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CHAPTER 1

Mark 16:8 as the Conclusion to the Second Gospel

DANIEL B. WALLACE

Comedian George Burns once pontificated on the key to homiletical success: “The secret of a good sermon is to have a good beginning and a good ending, then having the two as close together as possible.” This essay will abysmally fail at least that third criterion, but I suspect that Mark’s Gospel might have succeeded on all three fronts.

The question we are entertaining at this symposium is this: When does Mark’s Gospel end? For those who have studied the issue at all, there is an ancillary, though equally relevant question: When does the *debate* over the ending of Mark’s Gospel end? This particular debate will be over tomorrow, but that won’t be the end of the story. This conference, in fact, is intended to stimulate your thinking about the issues, getting you to wrestle with the questions far beyond tomorrow. Perhaps that is what Mark intended for his Gospel, too.

Introduction: Presuppositions

Before I get into the details of this notorious textual problem, I need to address the issue of presuppositions for a few minutes.

Two of mine are that, of the four Gospels, Mark wrote first and John wrote last. As well, I hold to the Doddian school that John was not at all dependent on the Synoptic Gospels; in fact, he was most likely unaware of their specific contents, possibly even of their existence. This means that *both* Mark *and* John were writing, in a sense, a new literary genre that would later be called *Gospel*. Yet, they are radically different.

There are several ironies here: If Mark wrote first, he *created* a genre¹ that would be followed by Matthew and Luke, thus giving some vindication to what he had done—since mimicry is a high form of flattery. Yet, Mark's Gospel was the least copied of the four, perhaps because it was almost entirely swallowed up by Matthew, with only the less appetizing parts left out. John, on the other hand, had no real literary followers, yet his is by far the most copied of the Gospels in early Christianity. The irony doesn't stop there. If Mark *intended* to end his Gospel at 16:8, as I will argue, it's a Gospel that leaves the reader hanging, wanting more. John, on the other hand, not only finishes his Gospel, he keeps on writing *after* he finished it. He finishes his Gospel *twice!* After his conclusion in chapter 20, he

1. Robert H. Gundry ("Recent Investigations into the Literary Genre 'Gospel,'" in *New Dimensions in New Testament Study*, ed. Richard N. Longenecker and Merrill C. Tenney [Grand Rapids: Zondervan, 1974]), after examining several attempts at locating the Gospels within a literary trajectory, notes the failure of all such attempts: "Supernaturalism in the gospels presents the biggest bugaboo . . . if aretalogists did not yet apotheosize human beings by attribution of miracles and if study of folklore discovers conservatism on the part of communities, the problem of the shortness of time between the ministry of Jesus and the evangelists' narrations of the supernatural becomes more acute than ever. . . . The more we deny or doubt the historicity of the materials . . . in the gospels, the more the questions become difficult to answer. We leave the gospels' literary uniqueness without an adequate historical cause" (112). He then adds, "On the other hand, high estimation of historicity would supply the missing cause. We could then say that the configurations and particulars of Jesus' career shattered literary conventions. . . . The greater the disparity between literary contents and historical actualities, the greater the likelihood of conformity to established conventions of writing. Conversely, the less conformity to established conventions of writing, the greater the likelihood of agreement between literary contents and historical actualities. The unsuccessfully denied uniqueness of the gospels' literary form therefore derives from the uniqueness of Jesus' life and ministry" (112–13). Gundry only slightly modified his views of the genre of Gospel in his commentary on Mark. See *Mark: A Commentary on His Apology for the Cross* (Grand Rapids: Eerdmans, 1993), 1049–52. More recently, R. T. France has argued that Mark's Gospel belongs to the genre of biography. Yet he cautiously adds, "Just as there is a wide variety among Graeco-Roman 'biographies,' so Mark is his own master, not bound to follow a pattern laid down by someone else. His book represents something distinctive within the field of biographical writing, in terms of its subject, its origin, and the use for which it was intended" (R. T. France, *The Gospel of Mark: A Commentary on the Greek Text*, NIGTC [Grand Rapids: Eerdmans, 2002], 6).

goes on with an appendix. It's almost as if John is a Baptist preacher who wants to add one more chorus of "Just As I Am" before he can conclude the service. Mark is rather more like a Methodist preacher who cuts his sermon short—giving a sermonette that contains deep thoughts, to be sure, but leaves his congregation with as many questions as answers.

Now, back to presuppositions. All of us have come with a set of them. Some have such strong presuppositions in one area that, regardless of the evidence in another, they are unwilling, or unable, to allow the second realm to cast doubt on the first. I want to bring to the conscious level some of the presuppositions that may be driving your view of this lengthy and difficult textual problem. I hope that you will think through these related disciplines as you wrestle with the question about Mark's ending. There are at least three important presuppositions to address.

First, one's view on *source criticism* plays a large role in deciding this particular issue. Let me put this plainly: If you hold to the Griesbach Hypothesis, or Matthean priority, you may have some trouble believing that Mark's Gospel ended at 16:8. The reason is not that the textual evidence is compelling for the Long Ending (LE), but a prior commitment about the synoptic problem is. If Matthew wrote first and Mark wrote last, would Mark really reject the rich material in Matthew 28 or Luke 24, and write a truncated Gospel that has no resurrection *appearances* by Jesus? It's much easier to believe that *if* Mark is last, he combined snippets from the other Gospels and wrote 16:9–20 than that he decided to excise the post-Resurrection narratives that were in Matthew and Luke.²

Perhaps the most scholarly defense of the LE of Mark was written by William Farmer, a man who was already committed to

2. For example, George Salmon "did not accept Markan priority[;] therefore, he thought it inconceivable that Mark's Gospel could have ended with ἐφοβοῦντο γάρ" (Steven Lynn Cox, *A History and Critique of Scholarship concerning the Markan Endings* [Lewiston, NY: Mellen, 1993], 78). Not all Matthean prioritists, of course, think that Mark ended at 16:8 (cp., e.g., C. S. Mann, *Mark: A New Translation with Introduction and Commentary*, AB [Garden City, NY: Doubleday, 1986], 672–76; on p. vii he says that he follows the Griesbach Hypothesis), but they have greater difficulty explaining the lacuna in Mark than Markan prioritists do. As David Parker succinctly notes, the adherent of the Griesbach Hypothesis "has some difficulty with the idea that Mark should have ended at 16:8. For it requires him to have rejected all the material contained in Matthew 28 and Luke 24, and to have decided to go against the tradition of recording resurrection appearances" (D. C. Parker, *The Living Text of the Gospels* [Cambridge: Cambridge University Press, 1997], 132).

Matthean priority when he wrote *The Last Twelve Verses of Mark* in 1974.³ In that monograph, he mentions with approbation the text-critical work of Harry Sturz in a lengthy footnote. Sturz considered the major texttypes to have all originated in the second century, and all independently of one another. His view is known as “the independent texttypes theory.” His method was to determine the archetypal reading of each texttype by examining the best witnesses and determine the wording of the original based on a majority of texttypes—or, more exactly, a majority of the reconstructed *archetypes* of those texttypes. So, for Sturz, it wasn’t a majority of manuscripts that had priority, but a majority of archetypes. Practically, though not theoretically, his views looked very much like the majority text theory. That is why he was one of the editors of the first published *Majority Text*.⁴

Farmer does not say that he is following Sturz’s method in his book, but one gets the distinct impression that he was heavily influenced by it. And in the draft for the second edition of *The Last Twelve Verses of Mark*,⁵ in his conclusion of the external evidence Farmer explicitly speaks against the “text critical tradition of . . . Westcott and Hort.”⁶ In short, Farmer started with the Griesbach Hypothesis—in fact, he was the world’s leading Matthean prioritist of the twentieth century—but he recognized that the Short Ending of Mark was a roadblock. So he sought out a text-critical method that would allow him to continue to maintain Matthean priority. At first, this method was most likely not clearly formulated in his mind, but twenty-six years later, he had more consciously

3. William R. Farmer, *The Last Twelve Verses of Mark*, SNTS 25 (Cambridge: Cambridge University Press, 1974).

4. *The Greek New Testament according to the Majority Text*, 2nd ed., ed. Zane C. Hodges and Arthur L. Farstad; assistant editor, William C. Dunkin; consulting editors, Jakob van Bruggen, Alfred Martin, Wilbur N. Pickering, and Harry A. Sturz (Nashville: Thomas Nelson, 1985). Of all the editors, only Sturz and van Bruggen were not Dallas Seminary graduates and thus not directly influenced by Zane Hodges. Sturz was the only non-MT editor of the seven.

5. The second edition was never published. Dr. Farmer gave me the draft of the external evidence section in 2000, asking for my critique, shortly before his death that year (December 31, 2000). In his cover letter, he also noted that it was already at the publisher’s. Why it was not published I do not know.

6. P. 50 of the prepublication draft. The statement reads, “Our examination of the external evidence, and in particular the manuscript evidence, leads us to the conclusion that scholars working in the text critical tradition of Griesbach, Lachmann, Westcott and Hort, and Streeter, who advocate the theory that Mark 16:9–20 is a later addition to the text of Mark, have an unfinished task that lies before them.”

and explicitly rejected reasoned eclecticism. Does this mean he had fully embraced Sturz's views? Ironically, the answer is no, because if he had, it would weaken his argument for the LE of Mark.⁷ In short, Farmer's source-critical views seemed to drive his text-critical decisions.

A second presupposition that may influence your view of Mark's ending is *the whole field of textual criticism*. Some of you have already become convinced of a particular text-critical theory, even though you may be uncertain about source criticism. If your views of the text are settled, you may be here simply to get more arguments for your conclusions about this textual problem. The proposed solutions to Mark's ending will probably not alter your theory, but your theory may dictate—or at least heavily influence—your solution to this textual conundrum.

Third, one's presuppositions about *bibliology* could have a large impact on how he or she views this particular problem. For example, if you believe in the doctrine of preservation—that God has preserved the Scriptures so that there must always be manuscript testimony to the original text—then you will not be open to the view that the original ending of Mark was lost. Now, it is possible that you would be open to the Gospel *intentionally* ending at v. 8,

7. In particular, Farmer has altered some of the language from the first edition of *Last Twelve Verses* regarding the archetype of the Alexandrian witnesses. Whereas before he said that such an archetype reached back to the late second century, now he says that it goes no further than the late third century (p. 20 and *passim*). There is no new evidence that convinced Farmer to change his stance, nor is there any new text-critical theory that pushed him in that direction. Indeed, his dependence on Sturz's method seems to be greater in the second edition than the first. Yet Sturz believed that all three major text-types originated in the second century. Further, Sturz considered the major and earliest witnesses to have priority in determining the archetype's reading. Yet, when Farmer examines the various witnesses for the Alexandrian, Western, and even Caesarean texts, he explicitly disregards this basic approach and appeals to numbers: in *Last Twelve Verses*, 50–51, Farmer enlists B. H. Streeter's approach of determining the text on the basis of regional archetypes (a view similar, in this point at least, to Sturz's). Streeter, like Sturz, thought in terms of primary witnesses, secondary witnesses, tertiary witnesses, and so on, to each text-form. But as Farmer discusses the external data, he recognizes that the *primary* witnesses to most of these text-forms support the omission. Significantly, the *only* region in which there was unanimous support for either reading was that of Italy-Gaul. Here, as Farmer notes, "[t]he primary authority . . . witnesses for inclusion, as do all the secondary, tertiary, supplementary and patristic witnesses" (50). To put this another way, in the majority of Streeter's regions, the primary witnesses ended Mark at 16:8, while the majority of secondary and tertiary witnesses supported the LE. And the western region unanimously supported the LE (according to Farmer). Yet Farmer would not allow this evidence to suggest either that (a) the LE was a later reading, or that (b) it originated in the West. But the evidence he presented and even the method he ostensibly followed argued against his case.

but *if* that interpretation is not rock solid, you just might opt for the LE because it puts you on safer bibliological ground.

