

## BOOK REVIEW

Bernier, Jonathan, *Rethinking the Dates of the New Testament: The Evidence for Early Composition* (Grand Rapids: Baker Academic, 2022). xvii + 318 pp. Pbk. \$29.99.

Attempts to date various New Testament texts early appear in New Testament scholarship. But this current volume by Bernier stands out not just as an attempt to date a few texts early (such as the Gospels) but as an entire re-evaluation of the New Testament, ascribing early dates to virtually all of the New Testament, along with several non-canonical texts. The work is, as a result, one of the more intriguing volumes to be offered in recent years.

The book is conveniently broken down into five sections and a conclusion. The first regards the Synoptic traditions (Matthew, Mark, Luke and Acts) and argues for early dates for all of them. While holding the traditional view of Mark being the earliest, Matthew postdating Mark and Luke postdating Matthew, the volume will no doubt shock some readers with just how early Bernier dates them (Matthew to 45–49 CE; Mark 42–45 CE; Luke as early as 59 CE). Part 2 judges the Johannine traditions, with John being dated ‘no later than 70’ (p. 110). Part 3 concerns the Pauline writings (the pseudepigraphs and authentic letters) and includes more surprising details than just the dates ascribed, such as Bernier’s apparent acceptance of 2 Thessalonians, Ephesians and Colossians (p. 134), which informs the rest of his dating discussions, placing all of them within the range of 47–64 CE, if one regards 1 and 2 Timothy and Titus as authentic (p. 181). Part 4 deals with Hebrews, James, Jude and 1–2 Peter. Again, Bernier ascribes incredibly early dates to all of them (pp. 210–11, 235). Lastly, four pseudepigraphical works (1 Clement, Didache, Shepherd of Hermas and the Epistle of Barnabas) are all dated fairly early. The Didache is given a range of 45–125 CE; 1 Clement 64–70 CE; Hermas 60–125 CE; and Barnabas 70–132 CE. Of all the texts evaluated, only Jude and 3 John appear to be rather late (he gives dates of ‘before 96’ and ‘before 100’ respectively [see p. 275]).

The volume has a lot to offer and will no doubt be a favorite discussion point among many scholars attempting to date the New Testament. By far, Bernier has provided the most accessible volume for early dating in recent years, though it does come with some unfortunate drawbacks. Being only a few hundred pages while covering so many texts, Bernier's arguments often feel either truncated or underdeveloped in some regards. For instance, reliance upon the deaths of Peter and Paul is noteworthy in that the first record of them is 1 Clement, which is ambiguous and does not clearly relate any datable events. The first evidence of Paul and Peter dying in a roughly datable time is in the mid-to-late second century in highly fictive texts like the *Acts of Paul*, which we surely cannot utilize with efficacy. Thus, one can question the entirety of Bernier's argumentation regarding the ending of Acts not mentioning Paul's death. Perhaps his death was simply unnoteworthy and made for no narrative or dramatic impact or, perhaps, the author of Acts simply did not know how Paul died (as it appears no first-century Christian text does in any detail). With answers that can be this simple in response to Bernier's points, one wonders why an early date is necessitated. If Paul died under unknown circumstances, then we have another (and just as simple as Bernier's alternative) reason why Acts would never mention it. As such, his death has absolutely no meaningful repercussions on the dating of Acts.

There are other places where Bernier's counterarguments either seem weak or unreasonably strict—e.g. in his discussion of possible Lukan allusions to the destruction of the Temple, with regard to Luke's reference to people fleeing before the armies surrounded the city (21.21). For Bernier, 'This is not sufficient to establish a pre-70 origin for this passage' (p. 53). Given the combination with other passages which do refer to the temple destruction, along with armies surrounding the city, and several other points of allusion, one wonders just how much Bernier would actually require to be convinced, apart from Luke just outright saying 'and the temple will be destroyed in 70 CE under the command of Titus and Julius Alexander.' It makes for rather one-sided reading, with Bernier applying a strictness in his allusions that historians analyzing other texts simply do not require.

