5-1-1949

The Ending of St Mark's Gospel A Study in Textual Criticism

Henry Hamann

THE ENDING OF ST. MARK'S GOSPEL

A Study in Textual Criticism

A Thesis Presented to

the Faculty of Concordia Seminary

Department of New Testament

In Partial Fulfilment

of the Requirements for the Degree

Master of Sacred Theology

By

Henry P. Hamann 1916-1988.

May, 1949

Approved by: W. Arndt

Paul M. Bretscher

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A Study in Textual Criticism

The textual critic of the New Testament is a detective on a heroic scale. A veritable multitude of facts is supplied him in thousands of Greek manuscripts, in translations with their various manuscripts, in quotations from ecclesiastical writers. To add to the complication, all this testimony is merely hearsay evidence. The original impeccable witnesses are no longer, and lost manuscripts, like dead men, tell no tales. We have their evidence only at second or third or who knows how-manyeth hand; hundreds of years separate most of this hearsay evidence from the original source. The witnesses that now exist are, moreover, truthful and untruthful by turns. Some are more untruthful than others, but all of them are so far from being unswervingly truthful that no reliance can be placed on the sole testimony of any of them when a specific case is to be tried. Neither is there any pattern in the truthfulness or untruthfulness of any witness. They arbitrarily bear now true, now false witness; they are, so to speak, stupidly unreliable and unreasoningly contradictory. All of which might

well drive even expert investigators to despair.

Various short-cuts have been suggested through this welter of assertion and contradiction, this maze of truth and falsehood: sheer weight of numbers, the good breeding of certain witnesses, and, recently, neglect of the witnesses altogether except for the ascertaining of their divergent witness combined with a judgment based wholly on the likelihood of the witness they offer. The most famous firm of criminal investigators in these matters for the last seventy years has been that of Westcott and Hort. These sleuths enunciated their principles with great skill and learning, with force and precision, and they have their followers down to the present day, who, in spite of various modifications in the original conclusions, still praise and employ their methods. Like good Englishmen Westcott and Hort were aristocratic in their tastes. They applied the words of Horace to the great mass of manuscripts

Odi profanum vulgus et arceo

and extolled the good breeding and blue blood of the few. Even at the height of their popularity they had their opponents, some extremely noisy like Burgon and Miller, and since then opposition to their conclusions as well as to their methods has been increasing. Many experts are convinced that Westcott and Hort have falsely procured the condemnation of many excellent and eminently worthy members of Biblical society in the scholarly world and wish to see these members take

their place again with an honor that is unbesmirched. With one of these members the present investigation has to deal. The writer, rather immodestly it may be held, can not accept the principles enunciated by WH and accepted by many textual critics since, and accordingly a rather large section of the present study is given over to a critical analysis of the principles of WH and the formulation of other critical principles, which form the foundation upon which the real work is raised. Pardon is asked by the writer for not quoting the witnesses directly - he has no training in the reading of ancient manuscripts - but only their reported statements in the critical apparatuses of Souter, Nestle, Merk, Tischendorf, von Soden, Huck and various others who have investigated these cases before. A comparison of all these apparatus critici renders it pretty certain that the existing witnesses to the text have not had their uncertain witness garbled and distorted into the bargain. A description of some of the more striking witnesses is given in the Sigla, which list has been compiled partly from Souter[1] and partly from Huck. For more complete character studies one must consult the large works of Tischendorf, Gregory and von Soden.[2]

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1. Souter A., The Text and Canon of the New Testament.
2. See the bibliography.

Sigla

Greek Manuscripts.

S — This symbol I use for the more common ℵ. This is Tischendorf's famous Codex Sinaiticus, now in the British Museum. It contains the O.T. nearly complete and the N.T. complete, with Barnabas and Hermas. One of our best MSS after B. Fourth Century (fifth?).

B — Codex Vaticanus, containing the O.T. with some 51 missing pages in the beginning of Genesis and in the Psalms and the N.T. as far as Hebr. 9:14. Our oldest uncial, from the Fourth Century.

A — Codex Alexandrinus, also in the British Museum; Fifth Century. Its text in the Gospels is not regarded very highly.

C — Codex Ephraemi; Fifth Century. A palimpsest containing the O.T. and N.T. but with many gaps. With S and B representative of von Soden's H-text.

D — Codex Bezae; Sixth Century (Fifth?). A Graeco-Latin MS, the Greek to the left; not in the University of Cambridge. Our chief Greek representative of WH Western text, and remarkable for its interpolations in Acts particularly.

W — A complete codex of the Gospels, which it contains in the Western order: Matthew, John, Luke, Mark. Fifth Century; famous for its addition to Mk. 16:14, the "Freer Logion".

(Θ) — Codex Koridethei; Seventh-Ninth Century. The Gospels nearly complete. One of the main sources of the Caesarean form of text.

L — Codex Regius from the Eighth Century; belongs to von Soden's H-text. The Gospels almost complete.

Fam.1 — This is a group of minuscules, including 1, 118, 131, 209, etc.,; also known as the "Lake Group", Caesarean text.

Fam.13 — The "Ferrar Group", consisting of minuscules 13, 69, 124,230, 346, 543, 788, 826, 983, 1689, 1709. These MSS are distinguished by the position they give to the pericope de adultera, viz. after Luke 21:38. Caesarean text.

Latin Manuscripts.

it Itala, the Old Latin Version as reconstructed by Adolf Juelicher.

a Vercellensis; V Century; some gaps in all Gospels; a good text, belongs to European family.

b Veronensis; IV/V Century; defective; European.

d Cantabrigiensis; the Latin text of D; not a translation of it.

e Palatinus; V; a very good text; African family.

f Brixianus; VI; approximates the Vulgate closely.

k Bobiensis; IV; the most valuable it MS, the chief representative of the African family; much damaged, containing only Matt. 1:1-15:36 and Mark 8:8-16:8.

Other Versions.

Syr. Sin. The old Syriac version as found in the palimpsest discovered in St. Catherine's Monastery on Mt. Sinai.

Syr. Cur. The Old Syriac as found in the Curetonian MS found in Egypt. This is a little more recent than the Sinaitic. These two MSS Burkitt edited in the Evangelion Da-Mepharreshe.

bo The Bohairic Version, representing the northern Coptic translation.

sa The Sahidic Version, representing the southern Coptic translation. Both edited by G. Horner, 1898 and 1911 respectively. sa is the older.

Other abbreviations.

MS (MSS) Manuscript(s), whether majuscule or minuscule.

WH Westcott and Hort.

JBL The Journal of Biblical Literature

HTR The Harvard Theological Review

ATR The Anglican Theological Review

TR Textus Receptus

Chapter One. Criticism of Westcott and Hort's Theory of the New Testament Text.

The Introduction of WH The New Testament in the Original Greek is a beautifully planned and brilliantly written work. The fundamental principles upon which the whole theory is based and from which the final conclusions are gradually and skilfully developed are enunciated by WH as follows:

> "Knowledge of documents should precede final judgments upon readings."[1]
>
> "All trustworthy restoration of corrupted texts is founded on the study of their history."[2]

It will be convenient for our purposes to take the second of these principles first and to see what results for the text flow from its application.

WH insist that documents should not be treated independently of each other but should be examined connectedly as forming parts of a single whole in virtue of their historical relationships. All documents form part of a genealogical tree of transmission. It is the business of textual criticism to assign to each document its proper place on that tree, whereby a historical picture of the whole complex transmission is gained as well as material of the most objective character for arriving at the original text. Up to the time of the writing of WH monumental work, 1882, much weight had been

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1. Westcott and Hort, The New Testament....., p. 31
2. WH, op. cit., p. 40

attached in certain quarters to the number of MSS attesting any particular reading. The principle of genealogy sets mere numbers in their proper perspective. If ten MSS can be shown to have a uniform text - and community in startling variations indicates this much in the same fashion that the teacher detects cheating by a series of strange errors in different papers - then, plainly, they have a common ancestor, and they are no longer ten witnesses to a reading, but one only. Besides setting mere number in its proper perspective genealogy can lead us back to earlier and earlier texts. As WH sum up the process,

> "The proper method of genealogy consists, it will be seen, in the more or less complete recovery of the texts of successive ancestors by analysis and comparison of the varying texts of their respective descendants, each ancestral text so recovered being in its turn used, in conjunction with other similar texts, for the recovery of a yet earlier common ancestor."[3]

Ideally, even a small number of documents would suffice for a complete restoration of an autograph text except for the earliest variations by genealogy alone, provided that the documents preserved were adequately representative of different ages and different lines of transmission. WH go so far as to claim:

> "So far as genealogical relations are discovered with perfect certainty, the textual results which follow from them are perfectly certain, too, being directly involved in historical facts; and any apparent presumptions against them by other methods are mere guesses against knowledge."[4]

But such perfect genealogical relations can not be discovered,

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3. WH, op. cit., p. 57
4. WH, op. cit., p. 63

do not exist. The greatest single factor complicating the genealogical tree is the fact of mixture. Documents, like human beings, have not fathers and grandfathers only, but frequently a number of grandparents. To overcome the complication caused by mixture in documents, and almost all of them contain mixture in varying degrees, WH fell back on what are known as 'conflates'. When we find a reading in three variations, two of which are simple alternatives to each other, while the third is a combination of the other two, the presumption is that the third is the last of the three and due to mixture, not that the third is the earliest and the other two due to independent simplifications of the text. If we find certain groups of documents habitually exhibiting the conflate readings, while other groups habitually avoid them, we are safe in asserting that the one set of documents certainly possesses a mixed text, while the other two sets still preserve some portion at least of two more ancient texts which were later fused together. We can go further and judge that, even in cases where no conflation is to be found, the mixed text merely supports one or the other of the variants but is not in itself an independent witness to the original text, the "documentary authority for the two variants respectively being then virtually reduced to that of the two groups habitually preserving the separate factors of mixture."[5]
In spite of this ingenious method of overcoming some of the

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5. WH, op. cit., pp. 51-52

difficulties caused by mixture the presence of mixture in gret abundance does make genealogical processes doubtful. To this complication must be added another fact that compels us to modify the claims made for the genealogical method, and that is the fact that our oldest texts at which we can arrive by that method are still separated from the original autographs by many ages of transmission, involving possibilities of corruption. All this is admitted by WH, but the claim is still made that

> "Genealogical presumptions ought however to take precedence of other presumptions, partly because their immediate basis is itself historical not speculative, and the subject matter of all textual criticism is historical, partly because the generalizations by which that historical basis is ascertained involve less chance of error than the analogous generalizations required for any kind of Internal Evidence."[6]

The application of this genealogical process reveals, first of all, the startling fact that almost all our MSS, both uncials and cursives, can in no way be considered independent witnesses to the text. For they all exhibit a text which was essentially an eclectic recension from the beginning of the fourth century, and which, gaining ecclesiastical favor, soon swept all competitors from the field, and became the famous Textus Recceptus of Stephanus, which underlies the Authorized Version of King James I in 1611. This text WH call the Syrian text. Modern critics refer to it as the Byzantine or Koine text and in most critical editions it is referred to by the symbol of a dark K. Proof for the assertion that the "Syrian text is only a modified

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6. WH, op. cit., p. 63

eclectic combination of earlier texts independently attested"[7] WH find in the fact that analysis of the readings of the Syrian text reveals the presence of a great number of conflate readings, and in the further facts that the Syrian readings lack all Ante-Nicene patristic evidence and that internal evidence is unfavorable to its variants. Besides this Syrian text WH distinguished three other texts: Western, Neutral and Alexandrian. The Western text they find attested in D, the MSS of the Itala and the Old Syriac, the Greek Ante-Nicene Fathers, with the partial exception of the Alexandrian divines, and the Latin Fathers. Occasional support for Western readings is found in S X Fam. 1 and Fam. 13. Neutral readings are preserved in B S T (in Luke and John) L 33 Δ (Mark) R (Luke) Q P. The Alexandrian readings are found in the combination S C L X 33; ≡ and R (in Luke), sa bo. The least inconstant supporters are C L bo. Besides these texts WH claim that there are a large number of variants for which it is difficult to assign a definite genealogy, the reasons for which fact are to be sought in the mixed composition of some of the principal documents and the not unfrequent opposition of documents usually agreeing.

There follows now a description of the four texts. The Syrian text has already been characterized well enough. As for the Western text, this text was not only the most widely-spread text of Ante-Nicene times, but it has the earliest readings which can be fixed chronologically. However,

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7. WH, op. cit., p. 118

except for those readings of the Western text which WH call "Western Non-Interpolations" - a strange term this, and one invented solely for the purpose of rescuing the fair name of the Neutral text, for Western Non-Interpolations are actually according to WH view, Neutral Interpolations - WH view the Western text with great distrust. In it they detect a love of paraphrase, the interpolation of phrases extending by a sort of parallelism the language of the true text, a disposition to enrich the text at the cost of purity by alterations and additions from tradition and perhaps non-biblical apocryphal sources, the ordinary tendencies of scribes being more powerfully at work here than elsewhere.

> In surveying a long succession of Western readings by the side of others, we seem to be in the presence of a vigorous and popular ecclesiastical life, little scrupulous as to the letter of the venerated writings, or as to their permanent function in the future, in comparison with supposed fitness for immediate and obvious edification.[8]

The Neutral text with its home in Egypt WH consider a comparatively pure text. A priori Alexandria would be a likely place, with its grammatical school and early Christian scholars, for the preservation of such a text. The versions of Egypt are the only extensive non-Western versions. This non-Western text is attested to also by those Western documents which attest both Western and non-Western readings, that is, mixed documents, the very mixture in this case, presupposing a relatively pure non-Western text. The fact that this early evidence is at once Greek, Latin and Syriac indicates

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8. WH, op. cit., pp. 123-126, the verbatim quotation being on p. 126.

that this non-Western text was not confined to Alexandria or Egypt. Hence the term Neutral is an adequate term for its description. The Alexandrian text, finally, is claimed by WH to be derived from the Neutral. It is supposed to be a degenerate Neutral text dating from the opening of the third century or earlier. Its changes from the Neutral text have more to do with language than matter, a striving for correctness of phrase being its distinguishing mark.

WH have laid the foundation for the history of the text of the New Testament. A summary of that history would run something like this: Early in the second century the Western text was moving towards an ever greater adulteration of the apostolic text which had its surest hold in Alexandria. The attempt was made at Antioch to remedy the growing confusion of text by the editing of an eclectic text combining readings from the three extant texts of the time. This edited text was itself further revised and in that form used by Antiochean divines about 350. This text was established at Constantinople and finally triumphed, leaving relics of its vanquished rivals in certain cursives. At each stage we find irregularities and obscurities. But if it is true, this history gives the key to the complexities of documentary evidence.