Note, for example, what Dr. Wilbur Pickering said, when he was the president of the Majority Text Society, concerning the possibility that the ending of Mark was lost:

Are we to say that God was unable to protect the text of Mark or that He just couldn't be bothered? I see no other alternative—either He didn't care or He was helpless. And either option is fatal to the claim that Mark's Gospel is "God-breathed." . . . If God was powerless to protect His Word then He wouldn't really be God and it wouldn't make all that much difference what He said. . . . If God permitted the original ending of Mark to be lost then in fact we do not have an inspired text.⁸

Nearly a century before Pickering made his statement, John Burgon, the famous antagonist to Westcott and Hort, wrote: "I am utterly *unable* to believe . . . that God's promise has so entirely *failed*. . . ." ⁹ He articulated two presuppositions in this one sentence that are relevant for the ending of Mark. First, his belief in divine preservation—and of a peculiar kind¹⁰—prevented him from even

8. Wilbur N. Pickering, "Mark 16:9–20 and the Doctrine of Inspiration," 1 (a paper circulated to members of the Majority Text Society, September, 1988). Pickering added that he was unaware of anyone who thought that Mark's Gospel intentionally ended at v. 8. Perhaps if he had realized that this was, in fact, the predominant view at the time he wrote, he might have been a bit less dogmatic in his declarations. Pickering's view is, in a sense, just the flip side of Dean John W. Burgon's about the doctrine of preservation. He said, "I am utterly disinclined to believe—so grossly improbable does it seem—that at the end of 1800 years 995 copies out of every thousand, suppose, will prove untrustworthy; and that the one, two, three, four or five, which remain, whose contents were till yesterday as good as unknown, will be found to have retained the secret of what the Holy Spirit originally inspired. I am utterly unable to believe, in short, that God's promise has so entirely failed, that the at the end of 1800 years much of the text of the Gospel had in point of fact to be picked by a German critic out of a waste-paper basket in the convent of St. Catherine; and that the entire text had to be remodeled after the pattern set by a couple of copies which had remained in neglect during fifteen centuries, and had probably owed their survival to that neglect; whilst hundreds of others had been thumbed to pieces, and had bequeathed their witness to copies made from them" (*The Traditional Text of the Holy Gospels*, ed. E. Miller [London: George Bell & Sons, 1896], 12). I say this is the 'flip side' of Pickering's point because Burgon was not arguing for the preservation of the Long Ending in this diatribe as much as arguing that the critically-reconstructed text of Westcott and Hort was based on manuscripts that had been unknown and unused. Nevertheless, both were speaking about preservation as a doctrinal presupposition.

9. Burgon, *The Traditional Text*, 12. Italics added. See full context of quotation in note 8.

10. Burgon linked inspiration to preservation and preservation to accessibility. Thus, for him, as for many if not most majority text advocates, it necessarily follows from in-

entertaining the possibility that the LE was not authentic. Second, he assumed that there actually are explicit promises in the Bible about its preservation—in spite of the likelihood that none of the texts that are so used are speaking about the preservation of the *written* word.¹¹

If, however, the doctrine of preservation is not part of your credo, you would be more open to all the textual options. I, for one, do not think that the real ending to Mark was lost, but I have no theological agenda in *this* matter because I don't hold to the doctrine of preservation. That doctrine, first formulated in the Westminster Confession (1646), has a poor biblical base. I do not think that the doctrine is defensible—either exegetically or empirically.¹² As Bruce Metzger was fond of saying, it's neither wise nor safe to hold to doctrines that are not taught in Scripture. I may be wrong in my view of preservation, but this presupposition at least keeps an open door for me for all the options in Mark 16.

My point in this preliminary treatment is to underscore the fact that we all bring a lot of presuppositions to the table that influence how we hear the evidence being presented; indeed, such presuppositions may even keep us from hearing the evidence.

Let me illustrate this point with a personal anecdote. I've mentioned Harry Sturz already. He was my first Greek professor at Biola University. At the time, he was teaching the only year-long college-level course on NT textual criticism offered in the country. As a young impressionable student, I came to embrace his views heartily. And I took several courses from him. When I came to Dallas Seminary, I studied under Harold Hoehner. He taught the Griesbach Hypothesis—that Matthew had written first. That view fit into my preunderstanding of textual criticism well. I also studied with

spiration that the original text *must* be found in the majority of manuscripts at all times. I have dealt with the internal contradictions of this view elsewhere. See Daniel B. Wallace, "The Majority Text Theory: History, Methods, and Critique," in *The Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis*, ed. Bart D. Ehrman and Michael W. Holmes (Grand Rapids: Eerdmans, 1995), 308–10; in particular, it is noted that "the Majority text position is based on a corollary (accessibility) of a corollary (preservation) of a particular dogmatic stance (verbal inspiration)," 308.

11. More recently, and in a significant scholarly journal, Matthew D. McGill asked the question, "Could a part of God's Word, inspired by the Holy Spirit, be lost?" (in "A Textual and Structural Analysis of Mark 16:9–20," *Filologia Neotestamentaria* 17 [2004]: 27–43; quotation from 35). The belief in some sort of doctrine of preservation continues to persist, even when it has been demonstrated to be indefensible exegetically and empirically.

12. See Daniel B. Wallace, "Inspiration, Preservation, and New Testament Textual Criticism," *GTTJ* 12 (1992): 21–51.

Zane Hodges and learned the majority text position from him. In practice, I was a majority text man, though in theory I held to the independent texttypes view.

I joined the Dallas Seminary faculty and taught both Matthean priority and the independent texttypes theory. I went on to teach at another seminary, and I continued to teach both views. I studied the literature, and saw it only through a particular lens. I read some essays that were devastating to my theories, but I was able to brush them aside. And I *thought* that my arguments against them were decent.

A few years later, I returned to Dallas for my doctorate.¹³ While in the program, I wanted to take the course on textual criticism. Unfortunately, there was no one on faculty who could teach it since Zane Hodges had retired. So, I was asked to lead a doctoral seminar in textual criticism. Thus, when I asked if a class was going to be offered, I was told that it *would* be as long as *I* offered it! In retrospect, it's a good thing I didn't ask about a course in Egyptian hieroglyphics!

I had my work cut out for me, because I knew I could no longer give glib answers. I started by reading over 10,000 pages of text-critical material—to quickly get up to speed. I had to think through the entire paradigm. The more I studied, the more I came to the conclusion that Sturz's views were faulty—that the Byzantine text was not equal to the Alexandrian and Western. In short, I became a reasoned eclectic.¹⁴ It was an enormously painful methodological shift for me, since I had held to Sturz's view for seventeen years. Nevertheless, I did not completely reject the Byzantine text, but felt that it still had a place at the table.

Two months after my text-critical foundation had crumbled, I abandoned Matthean priority. In my mind, the two were not con-

13. In 1982, when I was considering doctoral programs, I visited Bruce Metzger at Princeton Seminary. He told me that he was not allowed to take any more doctoral students because Princeton had a mandatory retirement age. Because studying with him was no longer an option, I looked elsewhere for my doctorate. If I couldn't study with Metzger, I reasoned, I should most likely concentrate on Greek grammar. I knew of Buist Fanning's reputation, having studied with him in my master's program at Dallas Seminary; that decided the direction of my studies. Ironically, here I am, debating a point about textual criticism when I am the least qualified person in this volume!

14. Credit needs to go to Darrell Bock as one who was instrumental in my shift. He simply made a quick comment, in passing, about how any text-critical theory has to be grounded in history if it is to be viable. It was indeed the historical question that Sturz's view, along with the majority text theory, did not seem capable of answering.

nected so much by their interdisciplinary nature as by their *similar method of investigation*.¹⁵ Both my text-critical and source-critical views had put a premium on the external data—and frankly, on a very mechanical approach to them. But, as Günther Zuntz noted, “At every stage the critic has to use his brains. Were it different, we could put the critical slide-rule into the hands of any fool and leave it to him to settle the problems of the New Testament text.”¹⁶

I began to read the *same* journal articles, monographs, and *Festschriften* in a different light. What had appeared to me to be dangerous and subjective viewpoints because they depended so much on the scholar’s ability to get into the head of the scribe or evangelist, now looked like compelling arguments. What changed were not the arguments, but my presuppositions. I came to the deep conviction that evangelical scholars *must* be in the business of pursuing truth, regardless of where it takes us, rather than protecting our presuppositions. That has been the most liberating conclusion I’ve drawn in my academic career.

We each have various convictions about interlocking disciplines that affect our take on the last twelve verses of Mark. This means that this symposium will not really settle the issue for most of you. But it also means that it’s good to take a step back and reflect on source criticism, your overall text-critical views, and how your bibliography impacts your view of the text. And I want to challenge you to wrestle with these interlocking presuppositions, and to be open to approaches that may be outside your comfort zone.

I don’t doubt the integrity or scholarship of any of my colleagues, and I hope they don’t doubt mine. But even though we are all looking at the same evidence, we are not all coming to the same conclusion. In part, we certainly do read the evidence differently. In part, we also each bring certain presuppositions to the discussion that impact our view of this textual problem.

With that introduction, I now turn to an investigation of the ending of Mark’s Gospel. The overall objective will be to determine

15. See Gordon D. Fee, “A Text-Critical Look at the Synoptic Problem,” *NovT* 22 (1980): 12–28.

16. G. Zuntz, *The Text of the Epistles: A Disquisition upon the Corpus Paulinum* (London: Oxford University Press, 1953), 12.

It should be noted that I did not simply replace one set of presuppositions with another, but had accumulated several unresolved issues over the years that I did not have satisfactory answers for when I held to Matthean priority and the independent texttypes theory.

the reading that best explains the rise of the others. I will limit my examination to two aspects to the problem. First, I want to consider the external evidence—Greek manuscripts, ancient translations, and patristic writings. I will not only look at the hard data, but try to construct some reasons for why it looks the way it does. There are curiosities concerning Mark’s ending that simply cannot be ignored. Second, I will look at internal evidence—though much more briefly. In particular, the focus will be on what the author was likely to have written.¹⁷ Internal evidence is not nearly as subjective as it may at first appear; likewise, external evidence is not nearly as objective as some might think.

The End of Mark’s Gospel—External Evidence

As for the external evidence, the raw data can be quite deceiving: at least 95 percent of all Greek MSS and ancient versions have the LE. In fact, that number may be too low. I used to be impressed by the sheer volume of MSS on one side of a textual problem, but our investigation must take us deeper than that. In particular, the major question we need to answer is this: Which is more likely—that scribes would intentionally omit vv. 9–20 or that they would add these verses?

The Long Ending

The LE of Mark is not found in the oldest MSS, but it is found in the majority of MSS. And it is found in all the major texttypes—Western, Caesarean, Byzantine, and even the secondary Alexandrian.¹⁸ Thus, there is a broad geographical spread for these verses.

As well, it is witnessed to by several church fathers, most likely beginning in the late second century with Irenaeus.¹⁹ But there is a

17. Technically, internal evidence includes an examination of both the intrinsic probability (what the author is likely to have written) and transcriptional probability (what the scribes were likely to have done with the text they copied from). But in larger portions of text, intrinsic evidence becomes much more important and decisive; consequently, we will examine only that aspect under internal evidence. Further, since the transcriptional evidence is strongly related to patristic data for this problem, we will subsume the transcriptional under external.

18. Today, the Caesarean text-form is strongly disputed, but for convenience’ sake we will consider it to be a legitimate texttype.

19. It is possible also that Justin Martyr (d. AD 165) alludes to Mark 16:20 in *Apol* 1.45 (compare his ἐξεληθόντες πανταχοῦ ἐκήρυξαν with Mark 16:20—ἐξεληθόντες ἐκήρυξαν πανταχοῦ). But he does not mention Mark in this reference. Nevertheless, since he is

curious feature in this patristic evidence. Farmer makes a case that the LE was omitted by Alexandrian scribes because of two reasons. First, there seems to be a discrepancy between v. 9 and the other Gospels' accounts regarding the time of Jesus' resurrection.²⁰ Second, vv. 17–18, with their promise of drinking poison and handling snakes without harm, caused embarrassment to early Christians. These two texts would have been sufficient reason for certain scribes to omit the *entirety* of vv. 9–20, according to Farmer.²¹

There are severe problems with both of these arguments, however. Regarding the first issue, if any Gospel was the “odd man out” it would be Matthew, not Mark: Mark 16:9 is actually in agreement with Luke 24:1 and John 20:1 on the time of the Resurrection, while Matthew's wording seems to be at some variance.²² Mark, Luke, and

speaking about the disciples spreading the gospel, and since the wording is virtually identical with what is in Mark 16:20, it is possible that Justin knew of the LE. Some also take a statement in Papias to be a possible reference to the LE (so Salmon, etc.). Papias relates a story in which Justus Barsabbas once drank poison without harm (Papias 3.9; 5.1). But to connect this to Mark 16:18 as the source for Papias's statement is gratuitous, especially since that sort of miraculous story would naturally have circulated in nascent Christianity, as can be seen by many other similar stories in the patristic and apocryphal literature.

20. Farmer, *Last Twelve Verses*, 21–22. The focus of the discrepancy was the time of Jesus' Resurrection and his appearance to Mary. This discrepancy is discussed by Eusebius in *Quaestiones ad Marinum*, in *Nova Patrum Bibliotheca* (NPB), vol. 4.219–309, esp. 255–57; ed. Cardinal Angelo Mai (1847); more accessible is the quotation by Farmer, *Last Twelve Verses*, 4, which gives the translation by John W. Burgon, *The Last Twelve Verses of the Gospel according to St Mark* (Oxford: James Parker, 1871), 265–66 (Mai's Greek text is reproduced in Burgon, 41–44).

