

The *Pericope Adulterae*: Where from Here?

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In what follows, I assess the cases for and against the *Pericope Adulterae* (hereafter, the PA) as an authentic part of the GJohn, and I also raise some questions about the early textual transmission of NT writings that are prompted in part by the evidence concerning the PA. But, first, I have to declare myself, particularly on the explicit issue that is the focus of this volume. On the one hand, having never published any analysis or view of the PA myself, I have no personal stake to defend on the matter. So far as I know my own mind, I do not have any particular need, wish, or preference *a priori* that the body of text in question should or should not be treated as authentic to GJohn, or as an interpolation. On the other hand, I must confess that (along with the overwhelming majority of NT textual critics) I bring to this task a prior view that the PA was probably inserted into GJohn at some point in its transmission, regarding this as superior to the alternative, that the PA was excised at some early point.¹ In short, I cannot claim to approach the subject without a prior opinion of my own, but I have tried to consider the various arguments put forth by my colleagues as fairly as I can.

The Manuscripts

I should also identify my approach to NT text-critical issues, which is what has been labelled “reasoned eclecticism.”² That is, I consider both “external” evidence (the age, diversity, and general quality of textual witnesses) important; but then I also take account of other factors as well, such as what we know about copyists’ practices and errors, and a variety of other matters relating to the transmission and use of texts, especially texts treated as scripture, in the early centuries. I have to say, however, that the place I start is with the textual witnesses. So, the lack of any manuscript support for the PA being a part of GJohn earlier than *ca.* the early fifth century (Codex Bezae) seems to me a strong initial basis for suspecting that the

¹ See, e.g., the discussion Bruce M. Metzger, *A Textual Commentary on the Greek New Testament, Second Edition* (Stuttgart: United Bible Societies, 1994), 187–89, who judged, “The evidence for the non-Johannine origin of the pericope of the adulteress is overwhelming” (187).

² For a recent discussion, see now Michael W. Holmes, “Reasoned Eclecticism in New Testament Textual Criticism,” in *The Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis, Second Edition*, eds. Bart D. Ehrman and Michael W. Holmes (Leiden: Brill, 2013), 771–802.

material was inserted at some point(s) chronologically into some copies of the GJohn in the process of its textual transmission. I think we require a very strong argument to think otherwise.

Moreover, as Wasserman notes in his contribution to this volume, for the GJohn we are in a comparatively stronger position as to early manuscript evidence, with remnants of some seventeen copies of GJohn that can be dated (palaeographically) to the second or third century.³ We can readily agree with Chapa that “the text of the Fourth Gospel is the best attested [NT] text from the second and third centuries.”⁴ Although only two of these early witnesses (P⁶⁶, P⁷⁵) are extant in the portion of GJohn where the PA could have appeared, I regard it as significant, nevertheless, that neither manuscript included the PA.

To stay with these two manuscripts a bit longer, I think that it is also relevant that they come from two different copyists with quite distinguishable copying abilities and practices, so we cannot easily write them off as the product of some particular recension of GJohn. Moreover, despite the evident differences in the copyists, both seem to have been concerned essentially to produce an accurate copy of their respective exemplars. Granted, the copyist of P⁶⁶ made lots of accidental errors, but then either this copyist and/or another person tried rather diligently to catch and correct these copying mistakes, some 465 corrections noted by scholars.⁵ These phenomena have led scholars more recently to conclude that, despite the copyist’s limited abilities to do so at first pass, P⁶⁶ “shows a concern to transmit a text responsibly.”⁶ It is also worth noting that there is no detectable theological interest exhibited in the errors and corrections.

As for the other major witness, P⁷⁵ (which contained at least the Gospels of Luke and John), it reflects a copyist with a skilful hand and who took care in copying. The corrections do not reflect any attempt to revise the text, but simply to ensure an accurate copy of the

³ I use “GJohn” (and “GMark” etc.) to refer to the written text in distinction from the traditional author, “John.”

