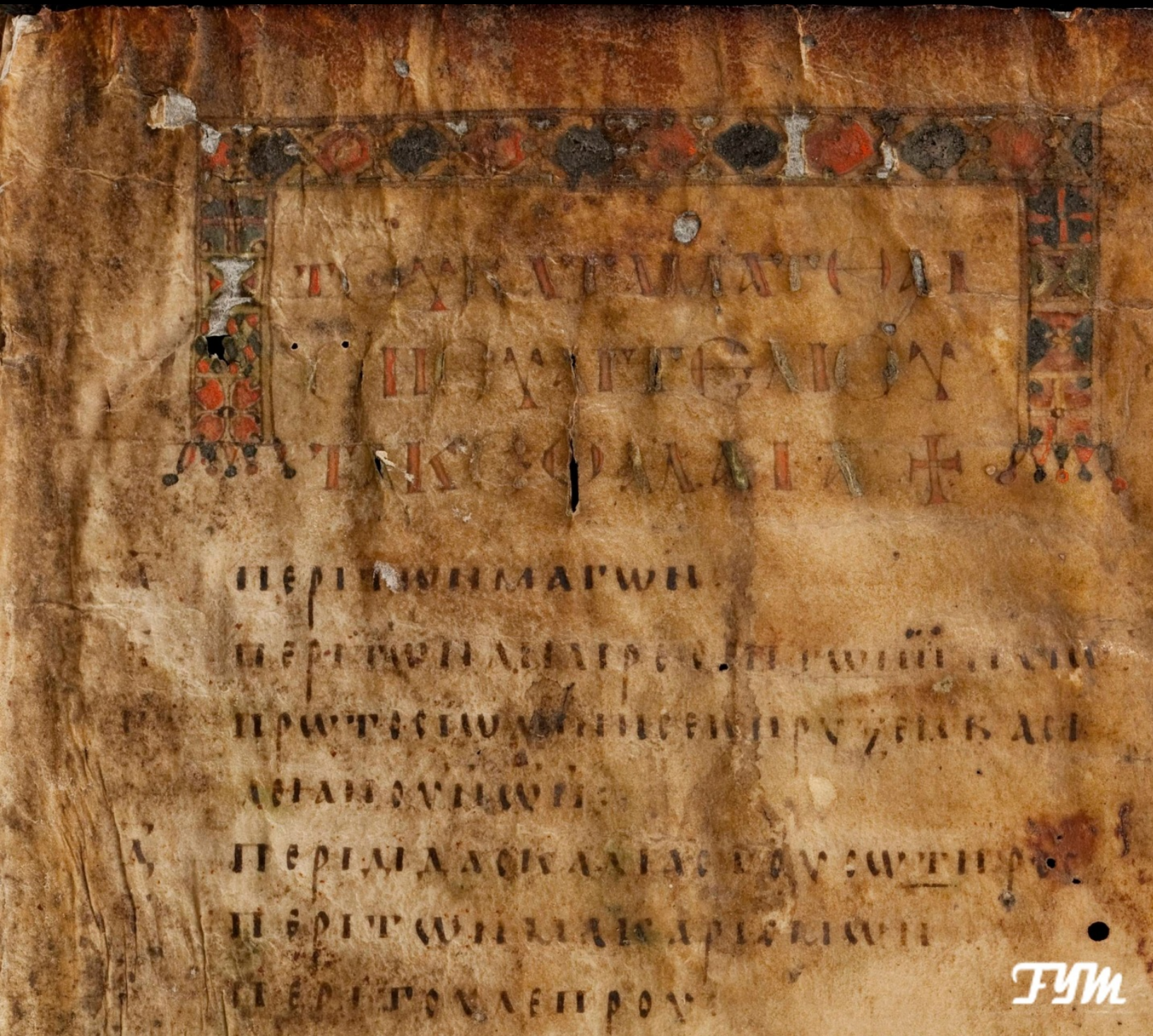


DIGGING FOR THE TRUTH

COLLECTED ESSAYS REGARDING THE BYZANTINE TEXT OF THE GREEK NEW TESTAMENT

- A FESTSCHRIFT IN HONOR OF MAURICE A. ROBINSON -



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PREFACE

Early in his seminary career, a young student had the opportunity to pursue additional study under a noted professor of textual criticism. As they were discussing the development of the Greek New Testament text, and noting some dissatisfaction with the then-current state of New Testament textual criticism, the professor referred to something he had previously written that changed the course of the young seminarian's life: "We must have a critical history of transmission. Some new angle, some novel experiment must be tried." The discussion regarding that comment then set in motion what has become a lifetime pursuit of digging for the truth.

Maurice A. Robinson received his undergraduate degree in English and Secondary Education from the University of South Florida in Tampa, Florida. Being called into the ministry, he attended Southeastern Baptist Theological Seminary in Wake Forest, North Carolina, where he obtained his Master of Divinity degree. That is when he had the privilege noted above of studying one-on-one with noted textual critic Kenneth W. Clark at Duke University. These studies whetted his appetite for scholarly endeavors, leading him to continue into the Master of Theology program at Southeastern, and the Doctor of Philosophy program at Southwestern Baptist Theological Seminary in Fort Worth, Texas.

Dr. Robinson has been engaged in various forms of teaching and research ministry for over thirty years. He currently is Senior Professor of New Testament and Greek at Southeastern Baptist Theological Seminary, where he has served for the past 22 years. He has presented numerous papers in various venues, with many of these being published, and has spoken frequently at various symposiums and seminars on topics that deal with New Testament textual criticism.

Robinson is best known for his defense of the Byzantine-priority theory of New Testament textual criticism. As he has stated, that position

evaluates internal and external evidence in the light of transmissional probabilities. This approach emphasizes the effect of scribal habits in preserving, altering, or otherwise corrupting the text, the recognition of transmissional development leading to family and texttype groupings, and the ongoing maintenance of the text in its general integrity as demonstrated within our critical apparatuses. The overriding principle is that textual criticism without a history of transmission is impossible. To achieve this end, all readings in sequence need to be accounted for within a transmissional history, and no reading can be considered in isolation as a 'variant unit' unrelated to the rest of the text.¹

His various papers and presentations both support his theoretical position as well as critique and oppose current critical views regarding New Testament text-critical praxis. A tangible result of his text-critical theory is his co-edited edition of *The New Testament in the Original Greek: Byzantine Textform 2005*. A current major research project involves the collation of all available Greek manuscripts and lectionaries in relation to the *Pericope Adulterae* (Jn 7:53-8:11), listing all variants, with the intent of defining and establishing the textual interrelationships among those manuscripts, with a goal of demonstrating an archetypal form of that pericope, detailing its further relationship to the Gospel of John.

In addition to these accomplishments, Robinson has also been a pioneer in the area of computerized study of the Greek New Testament. In this regard, beginning in the mid-1980's, he created electronic editions of a number of Greek New Testament texts, with grammatical lemmatization and parsing data. These were originally prepared for the *Online Bible* software program, but have also been incorporated into various major Bible software programs (including *Bibleworks*), with raw ASCII/DOS versions of these texts in CCAT-based format also available freely from dozens of internet web sites worldwide.

This book consists of various essays gathered in honor of Dr. Robinson's sixty-sixth birthday. The authors share his esteem for the Biblical text and recognize his unique contributions to the field and have chosen to recognize his scholarship by being included in this work.

Mark Billington --- Peter Streitenberger

¹ Maurice Robinson, "Appendix: The Case for Byzantine Priority," in *The New Testament in the Original Greek: Byzantine Textform 2005*, by Maurice A. Robinson and William G. Pierpont (Southborough, MA: Chilton Book Publishing, 2005), 544.



A MODEST EXPLANATION FOR THE LAYMAN OF IDEAS RELATED TO DETERMINING THE TEXT OF THE GREEK NEW TESTAMENT

by Timothy Friberg

Abstract

There are almost 6,000 partial or complete manuscripts of the Greek New Testament extant. It is possible, by the application of certain principles outlined in this essay, to weed out errors that crept into the manuscripts during the process of copying and thus to reconstruct a text that is identical to — or at least very close to — the original text. This resulting text is called the Byzantine Textform, which we attempt to show is superior to the text arrived at by the other main approach to textual criticism, i.e., the eclectic approach, which seems to be an arbitrary “pick and choose” process. The Byzantine Textform has been dominant through the centuries of the church and, we believe, deserves that dominant position today, not because it is the result of some research, though extensive research supports it, but because it has always been there with historical and transmissional integrity in its favor.

Note: few of the following thoughts are original to the writer. He is only trying to facilitate for nonspecialists a wider understanding of the thoughts of others, largely through simplification and restatement. Technical terms italicized in the text are defined in the glossary at the end of the paper. All quotes not identified in the text are taken from the writings of Maurice Robinson, as noted in the bibliography.

Introduction: on paper, ink and writing styles

Things weren't always as easy as they are now. This is especially true in the area of written communication: personal computers (1970s), typewriters (from 1874), indeed even printing presses with movable type (1440s) just did not exist. Documents were written by hand, whether personal letters and notations or official decrees. If there was a need for multiple copies, additional handmade copies were made one at a time.

At the time of the writing of the individual books of the New Testament, the most common form of “paper” used was *papyrus* sheets (from about 2400 BC), frequently put together into longer rolled scrolls. Ink was made of different substances, some as common as *lampblack*; and pens were made of sharpened reeds and, later, quills. Animal skins (*vellum*, *parchment*), properly prepared, were also used as writing material (from as early as 1468 BC). Animal skins had the advantage of being more permanent. Their disadvantage lay in their greater cost. Paper, the precursor of what we know today, was introduced into the Middle East by Arabs (from AD 795), who acquired the material from its inventors, the Chinese.

We cannot be absolutely sure of the materials on which the original New Testament writings were made, since those *autographs* (i.e., the original documents) have now all apparently perished. However, nearly all New Testament *manuscripts* available to us today from before the fourth century are found as papyrus documents, whereas nearly all manuscripts from the fourth to fourteenth centuries *extant* today were written on parchment. The early New Testament manuscripts could have been written in scroll format, but our existing evidence clearly shows that from the earliest time of *canonical* transmission, copies were made on another form of “book,” that of the *codex*, in which actual pages, as we know them today, were bound together along one edge.

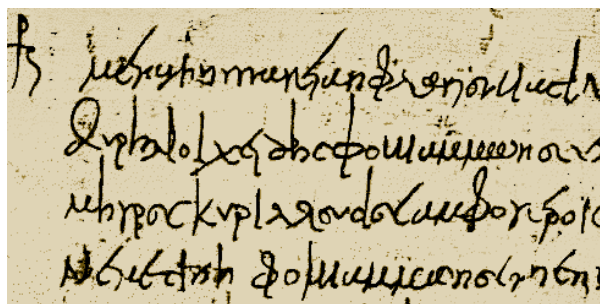


Writing in Greek characters was first done in *uncial* form, not unlike our capital letters. *Cursive writing*, an adaptation from the uncials, was like current longhand and lower-case printing, and was used in writings that were of a more personal nature. Both uncial writing in formal documents and cursive writing in informal documents existed side by side for centuries. All manuscripts made prior to the tenth century that still exist for us to examine were written in uncials.

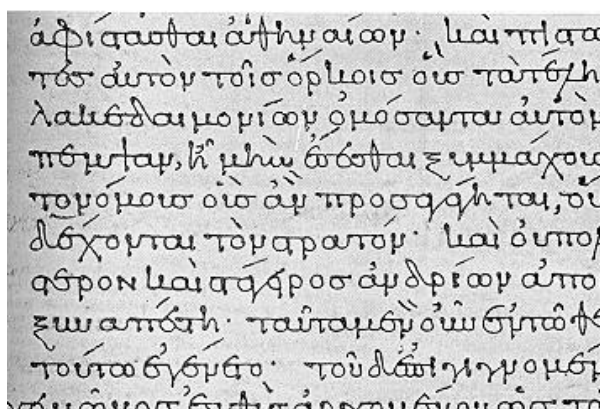
In the early ninth century a *minuscule* form of handwriting was developed, particularly by scribes interested in lightening their laborious duties with the efficiency of speedwriting. This new style was a modification of the cursive, being somewhat more formal in appearance. By the end of the tenth century all manuscripts copied (except for some *lectionaries*) were done in minuscule form.



(1) Uncial. Greek manuscript, 4th century, uncial script. Codex Sinaiticus, (Leipzig, Royal Library, Cod. Frid.Aug). Text: Bible, Esther 1:20–21. (from Wikipedia)



(2) Cursive. Greek manuscript in ancient cursive script, papyrus, dated 545 A.D., Brit. Mus. Pap. 1319. Text: Debenture (private contract; from Wikipedia)



(3) Minuscule. Greek manuscript, earliest form of minuscule script ("codex vetustissimus"), 10th Century, (Florence, Laurentian Library, Plut. 1xix). Text: Thucydides, Peloponnesian War, book 4, chapter 88f. (from Wikipedia)

GREEK SCRIPTS OF THE MIDDLE AGES

| CAPITALS | UNCIALS | CURSIVE | MINUSCLES |
|----------|---------|---------|-----------|
| Α | ΑΑΑα | ΑΑΑα | αααα |
| Β | ΒΒΒβ | ΒΒΒβ | ββββ |
| Γ | ΓΓΓγ | ΓΓΓγ | γγγγ |
| Δ | ΔΔΔδ | ΔΔΔδ | δδδδ |
| Ε | ΕΕΕε | ΕΕΕε | εεεε |
| Ζ | ΖΖΖζ | ΖΖΖζ | ζζζζ |
| Η | ΗΗΗη | ΗΗΗη | ηηηη |
| Θ | ΘΘΘθ | ΘΘΘθ | θθθθ |
| Ι | ΙΙΙι | ΙΙΙι | ιιιι |
| Κ | ΚΚΚκ | ΚΚΚκ | κκκκ |
| Λ | ΛΛΛλ | ΛΛΛλ | λλλλ |
| Μ | ΜΜΜμ | ΜΜΜμ | μμμμ |
| Ν | ΝΝΝν | ΝΝΝν | νννν |
| Ξ | ΞΞΞξ | ΞΞΞξ | ξξξξ |
| Ο | ΟΟΟο | ΟΟΟο | οοοο |
| Π | ΠΠΠπ | ΠΠΠπ | ππππ |
| Ρ | ΡΡΡρ | ΡΡΡρ | ρρρρ |
| Σ | ΣΣΣσ | ΣΣΣσ | σσσσ |
| Τ | ΤΤΤτ | ΤΤΤτ | ττττ |
| Υ | ΥΥΥυ | ΥΥΥυ | υυυυ |
| Φ | ΦΦΦφ | ΦΦΦφ | φφφφ |
| Χ | ΧΧΧχ | ΧΧΧχ | χχχχ |
| Ψ | ΨΨΨψ | ΨΨΨψ | ψψψψ |
| Ω | ΩΩΩω | ΩΩΩω | ωωωω |

(4) Sample of Greek scripts of the Middle Ages, p. 80 from *The Languages of the Word* by Stanley Wemyss, (Philadelphia, STANLEY WEMYSS, 1950)

With great certainty, we may assert that in the case of all original, individual New Testament writings at least one single copy was made and carried to one recipient. In keeping with the practice of the day, authors usually made and retained another copy of their writings, possibly a draft of the finished work.

Most New Testament writings were addressed to individuals or to individual churches; several (for example, Peter's) may have been made with the intention of being circulated among a larger prescribed circle of readers.

In any case, the canonical New Testament writings were recognized as the word of God, therefore valuable, and copies were made from them soon after the time of their composition. Some of what we know of the process has been told us by church fathers; other matters we know by inference. In time there were multiple copies in circulation. And after an unknown passage of time the autographs apparently perished.



By whatever means, the Scriptures were copied and circulated throughout an ever-enlarging area. Today nearly 6,000 partial or complete manuscripts of the Greek New Testament exist. We may assume that at least that same number of other copies was made over the centuries — indeed probably a far greater number than that — but that those copies perished for one reason or another. For all practical purposes manual copying of manuscripts ceased in the 1500s after the first published printing of the Greek New Testament (1516).

What New Testament manuscript groups exist today

If we look at extant manuscripts, we see that they are not identical (see the discussion under the section “Why there are differences between manuscripts” below). They seem to fall into four individually related groups, commonly termed *texttypes*. The first of these has been termed the **Western Text**. Its member manuscripts are characterized by “free expansion, paraphrase, and alteration of previously existing words.” There are relatively few manuscripts of this type of *text*, its most famous member being manuscript D/05. (For easy reference manuscripts are catalogued by either an Arabic numeral or a numeral associated with a capital letter-symbol [Latin, Greek or, in one case, Hebrew] attached.)

The second texttype is termed the **Alexandrian Text**. It is an important group, if only for the relatively early date of its primary manuscripts. Although its proponents might deny this, it seems to be correctly characterized as *recensional*, that is, it is considered by Byzantine priority theory to be a critical revision of other manuscripts, possibly a revision of the alterations and additions of the Western Text. The most famous manuscripts in this texttype are the Aleph (Ⲁ) and B codices, both fourth century, and a number of early papyri found preserved in Egypt, some dated before AD 200. As with any texttype, the Alexandrian is not uniform; indeed its two main manuscripts differ from each other 3000 times over the course of the four Gospels alone.

The **Caesarean Text**, though distinct enough to be called that, is not generally assumed to be a serious reflection of the autographs. It appears rather to be a blend of readings from various Byzantine and Alexandrian texts.

The **Byzantine Text** reflects a large body of data, represented by numerous manuscripts, supported by many *variant readings* found in early translations and commented on by the early church fathers. Approximately 95% of all Greek manuscripts of the New Testament contain a Byzantine type of text. The manuscript center for all this was the primarily Greek-speaking world of southern Italy, Greece and most of Turkey from the fourth century until the invention of printing. (Note in passing that this is also the geographic area that possessed a majority of the original autographs of the New Testament at the beginning of the Christian era.) Evidence from this region before the fourth century is scant or lacking; this will be discussed below where objections to the Byzantine-priority model are noted.

Why there are differences between manuscripts

If we assume that there was one original text, why are there any differences at all and why the four text groupings in particular? The primary reason is that the process of copying resulted in variations from the *exemplars*, that is, the manuscripts being copied. There were two major types of variations.

First, there is inadvertent scribal error. The adage “to err is human” merely comments on what is prevalent in the copying process. However careful a scribe might be, he would make errors in his efforts to produce a faithful copy of the source text before him. Granted, there are different types of copyists. Some are more careful, whether by nature or training; some are more careless. It can be shown that New Testament copyists were overwhelmingly of the more careful variety, though they still inevitably made inadvertent copying mistakes.





To clearly understand how there can be two texts otherwise identical but for a couple of variant readings, consider the following from 1Corinthians 13 (base text NIV):

If I speak in the tongues of men and of angels, but have not love, I am only a resounding gong or a clanging cymbal. If I have the gift of prophecy and can fathom all mysteries and all knowledge, and if I have a faith that can move mountains, but have not love, I am nothing. If I give all I possess to the poor and surrender my body to the flames, but have not love, I gain nothing. Love is patient, love is kind. It does not envy, it does not boast, it is not proud. It is not rude, it is not self-seeking, it is not easily angered, it keeps no record of wrongs. Love does not delight in evil but rejoices with the truth. It always protects, always trusts, always hopes, always perseveres. Love never fails. But where there are prophecies, they will cease; where there are tongues, they will be stilled; where there is knowledge, it will pass away. For we know in part and we prophesy in part, but when perfection comes, the imperfect disappears. When I was a child, I talked like a child, I thought like a child, I reasoned like a child. When I became a man, I put childish ways behind me. Now we see but a poor reflection; then we shall see face to face. Now I know in part; then I shall know fully, even as I am fully known. And now these three remain: faith, hope and love. But the greatest of these is love.

If I speak in the tongues of men and of angels, but have not love, I am only a resounding gong or a clanging cymbal. If I have the gift of prophecy and can fathom all mysteries and all knowledge, and if I have a faith that can move mountains, but have not love, I am nothing. If I give all I possess to the poor and surrender my body that I may boast, but have not love, I gain nothing. Love is patient, love is kind. It does not envy, it does not boast, it is not proud. It is not pompous, it is not self-seeking, it is not easily angered, it keeps no record of wrongs. Love does not delight in evil but rejoices with the truth. It always protects, always trusts, always hopes, always perseveres. Love never fails. But where there are prophecies, they will cease; where there are tongues, they will be stilled; where there is knowledge, it will pass away. Now we know in part and we prophesy in part, but when perfection comes, the imperfect disappears. When I was a child, I talked like a child, I thought like a child, I reasoned like a child. When I became a man, I put childish ways behind me. Now we see but a poor reflection; then we shall see face to face. Now I know in part; then I shall know fully, even as I am fully known. And now these three remain: faith, hope and love. But the greatest of these is love.

In the preceding display the left column is the NIV text, and the right column is the same except for three different readings. The differences are underlined in both texts. Their ultimate source among the manuscripts need not concern us here.

If you doubt whether scribes easily made errors in the copying process, try to copy several chapters of the New Testament for yourself. Then go back and check the correspondence between your printed text and your effort at copying. You might also try to copy the same several chapters from a Greek text (or any other language that you aren't perfectly at home in). The incidence of errors from your efforts will be shown to have increased greatly when copying from a text in a language other than your own. This is important, for many scribes through the centuries were not comfortable in Greek.

The second type of variation is that made intentionally. This type of variation is logically either of two kinds. First, there were intentional efforts to correct errors (whether real or perceived) introduced into previously copied manuscripts. Potentially, this effort corrected inadvertent errors in the text being copied (this effort to correct is easily demonstrated) and may have even "improved on" perceived original rough expressions (such efforts to improve are theoretically possible, but ever so much more difficult to demonstrate). Second, there were deliberate alterations in the copying process, to include harmonization, religious "improvement," and finally doctrinal alteration.

Harmonization involves, especially in the *Synoptic Gospels*, making a given passage more like a known parallel passage. We have numerous instances, of course, in which parallel synoptic passages have identical wording, with no variants. So when one of two variants is identical with its synoptic parallel, we should not automatically jump to the conclusion that the other is original and this one was a harmonization. Religious "improvement" is the process of adding to the text to make it more devout; thus, for example, a scribe might add "Lord" to a text containing simply "Jesus Christ." Finally, doctrinal alteration involves just what it says, namely, adjusting the text to make its reading more fully support a particular doctrinal position.





Before we move on, it is important to stress by way of summary that of the thousands of existing New Testament manuscripts no two are completely identical, even while nearly the whole of the text of all manuscripts is seen to be 94% or more identical. Since there was only one autograph or original writing, the fact that the many copies made are not identical means that at various places in each there are copying errors. The major task of *textual criticism* is to determine what the original looked like from the copies that exist — all of them showing differences at various points, most of which originated as copying errors.

The two main texttypes

Today there are two main schools of thought in the matter of the identity of the New Testament text. One school of thought we can call **Byzantine priority**, i.e., that the text found in the vast bulk of New Testament manuscripts is most likely to represent the original. The other school of thought we can call the **eclectic** approach, which picks and chooses each reading based on a set of principles. In practice, the eclectic approach, which has dominated the scholarly world of New Testament textual criticism for the past century or so, heavily favors the manuscripts of the Alexandrian texttype. In this essay the terms “Alexandrian texttype” and “eclectic” are largely interchangeable, though each retains its own lexical intent.

The Byzantine Text is often maligned for no other reason than that in large part it corresponds with the long-belittled so-called *Textus Receptus*. (The *Textus Receptus* is admittedly a poor representative of the Byzantine Text. In fact the *Textus Receptus* and the Byzantine *Textform* of today differ in more than 1800 places throughout the text of the New Testament. The differences between them are extensive enough to invalidate automatically extending criticism of the one to the other.) Apart from that largely irrelevant objection, the Byzantine Text is a reality that must be accounted for. Those that would seek to fault it must at the very least speak to its origin, rise and near dominance within the *stream of textual transmission* from at least the fourth century to the invention of printing. Indeed, since observable facts are best discussed in terms of an underlying theory, critics of the Byzantine Text need to address its dominance throughout history by developing a theory of textual transmission of their own.

Currently, objections raised for their own sake and not based on a true deficiency are the best that can be raised against the Byzantine Text, including two in particular. The first is an objection first put forth by Westcott and Hort some 125 years ago. They suggested that the Byzantine Text arose as a critical revision in the second half of the fourth century, perhaps commissioned by a church council, endorsed by the local churches, and then that went on to gain increasing, then near-universal, acceptance. The problem with this explanation is that there is not a shred of historical evidence to support it. The second objection (more fashionable today) hypothesizes that the Byzantine Text is the result of a process, but we find that the suggested details are at best sketchy and insufficient to explain the data. Both of these objections are based on the fact that we have no early (pre-fourth century) Byzantine manuscripts. But this fact is far from proof for or against any text type. Remember that we have only a very small number of pre-fourth century manuscripts of **any** type — and of those, only **one** manuscript (P⁷⁵) is thoroughly of one particular texttype (Alexandrian).

Here is an analogy: Suppose that in a particular rural county in Kansas we examine boxes of old files in the back room of the county clerk’s office. We find almost 6,000 receipts for hunting licenses. The vast majority of these licenses are for deer hunting, only a few hundred are for quail hunting. Almost all of these licenses are dated after 1950. Only a small number of pre-1950 licenses can be found, off in a corner somewhere — apparently someone tossed out most of the old files long ago. None of the pre-1950 licenses are for deer hunting; some are just general hunting licenses, a few are quail-hunting licenses. Would it be logical to conclude, “No deer hunting licenses were issued in this county prior to 1950”? Certainly not. Most of the pre-1950 files no longer exist, and the few that we have are insufficient to draw conclusions from. It is quite likely that deer hunting licenses were issued prior to 1950, but that





the old records are no longer available. Indeed, based on the post-1950 trends, it is logical to assume that in pre-1950 times more deer-hunting licenses were issued than quail-hunting licenses.

Perhaps the problem of acceptance is partly to be understood in this way: the position that the Byzantine Texttype is indeed the best representation of the autographs (Byzantine priority theory) is so straightforward as to be thought too simple by its critics. On the contrary, taking it as the explanation for the data found in the manuscripts more greatly simplifies the approach to the issues and problems before us than the explanation that advocates the Alexandrian Text.

The other texttype is the Alexandrian. Unlike the manuscripts of the Byzantine Texttype, which are found spread across the Aegean homeland of traditional Greek language and usage and also across the centuries, the Alexandrian is represented by manuscripts that come mainly from bilingual (Greek and Coptic) Egypt and from the early centuries of the Christian era only.

No single manuscript fully represents a single texttype — whether Byzantine or Alexandrian. Therefore there has to be an evaluation of the existing manuscript evidence based on some theory. For both the Byzantine and the Alexandrian texttypes there must be an underlying theory that motivates the choices made in the course of determining which texttype best represents the autographs. For the Byzantine Texttype the theory generally begins by tracing the history of how texts were copied and distributed (transmissional history). At every point these considerations are given priority and carefully weighed. Following this, the data from external sources are evaluated, and finally internal considerations are weighed. If at any point variant readings are found, the above considerations are applied by the theory in order to choose among the variants.

The eclectic approach to recovering the original text

On the other hand, preference for the Alexandrian Texttype is based on an apparent predisposition to favor a certain small selection of older manuscripts sharing a generally common minority (Alexandrian) texttype. Its advocates really have little or no theory of transmission of the text on which to base their decisions. What is certain is the disposition of the Alexandrian partisans to favor the text generally found in the Alexandrian manuscripts and to disfavor that found in the Byzantine manuscripts. In particular, scholars that practice the eclectic approach usually cite these two arguments to explain why they favor Alexandrian manuscripts: 1) The Alexandrian manuscripts are mostly from the early centuries of the Christian era and thus closer to the original autographs — the clear assumption is that, all other things being equal, an early manuscript is more likely to better represent the original than a later manuscript. 2) Also, these scholars favor Alexandrian manuscripts because of their assumption that the huge bulk of Byzantine manuscripts is the product of “mass production” by professional scribes of a smoothed-out officially-sanctioned church text. Both these points will be addressed below.

Moreover, advocates of the Alexandrian Texttype seem to assume some sort of radical textual dislocation very early on that left no single manuscript untouched. In their view, there is no single surviving text or texttype that can be used authoritatively to guide their decisions about the texts. Rather, the shredded remnants, as they might be called, must be pieced together on an item-by-item basis. The result of such an improvised process is a text that in some places of variation no longer represents what is found in any existing manuscript.

In the world of eclectic approach we remain dependent on the wit and skill of the textual critic. The decisions of the editorial committee are taken as nearly final. According to the Alexandrian Texttype practitioners, there is no basis in history for determining the original text; our own learned judgment is the ultimate authority. The analysis itself decides what theoretically was in the original. The testimony of countless generations of scribes and readers from the autographs through the present is simply discounted. To see how bad the result really is consider the following.





In a single given verse of the text of the Greek New Testament, assume that two competing variant readings are identified, A and B. The original was not A and B, but A or B (or some other reading). Reading A is supported by manuscripts *a, b, c, d, e, f*, whereas reading B is supported by manuscripts *g, h, i, j, k, l*. There is also in that same verse another set of competing readings, X and Y, supported, respectively, by manuscripts *k, l, m, n, o, p* and *a, b, g, h, s, t*. Our deliberations and discussions conclude that readings A and X are original. What does this say about the presumed autograph? And what does this say about our method of determining the original text/reading? It is a method that has produced a text found in not a single existing manuscript!

To underline this important point in the most transparent terms, consider this analogy: Suppose that five people have copied a list of twenty items. Now the original list has gone missing, and we must reconstruct it from the five copies. Persons A, B, and C produced identical lists. Person D's copy, however, differs from the copies of A, B and C in one item, let's say item 12. Person E's copy also differs from the copies of A, B and C in one item, but it is item 18. The eclectic approach may conclude that the original list from which all five people copied agreed with person D on item 12 and agreed with person E on item 18. But in fact that approach to reconstruction produces a list that is found in **none** of the five existing copies. What does this say about our method of determining the original reading?

The choices made among the readings for this verse (two paragraphs above), which now produce a combined or consecutive reading, are not found in any manuscript available to us, for the simple reason that the supporting manuscripts of A and X are mutually exclusive sets — in other words, no manuscripts contain both reading A and reading X. The committee has determined a hypothetical form of this verse, as they think the original manuscript read, that is at complete variance with what (so far as anyone can determine) any reader of the same verse through nearly twenty centuries ever laid eyes on. With regard to the other logical possible readings, namely, AY, BX, and BY, each has support from at least two manuscripts in our contrived example.

Now we illustrate with actual data. In John 5.2 there are two sets of variant readings found in existing manuscripts. First there are roughly seven readings that reflect various combinations on the phrase "(in/at) the sheep gate a pool called"; second there are six variants with respect to the name of the place: Belzetha, Bethesda, Bethsaida, Bedsaida, Bedsaidan, Bethzatha. The combined and consecutive reading of the eclectic approach is "There is in Jerusalem *at the sheep gate a pool called* in Hebrew *Bethzatha*, having five porticoes." The problem with this is that in determining the first reading, which incidentally follows the Byzantine Text, they followed the witness of a set of manuscripts that is mutually exclusive with those supporting their second-reading choice, Bethzatha. The simple fact is that their combined reading does not occur in any manuscript known to us. The Byzantine Text, on the other hand, reads "There is in Jerusalem *at the sheep gate a pool called* in Hebrew *Bethesda*, having five porticoes"; this is the text of John 5.2 as it occurs in many existing manuscripts and demonstrates the historical and transmissional integrity of the text.

What is shocking is that there are at least 105 single verses in the modern eclectic text of the Greek New Testament (i.e., the UBS and Nestle-Aland Greek texts) that, as in our illustrated choice AX above for consecutive variant readings within single verses, are demonstrably without any tangible manuscript support whatsoever. These verses therefore are "contrived." When two consecutive verses are examined — and then three — the resultant condition of "no supporting manuscript evidence" will continue to balloon — and that geometrically! (When we actually take two consecutive verses, the total jumps to 315.) The sad conclusion is that the predominantly Alexandrian reconstruction produced by the eclectic method and found in published Greek texts today *never* existed for *any* reader in the history of the Christian era. That is to say, "in short stretches of text, the best modern eclectic texts simply have *no* proven existence within transmissional history, and their claim to represent the autograph or the closest approximation thereunto cannot be substantiated from the extant manuscript, versional or patristic [see glossary] data."





Here is another way of stating the error that the eclectic approach makes in its attempt to reconstruct the original text. The textual critic that follows the eclectic method usually rejects the Byzantine texttype as “late” and favors the Alexandrian texttype. In rejecting the vast majority of manuscripts, all of which agree with each other, he is forced to pick and choose readings from what is left — manuscripts that vary widely from each other, even at the level of a single verse, clause or sentence. And after carefully applying his blend of rules for considering internal and external evidence, he is left with yet another text that lacks a pattern of agreement with any manuscript in existence.

Having first rejected the reading of the majority, he has now gone on to even reject the reading of every other manuscript, including those in his favored Alexandrian texttype! Such is the text found in the modern eclectic edition.

The basics of Byzantine priority

Just what is the essence of the method giving priority to the Byzantine Texttype? It is best stated by Hort, though he championed the Alexandrian Texttype based on various anti-Byzantine presuppositions: “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 [a minority]” [Westcott and Hort, Introduction, 45].

In the nonbiblical realm of scholarship, this statement is considered a basic assumption. It is assumed that a normal means of textual spread underlies the dissemination of all handwritten documents, whether secular or sacred. If we look at the writings of one secular and classic author (Homer), we find the same situation among the surviving manuscripts of his works. Indeed, there are three broad categories for these manuscripts: shorter text, longer text and in-between text.

Manuscripts containing the shorter texts of Homer are taken to show “Alexandrian critical know-how and scholarly revision.” (Remember, Alexandria was the great library center of the world of that time, second only to Rome in general importance.) And this shortening is seen in the Alexandrian Texttype of the Greek New Testament manuscripts as well: they clearly show editorial processes that (among other things) shortened the text being revised.

The longer texts are noted as reflecting popular expansion and improvement by scribes. Note the similarity of Homeric manuscripts of this kind with those roughly fitting the Western Texttype among New Testament manuscripts.

Finally, between the sidelines is the playing field where texts of a “medium” or “vulgate” nature are found. As with secular texts, so with sacred: the preponderance of manuscripts is of this kind. Without any imposed prejudice that would favor another conclusion, it appears that “normal scribal activity and transmissional continuity would preserve in most manuscripts ‘not only a very ancient text, but a very pure line of very ancient text’” (quote within single quotation marks from Hort).

The Byzantine-priority position is often dismissed as being only a counting of noses. But a simple mechanical counting is far from what this position actually does. Indeed there are many cases (particularly in John 7.53-8.11 and in Revelation) where the testimony of the manuscripts is divided among not just two, but several variant readings; there is no majority reading. What would nose counters do then? No, Byzantine-priority theory is not correctly characterized in this way.

Rather the principles (discussed below) that lead to the Byzantine-priority position “reflect a reasoned *transmissionalism* [a theory of transmissional development applied to variant readings that] evaluates internal and external evidence in the light of transmissional probabilities.” **Internal evidence** includes those items that yield an “intrinsic probability quotient” for the analyst, that is, answers to questions asked regarding the scribe, and what he probably would have done, whether intentionally or unintentionally, in





any given case. **External evidence** is the testimony of the manuscripts, early translations and comments of the fathers, all weighed to determine which reading has the best outside support.

Careful attention is given to the habits of scribes in preserving, altering or otherwise corrupting the text. This approach also recognizes that transmissional development does in fact lead to family and texttype groupings. Further, it emphasizes that the text in its general integrity has been maintained throughout transmissional history.

How we evaluate internal evidence

There is a large overlap between the principles used by those that practice Byzantine-priority theory and those that support the predominantly Alexandrian eclectic approach. However, the Byzantine approach rejects one of the principles of the eclectic approach, namely, that the shorter reading is to be favored. Whereas it might seem that the Byzantine outlook rejects this principle mainly to strengthen its case against the shorter Alexandrian Texttype approach (a difference of some two thousand words over the entire Greek New Testament, roughly 138,000 words in the Alexandrian Texttype versus 140,000 in the Byzantine Texttype), that is not the case. The reason it is rejected is simply that there is compelling evidence that errors made by the scribes tend to make the text shorter in far more cases than are alleged for intentional scribal expansion. Current and extensive studies regarding scribal habits provide clear indications of that fact. (See also principle eight below.) We will first cite the principles of internal evidence (IE) accepted by the Byzantine-priority theory. (These principles, in italics below, are taken directly from Robinson.)

IE1. *Prefer the reading that is most likely to have given rise to all others within a variant unit.* This principle is accepted by both schools of thought, but with this difference: The Alexandrian, eclectic approach uses this principle for each and every isolated variant. The Byzantine-priority theory, on the other hand, demands to know not only how the several competing variants could have derived from the first, but also how such a reading could have happened when a text was copied in relationship to neighboring variant units.

Specifically, note the following example from John 9.4. The modern eclectic critical text, found in the UBS Greek text, reads in translation: “WE must work the works of the one having sent ME”; the Byzantine text reads: “I must work the works of the one having sent ME.”

The support for the combinations *WE/ME*, *I/ME* and *WE/US* is as follows:

- *WE...ME* = B 070;
- *I...ME* = Byz ^a A C Θ Ψ f1 f13 33 lat syr;
- *WE...US* = ⁶⁶ ⁷⁵ ^a* L W bo;
- *I...US* — this combination is not attested.

It is not transmissionally logical that the reading found only in two manuscripts (B and 070) out of all the thousands of witnesses — manuscripts, early translations, commentaries of fathers — could have given rise to all other readings. It is far more likely that the Byzantine (Byz) reading (*I...ME*) is original, and was changed by Alexandrian scribes into a hortatory *WE...US*, which then became muddled in B 070 into the more problematic *WE...ME*. (The abbreviations for various manuscripts used above should not intimidate the reader. The point illustrated may have its impact without his knowing specific manuscript identities, which can be found in many critical Greek New Testaments.)





IE2. *The reading that would be more difficult for a scribe to create is to be preferred.* This principle is based on the assumption that a scribe would not choose to make a reading more difficult than a simpler one in the text he was copying from. That this happens accidentally cannot be denied. Considering the overall transmissional integrity of the text, the principle under consideration here needs to be modified as follows: *the more difficult reading is to be preferred when such is found in a larger transmissional body of witnesses rather than when such is limited to a single witness or an interrelated minority group.*

This principle is illustrated in Luke 6.1, where the Byzantine text reads EN SABBATW DEUTEROPRWTW (“on the second-first sabbath”), while the eclectic critical text reads only EN SABBATW (“on a Sabbath”). The reading of the eclectic text avoids the difficulty regarding the interpretation of “second-first,” — the meaning of which no one today is certain, but it is a word that simply could not have been invented without reason, or produced by accident. The following discussion pushes the notion of “a modest explanation for the layman” to the limits, but will be found instructive to the reader that perseveres!

Metzger’s *Textual Commentary* can only “resolve” the problem of the Byzantine reading by a highly convoluted process, which is hardly convincing: (1) “a Sabbath” was original; (2) a later scribe saw there was *another* Sabbath described at 6.6, so he chose to insert “first” before “Sabbath”; (3) a *second* scribe remembered [!] that something relating to “Sabbaths” (plural!) occurred earlier at 4.31, but he took that reference as if it were singular, and thus considered 4.31 the “first” Sabbath, but this then made 6.1 the “second Sabbath”; (4) this scribe accordingly added the word “second” in the *margin* and placed dots over the word “first” in the main text to indicate that it should be removed; (5) following this, a *third* scribe copying the text duly inserted the word “second” but failed to notice the dots over the word “first,” and this left the nonsensical reading DEUTERW PRWTW (“on a second on a first”) as the main text; (6) finally, a *fourth* scribe “resolved” the difficulty by combining the two-word form into the single DEUTEROPRWTW, changing the spelling in the process and being unconcerned as to whether the reading made any sense (had clarity been a concern, he would have eliminated either DEUTERW or PRWTW or both); (7) this one manuscript then became the parent of virtually all remaining manuscripts transmitted throughout history, with almost no correction whatever being made to “restore” the original, even if no one could comprehend the meaning of “second-first.”

The question of course is whether such a complex chain of events is at all likely, and even if so, whether on transmissional grounds anyone would claim that a single “corrupted” manuscript would even have become the parent of nearly all other manuscripts without massive attempts at correction of such an extremely difficult reading. Further against this hypothesis, there is no documentary evidence of any of the intervening steps along the chain.

By comparison the simpler (Byzantine-priority) solution is obvious: regardless of what “second-first” may have meant in the first century, the term was clearly difficult, and the meaning became unknown by the mid-second century. Thus, since it was the “more difficult” reading, a small number of scribes (mostly Egyptian or copying an Alexandrian type of text) simply omitted the term (P⁴ & B L W f1 33 579 1241 2542 pc it sy-p, sy-hmg sa bo-pt).

IE3. *Readings that conform to the known style, vocabulary and syntax of the original author are to be preferred.* This assumes that, all things being equal, scribes would be more likely accidentally to alter the style and vocabulary of the author they are copying than deliberately to conform to it. This principle must be used with caution, however. For example, OUN “therefore” is characteristic of John’s style. But DE “but” is used more frequently in John than OUN. So when two manuscripts vary between OUN and DE, this principle cannot tell us which to choose; either reading is characteristically Johannine. Historical-transmissional considerations may lend more weight to the manuscripts that read one way over those that read the other.





IE4. *Readings that clearly harmonize or adapt the wording of one passage to another are to be rejected.* While harmonization can occur during copying, its frequency is overemphasized. Proponents of the eclectic position seem quick to label a Byzantine reading as a “clear case of harmonization”; they assume that scribes copying Byzantine texts made a normal practice of harmonizing. But why would only Byzantine scribes harmonize? If this were a normal scribal habit, would it not be expected of all scribes whatever kind of manuscript stream being copied? In fact, actual cases of harmonization are relatively infrequent, appearing only sporadically. Besides, we have numerous instances of word-for-word agreement between parallel passages in the Gospels, with neither text in doubt. So when there is a variant, why assume that the variant that agrees with the parallel passage in another Gospel is a scribal harmonization and not due to the writer? Again, the *apparatuses* show us that harmonization or assimilation simply did not occur to any great extent. This again demonstrates that the history of transmission must be considered for all cases of possible harmonization.

IE5. *Readings that could be viewed as reflecting the piety of the scribe or as reflecting religiously motivated expansion and alteration are considered secondary.* (By “secondary” we mean that the reading was not in the original manuscript but was created by scribes.) With variants of this type, it is naïve to pick a shorter reading as being original, calling all others pious expansions. In the case of 1Corinthians 5.5, for example, the Byzantine Text reads “the day of THE LORD JESUS”; this reading is supported by the large number of Byzantine manuscripts, as well as the Alexandrian codex κ . The eclectic text, however, has chosen the shortest reading: “the day of THE LORD”; this reading is found in the Alexandrian manuscripts \mathfrak{P}^{46} and B, and a few other manuscripts. The reading with the fullest expansion is “the day of OUR LORD JESUS CHRIST”; this long reading (with or without the “OUR”) is found in the uncial A plus a few other manuscripts. Transcriptional probabilities point clearly to the middle form of the Byzantine Text as the reading from which all others are derived: “the day of THE LORD JESUS.” Critics of the Byzantine text accuse it of having “expanded readings that reflect the piety of the scribe”; but these critics should explain why the Byzantine text of 1Corinthians 5.5 contains a middle form (“the day of THE LORD JESUS”) — why did the Byzantine scribes not expand the text to “OUR LORD JESUS CHRIST”? In fact, there are numerous “longer forms” of the divine names throughout the Greek New Testament that show *no* variation whatsoever; obviously, all these were original.

IE6. *The primary evaluation of readings should be based on transcriptional probability.* This principle is as old as Westcott and Hort, who first published their eclectic Greek New Testament in 1881, but it is inconsistently applied. Put simply, a single error or deliberate alteration is unlikely to be perpetuated in any quantity. The many singular readings that were *not* copied, but died with the manuscript containing them, or at most were limited to only a few manuscripts, are proof that most cases of deliberate alteration or inadvertent error were *not* incorporated/perpetuated by later scribes into the various texts they copied.

IE7. *Transcriptional error rather than deliberate alteration is more likely to be the ultimate source of many sensible variants.* Many variant readings are the result of transcriptional errors. This includes all purely nonsense readings, but also many sensible readings arising from the omission of a letter, a syllable or words, on the one hand, and from *haplography*, *dittography*, *homoioteleuton* and other similar errors, on the other hand.

The intent of this principle is that we should seek a transcriptional explanation for a variant reading *before* resorting to an explanation that assumes deliberate alteration.

IE8. *Neither the shorter nor the longer reading is to be preferred.* The hypothesis of eclectic textual critics is that scribes have a strong tendency to expand, whether in sacred names or by combining dissimilar narratives. And yet habits of the scribes as evidenced in the existing data simply do not support such a hypothesis. Had their habits been such, divine titles should have been expanded far too frequently, parallel passages would have been more extensively harmonized and a far more *conflated* text would have resulted.





Such is not the case. That is, this primary working assumption of eclectic critics is refuted, an assumption that inherently favors the Alexandrian Text. At the same time, however, an assumption that the longer reading is to be preferred — an assumption that favors the Western Text — is equally without support.

How we evaluate external evidence

The Byzantine-priority method is not primarily concerned with how many manuscripts support a given reading, but with how these manuscripts interact over an extended portion of text (this involves what should be termed “transmissional considerations”). A textual sequence of readings, each supported by a reasonably significant number of manuscripts, suggests transmissional consistency. Readings that lack such a level of sequential transmissional support would therefore be suspect, and the sequence involved would not likely reflect the original form of the text for that portion of Scripture. We will now look at several principles of external evidence (EE).

EE1. *The quantity of preserved evidence for the text of the New Testament rules out pure guesswork (conjectural emendation [see glossary]) regarding how a supposedly erroneous text originally might have been written.* Only presuming that the original wording of the text totally disappeared from among all existing witnesses (manuscripts, versions, church-father commentaries) can justify the use of conjectural emendation. Since the New Testament is more widely attested than any other piece of hand-copied literature from ancient times, there is no justification for such an approach when its text is being established.

EE2. *Readings that appear only sporadically within transmissional history are suspect.* The root (the autograph) is expected to produce the branches (the transmissional outcome). This is the normal way a text is transmitted. In their growth and spread all subsequent generations of copies should deeply reflect the autographs.

- a. *A reading preserved in only a single manuscript, early translation or church-father commentary is highly suspect.* It is unreasonable to think that all but one manuscript have strayed from the original wording.
- b. *Readings preserved in only a few manuscripts are suspect.* The fewer the texts containing a given reading, the more suspect it is of not being original. Even if the reading is found in several manuscripts that form a small group or text type, that reading is still suspect, so long as that group of manuscripts or texttype remains smaller than a larger texttype.

EE3. *Variety of testimony is highly regarded.* In other words, if evidence for a certain reading is found in diverse kinds of sources, that reading is more likely original than if evidence for that reading is found only in one kind of source.

This principle has two parts, neither sufficient by itself to establish the text, but either of which lends support to a given reading.

- a. *A reading supported by various early translations and fathers demonstrates a wider variety of support than a reading lacking such.* The greater the variety of support, the better. But it is not sufficient that a reading is supported by various early translations and early fathers. Unless such a reading is evidenced in the **Greek** manuscripts, that reading is secondary.
- b. *Among Greek manuscripts, a reading found in manuscripts of differing texttypes is more strongly supported than that reading that is localized to a single texttype or family group.* The Byzantine Texttype benefits most frequently from this principle: that is, there are far more cases where a reading is found in both Alexandrian and Byzantine texts, or in both Western and Byzantine texts, than cases where a reading is found in both Alexandrian and Western texts but not in Byzantine texts.





EE4. *Wherever possible, the raw number of manuscripts should be intelligently reduced.* It is possible to show genealogical relationships among some manuscripts. For counting purposes, each group or family should be counted as a unit. But it is not legitimate to impose a genealogy that presumes an entire texttype to be a single unit for counting purposes (as Byzantine-Texttype opponents tend to assume for it), while treating manuscripts of other texttypes as valuable individual witnesses.

The texttype that is assumed to be original on the basis of transmissional factors can be termed the “original *textform*”, i.e., the exact wording of the original autograph as accurately as the evidence can determine it; from this textform all texttypes are derived. On this basis, the Byzantine Texttype alone becomes truly worthy of the designation “original textform.” All other competing forms of the text reflect (in order of declining size) “texttypes,” or “subtypes” or “families of manuscripts,” each of which ultimately developed transmissionally out of the single original textform.

EE5. *Manuscripts still need to be “weighed” and not merely counted.* Though principle four above is true, all manuscripts still need to be categorized regarding their text-critical value, or “weight.” Basic to “weight” is how reliable a manuscript might be transcriptionally. For example, a later manuscript may preserve a much earlier form of the text; a well-copied manuscript may preserve an inferior form of the text; a poorly copied manuscript may preserve an otherwise superior form of the text. Therefore, a determination of individual scribal habits becomes very important.

A scribe that shows a tendency for skipping words or phrases (haplography) is of no value in determining whether inclusion or omission of a questionable word, phrase or verse was in the original, even though the same scribe may be accurate in relation to other kinds of variation, such as substitution or transposition. Though much importance historically has been attached to this principle, relatively little work has been done regarding the evaluation of the habits of individual scribes. More often than not, this principle is cited solely to discredit the Byzantine Textform, and nothing more.

EE6. *It is important to seek out readings with demonstrable antiquity.* The age of a **manuscript** is not as significant as the age of the **text** that was copied on to that manuscript. Keep in mind that the age of a text is always older than the manuscript containing it — after all, the job of the scribe was to copy an old manuscript and thus make a newer one — and the age of a text is at least as old as the readings that are found in the text. Still, it is important to seek out the earliest known authentication for a particular reading within the existing evidence. When there is no early support for a given reading, it *may* be suspect; however, merely because a reading can be assigned an “early” date, it does *not* follow that it is any more likely to be “original” based on that fact alone.

How does one determine what is “early” and what “late”? In the nineteenth century, Westcott and Hort called many readings secondary (i.e., nonoriginal) and late. But manuscripts discovered since the time of Westcott and Hort — in particular among Egyptian papyri — proved many of those “late” readings to be extremely early. It now is well accepted that most, if not all, sensible readings very likely were in existence before the year AD 200; thus, we need to be careful when claiming a lack of antiquity for a given reading merely on the basis of the late date of manuscripts currently at our disposal containing that reading.

It is known and well accepted that some “late” manuscripts — even some manuscripts produced in the minuscule era, which began in the ninth century — have preserved early non-Byzantine readings or non-Byzantine types of texts. Is it then not reasonable to assume that some minuscule manuscripts containing a Byzantine type of text may also reflect a similar early character? If that is granted, we need to ask where we should draw the line of demarcation. There are valid reasons for considering all manuscripts extending into the late tenth or early eleventh century as containing an “early” text type with “early” readings.





How can this be? There were two major transitions in the history of New Testament textual transmission (we call them “copying revolutions”) that may account for the fact that later manuscripts reflect much older texts. At these two points numerous ancient manuscripts were subjected to massive recopying efforts that replaced *en masse* their source manuscripts.

The first of these occurred when Emperor Constantine legitimized Christianity in AD 313 with the Edict of Milan. Overnight the church went from a persecuted minority to an approved entity with government sponsorship. Not coincidentally, the change of writing material from cheap and fragile papyrus to costly and durable vellum (noted earlier) increased dramatically at this time. Suddenly there was a new demand for the Scriptures and on vellum at that. This resulted in a frenzy of copying activity. It is likely that some churches made careful copies of their New Testament manuscripts on to expensive vellum, and then respectfully destroyed their old tattered papyrus manuscripts.

The significance of this first copying revolution is this: There are some uncial manuscripts that have a very similar text and were almost certainly copied from a common ancestor; E, F, G and H comprise one such a set of closely related uncials, and another such set is S, U, V and Ω . There are also some relatively independent uncial manuscripts that were made as late as the ninth century. It is likely that all of these uncials were copied from early papyrus manuscripts. There is no more reason to doubt this than to doubt that the uncials Aleph (\aleph) and B were copied from early papyrus manuscripts. (\aleph and B are two old uncial manuscripts that are highly preferred by eclectic textual critics of today.) If so, then *all* vellum uncials should be utilized when attempting to restore the original text of the New Testament.

The second copying revolution occurred in the ninth century (AD 800-900), when handwriting switched rapidly from uncial to minuscule script. Within 150 years uncial writing ceased to exist among continuous-text New Testament manuscripts. The impact of this second copying revolution was similar to the first: within a short span of years, uncial manuscripts of far earlier date were copied into the new popular script and then destroyed. This development, coupled with a revival of learning in the Byzantine world, caused scholars to seek a better text by going back to more ancient manuscripts. That this actually occurred is demonstrated by the existence of various minuscule manuscripts with acknowledged “early” (generally Alexandrian) types of text.

EE7. *The concept of a single “best” manuscript or “best” small group of manuscripts is unlikely to have transmissional evidence in its favor.* Some poorly copied manuscripts can contain “good” texts, and carefully copied manuscripts can contain inferior texts. Within various texttypes some manuscripts will be more representative than others. All that being true, transmissional considerations tend to rule out the concept that a single manuscript or a small group of manuscripts holds a superior status. This principle holds true even for the set of manuscripts that support the Byzantine Textform. No one manuscript can be considered best.

EE8. *The concept that we should rely exclusively on the oldest manuscripts or witnesses is flawed on the basis of transmissional considerations.* That the “oldest is best” is a fallacious principle, since later manuscripts may well preserve a text more ancient than that of earlier witnesses.

EE9. *Transmissional considerations coupled with internal principles point to the Byzantine Textform as a significant force in the history of transmission.* The Byzantine Textform is not, before analysis, asserted to be the original form of the text or even the superior texttype. The conclusion follows only as a logical deduction from internal and external considerations regarding transmissional probabilities as viewed in an historical framework on a parallel with what is recognized in the transmission of other ancient works of antiquity.

“Note that the Byzantine-priority hypothesis can do nothing to resolve the many cases where the external evidence is divided and where no reading clearly dominates [the transmissional tradition]. In such cases, internal principles coupled with transmissional probabilities must be invoked to determine





the strongest reading. Similarly, in many cases internal principles offer no clear decision, and external principles must take a leading role. Cases also exist where the manuscripts are divided and where internal evidence is not determinative, in which cases a reasonable scholarly estimate is the best that one can expect. [Note in passing, however, while there are cases where the reading of the original text is difficult to determine, almost all such cases concern minor details, and none significantly affects the basic integrity of the New Testament text as we have received it.]

“The primary rules for balancing internal and external evidence are simple, and are ordered in accordance with known facts regarding scribal habits: (1) one should evaluate [variant] readings with the intention of discovering [what in the process of transcribing would have caused an accidental variation]; (2) readings [also] should be considered in the light of possible intentional alteration; (3) finally, [competing units of variant readings] must be evaluated from a transmissional-historical perspective to confirm or modify preliminary assessments.

“The rigorous application of this methodology will lead to valid conclusions established on a sound transmissional basis. [This agrees] with what we [find by studying] scribal habits and the extant manuscript evidence [when] considered in [the] light of [the] transmissional process.”

Some objections to the theory of Byzantine priority addressed

This elaboration of ideas underlying the Byzantine Textform necessarily supports that textform, and argues strongly for its transmissional priority over that of other texttypes. Yet there are some objections (OBJ) to the Byzantine-priority theory that should be discussed.

OBJ1. *There are no early Byzantine manuscripts prior to the fourth century.* There are several responses to this objection, the combination of which makes a strong reply.

- a. For classical works it has been noted that “the possibility exists that the extant copies (when few) do not accurately represent the original proportion [of variational types of the text]” [Bowers, *Bibliography*, 75]. Why not also for biblical texts? It is not only that no support for the Byzantine Texttype can be shown before the fourth century: the supporting manuscripts of the Alexandrian Texttype number only sixty-three papyri from those early years, and many of these are extremely fragmentary. Most of the more than 5000 manuscripts applicable to reconstructing the original form of the text date from later centuries.
- b. The two copying revolutions noted above seriously disrupted the transmissional stream. In the first revolution (fourth century), preexisting papyrus manuscripts were destroyed or otherwise abandoned. Similarly, in the second revolution, the need to preserve uncial manuscripts was effectively eliminated once minuscule copies had been made. No manuscript is inherently preferable just because of its age, material or script, particularly when the genealogically independent early minuscules appear to stem from now-lost independent uncials, which themselves appear to stem from independent early papyri. The fact that there are no early pre-fourth-century Byzantine manuscripts should not be taken as too problematical.
- c. The local form of the text found in Egypt must be considered just that. It strains the imagination to suggest a situation in which the Egyptian Alexandrian text predominated in the primarily Greek-speaking regions of the empire during the first four centuries and then disappeared almost entirely. On the other hand, it is far easier to maintain that the Byzantine manuscripts, which dominate all later eras throughout the Byzantine Empire, must have descended from earlier manuscripts in that region, now lost due to the deteriorating effects of climatic conditions outside the deserts of Egypt.





- d. Most modern eclectics seem to assume an early dominance of a *non-Byzantine* text in those areas that later became the strongholds of Byzantine support. Byzantine advocates, in contrast, claim that later dominance as strong evidence for an earlier dominance in that same region. As texts spread geographically from their point of origin, closeness to “home” may have provided some built-in control regarding the integrity of the text. If there was any question about a certain reading, it was easy to check with the congregation that still had the original manuscript as long as one were not too far from that congregation, but with increasing distance this became increasingly difficult. Increasing distance would most likely loosen that integrity because of a greater likelihood of localized alteration in more remote regions. If so, the primarily Greek-speaking region during the period of “geographical silence” would be expected to retain a purer text — a text that in its New Testament origins primarily began to be transmitted from that region, whether due to localized authorship or localized recipients in that region and would have been carefully maintained there, even through the silent years — and then that text would show itself as clearly dominant once the solid evidence from that region had appeared. That text is the Byzantine Text.
- e. By analogy to the source-theories of the Synoptic Gospels, just as the so-called Q document (meaning ‘source’) is argued for strongly, though not a shred of evidence for its existence has ever come to light, why should virtually the same scholars adamantly deny the pre-fourth-century existence of the Byzantine Textform, even though there exists far more abundant evidence than for Q? Presuppositions and biases do not die easily.
- f. Codex Vaticanus (identified as B or 03 in all references/apparatuses) contains a text that is of the Alexandrian Texttype and dates from the fourth century. Prior to the discovery of the third century papyrus P⁷⁵ in 1955, there was no certain proof that the Alexandrian Text existed before the fourth century. But then, there it was. Similarly, opponents of Byzantine Textform originality cannot rule out the possibility of a similar find of a second or third-century Byzantine manuscript in the sands of Egypt. Yet, should such a find be forthcoming, it would probably still get the usual and expected reaction from Alexandrian-based textual critics, as only “containing” more “Byzantine-like” readings than other early documents, without actually being “Byzantine.”

OBJ2. *Major disruptions in transmissional history eliminated non-Byzantine predecessors.* Two such disruptions are normally cited, namely, the persecutions under Emperor Diocletian and the Muslim conquests of the seventh and following centuries. Though both of these are historically validated events, there is no likelihood that non-Byzantine texts would have been singled out for elimination any more than Byzantine texts. The people that were destroying Christian Scriptures during those two time periods would not have asked about the texttype before deciding to light the fire. Therefore, it would be expected that manuscripts would survive persecution in a proportion relative to their former frequency.

OBJ3. *The influence of Chrysostom (who lived AD 347-407) or other church leaders made the Byzantine the preferred text of Constantinople; this text later was imposed on the Eastern Greek church by imperial or ecclesiastical decree.* It is not reasonable that a single father, however popular and influential he may have been, could have introduced a new or localized text (the Byzantine being intended by its critics) and thus cause the abandonment of a previously dominant text (here presumed to be the Alexandrian Text). Furthermore, there is no evidence regarding a later imposition of a different textual basis by an imperial or ecclesiastical decree. Even if such had occurred, it would be difficult to imagine how the total replacement of a previously dominant text would come about; but when the decree itself is imaginary, the claim totally breaks down.

Furthermore, the Byzantine Texttype evidences internal differences, i.e., it is composed of several independent strains, a fact that is not easily explained by a single-event replacement theory.





OBJ4. *The Byzantine Textform is the result of a process that over the centuries steadily moved away from the original form of the text in the interest of smoothness, harmonization, grammatical and other “improvements.”* The support usually given to buttress this objection is two-fold: (1) scribes do not automatically copy accurately, as they would if they were computer-controlled, and (2) close agreement among a myriad of manuscripts is possible only when there is some “control.”

In answer to the first point: on the contrary, it has been demonstrated (Royse, Colwell, Head) that most scribes were generally careful and reasonably accurate in their copying.

As to the second: there is no demonstrable enforced unity within the Byzantine Textform manuscripts and no evidence for any early imposition of “controls.” Hodges has dismantled the process-through-centuries objection with a classic statement: “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 [Western and Alexandrian] forms of text.... 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” [Hodges and Hodges, *Implications*, 132].

Summary and conclusion

In the foregoing pages we have tried to simplify a portion of the rather technical discussion that underlies Byzantine-priority theory. In short summary, this approach is decisive in this writer’s evaluation because of two overriding concerns.

First, the Byzantine-priority theory is just what it claims to be: a theory. A theory explains data, but is also accountable to the data. In modern eclecticism — the other main approach to textual criticism (though there are several schools of thought within that approach) — there is no theory, but much presumption. The Byzantine-priority theory is testable against the data and invites vigorous testing. At least here we have a theory. In the eclectic approach, although the data remain identical, it seems impossible to move from the data into a developed theory of the text. Thus the Byzantine-priority theory better meets the criteria of a theory than does eclecticism.

Second, this approach involves text. As speakers and writers — indeed as hearers and readers — of language, we process texts. Whether oral or written, texts have a history of transmission provided that they are recorded or memorized and passed on. To examine a text without a sense of its transmissional history is at best frustrating, at worst impossible. The extant Greek New Testament manuscripts are copies of original texts. And except for a small minority of manuscripts on the “fringes,” the vast majority of those copies contain a text that is remarkably consistent. That text has not been created for this moment, but has rather endured through long centuries in such a manner that every generation and century of the church has had a basic possession of that text, most generally in its clearly dominant form — that of the Byzantine Textform.

Note: the writer considers all quoted material to be understandable to the layman (except as altered and there noted by square brackets, []) and with good effect on the reader’s understanding.

This Layman’s Guide has received generous corrective input from scholars — not all favorable to its purpose — and laymen over the five years of its refining. The thirty or so that have contributed are too many to acknowledge here, though that doesn’t lessen my debt of gratitude to them. Suffice it here to name one as representative. Michael Martens has been a faithful and insightful editor over the many versions of this undertaking (and in both English and Indonesian) from the very beginning to this point. Even as I sought to clarify Byzantine priority theory, Michael has bested me by clarifying my attempt. Thank you, Michael.





Glossary

1. **apparatus** the set of notes, usually at the bottom of a page of text, concerning manuscript readings found therein, intended to give in succinct form enough information for the reader to weigh the evidence for himself; examples of such apparatuses are those found in the UBS and Nestle-Aland Greek texts, in the Byzantine Greek text edited by Pierpont and Robinson and in the Majority Text edited by Hodges and Farstad
2. **autograph** literally, something made or written with one's own hand; derivatively, an original manuscript — not a copy
3. **canonical** relating to or forming a canon; **canon** literally, a measuring reed; 1. referring to the books of the New Testament that were accepted as authoritative by God's people; 2. a synonym for principle, as in such terms as principle (canon) of internal/external evidence
4. **codex** (plural **codices**) a manuscript book, bound on one edge (and thus an innovation from earlier single sheets and scrolls)
5. **conflate (conflated, conflation)** the combining of two or more readings of a given text as found in different manuscripts into a composite whole in another manuscript or manuscripts; for example, in text A the reading occurring is "He entered the house"; in text B the reading is "He sat in the house"; in text C the reading found is "He entered sat in the house"; the reading of text C is a conflation of the readings found in texts A and B
6. **conjectural emendation** correcting a text, not by choosing among alternate readings, but by inserting a scholarly conjecture — guesswork — without supporting evidence
7. **cursive (writing)** 1. a handwriting style in which the letters are generally rounded and flow one into the next; 2. a manuscript made in such handwriting; generally used interchangeably with "minuscule" when speaking of New Testament manuscripts
8. **dittography** writing a letter, a series of letters, a word or a line twice when it should be written once
9. **eclectic** 1. (adj) selecting what appears best from several alternatives; composed of elements drawn from various sources; every textual variant is decided on its own merit; 2. (noun) someone that uses such a method in his work
10. **exemplar** a manuscript used as a source text for copying a new manuscript
11. **extant** currently or actually existing; not destroyed or lost
12. **external evidence** that witness from the testimony of the manuscripts, early translations and comments of church fathers weighed to determine which reading has the best outside support (cf. **internal evidence** below)
13. **haplography** 1. writing a letter, a series of letters or a word once when it should be written twice; 2. also, any omissions that do not appear to be caused by **homoioeleuton** or **homoioarcton**
14. **homoioarcton** a copying error caused when a scribe skips from one set of letters beginning a word or line to a similar set farther down the page, resulting in a loss of text in relation to the exemplar manuscript





15. **homoioteleuton** a copying error caused when a scribe skips from one set of letters ending a word or line to a similar set farther down the page, resulting in a loss of text in relation to the exemplar manuscript
16. **internal evidence** those items that yield a certain degree of probability to the analyst; answers to questions asked regarding the original author or copying scribe, what he probably would have written, whether intentionally or unintentionally, in any given case (cf. **external evidence** above)
17. **lampblack** a finely powdered black soot, frequently taken from the chimneys of lamps, and used as pigment for making various types of ink
18. **lectionary** a book or list of lections for the church year; **lection** a liturgical lesson/reading for a particular day, comprising discrete portions of the New Testament text that are themselves manuscripts
19. **manuscript** (usually abbreviated **ms** in singular, **mss** in plural) [the word “manuscript” is to be distinguished from “text,” which is the message contained on the physical medium; note the overlapping meanings of text and manuscript and our particular use of them in this glossary and essay]; 1. strictly speaking, the physical material (papyrus or vellum, as the case may be) with a message written on it; what may be held, examined, read (for its message), copied, stored, worn out, lost or destroyed; 2. the kinds of writing style found on particular manuscripts sometimes substitute for the word manuscript that rightly accompanies it and is to be understood; for example, cursive (-written manuscript), uncial (-written manuscript), minuscule (-written manuscript); 3. the kinds of physical material constituting the manuscript sometimes substitute for the word manuscript that rightly accompanies it and is to be understood; for example, papyrus (manuscript), vellum (manuscript), parchment (manuscript)
20. **minuscule** 1. a more formal handwriting style developed from cursive, having simplified and small forms; 2. a manuscript made in such handwriting; generally used synonymously with “cursive” when speaking of New Testament manuscripts
21. **papyrus** (plural **papyri** or **papyruses**) 1. a plant found in marshes, particularly in the Nile River Valley; 2. writing material made from the pith of this plant, whether in single sheets or assembled scrolls; 3. a writing made on such writing material
22. **parchment** 1. the skin of an animal, usually sheep or goat, prepared as a material for writing; 2. often the writing made on this material
23. **patristic** of or relating to the early church fathers, as in patristic commentary
24. **recension** a revising of a text on the basis of a critical examination of sources; a text established by such a critical revision; **recensional** of or relating to a recension
25. **reasoned transmissionalism** an attempt to determine the form of the autographs that weighs both internal and external evidence and recognizes the fact that the transmission of the text has a history
26. **scriptorium** (plural **scriptoria**) a copying room in medieval monasteries set apart for the scribes
27. **stream of textual transmission** the common flow of a transmitted text through multiple manuscripts; this stream can be narrow or wide, much as a stream or river, involving smaller or larger groupings of manuscripts





28. **Synoptic Gospels** the Gospels of Matthew, Mark and Luke, so called because their content shares a common view; in counter-distinction to the Gospel of John
29. **text** (to be distinguished from “manuscript,” which is the physical medium [paper, ink, kind of letters] conveying the text; there is some overlap of meaning between manuscript and text, so, for purposes of this paper, we are here giving the meanings behind our vocabulary choices) 1. the message of the New Testament contained in Greek words of particular spellings, inflections and order; 2. what points to the text as contained in the original manuscript written by the initiating author 3. since the original manuscripts containing the original texts are now lost, what we try to reconstruct by comparing the various manuscripts extant; 4. the result of our efforts to reconstruct the text by comparing manuscripts; since our efforts will be ongoing, this sense of text is never quite final; 5. what was found written on the exemplar manuscript being copied by the scribe and transcribed to become another manuscript; 6. what is found on a given manuscript, to be read, examined, meditated on and, for professional scribes, to be transferred to a blank manuscript (virtually identical to 5. above; 5. refers to past time; 6. refers to present time); can refer to a single word or phrase all the way up through the entire manuscript being read/compared/reconstructed; the contents thereof; 7. what is accepted as original, what we try to arrive at through our textual criticism; scholars of differing points of view will differ on the wording at various points of this text and thus on the very text itself; 8. a shorthand way of referring to a “text type,” i.e., a grouping of manuscripts containing a text of specific characteristics; for example, Western Text, Caesarean Text; 9. what can be held in our hands, bound in a book, whether secular or spiritual, whether original or reconstructed
30. **textform** 1. the form of the text that most closely appears to reflect the original; 2. the earliest transmitted form of the text, equivalent to the autograph; in principle, advocates of any texttype may make that claim; it is usually, however, advocates of the Byzantine Texttype that use the term
31. **texttype** a grouping of manuscripts whose texts share some basic characteristics; larger than a family, but not having the status of textform; there are usually considered to be four major texttypes: the Alexandrian, the Byzantine, the Caesarean and the Western; frequently used almost interchangeably with text, for example, the Western Text or the Western type of texts, thus the Western Texttype
32. **textual criticism** the attempt to determine the original form of a text from more than one manuscript
33. **Textus Receptus** Latin for “Received Text” (abbreviated TR); the common text dating from the first printed Greek New Testament, first published by Erasmus in the early 1500s but going through various editions and revisions; the Textus Receptus was based on only a few Greek manuscripts, and although it is generally Byzantine in texttype, it differs from the Byzantine mainstream in numerous particulars; the King James translation was based on the Textus Receptus; advocates of Byzantine priority theory should not be confused with those that advocate a “Textus Receptus only” or “King James only” position
34. **uncial** 1. a handwriting style largely composed of capital letters, sometimes slightly rounded and nearly cursive; 2. a manuscript made in such handwriting
35. **variant reading** what is written as intended to be read; strictly, material read or meant to be read is a reading, whether a single word or the entire text; this comes to have special significance when two or more manuscripts give evidence of competing readings; the idea is to reconstruct the original reading, of which all others are variants; since scholars differ on what is to be reconstructed as original, in one sense all readings are variants one of another, competing for the status of “original”; from the thousands of manuscripts containing a particular passage, there





are usually only two or three variant readings to be weighed, rarely upwards of ten; 94% of the text of the New Testament shows no variation whatsoever — that is, there is a unified text; on the other hand, the Alexandrian Text(type) and the Byzantine Text(type) differ from each other in some 6000 places

36. **vellum** 1. a fine-grained unsplit lambskin, kidskin or calfskin prepared especially for writing on; 2. often the writing done on such writing material
37. **version** usually an early translation of the Greek New Testament, whether into Coptic, Syrian, Gothic, Latin, etc.; **versional** of or relating to versions

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SCRIBAL HABITS AND THE NEW TESTAMENT TEXT

by Andrew Wilson

Maurice Robinson's 1982 Ph.D. thesis, *Scribal Habits among Manuscripts of the Apocalypse*,¹ has not perhaps enjoyed the recognition that it deserves in the thirty years since, despite the growing interest in the subject of scribal habits generally. Here we shall hopefully go some way toward rectifying this oversight firstly by comparing its findings and results with those of other subsequent studies, secondly, by weighing some of the objections that have been raised against these studies, and then finally, by surveying the significant implications for New Testament textual criticism from what the studies into scribal habits disclose about the transcriptional canons, the history of the text, the praxis of New Testament textual criticism and the current state of the text. We shall see that many of Robinson's conclusions from 1982 stand confirmed.

Studies into Scribal Habits

Ernest Colwell's 1965 study,² *Method in Evaluating Scribal Habits: A Study of* \mathfrak{P}^{45} , \mathfrak{P}^{66} , \mathfrak{P}^{75} , opened up a method for investigating the habits of scribes using singular readings (readings found only in one Greek New Testament manuscript). The logic of this method is that such idiosyncratic readings are far more likely to have been individualisms of scribes than to have been the genuine, original readings of the text. These singular readings, if collected in sufficient numbers, show us what sorts of errors scribes tended to introduce into the New Testament text.

Other studies which have examined scribal habits using singular readings have included James Royse's study³ into the scribal habits of six major early papyri (\mathfrak{P}^{45} , \mathfrak{P}^{46} , \mathfrak{P}^{47} , \mathfrak{P}^{66} , \mathfrak{P}^{72} and \mathfrak{P}^{75}), Peter Head's examination of the scribal habits of twenty-seven early but fragmentary papyri of the Gospels⁴ and Juan Hernández's study of three uncials in the Apocalypse (Sinaiticus, Alexandrinus and Ephraemi).⁵

The main finding of Royse's study was that, contrary to the traditional canon 'Prefer the Shorter Reading,' scribes actually tended to omit more than add. Among the six major papyri studied, there were 127 additions to 312 omissions.⁶ Peter Head's studies confirmed Royse's finding among other fragmentary papyri: scribes tended to omit rather than add, with 10 singular additions to 19 singular omissions.⁷ Likewise, Juan Hernández's main finding was again that scribes tended to omit rather than add. Among his three uncials, there were 57 additions to 87 omissions.

- 1 M. A. Robinson, *Scribal Habits among Manuscripts of the Apocalypse*, unpublished Ph.D. Dissertation, Southwestern Baptist Theological Seminary, Ft. Worth, TX, 1982.
- 2 E. C. Colwell, *Studies in Methodology in New Testament Textual Criticism* (NTTS 9; Leiden: Brill, 1969) 106-24, originally published as "Scribal Habits in Early Papyri: A Study in the Corruption of the Text," in J.P. Hyatt (ed.), *The Bible in Modern Scholarship*, (Nashville, TN: Abingdon Press, 1965) 370-389.
- 3 J. R. Royse, *Scribal Habits in Early Greek New Testament Papyri*, Th.D. Dissertation, Graduate Theological Union, Berkeley, CA, 1981. Subsequently published as *Scribal Habits in Early Greek New Testament Papyri* (Leiden: Brill, 2008).
- 4 P. M. Head, "Observations on Early Papyri of the Synoptic Gospels, especially on the 'Scribal Habits,'" *Bib* 71 (1990) 240-47, and "The Habits of New Testament Copyists, Singular Readings in the Early Fragmentary Papyri of John," *Bib* 85 (2004) 399-408.
- 5 J. Hernández, Jr., *Scribal Habits and Theological Influences in the Apocalypse: The Singular Readings of Sinaiticus, Alexandrinus, and Ephraemi* (WUNT 2.218; Tübingen: Mohr Siebeck, 2006).
- 6 In his original 1981 thesis, the figures were 130 additions to 337 omissions, the 2008 result only differing by 1 percentage point: 28:72 (1981) to (29:71).
- 7 Head's figures given in the summary sections of his articles are 7 additions and 12 omissions (1990 study) and 2 additions and 11 omissions (2004 study). However, these figures included some readings that were not truly singular, and additionally, Head's figures in the concluding paragraphs of his studies seemed to miscount the actual numbers of singular additions and omissions in his studies, as comparison with the body of the articles shows.





Maurice Robinson's study investigated scribal habits in ten chapters of the book of Revelation using singular readings gleaned from Hoskier's collation of 220 manuscripts in the Apocalypse. The results are entirely in keeping with these other studies: out of 4098 singular readings, scribes added 451 times (40%) and omitted 678 times (60%). One highly significant finding from Robinson's results is that later manuscripts tended to omit, just like earlier manuscripts.

| Manuscript | Ratio of Additions to Omissions (percentages) |
|------------|---|
| Papyri | 0:1 |
| Majuscules | 34:55 (38:62) |
| Minuscules | 408:612 (40:60) |

The present writer has also made a study⁸ of 4200 singular readings in 33 chapters of the NT text,⁹ extracting singular readings by comparing the standard critical apparatuses against each other (Tischendorf,¹⁰ von Soden,¹¹ NA27,¹² Swanson,¹³ IGNTP Luke,¹⁴ the *Editio Critica Maior*¹⁵ in the Catholic Epistles and Hoskier in Revelation¹⁶).