It is at this stage of the examination that WH turn to a thorough use of the second of their two great principles:

"Knowledge of documents should precede final judgments upon readings." Knowledge of documents follows upon an extension of the principle of Internal Evidence. We test the worth of a document by an analysis of its readings on disputed points. If a document in the majority of cases favors that reading which external and internal evidence show to be the best or the correct reading, then that document is a good one; if not, not. Similarly, if a group of documents, be it a group of two or more documents, on the whole favors the better readings, then we have a good group: if the opposite is true of a certain group, we have a poor group. When groups or documents are often right, a favorable presumption for them is created and very strong internal evidence is necessary to rebut their evidence. Practically, this means for WH that every group containing B is good, e.g. BS, BL, BC, BT, particularly BS. S in its binary combinations is poor. SD is Western and interesting. B is by far the best single document. Its individualisms are confined to mechanical inaccuracies, its omissions concern petty words, like the article and pronouns, and its other individualisms are simple and inartificial as one would expect from a dull and patient but sometimes negligent transcriber. The individualisms of S are bold and careless, its subsingular readings suspicious. Internal Evidence of Groups and Documents is unfavorable to the singular and subsingular readings of all other MSS and to all binary combinations of other MSS.

Where B and S differ the use of secondary documentary evidence is necessary as well as internal evidence. (Secondary documents are all those apart from B S D and, generally, the documents of the fourth and fifth centuries.) Secondary evidence is valuable in so far as it often proves readings of the primary witnesses to be not individualistic and because it throws back in time the secondary documents themselves, i.e., they represent early MSS in these readings. But the cumulative absence of attestation by late mixed documents is unimportant, because many certain readings lack this attestation.

WH admit, to conclude this review of their work, that there is no royal road to success in this work. They are distrustful of the method of Internal Evidence of Readings because of the uncertainty for which its subjectivism leaves room. They maintain that there is no justification for scepticism as to the possibility of obtaining a " trustworthy genealogical interpretation of documentary phenomena in the New Testament either in antecedent probability of in experience".[9]

> "When it is seen that variations in which decision is free from difficulty supply a trustworthy basis for ascertaining the prevalent character of documents and groups of documents, and thus for estimating rightly the value of their testimony in other places, little room is left for difference of estimate...the general course of future criticism must be shaped by the happy circumstance that the fourth century has bequeathed to us two MSS of which even the less incorrupt must have been of exceptional purity among its own contemporaries."

A period of some seventy years separates us at the present from the year of the publication of WH famous work.

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9. WH, op. cit., p. 287.

During that time much thorough and painstaking work has been done by textual critics and scholars throughout the world, and a mass of facts and pertinent information has been collected which makes it possible for us to subject the theory of WH to a thorough scrutiny. It might appear somewhat unfair and unkind for a mere beginner to sit in judgment and even condemnation over men who have contributed so much to an understanding of textual criticism. But that is but the way of the world. One generation corrects another because of the more complete knowledge to which it falls heir. Besides, the cause of truth is more precious than the reputation of any man no matter how worthy and signal his services may have been. Nor is the criticism of WH theory a mere knocking over of a straw man or, to use a coarser phrase, a mere shooting of a dead dog. On the contrary, WH have their followers still and the issue of Internal Evidence and External Evidence and their respective value has not yet been decided by critics, and it is doubtful whether it ever will. Criticism of the position of WH proceeds partly from the finds and the conclusions of scholars since their time, and partly from weaknesses inherent in the theory itself.

A mere catalogue of the finds since WH will show how important a modifying effect these finds must exert on their conclusions. These include the following: W, the Washington Codex from the fourth or fifth century and one of the six

primary uncials; Θ known as Koridethi, the text of which was made known to scholars only in 1913; the earlier of the two witnesses to the Old Syriac known as the Sinaitic Syriac, Syr. Sin., found by the twin sisters, Mrs. Lewis and Mrs. Gibson, in the Monastery of St. Catherine on Mount Sinai; and, above all, the discovery of the papyri, preeminent among which for the Gospels is P.p45. Besides, the best of the Old Latin (it) MSS for the Gospels, k, although only fragmentary, was edited only after WH, later cursives have been collated, the families 1 and 13 enlarged by the accretion of some new members, and the family Π, consisting of K Π and some minuscules, has been isolated from the Byzantine text and shown to occupy, together with A, a sort of half-way stage between the pre-Byzantine texts and the secondary stage of the Byzantine text as contained in S V Ω and others. Again, family Π seems to be descended from an ancestor something like p45 in Mark.[10] The discovery of Syr. Sin., W, and Θ, the editing of k, has had the effect of giving more weight to what WH called Western readings, while p45, a hundred or more years earlier than B and S, tends to weaken the position of B and S, for its text is definitely not Neutral but what critics now term pre-Caesarean, the text preserved also in fam. 1 and fam. 13[11]. In this fact is another indication, in addition to those to be given later on, that B and S are

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10. R.V. Tasker in HTR for April, 1948, An Introduction to the MSS of the New Testament, pp. 71-81.
11. It is the text of p45 in Mark which has been chiefly studied.

not remarkable survivals of an unadulterated text but the results of an edited text.

The developments in textual critical theory since WH have helped to bring about a certain distrust of WH fundamental position. In the main the various theories advanced since follow in the wake of WH, and are built upon the same basic principles, but the most recent divisions into texts, or text-types, to use a phrase of Colwell, diverge quite considerably from that of WH. Caspar Rene Gregory, who lost his life while fighting for the German army in World War I, was a completely faithful follower of WH. He speaks of Der Urtext, der Ueberarbeitete Text, der Polirte Text, and der Offizielle Text. The correspondences with WH texts are plain. Von Soden in his gigantic work, which on the whole has been treated with some disparagement by scholars, but which has been recently praised by Merk, has only three distinct texts: the Hesychian text (its symbol is a dark H) which treats the Neutral and Alexandrian as one; the Koine text, in which v. Soden recognized five sub-divisions; and the Jerusalem text (its symbol is a dark I) which is sub-divided into no less than eleven parts. From a combination of these three texts, all of which are, according to von Soden, founded on recensions, together with consideration of the readings of Origen and of Tatian's Diatessaron, the original text of the Gospels may be reconstructed. The theory of

von Soden never became popular. The next great name is that of Canon Streeter, who, like Gregory and von Soden[12], died a violent death, being killed in an aircraft accident in 1937. Streeter expounded the theory of local texts, or texts of the great episcopal centres, finding striking confirmation in the fact that the texts of the early versions, Coptic, Latin, and Syriac correspond, particularly in the first two cases, with the Greek texts current in Alexandria, North Africa and Antioch respectively. In the scheme as completely worked out we have the following local texts with their primary and secondary authorities: Alexandria with B and S L sa.bo, Antioch with Syr. Sin. and Syr. Cur., Caesarea with Θ and fam.1 fam. 13 28 565 700, Italy and Gaul with D and b a, Carthage with k (Mk. Mt.) and W (Mk.) e. The Caesarean text particularly is Streeter's baby and it is on this text that much labor has been expended in modern times. The work of Kenyon, Ayuso, Pere Lagrange, and the Lakes together with the finding of p45 has resulted in a bifurcation of the Caesarean text, a pre-Caesarean represented by p45 fam.1 fam.13 and a later Caesarean text as contained in 28 565 700 1424 Old Georgian etc. In fact, the whole text is rather nebulous and uncertain and lacks definite characteristics.[13] The net result of all this development since WH is scepticism as to the validity of a theory and a method which has produced such divergent conclusions. One

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12. Von Soden was killed in a subway accident in Berlin during the first World War.
13. See The Caesarean Text of the Gospels by Bruce M. Metzger in JBL, LXIV, pp. 457-490 for a full discussion and criticism of work on the Caesarean text since Streeter.

wonders whether the textual facts which we have collected so far are clear and unambiguous enough to form the basis of a method whereby we can arrive at the original text in a purely objective fashion without the extensive use of intrinsic evidence. As a matter of fact, the most recent trend in modern scholarship has been to call the whole method of WH into question, and more and more reliance is being put in the use of intrinsic evidence. But this point needs a whole paragraph or series of paragraphs for itself.

In taking up the direct and immediate criticism of the work of WH it is not without some importance to point out that, although the material of the Introduction is presented with the greatest of objectivity, a closer inspection shows that objectivity to be more apparent than real. Of this fact there are three indications. First of all, there underlies the whole argument the interest of WH in playing off the value of B and S against the TR. It would, certainly, be an unwarranted exaggeration to claim that the whole carefully expounded theory is but an elaborate rationalization of their preference for B, but there can be little doubt that the future value to be assigned to B was continually in their mind as they developed their argument. Secondly, there is the matter of conflate readings. This, it will be remembered, is the device used by WH to separate pure strands in a mixed text. The whole principle of genealogy was in danger of collapse because of the complications caused in the family tree by mixture. But who determines a

conflate? and on what principle? Answer: the critic determines the conflate and on purely subjective opinions. In many a case what one critic judges to be a conflate is by another critic claimed to be the original text from which the shorter texts have diverged through omission. WH argue that it is more likely that the two shorter versions have been fused into the longer than that the longer has been shortened by two different omissions. The odds are about even. In any case, the subjectivism of the principle of WH in this instance is plain, and yet the whole use of genealogy depends on it! And thirdly, the high value assigned to B certainly derives from a faithful, but possibly unwise, following of the rule brevior lectio probabilior. This rule was followed by WH with such slavish fidelity that they deserted even their favorite B in favor of the despised Western text in those instances where the Western text omitted material which B and almost all other MSS contained, the so-called 'Western Non-Interpolations'. Apart from these exceptions, however, the text of WH New Testament is almost the text of B with its manifest errors corrected. The theory of WH, then, is not without its subjective element.

In taking up some of the details in which the work of WH is open to criticism, we may start with the matter of genealogy. Ernest Cadman Colwell of the University of Chicago in a recent study in the JBL has taken up the matter of genealogy, analyzing its achievements and limitations.[14]

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14. Genealogical Method: Its Achievements and Its Limitations, JBL, LXVI, pp. 109-133.

In the course of the paper he criticizes WH chiefly on two counts: that these great critics did not appreciate sufficiently the difficulties caused by mixture, and that WH themselves, in forming their text, gave up their own genealogical method in favor of Internal Evidence of Documents and Group of Documents. This criticism is certainly completely justified. WH themselves declare

> Where the two ultimate witnesses differ, the genealogical method ceases to be applicable, and comparison of the intrinsic general characteristics becomes the only resource.[15]

Why, then, go through the tremendous labor entailed in the use of genealogy? Would it not be far easier and more economical of time and energy to find the binary (in rarer cases trinary) variations for any disputed reading from our earliest witnesses and proceed from there, since genealogy can not take us past them any way? The very terms, genealogy and genealogical tree, are misleading. There is never, or hardly ever, a direct father-son relationship. Centuries and generations of transmission and whole continents separate manuscripts. The family tree is full of great gaps and it is doubtful whether they will ever be filled. Add to this state of affairs that manuscripts have not only a sort of direct descent from father to son, but also from father and mother and from, possibly more grandparents, like human beings, - the fact of mixture that is - and it will be at once apparent that it will take more than the rather dubious mechanism of

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15. WH, op. cit., p.42

'conflate readings' to disentangle the varied strands of transmission. In further development of his subject of genealogy Colwell refers to a method to make the genealogical system 100 per cent efficient as devised by Dom Quentin. It was tried out on the members of fam.13 by Dr. William N. Lyons. The labor required is enormous and the results meaningless.[16] Colwell admits that the genealogical method has done some things. It has proved the homogeneousness of the Koine text and has isolated fams. 1 and 13.[17] But it is helpless to produce anything decisive for the New Testament text because of the complexity of the pattern of its transmission. The summing up of the value of the genealogical method by Colwell is as follows:

> It is clear that in a field where no manuscripts have parents, where centuries and continents separate witnesses, the genealogical method is not of primary importance. Its importance lies in the realm of provincial history.. In the larger realm, where the larger questions are settled, it still has to demonstrate its value for the reconstruction of the original text of the Greek New Testament.[18]

The real support for the conclusions of WH is found in Internal Evidence of Documents and Groups of Documents. At the bottom of these terms is the principle explained before

"Knowledge of documents should precede final judgments upon readings."

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16. Colwell, op. cit., p. 127. Colwell's whole discussion of the genealogical method is eminently readable and complete. His judgment of the value of genealogy is unhesitating: "When there is mixture, and Westcott and Hort state that it is common, in fact almost universal in some degree, then the genealogical method as applied to manuscripts (italics in text) is useless." p. 114.
17. Colwell, ibid., pp. 124-5.
18. Colwell, ibid., p. 132.

The use made of this principle, too, is open to grave objections. WH argue thus

> Where then one of the documents is found habitually to contain these morally certain or at least strongly preferred readings, and the other habitually to contain their rejected rivals, we can have no doubt, first, that the text of the second has suffered comparatively large corruption; and, next, that the superiority of the first must be as great in the variations in which Internal Evidence of Readings has furnished no decisive criterion as in those which have enabled us to form a comparative appreciation of the two texts.[19]

It is the second of the conclusions of WH in this quotation which can not stand. What WH say there is simply: general excellence implies correctness in particular cases. Practically stated that means: B is so excellent a manuscript that we must invariably follow its lead except where other evidence is overwhelming; in all other cases it must be given the benefit of the doubt because of its uniformly good text. This is so patently wrong that it is suprising that it has not been repudiated earlier more generally. Because Shakespeare is the most eminent of poets, does that mean that everything he wrote is supremely good, and that he never falls below goodness, that he never descends to mediocrity and even folly? And does a tennis champion never make a poor shot? Or a violin virtuoso never play a wrong note or one with an impure tone? And when any one of the great men of the world is guilty of mediocrity, does any one argue that his superiority is as great in his mediocrity as in his excellence?

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19. WH, op. cit., p. 32

To argue from general excellence to excellence in a particular case is possible only when dealing with the revelation of God. Because the Word of God is true, therefore it is infallible, even when it is disputed, and when the evidence against its statement is seemingly conclusive. We can argue from the excellence of a great number of particulars to the excellence of the whole. But the excellence of the whole still tells us nothing about any particular which may be in dispute. The particular matter must stand on its own excellence or lack of excellence; it has no right to shine in the reflected light of the whole. As a matter of fact, the so-called excellence of B is a very, very relative excellence indeed. Hardly any scholar is willing to value B as highly as did WH. Scrivener had already pointed out many a bad reading in B, and Hoskier in a thorough study of B and allied manuscripts had gone even further than Scrivener.[20] He claims that the composite picture of Codex B is opposed to a superior claim for the shorter text, for the neutral, unprejudiced text, for a text free from local preferences of grammar and syntactical structure. In detail, he claims a clear Coptic influence on B's antecedents, besides traces of Latin and Syriac influence; he cites examples of editing, changes introduced for the sake of more correct grammar, harmonistic additions and omissions, changes by the use of synonyms, and other improvements.

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20. H. C. Hoskier, Codex B and Its Allies, Bernard Quaritch, London, 1914.