⁴ Juan Chapa, “The Early Text of John,” in *The Early Text of the New Testament*, eds. Charles E. Hill and Michael J. Kruger (Oxford: Oxford University Press, 2012), 140 (140–56). He gives a list of witnesses (141) that updates the list I included in *The Earliest Christian Artifacts: Manuscripts and Christian Origins* (Grand Rapids: Eerdmans, 2006), Appendix 1 (209–29). Also, Chapa dates P⁸⁰ (P. Barc 83) to the fourth/fifth century (“The Early Text of John,” 142 n. 3). The “best attested text” among Christian manuscripts of this early period, however, seems to be Psalms, as I noted in *The Earliest Christian Artifacts*, 27–28, copies listed in Appendix 1.

⁵ See esp. the extended analysis of the corrections by James R. Royse, *Scribal Habits in Early Greek New Testament Papyri* (NTTS, 36; Leiden: Brill, 2007), 409–90; and, more briefly, Chapa, “The Early Text of John,” 143–47.

⁶ Chapa, “The Early Text of John,” 147. Royse (*Scribal Habits*, 544) concludes that the copyist “had the goal of producing an accurate copy.”

exemplar.⁷ Royse judged that the copyist worked carefully though “not with the unusual care that has sometimes been ascribed to him.”⁸ Among the copyist’s errors are a number of omissions, but these are most often only one or two words (one of six words and another of sixteen words), and are all evidently visual “leaps” (the copyist accidentally skipping words).⁹ There is no hint of an intentional deletion. Again, there is no discernible theological tendency, and no indication of revision by use of another exemplar of a character different from the one copied. So, there is scant reason to suspect that either \mathfrak{P}^{75} or \mathfrak{P}^{66} reflects any attempt to alter the text of John in any significant manner, and certainly no reason to posit any major excision or insertion, neither in these manuscripts nor (given the copyists’ concern to produce accurate copies) probably in their respective exemplars.

If we cast our net wider to take account of the more fragmentary papyri of GJohn, we get a similar impression. As others have noted, although the extant remains of early manuscripts of GJohn exhibit textual variation, it is actually modest in extent and nature. In the extant fragments, we see only small variations, e.g., in word-order, presence/absence of definite articles, tense variation, and a few other such minor matters.¹⁰ This suggests, again, that the various copyists (despite their varying skills in doing so) “attempted to remain as faithful as possible to the[ir] exemplar.”¹¹ Furthermore, the various copyist tendencies and abilities “suggest a diversity of places of provenance,” meaning that the extant fragments may well be representative of copying tendencies of their time more broadly, and at least from various places in Egypt.¹²

In sum, simply based on the character of the extant manuscript evidence, we have little reason for positing *either major deletions or major interpolations* in the text of GJohn in

⁷ The fullest discussion is now by Royse, *Scribal Habits*, 615–704. He counts fifty-five corrections in John (*Scribal Habits*, 625).

⁸ Royse, *Scribal Habits*, 704.

⁹ Royse, *Scribal Habits*, 662–70 on omissions and “leaps” in \mathfrak{P}^{75} .

¹⁰ See Barbara Aland, “Der textkritische und textgeschichtliche Nutzen früher Papyri, demonstriert am Johannesevangelium,” in *Recent Developments in Textual Criticism: New Testament, Other Early Christian and Jewish Literature*, eds. Wim Weren and Dietrich-Alex Koch (Assen: Royal van Gorcum, 2003), 19–38; and, more recently, the PhD thesis by Lonnie D. Bell, “Textual Stability and Fluidity Exhibited in the Earliest Greek Manuscripts of John: An Analysis of the Second/Third Century Fragments with Attention also to the More Extensive Papyri (P45, P66, P75)” (PhD, University of Edinburgh, 2015). He concludes that the evidence does not support claims about “wild” copying practices in the early period. Instead, “The data assessed in this study point to stability rather than fluidity and to continuity with the later period rather than discontinuity.” Similar results have been obtained in studies of early papyri of GMatthew: Barbara Aland, “Das Zeugnis der frühen Papyri für den Text der Evangelien: Diskutiert am Matthäusevangelium,” in *The Four Gospels 1992*, eds. F. Van Segbroeck, C. M. Tuckett, G. Van Belle and J. Verheyden (Leuven: Leuven University Press, 1992), 325–35; Kyoung Shik Min, *Die früheste Überlieferung des Matthäusevangeliums (bis zum 3./4. Jh): Edition und Untersuchung* (ANTF 34; Berlin: Walter de Gruyter, 2005).