21. Farmer, *Last Twelve Verses*, 65–72. He discusses various ways in which the ancient church dealt with the troublesome verses. First is containment: Farmer quotes from the *Apostolic Constitutions* (ANF 7: 479–81) as an illustration of ecclesiastical regulation of the sign gifts so as to maintain order in the service (but nothing is said in this passage about picking up snakes or drinking poison). Second is allegory: Farmer quotes the apologist in *The Apocriticus of Macarius Magnes*, who responds to Porphyry's taunt (or, more accurately, “the pagan philosopher who popularized Porphyry's work” [Farmer, 68]) that candidates to the priesthood should drink deadly poison because to refuse to do so would be “to confess that they do not believe in the things Jesus said.” To this the apologist says that the “deadly drug” must not be taken too literally (cp. *The Apocriticus of Macarius Magnes*, trans. T. W. Crafer; Translations of Christian Literature, Series 1: Greek Texts [London, 1919], 85–88). The third option was simply to omit copying the verses. Farmer goes to some length to suggest that the Alexandrian scribes would in fact have done this because of their offense over these verses, but he has no statement to this effect, only inference (Farmer, *Last Twelve Verses*, 70–72).

22. The primary issue that Eusebius wrestled with was the apparent discrepancy between Matthew's statement on the time of the Resurrection (ὅψε δὲ σαββάτων) and Mark 16:9 (Ἀναστὰς δὲ πρῶτῃ πρώτῃ σαββάτου). Although *ad Marinum* discusses two potential discrepancies, this one is obviously the more prominent (and is the only one that Farmer mentions in his summary on p. 22). But if Mark 16:9 contradicts Matt 28:1, then so do Luke 24:1 and John 20:1, for both of these Gospels also speak of the Resurrection as

John all say that Jesus was raised from the dead “early on the first day of the week,” while Matthew says it happened “late on the Sabbath” or “after the Sabbath.” In other words, Mark 16:9 is no more problematic than the Resurrection narratives in the other Gospels on this score. If scribes were prone to omit a verse because of this difficulty, it would have been Matt 28:1, not Mark 16:9.

So, v. 9 is not really the problem that Farmer thinks it is. This leaves vv. 17–18 as the sole reason for excision of all twelve verses by some scribes.

Second, then, regarding the drinking of poison and the handling of snakes, Farmer thinks that these verses would have been so problematic for many Christians that some even took the radical course of omitting all twelve verses from the Gospel. As the dust jacket of *The Last Twelve Verses* affirms, “Professor Farmer traces the history of the text tradition for omission back to Egypt, and argues that one important factor contributing to their omission was the dangerous teaching they seemed to contain: they appear to encourage Christians to handle deadly snakes and drink poisons to prove their faith. . . .”

But if the locus of embarrassment was the second half of vv. 9–20, we might expect to see scribes deleting just these verses and retaining vv. 9–14. There is, however, no evidence at all that this happened. In fact, one could argue that just the opposite was the case: the early patristic writers allude to the *second* half of this pericope (vv. 15–20) far more often than they do the first half (vv. 9–14).²³ Farmer gives the evidence himself: at least ten fathers quote from

occurring ‘early on the first day of the week’ (Τῇ δὲ μιᾷ τῶν σαββάτων ὄρθρου βαθείας in Luke 24.1; Μαρία ἡ Μαγδαληνὴ ἔργεται πρωὶ σκοτίας ἔτι οὔσης in John 20.1). Indeed, this particular question is followed by another by Marinus, which specifically pits Matthew against John regarding the Resurrection and Jesus’ appearance to Mary (see Farmer, *Last Twelve Verses*, 5).

23. Farmer, *Last Twelve Verses*, lists Justin Martyr as possibly alluding to v. 20; Irenaeus as quoting from v. 19, claiming it to be from Mark (*Against Heresies* 3.10.6); a third-century text attributed to Hippolytus (preserved in *Apostolic Constitutions* 8.1) as interpreting vv. 17–18; Vincentius (AD 256) as referring to vv. 15–18; *Acta Pilati* as quoting vv. 15–18; Aphraates, the ‘earliest known Father of the Syrian church,’ as citing vv. 16–17 in a sermon (AD 337); Epiphanius (c. 374–6) as referring to v. 19; the *Apostolic Constitutions* (c. 380) as quoting from vv. 15–18 (cp. AC 6.15 especially); Nestorius and Cyril as quoting from Mark 16:20; and Gregory of Nyssa as citing v. 19 (*Last Twelve Verses*, 31–34). On the other hand, he lists Eusebius as recognizing that some MSS had vv. 9–20, Ambrose (d. 397) as quoting from vv. 9–20 (although in the unpublished revision, Farmer only cites quotations from vv. 15–20 by Ambrose [p. 22, n. 57]), Chrysostom (d. 407) as referring to v. 9, Jerome (d. 420) as including vv. 9–20 in his Vulgate, and Augustine (d. 430) as discussing vv. 9–20 (70–72).

or allude to vv. 15–20 in a steady stream from the second to the fifth century, while no fathers mention the first half of the pericope until the *fourth* century!²⁴ The trend, in fact, is so one-sided that Eta Linnemann saw this as evidence that Mark 16:15–20 was part of the original text of Mark, but vv. 9–14 were not,²⁵ an argument similar to what Walter Schmithals would later make.²⁶

But the point of all this is simply to note that, contrary to Farmer's supposition that the early scribes excised the latter half of these twelve verses because of offensive language, the evidence shows that precisely the opposite happened: vv. 15–20 are quoted much earlier and more frequently than vv. 9–14. Yet, if as we have seen, v. 9 would cause no unusual turmoil,²⁷ what shall we make of Farmer's argument? It is evident that he has not really discovered a reason why scribes would omit these twelve verses.

And this contention brings us to the major question that we are wrestling with: *Why* would the scribes do what they did to this text? The motive for excising the passage is too negligible to explain the short ending. And it almost certainly cannot explain why important, early, and diverse witnesses lack these verses.

If there is no adequate explanation for why some scribes would omit the LE, then the only alternative is that other scribes added it. Can a reason be found for this? Would scribes really be motivated to *add* these twelve verses? The reason seems so palpably obvious that it is almost needless to mention: If Mark's Gospel ends at 16:8, there are *no Resurrection appearances* by Jesus to his disciples. The

It might also be pointed that if Justin Martyr is alluding to Mark 16:20 and Papias is alluding to Mark 16:18, then there are thirteen patristic writers through the first five centuries who allude to/quote *only* the second half of the pericope.

For a more complete discussion of the patristic data, see Joseph Hug, *La Finale de L'Évangile de Marc (Mc 16, 9–20)* (Paris: J. Gabalda, 1978), 188, 192–207. Less critically, but with a helpful layout, see Cox, *History and Critique*, Appendix A: "The Fathers' Use of the Longer Ending," 217–22.

24. Unless, of course, one counts the *Diatessaron* as a "father."

25. Eta Linnemann, "Der (wiedergefundene) Markusschluß," *ZTK* 66 (1969): 255–87. Her view was based on more than the patristic evidence, but also looked at internal features of the passage in question. Her thesis was strongly criticized by Kurt Aland, "Der wiedergefundene Markusschluß? Eine methodologische Bemerkung zur textkritischen Arbeit," *ZTK* 67 (1970): 3–13.

26. Walter Schmithals, "Der Markusschluß, die Verklärungsgeschichte und die Aus-sendung der Zwölf," *ZTK* 69 (April 1972): 379–411. Schmithals's argument is that these verses would have been a source that Mark used, and thus they would have been dated earlier than the Gospel itself.

27. After all, the Resurrection accounts in all four Gospels have several tensions with each other, regardless of whether Mark's Gospel included vv. 9–20 or not.

Gospel marches on toward the Resurrection, beginning with Jesus' response to Peter's confession that after his crucifixion he would be raised from the dead (8:31). After his transfiguration, he again tells his disciples that he would die and rise from the dead (9:31). But again, his disciples "didn't understand and were *afraid* to ask what he meant." Finally, in 10:34, he again tells them that after his crucifixion he would be resurrected. In each of these scenes, the disciples are slow to get the message. We will come back to this issue later, but for now I only wish to point out that the Resurrection of Christ is *prophesied* by the Lord to his disciples three times in Mark. Yet, Mark's Gospel remarkably ends without *any* post-Resurrection appearances that fulfill the prophecy. Is this not reason enough for some early scribes to want to add *something* to this Gospel—anything!—that would have him appear before the disciples? Indeed, it is not just something but several "somethings" that were added after v. 8.

The Short Ending

Now, let's consider the evidence for the "short" ending—that is, the MSS that conclude the Gospel at 16:8. This discussion won't take much space because there aren't very many of these documents. But MSS should be weighed rather than counted, so we will want to look at their pedigree as well.

On the side of the short ending we have the following:

- Codex Sinaiticus (Ⲙ)—This fourth-century codex is one of the principal witnesses to the Alexandrian texttype.
- Codex Vaticanus (B)—Vaticanus, also a fourth-century codex, is the most important witness to the Alexandrian text.

These two codices are the oldest Greek MSS for Mark 16. They also are the *only* "primary" Alexandrian witnesses to Mark 16 in Greek.²⁸ This is a relatively pure form of the text that must be given full consideration when making any textual decisions. Metzger-Ehrman note that "textual witnesses connected to Alexandria attest a high quality of textual transmission from the earliest times. It was there that a very ancient line of text was copied and preserved.... the

28. See Bruce M. Metzger, *A Textual Commentary on the Greek New Testament*, 2nd ed. (Stuttgart: Deutsche Bibelgesellschaft, 1994), 15*.

Christian scholars of Alexandria worked assiduously to preserve an accurate form of text.”²⁹

Yet Codex Sinaiticus and Codex Vaticanus contain several thousand differences in the Gospels alone, suggesting that their common ancestor must be several generations back. In addition, we know that the Alexandrian text-form existed in the second century since nearly a dozen Alexandrian papyri are from that period,³⁰ and because early versions and patristic writers utilized a Greek *Vorlage* that must have its roots deep in the second century.³¹ This MS, versional, and patristic evidence all points to the early second century as the beginning of the Alexandrian text, and it’s a threefold cord that is not easily broken.

Further, although no papyri witness to Mark 16, one might *cautiously* enlist the support of \mathfrak{P}^{75} here. The text of B is closer to that of \mathfrak{P}^{75} than it is to any other MS. And \mathfrak{P}^{75} is a MS that antedates Vaticanus by at least a century. However, this papyrus only contains portions of Luke and John. Nevertheless, David Parker argued that since “we have no reason to consider the text of B in Matthew and Mark to be inferior to that in Luke and John, . . .” Vaticanus surely

29. Bruce M. Metzger and Bart D. Ehrman, *The Text of the New Testament: Its Transmission, Corruption, and Restoration*, 4th ed. (Oxford: Oxford University Press, 2005), 278.

30. The NT papyri from the second century include \mathfrak{P}^{52} (100–150), \mathfrak{P}^{90} ,¹⁰⁴ (second century), \mathfrak{P}^{66} (c. AD 175–225), $\mathfrak{P}^{46, 64+67}$ (c. AD 200), \mathfrak{P}^{77} , \mathfrak{P}^{98} (second century?). As for texttype, as far as can be determined, all of these MSS are Alexandrian (Philip W. Comfort and David P. Barrett, *The Text of the Earliest New Testament Greek Manuscripts* [Wheaton, IL: Tyndale, 2001], 629, only dispute \mathfrak{P}^{98} , noting that there are ‘several differences’ from the text of NA²⁷, but by my count there are three differences, only one of which seems to be due to a different *Vorlage* than the text found in NA²⁷ here).

These eight manuscripts are the extent of those that the Institut für neutestamentliche Textforschung has identified as possibly or definitely from the second century. In addition to these, there are a few other candidates. Comfort-Barrett, *Earliest New Testament*, argue for at least half a dozen other manuscripts as possibly from the second century. Comfort and Barrett’s method, however, is generally to take the earliest date possible. Nevertheless, the date they suggest for \mathfrak{P}^4 (second century) is probably correct in light of some recent work done by T. C. Skeat of the British Library, and the date they offer for \mathfrak{P}^{32} (late second century) is quite possible. In addition, renowned papyrologist Herbert Hunger considered \mathfrak{P}^{66} to be from no later than the middle of the second century (“Zur Datierung des Papyrus Bodmer II (\mathfrak{P}^{66}),” *Anzeiger der österreichischen Akademie der Wissenschaften* 4 [1960], 12–33). The original editors of \mathfrak{P}^{75} also thought this manuscript should be dated late second to early third century (see Metzger-Ehrman, *Text*, 58). All of these, except perhaps for \mathfrak{P}^{32} , belong to the Alexandrian text.

