusebius of Emesa, Theodore of Heraclea, Didymus, Gregory of Nyssa, Gregory Nazianzus, Ephraem Syrus, Lactantius, Titus of Bostra and the Syriac version—eighteen witnesses.<sup>3</sup> The witnesses for "vinegar" are both older and more numerous than those for "wine."

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<sup>1</sup>Burgon, *The Traditional Text*, p. 29. I acknowledge with alacrity my considerable indebtedness to Dean Burgon, especially for arousing me to look at the **evidence**, but my treatment of the various "notes" is not identical to his. Fee says of these "notes", "all of which are simply seven different ways of saying that the majority is always right" ("A Critique," p. 423). It should be apparent to the reader, just at a glance, that Fee's statement is irresponsible. It is a clear illustration of the carelessness and superficiality which characterize much of his critique.

<sup>2</sup>Sturz, pp. 67-70.

<sup>3</sup>Burgon, *The Traditional Text*, pp. 107, 255-56. Regarding this statement of evidence Fee says the following: "I took the trouble to check over three-quarters of Burgon's seventeen supporting Fathers and **not one of them** [emphasis Fee's] can be shown to be citing Matthew!" (*Ibid.*, pp. 418-19). (The term *οξος*, "vinegar," also occurs in the near-parallel passages—Mark 15:36, Luke 23:36 and John 19:29.) Before checking the Fathers individually, we may register surprise at Fee's vehemence in view of his own affirmation that it is "incontrovertible" that "the Gospel of Matthew was the most cited and used of the Synoptic Gospels" and that "these data simply cannot be ignored in making textual decisions" (*Ibid.*, p. 412). We are grateful to Fee for this information but cannot help but notice that he himself seems to be "ignoring" it. We might reasonably assume that at least nine of Burgon's 17 citations are from Matthew. But we are not reduced to such a weak proceeding.

Even though a Father may not say, "I am here quoting Matthew," by paying close attention to the context we may be virtually as certain as if he had. Thus, although all four Gospels use the word "vinegar," only Matthew uses the word "gall", *χολε*, in association with the vinegar (and Acts 8:23 is the only other place in the N.T. that "gall" appears). It follows that any Patristic reference to vinegar and gall together can only be a citation based on Matthew (or Ps. 69:21). When Barnabas says, *ποτιζειν χολεν μετα οξος* (7:5), can there be any doubt as to his source? When the Gospel of Peter says *Ποτισατε αυτον χολεν μετα οξους* (5:16), must the source not be Matthew? When Gregory of Nyssa says, *χολε τε και οξει διαβροχος* (*Orat.* x:989:6), can there be any question at all? It may be noted in passing that Alford's Greek N.T.,

Of course, age by itself is not enough. We have seen that most significant variants date to the second century. What we are after is the oldest reading, the original, and to judge between competing old readings we need other considerations.

### Consent of Witnesses, or Number

A reading, to be a serious candidate for the original, should be attested by a majority of the *independent* witnesses. Please recall the discussion of weighing and counting given above. A reading attested by only a few witnesses is unlikely to be genuine—the fewer the witnesses the smaller the likelihood. Conversely, the greater the majority the more nearly certain is the originality of the reading so attested. Wherever the text has unanimous attestation the only reasonable conclusion is that it is certainly original.<sup>4</sup>

Even Hort acknowledged the presumption inherent in superior number. "A theoretical presumption indeed remains that a majority of extant documents is more likely to represent a majority of ancestral documents at each stage of transmission than *vice versa*."<sup>5</sup> The work of those who have done extensive collating of MSS has tended to confirm this presumption. Thus Lake, Blake, and New found only orphan children among the MSS they collated, and declared further that there were almost no siblings—each MS is an "only child."<sup>6</sup> This means they are independent witnesses, in their own generation. In Burgon's words:

. . . hardly any have been copied from any of the rest. On the contrary, they are discovered to differ among themselves in countless unimportant particulars; and every here and there single copies exhibit idiosyncrasies which are altogether startling and extraordinary. There has therefore demonstrably been no collusion—no assimilation to an arbitrary standard,—no wholesale fraud. It is certain that every one of them represents a MS., or a pedigree of MSS., older than itself; and it is but fair to suppose that it exercises such representation with tolerable accuracy.<sup>7</sup>

In accordance with good legal practice, it is unfair to arbitrarily declare that the ancestors were **not** independent; some sort of evidence must be produced. It has already been shown that Hort's "genealogical evidence," with reference to MSS, is fictitious. But it remains true that community of reading implies a common origin, unless it is the type of mistake that several scribes might have made independently. What is in view here is the common origin of individual readings, not of MSS, but where several MSS share a large number of readings peculiar to themselves their claim to independence is evidently compromised throughout. (The "Claremont Profile Method"<sup>8</sup> gives promise of being an effective instrument for plotting the relationship between MSS.)

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in *loc.*, says plainly that Origen and Tertullian both support the "Byzantine" reading under discussion. (The research reflected in the discussion above was done by Maurice A. Robinson and kindly placed at my disposal.)

Note also that Irenaeus wrote, "He should have vinegar and gall given Him to drink" (*Against Heresies*, XXXIII:12), in a series of O.T. prophecies that he says Christ fulfilled. Presumably he had Ps. 69:21 in mind—"they gave me gall for food, and in my thirst they gave me vinegar to drink"—but he seems to have assimilated to Mt. 27:34 (the "Byzantine" reading). The Gospel of Nicodemus has, "and gave him also to drink gall with vinegar" (Part II, 4). The Revelation of Esdras has, "Vinegar and gall did they give me to drink." The Apostolic Constitutions has, "they gave him vinegar to drink, mingled with gall" (V:3:14). Tertullian has, "and gall is mixed with vinegar" (Appendix, reply to Marcion, V:232). In a list of Christ's sufferings where the readers are exhorted to follow His example, Gregory Nazianzus has, "Taste gall for the taste's sake; drink vinegar" (*Oratio XXXVIII*:18).

Whatever interpretation the reader may wish to give to Fee's statement, noted at the outset, it is clear that the reading "vinegar" in Matthew 27:34 has second century attestation (or perhaps even first century in the case of Barnabas!). The reading in question passes the "antiquity" test with flying colors.

<sup>4</sup>Anyone who offers a conjectural emendation in the face of such attestation is claiming that his authority is greater than that of all the witnesses combined—but since such a person is not a witness at all, does not and cannot know what was written (having rejected 100% attestation), his authority is nil.

<sup>5</sup>Westcott and Hort, p. 45.

<sup>6</sup>Lake, Blake and New, pp. 348-49.

<sup>7</sup>Burgon, *The Traditional Text*, pp. 46-47.

<sup>8</sup>Cf. Epp, "The Claremont Profile Method for Grouping New Testament Minuscule Manuscripts," *Studies in the History and Text of the New Testament in Honor of Kenneth Willis Clark, Ph.D.* (*Studies and Documents*, 29) ed. B.L. Daniels and M.J. Suggs (Salt Lake City: University of Utah Press, 1967), pp. 27-38.

However, there is one situation where community of reading does not compromise independence. If the common origin of a reading is the original, then the MSS that have it may not be disqualified; their claim to independence remains unsullied. Of course, we do not know, at this stage in the inquiry, which is the original reading, but some negative help is immediately available. If one or more of the competing variants is an obvious mistake, then those MSS which attest such variants are disqualified, at that one point (recall that genealogy was supposed to be based upon community in **error**).

For the rest, the history of transmission becomes an important factor, but to trace it with confidence we must take account of at least two further considerations. In the meantime, the independent status of MSS agreeing in readings that could be original must be held in abeyance—there is not sufficient evidence to disqualify them, yet.

### **Variety of Evidence, or Catholicity**

A reading, to be a serious candidate for the original, should be attested by a wide variety of witnesses. By variety is meant, in the first place, many geographical areas, but also different kinds of witnesses—MSS, Fathers, Versions, and Lectionaries. The importance of "variety" is well stated by Burgon.

Variety distinguishing witnesses massed together must needs constitute a most powerful argument for believing such Evidence to be true. Witnesses of different kinds; from different countries; speaking different tongues:—witnesses who can never have met, and between whom it is incredible that there should exist collusion of any kind:—such witnesses deserve to be listened to most respectfully. Indeed, when witnesses of so varied a sort agree in large numbers, they must needs be accounted worthy of even implicit confidence. . . . Variety it is which imparts virtue to mere Number, prevents the witness-box from being filled with packed deponents, ensures genuine testimony. False witness is thus detected and condemned, because it agrees not with the rest. Variety is the consent of independent witnesses, . . .

It is precisely this consideration which constrains us to pay supreme attention to the combined testimony of the Uncials and of the whole body of the Cursive Copies. They are (a) dotted over at least 1000 years: (b) they evidently belong to so many divers countries, — Greece, Constantinople, Asia Minor, Palestine, Syria, Alexandria, and other parts of Africa, not to say Sicily, Southern Italy, Gaul, England, and Ireland: (c) they exhibit so many strange characteristics and peculiar sympathies: (d) they so clearly represent countless families of MSS., being in no single instance absolutely identical in their text, and certainly not being copies of any other Codex in existence,—that their unanimous decision I hold to be an absolutely irrefragable evidence of the Truth.<sup>9</sup>

Variety helps us evaluate the independence of witnesses. If the witnesses which share a common reading come from only one area, say Egypt, then their independence must be doubted. It seems quite unreasonable to suppose that an original reading should survive in only one limited locale. If the history of the transmission of the text was largely normal, as I believe it was, then we must conclude that a reading found only in one limited area cannot be original. It follows that witnesses supporting such readings are disqualified, just like those supporting obvious mistakes—they are not independent, at that point. They are disqualified as independent witnesses, but their combined testimony still counts as one vote; their common ancestor is still an independent witness.

As Burgon points out, it is variety that lends validity to number, because variety implies independence. Conversely, lack of variety implies dependence, which is why a reading that lacks variety of attestation has little claim upon our confidence. It is an eloquent testimony to the soporific effects of the W-H theory (with its "genealogy") that subsequent scholarship has largely ignored the factor of variety in attestation. There has been an occasional murmur of disquiet,<sup>10</sup> but nothing approaching a recognition of the

<sup>9</sup>Burgon, *The Traditional Text*, pp. 50-51. Anyone who has been taught that Burgon followed "mere number" will perceive that there is more to the story than that.

<sup>10</sup>Cf. Streeter, p. 148; Tasker, "Introduction to the Manuscripts of the New Testament," *Harvard Theological Review*, XLI (1948), 76; Metzger, *The Text*, p. 171; Clark, "The Manuscripts of the Greek New Testament," p. 3.

true place that "variety" should have in the practice of New Testament textual criticism. Burgon stated the obvious when he said:

Speaking generally, the consentient testimony of two, four, six, or more witnesses, coming to us from widely sundered regions is weightier by far than the same number of witnesses proceeding from one and the same locality, between whom there probably exists some sort of sympathy, and possibly some degree of collusion.<sup>11</sup>

Closely allied to variety is the factor of continuity.

### **Continuity, or Unbroken Tradition**

A reading, to be a serious candidate for the original, should be attested throughout the ages of transmission, from beginning to end. If the history of the transmission of the text was at all normal, we would expect the original wording "to leave traces of its existence and of its use all down the ages."<sup>12</sup> If a reading or tradition died out in the fourth or fifth century we have the verdict of history against it. If a reading has no attestation before the twelfth century, it is certainly a late invention.<sup>13</sup>

Where there is variety there is almost always continuity as well, but they are not identical considerations. Continuity also helps us in evaluating the independence of witnesses. Readings which form little eddies in the late "Byzantine" stream convict their supporters of dependence at those points. Readings which enjoy both a wide variety and continuity of attestation vindicate the independence of their supporters. Apart from some objective demonstration to the contrary (such as Hort claimed for "genealogy") it is not fair to reject the independence of such witnesses. They must be allowed to vote. The punch line is this: The majority of the extant MSS emerge as independent witnesses, in their generation, and they must be counted until such a time as complete collations permit an empiric grouping, like F. Wisse did in Luke 1, 10 and 20.<sup>14</sup>

Hort, followed by Zuntz and others,<sup>15</sup> rejected this consideration absolutely. But the reader is now in some position to judge for himself. Since there was no authoritative revision of the text in 300 A.D., or any other time, and since the evidence indicates a reasonably normal history of transmission, how can the validity of "continuity" as a "note of truth" be reasonably denied? In my view, the factors of number, variety, and continuity form the backbone of a sound methodology in textual criticism. They form a three-strand rope, not easily broken. But there are several other considerations which are helpful, on occasion and in their way.

### **Respectability of Witnesses, or Weight**

Whereas the previous four "notes" have centered on readings, this one centers on the witnesses. Whereas the "notes" of number, variety, and continuity help us to evaluate the independence of witnesses, this one is concerned with the credibility of a witness judged by its own performance. "As to the Weight which belongs to separate Copies, that must be determined mainly by watching their evidence. If they go wrong continually, their character must be low. They are governed in this respect by the rules which hold good in life."<sup>16</sup>

The evidence offered above in the discussion of the oldest MSS and of weighing versus counting must suffice to illustrate both the importance and the applicability of this "note." The oldest MSS can be objectively, statistically shown to be habitually wrong, witnesses of very low character, therefore. Their respectability quotient hovers near zero. Their great age only renders their behavior the more reprehensible.

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<sup>11</sup>Burgon, *The Traditional Text*, p. 52.

<sup>12</sup>*Ibid.*, p. 59.

<sup>13</sup>It seems to me to be frankly impossible that an original reading should have absolutely disappeared from the knowledge of the Church for over a millennium and then pop up magically in the twelfth century. I here refer to a single witness—hundreds of medieval MSS of necessity reflect an ancient text.

<sup>14</sup>F. Wisse, *The Profile Method for Classifying and Evaluating Manuscript Evidence* (Grand Rapids: Eerdmans, 1982).

<sup>15</sup>Westcott and Hort, p. 275; Zuntz, *The Text*, p. 84.

<sup>16</sup>Burgon, *The Traditional Text*, p. 58.

(I am reminded of young King Henry's rebuke to Falstaff.)<sup>17</sup> In particular, I fail to see how anyone can read Hoskier's *Codex B and its Allies* with attention and still retain respect for B and Aleph as witnesses to the New Testament text—they have been weighed and found wanting.

Since the modern critical and eclectic texts are based precisely on B and Aleph and the other early MSS, blind guides all, it is clear that modern scholars have severely ignored the consideration of respectability, as an objective criterion. I submit that this "note of truth" must be taken seriously; the result will be the complete overthrow of the type of text currently in vogue.

### Evidence of the Entire Passage, or Context

The "context" spoken of here is not what is usually understood by the word but is concerned with the behavior of a given witness in the immediate vicinity of the problem being considered. It is a specific and limited application of the previous "note."

As regards the precise form of language employed, it will be found also a salutary safeguard against error in every instance, to inspect with severe critical exactness the entire context of the passage in dispute. If in certain Codexes that context shall prove to be confessedly in a very corrupt state, then it becomes even self-evident that those Codexes can only be admitted as witnesses with considerable suspicion and reserve.<sup>18</sup>

An excellent illustration of the need for this criterion is furnished by Codex D in the last three chapters of Luke—the scene of Hort's famous "Western non-interpolations." After discussing sixteen cases of omission (where W-H deleted material from the TR) in these chapters, Burgon continues:

The **sole** authority for just half of the places above enumerated [Luke 22:19-20; 24:3, 6, 9, 12, 36, 40, 52] is a **single Greek codex**,—and that, the most depraved of all,—viz. Beza's D. It should further be stated that the only allies discoverable for D are a few copies of the old Latin. . . . When we reach down codex D from the shelf, we are reminded that, within the space of the three chapters of S. Luke's Gospel now under consideration, there are in all no less than 354 words omitted: **of which, 250 are omitted by D alone**. May we have it explained to us why, of those 354 words, only 25 are singled out by Drs. Westcott and Hort for permanent excision from the sacred Text? Within the same compass, no less than 173 words have been **added** by D to the commonly Received Text,—146, **substituted**,—243 **transposed**. May we ask how it comes to pass that of those 562 words not one has been promoted to their margin by the Revisionists?<sup>19</sup>

The focus here is upon Westcott and Hort. According to **their own judgment**, codex D has omitted 329 words from the genuine text of the last three chapters of Luke plus adding 173, substituting 146 and transposing 243. By their own admission the text of D here is in a fantastically chaotic state, yet in eight places they omitted material from the text on the **sole authority of D!** With the scribe on a wild omitting spree, to say nothing of his other iniquities, how can any value be given to the testimony of D in these chapters, much less prefer it above the united voice of every other witness?!?!

This Note of Truth has for its foundation the well-known law that mistakes have a tendency to repeat themselves in the same or in other shapes. The carelessness, or the vitiated atmosphere, that leads a copyist to misrepresent one word is sure to lead him into error about another. The ill-ordered assiduity which prompted one bad correction most probably did not rest there. And the errors committed by a witness just before or just after the testimony which is being sifted was given cannot but be held to be closely germane to the inquiry.<sup>20</sup>

<sup>17</sup>"How ill white hairs become a fool and jester!" (Shakespeare's *King Henry IV*, part 2, Act V, Scene V, about line 50).

<sup>18</sup>Burgon, *The Traditional Text*, p. 62.

<sup>19</sup>Burgon, *The Revision Revised*, pp. 77-78.

<sup>20</sup>Burgon, *The Traditional Text*, p. 65.

Apart from the patent reasonableness of Burgon's assertion, the studies of Colwell in P<sup>45</sup>, P<sup>66</sup>, and P<sup>75</sup> have demonstrated it to be true. We have already seen how Colwell was able, on the basis of the pattern of their mistakes, to give a clear and different characterization to each of the three copyists.<sup>21</sup> Here again, this "note of truth" seems to be completely ignored by current scholars. Why? Is its validity not obvious?

### Internal Considerations, or Reasonableness

This "note" has nothing to do with the "internal evidence" of which we have heard so much. It is only rarely applicable and concerns readings which are grammatically, logically, geographically, or scientifically impossible. Burgon considered that *παντων*, the reading of B,D in Luke 19:37 is a grammatical impossibility; that *καρδιας*, the reading of Ⲛ,A,B,C,D, etc. in 2 Cor. 3:3 is a logical impossibility; that *εκατον εξηκοντα*, the reading of Ⲛ,K,N,Θ,Π in Luke 24:13 is a geographical impossibility; that *εκλιποντος*, the reading of P<sup>75</sup>, Ⲛ(B)C,L in Luke 23:45 is a scientific impossibility (the Passover always coincides with a full moon, and a full moon cannot eclipse the sun); and that *αυτου*, the reading of Ⲛ,B,D,L in Mark 6:22 is an historical impossibility (it contradicts both Matthew and Josephus).<sup>22</sup>

I would offer *ος*, the reading of Aleph and three cursives in 1 Tim. 3:16 as a fine example of a grammatical impossibility—it is a nominative relative pronoun with no antecedent in the context; I regard the claim that it came from a primitive hymn to be gratuitous, a desperate effort to save an obviously bad reading. In the following section there will be some further examples.

Although Burgon apparently limited the use of this "note" to readings that he considered to be virtually impossible, I will expand it in the direction of what is normally understood by "reasonableness", namely the requirements of the context, which I consider to be an important consideration. A variant that is at odds with the context is suspect.

### Examples and Implications

The first edition of this book was criticized because it contained no examples to show how these principles apply to specific cases. The first revision included appendices D and E, which alleviated the criticism somewhat. In the intervening years my thinking on this subject has matured considerably, in part because of significant research that has become available in that interim, so I now propose to discuss some specific examples—they each offer some difficulty that has theoretical implications, and these will be discussed. One fundamental question for Majority Text theory is this: "Is there a ceiling above which a reading may be considered 'safe' or secure; that is, beyond reasonable challenge?" Personally, I have tended to regard 80% as such a ceiling; I believe others would settle for 70%. But what do we do if the attestation falls below 70% of the MSS, or below 60%, or below 50%? I believe we must agree with Burgon that "majority" cannot be the only criterion.

#### 1) Example—Luke 3:33

According to the International Greek New Testament Project for Luke, some 60% of the Greek MSS insert *του Ιωραμ* between "Aram" and "Hesron".<sup>23</sup> But, out of 27 extant uncials only nine include "Joram"; 18 do not and they are supported by the three earliest Versions. ("Joram" was possibly an early corruption of Aram [as per the ancestor of MS 1542] that was subsequently conflated with it; the conflation survives in a large segment of the "Byzantine" tradition, which is seriously divided here.)

#### 1) Implications

"Joram" has a clear majority attestation, albeit a weak one. However, the earliest MS to include it is from the 8th century; all earlier MSS lack it. In terms of Burgon's "Notes of Truth," Joram wins in "Number"

<sup>21</sup>Colwell, "Scribal Habits."

<sup>22</sup>Burgon, *Ibid.*, pp. 66-67.

<sup>23</sup>*The New Testament in Greek: The Gospel According to St. Luke*, Vol. I, ed. The International Greek New Testament Project (Oxford: Clarendon Press, 1984), p. 74.

but loses in "Antiquity," "Variety" and "Continuity". I believe Burgon would agree that "Joram" should be regarded as an interpolation.

## 2) Example—Acts 23:20

The *Institut für neutestamentliche Textforschung* in Münster, Germany has published an almost complete collation of the available MSS for selected variant sets in Acts. This permits a different statement of evidence than one usually sees—this and the following examples from Acts are based on that source.<sup>24</sup> The evidence looks like this:

1) <i>μελλοντες</i> --{TR}f <sup>18</sup> ,lat,syr,sa	160MSS = 33.1%
2) <i>μελλοντα</i> --{HF}	130 " = 26.9%
3) <i>μελλοντων</i> --	(3)82 " = 17.6%
4) <i>μελλων</i> --P <sup>74</sup> AB,bo	45 " = 9.3%
5) <i>μελλον</i> --{NU}S	36 " = 7.5%
6) <i>μελλοντας</i> --	(1)25 " = 5.4%
(one other) --	<u>1</u> " = .2%
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Rather a dismaying picture—what to do? To begin, the variants are all participial forms of the same verb. The key seems to be the perceived referent or antecedent of the participle. Is it "the Jews", "the Sanhedrin" or "the commander"? The best answer from the point of view of the grammar is evidently "the Jews", which would require a masculine, nominative, plural form—the only candidate is variant 1). However, there were those who took the referent to be "the Sanhedrin"—the Alexandrian MSS have *συνηδριον* next to the participle, separated only by *ως*. The grammar requires a neuter, accusative, singular form—variant 5). But, the Sanhedrin was made up of men, so perhaps some decided it would be more appropriate to make it plural—variant 2); and maybe even masculine besides—variant 6). Variant 3), being genitive, is really strange, unless somehow someone thought that the commander intended to inquire of the Sanhedrin, viewed as plural. Variant 4) presumably takes "the commander" as the referent, but puts the form in the nominative, sort of *ad sensum* since *σε* is accusative. But variant 2) could also be referring to the commander, precisely masculine, accusative, singular.

What are the requirements of the context? "The commander" as referent does not fit. Not only was it not his idea, he sent Paul away that very night to forestall the possibility. (That the Jews should attempt to tell the commander what was in **his** mind is scarcely credible.) "The Sanhedrin" as referent really doesn't fit either. *το συνηδριον* appears in the text as the object of a preposition, not as an initiating agent. It is "the Jews" that is the Subject of the main verb, and therefore of the two infinitives, and our participle is working with the second infinitive, "as ones intending to inquire."

Conclusion: variant 1) is the only one that really fits the context; it is also the best attested. Although it only musters 33.1% of the vote (including f<sup>18</sup>), it is also attested by the three ancient Versions—always weighty testimony.

## 2) Implications

Although the Majority Text is usually attested by over 95% of the MSS, every so often we get an unpleasant surprise where there is no majority reading at all. This case is as badly split as any I have seen. And yet, our "notes of truth" permit us to reach a convincing conclusion. "Number" fails us, but "Antiquity", "Variety" and "Continuity" do not. Although variants 4) and 5) are both ancient, so is 1), and it wins in

<sup>24</sup>*Text und Textwert der Griechischen Handschriften des Neuen Testaments*, ed. Kurt Aland (Berlin: Walter de Gruyter, 1993).



"Variety" and "Continuity"; it also wins in "Reasonableness". So, I am cheerfully satisfied that *μελλοντες* is the original reading.

### 3) Example—Acts 21:8

The evidence looks like this:

1) <i>οι περι τον παυλον ηλθον</i>	--{TR,HF}	(1)218MSS = 46.3%
2) — — — — <i>ηλθομεν</i>	--{NU}P <sup>74</sup> ΣA(B)Cf <sup>18</sup> ,lat,syr,cop	(4)180 " = 38.9%
3) <i>οι περι τον παυλον ηλθομεν</i>	--	(1)62 " = 13.3%
4) <i>οι αποστολοι απο τυρου ηλθον</i> --		[3](1)1 " = 1.1%
(one other reading)	--	<u>2</u> " = .4%
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Variant 3) would appear to be a not very felicitous conflation. Variant 2) best fits the context—since the beginning of the chapter, and before, the main participants have been presented in the first person plural. The closest finite verb on each side of the variant in question is *εμειναμεν*, 1st plural. The information in variant 1) is unnecessary but not objectionable; if variant 1) were original there would be no need to change it. Of course, if variant 2) were original there would be no need to change it either, unless some felt it was time to remind the reader who "we" was referring to. More likely it was the influence of the Lectionaries, since they have precisely variant 1). Since the MSS are quite evenly divided, the agreement of all three of the ancient versions makes variant 2) the better attested. (Again *f*<sup>18</sup> agrees with an ancient tradition.)

### 3) Implications

Once again we do not have a majority reading, though the split is not quite so bad as in the prior case. "Antiquity" and "Variety" are clearly with variant 2), and so "Continuity" is presumably more with 2) than with 1), also. I conclude that variant 2) has the best claim to be printed in the text.

### 4) Example—Acts 13:42

The evidence looks like this:

1) <i>δε εκ της συναγωγης των ιουδαιων</i>	--{TR,HF} <i>f</i> <sup>18</sup>	[1](1)285MSS = 60.2%
2) <i>δε αυτων</i>	--{NU}P <sup>74</sup> ΣABCD,lat,syr,cop	(1)77 " = 16.4%
3) <i>δε αυτων εκ της συναγωγης των ιουδαιων</i> --		[13]98 " = 23.3%
(one other reading)	--	<u>1</u> " = .2%
		477

I believe this variant set must be considered along with the presence of *τα εθνη* after *παρεκαλουν*, but Aland's group did not include the second set. However, from UBS<sup>3</sup> it appears that virtually the same roster of witnesses, including the three ancient versions (!), read variant 2) and omit "the Gentiles". Where then is the Subject of the main verb *παρεκαλουν*? Presumably for those witnesses it would be the Jews and proselytes who had just heard Paul and wanted to hear it all over again the next Sabbath. So why are they (Jews and proselytes) mentioned overtly again in verse 43? And on what basis would the whole city show up the next week (v. 44)? But to go back to verse 42, why would the first hearers want to hear the same thing (*τα ρηματα ταυτα*) again anyway? The really interested ones stuck with Paul and Barnabas to learn more (v. 43), just as we would expect.

The witnesses to variants 1) and 3) join in support of "the Gentiles", giving us a strong majority (over 80%). So the Subject of *παρεκαλουν* is *τα εθνη*—**they** want a chance to hear the Gospel too, and the whole city turns out. It fits the context perfectly. So, variant 3) appears to be a conflation and the basic reading is variant 1). [If variant 3) is viewed as the original, variant 2) could be the result of homoioteleuton, but not

variant 1).] The witnesses to variant 3), because they have "the Gentiles", are really on the side of variant 1), not 2), so presumably 1) may be viewed as having 80% attestation. For the witnesses to variant 1) the antecedent or referent of *εξιοντων* must be Paul's group, since the Gentiles would presumably address their request to the teacher.

In variant 2) *αυτων* presumably serves as Subject of both the participle and the main verb, but in that event the main verb should take precedence and the pronoun should be nominative, not genitive. However one might explain the motivation for such a change—from 1) to 2) and deleting "the Gentiles"—variant 2) is evidently wrong, even though attested by the three ancient versions (which troubles me). Perhaps someone faced with variant 1) took "of the Jews" to be the referent of the participle instead of modifying "synagogue" (like NKJV), and thought it should be Subject of the main verb as well—then, of course, "the Gentiles" were in the way and were deleted. Then 1) might have been shortened to 2) for "clarity".

#### 4) Implications

This time we do have a majority reading, although not as strong as we could wish. "Antiquity" and "Variety" are with variant 2), although  $f^{18}$  confers "Antiquity" on variant 1) as well and therefore 1) wins in "Continuity". But, "Context" (the performance of the MSS in the near context) comes into play this time— it clearly favors variant 1), as does "Reasonableness"—it enables us to say that the attestation for 3) really goes with 1), not 2), so 1) comes out with over 80%. In short, variant 1) has "Number", "Continuity", "Context", "Reasonableness" and "Antiquity"; variant 2) has "Antiquity" and "Variety". I take it that the original text had: *εξιοντων δε εκ της συναγωγης των ιουδαιων παρεκαλουν τα εθνη*, etc.

#### 5) Example—Acts 24:6<sup>b</sup>-8<sup>a</sup>

The evidence looks like this:

1) (without the long addition) --{HF,NU}P<sup>74</sup>⋈ABf<sup>18</sup>,lat<sup>pt</sup>,cop 280MSS = 58.1%

2) - 31):

*και κατα τον ημετερον νομον ηθελησαμεν κριναι παρελθων δε λυσιας ο χιλιαρχος μετα πολλης βιας εκ των χειρων ημων απηγαγεν κελευσας τους κατηγορους αυτου ερχεσθαι επι σε*. The five principle variations hinge on the three underlined words; they are:

2) *κριναι . . . επι σε* --lat<sup>pt</sup>,syr (6)42MSS = 10% [7 variants]

9) *κριναι . . . επι σου* -- (15)26 " = 8.5% [8 variants]

17) *κριναι . . . προς σε* -- (22)8 " = 6.2% [9 variants]

26) *κρινειν . . . επι σου* -- (2)18 " = 4.1% [3 variants]

29) *κρινειν . . . επι σε* --{TR} (5)4 " = 1.9% [3 variants]

32) replaces *απηγαγεν* with five words, plus two other changes:

*κριναι . . . επι σου*-- (6)15 " = 4.4% [6 variants]

38) completely rewrites the material:

*κριναι . . . προς σε*-- (2)11 " = 2.7% [3 variants]

(twelve further variants)-- 20 " = 4.1%

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Variant 2) presumably has the best claim to be the standard form of the addition: *κριναι* beats *κρινειν*, *επι* beats *προς*, *σε* beats *σου*. It is also attested by syr and lat<sup>pt</sup>. However, although some form of the addition commands 41.9% of the MSS, there are no less than 51 variants!

What about the context? The addition makes good sense, and it fits nicely. But, it is not really necessary; that information Felix already knew. The text reads quite well without the addition also. I conclude that the short form was judged to be abrupt or incomplete, giving rise to the addition; presumably the Autograph did not contain it. Since Tertullian was an orator he may well have actually said what is in the addition, plus a good deal more besides, but did Luke write it?

## 5) Implications

The external evidence, though divided, is adequate to resolve this case: 58.1% against a severely fragmented 41.9%. The ancient versions, being divided, do not help us much this time. Although 58% is not a strong majority, by any means, still, the severe fragmentation of the 42% sort of leaves variant 1) without a worthy opponent. Variant 1) wins in "Antiquity", "Number", "Variety" and "Continuity", so I have no doubt that it is original. [The reading of the TR, variant 29), really has little to commend it.]

## 6) Example—Acts 15:34

The evidence looks like this:

1) --- -- -- --- --- --- --{HF,NU}P <sup>74</sup> ⋈ABf <sup>18</sup> ,sy <sup>p</sup> ,bo	339MSS = 70.5%
2) εδοξεν δε τω Σιλα επιμειναι αυτου --{TR}{it <sup>p</sup> }sy <sup>h</sup> ?,sa	[2](12)83 " = 20.2%
3) " " " " " αυτοθι --	(3)33 " = 7.5%
4) " " " " " αυτους --C(D,lat <sup>p</sup> )	(4)2 " = 1.2%
(three other readings) --	<u>3</u> " = .6%
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UBS and H-F agree that variant 1) is correct, and indeed verse 33 seems to require that Silas returned to Jerusalem; "they were sent back . . . to the apostles", and "they" refers to Judas and Silas. The "problem" is that in verse 40 Paul chooses Silas to accompany him, so he had to be in Antioch, not Jerusalem. Accordingly the longer reading was created to solve the "problem". The "some days" of verse 36 could well have been a month or two. From Antioch to Jerusalem would be a trip of some 400 miles. Silas had time to go to Jerusalem and get back to Antioch.

## 6) Implications

"Reasonableness" makes itself felt here; variant 2) introduces a contradiction, which the TR unfortunately perpetuates. Variant 1) also wins in "Number" and "Continuity". "Antiquity" and "Variety" are divided. Thus, with a majority of 70.5% variant 1) is the best candidate for the original reading.