¹¹ Chapa, “The Early Text of John,” 140.

¹² Chapa, “The Early Text of John,” 142.

the second and early third centuries CE.¹³ By contrast, there is at least indirect manuscript evidence that Paul's epistle to the Romans circulated in a fifteen-chapter form and, likely, a fourteen-chapter form in the second and/or early third century.¹⁴ Granted, we cannot say that no such major alteration of the text of GJohn ever possibly took place in any act of copying. But neither do we have a basis for saying that it did take place. For, if any such thing did take place, there is no trace of it in the extant manuscript evidence. To cite a saying, we can know only what we can show.¹⁵

Now it will be evident that in the foregoing paragraphs I am essentially echoing Wasserman's discussion of the early papyri and what they tell us about the copying of the Gospels, and John in particular, in the earliest period from which we have manuscript evidence. On one or two broader points, however, I am not so confident of what he posits. These are not perhaps decisive for the specific question under dispute about the PA, but I will mention them anyway as I think they have at least some relevance.

Wasserman proposes that the proliferation of gospels and gospel-like texts in the early centuries reflects a situation in which "the fourfold Gospels had not yet achieved the kind of authoritative dominance that would characterize them later on."¹⁶ But, for my part, I rather suspect that it was precisely the rising status/significance of certain gospels that prompted the production of others. So, for example, it seems to me that the earliest reception of the Gospel of Mark likely helped to prompt the authors of Matthew and Luke to produce their own respective accounts of Jesus. That they each apparently drew so heavily on Mark suggests that it had a high status in their eyes, even if they felt moved to produce expanded accounts of Jesus' ministry supplemented with larger bodies of his teachings, birth accounts, and resurrection appearances.¹⁷ Likewise, the production of the various longer endings for Mark suggests that it had obtained a status that made it matter very much how it ended.¹⁸

¹³ In an earlier publication, I offered an overall view of the early transmission of NT writings: Larry W. Hurtado, "The New Testament in the Second Century: Text, Collections and Canon," in *Transmission and Reception: New Testament Text-Critical and Exegetical Studies*, eds. J. W. Childers and D. C. Parker (Piscataway, N.J.: Gorgias Press, 2006), 3–27.

¹⁴ The various placements of the final doxology in Romans (16:25–27), e.g., after 15:33 in \mathfrak{B}^{46} , is commonly taken as the key evidence. See Metzger, *Textual Commentary*, 470–73; and esp. Harry Y. Gamble, *The Textual History of the Letter to the Romans* (SD 42; Grand Rapids: Eerdmans, 1977).

¹⁵ I believe that this sentence echoes a saying attributed to Jacob Neusner, but I am not able to point to a given publication in which it appears.

¹⁶ Tommy Wasserman, "The Strange Case of the Missing Adulteress," 15.

¹⁷ I see no evidence that the authors of GMatthew and GLuke were distressed by GMark and sought to overturn it.

¹⁸ Along these lines, note E. C. Colwell's comment on the various intentional variants in NT manuscripts: "Most of the manuals and handbooks now in print . . . will tell you these variations were fruit of careless treatment which was possible because the books of the New Testament had not yet attained a strong

I also think that we should distinguish between the production of (“apocryphal,” or better, “extra-canonical”) gospel-like writings additional to the ones that became canonical and the deliberate and major alteration of the latter writings. There were certainly other gospel-like writings produced in the second century and thereafter, and we know this because we have remnants of some (a matter to which I return later). But I submit that the composition of such writings is not the same as, or evidence that, the text of the Gospel of John, for example, was very fluid and subjected to major alterations in this early period.

The Climate for Deletions and Insertions

Jennifer Knust argues that in the late second century and thereafter, particularly after Marcion and the controversy he occasioned, it became very difficult to make substantial deletions from the text of the Gospels. She also contends that the general ancient practice with regard to variants was to mark suspicious or doubtful passages, not delete them. Her discussion of Origen’s handling of the Susanna story is a case-study, Origen retaining the story as part of his text of Daniel even though he knew that it was doubtful on text-critical grounds. So far as the PA is concerned, she also points to indications that Origen may have known of a version of the story, but there is no suggestion that he knew it as part of GJohn. In any case, she urges that the climate of Christian circles of the late second and third centuries was not conducive to the excision of bodies of material from those writings that were regarded as scriptures, including the GJohn.

