

Four other gospels: review article

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Considerable scholarly attention has been paid in recent years to non-canonical gospel traditions. In this article our Church History Editor, who lectures at New College, Edinburgh, reviews a significant recent work in this area.

J. D. Crossan, **Four Other Gospels** (Winston Press: Minneapolis, Chicago, New York, 1985), 208 pp., £13.95, ISBN 0-86683-959-3.

This book, which is subtitled 'Shadows on the Contours of Canon', is a first response to the challenge thrown down to scholars by Helmut Koester (and also in effect by Richard Bauckham in the fifth volume in the *Gospel Perspectives* series produced by the Tyndale House Gospels Project, *The Jesus Tradition Outside the Gospels*, ed. D.

Wenham, Sheffield, 1985, pp. 369-403, esp. 369-374) to analyze the gospel tradition in primitive Christianity without isolating the canonical gospels from other gospel materials. Crossan's 'four others' are the *Gospel of Thomas*, Egerton Papyrus 2, the *Secret Gospel of Mark* and the *Gospel of Peter*. His method of study is the standard historical-critical one of 'transmissional analysis', as he prefers to call it. A careful reader of this book, he claims, will never see the canonical gospels the same way again. On the dust-cover James M. Robinson endorses this breathtaking claim, while George MacRae describes the volume more circumspectly as a work of 'intriguing speculation leading to new insights'.

Crossan's conclusions fall in with the growing tendency, particularly among American scholars, to regard non-canonical gospel traditions like those embodied in his four as basically independent of the canonical four. He writes for the general reader as well as for the

specialist (and hence includes in each case an account of the manuscripts and their discovery), and cannot avoid resting a good deal of weight on more substantial studies by other writers, although for each of his four he examines in detail one or more 'case studies'. It will be best to deal in turn with his evaluations of his four gospels.

(1) The *Gospel of Thomas* (*GT*) is a collection of sayings (*logia*) of Jesus, many of them very similar to their synoptic counterparts. It was discovered in Nag-Hammadi in Egypt in 1945. Though now in Coptic, it is widely believed to have been compiled in Greek or Aramaic (Syriac) by c. 150 at the latest. In Crossan's judgment, *GT* is completely independent of the canonical gospels, and they of it. Its independence, which does not necessarily mean that it is earlier or 'better' than our four, is accepted by an increasing number of scholars (but not by Craig Blomberg in his study of the parables of *GT* in *Gospel Perspectives* 5, pp. 177-205), but Crossan cannot be regarded as having strengthened the case for it. One of his two general arguments maintains that the apparently random sequence of sayings in *GT*, which has no compositional order at all, would be inconceivable if *Thomas* had derived them from our gospels. This is hardly convincing, partly because some elements of compositional design are evident in *GT* (e.g. keyword-linkage more significant than the trivial instances Crossan mentions, and more extensive than is normally allowed, if linkage by words like 'Lord' and 'many' is included, as Crossan implies it should be; and thematic connection, as in logion 33, which Crossan cites to prove his point about order), and also because Crossan's argument seems likely to count equally against *GT*'s dependence on any conceivable earlier (pre-canonical) collection of gospel traditions.

His second general reason argues that *GT* contains very little, if any, of the synoptists' redactional material. His example is logion 54, 'Blessed are the poor, for yours is the kingdom of heaven'. Whether it is easier to believe that *Thomas* was 'mentally unstable' than that he is here dependent on Luke (and perhaps Matthew) is at least open to question, especially when we remember, as Crossan does later, that *GT* hardly ever uses 'God' at all, and perhaps never in a good sense.

Crossan's two case studies are of logia 64 (the Great Banquet) and 65 (the Wicked Husbandmen). The latter has been recognized from the earliest days of *GT* study as offering one of its strongest cases for a logion more primitive in form than its parallels in the synoptic gospels (but see Blomberg, *op. cit.*, pp. 189-190). At the same time the former appears one of the most obvious examples of tendentiously secondary development in *GT*, with its rejection of all forms of business and mercantile activity and perhaps marriage also. Crossan recognizes this, but sees the versions in Matthew and Luke as similarly developed interpretations of the original parable of Jesus, in both cases as 'allegories of Christian history' including the mission to the Gentiles. This is, however, much less evident in Luke than in Matthew, and Luke's version is also much closer than *GT*'s to the original form of the parable as generally reconstructed.

