

CLEMENT OF ALEXANDRIA AND SECRET MARK:
THE SCORE AT THE END OF THE FIRST DECADE

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Late Spring of 1982 will see the tenth anniversary of the full publication of Clement's letter and its gospel fragments.¹ Accordingly, this seems an appropriate occasion to review the discussion of them to date. The following review is based on about 150 published items that have come to my attention. No doubt some have been missed, but I trust these are representative.

In brief, when the new material first appeared, there were many reports in newspapers and periodicals. Though some of these were surprisingly accurate and sympathetic, none went much beyond restatement or misstatement of the facts. Then came a swarm of attacks in religious journals, mainly intended to discredit the new gospel material, my theories about it, or both. Besides these, a few relatively objective studies of the material and/or the theories have appeared. These studies have commonly treated only particular aspects of the many problems raised by the new text. As yet, therefore, serious discussion has barely begun, but in a number of areas preliminary positions have been staked out, not only by the objective studies, but also by the attacks. Prejudiced and even malicious papers sometimes cite evidence and raise questions of scholarly importance.

Of the many publications received, I have listed in the bibliography at the end of this article those that seem to me significant for the scholarly discussion. "Significant" is a deliberately vague term; there are several I think worthless but influential. Since almost all are short, and since I shall not want to prove particular points, but only to categorize, I shall cite them merely by the authors' names.

As with any discovery of an ancient text, the first question was that of authenticity. Since the document consists of a letter, allegedly by Clement, quoting fragments said to come from

¹In M. Smith, *Clement of Alexandria and a Secret Gospel of Mark* (Cambridge, MA: Harvard University, 1973) hereafter: *Clement*; abbreviated in *The Secret Gospel* (New York: Harper & Row, 1973). *The Secret Gospel* has meanwhile gone out of print but will be republished in 1982, in paperback, by The Dawn Horse Press, Clearlake, CA 95422.

a gospel by Mark, four positions are possible: (1) Both elements are bogus. (2) Both are genuine. (3) The letter is genuine, but the gospel fragments are not by Mark. (4) The fragments are genuine, but the letter is not by Clement. Variants of these positions can be conceived — for instance, one gospel fragment might be accepted, the other rejected. However, no such variants have been proposed, so none need be considered. Even the second and fourth of the above positions have had, I believe, no defenders and can likewise be passed over. All critics have agreed in choosing one of the remaining two: Either, as I initially proposed, the letter is actually by Clement of Alexandria, though he was mistaken in attributing the gospel fragments to Mark, or the whole thing is a fake.

Of these two, the latter has rarely been maintained. Those who have read either *Clement* or *The Secret Gospel* will remember that when I sent the text with a first draft of my commentary to fourteen outstanding scholars, all but two, Munck and Völker, thought Clement had written the letter. Besides these, A. D. Nock held to his first impression that it was not by Clement. Had Nock and Munck lived to consider the whole of the evidence, I think they might have changed their minds — or mine. At all events, we should have had serious discussion. Of the scholars listed in the bibliography here following, twenty-five have agreed in attributing the letter to Clement,² six have suspended judgment or have not discussed the question,³ and only four have denied the attribution, namely, Kümmel, Murgia, Musurillo, and Quesnell. Quesnell's denial was part of an absurd attempt to prove me the author of the text. Unfortunately, nobody else has had so high an opinion of my classical scholarship. Quesnell, having persuaded himself that I *could* have forged the text, had no difficulty in making up evidence that I *did* do so. For that purpose he simply distorted passages in my earlier works. I must thank Father Clifford, then editor of *CSQ*, not only for permitting me to answer Quesnell, but also for sending me, "in the hope that they will be useful," the comments of two of his associate editors, one of whom wrote, "Smith deserves some redress. I took the trouble to check all *NTR* passages where S. claims Quesnell

²To wit: Beardslee, Brown (?), Bruce (?), Donfried, Fitzmyer (?), Friend, Fuller, Grant, Hanson, Hobbs, van der Horst (?), Johnson, Kee, Koester, MacRae, Mullins, Parker, Petersen, Pomilio, Richardson, Shepherd, Skehan, Trevor-Roper, Trucmè, Wink. Those whose names are followed by question marks accepted the letter's authenticity only as a working hypothesis.

