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CHAPTER 3

The Last Twelve Verses of Mark: Original or Not?

J. K. ELLIOTT

Any of us who are writers like to impress our intended readers, first by grabbing their attention immediately with a brisk, appropriate opening paragraph. Similarly, we like to conclude our writings with a satisfying climax or summary that our audience feels rounds off our narrative or arguments. The same applies to the biblical authors.

Matthew starts off in grand style with the genealogy; Luke has an elegantly crafted introductory sentence of four verses in length. Both Matthew and Luke have extensive infancy narratives, which set the scene for their stories of the ministry of the incarnate Jesus. John uses the Prologue, possibly a composition already in existence at the time he began writing, in order to emphasize his belief in the preexistence of Jesus. All of those make splendidly appropriate openings. Matthew ends his Gospel with the risen Jesus' stirring words from the mountain top, dispersing his followers. Luke and John have a satisfying selection of post-Easter appearances that reinforce in a positive way the announcement that the tomb is empty but that he has risen. John has even concocted two endings. John 20 reaches its conclusion in a satisfactory way, but later the author seems to have added chapter 21 as an appendix, again with its own convincing conclusion.

By contrast with these three evangelists, Mark seems rather blunted at both ends. His introduction is very brief, v. 1 looks like a short title, and then immediately after Old Testament citations we are suddenly introduced not to Jesus but to John the Baptist before being taken straight into the baptism story. We shall return to this later. Mark's ending is disputed. Do we end at v. 8? Do we proceed to v. 20, noting the strange jump from v. 8 to v. 9, even if—or especially if—Mark was responsible for those last twelve verses? Verse 8 ends bizarrely, and that is especially significant if this was the intended conclusion to the Gospel. The section following vv. 1–8 does not logically join on; in fact vv. 9ff seem to *parallel* vv. 1ff. Those problems are raised by the textual evidence too. Some MSS have vv. 9–20, others do not. And what do we do about the so-called shorter ending? And what about the longer text in W with the so-called Freer Logion after v. 14? The whole textual situation looks very unstable.

In this volume we are looking at the way (or ways) in which one of the evangelists, Mark, closed his Gospel. But I am going to extend my investigation by looking at the opening verses of Mark as well. I shall turn to that beginning section a little later.

But, first, we need to remind ourselves that the beginnings and ends of ancient books were particularly vulnerable. That applies to (sc)rolls and to books in codex format. Obviously an unbound codex was liable to be damaged at both ends, but so, too, was a roll—especially if its ending was occasionally exposed when it was not rewound to the beginning after each consultation.¹ The disputed ending of Mark may be compared with the various endings of Revelation (where there are nine different readings, seven involving Greek MSS), or Romans and the disparate textual support for its alternative endings. Each of these books has suffered and it may well be that we have lost all traces of their original conclusions. The irretrievable loss of some verses is an eventuality we may have to accept. As far as manuscripts are concerned, many otherwise complete MSS have lost their beginnings and endings. Two obvious instances are Sinaiticus that has lost the beginning of the Old Testament, and Vaticanus where the opening to Genesis has disappeared as has the end of the New Testament. Outside the Bible, there are numerous

1. See C. H. Roberts, "The Ancient Book and the Ending of St Mark," *JTS* 40 (1939): 253–57.

instances where manuscripts of literary texts are accidentally truncated. The means to restore such damaged texts were not always to hand, even when the mutilations were conspicuous.

We shall turn now to look at the external evidence for the ending of Mark at 16:8, as well as at the linkage of vv. 8–9. Then we shall examine the internal evidence for the Markan authorship of 16:9–20. This means that we shall look at the language and style of those verses, then their theological content. We shall then pose certain questions: How did Mark lose its original ending and thus circulate in some witnesses ending at v. 8? Was it deliberate or accidental? If vv. 9–20 are secondary we need to ask when they were added.

External Evidence

Only two early Greek MSS (actually the earliest we have containing the whole of Mark) and one other, medieval, Greek MS,² from among the thousand or so extant witnesses that contain Mark's Gospel, end Mark at 16:8, and yet it is on the authority given to these two early Greek Bibles that most critical editions of the Greek New Testament and most modern English versions reach their climax to Mark with the cliff-hanging but inconclusive v. 8 that ends with the particle γάρ.

The proverbial man in the street may say that these witnesses could just be unusual copies. And, he may go on to suggest that, surely, democratic principles require our siding with the majority of witnesses that include vv. 9–20.

Most scholars, however, would say that we are dealing not with any two MSS, but with Sinaiticus and Vaticanus, splendidly produced and evidently prepared as *deluxe* editions. Possibly these were written in response to Constantine's request for fifty Greek Bibles for his new capital. What is certain is that they represent attempts to define the Christian canon, as it had by then developed and to show this collection between one set of covers. Those editions did

2. These are Sinaiticus (Ⲙ 01), Vaticanus (B 03) and the twelfth century minuscule 304 (which had presumably been part of a four-Gospel codex). We exclude the twelfth century 2386 that at one time appeared in the apparatus (e.g., UBS¹) in support of Mark ending at 16:8. This MS merely has its last page of Mark missing; it may be used as an example of how the accidental shortening of Mark could have occurred at this exact place.

not spawn imitators.³ Generally, what continued to be copied by the church were the Gospels alone, or the Pauline corpus for example; and it was not until the Middle Ages that we again find complete New Testaments being produced. Whole Greek Bibles, Old Testament and New Testament, were not fashionable. One may therefore suspect that not only were these fourth-century witnesses, Sinaiticus and Vaticanus, peculiar in their scale and contents but that their texts too were untypical. As far as the endings of Mark are concerned, the examples set by Sinaiticus (Ⲱ) and Vaticanus (B), and possibly the other forty-eight copies also prepared for Constantine,⁴ were not followed. I do not wish to impugn B or even Ⲱ with generic unreliability or to suggest they were maverick copies. B in particular seems to have an ancient pedigree⁵ yet we cannot ignore its or Ⲱ's distinctiveness here at the end of Mark.