Nevertheless, Crossan's conclusions about *GT* would be among the least controversial in the book, were it not for his over-all assessment of the work. To assert that *GT* is 'what Jewish wisdom theology looks like after it has heard Jesus' message about the Kingdom of God' leaves out of account altogether *GT*'s advanced asceticism, which views the differentiation of the sexes as, in effect, the fruit of a fall to be overcome in the kingdom. Crossan leans too much on Stevan L. Davies' interpretation of *GT* in terms of a speculative wisdom theology. It is at best a misleadingly careless statement that *GT* uses 'Kingdom in place of the term Wisdom'; of scarcely one or two of *GT*'s many uses of 'Kingdom' is this at all plausible. Furthermore, Crossan is inaccurate in affirming that, in its understanding of Jesus as divine wisdom, *GT* 'either does not know or does not need any of those other titles used for Jesus elsewhere in early Christianity', for *GT* does use Son of Man, Lord and the Son (absolutely, alongside Father and Holy Spirit). Finally, if *GT* belongs to the borderlands between gnostic and catholic Christianity, was it ever hypothetically redeemable by some therapeutic 'Epistles of Thomas', 'just as the Pastoral Epistles redeemed Paul from such as the *Acts of Paul* and the Johannine Epistles redeemed John from such as the *Acts of John*'? Such a suggestion not only raises mind-boggling implications for the chronology of primitive Christianity but also fails

to give due weight to *GT*'s radical rejection of the divine creation of man and woman.

(2) The gospel fragments known as Egerton Papyrus 2 (EgP) are among the very earliest of all Christian manuscripts. Unearthed in 1934-5 and now in the British Library, the four scraps of Greek come from Egypt (probably Oxyrhynchus) and should probably be dated earlier than AD 150. Crossan goes into some detail on the make-up of a papyrus codex, but fails to redeem his promise to bring out the relevance of this discussion to his subsequent case study.

Again Crossan concludes that this 'Unknown Gospel' (as it has long been called) is independent of all the canonical gospels. Furthermore, it belongs to a stage prior to the separation of the synoptic and Johannine traditions, each of which may be dependent upon it, if there is any relation of dependence at all. I have re-examined the case for EgP's independence of our gospels in *Gospel Perspectives* 5, pp. 210-221 (cf. Bauckham, *ibid.*, pp. 377-378, 399 n. 5), and the reader is referred thither for a refutation by anticipation of much of Crossan's argumentation. I will confine myself here to a few selective comments.

Crossan repeats Koester's objection to the conclusion of Jeremias and others that EgP is dependent on our four gospels, namely that this would make EgP 'a spectacularly early witness for the four-gospel canon', but such a statement is doubly unfortunate. It forgets that one fragment of EgP reflects an obviously non-canonical miracle tradition, so that its dependence goes beyond the four gospels, and secondly, its alleged use of our four would not necessarily imply their canonical status at the time.

Crossan's case study focuses on the question about tribute (Mk. 12:13-17 par.; EgP lines 43-59, which probably break off incomplete. In this instance Crossan appears to regard the parallel in *GT* logion 100 as dependent on the Markan or broadly synoptic version.) In claiming that Mark is here a rephrasing of EgP, Crossan provides only one argument of detail – Mark's relocation (to 7:6-7) of the accusation in EgP lines 54-59 drawn from Is. 29:13. Two considerations persuade him that this is a correct account of the relationship: first (if I have read the somewhat elusive sequence aright), that the introduction 'Well did . . .' (καλῶς) to the Isaiah text is uncharacteristic of Mark; and second, that Mark's omission of 'with their mouth' from Is. 29:13 is without rhyme or reason unless he found it in that form, as he could have done in EgP. But Crossan has failed to notice that the omission of 'with/in their mouth' is a variant of the LXX text itself (missing in Codex Sinaiticus and Codex Alexandrinus). In any case, no special explanation is needed for an incomplete OT quotation such as this. To Crossan's first point it may be rejoined that we have no evidence to judge whether 'Well did . . .' was characteristic of EgP either, and, in any case, can it be truly said to be uncharacteristic of Mark in the light of 7:37 and especially 12:28,32? What can be affirmed with confidence is that Is. 29:13 fits in much better in Mk. 7 than in the tribute question in EgP, where Jesus' accusation seems out of place, for there has been no word of Jesus for his questioners to be faulted for disobeying. This comment does not apply in Mk. 7, where it is obvious that the main point of the citation is the last phrase of Is. 29:13; ' . . . teaching as doctrines the precepts of men'. A textual comparison here shows conclusively that Mark could not have got his version from EgP but is in fact very close to the LXX. Crossan's argument thus misses the wood for the trees.