Other arguments appear more convincing but likewise, on closer inspection, leave me unconvinced. For instance, his statement 'Why warn people of an event that has already happened?' (p. 48) assumes that this 'warning' is meant to be taken as such, and not as a reflexive proof of Jesus' prophetic power, for example, that he predicted and gave warning of events before

they transpired. Other arguments are also problematic. For instance, as for ‘and then (*kai tote*)’ in Mt. 24.30, he claims, ‘there is nothing in the text’ that requires that *kai tote* be rendered ‘and then [after some time]’ (p. 49). However, it often has an unstated caveat that there is also nothing in the text prohibiting it either, such as reading one of Matthew’s predictions of Jesus’ coming as ‘and then after some time’ versus ‘and then’ (24.30). Bernier also undermines his own arguments in some places. For instance, if the ‘desolating sacrilege’ (p. 49) occurred under Caligula, the same problem of ‘the Son of Man did not come on a cloud shortly after’ (p. 50) applies equally. There are also literary issues at play. Bernier makes a lot of statements about ‘probably’ and such, but often without much justification. For instance, he declares that ‘in those days’ (Mk 13.24) ‘probably does not permit a significant temporal interval between the desolating sacrilege and the parousia’ (p. 50). However, why this is ‘probable’ is far from convincing, especially because Mt. 24.29 (contra Bernier) is not clear about the immediacy. Matthew does not actually determine how long ‘those days’ are to be but speaks of many eschatological features of ‘those days’, in which Matthew could foresee a long age of terrors and tribulations befalling people. Matthew 24.24, for instance, speaks of the rising of *ψευδόχριστοι* and *ψευδοπροφήται* in ‘those days’ (things not clearly evidenced from the destruction of the temple or pre-70 CE events). In short, we could easily argue that this speaks of the destruction of the temple as heralding the *start* of an end time scenario, hence the length of time is unspecified and only to be ‘shortened’ by the coming of Christ at some undisclosed point when ‘those days’ end (24.29-30). It would also then alert readers that they may be, themselves, in the end times—and thus have great literary effect. The destruction of the Temple would not be the only predicted event, and therefore ‘those days’ could be of quite some length... in fact they could be of any length (hence their literary effectiveness even up to the present day with apocalyptic Christians). Bernier’s argument is based only on pieces of the prophecy and not a consideration of the whole. These issues likewise apply to Mark 13.

So far, the points that may leave readers most skeptical are Bernier’s rather wide acceptance of the authenticity of almost all the Pauline epistles. Bernier does not really treat the authenticity of them, and in fact dismisses the issues rather flatly (p. 134) and without much comment. The work on their dating proceeds axiomatically from the assumption of authenticity, but given that these are such critically disputed issues, Bernier’s work really should have engaged with their authenticity and at least laid out a brief case

for the authenticity of the more disputed ones, rather than simply asserting ‘I accept that Paul contributed to Ephesians, Colossians, and 2 Thessalonians, as I find the arguments against their authenticity to be particularly weak’ (p. 134).

There is an overemphasis on the authenticity of Acts’s information on Paul’s journeys, as well, which betrays a lack of critical engagement with scholars who have quite convincingly shown the literary quality of this work. Notably, if Acts does not rely on Paul’s epistles, I would contend we have no reason to trust his information, since we have no clue of its provenance. If he did rely on Paul’s letters, we can further show (as Bernier does) points of divergence which indicate Acts’s less than scrupulous attempts to construct an accurate history. Either way, it seems strange that Bernier would not pick this as a way to then assert potential authenticity for the Pastorals, instead of asserting that he offers two possible dates for the Pastorals (one for possibly authentic and one for inauthentic calculations) because of ‘our inability to convincingly situate the Pastoral Epistles within Paul’s career as can be reconstructed from Acts’ (p. 134). If one notably asserts that Acts’s chronology and outline of this career cannot be trusted (which one can attribute either to lack of known source material to confirm his accuracy or because he clearly diverges from Paul’s epistles on numerous grounds), then we have no reason to use this as a metric for dismissing the Pastorals.