31. The Sahidic version, an early third century version (see discussion below), is a primary Alexandrian representative; Clement of Alexandria (d. after 215) and Origen (d. 254) are primary witnesses to the Alexandrian text.

represents a text that already existed in the late second century.³² It can be added that the text of B, when compared to the text of \mathfrak{P}^{75} , actually gives evidence of having more primitive readings than \mathfrak{P}^{75} does.³³ Therefore, B is not a descendant of \mathfrak{P}^{75} but reaches back to their common ancestor. Thus, it is probable that B, even by itself, represents a text that existed early in the second century. In combination with \aleph , this likelihood is made stronger.³⁴ In any event, even Sturz would argue that the reading of \aleph and B is a second-century reading.

One other comment regarding Vaticanus is in order: There is a large gap at the end of Mark in this MS. Vaticanus has three columns per page;³⁵ Mark's Gospel ends at the bottom of the second column. The third column is left blank and Luke starts on the next page. The gap is clearly too small to allow for the LE, though Farmer tries to argue from this *lack* of evidence that the scribe knew about the LE "but disapproved" of it.³⁶ A more sober assessment is provided by William Lane. He makes the case that the blank column after the conclusion of Mark is "a wholly singular phenomenon, for in Codex

32. Parker, *Living Text*, 137. Contra Farmer, *Last Twelve Verses*, 30.

33. C. L. Porter, "An Evaluation of the Textual Variation between Pap75 and Codex Vaticanus in the Text of John," in *Studies in the History and Text of the New Testament in Honor of Kenneth Willis Clark*, vol. 29 of Studies and Documents, ed. Boyd L. Daniels and M. Jack Suggs (Salt Lake City: University of Utah Press, 1967), 71–80.

34. For a landmark study on the significance of the relation of \mathfrak{P}^{75} to B, see Gordon D. Fee, " \mathfrak{P}^{66} , \mathfrak{P}^{75} and Origen: The Myth of Early Textual Recension in Alexandria," in *New Dimensions in New Testament Study*, ed. R. N. Longenecker and M. C. Tenney (Grand Rapids: Zondervan, 1974), 19–45; reprinted in Eldon Jay Epp and Gordon D. Fee, *Studies in the Theory and Method of New Testament Textual Criticism*, Studies and Documents, vol. 45, ed. Irving Alan Sparks (Grand Rapids: Eerdmans, 1993), 247–73.

Farmer acknowledged that the evidence was sufficient that some MSS of Mark in the second half of the second century must have ended at 16:8 (*Last Twelve Verses*, 31, 74, and *passim*). Inexplicably, in the unpublished revision of this work, Farmer now alters the date to a significantly later period: "This suggests an origin for the text tradition for omission sometime early in the fourth century, possibly as early as some time during the second half of the third century. . . ." (20 of draft). But a line-by-line comparison with the published *Last Twelve Verses* revealed essentially nothing new; indeed, Farmer continued to show minimal acquaintance with the text-critical literature of the last half-century in the revision, a criticism that Birdsall had already leveled against the first work (J. N. Birdsall, "Review of William R. Farmer, *The Last Twelve Verses of Mark*," *JTS* 26 [1975]: 151–60).

35. Except in OT poetic sections where it has two columns per page.

36. Farmer, *Last Twelve Verses*, 59. He does admit, however, "Why the space left blank is more than necessary for the shorter ending or less than needed for the longer ending is not entirely clear" (58). At most, the reason why there is more space for the shorter ending is simply because almost every book in Vaticanus ends with *some* space at the end of the column. Further, *every* book begins at the top of a column.

B a new book follows in the next column as soon as possible.” He argues from this that the scribe of B knew of what is sometimes called the “Shorter Ending” or the “Intermediate Ending” (which we will discuss below), and made room for it.³⁷

However, even Lane’s understanding of the gap in B is in doubt. First, this is *not* a “wholly singular phenomenon.” Although it is certainly Vaticanus’ normal custom to begin a new book at the top of the column following the conclusion of the previous book, this MS breaks that rule on *four* occasions. Tobit ends with one and a half columns to spare³⁸; 2 Esdras (Nehemiah) has only two lines in the first column, followed by the entire rest of the page blank; and Daniel concludes half way down the first column, with the rest of the page blank.³⁹ All three of these books leave *larger* blanks than Mark does.

37. William L. Lane, *The Gospel of Mark*, NICNT (Grand Rapids: Eerdmans, 1974): 602n1.

38. Although the text of Tobit in Vaticanus is significantly shorter than Tobit in Sinaiticus, the additional material in the latter is spread throughout the book rather than at the end. There are approximately 1600 more words in the Sinaiticus version than in the Vaticanus version. The main verses that are longer in \aleph are Tobit 1.8, 18; 2.2, 3, 10; 3.6, 10, 17; 5.3, 4, 5, 6, 10, 17; 6.4, 13, 14; 7.1, 10, 12, 13; 8.19, 21; 9.5, 6; 10.6, 8, 11, 14; 11.16, 17; 13.13, 17; 14.2, 4, 7, 8, 15. In light of the fact that these are distributed throughout the book and comprise hundreds of more words, it is unlikely that the scribe of Vaticanus is leaving space for such variants (for only a small fraction of the variants could possibly fit in the space provided). The situation is paralleled to some degree by the D text of Acts, which is 8.5% longer than the Alexandrian text; here again, the additions are distributed throughout the book. Yet there, no extra column is found in Vaticanus. Some other explanation for the long gap at the end of Tobit thus must be sought for. This is also seen in 2 Esdras (Nehemiah), for no unusually lengthy textual variant—at the end or anywhere in the book—occurs.

It could be countered that after 2 Esdras, the Psalms begin and thus a new section of the OT created the gap; and since they are laid out in two columns they would have to begin on a new page. So, also, Daniel: the NT begins on the next leaf. The same could be said for the gap after John in Sinaiticus (see below). But this argument does not work for Tobit, nor does it work for the large gap at the end of Philemon in Sinaiticus (unless the distinction between Paul’s letters and Acts is in view). Further, if the “Western” order of the Gospels were the original order once the books were collected, as some have suggested, then Mark would have ended the Gospels section in the earliest canonical collection. The gap in Vaticanus could be a vestige from an earlier codex in which the Western order was followed. (The same argument could be used for Sinaiticus, since the cancel sheets on which Mark 16 and environs were written were in a different hand—expanded in Mark, cramped in Luke; the original hand of \aleph would have left some room at the end of Mark, but clearly not enough for the LE.) All in all, the reasons for the gaps are anything but clear, which makes arguments based on the supposition that the scribes of \aleph and B knew of the LE or even the Intermediate Ending tentative at best.

39. Although there were two distinct recensions for Daniel, a blank page would not even be close to adequate to allow for the alternative recension. Further, it cannot be argued that room was made for “Bel and the Dragon,” since this story is found in Vaticanus.

Second, it was recently discovered that the scribes of Vaticanus have indicated knowledge of textual variants by using two horizontal dots in the margin next to a line of text where a variant occurs.⁴⁰ There are more than 700 such “umlauts” in the NT of Vaticanus, forty-three of which are in Mark alone. Thus, Codex B marks out *half* as many variants as the UBS text does! It’s almost as if this is an ancient UBS Greek text. This is a remarkable discovery whose implications have yet to be fully explored. But, significantly, there is no umlaut at 16:8.⁴¹ Thus, the *non*-unique gap at the end of Mark and the lack of an umlaut here both seem to indicate that the scribe knew only that Mark’s Gospel ended at 16:8.⁴² To put this another way: of the three other gaps in Vaticanus, not one is used to indicate knowledge of textual variation.⁴³ So, to argue that this *must* be the case for the gap at the end of Mark is hardly compelling. And the fact that Vaticanus uses umlauts to indicate textual variants and that no umlaut is present at Mark 16:8 adds significant weight to the likelihood that the gap at the end of Mark does not imply knowledge of any other ending.

40. J. Edward Miller, “Some Observations on the Text-Critical Function of the Umlauts in Vaticanus, with Special Attention to 1 Corinthians 14.34–35,” *JSNT* 26 (2003): 217–36. Miller builds on Philip Payne’s work of seeing the “umlaut” in combination with a horizontal bar to indicate textual variation (see especially P. B. Payne, “Fuldensis, Sigla for Variants in Vaticanus, and 1 Cor 14.34–5,” *NTS* 41 [1995]: 240–62). But Payne did not see the umlauts *by themselves* as clearly indicating this.

41. The reasons for the variants being indicated is still not entirely clear. It is possible that the scribe knew of other variants, but chose only to list these. It is also possible that the scribe was indicating only those variants that were true competitors. These umlauts need to be explored more fully for what they are and are not telling us.

42. James Snapp (“The Authenticity of Mark 16:9–20” [unpublished paper, 2006]) notes that \aleph “has not merely a blank column but an entire blank page following the four Gospels. This may be merely an aesthetic touch to separate the Gospels. On the other hand, if a scribe had desired to leave room for Mark 16:9–20 as a freestanding text rather than as part of the Gospel of Mark, he could easily have selected just this location for it” (24). This hypothesis seems to be an unfounded speculation, for this would be the only place in a Greek MS in which the LE was placed (or made room for) at the end of the Gospels (although there is one Armenian MS that does this). Further, there is more than a column and a half at the end of Acts in Sinaiticus: Does this mean that the scribe was aware of a different ending for Acts? And after Philemon, there is an entire blank page. It seems that Snapp’s first hunch—that the large gaps in \aleph were aesthetic markers to distinguish different genre—fits the data better.

43. Although two of the three gaps have been explained as due to genre shifts, this is certainly not evidence that the gap *must* mean knowledge of a variant in Mark 16. The gap at the end of Tobit lacks sufficient explanation, and the possibility exists that Mark could have been the final Gospel for Vaticanus’s exemplar, with the gap there being residue from an earlier format.

Codex 304, an otherwise unremarkable twelfth-century Byzantine MS, also ends with ἐφοβῶντο γάρ in v. 8.⁴⁴

Besides the Greek MSS, there are a few ancient versions that lack vv. 9–20.

The Sinaitic Syriac MS (Syriac^s) displays the oldest form of the Gospels in the Syriac language. This MS dates from the fourth century, but it represents a text from the late second or early third century, for the most part following the *Western* texttype.⁴⁵ The Sinaitic Syriac MS concludes the Gospel at v. 8.

Approximately 100 of the Armenian MSS, including almost all of the earliest ones, lack the LE.⁴⁶ It has been conclusively demonstrated that the original Armenian text did not have these verses. Even William Farmer admitted as much.⁴⁷ The Armenian has roots

44. Two other Greek MSS lack vv. 9–20, but their testimony must be completely discounted because the leaf that would have contained these twelve verses is missing. Codex 2386 (an eleventh-century Byzantine codex) lacks the leaf that vv. 9–20 would have been on, and thus should not be counted on behalf of the Short Ending. See Kurt Aland, "Bemerkungen zum Schluß des Markusevangeliums," in *Neotestamentica et Semitica: Studies in Honour of Matthew Black*, ed. E. Earle Ellis and Max Wilcox (Edinburgh: T & T Clark, 1969), 157–80, especially 159–60; idem, "Der wiedergefundene Markusschluß?" *ZTK* 67 (1970): 3–13, esp. 8–9. Codex 1420 (a thirteenth-century MS) is also listed by J. K. Elliott as having a page missing at this point ("The Text and Language of the Endings to Mark's Gospel," *TZ* 27 [1971]: 256), a point repeated without further qualification in the reprint of this article in J. K. Elliott, *The Language and Style of the Gospel of Mark: An Edition of C. H. Turner's "Notes on Marcan Usage" Together with Other Comparable Studies* (Leiden: E. J. Brill, 1993), 204. *Das Markusevangelium* 1.2 in *Die synoptischen Evangelien*, Text und Textwert der griechischen Handschriften des Neuen Testaments in ANTF 27, ed. Kurt Aland and Barbara Aland (Berlin: Walter de Gruyter, 1998; from here on, called *Markusevangelium*, ANTF 27) only lists 1420 as lacking v. 14; it makes no comment about vv. 9–20 nor whether the leaf was missing (410). Remarkably, the 768-page tome, *Textkritik des Markusevangeliums* by Heinrich Greeven and Eberhard Güting (Münster: Lit, 2005) does not discuss the problem of Mark 16.9–20.

45. Metzger-Ehrman note that, together with the Curetonian Syriac MS, "the form of text that they preserve dates from the close of the second or beginning of the third century. When the two manuscripts are compared, it is seen that the Sinaitic Syriac represents a slightly earlier form of text than does the Curetonian. . . . In general, the Old Syriac version is a representative of the Western type of text" (96–97).