(3) The *Secret Gospel of Mark* (*SGM*) is known of only from a letter ascribed to Clement of Alexandria which was found by the American scholar, Morton Smith, in 1958 in the monastery of Mar Saba not far from Bethlehem. The manuscript was written in the eighteenth century. The letter warns against the abuse by the heretical Carpo-cratians of Mark's 'more spiritual' amplification (*SGM*) of his own gospel. Crossan accepts as a working hypothesis the authenticity of the Clementine letter, as have many, perhaps most, scholars qualified to judge, but he recognizes that a cloud of uncertainty will hang over the question so long as no other scholar is able to examine the original manuscript. The caution of this judgment, however, stands in stark contrast to the adventurous claims of the rest of Crossan's discussion of *SGM*. He concludes that 'canonical Mark is a very deliberate revision of *Secret Mark*', made necessary by the Gnostic

Carpocratians' misuse of parts of *SGM*. This is similar to Koester's view, but it is much weaker than it, for, unlike Koester, Crossan sees no need to posit a Proto-Mark used by Matthew and Luke. This means that, for Crossan, our Mark did not exist until c.120 or so at the earliest, for Irenaeus dates Carpocrates to this time, as a contemporary of Basilides and others (Eusebius, *H.E.* 4:7-9). Crossan unfortunately nowhere raises questions of dating.

His main argument for so bold a conclusion is that canonical Mark dismembered two passages in *SGM* and distributed the textual debris at various locations in the gospel. . . . Those dismembered fragments have kept ancient and modern interpreters puzzling over their meaning in canonical Mark.' Matthew and Luke independently found it necessary to depart from Mark's text at these points.

The credibility of this supposition rests or falls with the details of the case. According to the Clementine letter, *SGM* read, between Mk. 10:46a and 46b, 'And the sister of the youth whom Jesus loved and his mother and Salome were there, and Jesus did not receive them.' (Since it is hard to see how the Carpocratians could have made tendentious use of such a sentence, Crossan deduces that *SGM*'s text must have been more extended.) 'The youth whom Jesus loved', according to Crossan, was redistributed by Mark to 10:20-21, and 'Salome' to 15:40 and 16:1. Questions abound: What have the youth's sister and mother done not to deserve redistribution? Is 'Jesus loved him' in Mk. 10:21 any less 'dangerous' than the phrase in *SGM*? Is there any evidence that 'ancient and modern interpreters' have puzzled over the meaning of Mk. 10:20-21 — to say nothing of the two mentions of Salome?

From the longer misused passage in *SGM* (a version of the raising of Lazarus, it seems) Mark 'scattered . . . over the rest of the gospel' the following 'textual debris': Bethany to 11:1 (Luke obviously found no difficulty with Mark's debris); "Son of David, have mercy on me." But the disciples rebuked her' to 10:47-48 (but why the repetition of 'Son of David. . . .?' What was Mark's text before this redistribution? — the appeal makes better sense in Mk. than in *SGM*, where it is uttered by [Lazarus'] sister); 'rolled away the stone from the door of the tomb' to 16:3; 'youth' to 16:5; 'raised him, seizing his hand' to 1:31, 5:41, 9:27 (although Mark uses this 'almost as a stock phrase', Crossan does not regard this as evidence of its originality in Mark — contrary to his reasoning on 'Well did . . .' in EgP above; he also is wrong in claiming its omission from Mt. 8:15, the parallel to Mk. 1:31); 'began to beseech him that he might be with him' to 5:18; 'the youth, looking upon him, loved him . . . for he was rich' to 10:17-22 (the incident with the rich young man); 'came into the house' to 1:29, 2:15, 3:20 ('Mark intends to set up a rhythm of calling/visiting so that the case in *SGM* 2 is no longer anything particularly special' — but Crossan has here to admit that Matthew and Luke found no difficulty with Mk. 1:29 and 2:15; and is their omission of Mk. 3:20 attributable to its 'rather forced' reference to Jesus' being in a house?); 'after six days' to 9:2; 'the youth comes to him, wearing a linen cloth over his naked body' to 14:51-52; 'mystery of the kingdom of God' to 4:11 (why the sole change in Mt. 13:11 and Lk. 8:9, to 'mysteries', should be viewed as a 'more expected format' is not made clear); 'the other side of the Jordan' to 10:1 (Crossan's reference here to Lk. 9:51 is mistaken; the awkward element in Mark here is 'and' [which he did *not* get from *SGM*] which Mt. 19:1 omits).