## 7) Example—Acts 12:25

This is the last example from Acts, and one that I consider to be especially difficult (it has the potential to be damaging). The evidence looks like this (I arbitrarily neglect margins and correctors, except for the early uncials):

1) εις Ιερουσαλημ	--{HF,NU}⋈B(f <sup>18</sup> =30mss)	281MSS = 59.7%
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2) απο Ιερουσαλημ	--D(f <sup>18</sup> =6mss)lat(sy <sup>h</sup> )	51 " = 10.8%
3) εξ Ιερουσαλημ	--{TR}P <sup>74</sup> A bo (sy <sup>h</sup> )	16 " = 3.4%

4) <i>εξ Ιερουσαλημ εις Αντιοχειαν</i> --(f <sup>18</sup> =5mss) sa (sy <sup>p</sup> )	57	"	= 12.1%
5) <i>απο Ιερουσαλημ εις Αντιοχειαν</i> --(f <sup>18</sup> =10mss) it <sup>pl</sup> (sy <sup>p</sup> )	36	"	= 7.6%
6) <i>εις Αντιοχειαν</i> --(f <sup>18</sup> =21mss)	24	"	= 5.1%
7) <i>εις Ιερουσαλημ εις Αντιοχειαν</i> --	3	"	= .6%
(three other readings) --	<u>3</u>	"	= .6%
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There is indeed a majority reading, albeit a weak one, but within the context it can scarcely be correct.<sup>25</sup> Consider:

- Acts 11:30, *ο και εποιησαν αποστειλαντες*, "which they also did, having sent . . . by B. & S." An aorist participle is prior in time to its main verb, in this case also aorist—their purpose is stated to have been realized. The author clearly implies that the offering did arrive, or had arrived, in Judea/Jerusalem.<sup>26</sup> Note that the next verse (12:1) places us in Jerusalem.
- Acts 12:25 (12:1-24 is unrelated, except that vv. 1-19 take place in Jerusalem), *Βαρναβας και Σαυλος* —the action includes **both**.
- Acts 12:25, *υπεστρεψαν . . . πληρωσαντες την διακονιαν*, "they returned . . . having fulfilled the mission". Again, both the participle and the main verb are aorist, and both plural. "Having fulfilled the mission" defines the main verb. Since the mission was to Judea, which of necessity includes Jerusalem as its capital city, the "returning" must be to the place where the mission originated.
- Acts 12:25, *συμπαραλαβοντες και Ιωαννην*, "having taken John also along with them". Again, both the participle and main verb are aorist. Cf. Acts 13:13 where John returns *εις Ιεροσολυμα*.

Barnabas could be viewed as returning to Jerusalem, having completed his mission to Antioch, but this could not be said of Saul. There is no basis for supposing that Mark was in Antioch (cf. Acts 12:12) so he could return to Jerusalem with Barnabas and Saul. I conclude that "to Jerusalem" can hardly be correct here even though attested by 60% of the MSS. We observe that the other 40% of the MSS, plus the three ancient versions, are agreed that the motion was away from Jerusalem, not toward it. **However**, they are divided into five main variants, plus four isolated ones, so how shall we choose the original wording? I suppose that in a case like this we must indeed appeal to the basic "canon" of textual criticism, prefer the variant that best accounts for the origin of the others.

We must begin with presuppositions. Those who presuppose that the original text was not inspired, was not inerrant, will presumably choose variant 1).<sup>27</sup> It is the "harder" reading, being at odds with the context. Many copyists noticed the problem and attempted remedial action, producing variants 2), 3) and 6). Variants 4) and 5) would appear to be conflateions and thus subsequent developments. Variant 7) is an obvious conflation. It is none the less curious that although "to Jerusalem" is evidently ancient, none of the early versions follows it.

I am among those who presuppose that the original text was indeed inspired and therefore inerrant; it follows that I am predisposed against variant 1), it evidently being in error.<sup>28</sup> What then? If 4) and 5) are conflateions, then 2), 3) and 6) are earlier. Variants 2) and 3) would appear to be independent attempts to "fix"

<sup>25</sup>Note that scholars with presuppositions so diverse as an Alford, a Burgon, a Hort or a Metzger have reached the same conclusion.

<sup>26</sup>In Acts the author seems almost to use "Jerusalem" and "Judea" interchangeably, perhaps to avoid repetition. E.g. 11:1 Judea, 11:2 Jerusalem (were the Apostles not in Jerusalem, or immediate environs?); 11:27 Jerusalem, 11:29 Judea, 11:30 the elders (would not the ruling elders be in Jerusalem?); 12:1-19 took place in Jerusalem, but v. 19 says Herod went down from Judea to Caesaria; 15:1 Judea, 15:2 Jerusalem; 28:21 letters from "Judea" probably means Jerusalem.

<sup>27</sup>Please note that I am not saying that they are the only ones who might make such a choice, nor even that they will necessarily do so.

<sup>28</sup>Please note again that I am speaking only of myself. I am making the point that presuppositions must always be taken into account since they heavily influence the interpretation of the data. This is true of all practitioners in any discipline.

variant 1).<sup>29</sup> Forced to choose between 1) and 6), my presuppositions guide me to variant 6); but how did 6) give rise to 1)?

Well, a superficial reader could have focused on Barnabas and assumed that he was returning to Jerusalem, having finished his ministry in Antioch. Since 12:25 is the first mention of Barnabas (and Saul) after 11:30, and since 11:30 does not overtly say that they "went", "returned" or whatever, a superficial reader could easily decide that he had to get Barnabas back to Jerusalem. If the original of 12:25 read "to Antioch" this would be perceived as a problem, since to the superficial reader they would still be there, having never left. This "correction" evidently happened quite early, and possibly more than once, independently—if a number of separate copyists misunderstood the text in the way suggested, and felt constrained to "fix" it, presumably most of them would simply change "Antioch" to "Jerusalem".

Although 25.4% of the MSS, plus sy<sup>p</sup> and sa, read *εις Αντιοχειαν*, only 5.1% do so without conflation. But then, variant 3) has only 3.4% alone and 15.5% with the conflation. Variant 2) has 10.8% alone and 18.4% with the conflation. So, variant 6) beats 3) both alone and with conflations; variant 6) loses to 2) alone, but with conflations comes in ahead. I submit that variant 6) best explains the origin of all the others, and given the complexities of this case has the best claim upon our confidence. I conclude that the Autograph of Acts 12:25 read *εις Αντιοχειαν*, which is presumably precisely what happened (they returned to Antioch); it also leads nicely into 13:1—comparing Acts 1:1 with Luke 1:3 we may reasonably conclude that Acts also is designed to be an orderly account.

It seems to me that there is only one way to "save" the majority variant here: place a comma between *υπεστρεψαν* and *εις*, thereby making "to Jerusalem" modify "the ministry". But such a construction is unnatural to the point of being unacceptable—had that been the author's purpose we should expect *την εις Ιερουσαλημ διακονιαν οτι την διακονιαν εις Ιερουσαλημ*. The other sixteen times that Luke uses *υποστρεφω εις* we find the normal, expected meaning, "return to". As a linguist (PhD) I would say that the norms of language require us to use the same meaning in Acts 12:25. Which to my mind leaves *εις Αντιοχειαν* as the only viable candidate for the Original reading in this place.

## 7) Implications

The whole contour of the evidence is troubling. It is evident that all the variants were created deliberately; the copyists were reacting to the meaning of the whole phrase within the context (in this situation it will not do to consider the name of each city in isolation; the accompanying preposition must also be taken into account). Variants 2) through 6) are all votes against 1), but we must choose one of them to stand against 1)—the clear choice is 6). "To Jerusalem" has "Number", "Antiquity" and "Continuity". "To Antioch" has "Antiquity," "Variety," "Continuity" and "Reasonableness". As Burgon would say, this is one of those places where "Reasonableness" just cannot be ignored, but it is not alone; "to Antioch" also wins in "Variety" while "to Jerusalem" wins only in "Number" (not strong; "Antiquity" and "Continuity" are shared). So, the "notes of truth" confirm our conclusion that *εις Αντιοχειαν* is the original reading in this place.

It will have been observed that I included f<sup>18</sup> in the statements of evidence (in Acts). f<sup>18</sup> in Acts corresponds to M<sup>c</sup> in Revelation as used in the H-F Majority Text (M<sup>c</sup> = Hoskier's "Complutensian" family, about 33 mss). I am convinced that M<sup>c</sup> represents the best line of transmission (but not necessarily perfect) in Revelation and thus am especially interested in the performance of f<sup>18</sup> in Acts (and Paul's epistles). In Acts f<sup>18</sup> represents a core of 70-75 MSS which are usually in agreement. Not so in Acts 12:25—they split five ways. Variant 1) has the most, 30 mss, followed by 6), 21 mss. All those with "Antioch" = 36 mss; all those without it also = 36 mss [but six of them are against variant 1)]. Evidently f<sup>18</sup> is not monolithic; I would like to see it receive detailed study.

<sup>29</sup> *υποστρεφω εκ* is unprecedented (in the N.T.), *υποστρεφω απο* occurs four times, *υποστρεφω εις* occurs 17 times. The reading of the TR is highly improbable, statistically speaking. If we had to choose between *απο* and *εκ*, *απο* would win on all counts.

## 8) Example—Luke 6:1

Shall we read *σαββατω δευτεροπρωτω* (variant 1) or *σαββατω* (variant 2)? Variant 1) is attested by A,C,D,K,Θ,Π,Ψ, some 1,800 other Greek MSS, lat,sy<sup>h</sup>,goth,arm,geo, and a number of early Fathers. Variant 2) is attested by P<sup>4</sup>,ϝ,B,L,W, some dozen other Greek MSS, it<sup>pt</sup>,sy<sup>p</sup>,pal,cop,eth,Diat. The attestation for variant 2) is certainly early and varied, but it scarcely has more than 1% of the vote! The parallel passages in Matt. 12:1 and Mark 2:23 both have "the sabbaths" (plural). Although *δευτεροπρωτω* doubtless made excellent sense in the first century, we have since lost the relevant cultural information. So variant 1) is definitely the "harder" reading and the offending word could easily have been deleted, here and there, especially in places like Egypt and Ethiopia where the niceties of Jewish culture would probably not be known. That both Matthew and Mark use the plural suggests that Luke was simply more specific. Here we have an eloquent illustration of the faithfulness that characterized the vast majority of copyists down through the centuries of copying by hand. Even though they did not understand the word *δευτεροπρωτω*, presumably, they none the less reproduced it verbatim in their copies. We owe them a debt of gratitude.

## 8) Implications

Variant 2) has "Antiquity" and "Variety". Variant 1) also has "Antiquity" and "Variety", plus "Continuity" and "Number" (overwhelming). "Reasonableness" may not be urged against variant 1), in this case, because the difficulty arises from our ignorance, not from the context or demonstrable facts of history, science or whatever. The "note" of "Respectability" enters in this case: the specific MSS listed for variant 2) are all of demonstrably inferior quality. I have not the slightest doubt that variant 1) is the original reading.

I will now discuss the implications of overwhelming number. At the beginning of this section reference was made to a "ceiling" of attestation, and I suggested 80%. Where a reading commands 80% (not to mention 90% or 95%) attestation it evidently dominated the stream of transmission, or genealogical tree, and the chances of an error doing so are minute. (Of course an error could have done so, here and there, but each time we "cash that check" it increases the odds against any subsequent use of that expedient—a dozen bad checks are enough to close the account.) I personally would not grant even the theoretical possibility that an error could command so much as 95% of the attestation, and probably not even 90%. (My hypothetical "bad checks" would therefore fall between 80% and 90%. Please note the term **hypothetical**; I have yet to encounter an actual example.) Thus, "Jeremiah" in Matt. 27:9 must be original since it is attested by over 98% of the Greek MSS. In 1 John 5:7-8 fully 99% of the Greek MSS do not have the "three heavenly witnesses". Mark 16:9-20 is attested by no less than 99.8% of the extant MSS!

But why put the ceiling at 80% rather than 70%, or even 60%? Well, the choice is arbitrary. Anything with over 2/3 attestation is most likely to be correct, but there is a significant difference between 70% and 80%—a 70/30 split gives a 2.33:1 ratio, but an 80/20 split gives a 4:1 ratio, almost twice as strong (90% gives a 9:1 ratio while 95% gives a 19:1 ratio and 98% gives a 49:1 ratio!). The accidents of history could easily result in an uneven transmission such that an unworthy reading might come out with 60% attestation, or even more. I have seen several readings with up to 75% support that I suspect will prove to be in error. Where the attestation is badly split (or splintered) we must indeed "weigh" the witnesses, not just count them. On the basis of complete collations we must establish MS families or groupings and determine the "batting average" or credibility quotient of each one—special attention should be given to the groups that score the highest.

## 9) Example—Revelation 4:8

The statement of evidence is based on Hoskier and the H-F Majority Text.<sup>30</sup> The question is whether "holy" occurs three times (variant 1) or nine times (variant 2). Variant 1) is attested by A,P,**M**<sup>d,e,g,h</sup>, most "independents" and 38% of **M**<sup>a</sup>, for a total of 108 MSS. Variant 2) is attested by (ϝ),**M**<sup>c,b,f,a</sup> for a total of 95 MSS. **M**<sup>a</sup> and **M**<sup>b</sup> usually work together and derive from a common exemplar, I believe. **M**<sup>d</sup> and **M**<sup>e</sup> usually

<sup>30</sup>Hoskier, *Concerning the Text of the Apocalypse*; Hodges and Farstad, *The Greek New Testament according to the Majority Text*.

work together and derive from a common exemplar. **M<sup>d,e</sup>** and **M<sup>a,b</sup>** are usually at odds, while **M<sup>c</sup>** lines up now with one, now with the other, about half and half. This means that we have three independent lines of transmission, and they are older than Aleph since Aleph conflates them (in other places). The group led by **M<sup>a</sup>** is sometimes called "Q" and includes **M<sup>f</sup>** and **M<sup>g</sup>**. **M<sup>h</sup>** and the "independents" are hard to evaluate.

That **M<sup>a,b,f</sup>** are in agreement presumably indicates that the exemplar **M<sup>a,b</sup>** read variant 2). In that event the "Q" MSS that read variant 1) have deviated from their exemplar, either by mixture or independent simplification (if we took those 23 MSS away from variant 1) the numerical attestation would shift significantly). Surely it is more likely that variant 2) should be changed to variant 1) than *vice versa*. In fact, try reading "holy" nine times in a row out loud—it starts to get uncomfortable! Since in the context the living ones are repeating themselves endlessly, the nine "holies" are both appropriate and effective. I take it that **M<sup>c</sup>** and **M<sup>a,b</sup>** preserve the original, while **M<sup>d,e</sup>** went astray.

## 9) Implications

Because of the three (at least) independent lines of transmission, and because of the shifting alignments among them and their sub-groups, Revelation is the only book where we encounter many variant sets with no majority reading—about 150, plus another 250 where the majority is less than 60%. Those who argue that Majority Text theory makes its best case in Revelation are precisely mistaken; the reverse is the case. Hoskier's collations permit us to group the MSS empirically, so in evaluating variants we need to deal with the groups, not just count individual MSS. Burgon's "notes" are often difficult to apply in cases such as the 400 mentioned; most of the notes divide, giving no clear verdict. On the basis of its performance throughout the book, I would say that **M<sup>c</sup>** has the best "batting average", but if there are basically three independent lines of transmission then two against one should carry the day. Here we have **M<sup>c</sup>** and **M<sup>a,b</sup>** against **M<sup>d,e</sup>**, in favor of variant 2)—if the three "holies" were the original reading, however could the nine "holies" come to capture two of the independent streams?

## Conclusion

So then, how are we to identify the original wording? First we must gather the available evidence—this will include Greek MSS (including Lectionaries), Fathers and Versions. Then we must evaluate the evidence to ascertain which form of the text enjoys the earliest, the fullest, the widest, the most respectable, the most varied attestation.<sup>31</sup> It must be emphasized that the strength of the "notes of truth" lies in their cooperation. They must all be considered and taken together because the very fact of competing variants means that some of the notes, at least, cannot be satisfied in full measure. But by applying all of them we will be able to form an intelligent judgment as to the independence and credibility of the several witnesses.

Actually, the work of Hoskier and Wisse<sup>32</sup> shows us that it is possible to group the MSS empirically, on the basis of a shared mosaic of readings. Once this is done we are dealing with independent groups, not individual MSS. Thus, Wisse's study in Luke reduces 1,386 MSS to 37 groups (plus 89 "mavericks").<sup>33</sup> These must be evaluated for independence and credibility. The independent, credible witnesses must then be polled. I submit that due process requires us to receive as original that form of the text which is supported by a clear majority of those witnesses; to reject their testimony in favor of our own imagination as to what the reading ought to be is manifestly untenable.

I am sure that if Burgon were alive today he would agree that the discoveries and research of the last hundred years make possible, even necessary, some refinements on his theory. I proceed to outline

<sup>31</sup>Cf. Burgon, *The Revision Revised*, p. 339.

<sup>32</sup>The collations published in the *Text und Textwert* series edited by K. Aland represent an important contribution with reference to the variant sets treated.

<sup>33</sup>Please note that I am here concerned with the **principle** involved. Of course different scholars may argue for different alignments, assign individual MSS to different groups, etc., but none of this alters the principle that the MSS can be grouped, empirically.

what I consider to be the correct approach to N.T. textual criticism. (I venture to call it Original Text Theory.)<sup>34</sup>

- 1) First, OTT is concerned to identify the precise original wording of the N.T. writings.
- 2) Second, the criteria must be biblical, objective and reasonable.
- 3) Third, a 90% attestation will be considered unassailable, and 80% virtually so.
- 4) Fourth, Burgon's "notes of truth" will come into play, especially where the attestation falls below 80%.
- 5) Fifth, where collations exist, making possible an empiric grouping of the MSS on the basis of shared mosaics of readings, this must be done. Such groups must be evaluated on the basis of their performance and be assigned a credibility quotient. A putative history of the transmission of the Text needs to be developed on the basis of the interrelationships of such groups. **Demonstrated groupings and relationships supersede the counting of MSS.**<sup>35</sup>
- 6) Sixth, it presupposes that the Creator exists and that He has spoken to our race. It accepts the implied divine purpose to preserve His revelation for the use of subsequent generations, including ours. It understands that both God and Satan have an ongoing active interest in the fate of the N.T. Text—to approach N.T. textual criticism without taking due account of that interest is to act irresponsibly.
- 7) Seventh, it insists that presuppositions and motives must always be addressed and evaluated.

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<sup>34</sup>I have thought of resurrecting the term "traditional", but since Burgon and Miller are not here to protest, I hesitate; besides, that term is no longer descriptive. Terms like "antiochian" or "byzantine" carry an extraneous burden of antipathy, or have been preempted. So here's to **Original Text Theory**. Since I really do believe that God has preserved the original wording to our day, and that we can know what it is on the basis of a defensible procedure, I do not fear the charge of arrogance, or presumption, or whatever because I use the term "original". All textual criticism worthy the name is in search of original wording.

<sup>35</sup>Please note that I am not referring to any attempt at reconstructing a genealogy of MSS—I agree with those scholars who have declared such an enterprise to be virtually impossible (there are altogether too many missing links). I am indeed referring to the reconstruction of a genealogy of **readings**, and thus of the history of the transmission of the Text.



## CONCLUSION

As the reader now can see, the words from the preface of the RSV given on page 17 are highly misleading. The real potential which exists for improving upon the King James Version, and the *Textus Receptus*, has not been realized.<sup>1</sup> The distressing realization is forced upon us that the "progress" of the past hundred years has been precisely in the wrong direction—our modern versions and critical texts are several times farther removed from the original than are the AV and TR!<sup>2</sup> How could such a calamity have come upon us?!<sup>3</sup>

Nor is that the full tale of our woe. Such has been the soporific effect of the W-H theory that the available evidence has not been evaluated, has not been assimilated. In Aland's words,

. . . the main problem of NT textual criticism lies in the fact that little more than their actual existence is known of most of the manuscripts so far identified, and that therefore we constantly have problems with many unknowns to solve. We proceed as if the few manuscripts, which have been fully, or almost fully, studied, contained all the problems in question. . . .<sup>4</sup>

Further, much of the work that **has** been done is flawed. Thus, in his status report on The International Greek New Testament Project given to the Society of Biblical Literature on December 29, 1967, Colwell stated:

The preparation of a comprehensive textual apparatus has required attention to previous editions of the Greek NT, viz., Tischendorf, Tregelles, von Soden, Legg. Careful study showed that the textual evidence in these editions cannot be used in the IGNT apparatus, since they fail to cite witnesses completely, consistently, and in some cases accurately.<sup>5</sup>

This means that not only are we presently unable to specify the precise wording of the original text, but it will require considerable time and effort before we can be in a position to do so. And the longer it takes us to mobilize and coordinate our efforts the longer it will be.<sup>6</sup>

The picture is not so dark as it might be, however. The *Institut für neutestamentliche Textforschung* in Münster, Germany has a collection of microfilms of some 5,000 of the extant Greek MSS (around 90 percent of them) and scholars connected with the *Institut* are collating selected ones. Scholars connected with The International Greek New Testament Project are also doing some collating.

But it is the availability of sophisticated computers and programs that seems to me to hold the key. It is now feasible to collate the MSS in Münster (or better yet, scan the MSS and let the computer do the collating, verified by the human eye) and set up a computer program such that we can find out anything we want to know about the inter-relationships of the MSS (on the basis of shared mosaics of readings). In this way it should be possible to identify and trace the pure stream of transmission of the text and to declare with confidence, based on objective criteria, the precise wording of the original text. It will take dedicated,

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<sup>1</sup>The NKJV is an improvement upon the AV, but mainly in terms of modernizing the language—it is based on precisely the same Greek text. *The Greek New Testament according to the Majority Text* is definitely an improvement over the TR, in my view—I would say that it represents at least 99.8% of the original wording, while the TR represents about 98% (as compared with 92% for UBS<sup>4</sup>/N-A<sup>27</sup>)—however no translation of the Majority Text into English is yet available. One is being prepared and the Gospel of John is now in use (*Living Water, the Gospel of John—Logos 21 Version*; edited by Arthur L. Farstad and published by Absolutely Free Incorporated, Glide, OR).

<sup>2</sup>When all the evidence is in I believe the *Textus Receptus* will be found to differ from the Original in something over 1,500 places, most of them being very minor differences, whereas the critical texts (UBS/N-A) will be found to differ from the Original in over 6,500 places, many of them being serious differences.

<sup>3</sup>I have an answer, but it will have to appear under separate cover. To understand what has happened one must recognize the spiritual world—in my observation the great majority of N.T. scholars do not take account of that realm of reality.

<sup>4</sup>Aland, "The Significance of the Papyri," pp. 330-31.

<sup>5</sup>E.C. Colwell, *et. al.*, "The International Greek New Testament Project: a Status Report," *Journal of Biblical Literature*, LXXXVII (1968), 192, note 13.

<sup>6</sup>The present state of our knowledge (or ignorance) is such that we are left with some 400 places where we are not sure which of two competing readings should be followed. In most of them the difference in meaning is slight.

competent people as well as money—plenty of both—but will it not be worth it? May God burden His servants!

In terms of closeness to the original, the King James Version and the *Textus Receptus* were the best available until 1979 and 1982. In 1979 Thomas Nelson Publishers brought out the New Testament of the NKJV, and in 1982 a critical edition of the Traditional Text (Majority, "Byzantine")—in it we have an excellent interim Greek Text to use until the full and final story can be told.<sup>7</sup> Although we might wish to wait for the definitive text before proceeding to an authoritative revision of the AV and NKJV, a careful job based on the interim Text would be an improvement over both the AV and all the modern versions.

In conclusion, I would like to borrow the words found at the close of one of Burgon's works.

And so I venture to hold, now that the question has been raised, both the learned and the well-informed will come gradually to see, that no other course respecting the Words of the New Testament is so strongly justified by the evidence, none so sound and large-minded, none so reasonable in every way, none so consonant with intelligent faith, none so productive of guidance and comfort and hope, as to maintain against all the assaults of corruption

#### THE TRADITIONAL TEXT.<sup>8</sup>

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<sup>7</sup>Nashville: Thomas Nelson Publishers, 1982. It was edited by Zane C. Hodges, Arthur L. Farstad and others. In 1991 the Original Word Publishers (Roswell, GA) published *The New Testament in the Original Greek according to the Byzantine/Majority Textform*, edited by Maurice A. Robinson and William G. Pierpont. I would say that the actual text is probably closer to the original than the H-F Majority Text (99.9% compared to 99.8%), but it is not as "user friendly".

<sup>8</sup>Burgon, *The Causes of the Corruption*, p. 286. Although I have used the expressions "Byzantine Text" and "Majority Text" throughout the book as an aid to understanding, I prefer "Traditional Text." (As used in this book the three expressions are synonymous.) The term "Byzantine" is not only pejorative, to many, it also fosters the notion that the Traditional Text was confined to that area, although I believe it did in fact originate there—I would say that the "Byzantine" text was in place in the Aegean area by A.D. 150 at least. The term "Majority" fosters the continuing misimpression that the defense of the Traditional Text is predicated solely on "number" and "counting." By "Traditional" I mean that in every age, from the apostolic to the nineteenth century, the text-form in question (the Greek text only) was the one that the Church in general recognized, used, and transmitted.

# APPENDIX A

## Inspiration and Preservation

"I have deliberately avoided introducing any arguments based upon these doctrines in the preceding discussion in the hope that I will not be misrepresented by critics in the way that Burgon has been."<sup>1</sup> So I wrote in the first edition of this book (Nelson, 1977). It didn't do much good. Surely any person of at least average integrity must grant that in the body of this book I have argued on the basis of historical evidence and logical deductions from that evidence. And yet there are those who perversely persist in affirming that my case is based on theological presupposition.<sup>2</sup> Bart Ehrman's treatment of the subject is typical.

One cannot read the literature produced by the various advocates of the Majority text without being impressed by a remarkable theological concurrence. To one degree or another, they all (to my knowledge, without exception) affirm that God's inspiration of an inerrant Bible required [emphasis added] His preservation of its text.<sup>3</sup>

He then discusses three "appropriations" of this position. The first is that of E.F. Hills (and D.O. Fuller, and others) who argued that God "must" have preserved the N.T. text inerrantly.<sup>4</sup> I agree with Ehrman's critique of Hill's position, though his is not the first<sup>5</sup>—Hill's position is inconsistent and arbitrary, and does not square with the evidence. Granting the inspiration of the Text,<sup>6</sup> its preservation is merely a logical inference (see Appendix F for a philosophical discussion of the implications). I relinquish the claim that God must have preserved the Text, unless it can be demonstrated that He Himself said that He would. I think it is implied,<sup>7</sup> but nowhere does it say how He proposed to do it—we must deduce the answer from what He has indeed done. We discover that He did preserve it, whether or not He had to.

Ehrman limits the concept of preservation in a way that verges on the creation of a straw man.

Any claim that God preserved the text of the New Testament intact, giving His church actual, not theoretical, possession of it, must [emphasis added] mean one of three things—either 1) God preserved it in all the extant manuscripts so that none of them contain any textual corruptions, or 2) He preserved it in a group of manuscripts, none of which contain any corruptions, or 3) He preserved it in a solitary manuscript which alone contains no corruptions.<sup>8</sup>

He then proceeds to demonstrate, correctly, that no one of the three options is true. But there is a fourth option—He preserved the Text through a normal process of transmission, done by careful people, such that

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<sup>1</sup> *The Identity of the New Testament Text* (Nashville: Thomas Nelson, 1977), p. 143. Metzger is typical: "Burgon's argument was basically theological and speculative." (*The Text*, p. 135.) See also Greenlee, p. 81; Harrison, p. 73; Vaganay, p. 172; Sturz, p. 24; Paul McReynolds, *Journal of Biblical Literature*, XCIII (1974), 481; etc.

<sup>2</sup> For instance, Fee and Wallace. Gordon D. Fee, "A Critique of W.N. Pickering's *The Identity of the New Testament Text*: a Review Article," *Westminster Theological Journal* 41 (1979), pp. 397-98. [A condensed version of this article appeared in the January, 1980 (Vol. 31, No. 1) issue of *The Bible Translator*. I submitted a response to TBT which they refused to publish. I have a copy of a letter from Fee to Paul Ellingworth, the editor, suggesting he not print my response.] Daniel B. Wallace, "The Majority Text and the Original Text: Are They Identical?" *Bibliotheca Sacra*, April-June 1991, pp. 152-155. [I prepared an answer and took it personally to Roy Zuck, the editor. I showed him that Wallace had deliberately misrepresented my position—he agreed, but refused to publish my response.]

<sup>3</sup> I am quoting from a copy sent to me personally by the author, Bart D. Ehrman: "New Testament Textual Criticism: Search for Method," M.Div thesis, Princeton Theological Seminary, 1981, p. 40.

<sup>4</sup> *Ibid.*, pp. 40-44.

<sup>5</sup> Cf. Harry Sturz and D.A. Carson.

<sup>6</sup> I declare the divine inspiration of the NT to be a presupposition which I bring to my task. The evidence and arguments in defense of this position are well known and have been adequately stated by others, before and since B.B. Warfield.

<sup>7</sup> I take it that passages such as 1 Chr. 16:15, Ps. 119:89, Isa. 40:8, Matt. 5:18, Luke 16:17 and 21:33, John 10:35 and 16:12-13, 1 Pet. 1:23-25 and Luke 4:4 may reasonably be taken to imply a promise that the Scriptures (to the tittle) will be preserved for man's use (we are to live "by every word of God"), and to the end of the world ("for a thousand generations"), but no intimation is given as to just how God proposes to do it.

<sup>8</sup> Ehrman, p.44.

we can identify the original wording on the basis of the consensus of the independent, reliable witnesses (determined empirically). Although it is presumably true that every known MS has at least some careless copying errors, these can be readily isolated because the other MSS agree as to the correct reading.

The second position that Ehrman discusses is that of J.W. Burgon. He begins by allowing Burgon to speak for himself.

There exists no reason for supposing that the Divine Agent, who in the first instance thus gave to mankind the Scriptures of Truth; straightway abdicated His office; took no further care of His work; abandoned those precious writings to their fate. That a perpetual miracle was wrought for their preservation—that copyists were protected against the risk of error, or evil men prevented from adulterating shamefully copies of the Deposit—no one, it is presumed, is so weak as to suppose. But it is quite a different thing to claim that all down the ages the sacred writings must needs have been God’s peculiar care; that the Church under Him has watched over them with intelligence and skill; has recognized which copies exhibit a fabricated, which an honestly transcribed text; has generally sanctioned the one, and generally disallowed the other.<sup>9</sup>

After identifying Burgon’s position as one of “general providence”, he affirms: “The chief problem with Burgon’s position is that it is totally arbitrary. If one affirms God’s involvement in the transmission process in any way at all, is it anything but high handed to claim that He was generally, but not fully involved?”<sup>10</sup> Not at all. Both the Bible and human history agree that the human being was created with the power or ability to choose, and both God and men must live with the consequences of their choices. Burgon’s position is Biblical and historical.

Ehrman goes on to quote B.B. Warfield and concludes:

The fact that Warfield and Burgon both affirmed a doctrine of general preservation, and yet held antithetical views of how the text was preserved suggests that the doctrine is inappropriately used in support of any particular view of the text’s transmission history. Instead such affirmations can only be made subsequent to the assessment of the evidence for the progress of the history of transmission. The evidence must lead to the doctrine, not *vice versa*—else the doctrine will simply be adduced to support a certain set of historical conclusions.”<sup>11</sup>

Very good! I agree; and so would Burgon—he stated his conclusion after many years of scrutinizing the evidence (in contrast to Warfield). Ehrman’s criticism of Burgon is mistaken and unjust, perhaps because of his own presuppositions.

The third position that Ehrman discusses is that of Z.C. Hodges. I find Ehrman’s treatment of Hodges to be especially objectionable. He criticizes Hodges, and others, for declaring his presuppositions and affirms: “As a result the conclusions are unmistakably biased.” What is objectionable here is that Ehrman fails to recognize that it is impossible to work without presuppositions. Every practitioner in whatever discipline brings presuppositions to his work, inescapably. Ehrman criticizes Hodges for stating his presuppositions while failing to state his own. The nature of his criticism dishonestly implies that he himself does not have any, but obviously Ehrman is just as biased as Hodges. Ehrman is unfair and incorrect when he charges that Hodges’ presuppositions render him incapable of entertaining sensible arguments from other quarters. Since everyone has presuppositions and yet people constantly change their minds, and even their presuppositions, it becomes obvious that Ehrman’s charge is false.<sup>12</sup>

Ever since Burgon, who stated his presuppositions honestly and openly (as any true scholar must), there has been a constant and insistent attack against those presuppositions and even the stating of them. A psychosis has been created to the extent that even some modern defenders of the majority text have become paranoid on the subject. However, in Luke 11:23 the Creator, Jehovah the Son incarnate,

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<sup>9</sup>Burgon, *The Traditional Text*, pp. 11-12.

<sup>10</sup>Ehrman, p. 47.

<sup>11</sup>*Ibid.*, p. 48.

<sup>12</sup>*Ibid.*, pp. 49-51.

declares: “He who is not with Me is against Me, and he who does not gather with Me scatters.” Here is a plain statement—there are only two teams in this world; there are only two sides; there is no neutral ground; there is no true agnosticism.<sup>13</sup> If you are not with Jesus, you are automatically against Him; if you are not gathering with Him, you are automatically scattering. If you do not accept Jesus’ affirmations about Scripture, you have rejected them. Neutrality does not exist.

We must challenge the competence of those who pretend that they have no presuppositions, who refuse, or in any case fail, to declare their presuppositions openly. If those same people criticize us for declaring ours, we must question their basic honesty. Such an unscholarly and cowardly tactic should no longer be tolerated. They accuse us of using “ad hominum” argumentation as a cover up for their own despicable “ad hominum” procedure. They challenge us to publish but refuse our articles. Enough!