All this in the Gospels where B is particularly good. Hoskier and Scrivener are, I suppose, as subjective in their evaluation of B as WH, but the one fact that emerges from the criticism is that the excellence of B is not an absolute thing at all, but a very relative thing. It is superior to other manuscripts not because it has so few errors, but because it has relatively so few in comparison with the corruption of others. B opposes other manuscripts not like white and black, but rather like a dingy grey and black. Still less reason, then, exists for arguing from its general character to its individual readings. The scholar Kenyon has from a completely different side overthrown the authority of B in the sense of WH.[21] WH claimed for B not only that its descent was pure and uncontaminated by mixture, but that its text and that of its allies was not confined to any particular locality, hence the term Neutral. Kenyon shows discoveries since then do not confirm the theory of universal dominance; that if B is not the text of Egypt its claim to uncontamination becomes more difficult to demonstrate; and, finally and unkindly, that B is so homogeneous that it forces us to accept the preconclusion that it resulted either from a complete set of uncontaminated rolls (all of the books of the New Testament were originally separate rolls) or the exercise of editorial selection.

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21. Kenyon, The Text of the Greek Bible, pp. 207-208

The former of these alternatives is too unlikely to be believed; there remains accordingly, only the latter with all that it implies for the theory of WH. If we have an edited text in B, then its value as an objective witness to the original text is largely nullified. So, from two different sides the uniqueness of B has been attacked, and with that the theory of WH has received a mortal wound. WH put all their eggs in one basket, and the basket has been torn apart to the utter scrambling of the eggs.

In one final respect just criticism is to be urged against the method of WH, and that is their almost exclusive favoring of the brevior lectio. This has been hinted at a number of times already. The high value assigned to B on the intrinsic value of its readings ia a direct result of the application of the brevior lectio canon. B's readings are good, its character is good, because in so many cases its reading is the shorter. The canon of the brevior lectio has come in for much criticism of late. Streeter has a fine discussion of the matter, much of which appears below in quotation. He refers to one A. C. Clark who in his book The Descent of Manuscripts brings decisive proof for the fallacy of the brevior lectio in consideration of manuscripts of the classics. "A text," as Clark puts it, "is like a traveller who loses a portion of his luggage every time he changes trains." Commenting on Clark's words, Streeter admits that "while intentional interpolation is quite

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exceptional (marginal notes and various readings do creep into the texts), omission - commonly accidental, but sometimes, it would seem, intentional - is a constant phenomenon."[22] Streeter believes that Clark's conclusions have to be applied to the New Testament text with modifications; first, because just those manuscripts which are of particular value in their preservation of the local texts: B, S, fam. Syr. Sin. and k, are also characteristic for their omissions when compared with other texts; secondly, because there exists the antecedent probability that in the case of the Gospels some oral traditions would creep into the text. Streeter, however, believed that particularly with respect to the Western text the principle of what may be called the lectio longior is of real value and importance. Here I shall quote Streeter.

> Ever since Prof. Ramsay wrote his St. Paul the Traveller, scholars on purely historical grounds (italics in text) have been emphasizing the claims of quite a number of the Bezan additions to be authentic. Clark shows in a large number of these cases that, if we accept the longer text of D as original, we can explain the origin of the shorter B text. All we need to suppose is that one or more ancestors of B had suffered considerably from what is, after all, the commonest of all mistakes of careless scribes, the accidental omission of line. Wherever the grammar of a sentence was destroyed by the omission, some conjectural emendation of the injured text was made to restore the sense. The result of this process would inevitably be the production of a shorter text, by the side of which the original would look like a paraphrastic expansion.[23]

And more fully, with special reference to the actual state of S:

> Take a MS. like S. In this, in the Gospels alone,

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22. Streeter, The Four Gospels, p. 131. The same page contains the reference to Clark.

there are no less than 46 instances of accidental omission, which probably formed one or more complete lines of the exemplar from which it was copied, due to homoioteleuton.[24] There are other omissions, presumably of lines in the exemplar, where homoioteleuton can not be invoked in extenuation of the error. And there are innumerable omissions of single words. Almost all the longer and many of the shorter omissions have been added in the margin, by the first corrector or sometimes by the original scribe. If one glances through the photographic facsimile of S, there is hardly a page without such correction. But S is a handsome expensive copy produced in a regular scriptorium, written by a professional scribe and corrected by a careful διορθώτης Now let us suppose that the original text of Acts was something like D and that the first copy which reached Alexandria was separated from the autograph by half a dozen ancestors. And suppose that two or three of these ancestors had been copied by scribes neither better nor worse than the scribe of S, but had not been gone over by a διορθώτης . At each stage where the omission made nonsense or bad grammar the owner would make the minimum of conjectural emendation that would make the construction grammatical or restore what from the context appeared to be the sense intended. This process of omission and correction repeated two or three times would result in a copy of the Acts with a text like that of B. If this was the first copy of the book to reach Alexandria, the original being on papyrus, would soon be worn out; but all the earliest copies known in Alexandria would be derived from it. It follows that the more scrupulously subsequent scribed copied these, and the more anxious Alexandrian scholars were to go back to the earliest copies, the less chance would there be of the original omissions being repaired from MSS.

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24. Merk is still severer in his judgment of S: sufficit inspicere codicem Sinaiticum, qui tot omissionibus deformatus est, ut eius testimonium hac in re, nisi aliis testibus fulcitur, nullius fere sit auctoritatis. See the Prolegomena to his Novum Testamentum., p. 13. The number of omissions in B has been calculated as 2556 by Dr. Dobbin, quoted in the Burgon and Miller, The Causes of the Corruption of the Traditional Text, p. 131. This number is to be accepted with some caution, for nothing is said concerning the principles on which omissions were calculated, but since Burgon and Miller quote him with approval the Dr. Dobbin was probably a staunch upholder of the TR. This fact would raise considerably the number of omissions.

> brought in from outside. Even if a copy of the more complete text was brought from Rome, the Alexandrian scholar, like Hort, would condemn it as a corrupt and paraphrastic text.[25]

The chief reason, then for doubting the canon of the brevior lectio lies in the observed habits of scribes and copiers: they are at times inattentive, they get tired after a long period of transcription, the eyes may jump a line or two, and similar endings or beginnings or middles help this process along. But mere copyists rarely add. They may mistake a comment in the margin for part of the text, they may change the word order, a stray synonym or so may slip in here and there; but the copyist's great enemy is omission, not interpolation. This is true even to-day when printers' first galleys will show omissions by the dozens, but never a deliberate addition. It must have been far commoner in the early days of the New Testament text than now, for those were days of the scriptio continua and of copyists who, on the whole, copied less than those who do that sort of work to-day, and who, if we are to judge by the multitude of errors in orthography, were a rather ignorant group of men.

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25. Streeter, op. cit., pp. 134-135. That there existed criticism of a high order in the early Church is plain from a number of notes appended to various manuscripts that have come down to us. The first corrector of S marked for deletion by brackets and dots two famous passages in Luke, that dealing with the "Bloody Sweat" and the word from the cross: "Father, forgive them..." In 565 we have two interesting notes. The word of Luke I , "Blessed art thou among women" are omitted from the text, but are added in the margin with the note "not found in the ancient copies". The pericope de adultera is omitted with the explanation: "(This) I have omitted as not read in the copies now current." 1582, which gives Mark 16:9-20 as a sort of appendix, has a note in the margin: "Irenaeus, who was near to the apostles, in the third book against heresies quotes this saying (v.19) as found in Mark." These examples are taken from Streeter, op. [illegible] pp. 123-124

To the reason just outlined for rejecting a stringent application of the brevior lectio Merk adds the fact that Orientals love the more diffuse way of speaking, which would a priori arouse favor for the longer reading. Accedit quod orientales modum amant loquendi latum et diffusum, cuius rei exempla plurima ex evangeliis, epistulis, apocalypsi afferi possunt, ita ut etiam ex hac parte lectio prolixior saepe ut primitiva haberi possit.[26] This observation, of course, is a very general and indefinite thing and quite subordinate to the other criticism of the brevior lectio outlined above.

To sum up. For all its apparent objectivity the theory of WH is as subjective as the theories advanced by those who have opposed them so strongly. Which is as it should be, for as Vaganay has said, "All intelligent criticism is ultimately subjective."[27] It is doubtful, however, whether WH would have accepted this defence of the subjective element in their textual theory. The weight attached by WH to genealogy and to the internal evidence of documents and groups of documents has been shown to have been done so wrongly. The intricacy of transmission is much greater and presents more formidable problems than WH admitted. Application of the central principles of WH has led to widely differing results, and, of late, to complete scepticism in their

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26. Merk, op. cit., p. 13.
27. Quoted by Tasker, op. cit., p. 77.

validity for the reconstruction of the original text. WH had claimed that "there was no justification for scepticism as to the possibility of obtaining a trustworthy genealogical interpretation of documentary phenomena in the New Testament, either in antecedent probability or in experience." To-day, however, Colwell can agree with K. Lake who spoke of WH theory as "a splendid failure"[28] and can write: "Our dilemma seems to be that we know too much to believe the old; we do not yet know enough to create the new."[29] A new constructional hypothesis is required. Finally, in particular, the firmly accepted canon of the brevior lectio has been widely questioned and a strict use of it rejected. The modern critic must find other principles by which to reconstruct satisfactorily the original Greek of the New Testament.

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28. Quoted by Colwell, op. cit., p. 132.
29. Colwell, op. cit., p. 133.

Chapter Two. The Canons of Modern Textual Criticism.

The canons of criticism generally advocated at the present time, the canons which commend themselves to the present writer, differ very widely indeed from those urged by WH. What is called external evidence or documentary attestation takes on a relative unimportance when compared with the stress laid on intrinsic evidence. But before outlining the modern trends more fully we must indicate the limitations of the modern criticism.

The canons of criticism in vogue at the present time are not in any way final. They are rather tentative, and contingent on certain future developments. First of all, there is the possibility of new finds, which may blow sky-high the carefully constructed edifice of modern criticism. It may not be likely, but it certainly is not at all impossible that manuscripts may be found earlier even than our Chester Beatty papyri, perhaps dating from times immediately following the times of the writers of the New Testament. The discovery of manuscripts of Isaiah dating possibly from before the time of Christ is only an indication of other surprising discoveries still to be made. The finding of an autograph is not beyond the realms of possibility.

And secondly, there still remains work for a generation of scholars in studying, collating, tabulating the results of textual critical materials already available. There are hundreds upon hundreds of cursives of whose contents we know nothing; more study has still to be done on various versions; most of the fathers need critical editing. When all this has been done we may have the necessary data to form a new constructional theory to take the place of that of WH. The modern critic is in the position with respect to external evidence, to quote the words of Colwell again, "of knowing too much to believe the old and of not knowing enough to create the new." But he cannot simply leave the text alone. He must come to some sort of conclusion for his own peace of mind as to what the true text is. Accordingly, he must do the best he can with the material at his disposal, but he knows all the time that new truths may upset some of his most assured conclusions. In the long run, only the discovery of the original autographs would give us absolute certainty. Theoretically, that is to say, the last word is with external evidence, but practically, given the fact of the loss of the originals, intrinsic evidence will have to remain one of our chief means, perhaps the only means, of arriving at our conclusions concerning the true text.

The critical review of WH work in the first chapter of the paper has indicated how little value we can place in genealogy and in the testimony of the best documents and the best groups

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of documents. What is left in the way of external evidence? Fred. S. Grant goes so far as to say:

> What is required is not a choice between MSS or groups of MSS at all, but between readings, all of which stand upon their own feet and either maintain themselves or not according as they commend themselves to the patient judgment of objective scholarship.[1]

And again:

> The situation is completely changed from that in 1881. Instead of tracing back the text to its original in the autographs, by a steady process of convergence following back to a common source the divergent lines of descent, we shall have to stop when we get to the second century; and in place of some rule of preference for one type of text or another, or for their common agreements over their divergences, we shall have to trust a great deal more than heretofore to what is called internal criticism.[2]

According to Grant then, all that remains for documentary evidence to do is to tell us what readings were current in the second century. This is, I believe, too extreme a view to take. For documentary attestation can tell us how widespread any particular reading was at an early date. In this fact we have a real, although again not an infallible, guide in reconstructing the original text. Readings which have widespread attestation being found in areas widely separated geographically are readings that deserve special consideration. All things being equal, such readings are more likely to be original than variants attested to only in one region. The possibility of course that manuscripts originally current, say in Rome, may have been brought to Egypt or Syria or Armenia must be faced. On the whole, however, widely spread identical

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1. Grant, Studies in the Text of Mark, in the ATR, 1938, p. 106.
2. Grant, op. cit., pp. 109-110.

readings would argue the original true text rather than identical errors springing up at different places at approximately the same time. If these concurring witnesses to certain readings are at the same time widely different in their general character, then their concurrence becomes a still stronger testimony to the original text. Scrivener has well stated this rule as follows:

> We must assign the highest value not to readings attested by numbers of witnesses but to those which come from several remote and independent sources and have least likeness to each other in respect to genius and general character.[3]

With him agrees Tasker:

> We can also see, more clearly perhaps than Hort was able to do, that widespread geographical attestation at an early date is a more certain (though again not an infallible) guide to the probability of a particular reading being original than strong attestation from one particular locality even though that locality be Alexandria.[4]

Tasker refers in a footnote on the same page to an opinion of Burkitt agreeing with this principle. Burkitt maintained that the agreement of B ℵ Syr. Sin. was decisive for a reading. The question arises here: Are we to put no value in the recensions of the early fourth and the third century, those ascribed to Lucian at Antioch and Hesychius in Egypt, and the Caesarean, as opposed to the pre-Cæsarean, ascribed

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3. Scrivener, A Plain Introduction to the Criticism of the New Testament, p. 557.
4. Tasker, op. cit., p. 76.

by some to Origen? The argument is that these editors had a greater number of old manuscripts than we have, and that they were accordingly, in a better position to reconstruct the text than we are now.[5] To ignore altogether the work of these editors would certainly be both ungrateful and stupid. They no doubt did have better manuscripts than we now have and they were not without real critical ability, as indicated above. But, on the other hand, we are not bound by their conclusions; the documentary evidence from the second century is enough to enable us to draw our own conclusions; and we have at this time rather more critical experience and a more acutely developed critical sense. So, then, we may set down as our first critical canon that widespread geographical attestation at an early date argues correctness of reading. The reader will have gathered that this is no hard and fast rule, never to be deserted, but rather one that posits a probability antecendent to the application of other critical canons.

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5. This argument was advanced by defenders of the TR in the extreme form that, as the Church collected the New Testament Canon, so the Church was led by the Spirit to preserve the true text, i.e., the text which was dominant for 1500 years. J.O.F Murray argues that no such activity for the preservation of the pure text can be proved as it can for the fixing of the Canon, and then goes on to say finely: "Unless it can be proved that they (the Greek Fathers) ever took more than an occasional passing interest in the question, what is it but a gross abuse of a great principle to appeal to their authority in a matter like this, as if it stood on the same level as their authority on the great problems which we may well believe they were raised up by God to solve, not of their own generation only, but of all the generations that were to come after them?" This quotation is taken from Murray's article entitled Textual Criticism of the New Testament, in Hastings, A Dictionary of the Bible, Scribner's Sons, New York. 1923. pp. 208-236 in the Extra Volume.