But, in reading an apparatus of a critical edition of the Greek New Testament, it is not obvious that these MSS are not straightforward in this matter. Let us take B first.

It is essential to remember that B, uncharacteristically, leaves a blank column following his truncated ending of Mark. Such a gap is exceptional in the New Testament half of this manuscript.⁶ Elsewhere in B the text of each successive book starts at the top of the next column. It is almost as if the scribe hesitated here. Perhaps his exemplar had the so-called longer ending of Mark, i.e., vv. 9–20, and he had instructions not to include it. His hesitation made him leave

3. So much for Farmer's opinion that Alexandrian MSS like those two were particularly influential. See W. R. Farmer, *The Last Twelve Verses of Mark*, SNTS 25 (Cambridge: Cambridge University Press, 1974).

4. If Sinaiticus was held back and not sent to Constantinople, then we must say that only Vaticanus was sent (and we therefore must speak not of forty-eight but of forty-nine MSS). Other pandects did not survive and we cannot know if they too lacked the last twelve verses of Mark. There is no reason why the text of the fifty sent to Constantinople was identical in each MS, and the probability is that they are unlikely to have agreed with each other textually. Different exemplars would have been employed by the various scribes of each of the fifty copies.

5. See C.-M. Martini, *Il problema della recensionalità del codice B alla luce del papiro XIV* (Rome: Pontifical Biblical Institute, 1966), *AnBib* 26. B has much in common with the third century P⁷⁵, and is unlikely itself to have been due to a recension. This groundbreaking study is ignored by Farmer, who seems to have been influenced by the intemperate opinions of John W. Burgon, the Anglo-Catholic Dean of Chichester, who implies B was maliciously flawed in his *The Last Twelve Verses according to the Gospel of Saint Mark* (Oxford: Parker, 1871).

6. In the OT the gap after Nehemiah is explicable: Psalms, written in two not three columns per page, follows; the two-and-a-half column gap after Daniel is the end of the OT; only the gap of one column following Tobit is comparable to that after Mark. Codex L has a blank where the Pericope Adulterae could appear.

the gap to allow for second thoughts, even though, as we are often told, the missing verses could not in practice have been inserted in such a gap (the end of one column and the whole of the blank column) if the same sized handwriting was to be employed.⁷ A similar situation is observable in those MSS that include the last twelve verses but who decided (or a later reader decided) to indicate the same hesitation by marking this longer ending with asterisks, obeli or a wavy line or with a note.⁸ A splendid Armenian MS in London (Arm. MSS add. 21932), like most early Armenian MSS, lacks the longer ending, but this one has 16:7–8 written in uncharacteristically huge letters that have the effect of using up spaces which had been calculated and left for the inclusion of much more text, presumably vv. 9–20—and this is yet another indication that, regardless of what the scribe chose to do, he was at least alert to the fact that here there was indeed a choice to be made.

A critical apparatus typically fails to note these scribal warning signs either in a MS that chose to omit the verses, or to add them, albeit with hesitation.

Now to Aleph. Here again the evidence is not entirely unambiguous. There are three scribes of this MS—A, B and D. Most of the New Testament was written by scribe A, but occasionally a section was written by scribe D. There are examples of D's work at folio 74,2 and 7 in Matt; 84,3 in 1 Thess; 84,6 in Heb; 89,1 for only Rev 1:1–5. D also wrote parts of Genesis, Tobit, Judith, 4 Maccabees, and Psalms.⁹ As far as we are concerned, the bifolium containing the end of Mark and the beginning of Luke is in the hand of D, who wrote smaller than scribe A did. Milne and Skeat¹⁰ give the average for D throughout Sinaiticus as 692 letters per column and for A as 630–640. This replacement leaf contains the usual four columns per page thus making sixteen columns by D in total here. There are

7. The issue is not clear-cut. One of my students has demonstrated that it is possible to insert vv. 9–20 into the space available; he was concerned not with the average letters per column but the maximum text possible. Several years ago there was much discussion about this topic in the papers circulated by the Majority Text Society, of which I am admitted as a passenger, so I was informed. I have various documents on this issue, written by Mike Arcieri, Thomas Edgar and Maurice Robinson in early 1990s.

8. Verses 9–20 are preceded by a critical note in MSS 20 and 22; asterisks follow v. 8 in 137, 138; cp. also 156, 187, 1221. See Joseph Hug, *La finale de l'Évangile de Marc: Mc 16, 9–20* (Paris: Gabalda, 1978) = *Études bibliques*.

9. See the Table of Concordance pp. 94–112 of H. J. M. Milne and T. C. Skeat, *Scribes and Correctors of the Codex Sinaiticus* (London: British Museum, 1938).

10. Milne and Skeat, *Scribes and Correctors*, 9–11.

ten columns of Mark and six given to Luke. Column ten at the end of Mark is only partly utilized and the rest of that column is blank. The six columns of Luke are unusually cramped compared with D's usual letter count per column elsewhere. The last six columns of Mark are stretched out, although the opening four columns by D in this replacement are not stretched out; in fact, column four has the second highest letter count of all sixteen columns. The letter count for the last six columns containing Mark is reduced to 598, 556, 605, 598, 560, followed by the concluding column containing the colophon.

Possibly, something went wrong at the beginning of Luke that required the text up to Luke 1:56 to be redone. Skeat argued that the reason for the rewriting was that scribe A had written part of Luke twice, a dittography, hence the rewriting required Mark to be stretched. But why did the stretching of Mark not start until column five? That section obviously includes the ending of Mark, whatever was in the exemplar and whatever A had written in the now discarded pages. As published, Aleph has Mark end at 16:8.