³Achtemeier, Betz, Kolenko, Merkel, Reese, Schmidt.

has misrepresented him, and S. certainly is correct. Q's reading of those passages is so inaccurate as to be irresponsible."⁴

The dissents of Murgia and Musurillo were on a different level. Musurillo wrote a valuable paper on milieux in which an ancient or seventeenth-eighteenth-century forgery *might* have been produced, and on *possible* reasons for producing one; his conjectures are ingenious and learned, but unlikely and unnecessary. By far the simplest explanation of the text is, that it was written where it was found, copied from a manuscript that had lain for a millennium or more in Mar Saba and had never been heard of because it had never been outside the monastery. Murgia, though he fell into a few factual errors, argued brilliantly that the literary form of the new document is one found often in forgeries — a bogus introductory document, commonly a letter, explaining the appearance and vouching for the authenticity of the equally bogus material it presents. This is true, but the same form is often used for presentation of genuine discoveries or material hitherto secret. Forgers use it because it is regularly used. (So too, when they forge wills, they commonly use the standard legal forms, but this does not prove that any will in a standard legal form is a forgery.) In sum, stimulating, but inconclusive. The question has to be settled by the objective evidence, above all the details of literary style. None of these studies contained any substantial argument to show that Clement could *not* have written the letter; they merely suggested reasons for thinking that someone else *might* have written it. By contrast, KümmeI began by crediting Quesnell, and went on to assemble, as evidence against authenticity, an assortment of secondhand trivialities and several substantial objections I had already answered in *Clement*. His reiteration of these, without any attention to the answers or to the linguistic evidence for Clement's authorship, was a disgrace both to the *Theologische Rundschau* and to the objective tradition of German criticism.

In sum, most scholars would attribute the letter to Clement, though a substantial minority are still in doubt. No strong

⁴This part of Quesnell's paper was adapted without acknowledgment by L. Moraldi in an amusing attack on the Italian edition (*Il Vangelo Segreto* [Milan: Mursia, 1977] "Morton Smith col 'vangelo segreto' scandalizzò l'America," *Tuttolibri* 3 (1977) #38 3. In a letter to *Tuttolibri* (947 4) I pointed out M's source and the falsehoods he had himself contributed (he is an original thinker).

argument against the attribution has been advanced, and those few who have denied it have either ignored or resorted to fantastic conjectures to explain away the strong evidence presented in *Clement* from the letter's content and style, which attest *Clement's* authorship. Unless that evidence is faced and explained, no denial that *Clement* is the author should be taken seriously. Meanwhile, the recent "provisional" inclusion of the letter in the Berlin edition of *Clement's* works adequately indicates its actual status.⁵

If *Clement* wrote the letter, the gospel fragments quoted in it must be considerably earlier than his time. Writing between 175 and 200, he speaks of *Secret Mark* as a treasured heirloom of his church. He was not the sort of man to make up such a story. Consequently the latest plausible date for *Secret Mark* is about 150, while *Clement's* report that *Carpocrates* based his teaching on it would push it back to the beginning of the century, prior, at least, to *Carpocrates*, who flourished about 125. This brings it close to the canonical gospels, commonly dated about 70 to 100. The question of their relationship becomes sensitive, and resistance to the thrust of the evidence increases. Moreover, the thrust is difficult to determine. The evidence here is complicated, dull — much of it tables of verbal statistics and lists of variant readings — and apparently self-contradictory. The third chapter of *Clement*, which presents it, is hard reading, and the explanation finally proposed is complex — so complex that most critics did not try to present it, and several of those who did, got it wrong.⁶