The other critical canons to be enunciated now are all in the realm of intrinsic evidence. These may best be summed up in the words of Grant:

1. Each reading must be examined on its own merits, and preference must be given those readings which are demonstrably in the style of the author under consideration;
2. Readings which explain other variants, but are not contrariwise themselves to be explained by the others merit our preference.[6]

To these Jose M. Bover, Professor of the Holy Scriptures in the Colegio Maximo de S. Ignacio at Barcelona, and the author of a fine critical Greek New Testament,[7] adds the lectio non harmonizans and the lectio impolitor. The canons enunciated in the words of Grant he terms scriptoris stilus and originalis lectio. These canons are self-explanatory. But they all are difficult to apply, and when applied by different critics produce different results. With respect to the scriptoris stilus it is certainly a difficult claim to uphold that such and such a reading could not have been written by John or Luke or whoever the writer in question may be. The utmost we can usually claim is that such and such a reading is strangely unlike the rest of his writing, but further we can not go without laying claim to omniscience. We possess on the whole rather too little of the various writers of the New Testament to be able to make the dogmatic statement that any one of them could not have written such and such a statement. With respect to the originalis lectio Grant, too, admits that it "is a very subtle

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6. Grant, op. cit., p. 111.
7. On the authority of Bruce M. Metzger in an evaluation of Recent Spanish Contributions to the Textual Criticism of the New Testament in JBL, LXVI, p. 415. The other references to Bover come from the same article, p. 420.

process involving intangible elements and liable to subjective judgment on the part of the critic."[8] This is the case, because most of the variants can be explained either way, the determining of the originalis lectio providing usually a very knotty problem. Similar criticisms can be raised against the lectio harmonizans and the lectio impolitor. The modern critic must be one of scrupulous honesty, being continually on his guard lest in his application of these principles, by which he is to get at the mind and intention of the original writer, he actually arrive at a decision which really is what he would like to see in the text. With that he must indeed be a thorough scholar who knows the writer whose text he is criticizing and withal a man of imagination who can transport himself in thought and outlook to a scene and a time completely different from his own. The danger of subjectivism in the method of textual criticism advocated today is plain to see, but, in the absence of any alternative, the risk inherent in the method must be taken. It will be noticed that the generally adopted canons of an earlier day are no more in such favor: the brevior lectio probabilior and the difficilior lectio porior. The first of these has been amply discussed above, and the second is much like it. The second implies that scribes are continually thinking of what they are writing and are continually on the look-out, trying to make the text

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8. Grant, op. cit. p. 111.

as easy as possible. Scribes copy, editors correct. Every mistake a scribe makes in copying makes the text more difficult. The omission of a salient word or two will make the text hopelessly difficult. It is at least likely that the original writers tried to be as easily understood, as perspicuous as possible, and not the opposite. All of which adds up to the sum that a difficult reading is not right because it is difficult. Both of these two canons have their use in certain places, but they are particularly open to abuse by a wooden and mechanical application, and there are not many readings where the canons first mentioned will not yield more acceptable results.

In the enunciation of these canons of criticism we have gone all the way back to Lucian as against WH. Hort criticized the Lucian revision on the charge of eclecticism. This is just what the application of the principles enunciated will result in - an eclectic text. But, to quote Streeter once again, "the eclectic principle of deciding in each separate case on grounds of 'internal probability' what appears to be the best reading is, in spite of its subjectivity, theoretically (italics in text) sounder than the almost slavish following of a single text which Hort preferred."[9]

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9. Streeter, op. cit., p. 145.

Chapter III. The Ending of St. Mark's Gospel

The very attempt at making another study of the ending of the Gospel according to St. Mark will appear to most people as so much wasted time and effort. For the matter is decided in the learned world. Huck in his Synopsis of the First Three Gospels boldly heads the section containing Mark 16:9-20 Der unechte Markusschluss. That is the generally accepted opinion among scholars of all shades of theological opinion. Quotation from many writers is unnecessary. One specimen will suffice. Zahn: "It may be regarded as one of the most certain of critical conclusions that the words ἐφοβοῦντο γάρ, 16:8, are the last words in the book which were written by the author himself." No matter what view one takes, then, the very working with the problem is a Quixotic tilting at windmills. If one has no definite conclusion to make, why write at all? If one agrees with the majority, the almost unanimous majority, of critics, what is gained? And if one disagrees and makes an attempt to defend a lost cause, one is either a fool or a trifler with knowledge, or incurably argumentative. For all that, let the attempt be made, and the truth prevail.

Statement of Documentary Attestation.

The documentary attestation for the so-called Longer Ending, i.e., vv. 9-20 of chapter 16 in the A.V. can be easily stated. Every Greek manuscript except B and S contains the Longer Ending. All the versions and the manuscripts of the versions, except some of the Georgian and Armenian manuscripts and k and Syr. Sin. contain the Longer Ending. Among the Fathers up to 250 A.D. the Longer Ending is known, decidedly by Irenaeus, Tatian, Hippolytus, and almost certainly by Justin,[1] and very probably there is an allusion to it in the Shepherd of Hermas.[2] To those who know the Longer Ending Merk in his apparatus criticus adds Tertullian.[3] The documentary evidence against the Longer Ending are the Greek manuscripts, some of this is necessary repetition, B. and S, the Latin k, Syr. Sin., and certain manuscripts of the Armenian and Georgian versions. Besides this straightforward evidence, however, there is much evidence of a very complicated kind, the exact bearing of which is not easy to assess. This evidence is first to be summarized and then each part of it to be more carefully examined. A bare summary of the ambiguous evidence includes the following: the testimony of Eusebius; the witness of those Greek manuscripts which, besides containing the Longer Ending, give also the so-called Shorter Ending; the lack of quotation of the Longer Ending in certain of the early

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1. See Zahn, Introduction to the New Testament, Vol. II, p. 468, Streeter, The Four Gospels, p. 337.
2. Streeter, ibid.
3. The Epistula Apostolorum from the second century may have included the Longer Ending. See Streeter, op. cit., p. 70.

fathers; the note in an old Armenian manuscript concerning Ariston or Aristion; the empty space in B.

A few fragments remain of a lost work of Eusebius of Caesarea, fragments which were published by Cardinal Angelo Mai in 1847, on the so-called Inconsistencies in the Gospels. Among the fragments is a question by one Marinus and a longish answer by Eusebius, in the course of which the statement is made that the twelve verses in question are not in all codices, not in the most accurate codices, that they are met with seldom, that they are absent from almost all the codices. It is not clear which of these statements is the most factual. There is even some doubt from the way the statement is made whether the statements or statement represents Eusebius's own opinion, although, since he did not provide the doubtful verses with his canons, it is pretty plain that he for his own person did not consider the verses genuine.[4] This statement of Eusebius adds an indefinite number of unknown and lost manuscripts to those arrayed against the Longer Ending. It can be doubted whether that indefinite and unknown number is a very large one, and that for two reasons. No one knows how many of the manuscripts then extant Eusebius knew by personal acquaintance, but is far more likely that his knowledge extended to a minority of the manuscripts then in existence than to a majority of them: and secondly, if there were so many manuscripts without the ending

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4. See Appendix for the Greek text of Eusebius.

then, why have so few of them left traces of this lack, for the manuscripts that now exist display just the opposite state of affairs. Jerome has often been claimed as supporting Eusebius. In his answer to a certain Bedibia with respect to precisely the same question that Marinus once set Eusebius he makes the same reply, his words being almost a translation of those of Eusebius. Jerome is plainly using Eusebius, and so his words cease to be independent testimony.[5] What testimony Jerome does in this instance seem to bear against the Longer Ending is largely nullified by the fact that he translated the twelve verses in his Vulgate New Testament and that he actually quotes from them more than once. Tischendorf in his apparatus quotes a number of other writers, mostly later ones, but Burgon has shown that some of the references are mistaken and that others merely echo Eusebius, and modern editors never refer to them.

Besides the Longer Ending there exists also a Shorter Ending, which Goodspeed translates as follows, calling it An Ancient Appendix: "But they reported briefly to Peter and his companions all they had been told. And afterward Jesus himself sent out by them from the east to the west the sacred and incorruptible message of eternal salvation." Only k + Sy^{mg} has the Shorter Ending by itself. Wherever else the Shorter Ending is found it is found in conjunction with the Longer Ending, and

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51 Jerome, Ep. cxx, 3. Text in the Appendix.

usually, if not always preceding it, either as part of the text as in L 0112 099 579 ψ , or as a marginal note as in 274 and in the Harclensian Syriac and in various codices of the Sahidic, Bohairic and Ethiopian versions. There is no patristic evidence at all for the Shorter Ending. It is usually claimed by critics that the manuscripts with the Shorter Ending offer additional evidence against the Longer Ending and for the genuine ending at v. 8. It is difficult to see how this claim can be upheld. The existence of the Shorter Ending per se is an argument against the Longer Ending, but manuscripts which contain both the Longer and the Shorter Endings are not to be counted as witnesses against the Longer Ending, but as witnesses who frankly don't know what to say, and who say both to make sure, and as no one would hesitate in his choice between the alternative endings, they are more witnesses for the Longer Ending than witnesses against it. That the Longer Ending was felt as being is somehow strange, unsatisfactory, unfitting almost, unauthentic, ? may be seen from certain other notices in the manuscripts. For instance, in 1582, one of the older members of the Caesarean text has the note at verse 8: "In some copies the Gospel ends here, up to which point also Eusebius Pamphili made his canons, but in many (copies) there is also found this", whereupon the Longer Ending follows. This identical scholium is found in 1 and a similar scholium,

without the reference to Eusebius in 22. This latter minuscule has the word τέλος both at verse 8 and verse 20. According to Streeter some 30 manuscripts in all have notes marking the Longer Ending as somehow strange or not in the ancient manuscripts.[6] The cursives 239, 259, 237 have a note attached to Jn. 21:12, which note, in a summary of the appearances of Jesus to His disciples, passes over the incidents in Mark altogether. The oldest Georgian manuscript (Adysh) has the Longer Ending added as an Appendix to the Gospels at the end of John. Finally, in this enumeration of doubtful or semi-doubtful witnesses we must mention the Freer Logion found in W, the existence of which was known even before the finding of that valuable manuscript from a sentence in Jerome.[7] This is found as an addition, a long one, to verse 14 and is in the nature of an apology of the disciples for their unbelief, on account of which the Savior had upbraided them. The bearing of all these facts is not easy to assess. It is not easy to see why some call the Longer Ending as amplified by the Freer Logion to be another ending or use that amplified ending as an argument against the Longer Ending. The Freer Logion recalls some of the additions to be found in D and manuscripts of the Old Latin. The text in W is not a new ending nor does

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6. For more details concerning the ways in which the various endings appear in the manuscripts mentioned see Zahn, op. cit. p.484; Zahn, Geschichte des Neutestamentlichen Kanons, Voll II, pp. 910-927; WH, Appendix, pp. 29-46. Some of these details are given in summary form in the appendix of this paper.
7. See Appendix for the text of the Freer Logion and the reference to it in Jerome.

it in any way furnish additional manuscript evidence against the Longer Ending. The question of the Freer Logion is one quite apart from the Markan Ending and belongs in the class of textual problems known as Western Interpolations. The other peculiarities just enumerated, imply a certain doubt, and I may suggest, an editorial doubt, perhaps much the same doubt that exists in critics today, concerning the passage. The thirty cursives or so calling attention in one way or the other to the strangeness of the passage are all late, 1582 coming from the 10th century. When Streeter makes the claim that the original text of Caesarea was originally without the ending because of the scholium in 1582 and I, both members of that text, he is certainly engaging in some pleasant speculation and not a little wishful thinking. It is not at all impossible that we have in these particular cursives later echoes of Eusebius's opinion, for it is highly probably that his authority would have some influence on the text in a matter like this, particularly on the text at Caesarea where he lived and labored for many years. The various notes and curiosities in the manuscripts referred to are, strictly speaking, not evidence against the Longer Ending at all, but, partly, late evidence both for and against at the same time, <u>for</u>, inasmuch as they all contain the words of the Longer Ending: <u>against</u>, inasmuch as they testify to the opinion of some critic or critics unknown to us at the

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present time who believed for one reason or the other that vv. 9-20 are not the original continuation of vv. 1-8.

WH make much of the fact that the Longer Ending seems to be unknown in all the Greek Ante-Nicene literature except for the Fathers quoted before. Tertullian and Cyprian, too, according to WH know nothing of the Ending, or better, do not quote the Ending. This is the well-known argumentum e silentio, which is supposed to be particularly strong at this point because of the importance of the material contained in the twelve verses. Normally, the argument from silence is weak except when the circumstances are such as to make a reference almost inevitable. There is rather important matter in the twelve verses undoubtedly, both from the point of view of doctrine and also Gospel harmony. But before the argumentum e silentio can be construed as a definite vote of the writers in question against the existence of the disputed verses in their copies of Mark, it must be shown, not, generally, that the words are not referred to in any way, but that in such and such a definite passage where a quotation from or a reference to vv. 9-20 would be particularly apt and fitting and telling and where its omission is startling, there is no such reference or quotation. With respect to Tertullian and Cyprian, indeed, WH do point to such definite places in their writings where a quotation of the last verses of Mark would be a natural thing, as in Tertullian De Baptismo, who, when dealing with the relation of

faith and baptism quotes Matt. 28 and John 3 but does not refer to Mark. Similarly Cyprian omits all reference of Mark in the third book of his Testimonies from Scripture, which includes heads like these, Ad regnum Dei nisi baptizatus et renatus quis fuerit pervenire non posse (25), Eum qui non crediderit iam iudicatum esse, (31) Fidem totum prode esse et tantum nos posse quantum credimus (42), Posse eum statim consequi (baptismum) qui vere crediderit (43). This evidence certainly makes it likely that Tertullian and Cyprian did not know the last verses of Mark, although it is not conclusive. But a totally wrong impression is conveyed when, as happens frequently, writers are quoted as being witnesses against the disputed verses, simply because they do not quote from them, without a demonstration like the one given by WH with respect to Tertullian and Cyprian. By the way, Merk in his small apparatus criticus lists Tertullian as being a witness for the passage, on what authority I do not know. The evidence for that statement is no doubt very slender, for none of the other writers on this subject follow him or agree with him on this point.

A name has been found on whom the fatherhood of the Markan baby may be foisted, one Ariston or Aristion. An old Armenian manuscript contains the last twelve verses separated from the rest of the Gospel with the note "of the presbyter Ariston". Rendel Harris inclines strongly to the adoption of this note and identifies, with others, this Aristion with the Aristion

mentioned with the presbyter John by Papias, who also calls Aristion a disciple of the Lord.[9] Gregory and Swete hold to the Aristion authorship of the Longer Ending very strongly. Zahn thinks that only vv. 14-18 should be ascribed to Aristion and not the whole section. Papias, Zahn believes, incorporated this tradition of the presbyter Aristion in his own work, and that the compiler of the verses 9-20 in turn got it from him.[10] Streeter believes, in opposition to these men, and argues the case very well, that it is uncritical to place much value on an isolated statement found in an out-of-the-way manuscript, and advances as a guess, a plausible one, too, that the choice fell on Aristion in order to give to the Longer Ending the authority of an eye-witness.[11] The net result for the wider question is to add this Armenian manuscript to manuscripts like 1582 and others which say Yes and No at the same time, Yes by recording the passage, No by casting some sort of doubt on it. In this case, the No is more definite than the Yes, for the Markan authorship is plainly denied.