This was, in fact, one of Robinson's recommendations at the conclusion of his dissertation: an interim study of scribal habits in all individual manuscripts using Tischendorf and Von Soden to gather sufficient data.¹⁷ The results showed that scribes tended to omit rather than add, with 1088 singular additions (39%) and 1712 singular omissions (61%). The results for a number of notable manuscripts from this study are as follows:

| MSS | Ɔ ⁴⁵ | Ɔ ⁴⁶ | Ɔ ⁶⁶ | Ɔ ⁷⁵ | Ⲑ | A | B | C | D(05) | E(08) | L | W ¹⁸ | Θ | unc9 ¹⁹ |
|------|-----------------|-----------------|-----------------|-----------------|----|----|----|----|-------|-------|----|-----------------|----|--------------------|
| Add | 7 | 2 | 2 | 2 | 36 | 10 | 8 | 12 | 117 | 19 | 12 | 28 | 14 | 19 |
| Omit | 23 | 6 | 7 | 10 | 62 | 11 | 26 | 12 | 118 | 5 | 21 | 47 | 19 | 60 |

- 8 Andrew Gilson, "Scribal Habits in Greek New Testament Manuscripts," *Filologia Neotestamentaria* 24 (2011) 95-126
- 9 Matthew chapters 1, 2, 3, 6, 7, 10, 11, 12, 20, 21 and 26; Mark 1, 2, 3 and 6; Luke 10 and 11; John 4, 7 and 9; Acts 5, 10 and 15; Romans 12; 1 Corinthians 13; Galatians 1 and 2; James 1; 1 John 1, 2 and 5; and Revelation 1 and 5.
- 10 C. von Tischendorf (ed.) *Novum Testamentum Graece: ad antiquissimos testes denuo recensuit*. 8th ed. 2 vols. (Leipzig: Giesecke & Devrient, 1869-72).
- 11 H. F. von Soden (ed.) *Die Schriften des Neuen Testaments in ihrer ältesten erreichbaren Textgestalt hergestellt auf Grund ihrer Textgeschichte* (Göttingen: Vandenhoeck und Ruprecht, 1913).
- 12 B. Aland, K. Aland, J. Karavidopoulos, C. M. Martini, B. M. Metzger (ed.), *Nestle-Aland Novum Testamentum Graece*, 27th Edition, 8th (revised) impression (Stuttgart: Deutsche Bibelgesellschaft, 2001).
- 13 R. J. Swanson (ed.), *New Testament Greek Manuscripts: Variant Readings Arranged in Horizontal Lines against Codex Vaticanus* (Sheffield and Pasadena, CA: Sheffield Academic Press and William Carey International University Press, Vols. 1-5, 1995-98; Wheaton, IL and Pasadena, CA: Tyndale House and William Carey International University Press, Vols. 6-7, 1999 and 2001).
- 14 American and British Committees of the International Greek New Testament Project (ed.), *The New Testament in Greek: The Gospel according to Luke, Part One: Chapters 1-12, Part Two: Chapters 13-24* (Oxford: Clarendon Press, 1984-87).
- 15 B. Aland, K. Aland, G. Mink, K. Wachtel (ed.), *Novum Testamentum Graecum, Editio Critica Maior, 4: Catholic Letters, Installment 1: James, Installment 3: 1 John* (Stuttgart: Deutsche Bibelgesellschaft, 1997 and 2003).
- 16 H. C. Hoskier (ed.), *Concerning the Text of the Apocalypse: Collations of All Existing Available Greek Documents with the Standard Text of Stephen's Third Edition together with the Testimony of Versions, Commentaries and Fathers. A Complete Conspectus of all Authorities*, 2 vols. (London: Bernard Quaritch, 1929).
- 17 Robinson, *Scribal Habits*, 231. For estimates of how many manuscripts are recorded in this study, the *Kurzgefasste Liste Sigla Indices* (comparing Tischendorf, von Soden and Gregory numbers) give the following figures: over 2800 MSS in Von Soden; for Tischendorf 60 majuscules, about 1420 Gospel minuscules, 520 minuscules for Acts and the Catholic epistles and 525 minuscules for Paul (K. Aland, *Kurzgefasste Liste Der griechischen Handschriften Des Neuen Testaments* (ANTF 1; Berlin: de Gruyter, 1994) 377-401); NA27 lists over 1400 MSS; Swanson uses about 150 MSS; IGNTP Luke has 238 MSS; ECM has 182 in James and Hoskier has 220 in Revelation. These apparatuses thus claim to be based on well in excess of 3000 manuscripts, more than half of all Greek NT manuscripts. The majority of the manuscripts not included in these critical apparatuses are Byzantine MSS with a highly uniform text, the inclusion of whose evidence would add little. Lists of singular readings upon which the study is based and more detailed analysis of their results can be found at www.nttext.com and www.TyndaleArchive.com/NTVariants.
- 18 The figures for W include W^{sup} which had 5 additions and 5 omissions.
- 19 unc9 was the collective abbreviation used by Tischendorf for the readings of the Byzantine Gospel manuscripts E, F, G, H, K, M, S, U and V.





When omissions and additions were categorised according to the length of the add/omit variation unit, it is significant that additions and omissions of single words make up the majority of cases, and that more than 80% of add/omit variation units involve additions and omissions of one or two words length:

| Length (words) | 1 | 2 | 3-7 | 8+ | Totals |
|----------------|------------|-----------|-----------|---------|--------|
| Addition | 769 (71%) | 167 (15%) | 122 (11%) | 30 (3%) | 1088 |
| Omission | 1143 (67%) | 246 (14%) | 230 (13%) | 93 (5%) | 1712 |

When one-word omissions and additions were analysed according to the part of speech involved, the results showed scribes tending to omit all types of words except conjunctions and adverbs:

| | Pron. | Conj. | Art. | Noun | Verb | Adj. | Adv. | Prep. | Part. | Frag. |
|------|-------|-------|------|------|------|------|------|-------|-------|-------|
| Add | 119 | 227 | 156 | 53 | 57 | 35 | 41 | 45 | 23 | 13 |
| Omit | 246 | 228 | 259 | 81 | 141 | 58 | 41 | 60 | 28 | 1 |

When longer omissions (that is, three words or more) were examined, a majority were attributable to mechanical explanations like homoeoteleuton and homoeoarcton (199 out of 323 — 62%). However, this left 124 unexplained omissions (38%). When we include 1 and 2 word omissions, there were 1106 singular omissions with no discernable mechanical cause (64% of the total). This result will not surprise those familiar with early MSS like \mathfrak{P}^{66} with its 9 lengthy inexplicable omissions in the latter part of John's Gospel, reminiscent of the 'Western non-interpolations' of Codex Bezae towards the end of Luke's Gospel. It would appear that scribes quite often omit longer stretches of text for no detectable mechanical reason.

The following table compares the results of these five studies according to manuscript type:

| Ratios of Singular Additions to Omissions in various Studies (with percentages terms) | | | | | |
|---|--------------------|------------------------|------------------|------------------|-----------------------------------|
| | Royse | Robinson ²⁰ | Head | Hernandez | Wilson |
| Papyri | 127:312 (29:71) | 0:1 | 10:19 (34:66) | | 17:52 (25:75) |
| Majuscules | | 34:55 (38:62) | | 57:87 (40:60) | 359:539 (40:60 ²¹) |
| Minuscules | | 408:612 (40:60) | | | 646:1036 (38:62) |
| Lectionaries | | | | | 26/58 (31:69) ²² |

All of these studies into scribal habits using singular readings concur: scribes tended to omit material from the text rather than add material to it. The accumulating evidence from these studies shows the traditional preference for the shorter reading is unfounded.

Prefer the Harsher, Harder and Non-Harmonized Readings

The present writer's study was not simply restricted to the canon, 'Prefer the Shorter Reading,' but also investigated the three other main transcriptional canons. The second canon examined was 'Prefer the Harsher Reading.' This canon is based on the belief that scribes would tend to polish up and smooth out the text rather than make it more terse, austere or abrupt. Most presentations of the canons regard

²⁰ Robinson's figures (and Wilson's) exclude corrections that are themselves singular readings.

²¹ Excluding the distorting effect of Codex Bezae from these figures, the ratio of additions to omissions is 36.5:63.5

²² The lectionary figures are provided for sake of completeness, and are perhaps not representative of lectionaries as a whole due to the idiosyncratic nature of the lectionaries included in critical apparatuses.





'Prefer the Harsher Reading' as a separate canon to 'Prefer the Harder Reading' (see, for example, Griesbach and Metzger's statements of the canons²³). This canon therefore deals with textual variants that do not change the meaning of the text, but rather affect only its style, either making it more polished or terse.

In the study, all singular readings (including substitutions and transpositions, but excluding orthographic variants) in 11 chapters out of the 33 previously studied²⁴ under 'Prefer the Shorter Reading' were categorized in terms of their effect upon the text. Of these 2280 singular readings in the 11 chapters, there were 794 readings that affected only the style. Of these, 606 were harsher readings and 188 were stylistically easier readings (a ratio of 76:24).

This result, the exact opposite of what the traditional canon suggests, is not simply a matter of statistical regularity. Instead, it is almost a logical inevitability. This is because the majority of readings that affected the style were single word omissions and additions, the majority of which were omissions (see Metzger's version of the 'Harsher Reading' canon, point c). Consider the following examples of 'Harsher Readings' from Matthew 20:

1. Verse 5: K* omits de (producing asyndeton)
2. Verse 7: a(01)* omits hmaï (omission of object of verb)
3. Verse 13: 348(vSod) omits eni (omission of adjective makes style more stark)
4. Verse 13: 1346 omits eipen (verb now implied)
5. Verse 23: 69 omits mou (possessive pronoun omitted)
6. Verse 30: 565 omits Ihsouj (omission of subject)

These consisted of the omission of subjects and objects of verbs (which in Greek often leave the sense unaffected because the subjects and objects remain implicit), the omission of possessive pronouns (like "his disciples"), the omission of conjunctions (producing only asyndeton), verbs (particularly like the verb "to be" which remains implicit) and adjectives and adverbs which simply add colour.

Because scribes tended to omit single words like these rather than add, the resultant text means the same but it is now harsher, more austere, terse, jagged or abrupt after copying than before. Again, it would appear the traditional canon is wrong.

The third canon investigated, 'Prefer the Harder Reading,' is based on the belief that scribes were unlikely to make the text more difficult, but would rather remove difficulties and improve its sense. In testing this canon, the same 2280 singular readings in 11 chapters as examined under the 'Harsher Reading' canon were categorized in terms of their effect upon the text. This involved categorizing singulars according to whether they produced (a) nonsense (lexically, grammatically or in context), (b) a change in the sense or meaning of the text (either making it more difficult or easier), (c) a stylistic change (whether more polished or more terse, as already described under the second canon), or (d) having neither semantic nor stylistic effect (classified as neutral, or in some cases ambiguous). The results were as follows:

| Nonsense | Harder Sense | Harder Style | Neutral | Easier Style | Easier Sense | Total |
|--------------|--------------|--------------|--------------|--------------|--------------|-------|
| 558 (24%) | 244 (11%) | 606 (27%) | 676 (30%) | 188 (8%) | 9 (0.4%) | 2280 |

²³ Metzger's fourth canon dealt with the issue of stylistic changes: "Scribes would sometimes (a) replace an unfamiliar word with a more familiar synonym; (b) alter a less refined grammatical form or less elegant lexical expression, in accord with contemporary Atticizing preferences; or (c) add pronouns, conjunctions, and expletives to make a smoother text." *A Textual Commentary on the Greek New Testament*, 2nd ed. (London and New York: United Bible Societies, 1994) 13*. Griesbach also classified style separately under his third, fourth and fifth canons. See Griesbach's canons in E.J. Epp's essay, "The Eclectic Method in New Testament Textual Criticism: Solution or Symptom?" in E. J. Epp and G. D. Fee, *Studies in the Theory and Method of New Testament Textual Criticism* (SD45; Grand Rapids, MI: Eerdmans, 1993) 151-2.

²⁴ Matthew 20, Mark 2, Luke 10, John 4, Acts 15, Romans 12, 1 Corinthians 13, Galatians 2, James 1, 1 John 1, Revelation 1.





As can be seen from the table, the percentage of singular readings with an easier sense, that is, readings that improve the text semantically, as opposed to stylistically, was less than 1%. By contrast, there were 244 readings that made harder sense, more than 10% of the total and more than 30 times the readings which produced easier sense.

This was a startling result, but interestingly, comparison with Colwell's, Royse's, Robinson's, Head's and Hernández's studies showed similar figures. Results for singular readings that improved the text semantically or removed difficulties in these studies were:

| | Colwell | Royse | Robinson | Head | Hernández | Wilson |
|--------------|---------|-------|----------|------|-----------|--------|
| Singulars | 1014 | 1125 | 410 | 77 | 322 | 2280 |
| Improvements | 5 | 9 | 10 | 0 | 5 | 9 |
| Percentage | 0.5% | 0.8% | 2.4% | 0% | 1.5% | 0.4% |

Thus, out of Colwell's 1014 singular readings (after he had excluded itacisms), there were five readings (0.5%) that Colwell remarked upon that improved the sense: \wp^{45} in Luke 9:50, \wp^{75} in Luke 8:21, \wp^{66c} in John 8:25, \wp^{75} in John 10:7 and \wp^{45} in Acts 15:20. Out of Royse's 1125 'significant singulars', there were 4 other readings (in addition to the ones noted by Colwell) that improved the sense (0.8%): \wp^{45} in Acts 13:33, the omission in \wp^{46} in 1 Cor. 8:3, \wp^{72} in 2 Peter 3:10 and \wp^{47} in Rev. 13:6. In Head's studies there were 77 non-itacistic singulars, among which there were no readings that improved the sense (0%).

In Hernández's study, which primarily commented on singulars with theological (rather than logical) significance, there 322 singulars, of which 5 improved the sense logically (1.5%): Sinaiticus in Rev. 3:14, 3:16 and 7:15 and Alexandrinus in Rev. 2:22 and 20:4.²⁵

Robinson's study examined 4098 singular readings in 220 manuscripts, but of these, in-depth analysis was restricted to the singulars of 10 manuscripts in 10 chapters. Of these 410 readings, 64 were categorised under the headings of "logical changes," "removal of a difficulty," "theological changes" or "major editing" in these manuscripts, but of these, only 10 readings (2.4%) produced clear logical improvements: \aleph in 1:7, 3:14, 3:16, 5:13, 7:15; A in 5:9, 20:4 and 1854 in 1:12, 1:17 and 20:10. Other logical changes produced more difficult readings (e.g. \aleph in 17:2, 22:2-3, 22:14; A in 1:17, 12:1; C in 12:10, 1611 in 20:8; 1854 in 3:2, 3:10, 15:3, 15:6-7) or changed the meaning of the text without making it easier or harder (i.e. neutral readings, e.g. \aleph in 3:3, 5:4, 7:4, 7:10, 15:2, 15:6, 20:13, 20:15; A in 12:16, C in 3:8, 1854 in 3:3), whilst others were stylistic changes (e.g. C in 1:3, 1854 in 10:4).

Thus, despite the fact that such singulars showed what appeared to be deliberate editorial activity, few ended up improving the sense. Robinson's comments about some of these editing attempts are worth repeating: "once more the *more difficult* reading has been substituted for the *easier* reading, against the established canons of NT textual criticism";²⁶ "that such changes were effected with 'good intention' on the part of the scribe does not mitigate the fact that the text of the Apocalypse *has* seen corruption at his hand";²⁷ "a case of clear editorial activity with inferior results."²⁸ Indeed, Robinson titles one of his sections, "Intentional Bumbling in C."²⁹

Robinson's study's main conclusion was as follows: "Scribes were *not* the 'great corrupters' of the text as has been supposed. *Most* scribes — and especially those of the later 'Byzantine era' — were extremely careful, their few corruptions being mostly accidental and the deliberate alterations being most stylistic changes of a minor nature."³⁰ Bearing in mind that Robinson's 10 manuscripts included some of the

25 For more detailed examination of these and other readings, see www.nttext.com.

26 Robinson, *Scribal Habits*, 102-3, emphasis in original.

27 Ibid., 103, emphasis original.

28 Ibid., 171.

29 Ibid., 117.

30 Ibid., 190, emphasis original.





most editorial of all manuscripts in Revelation (Hernández described Sinaiticus in Revelation as a ‘maverick’³¹), it becomes apparent that the 2.4% figure would quickly drop if the in-depth analysis were extended to all of the 220 manuscripts.

Perhaps there were other singular readings that improved the sense or removed difficulties in these studies that the present author has missed, but there simply are not dozens, let alone hundreds, of them. For example, Royse does not even make reference to the ‘Harder Reading’ canon in the index of his monograph, while there is only one mention of the canon in the text itself (page 713, note 30). Similarly instructive is Tarelli’s opinion that the reading of ϣ⁴⁵ in Luke 9:50 is “the one singular reading of the papyrus which materially affects the meaning of the text.”³² Combing through Royse’s 1000 pages and 3000-odd footnotes will increase one’s respect for his magisterial and meticulous study, but it will not increase by much the count of singular readings that improve the sense of the text.

Thus, scribes appeared to improve the sense of the text of the NT about 1-2% of the time (excluding orthographic singulars). This is a surprising result; if it is true (and the alignment of a number of studies — within 1% of each other — suggests it is), then it hardly inspires confidence in the canon. On the contrary, the fact that scribes tended to create harder readings thirty times more often than they created easier readings (244 harder to 9 easier in the current author’s study) suggests that, as a generalised statement, the canon does not correspond with scribal reality.

The fourth transcriptional canon, ‘Prefer the Non-Harmonized Reading,’ is based on the belief that scribes would tend to assimilate the text to parallel influences (particularly in the synoptic gospels) rather than to disharmonize originally parallel accounts. Colwell’s and Royse’s studies also highlighted the tendency of scribes to harmonize to the immediate context (as well as to ‘general usage,’ that is, to common phraseology).

In the present author’s study, the canon was tested by comparing harmonizations against disharmonizations among singular readings in the 33 chapters studied. The result was 411 disharmonizations to 204 harmonizations. This might again be surprising for those whose knowledge of textual criticism is confined to what they have read in the handbooks as opposed to actual documents. But consider, for example, the singular harmonizations and disharmonizations from the verse about the Heavenly Father giving the Holy Spirit to those who ask him, Luke 11:13 (all harmonizations and disharmonizations to Matthew 7:11):

Harmonizations:

1. L292 substitutes εν τοις ουρανοις for ex ouranou
2. 477* omits pneuma
3. D substitutes αγαπον doma for pneuma agion
4. Q substitutes domata agaqa for pneuma agion

Disharmonizations:

1. 1247 substitutes ημεις for υμεις
2. 489 substitutes υμας for υμεις
3. 2487 transposes: ποιητοι υμεις
4. 1242* omits ποιητοι υπαρχοντες οιδατε
5. 2542 transposes δοματα διδοναι αγαθα
6. L80 substitutes διδοντες for διδοναι
7. 2643 omits υμων
8. G substitutes ποσων for ποσω
9. 2643* omits αυτον
10. 472 substitutes αυτω for αυτον

³¹ Hernandez, *Scribal Habits*, 49

³² C. C. Tarelli, “Omissions, Additions, and Conflations in the Chester Beatty Papyrus.” *JTS* 40 (1939) 382-7. Quoted in Royse, *Scribal Habits*, 2008, 185, n. 410.





This verse shows twice the disharmonizations to harmonizations, a situation not atypical in many synoptic gospel verses with close parallels. Even when disharmonizations that could be explained for “mechanical” reasons (like homoeoteleuton or dittography) were removed, the numbers of disharmonizations still outnumber harmonizations to parallel influences by 301 to 136. A major reason for the large numbers of disharmonizations was again to be found in the large numbers of short omissions, producing disharmony among originally-parallel stretches of wording. Note from the table below that harmonization seems to usually be the result of addition while disharmonization the result of omission:

| | Harmonizations | Disharmonizations |
|----------------|----------------|-------------------|
| Additions | 131 | 53 |
| Omissions | 46 | 265 |
| Substitutions | 26 | 75 |
| Transpositions | 1 | 18 |
| Totals | 204 | 411 |

Among one-word cases, there were 277 disharmonizations and 96 harmonizations (or 222 to 69, once mechanical explanations for disharmonization were excluded and only harmonizations to parallels were counted). Cases of disharmonization among 2-4 word stretches of text also outnumbered cases of parallel harmonization by 67 to 48 (having again eliminated singular readings with “mechanical” explanations). It was only in longer stretches of text (five words or longer) that harmonization started to outnumber disharmonization (there were 19 cases of harmonization to 12 cases of disharmonization).

We can therefore say that the fourth transcriptional canon needs to be more carefully balanced: among shorter variation units, textual critics should tend to prefer the harmonised reading, while among longer variation units, textual critics should prefer the non-harmonised reading.

How could it be possible that not one, but all four transcriptional canons are wrong? The reason is simple: the canons are related to each other, and like dominoes, when one falls, they all topple together. This is because omission, particularly in one-word cases, usually produces a harsher text and often a disharmonized text, while addition produces the opposite. At a more fundamental level, omission, harsher readings and disharmonization are effects which all, in different ways and degrees, damage the text, making it more difficult. Eberhard Nestle argued that Bengel reduced “all Gerhard von Maestricht’s 43 canons to one comprehensive rule: *proclivi scriptiōni praestat ardua*.”³³ Nestle also argued that ‘Prefer the Shorter Reading’ is “but another form” and “a subdivision of Bengel’s canon.”³⁴ Nestle would appear to be correct in arguing that the transcriptional canons stand (or, better, fall) together. The effect of scribal activity is largely to corrupt the text, not to evolve new and ever more beautiful forms of it.

Thus, it would appear that all four of the traditional transcriptional canons are either wrong, or in need of serious modification. This was, in fact, Robinson’s conclusion in 1982: “Generalizations concerning scribal habits are faulty; the textual handbooks are in need of *drastic* revision.”³⁵ Robinson’s language may have appeared extreme in 1982, but on current evidence, mere disquiet at the misinformation about scribal tendencies in the textual handbooks would appear inappropriate; his language appropriately captures the consternation that should accompany the fact that scribes did the very opposite of what the textual handbooks tell us.

33 E. Nestle, *Introduction to the Textual Criticism of the Greek New Testament* (London: Williams and Norgate, 1901) 16-17.

34 Ibid., 240, 245.

35 Robinson, *Scribal Habits*, Abstract, 2, emphasis in the original.



Objections

Before we move on to consider the implications of these studies for New Testament textual criticism, we need to consider some of the objections that have been raised against them. These objections have tended to be of two different varieties: firstly, objections evidencing a general reluctance to abandon the canons in view of their ancient, venerable and centrally important place in New Testament textual criticism, and secondly, objections of a more technical nature.

An example of the first is Barbara Aland's assessment that the canons of criticism should be not altered:

in the previous comments I did not go into the criteria that lead in each particular case to a decision about the original reading, since they of course have not changed and will not change. Rather the old-school rules of classical philology are to be applied, which must be observed by anyone who produces a text ... These rules have often been presented by various authors in the appropriate handbooks. To be sure, in this area one can dispute the details of specific formulations or difficult points, but not the kernel of these rules themselves.³⁶

Or, for an even more extreme example of the reluctance to embrace change when confronted by evidence, consider the reaction of Edward Hobbs on B-Greek, the Biblical Greek forum, on Thursday October 26, 1995. Hobbs' message starts by quoting (in italics) a previous message posted by someone anonymously (thus breaking one of B-Greek's courtesy rules).

Subject: Textual Criticism

*<Actually, due to the work of Colwell, Clark, Streeter, Royse, and Head, it
<has been shown that due to various factors scribes were more likely to omit
<than to add to the text.*

The Anonymous Poster has struck again!

This statement is nonsense. Colwell was my teacher. Clark was my friend Royse was my student (I was on his dissertation committee, one of three, and the only text-critic). And Streeter's writings on this subject were my bread and butter long before I took my Ph.D., almost half a century ago. The ONLY one of them who argued that scribes tended to add rather than omit [sic.] was my student Jim Royse..., who over-generalized the results of his extremely limited study of a few papyri. If several dozen more dissertations on the issue, studying some uncials, above all post-300CE uncials, were to show the same, we would have to rethink this question ...

Edward Hobbs

Hobbs' naked appeal to authority is disconcerting, but at least in 1995 it was true that a few more studies would probably be needed to justify any abandonment of the canon. Such excuses no longer exist, in light of the increasing number of studies of manuscripts from different periods, which show that scribes tended to omit rather than add.

As these subsequent studies have added their weight to the argument, other objections of a more technical nature have been advanced against their results.

One objection is that singular readings themselves are problematic. Thus, Barbara Aland, investigating scribal habits of early papyri by noting divergences from the Nestle-Aland text, criticized Colwell's and Royse's studies on the grounds that focusing exclusively on singular readings "considers only a limited part of the papyrus and leaves all the rest of the material out of view."³⁷ This argument attacks the logic of investigating scribal habits via singular readings.

36 B. Aland, "Neutestamentliche Textforschung und Textgeschichte: Erwägungen zu einem notwendigen Thema," *NTS* 36 (1990) 339 (Royse's translation).

37 B. Aland, "Kriterien zur Beurteilung kleinerer Papyrusfragmente des Neuen Testaments," in A. Denaux (ed.), *New Testament Textual Criticism and Exegesis: Festschrift J. Delobel* (BETL 161; Leuven: Leuven University Press and Peeters, 2002). Royse's translation is from *Scribal Habits*, 2008, 61.

However, singular readings are simply the most objective way to identify the readings most likely to have been introduced by scribes. While it is theoretically possible that a singular reading might be the authentic reading, nevertheless, if the weight and diversity of external support for readings count for anything in New Testament textual criticism, it would be extremely unlikely that large numbers of singular readings are genuine. Singular readings studied in sufficient numbers thus provide us with our best guide to typical scribal errors.

Further, Aland's alternative proposal (cataloguing deviations from the NA27 "standard") is far more problematic than using singular readings. Not only did the editors of NA27/UBS4 prefer shorter readings (Metzger says so in the *Textual Commentary*), but the editors also followed closely the readings of certain manuscripts which heavily tend to omit. It is impossible to regard NA27 as an impartial standard by which we may evaluate the validity of the 'Shorter Reading' canon when the editing of the text was so heavily biased towards shorter readings. Using NA27 as a comparison standard is like letting Bonnie cross-examine Clyde. Aland's method opens the gates to readings which do not have as high a likelihood of being scribal errors as singular readings, whereas singular readings offer a more objective basis and a higher standard for identifying scribal errors. To use James Royse's words, in singular readings, we attempt "to discover the actual habits of scribes on empirical grounds, in a manner as free as possible from any presuppositions about scribal behavior."³⁸ Juan Hernández writes that singular readings are "the safest place to speak confidently about scribal tendencies."³⁹ A related objection is that as singular readings are unusual readings unlikely to be included in the text, therefore rules derived from singulars are unsuited to extrapolation to more normal variants.⁴⁰

However, apart from nonsense readings and orthographic variants, singular readings trade in the same currency as other readings in a critical apparatus: transpositions, substitutions, omissions and additions. Most "normal" variant readings must have started their transmissional history as singular readings introduced by a scribe into a solitary manuscript, the only difference now being the additional support resulting from more copying generations. The claim that singular readings are "inherently different" is thus not only difficult to substantiate, but to even conceptualise. If we don't base our transcriptional canons on identifiable scribal errors, what should we be basing them on?

Another objection is that studies questioning the 'Shorter Reading' canon have not taken into account more nuanced versions of the canon. Thus, in response to Royse's 1981 study, Moisés Silva made a study of the scribal habits of \mathfrak{P}^{46} , Sinaiticus, Alexandrinus and Vaticanus in Galatians.⁴¹ Despite the fact that Silva's MSS omitted 68 times while only adding 25 times (a ratio of 73:27), Silva urged the continuing validity of the canon, arguing that Griesbach qualified his canon by excepting small omissions and cases of homoeoteleuton.⁴²

Silva is not alone in objecting that studies into scribal habits have been simplistic in their understanding of the canon. Thus, Dirk Jongkind also argued that "perhaps Royse comes close to misinterpreting Griesbach."⁴³ Jongkind's argument was again that most of the omissions noted in Royse's study fell into the categories of Griesbach's exceptions.

However, Silva's own study shows the problem with this objection, for one word omissions accounted for at least 55 (81%) of Silva's 68 omissions.⁴⁴ Silva's objection is flawed for the simple reason that to classify 81% of evidence as an exception to a rule is like saying that all Europeans are Frenchmen, with a few exceptions. We might also say that sheep are naturally carnivorous but in exceptional circumstances

38 Royse, *Scribal Habits*, 2008, 11.

39 Hernández, *Scribal Habits*, 2006, 154.

40 E.g. see discussion at <http://evangelicaltextualcriticism.blogspot.com.au/2009/02/more-difficult-reading.html>

41 M. Silva, "The Text of Galatians: Evidence from the Earliest Greek Manuscripts," in D. A. Black (ed.), *Scribes and Scripture: New Testament Essays in Honor of J. Harold Greenlee* (Winona Lake, IN: Eisenbrauns, 1992), 17-25.

42 See B. M. Metzger's translation of Griesbach's first rule in *The Text of the New Testament: Its Transmission, Corruption, and Restoration*, 3rd ed. (New York and Oxford: Oxford University Press, 1992) 120.

43 D. Jongkind, *Scribal Habits in Codex Sinaiticus* (TS 3rd series, 5; Piscataway, NJ: Gorgias Press, 2007) 139.

44 Silva's figures for addition and omission in the table on page 24 do not tally with the figures provided earlier in his study, and he provides three different figures for omissions in \mathfrak{P}^{46} : 39 (page 19), 35 (page 23) and 33 (the table on page 24).



nibble grass. The fact that the majority of Royse's evidence falls within Griesbach's exception clauses is precisely why Royse is right: Griesbach's canon is more honoured in its breach than its observance.

In the present author's study, when singular readings were analysed according to the length of the text omitted, one and two word omissions constituted 81% of all omissions. Short omissions are therefore not an exception, but, in large part, the rule itself. Griesbach wrote "the shorter reading ... is to be preferred to the more verbose, for scribes *were much more prone to add than to omit*,"⁴⁵ but the reality would be more correctly (and clearly) expressed by saying that scribes were more prone to omit than add, particularly in cases of small variation units. The attempt to defend his canon by appealing to the short omissions "exception clause" would not appear viable.

Another attempt to defend the canons by qualifying them involves distinguishing between intentional and unintentional scribal behaviour, an emphasis that can be traced back to Griesbach but more recently popularised by Harold Greenlee.⁴⁶ Thus, it could be argued that it is more difficult to explain why a scribe would deliberately omit material than add it.

Daniel Wallace has attempted to defend the Shorter Reading canon on this very basis. Thus, Wallace writes that "there are numerous guidelines under the broad umbrella of choosing the reading that best explains the rise of the others, but two stand out: the shorter reading is to be preferred and the harder reading is to be preferred."⁴⁷ Wallace justifies the preference for the shorter reading with the assertion, "scribes had a strong tendency to add words or phrases rather than omit," but he qualifies his statement with the following: "As long as an *unintentional* omission is not likely, the shorter reading is to be preferred."⁴⁸

The problem with this argument is that the vast majority of omissions are probably unintentional. Small omissions usually involve easily overlooked words that do not change the basic meaning of the text, while the majority of longer omissions are cases of parablepsis, again unintentional errors. Wallace's 'qualification' should be reworded as follows: As long as an omission appears clearly deliberate — a rare event — the shorter reading is to be preferred. The qualification thus fails to save the canon. Textual criticism rightly must concern itself with commonly encountered scribal events; rare cases make bad laws.

It is for reasons like these that few textual critics use the intentional/unintentional argument with regard to omissions and additions, reserving it instead for the 'Harder Reading' canon. They would argue that, assuming that scribes did not intentionally make the text more difficult, we should prefer a harder reading unless some obvious accidental explanation suggests itself.

This argument, however, quickly runs into practical difficulties. Firstly, it appears to commit a logical fallacy: the fallacy of the excluded middle (also known as the false-dilemma). By insisting that we categorize a reading as either a deliberate or accidental scribal alteration, the argument ignores the possibility of readings created sub-consciously, that is, as a result of a wandering mind, a lapse in concentration, haste or fatigue. The many singular harmonisations in our manuscripts are evidence of scribes operating on "auto-pilot," the mind drifting back to or anticipating events in the immediate context, or wandering off to parallel accounts in different books altogether.

David Parker writes that "in the 'Freudian slip,' we have learned to recognise how the unconscious can control our spoken words"; he argues that "only the production of complete nonsense can safely be called accidental."⁴⁹

45 Quoted from Metzger's translation of Griesbach's Prolegomena to the second edition of his Greek New Testament (1796-1806) in *The Text of the New Testament*, 120 (emphasis added).

46 J. H. Greenlee, *Introduction to New Testament Textual Criticism* (Grand Rapids, MI: Wm. B. Eerdmans, 1964) 114-5.

47 D. B. Wallace, "Laying a Foundation: New Testament Textual Criticism," in *Interpreting the New Testament Text: Introduction to the Art and Science of Exegesis*, (Ed.) Darrell L. Bock and Buist M. Fanning (Wheaton, IL: Crossway Books, 2006), 46.

48 Ibid., 48, emphasis added.

49 D. C. Parker, *The Living Text of the Gospels* (Cambridge: Cambridge University Press, 1997) 37.





Another problem with the argument is that it also ignores readings created in more than one stage, as salvage readings of earlier errors, as imperfect corrections or due to the confused interpretation of corrections in a *Vorlage*. These readings would show up as intentional variations (for the last stage, being an intentional correction, produced something sensible), but these readings are usually harder readings than all of the alternatives (except for the nonsense reading or mistake which prompted correction).

Anyone looking at singular readings will find many cases of harder readings. The fact that, in the present writers study, scribes created thirty times more “harder readings” (which make some sense in context, without being nonsense) than “easier readings” leads to two conclusions:

- The first, assuming it is true that scribes did not deliberately try to make the text harder, is that textual critics have entertained exaggerated estimates of how easy it is to correctly distinguish between deliberate, sub-conscious, multi-stage or accidental variations.
- The second, flowing from this, is that if scribes produced harder readings in a variety of ways and in large numbers, but rarely produced easier readings, there remains little reason for textual critics to prefer harder readings.

Another related reply might object that the transcriptional canons are simply common sense.⁵⁰ However, good arguments could also be made, on common sense grounds, against some of the canons. For example, the ‘Shorter Reading’ canon does not square easily with the common sense observation that omission is the easiest of all mistakes to make. Again, textual critics often find they have to explain the counter-intuitive logic of *lectio difficilior potior* to the uninitiated. See, for example, Bart Ehrman’s comment regarding Mark 1:41: “On the contrary, *and this may indeed seem backwards at first*, the fact that one of the readings makes such good sense and is easy to understand is precisely what makes some scholars suspect that it is wrong.”⁵¹

Bertrand Russell called common sense the “metaphysics of savages,”⁵² and while it may contain some truth (just like office rumours and election promises), common sense seems an insufficient basis for any critical conclusions. Text-criticism must set a higher standard of proof than mere theoretical speculation. The idea that we may dismiss the best line of evidence we possess (singular readings) in favor of common sense is a rejection of evidence-based approaches altogether in favor of ivory tower speculation. It is on a par with insisting that a particular folk remedy is able to cure ailments, despite clinical trials showing no evidence of this happening. A canon based on common sense is little more than an article of speculative faith.

One final objection to the results of these studies into scribal habits remains, and is best dealt with here because it leads nicely into our next subject. David Parker, in reviewing Royse’s 2008 monograph, writes:

One of the book’s conclusions is rather startling, namely a challenge to the traditional textual canon that the shorter reading is to be preferred. Royse amply demonstrates something which is true of many, perhaps most, manuscripts, namely that scribes tended to shorten the manuscript they were copying. That being the case, he argues that one needs to hedge around *lectio brevior* with caveats. But does this evidence really overturn one of the longest-held editorial principles? I do not think so.⁵³

Parker justifies his refusal to accept that the canon is dead, not on the basis of any evidence, but (even more surprisingly) for historical reasons. He writes: “The reason I believe lies in the difference between the purposes of the eighteenth century scholars who framed the canon, and those of a modern

50 Scrivener, for example, argued that the canons are based on common sense. See F. H. A. Scrivener, *A Plain Introduction to the Criticism of the New Testament*, 4th ed., (Ed.) Edward Miller (London: George Bell and Sons, 1894), Vol. 2, 247.

51 This comment comes from a lecture: Text and Tradition: The Role of New Testament Manuscripts in Early Christian Studies, The Kenneth W. Clark Lectures, Duke Divinity School, 1997, Lecture One: Text and Interpretation: The Exegetical Significance of the “Original” Text. The lecture transcript can be found at: <http://rosetta.reltech.org/TC/extras/ehrman-clarklec1.html> (emphasis added).

52 Cited by Bryan Magee, *Confessions of a Philosopher* (New York: Modern Library, 1997) 43, 105.

53 D. C. Parker, review of James R. Royse, “Scribal Habits in Early Greek New Testament Papyri,” *BASP* 46 (2009), 257.





scholar. *Lectio brevior* is addressed to a situation where the rather longer Received Text appeared to be secondary.” He goes on to quote Westcott and Hort on the subject of the fullness of the Byzantine text and continues: “Compare the Textus Receptus with Nestle-Aland or with Westcott and Hort’s own text, and you will see the importance of this maxim. But whatever may be true of the Byzantine text as it was received, there are Byzantine manuscripts also whose scribes tended to lose text, and the fact is that there seems to be a paradox: the text grows, even though individual copies tend to omit.”

We shall come back to Parker’s paradox later — it is the central puzzle of the history of the text — and we notice in passing that Parker admits that most manuscripts, even later Byzantine manuscripts, tend to omit. However, to return to his argument, he continues by saying it would be “unfortunate to abandon the rule as it was originally conceived. Westcott and Hort did not always follow \aleph or B into their shorter readings where these were clearly the result of scribal error, although they followed them in many places where the ‘full’ Syrian text needed reducing.”

Parker thus offers two defences of the canon. Firstly, Parker wants to hold onto the canon for historical reasons, and secondly, Parker wants to hold onto the canon because of the excellence of certain manuscripts which he names. Our immediate interest lies in Parker’s argument from history, for to really understand the reluctance of many textual critics to accept the evidence from studies into scribal habits, we have to understand the history of the canons. We shall come back to the argument from the excellence of certain manuscripts later.

The History of the Transcriptional Canons

Although there were earlier critical apparatuses,⁵⁴ it was John Mill’s 1707 Greek New Testament which, more than any other, led to the development of canons of transcriptional probability. Mill’s Greek New Testament reproduced Stephanus’ *Textus Receptus* (TR), but its critical apparatus contained about 30,000 variant readings from manuscripts, versions and fathers.

This large apparatus provoked the full range of reactions to textual criticism still seen today, from defences of the traditional text to attacks on scripture by sceptics. From a text-critical perspective, the most important response was that of Gerhard von Maestricht who produced a list of forty-three textual canons in his Greek New Testament of 1711. Gerhard’s purpose was to defend the TR; his canons were designed to discredit the variant readings that Mill’s apparatus had brought to light. But Gerhard’s canons prompted the counter-reactions of J.A. Bengel and J.J. Wettstein, who produced their own canons, including the first statements of ‘Prefer the Harder Reading’⁵⁵ and ‘Prefer the Shorter Reading’⁵⁶ respectively.

The transcriptional canons thus arose as new manuscripts were discovered and collated against the base text of the TR. Many of these readings tended to show up as being shorter, harder, harsher and less harmonized than the TR. Since it was assumed that earlier manuscripts were nearer to the original text, most early textual critics went a step further and reasoned that scribes must have added to, polished up, harmonized and attempted to improve the text to produce the TR; hence the canons.

To take an example of the logical steps by which textual critics formulated the transcriptional canons, consider the argument of S. P. Tregelles in his *Account of the Printed Text*,⁵⁷ who proceeds by means of an historical sketch of the various editors who published editions of the Greek New Testament. Tregelles was far from a disinterested chronicler of textual history; his writing has a moral tone to it. The heroes

54 Notably those of Robert Stephanus in 1550, which showed the readings of 16 manuscripts in the margins of his Greek New Testament, Brian Walton whose Polyglot of 1655-57 added the witness of Codex Alexandrinus and 15 other manuscripts to those of Stephanus, and John Fell in 1675 who claimed to show in his apparatus the witness of over 100 manuscripts.

55 In a 1725 ‘Prodromus’ to his forthcoming Greek New Testament (1734). Bengel’s phrase was *proclivi scriptioni praestat ardua*.

56 In Wettstein’s *Novum Testamentum Graecum*, 1751-2, Vol. 2, 863, for his version of *lectio brevior potior*.

57 S. P. Tregelles, *An Account of the Printed Text of the Greek New Testament with Remarks on its Revision upon Critical Principles* (London: Samuel Bagster and Sons, 1854).





of the discipline are those who championed ancient evidence while the villains of the piece are those who perpetuated the TR. Tregelles' history is one long argument: his desire is to establish the text upon the authority of ancient witnesses.

When Tregelles leaves the history of the discipline behind and sets out the principles by which textual decisions must be made, he again spends another 45 pages (from 174 to 219) repeating his primary rule: we must follow only ancient evidence. Having now argued his main point for over 200 pages, Tregelles devotes about a dozen paragraphs to a variety of transcriptional canons. Here is how he starts:

When once the position has been definitively taken, that the ancient evidence is that which we must especially regard, other considerations affecting various readings must have their place, in order to judge between the ancient authorities, when they differ among themselves. ... As copyists were *always* more addicted to amplification than the contrary, as a general rule it must be said, that less evidence is sufficient (other things being equal) in favour of an omission than of an insertion; especially if the insertion is one which might naturally be suggested.⁵⁸

Tregelles offers no evidence (other than an analogy from snowballs)⁵⁹ for his belief that copyists are always more addicted to addition than omission, and so we might well question on what basis he makes his assertions about the behaviour of copyists. However, in reality, he has given us two notable clues as to where he derived his canons from. The two main features of his book give us all the explanation we need.

Firstly, Tregelles' transcriptional canons are based on his belief in the superior authority of ancient witnesses, the point he has spent most of his book arguing, and the additional fact (unstated but easily demonstrated) that more ancient witnesses have a shorter text. Tregelles' transcriptional canons are therefore but an extension, an elaboration, of his primary rule. Tregelles takes this logical step almost unconsciously. Tregelles' second reason is also held on implicit grounds: his trust in the authority of the heroes of the discipline whose history he has spent the most part of the book retelling. Tregelles has set up his narrative in such a way as to show us who are the heroes we should respect (people like Bengel and Griesbach). Tregelles unhesitatingly repeats their transcriptional dictums with added dogmatism.

It is important to realise the connection between the transcriptional canons and the sometimes bitter polemic between defenders of the TR and those promoting readings of newly collated, more ancient, manuscripts. Early textual critics were vilified for departing from the TR. Being only human, their own arguments for a New Testament text based on evidence from earlier manuscripts were not altogether free from frustration with those who refused to accept the many faults of the TR. We notice an underlying conflict between pro and anti-TR positions whether we look at early textual critics like Gerhard von Maestrict responding to Mill's apparatus, Bengel and Wetstein responding to Gerhard or 19th century critics like Tregelles. It is the debate over the TR and the preferential treatment which most early textual critics gave to more ancient witnesses which overshadows (or rather, as we shall see, clouds) the issue of scribal habits.

Two problems arise from the way in which these early textual critics allowed the debate over the TR to shape their transcriptional canons: one methodological and one logical. Firstly, the process of determining scribal habits was ill-defined and thus methodologically far from satisfactory. Early textual critics did not draw up clearly formulated principles by which to detect scribal errors, nor did they publish comprehensive lists of variant readings which established patterns of scribal corruption.

Thus, J. J. Griesbach offered no evidence for his assertion that "the shorter reading ... is to be preferred to the more verbose, for scribes were much more prone to add than to omit"⁶⁰ (his first canon). Royse comments: "in fact, no specific reading of a manuscript is cited anywhere within Griesbach's Prolegomena ... [this] makes it difficult (if not impossible) for later students to know what exactly he would have considered as evidence, to check the evidence upon which his statements rest, or to revise his statements in the light of new evidence."⁶¹

⁵⁸ Ibid., 219-220, emphasis in the original.

⁵⁹ Ibid., 88, second footnote.

⁶⁰ Quoted from Metzger's translation of Griesbach's Prolegomena to the second edition of his Greek New Testament (1796-1806) in *The Text of the New Testament*, 120.

⁶¹ J. R. Royse, *Scribal Habits*, 2008, 4-5





Instead of offering a method for distinguishing scribal errors from genuine readings and publishing lists of such errors to justify their canons, early textual critics simply observed deviations from the TR, using Mill's apparatus or their own collations. The TR with all its faults is hardly an acceptable basis from which to determine textual principles. Even so, for all its faults, the TR coincides with the text of virtually all witnesses in many places in the New Testament. Variations from the TR cannot be assumed 'original readings', for they are sometimes scribal errors themselves. So, even when these early textual critics cited particular readings in defence of the canons "presumably as evidence — the evaluation of one reading as the original and another as arising by a scribal error is frequently suspect from a methodological point of view, and so one is left wondering why the direction of scribal error may not have been other than is stated."⁶²

Secondly, the rules of scribal habit contained a logical problem in that they formed part of a circular argument. Textual critics simply noticed that earlier manuscripts had a leaner, meaner text than the TR. From this observation (and the assumption of the superiority of earlier manuscripts) the rules of scribal habits were framed. To restate the argument in a logical formulation: (1) the TR is a late and degenerate text, (2) the TR is observed (relatively speaking) to be full of additions, improvements, harmonizations and smoothings, (3) scribes must have added, improved, harmonised and smoothed the New Testament text to produce the TR, (4) hence, the TR is proved to be a degenerate text, seeing it is guilty of typical scribal corruptions.

The scribal canons helped to dethrone the TR by proving its degeneracy while, at the same time, the scribal canons depended upon the assumption of the degeneracy of the TR. This was less logical syllogism, more logical circle. Whatever truth it contained, it proved nothing.

As an example of this sort of circular reasoning, consider a statement of Hort's under the curious heading of "Absence of Interpolations in B." He writes,

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. When therefore a text of late and degenerate type, such as is the Received Text of the New Testament, is consciously or unconsciously taken as a standard, any document belonging to a purer stage of the text must by the nature of the case have the appearance of being guilty of omissions; and the nearer the document stands to the autograph, the more numerous must be the omissions laid to its charge. If B is pre-eminently free from interpolations, Western, Alexandrian, or Syrian, it cannot but be pre-eminently full of what may relatively to the Received Text be called omissions.⁶³

Hort attempts to convert one of the more embarrassing features of his favourite manuscript Vaticanus, its frequent omissions, into an additional proof of its general excellence. His proof of Vaticanus' virtue here rests upon the canon 'Prefer the Shorter Reading,' but the canon itself rests upon the assumed superiority of manuscripts like Vaticanus that tend to omit.

Finally, to bring our historical survey up to date, we may observe that modern presentations of the canons in the standard handbooks offer us little in the way of footnotes or bibliographical details pointing us to technical studies validating the transcriptional canons. Instead, the most common method of substantiating the canons is to illustrate them rather than prove them, often by citing a few convenient examples.

Thus, both Metzger and the Alands in their manuals first state their canons and then illustrate them as they evaluate selected passages in the New Testament. The Alands give "selected examples"⁶⁴ and Metzger talks of "illustrative examples."⁶⁵

⁶² If we may quote Royse somewhat out of context, *Scribal Habits*, 2008, 4.

⁶³ B.F. Westcott and F.J.A. Hort, *The New Testament in the Original Greek*, Vol. 2: Introduction, 2nd ed., (London: Macmillan, 1896), 235.

⁶⁴ K. and B. Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism*, Translated by E. F. Rhodes, 2nd Edition, (Grand Rapids, MI: Eerdmans, 1989), 312.

⁶⁵ B. M. Metzger, *The Text of the New Testament: Its Transmission, Corruption and Restoration*, 3rd Enlarged Edition (New York and Oxford: Oxford University Press, 1992), 219.





The history of the canons can be summed up for us in Royse's words: "Regrettably, though, most presentations of these canons are not — as far as one can tell from the exposition — based on the actual knowledge of documents of which Hort speaks, but rather appear to rest upon *a priori* reflections on how scribes behaved (or *must* have behaved)." ⁶⁶ Or, to quote Robinson's dissertation, "lacking detailed research data, textual handbooks have postulated how scribes 'likely' would have corrupted the NT text." ⁶⁷

The problem, as we now know, is that scribes did not corrupt the text by adding to, polishing, improving and harmonising it, the way the handbooks and heroes of the discipline have told us. This, of course, brings us back to Parker's paradox: if, as the evidence shows, scribes actually tended to omit, how did the text grow over time?

Scribal Habits and the History of the Text

In this section, we will look at the two main theories that have been advanced to explain the history of the text; we will see why the evidence from scribal habits shows both of these two theories to be flawed. In their place, we will see that another theory of the history of the text not only explains all the evidence but also solves the paradox of how a text that was losing material (as scribes omitted) ended up becoming the later, longer, TR.

The first theory of the history of the text is, of course, the idea that the text expanded as scribes added to it, polished it up, harmonized it and removed difficulties. The main problem with this "transcriptional theory" is that scribes tended to omit rather than add. However, there was another less obvious problem that this theory could not handle. This was the fact that not only did the text become longer over time, but that it also became more homogeneous. If individual scribes added to the text howsoever the urge took them, then why did their texts converge the further the process went?

Zane Hodges best describes this problem:

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, and cultural circumstances in which the New Testament was copied, imposes impossible strains on our imagination. ⁶⁸

This second problem with the "transcriptional theory" prompted a second theory of the history of the text, suggested by various textual scholars like Bengel, Semler, Griesbach and Hug, but pre-eminently by Westcott and Hort. To the original idea of the "filling out" of the text by scribal activity was added the idea that later forms of the text were the result of recensions, works of wholesale editorial revision. Thus, Hort wrote, "As we have already observed, the Syrian [i.e. Byzantine] text must have been due to a revision which was in fact a recension, and which may with fair probability be assigned to the time when Lucianus taught at Antioch," ⁶⁹ and "a work of attempted criticism, performed deliberately by editors and not merely by scribes." ⁷⁰

Westcott and Hort (WH) based their recension theory on three pillars: (1) the evidence of confections in the Byzantine text, (2) the Ante-Nicene Patristic evidence, and (3) the internal qualities of the Byzantine text (see their Introduction pages 93 to 118).

⁶⁶ Royse, *Scribal Habits*, 2008, 4 (emphasis in original).

⁶⁷ Robinson, *Scribal Habits*, Abstract, 1.

⁶⁸ Z. C. Hodges, *A Defense of the Majority Text*, Unpublished course notes, Dallas Theological Seminary, 1975.

⁶⁹ Westcott and Hort, *Introduction*, 102

⁷⁰ *Ibid.*, 133.





Regarding pillar (2), there is no evidence, patristic or otherwise, since WH's time that has substantially changed the situation. Despite numerous individual Byzantine readings turning up in the early papyri, there are still no manuscripts or Fathers earlier than the fourth century who bear witness to its text as a whole.

However, the accumulating evidence from scribal habits renders pillars (1) and (3) problematic. This is because pillars (1) and (3) themselves lean heavily upon the transcriptional canons. Thus, WH argued that conflation of the two earlier "Western" and "Neutral" texts to form the Byzantine text were a more likely explanation than the idea that omission of material might have occurred in both the Western and Neutral texts. This is because WH believed that scribes acted "under the influence of an impulse to omit no recorded matter."⁷¹

The studies into scribal habits provide us with an alternative explanation for WH's "conflations." That is, the evidence from scribal habits shows that the main manuscripts of the Alexandrian and Western texts are given to precisely the sorts of small omissions that would account for the textual phenomena WH called attention to.

Of course, there are other problems with WH's theory of conflation. Against the eight examples they offered, there are hundreds of other cases where the Byzantine text neither conflates differing Alexandrian and Western readings nor, more significantly, follows their readings when they agree, in direct contradiction to the idea. The eight examples offered (whose evidence is now rendered doubtful) are swamped by hundreds of adverse cases.

The third pillar upon which the recensional argument was based was the internal evidence of the Byzantine text. We need not repeat WH's famous description of the Syrian text here;⁷² suffice it to say that the Byzantine text is a fuller, clearer, more harmonious and more sensible text than the Neutral text. WH's arguments for Byzantine inferiority on account of these characteristics lose much of their force once we discover that scribes tended to produce precisely the sort of corruptions that characterise the Neutral text: shorter readings, terse austerity, discord and less sensible readings.

The evidence about scribal tendencies (particularly in the earliest period) has changed the situation dramatically. When we ask the basic question of New Testament textual criticism, "Which reading is more likely to have given rise to the other readings?", the cumulative force of one textual variant after another leads to this inescapable conclusion: it is the Alexandrian text that is more easily explained as resulting from scribal corruption than the Byzantine text (leaving the Western text aside for the moment). Case by case examinations of internal evidence tends to vindicate a Byzantine-priority position.

In addition, as Harry Sturz noted in commenting on WH's description, "'The Byzantine text does tend to be simple, lucid, full, unpretentious and plain in style. Much of WH's description is *a propos*. However, it should be noted that their description of the 'Syrian' text, with few changes, could also be taken as an acceptable description of the Hellenistic Greek of the first century!"⁷³

Of course, many textual critics rejected recensional theories long before the evidence from scribal habits started accumulating.⁷⁴ Scrivener referred to recensions as a playground for "pleasant speculations which may amuse the fancy but cannot inform sober judgement."⁷⁵ The gravest problem for any "recension theory" is a lack of documented, credible historical evidence for such a thing ever having

⁷¹ Ibid., 97.

⁷² Ibid., 134-5.

⁷³ H. A. Sturz, *The Byzantine Text-Type and New Testament Textual Criticism*, (Nashville, TN: Thomas Nelson Publishers, 1984), 108.

⁷⁴ See, for example, Tregelles in his *Account of the Printed Text*, 90, or F.E. Kenyon in his *Handbook to the Textual Criticism of the New Testament*, 2nd ed. (Grand Rapids, MI: Eerdmans, 1951), 324-5.

⁷⁵ Scrivener, *A Plain Introduction to the Criticism of the New Testament*, 4th ed. 2 vols. (London: George Bell and Sons, 1894) 2:272-3.





taken place. When we ask the standard questions about a supposed recension (who, when, where, how, why), we are not offered answers that can be called historical in any real sense, that is, substantiated by documentation. Invoking a recension to solve Parker's paradox is to resort to a contrived, *deux ex machina* solution.⁷⁶

Thus, of the two main theories of the history of the text, one is false and one is fanciful. However, the growing number of studies in scribal habits offer the promise of progress in understanding the history of the New Testament text, providing us with an alternative explanation, not only for the history of the text, but also the two puzzles connected with the history of the text: (1) how a text that was shrinking managed to become longer over time, (2) how the text became progressively more homogeneous over time. This alternative explanation arises from evidence that is literally staring us in the face when we look at our manuscripts: corrections.

Corrections and the History of the Text

How does evidence from corrections suggest an explanation for the historical development of the NT text? In the present author's study, corrections of singular readings showed correctors disproportionately tending to remedy omissions. Thus, among the 431 singular additions or omissions later corrected, there were 133 additions and 298 omissions corrected (a ratio of 31:69). Notice that this disproportionate tendency to correct omissions is even stronger than the initial tendency to omit (at roughly 40:60). Among the corrections which were themselves singular readings,⁷⁷ there were 40 additions and 27 omissions (a ratio of 60:40).

To get a snapshot of how correction affected the text, we may look at two manuscripts. In the present author's study, Sinaiticus had 90% of the singular omissions corrected (56 out of 62) but only 78% of additions (28 out of 36). Similar results can be seen in the correction of \mathfrak{P}^{66} (drawn from Royse's data by the present author): 21 of the 38 singular additions were corrected (55%), whereas 61 of the 79 singular omissions were corrected (75%).

These substantial disparities in the corrections of addition and omission in individual manuscripts match the picture seen above for manuscripts as a whole: correctors seemed to have a far stronger inclination to correct omissions than additions. In fact, if we look at the data from Sinaiticus and \mathfrak{P}^{66} we see that the effect of correction was not simply to remedy the loss of text, but to markedly lengthen the text.

Thus, if we notice the Net Gain/Loss row of figures in the table below, we see that the correction of Sinaiticus resulted in a text that was 28 readings longer after correction than before, while the text of \mathfrak{P}^{66} was 40 readings longer after correction.

| | Sinaiticus | | | \mathfrak{P}^{66} | | |
|--------------------------------|------------|-----------|-----|---------------------|-----------|-----|
| | Singulars | Corrected | % | Singulars | Corrected | % |
| Additions | 36 | 28 | 78% | 38 | 21 | 55% |
| Omissions | 62 | 56 | 90% | 79 | 61 | 75% |
| Net Gain/(Loss) in Readings | (26) | 28 | | (41) | 40 | |

⁷⁶ Horace wrote: "do not bring a god on to the stage unless the problem is one that deserves a god to solve it" (*Ars Poetica*, line 191f.).

⁷⁷ Not necessarily all corrections of singular readings, although some fell into this category.





Similarly in Robinson's study, of the 1110 singular additions and omissions, there were 35 additions corrected (31%), but 79 omissions corrected (69%). The disproportionate tendency to correct omissions over additions is again prevalent. In Sinaiticus, the most corrected manuscript, there were 9 additions corrected (36%), but 16 omissions corrected (64%), despite the fact that the ratios of singular additions to omissions were much closer: 24 additions and 27 omissions (47:53).

These figures suggest three factors which explain the expansion of the text as it was corrected and later copied. Firstly, correction would ordinarily remedy and reverse the predominant fault, omission, thus lengthening the text. Secondly, in addition, there seemed to be a strong bias toward correcting omissions, while there was a more indulgent attitude to additions, reinforcing the tendency towards the expansion of the text. Thirdly, when correctors themselves created singular readings, these readings tended to be additions, not omissions. Thus, the process of correction heavily tended to produce a longer text relative to our early witnesses.

It might be argued that, if only 15% of singulars in the present author's study were corrected (10% in Robinson's), correction must not have been a major force in the textual transmission of the New Testament. However, while later manuscripts show less evidence of correction, our papyri and early majuscules have huge numbers of corrections. Thus, there are 'about 450'⁷⁸ corrections in \mathfrak{P}^{66} in John's Gospel alone, 183 in \mathfrak{P}^{46} , 86 in \mathfrak{P}^{72} and \mathfrak{P}^{75} has 116.⁷⁹ From the second through to the fifth centuries correction was far from an insignificant force in the process of textual transmission. Its effect was to reverse the shrinkage of the text we see in our early witnesses. The history of the text therefore is the story of shrinkage due to scribal omission followed by expansion due to correction, followed by stabilization.

When we turn to the other three transcriptional canons, the results follow on logically from the results for the Shorter Reading canon. Thus, in the present writer's study, correction of singular readings under the Harder and Harsher Reading canons presented as follows:

| Nonsense | Harder Sense | Harder Style | Neutral | Easier Style | Easier Sense |
|----------|--------------|--------------|---------|--------------|--------------|
| 145 | 42 | 100 | 41 | 18 | 0 |

Correctors heavily tended to deal with nonsense (41%) and harder sense (12%), thus improving the sense of the text. They did not correct any of the few singulars that had already improved the sense of the text. Thus, scribes and correctors tended to do the exact opposite of each other: scribes created difficulties while correctors cleaned up after them.

When corrections amongst singular readings that had a stylistic effect upon the text were analysed, there were 100 harsher readings corrected, as against 18 smoother readings corrected. Thus, correction again heavily tended to produce a smoother and more polished text.

This result aligns with Gordon Fee's study of \mathfrak{P}^{66} and its corrections.⁸⁰ Fee noted that correction heavily tended to smooth the text, and concluded: "on points of style, therefore, it should be carefully observed that this Egyptian (perhaps Alexandrian) scribe-turned-recensor is neither creating a 'scholarly' text nor seeking to preserve the 'Johannine original'; he is rather producing a good, readable text."⁸¹

Likewise, the results for the correction of singular readings catalogued under the last canon, 'Prefer the Non-Harmonized Reading,' are as follows. There were 14 harmonizations later corrected, as against 79

⁷⁸ G.D. Fee, *Papyrus Bodmer II* (\mathfrak{P}^{66}): *Its Textual Relationships and Scribal Characteristics* (SD34; Salt Lake City, UT: University of Utah Press, 1968) 57. Fee says "finality of judgement is difficult here." Royse counts 465 (*Scribal Habits*, 409). In Fee's Appendix C, there were 50 corrections which added to the text, but only 11 that omitted material, 123-27.

⁷⁹ Royse, *Scribal Habits*, 2008, 211, 558, 625.

⁸⁰ Fee, *Papyrus Bodmer II*, 57-75.

⁸¹ *Ibid.*, 73.





disharmonizations later corrected. Thus, disharmonization was five times more likely to be corrected than harmonization, resulting in a more harmonized text after correction than before.

These results are probably largely attributable to the tendency of correctors to deal disproportionately with omissions, which often result in disharmonization, and nonsense readings.

What sort of process of correction was this? The correction that is observed among the singular readings is best described in the opening words of the Epistle to the Hebrews: “at sundry times and in divers manners.” It was sporadic, localised, uneven in its results, disinterested and without agenda — exactly the opposite of a work of recension, where a uniform and ecclesiastically approved text was imposed from above. Sometimes correction was carried out thoroughly and sometimes scarcely at all. Sometimes, it involved little more than hasty proof-reading, while at other times it may have involved correction against the exemplar, at other times, correction against another manuscript, and occasionally, against another manuscript of very different textual ancestry.

Nevertheless, while the result of correction against other manuscripts would have been at the first simple mixture, over time it would have resulted in the ever-closer convergence of the texts of the manuscripts being used in certain locations (thus producing our text-types). This is because the sort of correction we often see operating in our manuscripts — correction via comparison against other manuscripts, as opposed to pure conjecture — tends to bring manuscripts into more conformity with each other. Added to the historical fact of the contraction of Christendom following the Muslim conquests of Christian lands, correction would appear able to account for the increasingly convergent Byzantine text-form.

This is not to argue that the later Byzantine text was a convergence of the Alexandrian and Western text-types; the innumerable and varied differences between these three texts argue for their substantial independence. Problems of persecution, geographical distance and available time make it unlikely that correction produced a hybrid, homogeneous Byzantine text from the other two by the mid A.D. 300s, when it was clearly already in existence.

Instead, by a process of elimination, we may discount a recensional origin for the Byzantine text (it is not the love-child of the other two text-types), as well as the idea of *de novo* creation of the Byzantine text (it is a cousin or nephew of the other text-types, not an adopted unrelated orphan). This leaves us with the default option for the origin of any text: the Byzantine text descended from earlier ancestors of its own. It was not edited, nor created — it was transmitted.

The Byzantine text-type, descending from ancestors that remain lost to our investigations, doubtless also underwent its own process of correction. This is because correction was itself part of the “default” process of transmission. It is therefore difficult to judge how much the Byzantine manuscripts we possess differ from their ancestors which we do not possess.

One small note concerning a current issue in studies of the history of the text is worth mentioning at this point. The last few decades have seen a rise in claims that the original form of the New Testament is irrecoverable due to the freedom that early scribes showed in their handling of its text. Unfortunately, this claim has often been based on a few, cherry-picked, atypical cases of scribal freedom. Robinson’s study showed, via his Table 5 which projected the totals of singulars in all of the 220 manuscripts, that most scribes were careful and faithful conservationists, not creative innovators. Apart from the Western text, which was in parts the ancient equivalent of one of our modern paraphrases, the vast majority of our early witnesses were characterised only by carelessness and sloppiness. Thanks to studies into scribal habits in these manuscripts in the earliest period, we are now familiar with the typical effects of such scribal behaviour.

In other words, while Western wildness was, by definition, unrestrained (and happily confined to very few witnesses), the early Alexandrian errors were mostly small omissions or clumsy mistakes that produced little more than a more terse and disharmonious text. The intention of most scribes of





Alexandrian manuscripts seems to have been to copy accurately, and although the reality never quite approached the aim, it is simply untrue to say that Alexandrian scribes treated the text as a plaything they felt free to remodel after their own tastes.

Finally, we must consider the result of the correction process. Despite the bias it showed toward correcting certain sorts of errors (omissions, harsher readings, disharmonizations and more difficult readings), we cannot view the correction process as a negative development. It is true to say that it had largely the opposite effect; as the very term ‘correction’ implies, the result was an improvement. This is not to say that correction did not introduce its own errors, but rather a generalised assessment of the fact that, as we see in our manuscripts, correction usually restored what had been unintentionally lost and damaged.

Colwell correctly described the effect of correction upon the history of the New Testament text as follows:

The progression when uncontrolled was characterized by scribal changes — when controlled it was characterized by editorial selection. Each of these includes improvements as well as corruptions; but in general scribal change meant corruption, and editorial selection with its consequent controls meant improvement over the preceding anarchy and meant also the blocking of major corruption ... and one comment on the editorial work of scribes and editors needs to be made now — capable scribes and editors often left the text closer to the original than they found it.⁸²

Scribal Habits and the Praxis of Textual Criticism

We return here to the attempt of some textual critics (like those whose objections we earlier reviewed) to retain the ‘Shorter Reading’ canon despite the fact that the evidence clearly shows the opposite.

What accounts for such an attitude? Parker gives the game away by urging the retention of the ‘Shorter Reading’ canon (even though contradicted by evidence) because of its usefulness as a stick with which to beat the Byzantine text. It becomes clear that the traditional canons and the preference for Alexandrian manuscripts are very nearly inseparable; to reject the canons would be tantamount to abandoning Alexandrian priority.

However, textual criticism cannot simply retain the transcriptional canons out of respect for the historical heroes of the discipline who framed them (the historical argument), nor can it retain the canons because of preferences for certain documents (the manuscript argument). It certainly cannot retain the canons because of the *ex cathedra* pronouncements of current authority figures.

Parker’s policy seems back to front: we must continue with *a priori* preferences for certain manuscripts, regardless of what the transcriptional due-diligence checks disclose about the nature of corruption in their text. Instead of becoming suspicious about Alexandrian manuscripts once we find out that they perfectly match the profile for typical scribal corruption, Parker’s advice seems to be that we should ignore the auditor’s report.

It is the exact opposite of what Hort’s famous dictum (“knowledge of documents should precede final judgement upon readings”) suggests we should do. First, gather evidence by which we may evaluate manuscripts, and after that, make textual decisions. Estimates of the value of individual manuscripts and groups of documents cannot be based on how closely they adhere to the “original text” (we do not possess it), or even some “standard text” (such a text will inevitably reflect our *a priori* manuscript preferences). Estimates must be based on objective evidence like that gleaned from scribal habits to establish the character and value of these same manuscripts.

Where once Byzantine witnesses were thought to have been corrupted by innumerable scribal additions, the auditor’s report now recommends that another document group pock-marked with omission have its

⁸² E. C. Colwell, “Hort Redivivus: A Plea and a Program,” in *Studies in Methodology in Textual Criticism of the New Testament* (Leiden: Brill, 1969), 164, 168.





credit rating cut (fittingly enough) to B-grade. Of course, by comparison, Western manuscripts make even Alexandrian manuscripts look good, but more objectively, Alexandrian manuscripts contain hundreds of singular readings (and were it not for their fragmentary nature, some of the early Alexandrian papyri would have thousands of singular readings). Even more tellingly, evidence from “groups of documents” shows Alexandrian texts sharing the same tell-tale signs of genealogical corruption: hundreds of omissions that produce a terser, more discordant, less sensible text. By comparison, the Byzantine text emerges from studies into scribal habits with its reputation relatively intact.

That the traditional canon is wrong means that there must, of necessity, be changes to the text, and not merely in a handful of cases. For, while individual cases are disputable, any text that consistently prefers shorter readings over longer readings, particularly in cases of small add/omit variation units, has a correspondingly diminished claim to represent the genuine text of the New Testament. Privileging antiquity over propinquity (to which we may also now add transcriptional probability), our current critical text tends to follow the readings of small related groups of documents whose common fault is large numbers of such omissions.

What, then, should be done about the New Testament text in view of the studies into scribal habits? The current critical text prefers a shorter reading about 60% of the time, in direct contrast with reality, but in keeping with the traditional canon. Thus, from this author’s count, the critical apparatus of NA27 in Matthew has a longer reading 256 times (39%), a “middle” reading 20 times (3%), and a shorter reading 379 times (58%).⁸³ Assuming it highly unlikely that the editors of the current critical text would convert to a position of Byzantine priority, what should they be doing with the fact that scribes tend to omit 60% of the time and add 40%?

Realigning the text with reality would mean promoting over 100 longer readings to the mainline text of Matthew. Extrapolating these figures to the rest of the New Testament, we are looking at nearly 1000 changes. This is, of course, a rough figure; each case would be judged on its merits in line with the customary caveat: all other things being equal. What the exercise demonstrates, however, is how serious is the need for an overhaul of the NT text: the current critical text has suffered the matador’s “death by a thousand cuts.”

Prospect

“Totschweigtaktik” (“death by silence”) was a 19th century Viennese society term for killing off ideas not considered culturally acceptable to the literary or artistic elites.

Thirty years on from Robinson’s dissertation and with other similar studies corroborating the findings, there does not appear to be any move towards rectifying the problems with the transcriptional canons, nor any interest in addressing the significant implications for virtually every area of New Testament textual criticism.

Astonishingly, not only was the 2010 SBL Greek NT an even shorter text than NA27,⁸⁴ but there has not been the slightest ripple or murmur of criticism on this account. Clearly, the discipline is either in a state of collective cognitive dissonance or, more likely, the chilling effects of “death by silence” are forcing any independent-minded scholars to keep their heads below the parapet.

Instead of calls for editors to heed Griesbach’s exception clauses and reinstate short omissions into the text, critics have instead directed their fire at studies like Royse’s for allegedly failing to take account of Griesbach’s exception clauses. The double-standard is ironical: the only text to reinstate short omissions

⁸³ Excluding singular readings and variant readings only found in minuscule MSS.

⁸⁴ The Bibleworks computer program counts 138020 words in NA27 and 137647 words in SBL. Even with the addition of the *Pericope Adulterae* (John 7:52 – 8:11) which SBL entirely omits but NA27 retains in double brackets, SBL would still have only 137834 words.





on the basis of the “exception clauses” remains Griesbach’s. Without the least attempt to display even-handedness, the calls emphasising “nuance” sound largely hortatory: they merely give comfort (and cover) to those wishing to contentedly persist with discredited canons and an outdated text.

How long New Testament textual criticism as a discipline will remain in denial about these matters is a question others may wish to answer, but it appears unlikely that anything will change in the near future.

For all that, we can say that Professor Robinson was right: the evidence from scribal habits necessitates a drastic revision of the transcriptional canons. Not only so, but revised transcriptional canons will also entail abandoning false and fanciful theories of the history of the text, re-evaluating the worth of our manuscript witnesses and reassessing many hundreds of places of scribal variation in the New Testament text.





A TRANSLATOR TAKES A LINGUISTIC LOOK AT MARK'S GOSPEL

by John R. Himes

Introduction

At the close of World War Two, the Allies had just released the Potsdam Declaration, in which unconditional surrender was demanded from the Japanese. On the Japanese side in that year of 1945, the hard liners opposed surrender, and especially any deal that deposed the emperor. In particular, Baron Suzuki, who was the Prime Minister at the time, used the term *mokusatsu* (黙殺), which is often translated “no comment,” but can be translated “ignore.” This word actually has a quite strong semantic content, since the first *kanji* (Chinese character) means “silent” (used in a Chinese compound noun here, but also as a verb, “to be silent,” or in the command “Silence!”) and the second *kanji* is the character for “kill.” The Japanese refused to surrender and the United States dropped the atomic bomb on Nagasaki and Hiroshima.

Though this event took place prior to the advent of discourse analysis (DA) in linguistics, it may be seen as a failure on the Japanese side to properly understand the complete discourse called the Potsdam Declaration. If this declaration had been viewed as a serious discourse rather than Allied posturing, the Japanese side would surely have seen that they had no option but unconditional surrender. The Potsdam Declaration used such phrases as “The Japanese military forces shall be completely disarmed,” and, “The alternative for Japan is prompt and utter destruction.” The whole discourse was a severe and strong warning. However, the Japanese were used to bluster and bluff in such documents and did not take the Allied declaration seriously enough. The rest is sad history.¹

This paper aims to introduce DA, discuss briefly how it is being used in Bible translation, then discuss its current and potential usage in textual criticism. For this last purpose, an analysis will be done of the literary functions and textual criticism in the Gospel of Mark of the adverbial form of *εὐθὺς* (occurring as both *εὐθὺς* and *εὐθὺς*).² Maurice Robinson has done a similar study on the critical text omissions of occurrences of *οὐν* in John's Gospel.³

Discourse Analysis

Discourse analysis was originally conceived by Zellig Harris in the late 1940's and early 1950's. Harris “sought a way to reduce the set of complex sentences of discourse to a form in which they would be susceptible to analysis by the methods devised for sentences and their parts. He proposed the use of certain ‘transformations’ to ‘normalize’ the discourse, to transform complex sentences into uniform simple structures to which the methods of structural linguistics might apply: segmentation of sequences, substitution of elements, classification, and so on.”⁴

- 1 Many histories of the war give an account of this event. For a typical account, see *The Pacific War 1941-1945*, by John Costello (New York: Harper Perennial, 1981, 2009), 587-588. This essay presents by necessity a simplified version of the historical events. Examining the nationalistic stance of the rightists in the Japanese government of the time would be a detour from the purpose of this essay.
- 2 Lexicons generally define these two adverbs similarly, though BDAG gives a slightly wider range of meaning to *εὐθὺς*. See Walter Bauer, Frederick Danker, Kurt Aland, Barbara Aland, F. W. Gingrich, eds., *A Greek-English Lexicon of the New Testament and Other Early Christian Literature* (3rd ed.; Chicago: University of Chicago Press, 2000), 405-406.
- 3 See Maurice Robinson, “The Recensional Nature of the Alexandrian Text-Type: A Response to Selected Criticisms of the Byzantine-Priority Theory,” *Faith and Mission* 11 (Fall 1993): 51-57.
- 4 Noam Chomsky, *Language and Responsibility* (New York: Pantheon Books, 1979), reprinted in Noam Chomsky, *On Lan-*





Brian Paltridge further notes, “The term discourse analysis was first introduced by Zellig Harris in 1952 as a way of analyzing connected speech and writing. Harris had two main interests: the examination of language beyond the level of the sentence and the relationship between linguistic and non-linguistic behavior. He examined the first of these in most detail, aiming to provide a way for describing how language features are distributed within texts and the ways in which they are combined in particular kinds and styles of texts.”⁵

In the years since Harris invented the concept, the usage of DA has increased considerably in scope. “Broadly speaking, discourse analysis concerns a wide array of linguistic dynamics that interplay in language, various forms of discourse expressed within languages, and specific contexts in which those forms are expressed.”⁶ Currently, DA is being viewed as simply any examination of a discourse. According to one scholar, the term is used “effectively of any analysis of discourse.”⁷ Another source says,

The term ‘discourse analysis’ has come to be used with a wide range of meanings which cover a wide range of activities. It is used to describe activities at the intersection of disciplines as diverse as sociolinguistics, psycholinguistics, philosophical linguistics and computational linguistics. Scholars working centrally in these different disciplines tend to concentrate on different aspects of discourse.⁸

In the light of that ambiguity,⁹ we may note that DA has been variously defined in recent years. One scholar defines DA in this way: “Discourse analysis may be defined as ‘a process of investigation by which one examines the form and function of all the parts and levels of a written discourse, with the aim of better understanding both the parts and the whole of that discourse.’”¹⁰ Another defines it as “an approach to the analysis of languages that looks at patterns of languages across texts as well as the social and cultural contexts in which the texts occur.”¹¹ This second definition in particular gives a benchmark for the DA method we will use in this paper.¹²

One area in which DA is being effectively used is in Bible translation. The translator may examine the functions of a certain word in the discourse in order to determine whether to translate by concordance or whether or not a certain context demands a different rendering. He may examine the source document to determine if the author uses a lexical unit in a unique way when compared to other authors. If he is using a form of generative/transformational grammar in his work, he may research the source document to learn if a rule should be lexically governed.¹³

Here is one simple example of how DA can help a translator. Japanese utilizes perhaps more “respect language” (i.e., honorifics) than any language in the world. Thus, when considering style in the Gospels in particular the translator must judge what level of respect language Christ’s discourses should use. The translator must analyze the discourse in question and determine whether to use a normal polite level, which would indicate Christ’s equal treatment of every individual (e. g., Matt. 9:36), or a less formal level of language, which might be used to show how Christ spoke with authority (e. g., Matt. 7:29).

guage (New York: The New York Press, 1998), 120.

5 Brian Paltridge, *Discourse Analysis* (London: Continuum, 2006), 4.

6 George H. Guthrie, “Discourse Analysis,” in *Interpreting the New Testament* (eds. David Alan Black and Daniel S. Dockery; Nashville: Broadman and Holman, 2001), 254.

7 *The Concise Oxford Dictionary of Linguistics*, by P. H. Matthews (2nd ed.; Oxford: Oxford University Press, 1997 and 2007), 107.

8 Gillian Brown and George Yule, *Discourse Analysis* (New York: Cambridge University Press, 1983), viii.

9 For a good discussion of the ambiguity of the terminology and methodology of DA, see Jeffrey T. Reed, “Discourse Analysis As New Testament Hermeneutic: A Retrospective and Prospective Appraisal,” *The Journal of the Evangelical Theological Society* 39:2 (June 1993): 223-240.