A unique omission in \aleph , due to homoioteleuton, occurs here: Mark 15:47–16:1 is omitted and that amounts to about five lines of text. Our question is whether 16:9–20 could have been fitted in columns five to ten. Again, as with our calculations regarding Vaticanus, the answer is “possibly,” although it would have been *very* tight, especially if the omitted words from 15:47–16:1 had also been reintroduced. The sixteen columns of this bifolium could not easily hold Mark 14:54–Luke 1:56 in D's hand if 16:9–20 were included. It is even less possible in A's hand. Luke cannot be written in less than six columns to reach the point at which scribe A's text continues. The stretching at the end of Mark and the cramping at the beginning of Luke suggest the original calculations by scribe D were unrealistic. Mark 16:9–20 contains somewhere between 966 and 973 letters. That would require one-and-a-half columns in the writing of scribe D and slightly more in scribe A's writing. All we may conclude is that the strange calculations suggest that the scribes were aware (as was the scribe of Codex Vaticanus) that the ending of Mark was disputed.

Scribe D of Sinaiticus was also very likely to have been one of two scribes of Codex Vaticanus—although not the one who wrote the end of Mark. But, nevertheless, this tells us that we are dealing

with two MSS from the same scriptorium. One thus sees that the two pandects, if we may use that word of Greek MSS, were connected. Some have argued that this means we are concerned effectively with only one MS witness to the text of Mark ending at 16:8 rather than with two independent early Greek manuscripts.

Thus the Greek external evidence is not crystal clear in its witness as far as \aleph and B are concerned. However, there are MSS that have the shorter plus the longer endings¹¹ and those MSS may be included as added witnesses to the fact that vv. 9–20 were not regarded as the authoritative and original ending to Mark. (I know of no one who defends the Markan authorship of the shorter ending.)

But the early versions support more strongly a text of Mark that ends at 16:8.

Versions

1. The fourth-century Latin MS Bobbiensis (*k*) is textually the oldest witness to the Latin Bible, with a text going back to the early third century. Vv. 9–20 are absent, and are replaced by the shorter ending. (It is therefore not surprising to see there is nothing from this ending quoted by Tertullian or by Cyprian in North Africa.) Lvt (*k*) differs from NA from Mark 15:45 onwards, e.g., at v. 16:1; the additions to v.3; and the absence of v. 8b.
2. The Sahidic Coptic usually ends at 16:8. See especially the Sahidic Barcelona fifth-century MS PPalau Rib. inv. Nr. 182.
3. Most early Armenian MSS lack the longer ending. In fact it has been calculated that some 99 of the 220 registered Armenian MSS lack the verses. The same is true of the oldest Georgian witnesses.
4. The Sinaitic Syriac is also an important witness for the omission.
5. The Byzantine lectionary system seems to have developed into a settled form by the eighth century—only after that

11. The shorter ending is found between v. 8 and vv. 9–20 in L Ψ 083 099 274mg. 579 / 1602. L has the shorter ending in the column following 16:8 and then has a decoration; following a critical comment comes the longer ending. None of these witnesses is older than the 7th century.

time do most lectionaries contain a reading from the longer ending. Certainly the Georgian and Armenian lectionaries, which are dependent on the Jerusalem, not Byzantine, lectionary system, lack this pericope.

Fathers

Eusebius gives us our clearest evidence that most NT MSS known to him ended Mark at v. 8. Jerome repeats Eusebius' observation, although Jerome's Vulgate contains the longer ending; Jerome was also aware of a text we know as the Freer Logion (found in MS W within the longer ending).

We need to recall that Mark was not popular in the second century; hence this Gospel was seldom cited. Westcott and Hort¹² and Cox¹³ and others list fathers who knew 16:9–20, of whom the most significant is Irenaeus. In AD 180 he knew 16:19 to be from Mark. But many other fathers did not quote from the longer ending and may therefore not have known it. That is of course an argument from silence.

This external evidence shows quite clearly that from the earliest times we have reliable information that Mark's Gospel circulated in different forms with differing endings.

To summarize, then, in the second century there is evidence that the longer ending was known and quoted; in the fourth century we have evidence that scribes were aware of a problem, the ending at v. 8 was known, as were the shorter ending and the longer ending; by the sixth century there is evidence of the shorter and longer endings together.

So what was it that caused the hesitation over verses 9–20, or their omission? Why do most printed editions and modern versions go with the minority of witnesses, and exclude vv. 9–20?

The answer is that the contents and theology of vv. 9–20 are uncharacteristic of Mark elsewhere. And also there is a significant difference in the language and style in those verses, compared with the rest of his Gospel. Let us look first at the grammar and vocabulary.

12. B. F. Westcott and F. J. A. Hort, *Introduction to the New Testament in the Original Greek* (London: Macmillan, 1881), "Notes on Select Readings," 39–41.

13. Steven Lynn Cox, *A History and Critique of Scholarship concerning the Markan Endings* (Leviston, Queenston, Lampeter: Mellen Press, 1993).

Internal Evidence

Language

This is not the place to indulge in a detailed analysis of the language, style and vocabulary of the longer ending. In 1971 I wrote a study of these features¹⁴ and it is flattering to see that that piece is still quoted with approval. Obviously, I could expand and nuance what I wrote then as a mere tyro.¹⁵

The following strike me as the most important features that are peculiar to the longer ending or are alien to Mark 1:4–16:8:

- vv. 10, 11, 13, 20: ἐκεῖνος is used as a pronoun.
- vv. 10, 12, 15: πορεύομαι is found as a simple verb. Elsewhere Mark uses it only compounded. I accept the *v.l.* giving the compound in 9:30. In the bulk of Mark πορεύομαι is used in the present and ἐλθεῖν in the aorist: here in the longer ending πορεύομαι is in the aorist.
- v. 10: τοῖς μετ' αὐτοῦ γενομένοις is used uniquely here of the disciples.
- vv. 11, 14: θεάομαι is not a Markan word.
- vv. 11, 16: ἀπιστέω is not a Markan word (n.b. ἀπιστία in 16:14).
- v. 12: ἕτερος is non-Markan.
- v. 14: ὕστερον is non-Markan. At 12:6 Mark has ἔσχατον where the Matthaean parallel (Mt. 21:37) has ὕστερον.
- vv. 17, 20: σημεῖα is Johannine not Markan. ταῦτα + anarthrous σημεῖα is not usual in the New Testament.
- v. 18: κἀν = 'and if' is non-Markan (at 5:28; 6:56 κἀν = 'even').
- v. 18: ἐπιθήσουσιν ἐπὶ + accusative is only here in Mark. At 5:23 the verb is followed by the direct dative where the Matthaean parallel has ἐπὶ + accusative.