Finally, we have the empty column of B to consider. This is the only vacant column in the whole codex. The reason for that phenomenon is not easy to determine at this date. The claim of Burgon that the blank column is an indication that the manuscript from which B copied must have contained the

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9. Rendel Harris, Side-Lights on New Testament Research, pp. 92f.
10. Zahn discusses the matter thoroughly, op. cit., pp. 473f., 485.
11. Streeter, op. cit., pp. 344-347.

Ending and that the copyist left them out on instruction is one of those rash statements of his which mar an otherwise thorough and scholarly discussion of just this problem. All we can infer is that most likely the ending was known by the scribe, but, it is plain, that the scribe or the authorities of the scriptorium, did not consider the ending genuine. There can be no doubt that B, in spite of the vacant column just here, gives a definite vote against the inclusion of the Longer Ending.[12]

It is now possible to sum up the whole documentary evidence. What we want in accordance with the principles of textual criticism enunciated in Chapter II is to know how widely-spread the variants in question were in the early church, say the second or third century. Essentially there is no difference between a variant which involves one word and one which involves a hundred in this matter of geographical distribution, although of course the complete study is much more difficult in a big omission, or addition, as you please, than in a minute one. If we were to apply Burkitt's dictum here of the decisive weight of B k and Syr. Sin. in combination, the matter would be decided for the excision of vv.9-20. But away with rules of thumb. In this case B and S in Egypt are opposed by all the other Egyptian manuscripts and by the

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12. Salmon in the work quoted above takes up the view of Burgon, amplifies it and endeavors to bring S too under suspicion as to its testimony. An inspection of the published facsimile of S shows almost a whole column blank after the conclusion of St. Mark's Gospel, while the last complete column of St. Mark contains 560 letters as contrasted with 678 in the first complete column of St. Luke. To quote the argument of Salo mon exactly: "I do not think these two phenomenon can be reasonably explained in any other way

(continued p. 51)

Egyptian versions, the oldest of which, sa, goes back to the third century (not of course our extant manuscripts of it); Syr. Sin. is opposed by Syr. Cur., written only somewhat later; k is opposed by all the other manuscripts of the Itala and by D. All Caesarean manuscripts own the Ending, although some of the later cursives have their doubts. Before summing this up more concisely, I must examine the statement of Streeter that the Gospel ended at v. 8 in the first copies of the Gospel that reached Africa, Alexandria, Caesarea, Antioch, and most likely Rome, because the African text came originally from Rome. The burden of proof in the case of Rome, too, he maintains rests on those who claim that the earliest manuscripts in Rome contained the Ending.[13]

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12. (continued) than that the leaf, as originally copied, had contained the disputed verses; and that the corrector, regarding these as not a genuine part of the Gospel, cancelled the leaf, recopying it in such a way as to cover the gap left by the erasure. It follows that the archetype of the Sinaitic MS. had contained the disputed verses. (Italics in text)..... Thus both manuscripts, when cross-examined, give evidence, not against, but for the disputed verses, and afford us reason to believe that in this place these MSS. do not represent the reading of their archetypes, but the critical views of the corrector under whose hand both passed; and as they were both copied at a time when the authority of Eusebius as a biblical critic was predominant, and possibly even under the superintendence of Eusebius himself (for Canon Cook thinks that these two were part of the 50 MSS. which Constantine commissioned Eusebius to have copied for the use of his new capital), we still fail to get distinctly pre-Eusebian testimony against the verses." p. 148. I copy this here for what it is worth, but believe that too much is made of too little. There is an elaborate investigation of the testimony of B and S in Stonehouse op. cit. pp. 92-94 and in Zahn, Geschichte des Ntl. Kanons, Vol. II, pp. 911-912. The investigations of these men show pretty clearly the exaggerated statements of which men like Burgon and Salmon have been guilty as to the testimony of B and S.

13. This statement is made in very definite form, Streeter, op. cit. pp. 336 and 348.

This statement is very definitely made, but it is very difficult to see just on what grounds. It really seems as if Homer nodded here slightly. (The argument of Streeter must be, for it is not outlined in detail, that since k, for example, is our earliest representative of the African Itala, therefore the first copies of the Gospel to reach Africa stopped at verse 8 as k does (except for the Shorter Ending). But the Itala as a translation goes back possibly to the second century already, while k is dated as belonging to the fourth century. Much can happen in two hundred years. Besides, there are manuscripts of the Itala almost as old as k, copied like k from earlier manuscripts, (although from how far back we do not know), which do contain the Ending.) Whence then the dogmatic statement that the earliest copies of Mark ended at verse 8 not only in Africa but also in Rome? Precisely the same is the argument underlying the claim concerning the first state of the text in the other localities mentioned and the counter-argument is the same in all those cases likewise. B and S are from the fourth century, yes, but the Sahidic translation is older; the Old Syriac antedates Syr. Sin.; and certain notes in the tenth century 1582 and the still later 1 etc. do not tell us what the first copies at Caesarea were like. Francis C. Burkitt argues in his Evangelion Da-Mepharreshe, vol. 2, p. 194, that Syr. Sin. which omits the verses more truly represents the genuine text of the Evangelion than Syr. Cur. which inserts them, arguing that it is impossible to conceive any Syriac-speaking community suppressing

the verses, and that, accordingly, the mere fact that positive evidence for their omission exists is enough to shew that the original form of the Evangelion did not contain them. However, I see no reason for accepting his major premise; it seems to me that Syriac-speaking Christians would not think differently from Christians speaking other languages, and arguments which weigh so strongly with most critics today and which appeared not without force to Eusebius and others in earlier ages of the church could easily have exerted a similar influence on the church in Syria. Despite the loose statement of Streeter, then, we can say that in the early church the Longer Ending was widely known, being witnessed in West and East, in all the centers of the church, by manuscripts, versions, and fathers. On the other hand, the ending at v. 8 is also early and is also widespread. There is not much difference in the geographical attestation on either side. On the whole, the Yeas are found more widely than the Nays, and they are certainly in greater numbers. The critic, of course, will attach no importance at all to the last mentioned fact, but it is handy to know when one meets a statement like this: "Some texts and versions add as 16:9-20 the following passage", or like this: Other ancient authorities add after verse 8 the following", whence follows the Shorter Ending. These are the introductory statements to the two endings in the Revised Standard Version, whereby, quite falsely and unforgivably, the two endings are spoken of as being of approximately the same

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authority, and a completely wrong picture is given of the documentary attestation for the various endings of St. Mark's Gospel.

Statement of the Internal Evidence

It is by internal evidence, intrinsic evidence, that the genuineness or otherwise of the Longer Ending (the Shorter Ending is plainly spurious) will be decided. It can safely be said that, if the Ending of Mark were knit as closely with the preceding words to make as unified a last chapter as the last chapter of Matthew or Luke, no one would have been led by the documentary evidence to reject it. Readings have been accepted by critics of all sorts and kinds on far more slender evidence than that which supports Mark's Ending. It would be comparatively easy to find reasons to account for the omission in certain manuscripts. It is the combination of the strong internal evidence against the last verses and the gaps in the tradition of the text attesting the Longer Ending which makes the case against it so strong. We turn first to the case for the opposition.

The case for the opposition rests chiefly and strongly on the fundamental irrelevance of vv. 9-20 to vv. 1-8, and, secondarily, on the marked differences in style and tone between the bulk of the Gospel and the verses in dispute. It is not stating the case too strongly to say that the two sections: vv.1-8 and vv. 9-20, really have nothing in common. The fear of v.8 is not taken up at all in the next section. Almost everyone feels that the Gospel could not have ended at v.8 (We shall

return to this later), "for they were afraid", but the following verses have nothing whatever to say about the allaying of that fear, do not refer to it at all. Then again, v. 9 refers to Mary Magdalene as if she were entering the story of the resurrection for the first time, although she is mentioned specifically in the previous section. Further, the phrases of v.9, ἀναστὰς δὲ πρωὶ πρώτῃ σαββάτου, πρῶτον all fit the beginning of a resurrection narrative, not one that was already begun and is to be continued. The command of the angel to the women in 16:7 is not referred to at all in the narrative that follows, quite unlike Matthew at this point, cf. 28:7,16. The conclusion seems inescapable that the section vv. 9-20 is in no way a continuation of vv. 1-8. The Longer Ending is no ending at all, but seemingly, an independent resurrection record, containing a summary of appearances (vv. 9-13), a narrative of one of the conversations of Christ with His disciples found nowhere else (vv. 14-18) and a summary statement of the work of the apostles (vv. 19 and 20).

Added to the irrelevance of the Longer Ending to the body of the Gospel, and particularly to the resurrection narrative, is the strangeness of the style and tone of the Ending when compared with the style of the rest of Mark. The tone is didactic, not historic, the historian has given place to the

theologian, it is John speaking rather than Mark. Here is no rushing movement as in Mark generally, a succession of short paragraphs, the style of vivid and lively narrative, but a carefully constructed passage as would be written or spoken by a teacher with an eye to the moral or lesson to be imparted, in this case faith and unbelief. Less significant are certain details of vocabulary, the lack of words like εὐθέως and πάλιν and the use of other words like ἀπιστεῖν and κύριος. Particularly the use of κύριος in the formula ὁ κύριος Ἰησοῦς is said to be unknown in the Gospels, the occurrence of this expression in Luke 24:3 being also rejected on that count. Most of this criticism based on style and vocabulary, however, is trifling and picayune, as WH admit. If no doubt existed concerning the passage on other grounds, the argument drawn from this trivial and intangible material would have been neglected. As it is, however, the general criticism gains added weight from the noticeable difference in style and language between the Gospel proper and the Longer Ending.

Although critics are almost unanimous in their conviction that the Longer Ending is not part of the Gospel. they differ sharply both in their evaluation of its intrinsic worth and in their explanation for the abrupt ending at verse 8, Pott claims of the Longer Ending: "wie ein Blick auf die Parallelstellen zeigt, ist der Schluss Vers fuer Vers zusammengeschrieben."[14] Zahn, more accurately and credibly, claims that

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14. Pott, Der Text des Neuen Testaments, p. 75.

that the compiler used the other Gospels and Papias's work and combined all into an indifferent unity.[15] WH speaks of the rich content of the twelve verses,[16] while Gregory with characteristic freshness of expression considers them to be as good or even better than Mark's original and lost ending, which, he hopes, may still turn up some time in Alexandria.[17]

While the division of the critics on the intrinsic value of the twelve verses is more interesting than important for a decision concerning genuineness, the split in their ranks in the explanation offered for the supposed genuine ending at v. 8 is more important. Right here is to be found the chief difficulty for those who deny the genuineness of the Ending. Most critics hold to a lost ending, so WH, Streeter, Gregory or to an uncompleted work, like Zahn and others. Some are satisfied with the ending at v. 8, like Wellhausen, Loisy, Ed. Meyer, Loofs, and, very recently, Stonehouse. The presentation of the case for the defence is most conveniently done by means of an examination and criticism of the conflicting explanations.

Can the ending at verse 8, ἐφοβοῦντο γάρ be regarded as the intended ending to the Gospel? This has recently been strongly maintained by Ned Barnard Stonehouse, Professor of New Testament in Westminster Theological Seminary, in his book, The Witness of Matthew and Mark to Christ, and the

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15. Zahn, Introduction..., Vol. II, p. 470 and p. 486.
16. WH, The New Testament, Appendix, in the discussion of this section.
17. Gregory, Einleitung in das Neue Testament, pp. 621-628.

answer to the question propounded in this paragraph is in effect an attempted rebuttal of the argument of Stonehouse. The one pillar for the contention that Mark intended to conclude his Gospel at verse 8 is linguistic in character. The argument as formulated by Stonehouse runs as follows:

> If a Greek sentence demonstrably could have ended as Mark 16:8 does, we consider that any objection, on formal grounds, to the interpretation of these words as the proper end of the paragraph and book would likewise disappear.[18]

Parallels to the admittedly bald Greek in ἐφοβοῦντο γάρ have been found. Stonehouse quotes the LXX rendering of Gen. 18:15, where, upon the Lord's rebuke for Sarah's laughter, she replies: οὐκ ἐγέλασα ἐφοβήθη γάρ . For the conclusion of a paragraph with a γάρ Plato's Protagoras is quoted, where an extensive speech ends with the words νέοι γάρ , and also Justin Martyr, who makes Trypho conclude an indictment of the Christian confession of Christ with the words ἐσταυρώθη γάρ . There are no close parallels either in Mark or in the rest of the New Testament for abrupt endings of paragraphs like these, but brief clauses introduced by γάρ are found. Mark 1:16; 3:21; 10:22; 16:4; and 9:6, of which the last is the best and closest parallel to 16:8. The verb φοβεῖσθαι used absolutely is found Mark 5:15; 33, 36; 6:50; 10:32. All of this is quite true and does serve to correct the exaggerated statements some have made about the impossible Greek of ἐφοβοῦντο γάρ , but the main point is still not proved: that a whole book can finish in that abrupt way. The question

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18. Stonehouse, p. 101.

whether such an ending is possible, and we are still arguing on formal grounds, has been discussed by one Wilfred Lawrence Knox in the HTR for 1942. In an analysis of Mark's endings of stories he shows that Mark follows the usual form of popular stories. Sometimes we have a summary of the actions of Jesus or others, as in 1:34, 39; 4:33; 5:20; 6:5; 10:52. On other occasions we have the effect of Jesus' miracles on the crowd: 1:45; 2:12; 7:36. Incidents are sometimes concluded by a natural action: 6:29, 43; 7:30; 10:16; 11:11. In this he differs markedly from John who is no stranger to dramatic aposiopesis, cf. 13:30; 18:27; 19:22. Knox claims that ancient biographies show no examples of such dramatic aposiopesis. The dramatic aposiopesis of John is a mark of elaborate literary technique, unparalleled in ancient literature of the narrative type, even when that literature is of the most sophisticated character.

> To suppose that Mark originally intended to end His Gospel in this way implies both that he was totally indifferent to the canons of popular story-telling, and that by a pure accident he happened to hit on a conclusion which suits the technique of a highly sophisticated type of modern literature. The odds against such a coincidence... seem to me to be so enormous as not to be worth considering.[19]

The conclusion at which Knox arrives is supported by the manuscript evidence. What Stonehouse considers a perfectly legitimate ending was not so considered by readers in the early church. For if they thought the Gospel ended satisfactorily at v. 8, why should they go to the trouble of inventing endings? Just so. That the Gospel so rarely is found as ending at v. 8 is an indication that generally the ending at v. 8 was considered

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19. Knox, The Ending of St. Mark's Gospel, op. cit., pp. 22 f.

impossible. The verdict of history is the same as the verdict of Knox's logic.