10 Guthrie, 255.

11 Paltridge, 1.

12 For one example of this type of approach, see Kathleen Callow, “The Disappearing De in 1 Corinthians” in *Linguistics and New Testament Interpretation, Essays on Discourse Analysis* (ed. David Alan Black; Nashville: Broadman Press, 1992), 183-193. For an example that better fits the first definition, see David Alan Black’s use of colon structure in “Hebrews 1:1-4: A Study of Discourse Analysis,” *Westminster Theological Journal* 49:1 (Spring 1987): 175-194.

13 Dynamic/functional equivalence and optimal equivalence, though very dissimilar in their methodology, both use transformational grammar in their approach to Bible translating.





Using Discourse Analysis in the Textual Criticism of the Gospel of Mark

The Gospel of Mark is often the first book of the New Testament translated in a new version of the Bible due to the simplicity of its original Greek and its utility in evangelism and discipleship. This makes it a good candidate for a DA from a translator's viewpoint. The tool of DA can also be used as a tool for examining internal evidence in the textual criticism of the Greek New Testament. Maurice Robinson is a pioneer of this technique. In defending the longer ending (LE) of Mark, Robinson notes first of all that "the pattern of promise/prediction and fulfillment permeates the Gospel of Mark."¹⁴ Robinson further points out, "The LE, however, follows Mark's pattern, and makes the cycle complete: OT prophecy (Mark 12:36), Jesus' prediction (Mark 14:62), and the actual fulfillment (Mark 16:19)."¹⁵

Robinson's point is that since the discourse of Mark has a theme of promise/prediction and fulfillment, this pattern of the Markan discourse serves as internal evidence for the longer ending of Mark, which has a similar pattern.

Again, Robinson discusses another example of the use of DA by Warren A. Gage, who saw a theme of Jesus as a "new Elijah" in Mark.¹⁶ Gage's point in regards to textual criticism is that there are definite parallels between the lives of Elijah and of Christ as presented in Mark's Gospel. One of those parallels is the ascensions of both Elijah and Christ. The lack of an ascension passage for Christ in Mark, as is included in the LE of Mark, would damage the evident theme of parallels between Elijah and Christ in the gospel.

In a further DA of Mark, Robinson explores various parallels between critically undoubted portions of Mark and the LE in which the same thematic endings are present.¹⁷ For example, during the commencement of Christ's ministry in Mark 1, Christ heals the sick, casts out demons, leads his disciples into the surrounding towns, etc., all of which have thematic parallels in the LE of Mark. Robinson examines similar thematic comparisons between the LE and the first commissioning of the twelve (3:14-15), the subsequent commissioning (6:7-13), and another segment concerning Christ's ministry (7:24-8:38). So Robinson clearly believes that examining the entire discourse of Mark is profitable for the textual criticism of Mark's ending.¹⁸

This paper will do a DA of the adjective *ευθους* in its various adverbial forms,¹⁹ words not exclusive to Mark but which, as is well known, have an extensive usage unique to Mark. Then, this writer will conduct an examination of the variants of *ευθους*/*ευθεως* in the Greek New Testament text, using this DA in an effort to supplement the traditional tools of internal evidence in the textual criticism of Mark.²⁰

14 Maurice Robinson, "The Longer Ending of Mark as Canonical Verity" in *Perspectives on the Ending of Mark*, (ed. David Alan Black; Nashville: Broadman and Holman, 2008), 66.

15 Ibid., 67.

16 Ibid., 67-68.

17 Ibid., 68-72.

18 For a very recent example of the use of DA in the textual criticism of Mark, see H. W. Shin, "The Historic Present as a Discourse Marker and Textual Criticism in Mark," *The Bible Translator* 63 (January 2012): 39-51.

19 There are three adverbial forms from the adjective *ευθους*: *ευθυ*, *ευθους* and *ευθεως*, the first of which does not appear in Mark. Oddly enough, the Alexandrian text type uses the *ευθους* adverbial form almost exclusively in Mark though it uses *ευθεως* elsewhere in the New Testament, while the Byzantine uses *ευθεως* exclusively in Mark. A possible explanation that may not yet have been pursued by scholars is that this is a regional variation. We might compare the Greek usage of the first century Roman Empire to English usage in modern times. Just as with English usage in Britain and America in modern times, the Byzantine region was where Greek was a first language to its speakers (one argument for Byzantine priority). However, in the Alexandrian region, Greek was usually a second language as English is in modern Singapore (remembering that there were also well known Greek scholars in first century Alexandria). Therefore, the possibility exists that *ευθυ* and *ευθους* as used wherever the scribes of the Alexandrian Mark lived were considered to be substandard forms of the adverb by those living in areas where Greek was the primary language.

20 For the purposes of this essay, Greek quotations will originally be taken from *The New Testament in the Original Greek, Byzantine Textform* (2nd ed., ed. by Maurice Robinson and William Pierpont; Southborough, MA: Chilton Book Publishing, 2005), to be referred to as the Byzantine Textform. All translations are by this writer. The Byzantine Textform will be compared with the United Bible Societies (UBS) *The Greek New Testament* (ed. Barbara Aland, et. al., 4th revised ed.; Stuttgart: United Bible Societies and Deutsche Bibelgesellschaft, 1994), with reference to the apparatus of *Novum Tes-*





The term occurs over forty times in Mark, with six of those times being textual variants. In 1:31, 2:2 and 5:13 and 5:36, the UBS does not have *ευθως/ευθεως* but the Byzantine Textform includes it. In 5:42 at the end of the verse, the UBS has *ευθως* in brackets, but the Byzantine does not have it. (Both have *ευθως/ευθεως* at the beginning of the verse.) Finally, in 7:35 the UBS has *ευθεως* in brackets, as opposed to its normal *ευθως*, thus agreeing with the Byzantine. In order to determine which manuscript tradition is correct in these cases, a DA of the functions of *ευθως/ευθεως* in Mark will be used.²¹

This adverbial form of *ευθως* appears in a number of contexts common to Mark. In virtually every usage the meaning is straightforward, with no idiomatic usage. One possible exception is when *και ευθεως* occurs at the beginning of its clause. According to G. D. Kilpatrick, in Mark,

Twenty-nine instances and two variant readings appear in the phrase *και ευθως* at the beginning of their clause. We have one example of *αλλ' ευθως* in the same position at vii 25. Where there is a preceding subordinate clause or a participle equivalent to a clause, *ευθως* begins the main clause at i 43; iv 15, 16, 17, 29; v 2; vi 54; ix 20, 24; xiv 45, in all ten instances. There remain five exceptions to the initial position, i 28; v 13, 36, 42; vi 25; vii 25 and the two variant readings at i 31; iii 6. v 36 is hardly an exception.²²

In Robert Stein's view, "Frequently, especially when beginning an account, the expression is little more than a mild conjunctive, meaning 'and then.' In such instances, it is unwise to associate a strong temporal meaning with the expression."²³ However, Stein's view of this weakened meaning is given without detailed analysis. The conservative translator will no doubt translate *και ευθεως* literally in most cases.

One common appearance of *ευθως/ευθεως* in Mark is in healing miracle pericopes. There are seven different times in the Byzantine Textform when the word occurs in healing accounts, including both immediate healing itself and actions taken immediately after the healing. The UBS text omits one of these times (1:31), and the Byzantine Textform omits the word one time when the UBS text has it in brackets (7:35).

In Mark's view, many healings occurred immediately. For example, in 1:42 we read, *Και ειποντος αυτου ευθεως απηλθεν απ' αυτου η λεπρά και εκαρθαρισκη*. ("And when he spoke, the skin disease immediately departed from him, and he was cleansed.") Again, in 5:29 we have, *Και ευθεως εξηρανθη η πηγη του αιματος αυτης*. ("And immediately her flow of blood was stanchd.")

A similar immediate healing occurs in 10:52, *Και ευθεως ανεβλεψεν*. ("And he immediately could see.") The function of the word in these cases is clearly to emphasize the instantaneous power of Christ to heal.

Another function of *ευθως/ευθεως* in Mark's miracle pericopes is to herald an action stemming from healing. This function also emphasizes the instantaneous power of Christ to heal. For example, in 2:12 we have, *Και ηγερθη ευθεως, και αρας τον κραββατον, εξηλθεν εναντιον παντων*. ("And immediately he stood up, picked up his sleeping mat, and went out in the sight of all.") It is evident that the paralytic was healed, and was able to stand as a result of that healing. Similarly, in 5:42 Mark says, *Και ευθεως ανεστη το κορασιον και περιεπατει*. ("And immediately the young girl stood up and walked.")

In eight cases in Mark *ευθως/ευθεως* is used of the response of others to Christ, with one case where the UBS omits the word (2:2). In 1:18 we read, *Και ευθεως αφεντες τα δικτυα αυτων, ηκολουθησαν αυτω*. ("And immediately they put away their nets and followed Him.") In 3:6 we read, *Και εξελθοντες οι φαρισαιιοι ευθεως μετα των Ηρωδιανων συμβουλιον εποιουν κατ' αυτου*. ("And the Pharisees immediately went out and conspired with the Herodians against Him.") A similar usage occurs in 5:2, 6:54, 9:15, 20 and 24. The literary function of the adverb in this usage is to emphasize the fact that no one reacted normally to

tamentum Graece (ed. Barbara Aland, et. al., 27th rev. ed.; Stuttgart: Deutsche Bibelgesellschaft, 1993), or Nestle/Aland.

21 Discourse analysis examines the functions of words in a discourse as opposed to lexical studies which examine the semantic content of a word, or researches the usage of a word in the contemporary literature.

22 G. D. Kilpatrick, "Some Notes on Markan Usage" in *The Language and Style of the Gospel of Mark* (J. K. Elliott. New York: E. J. Brill, 1993), 168.

23 Robert H. Stein, *Mark* (BECNT; Grand Rapids: Baker Academic, 2008), 56.





Christ. Since He was and is the Son of God and Son of Man, no one could remain neutral to Him.

The most common function of *ευθὺς/ευθεως* in Mark's discourse is to punctuate actions taken by Christ. This usage occurs eleven times in the discourse in the Byzantine Textform, including twice when the UBS omits it (5:13, 5:36). Mark's usage of the adverb in these cases thus presents Christ as decisive and perhaps even as aggressive on occasion.

In 1:10 we read, *Και ευθεως αναβαινων απο του υδατος*. ("And immediately He came up out of the water.") Again in chapter one, we read in verse 20, *Και ευθεως εκαλεσεν αυτους*. ("And He immediately called them.") Verse 21 has, *Και ευθεως τοις σαββασιν εισελθων εις την συναγωγην, ειδιδασκεν*. ("And immediately on the Sabbath He went into the synagogue and taught.") Another occurrence is in 6:45, *Και ευθεως ηναγκασεν τους μαθητας αυτου εμβηναι εις το πλοιον*. ("And immediately He made His disciples board the boat.") In 6:50 we read, *Και ευθεως ελαλησεν μετ' αυτων*. ("And immediately He spoke with them.") See also 1:43, 2:8, 5:30 and 8:10.

There are several other functions of *ευθὺς/ευθεως* in Mark: the normal adjectival usage of *ευθὺς* in 1:3, 1:12 and 1:28; *ευθεως* heralding new beginnings in 1:10, 1:18 and 1:20; used by Christ in parables in 4:5, 15, 16, 17 and 29; used to describe sinful actions taken against John the Baptist in 6:25 and 27, and against Christ in 14:43 and 45 and 15:1; commands from Christ in 11:2 and 3. Since these passages have no textual difficulties, they will not be treated in this essay.

Before continuing, it should be understood that external evidence trumps internal evidence in Byzantine Priority methodology. Using DA in the textual criticism of the NT is a form of internal evidence, and should be subsumed under the heading of intrinsic probabilities, secondary to actual external evidence. As Maurice Robinson points out,

Merely because *και* or *ευθὺς* are "characteristic" in Mark or *οὐν* in John does not mean that one automatically should prefer such a reading over the alternatives. Stylistic criteria taken in isolation can easily lead to wrong decisions if the degree and quality of transmissional support are not equally considered. A basic assumption is that scribes in general would be unlikely to alter the style and vocabulary of a given author when copying that which lay before them.²⁴

Another relevant point to note before continuing is that the "shorter reading is better" canon of eclectic textual criticism is coming under increasing fire. As Eldon Jay Epp says, "It is the shorter reading argument that has received the most vigorous reassessment in the past three decades or so."²⁵ Surely in cases such as those under consideration in this essay, due thought should be given to the possibility that a single word was inadvertently omitted in a given text.

Unfortunately, most modern descriptions of the "shorter is better" canon fail to consider the omission of a single word through carelessness.²⁶ However, ironically enough, the "shorter is better" canon of Johann Jakob Griesbach (who "laid foundations for all subsequent work on the Greek of the New Testament"²⁷) does allow for such omissions, saying that a reading might be genuine "if that which is lacking could

24 Maurice Robinson, "The Case for Byzantine Priority" in *The New Testament in the Original Greek, Byzantine Textform* (2nd ed., eds. Maurice Robinson and William Pierpont; Southborough, MA: Chilton Book Publishing, 2005), 547.

25 Eldon J. Epp, "Issues in New Testament Textual Criticism," in *Rethinking New Testament Textual Criticism*, (ed. David Alan Black; Grand Rapids: Baker Academic, 2002), 27.

26 See Bruce Metzger's explanations of this canon in *A Textual Commentary on the Greek New Testament*, 2nd ed. (New York: United Bible Societies, 2000), 13, and *The Text of the New Testament*, 4th ed., by Bruce Metzger and Bart Ehrman (New York: Oxford University Press, 2005), 166-167 and 303, and his discussion of haplography (Ibid., 253-254). Metzger might respond by calling the addition of *ευθὺς/ευθεως* in Mark the addition of a natural complement, an error he discusses on 263-264. However, surely the canon of the shorter ending should be amended to allow for the careless omission of one word, especially in light of the fact that in Mark, the UBS has *ευθεως* in brackets twice, indicating just this error. The UBS also deals with similar cases where single words are included in brackets in Mark 3:7-8 with *ηκολουθησεν*, 6:41 with *αυτου*, with *και* in 10:1 and with *οι* in 10:31. An older statement of this canon by Marvin Vincent also does not allow for the accidental omission of just one word: *A History of the Textual Criticism of the New Testament* (New York: The Macmillan Company, 1899. Accessed via Amazon Digital Services Kindle Edition, nd), 86.

27 Metzger and Ehrman, 165.





be lacking without harming the sense or the structure of the sentence, as for example incidental brief propositions, and other matter the absence of which would be scarcely noticed by the scribe when re-reading what he had written.”²⁸

The research of Ernest Colwell has been instructive in this area. According to Eldon J. Epp, “It was Ernest Colwell, in 1965, who provided the impetus for a critique of this principle in his analysis of variants in the singular readings . . . of three, early extensive New Testament papyri.”²⁹ Maurice Robinson has also done significant research in this area.³⁰ Furthermore, any translator (and translation may be considered a form of copying the text) knows that it is quite easy to omit a single word through carelessness.³¹ In the light of these points, due consideration should be given to the idea that εὐθὺς/εὐθεὼς was omitted inadvertently in the passages in Mark we will examine.

To continue, we can see some definite places from our DA of εὐθὺς/εὐθεὼς in Mark which can help with internal evidence. First of all, consider the text critical problems with εὐθὺς/εὐθεὼς in the healing pericopes of Mark. In 1:31 we read, Καὶ ἀφῆκεν αὐτὴν ὁ πυρετός εὐθεὼς, καὶ διηκονεῖ αὐτοῖς. (“And her fever disappeared, and she served them.”) This reading is fully attested in the Byzantine text, but the Alexandrian omits εὐθὺς/εὐθεὼς. According to Nestle-Aland 27, 0130 (a mixed text) and A (Byzantine in the Gospels³²) include εὐθεὼς while Alexandrian manuscripts ⲁ B C L W do not. This appears to be a clear case of the Alexandrian text type versus the Byzantine. The editors of the UBS and Nestle-Aland texts omit the word, while an editor using the Byzantine priority method will keep it. This essay’s DA concludes that the word fulfills an important literary function in healing pericopes, and further strengthens the internal evidence on the Byzantine side.

Another textual problem in a healing pericope is in 5:42. Our adverb occurs in the first half of the passage in all texts: Καὶ εὐθεὼς ἀνέστη τὸ κορασίου καὶ παρῆπατει. (“And immediately the young girl stood and walked.”) However, in the second half of the verse, the UBS includes the word in a second place in brackets where the Byzantine does not: Καὶ ἐξέστησαν [εὐθὺς] ἐκστασεὶ μεγάλῃ. (“And they were immediately astonished, having great surprise.”) There is some Alexandrian support for this reading (ⲁ B C L Δ 33 892 cop^{bo} eth; only the eth may be other than Alexandrian), which is why the word is in brackets in the UBS.³³

At first glance this usage may appear to fit the pattern of Mark’s discourse. However, there are important differences from Mark’s normal function for the adverb, that of giving literary impact to his pericopes. First of all, nowhere else does Mark use εὐθὺς/εὐθεὼς so soon after a previous usage. If the word is genuine in this position in the sentence, it would be the only time such a usage occurred. Secondly, Mark does not use εὐθὺς/εὐθεὼς elsewhere in describing human emotions in reaction to Christ (such as surprise), although in Christ’s parable of the sower it is used of the gladness of those receiving the Word on stony ground (4:16).

Therefore, from the internal evidence it is unlikely that this reading is genuine, though of course the final call must be based on the external evidence.

In a surprising twist, in another healing pericope, in 7:35 the UBS includes εὐθεὼς (as opposed to its usual εὐθὺς) in brackets, thus agreeing with the Byzantine Textform:³⁴ Καὶ εὐθεὼς διηνοιχθησαν αὐτοῦ αἱ

28 J. Griesbach, cited in Metzger and Ehrman, 166. Specific bibliographic data not provided by Metzger and Ehrman.

29 Epp, “Issues in new Testament Textual Criticism,” in Black, *Rethinking New Testament Textual Criticism*, 27.

30 See Maurice Robinson, “Scribal Habits among Manuscripts of the Apocalypse” (PhD Diss., Southwestern Baptist Theological Seminary, 1982).

31 I have committed this error many times, and of course been roundly rebuked by my Japanese co-translator or later editors for my carelessness.

32 Metzger and Ehrman, 67.

33 This information is taken from the UBS 3 apparatus. Strangely enough, the UBS 4 does not list the mss evidence in its apparatus, though it retains εὐθὺς in brackets here.

34 However, there are several other differences between the UBS and Byzantine in this verse.





ακοαί. (“Suddenly, his eyes were opened.”) Bruce Metzger says about this:

UBS: Mark’s fondness for εὐθὺς (which sometimes appears as εὐθεὼς in various manuscripts) makes it probable that the adverb was employed either here or before ἐλυθῇ. The external support, however, for εὐθὺς before ἐλυθῇ is extremely weak, whereas it is relatively strong for including εὐθεὼς here. At the same time, the combination of witnesses that lack εὐθεὼς (⌘ B D L Δ) is so impressive that a majority of the Committee considered it advisable to include εὐθεὼς within square brackets.³⁵

What is disappointing in this case is the refusal of the UBS editors to consider the fact that the Byzantine Textform is unanimous in this reading, meaning that external support for the reading is not just “impressive,” but very impressive from a Byzantine priority view. Consider the DA of this essay concerning the function of εὐθεὼς in healing pericopes. Not counting 1:31, which is omitted in the UBS text, there are six other healings in which εὐθὺς/εὐθεὼς describes what happened immediately after the healing. So for Mark to have εὐθεὼς in 7:35 is not only in alignment with his style, but with the literary function of εὐθεὼς in Mark as per the DA of this essay.

There is no reason for the word to be added here by a scribe harmonizing, since the parallel passage in Matthew (8:15) does not have εὐθὺς/εὐθεὼς. Consequently, the reading with εὐθεὼς in Mark 7:35 is the more difficult reading. So, since the external evidence is very strong, surely this strong internal evidence should carry the day, especially with the Byzantine priority method, and perhaps also the eclectic method.

Mark 2:2, which relates a response to Christ, has an interesting problem: Καὶ εὐθεὼς συνηχθησαν πολλοί, ὥστε μηκετι χωρεῖν μηδε τα προς την θυραν. (“And immediately many were assembled, so that there was no longer space to let them in, not even around the door.”) In our DA of Mark, this would have the literary function of describing the reactions of people to the actions of Christ, therefore showing how great the influence of Christ was becoming.

This is another case where the UBS omits εὐθὺς/εὐθεὼς but the Byzantine includes it. According to Nestle-Aland, witnesses C and D (Codex Bezae, the most important witness to the Western text) agree with the Byzantine. There is no legitimate reason for a scribe to add this word, since the parallel passages in Matthew 8:2 and Luke 8:36 do not have it, making the Byzantine reading the more difficult. Therefore from the Byzantine priority view and perhaps also the eclectic view, the internal evidence should win, considering the strong external evidence.

There are two places with textual problems with εὐθὺς/εὐθεὼς where the literary purpose is to show a decisive Christ. Consider Mark 5:13: Καὶ ἐπετρεψεν αυτοις εὐθεὼς ὁ Ἰησοῦς. (“And right away Jesus allowed them.”) To complicate the issue in 5:13, both the UBS and Nestle-Aland omit not just the one but three words here which are included in the Byzantine Textform: εὐθεὼς ὁ Ἰησοῦς. However, from a translator’s viewpoint the omission of ὁ Ἰησοῦς does not seriously affect the rendering, so it need not be discussed here.

As noted above, εὐθὺς/εὐθεὼς fulfills the literary function of portraying a decisive Christ eleven times in Mark in the Byzantine Textform, and nine in the UBS Greek text. Thus it is perfectly natural to include it in Mark’s Gospel, and strange for it to be omitted as it is in the UBS text, unless the Alexandrian scribe simply erred. Furthermore, the parallel phrase in Luke 8:32 does not have εὐθὺς/εὐθεὼς, meaning that there was no harmonization, and so including the word is the more difficult reading and thus the more likely.

Finally, in Mark 5:36 we have: Ὁ δὲ Ἰησοῦς εὐθεὼς ἀκουσας τον λογον λαλουμενον λεγει τῷ αρχισυναγωγῷ, Μη φοβού μονον πιστευε. (“So, as soon as Jesus heard what was said, He said to the synagogue ruler, ‘Do not fear, just believe.’”)³⁶ Just as in 5:13, we see a decisive Christ in this passage. So the inclusion of

³⁵ Metzger, *A Textual Commentary on the Greek New Testament*, 2nd ed., 82-83.

³⁶ This verse also has another textual problem, with the UBS having παρακουσας, from παρακουω, instead of ακουσας. From a





εὐθεως in this textual problem also easily fulfills an important literary function of εὐθεως in Mark. The UBS apparatus lists A and C in addition to the Byzantine as manuscript support for εὐθους/εὐθεως in this passage. Once again, this appears to be a simple case of the Alexandrian text type versus the Byzantine. The critic's choice of a reading will depend on his methodology. The average eclectic will follow the Alexandrian, but a critic using the Byzantine priority method will follow the Byzantine Textform.

Conclusion

Mark uses εὐθους/εὐθεως quite often in his discourse. As we have seen, it is not only part of his literary style, but in each case fulfills an important literary function. Therefore, in the cases where this word does not appear in one manuscript tradition while it does appear in others, the possibility that it is original must be considered.

Metzger has written, "Intrinsic Probabilities depend upon considerations of what the author was more likely to have written."³⁷ Robinson agrees, saying, "Readings which conform to the known style, vocabulary, and syntax of the original author are to be preferred."³⁸

Furthermore, in the eclectic statements of the canon of the shorter reading, the possibility that a single word may be eliminated from the text through scribal carelessness should surely be included. It goes without saying that the Byzantine priority theory considers this very possibility as a matter of course. In fact, recent research revealing scribal carelessness which often results in a mistaken shorter reading should be considered seriously by eclectics, in which case the Byzantine Textform will be considered with more weight than heretofore.

Finally, this essay has put forth the possibility that discourse analysis can be used as a tool for examining internal evidence in the textual criticism of the New Testament. Discourse analysis should not, of course, be used as a primary tool. The external evidence should be primary, and having good manuscript support for a reading is certainly extremely important. However, surely this and other linguistic tools have great potential for textual criticism and should be carefully considered for that purpose.³⁹

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translator's viewpoint, this could be translated as "ignore." However, this word does not occur anywhere else in Mark, and only in Matt. 16:17 in the NT, where it must mean "ignore." The meaning in Mark 5:36, should the UBS be correct, would have to be "overhear," a meaning which occurs nowhere else in the NT. Since in Mark in the Byzantine Textform the word ακουω occurs in some form 47 times, ακουσας is far more likely from a discourse analysis viewpoint to be the correct reading. Furthermore, though Metzger opts for παρακουσας, he admits that ακουσας has the support of such witnesses as ^a A C D K Θ II al, according to Metzger's *A Textual Commentary on the Greek New Testament*, 74.

³⁷ Metzger, 13.

³⁸ Robinson, "The Case for Byzantine Priority," 545.

³⁹ Many thanks to my son Paul, much the better scholar, for his invaluable help on this essay. Heartfelt thanks to the publishers for the opportunity to contribute to this volume in honor of Maurice Robinson. Dr. Robinson's work has shaped my thinking on textual criticism enormously, and I have learned much and enjoyed every second of my talks and email exchanges with him on this and other issues.





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EARLY TEXTUAL RECENSION IN ALEXANDRIA

An Evaluation of Fee's Arguments

by T. David Andersen

Introduction

In the debate between the relative merits of the Byzantine or Alexandrian text types of the New Testament, scholars who maintain that the Byzantine text type is closer to the autographs have suggested that the Alexandrian text type was due to a deliberate editorial inclination to make texts more succinct for stylistic reasons, eliminating sentences or words that were regarded as superfluous. Some scholars have suggested that scribes in Alexandria were influenced by the type of editorial changes Alexandrian scholars made to the Homeric epics. Metzger and Ehrman say,

there was a fairly well-developed scholarly discipline of textual and literary criticism in antiquity, localized chiefly at Alexandria and directed primarily toward the epics of Homer...It is less widely appreciated — indeed the question has seldom been raised — how far the methods of textual criticism current at Alexandria were adopted by scholars in the Church and applied to the text of the New Testament.¹

Citing the support of Farmer² and Robinson,³ Robinson says,

The shorter form in Homer is considered to reflect Alexandrian critical know-how and scholarly revision applied to the text; the Alexandrian text of the NT is clearly shorter, has apparent Alexandrian connections, and may well reflect recensional activity.⁴

Citing the support of Pickering,⁵ Robinson and Pierpont state,

Conflation is not exclusive to the Byzantine-era manuscripts; the scribes of Alexandrian and Western manuscripts conflate as much or more than what has been imputed to Byzantine-era scribal habits.⁶

As a rebuttal to this argument Carson cites the work of Fee.⁷ Carson states,

If recent work by Gordon D. Fee is correct..., then neither \mathfrak{P}^{75} nor B is recensional. If \mathfrak{P}^{75} , 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. If Fee's work stands up, then we must conclude

- 1 Bruce M. Metzger and Bart D. Ehrman, *The Text of the New Testament: Its Transmission, Corruption, and Restoration*, 4th ed. (New York: Oxford University Press, 2005), 198.
- 2 William R. Farmer, *The Last Twelve Verses of Mark* (Cambridge: Cambridge University Press, 1974), 13-17, referenced by Maurice A. Robinson, "New Testament Textual Criticism: The Case for Byzantine Priority," *TC: A Journal of Biblical Textual Criticism* 6 (2001), accessed Jan. 5, 2010, <http://rosetta.reltech.org/TC/vol06/Robinson2001.html>.
- 3 Maurice A. Robinson, "The Recensional Nature of the Alexandrian Text-Type: A Response to Selected Criticisms of the Byzantine-Priority Theory," *Faith and Mission* 11, no. 1 (1993): 46-74, referenced by Robinson, "The Case for Byzantine Priority."
- 4 Robinson, "The Case for Byzantine Priority."
- 5 Wilbur N. Pickering, "Conflation or Confusion," in *The Identity of the New Testament Text*, rev. ed. (Nashville: Thomas Nelson, 1980), 171-200, referenced by Maurice A. Robinson and William G. Pierpont, *The New Testament in the Original Greek according to the Byzantine/Majority Textform* (Atlanta: Original Word Publishers, 1991).
- 6 Robinson and Pierpont, *The New Testament in the Original Greek*, xxiii-xxiv.
- 7 Gordon D. Fee, " \mathfrak{P}^{75} , \mathfrak{P}^{66} , and Origen: The Myth of Early Textual Recension in Alexandria," in *New Dimensions in New Testament Study*, ed. Richard N. Longenecker and Merrill C. Tenney (Grand Rapids: Zondervan, 1974), 19-45, referenced by Donald A. Carson, *The King James Version Debate – A Plea for Realism* (Grand Rapids: Baker Book House, 1979).





that at least in John's Gospel the Alexandrian text-type is by far the closest to the autograph.⁸

In response to Carson's remarks, Robinson states,

J. C. O'Neill, "The Rules followed by the Editors of the text represented by the Codex Vaticanus," written post-Fee claims precisely the opposite. Further, no one to my knowledge has leaped on Fee's bandwagon claim in this regard.⁹

In the context of this debate, the present paper aims to summarize and evaluate the arguments of Fee.¹⁰

Summary of Fee's arguments

Fee's aim in his article is to disprove certain hypotheses made by some scholars that Codex Vaticanus (B) is the end result of scholarly textual recension in Alexandria. Fee quotes the views of Kenyon: "The Vatican text represents the result, not of continuous unaltered tradition, but of skilled scholarship working on the best available authorities."¹¹ He further quotes from Kenyon,

During the second and third centuries, a great variety of readings came into existence throughout the Christian world...In Egypt this variety of texts existed, as elsewhere; but Egypt (and especially Alexandria) was a country with a strong tradition of scholarship and with a knowledge of textual criticism. Here, therefore, a relatively faithful tradition was preserved. About the beginning of the fourth century, a scholar may well have set himself to compare the best accessible representatives of this tradition, and so have produced a text of which B is an early descendant.¹²

According to Kenyon (as cited by Fee), the Alexandrian editor

would be a trained scholar, whose guiding principle would be accuracy, not edification, who would be thinking of the author rather than of the reader. He would be careful to consult the oldest manuscripts accessible to him, and would compare their variant readings in the light of critical science, considering which was most likely to give the author's original words. He would tend to omit superfluities or insufficiently attested words or passages, and to prefer the more difficult reading to the easier, as more likely to have been altered.¹³

Fee notes that "It has been frequently posited...that Origen was the philological mind behind the production of the Egyptian recension (= edition) in the church of Alexandria."¹⁴ He marshals several arguments to show that Origen "did not have the kind of concern for the NT text that would make him representative of the 'philological mind' necessary for such a recension."¹⁵

Fee quotes from Pack that Origen's "handling of the text closely parallels the work done by later editors and textual workers in shaping the stylized Byzantine text."¹⁶ Fee later remarks,

In contrast to his work on the OT, Origen never shows a concern for a "critical text" of the NT writings. Furthermore, where editorializing may be shown to exist, he does not edit toward the text of \mathfrak{P}^{75} B on the basis of Alexandrian philological know-how, but rather away from that text on principles later to be found in the

8 Carson, *The King James Version Debate*, 117.

9 Maurice Robinson, e-mail message to author, December 23, 2009.

10 The previously cited article by Fee was reprinted in Gordon D. Fee, " \mathfrak{P}^{75} , \mathfrak{P}^{66} , and Origen: The Myth of Early Textual Recension in Alexandria," in *Studies in the Theory and Method of New Testament Textual Criticism*, ed. Eldon J. Epp and Gordon D. Fee (Grand Rapids: Eerdmans, 1993), 247-273. All future references will be to this later publication.

11 George Frederic Kenyon, *The Chester Beatty Biblical Papyri: General Introduction* (London: Emory Walker, 1933), 16, quoted in Fee, 248.

12 George Frederic Kenyon, "Hesychius and the Text of the New Testament," in *Memorial Lagrange*, ed. Hugues Vincent (Uppsala: Seminarium Neotestamentum Upsaliense, 1940), 250, quoted in Fee, 249.

13 George Frederic Kenyon, *The Text of the Greek Bible*, rev. ed. (London: Duckworth, 1949), 248-249, quoted in Fee, 250.

14 Fee, 256.

15 Ibid.

16 Frank Pack, "The Methodology of Origen as a Textual Critic in Arriving at the Text of the New Testament" (unpublished dissertation, University of Southern California, 1948), 346, quoted in Fee, 257.





Byzantine tradition.¹⁷

Fee then evaluates \mathfrak{P}^{75} and B for recensional features. First he claims that the scribe of \mathfrak{P}^{75} was not making recensional changes. He says, “In comparison with any of the other early papyri, this scribe produced a remarkably error-free copy.”¹⁸ Then with regard to B, he says, “there are no patterns or directions of ‘editorializing’ in B that are not already anticipated by its earlier — and closest — relative, which simply underscores a point made above as to the nonrecensional character of B.”¹⁹ He goes on, “both MSS are faithfully preserving textual phenomena which are anterior to them, which in turn means that \mathfrak{P}^{75} is not itself the recension.”²⁰

Thus far Fee’s argument seems to hold water. Certainly the close similarity between B and \mathfrak{P}^{75} indicates that B was not the result of recensional activity. But how can one evaluate whether some ancestor manuscript of \mathfrak{P}^{75} and B was not the result of recensional activity?

Fee attempts to evaluate this in relation to particular types of textual variants in Luke and John. In Luke, Fee analyzes variants in chapters 10 and 11 which relate to possible harmonizations. He finds 43 alleged harmonizations with substantial manuscripts support. The Western manuscript D has 14 of these harmonizing variants. The Byzantine manuscripts have 27 harmonizing variants. In contrast to this, \mathfrak{P}^{75} and B have 6 harmonizing variants. Fee concludes,

While not all of the harmonizations are necessarily secondary, it is surely true that the large majority are. And while it is also true that no MS has escaped corruption at this point, it is likewise true that \mathfrak{P}^{75} and B are “comparatively pure” when compared with either the Western or Byzantine traditions.²¹

In John, Fee analyses textual variants relating to certain stylistic features characteristic of John, and which are more frequent in John than elsewhere in the New Testament. These features are: his abundance of asyndeton, the frequent omission of the article with personal names in the nominative, the abundance of *oti* recitativum, the redundant nominative personal pronoun, and the frequency of the vernacular possessive.²²

Each of these features gives rise to numerous variant readings. In many places in John, one finds certain manuscripts supporting a reading including the Johannean feature, whereas other manuscripts support a reading in which this feature has been changed to conform to more common Greek usage. The problem is to evaluate which alternative represents the original reading, and which may be the result of a recensional change. Fee admits,

No fixed rules may be established as to whether a reading is original or recensional by its relationship to an author’s style, for a reading may be regarded as original because it conforms to that style, or recensional because a scribe may have conformed it to the author’s style.²³

A little later, however, he seems to ignore this caveat when he says, “in general it may be assumed that tendencies away from John toward either a more common or a more classical idiom are recensional in nature.”²⁴

It seems that this assumption is questionable, and Fee’s dependence on it is the key weakness of his argument. Whether recensional activity tends to change the text to conform to the author’s style, or to

¹⁷ Fee, 258.

¹⁸ Ibid., 260.

¹⁹ Fee, 261.

²⁰ Ibid.

²¹ Ibid., 272.

²² Ibid., 269.

²³ Ibid., 268-269.

²⁴ Fee, 269.





smooth away features of the author's style would depend on the motives and linguistic background of the editor. The type of recensional activity which smoothes away peculiar features of grammar would seem to be associated with the Byzantine textual tradition.

The type of recensional activity described by Kenyon seems to be using different principles. If an Alexandrian editor was thinking of the author, not the reader, and trying to correct places which might have departed from the original, then it is perfectly conceivable that he might try to change common or classical usage to make it conform to John's particular style.

Fee goes on to count how often \mathfrak{P}^{75} and B support readings which preserve John's style in these features. He reports,

With regard to asyndeton...in comparison with the Western and Byzantine MSS, \mathfrak{P}^{75} and B had a very high record in maintaining this Johannine feature. This is all the more remarkable when one considers that scholarly recension would almost certainly go in the other direction.²⁵

This last remark is again basing his argument on a dubious assumption that we can predict what sort of changes a scholarly editor would make.

Fee also reports,

All MSS and text-types showed remarkable ambiguity with variations of the *oti-recitativum*. But generally, MSS tended to reject it rather than to add it, and \mathfrak{P}^{75} B tended to preserve (add?) it more than others. Likewise with word order: \mathfrak{P}^{75} B far more often preserved Johannine features than other MSS: and in one list of "word order variants tending toward more logical juxtaposition," \mathfrak{P}^{75} B invariably had the *lectio difficilior*.²⁶

The problem with these remarks is that they are not quantified. Fee speaks of tendencies, but does not tell how strong the tendency is, or which particular manuscripts he is comparing \mathfrak{P}^{75} and B with in relation to these features.

Evaluation

There are several major weaknesses in the evidence that Fee presents to show that \mathfrak{P}^{75} and B are not descendants of a recensional manuscript. The first is his assumption "that what is to be regarded as recensional must be consistently so throughout the NT."²⁷ Based on this assumption he considers that if a feature is present in only one particular book, it must be original, not recensional. There is also a tacit assumption that if he can show that \mathfrak{P}^{75} and B are not recensional in Luke and John, it implies that they are not recensional in the other books of the New Testament.

But this is not a valid assumption. Each book circulated separately for a considerable period of time before it was incorporated into larger manuscripts. Much recensional activity could well have been carried out by early scribes copying individual books. And even when the Scriptures began to circulate in codices containing a corpus of books, manuscripts including the whole New Testament were the exception. It was common to have manuscripts including the four gospels, manuscripts containing the Pauline epistles, and manuscripts containing Acts and the Catholic epistles. Hence there is little reason to assume that recensional activity affecting gospel manuscripts should necessarily apply to epistolary manuscripts, or vice versa.

Fee has presented evidence regarding possible recensional activity or lack thereof in manuscripts of Luke and John. He has presented no evidence in relation to the other books of the New Testament.

²⁵ Ibid., 272.

²⁶ Ibid.

²⁷ Fee, 269.





Hence, based on this paper, no conclusions can be drawn regarding the possible recensional character of \mathfrak{P}^{75} or B in those other books.

Another major weakness of Fee's paper is that he only investigates a few limited types of possible recensional changes. His evaluation is restricted to harmonizations and to a few grammatical features favoured in John's style of writing. There are many other types of possible recensional changes besides these.

Of great interest, for example, is the question of possible additions or omissions, where often the Alexandrian text type has a shorter reading, and the Byzantine text type has a longer reading. This type of variant has more impact on the meaning of the text than grammatical variants which do not affect the meaning. As quoted earlier, those who suggest that Alexandrian scholars engaged in recensional activity suggest that they "would tend to omit superfluities or insufficiently attested words or passages."²⁸ Yet this type of variant is not investigated by Fee.

In sum, Fee has presented only a limited amount of evidence which suggests that there are certain types of recensional changes not evident in \mathfrak{P}^{75} and B in Luke and John. Some of his argumentation is based on dubious assumptions. To make a stronger case, much more comprehensive evidence is needed.

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²⁸ Kenyon, *The Text*, 250, quoted in Fee, 249.





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THE RELATIONSHIP OF THE VATICANUS UMLAUTS TO FAMILY 1

by Edward D. Gravely

Interest in the study of Codex Vaticanus was reinvigorated in 1995 when Philip Payne discovered hundreds of umlaut-like sigla¹ in the margins of Codex Vaticanus, apparently marking places of textual variation known to the scribe.² The text-critical community was shocked and thrilled that after sixteen centuries of the manuscript's existence and several hundred years of scholarly examination a new siglum had been found in its pages. There has been a great deal of on-going discussion concerning these "umlauts" since 1995, and a relatively small number of articles and chapters have been produced detailing the umlauts' meaning and significance. More work certainly needs to be done.

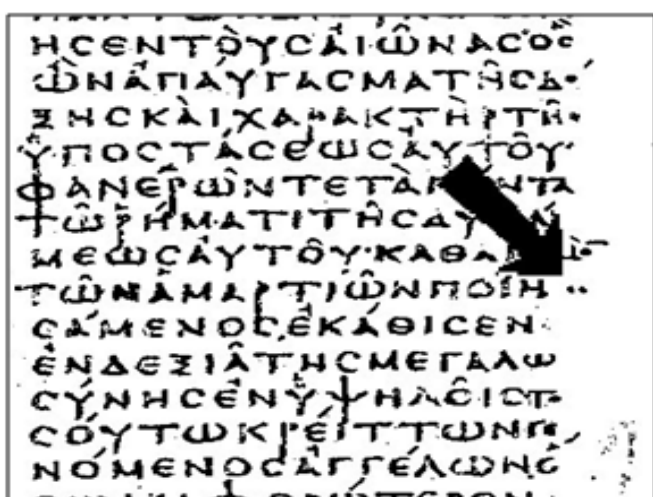


Figure 1 — An example of an "umlaut"

In 2008 I was privileged to work under the tutelage of Dr. Maurice Robinson to complete a doctoral dissertation on the umlauts in Vaticanus that was approved in 2009. The fruits of that labor are ongoing, and the examination of the possible relationship between Family 1 and the Vaticanus umlauts presented here is, in great part, the result of Dr. Robinson's excellent mentorship.

The umlauts do mark places of textual variation between Vaticanus and another manuscript or manuscripts. On this, all scholars appear to be in agreement. The statistical evidence is clear that lines marked by umlauts were considerably more likely to contain textual variants than unmarked lines. Additional tests performed by multiple scholars also confirm that the umlauts do mark places of textual variation.

Though it is *possible* that some of the umlauts were placed in the manuscript later, all of the evidence points to the fact that the umlauts were made very early, close to the time of the manuscript's production. Canart, a paleographer at the Vatican, is certain that the unretraced umlauts match the ink of the original scribe and, along with Payne, gives good evidence that other original ink umlauts have been retraced by

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- 1 Payne and others have lately argued in favor of calling the umlaut-like siglum „distigmai“. This essay continues to refer to them by their original name of „umlaut“.
 - 2 This discovery was first published in Philip B. Payne, “Fuldensis, Sigla for Variants in Vaticanus, and 1 Cor 14.34–5,” *NTS* 41 (1995): 240–62 where he makes substantial arguments in favor of the text-critical nature of the umlauts.



ink matching the rest of the retracer's work.³ Additionally, it is difficult to imagine a plausible scenario whereby two scribes, separated by as many as a thousand years, placed umlauts in the text of Vaticanus independent of one another, or that any reasonably modern scribe would make such marks in such an ancient text.⁴

There is some "crowding" that occurs regarding umlaut placement that could suggest the umlauts were placed in the text after the Vaticanus canon numbers were added to the manuscript.⁵ If true, this could mean that it was not the original scribe who placed all of the umlauts, though such a conclusion would not demand a date for the umlauts much later than the fourth century. Additionally there are two difficult instances of nonstandard umlaut placement that appear to be the result of crowding by considerably later marginalia. Given the overwhelming evidence to support the antiquity of the umlauts, these two instances of unusual umlaut placement are most likely coincidental, since a significant number of the umlauts are placed in nonstandard locations.

Many of the key questions regarding the Vaticanus umlauts relate to the nature of the manuscript(s) used to produce them. What kinds of texts were available to the scribe who made the nearly 900 umlauts in the margins of Vaticanus? Since there are often multiple variants that occur on lines marked with umlauts, variants that begin on the line above but extend to the line marked, and even some lines marked by umlauts where no known textual variants reside, making educated guesses at the texts that the scribe who made the umlauts had on hand is highly difficult. There is, however, at least one clue in Vaticanus that may shed some light on this question.

The Gospel of John ends in the first (A) column of a left-hand folio in Vaticanus. Only six lines of text are left to end the Gospel. The rest of the column is empty space, and Acts begins at the top of the B column. Later in the life of Vaticanus, ornate decoration was added to end the Gospel of John, and the title was repeated. The end of John, however, even with the illumination, still takes up only about two-thirds of the column; the rest is empty space. There is, however, about half-way down the empty part of the column, in what corresponds to approximately line 33 in the typical Vaticanus hand, an umlaut to the left of the column, marking no text.⁶ This raises an interesting question: what variant could the scribe of Vaticanus have been aware of that caused him to place the umlaut here? The most natural suggestion is that the scribe who placed the umlauts had a text which included the *Pericope de Adultera* (PA) at the end of John.

What makes this the most natural suggestion is that Vaticanus is missing the PA. The text moves seamlessly from John 7:52 to John 8:12. Both Payne and Miller have commented on this omission in articles on the umlauts, solely because there is an umlaut at (1361.C.3.R), the line above where the PA would have begun had it been included after John 7:52. Payne argues that the umlaut at (1361.C.3.R) is marking the omission of the PA.⁷ Miller responds that there is insufficient evidence to conclude that it was the PA that inspired the umlaut. He demonstrates by offering evidence that the umlaut normally

3 See Philip B. Payne and Paul Canart, "The Originality of Text-Critical Symbols in Codex Vaticanus," *NovT* 42 (2000): 105–113 and more recently "Distigmai Matching the Original Ink of Codex Vaticanus: Do They Mark the Location of Textual Variants?" pages 199–226 in Patrick Andrist, ed., *Le manuscrit B de la Bible (Vaticanus graecus 1209): Introduction au fac-similé, Actes du Colloque de Genève (11 juin 2001), Contributions supplémentaires*. Lausanne, Switzerland: Éditions du Zèbre, 2009.

4 For the most recent (and only current) arguments for a late date for all of the Vaticanus umlauts see "The Marginalia of Codex Vaticanus: Putting the Distigmai in Their Place" presented by Peter M. Head to the NT Textual Criticism Seminar Nov. 21, 2009 in New Orleans.

5 There are also umlauts in the first few pages of the much later Hebrews supplement in Vaticanus. For a detailed analysis of these with possible explanations see Edward D. Gravely, "The Text Critical Sigla in Codex Vaticanus," Ph.D. dissertation (Southeastern Baptist Theological Seminary, 2009), 81–89.

6 All mechanical observations for his study were made using the high quality facsimile of Vaticanus: Biblioteca Apostolica Vaticana. *Bibliorum sacrorum Graecorum Codex Vaticanus B*. 2 vols. Rome: Istituto poligrafico e Zecca dello Stato, 1999. There is clearly an umlaut at this location. It is easily visible on the facsimile of Vaticanus. There is nothing on the opposite folio that could have imprinted the ink here, nor is there anything obvious on the back side of the page that could have bled through.

7 Payne and Canart, "Originality," 112.



marks the line where the variant *begins* and not the line preceding.⁸ The presence of a variant on the actual line marked by the 7:52 umlaut supports Miller's claim. Payne, however, offers a rebuttal which — though part of a larger argument between Payne and Miller over an alleged interpolation in 1 Cor 14 — is germane to this study and the question of sources for the Vaticanus umlauts.

Payne suggests that the umlaut at (1361.C.3.R) must be marking the omission of the PA because, "The variants [Miller] proposes for 1 Cor 14:34–35 and for John 7:52 are so minor that neither is listed in the NA27."⁹ Payne then deals extensively with the issue at 1 Cor 14:34–35, but does not deal any further with John 7:52. Presumably, Payne's argument is that the variants actually found at line (1362.C.3.R) are so insignificant that they would likely be unnoticed or considered too insubstantial by the scribe making the umlauts as evidenced by the fact that they do not occur in NA27. This claim must be examined more closely.

The variant data at that line is as follows:

εκ της γαλιλαιας προ | φητης ουκ εγειρεται | παλιν ουν αυτοις ελαλη

εκ της γαλιλαιας προφητης) προφητης εκ της γαλιλαιας

εγειρεται) εχερται

εγειρεται) εγειγερται

εγειρεται) εγηγερται

℣^{66c} ⚭ D W Θ f¹¹³ 33 Byz lat

U

E G H M 1 28 565 1071 1424 Byz

L S Λ f¹³ 157 579 700

It is most natural to assume that the longer interpolation listed first is not the variant intended by the umlaut, because the variant would have been noticed first, and presumably marked, on the line above. There is, however, no way to be sure. Even if the longer interpolation is excluded as well as the itacism and the singular reading of U, there is a remaining variant unit: εγειρεται vs. εγειγερται. It is true that this variant is not listed in NA27, but it would hardly be so insignificant as to escape the notice of a Greek-speaking scribe who was making the umlauts. Any examination of a larger apparatus of lines marked by umlauts will provide numerous examples of umlauts marking variants characterized solely by changes in tense, person, or mood or even orthographic peculiarities.¹⁰ Given this, it seems most likely that the scribe who placed the umlauts in Vaticanus did *not* use an umlaut to mark the missing PA in John 7, but rather was marking a change in verb tense on the line before.

Payne, however, is ultimately correct that the scribe of Vaticanus did know about the PA and marked it with an umlaut, just perhaps not at John 7. At least one of the manuscripts that was used to produce the umlauts most likely did not have the PA at John 7:52 since there is no umlaut there, but the manuscript did have some text that varied from Vaticanus added to the end of John. The existence of the umlaut in the empty column at (1382.A.~33.L), therefore, raises the question of whether the scribe of Vaticanus who placed the umlauts had a knowledge of manuscripts or readings reflecting the Family 1 tradition, since the primary manuscripts of this tradition lack the *Pericope de Adultera* after John 7 and have that additional text located at the end of John. Further investigation, therefore, is warranted. In order to successfully claim a connection between the Vaticanus umlauts and Family 1, the data must be examined in two key areas.

First, one would have to find a significantly high number of Family 1 variants present on lines marked by umlauts in Vaticanus. The number of Family 1 variants at umlaut locations would also have to be significantly higher than the normal incidence of Family 1 variants on non-umlauted lines. This can only

8 J. Edward Miller, "Some Observations on the Text-Critical Function of the Umlauts in Vaticanus, with Special Attention to 1 Corinthians 14.34–35," *JSNT* 26.2 (2003): 232.

9 Philip B. Payne, "The Text-Critical Function of the Umlauts in Vaticanus, with Special Attention to 1 Corinthians 14.34–35: A Response to J. Edward Miller," *JSNT* 27.1 (2004): 110.

10 Some examples include δοξαζω vs δοξαω at (1456.A.18.L), καλεσουσιν vs. καλεσουσεις at (1236.A.6.L), and ειπαι vs. ειπον at (1387.B.16.L).





be checked by compiling a special apparatus in which unlauded lines are checked against all primary Family 1 manuscripts with some justification given as to which Family 1 manuscripts are sufficient to constitute a Vaticanus umlaut/Family 1 alignment. For example, if the only Family 1 variant on an unlauded line occurs in manuscript 872, it seems unlikely that such would be significant for this study; nor should such an occurrence serve to demonstrate a connection between the Vaticanus umlauts and Family 1, given 872's propensity to lean toward the Byzantine, as well as the apparent age of the umlauts over against 872's chronological location on any likely Family 1 stemma. Individual Family 1 manuscripts should be included in such a Vaticanus umlaut/Family 1 apparatus where the individual manuscripts depart from the text of Vaticanus; an unanimity of the tradition at a variant location is not required to suggest a possible Vaticanus umlaut/Family 1 connection. But any single manuscript's variation with Vaticanus at an umlaut location should be weighed carefully before it is counted as evidence. Also, since unlauded lines are more likely to contain a variant than non-unlauded lines,¹¹ it would be important to know whether a Family 1 variant occurring on an unlauded line was more likely to occur than non-Family 1 variant and if so by how much.

Second, the nature of the Family 1 variants on unlauded lines would need to be compared with what is known of the textual relationship between Family 1 and Vaticanus. If, for example, the majority of Family 1 variants found on unlauded lines are mostly spelling variations of proper names and minor changes in inflection, but it can be demonstrated that Family 1 has frequent and sizeable insertions of text when compared with Vaticanus, then it becomes increasingly less likely that it was a manuscript or manuscripts in the Family 1 tradition that were the source for the umlauts.

The existence of variants between Family 1 and Vaticanus that are not marked by umlauts would not necessarily discount the possibility of a relationship between the Vaticanus umlauts and Family 1; however, if a relationship existed, parity between the nature of variation among the two traditions and the nature of variation found in the umlauts could be expected. Only after this test is passed can an evaluation of relationship between Vaticanus and Family 1 be suggested.

The Make-up and Textual History of Family 1

Family 1 is a collection of manuscripts, cited in most text-critical apparatuses with the siglum *f*¹. According to modern apparatuses, Family 1 typically consists of manuscripts 1, 118, 131, 209, and 1582,¹² but this roster of manuscripts has developed over time. Lake was the first to postulate such a family in the early 20th century.

With the publication of *Codex 1 of the Gospels and its Allies*,¹³ Lake presented five manuscripts which he claimed to belong to this text family, though only four are dealt with in detail.

Of these manuscripts, Lake argues that Codex 1 is the most faithful to an ancient archetype in this textual tradition. According to Lake, the other manuscripts in the family (the "allies" of Codex 1) are manuscripts 118, 131, 205, and 209. Lake excludes manuscript 205 from further consideration in his apparatus, however, because he believes it most certainly to be a close copy of 209.

The strength of his conviction on this matter is easily demonstrated by his own words.

I was convinced when I studied the question at Venice that 205 was a copy of 209. An hour's work only revealed two or three differences between the manuscripts, and those clearly accidental. It is for this reason that no further notice has been taken of 205.¹⁴

11 See Payne, „Fuldensis,” 251–4 and Gravely, „Text Critical Sigla,” 46–50 for specific data.

12 The list given here is that which is assigned to the symbol *f*¹ in NA27. Other manuscripts in the Family 1 tradition are not consistently cited, though the primary manuscripts (1, 118, 131, 209, and 1582) are cited individually in NA27 if they disagree with the family reading and with Byz. A similar list of manuscripts and procedures is followed by the UBS 4th ed.

13 Kirsopp Lake, *Codex 1 of the Gospels and its Allies in Texts and Studies: Contributions to Biblical and Patristic Literature* (vol. 7 no. 3, ed. Armitage Robinson; Cambridge, Cambridge University Press, 1902).

14 *Ibid.*, xxi-xxii.





This rejection of 205 is not a hasty judgment but rather a judgment based on the value Lake places on 205 for showing the breadth and depth of the Family 1 tradition. Lake also suggests the possibility that 118 is a “carelessly made” copy of 209 but, unlike 205, Lake does not dismiss it so quickly for two reasons. First, Lake notes that 118 does depart from the readings of 1 and 209 many times, and there are a handful of places where 118 agrees with 1 against 209, though Lake dismisses these as, “no cases of importance.”¹⁵

These variations, though not especially significant according to Lake, make the readings of 118 noteworthy in a way that the readings of 205 are not. Second, Lake believes that it is possible that the paleographical evidence regarding the date of 209 is misleading. He argues that if 118 is a copy of 209, then the standard date for 209 based on paleographical considerations (a date in the fourteenth century) is incorrect, since 118 is clearly a thirteenth-century codex. Such a conclusion, Lake argues, should be made with care.

With regard to the possibility that 118 is not a copy of 209 but rather is a “carelessly made” copy of the same archetype of 209, Lake states,

“The question admits of doubt, but as all the readings of 118 and 209 are given, individual scholars may easily judge for themselves.”¹⁶

It is, apparently, because of this continued discussion of the dating of 209 that Lake is further convinced of the need to include the readings of 118 in his critical edition. Later in his work, however, as Lake considers the larger question of the relationship of the Family 1 manuscripts to each other and to their ancestors, he reluctantly but convincingly argues that the stemmatic evidence points to a common ancestor for 118 and 209 rather than 118 as a copy of 209.¹⁷

In the one hundred years since Lake’s work, many additional manuscripts have been suggested to belong to Family 1. Manuscripts 22, 872, 884, 1192, 1210, 1278, 1582, 2193, and 2542 have all been noted by various textual critics as representatives of the text family.¹⁸ The process began with the discovery of manuscript 1582 and the subsequent development of a “Caesarean” text-type theory.¹⁹ Not all of these manuscripts, however, have the same purity or authority as consistent representatives of the Family 1 tradition. This is especially important to note in this present study since it could not be the extant manuscripts of Family 1 that were the source of the umlauts but rather some much older and now lost manuscript(s) in the Family 1 tradition.

The Family 1 manuscripts fall into three basic categories, primary, secondary, and tertiary, based on their faithfulness to the Family’s ancient ancestor(s).²⁰ Variants found in manuscripts of these three categories at umlaut locations necessarily have different values for indicating places where the scribe of the umlauts may have known of the Family 1 tradition.

The primary category of Family 1 manuscripts almost certainly includes manuscripts 1, 118, 205, 209, and 1582. These manuscripts most likely all descend from a common ancestor and are

¹⁵ Ibid., xxi. Lake notes 27 places of disagreement.

¹⁶ Ibid.

¹⁷ Ibid., xxv.

¹⁸ These nine additional manuscripts are consistently cited as being aligned at some level with Family 1. J. K. Elliott, *A Bibliography of Greek New Testament Manuscripts* (2nd ed.; Cambridge: Cambridge University Press, 2000) was particularly helpful in confirming this. Also, as demonstrated below, Amy S. Anderson, *The Textual Tradition of the Gospels: Family 1 in Matthew* (vol. XXXII of *New Testament Tools and Studies*; ed. by Bruce M. Metzger and Bart D. Ehrman; Boston: Brill, 2004), 103–45 gives Family 1 classifications that are extremely helpful. For a recent example of the ongoing discussion see P. R. McReynolds, “Two New Members of Family One of the New Testament Text: 884 and 2542” in *Texte und Textkritik, eine Aufsatzsammlung* (ed. by Jürgen Dummer; vol. 133 of *Texte und Untersuchungen*; Berlin, 1987), 397–403.

¹⁹ See B. H. Streeter, *The Four Gospels: A Study of Origins* (4th rev. ed.; London: MacMillan, 1930), and Kirsopp Lake, Robert P. Blake, and Silva New, “The Caesarean Text of the Gospel of Mark.” *HTR* 21: 207–404, 1928.

²⁰ Anderson, *Tradition*, 103–45. The divisions presented below rely heavily on the work of Anderson.



the most faithful representatives of the tradition.²¹ The secondary category is made up of manuscripts which show an affinity with Family 1 but with some notable Byzantine influence.

According to Anderson, these manuscripts are descended from a common archetype (Y) which itself most likely represents a correction toward the Byzantine.²² Manuscripts which best belong in this category are 22, 1192, and 1210. The final and tertiary category is made up of manuscripts 131, 872, 884, 1278, 2193, and 2542. These are manuscripts with some Family 1 readings but which diverge substantially from the tradition as a whole, either in significant places or in a significant amount of their text.²³ Most notable on this list is Codex 131 which is cited by Lake and is listed as a primary Family 1 manuscript in most modern apparatuses. The codex, however, is not a consistent witness to Family 1. Lake found it only to preserve the Family 1 tradition in Mark 1–4 and Luke 1–24.²⁴ Anderson agrees, noting that the text of 131 outside of those passages is Byzantine.²⁵

If these categories are, indeed, the best way to understand the distribution of Family 1 manuscripts, this provides two substantial cautions for this study. First, Codex 1582 must be included in any Family 1 apparatus of the Vaticanus umlauts though it was not included in Lake's apparatus. Lake was not aware of 1582 at the time he produced *Codex 1 of the Gospels and its Allies*, but since then the importance of Codex 1582 to the text family is clearly understood.²⁶ Second, since Codex 131 most likely does not belong in the primary group of Family 1 manuscripts, variants at umlaut locations found only in Codex 131 should be considered suspect when evaluating agreements between an umlaut and a Family 1 reading. While Codex 131 can be counted as a representative of the Family 1 tradition where its text is in common with that tradition, the problem lies in the singular in 131, especially if (as Anderson claims) many of the Family 1 agreements in 131 are coincidental.²⁷

In any apparatus of the umlauts and Family 1 variants, therefore, Codex 131 should be included, but where Codex 131 is the singular Family 1 representative, its readings should be noted but excluded from any final tally.

The Date of Family 1 and its Ancestors

The extant manuscripts of Family 1 date to around the tenth century and later with 1582 being the oldest, having been inscribed in AD 948, and 209 being the youngest, having been inscribed most likely in the fifteenth century. Given that the most likely date for the production for at least some of the umlauts is in the fourth or fifth century, it is clearly not the extant manuscripts in Family 1 that were consulted for the production of the umlauts.²⁸ There is good evidence, however, that ancestors for the Family 1 text were in existence at the time of Vaticanus' production.

Lake was the first to propose a stemma for the Family 1 manuscripts, demonstrating how they descended from a common ancestor, but it is Anderson's recently proposed stemma that is the most detailed and the most helpful for inquiry into the Vaticanus umlauts. Anderson concludes that a text not identical to

21 Lake, *Codex 1*, xxiv. According to Lake, 1, 118, and 209 are descended from a common exemplar which he labels X with 205 descended directly from 209. On the other hand, Anderson, *Tradition*, 101, sees 118, 205, and 209 descended from a common exemplar which she labels X-1 (corresponding to Lake's X). According to Anderson X-1 is descended from a prior manuscript she calls X, from which 1 also descended. Codex 1582, according to Anderson is descended from the parent to X which she calls A-1.

22 Anderson, *Tradition*, 121. The hypothetical manuscript Y in Anderson's stemma is claimed to descend from a prior parent along with X and 1582, with 1582 being the most faithful representative of the exemplar.

23 Ibid, 132. Anderson also suggests that in many cases the Family 1 readings in these manuscripts tend to be trivial and are, "most likely the sort to have happened independently."

24 Lake, *Codex 1*, xxxiv and Anderson, *Tradition*, 133.

25 Anderson, *Tradition*, 132–4.

26 Ibid., 97 argues that 1582 is actually a better candidate for "lead" manuscript in the family than Codex 1, because, though 1 and 1582 are very close, 1582 more faithfully follows the Family 1 archetype.

27 Ibid., 132.

28 The existence of a large number of variants unique to Family 1 that are not marked by umlauts also bears this out.

but distinctly similar to Family 1 was extant in Caesarea as early as the third century.²⁹ She bases this conclusion on two lines of argumentation.

First, Anderson's reconstruction of the marginalia of 1582 suggests a date for the ancestor earlier than the tenth century. According to Anderson, Ephraim, the scribe of 1582, sought to faithfully reproduce his archetype, marginalia included.³⁰ There is present in 1582, however, a systematic but gradual decline in the number and length of marginal notations. This phenomenon, argues Anderson, suggests that a scribe prior to Ephraim (who himself copied meticulously) "gradually left off copying the apparatus."³¹

This leads Anderson to proffer at least two prior stages for 1582, the immediate exemplar with the less replete marginalia (A-1) and its exemplar (A-1²). Anderson further argues that the marginalia present in A-1 was present in the archetype, and offers the similar marginal apparatus present in Codex 1739 as evidence.³² As will be shown below, this pushes Anderson's date for the archetype back to at least the seventh century.

The second line of argumentation used by Anderson to suggest an ancient origin for Family 1 is that there are clear similarities between the text of Family 1 and the text used by Origen (ca A.D. 185–254). Though this connection was noted earlier by Kim and others,³³ Anderson's work focuses on Codex 1582 and Origen's commentary on Matthew, but her findings are exceptionally clear. Most notably Anderson states, "A series of rare and ancient readings, shared by few or no others, is common to both documents."³⁴

There are differences between Family 1 and Origen's text, and those differences are substantial and ancient. One text was not based on the other, but rather, Anderson argues,

"It appears more likely that both drew from a common source — a collection of biblical documents available in Caesarea in the early third century and containing distinctive readings."³⁵

Anderson's stemma, revised from Lake, is as follows:³⁶

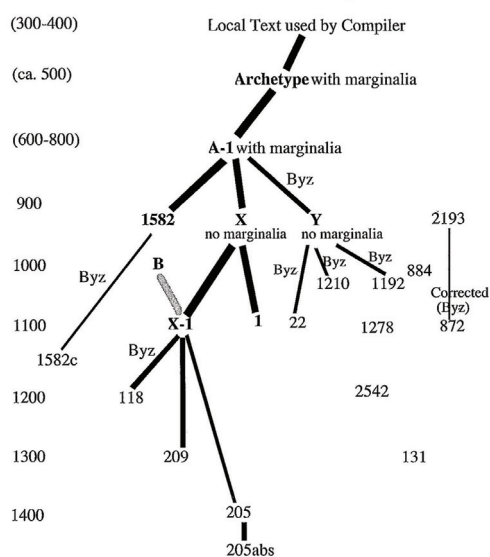


Figure 2 — Anderson's stemma for Family 1

29 Ibid., 83.

30 Ibid., 61.

31 Ibid.

32 Ibid., 72, suggests that 1739 is also descended from the archetype of Family 1 but with its Gospels now lost. Anderson also suggests that 1739 and 1582 were both copied by the same scribe, Ephraim.

33 For example see K. W. Kim, "Codices 1582, 1739, and Origen," *JBL* 69 (1950), 167–75.

34 Anderson, *Tradition*, 83.

35 Ibid.

36 Ibid., 101. The stemma is reprinted exactly as it appears in Anderson's *Tradition*.



Determining the exact date of the ancestors and ultimate archetype of Family 1 is not necessary to properly evaluate the umlaut evidence. As indicated above, Anderson provides ample evidence that the predecessors to Family 1 are ancient, at least as ancient as Codex Vaticanus and therefore sufficiently old enough to produce the umlauts therein.

If correct, Anderson's stemma also reinforces the need to focus on the "Category 1" manuscripts, that is the Family 1 manuscripts most faithful to the ancient archetype when considering the relationship of the Vaticanus umlauts to Family 1.

An Evaluation of the Data

Having established that it is chronologically possible for a Family 1 ancestor to be a source of the Vaticanus umlauts and having demonstrated that such an ancestor would most likely be reflected in the primary Family 1 manuscripts, a proper investigation of the Vaticanus umlauts and Family 1 can begin. In order to examine what, if any, Vaticanus umlaut/Family 1 connection exists, the lines of text in the Gospels that are marked by the umlauts in Vaticanus were examined carefully for Family 1 variants. Table 1 provides the general apparatus as well as the results described for each Gospel.

The findings for all four Gospels are summarized on the table below. Locations marked with "*" indicate places where a single umlaut marks a line of text with more than one distinct Family 1 variant extant.

The column entitled "Variant Type" uses a very abbreviated set of text critical symbols: "+" indicating an insertion, "-" indicating an omission, "]" indicating a replacement, and ")+" indicating a replacement that is significantly longer than the text replaced.

The column labeled "NA27" gives the variant notation listed in that edition's apparatus. In this column, a "-" indicates that the variant is not listed in NA27, and a notation of "B is unique" indicates that B and often a handful of other manuscripts have a unique reading that differs from Family 1 as well as many other manuscripts.

The column labeled "LAS" indicates the findings of a collation of umlaut lines using the apparatuses in Lake and Swanson as well as the helpful correction of Lake by Anderson, and lists the manuscripts in which the variant is found. In this column the designation "131*" indicates a variant in Codex 131 in those sections where 131 is believed to be faithfully following the Family 1 tradition.

Table 1 is followed by an analysis of the findings in each Gospel. Also, for the sake of completeness, umlaut locations marked with a "^" are locations where Payne and Canart have identified, through physical examination, umlauts in ink matching the ink of the original hand of Vaticanus.³⁷

Table 1: Possible Family 1 Umlauts

| Gospel | Location | Variant Type | NA27 | LAS |
|--------------|-------------|--------------|----------------|-----------------------------|
| Matt 2:18 | 1237.A.1.L | + | - | 209 |
| Matt 3:9-10 | 1237.B.37.L | + | - | 118, 209, 1582 ^c |
| Matt 3:15-16 | 1237.C.30.R | + | - | f ¹ |
| Matt 5:11 | 1239.A.40.L | + | f ¹ | f ¹ |
| Matt 5:22 | 1239.C.19.R | + | f ¹ | f ¹ |

³⁷ Payne, „Distigmai,“ 204–208. It is this author's opinion that the most likely scenario is that all of the umlauts are early and date to a time close to the manuscript's production (see Gravely, „Text Critical Sigla,“ 65–96), but the evidence for the antiquity of these umlauts identified by Payne and Canart is profoundly strong.





| | | | | |
|---------------|-------------|----|-------------------------|--------------------|
| Matt 5:44 | 1240.C.1.L |)+ | - | 209 |
| Matt. 5:47a | 1240.C.16.L |) | - | 118 |
| Matt 6:1 | 1240.C.23.R | + | f^1 | f^1 |
| Matt 6:13-14 | 1241.B.9.L | + | - | 118^2 |
| Matt 6:21 | 1241.C.7.R |) | f^1 | f^1 |
| Matt 6:25 | 1241.C.31.R | - | f^1 | f^1 |
| Matt 8:9 | 1243.C.11.R | - | B, <i>pc</i> are unique | f^1 |
| Matt 8:13 | 1243.C.40.R | + | f^1 | f^1 |
| Matt 9:8 | 1245.A.15.L |) | - | 1582^c |
| Matt 9:13-14 | 1245.B.6.R | + | - | $118^2, 1582^c$ |
| Matt 10:3-4 | 1246.B.30.L |)+ | f^1 | f^1 |
| Matt 10:12-13 | 1246.C.26.L | + | f^1 | f^1 |
| Matt 11:23 | 1248.C.28.L |) | - | f^1 |
| Matt 12:3 | 1249.B.1.L | + | - | $118, 1582^c$ |
| Matt 12:22 | 1249.C.41.R |) | f^1 | f^1 |
| Matt 13:3-4 | 1251.B.4.L |) | f^1 | f^1 |
| Matt 13:25 | 1252.A.31.L |) | - | $118, 209$ |
| Matt 13:50-51 | 1253.B.13.L | + | f^1 | f^1 |
| Matt 13:55 | 1253.B.39.L |) | - | $118, 209$ |
| *Matt 15:8 | 1255.A.39.L |)+ | (f^1) | $118, 209$ |
| *Matt 15:8 | 1255.A.39.L | + | - | $1, 131$ |
| Matt 15:16-17 | 1255.B.32.L |) | f^1 | f^1 |
| Matt 15:19 | 1255.C.1.R |) | - | $1, 131, 1582^*$ |
| *Matt 16:13 | 1256.C.31.L | + | f^1 | f^1 |
| *Matt 16:13 | 1256.C.31.L |) | f^1 | f^1 |
| Matt 18:7 | 1259.A.6.L | - | f^1 | f^1 |
| Matt 18:8 | 1259.A.10.L |) | - | $118, 209$ |
| Matt 18:10-12 | 1259.A.33.L | + | - | $118, 209$ |
| Matt 18:35 | 1260.A.34.L | + | - | $118, 209, 1582^c$ |
| Matt 19:17 | 1260.C.33.L |) | - | $118, 209, 1582^c$ |
| Matt 20:15 | 1262.A.2.L |) | - | $1, 118, 1582$ |
| Matt 21:3 | 1262.C.25.L |) | f^1 | f^1 |
| Matt 21:29 | 1263.C.40.R |) | f^1 | f^1 |
| Matt 23:3 | 1266.B.2.L |) | f^1 | f^1 |
| Matt 24:43 | 1269.B.18.L |) | - | $1, 1582$ |
| Matt 24:49 | 1269.B.42.L |) | - | f^1 |
| Matt 25:1-2 | 1269.C.17.R | + | f^1 | f^1 |
| Matt 26:11 | 1271.C.7.R |) | - | $118, 209$ |
| Matt 26:27-28 | 1272.A.40.L | - | - | f^1 |
| Matt 26:53 | 1273.B.4.L | + | f^1 | f^1 |
| Matt 26:60 | 1273.B.41.R |) | - | f^1 |
| Matt 26:65-66 | 1273.C.28.R | + | f^1 | f^1 |
| Matt 26:75 | 1274.A.21.L | + | - | f^1 |
| Matt 27:34 | 1275.B.10.L |) | - | 118 |
| Matt 27:35-36 | 1275.B.16.L | + | f^1 | f^1 |
| Matt 28:14 | 1277.A.19.L |) | B, <i>pc</i> are unique | f^1 |
| Mark 1:2 | 1277.C.3.R |) | f^1 | f^1 |
| Mark 1:7-8 | 1277.C.35.R | + | - | f^1 |
| Mark 1:10 | 1278.A.6.L |) | f^1 | f^1 |
| Mark 1:13 | 1278.A.14.L |) | - | f^1 |
| ^Mark 2:1 | 1279.B.1.L |) | f^1 | f^1 |
| Mark 2:5 | 1279.B.20.L |) | f^1 | f^1 |
| Mark 2:7 | 1279.B.26.L |) | f^1 | f^1 |
| ^Mark 2:16-17 | 1279.C.41.R |)+ | f^1 | f^1 |
| Mark 2:26 | 1280.B.20.L |) | f^1 | f^1 |
| *Mark 3:5-6 | 1280.C.10.L | + | - | $118^2, 1582^c$ |
| *Mark 3:5-6 | 1280.C.10.L | + | - | 131 |
| Mark 3:29-30 | 1281.B.37.L |) | f^1 | f^1 |
| Mark 4:10 | 1282.A.20.L |) | - | f^1 |
| Mark 5:40 | 1284.C.12.L |) | f^1 | f^1 |
| Mark 6:4 | 1285.A.14.L | - | - | f^1 |
| Mark 6:11-12 | 1285.B.12.L | + | f^1 | f^1 |
| *Mark 6:33 | 1286.A.37.L |) | - | f^1 |
| *Mark 6:33 | 1286.A.37.L | - | - | f^1 |
| ^Mark 7:17 | 1287.C.29.R |) | f^1 | f^1 |
| *Mark 7:28 | 1288.A.41.L |) | - | f^1 |
| *Mark 7:28 | 1288.A.41.L |) | f^1 | f^1 |
| *Mark 7:30 | 1288.B.9.L | - | f^1 | f^1 |
| *Mark 7:30 | 1288.B.9.L |) | f^1 | f^1 |
| Mark 7:32 | 1288.B.20.L | - | f^1 | f^1 |
| Mark 8:10-11 | 1289.A.10.L |) | f^1 | f^1 |
| Mark 9:20 | 1291.A.6.L |) | f^1 | f^1 |
| Mark 10:21 | 1292.C.30.L | + | f^1 | f^1 |
| Mark 10:29 | 1293.A.27.R |) | - | f^1 |
| Mark 12:6 | 1295.C.12.R | - | - | $131, 209$ |
| ^Mark 12:14 | 1296.A.14.L |) | f^1 | f^1 |
| *Mark 13:14 | 1297.C.33.R | + | - | f^1 |
| *Mark 13:14 | 1297.C.33.R |) | - | f^1 |





| | | | | |
|----------------|-------------|---|------------------|-----------------------------|
| Mark 14:19-20 | 1299.B.28.L | + | f^1 | f^1 |
| Mark 14:22 | 1299.C.3.R | + | - | 118 |
| ^Mark 14:39-40 | 1300.A.39.L |) | f^1 | f^1 |
| Mark 14:45 | 1300.B.30.L | + | f^1 | f^1 |
| Mark 14:51-52 | 1300.C.13.L |) | f^1 | f^1 |
| Mark 15:7-8 | 1301.C.20.R |) | f^1 | f^1 |
| *Mark 15:34 | 1302.C.5.L | - | f^1 | f^1 |
| *Mark 15:34 | 1302.C.5.L | + | B, pc are unique | f^1 |
| Luke 1:28-29 | 1305.A.17.L | + | - | 118 |
| Luke 2:15 | 1307.B.9.L |) | f^1 | f^1 |
| Luke 2:33 | 1308.A.11.L |) | - | 118, 209, 1582 ^c |
| ^Luke 3:5b | 1309.A.23.L |) | f^1 | f^1 |
| Luke 4:7 | 1310.C.21.L | + | - | f^1 |
| Luke 4:8 | 1310.C.25.L |) | f^1 | f^1 |
| Luke 4:10-11 | 1310.C.39.L | + | f^1 | f^1 |
| Luke 6:9 | 1314.B.26.L |) | (f^1) | f^1 |
| Luke 6:10-11 | 1314.B.36.L | + | - | f^1 |
| Luke 6:32 | 1315.B.17.L |) | - | 131* |
| Luke 7:11 | 1316.C.27.L |) | (f^1) | f^1 |
| Luke 8:26 | 1319.C.7.R |) | - | 118 |
| Luke 9:44 | 1323.A.4.L | - | - | 131* |
| Luke 9:54-55 | 1323.B.22.L | + | f^1 | f^1 |
| Luke 10:1 | 1323.C.18.R | - | f^1 | f^1 |
| Luke 10:17 | 1324.B.13.L | - | f^1 | f^1 |
| Luke 11:2 | 1325.B.41.L | + | - | 118, 131*, 209 |
| *Luke 12:31 | 1329.A.17.L |) | f^1 | f^1 |
| *Luke 12:31 | 1329.A.17.L | + | f^1 | 1, 118, 209 |
| *Luke 13:8-9 | 1330.C.1.L |) | - | 1 |
| *Luke 13:8-9 | 1330.C.1.L | - | - | f^1 |
| ^*Luke 14:14 | 1332.B.10.L |) | - | 1, 118, 209 |
| ^*Luke 14:14 | 1332.B.10.L | - | - | 131* |
| ^Luke 14:15 | 1332.B.15.L |) | - | 131* |
| ^Luke 14:24 | 1332.C.20.L | + | - | 118 |
| *Luke 15:22 | 1334.A.15.L | - | - | 131* |
| *Luke 15:22 | 1334.A.15.L | - | f^1 | f^1 |
| *Luke 15:22 | 1334.A.15.L | + | f^1 | f^1 |
| Luke 15:30 | 1334.B.15.L |) | - | f^1 |
| Luke 16:14 | 1335.A.18.L | + | f^1 | f^1 |
| Luke 17:4 | 1336.A.8.L | + | f^1 | f^1 |
| Luke 17:37 | 1337.A.24.R |) | f^1 | f^1 |
| Luke 18:14 | 1337.C.10.R |) | - | 118 ² , 131* |
| Luke 18:25 | 1338.A.19.L |) | f^1 | f^1 |
| ^Luke 19:17 | 1339.A.42.L |) | f^1 | f^1 |
| Luke 21:19 | 1342.C.7.L |) | f^1 | f^1 |
| ^Luke 21:25 | 1342.C.41.L |) | - | 131* |
| ^Luke 22:58 | 1345.B.11.L |) | f^1 | f^1 |
| ^Luke 23:23-24 | 1346.B.40.L | + | f^1 | f^1 |
| Luke 23:46 | 1347.B.8.L |) | f^1 | f^1 |
| ^Luke 24:47 | 1349.B.19.L |) | f^1 | f^1 |
| ^John 1:27-28 | 1350.B.18.R |) | f^1 | 1, 131 |
| ^John 1:42 | 1351.A.6.R |) | f^1 | f^1 |
| John 2:14-15 | 1351.C.34.R | + | f^1 | f^1 |
| John 5:2b | 1355.C.1.L |) | f^1 | f^1 |
| ^John 6:11a | 1357.C.1.R | + | - | 118 ² |
| John 7:29 | 1360.C.28.L | + | f^1 | f^1 |
| *John 7:39b-40 | 1361.A.40.L |) | B, pc are unique | f^1 |
| *John 7:39b-40 | 1361.A.40.L |) | - | 1582 ^c |
| John 7:52a | 1361.C.1.R |) | - | f^1 |
| *John 7:52b | 1361.C.3.R |) | f^1 | f^1 |
| *John 7:52b | 1361.C.3.R |) | - | 1 |
| John 8:25 | 1362.A.31.L | + | f^1 | f^1 |
| John 8:39 | 1362.C.6.L |) | f^1 | f^1 |
| John 10:14 | 1365.B.29.L |) | f^1 | f^1 |
| John 10:16 | 1365.B.39.L |) | - | 118, 209 |
| John 10:26 | 1365.C.39.R | + | f^1 | f^1 |
| John 10:29 | 1366.A.7.L |) | B, pc are unique | f^1 |
| John 11:19 | 1367.A.12.L |) | f^1 | f^1 |
| *John 11:29 | 1367.B.7.L |) | f^1 | f^1 |
| *John 11:29 | 1367.B.7.L |) | f^1 | f^1 |
| John 12:7b-8 | 1368.C.18.L |) | - | f^1 |
| John 13:23-24 | 1371.A.36.L | + | B, pc are unique | f^1 |
| John 13:26-27 | 1371.B.7.L |) | f^1 | f^1 |
| *John 14:13 | 1372.A.31.L |) | 1 | 1, 131, 1582 |
| *John 14:13 | 1372.A.31.L |) | B, pc are unique | f^1 |
| John 19:3 | 1377.C.38.L | - | f^1 | f^1 |
| John 19:17 | 1378.B.34.L |) | f^1 | f^1 |
| John 20:18 | 1380.B.7.L |) | f^1 | f^1 |





| | | | | |
|-------------|--------------|---|-----------------------|-----------------------|
| *John 21:15 | 1381.B.28.L |) | <i>f</i> ¹ | <i>f</i> ¹ |
| *John 21:15 | 1381.B.28.L | - | - | 1, 118, 131 |
| | 1382.A.~33.L | + | <i>f</i> ¹ | <i>f</i> ¹ |

Matthew

There are 94 umlauts in Matthew's gospel in Vaticanus, 49 of which certainly mark the location of a Family 1 variant. Of those 49 Family 1 variant umlauts, 30 mark lines of text containing a variant extant in all primary Family 1 manuscripts,³⁸ though two of these are locations where Vaticanus and a handful of other manuscripts have a unique reading. Six of the 49 Family 1 variant umlauts mark lines of text which contain a variant extant in only a single primary manuscript of Family 1. The remaining 13 umlauts mark lines of text containing a variant extant in multiple manuscripts of Family 1. Typically manuscripts 118 and 209 or manuscripts 1 and 131 are paired together. In these locations, manuscript 1582 is more likely to be associated with 1 and 131 than with 118 or 209 unless, however, 1582 has been corrected at that location. In Matthew's Gospel, on umlauted lines that mark places of variation with manuscript 1582, in places where 1582 has been corrected, 1582 has always been corrected to read with 118 and/or 209. There are no places in Matthew's Gospel where an uncorrected 1582 reads with 1 except in places where the entire Family 1 tradition agrees.