Essentially I conjectured that an original Aramaic gospel had been twice translated into Greek; *John* had used one translation, *Mark* another. (This accounts for their agreement in outline, but difference in wording.) Each left out some elements and added many. *Mark* was then variously expanded — by *Matthew*, by *Luke*, and by the author of *Secret Mark*, who imitated *Mark's* style, but added episodes from the old Greek translation, inserting them where they had stood in the original outline. (Hence the *Lazarus* story has the same location, vis-à-vis the outline, in *Secret Mark* as in *John*.) According to *Clement*, the *Carpocratians*, too,

⁵ *Clemens Alexandrinus* (GCS 4/1; 2d ed.; eds. O. Stählin and U. Treu; Berlin: Akademie-Verlag, 1980) xvii-xviii; cf. viii.

⁶ *Donfried, Fuller, Johnson, Merkel, Mullins, Shepherd, Wink.*

got hold of Secret Mark and expanded ("corrupted") it yet further.⁷

In this theory the two most important points are: (1) that Mark and John are both based on a single, earlier gospel; (2) that Secret Mark added to Mark elements from that earlier gospel, rewritten in Markan style. Of the dozen scholars who commented on the theory, about half seemed inclined to accept these points.⁸ Most commentators, however, after declaring the secret gospel later than Mark, went directly to their own theories of its origin. Then thought it a pastiche composed from the canonical gospels;⁹ five thought it a product of free invention like the apocryphal gospels of the second century (they ignored the fact that those differ from it conspicuously both in style and in literary form);¹⁰ five thought it used pre-Markan "floating pericopae" or oral traditions¹¹ — this comes close to the supposition of a pre-Markan gospel, but differs from it by failing to explain the similarities of order and geographical framework between Mark and John. The preponderance of the pastiche theory is due to three factors: (1) It is naively assumed that any occurrence in early Christian literature of an expression found in one of the canonical gospels is to be explained as a borrowing from that gospel. Though everybody pays lip service to the notion of oral tradition, few realize that it constituted a supply of expressions and motifs from which all Christian writers, including the authors of the canonical gospels, independently drew.¹² (2) No attention is paid to the facts that (a) we have these gospel fragments only in an eighteenth-century manuscript, (b) we must

⁷To this bare outline there were several modifications: Canonical Mark seemed to have been cut down from a longer text; Clement's secret gospel shows some signs of censorship; in sum, there seem to have been many minor alterations and we can grasp securely only the main outlines.

⁸So Beardslee, Donfried, Koester, MacRae, Pedersen, and perhaps Trocmé. *Contra*: Brown, Fuller, Johnson, Merkel, Pomilio, Shepherd. Koester's remarks are particularly important both because of his knowledge of the evidence for the extracanonical material, and for his role in developing the "trajectory" concept. His objection to its mechanical misuse (in his response to Fuller) deserves general attention.

⁹Brown, Bruce, Grant, Hanson (tentatively), Hobbs, van der Horst, Mullins, Richardson, Schmidt (with interesting parallels from the Diatessaron), and Skehan.

¹⁰Fitzmyer, Kümmel, Merkel, Parker, Shepherd.

¹¹Fuller, Frend (?), Johnson, Kee Wink. Frend did not make his position fully clear.

¹²For full evidence of this see H. Köster, *Synoptische Überlieferung bei den apostolischen Vätern* (Berlin: Akademie, 1957).

therefore suppose that they have been to some extent corrupted in transmission, and (c) we know that one of the commonest forms of corruption, in the transmission of gospel texts, is contamination by the wording of more popular gospels. (3) The problems posed by the agreement in *order* of Mark, John, and Secret Mark, and the priority in *form* of the resurrection story in Secret Mark to that in John, are neglected in favor of first impressions based on familiar wording.¹³ This reverses the proper ranking of the evidence. Verbal similarities can be accounted for in any number of ways; they may result from common Greek usage, from Christian oral tradition, from a common written source, from one evangelist's use of another's gospel, or from later textual corruption — to mention only familiar causes that have always to be considered. But questions of structural similarity between two stories, and a fortiori between two gospels, are fundamental and imply specific answers. Most people, of course, pay most attention to the superficial.