Stonehouse argues further that it is possible to defend the ending at v. 8, too, when one turns to the material aspects of the question. The particular difficulty is that Mark 16:7 and Mark 14: 28 point to a reunion in Galilee, and, if the proper ending is at v.8, the Gospel is left incomplete. To overcome this difficulty earlier champions of the view that the Gospel ends properly at v.8 insisted that v.7 disturbed the unity of the final section and should be removed as an interpolation. Stonehouse, however, develops carefully the argument that the true aim of Mark is not to lead on to the ascension, but to conclude the work with the tremendous awe inspired in the women by the fact of the resurrection. "Mark is not concerned here to depict the later course of events but only to describe the over-powering immediate impression created by these stupendous events."[20] The idea is developed from here that the note of fear and trembling on which the Gospel ends is in keeping with the whole emphasis of the Gospel of Mark, and by fear and trembling we are not to understand a fear which implies a want of trust or intention of not obeying, but rather a fear which is one of overwhelming awe and reverence. "Deep, religious prostration rather than terror, or slavish fear, marks the women's response to the stupendous events of the early resurrection morning."[21] Impressive

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20. Stonehouse, op. cit., pp. 104 f.
21. Stonehouse, Ibid., p. 107.

confirmation of this summing up of the emotions of the women on the day of the resurrection Stonehouse finds in the Markan account of the transfiguration, even down to the rather close parallel in v. 6 of chapter 9. After showing that there is nothing incongruous in bringing a narrative to a conclusion on the note of reverential awe, Stonehouse goes on to show that Mark's account gives sufficient motivation for such an overwhelming reaction on the part of the women. Finally, Stonehouse finds in the ending at v. 8 a remarkable parallel to the beginning of the Gospel, the circle is complete, unity is achieved. As Jesus is introduced suddenly, so he takes His departure suddenly. "If the incarnation of the Son of God, stupendous as that fact must have been in Mark's thought, is not described nor placed in an historical setting but merely intimated, may not the awe-compelling event of the resurrection likewise be set forth indirectly and abruptly?"[22] In the brevity of his resurrection account as compared with the Passion story Mark resembles the other evangelists, only that he is briefer than they. "Nevertheless, in spite of the brevity of the account, the integral and meaningful place which the resurrection occupies in the glad tidings is no less clearly and emphatically set forth in Mark than in the other accounts."[23] Plainly, this is an able defence of the abrupt ending at v. 8. It appears to me that the various positions taken up by the

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22. Stonehouse, ibid., p. 117.
23. Stonehouse, ibid., p. 118.

the verses, and that, accordingly, the mere fact that positive evidence for their omission exists is enough to shew that the original form of the Evangelion did not contain them. However, I see no reason for accepting his major premise; it seems to me that Syriac-speaking Christians would not think differently from Christians speaking other languages, and arguments which weigh so strongly with most critics today and which appeared not without force to Eusebius and others in earlier ages of the church could easily have exerted a similar influence on the church in Syria. Despite the loose statement of Streeter, then, we can say that in the early church the Longer Ending was widely known, being witnessed in West and East, in all the centers of the church, by manuscripts, versions, and fathers. On the other hand, the ending at v. 8 is also early and is also widespread. There is not much difference in the geographical attestation on either side. On the whole, the Yeas are found more widely than the Nays, and they are certainly in greater numbers. The critic, of course, will attach no importance at all to the last mentioned fact, but it is handy to know when one meets a statement like this: "Some texts and versions add as 16:9-20 the following passage", or like this: Other ancient authorities add after verse 8 the following", whence follows the Shorter Ending. These are the introductory statements to the two endings in the Revised Standard Version, whereby, quite falsely and unforgivably, the two endings are spoken of as being of approximately the same

The intended ending at v. 8, then, cannot maintain itself against the ancient witnesses, even if it might commend itself because of its piquancy to the modern mind. There remains only the choice between two alternatives: to hold that there was an original ending which has since been lost completely and without trace; or, to accept, in spite of all the difficulties involved, the ending which we now possess as the work of Mark. There is another possibility; and that is to hold, as Zahn does, that Mark never finished his Gospel, a view made use of in the solution to be proposed.

There are insuperable difficulties to the acceptance of the first alternative, and chiefly, there is absolutely no trace of an original genuine ending, no hint of its existence, no reference of any kind. To quote Zahn here

> Though the N.T. text can be shown to have met with varying treatment, it has never as yet been established from ancient citations, nor made really probable on internal grounds, that a single complete sentence of the original text has disappeared altogether from the text transmitted in the Church, i.e. from all the MSS. of the original and of the ancient translations.....Here, however, it is not a question of a short sentence, but the part which is wanting - which must, therefore, have been lost if originally in the text - must have been a narrative of considerable compass. Nor is it a case where the section was of such a character that it could disappear without notice, because an intelligible connection remained after it was left out; it is rather the question of the concluding section, which the reader must await with interest after what precedes, and the loss of which must leave the book noticeably incomplete.[24]

This argument holds whether the loss of the supposed original ending were accidental or deliberate. To make the suggestion

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24. Zahn, op. cit., p. 478.

now being considered at all credible one would have to imagine an almost immediate loss or excision of the original ending. But then, again, would not such a loss have been made good by the author himself, for a mutilated Mark would have once attracted attention and suggested investigation? To save the theory an explanation bordering on the desperate is offered: that Mark died almost immediately after finishing his Gospel, so that the chance of restoration of the original ending became impossible. To be sure, only such an explanation or that Mark was suddenly stricken with madness after the writing of the Gospel could save this particular theory. Besides, tradition seems to show that Mark published the book himself.[25] Streeter has seen particularly clearly the force of this argument, and his way of meeting it is worth mentioning, although Streeter himself calls it a speculation.[26] Streeter guesses that the original ending of Mark contained an account of the appearance to Mary Magdalene followed by one describing Jesus' appearance to Peter and others while they were fishing in Galilee; he believes, further, that St. John derived his version of these from Mark. This original ending was preserved in Ephesus for some time, but was lost in Rome, where the Longer Ending was added, but could not maintain itself, because of the fuller account of the same material in John, against the

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25. Zahn, ibid., pp. 433 and 479.
26. Streeter, op. cit., pp. 351-360.

Gospel with the Longer Ending, supported as that was by the influence of the church at Rome. Streeter himself writes in the concluding paragraph concerning his guess as follows:

> "Such cogency as the foregoing arguments possess is largely dependent on the correctness of the analysis of the sources of John essayed in a later chapter. And, even if the correctness of that analysis be assumed, they fall far short of proof. Yet the view that the earliest account of the Resurrection Appearances has disappeared without leaving a trace is in itself so improbable that I have thought it worth while to outline a hypothesis which makes it possible to affirm the contrary, even though from the nature of the evidence it can be no more than an interesting speculation."

When the propounder of a view himself admits its weakness we may be excused any further analysis of the argument. I would point out, however, that Streeter's view does not explain the lack of all traces of the original ending which maintained itself for some time in Ephesus. To say that it appears in an adapted way in John does not meet the difficulty. Besides, "an interesting speculation" like this is not really necessary to explain the facts. A simpler way remains.

We are left, then, with the other alternative: accept the Longer Ending as the intended ending of Mark, in spite of all the difficulties involved. Those who accept the Longer Ending as genuine will have to explain two things: 1. the lack of the ending in some manuscripts and the circulation of an alternative ending in other manuscripts, as

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well as the silence of some of the Ante-Nicene fathers (this last is not strictly necessary); and 2. the lack of unity between vv. 1-8 and 9-20.

The first of the two demands is comparatively easy to fulfil, the second very difficult. The loss of a final page of one of the early manuscripts of the Gospel would explain the transcriptional difficulty. That Mark's Gospel might suffer in this way is more likely than that some similar accident would happen to one of the other Gospels, for Mark was often last in the Gospel codices. A manuscript with the ending lost by some accident or other would explain the lack of the ending in other manuscripts. A mutilated Mark would invite completion by some sort of ending as we have in the Shorter Ending. The complete Mark might well be unknown by some of the early Christian writers. Manuscripts with both endings would result as a matter of course when comparisons were made in a matter so noticeable as this. There is also the possibility of a deliberate excision of the disputed ending on the grounds either of its supposed lack of consistency with the other Gospels or of its lack of continuity with the foregoing verses. The former of these reasons for excision I do not consider at all likely, but the latter is a distinct possibity. But that the first is not improbable is shown by the statement in Eusebius's comment quoted in Appendix C, where this motive for rejecting the Longer Ending is added to the evidence he

cites from the manuscripts then current. Naturally, whether the Gospel lost its ending accidentally or was deliberately mutilated, the result upon the history of the text would be the same, i.e. the situation would result which we now see to be the case. It should be carefully noted that the argument we are now using to explain the present state of the text, that is the gaps in the documentary evidence for the Longer Ending is, in spite of its similarity to the argument examined, and rejected, above to explain the loss of the supposed genuine ending, radically different. There the argument was used to explain the complete absence of a supposed original ending; here the argument is used to explain the occasional absence of the Longer Ending. There we said that a lost page sometime after the promulgation of the Gospel would not solve the complete absence of the genuine ending, and that an immediate loss of a page would demand the coincident miracle of the death or madness of Mark to explain the complete loss of the original ending; here we say that the lost page explains the gaps in the evidence for the Longer Ending, for which we have plenty of evidence as far as the history of the text takes us back. The lack of attestation here and there can accordingly be explained satisfactorily. Certainly the lack of such a long paragraph in some manuscripts is without parallel in the text of the New Testament, but, theoretically, that such a thing should happen is not

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at all impossible. Rather is it strange that some such thing did not happen much more frequently. Zahn (Geschichte... Vol. II, p. 934) claims that the loss of a page to explain the lack of the Longer Ending in many manuscripts in the early fourth century and earlier is an unsatisfactory explanation. He argues that such a loss would explain the existence of abrupt manuscripts in an isolated geographical region, but not the existence of such manuscripts throughout the Roman Empire. "In Rom oder Kleinasien, Aegypten oder Palaestina koennte sich diese Geschichte doch nicht abgespielt haben. Der Verkehr mit anderen Gemeinden, das Ab- und Zustroemen auswaertiger Christen musste diese Sonderbarkeit der betreffenden Kirchenprovinz bald ans Licht ziehen, and der Trieb den anstoessigen Buchausgang zu verbessern, dessen Macht auch bei dieser Annahme wenigstens II (the Shorter Ending) bezeugt, musste bis zur Zeit des Eusebius die zufaellig entstandene defekte Ausgabe ueberall wenn nicht verdraengen, so doch um die Herrschaft in dem weiten Kreise bringen, in welchen I (the ending at v. 8) nach den Zeugen fuer I and II tatsaechlich geherrscht hat." The argument is sound enough, but does it take into account the strangeness of the Longer Ending? When comparisons were made between copies with the abrupt ending and those purporting to be the true text, i.e., the text ending at v. 20, would the critic be inclined to accept the Longer

Ending? I think not, since he would ask himself, as we do now: How could that ending be original? One could recall here what Streeter says about the critical ability of the early Alexandrian scholars, and might imagine what their reaction would be, if, when looking for the true ending to v. 8, they were presented with vv. 9-20 (See above p. for the statement of Streeter here referred to). The point is that the Longer Ending is so strange that once lost in some manuscripts it would find it hard to gain recognition again. Many would prefer to have a Mark that ended at v. 8. abruptly than one that ended at v. 20, and so manuscripts would keep on being multiplied ending at v. 8, some of them, being amplified with the Shorter Ending, hardly many, for the Shorter Ending is not at all strongly attested. At the time of the recensions only would the Longer Ending come again into its own.

When we take up the question of the lack of unity of the Longer Ending with vv. 1-8, we come up against something much more difficult. (It must be admitted/at the very outset/ that the Longer Ending as it stands is no continuation of the matter of the beginning of chapter 16. All the particulars advanced to show this lack of continuation and essential unity, I think, must be admitted by everyone as really valid objections to the unity of the passage. But whether that proved that Mark could not have written the Longer Ending is another question altogether.

First, we may state that there is no good reason for believing that Mark was absolutely incapable of writing the actual words of and matter contained in the twelve verses. Zahn argues that content, the character of the narrative, and certain actual expressions of the twelve verses prove that Mark could not have written them. It is claimed that vv. 9-11 are taken from John 20: 1-18 with the insertion of a phrase from Luke 8:2 (the reference to the seven devils of Mary Magdalene); that vv. 12 and 13 come from Luke 24: 13-55, the dependence being in part verbal, but with omission of all details; and that, since John and Luke wrote later than Mark, Mark could not have copied from them, and, accordingly, could not have written the Longer Ending.[27] Now, that the material of vv. 9-13 corresponds with or covers material contained in Luke and John is plain as can be. The verbal likenesses are too inconsequential to prove anything. But, surely, even for the matter itself, why must one claim that Mark could not have written of the appearances of Jesus to Mary Magdalene and the disciples on the way to Emmaus? Those things were common knowledge among the early Christians. The story must have been told and retold. Mark was one of the early Christians, in constant touch with the disciples. And now we are to believe that he could not have told either of the stories, that he could not even do so much as to give

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27. Zahn, op. cit., pp. 475 f.

the shortest of resumes of those events! Is not his whole Gospel full of stories paralleling stories in Luke and Matthew, and more rarely, John? Certain critical theories of the Synoptic Gospels it is true make, now one, now the other of the evangelists depend on the third. Into these theories it is not the place to enter here, except to say that there is nothing inherently improbably that men should write independent lives or accounts of Jesus and His work which would yet show remarkable resemblances. The references to the appearances of Jesus in the Longer Ending although parallel to Luke and John still have their own independent emphasis, that of the unbelief of the disciples. Of this more later. The contention of Zahn, then, that Mark could not have written the twelve verses because of that parallelism, or, in his view, borrowing, is distinctly not proved. The two points of language which are supposed to show that Mark could not have written the twelve verses are the use of πρώτῃ σαββάτου for μιᾷ τῶν σαββάτων the only usage current in the Apostolic Church and the term, κύριος , which Mark does not use elsewhere. Can we say that we know the linguistic usage of the Apostolic Church completely? What if πρώτῃ σαββάτου is used only here? It is better Greek, as Zahn admits, than μιᾷ τῶν σαββάτων . Well, then, why could not Mark have used it? One really can not argue: Such and such an expression, although perfectly legitimate, is never used anywhere in the New Testament otherwise,