Crossan's comment on the last of these Markan relocations says it all: 'for Mark it was a simple question of storing some *SGM* debris somewhere safe!' He nowhere attempts to explain why Mark felt it necessary to scatter around *only these elements of SGM* debris. Why, for example, did 'going out of the tomb' not find a home elsewhere in canonical Mark, and likewise 'remained with him that night', to say nothing of the actual death and resurrection of the beloved youth? This is a crucial lacuna in Crossan's argument in view of his assertion that Mark's reason for retaining the dismembered fragments was 'to offset future Carpocratian usage' by being able to show that their favourite passages in *SGM* were later compilations built out of such fragments. Moreover, Crossan never faces the larger questions his theory provokes about the composition of Mark. He believes that *SGM* and canonical Mark were both produced by 'the same author or school'. Since canonical Mark is anti-Carpocratian and therefore not pre-Carpocrates, the composition of *SGM* cannot be dated before the

early 2nd century. Since Crossan is not sure (contrary to the clear implication of Clement) that *SGM* contained passion and resurrection narratives, where did 'the same author or school' get these from for canonical Mark c.125 in Alexandria?

Furthermore, the use Matthew and Luke made of Mark is strictly irrelevant to the pre-history of Mark itself. In fact, Crossan has considerably overstated their deliberate divergence from Mark's supposedly redistributed debris, and in any case, Crossan accepts that 'they found difficulties . . . with many other themes and topics in Markan theology'.

It may be sufficient to summarize and question such a hypothetical construction for it to start shaking alarmingly. We may bid it goodbye by noting how selectively Crossan chooses to use the Clementine letter. He rejects altogether its corroboration of the tradition of a Petrine basis to Mark's gospel, and its account of Mark's expansion of his original gospel into 'a more spiritual Gospel'. (Koester takes these items more seriously.) On the other hand, he takes with great seriousness Clement's account of the contents of *SGM*, making it in effect the fountain-head of the Markan gospel tradition, despite (a) the very limited information Clement gives us about it; (b) the complications for his (Crossan's) hypothesis of what Clement does tell us about it (Clement's summary statement of the additions that made *SGM* different from public Mark ['to the stories already written he added yet others and, moreover, brought in certain sayings. . . .'] must provoke us to ask of Crossan how much more of our Mark may consist of *membra disiecta*, like unsuspected nuclear waste, from *SGM*?); (c) obviously secondary features in the two special passages of *SGM* Clement reports (e.g. the great cry heard from the tomb before Jesus has rolled away the stone; the conversation the youth has with Jesus even before they have left the tomb; Jesus' 'not receiving' the women at Jericho — unparalleled in the gospels' picture of Jesus' relations with women. Cf. J. A. T. Robinson's account of *SGM*'s Lazarus story as 'a much inferior tradition . . . historically pretty worthless' [*The Priority of John*, London, 1985, p. 22]).

The Clementine letter certainly calls for further study. It is perhaps a reflection of persistent doubts about its authenticity that it remains relatively neglected. (For example, if from Clement himself, it provides us with our earliest evidence of the tradition linking Mark with Alexandria. Furthermore, what it says about the production of Mark's gospel, even down to details of vocabulary relating to the material aspects thereof, suggests a fascinating tie-up with the arguments of C. H. Roberts in particular about the early Christian adoption of the codex form of the book. See my review in *History* 69 (1984), pp. 443-444, of Roberts and T. C. Skeat, *The Birth of the Codex* [Oxford, 1983].) Even the translation is not always clear, as is obvious if one compares that of F. F. Bruce in *The 'Secret' Gospel of Mark* (London, 1974) with Morton Smith's which Crossan uses. Meanwhile, however, Crossan's account of its significance, which, one must stress, is considerably more vulnerable than Koester's, seems acceptable only on the Tertullianic principle *credibile quia ineptum*.