I find her arguments cogent and forceful in effect. If the major suggestion for why the PA may have been deleted is that some early Christians were bothered by its representation of Jesus’ treatment of an adulterous woman, Knust’s evidence and argument seem to me to make that suggestion implausible. She shows that there is scant reason in general to think that Christians performed such major excisions, especially after Marcion, and scant reason in particular to think that Christians would have been moved to delete the PA.

To add something to Knust’s discussion, I return to the evidence in our earliest NT manuscripts. In particular, I propose that we should take note of a body of studies of these early manuscripts that focus on scribal/copyist habits, and especially on the incidence of omissions and additions to NT texts.¹⁹ To develop further a point briefly registered earlier,

position as ‘Bible.’ The reverse is the case. It was because they were the religious treasure of the church that they were changed”: *What Is the Best New Testament?* (Chicago: University of Chicago Press, 1952), 52–53.

¹⁹ Now esp. Royse, *Scribal Habits*, 705–36, who cites other studies as well as his own work on the early papyri. I draw upon his discussion in these comments.

the basic results of these studies are that, at least in the NT papyri from the second and third centuries, contrary to the assumptions of some previous scholars, omission is notably more frequent than addition (calling into question the sometimes rigid use of the “prefer the shorter reading” canon in assessing textual variants). But, we must also note that many/most of these omissions are accidental “leaps” of the copyist’s eye and involving the loss of only one or two words (though sometimes an entire line). Other omissions seem to have been intentional but, again, rarely involving more than one or two words. Likewise, apparent additions or expansions of the text (which seem generally to have been intentional) are almost always only a few words at most.

As for motives of intentional changes, they are overwhelmingly expressions of stylistic preferences, but sometimes harmonizations (especially to the near context), and occasionally perhaps a theological concern (for example, to clarify a potentially ambiguous sentence). But we do not have evidence of any programmatic effort to bowdlerize NT writings wholesale on some particular theological basis (no programmatic “orthodox corruption” evident).²⁰ Instead, where we think that we can see theologically-motivated changes in the text, these are *ad hoc* and entirely small changes of a word or phrase, almost always reverential in tone and made to remove potential ambiguities, the latter often in the interests of a particular christological view.²¹

As I noted earlier, we do not see any example of either a major excision of material or a major insertion of material in what remains of these earliest manuscripts, nothing comparable to the putative excision or insertion of the PA. The only NT variants with a comparably sizeable body of text are the “long ending” of GMark and the previously-mentioned shortened edition(s) of Romans. But, again, it bears noting that neither of these variants is actually attested directly in the extant manuscript evidence from the second and third centuries. That an edition of Romans without chapter sixteen circulated in the second

²⁰ Cf. Bart D. Ehrman, *The Orthodox Corruption of Scripture: The Effect of Early Christological Controversies on the Text of the New Testament* (New York: Oxford University Press, 1993). To be fair, Ehrman does not really claim a *programmatic* “corruption” of NT writings. Instead, he offers a number of *ad hoc* examples of what he proposes were doctrinally-motivated changes made by copyists. Eldon Jay Epp identified a number of distinctive readings in Codex Bezae in Acts as indicative of an “anti-Judaic” tendency: *The Theological Tendency of Codex Bezae Cantabrigiensis in Acts* (SNTSMS 3; Cambridge: Cambridge University Press, 1966). I find his case persuasive. But this tendency seems to have been restricted to Acts, and is clearly witnessed only in Codex Bezae (with some weaker attestation in a few other “Western” text witnesses).

²¹ See, e.g., Peter Head, “Christology and Textual Transmission: Reverential Alterations in the Synoptic Gospels,” *NovT* 35 (1993): 105–29; and esp. Frederik Wisse, “The Nature and Purpose of Redactional Changes in Early Christian Texts: The Canonical Gospels,” in *Gospel Traditions in the Second Century: Origins, Recensions, Text and Transmission*, ed. William L. Petersen (Notre Dame/London: University of Notre Dame Press, 1989), 39–53.

century (if not earlier) is, as noted already, quite likely, but it obviously proved to have a short life-span. From our earliest copy of Romans (in \mathfrak{B}^{46}) onward, the full sixteen-chapter edition was favored.