46. Elliott gives the number at eight ("Text and Language," 256), which is apparently based on the apparatus of S. C. E. Legg, *Euangelium secundum Marcum* (Oxford: Clarendon, 1935), which, in turn, was presumably based on Frédéric Macler, *Le texte arménien de l'évangile d'après Matthieu et Marc* (Paris: Imprimerie Nationale, 1919). Metzger (*Textual Commentary*, 102) gives the number at 'about one hundred,' citing Ernest C. Colwell, "Mark 16[.9–20 in the Armenian Version," *JBL* 55 (1937): 369–86, as his basis. Colwell gives the number at 99 (371), but adds that another 33 marked vv. 9–20 as doubtful (376–77). In addition, four MSS place these twelve verses elsewhere (e.g., the end of Luke, end of John, or after 'according to Mark' at the end of the Gospel [378]). Colwell also notes that "out of 29 MSS earlier than the thirteenth century, only three include our passage and one doubts" (377).

47. Farmer, *Last Twelve Verses*, 39.

in the early fifth century. Joseph Alexanian, one of the world's leading Armenian scholars, notes that all extant Armenian NT MSS belong to the second revision, a revision that was based on Greek MSS brought from Constantinople shortly after the Council of Ephesus in 431.⁴⁸ As well, he notes that the text-form of the earliest of these MSS is either Caesarean or proto-Byzantine (*aka* "Early Koine").⁴⁹ Almost all Byzantine MSS extant today have the LE, but the Armenian version demonstrates (i.e., if it is truly Byzantine instead of Caesarean) that this was not always the case.

The two oldest Georgian MSS end at v. 8.⁵⁰ This version finds its roots in the fifth century.⁵¹

One Sahidic MS lacks any material past v. 8. The Sahidic is the oldest version of the Coptic NT, originally produced in the early third century,⁵² though, on the surface, whether this lone MS represents that text is more difficult to assess. However, the other Sahidic MSS, as we will soon see, confirm that the Sahidic original did not go beyond v. 8.

In addition to the Greek MSS and versions, there are several early patristic writers who may be witnesses to the Gospel ending at ἐφοβοῦντο γάρ. Origen is silent about the LE, even though he had opportunity to mention these verses.⁵³ Before him, Clement of Alexandria is also silent about the LE, though he also never quotes

48. Joseph M. Alexanian, "The Armenian Version of the New Testament," in *Contemporary Research*, 157.

49. *Ibid.*, 166.

50. The oldest Georgian MS, the Adysh Gospels (AD 897) and the Opiza Codex (AD 913) lack the LE. The third oldest, the T'bet MS (AD 995) has the LE. For the initial work, see Robert Pierpont Blake, *The Old Georgian Version of the Gospel of Mark*, PO 20.3 (Paris: Firmin-Didot, 1928).

51. The Georgian version was a translation of the Armenian, but has traces of the Old Syriac in it as well as readings akin to what is found in family 1. What is important to note is that the Georgian, like the Armenian and Old Syriac, seems to follow a tradition that is independent from the Diatessaron. Further, the later Georgian MSS begin to show signs of Byzantine intrusions. See J. N. Birdsall, "The Georgian Version of the New Testament," in *Contemporary Research*, 173–87, esp. 180–82.

Three Ethiopic MSS have also been alleged to lack the LE. But see Bruce M. Metzger, "The Ending of the Gospel according to Mark in Ethiopic Manuscripts," in *New Testament Studies* (Leiden: E. J. Brill, 1980), 127–47.

52. Metzger-Ehrman, *Text*, 110. This dating for the Sahidic version is doubted by Frederik Wisse, "The Coptic Versions of the New Testament," in *Contemporary Research* 133–34, who argues for the second half of the third century for the origins of the Coptic Bible.

53. One would think that he might have appealed to Mark 16:9–20 in his debate with Celsus about the Resurrection appearances (*Against Celsus* 2.56–70), though Farmer disputes this (*Last Twelve Verses*, 27–29).

from Matthew 28. Regarding the argument from the silence of both Clement and Origen, Parker notes:

Those who wish to argue for the originality of the Long Ending point to the weakness of this argument, and not unreasonably. It cannot be argued that the Long Ending was unknown to or rejected by Clement and Origen. But there is a more important point: neither can silence indicate that the Short Ending was unknown. The presence of evidence for the Long Ending is demonstrable. That there cannot be similar evidence for an absence of text requires us to accept that there is no evidence *against* the existence of the Short Ending in the second century.⁵⁴

In other words, since this particular textual problem involves the *addition* of a dozen verses, rather than the *alteration* of the same, there is simply no way to verify whether Clement and Origen knew of the LE. All we can say is that there is no evidence that they did.

Once we get to the fourth century, however, the situation looks decidedly different. Eusebius mentions MSS that end at v. 8 and those that end at v. 20. In discussing the differences between Matthew 28:1 and Mark 16:9, he says:

This can be solved in two ways. The person not wishing to accept [these verses] will say that it is not contained in all copies of the Gospel according to Mark. Indeed the accurate copies conclude the story according to Mark in the words ... they were afraid. For the end is here in nearly all the copies of Mark.⁵⁵

Eusebius thus indicates that most of the MSS in his day—the early fourth century—ended at v. 8; indeed, “the *accurate* copies conclude” here. That this is his opinion seems to be evident from the fact that the Eusebian Canons made no provision for Mark 16:9–20.⁵⁶

As might be expected, Farmer vigorously debates whether the statement about the accurate copies of the Gospel ending at v. 8 really represented Eusebius’s view of things, or whether Eusebius was

54. Parker, *Living Text*, 136–37.

55. *Ad Marinus*, NPB 4.255ff.; translation is found in Parker, *Living Text*, 134.

56. There is additional evidence that this was Eusebius’s opinion, though it is much later and its reliability has to be questioned: Several MSS of β^1 have a note to the effect that Eusebius “canonized” the Short Ending. See n. 74 for data.

simply quoting from an earlier source.⁵⁷ On the one hand, Farmer is not giving Eusebius enough credit for owning what is clearly stated as his opinion; on the other hand, *if* Eusebius is borrowing from an earlier source, then this shows that MSS with the short ending were in the majority even *before* the early fourth century. Farmer, in fact, suggests that Eusebius's statement about the more accurate and more plentiful MSS went back to Origen,⁵⁸ a concession that implies that the MSS ending at v. 8 were in a majority at least a century earlier than the time of Eusebius.

At the beginning of the fifth century, Jerome also notes that the LE is found in “scarcely any copies of the Gospel—almost all the Greek codices being without this passage. . . .”⁵⁹ His statement has been discounted since it is evidently a paraphrase of Eusebius's statement and since he did include the LE in the Vulgate. There are two reasons, however, why we should accept Jerome's statement as his own opinion. First, he *adds* information not found in Eusebius—viz., that almost all of the *Greek* MSS that he was acquainted with lacked the LE. This is an important point because Jerome's major work was the Latin Vulgate. He was very familiar with both Greek and Latin MSS. Yet he qualifies Eusebius's statement to refer *only* to the Greek MSS. The implication may be that the Latin MSS he knew often—or at least more frequently than the Greek—had the last twelve verses.

Second, Jerome was well acquainted with several MSS of Mark's Gospel. For example, he quotes from some verses that were found between vv. 14 and 15 of the LE, noting that he has found this material “in some exemplars and especially in Greek manuscripts of

57. Farmer, *Last Twelve Verses*, 7: “The import of all this is that if Eusebius is giving expression to his own critical judgment in saying that these questions from Marinus proceed from a misunderstanding of the text of Matthew, then the recorded discussion of the discrepancy between Matthew and Mark preserved in Mai's text in which reference is made to the existence of manuscripts which end at ἐφοβούντο γάρ must be older than Eusebius, and must have come to him as a twofold solution which was worthy of perpetuation in spite of the fact that Eusebius himself, on the basis of his own convictions, regarded the question as misconceived from the start.” The problem with Farmer's statement is that Eusebius represents at least a portion of the response to Marinus as his own opinion (viz., that the most accurate and most numerous copies of Mark ended at v. 8). Farmer does not distinguish the interpretive solution from the textual data that it is based on. Clearly, Eusebius rejects the first but accepts the second.

58. Farmer, *Last Twelve Verses*, 36.

59. In Jerome's letter to Hedibia, in *Epistola* 120, *PL* 22.980–1006. The translation (which is Burgon's) is conveniently found in Farmer, *Last Twelve Verses*, 23.

Mark in the end of his Gospel. . . .”⁶⁰ Until the twentieth century, the *only* evidence we had for such verses was Jerome’s quotation of them. But when Codex W was discovered, we finally had concrete proof of the verses that Jerome quoted. (These verses are known as the Freer Logion because they are found in Codex W, discovered by Charles Freer.) Jerome had seen this passage “especially in Greek manuscripts” but also in others. He thus makes a quantitative statement about three endings to Mark’s Gospel: most of the Greek MSS ended at v. 8; some of the Greek MSS as well as a few others added material between vv. 14 and 15; and a few MSS, almost none of them Greek, included vv. 9–20. All this shows that Jerome was well aware of the variations at the end of Mark’s Gospel precisely because he had access to numerous MSS. Thus, just because he was paraphrasing a statement from Eusebius is not sufficient reason to think that the evidence he was describing did not apply in his day.

Jerome’s statement has also been discounted because he included the LE in the Vulgate. Why would he do that? Perhaps for the same reasons that it is included in Bibles today—call it antiquity, tradition of timidity, or not wanting to rock the boat too much. In AD 400, a riot broke out in Tripoli when Jerome’s translation of Jonah 4:6 was read publicly. He used the word “ivy” (*hederem*) instead of the traditional “gourd” (*cucurbita*) to describe the plant that gave Jonah shelter. Augustine wrote to Jerome about the situation, pleading with him to temper how much he tampered with the traditional text. Even though Jerome wrote a defiant letter back, it is likely that there were limits to his alterations. If a riot had broken out over the description of a plant, how much more chaos could result if Jerome had omitted Jesus’ appearance to his disciples in Mark 16?⁶¹

Finally, Victor of Antioch, in the fifth or sixth century, notes that “very many copies” of the Gospel ended at v. 8, and “very many copies” ended at v. 20. He weighs in at this point and says that the

60. Jerome, *Against Pelagius* 2.15; translation is supplied by Parker, *Living Text*, 128.

61. In addition, it is possible that Jerome followed Eusebius’s advice of accepting *both* readings as what is “to be received” (Eusebius, *ad Marinum*, NPB 4: 255–56: “But another, on no account daring to reject anything whatever which is, under whatever circumstances, met with in the text of the Gospels, will say that here are two readings [as is so often the case elsewhere]; and that *both* are to be received,—inasmuch as by the faithful and pious, *this* reading is not held to be genuine rather than *that*; nor *that* than *this*” [Burgon’s translation found in Farmer, *Last Twelve Verses*, 4]). Obviously, in this instance, to accept the LE does not alter the Short Ending, so it would be possible to accept both without making any decision about authenticity.

more accurate MSS included vv. 9–20.⁶² Victor is important because his commentary was extremely popular, becoming the “established commentary on Mark for the later church.”⁶³

The patristic testimony thus reveals a very interesting trend: from the earliest discussion on the authenticity of this passage, the fathers indicate that most of the copies of Mark ended at 16:8. Yet, in later centuries, the short ending was increasingly looked on unfavorably, and in the standard commentary on Mark of the Middle Ages the short ending was rejected. Putting this on a trajectory, it takes little imagination to realize that what became the majority reading in the Middle Ages started out as a minority reading.

If this were all there were to the external evidence, it should be enough to convince us that Mark’s Gospel did not originally contain the LE. However, there is much more evidence that this is the case. First, there are alternative endings to the Short and Long Endings that made their way into the MSS. Codex Bobbiensis (it^k), an Old Latin MS written in c. AD 400, shows evidence of having been copied from a second-century papyrus.⁶⁴ This is the most important Latin MS of the Western texttype for the Gospels.⁶⁵ This MS does not have the LE, but instead has what is known as the Short Ending (SE) or the Intermediate Ending (IE) after v. 8, as follows: “But they reported briefly to Peter and those with him all that they had been told. And after these things Jesus himself sent out through them, from east to west, the sacred and imperishable proclamation of eternal salvation.”⁶⁶

62. Whether this scholium is actually from Victor or is from a later hand is difficult to assess. Farmer notes that if it is from a later hand then it may reflect the situation in the seventh or eighth century (Farmer, *Last Twelve Verses*, 25). He also notes that Victor is a “hostile witness” to the Short Ending, and thus “on actual count the number of copies of Mark circulating in his day which did not contain these verses may have exceeded the number which included them” (ibid., 26).