I believe in the verbal plenary inspiration of the Autographs. I believe that God has providentially preserved the original wording of the text down to our day, and that it is possible for us to know precisely what it is on the basis of a defensible procedure (though due to our carelessness and laziness we do not, at this moment). My beliefs become presuppositions which I bring to my study of the evidence—any thoughtful person will realize that it is impossible to work without presuppositions—but a serious effort should be made to let the evidence tell its own story. It is not legitimate to declare *a priori* what the situation must be, on the basis of one’s presuppositions. One’s presuppositions do inescapably figure in his interpretation, so that different sets of presuppositions usually result in differing conclusions, but the body of evidence should be the same for everybody. In the end, the reasonableness of the presuppositions themselves should be measured by the evidence.

So, how does my belief that God has preserved the N.T. text square with the evidence? I see in the Traditional Text (“Byzantine”) both the result and the proof of that preservation. Please note that I am not imposing my presuppositions on the evidence—the Traditional Text does exist and so far as I can see represents the normal transmission of the original.

We are still left with the necessity of carefully evaluating the evidence that has come down to us so as to be able to identify with confidence the exact original wording. Even when that is done, it will be necessary for us to candidly admit that we cannot *prove*, in any ultimate sense, that we have the original wording; we do not have the Autographs. In the end, my affirmation that God has preserved the original wording of the New Testament text is a statement of faith; an intelligent faith, a faith that accords with the available evidence, but faith nonetheless. It may be that God’s purpose in creating the human race entailed not allowing the truth to be inescapable; if the evidence were absolute there would be no test.

“Without faith it is impossible to please Him; for he who comes to God must believe that He exists, and that He is a rewarder of those who diligently seek Him” (Heb. 11:6).

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<sup>13</sup> Agnosticism is a passive rejection; the agnostic is not accepting the claim.

## APPENDIX B

### 7Q5

The identification of papyrus fragment 5 from Qumran cave 7 with Mark 6:52-53 by Jesuit scholar Jose O'Callaghan in early 1972 produced a flurry of reaction.<sup>1</sup> The implications of such an identification are such that I suppose it was inevitable that much of the reaction should be partisan. But the lack of objectivity and restraint on the part of some scholars can only be construed as bad manners, at best.

O'Callaghan is an experienced papyrologist, a careful scholar, and is entitled to a respectful hearing.

To my mind, the lack of restraint and objectivity in M. Baillet's response borders on the reprehensible.<sup>2</sup> Unfortunately Baillet's article has been widely quoted and seems to have influenced many people, including K. Aland.<sup>3</sup> Having myself done a little work with papyri from the Ptolemaic period (third century B.C.) I should like to comment upon Baillet's response to O'Callaghan's transcription of 7Q5. The fragment contains five lines of text and I will discuss them in order.

Line 1: All that remains is a vestige of the bottom of one letter—that it is the bottom can be seen by measuring the average distance between the other lines. O'Callaghan reconstructs an *epsilon* and puts a dot under it to show that what is left of the ink itself is not sufficient to allow a certain identification of the letter. This is in strict accord with the norm universally followed by papyrologists. Baillet calls it a "gratuitous hypothesis" even though he himself gave *epsilon* as one of four possibilities in the *editio princeps*. In fact, the vestige looks precisely like the bottom extremity of either an *epsilon* or a *sigma*. It is important to note that the identification of the fragment is not based on this letter at all; it does not play a positive role. It could play a negative role if the vestige did not seem to fit the letter required by the reconstruction. But far from being an embarrassment to O'Callaghan's reconstruction, the vestige of ink agrees very nicely with it. Baillet's criticism is entirely unwarranted.

Line 2: Since there is some ink left on the papyrus, O'Callaghan is at perfect liberty to reconstruct an *epsilon* provided he puts a dot under it, as he has. Baillet grants that it is possible. Again, the identification of the fragment is not based on this letter; it is only necessary that the ink traces not be against the identification.

Everybody agrees that the *tau* and *omega* are certain. Following the *omega* O'Callaghan reconstructs a *nu*, which initiative Baillet dignifies with the epithets "absurd" and "impossible" while opining that an *iota* "appears certain". Baillet's rhetoric is disappointing and I begin to doubt his competence as a papyrologist. The most sharply preserved letter on the whole fragment is the *iota* in line 3, and the vertical stroke immediately following the *omega* in line 2 differs substantially from it. What it more nearly resembles is the left-handed vertical stroke of the *nu* or the *eta* in line 4. The horizontal extremity of the following vestige could easily be the bottom extremity of the diagonal stroke of a *nu* (but not the horizontal stroke of an *eta*). In short, O'Callaghan's reconstruction of a *nu* here, with a dot under it of course, is perfectly reasonable.

As for the *eta* that completes line 2 in O'Callaghan's reconstruction, although Baillet prefers an *alpha* he concedes that *eta* is possible, and the *editio princeps* (of which Baillet was co-editor) suggested *eta* as a possibility. O'Callaghan remarks that for him this is the most difficult piece in the puzzle—his response to Baillet's discussion of line 2 is a model of restraint and competence.<sup>4</sup>

A further consideration must be kept in mind. It is a rule of thumb among papyrologists that any proposed reconstruction of a text be accompanied by a translation (or an identification with a known piece

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<sup>1</sup>J. O'Callaghan, "Papiros neotestamentarios en la cueva 7 de Qumran?" *Biblica*, LIII (1972), 91-100. 7Q5 is dated at around 50 A.D.

<sup>2</sup>M. Baillet, "Les manuscrits de la Grotte 7 de Qumran et le N.T." *Biblica*, LIII (1972) 508-516. Baillet was one of the two editors of the *editio princeps* that presented the 7Q fragments to the scholarly world in 1962.

<sup>3</sup>K. Aland, "Neue Neutestamentliche Papyri III," *New Testament Studies*, XX (July, 1974), 358-76.

<sup>4</sup>O'Callaghan, "Notas sobre 7Q tomadas en el 'Rochefeller Museum' de Jerusalén" *Biblica*, LIII (1972), 519-21.

of literature)—in other words, it must make sense. Frequently there are so many individual points that are uncertain, taken alone, that there is little point in offering a reconstruction unless a reasonable translation or identification can also be offered—it is the total picture that carries force. O'Callaghan has produced an identification, but Baillet has not.

Line 3: It is generally agreed that the line begins with an *eta* (with a dot under it) followed by a notable space, then the letters KAIT which are quite clear. After the *tau* O'Callaghan reconstructs an *iota*, which Baillet declares to be "impossible." I fail to see how any careful scholar could use the term "impossible" so freely. The letter in question is a close replica of the indubitable *iota* two spaces to the left, so much so that it could reasonably be written without a dot under it. But O'Callaghan does put a dot under it and is therefore above reproach.

Line 4: There is general agreement about this line. It begins with half a letter which is almost certainly a *nu*, followed by a clear *nu* and *eta*, followed by a dubious *sigma*. This is a very important line because of the unusual sequence of letters.

Line 5: There is general agreement that the first letter is a dubious *theta* and the second an indubitable *eta*. O'Callaghan calls the third letter a clear *sigma* while Baillet prefers to call it an *epsilon*. Just with the naked eye I would call it an obvious *sigma*, but O'Callaghan affirms that seen with a scope what appears to be a short crossbar is in reality two dots; how they got there or what they may signify is not known, but they evidently should not be used to interpret the letter as an *epsilon*.<sup>5</sup>

The last letter is given by O'Callaghan as a possible *alpha*; Baillet rises to new heights, "Mais jamais de la vie un *alpha*, . . ." <sup>6</sup> The papyrus is too lacerated at this point to tell much from a photograph, but after studying the original with a strong lens O'Callaghan affirms that the left half of an *alpha* is clearly visible, and he invites Baillet to go see for himself.<sup>7</sup>

In sum, I see no reason to take Baillet's criticisms seriously—on the contrary, wherever he says "impossible" we should understand "most likely". It seems to me that O'Callaghan's reconstruction is eminently reasonable, but there are several problems connected with identifying the fragment with Mark 6:52-53.

The fragment presents us with two variations from the wording found in all our printed texts. In line 3 the fragment has an indubitable *tau* where the text has a *delta*. More serious, the identification involves the omission of the words *επι την γην* between lines 3 and 4. Can anything be said in relief of these problems? Yes. Apparently the difference between a voiced and a voiceless alveolar stop (*delta* and *tau*) was not obvious to some users of Greek. At any rate, the substitution of one for the other is not infrequent in ancient Greek literature. O'Callaghan offers twenty examples from four biblical papyri of the very change in question.<sup>8</sup> What we have in 7Q5 could easily be just one more instance.

The omission of three words seems more awkward, until it is remembered that it is a characteristic of the earliest N.T. MSS that they are full of eccentricities. I have already discussed this at some length above. I will cite two specific examples.

P<sup>66</sup> is so full of errors that I suspect it would be nearly impossible to find any five consecutive lines such that if superimposed on a fragment the size of 7Q5 the reconstruction would not present us with singular variants. P<sup>9</sup> is similar to 7Q5 in that it also consists of only five lines, albeit with over three times as many letters. It has been identified with 1 John 4:11-12 by everyone. But it badly garbles a word in the first line, misspells a word in the second, omits a word and misspells another in the third and adds a nonsense word in the fourth (line 5 is all right). If only the first four or five letters of each line were preserved (instead of twelve or thirteen) I doubt that it would have been identified, or the suggestion of 1 John 4:11-12 accepted.<sup>9</sup>

<sup>5</sup>*Ibid.*, p. 523.

<sup>6</sup>Baillet, p. 511.

<sup>7</sup>O'Callaghan, "Notas", p. 524.

<sup>8</sup>O'Callaghan, "El cambio δ>τ en los papiros biblicos," *Biblica*, LIV (1973), 415-16.

<sup>9</sup>My discussion of P<sup>9</sup> is based on O'Callaghan, "Notas", pp. 528-30.

The point is, our whole experience with early papyri should lead us to expect unique variants in any new one that is discovered—it would be far more surprising to discover one that had no variants. The identification of 7Q5 with Mark 6:52-53 should not be rejected on such grounds.

In spite of the problems, there is evidence in favor of the identification. In the first place, the total effect of the reconstruction is impressive—to match fifteen clear or reasonably clear letters spread over four lines with a stichometry of 23, 20, 21, 21 for the respective lines is all but conclusive. The felicitous way in which the unusual letter sequence NNHC fits into the reconstruction is a favorable argument. The sequence would presumably indicate a form related to the Greek word "generation" or a proper name like "Gennesaret."

Even more striking is the obvious space (two letters' worth—recall that words are run together in early MSS so there are usually no spaces) which occurs precisely at the boundary between verses 52 and 53. Since verse 53 begins a new paragraph the space is appropriate, so much so that to ascribe the occurrence of the space to mere chance seems scarcely credible. The combination of the space at a paragraph break and a felicitous match for NNHC I believe to be compelling. I see no reasonable way to reject O'Callaghan's identification.<sup>10</sup> For further considerations and a discussion of some implications see the series of articles in the June, 1972 issue of *Eternity*.

Once 7Q5 is firmly identified with Mark 6:52-53 then the probability that 7Q4 is to be identified with 1 Tim. 3:16, 4:1,3 and 7Q8 with James 1:23-24 becomes very strong. The remaining fragments are so small that dogmatism is untenable—O'Callaghan's identifications are possible, but cannot be insisted upon. It seems to me that 7Q5, 4, and 8 may be viewed as relevant to the thesis of this book in the following sense. That someone should have such a collection of New Testament writings at such an early date may suggest their early recognition as Scripture and even imply an early notion of a New Testament canon.<sup>11</sup>

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<sup>10</sup>An international meeting of papyrologists reached the same conclusion. *Christen und Christliches in Qumran?* Bernhard Mayer, ed., Eichstatter Studien n.F. XXXII, Verlag Friedrich Pustet, Regensburg, 1992.

<sup>11</sup>One might even be inclined to join F.F. Bruce in his flight of the imagination (*Eternity*, June, 1972, p. 33, last paragraph). Anything hidden in those caves was presumably placed there before 70 AD, and any manuscript placed there would of necessity have been copied still earlier. Before O'Callaghan's identification, 7Q5 had been dated at around 50 AD. If it is a copy of Mark then the Autograph was written even earlier, and by an eyewitness.



## APPENDIX C

### The Implications of Statistical Probability for the History of the Text<sup>1</sup>

Zane C. Hodges and David M. Hodges

Today, the whole question of the derivation of “text-types” through definite, historical recensions is open to debate. Indeed, E.C. Colwell, one of the leading contemporary [1975] critics, affirms dogmatically that the so-called “Syrian” recension (as Hort would have conceived it) never took place.<sup>2</sup> Instead he insists that all text-types are the result of “process” rather than definitive editorial activity.<sup>3</sup> Not all scholars, perhaps, would agree with this position, but it is probably fair to say that few would be prepared to deny it categorically. At least Colwell’s position, as far as it goes, would have greatly pleased Hort’s great antagonist, Dean Burgon. Burgon, who defended the *Textus Receptus* with somewhat more vehemence than scholars generally like, had heaped scorn on the idea of the “Syrian” revision, which was the keystone to Westcott and Hort’s theory. For that matter, the idea was criticized by others as well, and so well-known a textual scholar as Sir Frederic Kenyon formally abandoned it.<sup>4</sup> But the dissent tended to die away, and the form in which it exists today is quite independent of the question of the value of the TR. In a word, the modern skepticism of the classical concept of recensions thrives in a new context (largely created by the papyri). But this context is by no means discouraging to those who feel that the *Textus Receptus* was too hastily abandoned.

The very existence of the modern-day discussion about the origin of text-types serves to set in bold relief what defenders of the Received Text have always maintained. Their contention was this: Westcott and Hort failed, by their theory of recensions, to adequately explain the actual state of the Greek manuscript tradition; and in particular, they failed to explain the relative uniformity of this tradition. This contention now finds support by reason of the questions which modern study has been forced to raise. The suspicion is well advanced that the Majority text (as Aland designates the so-called Byzantine family<sup>5</sup>) cannot be successfully traced to a single even in textual history. But, if not, how can we explain it?

Here lies the crucial question upon which all textual theory logically hinges. Studies undertaken at the *Institut für neutestamentliche Textforschung* in Münster (where already photos or microfilms of over 4,500 [now over 5,000] manuscripts have been collected) tend to support the general view that as high as 90 [95] percent of the Greek cursive (minuscule) manuscripts extant exhibit substantially the same form of text.<sup>6</sup> If papyrus and uncial (majuscule) manuscripts are considered along with cursives, the percentage of extant texts reflecting the majority form can hardly be less than 80 [90] percent. But this is a fantastically high figure and it absolutely demands explanation. In fact, apart from a rational explanation of a text form which pervades all but 20 [10] percent of the tradition, no one ought to seriously claim to know how to handle our textual materials. If the claim is made that great progress toward the original is possible, while the origin of 80 percent of the Greek evidence is wrapped in obscurity, such a claim must be viewed as monstrously unscientific, if not dangerously obscurantist. No amount of appeal to subjective preferences for this reading or that reading, this text or that text, can conceal this fact. The Majority text must be explained **as a whole**, before its claims **as a whole** can be scientifically rejected.

It is the peculiar characteristic of New Testament textual criticism that, along with a constantly accumulating knowledge of our manuscript resources, there has been a corresponding diminution in the confidence with which the history of these sources is described. The carefully constructed scheme of

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<sup>1</sup>This appendix is an edited abstract from “A Defense of the Majority-Text” by Zane C. Hodges and David M. Hodges (unpublished course notes, Dallas Theological Seminary, 1975) used by permission of the authors.

<sup>2</sup>His statement is: “*The Greek Vulgate—The Byzantine or Alpha texttype—had its origin in no such single focus as the Latin had in Jerome*” (italics in the original). E.C.Colwell, “The Origin of Texttypes of New Testament Manuscripts,” *Early Christian Origins*, p.137.

<sup>3</sup>*Ibid.*, p. 136. Cf. our discussion of this view under “Objections.”

<sup>4</sup>Cf. F.G. Kenyon, *Handbook to the Textual Criticism of the New Testament*, pp. 324ff.

<sup>5</sup>Kurt Aland, “The Significance of the Papyri for Progress in New Testament Research,” *The Bible in Modern Scholarship*, p. 342. This is the most scientifically unobjectionable name yet given to this text form.

<sup>6</sup>*Ibid.*, p. 344.

Westcott and Hort is now regarded by all reputable scholars as quite inadequate. Hort's confident assertion that "it would be an illusion to anticipate important changes of text from any acquisition of new evidence" is rightly regarded today as extremely naive.<sup>7</sup>

The formation of the *Institut für neutestamentliche Textforschung* is virtually an effort to start all over again by doing the thing that should have been done in the first place—namely, collect the evidence! It is in this context of re-evaluation that it is entirely possible for the basic question of the origin of the Majority text to push itself to the fore. Indeed, it may be confidently anticipated that if modern criticism continues its trend toward more genuinely scientific procedures, this question will once again become a central consideration. For it still remains the most determinative issue, logically, in the whole field.

Do the proponents of the *Textus Receptus* have an explanation to offer for the Majority text? The answer is yes. More than that, the position they maintain is so uncomplicated as to be free from difficulties encountered by more complex hypotheses. Long ago, in the process of attacking the authority of numbers in textual criticism, Hort was constrained to confess: "A theoretical presumption indeed remains that a majority of extant documents is more likely to represent a majority of ancestral documents at each stage of transmission than *vice versa*."<sup>8</sup> In conceding this, he was merely affirming a truism of manuscript transmission. It was this: under normal circumstances the older a text is than its rivals, the greater are its chances to survive in a plurality or a majority of the texts extant at any subsequent period. But the **oldest** text of all is the autograph. Thus it ought to be taken for granted that, barring some radical dislocation in the history of transmission, a majority of texts will be far more likely to represent correctly the character of the original than a small minority of texts. This is especially true when the ratio is an overwhelming 8:2 [9:1]. Under any reasonably normal transmissional conditions, it would be for all practical purposes quite impossible for a later text-form to secure so one-sided a preponderance of extant witnesses. Even if we push the origination of the so-called Byzantine text back to a date coeval with P<sup>75</sup> and P<sup>66</sup> (c. 200)—a time when already there must have been hundreds of manuscripts in existence—such mathematical proportions as the surviving tradition reveals could not be accounted for apart from some prodigious upheaval in textual history.

### ***Statistical probability***

This argument is not simply pulled out of thin air. What is involved can be variously stated in terms of mathematical probabilities. For this, however, I have had to seek the help of my brother, David M. Hodges, who received his B.S. from Wheaton College in 1957, with a major in mathematics. His subsequent experience in the statistical field includes service at Letterkenny Army Depot (Penna.) as a Statistical Officer for the U.S. Army Major Item Data Agency and as a Supervisory Survey Statistician for the Army Materiel Command Equipment Manuals Field Office (1963-67), and from 1967-70 as a Statistician at the Headquarters of U.S. Army Materiel Command, Washington, DC. In 1972 he received an M.S. in Operations Research from George Washington University.

Below is shown a diagram of a transmissional situation in which one of three copies of the autograph contains an error, while two retain the correct reading. Subsequently the textual phenomenon known as "mixture" comes into play with the result that erroneous readings are introduced into good manuscripts, as well as the reverse process in which good readings are introduced into bad ones. My brother's statement about the probabilities of the situation follows the diagram in his own words. [Because of spacing the diagram comes on the next page.]

Provided that good manuscripts and bad manuscripts will be copied an equal number of times, and that the probability of introducing a bad reading into a copy made from a good manuscript is equal to the probability of reinserting a good reading into a copy made from a bad manuscript, the correct reading would predominate in any generation of manuscripts. The degree to which the good reading would predominate depends on the probability of introducing the error.

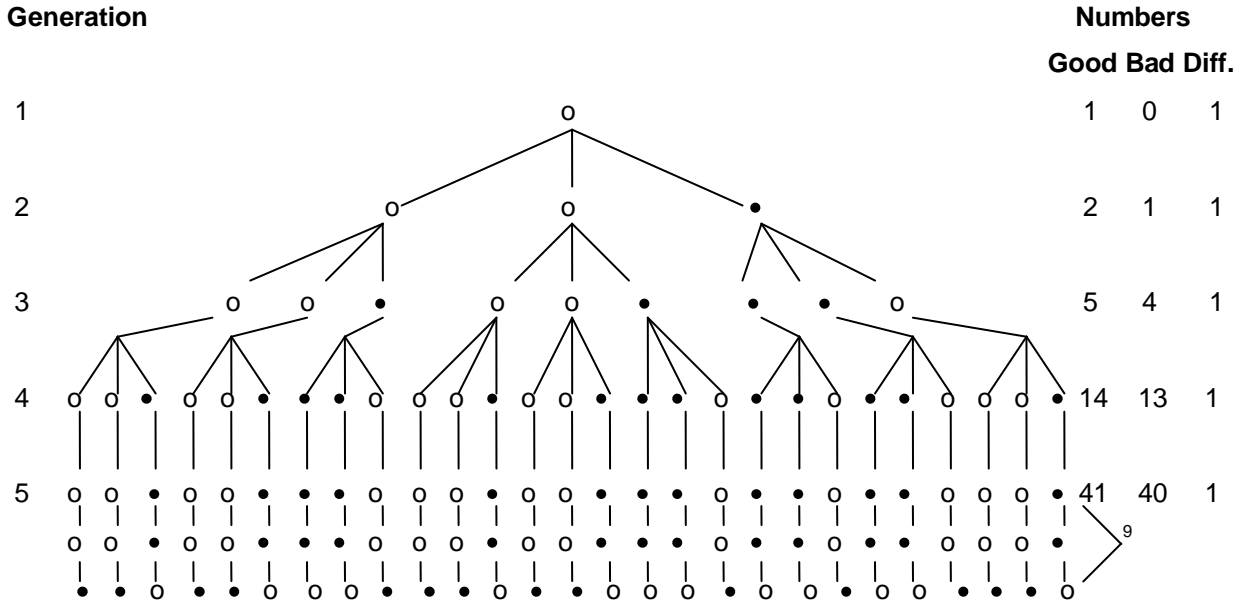
For purposes of demonstration, we shall call the autograph the first generation. The copies of the autograph will be called the second generation. The copies of the second generation

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<sup>7</sup> *Ibid.*, pp. 330ff.

<sup>8</sup> B.F. Westcott and F.J.A. Hort, *The New Testament in the Original Greek*, II, 45.

manuscripts will be called the third generation and so on. The generation number will be identified as "n". Hence, in the second generation, n=2.



Assuming that each manuscript is copied an equal number of times, the number of manuscripts produced in any generation is  $k^{n-1}$ , where "k" is the number of copies made from each manuscript.

The probability that we shall reproduce a good reading from a good manuscript is expressed as "p" and the probability that we shall introduce an erroneous reading into a good manuscript is "q". The sum of p and q is 1. Based on our original provisions, the probability of reinserting a good reading from a bad manuscript is q and the probability of perpetuating a bad reading is p.

The expected number of good manuscripts in any generation is the quantity  $pkG_{n-1} + qkB_{n-1}$  and the expected number of bad manuscripts is the quantity  $pkB_{n-1} + qkG_{n-1}$ , where  $G_{n-1}$  is the number of good manuscripts from which we are copying and  $B_{n-1}$  is the number of bad manuscripts from which we are copying. The number of good manuscripts produced in a generation is  $G_n$  and the number of bad produced is  $B_n$ . We have, therefore, the formulas:

- (1)  $G_n = pkG_{n-1} + qkB_{n-1}$  and
- (2)  $B_n = pkB_{n-1} + qkG_{n-1}$  and
- (3)  $k^{n-1} = G_n + B_n = pkG_{n-1} + qkB_{n-1} + pkB_{n-1} + qkG_{n-1}$ .

If  $G_n = B_n$ , then  $pkG_{n-1} = qkB_{n-1} = pkB_{n-1} + qkG_{n-1}$  and  $pkG_{n-1} + qkB_{n-1} - pkB_{n-1} - qkG_{n-1} = 0$ .

Collecting like terms, we have  $pkG_{n-1} - qkG_{n-1} + qkB_{n-1} - pkB_{n-1} = 0$  and since k can be factored out, we have  $(p-q)G_{n-1} + (q-p)B_{n-1} = 0$  and  $(p-q)G_{n-1} - (p-q)B_{n-1} = 0$  and  $(p-q)(G_{n-1} - B_{n-1}) = 0$ . Since the expression on the left equals zero, either (p-q) or  $(G_{n-1} - B_{n-1})$  must equal zero. But  $(G_{n-1} - B_{n-1})$  cannot equal zero, since the autograph was good. This means that (p-q) must equal zero. In other words, the expected number of bad copies can equal the expected number of good copies only if the probability of making a bad copy is equal to the probability of making a good copy.

If  $B_n$  is greater than  $G_n$ , then  $pkB_{n-1} + qkG_{n-1} > pkG_{n-1} + qkB_{n-1}$ . We can subtract a like amount from both sides of the inequality without changing the inequality. Thus, we have  $pkB_{n-1} + qkG_{n-1} - pkG_{n-1} - qkB_{n-1} > 0$  and we can also divide k into both sides obtaining  $pB_{n-1} + qG_{n-1} - pG_{n-1} - qB_{n-1} > 0$ . Then,  $(p-q)B_{n-1} + (q-p)G_{n-1} > 0$ . Also,  $(p-q)B_{n-1} - (p-q)G_{n-1} > 0$ . Also  $(p-q)(B_{n-1} - G_{n-1}) > 0$ . However,  $G_{n-1}$  is greater than  $B_{n-1}$  since the autograph was good. Consequently,  $(B_{n-1} - G_{n-1}) < 0$ . Therefore, (p-q) must also be less than zero. This means that q must be greater than p in order for the expected number of bad manuscripts to be greater than the expected number of good

<sup>9</sup>[N.B.—the fifth generation is represented by all three lines; in other words, each MS of the fourth generation was copied three times, just as in the other generations.]

manuscripts. This also means that the probability of error must be greater than the probability of a correct copy.

The expected number is actually the mean of the binomial distribution. In the binomial distribution, one of two outcomes occurs; either a success, i.e., an accurate copy, or a failure, i.e., an inaccurate copy.

In the situation discussed, equilibrium sets in when an error is introduced. That is, the numerical difference between the number of good copies and bad copies is maintained, once an error has been introduced. In other words, bad copies are made good at the same rate as good copies are made bad. The critical element is how early a bad copy appears. For example, let us suppose that two copies are made from each manuscript and that  $q$  is 25% or  $\frac{1}{4}$ . From the autograph two copies are made. The probability of copy number 1 being good is  $\frac{3}{4}$  as is the case for the second copy. The probability that both are good is  $\frac{9}{16}$  or 56%. The probability that both are bad is  $\frac{1}{4} \times \frac{1}{4}$  or  $\frac{1}{16}$  or 6%. The probability that one is bad is  $\frac{3}{4} \times \frac{1}{4} + \frac{1}{4} \times \frac{3}{4}$  or  $\frac{6}{16}$  or 38%. The expected number of good copies is  $p_k G_{n-1} + q_k B_{n-1}$  which is  $\frac{3}{4} \times 2 \times 1 + \frac{1}{4} \times 2 \times 0$  or 1.5. The expected number of bad copies is  $2 - 1.5$  or .5. Now, if an error is introduced into the second generation, the number of good and bad copies would, thereafter, be equal. But the probability of this happening is 44%. If the probability of an accurate copy were greater than  $\frac{3}{4}$ , the probability of an error in the second generation would decrease. The same holds true regardless of the number of copies and the number of generations so long as the number of copies made from bad manuscripts and the number from good manuscripts are equal. Obviously, if one type of manuscript is copied more frequently than the other, the type of manuscript copied most frequently will perpetuate its reading more frequently.

Another observation is that if the probability of introducing an incorrect reading differs from the probability of reintroducing a correct reading, the discussion does not apply.

This discussion, however, is by no means weighted in favor of the view we are presenting. The reverse is the case. A further statement from my brother will clarify this point.

Since the correct reading is the reading appearing in the majority of the texts in each generation, it is apparent that, if a scribe consults other texts at random, the majority reading will predominate in the sources consulted at random. The ratio of good texts consulted to bad will approximate the ratio of good texts to bad in the preceding generations. If a small number of texts are consulted, of course, a non-representative ratio may occur. But, in a large number of consultations of existing texts, the approximation will be representative of the ratio existing in all extant texts.

In practice, however, random comparisons probably did not occur. The scribe would consult those texts most readily available to him. As a result, there would be branches of texts which would be corrupt because the majority of texts available to the scribe would contain the error. On the other hand, when an error first occurs, if the scribe checked more than one manuscript he would find all readings correct except for the copy that introduced the error. Thus, when a scribe used more than one manuscript, the probability of reproducing an error would be less than the probability of introducing an error. This would apply to the generation immediately following the introduction of an error.

In short, therefore, our theoretical problem sets up conditions for reproducing an error which are somewhat too favorable to the error. Yet even so, in this idealized situation, the original majority for the correct reading is more likely to be retained than lost. But the majority in the fifth generation is a slender 41:40. What shall we say, then, when we meet the actual extant situation where (out of any given 100 manuscripts) we may expect to find a ratio of, say, 80:20? It at once appears that the probability that the 20 represent the original reading in any kind of normal transmissional situation is poor indeed.

Hence, approaching the matter from this end (i.e., **beginning** with extant manuscripts) we may hypothesize a problem involving (for mathematical convenience) 500 extant manuscripts in which we have proportions of 75% to 25%. My brother's statement about this problem is as follows:

Given about 500 manuscripts of which 75% show one reading and 25% another, given a one-third probability of introducing an error, given the same probability of correcting an error, and given

that each manuscript is copied twice, the probability that the majority reading originated from an error is less than one in ten. If the probability of introducing an error is less than one-third, the probability that the erroneous reading occurs 75% of the time is even less. The same applies if three, rather than two copies are made from each manuscript. Consequently, the conclusion is that, given the conditions described, it is highly unlikely that the erroneous reading would predominate to the extent that the majority text predominates.

This discussion applies to an individual reading and should not be construed as a statement of probability that copied manuscripts will be error free. It should also be noted that a one-third probability of error is rather high, if careful workmanship is involved.

It will not suffice to argue in rebuttal to this demonstration that, of course, an error might easily be copied more often than the original reading in any particular instance. Naturally this is true, and freely conceded. But the problem is more acute than this. If, for example, in a certain book of the New Testament we find (let us say) 100 readings where the manuscripts divide 80 percent to 20 percent, are we to suppose that in every one of these cases, or even in most of them, that this reversal of probabilities has occurred? Yet this is what, **in effect**, contemporary textual criticism is saying. For the Majority text is repeatedly rejected in favor of minority readings. It is evident, therefore, that what modern textual critics are **really** affirming—either implicitly or explicitly—constitutes nothing less than a wholesale rejection of probabilities on a sweeping scale!

Surely, therefore, it is plain that those who repeatedly and consistently prefer minority readings to majority readings—especially when the majorities rejected are very large—are confronted with a problem. How can this preference be justified against the probabilities latent in any reasonable view of the transmissional history of the New Testament? Why should we reject these probabilities? What kind of textual phenomenon would be required to produce a Majority text diffused throughout 80 percent of the tradition, which nonetheless is more often wrong than the 20 percent which oppose it? And if we could conceptualize such a textual phenomenon, what proof is there that it ever occurred? Can anyone, logically, proceed to do textual criticism without furnishing a convincing answer to these questions?

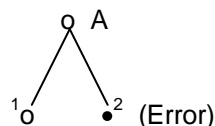
I have been insisting for quite some time that the real crux of the textual problem is how we explain the overwhelming preponderance of the Majority text in the extant tradition. Current explanations of its origin are seriously inadequate (see below under “Objections”). On the other hand, the proposition that the Majority text is the natural outcome of the normal processes of manuscript transmission gives a perfectly natural explanation for it. The minority text-forms are thereby explained, *mutatis mutandis*, as existing in their minority form due to their comparative remoteness from the original text. The theory is simple but, I believe, wholly adequate on every level. Its adequacy can be exhibited also by the simplicity of the answers it offers to objections lodged against it. Some of these objections follow.

### Objections

1. Since all manuscripts are not copied an even number of times, mathematical demonstrations like those above are invalid.

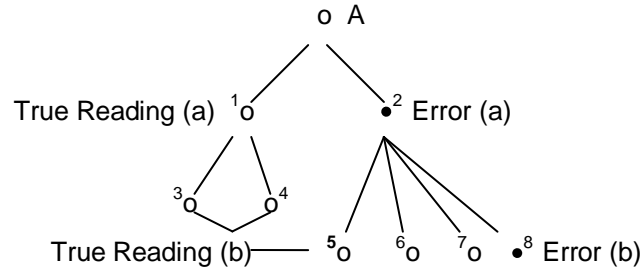
But this is to misunderstand the purpose of such demonstrations. Of course the diagram given above is an “idealized” situation which does not represent what actually took place. Instead, it simply shows that all things being equal **statistical probability** favors the perpetuation in every generation of the original majority status of the authentic reading. And it must then be kept in mind that the larger the original majority, the more compelling this argument from probabilities becomes. Let us elaborate this point.