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therefore, it could not have been used by one of the writers of the New Testament here. If the phrase were one very strange in itself, the argument would have more force, but it is a perfectly straightforward variation of μιᾷ τῶν σαββάτων (which Mark, by the way, does use in v. 2) and one which Mark might well have got used to from using Latin in Rome.[28] The argument drawn from the use of Κύριος in the Longer Ending similarly lacks all real cogency. Zahn's argument runs: Κύριος is not used in Matthew, rarely in Luke and John, and only in this disputed section of Mark. Ergo, Mark did not write the section where Κύριος is used. A perfect case of non sequitur on a number of counts. That other writers do not use the term, in this case Matthew, proves nothing; that Mark nowhere else uses the term proves nothing. That Mark must have known the term as applied to Jesus is certain, for he at one time was Paul's companion (on the first missionary journey) and had other close relations with him later (Col. 4:10; 2 Tim. 4:11; Phil. 24), and even if he had never heard the term Κύριος applied to Jesus before, a most unlikely presupposition, he must have heard it from Paul, most of whose letters, if not all, were written before Mark wrote his Gospel. That Mark, finally, uses the term Κύριος of Jesus in a particularly appropriate place, i.e. in speaking of the ascension and sitting at the right hand of the Father, should be evident. We really cannot, then, from the use of these two expressions, two words really, πρώτη

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28. Burgon, The Last Twelve Verses of the Gospel according to St. Mark, pp. 146- 151.

and κύριος argue that it was humanly impossible for Mark to have written the ending so long ascribed to him. To continue the argument from vocabulary a little further it is not hard to make up a list of words and expressions found in the last verses which are either wholly Mark and/or preeminently so, like the following: ἀναστάς, πρωί, κηρύσσω, ἐπὶ πᾶσαι χῆρας. Of course, it is also possible to make up another list which Mark does not use in the body of the Gospel proper and a list of words used in the Gospel proper which are not found here, but the words in the lists so constructed are common enough words which any writer might well have used or not, all depending upon the thing he had to say at the particular moment. There is an extensive study of the vocabulary of the ending as compared with the vocabulary of Mark generally in Burgon,[29] to which the curious reader may be referred. The matter of vocabulary is not really important, WH deprecate the number of inconsequential arguments which have been advanced on either side with respect to Mark's use of words. The only point we are making here in referring to vocabulary is that there is nothing in the actual words used by Mark which makes it absolutely impossible for him to have written the twelve verses, that the vocabulary speaks about as much for Mark as it would for any other writer. So neither content nor vocabulary make it impossible for Mark to have written the Longer Ending. Zahn refers also to the character of the ending as disqualifying Mark from its authorship. The difference in style is certainly

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29. Burgon, ibid., the pertinent chapter.

well marked, especially in vv. 9-13 and 19 and 20. But style is another of these intangibles, and the argument drawn from it may be very precarious and unconvincing. Where are we to draw the boundary in style and say: Such and such a sentence, paragraph, chapter Mr. X could write, but this chapter, paragraph, sentence he could not have written? It should be plain that such a statement can be made only in cases of most glaring differences. For instance, no modern American school-boy could be credited with Samson Agonistes, nor Carlyle with a comic strip. Of course, the reader must forget chronology here and think of these pairs purely from the point of style and expression. Naturally, the examples given are gross exaggerations, but the point to be made is clear: only in cases of really fundamental differences can we say: Mr. X could not have written that. This is all the more the case when we are dealing with rather short extracts. No one is likely to mistake the author of Adam Bede for the writer of Henry Esmond or Nicholas Nickleby, as no one would think that Mozart wrote the Mass in B Minor. In large chunks or slabs or pieces of writing the individuality of the writer will necessarily force its way through, but an individual paragraph, even a longer one, taken from its context, or written by itself, will often defy even the most sensitive critic to assign it to the true author. With respect to Mark, in particular, it

may well be questioned whether we know enough of his writing to say what he could and what he could not have written. The whole Gospel numbers not much more than 15000 enough to arrive at the essential characteristics of the Gospel, but not enough to characterize for us the style of the whole man. The Longer Ending is a short paragraph. That it does not read like the rest of the Gospel is elementary, but that it could not have been written by Mark is a different thing altogether. Let us proceed a little further into this examination of style. It is possible for the same man to write distinctly different styles when he is writing for completely different purposes, although again, if the material he writes is long enough, his individuality will almost certainly shine through somewhere. There may be considerable difference stylistically, for instance, between the sermon as written to be preached in thirty minutes and an abstract of 200 words of that sermon submitted for publication in the daily press. A priori, then, it may be doubted whether we know enough of Mark's style of writing as a whole to be able to assert dogmatically that Mark could not have written the Longer Ending, if for some reason he so desired to write it that way. A posteriori, we may even say that, even though the Longer Ending is so short, too short to be used as a basis for an assertion of its style as related to any particular writer, there are indications there of resemblances to the Mark of the whole Gospel. We have

picturesque details in the "mourning and weeping" of v.10, the going "into the country", v. 12, the "serpents" and the "deadly thing" of v. 18. We have new material in the "first" of v. 9, the sessio ad dextram of v. 19, in fact, much of the section, vv. 14-18. Summing up, there seems to me to be no good reason for asserting that Mark could not have written the Longer Ending, whether from reasons of content, vocabulary or style,

The writing of the Longer Ending by Mark, judged from the point of view of abstract possibility, can be maintained. I go further now to declare that it is more likely that Mark wrote that ending than that any one else wrote it. In defending this statement we must start from the lack of unity between vv. 1-8 and 9-20. This point has been referred to before and everything that has been said against the existence of such unity has been admitted. It is the conclusion drawn from it that I believe to be all wrong. The argument runs: the Longer Ending can not in any way be looked on as a continuation of and satisfactory ending to the narrative begun in chapter 16; ergo, Mark the writer of chapters 1-16: 8 did not write the Longer Ending. The true conclusion would be: ergo, no one wrote the Longer Ending. For what person, faced with the abruptness of v. 8 and anxious to supply a satisfactory ending and continuation, writes an ending that is no ending and a continuation that is no continuation? And if some misguided and very stupid compiler

writes such an ending and continuation that is none, how could the Christian Church, which presumably is not composed completely of ignorant people, accept such a continuation as good and satisfactory? The more strongly the case against the unity of the Longer Ending with the rest of the Gospel is put and expounded, the more difficult it becomes to believe that any body ever wrote it deliberately as a continuation of the Gospel. The fundamental difference between the two endings, the Longer and the Shorter, appears right here. The Shorter Ending does in its own rather clumsy fashion try to supply a real continuation. The first sentence does pick up the thread of the first verses: "but they reported briefly to Peter and his companions all they had been told". (k changes v. 8 considerably to supply a still smoother continuation of this sentence with what precedes.) Then follows in the Shorter Ending a short summary of the work of the apostles: "And afterward Jesus himself sent out by them from the east to the west the sacred and incorruptible message of eternal salvation." The Longer Ending, as everyone points out, does not provide any sort of continuation of v.8. The Longer Ending, then cannot be explained in the same manner as the Shorter Ending. It is in a class by itself. Given such an inexplicable continuation, it is inherently more probable that Mark wrote it than that any one else did. The creator or originator or author of a

work is bound by no such limitations as the mere completer or finisher of an unfinished work. The completer must be led by what has preceded and he will add a dull and consistent and logical conclusion, but the author is led only by the guidance of his own genius and originality. He can do as he pleases and bid defiance to what the critics expect of him. Paradoxical though it may seem, then, the very queerness and seeming complete unfittingness of the last verses is more easily explained as the work of Mark than that of any imaginable completer of an unfinished work. The actual material of the ending, further, adds to the likelihood that the ending as we now have it is from Mark. It is admitted by most scholars who oppose the passage we are discussing that the passage contains material that is very ancient, that it is rich in content. Thus Swete speaks of the silence of the fathers between Irenaeus and Eusebius with respect to a very rich passage as something to be explained by its defenders;[30] Gregory's opinion that it may almost be better than Mark's original we have referred to, as well as to that of WH who say that the Shorter Ending would never have been exchanged for the rich twelve verses. Tregelles believed much the same. His opinion is contained in the following quotation from Salmon who sums up neatly the point being made at this stage of the argument:

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30. Swete, H.B, The Gospel according to St. Mark, pp. 146-151.

> The twelve verses have such marks of antiquity that Dr. Tregelles, who refused to believe them to have been written by St.Mark, still regarded them as having "a full claim to be received as an authentic part of the second Gospel"... The twelve verses are clearly the work of one who wrote at so early a date that he could believe himself able to add genuine apostolic traditions to those already recorded. If he asserts that Jesus "was received up into heaven and sat on the right hand of God" he only gives expression to what was the universal belief of Christians at as early a period as anyone believes the second Gospel to have been written.... Further, the twelve verses were written at a time when the Church believed herself in possession of miraculous powers. Later, a stumbling-block was found in the signs which it was said (v.17) should "follow them that believe." The heathen objector, with whom Macarius Magnes had to deal, asked if any Christians of his day really did believe. Would the strongest believer of them all test the matter by drinking a cup of poison? The objection may have been as old as Porphyry, and may have been one of the reasons why Eusebius was willing to part with these verses. We must, therefore, ascribe their authorship to one who lived in the very first age of the Church. And why not to St. Mark?[31]

Burgon finds particular points of Markan (? I should say, apostolic) authorship in such phrases as: τοῖς μετ' αὐτοῦ γενομένοις, αὐτοῖς τοῖς ἑνδεκα, ἐξελθόντες.

There is a ring of apostolic originality about the references to Mary Magdalene, the weeping of the disciples, the great number of minute but important facts collected in the compass of the twelve verses and in the details which appear nowhere else. These facts all add to the likelihood that the Longer Ending was written by the writer of the rest of the Gospel.

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31. Salmon, op.cit., pp.150 f. It is here that it is convenient to refer to the opinion held that the Longer Ending, while not Markan, still emanated from Mark's immediate circle, and was an early addendum to the Gospel, bearing much the same relation to the rest of Mark, as John 21 is often held to bear to the rest of the Gospel of St. John. I repeat the argument of Salmon. If we are to ascribe the authorship of the disputed verses to one who lived in the very first age of the Church, why not to St.Mark? It is argued in the body of the paper that the difference of style does not demand some author apart from Mark, while the other argument made much of there: that it is difficult to imagine anybody's inventing such an odd continuation, would hold here also.

A Suggested Solution.

One thing more remains to be done to make the defence of the Markan authorship complete: to propose some explanation for the lack of a readaconnection between v. 8 and the Longer Ending. It is one thing to state that the mere words of the Longer Ending could have been written by St. Mark; it is quite another to say that it was the intention of St. Mark to conclude his Gospel in such a strange manner. It is one thing to maintain that it is more likely that Mark wrote the Longer Ending and appended that unorthodox ending to his Gospel than that some one else invented it and added it there to overcome an intolerable abruptness; it is quite another thing to maintain that the Longer Ending is likely in itself. There is a relative likelihood and an absolute likelihood. So far we have shown the Markan authorship to be but relatively likely. As far as real likelihood goes, the Longer Ending is confessedly unlikely. On the very face of it, it is very unlikely that any author, no matter how original and wilful, would follow v. 8 with v.9 and what follows it. There are certain laws of thought and expression to which any writer who wants to be understood and who is still compos mentis must conform. What is wanted, then, is a satisfactory explanation to cover the fact of Markan authorship and the evident lack of continuity. If such an explanation can be supplied, and if that explanation is not mere romancing but one that commends itself as being not improbable, then the case may be considered complete. The defender of the Markan

authorship can not be called upon to prove that such and such a thing must have happened. None of the explanations advanced for the various views held by critics have done or can do that. The whole difficult problem is not one that can be decided absolutely; no person holding a conviction here should be expected to show the absolute necessity of his solution. All we can do is hold up probability against probability and show that this or that solution is the more probable.

In propounding our solution we may well begin with Zahn. This eminent scholar believes that the Gospel was never completed, death or some other compelling circumstances arresting Mark's pen. Since tradition seems to show that Mark published the book himself, its incomplete form would be incomprehensible only in case a few lines were wanting which the author and editor could have added at any time. So the small compass of the work, when compared with the other historical books of the New Testament, "leaves room for the conjecture that Mark intended to add several portions to his work."[32] These portions might have included other things besides the resurrection appearances. From here I shall quote Zahn directly:

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32. Zahn, op. cit., p. 479.

> If he began writing the Gospel before the death of Peter (64), but did not publish the same until after the death of Paul (67), things enough could be mentioned which must have interrupted the pen of this spiritual son of Peter and younger friend of Paul in the city where both the apostles had died as martyrs, and which also in the time immediately following must have prevented him from at once completing his book as he desired. If, in these circumstances, he yielded to the request for its issue, it would not have been something unheard of or irrational. It is perfectly possible also that during the months and years while he and others were hoping for the completion of the interrupted work he had given the unfinished book to friends to read, and that they had made several copies without his being able to prevent it.[33]

This explains everything but the Longer Ending. Zahn puts the Longer and Shorter Ending in the same boat, but if the argument above is sound, the two are toto coelo disparate, nothing being surer than that no completer of the work would have done so with the Longer Ending. Zahn's explanation becomes completely satisfactory if we add to it that in the Longer Ending we have the outline of that completer ending which Mark intended to add at his own time, but from which he was prevented by some compelling circumstance. It is surely plausible in the extreme that if Mark found himself in the position of having to publish the work before he had completed it, he would have published it with a short summary of the intended conclusion rather than leave it with the strange abruptness of v. 8! The Longer Ending is the short summary of the intended concluding section of the Gospel of Mark. That being the case, we have a clear guide of the course the concluding section was to take, we have a clear indication of

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33. Zahn, op. cit. The whole discussion of this point is found on pp. 479f.

its underlying unifying motif, and a satisfactory explanation for all the problems the present Longer Ending sets us.

The complete Mark would have followed the incident describing the finding of the empty tomb and the commission of the angels with an account of the appearances of Jesus to Mary Magdalene and the disciples on the way to Emmaus. A conversation of Christ with his apostles, in which the Lord's parting instructions and promises of His presence and aid, was then to follow, after which a reference to the ascension, or perhaps a fuller account of it, was to be given. The whole was to finish with a summary of the apostles' work of proclaiming the Gospel everywhere, the end of Mark thus returning to the beginning. The underlying motif of the concluding section is plain from the outline we have: it was to present clearly the unbelief of the apostles and to oppose and contrast that unbelief with faith by which alone there is salvation for men. Compare vv. 11, 13, 14 (bis!), 16, 17 for a quite remarkable emphasis on the contrast: unbelief - faith. Such a longish ending, it most likely would have been at least as long as Luke's last chapter, would be remarkably in line with the plan of the other evangelists and would be in keeping with the general character of the relation of the evangelists to one another. The scheme of all the evangelists in their handling of the resurrection story is: empty tomb, angels' commission, appearances, last words, the removal of Jesus'

earthly, visible presence. There would be in the various Synoptists a general correspondence of scheme coupled with actual parallel accounts now with the one, now with the other, evangelist; but at the same time Mark would have material the others did not include in their accounts. In these particulars: much the same general plan, parallel accounts, unique material, the resurrection story of the three Synoptics would be in complete harmony with the whole life of Christ which the three present. This need not be labored: the whole remarkable picture of almost verbatim parallels coupled with wide differences, the picture that the first three Gospels presents to us, that is the very heart of the Synoptic problem. The complete resurrection story of Mark as indicated to us by his outline in the Longer Ending would, with the accounts of the other Synoptists, be but part of the bigger Synoptic problem.