63. Farmer, *Last Twelve Verses*, 24. Hort dismissed this scholium as from a later writer, noting that it was probably produced in “the sixth or some later, perhaps much later, century” (B. F. Westcott and F. J. A. Hort, *The New Testament in the Original Greek*, vol. 2 [London: Macmillan, 1881], 35). For a critique of Hort, see Snapp, “Authenticity of Mark 16:9–20,” 15–16.

64. Metzger-Ehrman, *Text*, 102.

65. Ibid.

66. Translation in Metzger, *Textual Commentary*, 103. Besides it^k is likely that it^a also lacked the LE. This is the second most important Itala witness (Metzger-Ehrman, *Text*, 102). However, it is missing some leaves, requiring a reconstruction of its contents for chapter 16. Nevertheless, based on lines of text and letters per line, it has been determined that the MS either ended at v. 8 or had only the Intermediate Ending. Cp. C. H. Turner, “Did Codex Vercellensis (a) Contain the Last Twelve Verses of St Mark?” *JTS* 29

This ending is patently secondary. It is simply added to the abrupt ending of v. 8 as a way to conclude the Gospel. But it is a colorless conclusion. Why would it have been added to the Gospel? The obvious answer is that the Gospel MS that the scribe had in front of him ended at v. 8. Thus, Bobbiensis should be added to the witnesses for the short ending.⁶⁷ In the least, it does not support the LE.

In several other MSS this Intermediate Ending is added *before* the LE. The list includes “four uncial Greek manuscripts of the seventh, eighth, and ninth centuries (L Ψ 099 0112 *al*), . . . the margin of the Harclean Syriac, several Sahidic and Bohairic manuscripts, and not a few Ethiopic manuscripts. . . .”⁶⁸ In *none* of these witnesses is the Intermediate Ending added *after* the LE.⁶⁹ Yet the Intermediate Ending is both a conclusion to the Gospel and is implicitly contradictory to the LE, because the Intermediate Ending speaks of Mary telling the disciples that Jesus had risen, resulting in his commissioning of them, while the LE has Mary also telling the disciples of his Resurrection, resulting in their continued disbelief! If the Intermediate Ending were added *after* the LE, this tension would at least be more relaxed. Yet no MS does this.

The MS situation, therefore, points to three important implications. First, the MSS that added both the Intermediate and Long Endings imply that their ancestors *only* had the Intermediate Ending. As Metzger observed, “No one who had available as the conclusion of the Second Gospel the twelve verses 9–20, so rich in interesting material, would have deliberately replaced them with a few lines of a colorless and generalized summary.”⁷⁰ He rightly concludes that these witnesses should thus be added to those that end in v. 8.

Second, once a reading made its way into the text, it was very hard to dislodge it, even if that reading ended up contradicting what

(1927–28): 16–18. The significance of this MS is that it is European and in conjunction with Bobbiensis speaks not only of the widespread nature of the short reading but also its early date outside of Egypt.

67. Kurt Aland, “Bemerkungen zum Schluß des Markusevangeliums,” 157–80, argued that the Intermediate Ending was early and/or done in a remote location. Hence, it did not have the impact on the MS tradition that the LE did.

68. Metzger, *Textual Commentary*, 103. Altogether, the MS testimony for the Intermediate Ending is both diverse and early: L Ψ 083 099 274^{mg} 579 11602 it^k syr^{hmg} cop^{sa} ^{mss} bo^{mss} eth^{mss}. Lane adds 1961 to this list (Lane, *The Gospel of Mark*, 602).

69. Technically, as Lane notes, “MS 274 has the shorter ending after Ch. 16:20, yet critical signs indicate an awareness that the shorter ending belongs after verse 8 and before the longer ending” (Lane, *The Gospel of Mark*, 602n3).

70. Metzger, *Textual Commentary*, 105.

was added later. The fact that the Intermediate Ending is found in eight Greek MSS, *all* the Sahidic MSS (except for one that ends at v. 8),⁷¹ and several other versional witnesses, is concrete testimony to this principle.

And third, the presence of the Intermediate Ending demonstrates that scribes were not satisfied with the Gospel ending with “for they were afraid.”

Again, we ask the question of motive: Which is more likely—that scribes would intentionally omit vv. 9–20 or that they would add *some* ending because Mark 16:8 ends without Jesus appearing to his disciples? The Intermediate Ending speaks eloquently to both of these alternatives: scribes were prone to add material, even if it *created* contradictions, to a Gospel that ended with the fearful women.

In addition to these witnesses, there are several MSS that indicate some doubt about the authenticity of the LE. They do this in one of two ways: First, the scribe may add a note after v. 8, such as that found in Codex 22: “The end. In some copies the evangelist ended here, but in many this also”—referring to vv. 9–20 which follow.⁷² Similar notes are found in key members of Family 1 (1 209 1582), as well as in several other codices.⁷³ Altogether, a dozen MSS have such a note.⁷⁴ Second, the scribe might simply place an asterisk or obelisk in the margin, indicating doubt about these verses. Such a symbol is found in at least five MSS.⁷⁵ That these same

71. For what it is worth, the Sahidic MSS are considered to be primary Alexandrian witnesses (Metzger, *Textual Commentary*, 15*). Farmer argues extensively that the scribal practices on classical texts in Alexandria would have transferred over to Christian scribes, and that Eusebius, the Armenian version, etc. would have been strongly influenced by such. In this way, he attempts to make a connection that is otherwise not at all apparent. But he does not comment on the Sahidic MSS, yet these are *primary* Alexandrian witnesses (unlike Eusebius, the Armenian and Georgian versions, etc., which are not even Alexandrian). If the Alexandrian impulses to excise any ending past v. 8 were so strong, how could such impulses not have affected the Sahidic version?

72. Translation provided by Parker, *Living Text*, 127.

73. MSS 15, 20, 199, 205, 215, 1110, 1192, and 1210.

74. Listed on 407 in *Markusevangelium*, ANTF 27. The Greek text found in the *A* MSS, with slight variations, is as follows: εν τισι των αντιγραφων εως ωδε πληρουται ο ευαγγελιστης· εως ου και ευσεβιος ο παμφλου εκανονισεν· εν πολλοις δε και ταυτα φερεται. It may be significant that these MSS claim that Eusebius considered the Short Ending canonical, but apparently not the long. The only MSS that have the statement about Eusebius are 1, 205, 209, and 1582.

75. MSS 138, 264, 1221, 2346, and 2812, listed on 407 in *Markusevangelium*, ANTF 27. Parker, *Living Text*, 127, adds 137 to this list. It should be noted that we are only discussing the *Greek* MSS here; the versional witnesses have not been completely examined. But of the approximately 200 Armenian MSS that Colwell examined, 99 lacked

scribes *nevertheless* included vv. 9–20 reveals a basic principle that ancient copyists followed: if in doubt, *don't* throw it out!⁷⁶

As many textual critics have observed, one of the key evidences that a text is unstable is a high degree of variation and displacement. If so, the ending of Mark's Gospel is a poster child for this principle. David Parker points out *six* different endings for Mark, along with multiple variations within them.⁷⁷ I have only scratched the surface of the textual fluctuation in my presentation of the external data.⁷⁸ But if we pause for a moment and take it in, the question that we raised earlier surfaces again: *Why* are there so many differences in the MSS here? The issue is not simply the LE vs. the SE, as Farmer seems to imply. If that were the case, then we might well expect all the MSS to line up in either camp. Instead, some MSS end at 16:8, others add an Intermediate Ending, while others indicate doubts about the authenticity of vv. 9–20. To be sure, the vast majority of MSS include vv. 9–20, but the diversity and age of the witnesses against the LE have to be accounted for.

Why is it that *this* Gospel, and *only* this Gospel, has major textual upheaval at the end? The reason can't be due to discrepancies about the Resurrection accounts because whether Mark is in the mix or not, there are still tensions in these accounts. Mark 16:9–20 is hardly the most difficult post-Resurrection narrative to harmonize with the other Gospels. And it can't be because of the promise about handling snakes and drinking poison because this portion of the LE is the most secure, patristically speaking, of the entire pericope. As we have seen, the only alternative is that scribes were

the LE, 33 indicated that vv. 9–20 were doubtful, and four placed the LE elsewhere (see data in n. 47).

76. Contra Farmer, *Last Twelve Verses*, 43, who states (without further argument) that the scribes who indicated some doubts about the LE nevertheless considered such verses to be authentic. This seems to contradict even the principle that he enlisted from Eusebius earlier in his work—viz., that the SE and LE should *both* be accepted since the MSS were not uniform (Eusebius, *ad Marinum*, NPB 4:255–56).

77. Parker, *Living Text*, 124–28. In all fairness, Parker includes the scholia and asterisks as well as text for what he calls an “ending.” Strictly speaking, scholia and asterisks are not part of the text but are notes *about* the text. The actual number of endings to Mark's Gospel should thus be reduced to five: (1) conclusion at v. 8; (2) the Intermediate Ending; (3) the Intermediate Ending, followed by the Long Ending; (4) the Freer Logion wedged between vv. 14 and 15 of the LE; and (5) the LE following immediately after v. 8. At the same time, it should be noted that some Armenian MSS place the LE at the end of Luke or the end of John, so in addition to the endings that Parker has noted, one might also recognize that the LE is, like the *pericope adulterae*, a floating text.

78. In particular, we have not discussed in any detail the Freer Logion, found only in Codex W.

uncomfortable with a Gospel ending without any Resurrection appearances.

Which is easier to believe—that scribes cut out rich theological material because they didn't like the abrupt change between vv. 8 and 9, even though that would obliterate any Resurrection appearances by Jesus to the disciples in this Gospel? Or that they felt that the Gospel came to a close too abruptly and needed to have a proper ending in which the Resurrection appearances of Jesus were included? The answer should be obvious: When scribes find small segments of text that are problematic, they do some plastic surgery. But they don't amputate the leg because of athlete's foot!

One final comment is needed on the external evidence. The MSS, versions, and patristic evidence on behalf of the short ending are early and widespread. They represent the Alexandrian (especially the *primary* Alexandrian), Western, and Caesarean texttypes, possibly even the proto-Byzantine.⁷⁹ The short reading is found in the *best* witnesses of the three most important early versions as well—the Latin, Coptic, and Syriac. There is no obvious connection between all these diverse witnesses, showing that this reading

79. Remarkably, Farmer says, in his unpublished revision of *The Last Twelve Verses*, that “the Georgian and Armenian manuscripts... all stand in the Alexandrian textual tradition” (33). The sometimes careless and uninformed statements in Farmer's work prompted Birdsall to not mince words in his critique, saying that Farmer was “ill acquainted with most of the text-critical work of the last fifty years” and that the book “is marred by a remarkable lack of documentation and reference, as well as by the slender acquaintance which he shows with the implications of our present knowledge of the documents and history of the New Testament text” (J. N. Birdsall, “Review of William R. Farmer, *The Last Twelve Verses of Mark*,” *JTS* 26 [1975]: 151–60; quotations on 153, 158). Unfortunately, the unpublished revision did not improve one's confidence in Farmer's acquaintance with the literature.

Metzger-Ehrman (*Text*, 323, 325, n 41) are also blunt in their assessment of Farmer's analysis of the external data: “The late William R. Farmer attempted to account for the absence of the last 12 verses of Mark in certain early witnesses on the supposition that, during the second century, Alexandrian scribes, being uneasy with the references in verse 18 to ‘picking up snakes’ and ‘drinking poison,’ decided to adopt techniques of Alexandrian Homeric scholars and consequently deleted all 12 verses in certain Greek manuscripts. . . . But this explanation is unsatisfactory: (1) Farmer misunderstood the work of Alexandrian philologists, who did not ordinarily delete lines from the Homeric epics but merely athetized with an obolos the line or lines deemed to be inauthentic; (2) no convincing explanation is given for why all 12 verses should have been deleted rather than only two phrases in verse 18 that Farmer thought were embarrassing to members of the Alexandrian Church; and (3) it is entirely unrealistic to suggest that the abbreviated Alexandrian text could account for the short text in such widely scattered versions as those preserved in the earliest Latin, Syriac, Sahidic, Armenian, and Georgian manuscripts. Not by the wildest stretch of one's imagination can it be argued that all these reflect the influence of the hypothetical suppression of the passage in Alexandria.”

must go back to a much earlier stage in the transmission of the text. Further, as the centuries rolled on, the LE was at first considered inauthentic, then timidly accepted, then finally considered to be part of the original text. Yet even then, many scribes registered their doubts about this ending.⁸⁰ If the internal evidence on behalf of the LE looks at all suspect, we should consider the matter closed: Mark's Gospel did not originally have vv. 9–20.⁸¹

The End of Mark's Gospel—Internal Evidence

Because J.K. Elliot's essay in this volume focuses on internal evidence, I will make my comments here rather brief. Indeed, I will only use very broad strokes.