14. J. K. Elliott, "The Text and Language of the Endings to Mark's Gospel," *TZ* 27 (1971): 255–62, reprinted as chap. 11 in J. K. Elliott, ed., *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: Brill, 1993) = *Supplements to Novum Testamentum* 71.

15. James A. Kelhoffer (*Miracle and Mission* [Tübingen: Mohr Siebeck, 2000] = *WUNT* 112: 65–121) has built on my study and expanded it considerably. Farmer (*Last Twelve Verses*, 83–103) also has a study of the language although he has rightly been criticized for ignoring vital features, e.g., he says nothing about ὕστερον, ταῦτα or σημεῖα and rather disingenuously attributes non-Markan features to the source used by Mark.

- v. 19: μὲν οὖν is unique.
- vv. 19, 20: ὁ κύριος: this Christological title occurs only here. At 11:3 κύριος = “master.”
- v. 19: ἀναλαμβάνω: only here in Mark.
- v. 20: this extended genitive absolute is rare, and here three words peculiar to the Epistles occur within the construction: συνεργέω, βεβαιῶ, ἐπακολουθέω.

(Note how many of the features listed above occur more than once in this longer ending. It is self-deceiving to pretend that the linguistic questions are still “open.”¹⁶)

Then, of course, there is the issue of the ending of the paragraph with γάρ. Much of the argument about this feature is well-known.¹⁷ The jury may still be out, but I am not inclined to think Mark intended his writing to end in this way and with a particle to boot, even though Vaticanus and Sinaiticus seem to have been prepared to let the Gospel end with γάρ. Whatever the scribes allowed, albeit with hesitation, I conclude that no author would have chosen to end a piece of writing, sentence, paragraph and even less a book, with a postpositional particle, and so we must decide that, originally, a continuation of v. 8 existed (alongside a possible Easter appearance) until the final page of the original Gospel of Mark was irretrievably lost.¹⁸

Just an aside: In assessing differences in language in this section, we ought to ask if we should expect authors to be consistent in such matters throughout. Could we perhaps find another passage in Mark of a length comparable to 9–20 and examine if its language and style are equally dissimilar to the rest of Mark? But I doubt if another similar passage betraying such anomalies exists. Yes, obviously, some stories have a higher than average run of different, distinctive vocabulary, but that is often explicable by the requirements

16. For an unnecessarily cautious view about the validity of the linguistic arguments see M. D. McDill, “A Textual and Structural Analysis of Mark 16:9–20,” *FilNeo* XVII (2004): 27–43.

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

18. As far as the verb φοβέομαι in v. 8 is concerned, the imperfect is followed by a direct object four times elsewhere in Mark (6:20; 9:32 11:18, 32) and that is normal in the NT and LXX, but there is no firm example of the imperfect in Mark where φοβέομαι is used absolutely. (At 10:32 *v.l.* by D and others omit the clause containing this verb.) At 16:8 we await a motive for the women’s fear. An English rendering that would indicate the interrupted sentence could end as follows: “. . . because they were fearful of.”

of the context. But the nature of the differences between 16:9–20 and Markan usage elsewhere is, as we have demonstrated, more than mere vocabulary.

Turner subjected the whole of Mark (excluding the longer ending) to a closely detailed linguistic analysis. Nowhere does he have to except any pericope because it stands out as markedly different. Mark's fingerprints, i.e., his distinctive style, are recognizable throughout. (We shall shortly turn to Mark 1:1–3 where I detect a significantly high level of non-Markan features, but that is a special case.)

Contents & Theology

Now to turn to the contents. Those have struck many commentators as peculiar, too. For instance, the opening words suggest that it is Jesus who is the subject in the preceding context and Mary is introduced as if for the first time. These verses hardly continue and explain what is written in vv. 1–8. The listing of the Resurrection appearances in a scanty manner in these verses looks more like a summary of Luke and John rather than a catalogue such as is found in the (earlier) 1 Corinthians 15; and it follows a *Jerusalem* tradition, *pace* Mark 16:7, which looks to a Christophany in Galilee. The reference to signs following believers looks more Johannine than Markan. “Tongues” are nowhere else in our Gospels. Drinking poison without harm is nowhere else in the New Testament; this detail seems to belong better in the New Testament apocrypha. The picking up of snakes differs from Luke 10:19. Some of these anomalies can be argued over but cumulatively they tell against Markan authorship.

The Longer Ending is not counted in the Eusebian canon numbers. The canon tables do not allow for Jesus' appearance to Mary Magdalene found in Mark and John. In some MSS attempts were made to extend the numbering system, without the canon table. These later insertions allow the numbers to reach beyond 233 with the new numbers in some manuscripts alongside vv. 9, 11, 12, 13 and 14.¹⁹

The evidence of the differences in language in the Longer Ending and indeed the problems of the theological contents of it seem

19. S. C. E. Legg, *Novum Testamentum Graece: Evangelium secundum Marcum* (Oxford: Clarendon, 1935), after xvi 9.

to demand an author different from Mark, the author of 1:4–16:8. For readers who are apprehensive about dismissing vv. 9–20 as a later accretion, it may be worthwhile exploring if Mark could have incorporated these traditional verses into the Gospel himself, possibly (according to David Black²⁰) at a later stage. After all, Luke seems to have taken over half a dozen canticles in chapters 1 and 2 that were probably in existence in Jewish circles prior to the composition of the Gospel. Some of them may even have already been used by John the Baptist's disciples in telling of his nativity and then were commandeered and incorporated by Christians when they too began telling of their Lord's birth.