If we imagine a stem as follows



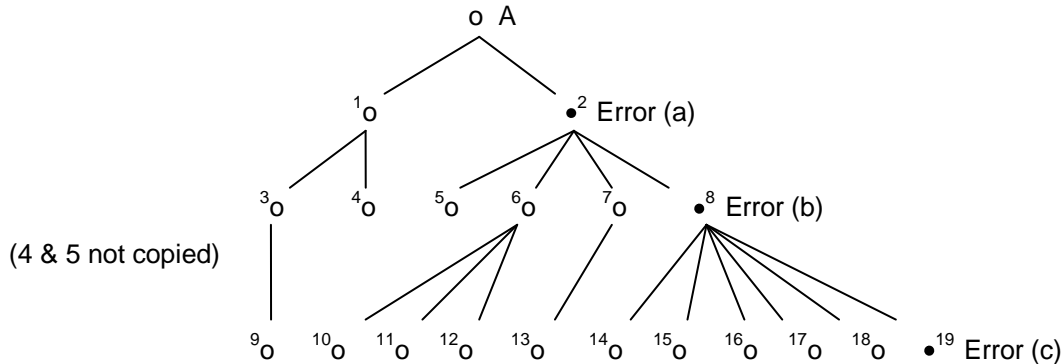
in which A = autograph and (1) and (2) are copies made from it, it is apparent that, **in the abstract**, the error in (2) has an even chance of perpetuation in equal numbers with the authentic reading in (1). But, of course, **in actuality** (2) **may** be copied more frequently than (1) and thus the error be perpetuated in a larger number of later manuscripts than the true reading in (1).

So far, so good. But suppose—



Now we have conceded that the error designated (a) is being perpetuated in larger numbers than the true reading (a), so that “error (a)” is found in copies 5-6-7-8, while “true reading (a)” is found only in copies 3 and 4. But when “error (b)” is introduced in copy 8, its rival (“true reading (b)”) is found in copies 3-4-5-6-7.<sup>10</sup> Will anyone suppose that at this point it is at all likely that “error (b)” will have the same good fortune as “error (a)” and that manuscript 8 will be copied more often than 3-4-5-6-7 combined?

But even conceding this **far** less probable situation, suppose again—



Will anybody believe that probabilities favor a repetition of the same situation for “error (c)” in copy 19?

Is it not transparent that as manuscripts multiply, and errors are introduced farther down in the stream of transmission, that the probability is drastically reduced that the error will be copied more frequently than the increasingly large number of rival texts?

Thus to admit that **some** errors might be copied more frequently than the rival, authentic reading in no way touches the core of our argument. The reason is simple: modern criticism repeatedly and systematically rejects majority readings on a very large scale. But, with every such rejection, the probability that this rejection is valid is dramatically reduced. **To overturn statistical probabilities a few times is one thing. To overturn them repeatedly and persistently is quite another!**

Hence, we continue to insist that to reject Majority text readings in large numbers without furnishing a credible overall rationale for this procedure is to fly blindly into the face of all reasonable probability.

<sup>10</sup>By “error (b)” we mean, of course, an error made in another place in the text being transmitted from the autograph. We do **not** mean that “error (b)” has been substituted for “error (a).” Hence, while copies 5-6-7 contain “error (a),” they also contain the original autograph reading which is the rival to “error (b).”

2. The Majority text can be explained as the outcome of a “process” which resulted in the gradual formation of a numerically preponderant text-type.

The “process” view of the Majority text seems to be gaining in favor today among New Testament textual scholars. Yet, to my knowledge, no one has offered a detailed explanation of what exactly the process was, when it began, or how—once begun—it achieved the result claimed for it. Indeed, the proponents of the “process” view are probably wise to remain vague about it because, on the face of the matter, it seems impossible to conceive of **any kind** of process which will be both historically credible and adequate to account for all the facts. The Majority text, it must be remembered, is relatively uniform in its general character with comparatively low amounts of variation between its major representatives.<sup>11</sup>

No one has yet explained how a long, slow process spread out over many centuries as well as over a wide geographical area, and involving a multitude of copyists, who often knew nothing of the state of the text outside of their own monasteries or scriptoria, could achieve this widespread uniformity of the diversity presented by the earlier forms of text. Even an official edition of the New Testament—promoted with ecclesiastical sanction throughout the known world—would have had great difficulty achieving this result as the history of Jerome’s Vulgate amply demonstrates.<sup>12</sup> But an unguided process achieving relative stability and uniformity in the diversified textual, historical, and cultural circumstances in which the New Testament was copied, imposes impossible strains on our imagination.

Thus it appears that the more clearly and specifically the “process” view may come to be articulated, the more vulnerable it is likely to be to all of the potential objections just referred to. Further, when articulation **is** given to such a view, it will have to locate itself definitely somewhere in history—with many additional inconveniences accruing to its defenders. For, be it remembered, just as history is silent about any “Syrian recension” (such as the one Hort imagined), so also history is silent about any kind of “process” which was somehow influencing or guiding the scribes as manuscripts were transmitted. Modern critics are the first to discover such a “process”, but before accepting it we shall have to have more than vague, undocumented assertions about it.

It seems not unfair to say that the attempt to explain the Majority text by some obscure and nebulous “process” is an implicit confession of weakness on the part of contemporary criticism. The erosion of Westcott and Hort’s view, which traced this text to an official, definitive recension of the New Testament, has created a vacuum very hard indeed to fill. More than ever, it appears, critics cannot reject the Majority text and at the same time also explain it. **And this is our point!** Rejection of the Majority text and credible explanation of that text are quite incompatible with each other. But acceptance of the Majority text immediately furnishes an explanation of this text and the rival texts as well! **And it is the essence of the scientific process to prefer hypotheses which explain the available facts to those which do not!**

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<sup>11</sup> The key words here are “relatively” and “comparatively.” Naturally, individual members of the Majority text show varying amounts of conformity to it. Nevertheless, the nearness of its representatives to the general standard is not hard to demonstrate in most cases. For example, in a study of one hundred places of variation in John 11, the representatives of the Majority text used in the study showed a range of agreement from around 70 percent to 93 percent. Cf. Ernest C. Colwell and Ernest W. Tune, pp. 28,31. The uncial codex Omega’s 93 percent agreement with the *Textus Receptus* compares well with the 92 percent agreement found between P<sup>75</sup> and B. Omega’s affinity with the TR is more nearly typical of the pattern one would find in the great mass of minuscule texts. High levels of agreement of this kind are (as in the case of P<sup>75</sup> and B) the result of a shared ancestral base. It is the divergencies that are the result of a “process” and not the reverse.

A more general, summary statement of the matter is made by Epp, “. . . the Byzantine manuscripts together form, after all, a rather closely-knit group, and the variations in question within this entire large group are relatively minor in character.” (Eldon Jay Epp, “The Claremont Profile Method for Grouping New Testament Minuscule Manuscripts,” p. 33.)

<sup>12</sup> After describing the vicissitudes which afflicted the transmission of the Vulgate, Metzger concludes: “As a result, the more than 8,000 Vulgate manuscripts which are extant today exhibit the greatest amount of cross-contamination of textual types.” (*Text of the New Testament*, p. 76.) Uniformity of text is always greatest at the source and diminishes—rather than increases—as the tradition expands and multiplies. This caveat is ignored by the “process” view of the Majority text.

## APPENDIX D

### CONFLATION OR CONFUSION?<sup>1</sup>

Conflation is the theory that when a scribe or editor had before him two or more manuscripts that at a given point had different readings that might “properly” be combined to produce a more “full” reading, he might do so. The result would be called “conflation” according to Hort.

When evaluating a putative example of conflation, due consideration should be given to the possibility that the differences may have come about because of the accidental (or intentional) omission of different parts of a “complete” original reading.

The list that follows comprises possible examples of conflation found to date from all sources. (There may be quite a few more discoverable by a sharp eye.) These are presented to the reader for his own evaluation and decision. They range from cases of obvious conflation and obvious omission to cases of sheer confusion where it is highly doubtful that the mechanism “conflation” was at work. Accordingly, the examples are classified into two sets of two groups each:

1. True, or simple “conflation”:
  - a) Simple addition or telescoping of readings, or omission;
  - b) Addition plus simple coupling links, or omission.
2. Marginal “conflation” or confusion:
  - a) Complicated by substitution, transposition or moderate internal changes, or omissions;
  - b) Substantial differences—“conflation” dubious.

The full extent of the confusion that exists will not be apparent to the reader since for most of the examples there are one or more further variations not included here because they are not relevant to the possible instances of conflation.

The symbols in the critical apparatus are essentially those in general use. The abbreviations *pc*, *al*, *pm* and *rell* have the same meanings as in the Nestle editions. I have represented *f*<sup>1</sup> and *f*<sup>13</sup> by the numbers only. Only one text-type symbol is used, *Byz*, which stands for the “Byzantine” manuscript tradition. I have used parentheses in two ways—enclosing a papyrus they mean there is doubt as to what reading is exhibited, enclosing any other kinds of witnesses they mean the witness(es) has a slight variation from the reading of the witness(es) not so enclosed. The reader cannot fail to note that the completeness of the apparatus varies considerably from example to example—this is a reflection of the sources that were available to me.

**Group 1.** a) Simple addition or telescoping of readings, or omission.

- |               |                                     |  |
|---------------|-------------------------------------|--|
| 1. Matt. 3:12 | <i>αυτου εις την αποθηκην</i>       | <i>Byz</i> ∓ C K Δ 0233 1 <i>pm</i> lat cop                        |
|               | <i>εις την αποθηκην αυτου</i>       | L 892 <i>al</i> b ff <sup>1</sup> g <sup>1</sup> sy <sup>p,h</sup> |
|               | <i>αυτου εις την αποθηκην αυτου</i> | B W <i>pc</i>  |

(This would appear to be a conflation on the part of B and W. Since Hort did not follow B here, he must have been of a similar opinion.)

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<sup>1</sup>The title and basic format for this appendix I owe to William G. Pierpont and use with his permission. I have, however, almost tripled the number of examples and the editorial comments are mine. The principal sources for the added examples are H.A. Sturz (*The Byzantine Text-Type*) and Maurice A. Robinson (unpublished paper). Peter J. Johnston has contributed significantly to the statements of evidence.



2. Matt. 16:11 προσεχειν *Byz* D<sup>c</sup> W X *pm sy*<sup>c,s,h</sup>  
 προσεχετε D Θ 13 124 *pc lat sy*<sup>p</sup>  
 προσεχετε δε Ξ B C L 1 *pc cop*  
 προσεχειν προσεχετε δε C<sup>c</sup> 33 237 *al q*

(An evident conflation on the part of some later MSS, building on the “Byzantine” and “Alexandrian/Western” readings.)

3. Matt. 17:25 οτε εισηλθεν *Byz* E F G K L W Y Π  
 ελθοντα B 1  
 εισελθοντα Ξ  
 εισελθοντων Θ 13  
 εισελθοντι D

(Might this be a conflation on the part of Ξ, with “Caesarean” and “Western” embellishments?)

4. Matt. 20:21 δεξιων σου . . . ευωνυμων D Θ 1 *pc lat*  
 δεξιων . . . ευωνυμων σου Ξ B  
 δεξιων σου . . . ευωνυμων σου *Byz* C L N W Z 085 13 *pm sy*<sup>p,h</sup>

(Is this a “Byzantine” conflation of the “Western” and “Alexandrian” readings, or are the latter independent simplifications of the former? It should be noted that Ξ and B are alone in omitting the first σου.)

5. Matt. 23:25 ακρασιας Ξ B D L Δ Θ Π 1 13 33 *al it sy*<sup>h</sup>  
 αδικιας *Byz* C K Γ *pm f sy*<sup>p</sup>  
 ακρασιας αδικιας W

(It seems clear that Codex W here conflates the “Alexandrian” and “Byzantine” readings.)

6. Matt. 24:38 εκειναις προ D 253 *pc it*<sup>pt</sup> *sy*<sup>h,pal</sup>  
 ταις προ *Byz* Ξ L W Θ 067 0133 1 13 *pl it*<sup>pt</sup> *vg bo*  
 εκειναις ταις προ B

(This would appear to be a conflation on the part of B. Since Hort used brackets here, he must have tended to a similar opinion.)

7. Matt. 26:22 εις εκαστος Ξ B C L Z 0281 33 *pc sa*  
 εκαστος αυτων *Byz* P<sup>37,64</sup> (P<sup>45</sup>) A W Γ Δ Π Σ Ψ 074 1 13 *pl sy*<sup>p</sup>  
 εις εκαστος αυτων (P<sup>45</sup>) D M Θ 69 *pc bo*

(This would appear to be a “Western” conflation of “Byzantine” and “Alexandrian” elements. A recent meeting of papyrologists dated P<sup>64</sup> in the first century [!] and confirmed that it supports the Byzantine reading.)

8. Matt. 26:36 ου *Byz* B E F G 067 *pm*  
 αν D K L W Δ Θ 074 1 69 *al*  
 ου αν P<sup>53</sup> A *pc*

(Before the advent of P<sup>53</sup> presumably all would agree that A has here conflated the “Byzantine” and “Western” readings. Although the papyrus antedates any extant witness to these two “text-types”, I suggest that the proper conclusion is that the conflation is a very early one.)

9. Matt. 26:70     αὐτῶν                    K *al*  
                                   πᾶντων                    ⚭ B D E G L Z Θ 090 13 33 *al* lat sy<sup>p,h</sup>  
                                   αὐτῶν πᾶντων            Byz A C W Γ Δ 0133 1 *pm*

(Shall we say that the “Byzantine” text has a conflation based on a handful of late MSS on the one hand and the combined “Alexandrian-Western” text-types on the other? It seems more probable that K etc. have simplified the “Byzantine” reading, an easy instance of homoioteleuton. In that event the “Alexandrian-Western” reading is best explained as a separate simplification of the original reading, a bit of parablepsis.)

10. Matt. 27:55     ἐκεῖ                    Byz B C *pl* lat  
                                   καὶ                    D 56 *aur d*  
                                   ἐκεῖ καὶ                F K L Π 33 sy<sup>h,pal</sup>  
                                   κακεῖ                    ⚭ (sy<sup>p</sup>)

(Here we seem to have varied witnesses conflating the “Byzantine-Alexandrian” and “Western” readings.)

11. Mark 1:4     ο βαπτίζων ἐν τῇ ἐρημῷ                    B 33 *pc*  
                                   βαπτίζων ἐν τῇ ἐρημῷ καὶ                Byz A K P W Π 1 13 *pl f* sy<sup>h,pal</sup>  
                                   ο βαπτίζων ἐν τῇ ἐρημῷ καὶ                ⚭ L Δ *pc bo*  
                                   (ἐν τῇ ἐρημῷ βαπτίζων καὶ)                D Θ *pc lat sy<sup>p</sup>*

(Here we have “Alexandrian” witnesses conflating the “Byzantine” reading and that of Codex B. Although there has been no accretion of new evidence, UBS<sup>3</sup> seems to espouse this obvious conflation whereas UBS<sup>1</sup> did not.)

12. Mark 1:28     εὐθὺς                    Byz A D E G H K M U V Y Γ Δ Π Σ Φ Ω 0104 *pm lat sy<sup>p,h</sup>*  
                                   πανταχοῦ                W 579 *pc b e q*  
                                   εὐθὺς πανταχοῦ                ⚭<sup>c</sup> B C L 0133 13 *pc*  
                                   (omit)                    ⚭ Θ 1 *al c ff<sup>2</sup> r<sup>1</sup> sy<sup>s</sup>*

(Is this not an obvious “Alexandrian” conflation? Yet the UBS text adopts it without giving any indication that there are other readings.)

13. Mark 1:40     κυριε                    C L W Θ *pc e c ff* sy<sup>pal</sup>  
                                   οτι                    Byz ⚭ A *pl sy<sup>h</sup>*  
                                   κυριε οτι                B

(This appears to be a clear conflation on the part of B. Since Hort did not follow B here he presumably tended to the same opinion.)

14. Mark 5:42     ἐξεστησαν                Byz P<sup>45</sup> A K W Θ Π 0133 1 13 *pl e* sy<sup>p,h</sup>  
                                   ἐξεστησαν εὐθὺς                ⚭ B C L Δ 33 892 *pc bo*  
                                   ἐξεστησαν πάντες                D *it sa*

(If the producers of the “Syrian” text followed a policy of conflation, why did they neglect this fine opportunity? Note that Hort’s “late Syrian” reading now has the earliest attestation.)

15. John 4:29     πάντα ὅσα                Byz P<sup>66,75</sup> A D L W Γ Δ Θ Λ Π Ψ 086 1 13 *pl lat sy<sup>h</sup>*  
                                   πάντα     α                ⚭ B C *e a d q sy<sup>p</sup> cop*  
                                   πάντα ὅσα α                579

(This is an obvious conflation in one late MS. Note the strong early attestation for the “Byzantine” reading.)

16. John 5:37     εκεινος             μεμαρτυρηκεν     P<sup>75</sup> Ξ B L W 213 *pc a ff<sup>a</sup> j sy<sup>p,h</sup>*  
                          αυτος μεμαρτυρηκεν     Byz P<sup>66</sup> A Γ Δ Θ Λ Π Ψ 063 1 13 *pl/lat*  
                          εκεινος αυτος μαρτυρει     D a b c l q

(This appears to be a case of “Western” conflation. Note that Hort’s “late Syrian” reading now has very early attestation.)

17. John 7:39     πνευμα                             P<sup>66c,75</sup> Ξ K N T Θ Π Ψ *pc bo*  
                          πνευμα αγιον                     Byz P<sup>66</sup> L W X Γ Δ Λ 0105 1 13 *pl*  
                          πνευμα             δεδομενον             lat sy<sup>c,s,p</sup> Eusebius  
                          πνευμα αγιον δεδομενον     B 053 *pc e q sy<sup>pal,h</sup>*  
                          (το πνευμα το αγιον επ αυτοις)     D d f

(It would appear that B here conflates “Byzantine” and “Western” elements. Since Hort did not follow B here he must have tended toward the same opinion. Note that Hort’s “late Syrian” reading now has very early attestation.)

18. John 10:19     σχισμα ουν                     D 1241 *sy<sup>s</sup>*  
                          σχισμα     παλιν             P<sup>(45)75</sup> Ξ B L W X 33 *pc lat sy<sup>p</sup> sa*  
                          σχισμα ουν παλιν             Byz P<sup>66</sup> A Γ Δ Θ Λ Π Ψ 1 13 *pl sy<sup>h</sup>*

(A century ago this could have been interpreted as a “Syrian” conflation, but now we can scarcely say that P<sup>66</sup> conflated P<sup>75</sup> and D. The possibility must at least be considered that Hort’s “late Syrian” reading is in fact the earliest, the original.)

19. John 10:31     εβαστασαν                     P<sup>45</sup> Θ  
                          εβαστασαν ουν                 D 28 1780 *pc lat sy<sup>s</sup> bo*  
                          εβαστασαν     παλιν             (P<sup>75</sup>) Ξ B L W 33 *pc sy<sup>p</sup>*  
                          εβαστασαν ουν παλιν         Byz P<sup>66</sup> A X Π Ψ 1 13 565 *pl/f sy<sup>h</sup>*

(A century ago this could have been interpreted as a “Syrian” conflation, but now we can hardly say that P<sup>66</sup> conflated B and D. The possibility must be entertained that Hort’s “late Syrian” reading is in fact the earliest. All three words end in *nu*, so both [or all three] shorter readings could be the result of homoioteleuton.)

20. John 11:22     αλλα                     1780  
                                      και                     P<sup>75</sup> Ξ B C X 1 33 *pc it<sup>pt</sup>*  
                          αλλα και                 Byz P<sup>45,66</sup> Ξ<sup>2</sup> A C<sup>3</sup> D L W Θ Ψ Ω 0250 13 *pl/lat sy<sup>p,h</sup> cop*

(It seems obvious that the “Byzantine” reading cannot be a conflation of the “Alexandrian” reading and that of one late MS. 1780 has dropped part of the “Byzantine” reading. I suggest the same explanation for the “Alexandrian” reading. Observe that the “Byzantine” reading now has very early attestation.)

21. John 12:9     οχλος     πολυς                     Byz P<sup>66,75</sup> A B<sup>2</sup> I Q X Θ Ψ 065 1 33 *pl (cop)*  
                          ο οχλος     πολυς                     Ξ B L *pc lat*  
                          οχλος ο πολυς                     W 1010  
                          ο οχλος ο πολυς                     P<sup>66c</sup>

(Conflation or confusion? Did P<sup>66c</sup> conflate B and W? Or should we say that P<sup>66c</sup> has the original reading that everyone else [including P<sup>66†</sup>] simplified? Note that Hort’s “late Syrian” reading now has the earliest attestation, with a vengeance!)

22. John 14:14     τουτο                     P<sup>75</sup> A B L Ψ 060 33 *a/c vg cop*  
                                      εγω                     Byz P<sup>66</sup> Ξ D E G Q X Γ Δ Π *pm it sy<sup>p,h</sup>*  
                          τουτο εγω                     P<sup>66c</sup>

(This is an instructive conflation on the part of P<sup>66c</sup>. Note the early attestation for the “Byzantine” reading.)

23. John 16:4    αυτων μνημονευητε                    Ἰ<sup>c</sup> L 13 *a/lat*  
    μνημονευητε αυτων                    Byz K Γ Δ Ψ 054 1 *pm ff<sup>2</sup> sy<sup>pal</sup>*  
                          αυτων μνημονευητε αυτων                    A B Θ Π 33 *a/sy<sup>p,h</sup>*  
    μνημονευητε                                    Ἰ D a *sy<sup>s</sup> cop*

(This would appear to be a not very felicitous conflation on the part of B, etc.)

24. John 17:23    και γινωσκη                    P<sup>66</sup> Ἰ W 1 *pc lat*  
    ινα γινωσκη                    B C D L 33 *pc a e sy<sup>s</sup>*  
    και ινα γινωσκη                    Byz A Θ Ψ 054 13 *pm f q sy<sup>p,h</sup>*

(This could be a “Byzantine” conflation, but the first two readings could just as easily be independent simplifications of the longer reading.)

25. John 18:40    παλιν                                    P<sup>60</sup> Ἰ B L W X 0109 *pc*  
    παντες                                    G K N Ψ 1 13 33 *a/it sy<sup>p,pal</sup> cop*  
    παλιν παντες Byz (P<sup>66</sup>) A Γ Δ Θ 054 0250 *pm vg sy<sup>h</sup>*  
    παντες παλιν D

(This could be a “Byzantine” conflation, but it could just as easily be the case that the two shorter readings are independent simplifications of the longer one; homoioarcton perhaps. Is the “Western” reading a conflation or simply a reversal of the word order?)

26. Acts 7:16    του Συχεμ                            Byz P<sup>74</sup> D Ψ 049 056 0142 *pm lat*  
    εν Συχεμ                                Ἰ B C *a/ cop*  
    του εν Συχεμ                            Ἰ<sup>c</sup> A E

(This is presumably a conflation of the “Byzantine” and “Alexandrian” readings.)

27. Acts 10:48    του κυριου                            Byz H L P 049 056 *pm*  
    Ιησου Χριστου                        P<sup>74</sup> Ἰ A B E 33 *a/ cop*  
    του κυριου Ιησου                        Lect. *a/*  
    του κυριου Ιησου Χριστου                        D 81 *d p*

(This would appear to be a “Western” conflation of the “Byzantine” and “Alexandrian” readings.)

28. Acts 14:15    τον θεον ζωντα                    D *pc*  
    θεον τον ζωντα                        Ἰ  
    τον θεον τον ζωντα                        Byz P<sup>45</sup> H L P *pm*  
    θεον ζωντα                                P<sup>74</sup> B C E 33 *a/*

(A century ago this might have been interpreted as a “Syrian” conflation, but now we can hardly say that P<sup>45</sup> conflated Aleph and D. Why not say that Hort’s “late Syrian” reading is not only the earliest but also the best? I would say that the “Alexandrian” reading is decidedly inferior in terms of the discourse structure of the text, the sort of thing that would appeal to scribes without native speaker control of Koine Greek.<sup>2</sup>)

<sup>2</sup>For a complete statement of what I mean by “discourse structure”, see my book, *A Framework for Discourse Analysis* (Dallas: Summer Institute of Linguistics and University of Texas at Arlington, 1980).

29. Acts 24:14 τοις προφηταις *Byz*  $\aleph^c$  A *pm* syr bo  
 εν τοις προφηταις B C D *al*  
 τοις εν τοις προφηταις  $\aleph$  E

(This seems to be a clear conflation on the part of Aleph.)

30. Acts 25:5 τουτω ατοπον *Byz pm*  
 τουτω ατοπον  $\aleph$  A B C E 33 *al/lat*  
 τουτω ατοπον  $\Psi$  69 614 *al/syr bo*

(This would appear to be a conflation of the “Byzantine” and “Alexandrian” readings.)

31. 1 Cor. 7:34 η αγαμος και η παρθενος P<sup>15</sup> B P *al cop*  
 και η παρθενος η αγαμος *Byz* D F G K L  $\Psi$  *pm it syr*  
 η αγαμος και η παρθενος η αγαμος P<sup>46</sup>  $\aleph$  A 33 *pc*

(Although unquestionably early, this really does appear to be a conflation on the part of P<sup>46</sup>, etc.)

32. Phil. 1:18 πλην *Byz* D E K L *pm*  
 οτι B sy<sup>p</sup>  
 πλην οτι P<sup>46</sup>  $\aleph$  A F G P 048 33 *pc sa*

(Modern editors have tended to regard the long reading as original, but now that we know that the “Byzantine” text goes back at least to the second century we should reconsider the possibility that P<sup>46</sup>, etc. have a conflation. In the example above they have demonstrated this ability.)

33. Col. 2:2 του Θεου και Πατρος και του Χριστου *Byz* D<sup>c</sup> K *pm Lect*  
 του Θεου και Πατρος του Χριστου  $\aleph^b$   $\Psi$  *pc sy<sup>h</sup>*  
 του Θεου Πατρος και του Χριστου 0208 1908 sy<sup>p</sup>  
 του Θεου Πατρος του Χριστου A C it<sup>pt</sup> sa<sup>pt</sup> bo  
 του Θεου Πατρος Χριστου  $\aleph$  048  
 του Θεου Χριστου P<sup>46</sup> B (alone of MSS)  
 του Θεου D<sup>b</sup> H P 436 1881 sa<sup>pt</sup>  
 (at least seven further variations)

(The editors of the UBS text make the reading of B their first choice, and that of the “Byzantine” text their last choice! They must consider the “Byzantine” reading to be a prime illustration of “conflation”, but how did it come about? Did “Syrian editors” borrow the two καις from  $\Psi$  and 0208 respectively, or did these drop parts of the longer reading? Was Πατρος borrowed from Aleph, A, C or did these drop still other parts of the original? Presumably the UBS editors feel that H omitted part of B, but B could easily show the result of omission also, a not very difficult case of homoioteleuton [four words end in -ου]. I submit that the reading which best explains the rise of all the others is precisely that of the “Byzantine” text.)

34. Col. 3:17 Κυριου Ιησου *Byz* P<sup>46</sup> B ( $\Psi$ ) *pl*  
 Ιησου Χριστου A C D F G  
 Κυριου Ιησου Χριστου  $\aleph$  D<sup>2</sup> 365 1175 *pc*

(Aleph conflates, presumably. Note the early attestation for the “Byzantine” reading.)

35. 1 Thess. 5:27 τοις αγιοις 103 1984 1985  
 τοις αδελφοις  $\aleph$  B D E F G *pc d e f g sa*  
 τοις αγιοις αδελφοις *Byz* (P<sup>46</sup>)  $\aleph^c$  A K L P  $\Psi$  33 *pl/it syr bo*

(The “Byzantine” reading can scarcely be a conflation based on 103, so 103 must have a simplification of the “Byzantine” reading. I suggest the same explanation for the “Alexandrian-Western” reading. Both short forms could easily be the result of homoioteleuton [3 x -οις].)

36. Heb 7:22	και	920	
	κρειττονος	Byz P <sup>46</sup> Ξ <sup>c</sup> A C <sup>c</sup> D E K L P Ψ <i>p</i> /lat syr cop	
	και κρειττονος	Ξ B C 33 <i>pc</i>	

(It is clear that B could not have a conflation based on 920, unless it is the sole survivor of a very early tradition, but neither may we say that P<sup>46</sup> is simplifying B. Note that here it is the “Alexandrian” text that has the “fuller, smoother” reading.)

37. Rev. 6:1/2	και ιδε	και ιδου	M <sup>a,b,ept</sup>
	και ειδον και ιδου		M <sup>c,d,ept</sup> (A C)
	και ιδε και ειδον και ιδου		Ξ (alone)

(Here Aleph conflates the readings of two groups of minuscule MSS. It follows that though these MSS are much later in date than Aleph they reflect an earlier form of the text. In 6:3/4 Aleph repeats this reading in a clear case of assimilation. The statement of evidence in examples 37, 38, 39 and 49 is taken from *The Greek New Testament According to the Majority Text* [Thomas Nelson, 1985].)

38. Rev. 6:5	και ιδε	και ιδου	M <sup>a,b</sup>
	και ειδον και ιδου		M <sup>c,d,ept</sup> C (A)
	και ιδε και ειδον και ιδου		Ξ (alone)

(Aleph repeats the conflation.)

39. Rev. 6:7/8	και ιδε	και ιδου	M <sup>a,b,ept</sup>
	και ειδον και ιδου		M <sup>c,d,ept</sup>
	και ιδον και ιδου		A (C)
	και ιδε και ιδον και ιδου		Ξ (alone)

(Aleph repeats the conflation again.)

**Group 1. b)** Addition plus simple coupling links, or omission.

40. Matt. 4:3	αυτω ο πειραζων	ειπεν	Byz C L P Θ 0233 <i>pm</i> k sy <sup>h</sup>
	ο πειραζων	ειπεν αυτω	Ξ B W 1 13 33 <i>al</i> vg sy <sup>p</sup> bo
	αυτω ο πειραζων και ειπεν αυτω		D it sy <sup>c,s,pal</sup>

(Here we presumably have a “Western” conflation of the “Byzantine” and “Alexandrian” readings.)

41. Matt. 9:18	εις ελθων/εισελθων	Byz Ξ <sup>2</sup> C D E K M N S V W X Θ 1 33 <i>pm</i> d f
	προσελθων	Ξ 69 157 <i>pc</i> q sy <sup>p</sup>
	εις προσελθων	Ξ <sup>1</sup> B lat <i>pc</i>
	τις προσελθων	L 13 <i>al</i> k
	τις ελθων	Γ <i>al</i>

(Codex B appears to have a conflation, an opinion with which the editors of the UBS texts evidently concur.)

42. Matt. 27:41	και πρεσβυτερον	A B L Θ 1 13 <sup>pt</sup> 33 <i>al</i> it <sup>pt</sup> vg sa
	και	D W <i>pc</i> it <sup>pt</sup> sy <sup>s</sup>
	και πρεσβυτερων και Φαρισαιων	Byz Δ Φ 13 <sup>pt</sup> <i>pm</i> sy <sup>p,h</sup> bo Diatessaron

(Here, at last, we seem to have a clear “Byzantine” conflation, albeit dating from the second century. The whole clause in the “Byzantine” text reads like this:

οι αρχιερεις εμπαιζοντες μετα των γραμματεων και πρεσβυτερων και Θαρισαιων ελεγον. It  
really seems to be a bit too full; so much so that editors trained at Alexandria might well have been tempted to improve the style by shortening it. Might the “Western” reading be the result of parablepsis? In fact, both short forms could easily be the result of homoioteleuton.)

- |                |  |                                |   |
|----------------|--|--------------------------------|---|
| 43. Luke 24:53 | αινουντες<br><br>αινουντες και ευλογουντες | ευλογουντες<br><br>ευλογουντες | D <i>it</i> <sup>pt</sup><br>P <sup>75</sup> ∓ B C L <i>cop sy</i> <sup>s,pal</sup><br>Byz A C <sup>2</sup> K W X Δ Θ Π Ψ 063 1 13 <i>p/it</i> <sup>pt</sup> <i>vg sy</i> <sup>p,h</sup> <i>Diat.</i> |
|----------------|--|--------------------------------|---|

(This is one of Hort’s eight “Syrian conflations”. According to Hort’s own judgment Codex D has omitted 329 words from the genuine text of the last three chapters of Luke, plus adding 173, substituting 146, and transposing 243. Since the producer of D was on something of an omitting spree in these chapters, it is not unreasonable to suggest that D has simply dropped “and blessing” from the original reading, an easy instance of homoioteleuton. Nor is it hard to imagine that editors trained at Alexandria might reduce the longer reading to the proportions exhibited by the “Alexandrian” text-type. Note that once more the “Byzantine” reading has second century attestation.)

- |                |   |      |   |
|----------------|---|------|---|
| 44. Acts 20:28 | του κυριου<br>του θεου<br>του κυριου και θεου | θεου | P <sup>74</sup> A C D E Ψ 33 <i>al cop</i><br>∓ B 056 0142 <i>al syr</i><br>Byz L P 049 <i>pm</i> |
|----------------|---|------|---|

(Here we have a fine candidate for a “Byzantine” conflation, provided that the opposite interpretation is rejected. The reading of A could easily be a case of homoioteleuton and that of B the result of parablepsis or stylistic revision.)

- |               |  |        |   |
|---------------|--|--------|---|
| 45. Acts 25:6 | πλειους η δεκα<br>οκτω η δεκα<br>πλειους οκτω η δεκα<br>ου πλειους οκτω η δεκα | η δεκα | Byz Ψ <i>pm</i><br>2147 <i>pc syr</i><br>E <i>al</i><br>(P <sup>74</sup> ∓) A B C 33 <i>pc lat bo</i> |
|---------------|--|--------|---|

(Is this an “Alexandrian” conflation?)

- |                 |  |               |   |
|-----------------|--|---------------|---|
| 46. 2 Cor. 11:3 | της απλοτητος<br>της αγνοτητος<br>της απλοτητος και της αγνοτητος<br>της αγνοτητος και της απλοτητος | της αγνοτητος | Byz ∓ <sup>c</sup> H K P Ψ 0121 0243 <i>pm vg syr</i><br>five early fathers<br>P <sup>46</sup> ∓ B G 33 <i>pc it cop</i><br>D |
|-----------------|--|---------------|---|

(It appears that the “Alexandrian” and “Western” texts have separate conflations. From their use of brackets we may conclude that the editors of both the Nestle and UBS editions recognize the possibility.)