The suggestion put forward here as to the proper understanding of the Longer Ending, it seems to me, would offer a reasonable and unforced explanation for all the problems in the way of intrinsic evidence which we face. No one would dispute the fact that the Longer Ending looks like a summary, sounds like one, runs like one. True, the vv. 14-18 are fuller than the other verses, but it is not at all out of the way for a writer, when making a summary or outline, to sketch out certain parts

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more fully and elaborately than others; some things he has at his finger tips, other points he wishes to fix in writing carefully as they suggest themselves, in case he overlook them later on or fail to find the same happy expression. So the difference between vv. 9-13 and 14-18 need not detain us. If the Longer Ending is but an outline then its strangeness of style as compared with the rest of the Gospel is at once accounted for. One can't write a one sentence summary of an incident as one would describe the whole incident in full. Naturally one will find the carefully constructed passage rather than a succession of short paragraphs; naturally when the historical description is only hinted at in the summary and the didactic purpose mentioned, it will appear more didactic than historical, the compressed account will of necessity upset somewhat for the reader the relation, the perspective of the various parts; naturally there will be no call for Mark's impetuous rushing εὐθύς . If we have in the last twelve verses, again, only a summary and outline, then we can explain too the lack of connection with the words of vv. 1-8 written in the finished form and the outline which is attached to them. In the final form St. Mark undoubtedly would have provided a smooth transition from the awesome fear of the women, in which they were placed by the words of the angel, to Christ's appearance to Mary Magdalene and the allaying of that fear. If, finally, we have in the Longer Ending an outline of Mark himself we can see why it was generally accepted in the Church (I believe that the external evidence argues a general acceptance in the early Church and not the opposite as some have maintained) inspite of the strangeness of the section itself

and its relation to what precedes. There was no doubt from the start that Mark wrote it, many no doubt knew from Mark's own mouth why he had to publish the Gospel provided with a summary of the last chapters instead of in its intended complete state. It was only after the later accident of the lost page that doubt would arise, and we might say, an understandable or even a justifiable doubt, for the combination of a strange ending together with manuscripts not containing the ending at all is a strong argument on the surface for the spuriousness of the last verses. But when the problem is seen in its deepest implications, there is no satisfactory explanation of the Longer Ending except that Mark wrote it himself.[34]

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34. Another view in defence of the Longer Ending is that of Bover. His view is given in Metzger's article on Recent Spanish Contributions.... referred to above. What is the reason for the multiplicity of endings to the Second Gospel? Bover answers this question on the basis of logic and syllogism. His first proposition is "The Second Gospel is the close reproduction of the evangelical catechesis of St. Peter." The second is, "The recounting of the resurrection of the Lord and his appearances to his apostles, who were to be his witnesses, did not pertain to the evangelical catechesis, directed to those who believed, but to the previous apologetic proof, directed to those who had not yet embraced the faith. The narration of the appearances, being known by the previous proof, did not need to be repeated in the evangelical catechesis." Accepting these propositions as true, Bover offers two hypotheses either of which he thinks, would account for the textual phenomena. According to one hypothesis, when Mark published his Gospel, he added to the Petrine catechesis his own account of the appearances of Jesus (Mark 16:9-20). This explains, Bover believes, the difference of style between the body of the Gospel and the canonical conclusion. This difference of style was detected by certain in the early church, who were led thereby to suspect the authenticity of the last twelve verses. As a result of their suspicions and doubts, these verses were omitted by several subsequent copyists, a circumstance which accounts for their absence in codices B etc.

(Continued p. 88.)

The Ending of Mark's Gospel, then, sets a special problem to the textual critic. In essence this problem is the same as any other, and it must be tackled the same way. We must weigh external evidence and internal evidence and transcriptional probabilities. But it still is a special problem, offering difficulties of a unique kind. No one can condemn the critic who proposes a special, even unique solution. For the solution offered in this thesis we can claim no more than a strong probability. Although much of the solution is based, as it appears to me, on sound arguments, some of it is plainly pure guess-work. There is no proof of a lost page, as there is no real proof that the Longer Ending is an outline merely which Mark hoped to expand, but from which intention he was kept by this or that compelling circumstance. These are but probable suggestions to explain, first, the gaps in the manuscript evidence and secondly, the strange lack of continuity between vv. 1-8 and 9-20. The person who will not accept them must find other guesses to bolster up his position, and all of the positions so far taken up by critics are open to far more serious objections both on external and internal

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34 (continued) But the Second Gospel, in this shortened form, seemed to others in the early church to lack a proper conclusion. These undertook to supply various endings two of which are extant to-day, one brief (in codex L, etc.) and one longer in W. (pp. 413, 414). An extended criticism of this is not called for. One can see that this view demands rather more arbitrary assumptions than the one defended above, and for that reason will hardly win much approval even among defenders of the Longer Ending.

grounds than the one defended here. In the long run not a great deal depends upon the inclusion or rejection of the last twelve verses. The Bible Christian, the Lutheran Christian perhaps more so than others, will regret the absence particularly of 16:15, 16, but the same truths these verses teach are taught in undisputed passages of the Holy Scriptures. However, there is no reason for rejecting parts of the Bible before such rejection is absolutely demanded by the evidence, and the Holy Scriptures of our God are so precious to all Christians that no effort of scholarship must be begrudged which will restore to us the original text of the New Testament.

Appendix A. Further Details of Documentary Evidence.

1. With respect to the testimony for the ending at v. 8. There is a reference at p.45 to the cursives 237, 239, 259. These are three Moscow manuscripts which, according to WH and Burgon, are related to the statement in cursive 255, and, according to Zahn, the complete scholium is found in cursive 36 from the tenth century. The whole question of this scholium is a highly complicated one and in details all three scholars mentioned are in disagreement. But both WH and Zahn are in agreement about this that the scholium comes from Eusebius. Now as to the bearing of all this on our problem. If it is from Eusebius, we have no new testimony at all, only stronger testimony to what Eusebius thought. If it is not the statement of Eusebius, then another name is added to the critics of the Longer Ending or rejection of it. See Zahn, Geschichte des Ntl. Kanons, pp. 915-917 and WH, Appendix, pp. 32 ff. for thorough ventilation of this minor point.

2. With respect to the Shorter Ending.

k reads as follows: a monumento fugerunt. tenebat enim illas tremor et pavor propter timorem (8b.) Then follows the Shorter Ending. Notice the omission of the words recording the failure of the women to say anything (οὐδενὶ οὐδὲν εἶπαν).

Zahn, op. cit., pp. 923 f. argues that the connection of the Shorter Ending with the foregoing is always closer than the Longer Ending wherever the two are found together. But

it is plain that the only possible arrangement, if you are going to write both the Longer and Shorter Ending after v. 8., is Shorter Ending, Longer Ending, since the Shorter Ending was definitely written to continue the thought uncompleted of v. 8., and any scribe comparing the two endings would see which followed smoothly and which did not.

Zahn's conjecture that the Shorter Ending sprang up in Egypt is very likely correct, since it is found chiefly in the uncials supporting the text of Egypt, and its appearance in Coptic and Ethiopian manuscripts supports this conjecture. Egypt, too, is the center where the Longer Ending has the the least attestation. Add as witness to this the Shorter Ending l 961, l 1566.

3. With respect to the Longer Ending.

It should be noted that in the Old Latin tradition the manuscripts a b e are defective at this point, i.e. they are not witnesses in this whole question at all. Although the manuscripts a and b are defective here, the Longer Ending is contained in n, which, according to Francis C. Burkitt, The Old Latin and the Itala in Vol. 4 of Texts and Studies, ed. J. Armitage Robinson, Cambridge, 1896, closely resembles a in St. Mark.

The following quotations from Justin Martyr are almost certainly indications that he knew the Longer Ending as Markan: Apology I, 45: οἱ ἀπόστολοι αὐτοῦ ἐξελθόντες πανταχοῦ ἐκήρυξαν. Dial. cum Trypho: 32, 9: εἰς τὸν οὐρανὸν ἀνελήφθη, following the previous words, God being the subject: ἀναγαγόντα αὐτὸν ἀπὸ τῆς γῆς καὶ καθίζοντα αὐτὸν ἐν δεξιᾷ αὐτοῦ.

There may be some doubt about the second, since Acts 1:11 might be the source. Among the Fathers or contemporaries of the early Fathers the following may be witnesses for the text on the admission of Zahn, op. cit., pp. 924-926: (This is aside from those mentioned as certain witnesses in the body of the article). Celsus, Papias, Porphyrius. Ignatius is mentioned as being acquainted with the passage by Pott, op. cit., p. 74.
Both Zahn and WH deny that the Sahidic version favors the Longer Ending, but Huck, who boldly heads the section Der unechte Markusschluss, quotes sa in his list of witnesses for the Longer Ending, as do Souter and Nestle. It is stated expressly in the introductory notes to Huck's work that all the data in the critical apparatus have been derived afresh from original sources and not merely copied from the apparatus critici of other editions.
Merk agrees with WH and Zahn. His apparatus shows that some sa manuscripts favor the Shorter Ending. Merk's edition is later than Lietzmann's edition of Huck, if that means anything.
Gregory in his Textkritik gives the following pertinent facts concerning the Sahidic translation, on page 534. The translation exists in a great number of fragments, which together gives us all the Gospels save 66 verses. He states that Mk. 16: 20 is to be found, but nothing from Mk. 15:41 on till that verse. He wrote before Horner's great work on this translation. Horner's edition shows 35 verses partly or wholly missing in Mark, according to Souter in The Text of the New Testament, but he gives no details. In a note in the same work on the same page, p. 66, he informs us

that since Horner's edition complete manuscripts of Matthew, Mark and John have been found, again without details. I have not been able to consult the necessary works to come to a decision on this point.

Appendix B. The Freer Logion.

Every critical edition of the Greek New Testament printed within the last thirty years will contain the text of this lengthy addition to Mark 16:14. I give, first, Moffat's translation of the Greek text, and then the statement of Jerome which contains a reference to this Logion. It is found in his Contra Pelagianos, ii.15. The Latin text given below has been taken from Lietzmann's edition of Huck's Synopsis.

1. "But they excused themselves, saying, 'This age of lawlessness and unbelief lies under the sway of Satan, who will not allow what lies under the unclean spirits (or, the unclean things that lie under the control of spirits) to understand the truth and power of God; therefore,' they said to Christ, 'reveal your righteousness now.' Christ answered them, 'The term of years for Satan's power has now expired, but other terrors are at hand. I was delivered to death on behalf of sinners, that they might return to the truth and sin no more, that they might inherit that glory of righteousness which is spiritual and imperishable in heaven.' "

2. In quibusdam exemplariis et maxime in graecis codicibus iuxta Marcum in fine eius evangelii scribitur: 'postea quum accubuissent--crediderunt(v.14). et illi satisfaciebant dicentes: saeculum istud iniquitatis et incredulitatis sub satana est, qui non sinit per immundos spiritus veram dei apprehendi virtutem. idcirco iam nunc revela iustitiam tuam.

Appendix C. Eusebius and Jerome.

Below will be found the Greek text of the famous statement of Eusebius concerning the ending of Mark. The text is that of Migne, Patrologia Graeca, vol. 22, coll.937, 938. The Greek text is followed by the English translation of it by WH, found on page 31 of their Appendix. The reader will notice the dependence upon the statement of Eusebius of the Latin extract from Jerome which follows. The Latin text comes likewise from WH, op.cit. p.33. The Latin extract is part of Jerome's answer to the third question of one Hedibia.

1. τούτου διττὴ ἂν εἴη ἡ λύσις· ὁ μὲν γὰρ τὸ κεφαλαιῶν αὐτὸ τὴν τοῦτο φάσκουσαν περικοπὴν ἀθετῶν. εἴποι ἂν μὴ ἐν ἅπασιν αὐτὴν φέρεσθαι τοῖς ἀντιγράφοις τοῦ κατὰ Μάρκον Εὐαγγελίου· τὸ γοῦν ἀκριβὲς τῶν ἀντιγράφων τὸ τέλος περιγράφει τῆς κατὰ τὸν Μάρκον ἱστορίας ἐν τοῖς λόγοις τοῦ ὀφθέντος νεανίσκου ταῖς γυναιξὶ καὶ εἰρηκότος αὐταῖς, Μὴ φοβεῖσθε, Ἰησοῦν ζητεῖτε τὸν Ναζωραῖον· καὶ τοῖς ἑξῆς, οἷς ἐπιλέγει· καὶ ἀκούσασαι ἔφυγον, καὶ οὐδενὶ οὐδὲν εἶπον, ἐφοβοῦντο γάρ. Ἐν τούτῳ γὰρ σχεδὸν ἐν ἅπασι τοῖς ἀντιγράφοις τοῦ κατὰ Μάρκον Εὐαγγελίου περιγέγραπται τὸ τέλος· τὰ δὲ ἑξῆς σπανίως ἔν τισι καὶ οὐκ ἐν πολλοῖς· φερόμενα περιττὰ ἂν εἴη, καὶ μάλιστα εἴπερ ἔχοιεν ἀντιλογίαν τῇ τῶν λοιπῶν εὐαγγελιστῶν μαρτυρίᾳ· ταῦτα μὲν οὖν εἴποι ἄν τις παραιτούμενος καὶ πάντα αἱρῶν περιττὸν ἐρώτημα. ἄλλος δέ τις οὐδ' ὁτιοῦν τολμῶν ἀθετεῖν τῶν φερομένων ἐν τῇ τῶν εὐαγγελίων γραφῇ φερομένων, διπλῆν εἶναι φησὶ τὴν διήγησιν, ἃς καὶ ἐν ἑτέροις πολλοῖς, ἑκατέραν τε παραδεκτέαν ὑπάρχειν, τῷ μὴ μᾶλλον ταύτην ἐκείνης ἢ ἐκείνην ταύτης παρὰ τοῖς πιστοῖς καὶ εὐλαβέσι νενοηκέναι.

2. The solution will be twofold. For one man, rejecting the passage by itself, the passage which makes this statement, will say that it is not current in all the copies of the Gospel according to Mark. That is, the accurate copies determine the end of the narrative according to Mark at the words of the young man who appeared to the women and said to them, "Fear not!...." And they, on hea ring this, fled and said nothing to anyone, for they were afraid. At this point the end of the Gospel according to Mark is determined in nearly all the copies of the Gospel according to Mark; whereas what follows, being but scantily current, in some but not in all (copies), will be redundant (i.e. such as should be discarded), and especially if it should contain a contradiction to the testimony of the other evangelists. This is what will be said by one whol declines and entirely gets rid of (what seems to him) a superfluous question. While another, not daring to reject anything whatever that is in any way current in the Scripture of the Gospels, will say that the reading is double, as in many other cases, and that each (reading) must be received, on the ground that this (reading) finds no more acceptance than that, nor that than this, with faithful and discreet persons..

3. Huius quaestionis duplex solutio est: aut enim non recipimus Marci testimonium, quod in paris fertur evangeliis, omnibus Graeciae libris pene hoc capitulum non habentibus, praesertim quum diversa atque contraria evangelistis ceteris narrare videatur, aut hoc respondendum....

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