Paul also borrowed hymns that were already in use in the early church, e.g., the *kenosis* hymn in Philippians 2:5–11; the “Christ hymn” in Colossians 1:15–20, and the hymn in 1 Thessalonians 5:16–22. The author of the Pastorals also uses a hymn in 1 Timothy 3:16. So there are adequate precedents in other New Testament writings to show that such things occurred (and we could also easily find similar literary borrowings within the Old Testament). But is Mark 16:9–20 one such literary composition taken over with approval by Mark to conclude his Gospel?

If it was, then the language, style, vocabulary, and even the theology are different from the undisputed words of the original author, as we have demonstrated. But it is rare (even unknown) for a NT author to lift, lock, stock and barrel, a *narrative* passage as opposed to a saying, a hymn, or a canticle, such as we are asked to believe Mark did when he found 16:9–20. It is an inferior piece of writing, plodding and grey, compared with Mark's racy, simple, and colloquial writing elsewhere. If he did find such a passage already in existence, he certainly did not refashion it in his own style.

I am unwilling to credit Mark with the incorporation of this allegedly previously composed ending into his new Gospel. There is no evidence he has taken over any other comparable portion. Obviously, like all the evangelists, he used materials from the oral tradition. Matthew and Luke took over texts from Mark, but they rewrote them and left their own literary fingerprints on them. I am disinclined to believe that it was Mark, the innovative composer and creative theologian, who took over, unrevised, a paragraph such as

20. David Alan Black, *Why Four Gospels?* (Grand Rapids: Kregel, 2001), 30.

vv. 9–20, especially as it may well have disagreed with his own theological stance.²¹

If I am right, then we are left with the argument that it was a later—probably second century—editor who found this paragraph and, despite its imperfections for such a purpose, used it (in time for Irenaeus to know it as part of Mark’s Gospel) to round off a dissatisfyingly incomplete Gospel—especially if that Gospel was by then being used to complete a fourfold Gospel canon.

Western Gospel Order

Another point that may be relevant is that in some early MSS Mark appears not as the Second Gospel to which we are accustomed but at the end, that is in the fourth position. This is the so-called Western order—Matthew, John, Luke, Mark—intended to put the writings by the two apostles before the two written by friends of the apostles. We find that sequence in MSS W, D, X,²² “several” of the older Greek minuscules,²³ Gothic, Syriac Peshitta, Old Latin MSS. For them, of course, 16:9–20 forms the climax of the whole collection.²⁴ Is the summary of the Easter events found in 16:9–20 particularly significant coming in that sequence, summarizing the preceding four Gospels’ accounts? May one even suggest that the addition of the longer ending occurred first when the Gospels were collected together and originally published in that sequence? If Mark really was circulating in a form that ended at 16:8 its ending would look even more inappropriate as the conclusion to the four-fold collection.²⁵ The longer ending would then be made the

21. For instance, the teaching that believers will be granted miraculous powers and that signs will prove the truth of the preaching is against Mark 8:11–13.

22. Early MSS \aleph A B C have Mark second in sequence.

23. According to B. M. Metzger, *The Canon of the New Testament* (Oxford: Clarendon, 1987), appendix 2 II 1.

24. See T. C. Skeat, “A Codicological Analysis of the Chester Beatty Papyrus Codex of the Gospels and Acts (P 45),” *Hermathena* 155 (1993): 27–43, reprinted as chap. B5 in J. K. Elliott (ed.), *The Collected Biblical Writings of T. C. Skeat* (Leiden: Brill, 2004) = *Supplements to Novum Testamentum* 113, 141–57, esp. 146–47. Skeat argued that the Western order was that originally found in the Alexandrian MS \mathfrak{P}^{45} ; cp. id., “Irenaeus and the Four-Gospel Canon” *NovT* 34 (1992): 194–99 reprinted as chap. A6 in id., *ibid.*, 73–78, which argues that Irenaeus knew the Gospels (in a codex) in that order. Also, in “The Oldest Manuscript of the Four Gospels?” *NTS* 43 (1997): 1–34 reprinted as chap. B6 in id. *ibid.*, 158–92 Skeat is prepared to conjecture that the earliest Western order was originally also in \mathfrak{P}^4 – \mathfrak{P}^{64} – \mathfrak{P}^{67} , despite their fragmentary nature.

25. Christian-B. Amphoux (“La ‘Finale longue de Marc’: Un epilogue des quatre évangiles” in C. Focant, ed., *The Synoptic Gospels: Source Criticism and the New Literary Criticism* [Leuven: Peeters, 1993], 548–55 = *BETL* 110) emphasizes the particular links

finale to all four Gospels and not just to Mark; its stories of the differing doubters about Jesus' resurrection and its stirring message on Jesus' lips are presumably intended as an appropriately hortatory climax to all four. Once added, this ending was kept even when Mark was placed in a different position among the Gospels.

Concerning Mark's Intent

Before we turn to possible reasons why Mark lost its original ending, let us first ask if Mark really intended his work to end at 16:8. Many commentators and scholars are prepared to defend the version of Mark ending at 16:8 as a viable literary and theological composition. The retired Lady Margaret professor in Cambridge, Morna Hooker, recently published two little monographs.²⁶ In these she showed the effectiveness of the ways in which each of the four evangelists opened and closed their Gospels. She tried to demonstrate that Mark's original intended ending was 16:8, which she sought to prove was a proper and significant conclusion. On such a theory, the women's silence must be understood to be temporary and positive.