There is also a level of undue credulity toward the author of Luke–Acts (and Christian tradition as a whole). Bernier specifically cites this in the prologue, attempting to establish that at least on some level, the author was attempting a historiographical treatise, yet any engagement with recent work on Greco-Roman fiction and *bioi* (particularly of the more fictional variety) would be informative, specifically in looking at how these authors quite frequently used claims of careful historiographical practice as a way to build authority and establish themselves in a ‘competitive’ literary environment (Robyn Faith Walsh, *The Origins of Early Christian Literature: Contextualizing the New Testament Within Greco-Roman Literary Culture* [Cambridge: Cambridge University Press, 2021], p. 88). It is notable that Bernier takes very peculiarly conservative positions on New Testament literature, and especially on Acts, but engages with very little of the current literature on Acts, and the volume appears one-sided as a result. The ‘We’ passages are illustrative in that Bernier argues that they are from an eyewitness source or that the author of Luke–Acts was present there himself. Yet he never engages with critical texts that demonstrate how shifts to the first-

person in fictional ancient literature were actually rather noteworthy literary devices (Warren S. Smith, 'We-Passages in Acts as Mission Narrative', in Marília P. Futre Pinheiro *et al.* [eds.], *The Ancient Novel and Early Christian and Jewish Narrative: Fictional Intersections* [Groningen: Barkhuis, 2012], pp. 171-88).

As one final point of critique, Bernier's attempts to ascribe traditional authorship to the Gospels and many other texts will find few takers, in my view. Bernier's sections on the biography of the authors are based on the same poor evidence that has been remarked upon by generations of scholars, and his arguments are often not much more than a series of speculations amassed together. For instance, there is no good evidence that Peter was ever in Rome. Bernier just uncritically accepts fourth-century tradition from Eusebius and Jerome. Bernier questions: 'if [the traditional] view is affirmed, can we also affirm Markan authorship in the early 40s?' (p. 75), but there is simply no reason to accept these traditional accounts as anything other than late fiction, as they lack any support among early sources, and Bernier provides no reason to accept them. There is also a lot of reasoning from 'possibility' and 'nothing excludes the possibility' (see pp. 44-5, 74, 75, 92, 192, etc.) such that one is not being handed much concrete data.

While there are many things to critique, the volume has many things to praise as well. The early dating of Revelation is a curious issue and one well worth considering again (and was among Soviet and Marxist scholars in the past). Of all of Bernier's datings, this is the one which I am most likely to accept, though I am still skeptical. Bernier's work should be praised for the material covered, even though this gives it a feeling of brevity. Many issues are not always fleshed out (as noted above), and likewise many critical texts are not engaged (e.g. Otto Zwierlin, *Petrus in Rom: Die literarischen Zeugnisse* [Berlin: de Gruyter, 2010], pp. 245-331, who convincingly dates 1 Clement into the second century). However, this has its perks, in that the book likewise is always direct and to the point, and readers will not be shortchanged on material to dive into. Bernier's writing is fluid, and he naturally guides readers to his conclusions and brings all of his points together (regardless of how convincing one finds them). There are, admittedly, points at which I simply cannot come up with rebuttals to a number of Bernier's points, such that this volume is surely worth the time and effort to engage. This volume belongs next to other such esteemed works as Bauckham's *Jesus and the Eyewitnesses* and may very likely prompt just as much debate and response. However much one disagrees with Bernier, this

volume is still a must-read for academics and students wishing to engage in the dating of the New Testament. Though it will probably face many of the same criticisms, especially in its authorial biography sections à la Bauckham, Bernier's volume will undoubtedly still find a welcome place in the field and present itself as a welcome challenge to the consensus positions. *Rethinking the Dates of the New Testament* is a volume that all scholars should engage, whether they agree with the ultimate conclusions or not, and it stands as a respectable and encouraging response to the broad consensus positions in New Testament scholarship.

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