Additionally, there are two umlauts, included in the totals above, which mark lines of text that each have two variants extant in Family 1 manuscripts. The first, the umlaut at Matt 15:8 (1255.A.39.L) has Family 1 divided between the two variants. The first variant, a lengthy replacement, is extant in manuscripts 118 and 209.

The second variant has a two-word addition extant in manuscripts 1 and 131. In the second case, the umlaut at Matt 16:13 (1256.C.31.L) also has two variants extant in Family 1 on that line. Both variants, an addition and a replacement, are extant in all of the primary Family 1 manuscripts.

Mark

The umlaut locations in Mark, with regard to their relationship to Family 1 variants, are not nearly as varied as they are in Matthew. There are 56 umlauts in Mark's gospel in Vaticanus, 34 of which mark the location of a Family 1 variant. In one of those 34 locations Vaticanus and a handful of other manuscripts have a unique reading, and all but three of those 34 locations contain variants extant in the entire Family 1 tradition.³⁹ Six of the umlauts mark lines of text that contain two distinct variants as represented in the Family 1 tradition. The umlauts at (1286.A.37.L), (1288.A.41.L), (1288.B.9.L), (1297.C.33.R), and (1302.C.5.L) all contain two Family 1 variants on the line marked. Both variants on all three of the lines are found in the entire Family 1 tradition.

The umlaut at (1280.C.10.L) also contains two variants extant in Family 1 manuscripts, a text addition found only in 118² and 1582^c and a text addition found only in 131. These corrections appear to reflect Byzantine influence.

Luke

There are 78 lines of text marked by umlauts in Luke. Of those, 36 contain lines of text with a Family 1 variant present. At least 22 of these mark lines where the entire Family 1 tradition varies from Vaticanus. At least seven of the 36 lines contain a variant extant only in a single Family 1 manuscript, and in at least three of the 36 lines which contain a Family 1 variant, the variant is extant in two or three manuscripts from the Family 1 tradition.⁴⁰ The

³⁸ As noted above, the manuscripts that will be considered primary are 1, 118, 209 and 1582. Codex 131 will also be cited because its readings are readily accessible, though its inclusion will be properly weighted in any conclusions drawn.

³⁹ The number may actually be four, not three if the umlaut at (1280.C.10.L) is included as noted at the end of this paragraph.

⁴⁰ The numbers given here (22 of 36, 7 of 36, and 3 of 36) could each be higher by as many as two or three because.





umlauts at (1329.A.17.L), (1330.C. 1.L), and (1332.B.10.L) each contain two Family 1 variants on the line marked. The umlaut at (1334.A.15.L) contains three distinct variants on the line marked, one found only in Codex 131, the other two representing a variant with all the primary Family 1 manuscripts.

John

John's Gospel in Vaticanus contains 52 umlauts marking 51 lines of text.⁴¹ Twenty-six of the 52 umlauts mark lines of text containing variants extant in Family 1 manuscripts. Of those, at least 19 contain variants representing the entire Family 1 tradition. At least one is represented by only a single manuscript, and at least two are represented by multiple manuscripts in the Family 1 tradition.⁴²

The umlauts at (1361.A.40.L), (1361.C.3.R), (1367.B.7.L), (1372.A.31.L), and (1381.B.28.L) each contain two distinct Family 1 variants on the line marked. At three of the 26 Family 1 umlaut locations, Vaticanus and a handful of other manuscripts contain a unique reading.

Establishing a Connection between Vaticanus and Family 1

What is abundantly clear is that data resulting from an examination of the umlauts for Family 1 variants passes the test set out above. There is a significantly high number of Family 1 variants found at umlaut locations in the Gospels. In total, 145 umlauts in the Gospels mark locations that contain Family 1 variants.

These locations make up 51.8% of the 280 total umlauts in the Gospels. This percentage is noticeably high, with half the lines of text marked by umlauts containing a Family 1 variant.

It should be noted, however, that a number of these 145 Family 1 variant locations are probably not indicative of a Family 1 variant known to the scribe of Vaticanus who made the umlauts. The vast majority of the umlauted lines contain variants from other text families as well, and it is impossible to know which variant the scribe intended to mark with the umlaut. Also, many of the umlauts listed above mark a line with a variant found in only a single manuscript in the Family 1 tradition. This does not exclude, but does cast doubt on, the likelihood that the scribe placing the umlauts was aware of a Family 1 variant. Additionally, a few of the umlauts listed above represent corrected manuscripts from the Family 1 tradition where the original text agreed with Vaticanus but was later corrected to a different reading.

There are also a number of locations noted in Table 1 where a Family 1 variant is extant at that location because Vaticanus and a handful of other manuscripts have a unique reading. In other words, Vaticanus disagrees with Family 1 as well as with the vast majority of other Greek manuscripts.

Again, this does not exclude the possibility that it was an ancient Family 1 reading that was the impetus for the umlaut, but since an umlaut at that location could have resulted from a variant in many text-types, these umlauts should be viewed skeptically with regard to a Family 1/Vaticanus relationship. It should also be noted that, though cited consistently in the apparatus, Codex 131 is problematic for determining an ancient Family 1 reading. As discussed above, 131 is not a consistent witness for the

as discussed above, four of the umlauts in Luke contain multiple variants on the line that are extant in the Family 1 tradition. At each of these, at least one of the variant units is extant in all Family 1 manuscripts; two are extant only in manuscripts 1, 118, and 209; and three are only extant in manuscript 1 or 131.

41 The final umlaut at (1382.A.~33.L) is marking the middle of almost an entire column of empty space. As will be demonstrated above, this is most likely a Family 1 variant, marking the location of the Pericope de Adultera.

42 These numbers (19 of 26, 1 of 26, and 2 of 26) could be as many as two or three higher because five of the umlauts in John contain multiple variants on the line that are extant in the Family 1 tradition. At each of these, at least one of the variant units is extant in all Family 1 manuscripts, two are extant only in a single manuscript, and two are extant in multiple manuscripts.





Family 1 tradition.

If Family 1 umlaut locations that have only a single manuscript witness (e.g., the only variant from Family 1 extant on an umlauted line is found exclusively in Codex 209) are dropped from the tally, the number of Family 1 umlauts drops from 145 to 126 or 45.0% of the umlauts, which is still a high total.⁴³

If Family 1 umlaut locations that only have variants found in corrected manuscripts are dropped, the tally is further reduced from 126 to 124 of 280 or 44.2%. If the Family 1 umlaut locations that are the result of a mostly unique reading of Vaticanus are excluded, the tally drops to 118 or 42.1% of the total umlauts in the Gospels.

If the tally of Family 1 umlauts is reduced to only include those locations containing a variant representing all of the primary manuscripts in the Family 1 tradition, the tally drops to 94 of 280 or 33.2%.

This means that the number of umlauts in the Vaticanus Gospels that most likely represent locations where the scribe who placed the umlauts could have been aware of a Family 1 variant is somewhere between 33.2% and 44.2%, though it could be as high as 51.8%. This appears to be a significantly high number. Without a control group, however, with which to compare these figures, it would be overly hasty to declare a Vaticanus umlaut/Family 1 connection.

In order to be demonstrated as significant, the number of Family 1 variants found on lines marked by umlauts would have to be significantly higher than the number of Family 1 variants found on non-umlauded lines. These non-umlauded lines will be the control group. For each line in Matthew's Gospel that contains an umlaut, the following twenty lines were also checked for a variant in one of the primary Family 1 manuscripts. For consistency, all variants were tallied — single manuscripts, corrected manuscripts, and Codex 131 — for the umlauted lines and for the non-umlauded lines.

The results are displayed on the chart below. The first column is the location of the umlaut in Matthew. The second column indicates whether or not a Family 1 variant was found at the umlauted line; a 1 indicates the presence of a Family 1 variant, a 0 indicates no Family 1 variant.

The 20 columns following represent each of the 20 lines following the umlauted line in Vaticanus, with a 0 indicating no Family 1 variant and a 1 indicating a Family 1 variant found on the line. Numerals marked **bold** indicate lines that are among the “next twenty lines” but are also marked by an umlaut because the umlauts were less than twenty lines apart in Vaticanus.

Table 2: Probability of Family 1 Variants.

| | | | | | | | | | | | | | | | | | | | |
|------------------------------|---|---|---|---|---|---|---|----------|---|---|---|---|---|---|----------|---|----------|---|---|
| Matt 1:18 (1235.C.18.R) | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 1:23 (1236.A.6.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Matt 2:18 (1237.A.1.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 3:8 (1237.B.30.L) | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 3:9–10 (1237.B.37.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |

⁴³ Note that dropping from the tally singular manuscripts in the Family 1 tradition also eliminates manuscript 131, except for those locations where 131 agrees with another manuscript in the Family 1 tradition.





| | | | | | | | | | | | | | | | | | | | | |
|-------------------------------|---|---|---|---|---|---|---|---|---|---|---|---|----|---|---|---|---|---|---|---|
| Matt 3:11-12 (1237.C.9.R) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 3:12 (1237.C.12.R) | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 |
| Matt 3:15-16 (1237.C.30.R) | 1 | 1 | 0 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Matt 4:16 (1238.B.27.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 5:11 (1239.A.40.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 5:22 (1239.C.19.R) | 1 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 5:41 (1240.B.33.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 |
| Matt 5:44 (1240.C.1.L) | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 |
| Matt 5:45 (1240.C.6.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0* | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 5:47a (1240.C.16.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Matt 5:47b (1240.C.18.L) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 6:1 (1240.C.23.R) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 6:5 (1241.A.7.R) | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 0 | 1 |
| Matt 6:9 (1241.A.36.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 |
| Matt 6:13-14 (1241.B.9.L) | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 |
| Matt 6:21 (1241.C.7.R) | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 6:25 (1241.C.31.R) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 7:16 (1242.C.31.L) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 7:21-22 (1243.A.12.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 1 |
| Matt 8:9 (1243.C.11.R) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 8:13 (1243.C.40.R) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 8:18 (1244.A.22.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Matt 8:30 (1244.B.40.L) | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 |
| Matt 9:4 (1244.C.40.L) | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |





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|--------------------------------|---|---|---|---|---|---|---|---|---|---|---|---|---|---|----|---|---|---|---|---|
| Matt 9:8 (1245.A.15.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 9:13-14 (1245.B.6.R) | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 9:25 (1245.C.30.R) | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 1 |
| Matt 10:3-4 (1246.B.30.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 10:12-13 (1246.C.26.L) | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 |
| Matt 10:29 (1247.B.33.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 1 |
| Matt 11:23 (1248.C.28.L) | 1 | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 1 |
| Matt 12:3 (1249.B.1.L) | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 |
| Matt 12:15 (1249.C.11.R) | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 12:22 (1249.C.41.R) | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 12:23 (1250.A.2.L) | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 |
| Matt 13:3-4 (1251.B.4.L) | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 |
| Matt 13:25 (1252.A.31.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 1 | 1 | 1 | 0 |
| Matt 13:47 (1253.A.38.R) | 0 | 0 | 1 | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 |
| Matt 13:50-51 (1253.B.13.L) | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Matt 13:55 (1253.B.39.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Matt 14:20 (1254.B.18.L) | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 15:5-6 (1255.A.31.L) | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0* | 0 | 0 | 0 | 1 | 0 |
| Matt 15:8 (1255.A.39.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 |
| Matt 15:9 (1255.B.3.L) | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Matt 15:14 (1255.B.23.L) | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Matt 15:16-17 (1255.B.32.L) | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 15:19 (1255.C.1.R) | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 1 |
| Matt 16:13 (1256.C.31.L) | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | 0 |





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|--------------------------------|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Matt 17:2-3 (1257.C.7.R) | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 18:7 (1259.A.6.L) | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 18:8 (1259.A.10.L) | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 |
| Matt 18:10-12 (1259.A.33.L) | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 |
| Matt 18:22 (1259.C.10.R) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 1 |
| Matt 18:35 (1260.A.34.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 1 | 1 | 0 |
| Matt 19:5 (1260.B.18.L) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 |
| Matt 19:17 (1260.C.33.L) | 1 | 0 | 1 | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 0 | 1 |
| Matt 19:23 (1261.A.21.L) | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Matt 20:7 (1261.C.9.R) | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 |
| Matt 20:15 (1262.A.2.L) | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 21:3 (1262.C.25.L) | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 1 |
| Matt 21:29 (1263.C.40.R) | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 |
| Matt 21:37 (1264.B.7.L) | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 21:41 (1264.B.22.L) | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Matt 22:32 (1265.C.30.R) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 0 | 0 |
| Matt 22:37-38 (1266.A.7.L) | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 1 | 0 | 1 |
| Matt 23:3 (1266.B.2.L) | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 |
| Matt 23:5 (1266.B.19.L) | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 |
| Matt 23:8 (1266.B.29.L) | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 24:1-2 (1267.C.31.R) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 |
| Matt 24:6-7 (1268.A.17.L) | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 |
| Matt 24:43 (1269.B.18.L) | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 0 |
| Matt 24:49 (1269.B.42.L) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 0 |





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|--------------------------------|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|
| Matt 25:1-2 (1269.C.17.R) | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 0 | 0 |
| Matt 25:13 (1270.A.18.L) | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 0 |
| Matt 25:34 (1270.C.32.L) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 26:11 (1271.C.7.R) | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 |
| Matt 26:17 (1271.C.31.R) | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 |
| Matt 26:26 (1272.A.35.L) | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 26:27-28 (1272.A.40.L) | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Matt 26:42-43 (1272.C.35.L) | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 26:53 (1273.B.4.L) | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 |
| Matt 26:60 (1273.B.41.R) | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 26:65-66 (1273.C.28.R) | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 1 | 1 | 1 |
| Matt 26:75 (1274.A.21.L) | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 |
| Matt 27:34 (1275.B.10.L) | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 27:35-36 (1275.B.16.L) | 1 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 |
| Matt 27:55 (1276.A.18.L) | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 |
| Matt 28:5 (1276.C.31.L) | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Matt 28:14 (1277.A.19.L) | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 |
| Totals: | 49 | 22 | 17 | 15 | 17 | 23 | 25 | 12 | 18 | 17 | 17 | 13 | 16 | 15 | 20 | 15 | 19 | 23 | 20 | 21 | 20 |

In the Gospel of Matthew there are 94 umlauts, 49 of which contain a Family 1 variant of some type or 52.1%. As noted above, this figure is congruous with the percentage of Family 1 variants to umlauted lines throughout the Gospels.

When the twenty lines following each of the 94 umlauted lines are checked for Family 1 variants, Table 2 above demonstrates that the percentages drop dramatically.

The highest incidence of Family 1 variants occurs on the sixth line following umlauted lines, with 25 Family 1 variants. The lowest incident of Family 1 variants occurs on the seventh line following the umlauted lines, with only 12 Family 1 variants. The twenty lines following umlauted lines average 19.8 Family 1 variants out of 94 lines examined or roughly 21%.



This helps demonstrate that there is a statistically significant number of Family 1 variants found on umlauted lines. An umlauted line is more than two times more likely to contain a Family 1 variant than a non-umlauted line. Depending on how rigorous the standard, somewhere between 33.2% and 51.8% of the umlauts contain a likely known and umlauted Family 1 variant. In the control group (a sample of non-umlauted lines in Matthew), however, only 21% of non-umlauted lines had a Family 1 variant, and this 21% was achieved by counting every possible Family 1 variant (in single manuscripts, in corrected manuscripts, and in Codex 131). This is a strong indication that the scribe who placed the umlauts in Codex Vaticanus used a manuscript or manuscripts that contained Family 1 readings as a source for some of the umlauts.

The data also indicates that there clearly is parity between the kind of variation expected between Family 1 and Codex Vaticanus and the kinds of variants found on lines marked by the umlauts. As with any umlauted line, it is impossible to tell which variant is being marked if more than one variant is extant on the line, and it is impossible to tell which manuscript or manuscript type was the source for the umlaut when a variant appears in more than one manuscript type on a single line. There are a few notable places, however, where the Family 1 variant appears more likely to be the variant that the scribe intended to mark with the umlaut. Also, it is worth noting that the Family 1 variants found on lines marked by umlauts are substantial.

Below are six umlaut locations where it is the Family 1 variant that appears to be the most likely candidate for the variant being marked by the umlaut. Only two of these are exclusively Family 1. The rest of these examples, like the majority of the 144 umlauted lines which contain Family 1 variants, also contain non-Family 1 variants or the line contains multiple variants that are attested to by other texts and text types along with Family 1. With each of the examples below, after the location is given, the relevant text from the line of Vaticanus is provided.

The bold text surrounded by “|” is the line in Vaticanus; any additional text is from the line above or the line below and is provided because some of the variant extends to that text.

| | | |
|--|--------------------------------------|--------------------------------------|
| 1. Matt 15:19 (1255.C.1.R): | λογισμοι πονηροι φο | νοι μοιχειαι |
| | φονοι) φθονοι | 1 131 1582* |
| | φονοι μοιχειαι) μοιχειαν φονοι | L |
| Note: It is possible that the variant marked here is the interpolation found in Codex L. The rest of Family 1 reads with B. | | |
| 2. Matt 20:15 (1262.A.2.L): | εν τοις | εμοις η ο οφθαλμος |
| | εν τοις εμοις) - | b ff ² g ^{1 2} l |
| | η ο) ει | 1 1582 |
| | η) ει | E 118 1424 |
| Note: It seems more likely that if the omission was the variant intended by the umlaut, the umlaut would have been placed one line above, next to the line where the scribe would have first noticed the omitted text (line 1261). This leaves the ει variant with a substantial Family 1 attestation. | | |
| 3. Matt 26:53 (1273.B.4.L): | μου και παραστησει μοι | |
| | μοι) + ωδε | κ * Θ f ¹ (I844) (bo) |
| Note: κ has been corrected to read with B. The only other continuous text Greek manuscript with a variant at this location is Θ. | | |
| 4. Luke 4:7 (1310.C.21.L): | συ ουν εαν προσκυνη | |
| | εαν) + πεσων | f ¹ 124 157 700 |
| Note: The entire Family 1 tradition reads πεσων with a few related miniscules. | | |
| 5. Luke 9:44 (1323.A.4.L): | υμεις εις τα ωτα υμων | |
| | ωτα) - | 131 |
| Note: In Luke 9, Codex 131 is widely regarded to reflect the Family 1 tradition. The rest of Family 1 reads with Vaticanus. | | |
| 6. (1382.A.~33.L) | | |
| | Include <i>Pericope de Adulterae</i> | f ¹ |
| Note: As stated above this is the most notable of the umlauts for determining a Family 1/umlaught alignment. It represents a uniform and exclusive marker for a Family 1 variant. | | |



Like so much work concerning the umlauts, the conclusions here must be considered cautiously, but the evidence does appear to point toward a connection between some of the Vaticanus umlauts in the Gospels and the Family 1 tradition. More than half of the umlauted lines in the Gospels mark the location of a Family 1 variant, and an umlauted line of text in the Vaticanus Gospels is almost three times more likely to contain a Family 1 variant than a non-umlauted line.

The statistical difference between the probability of finding a Family 1 variant on an umlauted line versus a non-umlauted line is also considerably greater than the statistical difference between finding any variant on an umlauted line versus a non-umlauted line.⁴⁴ There are clear examples of umlaut locations where the most likely variant marked by the umlaut is a Family 1 variant.

Additionally, the evidence seems to be clear that it was not an extant Family 1 manuscript or direct earlier predecessor that produced the umlauts in the Gospels. That the apparent age of the umlauts would preclude this is obvious, and a cursory survey of any modern apparatus to the Greek New Testament and Table 2 reveals that there are many Family 1 variants, some of them quite notable, that go unmarked by umlauts. There are more than 300 unmarked Family 1 variants in the Gospel of Matthew alone. It could be that the scribe who produced the umlauts had a manuscript or manuscripts that looked substantially like the text of Vaticanus and the scribe marked every place where the two manuscripts disagreed. It is probably more likely, however, given the wide assortment of variant types found at umlauted lines that the scribe who produced the umlauts had access to more than one manuscript, one of which was a manuscript that did not contain the PA after John 7:52 and which had additional text added to the end of John. The statistical evidence points to this manuscript being related to an ancestor of Family 1. It is also quite possible that the umlauts only reflect places of interest for the scribe umlauting lines, and do not reflect the totality of variation between a manuscript or manuscripts and Vaticanus.

Conclusion

More than one manuscript was most likely employed in the making of the umlauts even within separate textual units (Gospels, Acts/Catholics, Pauline Epistles). Given the nature of the variation marked and the nature of the texts suggested by the umlauts, it also seems possible that the scribe of Vaticanus making the umlauts was not marking every place of variation in the manuscripts he possessed, or even always the most notable places of variation, but rather was marking places of interest. This makes identifying the sources for the umlauts difficult, but candidates can be suggested. There is a noticeable connection between the umlauts and the Syriac text,⁴⁵ there may also be a connection between some of the umlauts and the Western text,⁴⁶ and there is a clearly demonstrable connection between the umlauts in the Vaticanus Gospels and the manuscripts in the Family 1 tradition.

It seems highly unlikely, therefore, that the scribe of Vaticanus had a single manuscript with a mixed text sufficient to produce all of the umlauts. It also appears highly unlikely that these suggested texts are sufficient to explain all of the umlauts. Other texts were likely employed.

Additionally, Vaticanus does not contain the *Pericope de Adultera*, and there is most likely no umlaut at 7:52 marking the PA's omission. There is, however, an umlaut in the column of empty space following the end of John. The most likely explanation for this is that the scribe of Vaticanus who placed the umlauts had access to a manuscript that *did not* contain the PA after John 7:52, but *did have* some additional text amended to the end of John. The most likely candidate for this manuscript is an ancestor of Family 1. Statistical analysis of the frequency of Family 1 variants at umlauted lines confirms this.

44 See Payne, „Distigmai,” 199-226 and Gravely „Text Critical Sigla,” 46-50 for detailed statistical analysis.

45 See Gravely „Text Critical Sigla,” 91-94.

46 Christian-Bernard Amphoux, „Codex Vaticanus B: Les Points Diacritiques des Marges de Marc,” *JTS* 58.2 (2007): 440-66.





VARIETIES OF NEW TESTAMENT TEXT

by Timothy J. Finney

Introduction

It is a privilege to contribute to this volume in honour of Maurice A. Robinson. I first came into contact with Dr Robinson through the TC biblical textual criticism discussion list which had been set up by James R. Adair Jr in the early days of the Internet. I have had the pleasure of meeting Maurice a number of times, once when he showed me some of the sights around the Blue Ridge Mountains, once briefly at an SBL meeting in Washington DC, and once at a colloquium in Münster, Germany. It has always been a pleasure to renew our acquaintance. Besides being a friend, Maurice Robinson strikes me as a man of integrity who takes the Bible seriously. He is no slouch when it comes to the academy, ably defending his conviction that the Byzantine text form deserves consideration when establishing the initial text of the New Testament.

What follows is a statistical examination of data derived from the apparatus of the second edition of the United Bible Societies *Greek New Testament* (1968), referred to as “UBS2” below.¹ The second rather than most recent fourth edition is used because someone took the trouble to convert the UBS2 apparatus data for the Gospels and Acts into a form suitable for analysis. That someone is none other than Maurice A. Robinson, who recorded the UBS2 apparatus entries on “Keysort” punch cards as part of his ThM thesis work. A mechanical sorting system was then used to create tables of percentage agreement from data stored in the cards. All this was done without the benefit of computers, an amazing achievement.² With characteristic generosity, Dr Robinson made his tables of percentage agreement available to me for analysis.

The Hand Copying Process

The New Testament was copied by hand from the time of the apostles until the advent of mechanised printing. Due to the human condition, hand copying does not produce a perfect reproduction of the exemplar. Any writing copied by hand will develop textual variations. Far from being a mark of inferiority, this is proof of authenticity. Variations caused by human copyists are plain to see in any textual tradition which has been through a hand copying phase unless such evidence has been suppressed. Concerning this point, I have on a few occasions asked theology students to copy a short section of the Authorised Version. The students are educated and the four hundred year old language they are copying is archaic, a somewhat analogous situation to copyists reproducing the Greek New Testament centuries after it was written. It is wonderful to behold the errors these students commit in the space of a few verses. Among the more memorable, a “he” was turned into a “she” and “sundry times” became “Sunday Times,” the latter being a local news paper. Compared with ancient copyists, today’s university students are sloppy and absent-minded.

In ancient times a text was reproduced by obtaining an exemplar (i.e. an existing instance of the work) then sitting down to copy. The copyist may have been paid to do the job or might have done it for free as a service to his or her Christian community.³ Literacy was not as prevalent then as now. Nevertheless, a typical Christian community would have people who could read and write. The copyist would read a phrase aloud, remember it, then write out a copy. This “see, speak, hear, remember, write” loop was sometimes confounded by errors of sight, hearing, and mind.⁴ The eyes could skip from one group of letters to a similar group, leaving out what was between. An unfamiliar phrase might be replaced with a familiar one. Other kinds of unconscious alteration could happen as well.

1 Aland and others, *Greek New Testament*, 2nd ed.

2 See “Determination of Textual Relationships” and “Textual Interrelationships” by Maurice A. Robinson. Claire Hilliard and Kay Smith transformed the type-written tables into computer files.

3 Eusebius mentions girls skilled in the art of elegant writing who made copies of Origen’s commentaries.

4 It might seem ludicrous that an error of hearing could occur when the scribe was listening to his or her own voice. However, the ancients always read aloud; their psychology of reading was different to ours. Also, copies were sometimes made by dictation, one person reading aloud while one or more others wrote.





A copyist would also make conscious alterations to the text. This might seem reckless in view of the biblical injunction against adding or subtracting words (Revelation 22.18-19). However, if faced with an apparent error in the exemplar, the scribe need not fear condemnation for attempting to right the wrong. Stylistic editing happened as well, changing an article here and a pronoun there, just as modern versions reflect current rather than centuries old usage.

A few scribes might even have made tendencious changes to “improve” the text, perhaps altering a phrase to suit current local sensibilities. This would have been rare because New Testament copyists were fundamentally conservative. I think that changes of this kind were more likely due to church authorities than humble scribes.

A completed copy might be checked by the scribe or, if produced in a scriptorium, by another charged with that task (i.e. the *diorthotes*). The copy would then enter service in the Christian community for which it was produced. Once there, a reader might continue to make changes to the text, correcting errors made by the scribe or marking up the manuscript with parallel phrases. These parallels might be from other copies of the same section of text. Parallels might also be gleaned from other books, as with the Gospels.

If the marked up manuscript was then used to produce another copy, its copyist would have to decide what to reproduce at every place where there were alternative readings. If there was another manuscript within reach then the two might be compared. Otherwise, the scribe would judge which alternative to propagate by weighing the merits of each one. Copyists thus acted as textual critics.

Comparing Texts

If multiple copies of the same text exist then they can be collated to isolate their differences. The result of a collation is a set of variation units, each comprised of a set of readings.⁵ The differences are of various kinds. Substantive variations are differences of wording. Orthographic variations relate to the surface form — the particular way a scribe spelled words, divided them, wrote letters and syllables, added diacritics, and punctuated. Substantive variations always affect the meaning of the text while orthographic ones usually do not. The apparatus of a typical critical edition will usually level orthographic variations and only present substantive ones. This is a sensible approach as most users of these editions are interested in the words that scribes wrote, not how they were spelled. Nevertheless, orthographic variations represent a largely untapped resource when it comes to analysing relationships among New Testament witnesses.

New Testament researchers are faced with an embarrassment of riches. Tens of thousands of hand written copies have survived, some complete and others fragmentary. Most do not include the whole New Testament. The majority are comprised of smaller collections of books, sometimes the Gospels, sometimes the Acts and General Letters, sometimes Paul’s works, sometimes a smaller subset or just a single book. Their state of preservation ranges from pristine to tattered. They might be in Greek, Latin, Coptic, Syriac, or any other language into which the New Testament was translated before the advent of mechanised printing. Manuscripts of the New Testament are not the only sources that can be compared to add to the thesaurus of readings comprising each variation unit. Patristic citations in Greek, Latin, and Syriac are often included. Another mine of readings is the lectionary manuscripts, copies of the scriptures restricted to those read on particular days of the church calendar.

The advent of computer technology has now begun to expand the number and completeness of witnesses available for comparison. In the past, various short cuts were taken to make the work of compiling a critical edition and its apparatus practicable.

A standard text, often the *Textus Receptus*, was used as a reference point for identifying substantive variations in a particular witness. Orthographic variations were levelled at the outset, immediately discarding this source of potential information. Now there is a sudden avalanche of minutely defined information that can only be handled through computer technology. Once comprehensive data is compared, a very large number of places in the New Testament text turn out to be affected by textual variation. To give an example, the Institute for New Testament

5 If each text is entered into a computer then it can be used to identify all of the places where differences occur, as done in my PhD dissertation, “The Ancient Witnesses of the Epistle to the Hebrews.” A variation unit, also called a variant passage, is a span of text where the readings of witnesses diverge. There is no agreed way to define a variation unit. The larger the span, the less frequently witnesses will share readings. One way to define a variation unit is to divide a standard text into “potential” variation units, where each one is the shortest possible phrase of the text which functions as a part of speech. (A longer text would be preferable for this purpose. There will be places where potential variation units have only one reading among extant witnesses.) Another way is to create a “synthetic” text then use each of its words as a potential variation unit. The synthetic text is a composite which includes the text of every witness in sequence. (See my PhD dissertation for details.)



Textual Research has identified 3046 places of substantive variation in the fascicles of its *Editio Critica Maior* which cover the General Letters. That translates to about seven places per verse. It is not far wrong to say that every place where textual variation can happen, it does happen.⁶

Does this mean that the New Testament is unreliable? God forbid! In fact, the embarrassment of riches is a solid rock providing an unshakable foundation on which to build, if one is willing to do the work required to establish the text. It is quite common for a variation unit to contain only one or two viable alternatives, the rest being so rare and quirky that they can be safely eliminated from consideration. Among those variation units which still have more than one viable alternative, many fall into the “so what” category. That is, if a group of people is presented with the alternative readings, most of them will say, “So what?”⁷

Finally, one is left with variation units where there are multiple viable contenders with significant semantic differences. When the attestations of the readings in these variation units are examined it is quite common to find recurring combinations of witnesses.⁸ A tendency for certain witnesses to share the same readings indicates clustering among them. If textual families do exist, one wonders which cluster or combination of clusters best preserves the apostolic text.

Data Sources

Multivariate analysis (MVA) is used to investigate relationships among items which have values defined for more than one variable. MVA can be applied to the New Testament textual tradition by treating witnesses (e.g. Greek manuscripts, patristic citations, and non-Greek versions) as the items and variation units (i.e. places where the witnesses disagree) as the variables. The possible values of each variation unit are symbols which represent readings. While any set of symbols could be used, including the readings themselves, it is convenient to encode the readings as numerals or letters. The result is a “data matrix” which provides all of the variation data in a compact form. The reading of a witness may be undefined at a variation unit, perhaps due to its text being illegible or the writing surface being entirely missing at the place. If so, a symbol such as NA (for “not available”) is inserted to indicate that the witness is undefined there.

Once the reading of each witness is encoded for each variation unit, a “distance matrix” is constructed which shows the proportion of disagreements for every combination of two witnesses. This proportion ranges from a minimum of zero, corresponding to complete agreement, to a maximum of one, corresponding to complete disagreement. The resulting matrix is square, having the same number of rows and columns. The diagonal, running from the top left to bottom right, is entirely populated by zeros because each witness is a distance of zero from itself. The matrix is also symmetrical about the diagonal because the distance from a witness “x” to a witness “y” is the same as the distance from witness “y” to witness “x.”⁹

Among modern critical editions, the most useful for constructing data matrices are those which explicitly cite every included witness where extant. This makes the United Bible Societies (UBS) editions of the *Greek New Testament* ideal data sources for the purpose.¹⁰ The UBS editions do not present every witness and variation unit.

6 See the Institute for New Testament Textual Research’s *Novum Testamentum Graecum, Editio Critica Maior: IV, Catholic Epistles*. The number of 3046 variant passages is given by Gerd Mink in “Contamination, Coherence, and Coincidence,” 147. There are 432 verses in the General Letters.

7 As an example, in Mark 1.2 one is faced with three alternatives: in the prophets; in the prophet Isaiah; by Isaiah the prophet. For one reason or another, most people don’t think that differences like these matter.

8 The attestation of a reading is the list of witnesses which support it.

9 There is more than one way to calculate the distance. I prefer to use the proportion of disagreements, which is the number of disagreements divided by the number of places at which the readings of both witnesses are known. This measure is also called the “simple matching distance” (SMD).

10 Editions which do not always list all included witnesses where extant are less useful in this respect. In the print world, hand editions which include more witnesses must somehow exercise economies in the apparatus. In the Nestle-Aland editions, these constraints make it necessary to consult the introduction and an appendix in order to determine whether a witness which is not explicitly mentioned in a variation unit has been subsumed under a group heading or is lacunose at the relevant place. In the computer world, considerations of space need no longer constrain the apparatus, as shown by data sources now being made available by the Institute for New Testament Textual Criticism (INTF). See, for example, the “Parallel Pericopes” apparatus located at the Institute’s web site: (<http://intf.uni-muenster.de/apparat/develop/PPApparatus/>).

Nevertheless, those given are intended to be representative:

In conformity with its purpose, this edition continues to offer in its apparatus only a limited selection from the entire range of variant readings of the New Testament text. But for each of the instances selected for the apparatus the reader will find clearly presented the complete range of extant variant readings from a sufficient and representative number of witnesses to provide a faithful reflection of the whole manuscript tradition of the text.¹¹

The data sources for this study are tables of percentage agreement for the Gospels and Acts prepared by Maurice Robinson in the 1970s. Instead of constructing a data matrix, he entered information from the apparatus of the second edition of the UBS *Greek New Testament* into punch cards that were then used to count agreements between each pair of witnesses on a book by book basis. This enabled him to construct the tables of percentage agreement which are the starting point of analysis conducted here. Dr Robinson's tables of percentage agreement were converted to distance matrices by transforming percentage agreements to distances (i.e. proportions of disagreement) according to this formula:

Equation 1. Distance Conversion

distance = proportion of disagreement = (100 - percent agreement) / 100

The resulting distance matrices are available through links in the table of analysis results presented at the following web site: <http://purl.org/tfinney/Views/>

They are located under the "distance matrices" column in rows labelled "UBS2." Scripts written in the **R** statistical computing language were used to convert then analyse Dr Robinson's data.¹² They are available here: <http://purl.org/tfinney/Views/scripts/>

Statistics

Not all in the field of biblical studies are good at statistics. One concept that escapes many is the notion of sampling error.¹³ Given a sample, one can calculate a statistic which estimates a parameter of the entire population. A "margin of error" can also be calculated for the statistic. It is often expressed as a "confidence interval" which is expected to contain the actual value of the parameter. The larger the sample, the smaller the margin of error in relative terms.¹⁴ In the present case, the parameter being estimated is the proportion of disagreement between two witnesses. Unless both witnesses are defined at every variation unit being considered, the proportion obtained is a mere estimate of what would be obtained if both were fully known.

If the number of places where both witnesses are defined is too small then the margin of error grows to include the entire range of possible values. Consequently, a proportion of disagreement calculated from a handful of variation units is virtually useless. (A "percentage agreement" calculated from a handful of variation units is equally worthless.) By the time the number of variation units reaches about twelve, the relative size of the margin of error has decreased to about one half of the range of possible values. For this reason, I set a minimum tolerable number of variation units at which both witnesses must be defined when calculating a proportion of disagreement. If a pair of witnesses does not satisfy this criterion then the least well defined member of the pair is dropped. This selection process is continued until all distances in the matrix are calculated from at least the minimum tolerable number of variation units. In the case of Dr Robinson's data every proportion of disagreement in the distance matrices used for this study is based on fifteen or more variation units.

While on the theme of statistics, it is possible to estimate the range of distances which are expected to occur by chance. As an example, consider a number of trials in which two fair coins are tossed one hundred times. There are four possible states for each toss: head-head, head-tail, tail-head, tail-tail. Two out of four (one half) represent disagreement. The expectation value of the proportion of disagreement is therefore 0.5. One will seldom obtain exactly fifty disagreements in one hundred tosses. However, by using the binomial distribution it is possible to

¹¹ Aland and others, *Greek New Testament*, 4th ed., 1*.

¹² R Development Core Team, *R Language and Environment for Statistical Computing* (<http://www.R-project.org/>).

¹³ The concept has not escaped Dr Robinson, who recorded how many variation units are defined for each witness in the apparatus of each New Testament book he examined.

¹⁴ See the "confidence interval" section of my "Mapping Textual Space" (<http://purl.org/TC/v15/Mapping/index.html>).



calculate that if an infinite number of trials were performed with fair coins, 95% would result in between forty and sixty disagreements (or between sixty and forty agreements). In the general case, one can use the binomial distribution to obtain a confidence interval for the proportion of disagreement across all witnesses given the probability of disagreement (which need not be one half) and the average number of variation units from which the proportion has been calculated for each pair.

Unfortunately, neither of these numbers can be known without a complete knowledge of all witnesses. If, say, one witness of a pair is fragmentary then the number of variation units at which the two are compared is less than for two complete witnesses. Also, to know the probability of disagreement across all witnesses, one needs to know the readings of all witnesses at all places of variation.

This is not the case when some witnesses are fragmentary or when a mere sample of variation units is available for analysis, as is true for real New Testament textual data. What to do?

One solution is to estimate the two values from the relevant distance matrix: the probability of disagreement is estimated by the mean proportion of disagreement; the number of variation units is estimated by the nearest whole number to the mean number of variation units at which each pair of witnesses has been compared. Happily, for all but extreme values, the confidence interval obtained is not terribly sensitive to changes in the value of either the probability of disagreement or the number of variation units. If the probability is not too close to zero or one and the number of variation units is not too small then it is reasonable to expect that the confidence interval obtained with estimated parameters is approximately the same as would be obtained if the exact values were known.

A distance outside the resulting interval indicates an adjacent or opposite relationship between the respective pair of witnesses. If the distance is less than the lower bound of the interval then the two are adjacent, closer than would be expected to happen by chance; if greater than the upper bound then they are opposite, further apart than expected. All New Testament witnesses are ultimately related. However, in the way of thinking outlined here, some are closer together and others are further apart than would be expected if readings had been chosen at random from those available.

Scope of this Study

Dr Robinson's data covers the Four Gospels and Acts. Interesting results emerge at every turn when multivariate analysis is applied to these five data sets. The results indicate that each book has its own textual history, although similarities are sometimes apparent when the broad structure of relationships is compared across books. Much as a full treatment of all five books is desirable, only one book will be examined in this study.

If proportion of representation among New Testament papyri is a useful indicator, Mark seems to have been the least popular of the Four Gospels, in Egypt at least. B. H. Streeter says in one place, "It is the Gospel least valued, least quoted, and most rarely commented on by the Fathers." In another place he says, "Mark provided very few lessons for the selection read in the public services of the Church." Streeter even suggests a canon of criticism whereby, "**Research into the pedigree of a MS. should begin with a study of its text of Mark.**"¹⁵

If Mark was the most humble of the Four Gospels then it may have attracted less attention from correctors, resulting in a less confused transmission history. I will focus on this Gospel to demonstrate how various modes of multivariate analysis can be used to explore textual variation data.

Exploratory Analysis Techniques

Four modes of multivariate analysis will be used to explore relationships between New Testament witnesses included in Dr Robinson's data for the Gospel of Mark. These modes are useful for discovering the broad outlines of the textual tradition, showing which witnesses are alike and how clusters of like witnesses relate to one another.

The first mode, ordering, reproduces parts of the distance matrix itself. A reference witness is chosen and the other witnesses are then ordered according to distance from the reference. When done in conjunction with a confidence interval of expected distances, this mode shows which witnesses have an adjacent or opposite

¹⁵ Streeter, *Four Gospels*, 10, 64.





relationship to the reference. A question remains concerning which witnesses to choose as references. The fourth analysis mode can be used to identify representative witnesses, and a number of these will be used as references when ordering witnesses.

Useful as it is, ordering witnesses by distance from a reference does not help much when it comes to seeing the big picture of how all witnesses relate to all others. The second analysis mode to be used in this study, multidimensional scaling (MDS), produces a spatial configuration of witnesses which is consistent with the distances between them recorded in the distance matrix. The analysis does the best possible job of reproducing distances found in the distance matrix given the available number of dimensions. Approximately as many dimensions as witnesses might be needed for a perfect representation. However, the human facility for spatial perception is limited to three dimensions, and only two dimensional figures can be printed on the pages of a book. Not all of the information in a distance matrix can be squeezed into a two- or three-dimensional multidimensional scaling map. A statistic called the “proportion of variance” gives the proportion of information in the distance matrix “explained” by the resulting MDS map. Its value is usually less than 0.5 for New Testament textual data, meaning that a typical MDS map tells less than half of the story of relationships among New Testament witnesses.

In view of the partial account told by an MDS map, it is fortunate that other modes of analysis are available to tell us more. The third mode to be treated here, divisive clustering (DC), progressively subdivides witnesses into ever smaller clusters to produce a tree diagram called a “dendrogram” in which similar witnesses occupy the same branches. According to the relevant program’s documentation,

The ‘diana’-algorithm constructs a hierarchy of clusterings, starting with one large cluster containing all n observations. Clusters are divided until each cluster contains only a single observation. At each stage, the cluster with the largest diameter is selected. (The diameter of a cluster is the largest dissimilarity between any two of its observations.) To divide the selected cluster, the algorithm first looks for its most disparate observation (i.e., which has the largest average dissimilarity to the other observations of the selected cluster). This observation initiates the “splinter group”. In subsequent steps, the algorithm reassigns observations that are closer to the “splinter group” than to the “old party”. The result is a division of the selected cluster into two new clusters.¹⁶

Each analysis technique helps to form a picture of the way witnesses relate to one another. However, none of those mentioned so far provide a direct indication of how many clusters exist in the data. The fourth mode that will be used to examine the data, partitioning, selects a number of representative witnesses called “medoids” then uses these as centres about which to form clusters containing similar witnesses. The partitioning program needs to be told how many clusters to produce at the outset. A statistic called the “mean silhouette width” (MSW) is a useful indicator of the number of clusters in the data.

Ordering

The first mode of analysis to be applied to the data set for Mark orders witnesses by distance from a reference. They are arranged in ascending order of distance, with each witness name followed by its distance in parentheses. In addition, a 95% confidence interval of distances is calculated from the mean distance between all pairs of witnesses in the distance matrix and the number of variation units used to calculate the distance between a witness and the reference. Any distance outside the confidence interval is unlikely due to chance. Distances marked with an asterisk fall inside the confidence interval, meaning that they are not a statistically significant distance from the reference.

This does not mean that a witness with an asterisked distance is not related to the reference witness. Instead, it means that the asterisked distance would not be unlikely to occur if the readings of the witness and reference had been randomly chosen from those available.

Performing the ordering exercise for a number of witnesses enables one to explore which witnesses tend to be alike and which ones tend to differ. The exercise could be repeated using every witness in the distance matrix as a reference. However, a more efficient approach is to select a few representative witnesses as references. As will be shown later, a statistic called the “mean silhouette width” indicates that it is not unreasonable to partition the Mark data set into five clusters. When this number is chosen, the partitioning procedure identifies L (Codex Regius), Byz (Byzantine witnesses), it-d (Latin side of Codex Bezae), 565 (Empress Theodora’s Codex), and vg (Jerome’s Vulgate) as medoids, meaning that they are identified as being central representatives of the respective clusters. The following table presents ordering results obtained when these five are used as references.¹⁷

¹⁶ Maechler and others, “Cluster Analysis Basics.” The “diana” program is contained in the “cluster” package of R. In the present context an “observation” is a witness.

¹⁷ The R programs used to perform the analysis are available at <http://purl.org/tfinney/Views/scripts/>. The preferred



Table 1: Witnesses Ordered by Distance from Five Representative References

| Reference | Ordered Witnesses |
|-----------|---|
| L | 892 (0.239); Psi (0.337); Delta (0.350); Aleph (0.356); B (0.356); cop (0.400*); P (0.412*); 33 (0.436*); 1241 (0.479*); vg (0.532*); K (0.534*); Lect (0.534*); 1195 (0.534*); 1646 (0.534*); A (0.540*); Pi (0.540*); 1079 (0.540*); 1365 (0.540*); Byz (0.552*); f-1 (0.552*); 1010 (0.552*); 1230 (0.552*); 1242 (0.552*); 1216 (0.558); 2174 (0.558*); 1344 (0.564); 1546 (0.564); Aug (0.567*); 1071 (0.571); 2148 (0.571); syr-pal (0.576*); 1253 (0.577); it-l (0.577); it-aur (0.580); eth (0.581*); 700 (0.583); arm (0.583); geo (0.586); X (0.587); 1009 (0.589); syr-h (0.589); 28 (0.595); syr-p (0.605); 565 (0.607); it-f (0.615); f-13 (0.620); W (0.625); Theta (0.626); goth (0.638); Dia (0.641); it-q (0.655); Orig (0.659); syr-s (0.661); it-c (0.669); it-ff-2 (0.713); it-i (0.720); C-2 (0.727); it-r-1 (0.743); it-b (0.752); D (0.763); it-a (0.768); it-d (0.787) |
| Byz | 1010 (0.052); 1646 (0.087); K (0.099); 1230 (0.099); 1195 (0.105); 1242 (0.105); 2148 (0.105); Pi (0.110); 1079 (0.110); 1344 (0.110); 1365 (0.110); P (0.118); X (0.119); 2174 (0.122); Lect (0.128); A (0.145); 1216 (0.163); 1009 (0.173); 1253 (0.174); 1546 (0.179); syr-h (0.190); 1071 (0.202); 1241 (0.224); goth (0.225); vg (0.301); 33 (0.308); it-l (0.313); C-2 (0.318*); 700 (0.327); syr-p (0.333); f-13 (0.343); it-f (0.369); Dia (0.370); f-1 (0.379); it-aur (0.395); 892 (0.407); 28 (0.409*); it-q (0.412*); eth (0.430*); cop (0.460*); Aug (0.467*); arm (0.506*); syr-pal (0.508*); geo (0.517*); 565 (0.523*); Theta (0.524*); Delta (0.530*); L (0.552*); W (0.556*); it-ff-2 (0.567); it-c (0.574); Psi (0.575*); it-r-1 (0.606); Aleph (0.634); it-b (0.640); syr-s (0.643); it-a (0.648); it-i (0.650); B (0.661); it-d (0.673); Orig (0.683); D (0.684) |
| it-d | it-a (0.216); it-r-1 (0.229); D (0.238); it-i (0.270); it-b (0.280); it-ff-2 (0.280); it-q (0.399); it-c (0.493*); it-aur (0.507*); it-l (0.507*); it-f (0.508*); vg (0.540*); Theta (0.547*); 700 (0.553*); 565 (0.560); goth (0.565*); Aug (0.567*); Dia (0.630); Lect (0.633); arm (0.633); f-13 (0.640); 1071 (0.640); 28 (0.653); 1253 (0.653); K (0.660); 1646 (0.660); A (0.667); 1010 (0.667); 1079 (0.667); 1230 (0.667); 1242 (0.667); 1344 (0.667); syr-p (0.667); eth (0.667); geo (0.672); Byz (0.673); 1195 (0.673); 2174 (0.673); Pi (0.680); 1365 (0.680); syr-h (0.680); Orig (0.683); W (0.687); 1009 (0.687); 1216 (0.687); 1241 (0.687); 2148 (0.693); 1546 (0.700); X (0.706); P (0.706); 892 (0.713); Delta (0.720); 33 (0.722); f-1 (0.740); Aleph (0.747); syr-s (0.750); B (0.760); cop (0.760); C-2 (0.773); syr-pal (0.780); L (0.787); Psi (0.850) |
| 565 | Theta (0.271); 28 (0.360); f-13 (0.376); f-1 (0.414*); 700 (0.415*); arm (0.417*); geo (0.422*); syr-pal (0.424*); Orig (0.439*); P (0.471*); Dia (0.478*); 1071 (0.482*); it-q (0.493*); 1242 (0.494*); 2174 (0.494*); 1216 (0.506*); 1646 (0.506*); it-l (0.509*); 33 (0.511*); 1009 (0.512*); it-a (0.512*); D (0.513*); syr-h (0.513*); 1253 (0.514*); 1010 (0.517*); 1230 (0.517*); 1365 (0.517*); Lect (0.518*); X (0.523*); Byz (0.523*); 2148 (0.523*); 1241 (0.524*); syr-p (0.524*); it-aur (0.525*); 1195 (0.526*); vg (0.526*); K (0.529*); Pi (0.529*); 1344 (0.529*); it-ff-2 (0.533*); 1079 (0.534*); A (0.540*); W (0.542*); goth (0.543*); syr-s (0.545*); C-2 (0.545*); it-c (0.547*); eth (0.548*); 892 (0.557*); 1546 (0.560); it-d (0.560*); it-i (0.560*); it-b (0.568*); it-f (0.569*); it-r-1 (0.569*); cop (0.570*); L (0.607); Delta (0.608); Aug (0.633*); B (0.655); Aleph (0.671); Psi (0.712) |
| vg | it-l (0.083); Aug (0.133); it-aur (0.179); it-f (0.285); Byz (0.301); Pi (0.308); 1010 (0.308); 2174 (0.308); 1646 (0.314); 1079 (0.321); 1242 (0.321); K (0.327); X (0.330); 1195 (0.333); 1230 (0.333); 1365 (0.333); syr-h (0.333); 1253 (0.340); 1344 (0.340); 2148 (0.340); 1241 (0.346); goth (0.348); 1216 (0.353); A (0.359); 1009 (0.359); Lect (0.365); 1546 (0.365); syr-p (0.367); 1071 (0.391); it-q (0.392); 700 (0.397); 892 (0.410); cop (0.410*); Dia (0.413*); it-c (0.426*); it-ff-2 (0.433*); syr-pal (0.458*); f-13 (0.462*); Theta (0.468*); 33 (0.474*); it-i (0.480*); geo (0.483*); it-r-1 (0.486*); f-1 (0.488*); C-2 (0.500*); it-a (0.504*); 28 (0.513*); eth (0.516*); Delta (0.526*); 565 (0.526*); arm (0.526*); P (0.529*); L (0.532*); it-d (0.540*); it-b (0.544*); Aleph (0.558*); B (0.564); W (0.611); syr-s (0.616); Psi (0.625); D (0.632); Orig (0.707) |

number of five clusters was identified by examining the profile of mean silhouette widths produced by the script MVA-PAM-MSW.r. Representative witnesses (i.e. medoids) were identified by MVA-PAM.r. Ordering was performed by rank.r.



These lists show how far witnesses are from representatives of five textual clusters in the Gospel of Mark. The first list orders witnesses by distance from L and shows that 892, Psi, Delta, Aleph, and B are closer than normal to this reference. These are often called “Alexandrian” texts. At the other end of the distance spectrum, 1216, 1344, 1546, 1071, 2148, 1253, it-l, it-aur, 700, arm, geo, X, 1009, syr-h, 28, syr-p, 565, it-f, f-13, W, Theta, goth, Dia, it-q, Orig, syr-s, it-c, it-ff-2, it-i, C-2, it-r-1, it-b, D, it-a, and it-d are further than normal from L. A number of these are what many would call “Western” (e.g. D and the Old Latins). Others are what some would call “Caesarean” (e.g. W (in Mark), Theta, f-13, 28, 565, 700, 1071, geo, arm, and Orig).¹⁸ Three Syriac witnesses (syr-s, syr-p, syr-h) are also further than normal from L. This implies that L-like texts are unlike “Western,” “Caesarean,” and Syriac texts in Mark.

The next row orders by distance from the Byzantine text (Byz), which the partitioning method identifies as the medoid of its cluster. A number of witnesses which are usually called “Byzantine,” such as K and Pi, are located in the immediate vicinity. Others which are not usually called “Byzantine,” such as syr-h (Harclean Syriac), 1071, 1241, goth (Gothic), vg, 33, it-l, 700, syr-p (Syriac Peshitta), f-13, it-f, Dia (Diatessaron), f-1, it-aur, and 892, still appear on the near side of the divide formed by distances that are not statistically significant. On the far side of the divide are it-ff-2, it-c, it-r-1, Aleph, it-b, syr-s, it-a, it-i, B, it-d, Orig, and D. That is, the Byzantine text is unlike texts such as B, D, and the Sinaitic Syriac.

The medoid of the third cluster of the five-way partition is Codex Bezae’s Latin text (it-d). All of the witnesses which are closer than normal to it-d are “Western:” it-a, it-r-1, D, it-i, it-b, it-ff-2, and it-q. Old Latin texts it-a and it-r-1 are closer to it-d than it-d is to D. This goes against the notion that Bezae’s Latin text (it-d) is a direct translation of its Greek text (D). The other side of the divide includes all kinds of texts apart from Western and Vulgate ones. Those furthest from it-d include “Alexandrian” (B, L, Psi) and Coptic (cop) texts as well as the second corrector of Codex Ephraemi Rescriptus (C-2), the Sinaitic Syriac (syr-s), and the Palestinian Syriac (syr-pal).¹⁹

The next row orders witnesses by distance from 565, which the partitioning procedure identifies as representative of the fourth cluster of a five-way split of Dr Robinson’s data for Mark. A number of what some call “Caesarean” witnesses appear at the head of the list. Streeter regarded Theta and 565 as primary authorities for this variety of text; he also included W (Mark), family 1 (f-1), family 13 (f-13), 28, 700, and the Old Georgian as secondary authorities.²⁰ Seven of Streeter’s eight primary and secondary authorities occur among the first eight members of the ordered list of nearest neighbours to 565, including 565 as its own nearest neighbour. The seventh position of the ordered list (counting 565 as first) is occupied by the Armenian version (arm), which Streeter regarded as a supplementary “Caesarean” authority. The location of W (Gregory-Aland 032) far from the head of the list is all that prevents a perfect correlation between Streeter’s primary and secondary “Caesarean” authorities and the list of closest witnesses to 565 presented here.

Only Theta, 28, and f-13 have smaller distances from 565 than would be expected to occur through random selection of available readings. This does not mean that the next closest witnesses are not related to 565. It means instead that one cannot be confident that the distances of witnesses such as f-1, 700, arm, and geo from 565 are not attributable to chance for the given data set. In the data compiled by Dr Robinson from the UBS2 apparatus of Mark, the mean distance between witnesses is 0.485 and the mean number of variation units from which those distances are calculated is 144. Using these numbers with the binomial distribution produces a 95% confidence interval of distances ranging from 0.403 to 0.569.²¹

The main reason why not many members of the 565 cluster satisfy the statistical test is that their distances from one another approach the mean distance between all witnesses for this data set. Including more variation units in the data from which the distance matrix is calculated would have the effect of shrinking the confidence interval of distances attributable to chance. However, if more variation units were included, the distances between members of the 565 cluster would still be relatively large. The 565 cluster is not nearly as compact as, say, the Byzantine cluster, which includes many very similar witnesses. Nevertheless, a cluster does not have to be compact to be real. Statistical analysis sometimes allows one to extract a signal amidst noise. In the case of the “Caesarean” text, Streeter thought that correctors had introduced readings from other textual varieties, thus blurring what was once more focussed. This is a reasonable explanation of how 565 and its allies came to be a less compact cluster.

18 The witnesses listed here as examples of the “Caesarean” text are taken from the “Caesarea” column of “MSS. and the Local Texts” in Streeter, *Four Gospels*, 108.

19 In Dr Robinson’s data, “cop” represents concurrence of the Sahidic and Bohairic Coptic versions. The Palestinian Syriac is actually an Aramaic dialect written in a Syriac script (Aland and others, *Greek New Testament*, 4th ed., 27*).

20 Streeter, *Four Gospels*, 108.

21 A separate confidence interval is calculated for each witness in the ordered lists based on its own number of variation units. Consequently, a few of the indications of statistical significance in the ordered list are not consistent with the interval of 0.403 to 0.569 calculated using the mean number of 144 variation units.





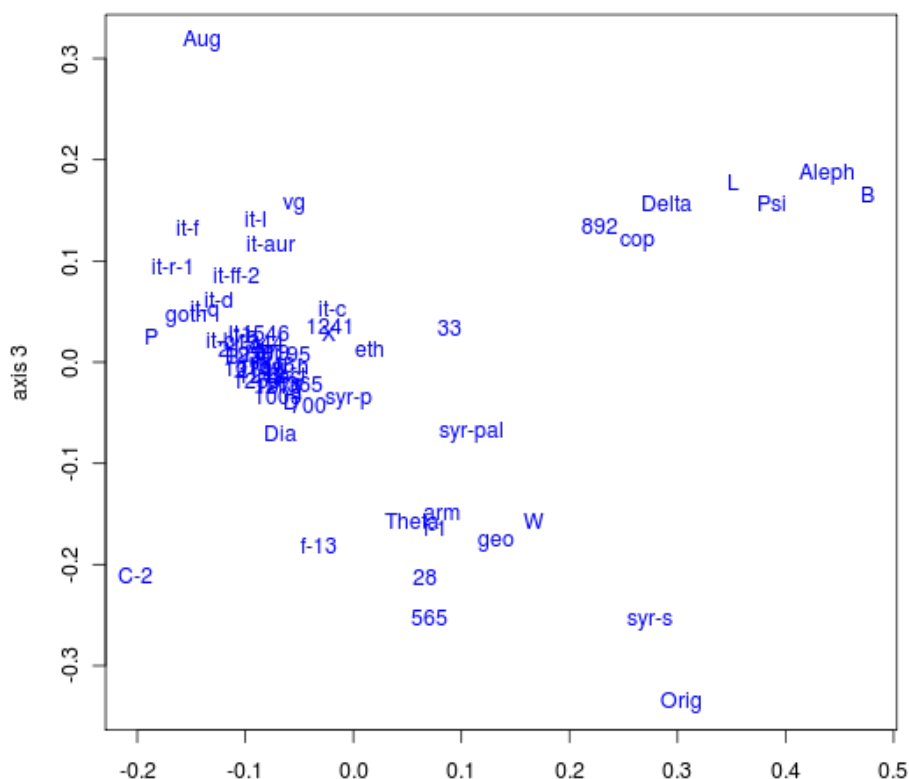
Jerome's Vulgate (vg) is identified as the medoid of the last cluster in the five-way partition. Its closest neighbours are it-l, Aug, it-aur, and it-f, followed by a number of "Byzantine" representatives. Texts such as it-l, it-aur, and it-f may represent the kind of Latin text that Jerome used as a basis for his revision of Mark. Then again they might be copies of Old Latin texts which had been corrected against the the Vulgate. Another possibility is that they are children of the Vulgate contaminated with Old Latin readings. If we didn't already know when and where Jerome had produced the Vulgate, we might have thought it was a late fourth century, North African text based on its proximity to the text used by Augustine. In this case, the date would be about right but the implied provenance would be wrong. At the other end of the scale, the Vulgate is significantly different from the texts of B, W, syr-s, Psi, D, and Origen (Orig).

Multidimensional Scaling

The ordered lists are useful for getting a sense of which texts are alike and which are significantly different. However, they do not give a sense of the direction in which a differing text lies; they do not tell us whether two texts which are equally distant from a reference text are similar or different. This is where multidimensional scaling (MDS) becomes useful. It adds dimensionality to analysis results, placing witnesses in a spatial configuration which gives the best possible approximation to their actual distances for a given number of dimensions. The number of dimensions required to produce a perfect representation might be anything up to one less than the number of witnesses. However, human spatial perception is limited to three dimensions and the printing press is limited to two.

Even with such a drastic constraint, a two-dimensional MDS map still gives a useful impression of witness locations. The many pieces of information contained in a distance matrix resolve into a comprehensible picture of the broad outlines of New Testament textual space. At the same time, one begins to see how wrong it can be to simply describe a text as, say, “Byzantine” or “Alexandrian” or, perhaps, a mixture of the two.

Figure 1. MDS Plot (Dimensions 1 and 2)

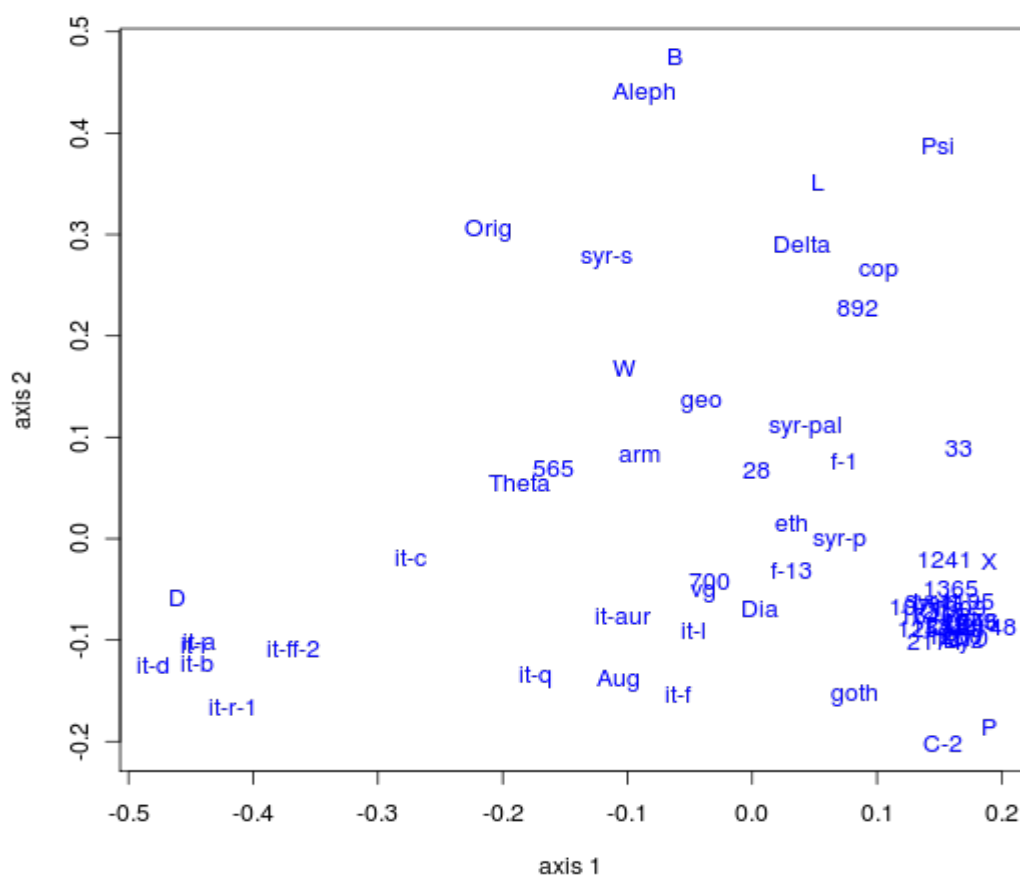


A number of textual varieties cluster together in their own parts of the plot. A dense cloud of “Byzantine” witnesses is in the lower right. “Western” texts such as the Old Latins and D are in the lower left. The Vulgate, it-aur, it-f, it-l, and Augustine’s text are located between the “Western” and “Byzantine” clusters.

“Alexandrian” texts such as Aleph, B, L, Delta, Psi, and 892 occupy the upper right quadrant. “Caesarean” texts such as Theta, 565, W, f-1, f-13, 28, 700, arm, and geo are located near each other towards the centre of the plot.

Such a plot is a mere shadow of the reality. It is a two-dimensional projection of a higher dimensional space. Nevertheless, it is an improvement on one-dimensional views of textual relationships. Other projections can be examined to get a better understanding of the distribution of witnesses in textual space. The following plot of the second and third dimensions makes the separation of the “Byzantine,” “Alexandrian,” and “Caesarean” clusters more clear. However, this projection does not show the separation of “Byzantine” and “Western” clusters as well as the plot of the first and second dimensions.²²

Figure 2. MDS Plot (Dimensions 2 and 3)



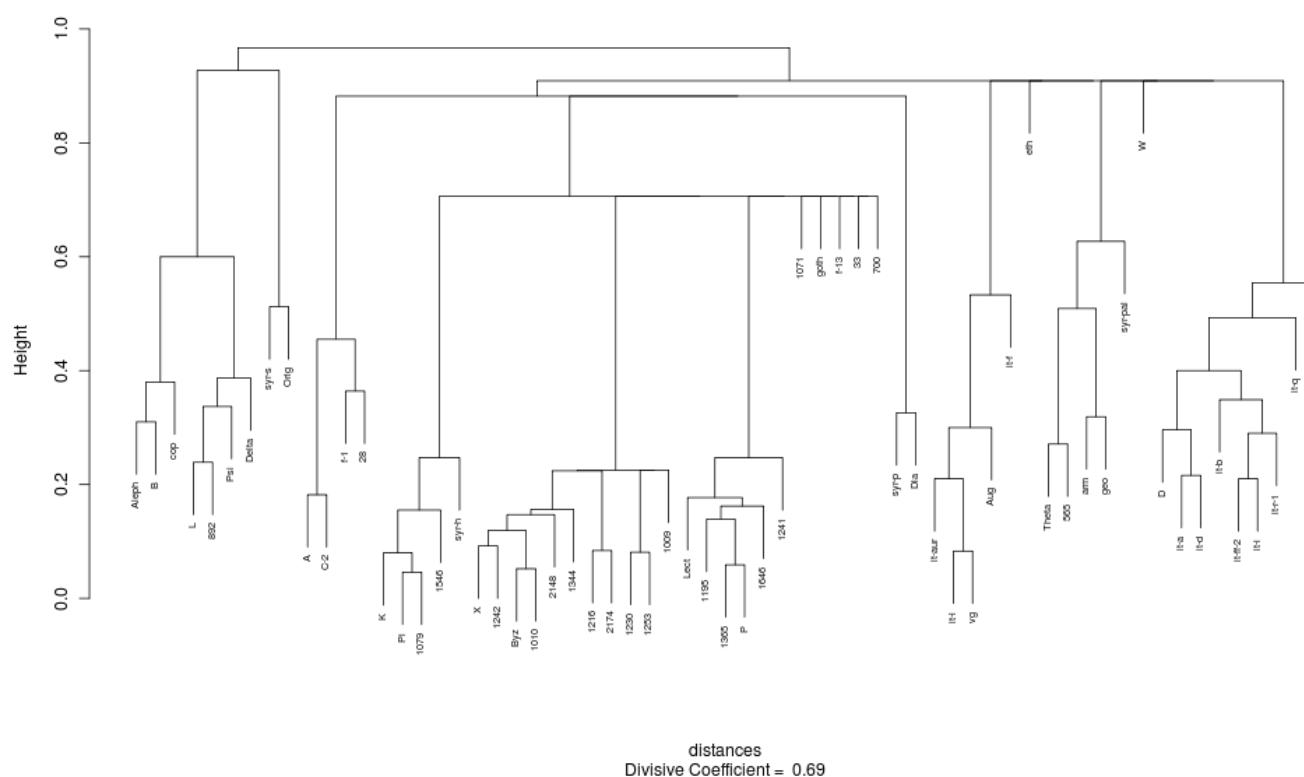
These maps show that one can travel from any one location in New Testament textual space to any other by a series of hops between closely related texts. Looking at the MDS maps shows that there is a relatively high density of witnesses between the Byzantine cluster and each non-Byzantine cluster. Significantly, the density of witnesses between non-Byzantine clusters is relatively low.

²² A rotating plot of the same data which gives the impression of a three dimensional view is available at <http://purl.org/tfinney/Views/cmds/Mark-UBS2.15.SMD.gif>. It shows that the “Byzantine,” “Western,” “Alexandrian,” and “Caesarean” clusters occupy different parts of a basically tetrahedral textual space.

Divisive Clustering

The proportion of variation in the distance matrix explained by a plot of the first two dimensions extracted by multidimensional scaling is only 0.34. The corresponding figure for three dimensions is 0.42, meaning that even a three-dimensional plot fails to explain over half of the information contained in the distance matrix. It is therefore prudent to use other means to examine textual relationships. The following dendrogram is produced by divisive clustering (DC) analysis of the distance matrix derived from Dr Robinson's data:

Figure 3. Dendrogram (Mark, UBS2)



The vertical “Height” scale indicates the distances at which larger groups separate into smaller ones. A partition is achieved by “cutting” the dendrogram with a horizontal line.

For example, these ten clusters are produced by cutting at a height of 0.8:

- Aleph B cop L 892 Psi Delta
- syr-s Origen
- A C-2 f-1 28
- K ... 700
- syr-p Dia
- it-aur it-l vg Aug it-f
- eth
- Theta 565 arm geo syr-pal
- W
- D it-a it-d it-b it-ff-2 it-i it-r-1 it-q it-c

A number of these clusters correspond to ones that also appear in the ordering and multidimensional scaling results, above. Specifically, “Alexandrian” (Aleph ... Delta), “Byzantine” (K ... 700), “Vulgate” (it-aur ... it-f), “Caesarean” (Theta 565 arm geo syr-pal), and “Western” (D ... it-c) families can be seen in all three views of the data.



Differences exist as well. Interestingly, a number of the smaller clusters isolated by this partition contain members of the “Caesarean” variety. In the MDS map, syr-s and Origen are located at the outer extremity, W, f-1, and 28, are more central, while syr-p and Dia are closer to the Byzantine cluster. The fragmentation of the “Caesarean” cluster in this ten-way partition suggests it is more diffuse than the other clusters. As a consequence, it is prone to splitting into smaller groups of texts.

Partitioning

The results obtained so far show that certain structures emerge when distances between New Testament texts are subjected to multivariate analysis. However, the structures are not very well defined. If they were then an MDS map would contain relatively dense islands separated by empty spaces, and a DC dendrogram would consist entirely of long-stemmed branches with tight bunches of leaves below.²³ The lack of clear definition helps to account for the ongoing controversy over identity, composition, and even existence of textual clusters among New Testament witnesses. Nevertheless, we would like to know whether one number of clusters is preferable to another for the data at hand. The final analysis mode to be demonstrated in this study provides guidance on the important question of how many clusters are inherent in the data.

Table 2: Partitioning around Medoids (Mark, UBS2)

| Number | MSW | Clusters | | Members |
|--------|-------|----------|-------------|--|
| | | Medoid | Label | |
| 2 | 0.312 | Byz | Byzantine | Aleph A B K L W X Delta Theta Pi Psi Byz Lect f-1 f-13 28 33 565 700 892 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 it-aur it-f it-l vg syr-s syr-p syr-pal syr-h cop goth arm geo eth P Dia Aug Orig C-2 |
| | | it-d | Western | D it-a it-b it-c it-d it-ff-2 it-q it-r-1 it-i |
| 3 | 0.309 | 892 | Alexandrian | Aleph B L Delta Psi 33 892 syr-s cop Orig |
| | | Byz | Byzantine | A K W X Theta Pi Byz Lect f-1 f-13 28 565 700 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 it-aur it-f it-l vg syr-p syr-pal syr-h goth arm geo eth P Dia Aug C-2 |
| | | it-d | Western | D it-a it-b it-c it-d it-ff-2 it-q it-r-1 it-i |
| 4 | 0.272 | 892 | Alexandrian | Aleph B L Delta Psi 33 892 cop Orig |
| | | Byz | Byzantine | A K W X Pi Byz Lect f-1 f-13 28 565 700 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 syr-p syr-h goth arm eth P Dia C-2 |
| | | it-d | Western | D it-a it-b it-d it-ff-2 it-r-1 it-i |
| | | vg | ? | Theta it-aur it-c it-f it-l it-q vg syr-s syr-pal geo Aug |

²³ Dendrograms are inverted with respect to real trees, placing the trunk above the leaves.





| | | | | |
|---|-------|------|-------------|--|
| 5 | 0.304 | L | Alexandrian | Aleph B L Delta Psi 892 cop |
| | | Byz | Byzantine | A K X Pi Byz Lect f-1 f-13 33 700 1009 1010 1071 1079 1195 1216 1230 1241 1242 1253 1344 1365 1546 1646 2148 2174 syr-p syr-h goth eth P Dia C-2 |
| | | it-d | Western | D it-a it-b it-d it-ff-2 it-r-1 it-i |
| | | 565 | Caesarean | W Theta 28 565 syr-s syr-pal arm geo Orig |
| | | vg | Vulgate | it-aur it-c it-f it-l it-q vg Aug |
| 6 | 0.126 | L | Alexandrian | Aleph B L Delta Psi 892 cop |
| | | Byz | Byzantine | A K X Pi Byz Lect 33 1009 1010 1071 1230 1242 1253 1344 2148 syr-p syr-h goth eth Dia C-2 |
| | | it-d | Western | D it-a it-b it-d it-ff-2 it-r-1 it-i |
| | | 565 | Caesarean | W Theta 565 syr-s syr-pal arm geo Orig |
| | | P | ? | f-1 f-13 28 700 1079 1195 1216 1241 1365 1546 1646 2174 P |
| | | vg | Vulgate | it-aur it-c it-f it-l it-q vg Aug |

The partitioning technique used here divides witnesses into a specified number of clusters using the algorithm described above. The process identifies a corresponding number of representative witnesses called “medoids” and calculates a statistic called the “mean silhouette width” (MSW) as well. A higher MSW indicates a better choice with respect to the number of clusters. Also, the MSW tends to decrease as the number of clusters increases. Consequently, searching for local maxima in a plot of MSW versus number of clusters is a useful way to identify preferred numbers of clusters. In the partitions given above, those with two, three, or five clusters stand out as better choices.