- |              |   |               |  |
|--------------|---|---------------|--|
| 47. Eph. 2:5 | τοις παραπτωμασιν<br>τοις παραπτωμασιν και ταις αμαρτιας<br>εν τοις παραπτωμασιν και ταις επιθυμιας | ταις αμαρτιας | Byz ∓ A D <sup>2</sup> <i>p/ cop</i><br>D (G) <i>lat</i><br>Ψ<br>B |
|--------------|---|---------------|--|

(Here we have separate conflations on the part of Ψ and B. Since Hort did not follow B here he must have tended to the same opinion. The editors of the Nestle and UBS editions evidently agree as well.)

- |               |  |            |   |
|---------------|--|------------|---|
| 48. Col. 1:12 | τω καλεσαντι<br>τω ικανωσαντι<br>τω καλεσαντι και ικανωσαντι | ικανωσαντι | D G 33 <i>pc it sa</i><br>Byz P <sup>46,(61)</sup> ∓ A C D <sup>c</sup> E K L P Ψ <i>p/ syr bo</i><br>B |
|---------------|--|------------|---|

(This obvious conflation on the part of Codex B was acknowledged by Hort [p. 240], a judgment with which the editors of the Nestle and UBS editions are in full agreement.)

49. Rev. 17:4	της πορνειας αυτης		<b>M</b> <sup>b,c,d,e</sup> A
	της πορνειας	της γης	<b>M</b> <sup>a</sup>
	της πορνειας αυτης και της γης		ℵ (alone)

(This would appear to be a clear conflation on the part of Aleph.)

Before going on to examples where the required phenomena for possible conflations are less clear, it will be well to pause and see what instruction may be gained from these clear possible examples. Ignoring probabilities for the moment, I will tabulate the “possible” conflations.

	Total	Examples
Western text-type	4	7, 16, 27, 40
Codex D	3	3, 25, 46
Alexandrian text-type	8	11, 12, 23, 31, 32, 36, 45, 46
Codex B	7	1, 6, 13, 17, 41, 47, 48
Codex Aleph	7	3, 29, 34, 37, 38, 39, 49
Byzantine text-type	13	4, 9, 18, 19, 20, 24, 25, 28, 33, 35, 42, 43, 44

None of the Western “conflations” has early papyrus support, and I believe there is general agreement among scholars that all seven of the “Western” instances are in fact conflations (or secondary readings). None of the B or Aleph “conflations” has early papyrus support. I believe there is general agreement among scholars that all 14 B and Aleph instances are in fact conflations (or secondary readings). (Since Hort was evidently aware of these conflations in B, it is difficult to understand how he could affirm that to the best of his knowledge there were no “Neutral” conflations.) Three of the “Alexandrian” instances (31, 32, 46) have early papyrus attestation. Modern editors have tended to include all eight “Alexandrian” readings in their texts, although some express doubt about 36 and 46. One cannot help but suspect that they are still wearing Hortian blinders, to use Colwell’s phrase.

Six of the “Byzantine” instances (18, 19, 20, 25?, 28, 35?) now have early papyrus attestation (another two are attested by the Diatessaron). It follows that although modern editors continue to reject these readings, it can no longer be argued that they are late. If they are conflations then they happened in the second century. It is significant that in fully 35 of the 49 examples given the “Byzantine” text is possibly being conflated by other witnesses, not vice versa.

It is evident that all “text-types” have possible conflations and that “Western” and “Alexandrian” witnesses have actual conflations. I would argue that all the “Byzantine” instances are original, but in any case it should be clear that “conflation” may not responsibly be used to argue for a late “Byzantine” text-type. On the contrary, examples like 8, 14, 16, 17, 21, 22, 31, 32, 36, 37, 42, 43, and 46 might reasonably be used to argue for a rather early “Byzantine” text-type.

**Group 2.** a) Complicated by substitution, transposition, moderate internal changes, or omissions.

50. Matt. 7:10	η και ιχθυν αιτησει	ℵ B C (1) 33 <i>pc</i>
	και εαν ιχθυν αιτηση	Byz (L W) Θ <i>al sy</i> <sup>p,h</sup>
	η εαν ιχθυν αιτηση	lat <i>sy</i> <sup>c</sup>
	η και εαν ιχθυν αιτηση	K <sup>c</sup> 13 <i>al</i>



(This could be either a “Western” or an “Alexandrian” conflation, but presumably not a “Byzantine”.)

51. Matt. 7:18 ποιειν . . . ενεγκειν      ℵ (alone of MSS)  
 ενεγκειν . . . ποιειν      B (alone of MSS)  
 ποιειν . . . ποιειν      Byz ℵ<sup>c</sup> C K L W X Z Δ Θ Π 0250 1 13 33 *p*/lat syr cop

(The editors of the UBS editions evidently agree that the “Byzantine” reading here is genuine.)

52. Matt. 8:1 καταβαντι δε αυτω      Byz K L (Δ) *pm* (lat sy<sup>p,h</sup>)  
 και καταβαντος αυτου      Z sy<sup>c,pal</sup>  
 καταβαντος δε αυτου      B C W Θ 33 (lat sy<sup>p,h</sup>) cop  
 καταβαντι δε αυτου      ℵ

(If anyone has conflated it would seem to be the “Alexandrians”. Aleph certainly has a conflation.)

53. Matt. 9:2 σου αι αμαρτιαι      ℵ B C W Δ 1 33 *pc*  
 σοι αι αμαρτιαι      D Δ<sup>c</sup> *pc k*  
 σοι αι αμαρτιαι σου      Byz L Θ 0233<sup>v</sup> 13 *pm* lat syr  
 σου αι αμαρτιαι σου      M

(Codex M has evidently conflated, but should we say the same of the “Byzantine” text? Or are the “Alexandrian” and “Western” readings independent simplifications?)

54. Matt. 10:3      Θαδδαιος      ℵ  
 και      Θαδδαιος      B *pc vg* cop  
 και Λεββαιος      D 122 d k  
 και Λεββαιος ο επικληθεις Θαδδαιος      Byz C<sup>2</sup> K L W X Δ Θ Π 1 *p*/syr

(The “Byzantine” reading does not really present the phenomena of a conflation. The reading of Aleph is clearly wrong. The “Western” reading could easily have resulted from homoioteleuton. It is not difficult to imagine that editors trained at Alexandria might prefer a shorter reading.)

55. Matt. 10:13 ει δε μηγη      D sy<sup>s</sup>  
 εαν δε μη η αξια      Byz ℵ B *p*/lat sy<sup>p,h</sup>  
 ει δε μη αξια      L

(This appears to be a conflation on the part of Codex L.)

56. Matt. 12:4 εφαγεν ους      Byz (P<sup>70</sup>) C K L Δ Θ Π 0233 1 33 *p/vg sy<sup>h</sup>* cop  
 εφαγον ο      B 481  
 εφαγεν ο      D W 13 *it sy<sup>p,(c)</sup>*  
 εφαγον ους      ℵ

(Aleph and the “Western” text appear to have separate conflations of the “Byzantine” reading and that of B. P<sup>70</sup> has εφαγεν but no pronoun [the papyrus is broken]—thus the “Byzantine” form of the verb has the earliest attestation.)

57. Mat. 12:46 ετι αυτου λαλουντος      ℵ B 33 *pc* lat  
 λαλουντος δε αυτου      D L Z 892 sy<sup>p</sup>  
 ετι δε αυτου λαλουντος      Byz C W Θ 1 13 *pm sy<sup>h</sup>*

(Is this a “Byzantine” conflation or are the other two readings independent simplifications?)

58. Matt. 13:28	οι δε αυτω λεγουσιν οι δε δουλοι ειπον αυτω οι δε δουλοι αυτω λεγουσιν λεγουσιν ουτω οι δουλοι οι δε δουλοι λεγουσιν αυτω	B 157 <i>pc cop</i> <i>Byz L W Θ 1 13 pm vg sy<sup>h</sup></i> C D it ( <i>sy<sup>c,s,p</sup></i> ) Σ
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(Conflation or confusion? Both C and Aleph appear to have conflations, both based on the “Byzantine” reading plus B and D respectively. Surprisingly, the UBS text follows Aleph, without comment, while Nestle<sup>24</sup> follows C. The reading of B would seem to be a clear error.)

59. Matt. 14:6	γενεσιων δε αγομενων γενεσιους δε γενομενοις γενεσιους δε αγομενοις γενεσιων δε γενομενων	<i>Byz W 0119 0136 13 pm ff<sup>1</sup> sy<sup>h mg</sup></i> Σ B D L Z <i>pc (syr)</i> 1 <i>pc</i> C K N Θ <i>al (syr)</i>
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(Codex C and f<sup>1</sup> appear to have separate conflations of the “Byzantine” and “Alexandrian” readings.)

60. Matt. 14:34	επι την γην Γεννησαρετ εις την γην Γεννησαρετ επι την γην εις Γεννησαρετ επι την γην εις Γεννησαρ	C N 13 <i>al/sy<sup>pal</sup></i> <i>Byz L 1 pm lat sy<sup>p,(c,s)</sup></i> Σ B W Δ 0119 33 <i>pc sy<sup>h</sup></i> D 700
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(Might this be an “Alexandrian/Western” conflation?)

61. Matt. 15:14	οδηγοι εισιν τυφλοι τυφλων οδηγοι εισιν τυφλοι οδηγοι εισιν τυφλων τυφλοι εισιν οδηγοι τυφλοι εισιν οδηγοι τυφλων	<i>Byz C W X Δ Π 0106 pm q</i> Σ <i>cop sy<sup>c</sup></i> K <i>pc sy<sup>s</sup></i> B D 0237 Σ <sup>c</sup> L Z Θ 1 13 33 <i>al/lat sy<sup>p,h</sup></i>
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(The “Alexandrian” reading appears to be a conflation of the “Byzantine” and “Western” readings. Codices Aleph and K appear to have separate reductions of the “Byzantine” reading, due to homoioarcton.)

62. Matt. 17:7	προσελθων . . . ηψατο αυτων και ειπεν προσηλθεν . . . και αψαμενος αυτων ειπεν προσελθων . . . και αψαμενος αυτων ειπεν προσηλθεν . . . και ηψατο αυτων και ειπεν	<i>Byz C L W 1 pm sy<sup>h</sup></i> Σ B <i>pc</i> Θ 13 <i>pc</i> D <i>lat sy<sup>p,pal,(c)</sup></i>
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(The “Western” and “Caesarean” readings appear to be separate conflations of the “Byzantine” and “Alexandrian” readings.)

63. Matt. 19:9	μη επι πορ. και γαμ. αλλην μοιχαται παρ. λογου πορ. ποιει αυ. μοιχευθηναι παρ. λογου πορ. και γαμ. αλλην μοιχαται μη επι πορ. και γαμ. αλλην ποιει αυ. μοι.	<i>Byz Σ C<sup>c</sup> K L N (W) Z Δ Θ Π 078 pm vg sy<sup>s,p,h</sup></i> (P <sup>25</sup> ) B 1 <i>bo</i> D 13 33 <i>pc it sy<sup>c,pal</sup> sa</i> C 1216 <i>pc</i>
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(The “Western” text and Codex C have independent conflations of the “Byzantine” and “Alexandrian” readings.)

64. Matt. 20:10	ελθοντες δε και ελθοντες ελθοντες δε και	<i>Byz Σ L W Z 1 pm sy<sup>h</sup> bo</i> B C D Θ 085 13 33 <i>pc e sy<sup>c,s,p</sup></i> N 473 <i>pc lat arm</i>
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(An assortment of witnesses conflate the “Byzantine” and “Alexandrian” readings.)

## 65. Matt. 22:13

αρατε αυτον ποδων κ. χειρων και β. α	D it <sup>pt</sup> sy <sup>c,s</sup>
δησαντες αυτου ποδας κ. χειρας εκβ. α.	Σ B L Θ 085 1 (13) <i>pc</i> it <sup>pt</sup> vg sy <sup>p</sup> cop
δησαντες αυτου ποδας κ. χειρας αρατε α. και εκβ.	Byz C W 0138 <i>pm</i> (M Φ <i>al</i> ) sy <sup>h</sup>

(Is this really a “Byzantine” conflation? The longest reading is perfectly reasonable as it stands; perhaps a bit too ‘full’ for editors trained at Alexandria, but just right for a Jew speaking Aramaic. Might the “Western” reading be a Latin revision?)

66. Mark 4:5	και οτι	D W it sy <sup>s</sup>
	οπου	Byz Σ A ρ/ vg sy <sup>p,h</sup>
	και οπου	B

(An evident conflation on the part of B.)

67. Mark 7:35	διηνοιχθησαν	Byz P <sup>45</sup> A N X Γ Π 0131 13 <i>pm</i> lat sy <sup>r</sup>
	ηνοιγησαν	Σ B D Δ 0274 1 892
	ηνοιχθησαν	L
	διηνοιγησαν	W Θ <i>pc</i>

(Has P<sup>45</sup> conflated L and W, or have these managed independent conflations of the “Byzantine” and “Alexandrian” readings? Note that Hort’s “late Syrian” reading now has the earliest attestation.)

## 68. Mark 9:49

πας γαρ πυρι αλισθησεται	πασα γαρ θυσια αλι αλισθησεται	B L (Σ W) Δ 0274 1 13 <i>pc</i> sy <sup>s</sup> sa Diat <sup>apt</sup>
πας γαρ πυρι αλισθησεται και πασα θυσια αλι αλισθησεται		D it
		Byz A E K N Π Σ (C X Θ Ψ) <i>pm</i> f l q vg sy <sup>p,h</sup> Diat <sup>apt,p</sup>

(This is another of Hort’s “Syrian conflations”. But the “Alexandrian” reading could easily be the result of homoioteleuton, and a different bit of parablepsis could have given rise to the “Western” reading. Does not the presence of the article with “salt” at the beginning of vs. 50 suggest that “salt” has already been introduced in the prior context? In any case, the “Byzantine” reading has early attestation and may not be dismissed as “late Syrian”.)

69. Mark 12:17	και αποκριθεις	W 258 <i>al</i>
	ο δε Ιησους	Σ B C L Δ Ψ 33 <i>pc</i> sy <sup>(p)</sup> cop
	και αποκριθεις ο Ιησους	Byz P <sup>45</sup> A N X Γ Π Φ 1 13 <i>pm</i> sy <sup>(s),h</sup>
	αποκριθεις δε ο Ιησους	D 700 <i>pc</i> lat
	αποκριθεις δε	Θ 565

(Who is conflating whom? It seems more likely that *Theta* has simplified the “Western” reading than that the latter builds on the former. But the “Western” reading may well be a conflation of the “Byzantine” and “Alexandrian” readings. It seems clear that P<sup>45</sup> cannot have conflated W and B, but might these have separate simplifications of the “Byzantine” reading? Note that Hort’s “late Syrian” reading now has the earliest attestation.)

70. Luke 9:57	και πορευομενων	P <sup>45,75</sup> Σ B C L Θ Ξ 33 <i>pc</i> sy <sup>c,s,p</sup> bo
	εγενετο δε πορευομενων	Byz A W Ψ 1 <i>pm</i> lat sy <sup>h</sup>
	και εγενετο πορευομενων	D 13 a c e r <sup>1</sup>

(This would appear to be a “Western” conflation.)

71. Luke 10:42 ενος δε εστιν χρεια *Byz* P<sup>45,75</sup> A C K P W Γ Δ Θ Λ Π Ψ 13 *pl* lat sy<sup>c,p,h</sup> sa  
 ολιγων δε χρεια εστιν η ενος B  
 ολιγων δε εστιν χρεια η ενος P<sup>3</sup> L C<sup>2</sup> 1 33 *pc* sy<sup>hmg</sup> bo  
 ολιγων δε εστιν η ενος Σ

(The MSS usually associated with the “Alexandrian” text-type are rather scattered here. Codex L and company might be said to conflate the “Byzantine” reading and that of B. Note that Hort’s “late Syrian” reading now has the earliest attestation, with a vengeance.)

72. Luke 11:12 η και P<sup>75</sup> Σ B L 1 13 33 cop  
 εαν δε και D  
 η και εαν *Byz* P<sup>45</sup> R W X Γ Δ Θ Π Ψ *pl* sy<sup>h</sup>

(Should we say that “Syrian” editors conflated the “Alexandrian” and “Western” readings, or is Hort’s “late Syrian” reading really the original?)

73. Luke 12:30 ζητει D it  
 επιζητουσιν P<sup>75</sup> Σ B L X 070 13 33 *pc*  
 επιζητει *Byz* P<sup>45</sup> A Q W Γ Δ Θ Λ Π Ψ 1 *pl*

(Conflation or confusion? Note that Hort’s “late Syrian” reading now has very early attestation.)

74. Luke 13:2 οτι ταυτα Σ B D L *pc* d e r<sup>1</sup>  
 τα τοιαυτα 69 *pc*  
 οτι τοιαυτα *Byz* P<sup>75</sup> A W X Γ Δ Θ Λ Π Ψ 070 1 *pm* lat syr

(Did P<sup>75</sup> conflate B and 69? Note that Hort’s “late Syrian” reading now has the earliest attestation.)

75. John 5:15 ανηγγειλεν *Byz* P<sup>66,75</sup> A B Γ Θ Λ Π Ψ 063 1 *pm* sa (lat sy<sup>h</sup>)  
 ειπεν Σ C L *pc* e q sy<sup>c,s,p</sup> bo  
 ανηγγειλεν και ειπεν αυτοις W  
 απηγγειλεν D K U Δ 13 33 *al* (lat sy<sup>h</sup>)

(Codex W appears to have a conflation involving the “Byzantine” and “Alexandrian” readings. Note that the “Byzantine” reading, which Hort tentatively rejected in spite of B, now has strong early attestation. The “Western” departure is based on the “Byzantine” reading, presumably the original.)

76. John 6:69 ο αγιος του Θεου P<sup>75</sup> Σ B C D L W  
 ο Χριστος ο υιος του Θεου *Byz* K Π Ψ 0250 13 (Δ Θ 1 33) *pl* lat syr Diatessaron  
 ο Χριστος ο αγιος του Θεου P<sup>66</sup> cop

(An instructive conflation on the part of P<sup>66</sup>.)

77. John 7:41 αλλοι ελεγον *Byz* P<sup>66\*</sup> Σ D W Γ Δ Π Ψ 0105 13 *pm* syr  
 οι δε ελεγον P<sup>66c,75</sup> B L N T X Θ 33 *al* lat  
 αλλοι δε ελεγον 1 *pc* e bo

(Is this a “Caesarean” conflation? Note that the corrector of P<sup>66</sup> has taken a “Byzantine” reading and changed it to an “Alexandrian”—since he did that sort of thing repeatedly it would appear that there were exemplars of each type in the scriptorium, the more so in that he frequently did the opposite as well, i.e. changed an “Alexandrian” reading to a “Byzantine”. This in A.D. 200!)

78. John 9:6 επεθηκεν B *pc*  
 εχρισεν 661  
 επεχρισεν *Byz* P<sup>66,75</sup> Σ A C D K L W Δ Θ Π Ψ 0124 0216 1 13 *pl* lat syr cop

(Presumably no one would wish to suggest that the “Byzantine” reading is a conflation of B and 661, even before the advent of P<sup>66,75</sup>! And yet, Hort followed B. . . . .)

79. John 9:8	τυφλος ην	<i>Byz C<sup>3</sup> Γ Δ pm</i>
	προσαιτης ην	<i>P<sup>66,75</sup> ⋈ B C D al lat cop sy<sup>s,p,h</sup></i>
	τυφλος ην και προσαιτης	<i>69 pc e sy<sup>pal</sup></i>

(An evident conflation on the part of a few MSS.)

80. John 11:44	αυτοις ο Ιησους	<i>Byz P<sup>45,66</sup> ⋈ A C<sup>2</sup> D X Γ Δ Θ Λ Π Ψ 0250 1 13 pl it</i>
	αυτοις	<i>157</i>
	ο Ιησους	<i>700 sy<sup>s</sup></i>
	ο Ιησους αυτοις	<i>L W</i>
	Ιησους αυτοις	<i>P<sup>75</sup> B C cop</i>

(157 and 700 have separate simplifications of the “Byzantine” reading. I suggest the same explanation for the “Alexandrian” reading—the editors of the UBS text evidently agree, whereas Hort did not.)

81. John 13:24	πυθεσθαι τις αν ειη π. ου λεγει	<i>Byz P<sup>66</sup> A (D) K W Γ Δ Λ Π 1 13 pl syr cop</i>
	και λεγει αυτω ειπε τις εστιν π. ου λεγει	<i>B C I L X 068 33 pc</i>
	πυθεσθαι τις αν ειη π. ου ελεγεν και λεγει αυτω ειπε τις εστιν π. ου λεγει	<i>⋈</i>

(This would appear to be an unusually blatant conflation on the part of Aleph, based on the “Byzantine” and “Alexandrian” readings.)

82. John 13:36	απεκριθη	<i>B C L pc lat cop</i>
	λεγει αυτω	<i>D</i>
	απεκριθη αυτω	<i>Byz P<sup>66</sup> ⋈ A C<sup>3</sup> K W X Γ Δ Θ Λ Π Ψ 1 13 pl</i>

(A century ago this might have been interpreted as a “Syrian” conflation of the “Alexandrian” and “Western” readings, but now the presence of P<sup>66</sup> rather encourages the opposite conclusion.)

83. Acts 11:7	ηκουσα δε	<i>Byz L P pm</i>
	και ηκουσα	<i>D pc sy<sup>s</sup></i>
	ηκουσα δε και	<i>⋈ A B E al cop</i>

(Might this be an “Alexandrian” conflation?)

84. Acts 23:9	τινες	<i>P<sup>74</sup> A E 33 pc bo</i>
	οι γραμματεις	<i>Byz pm</i>
	τινες των γραμματεων	<i>⋈ B C al sa</i>

(Might this be an “Alexandrian” conflation?)

85. Rom. 6:12	αυτη	<i>P<sup>46</sup> D E F G d f g m</i>
	ταις επιθυμiais αυτου	<i>⋈ A B C al lat cop</i>
	αυτη εν ταις επιθυμiais αυτου	<i>Byz K L P Ψ pm</i>

(Here is another fine candidate for a “Byzantine” conflation, unless the other two readings are independent simplifications. If the “Western” reading were original, however could the “Alexandrian” reading have come into being, and vice versa? But if the “Byzantine” reading is original the other two are easily explained.)

86. 1 Cor. 9:21	κερδησω	ανομους	Byz	ℵ <sup>c</sup>	K L Ψ	pl
	κερδανω	τους ανομους	ℵ	A B C P 33	pc	
	κερδησω	τους ανομους	P <sup>46</sup>			
	κερδανω	ανομους	F G			
	τους ανομους	κερδησω	D E			

(Might this case involve a “Western” conflation, or perhaps two of them? Note that P<sup>46</sup> supports the “Byzantine” form of the verb—if it has a conflation then the “Byzantine” and “Alexandrian” components already existed in AD 200.)

87. 2 Cor. 7:14	επι	τιτου αληθεια	ℵ	B	pc
	η προς	τιτον αληθεια	D E F G P Ψ	pc lat syr cop	
	η επι	τιτου αληθεια	Byz P <sup>46</sup>	ℵ <sup>c</sup>	C K L 0243 pl

(A century ago this might have been interpreted as a “Syrian” conflation, but P<sup>46</sup> now makes the “Byzantine” reading the earliest and enhances its claim to be the original—a claim with which the editors of the UBS text evidently concur.)

88. 1 Thess. 3:2	και διακονον του Θεου	και συνεργον ημων	Byz	K	pl syr
	και διακονον του Θεου		ℵ	A P Ψ	pc lat cop
		και συνεργον	B	1962	
		και συνεργον του Θεου	D	33 b d e mon	
	διακονον	και συνεργον του Θεου	G	f g	

(Both “Alexandrian” readings could be the result of homoioarcton [2 x και], or did B simplify the “Western” reading? Codex G evidently has a conflation and Codex D might be said to have one. Is the “Byzantine” reading a conflation, or is it the original with which all the others have tampered in one way or another?)

89. 2 Thess 3:4	και εποιησατε και ποιειτε		G		
	και ποιειτε και ποιησετε		Byz	ℵ <sup>c</sup> D <sup>c</sup> Ψ	pl
	ποιειτε και ποιησετε		ℵ	A	pc
	ποιειτε και ποιησατε		D		
	και εποιησατε και ποιειτε και ποιησετε		B	sa	

(This would appear to be a not very elegant conflation on the part of B, which is abandoned by both the Nestle and UBS texts. Codex D appears to have a separate conflation.)

90. Heb 9:10	και δικαιομασιν	D <sup>2</sup>	K L 056 075 0142 0150 0151 0209 0220 (532 MSS = 94%) <sup>3</sup>	a vg sy <sup>h</sup>
	δικαιωματα	P <sup>46</sup>	ℵ A I P 0278 (24 MSS = 5%)	b sa
	και δικαιοματα	ℵ <sup>2</sup>	B (8 MSS = 1%)	
	δικαιωμα	D	(alone)	

(An evident conflation on the part of B, building on the “Byzantine” and “Alexandrian” readings. Note that 0220 is III century, giving the “Byzantine” reading overt early attestation.)

**Group 2. b) Substantial differences—conflation dubious.**

91. Matt. 10:23	φευγετε εις την αλλην	Byz	C K X Δ Π	pl	
			φευγετε εις την ετεραν	ℵ B W 33	pc
	φευγετε εις την αλλην κ. εκ τ. δ. υ. φευγετε εις την ετεραν	Θ	(D L 1 13)	pc	

<sup>3</sup>This statement of evidence is based on the series *Text und Textwert*, ed. K. Aland. It represents an almost complete collation of extant MSS.

(The “Western” reading here seems to include a conflation of the “Byzantine” and “Alexandrian” readings.)

92. Matt. 27:23 ο δε εφη ο δε ηγεμων εφη  
λεγει αυτοις ο ηγεμων  
ο δε ηγεμων εφη

Ν B Θ 028113 33 *pc sa*  
D L 1 *pc lat sy<sup>p</sup> bo*  
Byz A W 064 0250 *pm sy<sup>h</sup>*

(Conflation or confusion?)

93. Mark 6:33

ε. και προηλθον αυτους και συνηλθον προς αυτον  
ε. και προηλθον αυτους  
ε. και προσηλθον αυτους  
ε. και προσηλθον αυτοις  
ε. και συνηλθον αυτου  
ε. και ηλθον αυτου  
προς αυτους και συνηλθον προς αυτον

Byz P<sup>84v</sup> E G K Π (A N Σ 13) *pm f (q) sy<sup>h</sup>*  
Ν B (0187<sup>v</sup>) *pc aur l vg (cop)*  
L *pc*  
Δ Θ  
D (28 700) b  
565 it Diat<sup>p</sup>  
33

(This is another of Hort’s eight “Syrian conflations”, but unless one is prepared to argue that the “Byzantine” reading is based on 33 it does not meet the requirements for a conflation and may properly be viewed as the original that all the others have simplified. Hort’s discussion of this case had been thought by some to be especially impressive, but I would say that he simply misunderstood the basic meaning of the text. In vs. 34 Jesus came out of the boat, not some secluded spot on land. The folks in Egypt could have had the same difficulty as Hort and produced the “Alexandrian” reading. The “Western” reading [and the “Alexandrian”] could be the result of a bit of parablepsis [homoioarcton—2 x και]. The reading of 33 is evidently secondary, however it came about.)

94. Mark 8:26

μηδε εις την κωμην εισελθης μηδε ειπης τινη εν τη κωμη  
μηδε εις την κωμην εισελθης  
μη εις την κωμην εισελθης  
υπαγε εις τον οικον σου και μηδενι ειπης εις την κωμην  
υπαγε εις τον οικον σου και εαν εις την κωμην εισελθης μηδενι ειπης μηδε εν τη κωμη

Byz A C E K N X Δ Π Σ 33 *pl sy<sup>p,h</sup> Diat*  
Ν<sup>c</sup> B L 1 *pc cop sy<sup>s</sup>*  
Ν W  
D d q  
13 (Θ *pc lat*)

(This is another of Hort’s “Syrian conflations”, but the “Byzantine” reading does not meet the requirements for a conflation and may reasonably be viewed as the original—the folks in Egypt may have felt that it was redundant, reducing it to the “Alexandrian” reading, although the latter could also be the result of homoioarcton [2 x MHΔEEI]. The “Western” text rewrites the material, as it often does. The “Caesarean” reading evidently involves a conflation.)

95. Mark 9:38

ος ουκ ακολουθει ημιν και εκωλυσαμεν αυτον οτι ουκ ακολουθει ημιν  
ος ουκ ακολουθει ημιν και εκωλυσαμεν αυτον  
ος ουκ ακολουθει μεθ ημων και εκωλυμεν αυτον  
και εκωλυσαμεν αυτον οτι ουκ ακολουθει ημιν  
και εκωλυμεν αυτον οτι ουκ ηκολουθει ημιν

Byz A E K N Π Σ *pm sy<sup>h</sup>*  
X (W 1) 13 *pc lat*  
D  
C *pc aur f cop*  
Ν B Δ Θ 0274 (L Ψ) *pc sy<sup>s,p,pal</sup> Diat*

(Here is yet another of Hort’s “Syrian conflations”. If this is a “Byzantine” conflation, it is built on the lesser “Western” and “Alexandrian” witnesses, and in that event where did D and B get their readings? Is it not more reasonable to regard the “Byzantine” reading as the original that the others have variously simplified? Nestle<sup>24</sup> seems to reflect essentially this opinion. In fact the “Western” reading could easily have resulted from homoioteleuton or a stylistic deletion of the third clause as being redundant. A glance at Luke 9:49 suggests that the Alexandrians harmonized Mark with Luke.)

96. Luke 9:10 τοπον ερημον πολεως καλουμενης Βηθσαιδαν *Byz A C W (1) 13 pm sy<sup>(p),h</sup>*  
τοπον ερημον *ℵ al sy<sup>c</sup>*  
πολιν καλουμενην Βηθσαιδα *ℵ<sup>c</sup> (P<sup>75</sup>) B L Ξ 33 pc (sy<sup>s</sup>) cop*  
κωμην λεγουμενην Βηδσαιδα *D*  
κωμην καλουμενην Βηθσαιδαν εις τοπον ερημον *Θ*

(This is still another of Hort's eight "Syrian conflations", but the "Byzantine" reading does not meet the requirements for a conflation and may reasonably be viewed as the original. Aleph omitted and B and D have separate revisions—the idea of "a deserted place belonging to a town" apparently gave them difficulty. *Theta* appears to have conflated elements from all four of the other readings!)

97. Luke 9:34 εκεινους εισελθειν *Byz P<sup>45</sup> A D P R W X Γ Δ Θ Λ Π Ψ 1 13 pl sa*  
εισελθειν *P<sup>75</sup> S*  
εισελθειν αυτους *ℵ B L pc bo*  
αυτους εισελθειν *C pc*  
εισελθειν εκεινους *pc*

(Conflation or confusion? Codex C would appear to have a conflation. Note that the "Byzantine" reading now has very early attestation.)

98. Luke 11:54 ενδρευοντες αζητουντες θηρευσαι τι εκ του στοματος αυτου ινα κατηγορησωσιν αυτου *Byz A C W (Ψ 1) 13 33 pm (lat sy<sup>p,h</sup>)*  
ενδρευοντες α. θηρευσαι τι εκ του στοματος αυτου *P<sup>45v,75</sup> (ℵ) B L pc cop*  
ζητουντες αφορμην τινα λαβειν αυτου ινα ευρωσιν κατηγορησαι αυτου *D (Θ sy<sup>s,c</sup>)*

(This is another of Hort's eight "Syrian conflation", but clearly it does not meet the requirements for a conflation. The solution of this problem is linked to textual choices in verse 53, but I submit that the "Byzantine" reading here is a serious candidate for the original. The loss of the last clause in the Alexandrian MSS could be an easy instance of homoioteleuton, or they could have felt it was redundant, which could also have been the motivation for deleting the second participle. Codex D simply rewrote the material.)

99. Luke 12:18 παντα τα γεννηματα μου και τα αγαθα μου *Byz A Q W Θ Ψ pm aur f vg sy<sup>p,h</sup>*  
παντα τα γεννηματα μου *ℵ D it (sy<sup>s,c</sup>)*  
παντα τον σιτον και τα αγαθα μου *P<sup>75c</sup> B L 070 1 (13) pc cop*

(This is the last of Hort's eight "Syrian conflations". The "Western" reading could easily have arisen through homoioteleuton [2 x AMOY] and the "Alexandrian" reading be the result of a stylistic retouching.)

100. Luke 24:47 αρξαμενον *Byz P<sup>75</sup> A F H K M U V W Γ Δ Λ Π 063 1 13 pm syr*  
αρξαμενοι *ℵ B C L N X 33 pc cop*  
αρξαμενος *S Θ Ψ pc*  
αρξαμενων *D pc lat*

(Conflation or confusion? Note that Hort's "late Syrian" reading now has the earliest attestation.)

101. John 2:15 ανετρεψεν *P<sup>66</sup> B W X Θ 0162 pc*  
κατεστρεψεν *P<sup>59</sup> ℵ 13 pc*  
ανεστρεψεν *Byz P<sup>75</sup> A G K L P Γ Δ Λ Π Ψ 1 pl*

(Conflation or confusion? Note that Hort's "late Syrian" reading now has very early attestation.)