As for the “long ending” of GMark, though not attested in any extant manuscript earlier than the fifth century (e.g., Alexandrinus and Bezae), scholars often posit that it was added to at least some copy/copies of GMark earlier, perhaps even in the second century. Some hold that it was excerpted from some other writing, whereas Kelhoffer argues that it was composed specifically to provide GMark with an ending suitable for, and influenced by, a four-Gospel collection.²² In any case, if it was attached to GMark in the second or third century, the witness of Sinaiticus, Vaticanus, and early versional manuscripts shows that it did not win universal (or perhaps even widescale) acceptance at first.²³ But ultimately, and later, the “long ending” became the familiar final verses of GMark. So (as observed by Keith), it is a prime case of a major interpolation obtaining widescale acceptance in the manuscript tradition.

I suggest that we can see from the history of these two sizeable variants that ancient Christians tended overall, and in the long run, to prefer a fuller or more inclusive or expansive text. In the case of Romans, this meant the preference for an edition with chapter sixteen, even if it was perhaps not terribly rich for liturgical or theological purposes. In the case of GMark, the long-term preference was clearly for an edition with 16:9–20, even if it was not supported in some early and important witnesses and took a while to win out. By contrast, we have no clear example of a successful excision of an equivalent body of text from any NT writing. If, therefore, the PA was excised from some copy/copies of GJohn for theological reasons (e.g., Jesus’ treatment of the adulteress deemed bothersome), this would be unique, perhaps one should say extraordinary. On balance, however, I reiterate that the other examples of large textual variants indicate a tendency toward inclusion rather than exclusion, even a long-term readiness to include a variant (Mark 16:9–20) that was not supported (or at least not uniformly so) in early witnesses (a matter to which I return later). As applied to the PA (despite the arguments of Robinson and Punch), the odds thus seem to

²² Cf. e.g., Metzger, *Textual Commentary*, 105; and James A. Kelhoffer, *Miracle and Mission: The Authentication of Missionaries and Their Message in the Longer Ending of Mark* (WUNT 2/112; Tübingen: Mohr Siebeck, 2000), 175.

²³ The small number of fragments of Mark extant in \mathfrak{B}^{45} make it impossible to say whether the manuscript originally included Mark 16:9–20. See esp. Theodore Skeat, “A Codicological Analysis of the Chester Beatty Papyrus Codex of the Gospels and Acts (P45),” in *The Collected Biblical Writings of T. C. Skeat*, ed. J. K. Elliott (NovTSup 113; Leiden: Brill, 2004), 154 (141–57), originally published in *Hermathena* 155 (1993): 27–43.

me far greater that it is another insertion that eventually won widescale acceptance rather than an authentic part of GJohn that was excised in some early copies.

Johannine Style

The main arguments (by Robinson and Punch) for the authenticity of the PA seem to be that it bears marks of Johannine vocabulary and style and so that it comports with GJohn. Of course, analysis of the language of a variant to assess arguments for and against authenticity is standard practice, as also has been done in the case of the Markan “long ending.”²⁴ But, to my mind, a similarity of some vocabulary and expressions, and even purported echoes and connections of a more indirect or subtle nature (Robinson’s claims of a “tapestry-”like connection) do not suffice to overcome the striking absence of the PA from our earliest manuscripts of GJohn. Moreover, to note something that I do not think was mentioned in the symposium essays, I propose that there is indication that there were early texts composed that were unquestionably not a part of GJohn but that, nevertheless, exhibit Johannine-like vocabulary and expressions. The clearest instance of this is the “Unknown Gospel” (UG), the remains of it comprising P.Egerton 2 (+ P.Cologne 255).²⁵

At several points in the UG it is easy to observe remarkable parallels with GJohn. Compare, e.g., the striking similarities between P.Egerton 2, fragment 1 verso, lines 7–10 and John 5:39, lines 10–14 and John 5:45, and lines 15–17 and John 9:29. As well, there are other lines on the fragments that echo expressions in GJohn, and also GLuke, and perhaps GMark.²⁶ The similarities to GJohn are particularly obvious, such that literary dependence seems undeniable.²⁷ Nevertheless, significantly, this dependence did not involve simple copying. To cite Nicklas’ judgement,

²⁴ See, e.g., William R. Farmer, *The Last Twelve Verses of Mark* (Cambridge: Cambridge University Press, 1974).