The seed of a new group is the most disparate witness of the cluster with the largest “diameter” (i.e. largest distance between any two members). The first cluster to separate from the initial, all-encompassing superset is the “Western” one. The “Alexandrian” cluster is second to split from the main group. A cluster comprised of Latin, Syriac, and Georgian witnesses, along with Augustine’s text and Theta, separates next. The corresponding MSW is lower than for three or five clusters, indicating that the witnesses less naturally divide into four groups. In the five-way partition, the cluster centred on 565 draws members from two clusters of the four-way partition to form a group comprised of W, Theta, 28, 565, syr-s, syr-pal, arm, geo, and Orig. Whether or not the “Caesarean” label is appropriate, this group of witnesses would seem to constitute a genuine variety of the New Testament text. Taking one more step in a process that could be continued until each cluster is reduced to a single witness, the six-way partition shows that the next cluster to separate draws most of its membership from the main group but takes 28 from the 565 cluster too.

A question mark is used to label a couple of the clusters. There is often an obvious association between the first few clusters which emerge during partitioning and conventional labels such as “Western” or “Alexandrian.” However, it soon becomes necessary to coin new labels because the conventional ones have been exhausted or do not fit a cluster being named. While any number of naming schemes might be proposed, one which commends itself in the present context is to use the medoid of a cluster as its label. There is a catch, however. If the membership of a cluster changes then the medoid can change as well, as happens in the “Alexandrian” cluster whose medoid changes from 892 to L. A modified scheme incorporates medoids which the present cluster had for fewer divisions. Accordingly, the “Alexandrian” cluster might be named “L/892” for the five- and six-way partitions. Unfortunately, it is not always clear which former cluster to associate with the current one. If this happens then some other scheme will have to be used to label the cluster.





Once a cluster separates, its composition often remains largely unchanged as the number of groups increases. This is true of the “L/892,” “it-d,” “565,” and “vg” clusters, all of which recur with substantially the same membership for more than one level of partitioning. Being the largest cluster, most members of new groups tend to be drawn from the “Byz” superset, whose core members remain as other witnesses separate.

A Few More Witnesses

In the process of constructing the distance matrix, witnesses without enough variation units are dropped to keep sampling error below a tolerable level. This sometimes causes interesting yet incomplete witnesses to be excluded from analysis. Happily, it is possible to force a witness to be retained when constructing a distance matrix, provided that the selected witness has at least the minimum required number of variation units. In this way analysis results can also be obtained for \mathfrak{P}^{45} , C, and it-k.

Table 3. Witnesses Ordered by Distance from \mathfrak{P}^{45} , C, and it-k

| Reference | Ordered Witnesses |
|---------------------|--|
| \mathfrak{P}^{45} | 28 (0.387); f-1 (0.419); arm (0.419); W (0.484*); f-13 (0.516*); 700 (0.516*); 565 (0.581); geo (0.581*); Delta (0.613); 1365 (0.613); it-i (0.613); Aleph (0.645); B (0.645); Theta (0.645); A (0.677); D (0.677); 1195 (0.677); 1216 (0.677); 1230 (0.677); 1241 (0.677); 1253 (0.677); it-a (0.677); it-b (0.677); it-d (0.677); it-q (0.677); syr-h (0.677); K (0.710); L (0.710); Pi (0.710); Byz (0.710); 1009 (0.710); 1010 (0.710); 1079 (0.710); 1242 (0.710); 1344 (0.710); 1646 (0.710); 2148 (0.710); 2174 (0.710); it-ff-2 (0.710); it-l (0.710); vg (0.710); syr-s (0.710); cop (0.710); Lect (0.742); 892 (0.742); 1071 (0.742); 1546 (0.742); it-aur (0.742); syr-p (0.742); goth (0.742); X (0.774); 33 (0.774); it-f (0.774); it-r-1 (0.774); eth (0.774); Dia (0.774); it-k (0.806); it-c (0.806); C (0.839); Psi (0.903); Orig (0.903); syr-pal (0.968) |
| C | Aleph (0.371); Delta (0.371); 892 (0.386); 1195 (0.402); L (0.409); 1230 (0.409); 1241 (0.409); 1365 (0.409); A (0.424*); Byz (0.424*); 1079 (0.424*); 1253 (0.432*); 1546 (0.432*); 1344 (0.439*); Pi (0.447*); 2148 (0.447*); K (0.455*); 1009 (0.455*); 1242 (0.455*); 1216 (0.462*); 1646 (0.462*); 2174 (0.462*); 1010 (0.470*); 1071 (0.470*); Lect (0.477*); syr-h (0.508*); Psi (0.512*); B (0.523*); f-1 (0.530*); f-13 (0.530*); it-l (0.530*); 33 (0.538*); 700 (0.538*); cop (0.550*); 28 (0.553*); vg (0.553*); it-aur (0.568); goth (0.583); X (0.587); syr-p (0.598); 565 (0.614); arm (0.621); Dia (0.641); Theta (0.644); it-q (0.644); it-c (0.674); it-f (0.677); eth (0.677); syr-pal (0.678); it-ff-2 (0.682); W (0.705); P (0.706); geo (0.724); syr-s (0.768); D (0.780); it-r-1 (0.780); it-d (0.795); Aug (0.800); it-i (0.810); it-b (0.816); it-a (0.832); Orig (0.927) |
| it-k | B (0.521*); Aleph (0.535*); it-ff-2 (0.545*); it-d (0.549*); it-c (0.592); D (0.620); Delta (0.620); it-a (0.634); Psi (0.648); it-aur (0.648); it-l (0.648); vg (0.648); W (0.662); it-b (0.662); syr-s (0.662); L (0.676); Orig (0.683); arm (0.690); Theta (0.704); 565 (0.704); 892 (0.704); it-q (0.704); cop (0.704); 1365 (0.718); it-i (0.718); 1253 (0.732); geo (0.732); f-1 (0.746); syr-p (0.746); syr-h (0.746); X (0.761); Lect (0.761); 700 (0.761); 1195 (0.761); 1216 (0.761); eth (0.761); 28 (0.775); 1646 (0.775); 2174 (0.775); it-f (0.775); it-r-1 (0.775); goth (0.775); A (0.789); Byz (0.789); 1009 (0.789); 1230 (0.789); 1241 (0.789); 1242 (0.789); 1010 (0.817); 1344 (0.817); 2148 (0.817); Pi (0.831); K (0.845); f-13 (0.845); 1079 (0.845); 1546 (0.845); 1071 (0.859); syr-pal (0.898); 33 (0.901); C-2 (0.909); Dia (0.930); Aug (0.967) |

A sense of where these three are located in textual space can be acquired by considering their nearest neighbours. \mathfrak{P}^{45} (P. Chester Beatty I) is closest to witnesses such as 28, f-1 and arm. C (Ephraemi





Rescriptus) is nearest to Aleph, Delta, and 892. Codex Bobiensis (it-k) is almost equidistant from B and Aleph on one hand, and it-ff-2 and it-d on the other.

Using the partitioning procedure with the \mathfrak{P}^{45} distance matrix, the MSW statistic indicates that a division into four clusters is preferable. When this many groups are produced, \mathfrak{P}^{45} 's cluster includes \mathfrak{P}^{45} , W, Theta, 28, 565, syr-s, syr-pal, arm, geo, and Orig. Five groups are indicated when the distance matrix which retains C is used. In a five-way partition based on that distance matrix, C's cluster includes Aleph, B, C, L, Delta, Psi, 33, 892, and cop. Analysis based on the it-k distance matrix reveals a local maximum in the MSW for six clusters. A six-way partition places it-k in a cluster comprised of D, it-a, it-b, it-k, it-d, it-ff-2, it-r-1, and it-i.

Discussion

The case of it-k shows why it is important to use more than one mode of analysis when examining a text. The ordering procedure places it-k nearest to the texts of Codex Vaticanus and Sinaiticus while partitioning includes it-k in the "it-d" cluster. These results are consistent with the it-k text sharing characteristics of both the "L/892" and "it-d" clusters. Indeed, an MDS map produced from the it-k distance matrix positions its text approximately the same distance from those two clusters.²⁴ Old Latin k is but one example of a witness that is peripheral to the textual centres associated with L, Byz, 565, it-d, and vg. Such a witness can be difficult to classify, especially if located about the same distance from more than one textual centre. A small change in the distance matrix can result in the partitioning procedure shifting a witness from one cluster to another. The same phenomenon occurs in divisive clustering, with a witness located between centres being prone to shift from one dendrogram branch to another. (Such a change in the distance matrix might be caused by using a slightly different sample of variation units or witnesses.) While the MDS procedure does not suffer from sudden shifts of this kind, it is still limited by the practical constraint of having only two or three dimensions with which to operate.

Despite the difficulty of classifying witnesses with shared allegiances, the various modes of analysis agree on the broad structure of the data. In particular, the multidimensional scaling, divisive clustering, and partitioning results closely correspond with respect to the composition of the "L/892," "Byz," "it-d," and "vg" clusters. The correspondence is not as close for the "565" cluster. Both the MDS and partitioning procedures place W, Theta, 565, syr-s, arm, geo, and Origen together. The divisive clustering result has Theta, 565, arm, geo, and syr-pal in the same branch of the dendrogram. The five-way partition also places syr-pal in the "565" cluster. According to the MDS result, families 1 and 13 lie between the "565" and "Byz" clusters. If these families had smaller Byzantine components then they might well have ended up in the "565" cluster of the five-way partition and the corresponding branch of the DC dendrogram.

Even though the "565" cluster is less cohesive than the others, a three-dimensional rendition of the MDS analysis result shows that it is still a distinct entity which occupies its own part of the textual space constituted by the witnesses of Mark's Gospel included in this survey. The spatial structure is hard to visualise from the flat MDS maps printed above but instantly resolves when viewed as a rotating map which adds depth to the picture. Such a view is available here:

<http://purl.org/tfinney/Views/cmds/Mark-UBS2.15.SMD.gif>

This result shows that it is wrong to think of 565 and its allies as mixtures of, say, "Western" and "Alexandrian" readings. If they were then they would be located directly between the "L/892" and "it-d" clusters. Instead, the presence of a unique component distances the "565" cluster from each of the "L/892," "Byz," and "it-d" clusters.

²⁴ The MDS map based on the it-k distance matrix is available from the author.



Streeter recognised the “565” cluster as a distinct variety of text:

So far as the minor variants are concerned — and these are much the most numerous, and are of course the most significant for the study of the relationship of different texts — the text of *fam.* Θ is almost equidistant from both the Alexandrian and Western texts. The balance inclines slightly, but only slightly, to the Western side, while there are a very large proportion of readings found neither in D nor in the typical Alexandrian MSS. We have therefore in *fam.* Θ a clearly defined and distinctive text which may properly be ranked side by side with the three great texts, Alexandrian, Western and Byzantine ... hitherto recognised.²⁵

Admittedly, Streeter was discussing minor textual variations in Mark. By contrast, the variation units of the UBS apparatus relate to differences of greater semantic effect. Nevertheless, Streeter’s statement serves as a remarkably good summary of the MDS result and its implication.

The three dimensional rendition of the MDS result also shows that the witnesses form a tetrahedral structure where texts tend to occupy lines running from the Byzantine variety to each of three non-Byzantine varieties. The Vulgate cluster (it-aur, it-c, it-f, it-l, it-q, vg, Aug) is located between the “Byz” and “it-d” clusters. This location is consistent with Jerome having mixed Byzantine readings with an Old Latin basis to produce his edition of the Gospels. In addition, the three dimensional structure exhibits a paucity of witnesses in regions between non-Byzantine varieties. Whatever they represent, the “L/892,” “it-d,” and “565” clusters tend not to mix.

What do these textual varieties represent? We know the circumstances surrounding the production of the Latin Vulgate and can therefore exclude the associated cluster from consideration as a primitive textual form. That leaves four of the varieties revealed by the five-way partition to be considered, and an attempt will now be made to build profiles to characterise them.

A first clue relates to the earliest translations of the New Testament into languages besides Greek. Each of the three non-Byzantine varieties of text contains a venerable version: “L/892” has the Coptic, “it-d” has the Old Latin, and “565” has the Old Syriac (syr-s). Here is evidence that the three are respectively associated with the Coptic-, Latin-, and Syriac-speaking populations which dwelt in Egypt, the western part of the Roman Empire, and eastward of the Mediterranean.

These versions set upper limits on the dates by which the associated non-Byzantine varieties of the Greek New Testament arose, if indeed Greek prototypes should be blamed for their disparate locations in textual space. According to Jacobus Petzer, the African variety of the Old Latin text, represented here by it-k, is most likely to be dated early in the third century while the European variety, represented here by the Latin members of the “it-d” cluster, appeared shortly afterwards. Frederik Wisse writes that “it is not unlikely that various books of the OT and NT had been translated into Coptic by the second half of the third century.” The fourth edition of the UBS *Greek New Testament* dates the Old Syriac to the third or fourth century.²⁶ Thus, the *terminus ante quem* implied by the associated version is about 250 AD for the “it-d” and “L/892” clusters while for the “565” cluster is about 300 AD.

Knowing which Church Fathers support a textual variety is a great help in building its profile. Unfortunately, most of the patristic data present in Dr Robinson’s tables succumbed to the vetting process required to eliminate witnesses which are too poorly represented to produce reliable analysis results. The two survivors, Origen and Augustine, have interesting things to say.

The text of Mark’s Gospel used by Origen is in the “565” camp, which pushes the latest possible date of this variety back to Origen’s time. Griesbach noticed a change in the textual character of quotations from Mark across various works of Origen. Streeter concluded that the change was associated with Origen’s relocation to Caesarea in 231, after which his quotations conformed to the “Caesarean” text.²⁷ Here is a hint that the “565” text was current in Caesarea early in the third century.

²⁵ Streeter, *Four Gospels*, 84.

²⁶ Petzer, “Latin Version,” 121; Wisse, “Coptic Versions,” 134; Aland and others, *Greek New Testament*, 4th ed., 26*.

²⁷ Streeter, *Four Gospels*, 92.



Augustine's text is allied with the Vulgate family, not an unexpected result. This case serves to warn us that a Church Father may use a text which came from another part of the world. In addition, Eldon J. Epp has shown how quickly and easily manuscripts could travel across the Empire.²⁸ It is therefore prudent to apply the precept found in Deuteronomy 19.15 (NIV) when attempting to associate textual varieties with geographical locations: "A matter must be established by the testimony of two or three witnesses."

So far, two indications point to an association between the "565" text and the geographical region encompassing Syria and Palestine. And perhaps there is a third. In the course of my PhD research on the Letter to the Hebrews, I made letter-for-letter transcriptions of the thirty or so first millennium Greek manuscripts of the book then performed multivariate analysis on their textual and spelling differences. The results were surprising: MDS maps of spelling and textual variations have many points of contact, with each witness often being collocated in both kinds of map. How does one account for this correlation between spelling and semantic variation? One possibility is that scribes spelled according to local custom and preferred to copy local texts rather than ones from far afield. According to this interpretation, textual and spelling clusters correspond to geographical locations. Under this hypothesis, the geographical context of a textual cluster can be fixed by members of known provenance such as P¹³, P⁴⁶, and 016 which were all recovered from Egypt. These three happen to occupy the same textual and spelling clusters.

The Hebrews maps reveal two other spelling clusters besides the one which contains the Egyptian manuscripts. One is associated with the Byzantine variety of text. The other is comprised of 015, 018, 020, 025, 044, 048, 075, and 0150. Circumstantial evidence, which is by no means conclusive, indicates that a number of these have connections with Syria or Palestine.²⁹ It may be that some of these manuscripts, particularly 025, 044, and 0150, belong to a textual cluster in Hebrews that corresponds to the "565" cluster in Mark. Only analysis of spelling variants will tell, something which an aspiring New Testament researcher might like to do.

Nothing has yet been said about the date or provenance of the Byzantine textual variety. While this kind of text is often dismissed as secondary, I would like to suggest a couple of reasons why it may be unwise to do so. One reason relates to the location of Jerome's Vulgate in textual space. The three-dimensional MDS map shows that if not for being displaced away from the "565" cluster, the "vg" cluster would lie directly between the "it-d" and "Byz" clusters. As mentioned before, this location is consistent with Jerome introducing Byzantine readings into a base text of the kind represented by the "it-d" cluster. Now Jerome's preface to the Vulgate says "Igitur haec praesens praefatiuncula pollicetur quattuor tantum evangelia ... codicum Graecorum emendata collatione, sed veterum." (Therefore this present little preface promises only four gospels ... revised by comparison with Greek codices, old indeed.) The MDS map implies that the really old Greek manuscripts Jerome used when editing the Latin text of Mark were of the Byzantine textual variety. Unless he was badly mistaken about the age of the Greek manuscripts, one can hardly date them any later than a century before Jerome wrote his preface. This would set the *terminus ante quem* of the "Byz" text somewhere in the third century, along with the other three varieties. Incidentally, the displacement of Jerome's edition away from the "565" cluster indicates that he avoided readings of this variety. Could the "565" cluster represent the codices of Lucian which Jerome disparages in the same preface?

The other reason relates to ancient Christian population centres. Quoting Streeter yet again,

The destruction of Jerusalem [in] A.D. 70 deprived the Church of its natural centre. The capitals of the larger provinces of the Roman Empire succeeded to the place left vacant; and among these the tradition of Apostolic foundation gave special prestige to Antioch, Ephesus, and Rome. The result was a period of about ninety years of more or less independent development, in doctrinal emphasis, in church organisation, and in the production of religious literature. Hence the history of the three succeeding centuries of Catholic Christianity is largely the story of progressive standardisation of a diversity which had its roots in this period.³⁰

28 Epp, "New Testament Papyrus Manuscripts."

29 Finney, "Ancient Witnesses," vol. 1, chap. 7.

30 Streeter, *Four Gospels*, 1.





Alexandria might be added to this list as well, giving four centres to associate with the four non-Vulgate varieties of text identified in the five-way partition. Few would object to linking the “it-d” variety with Rome and the Latin-speaking part of the Empire; not many would argue against linking the “L/892” cluster with Alexandria and Egypt. Some cause has been given above to associate the “565” cluster with Antioch and the region encompassing Syria and Palestine. This only leaves one cluster and one early Christian population centre to connect. The demand for copies of the New Testament was created by Christian communities, and none was larger than the one in Asia Minor during the second and third centuries.³¹ If the largest Christian population was in Asia Minor then one would expect its local text to be the hardest to dislodge. From this perspective, it would be strange indeed for the local texts of Alexandria, Antioch, and Rome to have survived while the text of Ephesus and Asia Minor vanished, only to be replaced by a late recension.

Conclusion

A five-way partition of Dr Robinson’s data for the Gospel of Mark reveals clusters associated with the kinds of text found in L, it-d, 565, the Vulgate, and Byzantine manuscripts. The Vulgate is known to be a late production. Each of the other four textual varieties can be associated with an early Christian population centre. If these four do reflect the ancient texts of Alexandria, Rome, Antioch, and Ephesus, then each should be given due weight in establishing the initial text of the New Testament. The text printed in the UBS *Greek New Testament* is a good representative of the Egyptian text but gives little ground to the other three. Unless the “L/892” variety can be demonstrated to be the source of the others, something which seems unlikely in view of the population argument, the UBS text is open to criticism as not representing the varieties of text used at the earliest centres of Christianity.

To conclude, I misquote Irenaeus:

It is impossible that the [textual varieties] should be in number either more or fewer than these. For since there are four regions of the world wherein we are, and four principal winds, and the Church is as seed sown in the whole earth, and the Gospel is the Church’s pillar and ground, and the breath of life: it is natural that it should have four pillars, from all quarters breathing incorruption, and kindling men into life.³²

I hope Dr Robinson will find this contribution interesting even though it stops short of endorsing the Byzantine variety as the initial text of the New Testament.

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³¹ According to Harnack, *Mission and Expansion*, 2.182, Asia Minor “constituted the Christian country κατ’ ἐξοχήν during the pre-Constantine era.”

³² Adapted from the translation provided by Streeter, *Four Gospels*, 8.





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THE ALEXANDRIAN PRESUMPTION OF AUTHENTICITY REGARDING THE MATTHEW 27:49 ADDITION

by Abidan Paul Shah

At the conclusion of Matthew 27:49, the major Uncial mss (a B C L along with a number of miniscule mss) include the words “another one, having taken the spear, pierced his side, and water and blood came out.” According to these mss, the piercing came before Jesus’ death. The UBS committee chose, presumably on internal grounds, to omit this strongly supported Alexandrian reading. It appears that modern eclectics have hastily abandoned their time-honored canons and principles in order to avoid a potential contradiction with John 19:34. The common rationale for this decision is that the reading was occasioned by the misplacement of a marginal gloss that was simply a harmonization to the similar account in John 19:34. In time the gloss was incorporated into the text but in the wrong place. The following response by Bruce Metzger in his *Textual Commentary on the Greek New Testament* is typical of most eclectics:

Although attested by a B C L *al* the words *ἄλλος δὲ λαβὼν λόγχην ἐνύξεν αὐτοῦ τὴν πλευρὰν καὶ ἐξῆλθεν ὕδωρ καὶ αἷμα* must be regarded as an early intrusion derived from a similar account in Jn 19:34. It might be thought that the words were omitted because they represent the piercing as preceding Jesus’ death, whereas John makes it follow; but that difference would have only been a reason for moving the passage to a later position (perhaps at the close of ver. 50 or 54 or 56), or else there would have been some tampering with the passage in John, which is not the case. It is probable that the Johannine passage was written by some reader in the margin of Matthew from memory (there are several minor differences, such as the sequence of “water and blood”), and a later copyist awkwardly introduced it into the text.¹

Similar reasoning and dismissal of the reading as an accidental gloss may be seen in all the major commentaries on Matthew.²

Such arguments against the reading are far from convincing. For starters, no existing manuscript displays this phrase as a marginal gloss. Even if this were the case, should not such a gloss be introduced after Matt. 27:50, thus following Christ’s death and be in harmony with John’s account? It seems highly unlikely that the Alexandrian scribes would have wrongly inserted this supposed harmonization at the end of Matt. 27:49 if they were aware of the contradiction with John 19:34. Given that the reading has such strong support, it appears that the longer reading was considered original and non-contradictory by the Alexandrian scribes. In rejecting the reading, modern eclectics are being instinctive and appear to be making a hasty exit out of a potentially awkward situation.

This brief study will begin by challenging eclectics to heed their key proponents and their statements of confidence in eclectic methodology. Then, the reading will be put to test under some major principles underlying modern eclecticism and their critical texts (NA²⁷/UBS⁴). If the reading appears to meet all the requirements of the eclectic canon, then modern eclectics are obligated to treat it with more than a passing rebuff. Since other readings with weaker support have found acceptance under the eclectic

1 Bruce M. Metzger, *A Textual Commentary on the Greek New Testament* [London: United Bible Societies, 1971, Stuttgart: Deutsche Bibelgesellschaft, 1994], 59.

2 Similar dismissals are found in some of the critical commentaries. See Craig L. Blomberg, *Matthew*, The New American Commentary (Nashville: Broadman, 1992), 420; Daniel J. Harrington, S.J., *The Gospel of Matthew*, The Sacra Pagina Series (Collegeville, MN: Liturgical Press, 1991), 400. Robert Gundry and Donald A. Hagner simply refer the readers to Metzger’s *Textual Commentary*. See Robert H. Gundry, *Matthew: A Commentary on His Handbook for a Mixed Church Under Persecution* 2nd ed. (Grand Rapids: Eerdmans, 1994), 574; Donald A. Hagner, *Matthew 14-28*, Word Biblical Commentary. (Dallas: Word Books, 1995), 842.





canons, it is hoped that modern eclectics would consider this reading authentic and include it as a legitimate portion of their critical text.

Modern Eclecticism³: Its Definition and Claims

Eclecticism has been *in vogue* for the past century and remains the choice methodology of most modern text critics in determining the text of the NT. It was defined by Eldon Jay Epp as follows:

when faced with any variation-unit, we would choose the variant reading that appears to be in the earliest chronological group *and* that also makes the best sense when the internal criteria are applied. Moreover, if no one cluster or type of text can be identified unambiguously as the earliest, then we would choose the variant reading in any given case that is in *one* of the earliest clusters *and* that best fits the relevant internal considerations. This method, therefore, utilizes both external and internal criteria and is called 'reasoned eclecticism' or 'moderate' or 'genuine' eclecticism, or simply the 'eclectic' method.⁴

Its various proponents have differed over certain recondite details⁵ and, occasionally, demurred over its limitations;⁶ but, at the end of the day, they remain solidly confident that it is the only legitimate methodology at present that can lead to the original text of the NT. As Michael W. Holmes remarks:

Whether one turns to the introductory discussions or chapters by Metzger, the Alands, Fee, Amphoux, or Holmes, or the more theoretical statements by Colwell, Birdsall, or Greeven, one finds, under the superficial differences of labels, categorization, or arrangement, a virtual unanimity regarding methodology, the key points and aspects of which can all be found in Zuntz. All of these stress the need for a balanced approach that takes into account both external and internal evidence.⁷

Here are a few statements of confidence from some key eclectics. They reflect the common sentiment of most eclectics:

1. Günther Zuntz — Holmes deemed his *Text of the Epistles: A Disquisition upon the Text of the Corpus Paulinum* as "one of the best extended example of a genuinely balanced reasoned eclectic approach to textual criticism."⁸ He expressed his confidence in the eclectic methodology in the following words:

Textual criticism is not a branch of science. Its criteria are necessarily different from those sought by the scientist: they are not, for that reason, less exacting nor less definite. The convergence of arguments drawn from the distribution of the evidence, the dependence of one reading upon the other, the known habits and typical faults of scribes, the characteristic proclivities of interpolators, the development of the language, the stylistic peculiarities of the writer, the context of the passage in question — these, and still other, factors combined can yield a certainty which is no whit inferior to that of the conclusion drawn from a Euclidean axiom.⁹

³ The Eclecticism in focus here is Reasoned more than Rigorous.

⁴ Eldon Jay Epp, "Decision Points in Past, Present, and Future New Testament Textual Criticism," in *Perspectives on New Testament Textual Criticism: Collected Essays, 1962–2004* Supplements to Novum Testamentum (Leiden: Brill, 2005), 267; Previously printed in Eldon Jay Epp and Gordon D. Fee, *Studies in the Theory and Method of New Testament Textual Criticism* Studies and Documents vol. 45 (Grand Rapids: Eerdmans, 1993), 35.

⁵ As Michael W. Holmes notes, "nearly all critics today practice some form of eclecticism." See Holmes, "Reasoned Eclecticism in New Testament Textual Criticism," in *The Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis*, edited by Bart D. Ehrman and Michael W. Holmes, Studies and Documents vol. 46 (Grand Rapids: Eerdmans, 1995), 338.

⁶ Eldon Jay Epp, "The Eclectic Method in New Testament Textual Criticism: Solution or Symptom?" in *Perspectives on New Testament Textual Criticism*, 125–173; Previously printed in *Harvard Theological Review* 69 (1976), 211–57; and Eldon Jay Epp and Gordon D. Fee, *Studies in the Theory and Method of New Testament Textual Criticism* Studies and Documents vol. 45 (Grand Rapids: Eerdmans, 1993), 141–173.

⁷ Michael W. Holmes, "Reasoned Eclecticism in New Testament Textual Criticism," 344.

⁸ Ibid., 339.

⁹ Günther Zuntz, *The Text of the Epistles: A Disquisition upon the Corpus Paulinum* (London: British Academy, 1953), 13.





2. Bruce Metzger — Although he reserved the term ‘eclectic’ for those who were later designated as the rigorous eclectics, he wrote profusely from the “reasoned” eclectic perspective. With regards to the Alexandrian addition in discussion, Metzger along with the UBS editors awarded a “B” rating (almost certain) against the Alexandrian reading — this, in spite of its overwhelming external support!

A similar faux pas by M. van der Valk of ignoring the external evidence in his defense of the longer ending in Mark provoked a tongue lashing by Metzger.

In his *Text of the New Testament*, he castigated van der Valk in the following words:

This essay is a singular exhibition of how not to practise textual criticism! Omitting entirely all consideration of the external evidence of manuscripts, van der Valk concentrates on more or less irrelevant and speculative considerations in order to arrive at what appears to be a predetermined conclusion.¹⁰

Is it not reasonable to expect the same treatment for the Alexandrian addition in Matt. 27:49, which also boasts strong external support?

3. Michael W. Holmes — Holmes has remained firmly confident that reasoned eclecticism is the only way out of the current impasse in NT Textual Criticism. He writes:

we may readily agree that we stand in great need of a more soundly based and persuasively presented history of the text, that the reasoned eclecticism many of us currently practice must be refined and developed further, and that recent discoveries (esp. the papyri) contain much to instruct us in this regard. At the same time, however, it is possible to disagree with the contention that a reasoned eclecticism is at best a temporary approach. Indeed, quite to the contrary, one may suggest that a reasoned eclecticism not only is but will remain for both theoretical and pragmatic reasons, our only option.¹¹

At the end of the paragraph, he reaffirms, “reasoned eclecticism is *not* a passing interim method; it is the only way forward”; and, again, “The methodology known as reasoned eclecticism is no stopgap measure; it is, I suggest, our only option.”¹²

Given such optimistic claims by reasoned eclectics that their approach is the most objective, thorough, rational, and only available option for determining the text of the NT, they should stay the course with regards to Matt. 27:49. The reading should be allowed to pass under normative eclectic canons for external and internal readings and let the chips fall where they may. The remainder of this study will apply the major canons of eclectic methodology, as listed in Metzger’s *Textual Commentary*, to the Alexandrian addition.

External Evidence¹³

The following two external criteria will be applied to the reading simultaneously:

Criterion#1:

“The date and character of the witness. In general, earlier manuscripts are more likely to be free from those errors that arise from repeated copying. Of even greater importance, however, than the age of the document itself are the date and character of the type of text that it embodies, as well as the degree of care taken by the copyist while producing the manuscript.”

10 Bruce Metzger, *The Text of the New Testament: Its Transmission, Corruption, and Restoration* 3rd edition (New York: Oxford University Press, 1992), 229, n. 2.

11 Holmes, “Reasoned Eclecticism in New Testament Textual Criticism,” 347.

12 Ibid., 349; cf. idem, “The Case for Reasoned Eclecticism,” in *Rethinking New Testament Textual Criticism* ed. by David Alan Black (Grand Rapids: Baker, 2002), 77–100.

13 Both the external and internal criteria are taken from Metzger’s *Textual Commentary on the Greek New Testament*, 11–14. Here we will consider only two major criteria under external and internal evidence. As eclectics often specify - not every criterion applies in every reading or with equal weight.





Criterion#2:

“Witnesses are to be weighed rather than counted Those witnesses that are found to be generally trustworthy in clear-cut cases deserve to be accorded predominant weight in cases when the textual problems are ambiguous and their resolution is uncertain.”

The Alexandrian addition¹⁴ is found in three different variations:¹⁵

First variation:

αλλος δε λαβων λογχην ενυξεν αυτου την πλευραν και εξηλθεν υδωρ και αιμα

This form is found in the major Alexandrian uncials a 01 (Codex Sinaiticus) — fourth century; B 03 (Codex Vaticanus) — fourth century; C 04 (Codex Ephraemi Syri Rescriptus) — fifth century¹⁶; and L 019 (Codex Regius) — eighth century¹⁷. The reading is also found in some later minuscules: 5, 871, 1010, 1011, 1057, 1300C¹⁸, 1566, 2126, 2585, and 2622L¹⁹. Finally, four minuscules carry the addition in the original hand: 26*, 175*, 1701*, and 2766*.

Second variation:

αλλος δε λαβων λογχην ενυξεν αυτου την πλευραν και ευθεως εξηλθεν υδωρ και αιμα

It is found only in the minuscule 1416. It adds ευθεως after the first και.

Final variation:

αλλος δε λαβων λογχην ενυξεν αυτου την πλευραν και εξηλθεν αιμα και υδωρ

This form inverts the order of water and blood. It is found in 20 mss. The first is the uncial, Γ 036 (Codex Tischendorfianus) — 10th century; followed by miniscules²⁰: 48, 67, 115, 160, 364, 782, 1392, 1448, 1555, 2117, 2139, 2283, 2328T²¹, 2586, 2680, and 2787. Three are in the original hand: 127*, 1780*, and 2437*.

When evaluated under eclectic methodology, the external evidence shows that the leading Alexandrian mss are united in support of the addition and there is no attempt to rectify the “misplacement” of the Johannine parallel. It could have been easily dismissed if the reading was found in just the three uncials (C L Γ) and the thirty-plus minuscules, but the inclusion of **Ν** and B in the list changed the weight of the decision. Referring to such joint attestations of **Ν** with B, the late eminent scholar of the early twentieth century, Sir Frederic Kenyon, remarked: “In many passages it is found in company with B, preserving obviously superior readings where the great mass of later manuscripts is in error.”²²

14 The Alexandrian addition is omitted in 1,595 mss. Out of this total, the Byzantine mss are 1,426 and the non-Byzantine are 169. There are 15 other mss that are missing the section but not fragmentary; One ms remains unreadable; 2 have a film problem; and about 117 are fragmentary and lacking.

15 The following data has been taken from the *Text und Textwert* series and, hence, only continuous Greek text and not lectionaries are being considered. See Kurt Aland, Barbara Aland, and Klaus Wachtel, eds., in collaboration with Klaus Witte, *Text und Textwert der Griechischen Handschriften des Neuen Testaments. Vol. 4: Die Synoptischen Evangelien. No. 2: Das Matthäusevangelium. 2.2: Resultate der Kollation und Hauptliste sowie Ergänzungen. Arbeiten zur Neutestamentlichen Textforschung*, no. 29. (Berlin:de Gruyter, 1999), 133–4.

16 It should be kept in mind that C is considered to be mixed in character. See Metzger, *Textual Commentary*, 15.

17 Although it was written by a scribe who committed many errors, it agrees quite frequently with B.

18 The reading was corrected to the addition by a later hand.

19 The reading appears as a marginal reading and is not in the main text.

20 Although some of the minuscules are Byzantine, the focus of this paper is on the united witness of the major Alexandrian mss for the longer reading.

21 The reading is the text reading.

22 Sir Frederic Kenyon, *Our Bible and the Ancient Manuscripts*, (New York: Harper & Brothers, 1958), 197.





A similar view was held over a century earlier by Westcott and Hort, who argued, “It is our belief (1) that readings of a B should be accepted as the true readings until strong internal evidence is found to the contrary, and (2) that no readings of a B can safely be rejected absolutely, though it is sometimes right to place them only on an alternative footing, especially where they receive no support from Versions or Fathers.”²³ With regards to Matt. 27:49, Hort²⁴ attempted to remain consistent to their preference for the **Ⲛ** B readings, albeit he placed it in brackets.

He expressed his reluctance in the commentary on significant variants — *Notes on Select Readings* — in the following words:

We have thought it on the whole right to give expression to this view by including the words within double brackets, though we did not feel justified in removing them from the text and are not prepared to reject altogether the alternative supposition.²⁵

By “alternative supposition,” Hort was referring to the possibility that the Western text omitted this “genuine text of the extant form of Mt...on account of the obvious difficulty.”²⁶ One wonders if Hort’s reasoning for placing the reading in brackets is also due to an “obvious difficulty.” If he was truly consistent with his principle of giving primacy to the readings supported by both **Ⲛ** and B, then the reading would have been included in the text without brackets.

Even though modern eclectics often caution against Hort’s fallacy of preferring the “Neutral” text i.e. B,²⁷ they follow suit in their high regard for B in determining the original text of the NT. Metzger described it as “One of the most valuable of all the manuscripts of the Greek Bible,” and Aland referred to it as “by far the most significant of the uncials.”²⁸ The critical eclectic texts of NA²⁷ and UBS⁴ show a distinct preference for B and its superiority over all other manuscripts. Robinson observed that, “statistically... the modern NA²⁷ text remains 99%+ identical to that of Westcott and Hort, even allowing for the 500 recent changes based upon the papyri.”²⁹ Modern eclectics seem to have a time-tested confidence in the major Alexandrian uncials, especially **Ⲛ** and B. In a similar example in Matt. 25:17, where the distribution of the external evidence is almost identical to that in Matt. 27:49, the editors of NA²⁷ and UBS⁴ chose to side with the major Alexandrian uncials **Ⲛ**, B, C, and L against the Byzantine reading (admitted —the textual problem in the former example is not as difficult). One hopes for a consistent application of eclectic principles with regards to the reading at hand.

It should also be pointed out that neither **Ⲛ** nor B is the source of the reading. Although, we have no known 2nd or 3rd century papyri from Egypt that would confirm or deny the presence of this reading, it does not seem to be a reading peculiar to any one of the Alexandrian uncials.

It probably has roots going into the second century, as seen in the joint attestation of **Ⲛ** and B. Furthermore, the external evidence even extends into some versional manuscripts. These include: some individual Latin manuscripts with independent readings (fourth and fifth centuries); some Palestinian Syriac

23 B.F. Westcott and F.J.A. Hort, *Introduction to the New Testament in the Original Greek With Notes on Selected Readings*, (New York: Harper & Brothers, 1882), 225.

24 Reason only Hort is mentioned from here on is because it is common knowledge that he was the primary author.

25 Westcott and Hort, *Introduction to the New Testament*, 22.

26 Ibid.,

27 Metzger, *Text of the New Testament*, 218-9; Kurt Aland and Barbara Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism*, Translated by Erroll F. Rhodes 2nd ed., rev. and enl. (Grand Rapids: Eerdmans, 1989), 14.

28 Metzger, *Text of the New Testament*, 47; Aland, *Text of the New Testament*, 109. The Alands have distinguished their position from eclecticism and prefer to call their approach the “local-genealogical method.” Nevertheless, Holmes considers them in the same general camp. See Holmes, “Reasoned Eclecticism in New Testament Textual Criticism,” 337–8, n. 8.

29 Maurice A. Robinson, “Investigating Text-Critical Dichotomy: A Critique of Modern Eclectic Praxis from a Byzantine-Priority Perspective,” ETS Southeastern Regional Meeting at Southeastern Baptist Theological Seminary, Wake Forest, NC, (March 19, 1999), 12.





manuscripts with independent readings (from about the sixth century); a Coptic Middle Egyptian (from the third century); an Ethiopic manuscript with independent readings (from about 500); and an Old Church Slavonic (from the ninth century). It should be noted that the Coptic versions are not united, unlike the Alexandrian mss. The Coptic Sahidic does not include the addition. It could be because the Alexandrian Greek versions occurred before the translation and creation of the Coptic version.

The passage, even as revised, was deemed non original and too far out and hence excluded from the original Coptic language copies of their national versions.³⁰ The testimony of the versions, albeit sporadic, not only attests that the addition was more than an accidental gloss but also points to an early date for the reading.

There have been some claims that the Diatessaron or the Harmony of the four gospels was the possible antecedent to the variant. In an article in *Biblische Zeitschrift* (1912), J. P. Vogels argued such, partly on the basis of a medieval eleventh century Greek manuscript (72) which contains the following two scholia:³¹

σημειώσις³² ὅτι εἰς τὸ καθ' ἱστορίαν εὐαγγέλιον Διαδώρου [sic]

Because into the gospel according to a report of Diodore

καὶ Τατιάου καὶ ἄλλων διαφορῶν ἁγίων πατέρων· τοῦτο πρόσκειται·

and of Tatian and various other holy Fathers, it adds this:

ση. ἄλλος δὲ λαβὼν λόγχην ἔνυξεν αὐτοῦ τὴν πλευράν·

But another, taking a lance, pierced his side,

καὶ ἐξῆλθεν ὕδωρ καὶ αἷμα· τοῦτο λέγει καὶ ὁ Χρυσόστομος.

and water and blood came out; Chrysostom also says this.³³

The above exhibit should be taken with caution since the interpolation is neither found in Harley 5647, nor is there any Diatessaronic witness that includes the interpolation.³⁴

If the scholia is accurate, then there should have been some evidence in Ephrem's *Commentary*, which is a key witness to the Diatessaron. J. P. van Kasteren even tried to argue that Tatian probably had a Matthean precedent for the interpolation.

Here again the argument seems to build on the actuality of the interpolation in the Diatessaron which still remains doubtful.³⁵ As to the reference to Chrysostom, it may seem that he had some knowledge of the tradition but the evidence is very weak. The context and the difference of wording lead to the conclusion that Chrysostom's quote is simply a "sermonic harmonization."³⁶

30 This suggestion was made by Dr. Robinson in a personal correspondence with me.

31 J. P. Vogels, "Der Lanzenstich vor dem Tode Jesu," *Biblische Zeitschrift*, 396-405; This scholia is also discussed in J.N. Birdsall, "The New Testament Text," in *The Cambridge History of the Bible*, vol. 1: *From the Beginnings to Jerome* edited by P.R. Ackroyd and C.F. Evans (Cambridge: Cambridge University Press, 1970), 325-26.

32 Or σημείωσαι

33 The scholia as well as the translation are taken from William L. Petersen, *Tatian's Diatessaron: Its Creation, Dissemination, Significance, and History in Scholarship* (Leiden: Brill, 1994), 58.

34 Petersen does note a "possible exception" in the Manichaean Homilies. Although the text is damaged on either margin, Petersen points out that line 27 speaks of giving him gall to drink; line 28 mentions the division of his clothing; and line 29 speaks of 'blood' on 'their lances.' Cf. Petersen, 58-9.

35 J. P. van Kasteren, "Der Lanzenstich bei Mt 27:49," in *Biblische Zeitschrift* 12 (1914), 32-4.

36 Maurice A. Robinson, "Investigating Text-Critical Dichotomy," 11, n. 14.





In concluding the section on external evidence, it is worth remembering Metzger's dictum for determining the original text:

In the evaluation of the evidence the student should begin with external considerations, asking himself which reading is supported by the earliest manuscripts and by the earliest type of text. Readings which are early and are supported by witnesses from a wide geographical area have a certain initial presumption in their favour. On the other hand, readings which are supported by only Koine or Byzantine witnesses (Hort's Syrian group) may be set aside as almost secondary.³⁷

Thus far, the evidence seems to be clearly in favor of the Alexandrian addition. As shown in the discussion, the interpolation is not a creation of either **Ν** or B but has a distinct Alexandrian source going into the 2nd or 3rd century, with support noted in some mixed versional manuscripts.

Although some have tried to argue that the reading was widespread, it seems that Egypt was the most likely home for the tradition. Nevertheless, under eclectic methodology, the addition seems to have a claim for authenticity in the eclectic critical texts based on the external evidence.

Internal Evidence

The following two internal criteria will be applied to the reading:

Criterion#1:

"In general, the more difficult reading is to be preferred, particularly when the sense appears on the surface to be erroneous but on more mature consideration proves itself to be correct." Here Metzger clarifies "more difficult" as "more difficult to the scribe" who would be tempted to make an emendation.

The reading is more difficult than the others due to its seeming contradiction with John 19:34. If it were an accident then would such valuable codices as **Ν** B include it in their text? If the scribes were careless enough to have mistakenly included it, then would not someone examining the finished manuscript have caught the error? If both the scribes and correctors were asleep then would it not have caught the attention of the readers or hearers when used? This would call into question other passages as well, since the Alexandrian scribes were not only gullible but slipshod.

This may be too much to claim. It seems safer to conclude that the scribes, correctors, and readers of the leading Alexandrian uncials were neither confused nor negligent but deliberate in keeping a reading that implied a pre-death piercing.

Criterion#2:

"Since scribes would frequently bring divergent passages into harmony with one another, in parallel passages (whether quotations from the Old Testament or different accounts in the Gospels of the same event or narrative) that reading which involves verbal dissidence is usually to be preferred to one which is verbally concordant."

The Matthean and Johannine readings might be similar but they are not the same. The differences between the two passages can be noted in the context of the piercing, change in word order, and absence of certain words.³⁸ To begin with, the Johannine passage is located in the context of the Jews' request of Pilate to break the legs of the crucified ones and take their bodies away. This was due to their high regard of the Preparation Day i.e. the Sabbath. The soldiers proceeded with their orders and broke the legs of the "first" and the "other." On approaching Christ and seeing that he was already dead, one of

³⁷ Metzger, *Text of the New Testament*, 212.

³⁸ Although David C. Parker admits that the wording and the order is not identical, he still thinks that the reading is an example of harmonization. See, David C. Parker, *The Living Text of the Gospels*, (Cambridge: Cambridge University Press, 1997), 40-1.





the soldiers pierced him since it was unnecessary to break the legs to speed up the death process. The spearing was not to inflict further pain but to verify death. The Matthean piercing, on the other hand, was not concerned with the Jews' request or to verify death.

It served to expedite the death of Jesus, as noted in his loud cry immediately following. If the Alexandrian scribes were attempting to harmonize with the Johannine passage, they completely ignored the context and time frame, which is too far-fetched. There are also differences in word order, as seen in the following comparison:

Matthew 27:49 add: αλλος δε λαβων λογχην ενυξεν αυτου την πλευραν
και εξηλθεν υδωρ και αιμα

cf. John 19:34: αλλ' εις των στρατιωτων λογχη αυτου την πλευραν
ενυξεν, και εξηλθεν ευθυσ αιμα και υδωρ

The Johannine account specifies that one of the soldiers pierced his side while the Matthean interpolation states "another one." Also interesting is the αλλ' εις of John as compared to the αλλος in Matthew. The action being instrumental, "with a lance" becomes "having taken a lance."

The verb ενυξεν in the Matthean reading comes before αυτου leaving one with no doubt that he is referring to the side of Jesus and not the owner of the lance. Ευθυσ is not present in the Matthean reading. Finally, the phrase αιμα και υδωρ is reversed to υδωρ και αιμα.

The internal evidence shows that the addition is not a marginal gloss or a harmonization. There are serious differences between the two references and "reflect deliberate editorial or recensional activity on the part of the Alexandrian scribes."³⁹

Possibility of an Early Liturgical Recension

Suggestions for the presence of the Alexandrian addition have been interesting, to say the least.⁴⁰ One stands out. Bart Ehrman, in his characteristic style, claims that the Alexandrian addition was an anti-docetic corruption by the orthodox and that "it suggests that Jesus really did suffer and shed blood while living, that his was a real body that bled, that his was a real and a tangible death."⁴¹ Would not blood flow after death have repudiated the docetics just as well? Were the orthodox so naïve as to create a harmonistic error in scripture while attempting to safeguard orthodoxy?

There is yet another possibility. It could be that the Alexandrian addition served a liturgical purpose in the Egyptian/Alexandrian Orthodox Church. The clue might reside in the Analavos/Analabos on the Great Schema garments of the monks.

As seen in the image below, it brings together all the instruments in the passion of Christ. The suffering of Christ, as depicted in the various objects in the image, was meant to inspire the monks to live a life of constant dying with Christ. It is beyond the scope of this paper to describe the role of each. Here it will suffice to point out that the reed with the sponge (on the right) is balanced with the spear (on the left). It seems that the spear thrust was also considered to be part of the suffering of Christ.

³⁹ Maurice A. Robinson, "Investigating Text-Critical Dichotomy," 10.

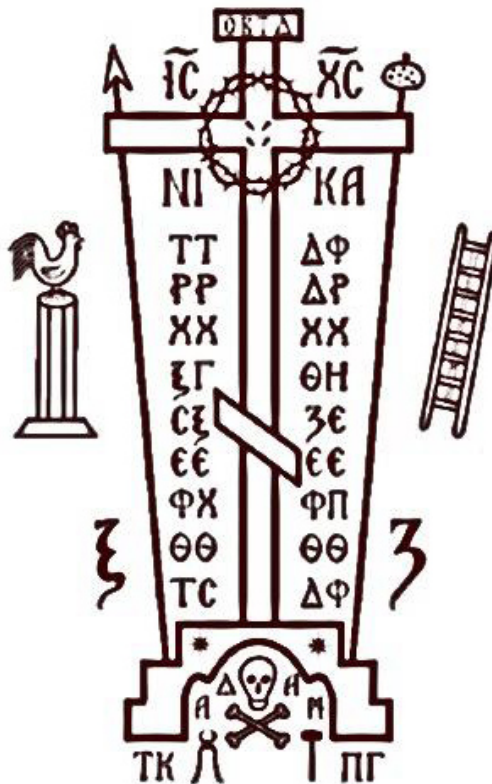
⁴⁰ Stephen Pennells has argued that the Alexandrian addition was actually the original account. It was later removed to avoid the gospel message to be "blunted", since a piercing before death implies "mercy killing." Much of Pennells' arguments seem to be grasping at straws and creates more problems than solution. See Stephen Pennells, "The Spear Thrust," in *JNT* 19 (1983), 102.

⁴¹ Bart D. Ehrman, *The Orthodox Corruption of Scripture: The Effect of Early Christological Controversies on the Text of the New Testament*, (Oxford: Oxford University Press, 1993), 195.





A post-death spearing cannot be an object of suffering, since life has ceased to exist. The Egyptian/Alexandrian Orthodox Church must have shifted the spearing prior to death and included it as an instrument of the suffering of Christ. Hence, the Alexandrian scribes may have followed suit and included the recension to avoid any contradiction with the Analavos/Analabos as well as any tradition of a “pre-death spearing” that was held by the Alexandrian/Egyptian Orthodox Church.⁴²



42 McNamara notes that a similar interpolation is also found in “almost all the Irish texts” (there are some minor variants readings to the text), but it is not from the Old Latin or Vulgate text.” The interpolation did not originate in Ireland but “may been preserved in Ireland for devotional if not liturgical reasons.” See Martin McNamara, “The Latin Gospels, with Special Reference to Irish Tradition,” in *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospels — The Contribution of the Chester Beatty Gospel Codex P⁴⁵*, ed. by Charles Horton JSNT SS258 (London: T & T Clark, 2004), 103-4.





THE TEXTUAL CRITICISM OF LUKE 24:53 AND ITS IMPLICATIONS

by James A. Borland

Introduction

The textual criticism of Luke 24:53 carries more weight than most passages because of its larger bearing on the nature of the autographic text. Luke 24:53 was the last of eight verses B. F. Westcott and F. J. A. Hort used to assert the primacy of the so-called “Neutral” text.¹ The logical result of their reasoning was the irrelevance of the majority of manuscripts that exhibited “Byzantine” readings.

The crux of the matter is whether Luke 24:53 says Christ’s disciples were in the temple “praising **and** blessing” God, or just performing **one** of those functions. Biblical manuscripts overwhelmingly attest the two functions, but a few have just one or the other, not both. Did the autograph say “praising and blessing”? Or as Westcott and Hort, and most biblical scholars today argue, was “praising and blessing” a later combination of two earlier separate strands of text?

The Texts of English Translations

The New Testament has appeared in English translations for more than 500 years. For nearly 400 years the translators saw both “praising and blessing” in their *Vorlagen*, the manuscripts that lay before them in the translation process. But after Westcott and Hort’s *The New Testament in the Original Greek* appeared in 1881 that changed. Their text printed only “blessing.” “Praising” was noted as some kind of early but deviant offshoot of the single original word “blessing.”

1. *English translations exhibiting blessing and praising.* The John Wycliffe Bible of 1395 read “heriyng and blessyng” in Luke 24:53.² William Tyndale’s 1534 edition of the New Testament read “praysinge and lauding.”

The Coverdale Bible by Miles Coverdale of 1535 has “geuyng prayse and thanks.” Similarly the Great Bible of 1540 has “praysyng & laudyng.” The 1560 Geneva Bible also has “praising and lauding.” The Bishop’s Bible of Matthew Parker issued in 1568 has “praysyng and laudyng.”

The 1611 KJV has “praising and blessing,” as does the 1769 KJV revision. Daniel Mace’s 1729 version read “praised and blessed.”

John Worsley and John Wesley each issued translations, respectively in 1770 and 1790, and both had “praising and blessing” as did Thomas Haweis’ 1795 *A Translation of the New Testament from the Original Greek*. Noah Webster’s 1833 translation, Alexander Campbell’s 1835 *Living Oracles Bible*, James Murdock’s 1851 translation, and the 1865 translation of the American Bible Union all had “praising and

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- 1 B. F. Westcott and F. J. A. Hort, *The New Testament in the Original Greek*, 2 vols. (London: Macmillan and Co., 1881), 2:93–107 (§§132–151). The eight proof-texts were Mark 6:33; 8:26; 9:38; 11:54; and Luke 9:10; 11:54; 12:18; and 24:53. George Salmon, *Some Thoughts on the Textual Criticism of the New Testament* (London: John Murray, 1897), 67, says that of the eight cases put forth by Westcott and Hort, “the simplest” is Luke 24:53. Salmon, 68, also notes that “Canon Cook elaborately discussed Hort’s eight cases, contending that in every one of them the conflation hypothesis gives the less probably account of the facts.”
- 2 Heriyng is Middle English for praise according to A. L. Mayhew and Walter W. Skeat *A Concise Dictionary of Middle English From A.D. 1150 To 1580* (EBook #10625, Release Date: January 7, 2004).





blessing.” Leicester Sawyer’s 1858 *The New Testament Translated from the Original Greek* contained both “praising and blessing” as did Robert Young’s *Literal Translation* of 1898, and John Nelson Darby’s *A New Translation* (1871). *The Amplified New Testament*, issued by the Lockman Foundation in 1958, reads “celebrating with praises and blessing and extolling” (italics in original). The NKJV (1978) reads “praising and blessing,” as does *The 21st Century King James Version* of 1994.

2. *English translations exhibiting only blessing.* Charles Ellicott’s ERV of 1885 had only “blessing” in Luke 24:53. The ASV (1901) followed suit along with Ernest Malan’s *Twentieth Century New Testament* in 1900, Richard Weymouth’s 1903 *The New Testament in Modern Speech*, and James Moffatt’s *A New Translation* (1913). In 1923 Edgar Goodspeed’s *An American Translation* read only “blessing” in Luke 24:53 and this trend was continued with the *Revised Standard Version* (1946), the NRSV (1989), the ESV (2001), *The NET Bible* (1996), the ISV (1998), and the HCSB (1999).

3. *English translations exhibiting only praising.* William Whiston’s 1735 *Primitive New Testament* translation printed only “praising” in Luke 24:53 as did George Noyes’ 1869 translation and Samuel Davidson’s 1875 version of the New Testament. The latter was meant to reflect Tischendorf’s text.³ Henry Anderson’s *The New Testament Translated from the Sinaitic Manuscript* and Charles B. Williams’ *The New Testament: A Translation in the Language of the People* (1937) both had only “praising.” The TEV of 1966 has “giving thanks,” probably a rendition of “praising.” The NEB (1961), NIV (1973), JB (1973), REB (1989), NJB (1990), God’s Word (1995; praised), NLT (1996), CEV, TNIV (2002), *The Message* (2002), the NCB (2005), and the CEB (2010) have only “praising” in Luke 24:53.

4. *What the translations are saying.* It is remarkable that so many good Greek scholars can differ greatly in evaluating the evidence for the various readings for Luke 24:53. Those who hold to either praising or blessing alone search for a credible explanation for how the other word appears in some manuscripts. The idea that ΕΥΛΟΓΗΣΕΝ could have migrated from Luke 24:50 and also morphed itself into ΕΥΛΟΓΟΥΝΤΕΣ at 24:53 is such a conjecture.⁴

Those who hold to *both* praising and blessing can reasonably assert that such was the original and that early in the copying process one of the terms was accidentally omitted. It can also be argued that one of the terms could have been intentionally omitted to remove what a scribe might have seen as redundant because of the similarity of the two terms. These ideas frame the intentions of this paper.

The Manuscript Evidence RE: LUKE 24:53

1. *Manuscript evidence for praising and blessing, αινουντες και ευλογουντες.* Robinson’s compiled Byzantine Textform for Luke 24:53 reads: και ησαν δια παντος εν τω ιερω αινουντες και ευλογουντες τον θεον. Αμην.⁵ The MS evidence for αινουντες και ευλογουντες consists of two uncials that date from

3 Samuel Davidson, *The New Testament, Translated from the Critical Text of Von Tischendorf; with an Introduction on the Criticism, Translation, and Interpretation of the Book* (London: Henry S. King & Co., 1875).

4 Cf. also Constantin von Tischendorf, *Novum Testamentum Graece* (ed. Caspar René Gregory and Ezra Abbot; 3 vols.; 8th ed. critica maior; Lipsiae: Giesecke & Devrient [vol. 3: J. C. Hinrichs], 1869–1894), 1:737: “The affinity of this verse with vv. 51–52 urges that ΑΙΝΟΥΝΤΕΣ, not ΕΥΛΟΓΟΥΝΤΕΣ, be received: of course nearly the same witnesses everywhere are in agreement. The improvement ΕΥΛΟΓΟΥΝΤΕΣ was written above ΑΙΝΟΥΝΤΕΣ, from which ΑΙΝΟΥΝΤΕΣ ΚΑΙ ΕΥΛΟΓΟΥΝΤΕΣ arose” (ΑΙΝΟΥΝΤΕΣ ut recipiatur, non ΕΥΛΟΓΟΥΝΤΕΣ, huius versus cum vv. 51–52 necessitudo suadet: quipped ubique iidem fere testes consentiunt. Pro ΑΙΝΟΥΝΤΕΣ correctum i.e. suprascriptum est ΕΥΛΟΓΟΥΝΤΕΣ, inde ortum ΑΙΝΟΥΝΤΕΣ ΚΑΙ ΕΥΛΟΓΟΥΝΤΕΣ). In their evaluation of the evidence for the text, Aland, Black, Metzger and Wikgren gave the single variant of ευλογουντες a [C] reading meaning, “there is considerable degree of doubt whether the text or the apparatus contains the superior reading.” *The Greek New Testament*, ed. Aland, Black, Metzger and Wikgren (New York: American Bible Society, 1966) xi. The same rating is in *A Textual Commentary on the Greek New Testament*, ed. Bruce M. Metzger, 3rd ed. (New York: United Bible Societies, 1971) 190–91. With no addition evidence, the translation committee upgraded their rating to [B] in the 4th rev. ed. of *The Greek New Testament* (1983).

5 Robinson, Maurice A. and William G. Pierpont, *The New Testament in the Original Greek: Byzantine Textform, 2005* (Southborough, MA: Chilton Book Publishing, 2005) 192.





the 4th and 5th centuries (A, and W), and fifteen uncials from the 9th and 10th centuries (F, H, K, M, S, U, V, X, Γ, Δ, Θ, Λ, Π, Ψ, and 063.⁶ C² also shows additional support among the uncials as a correction. Among the cursives the support for this reading is overwhelming. It includes f¹, f¹³, 28, 33, 157, 180, 205, 565, 579, 597, 700, 892, 1006, 1009, 1010, 1071, 1079, 1195, 1216, 1230, 1241, 1242, 1243, 1292, 1342, 1344, 1365, 1424, 1505, 1546, 1646, 2148, 2174, plus the bulk of the Byzantine MSS (Byz). The Lectionaries uniformly support praising and blessing. Among the OL MSS are aur, f, and q which all date from the 6th and 7th centuries and c from the 12th. In addition, the Vulgate no doubt reflects some very early uncial and/or papyri MSS that Jerome used. The Syriac Peshitta from the early 400s supports this reading as does the Harklean from the early 7th century. The 4th or 5th century Armenian and the 6th century Ethiopic versions also read praising and blessing, as does the later Slavonic. The Jerusalem church father Hesychius dating around AD 450 also reads this way. But the earliest evidence for “praising and blessing” is its appearance in the 2nd century Diatessaron of Tatian.⁷ William Peterson writes, “In raw chronological terms, the Diatessaron antedates all MSS of the NT, save that tiny fragment of the Gospel of John known as P⁵².”⁸

2. *Manuscript evidence for blessing, εὐλογούντες.* The evidence for the singular reading of blessing is scanty but notable. Merely five Gk MSS and three versions support this reading. P⁷⁵ dates from early 3rd century; codices κ and B are generally dated in the 4th century; and C* and L hail from the 5th and 8th centuries respectively. Two Syriac versions, the Sinaitic (3rd-4th C.) and Palestinian (6th C.), have this reading, as does the 5th century Georgian translation. Early support is seen in the Sahidic and Bohairic versions of the Coptic, dating from the 3rd century. There is a decided lack of any continuing support for this reading.

3. *Manuscript evidence for praising, αἰνούντες.* Only one Gk MS, D from the 5th century, contains the singular reading of praising. The “Western” type text found in D is also supported by the OL MSS a (4th C.), b, d, and e, each dating from the 5th century. Other OL MSS supporting praising are r from the 7th century and ff and l (9th C.). The Church father Augustine who died around AD 430 witnesses to praising. This reading also seems to have died out and lacks any continued support during the intervening centuries.

Applying Canons of Textual Evidence for Luke 24:53

1. *Which reading best explains the rise of the others?* The primary canon of textual criticism is to find the reading which best explains the origin of the other variants.⁹ Many scholars have been conditioned to explain the praising and blessing of Luke 24:53 as a classic case of conflation. Two early strains of the text were combined to form a third new one. One tradition had “praising.” Another had “blessing.” A later scribe copied both to make it “praising and blessing.” But this explanation has several difficulties. (a) If Luke penned but one of these words, praising, or blessing, there is no feasible reason to explain the rise of the other lone word through the copying process. If Luke wrote ΑΙΝΟΥΝΤΕΣ, where did ΕΥΛΟΓΟΥΝΤΕΣ come from? These words are clearly different. One cannot be confused with the other. Nevertheless, this fallacy is perpetuated in most modern translations.

6 These 17 uncial MSS are also respectively known as 02, 032, 010, 013, 018, 021, 028, 030, 031, 033, 036, 037, 038, 039, 041, 044, and 063. Alfred Plummer, *A Critical and Exegetical Commentary on the Gospel according to St. Luke* in ICC (New York: Charles Scribner's Sons, 1925) 566 cites X, Γ, and Π.

7 Nicholas Perrin, *Thomas and Tatian: The Relationship between the Gospel of Thomas and the Diatessaron*, (Leiden: Brill, 2002), concludes that the Diatessaron was completed in Syriac by AD 175 and that Thomas was dependent on the latter. For versional and MSS evidence of the Diatessaron see Bruce M. Metzger, *The Early Versions of the New Testament: Their Origin, Transmission, and Limitations* (New York: Oxford University Press; reprinted 2001) 10-36.

8 William L. Peterson, “The Diatessaron of Tatian,” in *The Text of the New Testament in Contemporary Research: Essays in the Status Quaestionis; A Volume in Honor of Bruce M. Metzger*, ed. by Bart D. Ehrman and Michael W. Holmes (Grand Rapids: Eerdmans, 1995) 77. The Diatessaron is listed in support of praising and blessing in *The Greek New Testament*, ed. Aland, Black, Metzger and Wikgren (New York: American Bible Society, 1966) 319.

9 Bruce Metzger, *The Text of the New Testament*, 2nd ed, (New York: Oxford, 1968) 207 introduces this canon first, as does B. B. Warfield who says to find the reading which will “account most easily for the origin of the others,” *An Introduction to the Textual Criticism of the New Testament* (New York: Thomas Whittaker, 1889) 83.





By what logic do the translators of the RSV, NRSV, ESV, and the HCSB argue that “blessing” was the original word and that “praising” came later? Likewise, what credible explanations do the translators of the NIV, TNIV, NLT, the Message, the NCB and the CEB give for claiming that “praising” was what Luke wrote and that “blessing” suddenly and strangely took its place later in some other MSS? Even more alarming is the idea that only one Gk MS, Bezae (D), retains the original. Godet has concluded that these two separate readings “mutually condemn one another, and so confirm the received reading, *praising and blessing God*.”¹⁰

(b) If, on the other hand, the autograph had “praising and blessing” there are credible explanations for how the singular words originated in a very small number of Greek MSS. Salmon argued, “It may be held that the fullest form was the original; and that the two simpler took their origin from one transcriber having omitted one of the participles, and a different transcriber having left out the other.”¹¹ This would mean that P⁷⁵, Aleph, B, C*, and L may have used an exemplar that had skipped over ΑΙΝΟΥΝΤΕΣ and instead copied only ΕΥΛΟΓΟΥΝΤΕΣ. It could also mean that after D’s scribe wrote ΑΙΝΟΥΝΤΕΣ his eyes found their place once again after the similar ending ΟΥΝΤΕΣ on the end of ΕΥΛΟΓΟΥΝΤΕΣ. By that action he would have omitted “and blessing” from his production.

2. Consider the external evidence of ms dates, geographical distribution, and MS text types. Fundamental to all textual criticism is to determine the dates of MSS, their geographical distribution, and the MS families into which they fall.¹²

a. *Dates of MSS.* The basic dates of the MS evidence have been given above. To summarize, each tradition has early support. This is because most variants in the text probably occurred in the immediate centuries after the writings appeared. Clearly, however, the reading of “blessing” by itself is shown to be early in P⁷⁵, as well as the early Coptic, both from the 3rd century. Then “praising” by itself can be traced to the 4th century in the OL.

The evidence for “praising and blessing” can be attested as early as the 2nd century in the Diatessaron, and is seen in the 4th or 5th century in Codex W. Michael Holmes attests a well known fact: “A text may be much older than the manuscript that conveys it; therefore, the date of a manuscript, which can be an important point of information in the analysis of a closed tradition, is no longer of as much significance.”¹³ MSS survived for various reasons. Obviously, the autographs perished along with the other most ancient Biblical MSS; only samples of the latter still remain. Each of the three variants of Luke 24:53 probably could be traced back to the early second century, or even before that, if more MSS had not perished. Interesting for this discussion is Lake’s statement that “it is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred books.”¹⁴ This conclusion is based on the fact that so few MSS exist in the great monasteries of Sinai, Patmos, and Jerusalem from earlier than the 10th century. No doubt some reliable MSS perished in the using. Thus, with such variables present, and with each reading demonstrating early support, the age of particular MSS cannot be the sole determining factor for genuineness. Early readings can figure largely in later MSS.

b. *Geographical distribution of MSS.* Apart from translations into other languages, not many individual NT manuscripts signal their precise place of origin. Luke’s Gospel may have originated in Greece

10 F. Godet, *A Commentary on the Gospel of St. Luke*, 3rd ed., trans. E. W. Shalders and M. D. Cusin (New York: Funk and Wagnalls, 1887) 515. He says, “Perhaps the omission in both cases arises from confounding the two-ντες.”

11 Salmon, *Some Thoughts*, 68.

12 Metzger, *Text of the NT*, 209.

13 Michael W. Holmes, “Working with an Open Textual Tradition: Challenges in Theory and Practice” in Klaus Wachtel and Michael W. Holmes, eds., *The Textual History of the Greek New Testament: Changing Views in Contemporary Research* (Atlanta: SBL, 2011), 74. For example, many would argue that the text of MS 33 far antedates its copy date. See also Gerd Mink, “Contamination, Coherence, and Coincidence in Textual Transmission: The Coherence-based Genealogical Method (CBGM) as a Complement and Corrective to Existing Approaches,” in Holmes and Wachtel, 141-216. Mink says, “What we have from the early phases of transmission is not likely to be representative of the text in those times; therefore, we have to rely on later sources to trace older variants,” p. 146.

14 Kirsopp Lake, Robert P. Blake, and Silva New, “The Caesarean Text of the Gospel of Mark,” *HTR*, 21:4 (Oct. 1928) 349.





or Asia Minor where Luke served many years as a missionary with Paul and came into contact with new believers like Theophilus.¹⁵ MSS copied and remaining in the original area of composition and distribution could be compared with the autograph until it expired. Whatever the original text of Luke 24:53 was, probability says that most early MSS would favor it and could be compared for accuracy as long as the autograph remained. Under normal circumstances the autographic text would be expected to spread to the greater number of manuscripts, even when allowance is granted for all sorts of copyists' mistakes. Errant manuscripts of Luke, if they remained in the composition area, could be detected by comparison with the autograph.

Most Greek MS copies of Luke's Gospel in the first century taken together should contain the autographic text. Moving down several centuries as MS copies multiplied, one need only consider just the papyri and the uncials listed above. The presumption is that copies represent their ancestors. The large number of 9th and 10th century uncials, and similarly the early cursives, were copied from even earlier uncials.¹⁶ For some reason the few texts of Luke 24:53 that had either praising or blessing, but not both, were largely left uncopied. Their text of Luke 24:53 may have been seen as errant. A serious indictment of these few MSS is that their text remains so singular and unused.

They reflect being off to the side of the accepted MS tradition. In fact, the entire evidence for "blessing" reflects only one text type, the Alexandrian, and none other.¹⁷

However, the evidence for "praising and blessing" draws support from the Alexandrian MSS, the Western text of the OL, the Caesarean text, and the geographical spread represented in the Latin Vulgate, Syriac, Armenian, and Ethiopic. Certainly, the sheer number of MSS containing "praising and blessing" reflect the common text used throughout the church in most of its geographical locations. This is supported by both OL texts and the Vulgate used continually in the Roman Church. This reading is also seen in the Syrian, Armenian, and Gothic versions. "Blessing" only is favored by some Syrian versions, the Coptic in Egypt, and the Georgian. "Praising" only is narrowly found in some OL but in no other tongues.

c. Determination of MS text types. Comparison of MSS has shown differences that allow MSS to be grouped into text types or families. Such terms as Alexandrian, Western, Syrian, and Caesarean have been used to describe these groupings. The blessing variant is exclusively Alexandrian.¹⁸ The praising variant is supported solely by Western texts. The huge majority of MSS are classified in the general category of "Byzantine."¹⁹ These MSS entirely support "praising and blessing." But additional text types such as the Alexandrian MSS 33, 579, 1241 lend support, along with part of the Western OL, and the Caesarean Θ , f^1 , f^{13} , 28, 565, 700. Aland and Aland place Θ , and 33 in their Category II, and some of the support for praising and blessing, such as A, W, Δ , f^1 , f^{13} , 28, 180, 205, 565, 579, and 1241 in Category III.²⁰

Metzger cautions, "Witnesses are to be weighed rather than counted."²¹ But certainly number cannot be entirely eliminated when considering something's weight. Hardly any MS demonstrates direct copying from another known MS. A corollary is that the multitudes of Byzantine MSS were copied from earlier exemplars, meaning early uncials and papyri of the time that contained both "praising and blessing."

15 Codex Bezae inserts a unique "we" reading in Acts 11:27 which would make Luke an early Antiochan convert prior to Paul's arrival there with Barnabas.

16 Even Westcott and Hort theorize that "a majority of extant documents is more likely to represent a majority of ancestral documents at each stage of transmission than vice versa." *The New Testament in the Original Greek*, I, 45.

17 Metzger lists P⁷⁵, Aleph, B, C, L, and the Sahidic and Bohairic as Alexandrian, *Text of the NT*, 216.

18 Metzger, *Text of the NT*, 216.

19 Other terms used to describe this large group are Syrian which references the key city of Antioch, Traditional text, and Majority Text, which references their numeric superiority over other groupings.

20 Kurt and Barbara Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism*, trans. Erroll F. Rhodes (Grand Rapids: Eerdmans, 1987) 105-106. Type II lists "Manuscripts of a special quality . . . of importance for establishing the original text," and Type III has "Manuscripts of a distinctive character with an independent text, usually important for establishing the original text, but particularly important for the history of the text."

21 Metzger, *Text of the NT*, 209.





The hypothesis that the Byzantine text was the result of a conflated revision of the NT text in the 4th century remains an unproven allegation.²² One of the pillars of that hypothesis, that Luke 24:53 is a conflation, is shown to be unfounded in this paper.

3. *Consider what mistakes scribes were prone to make.* One of the commonly repeated canons of transcriptional evidence for MS copying is that the shorter reading is to be preferred.

It is alleged that scribes tend to add to their text rather than omit material.²³ Metzger asserts that most scribes who faced two divergent readings in a passage would copy both into their production creating a conflation.²⁴ Robinson, however, counters that “a careful examination of scribal practices will reveal how *rarely* conflation or other supposed ‘scribal tendencies’ actually occurred, and how limited was the propagation of such among the MSS.”²⁵

But in the initial copying of an autograph the allegation of adding to a text should be seen as patently false. Though dittography, the repetition of the same word or phrase can occur, the most common error of the eye is to omit what is before it.²⁶ Secretaries who have typed my writing projects have frequently omitted single words, short phrases, and even whole sentences and sections. That is what the eye does when it returns to the page. I cannot produce a single example where a typist inserted material into any of my manuscripts.

With this most common process of omission being the case, I would argue that copies of *original* biblical manuscripts tend to become shorter. Once this occurs, a later MS with such omissions might be checked with a good copy that had not suffered from homoeoteleuton or other eye skipping functions. At that point the one checking the MS might make a correction from the good MS into the margin of his MS.

A still later scribe using the “corrected” MS might then place that correction into his new production. In that case, the supposed “conflation” would actually be a restoration of the original text!

a. *How did codex D come to have only “praising,” ΑΙΝΟΥΝΤΕΣ?* Codex D is the only extant Gk MS to read only “praising” at Luke 24:53. Is this the original text as many of the translations listed above contend? The evidence points to omission, a common mistake copyists make.²⁷ The scribe of Codex D omitted “and blessing.” His eye could have simply skipped from the ΟΥΝΤΕΣ on the end of praising to the ΟΥΝΤΕΣ on the end of blessing. Reinforcing this conclusion is the fact that D is faulty with this same type of omission three times in just his final four lines of text.

In Luke 24:51 D omits the concluding words, καὶ ἀνεφέρετο εἰς τὸν οὐρανόν. His eye skipped five words from the NKAI that precedes to the NKAI that follows. Perhaps unintentionally he omitted the

22 Klaus Wachtel states in part that the difficult Byzantine readings in James 2:4, 2:18, and 1 John 5:13 “make it improbable that the Byzantine text could have had its origin in a regular recension of the 4th century” (*Der byzantinische Text der katholischen Briefe: eine Untersuchung zur Entstehung der Koine des Neuen Testaments* [Arbeiten zur neutestamentlichen Textforschung 24; Berlin: de Gruyter, 1995], 199).

23 But as J. Harold Greenlee points out, this applies largely to scribes who make an “*intentional change*” in the text. Italics in original. *Introduction to New Testament Textual Criticism* Rev. ed. (Peabody, MA: Hendrickson, 1995) 112.

24 Metzger, *Text of the NT*, 200.

25 Maurice A. Robinson, “The Case for Byzantine Priority” in Robinson and Pierpont, *New Testament, Byzantine Textform*, 539.

26 This is confirmed by recent studies of scribal habits: James R. Royse, *Scribal Habits in Early Greek New Testament Papyri* (Leiden: Brill, 2008); Peter M. Head, “Some Observations on Early Papyri of the Synoptic Gospels, Especially Concerning the ‘Scribal Habits,’” *Biblica* 71 (1990): 240–47; idem, “The Habits of New Testament Copyists: Singular Readings in the Early Fragmentary Papyri of John,” *Biblica* 85 (2004): 399–408. Cf. also Kyoung Shick Min, *Die früheste Überlieferung des Matthäusevangeliums* (Arbeiten zur neutestamentlichen Textforschung 34; Berlin: De Gruyter, 2005), who says on p. 97, “What is customary with the old papyri, and also remarkable, is that omissions are more than twice as likely to be found than additions” (“Bei Omissionen ist auch bemerkenswert, dass sie mehr als doppelt so häufig vorkommen wie Additionen, was bei alten Papyri üblich ist.”).

27 Greenlee, says the Western text sometimes “substitutes synonyms for single words, such as . . . αἰνοῦντες instead of εὐλογοῦντες in Luke 24:53,” *Introduction*, 83, but this is a complete conjecture in this case.





ascension of Christ, which Luke reaffirms in the opening of the Book of Acts when he says Jesus “was taken up,” ἀνελήμφθη (Acts 1:2).²⁸

Codex D then almost immediately omits two more words in the same line (4th line from the end). In the sentence that begins καὶ αὐτοὶ προσκυνήσαντες αὐτὸν ὑπέστρεψαν (24:52), the two underlined words are missing from D.

No one seriously challenges the admissibility of those two words. But the omission still reads properly in the scribe’s mind. Αὐτοὶ fits as the subject of ὑπέστρεψαν. The scribe could have omitted the participle and its object intentionally just to shorten the text, or more likely it was another case of homoeoteleuton.²⁹

In the latter case, the tired scribe skipped from ΑΥΤΟΙ to ΑΥΤΟΝ. Without the ascension Christ’s apostles would be without a reason for their obvious joy. Jesus would simply have disappeared similar to v. 31. This scribe, or another before him, may have hastened as he saw the end of his work approaching, but with dire results.