The key issue for internal evidence is whether it is likely that Mark would have written vv. 9–20 or not. The typical points that are raised here are vocabulary, syntax, style, and context (especially how this passage relates to the immediately preceding text). Several scholars have pointed out the high number of words and phrases used nowhere else in Mark's Gospel except here, and this argument has been countered by showing that Mark *elsewhere* displays just as high a number of rare words in the same amount of space. Then there is the statistical argument about words and phrases that Mark is fond of, and how such words do not occur in the LE. This is countered by noting that the lack of these same characteristic words can be seen in several *other* passages in Mark. Sometimes syntax is brought into the mix, and it is largely detrimental to the LE; but frankly, this is an area that still needs more work.⁸² There are many syntactical issues that have not even been looked at.

80. This is all the more remarkable since there would have been great reticence to leave *any* Gospel without resurrection appearances.

81. Farmer (*Last Twelve Verses*) suggested that no one see had enough influence to promote the LE sufficiently that it would have made its way into the majority of MSS extant today. In the unpublished revision, he modifies this view to the effect that Rome may well have been "a metropolitan center in which some ecclesial effort was made to control the natural proliferation of textual change" (37n100). Here he has most likely put his finger on the place of origination of the LE. But he dismisses this possibility: "there is no evidence that that ecclesial control was strong enough to influence other scriptoria in metropolitan centers in the West like Milan or Lyons." One wonders how Rome, which was admittedly influential far beyond its borders, could not have influenced other *Western* centers.

82. Internal arguments for the inauthenticity of the LE can be found in J. K. Elliott, "The Text and Language of the Endings to Mark's Gospel," *TZ* 27 (1971): 255–62; Metzger, *Textual Commentary*, 104–6; Cox, *History and Critique*, 147–205 (Cox looks at

What marks most of these studies is an unprincipled approach that picks bits of data willy-nilly, but never argues on sound linguistic principles as to whether Mark could have written this section. Two recent studies, however, have raised the methodological bar; after a detailed examination, both of them concluded that Mark did *not* write vv. 9–20.⁸³

For my own take on things, I wish to simply highlight a few points about these arguments. First, the most important internal argument is a *cumulative* argument. Thus, it is hardly adequate to point out that Mark, in *other* passages, uses seventeen words not found elsewhere in his Gospel, or that *elsewhere* he does not write εὐθέως for an extended number of verses, or that *elsewhere* he has other abrupt stylistic changes. The cumulative argument is that these ‘elsewheres’ are all over the map; there is not a *single* passage in Mark 1:1–16:8 comparable to the stylistic, grammatical, and lexical anomalies in 16:9–20.⁸⁴ Let me say that again: there is not a *single* passage in Mark 1:1–16:8 comparable to the stylistic, grammatical, and lexical anomalies that we find clustered in vv. 9–20. Although one might be able to parry off individual pieces

the LE in terms of form, redaction, and literary criticism); James A. Kelhoffer, *Miracle and Mission: The Authentication of Missionaries and Their Message in the Longer Ending of Mark*, WUNT 112 (Tübingen: J. C. B. Mohr, 2000), 65–122 (Kelhoffer especially focuses on syntactical anomalies); Travis B. Williams, “Bandaging Up the Red-Headed Stepchild: Reexamining the Style of the Longer Ending of Mark,” paper presented at the southwestern regional conference of the Evangelical Theological Society, March 23, 2007 (this paper in particular lays out a proper method for attacking the linguistic problem; to Williams’ credit, he is critical of both sides of the debate in how they have handled the data); and many others. Internal arguments for the authenticity of the LE can be found in John A. Broadus, “Style of Mark xvi. 9–20, as bearing upon the question of genuineness,” *Baptist Quarterly* 3 (1869): 355–62; Burgon, *The Last Twelve Verses*; Farmer, *Last Twelve Verses*, 79–109; Bruce Terry, “The Style of the Long Ending of Mark,” <http://matthew.ovc.edu/terry/articles/mkendsty.htm>. (Although Terry makes a decent case, unfortunately he interacts with virtually no one, but simply speaks of the arguments against the LE. This allows him to not examine in detail what these arguments really are, keeping them almost at the level of hearsay.)

83. Kelhoffer, *Miracle and Mission*, and Williams, “Bandaging Up the Red-Headed Stepchild.”

84. In particular, Terry’s treatment of the cumulative argument is weak, even though he is able to show quite successfully that many of the anomalies in 16:9–20 can be found scattered throughout the rest of Mark. He speaks about literary peaks, such as the Resurrection accounts would be, as narratives that would alter an author’s style; but he does nothing to show that such peaks significantly altered the style of Matthew, Luke, or John in the way that Mark 16:9–20 is different from the rest of Mark. Further, if such literary analysis was applied to the Intermediate Ending or the Freer Logion, it could be argued just as cogently that the lack of Markanisms in these texts were due to the nature of the Resurrection accounts.

of evidence, the cumulative effect is devastating for authenticity. Further, if the text is *already* suspicious because of external data, then these linguistic peculiarities are strong evidence of the spurious nature of the LE.

Farmer conceded that the external data were not decisive on behalf of the LE: "... the external evidence... does not produce the evidential grounds for a definitive solution to the problem. It will be necessary to consider the internal evidence bearing on this question before further conclusions can be drawn."⁸⁵ This is a remarkable concession about the external evidence—especially since Farmer never seriously considered the impact that the *Intermediate* Ending had on the history of the transmission or whether scribes would be prone to add a Resurrection narrative to Mark's Gospel. Had he wrestled with those considerations, his conclusions may have been different. But at least he says that scales should be tipped when the internal evidence has been examined. Yet this is where Farmer's argument is very weak. In my judgment, Farmer did not do an adequate job on the internal evidence to convince us that the LE was truly Markan. He looked at truncated evidence and parried off individual problems, while ignoring the cumulative effect of the entire linguistic argument against his case.

It is not just vocabulary, but syntax, style, and contextual flow that must be taken into consideration. As well, source criticism plays a role: Mark 16:9–20 deviates strongly from the pattern we see everywhere else in the Gospel when it comes to synoptic parallels. By far the best explanation for the Matthean and Lukan Resurrection accounts looking so *different* from each other is that they had lost their template because Mark ended his Gospel at v. 8.

Second, although some have pointed out a few terms and expressions in the LE that seem to be Markanisms, we might expect this from an ending that was intentionally attached to this Gospel. Obviously, the author of the pericope had read Mark carefully enough to see a problem with how it concluded. That he occasionally picked up some of Mark's vocabulary is not surprising. A modern analogy might be Morton Smith's "discovery" of the *Secret Gospel*

85. Farmer, *Last Twelve Verses*, 74–75. In the unpublished revision, his language was much more certain. Rather than calling the last section of the external evidence a "tentative conclusion," he simply labeled it "conclusions." And he adds that "the Greek manuscript evidence, in balance, weighs strongly in favor of inclusion, as does the versional and patristic evidence when considered as a whole" (50).

of *Mark*. Smith went to some lengths to show how this secret Gospel was linguistically in line with Mark.⁸⁶ Another scholar, after a careful examination of the evidence, thought the internal evidence conclusively showed that the secret Gospel was penned by Mark.⁸⁷ As it turns out, *Secret Mark* was an elaborate hoax created by Morton Smith himself.⁸⁸ If a modern scholar whose native tongue is not Greek could palm off a document as having been written by Mark, surely it is not unreasonable to think that a native Koine Greek speaker could have done something similar.⁸⁹ Indeed, Smith's hoax is *far* more Markan than the LE! But it certainly illustrates that the mere identification of a few Markanisms in a passage is not a sufficient basis for declaring it authentic.

Third, along these lines, as far as I know, no detailed linguistic analysis has been conducted on the Intermediate Ending of Mark or on the Freer Logion. Even though no scholar considers these passages to be authentic, it is instructive that both of them have Markan features—indeed, several Markan features that are not found in the LE!⁹⁰ If 'Markanisms' can occur in these two endings that are

86. Morton Smith, *Clement of Alexandria and a Secret Gospel of Mark* (Cambridge: Harvard University Press, 1973), *passim*; see also Scott G. Brown, *Mark's Other Gospel: Rethinking Morton Smith's Controversial Discovery*, ESCJ 15 (Waterloo, Ontario: Wilfrid Laurier University Press, 2005), 163–214 (Brown is persuaded by the linguistic evidence that *Secret Mark* is authentic).

For other decent treatments on Mark's style, see John Charles Doudna, *The Greek of the Gospel of Mark*, JBL Monograph 12 (Philadelphia: Society of Biblical Literature, 1961); and especially, Elliott, *Language and Style* (which includes essays by C. H. Turner, G. D. Kilpatrick, Nigel Turner, and J. K. Elliott).

87. Brown, *Mark's Other Gospel*, 220–30.

88. See Stephen C. Carlson, *The Gospel Hoax: Morton Smith's Invention of Secret Mark* (Waco: Baylor University Press, 2005).

89. At the same time, it should be noted that Smith was far more successful than the ancient author, since the evidence for his forgery has come not from linguistic evidence but from external and circumstantial evidence. Kelhoffer makes an excellent case that "the LE's author composed this passage to improve Mark's ending in conscious imitation of the NT Gospels" (*Miracle and Mission*, 65), and that "The LE's numerous Markan words and phrases reflect an effort to imitate the style of the work which this author augmented" (121). Thus, the author was intentionally appending this ending to Mark, but doing so on the basis of material from the other Gospels. To some degree, of course, the style of Luke, Matthew, and John would overlap that of Mark (especially if Matthew and Luke had used Mark to write their Gospels), so the converse might also be true: the author of the LE, by using Matthew and Luke, would end up writing a narrative that bore some resemblance to Mark.

90. For example, in the Freer Logion, there are three imperfect verbs, while there are none in the LE. In Mark 1:1–16:8 there are 293 imperfections. The ratio of imperfections to words in the Freer Logion is the same as the rest of Mark. As well, the Freer Logion uses vocabulary that is distinctive of or frequent in Mark (such as *σατάν*, *χριστός*, etc.)—words that are not found in the LE.

obviously not authentic, they can occur in the LE without implying anything about authenticity.

We will have to defer to Professor Elliott for additional details of internal consideration.

Irony in the End—The Case for Mark’s Intentional End of His Gospel at 16:8

Finally, I will briefly make a case that Mark intended to end his Gospel with ἐφοβοῦντο γάρ. Again, these points will be in germ form.

Those who object to Mark intentionally ending his Gospel at v. 8 use essentially three arguments:⁹¹ (1) an open-ended book is a modern convention, not an ancient one; (2) it is likely that the last leaf of the Gospel was simply lost before copies were made of it; and (3) books don’t end in a γάρ.⁹²

Regarding the first objection, W. L. Knox said, “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 storytelling, 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.”⁹³ Or as Robert Stein colorfully put things, “Are [the ancient] ‘less sophisticated readers’ more reliable or less reliable guides for interpreting Mark’s Gospel than modern-day literary critics deeply entrenched in Kafka-like existentialism and a reader-response hermeneutic?”⁹⁴

91. Among the scholars who do not think Mark intended to end his Gospel at v. 8 are Griesbach, Lachmann, Alford, Westcott and Hort, Gregory, Bultmann, Streeter, Schlatter, Cullmann, Dodd, Hengel, Moule, Metzger, Stein, Gundry, Elliott, and Evans—to name a few! For a more complete list, see N. Clayton Croy, *The Mutilation of Mark’s Gospel* (Nashville: Abingdon, 2003), 174–77.

92. An alternative to the idea that the ending was lost or that the Gospel concludes at 16:8 is that John 21 is the actual ending that Mark wrote. According to Evan Powell, *The Unfinished Gospel* (Westlake Village, CA: Symposium, 1994), someone in the early church placed that chapter at the end of John to make peace between the factions of the Peter-party and the John-party (on the assumption that John’s Gospel elevates John over Peter and Mark’s Gospel elevates Peter over John). Powell further suggests that these two Gospels were the first ones written. Although the thesis makes for interesting reading, it is hardly a study of plausible history.

93. Wilfred Lawrence Knox, “The Ending of St. Mark’s Gospel,” *HTR* 35 (1942): 22–23.

94. Robert Stein, “The Ending of Mark” (penultimate draft of a paper presented at the annual Institute for Biblical Research conference, Washington D.C., 2006), 16.