²⁵ H. Idris Bell and T. C. Skeat (eds.), *Fragments of an Unknown Gospel and Other Early Christian Papyri* (London: British Museum, 1935). In 1987 a further small fragment (P.Cologne 255) was identified: Michael Gronewald, “Unbekanntes Evangelium oder Evangelienharmonie (Fragment aus dem ‘Evangelium Egerton’),” in *Kölner Papyri (P. Köln), Vol. VI* (Cologne: Rheinisch-Westfälischen Akademischer Wissenschaften unter Universität Köln, 1987), 136–45. See now Tobias Nicklas’ thorough discussion, “The ‘Unknown Gospel’ on *Papyrus Egerton 2 (+Papyrus Cologne 255)*,” in *Gospel Fragments*, eds. Thomas J. Kraus, Michael J. Kruger, and Tobias Nicklas (Oxford: Oxford University Press, 2009), 11–120.

²⁶ Nicklas, “The ‘Unknown Gospel’,” sets out the various similarities, 28–30, 44–52.

²⁷ So, e.g., Nicklas, “The ‘Unknown Gospel’,” 39; and also John W. Pryor, “Papyrus Egerton 2 and the Fourth Gospel,” *ABR* 37 (1989): 9 (1–13). Cf. Francis Watson, *Gospel Writing: A Canonical Perspective* (Grand Rapids: Eerdmans, 2013), 286–340, who mounts an ingenious, but to my mind unpersuasive, case that the GJohn was dependent on the UG.

The author of the UG [Unknown Gospel] thus does not deal slavishly with Johannine texts; he does not simply copy them, but he uses Johannine wordings and sentences to develop his own theological ideas in a new composition.²⁸

I suggest that P.Egerton 2 may give us an analogy for the possible dynamics that produced the PA in the form that we have in GJohn. The core story in the PA may well have been earlier, and there may even have been one or more earlier versions in writing, but the PA that we know shows obvious connections with GJohn in vocabulary and some phrasing. P.Egerton 2 shows, however, that resemblances to Johannine language do not signal common authorship.

Heretical Reflections

In the interest of trying to contribute something of my own, particularly where we might go from here, I also want to raise a wider topic of which the PA is illustrative. I have not yet had the opportunity to ponder this matter further, or to have the benefit of critical response from fellow scholars, and so I offer the following reflections tentatively and to stimulate further discussion. Specifically, I want to query the commonly shared picture of the history of the textual transmission of NT writings. According to this picture, in the earlier period (second century) copyists supposedly exercised much greater (even “wild”) freedom in changing the texts, with many and even substantial variants being introduced willy-nilly. Then, there followed a later period (fourth century and thereafter) of much greater textual stability and fixity, the texts treated then with much more regard and with fewer textual variants. This picture of the matter hardly requires documentation in scholarship, as it is echoed so often and taken for granted.²⁹ But the awkward question I raise here is whether this construct really reflects the manuscript evidence. I am not the first to do so, but it has not obtained the attention that I think it justifies.³⁰ I will be brief in what may be regarded as a heretical line of reflections.

Let us start by noting again that none of the most sizeable textual variants is witnessed in the earliest NT manuscripts. The absence of testimony to the PA in the earliest extant manuscripts of GJohn is illustrative of this. As noted already (and in other essays in this volume), the earliest manuscript support for the PA as a part of GJohn is in the fifth century

²⁸ Nicklas, “The ‘Unknown Gospel’,” 41.

²⁹ See, e.g., the summary of this picture in Ehrman, *Orthodox Corruption*, 28–29, reflective of the view of a number of other scholars as well.