Another factor that lends weight to the same conclusion, that Codex D should not be trusted in Luke 24:53, is D’s penchant for aberrations. Codex D is characterized by

innumerable additions, transpositions, omissions, etc. Undoubtedly the achievement of the original editor was significant, but only as a reviser who altered radically the text of his early exemplar in numerous passages. These alterations can make no claim to consideration as original.³⁰

Metzger notes, “No known manuscript has so many and such remarkable variations from what is usually taken to be the normal New Testament text. Codex Bezae’s special characteristic is the free addition (and occasional omission) of words, sentences, and even incidents.”³¹ Codex D has been noted to be particularly flawed in Luke 22-24.

Some major omissions of Codex D and several OL MSS in Luke 24 are found in vv. 3, 6, 12, 36, 40, 51, and 52.³² Kenyon remarks “all mention of the Ascension disappears by the omission of the words καὶ ἀνεφέρετο εἰς τὸν οὐρανὸν in 51.”³³ Robert Stein concludes that “the manuscript evidence in favor of including them is so great that they must be accepted as part of the text.”³⁴ Moreover, D is deficient in all those verses leading up to Luke 24:53.

b. How did P^{75} , N , B, C*, and L come to have only “blessing,” ΕΥΛΟΓΟΥΝΤΕΣ?

If ΑΙΝΟΥΝΤΕΣ ΚΑΙ ΕΥΛΟΓΟΥΝΤΕΣ appeared in the autograph, the eye skipping to ΕΥΛΟΓΟΥΝΤΕΣ could explain the omission of the first verb, leaving only “blessing.” Or, an early scribe may have sought to eliminate what he considered a redundancy in the two verbs.

28 Theodor Zahn explains that “it is shown by Acts i.2 that the author is conscious of having already given an account of the ἀνάληψις in Luke xxiv.51.” *Introduction to the New Testament*, trans. John Trout, et. al. (3 vols.; 2nd rev. ed.; New York: Charles Scribner’s, 1917) 3:87.

29 Henry Alford, *The Greek Testament*, Rev. Everett F. Harrison (Chicago: Moody, 1968) 675-76 believes D erred by homoeoteleuton once in each of the final three verses of Luke as shown herein. H. A. W. Meyer, *Critical and Exegetical Handbook to the Gospels of Mark and Luke* (trans. R. E. Wallis; 6th ed.; New York: Funk and Wagnalls, 1884) 573, concurs.

30 Kurt and Barbara Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism*, trans. Erroll F. Rhodes (Grand Rapids: Eerdmans, 1987) 51.

31 Metzger, *Text of the NT*, 50.

32 These turn out to be seven of the nine “Non-Western Interpolations” Westcott and Hort identified. The other two are Mt 27:49 and Lk 22:19b-20.

33 Frederic G. Kenyon, *The Text of the Greek Bible*, New ed. (London: Gerald Duckworth & Co., 1949) 92.

34 Robert H. Stein, *Luke. The New American Commentary. vol. 24* (Nashville: Broadman and Holman, 1992) 616.





However, if ΕΥΛΟΓΟΥΝΤΕΣ was what Luke penned in Luke 24:53 it is a conjecture as to how codex D morphed the word into ΑΙΝΟΥΝΤΕΣ. The easiest way to explain the appearance of ΑΙΝΟΥΝΤΕΣ is that it appeared in the original text along with ΕΥΛΟΓΟΥΝΤΕΣ. Similarly, if just “praising” was what Luke wrote, it would be a conjecture as to how “blessing” appeared in these few MSS. As Godet observed, the two singular readings “mutually condemn one another.”³⁵

In any event, this reading did not spread far or last long. The singular usage of ΕΥΛΟΓΟΥΝΤΕΣ appears in 8th century codex L, and then disappears entirely.

However, at nearly that same time, the dual reading of praising and blessing was being copied in numerous other extant uncials and in all extant minuscules that followed before the invention of the printing press.

4. *Which reading is most characteristic of the author’s vocabulary, style and theme?* The Gospel of Luke is a masterpiece of organization. Luke begins his account with a temple scene where Zachariah, the Jewish priest, is unable to bless the waiting crowd after his temple service (Lk 1:5-23).³⁶ It ends with Christ, our great high priest granting his waiting disciples a farewell blessing as he ascends back to heaven (Lk 24:50-51).

a. *The temple is an important theme in Luke.* Luke begins and ends his book with a temple scene. Initially, Zechariah ministers in the temple (Lk 1:9). Mary and Joseph presented the infant Jesus to the Lord at the temple in Jerusalem (2:22). They were met there by Simeon who “came by the Spirit into the temple” (2:27), and took Jesus “up in his arms and blessed God” (2:28). Aged Anna was constantly in the temple, saw the baby Jesus, then “gave thanks to the Lord, and spoke of Him to all who looked for redemption in Jerusalem” (2:37-38). At the age of twelve Jesus visited Jerusalem, lingered behind, and was found “in the temple, sitting in the midst of the teachers” (2:46). As Christ begins his ministry Satan tempts him on the pinnacle of the temple (4:9).

Luke records Jesus’ unique parable of the Pharisee and publican praying in the temple (18:10). Luke tells of Jesus teaching in the temple during his passion week (19:47; 20:1; 21:37-38), and how he spoke of the destruction of the temple in his Olivet Discourse (21:5-6). At his arrest Jesus said, “I was daily with you in the temple” (22:53). During Jesus’ agony on the cross “the veil in the temple was torn in two” (23:45). As Luke ends his account he says that Christ’s disciples were “continually in the temple praising and blessing God” (24:53).

b. *Praising and blessing are characteristic of Luke.* The Greek verbs for praising, αἰνεῶ, and blessing, εὐλογεῶ, are characteristic of Luke’s usage. In fact, αἰνεῶ is found just nine times in the NT, and seven of those usages are by Luke, four in the Gospel and three in Acts. Εὐλογεῶ is found 45 times in the NT.

Luke uses the word 15 times; once in Acts, and 14 times in the Gospel; twice as many times as the other three Gospels combined (Matt = 6x; Mark = 6x; John = 1x). Gabriel, Elizabeth, Zechariah, Simeon, Jesus, and the disciples all utter εὐλογεῶ. The various personal objects of this verb are Mary, Jesus and Jesus’ parents, but primarily God.³⁷

Αἰνεῶ, on the other hand, is found in Luke exclusively with God as its object. The angelic host (2:13), the shepherds (2:20), and a host of disciples on Palm Sunday (19:37), all praised God. The unique construction in most of these uses of αἰνεῶ is also to join it with another verb. The shepherds were “glorifying **and** praising God” (2:20), and “the disciples began to rejoice **and** praise God” (19:37).

³⁵ Godet, *Commentary on St. Luke*, 515.

³⁶ Zechariah “is expected to pronounce the Aaronic blessing on the people” according to William Hendriksen, *Exposition of the Gospel according to Luke*, New Testament Commentary (Grand Rapids: Baker, 1978) 68-69. Alfred Edersheim, *The Temple: Its Ministry and Services* (Grand Rapids: Eerdmans, reprinted, 1972) 170 notes, “one of their number [priests] probably the incensing priest, repeated in audible voice, followed by the others, the blessing in Num. vi. 24-26.” In the LXX of Num 6:24, “The LORD bless thee,” uses εὐλογεῶ.

³⁷ Zacharias (1:64), Simeon (2:28), and the disciples (24:53) all bless God.





I would submit that the joyous disciples were in the temple “praising **and** blessing God” (24:53). This is characteristic of Luke’s usage of αἰνεῶ in his Gospel.

c. *Ancient temple worship regularly connects praising with other verbs.* When David initiated divisions of priests and Levites, their duty was “to praise and serve” (2 Chr 8:14). At Solomon’s temple dedication the singers were engaged “in praising and thanking the LORD” (2 Chr 5:13). Later Hezekiah reappointed priests and Levites “to serve, to give thanks, and to praise” (2 Chr 31:2). Later in Nehemiah’s time the Levites were still assigned “to praise and give thanks” (Neh 12:24; LXX 24:12). These passages demonstrate a longstanding and continued liturgical coupling of worship terms with αἰνεῶ in the OT LXX. Luke’s familiarity with the LXX may have informed his thinking.

d. *Why would Luke join praising and blessing in the disciples’ temple worship?* Even though Luke was probably not Jewish, he was familiar with Jewish worship at their temple in Jerusalem. He was also a longtime associate of Paul who visited the temple numerous times and was even arrested there. Luke would have been familiar with the Psalms of Ascent that were sung on the way to the temple (Psalms 120–134). In many ways Psalm 134 is most noteworthy for its call to praise and bless God.³⁸ In the LXX, vv. 1–3 command the worshippers around the temple to praise the Lord three times using the verb αἰνεῶ, and it concludes in vv. 19–21 urging the worshippers to bless the Lord, using the imperative of the verb εὐλογεῶ four times. The final word of this LXX Psalm is Jerusalem. It would not be unlike Luke to combine the praising and blessing commanded of these Jewish worshippers with the same actions exhibited by the earliest Hebrew Christian believers who met in the precincts of God’s House of Prayer, the Jerusalem temple.

Conclusion

The “praising and blessing” in Luke 24:53 should be viewed as the original reading. It has the early witness of the Diatessaron, the most breadth among the uncials, and is the only reading with practically any continued support past the 5th century. This reading best explains the rise of the other variants, has the greatest geographical support, and is the sole reading to be seen in the Alexandrian, Western, Caesarean, and Byzantine text types. In addition, it best explains the most common error scribes make when copying MSS. They tend to omit material. Lastly, the reading of “praising and blessing” is characteristic of the design and purpose shown in Luke’s Gospel with his emphasis on worship in the temple and the prominent use of both praising and blessing as Jewish worshippers gathered at the temple.

³⁸ Psalm 134 in the LXX is Psalm 135 in our English translations.





THE ADULTERESS AND HER ACCUSERS

An Examination of the Internal Arguments relating to the *Pericope Adulterae*¹

by Andrew Wilson

Introduction

Most textual critics believe that the *Pericope Adulterae*, the famous story concerning Christ and the woman taken in adultery (John 7:53–8:11), was not originally part of John's Gospel. Bruce Metzger says "the evidence for the non-Johannine origin of the pericope of the adulteress is overwhelming."² David Parker calls the passage "demonstrably spurious."³ William Petersen says the reasons for regarding the story as a later insertion are "massive, convincing, and obvious."⁴

This verdict is not surprising, considering the external evidence ranged against the *Pericope Adulterae* (henceforth PA)⁵ and the high regard that most textual critics have for the 'early and best witnesses' that omit the incident. Professor Robinson has made the manuscript evidence for the PA one of his research interests, and for obvious reasons: not only because the PA gives us a glimpse into the various text-streams that make up the Byzantine text, but also because his opinion about the originality of the PA differs from that of other textual critics.

However, internal evidence is also frequently claimed to be against the PA and it is with the internal evidence that we shall primarily concern ourselves here. The two commonly-voiced internal arguments against the incident are, firstly, that its style and vocabulary differ markedly from the rest of John's Gospel and secondly, that it interrupts the flow and themes of John's Gospel. Samuel Tregelles complained of "that kind of traditional inertness of mind" of those who think that the PA is original to John's Gospel.⁶ Having no desire to be numbered amongst those suffering from inertness of mind, we shall here examine the internal considerations claimed to provide additional evidence against the genuineness of the PA.

The Style and Vocabulary of the PA

One of the earlier critics to make this argument, Samuel Davidson, wrote, "The diction and manner of the paragraph present few of the characteristics of John. They are strikingly foreign to him."⁷ Metzger asserts that "the style and vocabulary of the pericope differ noticeably from the rest of the Fourth Gospel (see any critical

1 This article is an updated and expanded version of the author's 2004 online essay of the same name.

2 B. M. Metzger, *A Textual Commentary on the Greek New Testament* (2nd Ed., London and New York: United Bible Societies, 1994), 187.

3 D. C. Parker, *The Living Text of the Gospels* (Cambridge: Cambridge University Press, 1997), 95.

4 W. L. Petersen, "Οὐδε ἐγὼ σε [κατὰ]κρίνω: John 8:11, the Protevangelium Iacobi, and the History of the Pericope Adulterae", in *Sayings of Jesus: Canonical and Non-canonical: Essays in Honour of Tjitze Baarda*. Edited by W.L. Petersen, J. S. Vos and H. J. de Jonge, *Novum Testamentum Supplements* 89 (Leiden: Brill, 1997), 192.

5 The early MSS of John's Gospel that omit the passage include Papyrus Bodmer II (P66), Papyrus Bodmer XV (P75), Codex Sinaiticus (01), Codex Vaticanus (B), Codex Purpureus (N), Codex Borgianus (T) and Codex Washingtonianus (W), and, in all probability, Codices Alexandrinus (A) and Ephraemi (C), all dating from the 3rd to the 6th centuries. In addition, the breadth of versional support is against the passage, most manuscripts of the Syriac, Coptic, Gothic, Armenian, Georgian and Slavonic Versions omitting the incident. Further, early Greek Church Fathers who provide commentaries on John omit it, including Origen and Chrysostom. The placement of the account in various positions, including at the end of John's Gospel or at the end of Luke chapter 21 in some manuscripts, also casts some doubt upon it, as does the marking of the passage as dubious with an asterisk or obelus in many manuscripts which contain it. Finally, the disturbed state of the text in the manuscripts that contain it only increases disquiet about it; von Soden classifies seven different strains of the text amongst the manuscripts.

6 S. P. Tregelles, *An Account of the Printed Text of the Greek New Testament, with Remarks on its Revision upon Critical Principles* (London: Samuel Bagster and Sons, 1854), 240.

7 S. Davidson, *Introduction to the NT* (London: Samuel Bagster and Sons, 1848), 1:359.



commentary).⁸ Nowadays, this is considered so incontrovertible that many commentators follow Metzger not only in repeating the claim, but also in providing little argumentation. However, as we shall see, there is a distinct benefit to be gained from examining the actual arguments from first principles. We shall look at ten criticisms leveled against the PA.

1. One of the most common arguments against the style and vocabulary of the PA is what we shall call the raw numbers argument. Thus, one is struck in some commentaries by the cumulative force of repeated comments like “John never uses this word,” “a word not used by John,” or “not Johannine” and the like.⁹ There are up to sixteen words in the PA (depending on preferred variants,¹⁰ some of which are mutually exclusive) that are nowhere else found in John’s Gospel: ἔλαια (Olives, 8:1), ὀρθρος (early, 8:2), γραμματεὺς (scribes, 8:3), μοιχεία (adultery, 8:3), μοιχεύω (commit adultery, 8:4), αὐτοφῶρος or ἐπαυτοφῶρος (in the very act, 8:4, found nowhere else in the NT), κυπτω (stooped, 8:6), καταγράφω (wrote, 8:6), ἐπιμένω (continued, 8:7), ἀνέκυπτο (lifted himself up, 8:7, 10), ἀναμαρτητός (without sin, 8:7, nowhere else found in the NT), κατακυπτω (stooped down, 8:8), συνειδήσις (conscience, 8:9), πρεσβύτερος (oldest, 8:9), κατέλειπω (left, 8:9) and κατακρίνω (condemned, 8:10 & 11).

Such a list, it is argued, shows that the passage exhibits too many peculiar and non-Johannine words for it to have been written by the NT writer famous for his down-sized vocabulary. We do not know who it was that added the incident to John’s Gospel, but whoever it was, his speech — like Peter’s — betrayed him.

However, closer inspection shows that this statistical argument has two problems. Firstly, John’s Gospel has a number of other passages that similarly contain a high number of unique words. Four other comparable passages, over which there is no textual uncertainty, also provide many words that are nowhere else found in John’s Gospel.¹¹

For example, John 4:4-16, the incident concerning Christ and the Samaritan woman — a passage of equal length (13 verses) — contains eighteen words nowhere else found in John’s Gospel, two of which are found nowhere else in the NT: Σαμαρεία (Samaria, 4:4, 5, 7), διέρχομαι (go through, 4:4, 15), Συχαρ (Sychar, 4:5), πλησίον (near, 4:5), χωρίον (field, 4:5), Ιακώβ (Jacob, 4:5, 6, 12), ὁδοιπορία (journey, 4:6), πηγή (spring, 4:6, 14), τροφή (food, 4:8), Σαμαριτις (Samaritan woman, 4:9), συγχράομαι (associate with, 4:9 — only time in NT), δῶρεα (gift, 4:10), ἀντλήμα (bucket, 4:11), φρέαρ (well, 4:11, 12), βάθος (deep, 4:11), θρέμμα (cattle, 4:12 — only time in NT), αλλομαι (springing up, 4:14), ἐνθαδὲ (here, 4:15, 16).

John 6:3-14, the feeding of the five thousand, contains thirteen words nowhere else found in John’s Gospel, three of which are found nowhere else in the NT: ἀνέρχομαι (go up, 6:3), βραχύς (little, 6:7), παιδαριον (little boy, 6:9 — only time in NT), κριθινός (barley, 6:9, 13 — only time in NT), χορτός (grass, 6:10), ἀριθμός (number, 6:10), πεντακισχίλιοι (five thousand, 6:10), διαδίδωμι (distributed, 6:11), ἐμπλήνιμι (filled, 6:12), περισσεύω (left over, 6:12, 13), κλάσμα (broken pieces, 6:12, 13), κοφίνος (baskets, 6:13), βιβρώσκω (eat, 6:13 — only found here in NT).

Likewise, seven or eight Greek words can be found in the first 12 verses of John 9, in the incident of the healing of the man blind from birth, that are nowhere else found in John’s Gospel: παραγῶ (passed by, 9:1, although the word is found in the Byzantine text of John 8:59), γενετή (birth, 9:1 — only here in NT), πτυῶ (to spit, 9:6), πτυσμα (spittle, 9:6 — only here in the NT), ἐπιχρίω (anointed, 9:6 and 11 — only here in NT), Σιλωάμ (Siloam, 9:7 and 11), προσαιτης (beggar, 9:8 — a different word from the usual, nowhere else found in John), γείτων (neighbours, 9:v8 — again, a different word from the usual Greek word, nowhere else found in John).

⁸ Metzger, *Textual Commentary*, 188.

⁹ See, for example, J. H. Bernard, *A Critical and Exegetical Commentary on the Gospel according to St. John*, ICC (Edinburgh: T&T Clark, 1928), 2:715-722.

¹⁰ Thus, Bernard says about verse 4, “the phrase with ἐκπεπραῖν is Lucan; cf. Lk 10²⁵” (Bernard, *John*, 718). However, ἐκπεπραῖν is a textual variant only found in two MSS (D and 1071). The majority of manuscripts use πεπραῖν, which is also used in exactly the same sense in John 6:6. It would appear that Bezae is simply harmonizing to Luke here. There are a number of other words and expressions which have been criticised as unJohannine over the years, but which are doubtful variants. Some of these are Textus Receptus readings with less likelihood on internal grounds of being original. Included among these are the synoptic harmonization λιθοβολεῖσθαι (8:5) and πλὴν (8:10).

¹¹ J. D. Punch points out a number of other examples. Thus John 1:14-27 contains 11 words nowhere else used in John and John 2:5-17 contains 19 unique words. J. D. Punch, *The Pericope Adulterae, Theories of Insertion and Omission*, Ph. D. Thesis, Radboud University Nijmegen, 2010.



If we include the words that are repeated in the many retellings of the story later in the chapter, the total goes up to nine or ten words: γονεὺς (parents, 9:2, 3, 18, 20, 22, 23) and πηλός (clay, 9:6, 11, 14, 15).

Lastly, a study of John 21:1-12 yields sixteen words nowhere else found in John's Gospel, three of which are unique in the NT: Ζεβεδαῖος (Zebedee, 21:2), ἀλιεύω (to fish, 21:3 — only time verb form is found in NT), πρωῒα (early, 21:4), αἰγιαλός (shore, 21:4), προσφάγιον (food, 21:5 — only time in NT), δικτυόν (net, 21:6, 8, 11), ἰσχυώ (able, 21:6), ἰχθύς (fish, 21:6, 8, 11), ἐπενδυτής (outer coat, 21:7 — only time in NT), γυμνός (naked, 21:7), μακρὰν (far, 21:8), πήχυς (cubit, 21:8), σὺρω (drag, 21:8), ἀποβαίνω (get out, 21:9), τολμάω (dare, 21:12), ἐξετάζω (examine, 21:12).

The obvious common factor between these passages and the PA is that they are “story” narratives in John's Gospel. Due to the fact that dramatic events in John's Gospel (as opposed to dialogues and discourses) are so few in number and so varied in content, it is easy to find at least half a dozen unique words in them. Thus, in passages of similar length, we find as much so-called “non-Johannine” vocabulary as we do in the PA.

A second problem with the raw numbers argument is that there are 373 words that John uses only once in his Gospel. Of these 373 words, 75 are *hapax legomena*, words only found once in the entire NT.¹²

Thus, the mention of a word like ἐπιμένω (John 8:7) in the PA is hardly ‘smoking gun’ evidence against its authenticity. Some people might argue that ἐπιμένω is an unusual word for John's Gospel, but then, so are the other 372 words only found once in John's Gospel. If we are to make a fuss about ἐπιμένω, why not question the entire story of the woman at the well in John 4, which has even more ‘unusual’ words? Why not place John 3:16 in brackets as suspicious because it contains the conjunction ὥστε (only found once in John, despite being found 83 times elsewhere in the NT)?

Once put in proper perspective, then, the fact that there are up to sixteen words nowhere else found in John's Gospel or two *hapax legomena* in the PA is hardly the impressive evidence against its authenticity that it is repeatedly claimed to be. Instead, curiously enough, it turns out that the PA contains no different numbers of (a) singularly Johannine words and (b) *hapax legomena* to other similar Johannine narratives.

2. A second commonly repeated argument is that the vocabulary of the PA is Synoptic. F. F. Bruce writes, “In style it has closer affinities with the Synoptic Gospels than with John.”¹³ Others also draw attention to vocabulary characteristic of Luke or the Synoptic Gospels.¹⁴ Thus, much is made of the use of words like ὀρθρός (verse 2), nowhere else found in John, who “has πρωῒ instead.”¹⁵

However, to repeat, there are 373 words only used once in John's Gospel, of which, 75 are *hapax legomena*. By simple arithmetic, this leaves 298 words only found once in John but also found somewhere else in the NT. In fact, of these 298 words, at least 200 are found in Luke or Acts. Some, like ὀρθρός, are only found elsewhere in the NT in Luke's gospel or Acts (ὀρθρός is found only here in John 8:2, Luke 24:1 and Acts 5:21).

However, there are other words which (a) are only found once in John and (b) are only found elsewhere in the NT in Luke or Acts. This hardly makes them unJohannine words, let alone Lukan. For example:

- ἀλλομαί (to leap, spring, only found in John 4:14, Acts 3:8, 14:10)
- βραχιῶν (arm, only found in John 12:38, Luke 1:51, Acts 13:17)
- γείτων (neighbour, only found in John 9:8, Luke 14:12, 15:6, 15:9)
- διαδιδωμί (to give out, only found in John 6:11, Luke 11:22, 18:22, Acts 4:35)
- ἐξηγεομαι (to relate, explain, only found in John 1:18, Luke 24:35, Acts 10:8, 15:12, 15:13, 21:19)

This objection has a further problem, for even if we were to grant its contention (that certain words in the PA are borrowed from Luke's gospel) this would still not warrant its conclusion (that the PA is not originally part of John's gospel). The reason for this is that there are a number of other passages in John's gospel which appear to have clear verbal links and parallels with the Synoptic gospels.

¹² The modern use of computers enables this sort of analysis.

¹³ F. F. Bruce, *The Gospel and Epistles of John* (Grand Rapids, MI: Eerdmans, 1983), 413.

¹⁴ Bernard, *John*, 717-8; R. E. Brown, *The Gospel According to John* (AB 29; New York: Doubleday, 1966), 336; C. K. Barrett, *The Gospel According to St. John* (2nd Ed., London: SPCK, 1978) 590; D. A. Carson, *The Gospel According to John* (Grand Rapids, MI: Eerdmans, 1991) 334; Parker, *The Living Text of the Gospels*, 101.

¹⁵ Bernard, *John*, 717.





These passages appear to show John either subconsciously or deliberately sharing or borrowing material from the Synoptics:

- John 4:44-45: In John 4:44 we read that “Jesus himself testified that a prophet has no honour in his own country.” John has lifted the verse directly from the synoptic Gospels (Mt. 13:57, Mk. 6:4, Lk. 4:24). Even the wording is synoptic, in particular the word πατρις (“country,” used six times in the parallel passages in the synoptic gospels, but only here in John), instead of John’s more usual words τοπος (16 times) or γη (Jn 3:23) or χωρα (Jn. 11:54, 55). In 4:45, we have the expression “the Galileans received him,” but here John again uses δεχομαι (“received,” common in the synoptic gospels, where it is used 21 times), instead of John’s normal word for “receiving,” λαμβανω (41 mentions in John). Only once (here in 4:45) does John use the synoptic word δεχομαι. The word “Galilean” is also used 10 times in the Synoptics and Acts, but nowhere else in John.
- John 12:1-8 and 11:2: There are ten words in the account of Jesus being anointed by Mary of Bethany (John 12:1-8 and 11:2) nowhere else used in John’s Gospel, and of these, five are also found in Mark’s account of the incident (Mark 14:3-9), and all but one are found in the synoptic gospels: ναρδος (John 12:3 and Mk. 14:3), πιστικός (John 12:3 and Mk. 14:3), πολυτιμος (John 12:3, Mt. 13:46 and 1 Peter 1:7), οσμη (John 12:3 and Paul’s letters), πιπρασκω (John 12:5, Matthew 4 times, Acts 3 times, Romans once), τριακοσιοι (John 12:5 and Mark 14:5), ενταφιασμος (John 12:7 and Mk 14:8), αλειφω (John 11:2, 12:3 synoptic gospels and James 5:14), μυρον (John 11:2, 12:3, 5, Mk. 14:3, 4, 5, synoptic gospels and Rev. 18:13) and θριξ (Jn. 11:2, 12:3, synoptic gospels, elsewhere).

This suggests that John is heavily reliant upon Mark’s gospel. More confirmation is found in the expressions, “why was this ointment not sold for three hundred denarii and given to the poor?” (Jn 12:5, Mk. 14:5), “let her alone” (Jn 12:7, Mk 14:6) and “the poor you have always with you, but me you do not always have” (Jn. 12:8, Mt. 26:11, Mk. 14:7). John’s account has such close parallels, factual and verbal, with the synoptic gospels (particularly Mark), that it seems futile to deny the charge.

- John 13:20: Jesus says “Truly, truly, I say to you, He who receives (λαμβάνων) whoever I send, receives me, and he who receives me, receives Him who sent me.” This verse has few logical connections with the context (Judas’ betrayal); the best connections with this verse are, in fact, the synoptic gospels (Mt. 10:40, Mk. 9:37, Lk. 9:48). John appears to be again inserting synoptic material into his gospel.

Thus, the fact that John’s Gospel occasionally borrows language from the Synoptic Gospels means that any verbal similarities between the PA and the Synoptic gospels cease to be particularly remarkable. The similar wording perhaps suggests that some verses in the PA were influenced by the Synoptic gospels, but this hardly proves that the PA is not original to John’s Gospel, for there is no reason to suppose that John himself was not familiar with the Synoptic Gospels.

3. The PA uses an expression in 7:53 and 8:1 which some commentators on John’s Gospel (eg. Alford,¹⁶ Bernard,¹⁷ Leon Morris¹⁸) have criticized as unJohannine — the use of the verb πορευομαι (to go) followed by the preposition εις (to). Nowhere else does John use this combination of words. Instead, we are told, John normally uses the preposition προς (also meaning “to”) after πορευομαι. On the other hand, other Gospel writers like Luke prefer the expression πορευομαι εις. The use of this expression thus shows that John was not the author of the PA, but rather a scribe who was more used to the style of Luke’s gospel.

However, it would appear inaccurate to argue that πορευομαι προς is a peculiarly Johannine stylistic choice. The reasons for the use of πορευομαι with different prepositions instead appear to be based on a rule of syntax seen in all of the gospels: when someone is going to some *place*, πορευομαι is used with εις, but when someone is going to a *person*, πορευομαι is used with προς. Thus, in Matthew, πορευομαι εις is used in 2:20, 17:27, 21:2, 28:11 and 28:16 — because people are going to some place. However, πορευομαι προς is used in 10:6, 25:9 and 26:14 because people are going to other people. Similarly, in Luke πορευομαι εις is used 17 times when people are going to places, but πορευομαι προς is used in Luke 11:5, 15:18 and 16:30 when someone is going to a person. Πορευομαι is used 16 times in John’s Gospel, and in only four places is the preposition προς used (so it is hardly true to say that προς is the “standard” Johannine style when using πορευομαι), but in every case, the word προς is used when someone is going to a person (“the Father,” John 14:12, 14:28, 16:28 and 20:17). The only uses of πορευομαι εις in John are found in the PA, and are both cases where someone is going to a place.

¹⁶ H. Alford, *The Greek Testament, Vol. 1, The Four Gospels* (London: Rivington, 1849), 564.

¹⁷ Bernard, *John*, 717.

¹⁸ L. Morris, *The Gospel according to John* (NICNT; Grand Rapids, MI: Eerdmans, 1971), 883, n.3.





The same is true of the verb *ερχομαι* (to come) and the two prepositions. *Ερχομαι* is used 34 times in John with *προς*, and on every occasion without exception (including the reference in John 8:2), the expression is used when someone comes to another person. On the other hand, *ερχομαι* is used with *εις* 28 times in John and on every occasion a person is coming to a place or a thing.

The rule also holds true for the book of Acts: *προς* is used when people are involved, *εις* is used when places or things are involved. Take Acts 20:6 as an example: *ημεις ... ηλθομεν προς αυτους εις την Τρωαδα*.

Thus, the PA simply gets its Koine syntax right. This would appear to argue for (rather than against) the idea that the author of the PA lived and wrote in the same era and cultural milieu as the other gospel writers. Indeed, he seems more familiar with the common parlance of the apostolic age than the textual experts who repeatedly advance this argument.

4. The mention of the Mount of Olives in John 8:1 excites the suspicion of some critics. It is thus sometimes claimed to be a sure sign of the non-Johannine origin of the story, for John nowhere else mentions the Mount of Olives in his Gospel, not even in John 18:1.

However, numerous geographical locations are only mentioned once in John's gospel, including Aenon (3:23), Salim (3:23), Sychar (4:4), Bethzatha (or better, Bethesda — 5:2), Bethlehem (7:42), Solomon's Porch (10:23), Ephraim (11:54), Kedron (18:1), Gabbatha (19:13) and Golgotha (19:17). Furthermore, the reason why the Mt. of Olives was not mentioned in John 18:1 could possibly be explained on the basis that Jesus never went there — he went to a garden in the valley below it.

5. Another stylistic argument that is frequently repeated is that the PA uses the conjunction *δε* instead of John's characteristic *ουν*. Thus, Alford writes, "John does not usually connect with *δε*, more commonly with *ουν*."¹⁹ Daniel Wallace argues that "only here are verses continuously connected by *δε* (vv. 2, 3, 6, 7, 9, 10, 11)."²⁰

The main problem with this argument is that John uses *δε* more times in his Gospel than *ουν* (212 times to 200 in NA27, 230 to 201 in the Byzantine text). Thus, the claim that John prefers *ουν* over *δε* can only be true by a process of creative accountancy. It is true that in the Synoptic Gospels, *δε* is far more common than *ουν*. However, John must be characterised on his own stylistic terms, not simply by contrast and comparison with others.

Δε is found 10 or 11 times in the PA, the difference depending upon a variant reading in 8:1, however there are other passages in John's Gospel with similarly high counts: 9 occurrences in John 18:14-25 (Jesus' trial before Annas) and 10 occurrences in John 19:9-19 (the trial before Pilate) in the Byzantine text. Interestingly, the occurrences of *δε* drop off significantly in John chapters 13-17 where Jesus is teaching (only 30 occurrences in these five chapters, compared with 20 occurrences in John 11 alone). These figures would suggest that *δε* is more prevalent in dramatic incidents (and especially adversarial, judgment scenes, like the PA, and chapters 18 and 19) than in discourses (such as chapters 13-17).

J. P. Heil, to whom Wallace was responding, replied by saying that Wallace's argument was "misleading, implying that the verses of our story are syntactically connected in a way unlike other Johannine stories. But verses of both the healing story in 5:1-11 (vv. 2, 5, 7, 9, 11) and the feeding story in 6:1-15 (vv. 2, 3, 4, 6, 10) are nearly as connected by *δε* as is our story. At any rate, the verses of both these Johannine stories are continuously connected, whether by *δε* or *ουν* (5:10; 6:5, 10, 11, 13, 14, 15)."²¹

Actually, by far the most "characteristic" Johannine conjunction is neither *ουν* nor *δε*, but *και*. There are over 500 occurrences of *και* in John, compared to 200-odd for *ουν* and *δε*. There are between 10 and 12 occurrences of *και* in the PA (depending on preferred variant readings) and this is consistent with the statistical range of *και* in John's gospel. Thus, there are 14 occurrences of *και* in John 9:1-12, 7 in John 11:1-15, 13 in John 13:1-12 and 13 in John 18:1-12. Likewise, although there is only one occurrence of *ουν* in the PA, there are other narrative passages in John with similar figures: 0 occurrences in John 2:1-13, 2 occurrences in John 5:1-15, 2 occurrences in John 6:1-12.

What do all these statistics prove? Not much beyond old proverbs about the need to treat statistics with caution. These figures do not prove that the PA is unJohannine.

¹⁹ Alford, *The Greek Testament*, 1:564.

²⁰ D. B. Wallace, "Reconsidering 'The Story of Jesus and the Adulteress Reconsidered,'" *NTS* 39 (1993), 291.

²¹ J. P. Heil, "A Rejoinder to 'Reconsidering 'The Story of Jesus and the Adulteress Reconsidered,'" *Eglise et Theologie* 25 (1994), 361.





7. Another criticism of the PA²² is the expression *ἐν μέσῳ* (“in the midst”) in verses 3 and 9. This expression is used 13 times in the synoptic Gospels, but nowhere else in John’s gospel. Instead, it is argued, John prefers the expression *εἰς τὸ μέσον*.

There are two problems here. Firstly, John only uses the so-called Johannine expression *εἰς τὸ μέσον* two times (John 20:19, 26); it is hard to see how two occurrences of an expression make for characteristic usage. Secondly, John also uses a diversity of other expressions, like the simple *μέσος* (John 1:26, 19:18) and even the unusual verb form *μέσσω* (John 7:14), nowhere else used in the NT. Thus, this is just another weak argument.

8. One of the most frequently repeated arguments against the PA concerns the use of the word “scribes” in 8:3.²³ The weight of this objection lies in the fact that John could elsewhere have referred to the scribes had he wanted to. Perhaps he did not have much opportunity to elsewhere mention barley or grass (Chapter 6) or fishing and nets (Chapter 21). But John mentions the Pharisees twenty times in his gospel and the other Gospels mention the scribes almost as frequently. Surely John would have done so too. The singular use of the word here in the PA thus shows a genuinely non-Johannine word in the passage, it is argued.

Much of the popular interest surrounding the incident centres on what Jesus wrote on the ground and various suggestions have been put forward by different commentators as to what was written. However, perhaps the more important issue is not what Jesus wrote on the ground, but why Jesus stooped to write at all.

Jesus was writing on the ground in the very presence of men who — so we are specifically told — were scribes. That is, these were men who wrote out the words of the Law of God and taught it to the people; men with great reverence for the Law who maintained extremely high standards in their copying of it. Yet they stood accusing a woman of sin, despite the fact that they should have been convicted, from the onerous demands of the Law that they copied, of many sins and shortcomings. Christ’s reply, “he who is without sin let him cast the first stone,” only resulted in wholesale conviction because of the cumulative effect of three factors: (a) Christ’s calligraphy providing a vivid illustration of what these men did for a living, (b) the high standards of God’s laws that they copied out, and (c) Christ’s words accusing them of hypocrisy.

The mention of scribes as the prime movers in condemning the woman seems a rather important detail in the piece. The argument that there are plenty of other occasions that John could have mentioned the occupation “scribes” is true, but vacuous. There are also plenty of other occasions, aside from John 21, that John could have mentioned the fact that he, and certain other main characters in his narrative, were by occupation fishermen.

John could have likewise used the word “journey” found only in John 4:6 on numerous occasions, considering that the fact that the first twelve chapters of the Gospel contain so many of Jesus’ journeys, to and from Judea. The fact that John only mentions the occupation of certain men as scribes in the PA is not exactly surprising for a Gospel famous for its narrow focus and its numerous omissions of information that rate prominently in the Synoptic accounts of the life of Christ.

9. The use of *παρεγενετο* (“came”) in verse 2 has also been claimed to be unJohannine.²⁴ However, the verb *παραγινομαι* is used elsewhere by John in 3:23 in exactly the same sense. Similarly, the word *λαος* in verse 2 has also been claimed to be unJohannine, despite the fact that it is used elsewhere in John two times (11:50, 18:14).²⁵ Carson says, “*paraginomai* (“appear”) and *laos* (“people”) are common in Luke-Acts, rare in John.”²⁶

Thus, if the PA uses “non-Johannine” words, it is accused of inauthenticity, but even when it uses genuinely Johannine words, this is also somehow considered evidence of its non-Johannine origin. With double-standards like these, it seems the PA is hardly being given a fair hearing.

10. One last issue related to vocabulary and style is the fact that the PA is only 12 verses long, roughly the same length as other Johannine narratives, for example, the miracle at the wedding in Cana of Galilee or the feeding of the 5000. Of course, few have criticized the PA on this account — but that is the point. The PA does not disqualify itself by padding out the story with miraculous deeds, verbose commentary or apocryphal details. While some scribes did indeed try to expand some details in

22 Davidson, *Introduction to the NT*, 359.

23 See, for example, Alford, *The Greek Testament*, 564.

24 Davidson, *Introduction to the NT*, 359.

25 Alford, *The Greek Testament*, 1:564.

26 Carson, *The Gospel According to John*, 335.





the story, yet the account retains the terse integrity of something that John would have written.

Raymond Brown writes, “Its succinct expression of the mercy of Jesus is as delicate as anything in Luke; its portrayal of Jesus as the serene judge has all the majesty that we would expect of John.”²⁷

On the other hand, if “non-Johannine” vocabulary is supposedly evidence of a non-Johannine origin, then by the same token, positive examples of Johannine vocabulary in the PA equally deserve to be noted as evidence for a Johannine origin:

- “this they said” (8:6 — also found in John 6:6, 7:39, 11:51, 12:6 and 21:19)
- the use of the vocative *γυναῖ* in 8:10 (see also John 2:4, 4:21, 19:26, 20:13, 15)
- “sin no more” in 8:11 is only elsewhere found in the NT in John 5:14
- the historic present *αγορευειν* (also in John 9:13 and 18:28)²⁸
- the scornful use of the word “this” to refer to a person the Pharisees did not approve of (here referring to the woman) in 8:4 is commonly found in John’s Gospel. Christ is derisively referred to as “this (fellow)” in John 6:52, 7:15, 9:29 and 18:30

More than this, as we saw in the raw numbers argument, the PA manages to match the statistical numbers of (a) singularly Johannine words and (b) *hapax legomena* found in other dramatic passages that John himself wrote. This raises the question of how a scribe with a different mind, writing in a different century, under different circumstances, and without the benefit of advanced computer analysis, would manage to hit the same statistical target ranges as other Johannine narratives in not one, but two distinct vocabulary tests. Some textual critics will suggest a sophisticated interpolator, but surely a serious Johannine imitator would have done as modern textual scholarship insists, and not included any ‘unJohannine’ words or *hapax legomena*. Instead, just like Goldilocks’ porridge, the PA is neither too hot nor too cold; it is just right. How exactly would a second or third century scribe, lacking even the manual concordances of pre-21st century scholarship (which advanced the idea that the vocabulary of the PA was all wrong), manage to get John’s vocabulary ranges just right?

The vocabulary leaves us with three options: (1) the interpolator framed a Johannine narrative in John’s style, by sheer luck hitting the vocabulary bulls-eye twice, (2) the interpolator was beyond ordinarily sophisticated, aware of the Johannine vocabulary statistics we have discussed, as well as capable of crafting a story with compositional brilliance, or (3) the interpolator managed to write a Johannine narrative in John’s style, naturally and without affectation, because he was, in fact, John.

Of course, the textual experts will not tolerate option (3), but on internal stylistic grounds it remains a distinctly reasonable possibility, while the odds for options (1) and (2) are so unlikely that to choose either of them would appear to be a resort of desperation.

Thus, the oft-repeated style and vocabulary arguments against the authenticity of the PA have little real merit. They involve selective use of evidence and statistical sleight of hand beneath the normal standards of integrity and scholarship.

Of course, the problem here is not that these arguments are somehow consciously dishonest; what is most disturbing is the uncritical repetition of these flawed arguments in numerous commentaries.

The PA Interrupts John Chapters 7 and 8

The second internal argument commonly advanced against the authenticity of the PA concerns the suitability of the PA to John’s Gospel. As we shall see, there are actually two slightly different but related forms of this argument. The strong version of this argument is given by Metzger, who says that the PA “interrupts the sequence of 7:52 and 8:12ff.”²⁹ At other times the argument is stated more modestly. Thus, we find the milder form of this argument in Bernard who writes of the PA that “nor in its traditional place does it harmonize with the context.”³⁰

²⁷ Brown, *The Gospel According to John*, 336.

²⁸ These four examples are taken from Z. C. Hodges and A. L. Farstad, *The Greek New Testament according to the Majority Text* (Nashville, TN: Thomas Nelson Publishers, 2nd ed., 1985), xxiii-xxiv.

²⁹ Metzger, *Textual Commentary*, 188.

³⁰ Bernard, *John*, 715.





We shall take the strong form of the argument first. Strange to say, most commentaries produce little explanation or demonstration of how the PA interrupts John's Gospel. For example, Metzger (quoted above) provides no substantiation of the claim that the PA interrupts John's Gospel beyond the mere assertion that the PA is an interruption. J. D. Punch, after surveying the commentaries, argues that "to date there appears to be no scholar who has laid out for us exactly *how* the Pericope Adulterae interrupts the flow of (sic) between John 7-8."³¹

Indeed, at face value, the PA does not appear to be an interruption in any normal sense of the term. For example, children are prone to interrupt conversations by speaking while their parents are in the middle of a sentence, instead of waiting for a natural break in conversation. But the PA is not placed in the middle of a conversation or dramatic event. Instead, it is placed at a natural break: the council meeting has finished in 7:52, and Jesus' speech in 8:12ff. has not yet started.

In fact, absent substantiation, the argument almost appears to be a case of circular reasoning. The conclusion (that the PA is an interpolation/interruption) is smuggled back into the syllogism as one of the supporting arguments. This is pure *petitio principii*: the argument just assumes what needs to be proved.

Hort provided a slightly less circular form of argument, suggesting that the PA interrupts the connections between the Feast of Tabernacles' water pouring ceremony (7:37-39) and lamp lighting ceremony (8:12), the "two great symbolic and commemorative acts ... of the Feast of Tabernacles," thus interposing "a heterogeneous incident" that "dissevers the one from the other."³² The problem with this idea, of course, is that there is another intervening event between these two incidents which provides no reference to a Feast of Tabernacles ceremony: the council meeting of 7:45-52. Further, and contrary to what Hort says, water-pouring and lamp-lighting were hardly "the two great symbolic and commemorative acts of the Feast of Tabernacles"; the most important symbolic and commemorative act of the Feast of Tabernacles was the dwelling in temporary shelters (hence the Feast's name). In fact, *contra* Hort, it could equally be argued that the PA provides a reference to this central observance of the Feast with the Pharisees in 7:53 "each going to his house" at the conclusion of this period of booth-dwelling. In any case, it would seem unjustified to insist that John must line up his thematically-linked incidents one after another, without any intervening material, lest his readers be too dull-witted to notice the connection.

More vaguely, other commentators appear to object to the fact that the PA interrupts the building tension of Jesus' verbal clashes with the Jews in John 7 and 8 during the Feast of Tabernacles. Thus, Köstenberger, who entitles John 7 "First Teaching Cycle" and John 8 "Second Teaching Cycle," writes of "the interruption of the narrative flow from 7:52 to 8:12, breaking up the literary unit 7:1-8:59."³³ The fact that the PA involves a dramatic incident provides an interruption to the rhetorical dueling that continues through John chapters 7 and 8.

This argument, however, appears to misunderstand the *modus operandi* of John's Gospel, which is to use Jesus' actions to corroborate Jesus' words (cf. John 14:10-11). John intersperses dramatic events alongside discourses and dialogues throughout his gospel, much as a preacher might intersperse explanatory illustrations through his sermons. This is the "Johannine style" and it would seem somewhat presumptuous for modern commentators to refuse *a priori* the possibility that John might have interjected a dramatic event, illustrative of the clash between Jesus and the Jewish leaders, in chapters 7 and 8.

Leaving aside the issue of whether John may be permitted to intersperse drama and dialogue throughout his Gospel, then, we are going to put the PA to a test to see whether it exhibits the characteristic features of literary interpolations and to gauge whether the PA interrupts John's Gospel at this point.

The Characteristic Features of Literary Interpolations

Harry Gamble³⁴ helpfully suggests a number of tell-tale features which have been claimed by 'partition theories' to show that certain texts are composed of different parts which were later brought together into a united whole. His tell-tale signs of compilations would also appear to apply to interpolations.

31 Punch, *The Pericope Adulterae*, 101-2, emphasis in original.

32 B. F. Westcott and F. J. A. Hort, *Introduction to the New Testament in the Original Greek; Appendix I. Notes on Select Readings* (2nd ed., London: Macmillan, 1896), 87-8. Philip W. Comfort also repeats this argument in "The Pericope of the Adulteress," *The Bible Translator* 40 (January 1989), 145-7.

33 A. J. Köstenberger, *John* (BECNT; Grand Rapids, MI: Baker Academic, 2004), 247.

34 H. Gamble, *The Textual History of the Letter to the Romans: a Study in Textual and Literary Criticism* (SD 42; Grand Rapids, MI: Eerdmans, 1977), 137.



He argued that such “partition theories” depend upon:

1. *abrupt changes* in the subject matter
2. *interruptions* in an otherwise continuous train of thought
3. seeming *inconsistencies* or even contradictions that conflict with other material in the document
4. the presence of *certain formulae* in supposedly inappropriate or uncustomary contexts
5. *repetition* of redundant elements
6. perceived *changes* in tone or style
7. the *assumption* by the writer of different circumstances on the part of the intended audience

This list of features will provide us with tests of the genuineness of the PA. Thus, if the PA has been inserted into John’s Gospel by a later scribe, we would expect to find it exhibiting some or all of the features considered characteristic of such innovative literary interpolations. That Gamble’s tests are good ones can be seen by noticing how some of these features crop up in a number of examples of textual variants in the NT.

Thus, we see tell-tale signs of abrupt change in subject matter (Point One), interruption of a continuous train of thought (Point Two), and repetitive redundancy (Point Five) with the ending of Mark’s Gospel; it switches the subject (to Jesus), there is discontinuity between verses 8 and 9, and it re-introduces Mary Magdalene to the readers (repetitive redundancy).

As another example of Gamble’s Point Five, we may take the Family 13 group of MSS (which insert the PA after Luke 12:38), and notice the way the information about Jesus staying on the Mount of Olives and teaching in the temple is repeated in an awkward and ungainly way.

A final example from Gamble’s own work will suffice: the Byzantine text’s placement of the Roman doxology after 14:23. While Byzantine Priority theorists might argue for this reading on grounds of external evidence, the abrupt change in subject matter presents a problem for this reading on internal grounds.

Thus, abrupt changes and interruptions of continuous trains of thought, different subject matter, inconsistencies or contradictions and unnecessary repetition show the traces of compilations and interpolations. Turning our attention to the PA, we may ask whether any of these elements are associated with the PA?

Does the PA interrupt a continuous train of thought? Does the passage run smoothly without the PA? Does the PA result in awkward joins?

The very reverse is true. It is the omission of the PA that provides an awkward transition of events. There are in fact three effects produced by the removal of the PA. First, we have an abrupt change of events. In 7:52, we have the Jewish leaders arguing over Jesus *in camera*, but in 8:12, Jesus is in the temple courts publicly preaching. Secondly, we have a change of subject, for in 8:12 the subject is not the Pharisees of 7:52, but Jesus. Thirdly, we also have an abrupt change of object, for we read in 8:12 that “Jesus spoke to *them*” — but the “them” of 8:12 is not the Pharisees of 7:52 but the crowds in the temple courts. John does not even tell us in 8:12 who the “them” are.

To use Gamble’s language, the change of object here is similar to “the apparent assumption by the writer of different circumstances on the part of the addressees.”³⁵ John seems to assume that we (his readers) will know that the “them” of 8:12 is not the Pharisees arguing privately, but the crowds in the temple courts. We might have expected something like “Then Jesus spoke to *the crowd* again” (as elsewhere in John chapters 7-10, e.g. 7:12, 7:20, 7:31, 7:32, 7:40, 7:44, 7:49). The unexplained “them” produced by omitting the PA is reminiscent of the unexplained “he” of Mark 16:9, where the writer (let us say, Mark) assumes his audience will work out that the subject has changed to Jesus, without being told.

In fact, the argument that the PA somehow interrupts a continuous series of events borders on the bizarre, for there is little continuity either of dramatic events or subject matter between 7:52 and 8:12. There is no connection between the question of whether prophets come from Galilee (7:52) and Jesus’ teaching in 8:12ff., nor mention of Galilee or Jesus as a prophet anywhere in John 8.

³⁵ Gamble, *Textual History of Romans*, 137.



Instead, the introductory verses of the PA (7:53-8:2) constitute a bridging feature between the material at the end of chapter 7 and 8:3-11 (the main part of the PA), showing how the main players at the end of chapter 7 go their various ways and then meet up again in the temple courts in chapter 8. The narrative, taking place as it does in the outer court of the temple where women are permitted, then moves without difficulty on to Jesus speaking to the crowds in the temple courts from 8:12 onwards. The PA does the very opposite of interrupting a continuous train of thought, providing a two-part bridge that connects 7:52 with 8:12. In fact, it is surprising that those indisposed toward the PA do not argue that the PA was concocted and inserted to provide a transition to smooth the discontinuity between 7:52 and 8:12. Instead, strangely, most commentators continue to repeat the argument that the PA somehow interrupts events here.

Finally, we have already seen that the PA passes Gamble's tests 4 and 6, in that the vocabulary and style objections do not stand up under sustained scrutiny.

The excision of the PA is therefore more of a problem for the flow of John's Gospel than its inclusion. To return to Gamble's list of seven tell-tale signs of literary compilations (and interpolations), not one is found in relation to the PA; it is entirely blameless on all counts. Therefore, we conclude that the PA passes Gamble's tests with little difficulty: it provides no interruption to John 7:52-8:12ff. Here then is a second argument against the PA, entirely without substance or merit, but nevertheless regularly repeated in the commentaries.

The PA does not Harmonize with the Themes of John chapters 7-10

Secondly, we must address the milder form of the objection: that the PA does not harmonize with the thought-flow of John 7 to 10; or, to put it another way, the PA is of little value in illustrating the major themes that John is developing in his Gospel at this point.

We are going to examine the PA against the backdrop of John chapters 7 to 10. Some readers may wonder why we have chosen these four chapters (as opposed to just chapters 7 and 8). The answer is important: this appears to be the way that John has himself structured his gospel. Thus, in the first half of the gospel (chapters 1 to 12), Jesus makes five journeys to Judea, presenting himself and his credential to his own people. These journeys illustrate the theme contained in 1:11, "He came to his own, and his own did not receive him," as John 12:36 sums up: "although he had done so many signs before them, they did not believe in him." From chapter 13 onwards, the expression "his own" changes meaning and refers to those who "received him ... who believe on his name" (1:12). The second half of the gospel thus focuses upon Jesus' relationship with his new "people," his believing followers.

John chapters 7 to 10 stand together as the fourth of these "journey to Judea" sections. To show one way in which John chapters 7 to 10 stand together, we may look at the first issue that is raised in John 7: secrecy versus openness. Jesus' brothers taunt him over his apparent reluctance to go up from Galilee to Jerusalem for the feast in 7:4 with the words, "no one does anything in secret (ἐν κρυπτῷ) while he himself seeks to be known openly (ἐν παρρησίᾳ). If you do these things, show (φανερωσον) yourself to the world." Their words mockingly suggest that such reluctance was unbecoming for a messianic pretender. Then, in the next incident, after Jesus has gone up to the Feast of Tabernacles, we read that he went up "not openly (φανερῶς), but as it were in secret (ἐν κρυπτῷ)" (7:10). Jesus' possible appearance was the subject of intense discussion among the people at the feast, we are told, but "no one spoke openly (παρρησίᾳ) about him, for fear of the Jews" (7:13).

The repetition of the ideas of secrecy and openness shows the development of a theme here by John. This theme of "secrecy versus openness" is also continued in chapters 7 and 8 with its "hide and seek" language in 7:34, 36, and 8:21, 59. Indeed, chapter 8 ends with Jesus "hiding himself" from the Jews trying to stone him (verse 59).

When we come over to John chapter 10, the section concludes with the same thematic concern. Jesus tells the "parable" of the shepherd, who presents himself at the door of the sheep-fold, whereas false shepherds (thieves) climb in some other way (10:1-2). The point seems to be that a true Messiah presents himself openly before his people and shows his credentials; he does not sulk in obscurity or employ crafty and deceitful methods to get access to the sheep.

Finally, after the ensuing discourse about Jesus being the good shepherd, John relates how the Jews surround him and ask, "How long do you keep us in suspense? If you are the Christ, tell us boldly (παρρησίᾳ)" (10:24). Jesus strangely answers evasively (10:25-39).

The repetition of the secrecy versus openness motif and the connections between John 7 and 10 are obvious.





Further, this theme of secrecy versus openness links up with the way John has structured his presentation of Christ in his gospel: as a succession of sections in which Jesus the Messiah journeys to Judea to present himself and his credentials openly before his nation and people, inviting faith.

Of course, we could justify the fact that John chapters 7 to 10 stand together as one section by pointing out that they flow continuously without any interruption into each other (or, at least, chapters 8, 9 and 10 do). But John also weaves repeatedly-mentioned and interlinking thematic threads throughout the chapters, binding the section together.

Our task, then, will be to survey John chapters 7 to 10 for themes that John is developing, and see whether we pick up any similar thematic threads running through the PA. If the PA were original to John's Gospel, we would expect it to complement the themes being woven through the section; on the other hand, if the PA were an interpolation, we would expect it to provide us with "loose ends" that introduce novel, unrelated and irrelevant themes. We shall highlight the four main themes that John chapters 7 to 10 present.

Theme One: The Brilliance of Christ's Words and Teachings

John chapters 7 to 10 focus our attention on the greatness of Christ's words and teachings. This should not surprise us for, if John primarily presents Christ as the Word (1:1) in his gospel, we would expect some attention to be focused upon this theme. Here, in the central (and longest) of the seven sections of the Gospel,³⁶ Christ's words and teachings are of major importance. John chapter 7 focuses our attention on Jesus' teaching in the temple during the feast of Tabernacles (see 7:14, 15, 16-18, 28, 33-35 and 37-40). John 7:46 sums up the point: "No man ever spoke like this man!"

Similarly, the emphasis on Jesus' "word," "words," "teaching," "speaking" and "saying" is maintained in John chapter 8 (see 8:20, 25-29, 30, 31, 37, 38, 43, 47, 51-52 and 55). It becomes obvious that Jesus' words and teachings are a key theme of John 7 and 8. Finally, and as already noted, chapters 7 and 8 largely involve one long word-fight between Christ and his adversaries.

Now, we turn to consider the question of the thematic suitability of the PA in such a context. If the PA is arguably the most famous story about Jesus in the gospels,³⁷ then it is equally true that the words he spoke in this story are possibly the most famous words Jesus ever said: "He who is without sin among you, let him cast the first stone?" (8:7).

There is no more spectacular example of the brilliance of Christ's words in any of the Gospels. Schnackenburg says "the sentence is unique and unforgettable."³⁸ Petersen calls it "one of the best-known sayings in world literature."³⁹ The words are proverbial to this day, particularly in tolerant, non-judgmental western 21st Century society. Christ's reply is so characteristic of his unique genius that it induces most commentators to admit that the incident is truly historical, even though they do not think it was originally part of John's gospel. Lindars writes, "The point [of the PA] is the skill with which Jesus maintains his own position without falling into the trap of publicly repudiating the Law."⁴⁰ The trap the Pharisees and Scribes set for Christ was intended to either destroy Christ's religious authority (if he undermined the Law of Moses) or to lead to politically dangerous consequences (if he sanctioned a stoning). Christ's reply not only upheld the Law of Moses and avoided political trouble, but publicly silenced his enemies, all in a spectacular one-liner; no political circumlocution or academic equivocation. No wonder the soldiers sent to arrest Christ in the previous chapter of John's Gospel said "no man ever spoke like this man." In addition, the words are entirely in keeping with Christ's well-documented distaste for hypocrisy.

The PA also harmonizes with the thematic context in its two references to Jesus teaching. Firstly, in 8:2, we read, "early in the morning he came into the temple, and all the people came to him, and he sat down and taught them."

Then, in 8:4 the scribes and Pharisees address him (with sarcasm), as "Teacher." One reference to Jesus teaching might be entirely inconsequential, but the repetition provides a thematic link with the main theme of John 7 and

36 The last two sections of the gospel are chapters 13-17 and 18-21 (or, perhaps chapter 21 might be considered a separate epilogue).

37 Bart Ehrman has called it "arguably the best-known story about Jesus in the Bible," in *Misquoting Jesus: The Story Behind Who Changed the Bible and Why* (San Francisco: HarperSanFrancisco, 2005), 63; Chris Keith entitled the introductory chapter of his monograph, "The Most Popular Story in the Gospels" in *The Pericope Adulterae, The Gospel of John and the Literacy of Jesus* (Leiden: Brill, 2009), 1.

38 R. Schnackenburg, *The Gospel According to St. John* (Tunbridge Wells: Burns and Oates, 1980), 2:162.

39 Petersen, "Οὐδε εἷς ἐστὶν ὁ ἀκατάκριτος" John 8:11," 191.

40 B. Lindars, *The Gospel of John: Based on the Revised Version* (New Century Bible; Grand Rapids: Eerdmans, 1981), 308.





8: Jesus' teaching.

In the PA, then, we have perhaps the most powerful illustration of what John has spent much of Chapters 7 and 8, trying to demonstrate — the marvel of Christ's words and teachings.

Chris Keith suggests other thematic links that we may place under this same general heading. Following Edgar Goodspeed, Keith argues that the PA was inserted in John 7:53-8:11 to “disprove the Jews” assumption that Jesus did not “know letters” in John 7:15.⁴¹ Keith's exposition of John 7 highlights John's concern with the themes of Jesus' authority to teach Moses' law and the haughty attitude of the Jewish leadership toward Jesus because of his lack of formal education and Galilean origin.

Then he moves on to show that in Jesus' repeated act of writing in the PA, a “sophisticated interpolator” has emphasized Jesus' superiority to the formally-educated scribes (and even to Moses himself) in his presentation of “a (divinely) grapho-literate Jesus.”⁴² Keith provides some parallels between Jesus' writing in the PA and God's writing of the Ten Commandments in Exodus 31, arguing that the interpolator is projecting a picture of Jesus as the Divine writer.

Some might suggest that the issues of Jesus' *authority* to teach (Keith's point) and his *ability* to teach (the general theme we have traced here) are really the same, however there is a difference. Not all academics are engaging public speakers, while some powerful public speakers have little formal education. Jesus stood in contrast to the Jewish teachers, offering not simply electrifying public-speaking ability but also the authoritative force of divinely-inspired insights in his teachings (a point upon which Matthew 7:28-9 and Mark 1:22 also remark).

Keith's exposition of John 7 similarly sparkles with genuine insights into the text. Many other commentators, however, feed us with chaff: a Rabbinic quote here, a note on Greek grammar or historical background there, a possible OT cross-reference to pad things out, and a quote from a fellow-commentator to conclude, but as for illuminating the message and thought-movement of John's Gospel, very little beyond the superficial. Keith thus provides us with numerous valuable and convincing points of connection between John 7 and the PA:

- the γραφ-morpheme running through John 7 and into John 8: John 7:15 (“letters”), 7:38, 42 (“scripture”), 8:3 (“scribes”), 8:6, 8 (“wrote”) and 8:17 (“written”)
- the themes of Moses and Mosaic law in John 7 and the PA (John 7:19, 22, 22, 23 and 8:5)
- the issue of the correct interpretation of the law (John 7:21-24, 51-52, 8:5)
- the question of Jesus' authority to teach without formal education (John 7:15-18 and 8:2),
- the Jewish leaders' dismissive attitude because of Jesus' Galilean origin (7:1, 8, 41, 52)

Once we add these sub-themes to the main point of Jesus' brilliant words and teachings, we may conclude that the PA harmonizes remarkably well with the contextual concerns of John 7. This is, in fact, the reason why Keith's exposition is so illuminating: because of the numerous reasons Keith provides for the special fitness and suitability of the PA after John 7.

Theme Two: Judgment

A second major theme of John chapters 7 to 10 is judgment. This is a theme continued from earlier in John's Gospel. Thus, chapter 5 is heavily concerned with the subject of judgment (verses 19-30), prompted by Jesus' healing a man on the Sabbath, and the Jews' response in seeking to kill him (5:16, 18). Jesus defends himself by calling upon four witnesses to verify his claim to be Messiah (verses 31-47).

When we come to John 7, the subject of judgment rises to prominence again. In 7:19-24, Jesus himself raises the fact that the Jews are seeking to kill him for breaking the law and defends his actions in healing the man on the Sabbath. This leads on to his statement in 7:24 about standards of judgment: “Do not judge according to appearance, but judge with righteous judgment.”

Jesus' teaching during the Feast arouses the anger of the religious authorities, who send officers to arrest him (7:32). In verse 45, after Jesus' teaching on the last day of the feast, the officers return empty-handed, saying “No

⁴¹ Keith, *The Pericope Adulterae*, 141; E. J. Goodspeed, *A History of Early Christian Literature* (Chicago: University of Chicago Press, 1942) 70.

⁴² *Ibid.*, 203.





man ever spoke like this man.” This enrages the Pharisees, which in turn leads to Nicodemus’ interjection on Jesus’ behalf in 7:51: “Does our law judge a man before it hears him?”

Likewise, in chapter 8, Christ starts speaking about judgment again (8:13-18). Thus, we have repeated reference to “testimony, witness, or witnesses” in 8: 13, 14, 17 and 18, as well as references to “judge” and “judgment” in verses 15 and 16. The forensic language continues in John 8, where Jesus refers to judgment in verse 26, “I have many things to say and to judge concerning you.” The legal overtones are also obvious when he later asks, “Which of you convicts me of sin?” (8:46).

Similarly, John 9 contains the long trial of the blind man who Jesus healed on the Sabbath day. At the conclusion of this chapter, Jesus says, “For judgment I have come into this world, that those who do not see may see, and that those who see may be made blind” (9:39). However, the most noticeable reference to judgment, particularly in connection with the PA, is found in 8:15, where Christ says: “You judge according to the flesh. I judge no one.”

The PA provides such a striking illustration of these words that not a few commentators argue that these words were what prompted the interpolator of the PA to place it where it is.⁴³ Thus, Beasley-Murray writes, “If we ask why [the PA] was set in its present place, the answer must be a genuine sense of fitness to context. The theme of judgment is strong in chaps. 7-8; the story could well be regarded as illustrative of 7:24 and 8:15-16; and we note the opposition of the Pharisees to Jesus in 7:46-52 and 8:13.”⁴⁴

However, we can go further and say that the corollary is also true: not only does the PA illustrate the words “I judge no one” in 8:15, but the words in 8:15 (in turn) provide an explanation for one of the most puzzling features of the PA: Christ’s writing on the ground. Jesus’ disinterest in judging (John 8:15) explains why Jesus ignored the scribes and Pharisees question instead of replying verbally to them. The PA and the words in 8:15 stand in symbiotic relationship; they illuminate, illustrate and commentate upon each other.

Some might perhaps argue that this theme of judgment is merely part of the previous theme — Jesus’ teaching authority — for the authority and ability to interpret Moses’ law carried with it the responsibility to judge. However, it might be better to say that the PA and the words in 8:15 are about non-judgment. While Jesus shows he has the authority to interpret Moses’ law in John 7 (and therefore to judge), Jesus’ refusal to judge in John 8:15 introduces a new idea. The two issues of teaching authority and non-judgment are separate and distinct: the first relates to the law, the second is based on grace (cf. John 1:17).

Another sub-theme worth noticing in these chapters is the way that Jesus repeatedly turns from being the defendant to the judge. Stibbe writes, “The irony of the chapter [John 8] is the fact that Jesus himself becomes the prosecutor. Having begun a process of questioning and prosecuting Jesus, the three Jewish groups find themselves almost imperceptibly taking on the role of defendants. The old irony of the judged becoming the judge is played out here.”⁴⁵ Just as in John 5, where Jesus goes from defendant to judge (5:22, 27, 30), in John 8 Jesus goes from defendant (8:13-14) to arguing that the Jews are sinners (8:24, 26, 34); then, taunted by the Jews over who his father is (8:19), Jesus proceeds in verses 38-47 to show that their father is the Devil.

This pattern is illustrated in the PA, where the Jews try to trap Jesus over the woman taken in adultery, but end up being “convicted by their own conscience” (8:9) at Jesus’ words. Thus, the PA clearly harmonizes with one of the major themes of John chapters 7-10: the idea of non-judgment.

Here is also where we run up against some drawbacks and problems with Keith’s thesis. Keith surprisingly downplays the idea that the words in 8:15 are what prompted an interpolator to place the PA in 7:53-8:11. He writes, “If the PA’s original interpolator wanted to draw an explicit connection between the PA and John 8:15, why did he not place the story immediately prior to (or even after) that passage?”⁴⁶

If the PA were three chapters away from John 8:15, Keith’s argument here would carry some force, but the PA is only three verses away.

Keith complains that the PA is not close enough to John 8:15 to be connected, but the key verse for Keith’s own

43 For example, F. F. Bruce, *The Gospel & Epistles of John*, 413; Carson, *The Gospel According to John*, 335.

44 G. R. Beasley-Murray, *John* (WBC 36; Waco, TX: Word, 1987), 144.

45 M. W. G. Stibbe, *John* (Sheffield: Sheffield Academic Press, 1993), 101.

46 Keith, *The Pericope Adulterae*, 142.





theory (about Jesus not knowing letters, John 7:15) is over 30 verses away. The double-standard is illuminating: Keith is prepared to casually dismiss John 8:15 as irrelevant on grounds that would provide far more reason for disqualifying his own theory.

Keith thus tries to disparage any link between the PA and the words in 8:15, despite the obvious and oft-mentioned connection. The reason appears to be that it does not suit Keith's purpose to investigate any thematic links between the PA and any other possible themes from John 7-10. This is despite the fact that John 8 presents immediate and striking connections with the PA, although on quite different lines to Keith's "literate Jesus" theory, starting with the words in John 8:13-18 about judgment. Keith appears only open to one possible option: his own thesis about Jesus' education and literacy being what prompted the insertion of the PA. As a result, Keith fails to explore other options — either to confirm or eliminate them.

This is probably the most disappointing feature of Keith's work: his failure to continue his excellent exposition any further than John 7. The fact, however, that John 8 provides striking connections with the PA, but of a quite independent and unrelated nature to issues of teaching authority which Keith explored in John 7, raises the question of how it is possible that the PA could intersect so neatly with not one, but two entirely independent sets of contextual themes, firstly in John 7, and then in John 8.

Theme Three: The Light of the World

The third theme — indeed perhaps the master-theme — of John chapters 7-10 is Christ as the "Light of the World," which Jesus twice declares himself to be in 8:12 and 9:5. In 8:12, the theme of light is contrasted with darkness ("whoever follows me will not walk in darkness"), whereas in 9:5, light is contrasted with darkness of a slightly different sort: blindness. This theme of light versus darkness brings us back again to the opening idea of John chapters 7 to 10: the openness versus secrecy motif.

John chapters 8 and 9 are linked together and dominated by these two "Light of the World" declarations. Following his declaration in John 8:12, Christ argues at length with the Jews over issues of parentage and identity: firstly, over who Jesus is the One sent by his Father, 8:12-30) and then secondly, who the Jews truly are (slaves to sin and sons of the Devil, 8:31-47). This issue of parentage, of course, provides another tangential connection with the idea of adultery found in the PA. However, Jesus' words in chapter 8 generate as much heat as light — the Jews call him a Samaritan (verse 48), demon-possessed (verses 48 and 52), and end the chapter by trying to stone him (8:59). In chapter 9, it is not Jesus' teachings but instead his actions that bring light; he gives sight to the blind man, while the Pharisees show increasing signs of blindness by refusing to believe that Jesus healed the blind man.

What, then, is the connection between the "Light of the World" in chapters 8 and 9? Christ is the "Light of the World" in John 8 in that he reveals to us and exposes what we truly are: sinners. But in John 9, Jesus presents himself as the "Light of the World" by giving us sight. We could say that in John 8, Christ diagnoses our sin-sickness, but in John 9, he cures it. The big issue that John 8 and 9 highlight is humanity's obstinate refusal to accept the truth, whether about Jesus or ourselves. For us to come to spiritual wholeness, then, we not only need the light (chapter 8), but we also need sight (chapter 9); we not only need Jesus' teachings, but also his healing touch.

The relevance of the PA is at once obvious, for not only has a woman been caught in sin, but its central lesson concerns how Jesus exposes the men who brought her to him as sinners too. If we may return to Keith's complaint about the PA not being placed immediately before the judgment verse of 8:15, we might reply by saying that the PA is placed before 8:12 because the "Light of the World" theme in 8:12 presents us with an even more important and immediate connection with the PA than the themes of judgment or Jesus' teachings (or the side-issue of Jesus' literacy).

The light versus darkness theme operates at a surprising number of levels in John 7 to 10: physically, morally and spiritually. In John 9:39-41 Jesus brings two of these levels of imagery together, connecting moral darkness (sin) and spiritual darkness (blind unbelief), when he says,

For judgment I have come into this world, that those who do not see may see, and that those who see may be made blind. Then some of the Pharisees who were with Him heard these words, and said to Him, "Are we blind also?" Jesus said to them, "If you were blind, you would have no sin; but now you say, 'We see'. Therefore your sin remains.

Remarkably, the PA manages to tie all three levels of this imagery into the account of the woman taken in adultery.

- Physically: Firstly, the fact that the woman had been caught in the act of adultery during the previous night and is





now brought before Jesus early in the morning ties in with this light versus darkness theme. Secondly, Jesus' desire to avoid arrest yet present his Messianic credentials leads to a tension between secrecy and openness. The PA is in keeping with the threat to Jesus' safety due to the Jews' continual attempts to arrest him or catch him in his words (7:30, 7:32, 7:44, 8:20, 8:37, 8:40, 8:59, 10:31 and 10:39). His initial refusal to speak in the PA, instead writing on the ground, is part of this tension between ambiguity and openness, cryptic sayings and loud cries, evasive and illuminating language that abounds in these chapters (cf. 7:14, 26, 34-36, 37, 8:21-22, 38-39, 9:39-41, 10:6, 34-36).

- **Morally:** The equation of light and darkness with good and evil is a Johannine theme, and indeed a familiar NT refrain.⁴⁷ John chapters 7 to 10 repeatedly mention the subject of sin. Thus, there are only two references to sin and sinners in John chapters 1 to 6, and eight references in John chapters 11 to 21, but in John chapters 8 and 9, there are fifteen references (excluding the PA), including John 8:21, 8:24 and 8:34. The moral connections between light, darkness, sin and the PA are obvious. To quote William Hendrikson, "The story fits very well into the present context. It can be viewed as serving to prepare for and to elucidate the discourse of the Lord in 8:12ff. Let it be borne in mind that this woman had been walking in moral *darkness*. It is probable that Jesus dispelled her darkness. So, we are not surprised to read in verse 12: 'I am the light of the world.'"⁴⁸ Jesus' famous statement in John 8:7 ("he that is without sin among you, let him cast the first stone") serves to "expose the deeds" (John 3:20) of the woman's accusers.

A related sub-theme connecting John 8 and the PA concerns the question of Jesus' sinlessness. In John 8, Christ twice asserts his sinlessness, firstly in John 8:29 ("I do always those things that please Him") and then in verse 46 ("Which of you convicts me of sin?"). Jesus' famous statement "he who is without sin among you, let him cast the first stone" also raises the issue of sinlessness, and his final words to the woman in the PA repeat the same standard: "Neither do I condemn you; go and sin no more" (John 8:11). This all suggests, in David Ellis' words, "an essential condition of his treatment of this woman is the certainty of his own sinlessness."⁴⁹ Carson follows Barrett in going even further, writing that the reason for the insertion of the PA in between John 7 and 8 "may have been to illustrate 7:24 and 8:15 or, conceivably, the Jews' sinfulness over against Jesus' sinlessness (8:21, 24, 46)."⁵⁰

- **Spiritually:** Here light equates to spiritual perception and darkness conversely equates to spiritual blindness. Greek (with $\epsilon\iota\delta\omega$) is not the only language where the word "seeing" often carries with it the "idea" of "knowing." This is seen particularly in chapter 9, but also throughout the four chapters, as certain people come to see who Jesus really is, while others remain obstinate in their unbelief. The subject of "knowing" features prominently in John chapters 7 to 10; out of the 137 references to "know" (and associates) in the Gospel, 44 are found in these chapters.

In keeping with the biblical idea that light equates with perceiving the truth,⁵¹ John 8 focuses heavily on the issue of "truth": there are twelve references to "true" or "truth" in the chapter,⁵² as well as a number of occurrences of the "truly, truly, I say to you" formula.

In the second half of John 8, Jesus patiently allows the Jews to come to a realization of the facts that they are slaves to sin and sons of the Devil. Just as in Chapter 9, the Jews show their spiritual blindness by obstinately fighting the light and arguing that they are not sinners or slaves but children of Abraham and God.

This episode bears a curious similarity to the way Jesus allows the woman's accusers in the PA to come to a realization of their own sinfulness. Jesus' patient exposure of people to the truth is therefore seen both in the PA and afterwards in the rest of chapter 8.

⁴⁷ John 3:19-20 speaks about how "men loved darkness rather than light, because their deeds were evil, for everyone practicing evil hates the light and does not come to the light, lest his deeds should be exposed." Romans 13:12 speaks of "the works of darkness," Ephesians 5:3-14 of "the unfruitful works of darkness ... shameful even to mention," and these themes are repeated in Philippians 2:15 and 1 John 1:5-10 and 2:9-11.

⁴⁸ W. Hendrikson, *A Commentary on the Gospel of John* (Grand Rapids, MI: Baker, 1970) 34, emphasis in original.

⁴⁹ D. J. Ellis, "John" in the *Zondervan Bible Commentary* (Grand Rapids, MI: Zondervan, 2008), 1237.

⁵⁰ Carson, *John*, 335; Barrett, *John*, 590.

⁵¹ The revelation of truth is often symbolized in the Bible by light, for example, in Daniel 2:22, where Nebuchadnezzar's private dream was revealed to Daniel: "He (God) reveals deep and secret things, He knows what is in the darkness and light dwells with Him." See also the interplay of light and truth in 1 John 1:5-10.

⁵² John 8:13, 14, 16, 17, 26, 32, 32, 40, 44, 44, 45, 46.





Hort's Temple Lampstand

Hort rejected any connection between the PA and John 8:12 (and also the words in 8:15 about not judging), writing, "the declaration 'I am the light of the world' has been supposed to be called forth by the effect of Christ's words on the conscience of the accusers: but in both cases the resemblances lie on the surface only."⁵³

However, there are four drawbacks with Hort's view. Firstly, contrary to Hort's allegation, the issues of physical, moral and spiritual light and darkness are not superficial; they are a deep-seated and multi-layered complex of concerns within John 7-10.

Secondly, Hort's argument also has the disadvantage that it burns any bridges connecting Jesus' words in 8:12 with the rest of chapter 8 in which Jesus exposes the Jews' sinfulness. The statement in 8:12 is thus reduced to a stand-alone, contextually-isolated comparison between Jesus and a temple lampstand. Hort's idea offers no connection with the context; it is an expository dead-end.

Thirdly, Hort's suggestion that Christ was comparing himself to a temple lampstand in John 8:12 becomes problematic once we look at the occasions that John employs the theme of light in his gospel. John appears to consistently use the illustration of the sun and daylight — not a lampstand — as the point of reference (see 1:4-9 with its creation overtones, 8:12 and 9:5 with their references to the light of the *world*, 11:9-10 with its reference to the twelve-hour day and 12:35-36 with its picture of the coming night). The idea that Jesus is comparing himself to a Temple lampstand in John 8:12 has little basis; John's gospel nowhere refers to any Temple lampstands.