(4) The headiest wine of all Crossan reserves for last, for he believes that the Passion-Resurrection Source which in his view constitutes most of the *Gospel of Peter* (*GP*) is not only independent of our four gospels but was in fact used by all four. He also holds that the present *GP* is a composite text, incorporating short units derived from our gospels.

The *Gospel of Peter* is more recognizably a 'gospel' than any of Crossan's other three candidates, although all that survives is an account of the passion and resurrection of Jesus. Until a few years ago (see below) its sole manuscript was one dating from the eighth or ninth century discovered at Akhmim in Egypt in 1886-7. I have dealt with aspects of this work elsewhere (cf. the essay referred to above, and also 'Apologetic and Apocalyptic: The Miraculous in the *Gospel of Peter*', in *The Miracles of Jesus [Gospel Perspectives 6]*, Sheffield, 1986), eds. D. Wenham and C. Blomberg, pp. 401-418; cf. also 'Papyrus Egerton 2 (the 'Unknown Gospel') — Part of the *Gospel of Peter*?' forthcoming in *The Second Century*). Nevertheless, certain comments are called for here.

(a) The *GP* that Crossan discusses is the text in the Akhmim manuscript. Although he records the recently discovered Oxyrhynchus

Papyrus 2949, which consists of a fragment of *GP* and dates from c.200, he nowhere assesses the significance of its considerable divergences from the much later Akhmim text (see my essay in *Gospel Perspectives* 5, pp. 222-225). The most obvious inference to draw is that the Akhmim text of *GP* is a markedly developed one, and cannot any longer with much confidence be presumed to present *GP* in its original form. The failure to consider the possible implications of this new papyrus, whose dating is not in doubt, is remarkable in a volume characterized by the boldest hypothetical reconstructions of textual relationships.

(b) One general effect of Crossan's account of the substantial priority of *GP* to the canonical gospels is that the most miraculous resurrection narrative of all becomes the earliest of the five. It is marked by (i) the heavenly descent and entry into the tomb of two men enveloped in light; (ii) the stone rolling back of its own accord; (iii) the exit from the tomb of the two, supporting or escorting a third figure (the two as tall as, and the third taller than, the heavens), followed by the cross; (iv) the cross responding 'Yes' to a question from heaven, 'Have you preached to those who sleep?'; (v) a further descent of a man from heaven and his entry into the tomb; (vi) this resplendent young man's message to the women, from his seat in the tomb, that the one who was crucified had risen and departed whence he had been sent.

(c) Only the last of these elements is clearly paralleled in the four gospels. (There may also be a partial parallel between (i) and (ii) and Mt. 28:2.) Crossan recognizes that very little of *GP*'s narrative is reflected in the canonical accounts, but holds that parts of it have left their mark elsewhere. Thus Luke's 'two men' at the Transfiguration, empty tomb and ascension (9:30; 24:4; Acts 1:10) are said, without any evidence being advanced, to derive from *GP*. Mark in turn has transferred to his Transfiguration story the two men (who become his Elijah with Moses in 9:4), the heavenly height of the three (which becomes in 9:2 'led them up a high mountain' — a tall stor(e)y indeed!), and the great brightness and the heavenly voice (despite the sole linguistic parallel for either item being Mark's and *GP*'s common use of *φωνή* — scarcely the most distinctive of Greek words!).

Crossan frankly admits that these parallels are not 'very persuasive in themselves', and that 'it is very easy to dismiss the very idea of such a relocation and reinterpretation'. What predisposes him to accept it is Mark's relocation of the centurion's confession from a post-resurrection context in *GP*, subsequent to the happenings (i)-(v) in paragraph (b) above, to a crucifixion context (Mk. 15:39). Crossan believes that its position here is a clear instance of Markan redaction in the interests of his passion Christology. What he does not consider is how well the confession's form and location in *GP* fit in with its blatantly obvious tendencies (on which see my essay in *Gospel Perspectives* 6). These include the concern that the resurrection should take place before the eyes of the world — elders and scribes, the centurion Petronius and his troops and 'a crowd from Jerusalem'. Hence the confession has become a corporate one by the centurion and his company, as part of their report to Pilate, who himself shares their confession ('I am pure of the blood of the Son of God', 11:46) before commanding them to silence. This he does at the request of 'all', apparently another body of people who have also arrived at Pilate's door, where they profess their preference to 'incur the guilt of very grave sin before God' rather than be stoned at the hands of 'the people of the Jews'. Who are these 'all'? In Crossan's view, the Jewish authorities were part of the centurion's company who reported to Pilate, but he also identifies them with the 'all', even though the latter's arrival *chez* Pilate is clearly later than that of the group around the centurion. The incoherence here in *GP*, which Crossan fails to identify, is a consequence of the extraordinary lengths to which *GP* presses its apologetic *Tendenz*. In this light, relocation of the confession by Ps-Peter, prompted perhaps by its form in Mt. 27:54, is at least as probable as relocation by Mark.