According to the lost-ending theory, modern literary criticism is viewed as just that—*modern*. Yet, if there is evidence that Mark's conclusion would have fit into the literary conventions of *his* day, it may be invalid to assume that *all* ancient readers were incapable of getting Mark's point.⁹⁵

J. Lee Magness, in *Marking the End: Sense and Absence in the Gospel of Mark*, demonstrates that suspended endings—that is, endings that leave the reader hanging—can be found in Graeco-Roman literature, in the Old Testament, and in the New Testament. In other words, he shows that such a literary approach is not due to a Kafka-like existentialism, but is rooted in ancient culture. To be sure, such suspended endings were rare, but they nevertheless existed, and they were effective. After canvassing several examples of suspended endings, Magness notes the literary effect of such: “literature which expresses more than it says demands an act of finding[,] which forces readers into a future of which the text is the foundation but they themselves are the builders.”⁹⁶ In other words, the readers find themselves as active participants in the story rather than just passive observers. Magness goes on to note that “the power that is generated from the phenomenon is fairly well focused: when readers supply this ending they participate in it and experience it more fully than if the writer had supplied it to them.”⁹⁷

When Magness turns to the NT examples, he finds one in the conclusion to Acts: “Luke is silent on two points in which he and his readers have invested a good deal of attention—the results of Paul's preaching in Rome and the results of Paul's legal appeal. The tantalizing mention of the ‘two whole years’ impels us to ask, ‘What happened then?’”⁹⁸

Just as Markan scholars have found the ending of the Gospel to be unsatisfactory, so also Lukan scholars have complained about Luke's conclusion. William Ramsay said, “No one can accept the

95. To be sure, I suspect that most readers would not have understood what he was after. But what the average reader understands and what the author intends are two different things. We have plenty of examples within the NT of authors being misunderstood; but reader inability does not nullify the author's meaning. See, e.g., 1 Cor 5:9–10; 2 Pet 3:15–16. Multiple illustrations in the ministry of Jesus could also be mentioned.

96. J. Lee Magness, *Marking the End: Sense and Absence in the Gospel of Mark* (Atlanta: Scholars, 1986), 17.

97. *Ibid.*, 47.

98. Magness, *Marking the End*, 84.

ending of *Acts* as the conclusion of a *rationaly* conceived history.”⁹⁹ I can’t help but wonder if Luke at least caught what Mark was doing literarily and consciously emulated his style in the book of *Acts*—intentionally leaving the book open-ended to bring his readers into the narrative. If so, Luke used and *respected* Mark—both for its content and for its literary technique.

Mark fits this ancient literary technique well: Magness argues that “the suspension of the narration of a post-resurrection appearance makes the impact of that foreshadowed event even greater in the expectant minds of the readers who have followed Jesus in the story.”¹⁰⁰

Second, the argument that the final leaf of the Gospel would have been destroyed or lost is an old one. Before serious work had been done on the birth of the codex, this argument seemed reasonable enough. But recent research has pointed to the *end* of the first century as the probable *beginnings* of the codex-form of books. Before then, rolls or scrolls would have been used.¹⁰¹ Now, if Mark was the last Gospel to be written, and if it was written in the 80s or 90s,¹⁰² there is a *very slight* chance that he utilized the new technology when he wrote his Gospel. If so, there is some possibility that before the copying of the MS took place, the last leaf became detached. However, if Mark’s Gospel is earlier than this—as virtually all scholars acknowledge, regardless of their view of the synoptic problem—then he would have written his Gospel on a roll, and the first generation of copies would also have been on rolls. And if the Gospel was written on a roll, then the most protected section would be the *end*, because when someone rolled the book back up, the end would be on the inside. To be sure, some lazy readers might not rewind the book when finished—of course, they would get fined a denarius at their local Blockbuster for such an infraction! But the reality is that this sort of thing was the rare exception, not the

99. William M. Ramsay, *St. Paul the Traveller and Roman Citizen* (reprint; Grand Rapids: Baker, 1982), 321–22 (cited also in Magness, *Marking the End*, 84) [italics added].

100. Magness, *Marking the End*, 121.

101. Cp. Colin H. Roberts and T. C. Skeat, *The Birth of the Codex* (London: Oxford University Press, 1983).

102. The earliest recorded instance of the codex-form was by Martial, c. AD 84–86 (Roberts-Skeat, *Birth*, 24). However, the new book-form was so far from a raging success that Roberts and Skeat conclude that “Martial’s experiment was still-born” (29).

rule.¹⁰³ Consequently, if Mark was originally written on a roll, it is hard to imagine how the ending could have gotten lost before any copies were made.

The third argument against Mark intending to end his Gospel at 16:8 is that the last sentence is simply ἐφοβοῦντο γάρ. For many years, scholars felt that no book could end with a γάρ, so they categorically rejected the possibility that Mark did so. It was not until 1972 that an actual book ending with γάρ was found.¹⁰⁴ The discovery was made by P. W. van der Horst, who concluded his article by the observation, “The proof was really not necessary for common sense alone could argue that, if a sentence or paragraph can end with γάρ, a book can too.”¹⁰⁵ Coupled with the increasing popularity of narrative criticism, more and more scholars have come to embrace the notion that Mark *intended* to end his book with ἐφοβοῦντο γάρ.

But this view was again challenged recently by Clayton Croy, who asked in his well-researched book, *The Mutilation of Mark's Gospel*, “what *kinds* of sentences end with *gar*? ...such sentences

103. F. G. Kenyon (“Papyrus Rolls and the Ending of St Mark,” *JTS* 60 [1939]: 56–57), though acknowledging that Mark’s Gospel would have been a roll instead of a codex, argued that “since the title [of a document] was habitually written at the end, this seems to be evidence that the roll was normally left with its end outside” (57). However, Kenyon actually supplies the very reason why this would not be the case, for he notes that labels would often be affixed to the beginning of such rolls, but they would often become detached; hence, “it was an obvious precaution to inscribe the title on the roll itself. . . .” (ibid.) What makes much greater sense, though, regarding such a precaution, is that the title would be in the most protected portion of the MS. Since the titles were usually put at the end (a practice that continued in codices, though for no other reason than emulation of the habit on rolls), the evidence seems to go against Kenyon’s suggestion. Stephen Pfann of the Jerusalem Institute once told me that of all the Dead Sea Scrolls, only one was not properly rolled up, with the end still being on the outside. This, at least, suggests that Kenyon’s suggestion is contradicted by the evidence. More recently, N. T. Wright has suggested that the Dead Sea Scrolls actually may be evidence for a lost ending: “A glance at any edition of the Dead Sea Scrolls, in particular at facsimile photographs, will reveal that even the scrolls which are preserved almost in their entirety are in many cases damaged at both ends” (N. T. Wright, *The Resurrection of the Son of God* [Minneapolis: Fortress, 2003], 619). However, what he did not take into account was that all MSS—whether in roll or codex form—are subject to deteriorate *over time*. The problem that his hypothesis has for Mark’s Gospel is that he presupposes that before the Gospel was even copied, it would have become significantly frayed. Wright suggests that some scholars might argue for an intentional ending at v. 8 because that would allow them to deny evidence for the bodily Resurrection in the earliest Gospel (ibid.). But Wright cites no one who actually holds to this position.

104. P. W. van der Horst, “Can a Book End with a ΓΑΡ? A Note on Mark XVI.8,” *JTS* 23 (1972): 121–24, citing Plotinus, *Ennead* 5.5.

105. Van der Horst, “Can a Book End with a ΓΑΡ?,” 124.

occur most often in certain kinds of literature.... Sentences ending in *gar* are much less common in narrative.”¹⁰⁶

As insightful as Croy’s work is, it does not make an airtight case. Kelly Iverson took it to task for missing several important pieces of data. Iverson produced numerous examples in *narrative* literature of sentences ending in γάρ.¹⁰⁷ He concludes his article by conceding that γάρ-ending sentences were rare in *all* literature, but adds: “... if Mark consciously and purposefully intended to end his account abruptly, why use a clichéd or even widely used phrase? If Mark’s intent was to shock his readers contextually, . . . why not a stylistic punch with an unusual concluding γάρ statement?”¹⁰⁸

This brings us back to where we began: If Mark wrote first, he created a new literary genre that would later be called *Gospel*. I suspect that because of the magnitude and uniqueness of Jesus, Mark also wanted to point to him in such a way that got the readers involved in the story. Of *all* the Gospels, Mark tends to leave it to the *reader* to form an opinion about Jesus rather telling the reader what he must believe. Throughout his Gospel, the disciples are asking, “Who is this man?” And the reader is drawn into the story, sitting in the boat with the disciples, wrestling with the same question. Mark wants them to work things out for themselves—not in an academic, detached way, but by coming to grips with Jesus of Nazareth.

If the fulcrum of the Gospel is Peter’s confession of Jesus as Messiah—a confession in which he is still half-blind because he only sees the Messiah in terms of a militant warrior, not a dying Savior—the scene sets us up for the end. The disciples are slow to get the message. In 9:32, after Jesus’ second prophecy about his death and resurrection, Mark tells us that “they did not understand this statement and were *afraid* to ask him” (οἱ δὲ ἠγνόουν τὸ ῥῆμα,

106. Croy, *Mutilation*, 48.

107. Using the *Thesaurus Linguae Graecae*, Iverson was able to discover several such instances. With extensive tables and data, his brief article lays out much of the data (Kelly R. Iverson, “A Further Word on Final Γάρ (Mark 16:8),” *CBQ* 68 [2006]: 79–94). At the same time, it is incomplete since the only sentences he examined were those that ended in a period. For an older use of *TLG*, but one that examined sentences ending in a question mark, see Cox, *History and Critique*, 149–57, 223–28. Altogether, Cox found more than 1500 instances of sentences ending in γάρ. One could also add sentences ending in the (semi-) colon, since such are often represented in translation by a full stop.

108. Iverson, “Final Γάρ,” 93. Iverson (87) noted that “the most impressive parallel” was in Gen 18:15, where Sarah at first laughed at the angel’s announcement that she would conceive in her old age; but when Abraham confronted her, she denied her response, “for she was afraid” (ἐφοβήθη γάρ).

καὶ ἐφοβοῦντο αὐτὸν ἐπερωτῆσαι). Then the pericope just quits: Mark leaves us hanging. And how he leaves us hanging is with the *imperfect* verb ἐφοβοῦντο—“they were *continually* afraid.” The imperfect tense has an internal perspective; its focus is not on the *end* of the action or the beginning; instead it takes an inside look. By its very nature, the imperfect is open-ended.¹⁰⁹

Mark then uses the *same imperfect verb* in 16:8. Mark 9:32 is a pericope that foreshadows both Jesus’ death and resurrection, as well as the disciples’ lack of belief in the same. At bottom, the disciples are seen as slow to get the message that Jesus was the suffering servant, and slow to get that they were called to suffer too. Like Peter, they continued to be half-blind. The Gospel then abruptly ends without any of the disciples *seeing* Jesus. Mark 9:32 may give sufficient hints that this will indeed be how the Gospel ends: they were afraid there when thinking about the Resurrection; they would be afraid again. The point is that if someone did not embrace Jesus in his suffering, he did not get to see him in his glory.

Mark could have narrated what all the readers knew: that the disciples did in fact see Jesus and they believed and understood. But he leaves off this ending precisely to draw the reader *into* the story. It’s as if he’s saying, What are you going to do with Jesus? Will you continue to be half-blind, or will you accept his crucifixion as an essential part of what it means for him to be the Messiah?

Although not all readers would have grasped the dramatically abrupt ending to this Gospel, all would have entered into the story—consciously or otherwise—and would have thought about the unstated Resurrection appearances. The *absence* of appearances in fact heightens the presence of Jesus in the readers’ minds.¹¹⁰

Conclusion

In sum, I have laid out a few of the major arguments for Mark’s Gospel originally not going beyond 16:8. As the reasoned eclectic on

109. See Buist M. Fanning, *Verbal Aspect in New Testament Greek* (Oxford: Clarendon, 1990) for a full treatment of verbal aspect; he speaks of the present and imperfect tenses as having an internal perspective (103, *passim*). Cp. also Daniel B. Wallace, *Greek Grammar Beyond the Basics: An Exegetical Syntax of the New Testament* (Grand Rapids: Zondervan, 1996), 500–501, 540–53.

110. So, Magness, *Marking the End*. Much more could be said about the open-endedness of Mark’s Gospel and the clues that he gives along the way in this direction. But due to space considerations, we will have to leave it here.

this panel, I try to take a balanced look at both external and internal evidence. However, my discussion is weighted far more on the external side because Professor Elliott will supplement the internal side. I also tried to show briefly why Mark may have intentionally ended his Gospel with ἐφοβοῦντο γάρ.

You will read other views on this thorny problem in this volume, and the presuppositions, methods, and conclusions will go in several different directions. If this seems bewildering, at least you should take comfort that Mark 16 is not alone in creating diversity of opinion. Or, to paraphrase George Bernard Shaw, “If all exegetes were laid end to end, they would not reach a conclusion.”¹¹¹

111. The actual quotation from Shaw speaks of “economists” instead of exegetes.