If Mark was subtly leaving his Gospel with a cliff-hanging ending, emphasized by his deliberate use of the provocative γόρ, then that subtlety was lost on his immediate followers and into the next century. Because his purposes were misunderstood early on, certainly before AD 180, the alternative endings we know today were tacked on. If Matthew and Luke used Mark they also found Mark's ending deficient and added different Easter appearances to conclude their accounts. It seems they did not have anything to use in Mark beyond v. 8.²⁷ Similarly, later scribal copyists of Mark also clearly found a Mark that ended at 16:8 dissatisfying—and wrong.

between the longer ending and the language found in some of the distinctive variants by D in Luke as well as the sequence of the summary scenes in the longer ending and the order of the Gospels in D.

26. Morna D. Hooker, *Beginnings: Keys That Open the Gospels* (London: SCM, 1997) and *Endings: Invitations to Discipleship* (London: SCM, 2003).

27. On the basis of Markan priority there is no evidence that Matthew and Luke were dependent on Mark's Easter narratives. Both have different Easter stories. We cannot use Matthew or Luke to make claims about what they may or may not have read in their copies of Mark in chap. 16. If Mark was composed in AD 65 then its ending may have been lost before Luke and Matthew in the 70s-80s found it. Mark M.W. Waterman (*The Empty Tomb Tradition of Mark: Text, History and Theological Struggles* [Los Angeles: Agathos, 2006]) is another recent scholar who argues that Mark did not intend to go beyond 16:8 because his main interest was in the empty tomb. He lists (in his chap. 3 and in the

A sophisticated author could conceivably end his work in such an apparently truncated way in the knowledge that his audience were aware of what happened next, probably that these women, having regained their confidence did, indeed, do as the angel commanded and told the disciples. But to my eye Mark is not that sort of sophisticated author.²⁸

The message that the women disobeyed the angelic command out of fear is a bizarre climax. Such an ending leaves unfulfilled the expectation that Peter and the other disciples will see the risen Jesus in Galilee. Are we to assume that the continuation of the story containing this promised Christophany was so well known that Mark felt he could withhold it? I doubt it: it is not in the nature of this Gospel that Mark points us forward without giving us the completion. His emphasis on Jesus' divine foreknowledge of forthcoming events in the passion narrative is a significant part of his Christology—for examples, his predictions about the preparations in the upper room, the double cock crowing, the desertion of the disciples after the arrest, the betrayal by one of the twelve and so on are all fulfilled in the stories that follow. In all these cases Jesus knows what is to happen and inevitably these things come to pass.

The other Gospel writers show no reluctance to report the announced Christophany. Why should they? Even Paul, who shows little interest in or knowledge of what Jesus said and did during his ministry, is actively aware of the earliest traditions about the end of Jesus' career and its continuation. Post-Easter stories belong to the earliest stratum of the Jesus story, and had been in existence for a generation before Mark wrote. It would be odd if he were the only one to be reluctant to include such incidents. It would be even odder if, by ending at 16:8, he gave the impression that the Resurrection message was not passed on!

Once the original ending beyond v. 8 was lost, an alternative ending that seemed to meet with general approval was concocted—that is to say, the ending known to us in the majority of our MSS.

Appendix) scholars who have accepted this position and the other main theories about the endings of Mark.

28. N. Clayton Croy (*The Mutilation of Mark's Gospel* [Nashville: Abingdon, 2003]) is particularly good at exposing the weaknesses in arguments that 16:8 was Mark's intended conclusion. He shows that it was the result of new critical methodologies, such as "reader-response" theories and the like, that anachronistic attempts to recruit Mark as a sophisticated author were made.

Only then could Mark's composition be accepted unreservedly and read satisfyingly.

Concerning Others' Intentions

Although we have suspected that the original ending to Mark's Gospel was vulnerable to accidental shortening, it is worth asking if the shortening of the now lost ending could have been deliberate.

Could it be that a motive for the deliberate suppression of the original ending of Mark was indeed the very fact that it contained an appearance to *Peter*, as promised in 16:7? As we have it in the longer, spurious ending, *Mary* is specified as the first (πρώτον) to witness to and believe in the resurrected Jesus. The summaries following emphasize that, by contrast, others, including Peter, are all doubters.

There seems to have been a reluctance from the beginning to tell of Jesus' Christophany to Peter, especially if it was independent of a general appearance to the Eleven or if it occurred first. But some echoes of the tradition have survived. In Luke, Cleopas and his companion are keen to relate to the disciples that they have just witnessed the risen Christ in Emmaus, only to be informed that these two, otherwise unknown, followers, were *not* the first to see him; they are told that he has "already appeared" to Simon (Peter) but the episode itself is not included. Paul, despite his rivalry with Peter expressed through his letters,²⁹ is also aware of this private Christophany first to Peter (1 Cor 15:5). Obviously, an appearance of the risen Christ to Peter was so well known that reference to it could not have been expunged entirely from the tradition, but it is mysterious that no record of the encounter has survived.

We do not need to be seduced by modern preoccupations with Mary Magdalene and associated alleged conspiracy theories to note the significance that she is credited in this non-Markan ending (16:9) with a private, first, Christophany. That information thus superseded a story of Jesus' appearance to Peter.

There seem to have been rival claims concerning Peter's supremacy. We see this theme of questioning Peter's primacy emerge elsewhere. The idealized Beloved Disciple and Peter are portrayed strangely in the Easter stories of the Fourth Gospel, where the issues are: Who wins the race to the tomb? (The Beloved Disciple); Who looks inside first? (Peter); Who believes first? (The Beloved

29. E.g., in Galatians 2.

Disciple). And just who is this rival to Peter anyway, this “beloved” but anonymous, disciple? John’s story of the first Easter morning matches the Gospels’ uncertainty about the Christophany to Peter; all this is peculiar, given his prominence. (We recall the saying about the keys of the Kingdom in Matthew 16:18–19, and the rehabilitation of Peter in John 21.) The overall picture is ambivalent.