The fourth problem with reducing Jesus' statement in 8:12 to a stand-alone reference to a temple lampstand, unrelated to anything else in chapter 8, is that every time Christ makes an "I AM" declaration in the gospel, the statement is illustrated by an action or is followed by an explanatory discourse. In John 9, for example, Christ's declaration that he is the "Light of the World" is immediately understandable and appropriate, for he gives the blind man sight. The action is suited to the declaration. The idea that the declaration in 8:12 is a stand-alone statement about a lamp-lighting ceremony, unaccompanied by any illustrative action and reinforced by no explanatory discourse, seems out of character. The PA, on the other hand, provides an illustrative action. This is then followed by the discourse in chapter 8 which reinforces the lesson of the PA: the exposure of human sinfulness. By contrast, any connection between a Tabernacles lampstand ceremony and human corruption seems tenuous and, dare we say, superficial.

The result is that Hort neither provides any substantiation from within John's gospel for the claim that the Light of the World declaration is an allusion to a temple lampstand, nor does he bother to interact with or argue against the idea that light equates with the exposure of moral corruption. Hort dismisses the idea as superficial without saying why, offering only assertion for argument. However, it is Hort's idea which is superficial, combining the appearance of an expository insight with the absence of its reality.

Theme Four: Jesus the Shepherd

The fourth main theme of John 7-10 is its presentation of Jesus as the shepherd. This is primarily seen in John 10, but the theme of Jesus the shepherd is also illustrated from John 9 with Jesus' shepherd-care for the blind man.

It is important to notice how much the shepherd's work involves speaking. In John 10:3, "the sheep hear his voice, and he calls his own sheep by name." Similarly, in verse 4, "the sheep follow him, for they know his voice." Altogether, the shepherd's (and false-shepherd's) voice is mentioned five times in John 10 (verses 3, 4, 5, 16, 27). John is returning us again to the theme of Jesus' words and teachings, connecting up with how the section began in John 7.

Thus, the four chapters present us with a progression of ideas. John 7 presents the brilliance of Jesus' teachings, John 8 presents Jesus as the teacher of righteousness preaching against sin and John 9 provides Jesus' miracle-work as the confirmation of his words. But John 10 adds another dimension: Jesus is the pastor-teacher. He calls his sheep to salvation, and his teachings lead them out into a new life of freedom and fullness.

Another way of comparing these chapters is to say that, whereas in John 7 and 8 Jesus preaches to the crowds, in John 9 and 10 Jesus pastors individuals. Of course, this division of material is not absolute; Jesus speaks to the crowds in chapter 10 (see verses 19-21).

⁵³ B. F. Westcott and F. J. A. Hort, *Notes on Select Readings*, 87.





One of the obvious messages of John 9 and 10 is the contrast between Jesus the shepherd and the tyranny of the Jewish leaders. Their treatment of the blind man in John 9 is the immediate reason for Jesus talking in John 10 about false-shepherds who only care for themselves (10:13), who seek to rob from the sheep, and even kill them (10:10).

Rather than caring for those under them, the Jewish leaders delight in treating their people harshly. The shepherd is instead characterized by selfless interest in others' well-being. Although the word "care" is only used once (in relation to the hireling who does not care for the sheep, 10:13), the implication is obvious and the principle is fundamental: the shepherd is someone who cares. He calls his sheep by name (10:4), he knows his sheep (10:14) and he lays down his life for the sheep (10:15).

Here is where we notice another connection with the PA. Jesus famously scandalized Jewish society by extending mercy to sinners. Lindars writes, "Jesus' compassion towards sinners is a presupposition of the [PA], without which verses 4-6 would be senseless."⁵⁴ Earlier, Lindars wrote of "the motif of special concern for the outcast."⁵⁵ He goes on to argue that the trap set for Jesus involved having to choose between upholding the Jewish Law and maintaining his own position (of mercy towards sinners). Schnackenburg says that "since Jesus shows mercy and love towards sinners, they want to see what he has to say on this concrete case."⁵⁶

Thus, Jesus as pastor involves a number of important concepts: (1) his interaction with individuals, (2) his special interest in outcasts and sinners, (3) his care, mercy and love shown towards them (instead of ostracism or condemnation), and (4) his words which bring healing. It would seem far from inappropriate to class the PA as an illustration of Jesus the shepherd of John 10. It contains all the elements, for it is the story of Jesus and an individual, a sinful outcast, whom he refused to judge, instead speaking words of mercy and guidance. Even though Jesus' interaction with the adulteress is more marked by confrontation than by care, nevertheless the pastor's ministry contains both these facets: conviction of sin as well as the comfort of grace.

Summary

The PA clearly harmonizes with the four main themes of John chapters 7 to 10: Jesus' brilliant words, judgment, light and shepherding. In addition, the PA intersects with numerous other minor themes and sub-themes of these chapters.

Finally, the PA magnifies other general Johannine themes like Jesus' deity (in the writing on the ground), Jesus not coming to judge but to save, Jesus versus Moses and law versus grace. The commentators, most of whom reject the PA as not originally part of John, nevertheless present us with numerous different explanations for the insertion of the PA after John 7:

- Schnackenburg's description of Jesus' words in the PA being "unique and unforgettable" hint at 7:46 with its "no man ever spoke like this man,"
- Keith suggests it demonstrates Jesus' divine authority to interpret the law of Moses,
- Numerous commentators present its genuine sense of fitness to the concept of judgment and particularly the Jesus' words, "I judge no one" in 8:15
- Hendriksen argues that it serves to prepare for and elucidate the light of the world statement,
- Barrett and Carson point to its illustration of Jesus' sinlessness over against the Jews' sinfulness,
- Schnackenburg's description of Jesus' mercy towards sinners hints at Jesus the Shepherd of John 10.

Every one of these contextual Johannine themes harmonizes with the PA and seems to present a valid reason for the inclusion at this point in John. The PA also manages to illustrate numerous verses in the immediate context: John 7:15, 7:46, 8:12, 8:15 and 8:46. Even the explanation that a later scribe was so sophisticated that he either adopted or crafted a story (Keith's theory requires that the very words are the interpolator's) and integrated it into John's Gospel after chapter 7 is still a de-facto admission that the PA is well-suited to its context.

The one thing we can say with certainty is that the commentators who repeat the old canard that the PA does not harmonize with the themes being developed in John 7 to 10 are incorrect, for they have somehow failed to see that the main thematic concerns of chapters 7 to 10 are exactly the same as the lessons reinforced and illustrated in the PA.

⁵⁴ Lindars, *John*, 308.

⁵⁵ Ibid., 306.

⁵⁶ Schnackenburg, *John*, 165.





Conclusion

The author visits a public school every week to mentor a child with behavioral problems. This mentoring activity might sound noble, but it usually involves little more than some simple activity designed to take the child's mind off problems at home and at school. On recent visits, we attempted some jigsaw puzzles together, and the author was taught a simple lesson in logic.

What should we deduce from the fact that a particular piece not only correctly physically interlocks and aligns with four adjoining pieces, but also is in keeping with their general color-scheme and matches the fine-details of the picture? The conclusion that any schoolchild draws is that he has correctly found a missing piece.

An academic, on reflection, might say that the intersection of so many independent avenues of evidence, each separately establishing that a particular piece correctly fits into the puzzle, overwhelmingly argues for the identification.

Here in the PA we have the intersection of seven features, each quite independent, each of which aligns with the setting in John's Gospel:

1. The numbers of singularly Johannine words in the PA match other Johannine narratives
2. The numbers of *hapax legomena* in the PA match other Johannine narratives
3. The PA provides a bridge (not an interruption) between 7:52 and 8:11
4. The theme of Jesus' brilliant, authoritative words and teachings matches John 7
5. The theme of non-judgement suits John 8 perfectly
6. The multi-faceted theme of light versus darkness is found in John 8 and 9, with the ideas of escape and entrapment, sin and righteousness, and spiritual blindness and perception
7. The theme of Jesus' personal, pastoral, care for sinners connects with John 10

Let us now change the analogy and choose some imagery more in keeping with the PA. It might be possible, if extremely unlikely, to hit two birds with one stone. The proverb suggests it is theoretically possible, and shooters have been known to kill two birds with one bullet. However, it would be much more difficult to hit seven targets with one stone, or even one bullet. In fact, if a shooter were to claim that he had managed to hit seven bulls-eyes with one bullet, we would probably suspect that the achievement had been manipulated: the targets had been set up and aligned in such a specific way as to permit one bullet to pass straight through all seven.

The fact that the PA has managed to hit seven targets with one shot, matching every stylistic and thematic feature that John's Gospel presents for comparison, seems too contrived to be good fortune. Nor does it appear to be the result of sophisticated interpolation for, like the shooter's seven targets, John's Gospel itself appears to have been designed with the PA in mind. The PA not only suits the Gospel, but the Gospel itself has been arranged to incorporate and align with the PA. The PA is rightfully to be seen as the central illustrative incident of John 7 and 8, the very keystone to the arch that John is building in these chapters.

Again, the textual experts will doubtless gnash their teeth at the suggestion, but one has to wonder why so many commentators include internal arguments like those we have surveyed among the reasons for the "overwhelming" non-originality of the PA. The rejection of the PA has become the academic *consensus communis*, but the constant repetition of flawed internal arguments suggests that the rejection is based on sociological rather than scientific reasons.

But still some will ask: How could the PA possibly be original, when so many of the "ancient and best authorities" omit it? Actually, here is where the group-think in modern text-critical thought is most pronounced and problematic. It is surely because the external evidence is considered so "overwhelming" in its opposition to the PA (despite the fact that, of the three main text-types, the Byzantine and Western both contain it) that the commentators recycle the half-truths concerning the internal evidence. It is the reverential regard for certain manuscripts that has rendered so many unconscious to the clear internal evidence of the fitness of the PA.

The reverential regard for certain ancient manuscript relics is of course accompanied by the relegation of the Byzantine text to the lowest level of textual credit-worthiness. This rejection, in turn, is based on the theory that the Byzantine text was the product of an official 3rd century doctoring of the text (a recension theory devoid of any credible, historically-documented evidence). Most textual critics still quietly maintain this incredible piece of historical fiction, for to abandon it would be to admit that the Byzantine text which dominated the Greek





heartland of the church from the 4th to the 15th centuries must have descended from yet earlier manuscripts, making it at least the equal of any other text-type.

“Byzantine Priority” theory instead holds that, just as in the analogous case of the Masoretic Text of the Hebrew OT, the Byzantine text which constitutes 90% of our manuscript evidence is the primary witness to the Greek New Testament text. Byzantine Priority theorists may profitably debate the extent to which this text-type represents the original text of the NT, but internal evidence in cases like the *Pericope Adulterae* gives confidence that, whatever its faults, the Byzantine text must be given its proper place at the high table of textual decision-making.

It is to Professor Robinson’s great credit that he has not been characterized by a text-critical herd-mentality. Instead, he has been prepared to champion worthy but unfashionable causes like the Byzantine text and the *Pericope Adulterae*. It is to be hoped that his example will inspire others to similarly think through the big textual issues for themselves.





‘BURNED UP’ OR ‘DISCOVERED’?

The Peculiar Textual Problem of 2 Peter 3:10d

by Paul A. Himes

Introduction

Second Peter 3:10 remains one of the most perplexing textual problems within the General Epistles. For one modern scholar, the passage “... is a curious text, not only because it is incomprehensible as it stands but also because of the range of variants in the manuscripts and emendations to the text proposed by modern commentators.”¹

Nevertheless, the particular Alexandrian reading found in \aleph and B, τὰ ἐν αὐτῇ ἔργα εὕρεθήσεται (future passive of εὕρισκω), “and the works in it will be found/discovered,”² is almost universally accepted by modern scholars despite significant divergence over interpretation.³

In other words, most scholars strongly argue that this reading is authentic, but not all agree as to what, exactly, it means. In contrast, the considerably easier Byzantine reading τὰ ἐν αὐτῇ ἔργα κατακαήσεται (future tense of κατακαίω), “and the works in it will be burned up,” is rarely defended in modern scholarship. Scholars often argue against the Byzantine (and other) readings on the basis of the superiority of the Alexandrian text (specifically \aleph and B). Far more often, however, the argument against the Byzantine reading draws from the axiom that “the more difficult reading is generally authentic.”⁴ Since “εὕρεθήσεται” is much more difficult to interpret in its context than “κατακαήσεται” the former (according to most textual critics) must be authentic.

Nevertheless, in this writer’s opinion a thorough examination of 2 Pet 3:10 will demonstrate both that the expression “the earth and the works in her will be found/discovered” does not make sense in the context (despite

- 1 G. Van Den Heever, “In Purifying Fire: World View and 2 Peter 3:10,” *Neot* 27 (1993): 107. Bruce Metzger, *A Textual Commentary on the Greek New Testament* (2nd ed.; Stuttgart: Deutsche Bibelgesellschaft, 1994), 636–637, seems to agree, stating, “. . . εὕρεθήσεται, though the oldest of the extant readings, seems to be devoid of meaning in the context . . .” before listing the various conjectural emendations.
- 2 All translations in this essay are the work of this writer, unless otherwise noted.
- 3 See, for example, the following commentaries: Richard J. Bauckham, *Jude, 2 Peter* (WBC 50; Waco, Tex.: Word Books, 1983), 303; Peter H. Davids, *The Letters of 2 Peter and Jude* (Pillar New Testament Commentary; Grand Rapids, Mich.: Eerdmans, 2006), 286; Karl Hermann Schelkle, *Die Petrusbriefe-Der Judasbrief* (HTKNT; Freiburg: Herder, 1970), 228; Anton Vögtle, *Der Judasbrief, Der Zweite Petrusbrief* (EKK 22; Solothurn: Benziger, 1994). A rare example of a modern scholar who seems to support the Byzantine reading is Edna Johnson, *Semantic Structure Analysis of 2 Peter* (Dallas, Tex.: Summer Institute of Linguistics, 1988), 156. To this writer’s surprise, however, modern Bible translations reflect more diversity and uncertainty on this reading than scholarship as a whole. For example, the NASB (1977; “the earth and its works will be burned up”; the same in the 1998 updated edition) and the German *Gute Nachricht Bibel* (2000; “und die Erde und alles, was auf ihr ist, wird zerschmelzen”) actually agree with the Byzantine! Indeed, the latter frankly admits that the Alexandrian reading, as it stands, is incomprehensible (fn a: “Die bestbezeugte lautet: *wird gefunden werden*, was ohne willkürliche Ergänzungen unverständlich ist”). On the other hand, the modern revision of the *Luther-Übersetzung* not only follows the Alexandrian, but makes more explicit what many modern scholars believe the clause implies (1999; “und die Erde und die Werke, die darauf sind, werden ihr Urteil finden”). Also surprisingly, the modern Japanese *Shinkaiyaku* (1973), which does *not* generally rely on the Byzantine or the TR, agrees with the Byzantine in this case by using the verb *yaki-tsukusaremasu*.
- 4 For various discussions of this principle, see the following: J. Harold Greenlee, *The Text of the New Testament: From Manuscript to Modern Edition* (Peabody, Mass.: Hendrickson, 2008), 59–63; Bruce M. Metzger and Bart D. Ehrman, *The Text of the New Testament: Its Transmission, Corruption, and Restoration* (4th ed.; New York: Oxford University Press, 2005), 300–303; Eugene E. Nida, “The ‘Harder Reading’ in Textual Criticism: An Application of the Second Law of Thermodynamics,” *The Bible Translator* 32 (January 1981): 101–107; Brooke Foss Westcott and Fenton John Anthony Hort, *The New Testament in the Original Greek* (New York: Macmillan, 1944), 542.





various scholarly attempts to clarify it) and that the argument from internal evidence is not quite so obviously in favor of the Alexandrian reading. Consequently, the Byzantine reading should be given serious consideration once again.

The first section of this paper will provide an overview of this variant and the readings of the various manuscripts while discussing why even modern eclectic scholars have regarded this passage as enigmatic.

The second section of this paper will survey and critique the various explanations of what, exactly, the phrase *εὑρεθήσεται* would mean in this context. The third section will advance a case for the Byzantine *κατακαήσεται*.⁵

The Problem of 2 Peter 3:10

The extent of the textual difficulty of 2 Pet 3:10 becomes obvious when one considers that the reading *εὑρεθήσεται* is given the extremely rare rank of “D” by the 4th edition of the United Bible Society’s *Greek New Testament*. In earlier editions, this would not be as significant, but 2 Pet 3:10 is just one of nine such “D” ranked variants in the entire 4th edition (1%, down from 125 “D” rankings from the last edition).⁶

The following two reasons account for much of the uncertainty: the divergence of the manuscripts and the difficulty of the reading “they will be found.”

On the one hand, the key 4th century Alexandrian manuscripts *Ⲁ* (aleph) and B both support a simple *εὑρεθήσεται*. The following significant manuscripts also support this reading: K (c. 9th century), P (9th century), 0156^{vid} (8th century), 1175 (10th century), 1739^{txt} (10th century), etc.⁷ For the Byzantine reading *κατακαήσεται*, however, we have the 5th century manuscripts A and 048, as well as 33 (9th century), 81^{vid} (11th century), 436 (11th or 12th century), 945 (11th century), 1739^{vtr} (10th century), in addition to the vast majority of manuscripts. Similar readings occur in the 9th century 2464 (*καήσονται*) and the later documents 1243 and 1735 (11th and 10th century, respectively; the reading here is *κατακαήσονται*). Surprisingly, no Greek manuscript contains *οὐχ εὑρεθήσεται*, although the equivalent does appear in the Sahidic Coptic (*cop^{sa}*). The majuscule Codex Ephraemi (“C”, 5th century) stands out from the rest with its unique reading of *ἀφανισθήσονται* (“they will disappear,” the future passive of the relatively rare *ἀφανίζω*).

The oldest manuscript on 1 Peter, *ⲑ⁷²*, also contains a unique reading, *εὑρεθήσεται λύόμενα*.⁸ Sakae Kubo calls this a “singular addition,” arguing that “the best thing to do at present is to maintain the reading of B until more light can be shed on the problem through further discoveries.”⁹

Most of the scholarly discussion, however, revolves around the internal evidence. Naturally, a truly thorough

- 5 In general, this writer will be comparing the Greek text of *The New Testament in the Original Greek: Byzantine Textform* (2005), eds. Maurice Robinson and William Pierpont (2nd ed.; Southborough, Mass.: Chilton, 2005) with the text found in *Novum Testamentum Graece*, ed. Barbara Aland, et. al. (27th rev. ed.; Stuttgart: Deutsche Bibelgesellschaft, 1993) and *The Greek New Testament*, ed. Barbara Aland, et. al., (4th revised ed.; Stuttgart: United Bible Societies and Deutsche Bibelgesellschaft, 1994).
- 6 Kent D. Clarke, *Textual Optimism: A Critique of the United Bible Societies’ Greek New Testament* (JSNTSS 138 Sheffield: Sheffield Academic, 1997), 91; note also on 114, chart 3, that this D rating has been consistent throughout every edition of the UBS. See also Clark, *Textual Optimism*, 200, appendix I.
- 7 The data in this paragraph is taken from the UBS *Greek New Testament* (4th ed.). For a complete list of all manuscripts supporting the various readings, however, see *Text und Textwert der Griechischen Handschriften des Neuen Testaments: I. Die Katholischen Briefe*, vol. 1, ed. Kurt Aland in conjunction with Annette Benduhn-Mertz and Gerd Mink (Berlin: Walter de Gruyter, 1987), 116–119. Note that *Text und Textwert* lists 16 total manuscripts as supporting the reading *εὑρεθήσεται* with one additional manuscript (398) reading *εὑρεθήσονται*.
- 8 Regarding the date of *ⲑ⁷²*, see especially the discussion by Sakae Kubo, *ⲑ⁷² and the Codex Vaticanus* (SD 27; Salt Lake City: University of Utah Press, 1965), 3. Kubo suggests a 3rd century date for *ⲑ⁷²* in contrast to a 4th century date for B (Vaticanus). See also Metzger and Ehrman, *The Text of the New Testament*, 58, and the apparatus of the UBS *Greek New Testament* (4th ed.).
- 9 Kubo, *ⲑ⁷² and the Codex Vaticanus*, 14–15, 16. Kubo does acknowledge the possibility that P⁷²’s *λύόμενα* might have been original yet omitted; he states, “The only worthwhile reasons that can be found for its omission is homoteleuton the *αι* [sic] of *εὑρεθήσεται* looking like *λυόμενα*.” He then proceeds to argue, however, that “it is hard to believe that this could have happened so generally” (16).





examination of the textual problem would hardly focus on internal evidence alone. Indeed, Ernest Colwell has aptly chastised the scholarship of the 20th century for its “deplorable condition” resulting from “the growing tendency to rely entirely on the internal evidence of readings, without serious consideration of documentary evidence.”¹⁰ Westcott and Hort bluntly state, “All decisions made solely or chiefly on the ground of internal evidence are subject to the chances of mistake inseparable from single and isolated judgments.”¹¹ Furthermore, the Byzantine priority theory that this writer and others hold to demands that one not take variant units in isolation, as Robinson has aptly argued.¹²

Nevertheless, there is not much left to debate on the external evidence for 1 Pet 3:10, and it is doubtful whether or not anything this writer could add would convince those holding to the mainstream view. Consequently, this writer will focus on internal evidence since there are issues here that have not been thoroughly explored. The most significant issue, of course, is that εὐρεθήσεται does not seem to make sense in the context. Taking the verse at face value, one is forced to ask, “How, exactly, can the works of the world be *found*, and why would that be a negative development?” Indeed, the very fact that this reading seems incomprehensible has driven many scholars to argue for its status as the original reading. R. Larry Overstreet, for example, admits that the Byzantine reading makes more sense, but then argues, “Because of this, it is easy to see how this verb [κατακαήσεται] could have crept into the text by an early scribe who also thought εὐρεθήσεται did not fit in with the context of the passage . . .”¹³ Consequently, most scholars prefer to interpret εὐρεθήσεται as a difficult verb that nevertheless can make sense within the context with enough thought. The next section of this essay, then, will examine and critique the various views on the meaning of εὐρεθήσεται.

Various Views on Εὐρεθήσεται

Conjectural Emendation

To begin with, a few scholars do, indeed, view εὐρεθήσεται as nonsensical and thus nonviable as a candidate for the original text. Even Brook Westcott and Fenton Hort were uncomfortable enough with 2 Pet 3:10d that they suggested, “Some primitive error probable; perhaps text for ῥύσεται, or some form of that stem.”¹⁴ Similarly, Heever bluntly states that 2 Pet 3:10 “is incomprehensible as it stands”; indeed, he suggests that “as early perhaps as the first half of the third century the original (?) meaning had already been lost.”¹⁵

Frederick W. Danker, like those scholars who view εὐρεθήσεται as original, acknowledges a “sense of judicial inquiry” within the semantic range of εὐρίσκω, as seen in such texts as Psalm of Solomon 17:10.¹⁶ In contrast with others, however, Danker aptly points out that a qualifying phrase would be necessary for the verb to make sense in 2 Pet 3:10. Thus, for Danker, “We are forced to the conclusion that B and **ⲛ** and their counterparts present a text already corrupted, but preserve the corruption with fidelity.” Subsequently, Danker proposes replacing the text of B and **ⲛ**, KAI ΓΗ KAI TA EN AYTH EPΓA EYPEΘΗΣΕΤΑΙ with KAI ΓΗ KATA TA EN AYTH EPΓA EYPEΘΗΣΕΤΑΙ (thus replacing καὶ τὰ with κατὰ τα).¹⁷

While it is beyond the scope of this paper to thoroughly discuss the issue of conjectural emendation, a few observations should be made. First, in light of the very robust transmission and preservation of biblical texts in antiquity (compared to many secular texts), this writer is not inclined towards the pessimism that characterizes Heever’s brief discussion of the textual issue.¹⁸ Secondly, this writer is hesitant to take seriously any conjectural

10 Ernest C. Colwell, *Studies in Methodology in Textual Criticism of the New Testament* (NTTS 9; Grand Rapids, Mich.: Eerdmans, 1969), 152; cf. also 153.

11 Westcott and Hort, *The New Testament in the Original Greek*, 542.

12 Maurice A. Robinson, “The Recensional Nature of the Alexandrian Text-Type: A Response to Selected Criticism of the Byzantine-Priority Theory,” *Faith and Mission* 11 (Fall 1993): 46–74.

13 R. Larry Overstreet, “A Stud of 2 Peter 3:10–13,” *BibSac* 137 (Oct–Dec 1980): 355.

14 Westcott and Hort, *The New Testament in the Original Greek*, 585.

15 Heever, “In Purifying Fire,” 107, 109. The question mark is Heever’s.

16 Frederick W. Danker, “II Peter 3:10 and Psalm of Solomon 17:10,” *ZNW* 53 (1962): 84–85.

17 Ibid., 85–86.

18 Heever, “In Purifying Fire,” 107, 109, and 116. Heever, at the end of his paper, states, “All that is now left for me to say is that I have to repeat: εὐρεθήσεται in this context is unintelligible, and all interpreters have, of necessity, to fall into its trap. But I have no way out, as yet.”





emendation, since by definition such proposals lack any shred of physical evidence.¹⁹ As for Danker's suggestion, one would think that it would have been more likely for the entire word ΚΑΤΑ (if original) to drop out rather than for the last two letters to drop out and be replaced by an Ι (*iota*).²⁰

As for Danker's comparison of 2 Peter 3:10 with Psalm of Solomon 17:10, one can fully acknowledge the judicial sense here (when the verb occurs in the same sentence as ἔργον), and both passages use the passive voice of εὐρίσκω. Yet in the latter, it is clearly evil men who are being judged, whereas in the former it is the earth.

The problem persists, then, as to how exactly the earth can be said to be "judged," especially when a *prima facie* reading of "the earth" seems to indicate the actual physical/geographical sense (this will be discussed more thoroughly below).²¹

Εὐρίσκω as a Metallurgical Expression

Other scholars take the reading of Ⲭ and Β at face value and suggest various interpretations. Al Wolters, for example, argues that εὐρεθήσεται can be read as "a metallurgical term appropriate to smelting and refining."²²

Wolters cites 1 Pet 1:7 and Barnabas 21:6 as an example where εὐρίσκω in the passive "is used in an absolute sense" and indicates "proving."²³

Wolters further observes,

It is striking that for the two occurrences of the absolute use in the letters of Peter the context in both cases evokes the image of a metal's purification in a melting pot or crucible. Could it be that the common Greek verb *heuriskesthai* has a precise technical sense in the vocabulary of the smelter and refiner? Its meaning would then be something like "emerge purified (from the crucible)," with the connotation of having stood the test, of being tried and true.²⁴

Wolters' comparison of 2 Pet 3:10 with 1 Pet 1:7 and Barnabas 21:6 is a valid point, but some key differences exist. In Barnabas 21:6, the admonition to be taught by God and to seek out what the Lord desires ("so that you may be found/discovered in the day of judgment") is clearly directed at men, and this makes very good sense regardless of whether or not one argues for the technical metallurgical sense that Wolters advocates.²⁵

Furthermore, in 1 Pet 1:7, it is the faith of believers that will be found/discovered, yet all ambiguity is dispensed with by the two qualifying prepositional phrases "into the praise and glory and honor" and "in [at the time of] the Revelation of Jesus Christ." This differs radically from 2 Pet 3:10, which does not contain any explanatory prepositional phrases and where the context contains a decidedly negative implication (i.e., whatever happening to the earth is not a good thing).

¹⁹ See Overstreet, "A Study of 2 Peter 3:10," 357, for his critique of Danker on this same point.

²⁰ To be fair, however, Danker, "II Peter 3:10," 86, argues that this is a relatively simple mistake that actually happened in 2 Peter 3:13 in the transmission of Codex Alexandrinus.

²¹ For a brief list of other proposed conjectural emendations, see Metzger, *Textual Commentary*, 637. Note also a "letter-to-the-editor" by George Milligan in response to Wilson's article in *ET* (Milligan, "2 Peter iii. 10," 32 [April 1921]: 331); Milligan alerts the reader to a conjectural emendation by Frank Olivier from the year before.

²² Al Wolters, "Worldview and Textual Criticism in 2 Peter 3:10," *WTJ* 49 (1987): 408.

²³ *Ibid.*, 411.

²⁴ *Ibid.*, 412.

²⁵ *Ibid.*, 412, declares that "A number of passages in extrabiblical Greek authors dealing with the refining of metals use *heurisko* in a way which is consistent with this hypothesis [i.e., that the word can have "a precise technical sense in the vocabulary of the smelter and refiner"]" In footnote 33, he cites Strabo 9.1.23 as one of the prime examples of such usage. Yet Strabo's usage, εὐρίσκον ἔτι ἐξ αὐτῆς ἀποκαθαίρομενον ἀργύριον, while admittedly within the context of a discussion of a metallurgical event, does not necessitate a technical usage because the normal usage still makes sense. One can still say that they "found/discovered" silver without necessarily having to refer to the extraction process itself (though Horace Jones' translation here does have "extracted"). It is quite possible that the author here is still focusing on the act of "discovery" of the purer silver via the smelting process rather than the technical act itself. Nevertheless Wolters' point should be considered more thoroughly by future lexicographers (for this discussion, the text of Strabo and Jones' translation were taken from *The Geography of Strabo*, vol. 4, in the Loeb Classical Library [ed. G. P. Goold; Cambridge, Mass.: Harvard University Press, 1988]).





This would go against Wolters' suggestion that a technical, metallurgical sense of εὐρίσκω "would be equivalent to the English 'to show one's mettle,'" a decidedly *positive* expression.²⁶ In other words, Wolters' suggestion implies a positive sense to the verb, whereas the context of 2 Pet 3:10 clearly implies a negative sense. Whatever happens to the earth and her works, they clearly will not be given their opportunity to "show their mettle."²⁷

Furthermore, a *prima facie* reading of 2 Pet 3:10 would seem to indicate that the verb refers to the earth and her works, not to humans and/or their faith. In other words, it is directed against inanimate objects, and this is what makes the verse difficult to understand. One can definitely see how men could be "found/discovered" in the judgment day of Barnabas 21:6, since the alternative (not to be found, i.e., to have vanished), is a frightening possibility. Yet what would it mean for the earth herself to be found? It would have to be the exact opposite of Barnabas 21:6 and 1 Peter 1:7, and the fact that there are no qualifying prepositional phrases makes the expression even more difficult. Thus Wolters' solution falls short, not because his suggestion of a metallurgical sense for the word is necessarily faulty (it may, indeed, be accurate, and this writer believes more scholars should look into the possibility), but because it is difficult to see how it could apply to 2 Pet 3:10, especially when there are no qualifying phrases to clarify.

Εὐρεθήσεται as a Rhetorical Question

A few scholars view the last part of 2 Pet 3:10 as a rhetorical question. Donald P. Senior and Daniel J. Harrington suggest that "The form *heurethesetai* can only be retained if one inserts the negative participle [sic] *ouk* ('will not be found') or takes it as a question ('will they be found?') that implies the earth and its works will be totally destroyed."²⁸

Similarly, J. N. D. Kelly builds on the work of B. Weiss and asserts that the expression must be a rhetorical question. He argues that εὐρίσκω in Scripture "frequently approximates to 'be' or 'exist', and when used in the negative or cast in the form of a question can convey the sense of non-existence . . ." He then cites Prov 31:10 (LXX) and 1 Pet 4:18 (citing Prov 11:31) as examples of rhetorical questions which parallel 2 Pet 3:10.²⁹ R. Larry Overstreet holds a similar position, arguing for a sense of judgment inherent in the text:

Peter is not making a declarative statement regarding the earth and the works therein, but is asking a solemn and thought-provoking question; therefore, the verse should be punctuated accordingly . . . This was a question to which Peter did not expect a simple yes or no answer, but a question designed to cause his readers to stop and ponder this cataclysmic event which was to come.³⁰

In response, it is perhaps significant that the primary motivation behind these three scholars to seek such an interpretation is their dissatisfaction with the statement as it stands. Indeed, Kelly even goes so far as to say, "Admittedly there are difficulties about this exegesis, notably the abrupt switch to an interrogation, but it has the virtues of making sense of what has every claim to the correct text . . ."³¹

Nonetheless, it must be admitted that their interpretation of the verb as a rhetorical "will the earth be found?" would mitigate this writer's earlier objections against Wolters, since such a rhetorical question would have the implied answer of "No, she will not be found"; i.e., the earth's situation would be clearly negative, as the context demands.

26 Wolters, "Worldview and Textual Criticism," 412. See the *Merriam-Webster Online Dictionary* (2012), s.v. "mettle," n. p. [cited 1 April 2012]. Online: <http://www.merriam-webster.com/dictionary/mettle?show=0&t=1333303229>, where "on one's mettle" is defined as "aroused to do one's best."

27 For a somewhat similar critique of Wolters' argument, see Heever, "In Purifying Fire," 109, where he argues, contra Wolters, that εὐρίσκω "is not used unqualified (that is, without an implied subject or predicate) in 1 Pt 1:7, 2 Pt 3:14 and Barnabas 21:6 as it is in 2 Pt 3:10. This makes the use of those texts as reference material for comparison questionable."

28 Donald P. Senior and Daniel J. Harrington, *1 Peter, Jude and 2 Peter* (Sacra Pagina 15; Collegville, Minn.: Liturgical, 2003), 289.

29 J. N. D. Kelly, *The Epistles of Peter and of Jude* (BNTC; Peabody, Mass.: Hendrickson, 1969), 365–366.

30 Overstreet, "A Study of 2 Peter 3:10–13," 356–358.

31 Kelly, *The Epistles of Peter and of Jude*, 366; cf. also Senior and Harrington, *1 Peter, Jude and 2 Peter*, 289, and Overstreet, "A Study of 2 Peter 3:10–13," 355.





Despite this concession, one must point out that the last phrase of 2 Pet 3:10 does not contain any of the interrogative particles that often occur in rhetorical questions. In Kelly's examples of Prov 31:10 and 1 Pet 4:18, for example, there are clear interrogative markers (τίς and ποῦ, respectively).³² Thus there is a major difference between 2 Pet 3:10 and the texts he cites, and his argument remains unconvincing. When this consideration is combined with Kelly's own admission concerning the unexpected switch to an interrogative, this writer concludes that 2 Pet 3:10 would not naturally come across as an interrogative sentence to the average reader or listener.

While such a proposal is intriguing, it ultimately falls short and is possibly the result primarily of a desire to avoid the clear confusion resulting from a *prima facie* reading of ⲁ and B.

Εὐρίσκω in the Sense of Judgment

Most scholars prefer to find a more general sense of "judgment" inherent in the term εὐρίσκω; i.e., for the earth to be "found/discovered" means that it will be "found in judgment." Nearly one hundred years ago William Wilson argued that εὐρεθήσεται was original because he thought it meant that "the earth and its works (*i.e.* men and their deeds) are laid bare before God." Wilson, drawing a parallel between 2 Pet 3:10 and Isa 2:19, Hos 10:8, and Rev 6:15–16, declares,

Just as these passages portray [sic] the wicked as dreading above all things the presence of God, and therefore desiring to be hidden from Him; so the author of 2 P. with a fine sense of climax makes the passing away of the heavens and the destruction of the intermediary spiritual beings, while terrible in themselves, even more terrible in that they lead up to the discovery, naked and unprotected of the earth, of men and all their works by God.³³

Similarly, J. W. Roberts sees a parallel between 2 Pet 3:10 and Romans 3:10 and notes the connection to the "judgment day" in both texts. For Roberts, "The sense is that everything in the world is to be destroyed and pass away. Only man will remain to be visible and account for his deeds in that great day."³⁴ Peter H. David concurs, arguing, "And that is the goal: to expose all that has gone on and is going on the earth so that all those things that human beings thought that they were getting away with or thought that God did not see are suddenly exposed to his unblinking eye."³⁵

Gene Green likewise sees the text in terms of "a judicial inquiry through which God will discover the deeds of humanity ..." while Hellmut Lenhard succinctly states, "Die gesamte sichtbare Welt vergeht, nur der Mensch muß sein Handeln vor Gott verantworten" ("The whole visible world passes away, but only mankind must answer for his actions before God").³⁶

Possibly the most significant treatment of this position, however, is that of Richard Bauckham. On the one hand, Bauckham admits that there are no exact parallels in the LXX of 2 Pet 3:10's use of εὐρίσκω.³⁷

On the other hand, he asserts,

However, although the OT usage provides no exact parallel to εὐρεθήσεται in 2 Pet 3:10, it is possible that general familiarity with the usage could have influenced the choice of words, either by the author of 2 Peter or by the author of his source.

At least it could provide the word with general judicial overtones, and when full weight is given to the passive form as a "divine passive," meaning "will be discovered by God," a plausible sense is obtained which is by no means such a

32 Cf. also a similar observation by Bauckham, *Jude, 2 Peter*, 318, regarding any attempt to draw a parallel with Prov 11:31. Overall, Bauckham sees the idea of a rhetorical question in this text as somewhat forced. It should also be noted that not all of the examples Kelly gives are rhetorical questions (e.g. Prov 20:6), though Kelly never claims that they are (Kelly, *The Epistles of Peter and of Jude*, 365–366).

33 William E. Wilson, "Εὐρεθήσεται in 2 Pet. iii. 10," *ExpTim* 32 (1921): 44.

34 J. W. Roberts, "A Note on the Meaning of 2 Peter 3:10d," *ResQ* 6 (1962): 33.

35 Davids, *The Letters of 2 Peter and Jude*, 286–287.

36 Gene L. Green, *Jude & 2 Peter* (Baker Exegetical Commentary on the New Testament; Grand Rapids, Mich.: Baker Academic, 2008), 330–331; Hellmut Lenhard, "Ein Beitrag zur Übersetzung von II Ptr 3 10 d," *ZNW* 52 (1961): 129.

37 Bauckham, *Jude, 2 Peter*, 318.





weak climax to the v as the English translation suggests.³⁸

Bauckham also sees a clear relationship between 2 Pet 3:10 and 2 Clem 16:3, where both parallel Mal 3:19 in the LXX. Indeed, “The puzzling final words of 2 Pet 3:10 are illuminated if 2 *Clem.* 16:3 represents their source . . .”³⁹ Furthermore, 2 Clem 16:3’s use of φαίνω may be an attempt to clarify 2 Pet 3:10’s use of εὐρίσκω, though Bauckham does not dogmatically assert this point.⁴⁰ Bauckham then proceeds to make the following argument from the context of 2 Peter:

The section 3:5–10 is by no means concerned solely with the Parousia as a cosmic dissolution, but is primarily concerned with the Parousia as judgment of the wicked. The destruction of the universe is of interest to the author only as the means of judgment on men and women. The previous reference to the coming conflagration, in v 7, concludes on the same note of judgment as, according to the proposed interpretation, v 10 does. In v 10 itself, the introductory reference to the thief requires that which follows to describe not simply a dissolution of the physical universe, but a judgment which threatens the unrepentant (see above). Similarly the succeeding vv (11–14) focus very explicitly on the moral dimension of eschatology. . . . In contrast to the wicked whose evil deeds will be “found” by God to their condemnation (v 10), 2 Peter’s readers are to strive to be “found” innocent (v 14).⁴¹

Regarding the possible parallel between 2 Pet 3:10 and 2 Clem 16:3, it can be acknowledged that 2 Clem 16:3 and 2 Pet 3:10 share similar themes (the “day of the Lord” in the latter is certainly parallel to “the day of judgment” in the former, and some of the terminology is similar), though even a cursory examination will show that this is not a direct citation. However, it is significant that the last clause of 2 Clem 16:3 (καὶ τότε φανήσεται τὰ κρύφια καὶ φανερᾷ ἔργα τῶν ἀνθρώπων), while evidencing a similar theme to 2 Pet 3:10d, bears very little lexical resemblance *either* to the last clause of 2 Pet 3:10 *or* to anything in LXX Mal 3:19. The words φαίνω, κρύφιος, φανερός, or ἄνθρωπος do not appear anywhere in 2 Pet 3:10; only ἔργον does, and at face value this is in reference to the works of the world, not the works of men (regardless of whether or not they refer the same thing). Thus there is very little lexical parallel between the last clauses of the two texts.⁴²

As a result, it becomes more unlikely that 2 Clem 16:3 deliberately substituted φαίνω for a confusing εὐρίσκω. Indeed, one could even argue that 2 Clem 16:3 is thematically more similar to Christ’s teaching in Luke 12:2–3, since here explicit reference is made to that which is hidden being made public (albeit with different terminology, though κρύφια is similar enough to κρυπτός). Thus until it is clearly established that 2 Peter and 2 Clement are parallel at this point (especially in the last line), and that their thematic similarities cannot be explained any other way, one cannot argue that φαίνω in 2 Clem 16:3 is explaining εὐρίσκω in 2 Pet 3:10, or that either of the two verses deliberately parallel LXX Mal 4:1.⁴³

Bauckham’s assertion that εὐρίσκω may possess judicial overtones, however, is certainly valid. David Wenham has written in support of Bauckham’s argument and points out that “Jesus’ eschatological parables refer on several occasions to the returning lord ‘finding’ his servants (Mt 24. 46/Lk 12. 43, Mk 13. 36, Lk 12. 37, 38). It is not specifically said that the master ‘finds’ the works of his servants, but this is certainly the meaning of the parables in question, since they refer to the master leaving his servants with tasks to do.”⁴⁴

38 Ibid., 319; emphasis is Bauckham’s.

39 Ibid., 304–305; see also 320–321.

40 Ibid., 320–321.

41 Ibid., 319–320.

42 Although Bauckham does not overly stress the possible connection between 2 Peter 3, 2 Clement 16, and Malachi 3, it should be pointed out that none of the above five words from 2 Clem 16:3 occur in the LXX of Malachi 3:19. True, Mal 3:19 does speak of “foreigners and those who do evil,” which has a similar theme to parts of 2 Clem 16 (as do a lot of other texts), but there is no mention of anybody’s works being exposed. The overarching theme of Mal 3:19 is the annihilation of evildoers, not their public judgment. It is this writer’s opinion that it is highly unlikely that the final clauses of either 2 Pet 3:10 or 2 Clem 16:3 owe anything to Malachi 3:19.

43 Contra Wolters, 411, who writes that “the verbal parallels with 2 Peter 3 [and 2 Clement] are unmistakable”). To be fair, Bauckham, *Jude, 2 Peter*, does not hold dogmatically to a direct parallel between 2 Peter and 2 Clement here. For further discussion of Bauckham’s theory, see Vogtle, *Der Judasbrief, Der Zweite Petrusbrief*, 237–238; Vogtle also briefly entertains the idea that the authors of both 2 Peter and 2 Clement may have had access to a source we do not possess today: “Beide Autoren könnten allenfalls eine nicht mehr rekonstruierbare gemeinsame Vorlage kennen” (238).

44 David Wenham, “Being ‘Found’ on the Last Day: New Light on 2 Peter 3. 10 and 2 Corinthians 5. 3,” *NTS* 33 (1987): 477; cf. also Jerome H. Neyrey, *2 Peter, Jude: A New Translation with Introduction and Commentary* (AB





Wenham and Bauckham both have a valid point, *if* the text in 2 Pet 3:10 referred to the works of men. Indeed, had the text of **8** and B read “the earth and works *of men* in her will be discovered,” it is highly unlikely that the UBS committee would still have given this a “D” reading! The problem, however, is that the works of the earth being revealed/judged does not make near as much sense as the works of the earth being destroyed. To rectify this, many scholars prefer to see “the earth and the works in her” as a reference to human deeds.⁴⁵

Thus Hellmut Lenhard, for example, argues that “the earth and her works” carries the implications of “historical events, accomplished deeds” [of men]⁴⁶ Roberts specifically follows Lenhard here, arguing that the mention of “the earth” in 2 Pet 3:10d becomes “either needlessly repetitious or it means something else besides the material substance.”⁴⁷ For Roberts, then, “The sense is that everything in the world is to be destroyed and pass away. Only man will remain to be visible and account for his deeds in that great day.”⁴⁸ Thus Roberts argues that “earth” here means “mankind,” for “in what sense does the material substance have ‘works’ or ‘deeds’?”⁴⁹

Yet one key fact prevents the interpretation of “earth” here as a reference to mankind. In Greek, at least within certain Jewish Hellenistic works, reference to the “works of the earth” seems to refer exclusively to *agriculture production or vegetation*.⁵⁰

While the exact expression that occurs in 2 Pet 3:10 does not occur elsewhere in the NT, the LXX, Josephus, or Philo, the closest expressions that do occur refer only to agriculture. Thus, for example, the only such reference to occur in the LXX, Jeremiah 14:4, contains the expression καὶ τὰ ἔργα τῆς γῆς ἐξέλιπεν, ὅτι οὐκ ἦν ὑετός (“and the works of the land died out, because there was no rain”), where clearly the physical earth (more specifically, a particular geographical region), not humanity, is in view, and “the works” refers to agricultural produce.

So far as this writer could determine, this is the only instance in both the NT and the LXX where “works” (ἔργον) and “earth” (γῆ) occur in any syntactical relationship even remotely similar to 2 Pet 3:10. However, similar expressions occur a few times in Josephus, specifically, *Ant* 5.132 (the Israelites “took care of the earth and her works” [τῆς δὲ γῆς καὶ τῶν ταύτης ἔργων ἐπεμελοῦντο]); 5.173 (after marrying, the Benjamites “took hold of the works of the land,” i.e., they began to farm); and 5.176 (the Israelites leave off warfare and devote themselves to agriculture).

Significantly, then, whenever “the works of the earth” are mentioned in the LXX and Josephus, they refer to agriculture. Conversely, this writer could not find a single instance where any reference to works that are in the earth meant “the deeds of humanity” or something similar. Consequently, to argue that 2 Pet 3:10d refers to the judgment of mankind’s works is to posit a usage that, so far as this writer could determine, does not occur in any

37C; New York: Doubleday, 1993), 243–244.

45 One noteworthy exception is Douglas J. Moo, *2 Peter, Jude* (The NIV Application Commentary; Grand Rapids, Mich.: Zondervan, 1996), 191. Here Moo argues that the physical earth is in view, but he still takes εὐρεθήσεται to refer to judgment. He states, “‘Heavens’ and ‘earth’ refer to the physical universe throughout this passage (see also vv. 5 and 7). And the continuation of Peter’s thought in verse 11 also suggests that physical dissolution has been his point in verse 10” (191).

46 Lenhard, “Ein Beitrag zur Übersetzung von II Ptr 3 10 d,” 129 (“Die ἔργα sind dann die auf der γῆ als Träger der historischen Ereignisse, vollbrachten Taten.”).

47 Roberts, “A Note on the Meaning,” 32.

48 Ibid., 33.

49 Ibid., 32.

50 For the data in this paragraph and for all lexical searches, this writer utilized *Accordance* 8.4 (OakTree Software: 2009). In order to determine the meaning of “the earth and the works in her,” this writer used the following command line in *Accordance*: “= γῆ <WITHIN 6 Words> = ἔργον”. Within *Accordance*, this writer searched the LXX (*Septuaginta*; ed. Alfred Rahlfs [Stuttgart: Deutsche Bibelgesellschaft, 2006]), Josephus (the 1890 Niese ed.), and Philo (*The Norwegian Philo Concordance Project*, eds. Peder Borgen, Kåre Fuglseth, Roald Skarsten [2005]). Naturally, not every hit referred to this kind of relationship between “earth” and works, and this writer focused solely on instances where the “works” are said to “belong” in some way to the earth (whether through the genitive case or through a prepositional phrase). Instances where the “works” clearly belong to people (e.g., Isa 60:21) are irrelevant to this writer’s argument, regardless of whether or not they refer to agricultural production. Similarly, in Josephus *Ant* 1.21, the two words appear side-by-side, but “works” here has “of God” following in the genitive case and thus is not the sort of construction we are concerned with. None of the four hits in Philo were relevant to the discussion. The exact construction that appears in 2 Pet 3:10 does not occur in the sources this writer searched, but this writer believes that the constructions are similar enough to be presented as evidence; whether a preposition phrase or the genitive case is used, we are discussing every instance where “works” (ἔργον) somehow belong to the “earth” (γῆ).





of the most significant Hellenistic Jewish writings at this time. The burden of proof, then, is those scholars who wish to demonstrate that the average hearer or reader of 2 Pet 3:10 would understand the expression that way rather than (as this writer argues) scratching their head in confusion at the concept of the earth's vegetation and agriculture being "found in judgment."⁵¹

The argument that the overall context of 2 Peter 3 refers to the judgment of sinners is valid, but cannot change the fact that "the earth and the works which are in her" would naturally be taken to idiomatically refer to agricultural produce or vegetation. To say that agricultural produce "will be found" or "will be judged" makes no sense, and thus the attempts of mainstream scholarship to clarify the primary Alexandrian reading remain unconvincing.

The Argument from Internal Evidence for Byzantine Reading

So far, this writer has attempted to demonstrate that none of the explanations of the Alexandrian reading make sense. It is unlikely, however, that reasoning from external evidence for the Byzantine reading would be enough to convince those not already drawn to the Byzantine text.

While this writer would ultimately hold to the priority of external considerations over internal, it is also this writer's contention that a significant argument can be made from the internal evidence alone for the Byzantine reading κατακαήσεται.

As noted above, references to "works" belonging to the "earth/land" in Jewish Greek invariably refer to agricultural production. Along with this fact, one can argue that the immediate context of 2 Pet 3:10 supports a reference to the destruction of the earth, not judgment of the wicked.⁵² Bauckham, however, counters that

... a reference to the judgment of the wicked is, in context, a more appropriate climax to v 10 ... It is true that in this context γῆ cannot be given the sense of 'humanity,' but it can easily mean the physical earth *as the scene of human history*, the earth as the dwelling place of humanity (cf. Matt 5:13; 10:34; Luke 12:49, 51; 18:8; John 17:4; and especially Rom 9:29). Given that the author is thinking, certainly, of a cosmic conflagration, but of a cosmic conflagration as the means of judgment on the wicked, this usage is entirely natural.⁵³

While Bauckham's point on the overall theme of "judgment on the wicked" is certainly valid, the *immediate* context of 2 Pet 3:10 itself clearly focuses on the destruction of physical elements.⁵⁴ Furthermore, none of the references Bauckham gives (e.g., Matt 5:13, etc.) mention "works" that belong to the earth, a description which we have demonstrated refers to vegetation and agricultural produce.

As far as internal evidence is concerned, the main issue is the principle that the more difficult reading is generally superior. Yet even if this principle is accepted as generally valid (and this writer is willing to acknowledge such, when strictly internal evidence is being considered), the fact remains that of all the options, κατακαήσεται is not the easiest possible reading. That honor of that title would go to the addition of οὐχ, a reading which is not found in any Greek manuscript but which has an equivalent in the Sahidic Coptic (cop^{sa}). It is thus difficult to understand why, if εὐρεθήσεται were the original reading, a perplexed scribe would not take the simple step of adding οὐχ to the verb instead of changing the extremely common εὐρίσκω to an entirely different word (the resulting "will *not* be found" would make clear sense as a reference to the earth's destruction). The problem is compounded when we consider that κατακαίω is a relatively rare word in the NT, only occurring 11 times (not counting 2 Pet 3:10)

51 Contra those who argue that εὐρεθήσεται is original, we may, perhaps, echo Heever's statement that "If εὐρεθήσεται did make sense, the major part of the tradition would not have found it necessary to change the text into something more intelligible" ("In Purifying Fire," 116).

52 Whether or not the actual *annihilation* of the earth is in view is, of course, a different discussion. For various treatments of the terminology in 2 Pet 3:10, see, for example, Davids, *The Letters of 2 Peter and Jude*, 284–286; Green, *Jude & 2 Peter*, 330; Kelly, *The Epistles of Peter and of Jude*, 364–365; and Lenhard, "Ein Beitrag zur Übersetzung von II Ptr 3 10 d," 128. In addition, see especially the interesting treatment by Wolters, "Worldview and Textual Criticism," 409–"The apostle is describing the Day of the Lord in the terms of cosmic elements which, as the result of intense heat, become incandescent and melt. They do not 'burn up,' as frequently imagined. To use the language of contemporary scientists in describing nuclear accidents, the future cataclysm is not a 'burnup' but a 'meltdown.'"

53 Bauckham, *Jude, 2 Peter*, 320.

54 Cf. also Kelly, *The Epistles of Peter and of Jude*, 365.





and only one other time in the general epistles (Heb 13:11). Why, then, would a scribe chose such a word, a word that does not occur elsewhere in either Petrine epistle or Jude, when that scribe could have simply added the much more common λύω (as does P⁷²), the term καυσόω (which occurs twice in 2 Peter but only in 2 Peter), or a much simpler οὐχ (all three options being able to provide more or less the same sense as the Byzantine reading)?

In light of the fact that the author of 2 Peter prefers “complicated, eccentric Greek” and that “the vocabulary in the letter is unusual and individual” with a relatively high rate of *hapax legomena*,⁵⁵ the use of a rare word such as κατακαίω would not be difficult to explain if it was original, but it would be odd indeed for a scribe to gravitate to this vocabulary. Thus the Byzantine reading actually becomes somewhat difficult to explain, if one grants the assumption that scribes are less likely to use rare words.⁵⁶

If one starts with the Byzantine reading, however, it is not implausible to suggest that a scribe deliberately simplified the rare κατακαίω to the less harsh P⁷² reading (εὐρεθήσεται λυόμενα), a reading which would basically say the same thing as the Byzantine reading, only less dramatically.⁵⁷ From there, the λυόμενα could accidentally drop off and thus give rise to the handful of Alexandrian texts which contain only εὐρεθήσεται, which in turn would give rise to the Coptic version’s “they will *not* be found” while the original κατακαίησεται continues on elsewhere.⁵⁸ The perplexing lack of οὐκ εὐρεθήσεται, which would have been abundant had εὐρεθήσεται been the original reading and perplexed scribes scrambled to make sense of it, is explained away due to the relatively small number of manuscripts that have followed an accidental emendation of P⁷² (i.e., the dropping of λυόμενα).

This, then, provides a hypothetical scenario that could explain both the Byzantine reading as original and the rise of the Alexandrian reading at an early stage in the text’s transmission.

In summary, then, the following points support the Byzantine reading: 1. The expression “works of the earth” refers to agricultural production (whether natural or manmade) and this cannot possibly make sense if paired with εὐρίσκω in 2 Pet 3:10d; 2. The immediate context of 2 Pet 3:10 argues for a reference to the physical earth, not humanity; and 3. The argument from “the more difficult reading” does not support the Alexandrian reading as strongly as most scholars hold, since a scribal emendation to κατακαίω is difficult to explain when other, simpler options existed.

Conclusion

It is not without good cause that scholars and translators have struggled with the odd textual variant in 2 Pet 3:10d. A thorough examination of the terminology, however, has demonstrated that attempts of scholars to make sense of the UBS reading have not been successful. The expression “the earth and the works in her” must refer to the

55 Lauri Thurén, “Style Never Goes Out of Fashion: 2 Peter Re-Evaluated,” in *Rhetoric, Scripture and Theology: Essays from the 1994 Pretoria Conference* (eds. Stanely E. Porter and Thomas H. Olbricht; JSNTSS 131; Sheffield: Sheffield Academic, 1996).

56 A similar argument is used by Peter J. Williams, “An Examination of Ehrman’s Case for ὀργισθεῖς in Mark 1:41,” *Novum Testamentum* 54 (2012): 7–8, though it is fair to point out that Williams’ argument deals with the choice of two different words and hinges on the commonness of works ending in “ισθεις” in the NT and the LXX. In contrast, this writer’s argument is much more general, namely that a scribe would more readily add a 3-letter word or a much more common verb than replace a common verb with a relatively rare κατακαίω. Note also, as Williams points out, Bart D. Ehrman’s statement in *The Orthodox Corruption of Scripture: The Effect of Early Christological Controversies on the Text of the New Testament* (Oxford: Oxford University Press, 1993), 147, that “... readers typically confuse unusual words for common ones and make simple what is complex, especially when their minds have partially strayed.” This writer is virtually certain that Ehrman would not approve of the application of his statement to 2 Pet 3:10d, but at least it should be considered (i.e., that a scribe would have replaced a more difficult κατακαίω with simpler words).

57 I am indebted to a private conversation with Dr. Robinson for stimulating my thought on this particular point, though Dr. Robinson does not necessarily hold to the same views as I hold on the development of the various textual traditions after κατακαίησεται.

58 Although Schelkle ultimately agrees with the primarily Alexandrian reading, he does briefly raise the possibility of how the λυόμενα might have dropped off (*Die-Petrusbriefe-Der Judasbrief*, 228 fn3). Regarding P⁷², he asks, “Ist bereits dieser-durchaus sinnvolle Text-eine erleichternde Korrektur, oder ist er vielleicht ursprünglich, und ist etwa daraus λυόμενα später durch Haplographie neben λυθήσεται (V11) und λυομένων (V 12) ausgefallen?” [sic: Schelkle has apparently reversed the forms of λύω in verses 11 and 12]





physical earth and her agricultural produce, based both on the context of 2 Pet 3:10 and the use of the expression “the earth and her works” by Jewish authors or translators writing in Greek. Furthermore, the argument from the more complex reading so often utilized in defense of εὐρίσκω in this passage is not as convincing as it would first appear. The Byzantine reading, then, deserves another look by modern scholarship.

Nevertheless, as many scholars have acknowledged, the apocalyptic imagery in 2 Pet 3:10 functions as a powerful paraenesis designed to shake the hearers and readers out of complacency. The destruction of the earth and all that is in her remains a sober reminder for the listener that the current era will someday come to an end, yet even now opportunity exists to embrace the patience and salvation of the merciful Lord Jesus.⁵⁹

59 I am grateful for the opportunity to contribute to a recognition of Dr. Maurice Robinson’s work. I am grateful for the friendship, encouragement, and interaction with me on this topic of both Dr. Robinson and my father, John R. Himes. I am also grateful to my friend David Barnhart for some assistance when working with German sources (though all translations are the final work of this writer). Any mistakes, faulty logic, or mis-translations are the sole responsibility of this writer.





ARGUMENTS FOR AND AGAINST THE BYZANTINE AND ALEXANDRIAN TEXT TYPES

by T. David Andersen

As a Bible translator and translation consultant, when the Greek text has variant readings, I need to recommend to my co-workers which reading should be followed in the translation. Until recently, I assumed that I could trust the United Bible Societies Greek New Testament to give a reliable evaluation of the relative merit of variant readings. Recently, however, my faith has been shaken by reading and discussing arguments in favour of the Byzantine text type as being closer to the original autographs than the Alexandrian text type which is favoured by the editors of the eclectic text found in the UBS Greek New Testament.¹ As a way of clarifying my thinking on this topic, I have attempted to set out a number of arguments for and against the Byzantine and Alexandrian text types with regard to which one is closer to the original autographs.

The primary audience of this paper is myself. It is a working paper intended to document my growing understanding of this issue. Because of this, I have not hesitated to include quotations from unpublished sources, including personal emails, if these represent the clearest expressions of certain arguments. Because my access to recent scholarship is somewhat limited, some of the sources cited are rather dated. As the writing process continues, I hope to increase the citations of recent published sources relevant to the questions raised.

Competing text types

Among the manuscripts of the Greek New Testament there are four major text types commonly recognized: the Byzantine, the Alexandrian, the Western, and the Caesarean. This paper focuses on the question as to which of the major text types is closer to autographs written by the authors of the New Testament.

Each text type is represented by a group of manuscripts quite similar to each other, but significantly different from manuscripts of the other text types. Some of the differences between text types could well be due to an accumulation of accidental scribal errors as manuscripts were copied and recopied.

However, the degree of difference between these text types is great enough that it cannot be explained by accidental errors only. There are numerous places where whole sentences are included in one text type and omitted from another. There are even whole passages included in one text type and omitted from another (e.g. the story of the woman caught in adultery). These sort of differences can best be accounted for by the hypothesis that there was a deliberate scribal effort to edit the text. Either certain sentences and passages were deliberately added, or they were deliberately deleted. There are also other types of alterations of words which seem best explained as being the result of deliberate editing rather than accidental errors.

This leads to the hypothesis that one of the text types represents a relatively accurate reflection of the originals, whereas the others are reflections of a later edited manuscript or manuscripts. Robinson

¹ Some of the arguments in favour of the Byzantine text are set out in an unpublished paper by Timothy Friberg entitled "A modest explanation for the layman of ideas related to determining the text of the Greek New Testament: a minority view." That paper summarizes arguments found in Robinson (2001). I am also indebted to an unpublished paper by Robinson entitled "A brief reply to Dr. Deibler's questions."





says (2009a:9) “The one thing that is certain is that all three major texttypes (Alexandrian, Western, Byzantine) cannot all be the direct descendant from the autograph, and therefore either one texttype only must represent the autograph, with the others reflecting recensions; *or* none of the existing texttypes represent the autograph, and are all later hyparchetypal developments, with whatever the autograph may have been being totally ‘lost’ and dispersed equally or unevenly among the three existing major texttypes.”

Byzantine text type

The Byzantine text type is represented by the following manuscripts according to Metzger and Ehrman (2005:306-307). The dates and other information are taken from Metzger and Ehrman (2005:67-86) or Aland et al (1983:xiii-xxvii). The manuscripts are listed according to date.

- W (in Matthew and Luke 8:13-24:53), late 4th to early 5th cent.
- A (Codex Alexandrinus) (in gospels), 5th cent.
- P (Gospels), 6th cent.
- E (Gospels), 8th cent.
- F (Gospels), 9th cent.
- G (Gospels), 9th cent.
- H (Gospels), 9th cent.
- K (Gospels), 9th cent.
- V (Gospels), 9th cent.
- Π (Gospels), 9th cent.
- Ω (Gospels), 9th cent.
- H^a (Acts), 9th cent.
- L^{ap} (Acts, Epistles), 9th cent.
- P^a (Acts), 9th cent.
- 049 (Acts, Epistles), 9th cent.
- Ψ (in Luke and John), 9th or 10th cent.
- S (Gospels), A.D. 949.
- 046 (Revelation), 10th cent.
- 051 (Revelation), 10th cent.
- 052 (Revelation), 10th cent.
- Most minuscules (out of 2877 manuscripts), 9th to 18th cent. (numbers from Metzger and Ehrman 2005:50)

To this list should be added:

- ℙ⁸⁴, 6th cent. (Epp 1992:431)
- ℙ⁶⁸ (Pauline Epistles), 7th cent. (Epp 1992:431)
- ℙ⁴², 7th-8th cent. (Epp 1992:431)
- Δ(in Matthew, Luke, John), 9th cent. Metzger and Ehrman (2005:82) state, “In Mark, its text belongs to the Alexandrian type...; in the other Gospels, however, it belongs to the ordinary Koine or Byzantine type.”
- Θ(in Matthew, Luke, and John), 9th cent. Metzger and Ehrman (2005:83) state, “In Matthew, Luke, and John, the text is similar to most Byzantine manuscripts.”
- Most lectionaries (out of 2432 manuscripts) (numbers from Metzger and Ehrman 2005:50).





Arguments for and against the Byzantine text type

Since the time of Westcott and Hort in the nineteenth century, a strong case has been made to suggest that the Byzantine text type derives from a relatively late highly-edited manuscript. A number of arguments have been put forward to suggest that the Byzantine text type is significantly different from the original text. A number of scholars have contested these claims and put forward arguments to maintain that the Byzantine text type is a closer representation of the original than the other two main text types. This is sometimes called the Byzantine priority theory. The main arguments and counterarguments are set out below.

Lack of early manuscripts

Argument against Byzantine text type

The earliest manuscript which can be characterized as belonging to the Byzantine text type is the uncial manuscript W dated to the late fourth or early fifth century. Metzger and Ehrman (2005:80) state that “In Matthew and Luke 8.13-24.53 the text is of the common Byzantine variety.”

Carson (1979:44) states, “There is no unambiguous evidence that the Byzantine text-type was known before the middle of the fourth century”. No Byzantine manuscript exists before the late fourth century.

This is hard to explain if the Byzantine text type is an accurate reflection of the original autographs. If that were the case it should have been widely copied in various geographical areas from the first century onwards.

Counterargument

Although no Byzantine manuscripts occur before the late fourth century, Byzantine readings are found in early papyri. Sturz (1984) presented evidence to show that 150 typical Byzantine readings in various passages throughout the New Testament are supported by early papyri.

He concluded that “the Byzantine text derives from at least the second century and represents a stream of tradition independent of other early traditions.” (Metzger and Ehrman, 2005:221). In addition, Zuntz made similar claims regarding Byzantine readings in \mathfrak{P}^{46} , and Fee equally in regard to \mathfrak{P}^{66} (Robinson 2009a:11). Robinson also states (2009a:11) that “Colwell, among others, specifically acknowledged that *all* sensible readings that have enjoyed repeated transmission over the centuries were likely in existence prior to AD 200.” He adds (2009a:12) “researchers at the Aland Institut in Münster (specifically in recent papers by Klaus Wachtel and the Institut’s director Holger Strutwolf, the latter most recently at the last SBL meeting in New Orleans) now acknowledge that the Byzantine Textform has ancient origins, equal in date to the earliest papyri; and it is this consideration that has caused them to reevaluate the Byzantine testimony throughout the NT, even while continuing to favor a predominantly Alexandrian text.”

Rebuttal to counterargument

In responding to the of evidence Sturz (1984) regarding the 150 typical Byzantine readings found in early papyri, Metzger and Ehrman (2005:222) state, “Unfortunately, few of the 150 readings that Sturz lists are distinctively Byzantine; most of them have significant non-Byzantine witnesses supporting them as well. Moreover, one must also ask whether the evidence of this or that Byzantine reading among early papyri demonstrates the existence of the Byzantine text type. A text type involves a particular constellation of readings in a characteristic pattern, and the fact is that not one of the papyri collated by Sturz can be characterized as Byzantine in the text that it presents.”





Rebuttal of rebuttal

Robinson (2001:§8), states, “One of the complaints against the Byzantine Textform has been that such could not have existed at an early date due to the lack of a single pre-fourth century MS reflecting the specific *pattern of agreement* characteristic of that Textform, even though the Byzantine Textform *can* demonstrate its specific pattern within the vast majority of witnesses from at least the fourth century onward. Yet those who use the modern eclectic texts are expected to accept a proffered ‘original’ which similarly lacks any pattern of agreement over even a short stretch of text that would link it with what is found in *any* MS, group of MSS, version, or patristic witness in the *entire* manuscript tradition. Such remains a perpetual crux for the ‘original’ text of modern eclecticism. If a legitimate critique can be made against the Byzantine Textform because early witnesses fail to reflect its specific pattern of readings, the current eclectic models (regardless of edition) can be criticized more severely, since their resultant texts demonstrate a pattern of readings even *less* attested among the extant witnesses.”

Discussion

It is unclear to me exactly how these statements support or refute the hypothesis that the Byzantine text type is closest to the original. If it were, what hypothesis of textual transmission would explain the scattering of Byzantine readings among various papyri? If the Byzantine text type is a late recensional text type, what hypothesis of textual transmission would explain how these early readings are found in it? Does the fact that these readings are also found in non-Byzantine witnesses tend to support or refute the claim that the Byzantine text type is the closest to the original. Why or why not?

Lack of attestation in early patristic writings

Argument against Byzantine text type

Church Fathers writing in the second and third centuries do not quote typically Byzantine readings when they quote Scripture. Metzger says (2005:222), “patristic writers prior to Basil the Great and Chrysostom show no acquaintance with the Byzantine text.”

Counterargument

Robinson and Pierpont (1991) state that, “the common practice among patristic scholars is to dismiss distinctively Byzantine readings found in the writings of the Fathers unless the Father expressly comments on the significance of the Byzantine reading. This is due to their hypothesis that the scribes (who also copied the works of the Fathers as well as the New Testament manuscripts) would habitually and deliberately tend to alter the scriptural quotations of the Fathers into those with which they were familiar, namely, the Byzantine readings.... If the Byzantine readings now summarily dismissed in the early Fathers were legitimately included, the Fathers’ overall text would be seen as more ‘Byzantine’ than current scholarly opinion claims.”

Robinson and Pierpont (1991) state, “Early Fathers quoted a ‘mixed bag’ of Alexandrian, Western, and commonly shared readings with the Byzantine text.” They add that among modern textual critics, “Those readings which are supported by a Byzantine-Alexandrian combination are termed ‘Alexandrian,’ and are considered to have been ‘later’ incorporated into the emerging Byzantine text. Likewise, readings supported by a Byzantine-Western combination are considered solely ‘Western,’ later adopted by the Byzantine-era scribes From the present perspective, the Byzantine-Alexandrian and Byzantine-Western alignments are merely those autograph readings of the Byzantine Textform from which the Alexandrian or Western manuscripts did not deviate -- a very different picture.” Robinson (2009b) says, “as Burgon/Miller have shown, when these commonly shared readings are counted as Byzantine (for the sake of the hypothesis), the proportion of Byzantine to non-Byzantine readings in the pre-4th century period is 3:2.”





In addition to the above arguments, patristic writings from the primary Greek-speaking areas of the Roman Empire give strong support to the Byzantine text type. Robinson says (2009a:14): “as soon as writing fathers appear in the primary Greek-speaking region (S. Italy, Greece, Turkey) from the fourth century onward (there are *no* such fathers in that region prior to the 4th century that offer anything of text-critical importance; cf. Ignatius as an example), the text they are using is *not* the Alexandrian, *nor* the Western, but the Byzantine.”

A pattern suggestive of deliberate editing

Argument against Byzantine text type

The differences characteristic of the Byzantine text type exhibit a regular pattern which suggests that they are the result of deliberate editing. Metzger (1994:7) says, “The framers of this text sought to smooth away any harshness of language, to combine two or more divergent readings into one expanded reading (called conflation), and to harmonize divergent parallel passages.” Carson (1979:52) refers to a particular section of the Synoptic Gospels “in which the Byzantine text contains some thirty-eight major harmonizations, as compared with one harmonization in the Alexandrian text...The only way to circumvent the evidence is to deny that they are harmonizations, or to argue that harmonizations are not secondary; and I find it very difficult to conceive how either of these alternatives can be defended by the person who has spent much time poring over the primary data.”

Counterargument

Robinson (2009b) states, “Since Carson made that 1978 claim, the published dissertation research of Wisselink (‘Assimilation as a Criterion for NT Textual Criticism’) has convincingly demonstrated that in fact the so-called harmonizations (which he called ‘assimilations’) are not necessarily secondary, and that such alleged cases actually occur in near-equal proportion among all texttypes.”

Robinson states (2001:§34), “When harmonization or assimilation did occur, it was sporadic. The MSS which systematically harmonized to parallel passages were few (the scribes of Codex Bezae and various Caesarean witnesses are more typically harmonistic than what is alleged against Byzantine scribes). While certain Byzantine readings *may* appear to harmonize at various points, it would be a fallacy to charge the Byzantine scribes with a harmonistic tendency for the following reasons:

- (a) the Byzantine MSS fail to harmonize in most situations;
- (b) the alleged harmonizations within the Byzantine Textform are relatively infrequent;
- (c) alleged Byzantine harmonization often fails to conform precisely to the parallel passage; and
- (d) the Byzantine scribes *fail* to harmonize in hundreds of places where a minority of supposedly earlier MSS had created highly persuasive and attractive harmonizations.”

The inferiority of Byzantine readings based on internal evidence

The strength of a variant reading can be evaluated by criteria of internal evidence. These include, among others:

- (a) A reading from which all the other variants could plausibly have been derived is more likely to be original.
- (b) The more difficult reading is more likely to be original. This is because it is more likely a scribe would make a hard-to-understand reading clearer, than to make a perfectly clear reading difficult to understand.





- (c) The shorter reading is more likely to be original, unless there is a clear reason which would have caused a scribe to delete certain words. Such reasons include: (1) When two nearby words start or end with the same letters, thus causing a scribe's eyes to skip from one to the other and delete the words in between; (2) Deleting words possibly regarded as superfluous, harsh, or contrary to pious belief or practice.
- (d) In parallel passages, a reading in which the two passages are worded differently is more likely to be original than a reading in which the two passages are worded identically. This is because the identical reading may have been a deliberate harmonization.

According to Westcott and Hort, based on such criteria, "when the Syrian [that is, Byzantine] readings are compared with the rival readings, their claim to be regarded as original is found gradually to diminish and at last to disappear." (Metzger and Ehrman, 2005:181)

Counterargument

Robinson and Pierpont state (1991), "Byzantine-priority advocates maintain that a successful internal-evidence case can be made for nearly every Byzantine reading over against the Western, Caesarean, and Alexandrian readings."

They cite Kilpatrick (1965) and Elliott (1972) in support of this statement. They also refer to "any other articles by Kilpatrick or Elliott which favor the 'rigorously eclectic' methodology, and as a result defend on internal principles the authenticity of many 'distinctively Byzantine' readings."

Original readings tend to predominate

Argument in favour of Byzantine text type

When trying to determine the original text of the New Testament, a major consideration should be the actual number of manuscripts supporting a particular reading. This is because, when multiple copies are made of a manuscript, each manuscript will correctly reproduce the original most of the time. Errors are the exception, not the rule. When one copy introduces an error in a particular verse, it is a minority of one compared to the other copies which accurately copied that verse.

The error will be reproduced in the next generation of copies made from the manuscript with the error, but the correct reading will be reproduced in even greater quantity in the next generation of copies made from the accurate copies. Hence when a large majority of manuscripts agree on a reading, it is likely to be original, whereas if only a few manuscripts support a reading, it is likely to be an introduced error. Robinson (2009a:6) quotes the dictum of Hort: "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*." Robinson states (2001:§40) "The chances that any sensible alteration subsequent to the autograph would extend beyond a small group of localized witnesses would be slim. Indeed, such readings as characterize minority texttype witnesses generally remain small and localized. That any deliberate alteration or transcriptional error would gain the cooperation of scribes so as to dominate the entire stream of transmission is a null proposition: scribes demonstrably did not engage in such a practice on the grand scale." The fact that the vast majority of manuscripts belong to the Byzantine text type is most easily explained by the hypothesis that the Byzantine text type is the one that most accurately reflects the originals.

A further development of this argument is that the dominance of original readings will be greatest in the region where the autographs were located, and will tend to decrease as the distance from that location increases. Robinson (2001:§77) states, "It is reasonable to suppose that, as texts spread geographically from their initial locale, regional alteration would increase proportionally to distance. This is especially the case given the 'uncontrolled popular text' phenomenon of the early centuries. Copies produced





within a close proximity to the site of origin or initial reception of a given text would be expected to retain a more uniform textual complexion closely resembling that of the autograph; this would occur *without* the imposition of formal ‘controls’ upon the copying or dissemination of the text. Copies produced at a more remote distance from the site of origin would tend to diverge in greater quantity. If such a hypothesis is correct, the primary Greek-speaking region during the period of ‘geographical silence’ would be expected to retain a Byzantine text, just as other localized regions preserved their disparate texts in the European and African West as well as in Egypt and Palestine.”

Relative uniformity of Byzantine manuscripts

Argument in favour of Byzantine text type

The relative uniformity of Byzantine manuscripts is due to the fact that they are relatively accurate reflections of the original autographs.

Counterargument

Metzger and Ehrman state (2005:307 fn. 12), “The Byzantine text of the Book of Revelation is less homogeneous than it is in other books of the New Testament, for the Greek Orthodox Church has never included readings from the Apocalypse in its lectionary system — a system that exerted a stabilizing influence on the Byzantine text of the other books of the New Testament.” This suggests that the relative homogeneity of the Byzantine text type is due to the later influence of the lectionary system.

Discussion

It is not clear exactly how uniform or diverse the Byzantine text type is. Robinson (2009b) refers to “its diversity in its numerous subtypes.” Robinson (2001:§91) quotes Scrivener (*Augiensis* xiii), who says, “No one who has at all studied the cursive MSS. can fail to be struck with the *individual character* impressed on almost every one of them... The fancy which was once taken up, that there existed a standard Constantinopolitan text, to which all copies written within the limits of that Patriarchate were conformed, has been [quoting Tregelles] “swept away at once and forever...by a closer examination of the copies themselves.” It would be helpful to have some figures on the percentage of agreement among various Byzantine subtypes, and compare that to the percentage of agreement among Alexandrian subtypes and among Western subtypes.

Byzantine manuscripts not related genealogically

Argument in favour of Byzantine text type

Robinson and Pierpont (1991) state, “except for a few small ‘family’ relationships which have been established, the bulk of the Byzantine-era documents are not closely-related in any genealogical sense. A presumption, therefore, is toward their relative independence from each other rather than their dependence upon one another. This makes the Byzantine majority of manuscripts highly individualistic witnesses which cannot be summarily lumped together as one ‘mere’ texttype, to be played off against other competing texttypes.” They cite Lake, Blake, and New (1928) in support of this statement, based on their examination of several hundred manuscripts in three monasteries.

Discussion

It is unclear to me how much evidence there is regarding the genealogical relationship or lack thereof between Byzantine manuscripts. Would it not require a lot of detailed research to establish this? And has little such research been done because of the widespread view that such research would contribute little to the establishment of the original New Testament text?





It is also unclear to me as to whether such research would provide evidence to help determine whether the Byzantine text type is the one closest to the originals. Proponents of both opposing views seem to agree that the Byzantine text type derives from a single archetype. Their disagreement is regarding what that archetype is. Is it the original autographs or a later recensional archetype? Whether genealogical research shows that Byzantine manuscripts represent a very large number or a smaller number of genealogical stemmata, it is not clear to me how this will help in determining the status of the archetype.