The positions outlined above allow for some overlap. All critics would recognize some borrowings from canonical gospels

¹³To this generalization the outstanding exception is Brown's paper, which tries to prove "that it is not impossible [his italics, p. 474] that SGM [the Secret Gospel of Mark] drew upon John." This attempt, and the clarity with which Brown saw the problem, makes his paper one of the most important of those in the bibliography. However, I think it fails. The attempt to discredit the argument from order fails to explain why, if using John, Secret Mark would not have located the visit to Bethany after that to Jericho. (The suggestion that it was located earlier so that its conclusion could be modeled on that of the Nicodemus story is desperate. Why could that conclusion not have been used in a later location?) The attempt to explain the resurrection story as an adaptation of John's Lazarus story is even weaker. Brown astutely concentrated on the verbal similarities, but it is the structural differences that are here decisive. He cannot explain how the author of Secret Mark, using John, happened to eliminate one of the two sisters (whose contrast is so important in the Johannine story!), to eliminate the Jews, etc. The appeal to "rewriting in Markan style" does not account for these structural changes; Brown accordingly avoids them. As for his suppositions, let me quote the comment I sent him: "If one sets out with a determination to explain the origin of SGM from the canonical gospels, and if one supposes that somebody who had a vague memory of John wanted (for what reason?) to produce a copy of Mark which would contain versions of the Lazarus and Nicodemus stories, combined and rewritten in Markan style, and if one supposes that his memory of John — in spite of being vague (so that he would leave out and distort most of the stories) — was also extraordinarily good (so that he could range all over the gospel and take one element from here, another from there . . .), and if one supposes that his notion of Markan style was such that he could fill his story out at will with words and phrases from other gospels, then it would be possible to suppose that he had in this way produced the text we have . . .; but one would still be unable to explain . . . why he just happened to forget all the clearly secondary, distinctively Johannine traits of the Johannine story which was his main source. (This was the point at which you dropped the effort to explain and took flight to Fortna's reconstruction — scarcely a tower of strength.) . . . The main question in my mind is why one should make all these obviously unlikely suppositions."

other than Mark; most would probably admit some invention. The great divide, however, lies between the eleven who think that behind Secret Mark lies some pre-Markan tradition (whether oral or written in one form or another), and the fifteen who think the secret material composed entirely from scraps of the canonical gospels and free invention. For the first, Secret Mark can serve as evidence of what was said in pre-Markan Christian communities, and thence, perhaps, of what Jesus did. For the second, it is evidence only of the second-century community that produced it. The latter group probably gained some adherents because I had shown that the gospel fragments represented Jesus as practicing some sort of initiation, and I had argued that this initiation was a baptism supposed to admit the recipient into the kingdom of God and free him from the Mosaic law, this being effected by an illusory ascent to the heavens, of the sort described in the magical papyri, and by union with Jesus, also magical.

Of course nobody accepted the proposed explanation. I was amazed that so many went so far as to concede that Jesus might have had some secret doctrines and initiatory ceremonies,¹⁴ or to recognize, even if unwillingly and with reservations, that magic did have a role in the first-century Church.¹⁵ The most violent abuse (from scholars) came from two circles, one, the *dévots* (e.g., Fitzmyer, "Faust sells his soul!", "venal popularization"; Skehan, "morbid concatenation of fancies"; etc.), the other, the adherents of current exegetic cliques (form criticism, redaction criticism, etc.) who were outraged that I had not given their literature of mutually contradictory conjectures the attention they thought it deserved.¹⁶ These latter, at least, had a legitimate objection — neglect of much contemporary scholarship — that did deserve an answer. Let me answer it here.

¹⁴Betz, Johnson, Koester, Grant, van der Horst. That none would go further was due, I think, rather to unfamiliarity with the terrain than to religious prejudice. None had given much attention to the evidence for the outsiders' view of Jesus, none was very well acquainted with ancient magical texts, or had then considered their parallels to the gospels, and, most important, all were used to interpreting the relevant NT texts in other ways, so the new interpretations, because they differed from the accepted ones, seemed "wrong." Here the outstanding exception was Trevor-Roper, who read my work as a professional historian, without the preconceptions of a NT scholar. I am proud of his approval.