Why, then, is there no surviving story of a Christophany to Peter? My suggestion is that there had been and it stood in the original ending to Mark’s Gospel but, because of the later reluctance to credit him with this revelation, that ending needed to be removed.

I leave these interesting speculations to one side now, but merely repeat the questions if the original ending to Mark contained an appearance to Peter and if it was that which was the motive for its deliberate suppression.

Concerning Accidental Removal

Let us now approach the reason why some MSS have Mark end at 16:8 from a different angle. If vv. 9–20 had been written by Mark or even if a different, now lost, ending was there, some have asked how the ending may have been *accidentally* removed. Burgon and others, reluctant to accredit the loss to mere wear and tear or to carelessness, latched on to the little word τέλος (presumably intended to indicate the end of a church lection) found in some MSS alongside 16:8.

It was therefore said that a scribe, coming across this word at the end at v. 8, thought that he had reached the end not of a lection but of the whole Gospel, and therefore expunged everything written beyond that point. Such an argument strikes me as weak and unnecessary. It is unlikely scribes would have been so deceived or beguiled by such a meaning for τέλος when they could see more was to come.

Additional Considerations

Let us now briefly examine the problems I have detected in the first three verses of Mark and which are more fully expounded in an article.³⁰ First we must note that here there are no complete MSS of

30. J. K. Elliott, “Mark 1:1–3—A Later Addition to the Gospel?” *NTS* 46 (2000): 584–88. See C. F. D. Moule, *The Birth of the New Testament*, 3rd ed. (London: Black, 1981), 131–32n1.

Mark that lack the opening verses (although we recall that there are no very early MS witnesses to this Gospel).

The problems are as follows:

1. Punctuation. Where do we print a full stop in these opening verses? It could occur after v. 1 or v. 3 or v. 4.
2. Textual. The text is uncertain, especially in v. 1.
3. Theological. Is the “beginning of the Gospel” the coming of John the Baptist, the forerunner? In the other Gospels Jesus’ pre-eminence is emphasized before John is introduced.
4. Language and style.
 - v. 1: The names Ἰησοῦ Χριστοῦ occur together only here in Mark.
 - εὐαγγέλιον occurs eight times in Mark, seven of these refer to the divine plan in Jesus’ *teaching*. The one exception is 1:1 where it refers to the messianic *action*. Of the other seven, only at 1:14 is εὐαγγέλιον qualified (by τοῦ θεοῦ or *v.l.* τῆς βασιλείας): that seems particularly appropriate, if this is indeed the first mention (and on Jesus’ lips). The qualification Ἰησοῦ Χριστοῦ in v. 1 is unique.
 - καθὼς always follows its main clause elsewhere and proves something stated; it is not used in anticipation. Even if v. 1 runs on, that rule cannot be made to apply. If v. 1 is a title ending with a full stop then καθὼς is, uniquely, at the beginning of a sentence.
 - ἀρχή elsewhere is used temporally. Possibly v. 1 runs onto v. 4 but that is unlikely.³¹
 - The quotation from the Old Testament is not only unusually extensive for Mark but this is the only citation from the narrator. Elsewhere a citation is within speech. And this is the only citation introduced by the words καθὼς γέγραπται.
 - If it is an independent verse, v. 1 is verbless. Such a practice is exceptionally rare in Mark.

31. Croy (*Mutilation*, 124–30) shows how in later MSS this word served as a “marker” to show where a new reading began.

Thus there is a very high density of un-Markan usage—far higher than in 16:9–20, as it happens!

We also ought to note that the citation from Isa 40:3 found in Mark 1:2–3 is in the Matthaean parallel (Matt 3:3). The rest of Mark's citation is found in Matthew 11:10 and paralleled in Luke 7:27. If Mark 1:2–3 were original and read as such by Matthew (and Luke) it is odd that they then unravelled Mark's citation, putting it into two different contexts, one Markan, the other Q.

If these arguments are correct, then we have corroborative evidence that Mark's Gospel was damaged at both ends (although not necessarily simultaneously nor, initially, in the same manuscript).

Let us also return to Kelhoffer. What he tried to do was to investigate where 16:9–20 could have come from, assuming, rightly, that it was not Markan nor traditional material in existence prior to AD 65, found by Mark and used by the evangelist to conclude his own writing. Arguing that it was added to a truncated Mark in the mid-second century, he investigates what its context could have been. He sees these verses almost as floating material, comparable to some of the second century apocryphal fragments that have been discovered. The difference here is that the fragment has been attached to what was soon to be accepted as a canonical Gospel, and as such it has remained. It did not survive independently of Mark. Kelhoffer looks in particular at the miracles referred to in 16:17–18. Those are seen as uncharacteristic of a first-century writing such as Mark, but are entirely compatible with second-century Christianity, and he finds many parallels in that century's literature.

Kelhoffer has been rightly commended for locating a plausible context for this passage. I find his arguments overwhelmingly convincing. Kelhoffer says that the ending was no hastily compiled epitome, although we must make it clear that it was inappropriately cobbled on as a conclusion that can scarcely be said to develop or belong to vv. 1–8.

Thus 16:9–20 may be compared with additions to some Old Testament writings, e.g., the apparently later additions to Ruth, or to the Psalter, or to Deuteronomy. Those contrast with the abrupt ending to Ezra and the unfinished nature of Jonah, which serve as examples of texts that cry out for additions. Several pieces of literature in antiquity were revisited, edited, shortened, lengthened

and (in the case of Acts) reissued in a second edition. What we thus witness with ancient literature shows that texts that were read and used were subsequently liable to reediting.

Conclusion

The textual problems at the end of Mark and indeed the fluid text in much of the New Testament as a whole make talk of inerrancy, as narrowly defined by some, indefensible. Realistically applied to the New Testament the term must allow for errors in the transmission of the text. Unfortunately, the misuse of this word is bandied around rather like the use of “infallibility” when applied to *ex cathedra* pronouncements by the Pope.