102. John 11:21 ο αδελφος μου ουκ αν ετεθνηκει      *Byz* E G U Γ Δ Θ Λ Π Ω 13 *pm*  
ο αδελφος μου ουκ αν απεθανεν      P<sup>45,66</sup> K 0250  
ουκ αν απεθανεν ο αδελφος μου      P<sup>75</sup> Ξ B C L W *pc*  
ουκ αν απεθανεν μου ο αδελφος      (Ψ) 1 33 565 *pc*  
ουκ αν ο αδελφος μου απεθανεν      (A) D *pc*

(Conflation or confusion? Note that Hort's "late Syrian" word order now has very early attestation. Might P<sup>45,66</sup> have a conflation, albeit early?)

103. John 11:32 απεθανεν μου ο αδελφος      *Byz* P<sup>45</sup> A E G K S X Γ Λ Π 1 *pl*  
μου απεθανεν ο αδελφος      P<sup>66,75</sup> Ξ B C L W Δ Θ 33 *pc*  
απεθανεν ο αδελφος μου      66 *lat*  
μου ο αδελφος απεθανεν      D

(Conflation or confusion? Note that Hort's "late Syrian" reading now has very early attestation.)

104. John 13:26 και εμβαψας      *Byz* P<sup>66c</sup> A K W Γ Δ Θ Λ Π Ψ 1 13 *pl lat syr cop*  
βαψας ουν      Ξ B C L X 33 *pc*  
και βαψας      D *pc*

(Is this a "Western" conflation? Note that the "Byzantine" reading now has the earliest attestation.)

105. John 14:5  
δυναμεθα την οδον ειδεναι      *Byz* P<sup>66</sup> A L N Q W X Γ Δ Θ Λ Π Ψ 1 13 *pl lat syr cop*  
την οδον ειδεναι δυναμεθα      Ξ K  
την οδον οιδαμεν      D  
οιδαμεν την οδον      B C a b e

(Is B based on D, or did D conflate B and the rest? Note that the "Byzantine" reading now has the earliest attestation. The editors of the UBS text evidently agree that it is original.)

106. 1 Pet. 5:8 τινα καταπιη      *Byz* P<sup>72</sup> A 056 (33) *pm lat syr*  
τινα καταπιν      Ξ  
τινα καταπιει      0142 *pc*  
καταπιειν      B Ψ 0206 1175 *pc*  
τινα καταπιειν      Ξ<sup>c</sup> K L P 049 *al/bo*

(Line 5 could be a conflation of 1 and 4. Line 2 is probably a misspelling of 1—H became N—while 3 is also a misspelling of 1. Note that the "Byzantine" reading now has the earliest attestation.)

Although many of the examples in Group 2 scarcely offer the required phenomena for possible conflation, others do, to a greater or lesser extent. I will make some observations and draw some conclusions while recognizing that the evidence is not as clear as in the first section.

Ignoring probabilities for the moment, I will tabulate the "possible" conflations (many of which are entirely improbable).

	Total	Examples
Western text-type	15	50, 56, 60, 62, 63, 64, 69, 70, 86, 88, 89, 91, 93, 104, 105
Alexandrian text-type	8	50, 52, 60, 61, 71, 83, 84, 110
Codex B	3	66, 89, 90
Codex Aleph	4	52, 56, 58, 81
Byzantine text-type	24	
with early attestation	9	69, 72, 73, 74, 78, 80, 82, 87, 101
lacking phenomena	5	54, 93, 94, 96, 98
really “possible”	10	51, 53, 57, 65, 68, 85, 88?, 92?, 95, 99

None of the Western “conflations” has early papyrus support, and I believe there is general agreement among scholars that none of the “Western” instances, except 88, is original, whether or not the mechanism that gave rise to the readings was actually conflation in every case.

None of the Alexandrian “conflations” (including those of B and Aleph) has early papyrus support. I believe that all of B’s instances and most of Aleph’s are universally rejected (the UBS text follows Aleph in 58). Modern editors continue to adopt the “Alexandrian” instances.

Nine of the Byzantine “conflations” have early papyrus attestation (and in only five of the instances do any of the other readings have such support), so they may not be used to argue for a late “Byzantine” text-type. Of the fifteen cases without early papyrus attestation, in only four of them do any others have such support (85, 96, 98, 99). I submit that in at least five instances (I think 88 and 92 should also be included) the “Byzantine” reading does not exhibit the required phenomena for a conflation. Most of these are among Hort’s eight “Syrian conflations”, so I felt obliged to include them lest I be accused of suppressing unfavorable evidence. With reference to the remaining eight instances that may fairly be described as possible conflations, I believe they are most reasonably explained as being the original readings (see the comments under each one). It is significant that in thirty-two of the examples given in Group 2 the “Byzantine” text is being possibly conflated by other witnesses and in twenty-five examples (not necessarily the same ones) the “Byzantine” reading has early papyrus support—in three further cases some significant feature of the “Byzantine” reading has early papyrus support, and in yet another case support from the Diatessaron (2nd cent.). Of the possible “Byzantine conflations” there is general agreement that 51, 80 and 87 are the original reading.

## Conclusion

The evidence presented in this appendix justifies the following statements:

- 1) “Western” witnesses have clear, undoubted conflations;
- 2) “Alexandrian” witnesses have clear, undoubted conflations;
- 3) many putative conflations build upon “Byzantine” readings;
- 4) numerous readings that were once thought to be late “Syrian conflations” now have overt early attestation;
- 5) it follows that Hort’s statement and use of “conflation” are erroneous.

It has been customary to refer to the “Byzantine” text as “the later, conflated text,”<sup>4</sup> as if “conflation” were a pervading characteristic of this text. The evidence presented above scarcely supports such a

<sup>4</sup>Metzger, *The Text*, p. 136. To my astonishment, D.A. Carson appears to still be of this opinion so recently as 1979. In his critique of the first edition of this book (*The King James Version Debate*, Grand Rapids: Baker, “Appendix”) he declares that “textual scholars hold that

characterization since in fully sixty percent of the examples the “Byzantine” text is being built upon and not vice versa. Reference has already been made to Hutton’s *Atlas* (on p. 31) which provides evidence that there are over eight hundred places where the producers of the “Byzantine” text could have conflated “Western” and “Alexandrian” readings (following Hort’s hypothesis) but did not.

I trust that the reader will not judge me to be unreasonable if I express the hope that all concerned will loyally concede that the specter of “Syrian conflation” has been laid to rest. Henceforth no one may reasonably or responsibly characterize the “Byzantine” text-type as being “conflate” nor argue therefrom that it must be late.<sup>5</sup>

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a primary feature of the Byzantine text-type is its tendency to conflate readings” (p. 110) and speaks of “the Byzantine tradition in its mature conflated form” (p. 112). The reader is now in some position to form his own opinion on this subject.

<sup>5</sup>I am aware that the mechanism at work, especially in the Gospels, was probably harmonization in many/most cases rather than conflation. Since both mechanisms produce secondary readings the basic thrust of this appendix is not altered by a choice between them. I am also aware that I cannot **prove** conflation or harmonization in any instance, but then, of course, neither could Hort, and neither can anyone else.

## APPENDIX E

### Text Determination in the “Plucking Grain on the Sabbath” Pericope

(Matthew 12:1-8; Mark 2:23-28; Luke 6:1-5)

by Dr. Jakob Van Bruggen (The Netherlands)<sup>1</sup>

#### **Introduction**

Between the Authorized Version and the modern English translations there are various differences which go back to differences in the basic text followed. Sometimes Nestle<sup>25</sup> (N25) places the siglum p) beside the reading now abandoned, to indicate the opinion that these readings have “probably crept in from one of the other Gospels” (Preface N25 p. 80\*). In total N25 mentions six cases of parallelising readings in the Koine-text (K-text) (see Table I). This arouses suspicion about the K-text in general and against the readings at issue in particular. Thus Metzger in his *Textual Commentary* writes at Matthew 12:4: “Although *evfagon* is supported by only  $\aleph$  B and 481, as the non-parallel reading it is more likely to have been altered to *evfagen* than vice-versa.”

**Table I**

List of readings which are qualified as p) in N25 and belong to the K-text			
	N25	K-text	Parallel passage
Mt 12:4	B $\aleph$ 481 <i>evfagon</i>	<i>rell. evfagen</i>	Mk 2:26; Lk 6:4 <i>evfagen</i>
Mt 12:4	B D $\Phi$ it $\dot{o}o`$	$\aleph$ C $\Re$ Q <i>pl ou`j</i>	Mk 2:26 <i>ou`j</i>
Mk 2:26	B D --	$\aleph$ C $\Re$ Q <i>pl pw/j</i>	Mt 12:4 <i>pw/j</i>
Lk 6:2	P <sup>vid</sup> B (D) <i>pc lat --</i>	$\aleph$ C $\Re$ Q <i>pl poien</i>	Mt 12:2 <i>poien</i>
Lk 6:4	B <i>al --</i>	$\aleph$ $\Re$ D Q <i>pm kai</i>	Mk 2:26 <i>kai</i>
Lk 6:5	B $\aleph$ <i>tou sabbÅ o` ui`oj tÅ avnqrÅ</i>	$\Re$ (D) Q <i>pl ò o` ui`oj tÅ avÅ kai tÅ sabÅ</i>	Mk 2:28 <i>o` ui`oj tÅ aaa .aaa a .avÅ kai tÅ sabÅ</i>

Comparison with the Critical Apparatus in Aland’s *Synopsis Quatuor Evangeliorum* (1964) shows that the suspicion about the abandoned readings does not always remain. Twice the Synopsis allows the qualifying designation p) to drop out and in one of these cases it also abandons the hesitation about the correctness of the text originally followed (in Mark 2:26 the square brackets around *pw/j* disappear; in UBS<sup>3</sup> they do not reappear). On the other hand a variant not mentioned in N25 is included as a p)-variant to the discredit of the K-text (see Table II).

**Table II**

Differences between Aland (Synopsis) and N25 regarding the so-called p)-variants in the K-text	
Mt 12:4	the qualification p) abandoned with the reading <i>evfagen</i>
Mk 2:26	square brackets around <i>pw/j</i> disappear; designation p) in critical apparatus abandoned
Mk 2:26	variant <i>toij i` ereusin</i> (C $\Re$ A D W Q <i>pm lat</i> ) is included as p) reading

#### **Number and distribution of the p)-variants**

<sup>1</sup> Jakob van Bruggen is professor of New Testament exegesis at the Reformed Theological College in Kampen, The Netherlands. The material in this appendix comes from an unpublished lecture first given in Dutch. With the exception of a few stylistic changes, the translation into English sent to me by Dr. van Bruggen is reproduced verbatim and with his permission.

Critics of the first edition have pointed out, appropriately, that my discussion of “harmonization” was weak. I include this appendix as a partial response to such criticisms. I believe it justifies two conclusions: Many apparent harmonizations may reasonably be interpreted in other ways, and the “Alexandrian” and “Western” texts may be said to be just as guilty of harmonizing as the “Byzantine” text.

A study of the readings which can be qualified as p) leads to the result that all types of text include such readings.

In the list of possible p)-variants (see Table III) the two readings which were qualified in N25 (but not in the Synopsis) as p)-readings are included. Also included was the variant +deuteroprwtw from Luke 6:1 (omission of this word is very similar to the omission of the words epi Abiaqar arcierewj in Mark 2:26; this last case is qualified as p) by Aland in the Synopsis). Also, the insertion of tw n in Luke 6:1 was considered as a p)-variant. So, four other variants were included, besides the nineteen mentioned by Aland in the Synopsis as p)-variants, according to the criteria which Aland evidently had used in the other nineteen cases. Inclusion of these four readings (numbers 2, 11, 12, and 13) is more to the detriment of the K-text than to its advantage; B has only one while the K-text has three of the four variants indicated as p). The distribution of these possible p)-variants and their number per manuscript or group can be found on the left half of Table III.

**Table III**

Survey of possible p)-variants													
Present in:					# Text	Variant	Evaluation	Present in:					
Ⲛ	B	W	D	Ⲓ				Ⲛ	B	W	D	Ⲓ	
		x			1. Mt 12:1	+ en @toij sabb)#	// Mk			x			
		x	x	x	2. Mt 12:4	efagen ⲓpro efagonÄ	// Mk-Lk			x	x	x	
x		x		x	3. Mt 12:4	ou j ⲓpro o )	// Mk-Lk	x		x			x
	x		x		4. Mk 2:23	diaporeuesqai \$pro paraÄ%	// Lk		x		x		
			x		5. Mk 2:24	! oi maqhtai sou	// Mt				x		
					6. Mk 2:24	! poien	// Mt-(Lk)						
		x			7. Mk 2:25	oude touto \$pro oudepote%	// Lk			x			
					8. Mk 2:25	om. creian escen kai	// Mt-Lk						
		x	x		9. Mk 2:26	om. epi Abiaqar arcierewj	// Mt-Lk			x	x		
		x	x	x	10. Mk 2:26	toij iereusin \$pro touj iereij%	// Mt; not // Lk			--	--	--	
x		x		x	11. Mk 2:26	! pw`j	BD: om. pw`j (Mk), w`j (Lk) all: Matthew: pw`j ; Luke: w`j	--		--			--
x	x	x			12. Lk 6:1	om. deuteroprwtw	// Mt-Mk	x	x	x			
			x	x	13. Lk 6:1	! tw n @sporimon#	// Mt-Mk				x	x	
			x		14. Lk 6:1	hrxanto tillein \$pro etillon%	In D // Mk				?		
			x		15. Lk 6:2	! autw	// Mt-Mk				x		
			x		16. Lk 6:2	ideÄ ti poiou sin oi maqÄ sou toij sabbÄ o ouk ex.	in D // Mk				?		
(x)		(x)		x	17. Lk 6:2	! poien enÄ \$poiein%	// Mt; not // Mk	--		--			--
			x		18. Lk 6:3	oudepote \$pro oude touto%	// Mk				x		
x	x	x	x		19. Lk 6:3	o`te \$pro oo`pote%	// Mt-Mk	x	x	x	x		
x	x	x	x		20. Lk 6:3	omÄ ontej	// Mt-Mk	x	x	x	x		
X			x	x	21. Lk 6:4	! kai @toij metV autou#	partim // Mk	?			?	?	
X		x	x		22. Lk 6:4	omÄ labonÄ elaben kai	// Mt-Mk	x		x	x		
				x	23. Lk 6:5	oti @kurioj estin# o` ui`oj tÄ anqropou kai tou sabbÄ \$pro @kurioj estin# tou sabbatou o` ui`oj tÄ anqrÄ%	// Mk; not // Mt						--
<b>Total:</b>								<b>Total:</b>					
7 4 11 14 8								5 4 9 10 3					
(8) (12)								(6?) (13?) (4?)					

The complete list of p)-variants must now be closely examined. If of two readings the one is similar to parallel-gospel I and the other to parallel-gospel II, none of the two readings can be qualified as p) (see numbers 10, 17, 23). In the case of number 11, there are special reasons for not using the description p): if BD have the original text, one must admit that at some moment, simultaneously, p w j was inserted in Mark and w` j in Luke. In addition, there are still a number of unclear cases: in numbers 14, 16, and 21, the possible assimilation to another gospel is only present in one manuscript, or it is accompanied by simultaneous dissimilation. When we look at the variants which remain as serious candidates for the title p), it appears that these harmonising readings occur *the least* in the K-text (see the right half of Table III).

### **Internal and external criticism**

The omission of all the p)-variants would lead to a text which sometimes follows B (numbers 2, 3, and 13) and sometimes the K-text (numbers 4, 12, 19, and 20). The remaining five points of difference between B and the K-text (numbers 10, 11, 17, 21, and 22) could be solved on external grounds. In four of these five cases the Koine-reading is not a specific K-lectio but also occurs in D (10, 21), W (10, 11, 17), or Sinaiticus (11, 17, 21). In these four cases the K-lectio may be chosen. This means that B is followed three times against eight times for the K-text. There is reason to ask whether the K-text should not be followed in number 23 as well: it also is not a specific K-lectio.

In the Synopsis of Aland, B is followed eight times (2, 3, 10, 12, 13, 17, 21, 23) and the K-text four times (4, 11, 19, 20) in the twelve differences between B and K-text now being discussed. In the text of UBS<sup>3</sup> the K-text has also been abandoned in number 19 and has been placed between square brackets in number 20. Thus B has been subscribed to nine or ten times and the K-text three or two times. This means that the number of readings in the text of UBS<sup>3</sup> qualified as p) increases by two or three (12, 19 and (20)). This increase in the amount of assimilating readings in the text of the United Bible Societies is the result of the abandonment of K-readings under the influence of P<sup>4</sup> (this papyrus evidently turned the scale in favor of an altered point of view in Luke 6:3; also in Luke 6:4 the first word w` j is placed between square brackets under the influence of P<sup>4</sup>). Is assimilation objectionable when it is found in the K-text and not when it occurs in a papyrus?

Or is a reading no longer called assimilating when it appears in a papyrus? This suggestion finds support in the strange fact that the designation p) for the reading ef agen in Matthew 6:4 is abandoned in Aland's Synopsis as soon as P<sup>70</sup> is also mentioned as a witness to this reading. Such a proceeding raises questions concerning the applicability of the rule of internal criticism that a nonparallel reading deserves preference.

### **Are p)-variants really p)-variants?**

From the presupposition that in the transmission of the text there was a process of assimilation and harmonization, scholars began to distinguish between so-called parallel and nonparallel readings. The question arises whether a framework has not been pressed upon the data (see last paragraph).

Example (number 2):

The reading ef agen seems to be a complete formal assimilation to Mark and Luke. As an assimilating reading it must, however, be of a later date. How can it then appear in P<sup>70</sup>? Aland, Synopsis, now abandons the sign p) with this reading. But surely the nature of readings does not change when they occur in papyri? Aland's omission of the designation p) makes us ask for a different kind of approach to these and other readings. Now the apparent nonparallel reading ef agon can be described as internal (inside Matthew) assimilating. It gives a better association with the words directly preceding it (epein asen kai oi met ,aut on), and the words directly following it (aut w` )) ) oude to ij met ,aut on). However, while the reading ef agen does correspond formally with Mark and Luke, it does not do so materially, for by means of this reading the emphasis lies more on *David's deed* ("they that were with him" now stands in the shadow). It appears to be characteristic and specific for Matthew that

he places the emphasis in this pericope more on (David and) Christ personally (cf. Matt. 12:5-7, but not in Mark and Luke). The notion of "giving to those who were with David" is absent in Matthew. This implies that the reading of  $\epsilon\gamma\omega\nu$  can be described as an alteration of the text with the purpose of improving it philologically, but with the effect that it becomes more vague in content and more assimilated to Mark and Luke, where the eating is explicitly related to Jesus *and* the disciples, to David *and* the people in his company.

A new look at the readings which have by now been selected as real p)-variants leads to the conclusion that these variants can also be explained without the p)-model.

1. A number of readings can be interpreted as the making explicit of a preposition (number 1), subject (number 5), article (number 13) or prepositional object (number 15); in all these cases it is clear from the context that these are meant. Regarded as p)-variants these readings would be secondary, but as explicit-making readings they can be authentic: (1) because of the more "Semitic" character; (2) because of the circumstance that with the dictation of a text the omission of apparent dispensable details is more acceptable than insertion by the writers.

2. In one case the thought of mutual influence between the Gospels seems acceptable because this influence is reciprocal (numbers 7 and 18 vice versa).

3. In a reasonably large number of cases variants can be regarded as the result of philological improvement of the text:

Numbers 2 (see discussion above).

3 (Mark-Luke:  $\omicron\upsilon\grave{\eta}\ \omicron\upsilon\upsilon\kappa\ \epsilon\upsilon\chi\epsilon\sigma\tau\iota\eta\ \mu\tau\ \omicron\grave{\eta}\ \omicron\upsilon\upsilon\kappa\ \epsilon\upsilon\chi\omicron\nu\ \text{.....} \eta\upsilon\eta$ ).

4 (diaporesqai in connection with dia twn sporimwn).

9 (omission of a difficult, apparently incorrect, and also dispensable element).

12 (like 9).

19 ( $\omicron\grave{\eta}\ \tau\epsilon$  in this case better Greek than  $\omicron\grave{\eta}\ \rho\omicron\tau\epsilon$ ).

20 (omission of  $\omicron\upsilon\nu\tau\epsilon\eta$  as the removal of needless redundancy).

22 (the sentence does not run well; this is solved by the omission of  $\lambda\alpha\beta\ \omega\ \nu$  (Sinaiticus D W), or by the omission of  $\omega\grave{\eta}\ \eta$  (P<sup>4</sup>BD), cf. number 11).

A review of the distribution of the readings placed in the right column of Table III as real p)-variants according to the evaluation now offered in three groups leads to the conclusion that the ph)-variants (philological improvement variants) are totally absent in the K-text (see Table IV).

**Table IV**

Distribution of the possible p)-variants according to another evaluation-system					
⌘	B	W	D	⌘	
-	-	1	3	1	more explicit
-	-	1	1	-	assimilating
5	6	5(6)	6	-	philological improvement

The application of the p)-criterion led to confusion; the application of the ph)-model leads to the coincidence of internal criteria and external data. The absence of ph)-variants in the K-text gives us occasion to grant this text our trust.

### ***Text-determination***

With the K-text as a basis it is possible to explain the divergent variants (more explicit, assimilating, philological improvement), but it is not possible to explain from the B-text or the D-text how a K-text originated (especially where the readings in the K-text are definitely *not* philological improvements).

If N25 gives the impression that a non-K-text can be taken as a basis, this is due to the fact that N25 neither follows B completely nor D; if all the variants from B rejected as p) had been maintained in the text, no model could be developed for the explanation of the variant-groups.

Example: If in Luke 6:4  $w`j'pwj$  was originally absent and  $labwn$  was to be found there, then it would be inexplicable why  $pwj$  was added (via assimilation to Mark?) without a simultaneous omission of the consequently difficult  $labwn$  also by assimilation to Mark. If the K-text is followed then the embellishment of the somewhat paratactically jerky  $elaben kai to labwn$  calls for measures regarding  $w`j$ .

Another example is the choice of the reading  $o`te$  by UBS<sup>3</sup> in Luke 6:3. How can one explain that this reading would ever at a later time be substituted by the reading  $o`pote$  which deviates from Matthew and Mark and is poorer Greek?

### **Conclusion**

For the pericopes under consideration the K-text is evidently the most recommendable. This conclusion may not automatically be transferred to other pericopes. Yet, in applying the model presently in use for the selection and evaluation of readings elsewhere, it does induce one to test it critically against the totality of a literary text-unit, as well as the variation of readings occurring within. Atomistic treatment of variants is not the same as text-determination.



# APPENDIX G

## A DIFFERENT STEMMA FOR JOHN 7:53-8:11

Wilbur N. Pickering, ThM PhD

The criticisms of *The Greek New Testament according to the Majority Text* that I have seen have almost invariably fastened upon the genealogical<sup>1</sup> reconstruction of John 7:53-8:11 (among other things). They note that sixteen times, out of 27 (where the MSS are significantly divided, as reported in the apparatus), the preferred reading is a minority one. Eleven of those times the minority is less than 30%! They discover that the discussion of the variant readings is redolent of Hort—minority readings are preferred against majority readings on the basis of internal evidence. Such a procedure sets aside the argument from statistical probability (which is usually associated with Majority Text Theory). They point out the discrepancy between theory and practice and wonder what went wrong. Can it be that when confronted with reasonably complete collations of the extant MSS the theory just won't work? In any event, why say the text is "according to the Majority" when it isn't?

In the Apocalypse we are presented with a situation where a clear majority is frequently unavailable. Where a majority reading does not exist we are obliged to use a minority reading, and defend our choice as best we may.<sup>2</sup> John 7:53-8:11 is quite different—out of 33 significant variant sets only one lacks a clear majority reading. If the stemma offered on page xxv of the "Introduction" (H-F *Majority Text*) were incontrovertible then I suppose we should all loyally accept the consequences, but I find both the stemma and the discussion of the 21 variant sets to be less than convincing. Since the author of the "Introduction" recognizes that the stemmatic reconstruction (of Jn. 7:53-8:11) needs to be "searchingly evaluated", and this "calls for the cooperation of many minds" (p. xxxii), I venture to offer the following alternate reconstruction as my bit of "cooperation".

This article is based on a careful check of von Soden done by W.G. Pierpont (personal communication). My reconstruction of the text will be based exclusively on Soden's seven MS groups. Their relative size is as follows:

7 = a. 260 MSS, which = 29% of the total  
6 = 246 MSS (216 "relatively pure" + 30 others), which = 27%  
5 = a. 280 MSS, which = 31%  
4 = 29  
    } = 45  
3 = 16        }  
    } = 118 MSS, which = 13%  
2 = 50        }  
    } = 73  
1 = 23

With this background I now present the 33 variant sets upon which this study is based. The total is sometimes 99% or 101% because I used whole numbers. Groups **M**<sup>4,3,2,1</sup> are frequently internally divided, and their constituent MSS often go astray with a variety of added variants which are not recorded on the following chart, which is why the groups do not always add up to 100%. I use [ ] for 1-20%, ( ) for 21-95% and the whole # for 95+%.

# | vs. | variants | **M**<sup>7</sup> | **M**<sup>6</sup> | **M**<sup>5</sup> | **M**<sup>4</sup> | **M**<sup>3</sup> | **M**<sup>2</sup> | **M**<sup>1</sup> | %

<sup>1</sup>Robinson-Pierpont continue to misunderstand (p. 494) what Hodges-Farstad and I mean by "stemma". We are not talking about the genealogy or descent of MSS; we are talking about the genealogy or descent of **readings**. Soden's MS groups are based on "profiles" of readings in common, a concept that R-P seem to accept; a concept that seems to me to be obviously valid, and necessary. I think we all would agree that "genealogy" as applied to MSS is unworkable.

<sup>2</sup>But see chapter 7, "Examples and Implications" and "Conclusion" where I argue that demonstrated MS groupings and relationships supersede the mere counting of MSS.

01	7:53	απηλθεν <b>vs</b> απορευθη επορευθησαν	7   6   -   (48%)   [6%]   [4%]   [9%]   57
			-   -   -   (48%)   [12%]   [4%]   (22%)   2
			-   -   5   -   (76%)   (24%)   [4%]   34
			-   -   -   [3%]   [6%]   (66%)   (65%)   5
02	8:1	ησους δε <b>vs</b> και ο Ιησους και Ιησους	7   -   5   -   (76%)   (68%)   (65%)   67
			-   6   -   [3%]   [6%]   (26%)   (22%)   29
			-   -   -   4   [18%]   -   -   3
03	8:2	--- <b>vs</b> βαθως	7   -   5   (86%)   3   (90%)   1   73
			-   6   -   [14%]   -   [10%]   -   28
04	8:2	παρεγενετο αραγινηται <b>vs</b> ηλθεν ο Ιησους ηλθεν	7   -   5   [7%]   3   (90%)   (70%)   67
			-   -   -   -   -   -   (22%)   1
			-   6   -   -   -   [10%]   -   28
			-   -   -   (93%)   -   -   -   3
05	8:2	προς αυτον <b>vs</b> --- (omit 8:2 <sup>bc</sup> )	7   6   -   (45%)   3   2   (83%)   66
			-   -   5   -   -   -   [17%]   32
			-   -   -   (55%)   -   -   -   2
06	8:3	προς αυτον <b>vs</b> ---	7   (53%)   5   [14%]   (69%)   [16%]   (61%)   78
			-   (47%)   -   (86%)   (31%)   (84%)   (39%)   22
07	8:3	επι <b>vs</b> εν	7   6   -   4   3   2   (74%)   68
			-   -   5   -   -   -   (26%)   32
08	8:3	κατελημμενην <b>vs</b> καταληφθεισαν	7   6   -   4   3   2   (61%)   68
			-   -   5   -   -   -   (30%)   32
09	8:3	εν <b>vs</b> εν τω	7   -   5   [7%]   -   (78%)   1   67
			-   6   -   (93%)   3   (22%)   -   33
10	8:4	λεγουσιν <b>vs</b> ειπον	7   -   5   -   -   (78%)   (91%)   67
			-   6   -   4   3   (22%)   [9%]   33
11	8:4	- - - <b>vs</b> πειραζοντες	7   6   -   (93%)   3   2   (35%)   67
			-   -   5   [7%]   -   -   (57%)   33
12	8:4	αυτην <b>vs</b> αυτη η γυνη	7   6   -   -   -   -   -   56
			-   -   5   4   3   2   1   44
13	8:4	ευρομεν <b>vs</b> κατεληφθη ειληπται κατεληπται	7   6   -   -   -   -   [13%]   56
			-   -   5   [17%]   -   [8%]   -   31
			-   -   -   (76%)   3   (80%)   [13%]   9
			-   -   -   [7%]   -   [12%]   (74%)   3
14	8:4	μοιχευομενην <b>vs</b> μοιχευομενη	7   6   -   -   -   -   -   56
			-   -   5   4   3   2   1   44

15	8:5	ημων Μωσης				7	6	-	-	(50%)	2	-	62
		ημιν Μωσης				-	-	-	4	(50%)	-	(20%)	5
		Μωσης				-	-	-	-	-	-	(23%)	
		<b>VS</b>											
		Μωσης ημιν				-	-	5	-	-	-	(57%)	32
-----													
16	8:5	λιθοβολεισθαι				7	-	5	-	-	-	(48%)	61
		<b>VS</b>											
		λιθαζειν				-	6	-	4	3	(88%)	(52%)	38
		λιθαζεσθαι				-	-	-	-	-	[12%]	-	1
-----													
17	8:5	---				7	(39%)	5	-	-	(24%)	(48%)	73
		<b>VS</b>											
		περι αυτης				-	(61%)	-	4	3	(76%)	(52%)	27
-----													
18	8:6	εχωσι				7	6	5	-	(31%)	(26%)	[13%]	90
		<b>VS</b>											
		σχωσι				-	-	-	-	(69%)	(74%)	[13%]	5
		εχουσι				-	-	-	4	-	-	-	3
		ευρωσι				-	-	-	-	-	-	(73%)	2
-----													
19	8:6	κατηγοριαν κατ				7	6	-	4	3	2	(35%)	67
		<b>VS</b>											
		κατηγορειν				-	-	5	-	-	-	(65%)	33
-----													
20	8:6	μη προσποιουμενος				7	(45%)	5	-	-	-	(48%)	73
		<b>VS</b>											
		---				-	(55%)	-	4	3	2	(52%)	27
-----													
21	8:7	ερωτωντες				7	-	5	4	3	(62%)	1	71
		<b>VS</b>											
		επερωτωντες				-	6	-	-	-	(38%)	-	29
-----													
22	8:7	ανακυψας				7	-	5	[10%]	[12%]	(30%)	(61%)	65
		ανεκυψε και				-	-	-	(90%)	(70%)	(66%)	[17%]	8
		<b>VS</b>											
		αναβλεψας				-	6	-	-	[18%]	[4%]	(22%)	27
-----													
23	8:7	προς αυτους				7	[4%]	5	-	-	-	(39%)	61
		<b>VS</b>											
		αυτοις				-	6	-	4	3	2	(61%)	38
-----													
24	8:7	DO	IO	V									
		τ. λ.	ε. -η	β.		7	-	-	-	-	-	-	29
		<b>VS</b>											
		DO	V	IO									
		λ.	β.	ε. -ην		-	6	-	-	[19%]	-	[9%]	27
		<b>VS</b>											
		IO	DO	V									
		ε. -ην	τ. λ.	β.		-	-	5	-	-	-	-	31
		ε. -η	τ. λ.	β.		-	-	-	-	(25%)	-	(48%)	2
		ε. -ην	λ.	β.		-	[4%]	-	[3%]	-	[14%]	-	2
		<b>VS</b>											
		IO	V	DO									
		ε. -ην	β.	λ.		-	-	-	4	-	(42%)	(43%)	7
		ε. -ην	β.	τ. λ.		-	-	-	-	-	[16%]	-	1
		ε. -η	β.	τ. λ.		-	-	-	-	(56%)	(28%)	-	2
-----													
		there is a clear majority for τ. λ. = 65%											
		there is a clear majority for ε. -ην = 68%											
		there is a clear majority for DO in first place = 56%											
		there is a clear majority for V in last place = 64%											
		—so IO must be in the middle. So presumably the majority reading is											
		τ. λ. ε. -ην β. (a patchwork quilt!)											

