

Nicholas P. Lunn *The Original Ending of Mark: A New Case for the Authenticity of Mark 16:9-20* (Pickwick Publications, 2014).

Nicholas P. Lunn challenges the scholarly consensus that the last twelve verse of Mark (16:9-20) are an addendum to the Gospel, developing a thesis represented in the work of William R. Farmer, *The Last Twelve Verses of Mark* (Cambridge University Press, 1974). Lunn's argument is methodical and meticulous, making it easy to read but challenging to review. Thus, I offer an overview of the book and then discuss its more salient points.

The preface highlights Lunn's apologetic aims. The thesis arises from his expressed concern to provide for the earliest Gospel a legitimate resurrection account – defined as a resurrection appearance – in order to prevent skeptics from mounting evidence against the tenets of the Christian faith (p. vii). The Introduction (ch. 1) establishes the rationale and approach for the study: while Lunn examines external evidence, his case rests largely on new internal evidence (ch. 1). In the next two chapters Lunn argues on the basis of manuscript evidence (ch. 2) and patristic allusions and citations (ch. 3) that the textual problem at the end of the Gospel is both temporally and geographically isolated. In the following chapters he argues on the basis of internal evidence that the longer ending (LE) best fits the vocabulary, style, narrative logic, and themes of the Gospel (chapters 4-7). After dealing with other objections to the authenticity of the LE, such as its supposed literary dependence on the earlier canonical accounts (ch. 8) and the awkward linkage between Mark 16:1-8 and vv. 9-20 (ch. 9), Lunn offers the following historical reconstruction of the cause of the textual problem (chapter 10): Sectarian groups in Alexandria opposed to bodily resurrection likely omitted the "original ending" (vv. 9-20) some time between the second and third centuries. Scribes in Egypt appended the shorter ending (SE) in order to correct what was then an abrupt ending. Later, the LE regained acceptance in Egypt and was reattached, in some manuscripts after the SE and in others directly after v. 8. Lunn draws together his argument with a concluding chapter (ch. 11).

The book appropriately problematizes arguments that have become axiomatic in the case against the authenticity of the LE. For example, Lunn challenges arguments that the vocabulary and style of the LE are decidedly non-Markan. He compares various other Markan pericope of similar length to the LE in order to demonstrate a similar pattern of unique words and phrases. Lunn argues persuasively that the unique vocabulary in any given pericope may be due to its subject matter or source material rather than to different authorship. In addition, he uses recent methods of stylistic analysis to look carefully at the linguistic evidence in Mark such as parts of speech, participant reference (that is, how the language refers to agents – by nouns, pronouns, or inflected verbs), collocations, and syntactical structures in order to demonstrate that the LE indeed reflects the overall style of the Gospel more than is commonly supposed. In his examination of participant reference, for example, Lunn shows that at the beginning of an episode, Mark typically refers to Jesus implicitly in verbal form, without a full noun or pronoun. Matthew, on the other hand, prefers the proper noun at the beginning of episodes. Lunn concludes that the style of the LE matches the style of the rest of Mark's Gospel in its implicit reference to Jesus in verbal form at the beginning of each of its first three sections (16:9, 12, 14).

Nevertheless, the book is riddled with methodological and logical problems that hamper its overall success. I give two sets of examples, first from Lunn's treatment of external evidence, and then from his treatment of internal evidence.

Lunn argues that the external evidence commonly marshaled against the LE is less, later, and more isolated than scholars have believed; and that the evidence in support of the LE is greater, earlier, and more diverse than scholars have accepted. He seeks to weaken manuscript evidence against the LE by isolating Codices Vaticanus and Sinaiticus as the earliest extant manuscripts in which the LE is absent, and emphasizing their 4th c. date as late relative to the autograph. Lunn characterizes these codices as aberrant compared to the many subsequent Alexandrian manuscripts with the LE. He similarly isolates Codex Bobiensis and Sinaitic Syriac for their lack of the LE from other manuscripts of the Old Latin and Syriac versions, respectively. Moreover, he associates the provenance of Bobiensis and Sinaitic Syriac with Egypt, suggesting that the same textual corruption that influences Vaticanus and Sinaiticus influences them, thereby divesting the earliest negative witness adduced for the abrupt ending of geographical diversity. As a result, Lunn views the two versions, like the Greek witnesses, as anomalies in the textual tradition. Lunn then seeks to strengthen the evidence supporting the LE by identifying patristic allusions prior to 150 CE. Throughout his discussion, Lunn relies more heavily on previous (and selective) scholarship than on his own examination of manuscripts, overlooks alternate ways to construe manuscript evidence, and marshalls tenuous data for patristic allusions. Nevertheless, he comes to the astonishing conclusion that, “the textual issue relating to the end of Mark does not have the complexity which it is often claimed to have” (p. 60), and, “the external evidence weighs on the side of the originality of the longer Markan ending” (p. 115). Lunn’s analysis does not merit such strong conclusions.

