

The Yarbro Collins Pre-Markan Passion Narrative (2007)
Critique 1 (Mk 14:32-41)
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Prolegomena. It seems to have been Karl Ludwig Schmidt (see the survey in Yarbro Collins **Beginning** 92f) who in 1919 first proposed the idea that the Markan Passion Narrative, Mk 14-16, is based in whole or in part on an earlier document describing the events surrounding the death of Jesus, the rest of Mark being assembled directly from bits of unconnected specific “traditions,” thought by many to be “oral” in nature. Schmidt relies in part on the fact that the Passion narrative in Mark has greater situational specificity than the preceding material. Eta Linnemann has noted that the temporal organization in that part of Mark is to some extent due to its subject. To invent an example, one could relate the Feeding of the Five Thousand and the Gerasene Demoniac stories in either order with little loss of effect, but it will not do to portray Jesus as preaching in the Temple before he enters Jerusalem, or as cleansing the Temple after he is arrested. That part of Schmidt’s argument is thus weak, but the hypothesis remains, and is entertained by serious scholars, and invites consideration by the serious student.

Issues. If a Passion document *and no other connected narrative* preceded Mark, itself the earliest Gospel, then the Passion was the first part of the Jesus story to be reduced to writing. This implies that the Passion story had a special standing in the early Church. It will be obvious that doctrinal issues readily become involved. In the present comments, I try to keep these issues at a distance, and address only the specific literary-critical problem: whether a prior Passion document existed, and if so, what it was like.

Alternatives. Earlier than all PPN theories is the 1904 stratification theory of von Soden (reviewed, with the 1905 theory of Emil Wendling, by Carl Patton in 1913). von Soden finds that two layers run throughout Mark, save for the Passion Narrative, which von Soden considers not to be primitive. Wendling in effect divides von Soden’s second layer into two; he also includes Mk 14-16 within his theory. Wendling’s construct thus amounts, not to a *Pre-Passion* theory, but to a *Proto-Gospel* theory, the prior text being explicitly called *Urmarcus* by Wendling.

The Basic Questions, as it seems to me, reduced to a form in which philology can deal with them, are four: (1) Is Mark an integral authorial text, or is it stratified? We are here concerned with the validity of the methods used to identify strata. If it *is* stratified, then PPN is a possibility. (2) If it has two layers, are both of them “Markan” in some sense, in which case we are dealing with successive revision under the same aegis; or is only one Markan, which implies either a “source” model (an outside original plus later Markan redaction) or an “expansion” model (a Markan text plus later extraneous addenda)? It is the “source” model which corresponds to the PPN theory as usually stated: according to that theory, the Passion Narrative is pre-Markan, and only later addenda should show Markan traits. (3) If stratification exists, is it confined to Mk 14-16, or is it general? If it thus confined, the PPN theory is supported; if it is more general, then something like a Pre-Gospel (PG) theory may be indicated. Finally, (4) Does the oldest stratum in Mk 14-16, if there is one, suggest an *originally independent* document? In simple terms, does it read consecutively? If yes, then the theory of a PPN is supported against the PG alternative.

Procedure. I here take up the most recent representative of the PPN hypothesis: the one set forth in Adela Yarbro Collins, *Mark* (Hermeneia 2007; hereafter AYC), 625f and 635f, and ad loc; she prints a reconstructed text as an Appendix, p819. I will at a few points also consider (1) Grant’s 1957 proposal, based on a synthesis of Dibelius, Bultmann, Lietzmann, Klostermann, Lightfoot, Olmstead, Goguel, Klausner, and Taylor; the passages included in the PPN according to that proposal are 14:1-2, 10-11, 17-18a, 21-27, 29-31, 43-53a; 15:1-5, 21-22, 24a, 25-27, 29a, 32b-37, and 39; (2) Vincent Taylor’s 1959 reconstruction (see his Additional Notes H and J), which divides the Markan Passion Narrative into segments which do and do not exhibit Semitisms of language, and suggests, to Taylor’s surprise, that the former were added to the latter; and (3) Eta Linnemann’s schema in *Studien zur Passionsgeschichte* (1970), for one sample of which see the Supplement below.

I find that at several points AYC’s argument is superior to that of her predecessors, but for a few other details, and on the main question of whether a PPN text existed at all, I reach, in the end, a somewhat different conclusion. In brief, I find that her detection of strata is generally sound, but that the text thus identified is not valid as an independent document, and that equally strong evidence for early and late strata (as AYC herself note at several points in her main commentary) can also be found in the rest of Mark, so that the PPN theory proves inferior to the PG alternative. Further, since Markan signature traits seem to occur in both strata, the Accretional versus the Editorial version of the PG hypothesis seems to be preferable. In a separate Alternative Reconstruction note, I will attempt to outline a solution of Mark along these lines.