³⁰ See, esp. Wisse, “The Nature and Purpose of Redactional Changes in Early Christian Texts,” who somewhat similarly queried the commonly-held picture of the early textual history of NT writings.

and later.³¹ Likewise, the earliest manuscripts with the long ending of Mark are from the same period.³² To note yet another example, the earliest (and, thus far, sole) Greek witness to the distinctive “Freer Logion” in GMark 16 is Codex W, again a fifth century (or later) manuscript.³³ Also, in addition to having a goodly number of smaller “singular” variants, Codex W is the only Greek witness that adds Isaiah 40:4–8 in its entirety at the end of Mark 1:3.³⁴ In short, it appears that the most substantial textual variants in the Gospels are first attested, not in the earliest manuscripts where we might expect them, but in somewhat later ones. Of course, it may be replied that these later manuscripts only preserve variants that originated much earlier and circulated in earlier manuscripts that happen not to have survived. Perhaps. But how do we know this? Granted, some variants such as we have now in Codex Bezae seem to be cited by earlier Christian writers, e.g., sayings of Jesus not otherwise known, but there is scant indication that these writers knew them as part of a copy of any of the canonical Gospels (and in some cases they are specifically identified as part of some non-canonical writing).

These (merely illustrative) data prompt my awkward question of whether the standard picture of the early history of the text of the NT is as sound as we typically assume. To pursue related questions, instead of that standard picture, is it possible that the process of the creation and dissemination of textual variants, including particularly major ones, went on vigorously far longer and later than we usually presume, well into the Byzantine period? Moreover, is it possible that we have not accurately grasped the dynamics at work particularly in the production of major, intentional textual variants such as the PA, and the factors that made them (later) widely accepted and a part of what became the “majority text” of the NT?

But let us presume for the sake of discussion the common notion that all, or most, of these major variants arose early. So, let us accept provisionally Chris Keith’s proposal that

³¹ In his recently published analysis of Codex Alexandrinus, W. Andrew Smith agrees that it probably did not contain the PA: *A Study of the Gospels in Codex Alexandrinus: Codicology, Palaeography, and Scribal Hands* (Leiden: Brill, 2014), 60–64. Granted, as noted by Keith and others, Ambrose (ca. 339–397 CE) seems to have read the PA as part of the GJohn, and it is commonly assumed that Jerome (ca. 342–420 CE) did so as well, as the PA is part of GJohn in the Vulgate.

³² To reiterate a previous note, the few fragments of GMark extant from \mathfrak{P}^{45} do not permit to judge whether this copy of GMark did or did not include 16:9–20.

³³ Codex W is still presumed to be fifth century, but cf. Ulrich Schmid, “Reassessing the Palaeography and Codicology of the Freer Gospel Manuscript,” in *The Freer Biblical Manuscripts: Fresh Studies of an American Treasure Trove*, ed. Larry W. Hurtado (Atlanta: Society of Biblical Literature, 2006), 227–49, who contends that a sixth-century date (or later) is just as likely.

³⁴ In a book some decades ago I gave a brief analysis of 134 “singular” readings of Codex W in GMark: Larry W. Hurtado, *Text-Critical Methodology and the Pre-Caesarean Text: Codex W in the Gospel of Mark* (Grand Rapids: Eerdmans, 1981), 67–84.

the PA was inserted into a copy of GJohn initially sometime in the late second or early third century.³⁵ If so, to judge by the manuscript evidence to hand (esp. \mathfrak{P}^{66} , \mathfrak{P}^{75} , B and \mathfrak{K}), this insertion apparently did not obtain wide duplication and acceptance for some time. Yet, from approximately the fifth century onward, the PA made marked progress in commending itself as a proper/authentic portion of the GJohn. This suggests to me that, whenever the PA may have been inserted initially into some copy of GJohn, there were factors *in this later period* that were influential in *securing* its place in GJohn thereafter.³⁶ That is, there seem to have been influential factors *then* that were not so influential in the *earlier* period (second-third centuries).