¹⁵Bruce, Frend, Betz, Koester, Grant, van der Horst, Richardson, Trevor-Roper, Wink.

¹⁶So Achtemeier (a conspicuously incompetent review, swarming with gross errors even in his reports of what I had said), Donfried, Fuller, Merkel, Wink.

To show that a text has been altered, one can use three kinds of evidence: from manuscripts (textual differences, erasures, insertions, etc.), from historical reports indicating changes, and from inconsistencies (changes in wording, content, grammatical usage, etc.). This last kind of evidence is occasionally conclusive,¹⁷ but often uncertain. Whether or not an introduction to a story comes from its original teller, whether details that seem to break the narrative are additions or mere asides — such questions often come down to matters of feeling. Consequently much that passes as form criticism and the like is actually autobiography — "How I feel about this text." Hence the welter of contradictions these schools have produced.

Now the mass of factual data that had to be dealt with in evaluating the letter of Clement and Secret Mark was such that my full presentation, *Clement*, is a dreadfully complex book. To have further cluttered its complexity with innumerable discussions of form-critical theories, leading to conclusions in which nothing could be concluded, would have produced a work practically unreadable. All the more so, *The Secret Gospel*, in which I tried to make the results of *Clement* accessible to ordinary readers, had to be clear of such clutter. A good example of what I mean has been furnished by Fuller, the arbitrariness of whose conjectures was pointed out in my reply to him. In another reply, Hobbs produced a completely different account of the secret gospel's origin by his conjecture that it was a pastiche. Yet another conjectural explanation was offered by Grant.

Admittedly conjectures are necessary, but they must be based on substantial evidence and must accord with historical knowledge of ancient society and of the general course and circumstances of the events concerned. The general historical framework must serve as our guide in locating and assessing all early Christian documents, canonical or not. Within this general framework there are smaller frames that should both limit and guide conjectures concerning smaller fields — the public life of Jesus, the missionary career of Paul, and so on. When writing *Clement* I overestimated the professional readers for whom the book was written, supposing they would recognize, when I merely mentioned them, the elements of these frames, and that they would see their relevance to the argument. *The Secret Gospel*, summarizing *Clement*, suffered

¹⁷For instance, if one can show, in a text, a series of parenthetical comments, all of which, in the same way, contradict or modify the main argument. But even in such cases piety and perverse ingenuity may continue to defend the text's integrity. See the commentaries on Ecclesiastes.

even more from the same misjudgment. Consequently a number of critics complained of the "lack of historical criteria" to justify distinctions between trustworthy and unreliable material. To these complaints I have responded in *Jesus the Magician*¹⁸ which begins with a study of the evidence for the basic events of Jesus' career and the role he played in his society, ascertains these, and proceeds from them to evaluate particular gospel passages. Only by reference to the historical framework can we discover which pieces fit and which do not.

In sum, "the state of the question" would seem to be about as follows: Attribution of the letter to Clement is commonly accepted and no strong argument against it has appeared, but Clement's attribution of the gospel to "Mark" is universally rejected. As to the gospel fragments, the field is split three ways. The weakest position seems to be that of those who declare them an apocryphal gospel of the common second-century sort; this overlooks their conspicuous differences from that type. The most popular opinion declares them a pastiche composed from the canonical gospels. Since such pastiches are reported, the fact that no early one is extant is a less serious objection to this theory than is its failure to explain the apparent priority of the new resurrection story to John's Lazarus story, and its relation to the Markan-Johannine outline. The third opinion is that the new text comes from an expansion of Mark which imitated Markan style, but used earlier material. This escapes the previous objections, but those who hold it are much divided as to what sort of earlier material was used.¹⁹

¹⁸ San Francisco: Harper & Row, 1978. This has now been republished in paperback by Harper & Row.