(d) One final illustration of Crossan's questionable interpretation of relationships within the gospel tradition is provided by his making Matthew's version of the setting of the guard at the tomb (27:62-66) a variant of *GP* 8:28-33. The reason for the request in Matthew is the fear, occasioned by the memory of Jesus' prophecy that he would rise after three days, lest his disciples steal the body and fraudulently

declare his resurrection. *GP*'s reason is much more complex: the Jewish authorities, on learning that 'all the people' were lamenting and repenting of what had happened, took fright and asked Pilate for a guard for the tomb lest the disciples remove the body, the people believe that Christ was risen and then 'do us harm'. Crossan draws out the differences between the two accounts, but offers virtually no evidence for regarding *GP* as the original. He thinks the fact that Matthew locates the request on the Saturday was 'not very wise, since the tomb was then unguarded one whole night'. *GP*, he thinks, places it on the Friday. In its present form it does not, but Crossan holds that 7:27 ('we fasted . . . until the sabbath') belongs to the redactional additions made to the Passion-Resurrection Source by the compiler responsible for the final shape of *GP*. In any case, is *GP*'s 'failsafe apologetic' (cf. W. L. Craig, 'The Guard at the Tomb', *NTS* 30, 1984, pp. 273-281) not more likely to be secondary than Matthew's allegedly unwise failure to cover the Friday/Saturday night?

Furthermore, *GP*'s coherence becomes vulnerable on closer inspection. At 7:25 it is 'the Jews and the elders and the priests' who realize what great evil they had done themselves and begin to 'beat themselves' and cry woe, whereas at 7:28 'the scribes, the Pharisees and the elders' meet after hearing that 'all the people' were murmuring and 'beating their breasts' (Crossan's translations obscure the repetition of *κόπτοιμαι*). The solidarity of people and leaders at 7:25 dissolves into the polarization of 7:28 and sequel. Moreover, where does *GP*'s interest in having the tomb watched 'for three days' come from, if not from the tradition's memory of the prophecy found in Mt. 27:63? Yet again, why should the people suppose Jesus had risen from the dead merely because the disciples had stolen the body (*GP* 8:30)? Surely *GP* here also reflects a tradition that referred explicitly to some expectation or forewarning of resurrection, or at least linked the absence of the body to the people's belief by a mention of the disciples' proclamation of his resurrection. Moreover, if the people came to believe that Jesus was risen, why should the Jewish leadership fear harm at their hands? *GP*'s references to the leaders' own contrition (7:25; 11:48 — see above), albeit inconsistent with 8:28ff., suggest that they should themselves have welcomed the resurrection of Jesus and readily acknowledged their error in promoting his death. Once it is recognized that the reason given in *GP* 8:30 for wanting the tomb to be guarded makes little sense unless it reflects an earlier, fuller tradition (e.g. Mt. 27:63f.), its complexity is further compounded and its secondary character even more evident.

Nor should we fail to note the contrast between Matthew's brief and restrained mention of the sealing of the tomb (27:66) and *GP*'s elaborate description (8:31-33): the elders and the scribes went with Petronius and his troops to the tomb, rolled the stone across the entrance (why had this not taken place at the burial? — obviously in order that it might be done now by Jews and Romans together, thereby strengthening the 'failsafe apologetic' of *GP*), affixed seven seals, set up a tent and posted guard. What price *GP*'s originality over Matthew at this point?