Relative proportion of unique readings and shared readings

Argument in favour of Byzantine text type

All things being equal, one would expect that a manuscript closer to the originals would have relatively few unique readings, and relatively many readings shared with other manuscripts from other text types. A manuscript which in its chain of transmission was subject to many accidental or deliberate changes is likely to have relatively many unique readings.

These considerations favour the Byzantine text type as being closer to the originals. Robinson (2001:§53) states, "Diversity of support for a reading is far stronger than the testimony of any single manuscript or small group of MSS. Overlooked by many is the fact that the Byzantine Textform is the most frequent beneficiary of such diverse support: there are far more instances wherein an Alexandrian-Byzantine or Western-Byzantine alignment exists than an Alexandrian-Western alignment wherein the Byzantine stands wholly apart."

Counterargument

Metzger and Ehrman state, (2005:279), "It does appear, however, that the Byzantine editors formed their text by taking over elements of the earlier extant traditions, choosing variant readings from among those already available rather than creating new ones that fit their sense of an improved text."

In other words, they claim that the reason the Byzantine text has relatively few unique readings and usually aligns with other witnesses is because it was a deliberately created eclectic text.

Discussion

If the Byzantine text is the fruit of the work of eclectic editors, the question arises, how did they manage to do such a good job? If they had a selection of manuscripts available and chose what seemed to them to be a better reading from one of them, one would expect that sometimes they might choose an inaccurate minority reading from a particular manuscript. Such a minority reading would only be present in a few manuscripts, and perhaps none of those manuscripts would be extant today. Yet such minority readings seem rare in the Byzantine text; usually other non-Byzantine witnesses support the Byzantine reading.

We can compare the situation to the work of Erasmus in publishing his New Testament. Some of the inaccurate readings in the Textus Receptus are due to Erasmus adopting some minority readings from poor manuscripts he happened to have available. Were the Byzantine editors able to avoid that danger? How did they manage it?

Dominance due to official church sanction

Argument against Byzantine text type

Westcott and Hort hypothesized that the archetype of the Byzantine text type "was taken to Constantinople, whence it was disseminated widely throughout the Byzantine Empire." (Metzger and Ehrman, 2005:177).





Streeter suggested that the Byzantine text was adopted in Constantinople by about the year 380 (Metzger and Ehrman, 2005:215). If the Byzantine text type received official church sanction, this would help explain why it became so dominant in later centuries within the Greek-speaking Eastern church.

Counterargument in favour of Byzantine text type

There is no historical evidence to suggest that the church gave official sanction to an edited text type, and tried to repress other text types. Robinson and Pierpont (1991, fn. 28), say “Kurt Aland had to posit an ‘officially-imposed’ authoritative decision in order for his so-called ‘Byzantine Imperial Text’ to spread rapidly and dominate Eastern Christianity in such a short time. Such an imposition of ecclesiastical authority, however, once more falls under the same condemnation that seriously weakened Hort’s ‘revision’ hypothesis: there simply is no historical data to support such a contention.”

Rebuttal of counterargument

Carson (1979:51) cites Fee (1978) to offer a rebuttal to this argument:

“(1) The influence of John Chrysostom can scarcely be overestimated. He used the Byzantine text in Antioch and carried it with him to Constantinople. By far the most popular preacher of the age, he exercised vast influence in popularizing the text throughout what was left of the Greek-speaking world.

(2) As the church became more and more institutionalized, manuscripts were copied and kept in monasteries, schools, and churches. This meant less interweaving of the textual traditions, more freezing, and more imposed unanimity in a given area.”

Rebuttal of rebuttal

Robinson (2001: §90) says, “A ‘new’ or localized text, even if used by a popular Greek Father would not become transmissionally popular merely due to his reputation. A previous traditional textual dominance over a wider region would not be abandoned on such grounds.”

Alexandrian text type

According to Metzger and Ehrman (2005:312-313), the Alexandrian text type can be divided into primary (earlier) and secondary (later) Alexandrian witnesses, as represented by the following manuscripts.

The dates and other information are taken from Metzger and Ehrman (2005:54-92), Nestle et al (1993:684-689), or Aland et al (1983:xiii-xxvii).

The manuscripts are listed according to date:

Primary Alexandrian

- P⁷⁵ (Luke, John), A.D. 175-225
- P⁴⁶ (Pauline Epistles), c. A.D. 200
- P⁴⁵ (in Acts), 3rd cent.
- Most papyrus fragments with Pauline text.
- Ⲛ (Codex Sinaiticus) (except for John 1:1-8:38), 4th cent.
- B (Codex Vaticanus) (Gospels, Acts, Epistles), 4th cent.



Secondary Alexandrian

- P²⁰ (James), 3rd cent.
- P²³ (Catholic Epistles), 3rd cent.
- P⁵⁰ (Acts), 4th to 5th cent.
- W (in Luke 1.1-8.12 and John 5.12-21.25), 4th to 5th cent.
- A (Codex Alexandrinus) (Acts, Epistles, Revelation), 5th cent.
- I (Pauline Epistles), 5th cent.
- T (fragments of Luke and John), 5th cent.
- H^p (Pauline Epistles), 6th cent.
- Z (Matthew), 6th cent.
- R (Luke), 6th cent.
- L (Gospels), 8th cent.
- Δ (in Mark), 9th cent.
- 33 (Gospels, Acts, Epistles), 9th cent.
- 892 (Gospels), 9th cent.
- 1739 (Acts, Epistles), 10th cent.
- 81 (Acts), 1044.
- 104 (Acts, Epistles, Revelation), 1087.
- 1006 (Gospels, Revelation), 11th cent.
- 1854 (Acts, Epistles, Revelation), 11th cent.
- 2344 (Acts, Epistles, Revelation), 11th cent.
- 326 (Acts, Epistles), 12th cent.
- 1611 (Acts, Epistles, Revelation), 12th cent.
- 579 (in Mark, Luke, John), 13th cent.
- 2053 (Revelation), 13th cent.

Metzger and Ehrman also include the following manuscripts in their list of Alexandrian witnesses, but they seem better characterized as being mixed:

- P⁶⁶ (fragments of John), *c.* A.D. 200. Metzger and Ehrman (2005:57) say the text “is mixed, with elements that are typically Alexandrian and Western.”
- P⁴⁷ (Revelation), 3rd cent. Metzger and Ehrman (2005:55) say “the text...agrees more often with that of Codex Sinaiticus than with any other, though it often shows a remarkable independence.”
- C (New Testament), 5th cent. Metzger and Ehrman (2005:70) describe it as “agreeing frequently with secondary Alexandrian witnesses but also with those of the later Koine or Byzantine type.”
- X (Gospels) 10th cent. Metzger and Ehrman (2005:80) state, “Though its text is mainly of the Byzantine type, it also contains occasional readings of an earlier type, akin to Alexandrian.”
- Ψ (Gospels, Acts, Epistles), 9th or 10th cent. Metzger and Ehrman (2005:85) state, “its text in Mark... [has] readings both Alexandrian and Western... The others Gospels are predominantly Byzantine, with a somewhat larger proportion of Alexandrian readings than in Δ.
- 1241 (Gospels, Acts, Epistles), 12th cent. Metzger and Ehrman (2005:90) state, “In the Gospels, its text has some agreements with C, L, Δ, Ψ, and 33. According to Kirsopp Lake, in Matthew and Mark its text shows a larger infusion of Byzantine readings than in Luke and John.”

Most manuscripts of the Alexandrian text type come from Egypt.



Arguments for and against the Alexandrian text type

Scholars who adhere to the eclectic text claim that the Alexandrian text type is the closest to the original text. Those scholars who maintain the superiority of the Byzantine text type have put forward arguments to suggest that the Alexandrian text type is based on a highly edited manuscript, and not so close to the original text. The arguments and counterarguments are set out below. In some cases I have added further rebuttals to the counterarguments.

Early date of manuscripts

Argument in favour of Alexandrian text type

When trying to determine the original text of the New Testament, more weight should be given to early manuscripts rather than late manuscripts.

The later a manuscript, the more generations of copying it has been subject to, and the greater chance that more and more errors have been introduced into the manuscript. An earlier manuscript is likely to be more reliable because there are fewer links between it and the autograph.

The early date of the main Alexandrian manuscripts, and the support for the Alexandrian text type from the early papyri makes it more likely that they are close to the original autographs.

Counterargument

Robinson (2009b) says, “The fallacy here is that there is no way to assume more or fewer generations of copying, since a 10th century MS could be copied from a 4th century MS directly (such in fact is the claim regarding Alexandrian minuscules), while a 6th century MSS may have been copied through several intermediaries from a 4th century MS. Therefore the assumption is essentially gratuitous.”

Robinson (2001:§67) argues, “Given the exigencies affecting early transmissial history and the limited data preserved from early times, it is a methodological error to assume that ‘oldest is best.’ Since the age of a MS does not necessarily reflect the age of its text, and since later MSS may preserve a text more ancient than that found in older witnesses, the ‘oldest is best’ concept is based on a fallacy. While older MSS, versions, and fathers demonstrate a *terminus a quo* for a given reading, their respective dates do not confer authenticity; they only establish the existence of a given reading at a given date. All readings within a variant unit should be considered under *all* aspects of transmission: minority readings which leave no continual trace throughout transmissial history are suspect; they are *not* made more authentic merely by an appearance in one or a few ancient witnesses.”

Robinson also states (2009a:11) that “Colwell, among others, specifically acknowledged that all sensible readings that have enjoyed repeated transmission over the centuries were likely in existence prior to AD 200.” If this is true, it implies that virtually all the significant errors caused by scribal editorial changes were in existence prior to AD 200. This suggests that any manuscript from the third century on is equally susceptible to being contaminated by such errors. The relatively early date of some Alexandrian manuscripts gives no guarantee that they are less contaminated by such errors than later manuscripts of another text type.

With regard to the early papyri, Robinson (2009b) states that these consist of, “only about 120 papyrus fragments stemming from Egypt (of which most reflect a ‘mixed’ text, and hardly a pure or even extensively Alexandrian text.”





Patristic and versional support

Argument in favour of Alexandrian text type

Carson states (1979:53), “Not only is the Alexandrian text-type found in some biblical quotations by ante-Nicene fathers, but the text-type is also attested by some of the early version witnesses.”

Metzger and Ehrman (2005:313) list the following fathers and versions as witnesses to the Alexandrian text type:

Primary Alexandrian

- Clement
- Origen
- Sahidic (in part)

Secondary Alexandrian

- Didymus the Blind
- Athanasius
- Bohairic

Counterargument

If the primary Alexandrian manuscripts are the ones closest to the autographs, one would have expected that their readings would have been more widely distributed and more widely quoted. Only two fathers is pretty sparse support.

Superiority of the shorter reading

Argument in favour of Alexandrian text type

When evaluating variant readings from different manuscripts or text types, one criterion for evaluating the internal evidence to help determine which reading is likely to have been original is that the shorter reading is more likely to be the original one. In other words, it is more likely that a scribe would add something than delete something. The Alexandrian text type tends to be more succinct than the other text types, hence it is more likely to be closer to the original.

Counterargument

A number of scholarly articles have thrown doubt on the criterion that the shorter reading is superior. These include Lake (1928), Head (1990), Royse (1979, 1995). Metzger and Ehrman state, (2005:214), “the case against the criterion *brevior lectio potior*, at least for the earliest New Testament witnesses, has been taken up by James Royse, who, on the basis of a careful study of the papyri, has concluded that in fact the opposite scribal tendency appears to hold, that is, that the scribes of our surviving papyri were more likely to omit portions of the text rather than add to it.” They also state (2005:214 ft. 22), “before a book has become sacred, careless copying is much more likely to result in omissions than additions.”

This suggests that in the very early period of copying the individual manuscripts of books which were later incorporated into the New Testament, there might have been a tendency to omit words, so that in cases where such a process seems likely, longer readings might have some claim to superiority.





Robinson (2001:§43) states, “Neither the shorter nor longer reading is to be preferred. The reasoned eclectic principle here omitted is the familiar *lectio brevior potior*, or giving preference to the shorter reading, assuming all other matters to be equal — a principle which has come under fire even from modern eclectics. Not only can its legitimacy be called into question, but its rejection as a working principle can readily be justified. The net effect of such a principle is to produce an *a priori* bias on insufficient internal grounds which favors the shorter Alexandrian text. The underlying premise is faulty: it assumes that scribes have a constant tendency to expand the text, whether in regard to sacred names, or by a conflationary combination of disparate narratives, lest anything original be lost. Yet scribal habits as exemplified in the extant data simply do not support such a hypothesis.”

Destruction of Alexandrian manuscripts during persecutions

Argument in favour of Alexandrian text type

The relative lack of later Alexandrian manuscripts can be explained as being due to widespread destruction of New Testament manuscripts during periods of persecution. Metzger and Ehrman (2005:220) state, “during the pre-Constantinian persecutions New Testament manuscripts were sought out and burned by imperial order... The further spread of Islam in the seventh century meant that Christian in three of the five ancient patriarchates (Alexandria, Jerusalem, and Antioch) came under the sway of Muslims and the Christian populations of North Africa, Egypt, Palestine, Syria, and Mesopotamia were greatly reduced, with corresponding effects upon the transmission of Scriptures in those areas.”

Counterargument

The various explanations put forward to explain the disappearance of Alexandrian text type manuscripts are regarded as implausible. Robinson (2001:§81) says “The claim is that various persecutions, and especially that of Diocletian, so decimated the number of NT MSS that previously dominant texttypes were all but eliminated, leaving the rising Byzantine to fill the gap. This really assumes too much: an initial presumption is that a non-Byzantine text dominated the Eastern Empire; then, when persecutors demanded scriptures for destruction, the Alexandrian text alone was overwhelmingly surrendered. Persecutions, however, were not selective in their textual targets. The MSS surrendered and destroyed in a given region would reflect the general proportion of existing MSS, regardless of texttype; so too those which survived. Were 1000 MSS destroyed in a local area of which only 100 were Byzantine, even a 90% decimation still would leave a survival proportion similar to that which was destroyed. One cannot stretch credulity to presume a reversal of texttype dominance as the result of basically random persecutions.”

With regard to the Islamic conquest, Robinson (2001:§85) says, “The Islamic Conquest was not as totally destructive to NT MSS as has been claimed. Monasteries and churches in both Palestine and Egypt continued literary activity following the conquest and maintained communication with the Eastern and Western Empire, even while facing pressure to abandon Christianity and convert to Islam.”

Small number of localized manuscripts

Argument against Alexandrian text type

Relatively few manuscripts support the Alexandrian text type, and these are mainly restricted geographically to Egypt. This can be explained if the Alexandrian text type was a local text type which is derived from an archetype produced and promoted in Egypt. The fact that the Alexandrian text type is so rarely found outside Egypt suggests that it became popular only in the geographical area in which it was edited.





Counterargument

Carson (1979:49) offers the following rebuttal of this argument: “Although most early non-Byzantine manuscripts have been found in Egypt, it is naive to postulate that the textual tradition they represent is restricted to that area. It is naive not only because not *all* such manuscripts come from Egypt, but more importantly because the early fathers quoted from the text-types represented by those manuscripts, and these early fathers did not all live in Egypt! Moreover the early versions reflect the same text-types, and they too represent wide geographical distribution.”

Epp (1992:415) states, “As the study of papyri in Egypt demonstrates with considerable specificity, the Roman Empire facilitated the fairly swift and widespread movement of letters and literature, and there is no reason why Christian writings would not have enjoyed the same mobility. This is evidenced, e.g. by the fact that early NT papyri contain at least remnants of all the major kinds of texts identified as existing in the first centuries of Christianity, and yet there is no compelling reason to think that all of these kinds of texts originated in Egypt; rather, many texts now survive in early Egyptian papyri that were brought there from other areas of the Christian world.”

Lack of attestation in the main Greek-speaking areas

Argument against Alexandrian text type

If the Alexandrian text was close to the original, than copies of this text type would have been widely copied in the first and second centuries throughout the Greek speaking world. It should have been the dominant text type in those areas before the subsequent rise of competing text types based on highly-edited manuscripts.

There is no plausible reason that would cause a more accurate dominant text type to be almost completely displaced by a subsequent less accurate text type. Robinson states (2009a:14), “what ‘strains the imagination’ is the assumption of Alexandrian originality permeating the Greek-speaking Eastern Empire and then suddenly dying out and being replaced ...with not a comment or complaint from anywhere else in the Eastern Empire about the sudden change of text from Alexandrian to Byzantine — a change that would have occurred without any ecclesiastical revision or imposed control.”

A pattern suggestive of deliberate editing

Argument against Alexandrian text type

It is suggested that the Alexandrian text type was due to a deliberate editorial inclination to make texts more succinct for stylistic reasons, eliminating sentences or words that were regarded as superfluous.

It is suggested that scribes in Alexandria were influenced by the type of editorial changes Alexandrian scholars made to the Homeric epics. Metzger and Ehrman say (2005:198), “there was a fairly well-developed scholarly discipline of textual and literary criticism in antiquity, localized chiefly at Alexandria and directed primarily toward the epics of Homer... It is less widely appreciated — indeed the question has seldom been raised — how far the methods of textual criticism current at Alexandria were adopted by scholars in the Church and applied to the text of the New Testament.”

Robinson (2001:§18) says, “The shorter form in Homer is considered to reflect Alexandrian critical know-how and scholarly revision applied to the text; the Alexandrian text of the NT is clearly shorter, has apparent Alexandrian connections, and may well reflect recensional activity.” Robinson cites Farmer (1974:13-17) and Robinson (1993) to support this argument.





Robinson and Pierpont (1991) state, “Conflation is not exclusive to the Byzantine-era manuscripts; the scribes of Alexandrian and Western manuscripts conflate as much or more than what has been imputed to Byzantine-era scribal habits.” They cite Pickering (1980) in support of this statement.

Counterargument

As a rebuttal to this argument Carson cites the work of Fee (1974). Carson states (1979:117) “If recent work by Gordon D. Fee is correct..., then neither \mathfrak{P}^{75} nor B is recensional. If \mathfrak{P}^{75} , 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. If Fee’s work stands up, then we must conclude that at least in John’s Gospel the Alexandrian text-type is by far the closest to the autograph.”

Rebuttal of counterargument

Robinson (2009b) states, “J. C. O’Neill, ‘The Rules followed by the Editors of the text represented by the Codex Vaticanus,’ written post-Fee claims precisely the opposite. Further, no one to my knowledge has leaped on Fee’s bandwagon claim in this regard.”

Western text type

The Western text type is represented by the following manuscripts according to Metzger and Ehrman (2005:310). The dates and other information are taken from Metzger and Ehrman (2005:62-89), Nestle et al (1993:684-689), or Aland et al (1983:xiii-xxvii). The manuscripts are listed according to date.

- \mathfrak{P}^{48} (Acts), late 3rd cent.
- \mathfrak{P}^{29} (Acts), 3rd cent.
- 0171 (fragments of Luke), c. 300, from Egypt
- \mathfrak{P}^{38} (Acts), c. 300,
- \aleph (Codex Sinaiticus) (in John 1:1-8:38), 4th cent.
- W (in Mark 1:1-5:30), 4th to 5th cent.
- D, (Codex Bezae) (Gospels and Acts), 5th cent.
- D^p (Codex Claromontanus) (Pauline Epistles), 6th cent.
- F^p, (Pauline Epistles), 9th cent.
- G^p (Pauline Epistles), 9th cent. “it is closely akin to F^p, and both of them probably go back one or two generations to a common archetype” (Metzger and Ehrman 2005:76)
- E^p, (Pauline Epistles), 9th or 10th cent., copied from D^p “and therefore of no independent value” (Metzger and Ehrman 2005:74)
- 383 (Acts), 13th cent.
- 614 (Acts), 13th cent.

Western readings are also found in some early Latin and Greek fathers, Tatian’s Diatessaron, and Syrian fathers to about A.D 450.

Problems with the Western text

There are two types of evidence which make it unlikely that the Western text type is the one closest to the original.





A pattern suggestive of deliberate editing

There is a regular pattern to the differences characteristic of the Western text, which support the hypothesis that they were the result of deliberate editing. Metzger (1994:6) says: "The chief characteristic of Western readings is fondness for paraphrase. Words, clauses, and even whole sentences are freely changed, omitted, or inserted. Sometimes the motive appears to have been harmonization, while at other times it was the enrichment of the narrative by the inclusion of traditional or apocryphal material."

A small number of manuscripts

There are a relatively small number of manuscripts which support the Western text type. Because none of the manuscripts represent the whole New Testament, there are only a few separate witnesses for any particular book. Based on the manuscripts listed above, the number of independent Western manuscript witnesses for various parts of the New Testament are as follows:

- Matthew: 1 (D)
- Mark: 2 (D,W)
- Luke: 2 (D,0171)
- John: 2 (D, Ⲱ)
- Acts: 6
- Pauline Epistles: 2 (D^p, with F^p and G^p counted as one witness, and not counting E^p).
- Catholic Epistles and Revelation: None.

If indeed Western manuscripts are copies of a recensional archetype manuscript which was somewhat later than the autographs, it is understandable that relatively few copies would be made of it, compared to a much larger number of manuscripts which were derived from the original autographs without such alterations.

Strong points of the Western text

On the other hand, there are a number of features of the Western text which support the contention that it may preserve original readings.

Early date of patristic and versional support

Metzger and Ehrman (2005:308) say, "Because the Western type of text was used by such second- and early third-century authors as Marcion, Justin (and probably Tatian), Heracleon, Irenaeus, and Tertullian, most scholars date the emergence of the Western text to the mid-second century or shortly thereafter. But they also, as Martini has put it, 'leave the door open to an appreciation of the presence of particular readings in which D or other "Western" witnesses have, perhaps, preserved the most ancient reading.'"

Wide geographical support

Metzger and Ehrman (2005:309) say, "So called Western texts of the Gospels, Acts, and Pauline Epistles circulated widely not only in North Africa, Italy, and Gaul (which are geographically western) but also in Egypt and (in somewhat different forms) the East.

These latter text forms are represented by the Sinaitic and Curetonian manuscripts of the Old Syriac, by many of the marginal notes in the Harclean Syriac, and perhaps by the Palestinian Syriac."





Superiority of the longer reading

In contrast to the more popular view that the shorter reading is superior, some scholars such as A. C. Clark have argued that the longer reading is superior. Metzger and Ehrman (2005:212) state, “Clark’s researches on the manuscripts of Cicero’s orations led him to believe that accidental omission was a much more common fault than deliberate interpolation by scribes.”

If this were true, then many of the longer readings characteristic of the Western text would have a stronger claim to be in accordance with the autographs.

Counterargument

Robinson (2001: §45) says, “The converse principle — that the *longer* reading should be preferred — is equally rejected. A few may argue thus, such as A. C. Clark and C.-B. Amphoux, who favor the Western type of text, but such no more can be applied mechanically to the text than can the ‘shorter reading,’ despite any apparent logic or plausibility which may be adduced. Such a principle simply will not work within a transmissional framework.”

Caesarean text types

The Caesarean text type was posited by B. H. Streeter, who “identified the text that Origen used at Caesarea, and associated it with the text in Θ, fam. 1, fam. 13 and other witnesses” (Metzger and Ehrman 2005:310).

Recent scholarship, however, has cast doubt on whether there is sufficient evidence to consider it to be a unified text type. In particular, Hurtado (1981) showed that the manuscripts traditionally considered Caesarean and “pre-Caesarean” belonged to two unrelated groups.

The manuscripts belonging to these groups are set out below. The dates and other information are taken from Metzger and Ehrman (2005:54-89). The manuscripts are listed according to date. These two text types only seem to be established for the Gospel of Mark.

So-called “pre-Caesarean”

According to Metzger and Ehrman (2005:310), these manuscripts preserve the text which “probably originated in Egypt and was brought by Origen to Caesarea, whence it was carried to Jerusalem.”

- P⁴⁵ (in Mark), 3rd cent.
- W (in Mark 5:31-16:20), late 4th to early 5th cent.
- Family 13, 11th to 15th cent. It includes minuscule manuscripts 13, 69, 124, 230, 346, 543, 788, 826, 983, 1689, and 1709 (Metzger and Ehrman 2005:87).

So-called “Caesarean”

Besides the manuscripts listed below, this text type is found in “many citations of Origen and Eusebius, the Old Armenian and Old Georgian versions ... and to some extent, the Old Syriac” (Metzger and Ehrman 2005:311).

- Θ (in Mark), 9th cent.
- 565 (in Mark), 9th cent.
- 700 (Gospels), 11th cent. I presume it is only Mark which is relevant.
- Family 1 (in Mark), 12th to 14th cent. It includes minuscule manuscripts 1, 118, 131, and 209. Metzger and Ehrman state (2005:87), “often agrees with Codex Θ and appears to go back to the type current in Caesarea in the third and fourth centuries.”





Features of the Caesarean text types

The following description of the Caesarean text does not distinguish the two separate text types, so it is unclear to what extent the description applies to each group. Metzger and Ehrman (2005:310-311) state, “The special character of the Caesarean text is its distinctive mixture of Western and Alexandrian readings ... followed the Alexandrian text while retaining any Western readings that did not seem too improbable ... resemble the conscious harmonizations, paraphrases and smoothing of grammatical details also found in Western sources.”

Reflections on unresolved questions

In weighing up the relative merits of the Byzantine and Alexandrian text types, further light is needed on a number of debated questions. The following sections represent the speculations of someone who has read relatively little in the area of text criticism. Therefore I do not know how these issues have been addressed in the literature, and whether my speculations are completely off-base. The purpose of this paper is to record my speculations so I don't forget them, and to stimulate myself to seek out papers in the technical literature which will shed light on them. I also hope that those who know much more about these matters than I do will point out flaws and errors in my reasoning, or relevant issues I have ignored.

Implausible scenarios

It seems to me that the arguments in favour of the Byzantine and Alexandrian text types both have major weaknesses, in that each depends on an implausible scenario.

The area of implausibility which weakens both arguments relates to the dictum of Hort's quoted earlier: “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*.”

Since one presumes that many more copies would be made of original readings compared to inaccurate readings, it is implausible to claim that a text type representing a small minority of witnesses represents the original text type.

In the early period of the second and third centuries, it is the Byzantine argument which is weak in this regard. If the Byzantine text type was closest to the original, would one not expect that Byzantine readings would be represented in the majority of manuscripts being copied, quoted from and translated from during that period? Why then is the Byzantine text type not quoted more by the early fathers, translated more in the early versions, or preserved more in any of the early extant manuscripts?

Although arguments have been cited above suggesting that there are more Byzantine readings present in early patristic quotations than generally acknowledged, it still seems that there are much fewer than one would expect if Byzantine manuscripts were in the majority.

In the period from the fifth century on, it is the Alexandrian argument which is weak in this regard. If the Alexandrian text type was closest to the original, would one not expect that Alexandrian readings would be represented in the majority of manuscripts being copied, quoted from and translated from during that period in the major Greek-speaking areas? Or at least a significant minority? Why then is the Alexandrian text type so little quoted by the fathers in the Byzantine Empire, or preserved in the later manuscripts?

The proponents of each text type have put forward counterarguments to try to explain the implausible scenario. The Alexandrian counterargument cites some factors which would have tended to promote the spread of the Byzantine text type in the later period, to the detriment of the Alexandrian text type. If the Alexandrian text type is to be defended, these arguments would need to be strengthened.





The Byzantine counterargument stresses that there is no manuscript, patristic or versional evidence from the Greek-speaking heartland for the second and third centuries. Hence the main evidence regarding what text type was dominant there at that time is the situation in the fourth century. This is plausible enough. But it is harder to see what factors in the earlier period would have caused such widespread displacement of original Byzantine readings outside the Greek-speaking heartland. So one question that needs to be answered is: What factors mitigated against the use of the Byzantine text type by early fathers and translators?

Accuracy of early copyists

There is evidence to suggest that early copyists were less accurate than later copyists. Metzger and Ehrman (2005:275) say, "It is a striking feature of our textual record that the earliest copies we have of the various books that became the New Testament vary from one another far more widely than do the later copies, which were made under more controlled circumstances in the Middle Ages."

They add, "The earliest copyists would not have been trained professionals who made copies for a living but simply literate members of a congregation who had the time and ability to do the job. Since most, if not all, of them would have been amateurs in the art of copying, a relatively large number of mistakes no doubt crept into the texts as they reproduced them."

If this is true, it creates problems for proponents of both the Alexandrian text type and the Byzantine text type. Each group claims that their text type is relatively free of alterations. But if inaccurate copying was rife in the early period, while it is possible that a particular text type might have been relatively unaffected, it is also conceivable that every text type was affected to more or less the same degree. In that case the situation would be like the hypothetical scenario mentioned by Robinson (2009a:9), "none of the existing texttypes represent the autograph, and are all later hyparchetypal developments, with whatever the autograph may have been being totally 'lost' and dispersed equally or unevenly among the three existing major texttypes." It is thus conceivable that each text type might be more or less equally inaccurate, with each one being inaccurate in different ways.

The proponents of each text type argue against such a scenario by suggesting factors which caused the copyists in the chain of transmission which produced their text type to be more accurate than the average run-of-the-mill copyist. Metzger and Ehrman (2005:312) claim, "It is widely agreed that the Alexandrian text was prepared by skillful editors, trained in the scholarly traditions of Alexandria." They further claim (2005:278), "It is no surprise, then, to find that textual witnesses connected to Alexandria attest a high quality of textual transmission from the earliest times In light of the striking similarities in text between the fourth-century B and the early third-century \mathfrak{P}^{75} , it is clear that the Christian scholars of Alexandria worked assiduously to preserve an accurate form of text."

This claim is not entirely convincing. The Alexandrian editors would have had no control on the errors produced by early copyists in the localities in which the autographs were first copied and along the chain of transmission until copies were obtained in Alexandria. And the similarity of \mathfrak{P}^{75} and B doesn't necessarily prove much about the general quality of copying, only that B was probably a direct descendant of \mathfrak{P}^{75} , perhaps only a generation or two removed. The relatively large number of differences between Alexandrian manuscripts also casts doubt on the general level of accuracy of Alexandrian copyists.

Another factor to consider is the warning of Metzger and Ehrman that intelligent scribes can be more dangerous than those who are not. They say (2005:259), "Odd though it may seem, scribes who thought were more dangerous than those who wished merely to be faithful in copying what lay before them. Many of the alternations that may be classified as intentional were no doubt introduced in good faith by copyists who believed that they were correcting an error or infelicity of language that had previously crept into the sacred text and needed to be rectified." It would seem to me that the skillful editors of Alexandria might be more susceptible to this than some other less professional scribes elsewhere.





The proponents of Byzantine priority are perhaps in an even more difficult position. The Alexandrian text type proponents tend to make claims of accuracy only for one manuscript, namely B.

Even if the large majority of manuscripts are supposedly contaminated by the accumulated errors of second-rate copyists, it is plausible enough to suggest that there are a few manuscripts at the upper end of the quality scale, and it happens that B is one of those. But the Byzantine priority proponents claim that a whole slew of relatively late manuscripts are at the upper end of the quality scale. This is only plausible if a significant number of manuscripts escaped the effects of poor copying during the early period.

Like the Alexandrian text type proponents, the Byzantine priority proponents try to come up with factors that would explain why the manuscripts which were supposed to be the ancestors of the Byzantine text type managed to avoid being corrupted by large numbers of alterations. One argument is to claim that the general standard of copying was quite high. Robinson (2001:§97) says, “Scribes for the most part were generally *careful* and reasonably *accurate* in their copying endeavors. Were this not so, the MSS of the NT and all ancient works swiftly would have become a mass of confusion, and one would despair at ever recovering an original form of the text. While all scribes blundered or made intentional alterations to the text at various times, the overall character of the copied text was *not* so affected as to preclude a reasonably accurate transmission on ‘normal’ terms, thus facilitating the recovery of an original from comparison of various witnesses.”

Pickering (n.d. Ch. 5) argues that the Christians “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.”

Another argument is to claim that the standard of copying would have been higher than average in the region where the Byzantine text type was preserved (very similar to the Alexandrian argument above). Robinson (2001:§77) says, as cited earlier, “Copies produced within a close proximity to the site of origin or initial reception of a given text would be expected to retain a more uniform textual complexion closely resembling that of the autograph; ... Copies produced at a more remote distance from the site of origin would tend to diverge in greater quantity. If such a hypothesis is correct, the primary Greek-speaking region during the period of ‘geographical silence’ would be expected to retain a Byzantine text.”

Pickering (n.d. Ch. 5) uses several arguments that suggest that the Christians in the Aegean area were more qualified to copy the sacred text accurately than those in other areas, especially as compared to Egypt.

The arguments are:

1. They would have had better access to the autographs, whereas there were no autographs in Egypt.
2. They were more proficient in Greek, whereas “the use of Greek in Egypt was already declining by the beginning of the Christian era.”
3. The church in that region was stronger. Pickering quotes Kurt Aland as saying “about 180 the greatest concentration of churches was in Asia Minor and along the Aegean coast of Greece.” Whereas “the Alexandrian church was weak and insignificant.”

Pickering (n.d. Ch. 5) quotes Tertullian as evidence of the importance of particular cities where the autographs had been kept. Tertullian said, “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" (*Prescription against Heretics*, 36, using Holmes' translation). Pickering comments, "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."

Another factor to be considered is the influence of the public reading of Scripture. Metzger and Ehrman (2005:307 fn. 12) claim that the lectionary system of the Greek Orthodox Church "exerted a stabilizing influence on the Byzantine text."

Presumably if both potential copyists and potential readers heard the text being read aloud regularly week after week, year after year, their familiarity with it would act as a control that would reduce the likelihood of alterations, or create pressure to correct alterations which might have occurred.

This raises the question as to whether the public reading of Scripture could have had a similar influence in the first couple of centuries after the autographs were written.

Justin Martyr (martyred in 165) says (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." If the apostolic writings were read extensively every week in many early Christian congregations, would this have exerted a stabilizing influence on the text? Would this stabilizing influence have been greater in the regions where the church was stronger and Greek was the native language?

As with the Alexandrian argument, the arguments for Byzantine scribal accuracy are not entirely convincing. The factors suggested are plausible enough and probably would have exerted some influence to make the level of copying more accurate. But it is impossible to measure and difficult to guess how much influence each factor would have had, and whether their combined influence lifted the level of accuracy to the high level which would have had to have been maintained if the Byzantine priority theory is correct.

Predominance of original readings

Assuming that most scribes copy accurately most of the time, it may be presumed that original readings will predominate over inaccurate readings at each stage of transmission if the transmission process is not disrupted.

There are at least three types of evidence upholding this presumption:

1. Robinson (2001:§107) says, "Under all theories, *ca.* 90% of the original text of the NT is considered established." This means that for about 90% of the New Testament, there is virtually no disagreement about the text, so this presumption holds for that proportion of the text.
2. It also holds for some variants in which only a small number of manuscripts differ from the reading of the majority, and the majority reading is accepted as original.
3. It holds for the great majority of Byzantine manuscripts, which preserve the readings of the Byzantine archetype with relatively high fidelity and uniformity.

For other variants, however, this presumption does not seem to hold, especially in the early period.





If an Alexandrian reading is regarded as original, and the Byzantine reading differs from it, it is clear this presumption has not been fulfilled among the later minuscules. And in many cases, it has not been fulfilled among the early manuscripts either.

If the Byzantine reading is regarded as original, in many cases the presumption is not fulfilled with regard to early manuscripts, when a majority of early manuscripts do not have the Byzantine reading.

How can this breakdown of the presumption be explained? In the sections below I suggest a number of factors which might cause an inaccurate reading to become predominant.

Very early date of the error

If an error occurred in the first or second generation of copying, this could possibly explain the dominance of an inaccurate reading. Let's assume for sake of argument that ten copies were made of the autograph of the epistle of Jude. If one of them had an error in a particular reading, and each of the ten copies were copied in equal proportion, then ten percent of subsequent copies would have the error. Whereas if the error occurred in the second generation, only one percent of subsequent documents would have the error. Hence the earlier an error occurs, the greater chance it has of becoming widespread. But by itself, this factor is insufficient to make an inaccurate reading predominant. It needs to be combined with one or more of the factors mentioned below.

Disproportionate copying of influential inaccurate manuscripts

Let's imagine a scenario around the time of the second generation of copying, when there are about one hundred extant copies of a particular autograph. Each copy has its own particular set of errors, some accidental, some deliberate. Some of these copies are located in small village congregations, or owned by individuals with few friends. Such copies would seldom if ever be copied by others. But other copies are located in a large urban congregation, or owned by an influential individual with friends everywhere. Many copies would be made of such a manuscript. Economic factors are also relevant, since it takes time and money to copy a manuscript. Hence more copying will be done in places with greater financial resources. The result of these factors is that the particular set of errors found in an influential manuscript will be propagated much more widely in future generations of copies than the errors found in the less influential manuscripts. If that influential manuscript happens to be one containing a great deal of recension, then that recensional text type will be propagated more widely.

One could argue that in the majority of cases in which an error is found in a minority of witnesses, it is because none of the manuscripts in which the error occurred was very influential. But there are a few percent of errors which happened to occur in very influential manuscripts that were copied disproportionately. Those are precisely the errors which dominate over the original reading.

Cross-correction of accurate manuscripts based on inaccurate manuscripts

An influential inaccurate manuscript can have wider influence than that arising from the large number of copies made from it.

It can also be the basis for the correction of other manuscripts which derive from a different archetype. If a manuscript with a more accurate reading is corrected based on an influential manuscript with an inaccurate reading, this will assist the increased dominance of the inaccurate reading.

Repeated similar errors by different scribes due to difficulty of passage

If a particular reading in the autograph was difficult, many scribes with recensional tendencies will be tempted to correct it. If the difficulty is such that there is one particular correction which obviously presents itself, then numerous scribes might independently make the same correction on different





occasions in different chains of transmission. Such a process could make an inaccurate reading predominate over the accurate reading.

If there is not one obvious way to correct the difficult reading, a set of competing corrections might arise in different chains of transmission. Although no one reading might predominate, it might be that the various pseudo-corrections in total might predominate over the original reading.

Disproportionate destruction of manuscripts with accurate readings

Because certain manuscripts become influential in particular localities, it is likely that particular geographical areas may be dominated by a local text type. Each local text type will have its own set of inaccurate readings. Some local text types would have more inaccurate readings than others.

It is quite feasible that destruction of manuscripts for various reasons would be greater in some geographical regions than others. Destruction due to climactic factors is greater in Europe than in Egypt, particularly for papyrus. Destruction due to Islamic rule was greater in North Africa and the Middle East.

If it happened that the area where a more accurate text type was dominant was subject to greater destruction of manuscripts than an area where a less accurate text type was dominant, this would aid the predominance of inaccurate readings.

A hypothesis of textual transmission

I set out below a hypothesis of the process of textual transmission from the time of the autographs. This represents my attempt to imagine what the different stages of transmission might have been. Some of it is based on remarks I have come across in my limited reading of the literature, and at a few points I have added supporting quotations. Other parts are based on my personal speculation. The purpose is to set up a framework which I can modify and explore as I read further and interact with others who know the subject much better than I do.

Stage one: copies of individual books

- (1) Multiple copies were made of autographs of individual books.
- (2) Second and third generation copies were made of some or all of the first generation copies.
- (3) Some accidental errors occurred in every copy, and some editorial changes occurred in many copies. Metzger and Ehrman (2005:275) say, "The earliest copyists would not have been trained professionals who made copies for a living but simply literate members of a congregation who had the time and ability to do the job. Since most, if not all, of them would have been amateurs in the art of copying, a relatively large number of mistakes no doubt crept into the texts as they reproduced them."
- (4) Occasionally a copyist made extensive editorial changes, so the manuscript he produced could be termed recensional. Subsequent generations of copies from that recensional archetype propagated a recensional text type.
- (5) After half a dozen or so generations, I hypothesize that the manuscripts extant at that time would fall into a bell curve with regard to closeness to the autographs. At one end of the bell curve would be manuscripts which in their chain of transmission have had the least encounters with careless copyists (reducing the number of accidental errors) and recensional copyists (reducing the number of deliberate changes). But it is virtually inconceivable that any manuscript would have avoided accumulating some accidental errors, and also, in my opinion, unlikely that it would have completely avoided editorial changes. Hence every manuscript is likely to be recensional to some degree or other. At the other end of the bell curve would be manuscripts which in their chain of transmission had major and/or numerous encounters with careless copyists and recensional copyists, with the result that they have many errors.



- (6) Up to this point, the transmissional history of each individual book was quite different. Since they were written at different times and were located in different places, the copying of the initial generations of copies of each book would have been done at different times and different places, involving different sets of copyists. The types of changes the manuscripts of each individual book were subject to in the initial chains of transmission would have hence been quite different from each other.

Stage two: copies of multiple books

- (7) After a while some people would have started owning manuscripts of several books. This would have increased the possibility that several books would be copied by the same copyist.
- (8) If the owner was a copyist, or had a particular scribe working for him or her, when the owner located a new manuscript they wished to own, the same copyist might copy it who had copied previous manuscripts.
- (9) If someone else wanted to get copies from someone who owned multiple books, the same copyist might have made copies of many of the books.
- (10) As a result, several books might be subject to the same types of scribal changes at the same time. If the copyist was careless, he might make a similar set of accidental errors in a set of books. If he had recensional tendencies, he might make similar editorial changes in several books at once.

Stage three: manuscripts including multiple books

- (11) The next stage would be when a fixed set of books were incorporated into one manuscript. The main sets which became popular were manuscripts including the four gospels, and manuscripts including the Pauline Epistles. Metzger (1995:60) says, "scholars including Hans-Martin Schenke and Kurt Aland, think that several persons independently began to assemble *corpora Paulina*, each containing varying numbers of Paul's epistles, which at a later time, perhaps in the second century, were amalgamated into an edition of ten, to be augmented still later by the three so-called pastoral Epistles (1 and 2 Tim. And Tit.)." Metzger adds (1995:61), "The four Gospels were probably collected early in the second century. At any rate, evidence in the writing of Justin Martyr of Rome show that by the middle of the second century he knew of the existence of at least the three synoptic Gospels and perhaps he was acquainted also the Gospel according to John."
- (12) In order to create such a manuscript, for example of the gospels, someone would need to have access to individual copies of each gospel and then copy them all onto one manuscript. It is quite possible that the transmissional history of the particular gospel manuscripts would have been quite different, and the quality of each one might have been significantly different.
- (13) Henceforth, when further copies are made of this combined manuscript, the transmissional history of the included books would usually be the same.
- (14) An exception would be when a combined manuscript was copied not from one combined manuscript, but from several, as happened apparently with manuscript W.
- (15) Once these combined manuscripts became popular, it is likely that much less copying would be done of individual books. Hence many manuscript chains of transmission would terminate. Only those manuscripts of individual books which got copied into combined manuscripts would transmit their readings to further generations of copies. Most others would not. With regard to the Pauline Epistles, there are no longer extant copies of manuscripts containing an individual book. Metzger says (1995:60), "Except for some stray leaves from one or another epistle, none of the more than seven hundred Greek manuscripts of Paul's epistles presents a single epistle standing alone; all contain a collection of his epistles."

Stage four: Institutionalization of copying

- (16) After Christianity became the official religion, it is likely that the copying process became more and more institutionalized. Copying by individuals would have decreased. Robinson (2001:\$60) says, "The church of the early fourth century moved from a persecuted minority to an approved entity with governmental sponsorship. It is no coincidence that a change in writing material (from cheap and fragile papyrus to costly and durable vellum) occurred at this time. The earliest extant vellum MSS (i.e., the fourth- and fifth-century uncials (Ⲛ, A, B, C, D, and W) and many later uncials would have been copied directly from papyrus exemplars."
- (17) For the most part, only those manuscripts which were acquired by the copying institutions, such as churches and monasteries, would have transmitted their readings to further generations of copies. Many manuscripts owned by individuals would have had their chains of transmission terminated.
- (18) Another reduction in genealogical stemmata would have occurred at the time of the change to minuscule manuscripts. Only those uncial manuscripts which were chosen to be copied to a minuscule manuscript would have transmitted their readings to further generations of copies.

An implication of the hypothesis

It is unclear what proportion of non-original readings in an individual book would have occurred in the first two stages of copying, before the introduction of combined manuscripts. One factor influencing this is how long these stages lasted. For books that were written at an early date, these stages would have lasted longer than for books written later.

For example, if we assume for the purpose of argument that Mark was the first gospel written, and John the last, then individual copies of Mark might have circulated several decades longer than individual copies of John. For combined manuscripts of the gospels could only be made, at the earliest, not long after the last gospel became widely circulated. Hence manuscripts of Mark would have been subject to all sorts of encounters with early copyists which would have not affected manuscripts of John. Similar scenarios can be imagined for early and late epistles.

It is unclear whether early copyists copying individual books would have been more or less accurate than later copyists copying combined manuscripts. But one hypothesis is that they would have been less accurate, perhaps because the books had not yet achieved canonical status. Metzger and Ehrman (2005:275) say, "It is a striking feature of our textual record that the earliest copies we have of the various books that became the New Testament vary from one another far more widely than do the later copies, which were made under more controlled circumstances in the Middle Ages."

By this hypothesis, it is possible that a significant proportion of errors could have been introduced into individual books before they became incorporated into combined manuscripts. This might suggest that many of the particular readings associated with certain text types might have arisen in individual books before they were incorporated into combined manuscripts.

The implication of this is that text type relationships are likely to be significantly different for each individual book. By making this point I want to challenge the point of view which I seem to detect in the literature that generalizations about text types can apply to all the New Testament books.

It seems to me that text type definitions and genealogical relationships should be established separately for each individual book. It is not enough to declare a manuscript Alexandrian or Byzantine, and assume that there are supposed "Alexandrian" or "Byzantine" characteristics in the readings in each individual book. Each book of each manuscript needs to be judged on its own merits.



An example of a generalized statement is Metzger and Ehrman's remark (2005:312), "It is widely agreed that the Alexandrian text was prepared by skillful editors, trained in the scholarly traditions of Alexandria. The text on which they relied must have already been ancient in all important points." According to the scenario I have hypothesized above, in particular books, a significant number of changes from the original could well have occurred before the text got into the hands of the skillful Alexandrian editors.

Although a document like Codex Vaticanus may be at the top of the bell curve of accuracy in many respects, one can conceive the possibility that for one or two books of the New Testament, it happened that the individual manuscript incorporated into the combined manuscript that is the ancestor of Vaticanus happened to be one that was towards the bottom of the bell curve. Similarly, it may be that a Byzantine manuscript towards the bottom of the bell curve of accuracy in many respects, has one or two individual manuscripts that got incorporated into the combined manuscript that was the ancestor of the Byzantine archetype and are of high quality.

To give some concrete examples, I examined the apparatus of Nestle-Aland 27th edition *Novum Testamentum Graece* for the books of 2 Peter, 2 John, 3 John, and Jude. I counted every occasion in which the manuscripts \aleph , B, and the Majority text departed from the reading chosen by the editors as closest to the original text.

The figures are shown in the following table "Number of readings different from the NA²⁷ eclectic text."

| Book | \aleph Sinaiticus | B Vaticanus | Majority text |
|---------|---------------------|-------------|---------------|
| 2 Peter | 47 | 17 | 33 |
| 2 John | 7 | 2 | 10 |
| 3 John | 6 | 3 | 9 |
| Jude | 18 | 17 | 15 |

Given that the editors of the eclectic text tend to favour Alexandrian witnesses, of which B is regarded as the best exemplar, the figures for 2 and 3 John are not surprising. B only departs from the eclectic text a few times, and \aleph is relatively close. The Majority text departs from the eclectic text somewhat more.

The figures for 2 Peter and Jude, however, show a different pattern. For 2 Peter, it is \aleph which departs most from the eclectic text. This would suggest that for the book of 2 Peter, the Alexandrian manuscript \aleph is a lower quality witness than the Byzantine text. If B is the standard for a good Alexandrian manuscript, then the Byzantine text has a better claim to being called "Alexandrian" in 2 Peter than \aleph does. This suggests that the term "Alexandrian witness" is sometimes being used without being clearly defined. For the book of Jude the figures suggest that the Byzantine text is a marginally better witness to the supposed original text according to Nestle and Aland than the two primary Alexandrian witnesses.

My conclusion is that the supposed superiority of any manuscript should be demonstrated separately for each individual book.

Significance of manuscripts with mixed readings

I wonder whether more weight should be given to the witness of manuscripts with mixed readings, as compared to those representing a particular text type. One can construct hypotheses which would suggest that manuscripts with mixed readings reflect an earlier archetype than those belonging to a particular text type.





I will set out two such hypotheses below, one assuming that the Alexandrian text type is closest to the originals, the other assuming that the Byzantine text type is closer to the originals

Alexandrian text type closer to originals

For the purpose of this hypothesis, we assume that the autographs were fairly similar to the Alexandrian text type. From these originals, different chains of transmission gave rise to differing subtypes. The manuscripts in one chain of transmission underwent various changes which gave rise to a proto-Alexandrian archetype. This archetype included many of the typical Alexandrian readings which are presumed to be non-original, as well as the great majority of Alexandrian readings which are presumed to be faithful to the autographs. From this archetype arose the Alexandrian text type.

Another chain of transmission underwent some of the changes typical of the Western text type. This gave rise to an archetype with mixed Alexandrian and Western readings.

Most of the Alexandrian readings in this archetype would be faithful to the autographs, whereas most Western readings would represent recensional changes. This archetype would be the ancestor of manuscripts with a mixture of Western and Alexandrian readings. Several chains of transmission from this archetype underwent another stage of changes typical of the Western text type. These gave rise to archetypes of the Western text type. I use the plural “archetypes” because of the diversity found in the Western text type. Note that according to this hypothetical reconstruction of textual transmission, texts with mixed Western-Alexandrian readings go back to an earlier archetype than those with Western readings.

A similar scenario is envisaged for the development of the Byzantine text type. From the Alexandrian-like autographs, there was another chain of transmission which underwent some of the changes typical of the Byzantine text type. This gave rise to an archetype with mixed Alexandrian and Byzantine readings. Most of the Alexandrian readings in this archetype would be faithful to the autographs, whereas most Byzantine readings would represent recensional changes.

This archetype would be the ancestor of manuscripts with a mixture of Byzantine and Alexandrian readings. One chain of transmission from this archetype underwent another stage of changes typical of the Byzantine text type. These gave rise to an archetype of the Byzantine text type. Note that according to this hypothetical reconstruction of textual transmission, texts with mixed Byzantine-Alexandrian readings go back to an earlier archetype than those with Byzantine readings.

What about manuscripts with a mixture of Western and Byzantine readings? Under the present hypothesis two possibilities of the origins of such manuscripts present themselves. One possibility is that both the Byzantine and Western archetypes derive from a common earlier archetype which had some typical Byzantine readings and some typical Western readings.

But this possibility introduces complications: if such a “mixed” archetype was the ancestor of both the Western and Byzantine text types, why would the typical Western readings be lost in the chain of transmission that gave rise to the Byzantine text type, and why would the typical Byzantine readings be lost in the chain of transmission that gave rise to the Western text type?

The second possibility is that a manuscript with a mixture of Western and Byzantine readings is a manuscript that was copied after both the Western and Byzantine text types were established. It could have been derived from a manuscript copied from a Western manuscript and then corrected from a Byzantine manuscript, or vice versa. It would thus represent a type of eclectic text, which some readings taken from one text type, and others taken from the other text type. Such a manuscript would be the result of a secondary editorial process, and hence perhaps less valuable than a manuscript going back to an early archetype.





On the other hand, since this eclectic procedure may have been carried out only a few centuries after the autographs were written, it is possible that the editors may have used relatively good manuscripts which preserved many original readings.

Byzantine text type closer to originals

The parallel hypothesis is that the autographs were fairly similar to the Byzantine text type. From these originals, different chains of transmission gave rise to differing subtypes. The manuscripts in one chain of transmission underwent various changes which gave rise to a proto-Byzantine archetype. This archetype included many of the typical Byzantine readings which are presumed to be non-original, as well as the great majority of Byzantine readings which are presumed to be faithful to the autographs. From this archetype arose the Byzantine text type.

Another chain of transmission underwent some of the changes typical of the Western text type. This gave rise to an archetype with mixed Byzantine and Western readings. Most of the Byzantine readings in this archetype would be faithful to the autographs, whereas most Western readings would represent recensional changes. This archetype would be the ancestor of manuscripts with a mixture of Western and Byzantine readings. Several chains of transmission from this archetype underwent another stage of changes typical of the Western text type. These gave rise to archetypes of the Western text type. According to this hypothetical reconstruction of textual transmission, texts with mixed Western-Byzantine readings go back to an earlier archetype than those with Western readings.

A similar scenario is envisaged for the development of the Alexandrian text type. From the Byzantine-like autographs, there was another chain of transmission which underwent some of the changes typical of the Alexandrian text type. This gave rise to an archetype with mixed Byzantine and Alexandrian readings.

Most of the Byzantine readings in this archetype would be faithful to the autographs, whereas most Alexandrian readings would represent recensional changes (such as making longer readings more succinct). This archetype would be the ancestor of manuscripts with a mixture of Alexandrian and Byzantine readings.

One chain of transmission from this archetype underwent another stage of changes typical of the Alexandrian text type. These gave rise to an archetype of the Alexandrian text type. Note that according to this hypothetical reconstruction of textual transmission, texts with mixed Alexandrian-Byzantine readings go back to an earlier archetype than those with Alexandrian readings.

What about manuscripts with a mixture of Western and Alexandrian readings? Under the present hypothesis two possibilities of the origins of such manuscripts present themselves.

One possibility is that both the Alexandrian and Western archetypes derive from a common earlier archetype which had some typical Alexandrian readings and some typical Western readings. But this possibility introduces complications: if such a “mixed” archetype was the ancestor of both the Western and Alexandrian text types, why would the typical Western readings be lost in the chain of transmission that gave rise to the Alexandrian text type, and why would the typical Alexandrian readings be lost in the chain of transmission that gave rise to the Western text type?

The second possibility is that a manuscript with a mixture of Western and Alexandrian readings is a manuscript that was copied after both the Western and Alexandrian text types were established. It could have been derived from a manuscript copied from a Western manuscript and then corrected from a Alexandrian manuscript, or vice versa. It would thus represent a type of eclectic text, which some readings taken from one text type, and others taken from the other text type.





Implications of the hypotheses

If either of these hypotheses bear some partial resemblance to the actual history of textual transmission, a number of implications arise. The main point is that manuscripts representing mixed text types may well derive from archetypes which are earlier than the archetype of manuscripts which are purer representatives of a particular text type. This suggests that when weighing external evidence, manuscripts representing mixed text types should be given equal consideration to manuscripts representing a particular text type.

It is unclear to me to what extent this is done when text critics weigh the variant readings and choose the one they feel is most likely to be the original text. I get the impression when I read the discussions in Metzger (1994), and various discussions of the relative merits of the various text types, that most of the attention is on the manuscripts belonging to the major text types, and less attention is given to manuscripts of mixed text type.

Perhaps one could argue that the opposite should be the case. One can use the concept of assigning votes to the question of the relative weight of external evidence. Since manuscripts belonging to a common text type are genealogically derived from a common archetype, their combined witness should arguably be only given one vote.

Since a single mixed text-type manuscript represents a different genealogical stemma than the text type, it should arguably be given one vote. Unless two mixed text-type manuscripts are quite similar to each other, each one should be given a separate vote.

Manuscripts of mixed text type

The following is a list of manuscripts which are described as being of mixed text type.

Mixed Alexandrian-Western

- \mathfrak{P}^{66} (fragments of John), c. A.D. 200. Metzger and Ehrman (2005:57) say the text “is mixed, with elements that are typically Alexandrian and Western.”
- Ψ (Gospels, Acts, Epistles), 9th or 10th cent. Metzger and Ehrman (2005:85) state, “its text in Mark... [has] readings both Alexandrian and Western.”

Mixed Alexandrian-Byzantine

- C (Gospels, Acts, Epistles), 5th cent. Metzger and Ehrman (2005:70) describe it as “agreeing frequently with secondary Alexandrian witnesses but also with those of the later Koine or Byzantine type.”
- X (Gospels) 10th cent. Metzger and Ehrman (2005:80) state, “Though its text is mainly of the Byzantine type, it also contains occasional readings of an earlier type, akin to Alexandrian.”
- Ψ (Gospels, Acts, Epistles), 9th or 10th cent. Metzger and Ehrman (2005:85) state, “The other Gospels [besides Mark] are predominantly Byzantine, with a somewhat larger proportion of Alexandrian readings than in Δ .”
- 1241 (Gospels, Acts, Epistles), 12th cent. Metzger and Ehrman (2005:90) state, “In the Gospels, its text has some agreements with C, L, Δ , Ψ , and 33. According to Kirsopp Lake, in Matthew and Mark its text shows a larger infusion of Byzantine readings than in Luke and John.”





Mixed Byzantine-Western

- E^a (Acts), 6th cent. Metzger and Ehrman (2005:74) state, “exhibits a mixture of types, sometimes agreeing with Codex Bezae but more often with the Byzantine type.”

Mixed Byzantine-Caesarean

- M (Gospels), 9th cent. Metzger and Ehrman (2005:77) state, “contains a text that is mainly Byzantine but with admixture of Caesarean readings as well.”
- N (Gospels), 6th cent. Metzger and Ehrman (2005:79) state, “belongs predominantly to the Byzantine type, but it preserves a number of readings of earlier types...a weak member of the Caesarean type.”
- Σ (Matthew, Mark), 6th cent. Metzger and Ehrman (2005:84) state, “closely akin to that of N, agreeing frequently with the Byzantine type of text but with certain Caesarean readings as well.”
- Φ (Matthew, Mark), 6th cent. Metzger and Ehrman (2005:84) state, “generally of the Koine type, but it contains the long Western addition after Matt. 20.28...a tertiary witness to the Caesarean text.”

Other mixed

- P⁴⁷ (Revelation), 3rd cent. Metzger and Ehrman (2005:55) say “the text...agrees more often with that of Codex Sinaiticus than with any other, though it often shows a remarkable independence.”

Text types derived from multiple ancestors

When many manuscripts are similar enough to each other to be regarded as belonging to one text type, the most straightforward hypothesis is that all of them were descended from the same archetype.

Because text types are usually defined in terms of combined manuscripts containing several books, this implies the existence of an archetype manuscript which is dated later than the autographs. For example, the Byzantine text type for the Gospels is characterized by a certain set of readings in Matthew, a certain set in Mark, a certain set in Luke, and a certain set in John. Hence the archetype gospel manuscript for the Byzantine text type would have been a particular manuscript which contained all four Gospels and which had those sets of readings. Even if we accept the hypothesis of the proponents of Byzantine priority that the Byzantine text type is very close to the autographs, there would still be a necessary distinction between the Byzantine archetype and the autographs.

All the changes which occurred during the chains of transmission of the individual gospel manuscripts before they were incorporated into a combined manuscript would distinguish the readings of the Byzantine archetype and the readings of the autographs. And if we accept the hypothesis of the proponents of the Alexandrian text type, namely that the Byzantine text type is based on a relatively late recensional or eclectic archetype, then there would be many more differences between the archetype and the originals.

A similar argumentation can be made for the Alexandrian text type. Even if it has relatively few recensional readings, and is a relatively pristine text, it still would derived from archetype manuscripts which contained multiple individual books. We probably need to think of at least three or four archetype manuscripts as being the ancestors of a text type which is defined for all the New Testament books. This is because the combined manuscripts usually only included one portion of the New Testament.





To cover the whole New Testament one would might need to bring together the following: a Gospels manuscript, a Pauline Epistles manuscript, an Acts manuscript, a Catholic Epistles manuscript, and a Revelation manuscript (some of the latter three might be combined together). Hence one could talk in terms of one set of archetype manuscripts from which the text type is derived.

It is also possible to imagine that a text type could be derived from multiple ancestor manuscripts rather than from one set or archetypes. I will illustrate this from the Alexandrian text type. Let us assume for sake of argument that the Alexandrian text type is quite close to the autographs. In this case, it is possible that separate Alexandrian Gospel manuscripts do not derive from the same archetype. Rather one can imagine a scenario in which two Alexandrian manuscripts containing the Gospels, say **Σ** and B, derive from separate ancestor Gospel manuscripts.

Let us imagine that copies of the individual Gospels are copied and recopied until four of them are incorporated into the combined manuscript which I will call Proto-**Σ**-Gospels. During this process certain alterations occur in the text, creating new variant readings.

At the same time, four somewhat separate chains of transmission produce four gospel manuscripts which are incorporated into Proto-B-Gospels. During this process certain alterations occur in the text, creating new variant readings, which are different from some of the readings that end up in Proto-**Σ**-Gospels.

According to this scenario, the number of alterations which occur during the process of copying between the autographs and the Alexandrian ancestor manuscripts Proto-**Σ**-Gospels and Proto-B-Gospels are relatively few. In a large majority of readings, Proto-**Σ**-Gospels and Proto-B-Gospels agree. This is why they are regarded as belonging to the same text type.

The scenario imagined above suggests that if a text type is close to the originals, it may be descended from more than one ancestor manuscript. I illustrated this with two combined gospel manuscripts being the ancestors of the two main Alexandrian manuscripts which contain the Gospels. One could imagine a similar scenario for the Pauline Epistles.

It is conceivable that several Alexandrian manuscripts which contain the Pauline Epistles, such as **Σ**, B, **℘**⁴⁶, A, and I, all descended from separate ancestors. These ancestor manuscripts would be the first combined manuscript containing the various Pauline epistles which was the ancestor of each of these later manuscripts. These could be called Proto-**Σ**-Pauline, Proto-B-Pauline, Proto-**℘**⁴⁶, and so on.

Under this scenario, the text type would not derived from one set of archetype manuscripts, but from multiple sets of ancestor manuscripts. There might be several Gospel ancestor manuscripts, several Pauline Epistles ancestor manuscripts, several Acts ancestor manuscripts, several Catholic Epistles ancestor manuscripts, several Revelation ancestor manuscripts.

There is no reason why the numbers or archetype manuscripts would be the same for the various portions of the New Testament. Or it might be that the ancestors of Alexandrian manuscripts include two Gospel ancestor manuscripts, three Acts ancestor manuscripts, but only one Pauline Epistles ancestor manuscript. All sorts of possibilities are conceivable. The more difficult question is as to whether there are types of evidence which could shed light on the probable number of archetype manuscripts there might have been for different portions of the New Testament.

Direct descent from autographs

Robinson says (2009a:9) "The one thing that is certain is that all three major texttypes (Alexandrian, Western, Byzantine) cannot all be the direct descendant from the autograph, and therefore either one





texttype only must represent the autograph, with the others reflecting recensions; or none of the existing texttypes represent the autograph, and are all later hyparchetypal developments, with whatever the autograph may have been being totally 'lost' and dispersed equally or unevenly among the three existing major texttypes."

I want to speculate as to what it means to be a direct descendant from the autograph. I wonder how it would relate to my speculations above regarding a text type being derived from multiple ancestor manuscripts.

Robinson and Pierpont (1991) state, "except for a few small 'family' relationships which have been established, the bulk of the Byzantine-era documents are not closely-related in any genealogical sense. A presumption, therefore, is toward their relative independence from each other rather than their dependence upon one another. This makes the Byzantine majority of manuscripts highly individualistic witnesses which cannot be summarily lumped together as one 'mere' texttype, to be played off against other competing texttypes."

My guess is that one can integrate these statements with my speculations about multiple ancestor manuscripts. If the Byzantine manuscripts are claimed not to descend from a common archetype, and to be unrelated genealogically, this could mean that many of them are supposed to be descended from separate sets of ancestor manuscripts. For the Gospels, there were multiple ancestor manuscripts, all rather similar to each other, but distinct, and from each of them there was a chain of transmission to a different genealogically unrelated Byzantine manuscript. In a similar way there would be multiple ancestor manuscripts of the Pauline Epistles, and for the other portions of the New Testament.

Under this scenario, the remark "represent the autograph" would be interpreted to mean that a particular text type was derived from multiple sets of ancestor manuscripts of combined books, all of which were relatively close to the autographs. This might be because the chains of transmission of each individual book that ended up in the ancestor manuscripts were relatively free from recensional alterations. In contrast to this, the other text types are supposedly derived from a single set of archetypes which had undergone considerable recensional alterations.

According to this understanding, the reason the Byzantine manuscripts "cannot be summarily lumped together as one 'mere' texttype", is because many of them derive from distinct ancestor manuscripts, and hence each should be given due weight in evaluating the external evidence for a reading.

Each manuscript or group of related manuscripts deriving from a separate ancestor manuscript represents a different genealogical stemma and hence, an independent witness to the autographs.

This raises the question as to how many different genealogical stemmata are supposed to exist among the Byzantine manuscripts according to the proponents of Byzantine priority. Is it a dozen, or dozens, or hundreds? What sort of evidence could give an indication of how many genealogically distinct stemmata there might be, each presumably going back to a different ancestor manuscript?

I do not know whether the above speculations tally with any scholarly speculation in the literature, or whether they have any validity as a reasonable hypothesis.

Causes of variant readings within a text type

As a follow-on from the above discussion, I want to discuss the question of various hypotheses which might explain variant readings within a text type. For example, how can we explain cases in which the Byzantine text type is divided between two readings? Or cases in which primary Alexandrian witnesses such as **Σ** and **B** have different readings? For the sake of clarity I will reiterate some of the discussion already found above.





Perhaps the most straightforward hypothesis is that in the early stage of copying the text type archetype, a scribe introduced a new reading into one manuscript. This new reading was propagated by further copying to a certain proportion of manuscripts belonging to the text type.

Within the Byzantine text type, if most manuscripts support one reading and relatively few manuscripts support the other, the principles of external evidence would suggest that the majority reading is more likely to be the original reading of the archetype manuscript. If the numbers are relatively evenly divided, internal evidence would have more weight.

Within the Alexandrian text type, it is more difficult to use external evidence in this way due to the small number of primary Alexandrian witnesses. Internal evidence is generally the main way to decide between the readings.

What if particular readings are supported by manuscripts outside the text type? How can we interpret that evidence?

Let us consider the case of the Byzantine text type. For the sake of argument, let us assume that the Byzantine archetype manuscript was a relatively late recensional manuscript. By the time it came into existence, most of the readings found in other text types, such as the Western or Alexandrian, were already in existence.

Subsequent to the time of the Byzantine archetype manuscript, a scribe introduced a new reading into one of its descendants. Either the new reading would be a unique reading, not found in any other text type, or it would be the same as a reading found elsewhere.

If the scribe were merely copying one manuscript, without consulting any others, it is more likely the reading would be unique. An exception to this would be if the new reading was an obvious correction of a difficult reading, in which case the scribe might independently duplicate a correction made in another text type. On the other hand, if the scribe was consulting another manuscript from a different text type, he might correct the Byzantine manuscript based on the other manuscript, and thus introduce a reading from the other text type into one portion of the Byzantine text type.

There is an alternative hypothesis that can explain how readings from outside a text type could match two separate readings within a text type. I will illustrate this from the Alexandrian text type. Let us assume for sake of argument that the Alexandrian text type is quite close to the autographs. In this case, it is possible that separate Alexandrian manuscripts do not derive from the same archetype. As discussed above, they might derive from separate ancestor manuscripts.

Let us restrict our discussion to the gospels. As discussed above, let us imagine that there are two ancestor manuscripts, Proto-**N**-Gospels and Proto-B-Gospels. We imagine that copies of the individual gospels were copied and recopied until four of them are incorporated into Proto-**N**-Gospels. During this process certain alterations occurred in the text, creating new variant readings. At some later point in the chain of transmission, a copy of an individual gospel was made which later becomes the ancestor of a manuscript of the Western text type. Hence the same variant reading would be found in the Western manuscript as well as in Proto-**N**-Gospels.

At the same time, we imagine that four somewhat separate chains of transmission produced four gospel manuscripts which were incorporated into Proto-B-Gospels. During this process certain alterations occurred in the text, creating new variant readings, which were different from some of the readings that end up in Proto-**N**-Gospels. At some later point in the chain of transmission, a copy of an individual gospel was made which later becomes the ancestor of a manuscript of the Byzantine text type. Hence the same variant reading would be found in the Byzantine manuscript as well as in Proto-B-Gospels.





After Proto-**N**-Gospels and Proto-B-Gospels were produced, there would be further copying and recopying of Proto-**N**-Gospels and Proto-B-Gospels until **N** and B were produced. During this process further alterations occur. Many of the readings of **N** which are unique or supported by only a handful of witnesses would presumably have occurred during this stage of the copying process, and similarly for B.

The scenario sketched above suggests that variants in which **N** differs from B and one of the variants has little or no outside support are more likely to be recent alterations in the chain of transmission which produced **N** or B. Whereas in places where the reading of **N** differs from B, and some outside witness support **N**, whereas others support B, it could be that the variant reading was introduced at an early stage of copying before the combined ancestor manuscript was produced, and was propagated to two different text types.

An alternate hypothesis to explain why both Alexandrian readings are found in manuscripts of other text types would be as follows. Let us presume that there is a place where the readings of Proto-**N**-Gospels and Proto-B-Gospels are different, and Proto-**N**-Gospels preserves the original reading. It is natural, then, that the reading of Proto-**N**-Gospels would be found in another text type. The reading of Proto-B-Gospels could enter another text type if a manuscript which was the ancestor of that text type was corrected based on the reading in Proto-B-Gospels. This process would be more feasible if that text type was based on a relatively late archetype, and/or was the result of a deliberate eclectic editing process, as is claimed to be the case for the Byzantine text type.

Is there any evidence that might shed light on the probable origin of variant readings within a text type? I will suggest a few possibilities which occur to me. First, when manuscripts of a text type are divided between different readings, and in a large proportion of cases, one of the readings has little or no support from manuscripts from outside the text type, this might tend to suggest that it is more likely text type is derived from only one set of archetypes. The variants might be more easily explained as being due to alterations done during the chain of transmission from the archetype to the extant manuscripts.

One the other hand, if in a large proportion of cases, both variant readings have significant support from manuscripts outside the text type, it is harder to draw any conclusions. It is unclear how one could distinguish between the possibilities that the variants occurred before the multiple ancestor manuscripts were produced, or as a result of cross-correction after the single set of archetype manuscripts were produced.

Proportion of differences between individual books

I want to suggest one type of evidence which might give some indication as to whether a text type was derived from one set of archetype manuscripts, or from multiple ancestor manuscripts (with the implication that the text type is relatively close to the autographs). Let us consider the case of the Pauline Epistles. Let us imagine that all the manuscripts of a particular text type which include Pauline Epistles are descendants of one archetype manuscript containing the Pauline Epistles. This means that any variants among manuscripts of that text type must be due to alterations which occurred during the process of copying subsequent to the time of the archetype manuscript.

And one might expect that the proportion of variant readings occurring in each individual Epistle would be more or less the same. This is because each individual book would have encountered the same set of scribes during the chain of transmission.

If a careless scribe copied one of the manuscripts in the chain of transmission at some point, it is likely that he made about the same proportion of careless mistakes in each individual book he copied. Or if a scribe with recensional tendencies copied one of the manuscripts, it is likely that he made about the same proportion of recensional alterations in each book he copied.





There is probably some sort of statistical test one could use to measure this. Perhaps one could compare two manuscripts of the same text type by counting the number of places their readings differed in each individual epistle, and then calculating the percentage of differences among genealogically significant variants. Then one would apply some statistical measures such as standard deviation and so forth which would indicate whether the figures fell into the normal bell curve that one would expect if all of the manuscripts belong to the same population (that is, they were all derived from the same archetype).

On the other hand, if the two manuscripts derived from different ancestor manuscripts, one might expect the pattern of differences to be different. If one could compare the individual epistles between the two purported ancestor manuscripts one might expect that the proportion of differences between individual epistles would be different. Some epistles in the two ancestor manuscripts might share almost identical readings, whereas others might have a much larger proportion of different readings because of the different chains of transmission those particular epistles underwent as individual books before they were incorporated into a combined ancestor manuscript. These differences in the ancestor manuscripts would be inherited by the descendant documents. If then, when comparing two extant manuscripts of one text type, statistical tests showed that the proportion of different readings in individual epistles varied significantly, this would be an indication that it might be more likely that these two manuscripts were descended from different ancestor manuscripts.

Must the Byzantine majority always be correct?

One disturbing aspect of the Byzantine priority theory is the implication that the original reading will always be found among Byzantine manuscripts. This is ensured by a combination of two factors. The first factor is that Byzantine priority proponents put relatively more weight on external evidence in terms of transmissional history compared to internal evidence. Robinson for example, says (2001:§26), “final judgment on readings requires the strong application of internal evidence *after* an initial evaluation of the external data has been made. Being primarily transmissionally-based, the Byzantine-priority theory continually links its internal criteria to external considerations. This methodology always asks the prior question: does the reading which may appear ‘best’ on internal grounds (no matter how plausible such might appear) *really* accord with known transmissional factors regarding the perpetuation and preservation of texts.”

The second factor is that Byzantine priority proponents claim that Byzantine manuscripts are not all genealogically related to one archetype. Robinson and Pierpont (1991) state, “the bulk of the Byzantine-era documents are not closely-related in any genealogical sense...This makes the Byzantine majority of manuscripts highly individualistic witnesses which cannot be summarily lumped together as one ‘mere’ texttype.”

Because there are a very large number of Byzantine manuscripts, even excluding those which can be shown to belong to related subgroups, when the external evidence is weighed on their scales, it seems that the judgment will always be in favour of a Byzantine manuscript. All the non-Byzantine witnesses seem to be as but dust on the scales.

A problem arises if the internal evidence seems to be strongly against the Byzantine reading, especially if it is a distinctively Byzantine reading, with no external support from non-Byzantine witnesses. Even in such cases, I wonder if the Byzantine priority proponents are forced by their theoretical position to manufacture a counterargument based on the internal evidence, even if it does not seem so plausible.

Robinson and Pierpont state (1991), “Byzantine-priority advocates maintain that a successful internal-evidence case can be made for nearly every Byzantine reading over against the Western, Caesarean, and Alexandrian readings.” If they say “nearly every” reading, I wonder what the exceptions are. Are there a few cases in which the weight of internal evidence against the Byzantine reading is so strong that they would admit that the original reading is found elsewhere? And if they do, what would the implications



be for the theory? For it seems such an admission would be the top of a steep slippery slope. It would be an admission that an inaccurate reading can successfully spread to the extent that it is found in a majority of manuscripts, a possibility the Byzantine priority proponents tend to discount. For example, Pickering (n.d. Ch. 5) says, "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." Yet if it could happen in one case, why not in others?

I wonder whether there is theoretically valid middle position, between the position of the proponents of the Alexandrian text, who regard the Byzantine text as corrupt and of little value for determining the original text, and the position of the Byzantine priority proponents, who seem to regard it as the only important text type to consult.

Thoroughgoing eclecticism, which "gives almost exclusive consideration to the style of the author and the demands of the context" (Metzger and Ehrman 2005:223), seems to be in such a middle position, but this approach virtually ignores external evidence, which seems dubious from the theoretical point of view. It is unclear to me what the theoretical underpinnings of such a middle position should be.

Conclusion

In a personal email, Robinson said "I would be interested in learning precisely what it would take in his mind to either (a) discredit the eclectic text, (b) accredit the Byzantine text, or (c) reject both as viable alternatives. It could be interesting to see what level of proof might be desired in such a case."

Let me try to spell out several types of evidence which I feel might significantly strengthen the case for Byzantine priority. I will try to focus on evidence which it should be possible to ferret out from the extant manuscripts. With regards to level of proof, one always hopes that one can find more and more convincing evidence that will gradually increase the level of proof in one direction or another. As the level of proof increases, one leans increasingly to whatever hypothesis has the greater amount of evidence.

1. A detailed study of the genealogical relationships between Byzantine manuscripts. The results of such a study could strengthen or weaken the claims that many Byzantine manuscripts should be regarded as genealogically independent witnesses to the autographs.
2. A detailed examination of the extent of Byzantine, Alexandrian, Western and other readings found in the ante-Nicene Fathers, exploring different hypotheses which might explain the data.
3. Detailed explanation to back up the claim that a successful internal-evidence case can be made for nearly every Byzantine reading over against the Western, Caesarean, and Alexandrian readings. Perhaps it would be sufficient to choose several dozen readings for which the internal evidence for the Byzantine reading seems particularly weak and see whether a strong argument can be made for the originality of the Byzantine readings.
4. Detailed explanation to discount the claims that the Byzantine text type is characterized by many harmonizations which are demonstrably secondary. The first step would be to evaluate the argument of Wisselink ("Assimilation as a Criterion for NT Textual Criticism").
5. Use the manuscripts of the Vulgate as a test case to test the claim that it is highly unlikely for a non-original reading to become the majority reading. This is based on the assumption that it is easier to determine the likely original readings of the Vulgate than it is of the Greek New Testament. One could then try to quantify the degree to which readings which seem virtually certainly non-original ever manage to become the majority reading.

6. As described in section 2.6.3, conduct research using statistical measures to compare a sample of manuscripts of the Byzantine text type by counting the number of places their readings differed in each individual epistle or gospel, and then calculating the percentage of differences among genealogically significant variants. If the proportion of different readings in individual epistles or gospels varied significantly, this would be an indication that it might be more likely that these manuscripts were descended from different ancestor manuscripts, and hence possibly closer to the autographs.

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