So, as the final part of my heretical reflections, I offer very tentatively a hypothesis for critical scrutiny. I suggest that the second and third centuries were *not* a time significantly more “wild” in the textual transmission of NT writings than the fourth-fifth centuries. Accidental and intentional variants arose from the earliest period, to be sure, but no more frequently and of no more consequence than in the somewhat later period. Indeed, perhaps in the second and third centuries textual variants were less consequential (or at least less successful) than they were later. It seems to me consistent with this suggestion that, for example, the distinctive kind of text that we have (later) in Codex Bezae, particularly the many sizeable expansions, is not found in second and third-century witnesses.³⁷

Perhaps in the early post-Constantinian centuries, there were concerns among some Christian circles and/or copyists about preserving (and perhaps incorporating into canonical texts) variants that were judged useful, edifying, worth keeping, and treating as part of scripture. Maybe this was what we might think of as an expansive attitude of sorts, and perhaps it developed particularly in a time and context when Christianity was becoming the official and dominant religion of the Empire, when it may have seemed, at least to some, that the Christian scriptures should confidently incorporate such variants as the PA, rather than

³⁵ Chris Keith, *The Pericope Adulterae, the Gospel of John, and the Literacy of Jesus* (NTTSD 38; Leiden: Brill, 2009), 203–56.

³⁶ As Keith notes in his contribution to this volume, the several other places where the PA appears in the Gospels were never influential.

³⁷ E.g., J. N. Birdsall, “The Western Text in the Second Century,” in *Gospel Traditions in the Second Century: Origins, Recensions, Text and Transmission*, ed. William L. Petersen (Notre Dame/London: University of Notre Dame Press, 1989), 3–17. Barbara Aland proposed a three-stage development of the so-called “Western” text, the earlier stages comprising much more modest kinds of variants: “Entstehung, Charakter und Herkunft des sog. westlichen Textes untersucht an der Apostelgeschichte,” *ETL* 62 (1986): 5–65. Cf., however, Carroll D. Osburn, “The Text of the Pauline Epistles in Hippolytus of Rome” *Second Century* 2 (1982): 97–124; and Bart D. Ehrman, “Heracleon and the ‘Western’ Textual Tradition” *NTS* 40 (1994): 161–79, who posit readings in these authors that agree with readings found in “Western” text witnesses. But, again, these appear to be rather modest variants, perhaps more accurately illustrative of a slightly freer copying such as we see, e.g., in \mathfrak{P}^{45} . The term “Western text” is a wax nose, and we need to be clear always what we mean!

reject them or allow their status to continue contested and uncertain. Is the inclusion of 1 Clement and 2 Clement in Codex Alexandrinus yet another expression (albeit less widely imitated) of what I mean by an expansive attitude reflected in witnesses in the fifth century and thereafter?³⁸

To be sure, the commonly-accepted schema of the early textual transmission of NT writings seems intuitively correct, given the other indications that second-century Christianity was diverse and lacking the ecclesiastical structures of the post-Constantinian period. But, to put the question directly, does our widely-assumed historical schema of early textual transmission of NT writings rest too heavily on its intuitive appeal and insufficiently on the actual evidence, especially the evidence of manuscripts, which is our most direct indication of that early textual transmission?³⁹

Conclusion

On the central question addressed in this volume, given my admitted predisposition, perhaps it is not so surprising that I judge the matter as I have, finding the PA much more likely an insertion into the GJohn at some secondary stage (whether earlier or later) of textual transmission. This means that I find the cases made by Wasserman, Knust, and Keith persuasive. I hope, however, that I have shown my judgment to be shaped by my methodological commitments rather than by any particular prejudice for or against the PA itself.

I hope also that my effort to contribute to the discussion of the PA and the wider topic of the textual transmission of NT writings will be of some modest value. Even though at this point it is little more than posing a few awkward questions, I hope that it may stimulate further critical engagement with this important matter.

³⁸ On the early reception of 1 Clement, which seems to have been treated as scripture by some, see, e.g., the brief discussion (with references) by Michael W. Holmes, *The Apostolic Fathers: Greek Texts and English Translations, Third Edition* (Grand Rapids: Baker Academic, 2007), 38. I know of no indication, however, that 2 Clement was treated as scripture (e.g., read liturgically). On Codex Alexandrinus, the key work now is Smith, *A Study of the Gospels in Codex Alexandrinus*.

³⁹ I acknowledge a somewhat similar schema offered for earlier and later copies of some classical texts, as discussed by E. G. Turner, *Greek Papyri: An Introduction* (Oxford: Oxford University Press, 1980), 100–12. But Turner's discussion is based on the relevant manuscript evidence, comparing early Ptolemaic literary papyri with copies of the same texts from the Roman and medieval period. My plea is that our schema of NT textual transmission should likewise be based more on manuscript evidence.