We may argue that the original authors of the biblical texts were themselves inspired but to pretend that their words were transmitted unchanged is stretching credulity to its breaking point. Further, to argue that a particular strand of the MS tradition, typically the text represented by the Textus Receptus, or the Majority text, uniquely preserves, through “providential care,” those inspired words in their entirety ignores the scientific results of textual criticism as practiced in the past century or more, and such preconceived conclusions alienate academic discussion that depends on open and free inquiry. My work on MSS makes it clear to me that the New Testament is nowhere free from accidental and deliberate error and that the text certainly was never transmitted free from blemishes. It is our task as text critics to identify these secondary accretions, wherever they may occur and in whatever MSS.

The Münster Institut speaks of the *Ausgangstext*, a reconstruction as close as scholarship enables one to get to the possible original, authorial wording but one that explains the starting place from which subsequent existing corruptions arose. This they are trying to do in their *Editio Critica Maior* at present containing only the Catholic Epistles. As with other printed critical editions, I have my reservations about the resultant text they produce, but it is differing from NA²⁷—although not enough in my estimation. However, they are arriving at their text independently of any preconceived assessment about the Byzantine text or of any MS, Egyptian or otherwise.

As a thoroughgoing text critic, I think that that seems a step in the right direction.

Inerrancy is not coterminous with canonicity. When the fathers promoted certain texts as authoritative for Christians to read, they would accept, e.g., the Gospel of Mark or the Epistle to the Romans. They did not require a particular form of Mark or Romans. They never stated that in commending Mark they had in mind only the version of Mark running up to 16:8 or to 16:20. The Mark accepted as canonical was the form of the text the person, individual church, or monastery happened to possess. We noted earlier that certain MSS containing the Longer Ending do so with a health warning, an obelus, marginal note, or other sign; antiquity was reluctant to omit verses deliberately, especially if the text in question was seen as a part of Jesus tradition. Church usage recognized the Longer Ending since it was of great antiquity, and once it was eventually included in Gospel MSS, it was read in public services. This was pious inclusiveness. Moreover, we cannot nowadays forget that for most of its history the Gospel of Mark was interpreted and used by those who assumed (wrongly, we might say) that vv. 9–20 were part of Mark's original composition.

On the other side, to denigrate the text found in \aleph or B as if these are particularly corrupt specimens fails to recognize that at the very least the users of those MSS in antiquity read them as their form of canonical Scripture. My own approach to textual criticism allows that the correct, original text (or, better, the *Ausgangstext*) has been preserved, by sheer chance, somewhere in our 5,000 surviving Greek witnesses. Sometimes it may well be found in the majority of Byzantine MSS but at other times in \aleph or B (as in the case of the ending of Mark), or at other times in an occasional minuscule or a unique majuscule. I am, however, reluctant to accept conjectural emendation, and that is why I am cautious in my proposal about the secondariness of Mark 1:1–3, although I am prepared to admit that such a necessity may exist on occasions. The opening of Mark may well be one such occasion.

The word “canonical” does not imply “original” and it certainly does not involve appeals to divine protectionism, inerrancy, or inspiration (whatever those words are said to mean). Such judgments are made about texts by a certain brand of church tradition. The Bible may well be a collection of inspired writings or an inspired collection

of writings (i.e., writings not intended by their authors to be so, but deemed to be so by later Christians) but whether those definitions can help to decide on the rough and tumble of textual variation is improbable. The sooner that the language of inerrancy is dropped in the context of textual criticism the better it will be for scholarship.

Obviously, most of our surviving witnesses contain vv. 9–20 and, as such, these verses have been accepted as canonical Scripture, i.e., recognized by the church as definitive for Christian theology and practice. Bridget Gilfillan Upton, in *Hearing Mark's Endings*,³² makes a case for all *three* major endings to Mark. For her each was designed to have an effect on an audience that was hearing them read. She is not concerned with originality but merely to show that each ending known in the MS tradition was usable and could be effective rhetorically—and, of course, the users of the differing MS traditions were obliged to work with the form of Mark that they happened to possess.

But her work does not prove that any one of these texts was the text published by its author. Even the shorter ending would have been accepted as canonical where it appears in a MS. Let us take another example. The saying about the man working on a Sabbath found after Luke 6:4 in Codex Bezae (D) is a logion not accepted as authentic to Luke, but the owners of D would not have known that. There we are dealing with a saying found in a canonical text; other *ipsissima verba* may not occur in a text that became canonical Scripture. Paradoxically therefore, it may happen that a non-authentic saying attributed to Jesus may occur in a canonical Gospel and therefore by definition itself became canonical and authoritative, whereas an authentic saying may have survived only in a church father's writing or in apocryphal New Testament texts. The pericope of the adulteress is a floating piece of Jesus tradition (perhaps even an authentic story, and certainly as historically accurate as some other similar episodes), but it is probably a piece of an apocryphal text that never belonged to the original form of Luke or of John. Because it is now found in some MSS of the New Testament, it was not only preserved but branded as a part of canonical Scripture, Holy Writ. *Mutatis mutandis*, the same applies to the differing forms of Mark's Gospel.

32. B. G. Upton, *Hearing Mark's Endings* (Leiden: Brill, 2006) = *Biblical Interpretation Series* 79.

I conclude with two citations:

- Samuel Tregelles³³: “. . . [T]he remaining twelve verses, by whomsoever written, have a full claim to be received as an authentic part of the second Gospel, and that the full recognition of early testimony on this question does not in the least involve their rejection as not being part of canonical scripture.”
- Raymond Brown³⁴: “. . . [W]hile New Testament books are canonical, no *particular* Greek text should be canonized; and the most one can claim for a critically prepared Greek New Testament is scholarly acceptance” (italics mine).

33. S. P. Tregelles, *An Account of the Printed Text of the Greek New Testament* (London: Bagster, 1854), 258.

34. R. E. Brown, *An Introduction to the New Testament* (New York: Doubleday, 1997), 52.