25	8:9	και υπο της σ. ε. και vs ---	7   (46%)   5   (21%)   -   (24%)   -   74	-   -   -   (79%)   -   -   -   2	-   (54%)   -   -   3   (76%)   1   24
26	8:9	εως των εσχατων vs ---	7   6   -   4   3   (88%)   -   62	-   -   5   -   -   [12%]   1   38	
27	8:9	μονος ο Ιησους μονος vs ο Ιησους μονος ο Ιησους	7   (38%)   5   -   -   (48%)   (78%)   74	-   -   -   -   -   [10%]   (22%)   2	-   (62%)   -   (24%)   -   (28%)   -   19
			-   -   -   (72%)   3   [14%]   -   5		
28	8:10	και μ. θ. π. της γ. vs ειδεν αυτην και vs ---	7   [4%]   5   -   (31%)   [14%]   -   63	-   6   -   4   (69%)   [10%]   -   32	-   -   -   -   -   (76%)   1   6
29	8:10	αυτη vs γυναι vs αυτη γυναι τη γυναικι	7   [3%]   5   -   -   -   (65%)   62	-   6   -   -   (35%)   [16%]   -   28	-   -   -   4   (65%)   (84%)   -   9
			-   -   -   -   -   -   (26%)   1		
30	8:10	εκεινοι οι κ. σου vs οι κ. σου ---	7   (35%)   5   -   -   [4%]   -   69	-   (65%)   -   (66%)   3   (48%)   (74%)   27	-   -   -   (31%)   -   (48%)   (26%)   4
31	8:11	ειπεν δε αυτη ο Ιησους ειπεν δε — ο Ιησους vs ο δε Ιησους ειπεν αυτη ο — Ιησους ειπεν αυτη	7   6   -   -   -   (27%)   (48%)   59	-   -   5   -   -   (63%)   (26%)   35	-   -   -   (62%)   3   [10%]   [13%]   5
			-   -   -   (35%)   -   -   -   1		
32	8:11	κατακρινω vs ---	7   6   -   4   3   2   1   69	-   -   5   -   -   -   -   31	
33	8:11	και απο του νυν vs και ---	7   6   -   [10%]   (82%)   2   (57%)   64	-   -   5   (66%)   [18%]   -   (39%)   34	-   -   -   (24%)   -   -   [4%]   1

I will start with some general comments about the MS groups. Based on the 33 significant variant sets (significant for the reconstruction) we observe the following:

- 1) **7** is never alone, except for the one time when **5**, **6** and **7** go separate ways (#24).
- 2) **7** and **5** are the only groups that never divide (according to von Soden).
- 3) **5** and **6** each stand alone against the rest of the stemma—a condemning circumstance. **6** does so three times (3, 21, 22), plus two more times where it influences group **4** (2, 4). **5** does so four times (5, 7, 8, 32), plus four more times where it influences group **1** (11, 15, 19, 26).
- 4) **5** and **6** (entire) never agree (where one of the three major groups diverges)—they are the extremes (which makes it unlikely that either one stands closest to the Autograph).<sup>3</sup>

<sup>3</sup>Robinson-Pierpont claim that both **5** and **6** read "the sins of each one of them" at the end of 8:8, because Soden seems to indicate this in two places (II:427 and I:514). However, in two or three other places where Soden should mention it, he does not. Both UBS<sup>3</sup>

- 5) **3,4** and **1,2** evidently form groups at the next level up, which are themselves closely related.  
 6) **1** is fairly close to **5** but far from **6**, while **4** is just the opposite.

Going into more detail, I will start with groups **1-4**. It is obvious that they are by no means monolithic, and one has to wonder if Soden's assigning of individual MSS to these groups was altogether felicitous. Ignoring solitary MS deviations from a group:

- 4** divides 14 times (2 being three-way splits),  
**3** divides 10 times (2 three-way),  
**2** divides 21 times (5 three-way and 3 four-way!),  
**1** divides 24 times (7 three-way),

within our 33 variant sets—they each have further divisions.

However, it seems clear that four such groups do exist and it is generally possible to determine the reading of the exemplar. So I next ask how these four groups interrelate:

	<u>unique agreements</u>	<u>total agreements</u>
<b>4,3</b>	5 + 1(not among the 33)	23 + 1
<b>4,2</b>	1	18
<b>4,1</b>	--	10
<b>3,2</b>	1	23
<b>3,1</b>	1	15
<b>2,1</b>	7	20

It is evident that **2,1** form a group and **4,3** form a group; that together they form a larger group is clearly demonstrated by the next chart. The seeming closeness of **3,2** is because they are the steadier members of their sub-groups, **4** and **1** being more erratic (**4** has five singular readings plus assimilating to **6** three times; **1** has two singulars plus assimilating to **5** five times; while **3** and **2** have none). Where **3,2** agree they preserve the reading of the grandparent. The parent of **2,1** I call **j**, and that of **4,3** I call **k**. The parent of **j** and **k** I call **h**.

The following chart makes it easier to see the pattern. The reading of **7** is always "x"; if **6** differs it is always "y"; if **5** differs it is always "z". "w" is used for any variant distinct from the first three. Brackets are used when a variant is clearly derived from another. Braces are used for a second derived variant. Parentheses are used to indicate separate groupings. Backward slant lines are used when a grouping is anomalous. A question mark means that I don't know what is going on. The "=" column gives the reading followed by **h**, and demonstrates that **h** had a mixed text.

	<u>7</u>	<u>6</u>	<u>5</u>	<u>4</u>	<u>3</u>	<u>2</u>	<u>1</u>	<u>h</u>	=
01)	x	x	z	x/[x]	z	[z]	[z]	3[2,1]	z
02)	x	y	x	[y]	x	x	x	3,2,1	x
03)	x	y	x	x	x	x	x	4,3,2,1	x
04)	x	y	x	[y]	x	x	x	3,2,1	x
05)	x	x	z	x/w	x	x	x	/,3,2,1	x
06)	x	x/y	x	y	x	y	x	\4,2\3,1\	?
07)	x	x	z	x	x	x	x	4,3,2,1	x

and N-A<sup>26</sup> would appear to disagree with R-P (although the **text** of both editions is virtually identical, being the work of the same five editors, I understand that the **apparati** were prepared separately). UBS<sup>3</sup> uses *Byz*<sup>dt</sup> to refer to percentages of MSS ranging from 74% to 24% (in the *Pericope*), **M**<sup>5</sup> and **M**<sup>6</sup> each receiving this classification, when alone or with scant company. In 8:8 the gloss does not rate even a *Byz*<sup>dt</sup>, the absence of the gloss rating *Byz*. Since **M**<sup>5</sup> + **M**<sup>6</sup> would represent 58% of the MSS the gloss would have to rate *Byz*<sup>dt</sup>—it follows that UBS<sup>3</sup> does not agree with R-P. Although the use of *pm* and *al* in N-A<sup>26</sup> is not consistent, that the gloss rates only an *a* would seem to indicate that N-A<sup>26</sup> also disagrees with R-P. (Both codices **U** and **IT** have the gloss, which is a curious circumstance in that **U** usually goes with **M**<sup>6</sup> and **IT** with **M**<sup>5</sup>. Perhaps one of Soden's assistants took the two codices as representative of the two groups without really checking out the bulk of the MSS.) [Maurice Robinson has recently (1998) completed a collation of 1,635 MSS for the *Pericope* and *Pierpont* (personal communication) informs me that a small minority of MSS, from groups 5 and 6, have the gloss. Robinson's collation confirms that 5 and 6 (entire) never agree (where one of the three major groups diverges).]

08)	x	x	z	x	x	x	x	4,3,2,1	x
09)	x	y	x	y	y	x	x	(4,3)(2,1)	y/x
10)	x	y	x	y	y	x	x	(4,3)(2,1)	y/x
11)	x	x	z	x	x	x	z	4,3,2	x
12)	x	x	z	z	z	z	z	4,3,2,1	z
13)	x	x	z	[z]	[z]	[z]	{z}	[4,3,2]{1}	z
14)	x	x	z	z	z	z	z	4,3,2,1	z
15)	x	x	z	[x]	x/[x]	x	z	[4]/,2	x
16)	x	y	x	y	y	y	x/y	4,3,2,/	y
17)	x	x/y	x	y	y	y	x/y	4,3,2,/	y
18)	x	x	x	[w]	w	w	{w}	[4]3,2{1}	w
19)	x	x	z	x	x	x	z	4,3,2	x
20)	x	x/y	x	y	y	y	x/y	4,3,2,/	y
21)	x	y	x	x	x	x	x	4,3,2,1	x
22)	x	y	x	[x]	[x]	[x]	x	[4,3,2]1	x
23)	x	y	x	y	y	y	y	4,3,2,1	y
24)	x	y	z	w	w	w	z/w	4,3,2,/	w
25)	x	x/y	x	w	y	y	y	3,2,1	y
26)	x	x	z	x	x	x	z	4,3,2	x
27)	x	x/y	x	w	w	x/?	x	(4,3)(2,1)	w/x
28)	x	y	x	y	y	w	w	(4,3)(2,1)	y/w
29)	x	y	x	w	w	w	x	4,3,2	w
30)	x	x/y	x	y	y	y/w	y	4,3,/,1	y
31)	x	x	z	w	w	z	x/?	4,3	w
32)	x	x	z	x	x	x	x	4,3,2,1	x
33)	x	x	z	z	x	x	x/z	?,3,2,/	x

The crucial question for our stemma is the interrelationship of the four main groups: **7,6,5** and **h**. That **h** follows "x" 13 times, follows "y" 6 times, "z" 4 times and "w" 3 times shows that it has a life of its own and must be treated as a separate group. We observe the following:

**7** and **5** agree 17 times (ignoring #18).

**7** and **6** agree 14 times (ignoring #18), plus six further times where a significant part of **6** goes with **7** and **5** (53%, 46%, 45%, 39%, 38%, 35%).

**7** and **h** agree 14 times.

**6** and **h** agree 11 times.

**5** and **h** agree 9 times.

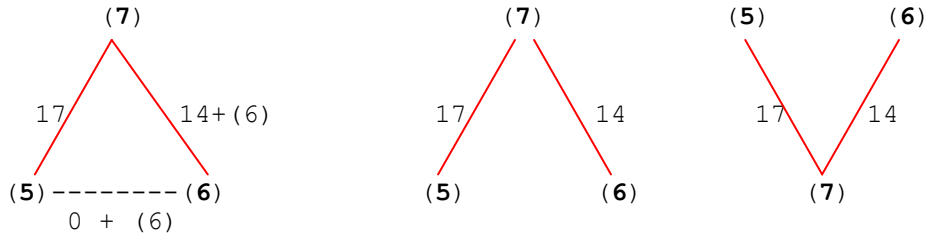
**5** and **6** (entire) never agree (except in #18 where **7,6,5** all agree—I included #18 as an added illustration that **h** has a life of its own).

So, **5** and **6** are the extremes and **7** and **h** are candidates for mixed texts—and yet they do not depend directly upon each other, for **7** and **h** never agree unless **5** or **6** is with them. However, **6** and **h** agree alone two times (+ eight further times when one or the other is divided), while **5** and **h** agree alone four times; further, **h** splits, going with both **5** and **6** four times. Since **7,6,h** agree nine times and **7,5,h** agree five times (+ four further times when **h** is divided), we must posit nodes above **5** and **6** but which are separate from **7**. A careful scrutiny of **h** makes clear that it is a mixed text, drawing from the exemplars of **5** and **6**. Since **6** has seven singular readings and **5** has five it is clear that each has corrupted its exemplar and neither can stand closest to the Autograph. **7** gives no evidence of being mixed; it has only one singular reading, precisely in set 24 where each of the four main groups has a different reading.

I conclude that **7** is independent and at the same rank as the exemplars of **5** and **6**. This accords with a normal transmission where rarely will more than one copy at a given rank create a variant at the same point, and almost never will two make the same mistake independently (except for very easy and common transcriptional ones).

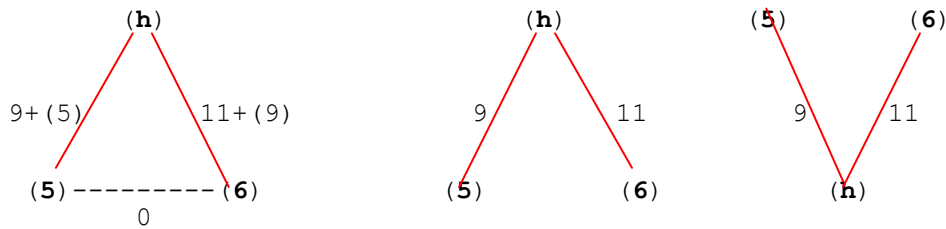
Perhaps a design will help to visualize the options. Consider:

7,6,5 31 sets (-18,24) = either a) or b)



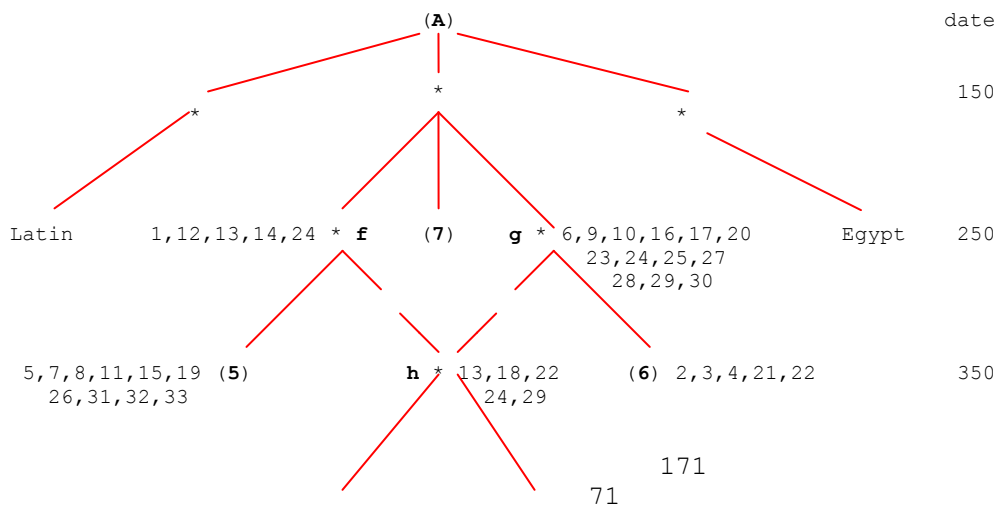
If 7 is viewed as a mixture of 5 and 6 the even selection is strange, plus a total lack of conflations. The six splits in 6 plus the fact that 6 has seven singular readings and 5 has five singular readings, while 7 has only one singular (precisely where all three disagree), point to a) as the best interpretation.

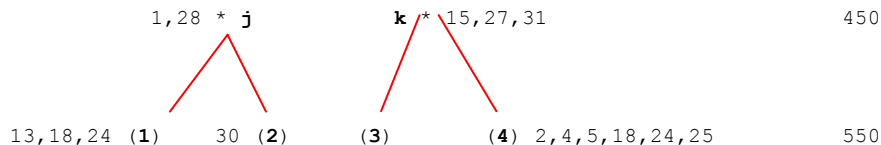
6,5,h 29 sets (-18,24,29,31)=either a) or b)



Since h has obvious secondary readings, including two conflations, plus a deal of mixture, b) seems to be the required interpretation. Evidently h drew more heavily from the ancestor of 6 than from the ancestor of 5.

I now offer a stemma that I believe matches and accounts for the statistical evidence just described. The Autograph is represented by "A". The letters f,g,h,j,k designate reconstructed archetypes. The numbers within nodes are those of Soden's MS groups. The numbers beside the nodes refer to the variant sets in which the node introduces an error (such errors vindicate the posing of a node, especially the reconstructed ones): thus f introduces five errors, 7 introduces none, g introduces thirteen, etc. The solid lines indicate linear descent; the broken lines indicate occasional influence (smaller dashes = lesser influence). The symmetry is arbitrary, as is the chronology. I am not concerned to defend the chronology, but I do happen to think that it is "in the ballpark"—a possible, even reasonable, approximation.





Before returning to the variant sets to discuss transcriptional probabilities, I wish to comment on the stemma. **7**, **f** and **g** are independent of each other, yet only one abandons the original at any point (except for #24 where two of them [or all three] do, in separate directions). This is just what we would expect in a normal transmission, where the archetypes at a given rank are independent (of each other). With the exception of #24 there is always a clear majority, of which **7** is always a part. I consider that **7** has faithfully preserved the Autograph,<sup>4</sup> including in #24 (see the discussion of transcriptional probabilities below). **f** is tolerable, but **g** is a maverick, as is **5**. **h** has a mixed text. (The placing of "Latin" and "Egypt" is deliberate: it appears to me that **6** and **4** reflect "Egyptian" influence, whereas **5** and **1** reflect "Latin" influence.)

And now for the probabilities. The variant sets are taken in linear sequence, beginning in 7:53:

- 1) **7** and **g** preserve the original; **6** influences **4**. **f** creates a variant and influences **h**; **j** changes **h**. Presumably the plural forms were unthinking assimilations to the Subject in verse 52. The reading of **3,5** is possibly a harmonization with the same verb in the next line.
- 2) **7**, **f** and **g** preserve the original. **6** creates a variant and influences **4**. Since the main participant or focus changes from 7:53 to 8:1, the  $\delta\epsilon$  is required (a norm of Koine discourse structure). **6** has an inept stylistic change. **4** follows **6** but drops the article.
- 3) **7**, **f** and **g** preserve the original. **6** creates a variant. We may never know what got into **6** here and in the next example. In Luke 24:1 it really was "very early" (still dark) but here there were already people in the temple. That "very" separates the adverb "again" from its verb is awkward. Anyway, it is scarcely credible that **6** could be right against the whole stemma.
- 4) **7**, **f** and **g** preserve the original. **6** creates a variant and influences **4**. Since there is no ambiguity the repetition of "Jesus" is unnecessary, if not grammatically bad. **4** seems to have replaced the verb of his exemplar with that of **6**.
- 5) **7**, **f** and **g** preserve the original. **5** creates a variant, as does **(4)**. Just why **5** dropped the prepositional phrase here is hard to say, but that is not sufficient reason to prefer it against the whole stemma.
- 6) **7** and **f** preserve the original; **g** creates a variant and is followed by **6** and **h**. **7** influences **(6)**; **5** influences **(1)**; if **(3)** was influenced by **7** or **5** it is the only such case—perhaps it managed a lucky gloss on its own. The omission was probably thought to improve the style. For someone to supply the words would also be easy. External evidence must decide the issue.
- 7) **7**, **f** and **g** preserve the original. **5** creates a variant, attempting a stylistic, or grammatical, "improvement".
- 8) **7**, **f** and **g** preserve the original. **5** creates a variant and influences **(1)**. This variant evidently goes with the preceding one. I see nothing sufficient to overturn the external evidence.

<sup>4</sup>Both H-F and R-P seem to accept Soden's equating of **M**<sup>7</sup> with his **K**<sup>r</sup> group, which is defined as the later Byzantine text, as distinct from the main tradition (**K**<sup>x</sup>). I am inclined to suspect that Soden's judgment derives from his presuppositions far more than from the evidence. I believe that an independent review of the evidence will show that **M**<sup>7</sup> is really ancient. An analysis of the collations in Acts published in *Text und Textwert* and in Luke 1, 10 and 20 done by F. Wisse shows that **f**<sup>18</sup> (alias **K**<sup>r</sup>) is both independent and ancient. **f**<sup>18</sup> enters into a considerable variety of shifting alignments, quite distinct from **K**<sup>x</sup>, and frequently shares readings with ancient MSS, Versions, Fathers (against **K**<sup>x</sup>), but the sharing and aligning are not systematic, are not predictable—therefore **f**<sup>18</sup> must be both independent and ancient. In fact, the collations done by Robinson (see footnote 3) have led him to affirm that all the major groups are ancient and independent—that is, he did the "independent review of the evidence" and arrived at the conclusion I predicted: **M**<sup>7</sup> is ancient (3<sup>rd</sup> if not 2<sup>nd</sup> century).



- 9) **7** and **f** preserve the original; **g** creates a variant and is followed by **6** and **h**; **5** influences **j**. The article may have been thought to be stylistically better. When *μεσος* is part of a prepositional phrase and is modified by a genitive construction no article occurs; without the modifier an article frequently occurs, but not always—as in v. 9 below. External evidence must decide.
- 10) **7** and **f** preserve the original; **g** creates a variant and is followed by **6** and **h**; **5** influences **j**. This verb is parallel to the one that begins v. 3, so the present tense is appropriate. The aorist tense is presumably a superficial assimilation to the participle a few words before.
- 11) **7**, **f** and **g** preserve the original. **5** creates a variant and influences **(1)**. The gloss seems officious. I see nothing sufficient to overturn the external evidence.
- 12) **7** and **g** preserve the original. **f** creates a variant and influences **h**. This set and the next two go together. The change here was deliberate, but it is hard to know what the motivation may have been. Perhaps the demonstrative pronoun was felt to be too scornful.
- 13) **7** and **g** preserve the original. **f** creates a variant and influences **5**; **h** corrupts **f**; **1** conflates **j** and **5**. The confusion within **h** does not inspire confidence. **5** could represent a stylistic "improvement", making the verb (of **f**) more graphic (in which case **5** would create the variant and **h** reproduce **f**).
- 14) **7** and **g** preserve the original. **f** creates a variant and influences **h**. The choice here is controlled by the choice in 13).
- 15) **7**, **f** and **g** preserve the original; **k** corrupts **h**. **5** creates a variant and influences **(1)**. This could have gone either way, though perhaps "our law" is more typical of John. External evidence decides.
- 16) **7** and **f** preserve the original; **g** creates a variant and is followed by **6** and **h**; **5** influences **(1)**. The passive seems more appropriate. I would say that our Lord's subsequent use of the words *λιθον βαλετω* reflects precisely the scribes' *λιθοβολεισθαι*.
- 17) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**; **7** influences **(6)**; **5** influences **(1)**. The absence of the phrase is stylistically and semantically better. An officious but not very perceptive copyist thought he was filling in implied information, but "her" does not match the plural "such".
- 18) **7**, **f** and **g** preserve the original. **h** creates a variant, as do **4** and **1**. **h**'s exemplar may have had a weak cross stroke in the *epsilon* so he read it as a *sigma*.
- 19) **7**, **f** and **g** preserve the original. **5** creates a variant and influences **1**. The infinitive is the more awkward form, sufficient reason for modern critics to prefer it, but the external evidence seems clear.
- 20) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**; **7** influences **(6)**; **5** influences **(1)**. Is it a gloss or an omission? It could have happened either way. Perhaps **g** felt that "not paying attention" made Jesus seem impolite, especially considering the prominence of His interrogators.
- 21) **7**, **f** and **g** preserve the original. **6** creates a variant; if it influences **2** it is the only time—**(2)** may have made the same stylistic change independently, having done something similar in the preceding sentence. The heightened form was probably a stylistic "improvement". In any event, it is scarcely credible that **6** could be right against the whole stemma. Notice that **(2)** also heightened the verb in the preceding clause.
- 22) **7**, **f** and **g** preserve the original; **h** corrupts **g**; **5** influences **1**. **6** creates a variant. It is clear from the text that Jesus squatted and stood up twice, but **6** has Him squatting twice without standing up in between—if a scribe didn't notice that Jesus squatted twice he might have decided to remove the "inconsistency" of His standing up twice. The finite form could be a stylistic retouching.
- 23) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**; **5** influences **(1)**. A toss-up, although the prepositional phrase seems to be more dramatic, delightfully appropriate.
- 24) This is a tough one, the only time that the three major groups go separate ways. That is doubtless why this set has the most variants and confusion. There are three variables: the Case of the pronoun, the article, and constituent (word) order. They all have different alignments:

*αυτην*—**(1),(2),4,5,6** VS *αυτη*—**(1),(2),3,7**

*λιθον*—(1),(2),4,6 VS *τον λιθον*—(1),(2),3,5,7

IO,V,DO—(1),2,(3),4 VS IO,DO,V—(1),5 VS DO,V,IO—6 VS DO,IO,V—7

The line-up is the same for the first two variables, except for **5** which switches. It goes with **6** to give the accusative majority status, but goes with **7** to give a majority to the article. The constituent order seems to be hopelessly confused, but it may be noted that there is a clear majority for the direct object in the first position (and therefore for the relative sequence DO,IO within the clause) and for the verb in the last position (which leaves the middle position for the indirect object). But how can we explain the fluctuating alignments?

I reconstruct the sequence of transmissional history thus:

- 7** = *τον λιθον επ αυτη βαλετω*  
**g** and **6** = — *λιθον βαλετω επ αυτην*  
**f** = *επ αυτη τον λιθον βαλετω*  
**5** = *επ αυτην τον λιθον βαλετω*  
**h,j,k,3** = *επ αυτη βαλετω τον λιθον*  
**4** = *επ αυτην βαλετω — λιθον*  
**1** = *επ αυτη τον λιθον βαλετω*

Being freely interpreted this means:

- a) **7** preserves the original.
- b) **g**, followed by **6**, creates a variant: changing the case, dropping the article and inverting IO and V.
- c) **f** creates a variant, inverting DO and IO.
- d) **5** modifies **f**, changing the case (independently—*επι* takes both).
- e) **h**, followed by **j**, **k** and **3**, creates a variant (building on **f**), moving DO to last place (while preserving case and article).
- f) **4** changes the case and drops the article, presumably influenced by **6** (a frequent occurrence).
- g) Four of the sixteen **M**<sup>3</sup> MSS invert V and DO (while retaining the case and the article); another three assimilate to **6**.
- h) **2** splinters: 28% staying with **j**, 16% changing only the case, 42% changing the case and dropping the article, with 14% going beyond the 42% to invert V and DO besides. (Again one wonders if von Soden correctly assigned some of the MSS.)
- i) As for **1**: 48% retain the case and article of **j** but invert V and DO, presumably influenced by **5** (a frequent occurrence); 43% retain the word order of **j** but change the case and drop the article (presumably influenced by **6**, since two **M**<sup>1</sup> MSS assimilate to **6** completely).

This reconstruction gives a reasonable explanation for the peculiar range of attestation for the Dative case. It also accounts for the range of attestation for the article. In fact, it pretty well accounts for the whole snarl of variation. **M**<sup>7</sup> preserves the original at every point.

Assuming that **7** is right all the other times, it has far and away the best "credibility quotient". If we start with **7**, **6** would be a stylistic "improvement", dropping the article and placing the indirect object last (perhaps he took "first" to be modifying "stone", as a number of prominent English versions have done). Why **f** would put the indirect object first is not clear, unless it be for emphasis. **h** devised a new constituent order, which was retained by all its descendants, except for **(1)** which was influenced by **5**. Considering the fluctuating alignments throughout the variant sets it would appear that copyists frequently had access to more than one exemplar and were not above picking and choosing—it seems reasonable to suppose that they would be most willing to do so when confronted with confusion in the tradition.

The split of **(1),(2),3** for the article and **(1),(2),4** against it is stemmatically anomalous—I feel obliged to conclude that a lot of picking and choosing took place in this case. The same split for the dative versus the accusative is also anomalous, although since *επι* takes both cases this particular change could have happened independently more than once. It is not my custom to prefer "harder" or more awkward readings, but perhaps this is an appropriate place to cash that check. If **7** is stylistically more awkward than some of the other variants then it may have given rise to them.

25) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**; **7** influences (**6**); **4** creates a variant. Surely it would be easier to omit a clause of this sort and size than to invent it, especially if it occupied just one line. **4** is wild; speculation is pointless.

26) **7**, **f** and **g** preserve the original. **5** creates a variant and influences **1**. The omission could be the result of homoioteleuton. In any event, **5** could scarcely be right against the stemma.

27) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**; **k** corrupts **h** and **5** influences **j**. A toss-up, unless it be that **g** dropped "only", followed by **h**, and in subsequent efforts to fix it (**6**), (**4**) and (**2**) replaced it but in the wrong order. In that event **7** influenced (**6**).

28) **7** and **f** preserve the original. **g** creates a variant, followed by **6** and **h**; **j** omits. The solution here involves the following set as well. Faced with a lacuna before *ειπεν γυναι*, if someone felt the need to create a gloss like the longer reading would he also change "woman" to "to her"? It is much more likely that he would supply something like the shorter variant and leave "woman" intact. But on the face of it the longer reading is not the sort that a copyist would concoct out of nowhere. If we start with the longer reading it is easy to see where someone could be troubled by the seeming contradiction—it says that Jesus "saw nobody" except the woman when in fact there was a crowd taking in the proceedings. So he "solves" the problem by replacing it with the innocuous shorter reading; but now he has two pronouns too close together, so he changes the second one to "woman". It may be instructive to combine sets 28) and 29):

<i>και μηδενα θεασαμενος πλην της γυναικος ειπεν αυτη</i>	7,5
<i>και μηδενα θεασαμενος πλην της γυναικος ειπεν γυναι</i>	(3)
<i>ειδεν αυτην και ειπεν γυναι</i>	6
<i>ειδεν αυτην και ειπεν αυτη γυναι</i>	4(3)
<i>ειπεν αυτη γυναι</i>	2
<i>ειπεν αυτη</i>	1

The descendants of **g** are evidently confused, which tends to diminish their credibility. Whether the longer reading was accidentally dropped (if it occupied a full line) or deliberately changed, there is no "internal evidence" sufficient to set aside the clear external evidence.

29) **7** and **f** preserve the original; **g** creates a variant; **h** conflates **f** and **g**; **5** influences **1**. (**3**) drops the pronoun independently. See the discussion above.

30) **7** and **f** preserve the original; **g** creates a variant, followed by **6** and **h**, which (**4**), (**2**) and (**1**) omit. **7** influences (**6**). If the shorter reading were original, who would have thought of adding the demonstrative? The shorter form is very nice the way it is! If the longer reading were original someone might well have felt that the demonstrative was scornful and unbecoming. Or the demonstrative could have been dropped through parablepsis. (**4**), (**2**), (**1**) evidently perpetrated a further omission, whether wittingly or not is impossible to say.

31) **7**, **f** and **g** preserve the original; **k** corrupts **h** (varying the word order but keeping the pronoun). **5** creates a variant, and influences **j**. The external evidence for the word order is overwhelming. **5** dropped the pronoun, either by accident or as being superfluous.

32) **7**, **f** and **g** preserve the original. **5** creates a variant. This omission by **5** has little to recommend it.

33) **7**, **f** and **g** preserve the original. **5** creates a variant and influences (**1**); **4** makes the same mistake. The prepositional phrase could have been thought to be redundant, or lost through parablepsis. Presumably **5** and the archetype of **4** omitted independently; later (**4**) also dropped "and".

I confess that I find discussions of probabilities to be wearisome and frustrating. It is seldom possible to rise above mere speculation. However, perhaps we can glean a few reasonably solid tidbits from the foregoing exercise. I would say that in #2 group **6** is clearly wrong, grammatically bad. In #3 group **6** is unacceptable; it is also inferior in #4. In #11 and #12 the reading of **5** is unacceptable. In #16 group **6** is unacceptable, as also in #22. In #32 group **5** seems inferior. Group **4** is erratic, but is not a serious contender in any case. Group **1** is badly splintered, and is not a contender. I submit that our exercise confirms that neither **6** nor **5** can stand closest to the Autograph.

So, what have we learned with reference to our larger concerns? To my mind, rather than constituting an embarrassment John 7:53-8:11 confirms the validity and workability of Majority Text Theory. The transmission was "normal" in the main. In a normal transmission the copyists of a given generation would almost never commit the same error independently, and seldom would more than one introduce an error at any given point—thus the true reading should have majority attestation at all levels. In spite of a rather high incidence of variation (considering that "Alexandrian" and "Western" witnesses were excluded to begin with—thereby avoiding most of the results of the "abnormal" transmission) the pericope vindicates our expectation. Even though it evidently passed through more turbulence than any other passage in John's Gospel, still the original text is attested by a clear majority of the MSS.

To conclude, if the essential thrust of my discussion is convincing to my peers, I earnestly recommend that in any future reprinting of the H-F *Majority Text* the text of John 7:53-8:11 be corrected to reflect the majority attestation. (The same holds for the R-P *Byzantine/Majority Textform*.)<sup>5</sup> It would also be necessary to revise the corresponding discussion in the "Introduction". To the extent that our theory is viewed as a threat to the Establishment it will certainly be attacked, that we know, but I see no virtue in giving them gratuitous ammunition.

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<sup>5</sup>*The New Testament in the Original Greek according to the Byzantine/Majority Textform* contains an appendix about the *Pericope Adultera*, pp. 494-505. R-P print various forms of the pericope including that of **M<sup>7</sup>** (my choice), the H-F *Majority Text* and their own *Byzantine/Majority Textform*. They state that they generally prefer **M<sup>5</sup>**, consider **M<sup>7</sup>** to be inferior to both **M<sup>5</sup>** and **M<sup>6</sup>**, and yet **M<sup>5</sup> + M<sup>7</sup>** always outweigh **M<sup>6</sup>** (pp. 496-97). When we inspect the R-P reconstruction, however, we discover the following: they reject the reading of **M<sup>5</sup>** nine times and place it in doubt five further times; they place **M<sup>5,7</sup>** in doubt twice (against their stated principle); they follow **M<sup>5</sup>** (alone) without question only once—in fact, their reconstruction is closer to **M<sup>7</sup>** than it is to **M<sup>5</sup>**. It all leaves me a little puzzled: why then do they say that **M<sup>5</sup>** is the best and **M<sup>7</sup>** the worst among the three major groups?

## APPENDIX F

### MARK 16:9-20 AND THE DOCTRINE OF INSPIRATION

*Wilbur N. Pickering, ThM PhD*

For over a hundred years it has been a commonplace of New Testament textual criticism to argue that Mark 16:9-20 was not and could not have been written by Mark (or whoever wrote the rest of the book), that it was a subsequent accretion. However, among those who wish to believe or claim that Mark's Gospel was inspired by the Holy Spirit, that it is God's Word, I am not aware of any who are prepared to believe that it could really have been God's intention to terminate the book with *εφοβουντο γαρ* (v. 8). The most popular hypothesis seems to be that the Autograph was produced as a codex (not a scroll) and that the sheet (or sheets) containing the original ending was torn off and lost before any copies were made.<sup>1</sup> I wish to examine the implications of the claim that vv. 9-20 did not form part of the Autograph and that the original ending has vanished (whatever the explanation offered for such a circumstance).

I am writing from the position of one who believes in the verbal, plenary inspiration of Scripture and am addressing those who believe (or would like to believe) that the Bible is God's Word written—“*all Scripture is God-breathed*” (2 Tim. 3:16).