I look now at three parts of Lunn’s literary and thematic analysis that are linchpins of his argument and key examples of his approach to the text.

First, Lunn builds a case that the LE is a crucial part of Mark’s macrostructure. He offers a four-part, chiasmic outline of the Gospel, summarized as follows: A¹– 1:1-6:13 (Beginning of Jesus’ ministry in Jerusalem); B¹– 6:14-8:26; B²– 8:27-10:52 (Jesus moves outside Galilee, then moves towards Jerusalem); A²– 11:1-16:20 (End of Jesus’ ministry in Jerusalem) (p. 236). According to this structure, the risen Jesus appears to and commissions his disciples in Jerusalem at the end of the Gospel; however, Lunn does not sufficiently address the implausibility that the larger narrative context creates: Galilee is the meeting place for Jesus and his disciples, (16:7; cf. 14:28), and Jerusalem is a place of conflict for Jesus throughout the Gospel.

Second, Lunn argues that the three predictions of Jesus’ resurrection (8:31; 9:31; 10:34) must be fulfilled within the narrative, but that 16:1-8 does not suffice. He gives extratextual evidence: a prediction of this magnitude requires visible proof, so there must be an appearance, not merely an announcement. Then Lunn gives textual evidence: 16:6 uses the verb ἠγέρθη (“he is risen”) in the context of Jesus’ promise to meet his disciples in Galilee (v. 7; cf. 14:28, ἐγερθήναι); yet, all three Passion predictions use a different verb, ἀνίστημι, to predict the resurrection (8:31; 9:31; 10:34). He concludes that vv. 6-7 refer to Jesus’ saying in 14:28 and, consequently, neither refers to nor fulfills the Passion predictions. These are fulfilled instead, Lunn argues, when the evangelist introduces Jesus’ bodily appearances in 16:9, “After he rose (ἀναστὰς) on the first day of the week... .” But Lunn’s logic is faulty. The Passion predictions and 14:28 foretell different key events (the Son of Man will rise; I will go before you to Galilee), but the verbs ἐγείρω and ἀνίστημι in the respective verses refer to the *same* event, that is, Jesus’ resurrection (the LE even uses these two verbs

synonymously, 16:9, 14). The announcement of the resurrection (v. 6) is the basis of the young man's statement that Jesus' promise is ready to be implemented (v. 7). It is illogical to conclude, as Lunn does, that 16:6 "may be excluded from consideration as the fulfillment of the prediction by virtue of the fact that the young man in the tomb is actually referencing another separate utterance of Jesus" (p. 247).

Third, Lunn seeks to demonstrate the fit of the LE through its development of the Gospel's themes. For example, he argues that the disciples' response of belief to Jesus' appearance in the LE brings the narrative contrast between fear and faith to its proper finale. Yet the LE focuses rather on the disciples' *unbelief* and never narrates the disciples' response to Jesus' appearance. Also, Lunn argues that the signs to accompany the preaching of the gospel (like snake-handling) echo signs in the Jewish scriptures (like Moses' snake-handling) to develop the New Exodus motif prevalent throughout Mark. But he does not address the LE's failure to cohere with a narrative in which suffering, not signs, will accompany the preaching of the gospel (e.g., 8:34-38; 10:28-31; 13:9-13).

In conclusion, Lunn is to be commended for attempting an exhaustive approach to a difficult textual issue. But the book gives the impression that his apologetic aims not only generate his research question, but also restrict his analysis and determine his conclusions. In this case, the argument has had the opposite effect that Lunn intends: I am now less convinced than when I picked up the book that Mark 16:9-20 is authentic.

Reveiwed by Elizabeth E. Shively, St. Mary's College, The University of St. Andrews, St. Andrews, Fife, KY16 9JU, UK