(e) Crossan distinguishes, as we have seen, between the original Passion-Resurrection Source, comprising most of our *GP*, and some elements taken from our gospels, and also the redactional links provided by Ps-Peter himself to facilitate the integration of these elements. The Passion-Resurrection Source presumably cannot be ascribed to the same writer as the second and final stage in the production of *GP*. Crossan does not touch on this question explicitly, but his theory implies a considerable interval between the two stages, during which the Passion-Resurrection Source enjoyed sufficient currency to be used in the compilation of all four canonical gospels and they in turn enjoyed sufficient currency to be all used by the final Ps-Peter. This last stage can hardly have been much later than c.150 (from the implications of Eusebius's report of the unmasking of *GP*) — which makes *GP*, to avoid coining a phrase, 'a spectacularly early witness for the four-gospel canon'! (Crossan accepts a date c.150 for Egerton Papyrus 2. Scholars have repeatedly noted parallels between *GP* and Egerton's 'Unknown Gospel'. Cf. my article forthcoming in *The Second Century*, referred to earlier.)

Since Crossan's hypothesis supposes two main contributors to *GP* (plus identifiable dependence on the canonical gospels in some

verses), one might expect some differences of vocabulary and style between them. This is another issue Crossan does not tackle. Although he might conceivably argue that Ps-Peter assimilated his redactional links to the style and language of the Passion-Resurrection Source and/or his borrowings from the canonical gospels, a comparison is one of the tests that his hypothesis invites. This is not the place to embark on a detailed exercise, if only because it is complicated by the fact that one of Crossan's redactional connections (2:3-5a) coincides very largely with the main fragment of P Oxy 2949, which has a divergent text (e.g. it lacks *σταυρίσκειν* of 2:3, one of *GP*'s two hapax legomena). But a preliminary enquiry reveals the following points: (i) Ps-Peter (i.e. the final compiler of *GP*) uses only *GP*'s characteristic designation of Christ, 'the Lord', at 2:3; (ii) his use of *ἐκεῖνος* (9:37; 11:43) to create a clear connection with what has gone before, also accords with *GP*'s usage (4:13; 10:38; 12:52; 13:56); (iii) Ps-Peter agrees with *GP* in the frequency of its use of *καί* as a conjunction — proportionately far more often than in any of the canonical gospels; (iv) some unusual words are used by our putative Ps-Peter (e.g. *τιτρώσκω*, *κρύβω*, 7:26; *ἐπιχωρέω*, 9:37; *συνσκέπτομαι*, 11:43), but the same is true of *GP* as a whole (see my study in *The Second Century* for a full listing); (v) while a range of vocabulary is shared by Ps-Peter and the rest of *GP* (e.g. *μνήμα*, *κυλίω*, *ἐπιφώσκω*), there is only one possible candidate for a distinctive Ps-Petrine vocabulary: *διανοέομαι* (11:44), which is not used in the NT, and *διανοία* (7:26), which is, but very rarely. There is, it seems, little to be uncovered by this line of enquiry to support Crossan's analysis of *GP*'s text.

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A colleague of mine used to play chess with John Dominic Crossan. He remembers that his opponent tended to fall foul of the very

ingenuity of his own (Crossan's) elaborate schemes of play. What shall we say? Plus ça change . . . ? *Si parva licet componere magnis?* Eschewing the temptations offered by the language of gambit and checkmate, we merely observe that the gauntlet thrown down by Koester and Bauckham deserves to be taken up more happily than by this player.

Meantime, the traditional evaluation of the apocryphal gospels ought not to be hastily discarded, even if it requires qualifications, particularly in the light of the *Gospel of Thomas*. One or two of them may deserve to be regarded as later contemporaries of the latest canonical gospel, and the possibility that a handful of them may preserve primitive traditions independent of the canonical gospels or their sources cannot be discounted, at least for the *Gospel of Thomas*. But *Thomas* is almost in a class of its own (and in any case is a doubtful claimant to the title 'gospel', given its lack of anything but sayings of Jesus), and even Crossan's quartet represents an exiguous selection from a much larger number. On the great majority of these the traditional verdict remains incontestable.

Moreover, even if generalizations about the apocryphal gospels have become more vulnerable, it is still true that the exceptions to the general rule — much later and worthless for the quest of the historical Jesus — leave the big questions about the canonical gospels much as they have always been. To the historical-critical task of assessing the reliability of the canonical accounts, this tiny minority of their apocryphal rivals contributes at best nothing but additional material evidence of the same kind as that already available in the gospels themselves. They pose no challenge beyond those familiar from comparative gospel study, nor furnish new reasons for doubting the historical trustworthiness of their record.