So, we claim that the Holy Spirit inspired Mark's Gospel. And why would He do something like that? Evidently God wanted subsequent generations to have an official biography of Jesus Christ, a description of His life, death and resurrection whose accuracy was guaranteed and whose content was sufficient for His purpose. (That there are several official biographies written from different perspectives does not obviate the integrity of each one individually.) I find it inconceivable that an official biography, commissioned by God and written subject to His quality control, should omit proofs of the resurrection, should exclude all post-resurrection appearances, should end with the clause “*because they were afraid!*”

But most modern critics assure us that such is the case, that the genuine text ends at v. 8. So where was God all this time? If the critics' assessment is correct we seem to be between a rock and a hard place. Mark's Gospel as it stands is mutilated (if it ends at v.8), the original ending having disappeared without a trace. But in that event what about God's purpose in commissioning this biography? Are we to say that God was unable to protect the text of Mark or that He just could not be bothered? Either option would be fatal to the claim that Mark's Gospel is “*God-breathed*”.

If God tried but was powerless to prevent the mutilation of Mark in this way, how can we be sure that the book has not been mutilated in other ways and places, or even systematically? For that matter, how can we be sure that other New Testament books have not been mutilated too, or maybe even all of them? Anyway, the degree of mutilation would no longer be an issue because if God was powerless to protect His Word then He would not really be God and it would not make all that much difference what He says. The Bible would lose its authority and consequently its importance.

What about the other option—that God could have protected Mark but chose not to? Of what value would quality control be if it extended only to the writing? If God permitted the original ending of Mark to be lost before any copies were made then the biography was “published” in a seriously incomplete form, and it becomes decidedly awkward to speak of its “verbal, plenary” inspiration. If God would permit a mutilation of such magnitude, then what assurance do we have that He would not permit any number of further mutilations? Again, the problem extends to the other New Testament books. Quality control would be gone out the window and we would be left “whistling in the dark.” If God is not going to protect His text will not the purpose of inspiration be frustrated?

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<sup>1</sup>See, for example, B.M. Metzger, *A Textual Commentary on the Greek New Testament* (New York: United Bible Societies, 1971), p. 126, fn. 7.

## BUT, WHAT ABOUT ALL THE VARIANTS?

It is a plain fact that the extant manuscripts contain a great many copying mistakes and even deliberate alterations. Since we cannot deny that God permitted this to happen, it remains to ask why and with what implications. First, the why.

Why would God permit mistakes and alterations in the copying process? I have no direct revelation to offer on the subject but I suppose the answer begins with God's purpose in creating the human race. It appears that He desired a type of being that could respond to Him in worship and love, a being that could choose (John 4:23-24). In Hebrews 11:6 we are taught that God demands faith and rewards those who **diligently** seek Him. It would seem that His purpose in creating man entails an element of test. The evidence may not be overwhelming, crushing, inescapable or there would be no adequate "test". Thus, God permitted textual variants to test our faith and determination, to test our attitude, to test our willingness to humbly and patiently look for answers (Prov. 25:2 and Rev. 5:10).

Another aspect of the creation of beings with volition is that both God and man must live with the consequences of the exercise of that volition. If He exerts complete control, we become robots and the whole point of the experiment is lost. Alas, most of man's volition is expressed in rebellion against our Creator. A fair share of that rebellion has been directed against His Word—usually by rejecting it, but sometimes by trying to alter it.

Besides all that, our abilities and capacity to understand are limited. As it says in 2 Corinthians 4:7, we are mere "*earthen vessels*", clay pots. Even if the Autographs had been engraved on gold tablets and miraculously preserved intact to this hour, who among us could offer a "perfect" interpretation of that Text? (Anyone working from a translation is dealing with some imperfection before he even starts because no translation can be perfect—the nature of language does not permit it.) Since our understanding is condemned to be imperfect in any case, is it really necessary to have a perfect Text? If not, is there some point at which the amount of imperfection ceases to be "tolerable"? Which brings us to the implications. I will begin with some analogies.

Our everyday lives furnish several analogies that illumine this question. All our lives we use measuring devices—rulers, yardsticks, tapes—that vary slightly from each other. We buy many things according to measure without questioning the accuracy of the instrument, even though a precise comparison would reveal discrepancies between instruments. Why? Because the discrepancies are not big enough to concern us and because we know there is an absolute standard to refer to should the need arise. At the Bureau of Standards in Washington, D.C., in a hermetically sealed case, is the absolute, unvarying standard yardstick. How many Americans have ever seen that standard? Very few, comparatively. Yet we are born, live and die without seeing the standard and without feeling any inconvenience. We assume that our rulers are close enough for ordinary practical purposes, as indeed they are, and live happily with them. We know that we can go to Washington if a question arises that warrants the expense.

If someone asks a group of people for the time of day he may well get up to ten different answers, scattered along a ten-minute continuum. We daily live with one or two-minute discrepancies among the several time pieces we may consult and think nothing of it. Two different radio stations in a city often differ from each other by a minute or two, and so on. The system works well enough because there is a recognized standard in Greenwich, England. I have never been there and I suppose few Americans have, but we get along handily just the same. But if there were no standard we would soon be in trouble.

When a legislature draws up a law great care is taken with the precise wording, because once it is published it is law—it becomes a standard, binding upon the people under its jurisdiction. Great care is taken with the standard, but law enforcement officers are not expected to memorize it. All they need is a reasonably accurate understanding of the intent and provisions of the law. When an officer arrests an offender and cites the law he will probably only give the gist of it. No court will countenance a plea by the defendant that the arresting officer did not cite the law verbatim. (Similarly, I doubt that God will countenance an unbeliever's plea that he did not have access to the Law verbatim—it is enough to have

the gist.) However, during a trial emphasis is often given to the precise “letter” of the law and the whole disposition of the case may depend on the interpretation given to that “letter”.

Alcohol (ethanol) may be found on the shelf at any drugstore, but seldom exceeding 92%; perhaps the pharmacist has a private supply of 96% for special purposes. For ordinary household use 92% is more than adequate—in a pinch a stout 60% rum will burn and may be used to disinfect. It may be that certain scientific experiments require 100% alcohol but it will be hard to come by and quite expensive. As with all manufactured goods, the higher the degree of precision or “perfection” the more difficult and costly it is to attain. Different purposes require different degrees of precision (in any area), but for most people and most purposes most of the time the degree of precision does not have to be very high. In fact, in the majority of cases a superlative degree of precision would be wasted—the context simply does not allow for its full utilization or appreciation.

So, why has God allowed errors to get into the Text, or why does He permit faulty interpretation? In the first place the whole point of having a human race apparently involves giving us the ability and freedom to sin and taking the consequences (both individually and corporately—the larger the group is that participates in a sin, the more serious and far-reaching are the consequences). But in the second place normal and daily use does not require a superlative degree of precision—in any event we have more of God’s Truth than we can possibly appropriate. However, it is the availability of a recognized standard that enables us to tolerate minor imperfections, in a given area. We have the treasure in “*earthen vessels*”, but the “*treasure*” must exist!

## **BUT, ARE NOT THE AUTOGRAPHS LOST?**

The question of a lost standard remains. Returning to the analogy of measuring devices, what would happen if someone stole the “inerrant” yardstick from the Bureau of Standards? Well, there would be no inconvenience so long as we did not know about it—we would continue happily as we always have. But if the loss became known then confidence in the individual instruments would be undermined and our business dealings would become complicated by arguments about the standard of measurement (as I have observed in certain places). I believe we have seen this syndrome with reference to the Bible. Until the 19th century there was no question (to speak of) about the standard, and the Bible was accepted as authoritative even though in fact the text they were using was not identical to the Original. But during the past 200 years critics have convinced the majority (in Europe and North America) that the standard is gone, with the resulting spiritual and moral confusion we see on every side.

The problem is largely one of perception. Generations have lived and died happily using their imperfect rulers and yardsticks without suffering any damage or inconvenience—the discrepancies were not big enough to matter. (If someone had convinced them that they had an insuperable problem, however, they would have been damaged—gratuitously.) Similarly, our manuscripts and versions contain discrepancies, most of which are not serious enough to matter for ordinary purposes. However, if someone makes a “court case” out of some issue then the existence and identity of the relevant standard become crucial.

I submit that the central “issue” has to do with the authority of Scripture. When the Protestant Reformation appealed to the Scriptures (in the original languages) as the supreme authority, the Roman Catholic Church countered by pointing to the textual variation in the manuscripts and challenging the Reformation leaders to produce the standard.<sup>2</sup> In the eighteenth and nineteenth centuries destructive critics went beyond the variants to challenge the date, authorship and composition of the individual books of the Bible. I consider that these challenges have been adequately handled by others and return to the problem of textual variation.

How does textual variation affect the authority of Scripture? It depends. Is that authority to be seen as absolute or relative? If we are prepared to settle for a relative authority, the “Neo-orthodox” position, we can assimilate an admixture of error in the Text. But if we wish to claim absolute authority the standard

<sup>2</sup>See Theodore P. Letis, “John Owens Versus Brian Walton,” *The Majority Text: Essays and Reviews in the Continuing Debate* (Fort Wayne: The Institute for Reformation Biblical Studies, 1987), pp. 145-90.

must be perfect. Scripture derives its authority from divine inspiration, but if any part of the text is not inspired that part lacks authority. Specifically, the errors and alterations introduced by fallible men down through the centuries of copying lack authority. For this reason those who claim that the Bible is inerrant usually limit the claim to the Autographs. But since the Autographs are gone (they were probably worn out from use within the first one hundred years) what good does that do us? It depends.

The analogies already given show that we can coexist with minor discrepancies quite handily without feeling that we have been cheated or deceived. In fact, in most contexts to insist on absolute perfection would be deemed unreasonable, if not intolerable. We accept small discrepancies, but not **big** ones! If we feel that someone is trying to take advantage of us our reaction is prompt. Similarly, we must distinguish between honest copying errors, due to inattention, and deliberate alterations. Further, many alterations appear to be relatively 'harmless', while others are overtly damaging.

In Matthew 13:25 and 39 the Lord Jesus explains that Satan sows tares among wheat—this is true of the Church and it is true of the Biblical text; although the analogy is not perfect, in the latter case the “tares” may be likened to poison mixed with the Bread of Life. To give a few quick examples: the variants in Matthew 1:7 and 10 that introduce Asaph and Amos into Jesus’ genealogy are poison; the variant in Matthew 1:18 that ascribes to Christ a “beginning” is poison; the variant in Mark 6:22 that turns Herodias into Herod’s daughter is poison; the variant in Luke 3:33 that inserts the fictitious Admin and Arni into Jesus’ genealogy is poison (these were probably the result of scribal carelessness, or ignorance, but for modern editors to intrude them into the printed text is irresponsible); the variant in Luke 23:45 that has the sun being eclipsed is poison; the variant in John 1:18 that reads “an only begotten god” is poison; the variant in 1 Corinthians 5:1 that denies the existence of incest among the Gentiles is poison; the omission of Mark 16:9-20 is poison; the use of brackets in printed Scripture (in whatever language) to insinuate to the user that the enclosed material is spurious is poison. By “poison” I mean violence done to the Biblical text that undermines its credibility.<sup>3</sup>

So where does that leave us? It leaves us with thousands of manuscript copies (of the NT writings) from which we may recover the precise wording of the Autographs, provided we evaluate the evidence on the basis of what the Bible says about itself, about God and His purposes, about man, and about Satan and his ways. To these must be added the declarations of the early Church Fathers and the facts of history that have come down to us. By careful attention to all relevant considerations we can weed out the errors and alterations and affirm with reasonable certainty what must have been the wording of the Autographs. (I would say that *The Greek New Testament According to the Majority Text* [Thomas Nelson Publishers, second edition, 1985] is at least 99.8% pure, with no admixture of “poison”—we must pay a high price in detailed research in order to achieve 100%.)

Since God the Son on earth emphatically declared, “*till heaven and earth pass away not one jot or one tittle will by any means pass from the Law till all is fulfilled*” (Matt. 5:18), I conclude that He would never permit a true reading to disappear from the manuscript tradition. I am well aware that Jesus was presumably referring specifically to the Pentateuch. How then can I apply His statement to the NT? First, jots and tittles refer to **letters**, not concepts or ideas; in fact they are the smallest of letters. Our Lord’s words constitute a rather radical declaration about the preservation through time of the precise form of the Sacred Text. The third chapter of 2 Corinthians makes clear that the “*new covenant*” (v. 6) is “*more glorious*” (v. 8) than the old, including the very Decalogue itself (“*engraved on stones*,” v. 7). Chapters 7 through 9 of Hebrews demonstrate the general superiority of the new covenant over the old and Jesus Himself both guarantees (7:22) and mediates (8:6) this “better” covenant. I conclude that God’s protective interest in the New Testament must be at least as great as His protective interest in the Old. 1 Chronicles 16:15 declares that interest to extend to a thousand generations; in other words, to the end of the world (there have yet to be 300, since Adam).

To be faced with the task of recognizing the genuine reading among two or more variants is one thing; to affirm that something so crucial as the ending of a Gospel has disappeared without a trace is altogether different. If Mark 16:9-20 is not genuine then it would seem that Christ’s statement in Matthew 5:18 is in error.

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<sup>3</sup>I have a fuller treatment of the subject of variation in Appendix H.



## THE MATTER OF CANONICITY

There is a further question—why do we claim that Mark is “Gospel” in the first place? Where did it get its canonicity? Or to put it another way, if God is going to inspire a text for the use of subsequent generations He has to make sure that people recognize it for what it is. If the nature of such a text is not perceived and it is relegated to oblivion, or treated with no more respect than any other bit of literature, then God’s purpose is frustrated. So why do we say that Mark’s Gospel is “Bible”? Because the Church, in her corporate capacity, has so declared, and she has done so down through the centuries, beginning in the second (at least). (We do not have hard evidence from the first century, but we do from the second and all subsequent centuries.) Of necessity God worked through the Church to achieve both canonicity (the public recognition of its quality) and preservation. (I would say that the superior quality of the inspired writings is intrinsic and can be perceived by a spiritual person in any age, but if the early Church had not recognized them they would not have been copied through the centuries and thus would not have come down to us.)

What has the Church, down through the centuries, said about Mark 16:9-20? With united, almost unanimous voice she has declared its canonicity. If she was deceived on this point, how do we know she was not deceived about the rest of the book? But since her voice was not unanimous, the next thing to do is review the evidence.

## THE EXTERNAL EVIDENCE

The passage in question is contained in every extant Greek manuscript (about 1800) except three: codices B (Vaticanus) and Ⲛ (Sinaiticus) and the twelfth century minuscule 304. It is also contained in all extant lectionaries (compendia of the established Scripture lessons linked to the ecclesiastical calendar). The importance of this lectionary evidence has been explained by J. W. Burgon: “That lessons from the New Testament were publicly read in the assemblies of the faithful according to a definite scheme, and on an established system, at least as early as the fourth century,—has been shown to be a plain historical fact.”<sup>4</sup> And again:

It is found that, from the very first, S. Mark xvi. 9-20 has been everywhere, and by all branches of the Church Catholic, claimed for **two** of the Church’s greatest Festivals,—Easter and Ascension. A more weighty or a more significant circumstance can scarcely be imagined. To suppose that a portion of Scripture singled out for such extraordinary honour by the Church universal is a spurious addition to the Gospel, is purely irrational.<sup>5</sup>

Although after a time there came to be prescribed Scripture passages for every day of the year, the practice evidently began with the weekends, and most especially the most important ones. According to Baumstark’s Law the lections associated with the great festivals seem to have been the earliest to have been adopted.<sup>6</sup> Since the idea was borrowed from the Jewish synagogue the practice may well have been generalized during the second century.

Before the Church started producing lectionaries as such, regular manuscripts were adapted by putting symbols in the margins (or in the text) to indicate the beginning and ending of lections. These included the word *τελος* “end”, either in full or abbreviated. Statements of evidence for omitting vv. 9-20 usually mention a number of MSS that have such symbols at the end of v. 8 (and thus at the beginning of v. 9), claiming that they were put there to indicate doubt about the genuineness of the following verses. It happens that not only is Mark 16:9-20 itself one of the most prominent of all lections in the liturgical calendar, but a separate lection ends precisely with v.8.

Consider what Bruce Metzger writes concerning MS 2386:

<sup>4</sup>*The Last Twelve Verses according to S. Mark*, 1871, p. 207. Reprinted in 1959 by the Sovereign Grace Book Club, but the pagination given refers to the 1871 edition (to find the corresponding place in the 1959 edition add 78 to the page number).

<sup>5</sup>*Ibid.*, p. 210.

<sup>6</sup>W.R. Farmer, *The Last Twelve Verses of Mark* (Cambridge University Press, 1974), p. 35. On pp. 34 and 35 he gives a good summary of the lectionary evidence.

The latter, however, is only an apparent witness for the omission, for although the last page of Mark closes with *εφοβουντο γαρ*, the next leaf of the manuscript is missing, and following 16:8 is the sign indicating the close of an ecclesiastical lection . . . , a clear implication that the manuscript originally continued with additional material from Mark.<sup>7</sup>

Notice his “clear implication.” Is it not obvious? One cannot read beyond the end of a book so there is no point in putting a lection sign there. Which makes one wonder about the intentions of the editors of UBS<sup>3</sup>. In their apparatus, as evidence for the omission of vv. 9-20, they include “(Lect? *Lection ends with verse 8*)”—this presumably refers to lection signs in the margins since it cannot mean that the lectionaries do not have vv. 9-20. But lection signs in the margin are evidence for, not against! Notice that in discussing the evidence for variant sets within vv. 9-20 UBS<sup>3</sup> invariably cites *Byz Lect*, which means that they recognize that the lectionaries contain the passage. In fact, from the circumstance that they also list 1185m it appears that lectionary 185 is the only one that does not have the verses in the Synaxarion (just in the Menologion).

The Syriac, Latin, Coptic and Gothic versions all massively support the passage. Only the Armenian and Georgian versions (both fifth century) omit it. To be more precise, every Syriac MS (about 1,000?) except one (the Sinaitic, usually dated around 400) contains the passage. Although the Sinaitic is the oldest extant Syriac MS, apparently, it is not representative of the Syriac tradition. B. F. Westcott himself, writing in 1864, assigned the Peshitta to the early second century, in accord with the general opinion of the Scholarly world of the time.<sup>8</sup> The demands of the W-H theory subsequently led them to assign the Peshitta to the fifth century, but Vööbus demonstrates that the Peshitta goes back to at least the mid-fourth century and that it was not the result of an authoritative revision.<sup>9</sup> The Sinaitic is a palimpsest; it was scraped off to make way for some devotional material, which is an eloquent commentary upon the contemporary evaluation of its quality!

Every Latin MS (8,000?) except one (Bobiensis, usually dated about 400) contains the passage. But Bobiensis (k) also seems to be the only witness of any kind to offer us the so-called “shorter ending” by itself—every other witness that contains the “shorter ending” also contains the “longer ending,” thereby displaying a conflation (an incredibly stupid one!). Now then, so far as I know everyone recognizes the “shorter ending” to be an aberration, which means that Bobiensis is aberrant at this point and does not represent the Latin tradition. If the Latin tradition dates to the second century here we have second century support for the “longer ending”. It appears that the only Coptic witness that omits the passage is one Sahidic MS, although there are a few that exhibit the conflation already mentioned (they are thereby convicted as being aberrant).

The Diatessaron (according to the Arabic, Italian and Old Dutch traditions) and Irenaeus clearly attest the last twelve verses in the second century! As does Hippolytus a few years later. Then come Vincentius, the Gospel of Nicodemus and the Apostolic Constitutions in the third century; Eusebius, Aphraates, Ambrose and Chrysostom in the fourth; followed by Jerome, Augustine, Cyril of Alexandria, Victor of Antioch, etc.

Clement of Alexandria and Origen are usually cited as being against these verses, but it is an argument from silence. Clement’s surviving works seem not to refer to the last chapter of Mark, but neither do they refer to the last chapter of Matthew. So?

The main patristic source used to argue against Mark 16:9-20 is Eusebius. It appears that he wrote a defense against four alleged discrepancies between resurrection accounts of the Gospels put forward by a certain “Marinus” (our knowledge is based on a tenth century abridgement of what he presumably wrote, an abridgement that lacks internal consistency). The first alleged discrepancy is between Matthew 28:1 and Mark 16:9. On the face of it “Marinus” is assuming that v. 9 is genuine “Gospel” or there would be no problem, so we may conclude that he understood that to be the position of the Church. That Eusebius takes

<sup>7</sup>Metzger, p. 122, fn. 1.

<sup>8</sup>*The Bible in the Church* (London: MacMillan) p. 132 (reprintings in the 1890s still contain the statement).

<sup>9</sup>*Early Versions of the New Testament* (Stockholm: Estonian Theological Society in Exile, 1954), pp. 100-102.

the time to answer as he does points in the same direction. Further, in answering the second alleged discrepancy Eusebius simply assumes the genuineness of the Marcan account and argues that Matthew's turn of phrase has been misunderstood. However, in answering the first allegation (according to the abridgement) he offers two options: "One might say that the passage is not contained in all the copies of Mark's Gospel . . . ; another says that both accounts (Matthew and Mark) are genuine and must be properly understood." With the first option he employs the optative mood, appropriate to the genre of hypothetical rhetoric (which means that nothing said by the hypothetical speaker is being vouched for by Eusebius), while with the second he switches to the indicative mood, presumably an indication of what he himself considered to be the correct position—so much so that when he moves on to the second "discrepancy" he does not offer the option of rejecting the passage.

However, the "canons" or "sections" of Eusebius (but not the so-called "sections of Ammonius") may not have included vv. 9-20. In some Greek MSS the sectional number "233" is placed in the margin beside v. 8 and is the last such number (in Mark)—which means that section 233 started at v.8, but since many "sections" contained more than one verse we do not know the extent of this one. But, there is more to the story. Burgon checked out 151 Greek MSS that have "Eusebian sections" marked in the margin and offers the following tabulation of results:

in 3 MSS the last section number is 232, set against v. 6,  
 in 34 MSS the last section number is 233, set against v. 8,  
 in 41 MSS the last section number is 234, set against v. 9 (?),  
 in 4 MSS the last section number is 235, set against v. 10 (?),  
 in 7 MSS the last section number is 236, set against v. 12 (?),  
 in 12 MSS the last section number is 237, set against v. 14 (?),  
 in 3 MSS the last section number is 238, set against v. 15,  
 in 1 MS the last section number is 239, set against v. 17,  
 in 10 MSS the last section number is 240, set against v. 19,  
 in 36 MSS the last section number is 241, set against v. 20.

Added to this, the following information may be of interest:

the oldest MS that stops with 232 is A of the 5th century,  
 the oldest MS that stops with 233 is L of the 8th century,  
 the oldest MS that stops with 234 is Δ of the 9th century,  
 the oldest MS that stops with 237 is Λ of the 9th century,  
 the oldest MS that stops with 239 is G of the 9th century,  
 the oldest MS that stops with 240 is H of the 9th century,  
 the oldest MS that stops with 241 is C of the 5th century.<sup>10</sup>

For sections 235, 236 and 238, the earliest MS is 10th century or later. So, in three-fourths of these MSS the section numbers overtly go beyond v. 8, and the two oldest ones (A and C) do not aid the case for omission.

Jerome is cited as being against the passage because he put Marinus' questions in a certain "Hebidia's" mouth and used an abridgement of Eusebius' answers in reply. However, Jerome's own evaluation is clear from the fact that he included Mark 16:9-20 in his Latin Vulgate; he also quotes verses 9 and 14 in his writings. Hesychius of Jerusalem (not Severus of Antioch, nor Gregory of Nyssa) reproduces Eusebius in his own words in a treatise about the familiar "problems". However, since he quotes Mark 16:19 and expressly states that St. Mark wrote the words, his own position is clear. Victor of Antioch repeats Eusebius yet again, and acknowledges that "very many" copies of Mark lack vv. 9-20 (it is not clear whether he had verified this to be true or was just quoting Eusebius). Then he affirms that he himself has verified that "very many" contain them, and appeals to "accurate copies" and most especially to "the Palestinian exemplar of Mark which exhibits the Gospel verity" in support of his own contention that the passage is genuine. He even blames the omission on individuals who thought the verses to be spurious.<sup>11</sup>

<sup>10</sup>Burgon, p. 313; for the general discussion see pp. 127-134 and 297-314.

<sup>11</sup>For detailed documentation and an exhaustive discussion, see Burgon, pp. 19-31, 38-69, 265-90.

Unfortunately, commentaries can still be found that reproduce certain misstatements of yesteryear about “scholia” and “catenae”. The “catenae” may not be adduced for the omission, as demonstrated by Burgon (pp. 135-157). As for the “scholia” (critical notes), the situation seems to be something like this: at least 22 MSS simply repeat Victor of Antioch’s statement, which includes the affirmation that he himself had verified that “very many” copies, including “accurate” ones and most especially the “true Palestinian exemplar,” contained vv. 9-20; several have footnotes defending the verses on the basis of “ancient copies at Jerusalem” (attention is directed to the footnote by a “+” or “\*” in the text which is repeated before the footnote—much as we do today); two MSS say the passage is missing in “some” copies but present in “many”; four MSS say it is missing in “some” copies while present in “others”; three say it is missing in “many” and present in “many”.<sup>12</sup> Now the earliest of these MSS is from the 10th century (most are later), so the copyists were repeating the “scholia” blindly, with no way of knowing if they were true or not. The fact remains that of the extant MSS only three lack the passage.

Codices L, Ψ, 099, 0112 and 579 are sometimes claimed as being against the genuineness of vv. 9-20 because they also contain the so-called “shorter ending.” Metzger’s comment (p. 126) is misleading—these five MSS did not “replace” one ending with another, they conflated both. A conflation condemns the MSS that contain it, at that point, but says nothing about the relative merits of the component parts.

We must return to codices B and ℵ, both of the 4th century and both from Egypt (presumably, see Farmer, p. 37), being generally regarded as the two most important MSS of the New Testament (frequently referred to as the “oldest and best”). Their agreement in omitting vv. 9-20 has been an important factor in the thinking of those who reject the passage (since they generally regard the “Alexandrian text-type” as superior to all others). However, the evidence is not quite straightforward. Codex B is written in three columns and upon completing a book it normally begins the next book at the top of the next column. But between Mark and Luke there is a completely vacant column, the only such column in the codex. Considering that parchment was expensive, the “wasting” of such a space would be quite unusual. Why did the copyist do it?

As for Codex ℵ, the folded sheet containing the end of Mark and beginning of Luke is, quite frankly, a forgery. Tischendorf, who discovered the codex, warned that those four pages appeared to be written by a different hand and with different ink than the rest of the manuscript. However that may be, a careful scrutiny reveals the following: the end of Mark and beginning of Luke occur on page 3 (of the four); pages 1 and 4 contain an average of 17 lines of printed Greek text per column (there are four columns per page), just like the rest of the codex; page 2 contains an average of 15.5 lines of printed text per column (four columns); the first column of page 3 contains only **twelve** lines of printed text and in this way v. 8 occupies the top of the second column, the rest of which is blank (except for some designs); Luke begins at the top of column 3, which contains 16 lines of printed text while column 4 is back up to 17 lines. On page 2 the forger began to spread the letters, displacing six lines of printed text; in the first column of page 3 he got desperate and displaced **five** lines of printed text, just in one column! In this way he managed to get two lines of v. 8 over onto the second column, avoiding the telltale vacant column (as in B). That second column would accommodate 15 more lines of printed text, which with the other 11 make 26. Verses 9-20 occupy 23.5 such lines, so there is plenty of room for them. It really does seem that there has been foul play, and there would have been no need for it unless the first hand did in fact display the disputed verses. In any event, ℵ as it stands is a forgery and therefore may not legitimately be alleged as evidence against them.

To sum up: every extant Greek MS (about 1,800) except two (B and 304—ℵ is not “extant” because it is a forgery at this point) contains vv. 9-20. Every extant Greek lectionary (about 2,000?) contains them (one of them, 185, doing so only in the Menologion). Every extant Syriac MS (about 1,000?) except one (Sinaitic) contains them. Every extant Latin MS (8,000?) except one (k) contains them. Every extant Coptic MS except one contains them. We have hard evidence for the “inclusion” from the 2nd century (Irenaeus, Diatessaron?). We have no such hard evidence for the “exclusion”.

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<sup>12</sup>Burgon, pp. 116-125, 290-292.

It would appear that sometime during the 3rd century MSS lacking the passage began to be produced in Egypt, probably in Alexandria, of which two (or one) from the fourth century have survived to our day. Although the idea gained some currency in Egypt, it did not take over even there since most Alexandrian witnesses, including the Coptic version, contain the verses. The translators of the Armenian version had studied in Alexandria, and the Georgian version was based on the Armenian, which explains how the idea escaped from Egypt. The rest of the Christian world seems not to have picked up this aberration. As stated at the outset, with united voice, down through the centuries, in all parts of the world (including Egypt), the Church universal has affirmed and insisted that Mark's Gospel goes from 1:1 to 16:20. Since that is so, how can someone who denies the authenticity of Mark 16:9-20 still affirm the Divine Inspiration of Mark 1:1-16:8? Is he not being inconsistent?

## THE INTERNAL "EVIDENCE?"

It should not be necessary to prolong this exercise, but something probably ought to be said about the "internal evidence" that some critics evidently feel to be fatal to the passage. We are told that Mark "never" uses certain words or phrases, which nonetheless occur there; that others which he "always" uses are missing; that the style is "foreign" to Mark; that there are insuperable problems with the discourse structure and the very content; in short, that it is "impossible" that the same person could have penned 1:1-16:8 and 16:9-20. Alas, what to do?

Most of the "arguments" of this sort that have been advanced reveal a disappointing degree of superficiality in research and ignorance of language. Such supposed arguments were thoroughly refuted over 100 years ago by J.A. Broadus (*The Baptist Quarterly*, July, 1869, pp. 355-62) and Burgon (pp. 136-90). A modern (1975) treatment is offered by Farmer (pp. 79-103). I will take up one argument that might seem impressive to the uninitiated reader.

It has been alleged as a sinister circumstance that Jesus is not mentioned by name in v. 9 (or in the following verses). The rules of discourse structure have been violated, so they say. Really? Let's consider Mark's practice elsewhere. Between Mark 9:27 and 39 Jesus is not mentioned by name, although there are two paragraph breaks and one section break in between, plus two changes in location. Jesus is next named in 10:5, five verses after a section break and another change of location. Between Mark 3:7 and 5:6 (75 verses) Jesus is not named even though there are numerous participants and several radical changes in location, scene and content. In each case it is only when another man is introduced in the narrative, creating a potential for ambiguity, that Jesus is again named since a mere pronoun would be ambiguous in reference. In Mark 16 there is only one dead person in focus, precisely the participant who has dominated the whole book, so v. 9 could only refer to Him—there is no ambiguity so a proper name is not called for. Throughout vv. 9-20 no other singular masculine participant is introduced so there is no need to identify Jesus by name. By way of contrast, Mary Magdalene had to be fully identified, because not only is there more than one woman in the account, there is more than one Mary! (The background information, "out of whom He had cast seven demons," is entirely appropriate here, and only here, because this is the first time she is brought into focus—in the prior references she was just part of the group.)

There is one aspect of this situation that has not received sufficient attention that I am aware of. The more strident and caustic a critic becomes in proclaiming the "impossibility" of accepting Mark 16:9-20 as genuine (because of style, vocabulary and discourse features), the more he insults the ancients and undercuts his own position. After all, Irenaeus was a native speaker of Koine Greek (presumably)—why didn't he notice the "impossibility"? How come the native speakers of Koine Greek who lived in Greece and Asia Minor and copied Mark down through the years didn't recognize the "obvious stupidity", the "odious fabrication"? How come? How is it that modern critics who deal with Koine Greek as a dead language, and at a distance of 1800 years, are more competent to judge something like this than the native speakers who were on the scene? Irenaeus knew Polycarp personally, who knew the Apostle John personally, who knew Mark personally. Irenaeus declares that Mark wrote 16:19. Who among us is qualified to say that he was deceived?

It would seem to be obvious that the more preposterous the pericope is affirmed to be, the more difficult it becomes to explain how it imposed itself on the Church universal, beginning in the second

century (at least). In fact, if the passage contains difficulties this would easily account for its omission in certain quarters. The perceived difficulties would be a more than sufficient stimulus to activate editors and copyists trained in the Alexandrian school of textual criticism. Indeed, in our own day there are not a few who find the content of Mark 16:9-20 to be unpalatable and greet the claim that the passage is spurious with relief.

Hopefully all concerned will agree that the identity of the text of Scripture is to be established on the basis of the evidence, not personal prejudice. I submit that the evidence in this case is perfectly clear and that the overwhelming testimony of the Church down through the centuries should be loyally accepted.

I see a corollary here: not only is Mark 16:9-20 vindicated, but codices B and  $\delta^s$  stand convicted of containing "poison". They also contain the poison (mentioned above) in Matthew 1:7, 1:10 and 1:18, Mark 6:22, Luke 3:33 and 23:45, John 1:18 and 1 Corinthians 5:1. Does this not diminish their credibility as witnesses?

I confess that I am puzzled at the dedication and industry of the opponents of these verses. Why do they go to such lengths and expend so much energy to discredit them? Another curious feature of their work is the frequent misrepresentation of the evidence. For instance, in his advice to translators about how to proceed at the end of v. 8, A. Pope suggests putting the following:

"[Some manuscripts end at this point]  
 [In some manuscripts the following words are found]  
 SHORTER ENDING  
 [In some manuscripts the following words are found after verse 8]  
 LONGER ENDING"<sup>13</sup>

What interests me here is the lack of semantic precision in the use of the word "some". The first time it means "three". The second time it means "six". The third time it means "about 1800"! Will the unsuspecting reader of Pope's article not be misled? And if anyone follows Pope's advice will not his readers also be misled?

I wonder sometimes if people really believe what the glorified Jesus said in Revelation 22:19.

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<sup>13</sup>*Selected Technical Articles Related to Translation*, Oct., 1984, p. 22.