¹⁹ This article has dealt only with the works or passages bearing directly on the origin of the letter of Clement or of the gospel material it contains. A number of other studies, however, have mentioned in passing either the letter or the gospel material. Of these, one group deserves mention—those attempting to explain the mysterious young man of Mark 14:51. These began with an "Addendum" by R. Scroggs and K. Groff on pp. 547-48 of their article "Baptism in Mark" (*JBL* 92 [1973] 531-48). They took the young man as an allegorical figure representing both Christ and the initiate in Christian baptism. This interpretation neglects only the main facts: this young man deserted Christ and saved himself. However, the authors concluded on p. 548 that the initiation reported in the secret gospel "is, or is related to, baptism" and therefore proves that "the baptismal interpretation of 14.51-52 is ... 'orthodox' and fairly early"; they thought the secret gospel "probably ... no later than the second century." Their article initiated a series of implausible suggestions by theologians whose candor compelled them to say nothing at all about the new evidence: P. Beernaert, 1974; J. Crossan, 1978; B. Standaert, 1978; J. Gnilka, 1979; H. Fleddermann, 1979. By contrast, F. Neirynek ("La Fuite du Jeune Homme," *ETHL* 55 [1979] 43-66 [= *ALBO*, Ser. 5, 39]) was more thorough. He admitted the existence of the secret gospel story,

TERMINAL NOTE

Since the preceding article was completed, I have learned of three more studies (two of them still forthcoming) dealing seriously with the text. A paper by Prof. Thomas Talley of General Theological Seminary, New York, "Le Temps liturgique dans l'Église ancienne" (*La Maison-Dieu* 147 [1981] 29-60), surveys current discussions of the early dates of the major Christian festivals and points out (pp. 51ff.) that the secret gospel and letter of Clement contain the tradition needed to explain the dating of the early Egyptian baptismal liturgy on the sixth day of the sixth week of a lent beginning immediately after Epiphany. In this connection Fr. Talley reports that he asked about the manuscript of the letter of Clement when he was in Jerusalem in January 1980. The Archimandrite Melito told him that he had

but dismissed it as based on John 11, saying nothing of the formal evidence that it is earlier than the Johannine version. Then he argued (pp. 51-52) that it is historically insignificant because composed of random reminiscences of the canonical gospels. His evidence was (1) the fact that it contains many parallels to those gospels; (2) the tacit assumption that, in early Christian literature, all parallels to the canonical gospels must derive from them. This assumption being false, his argument was worthless. He then went on to argue (pp. 62 ff.) that since the words *epi gymnou* in 14:51 are omitted by W and λ, while δ, φ, and 565 have a variant, *gymnos*, they should be deleted! (One can hardly imagine what he would say of a proposal to delete, on so little evidence, any phrase which was not embarrassing. And what would the text of the gospels look like if all words against which there is this much evidence were deleted?) Having thus got rid of the embarrassment, the more difficult reading, he placidly observed, "Quant à *gymnos* au v. 52, l'emploi du mot au sens de *en chitonī monōī* n'a rien d'extraordinaire" (p. 64). It never occurred to him that this is just what proves that neither the sense nor the omission can be right. The evangelist was not concerned to report matters which were "in no way extraordinary," and the dramatic position of this detail, at the conclusion of Jesus' free ministry and the beginning of his passion, proves it significant. In the Mar Saba letter Clement tells us that the Gospel of Mark served as a collection of texts from which Christian teachers began their explanations. Those explanations had to be, in many points, apologetic; the opponents of Christianity were still telling their versions of the events — so Justin reports (*Dialogue*, 17, 108, 117) and Celsus proves. This event must have needed a good deal of explaining: "Holy man arrested... naked youth escapes." No further explanation is needed for the omissions by Matthew and Luke, or the partial bowdlerization by some MSS of Mark. Uncritically accepting the bowdlerized version, Neiryck cautiously concluded that the episode was historical, but he did not think of asking what the young man was doing there. How did he just happen to be present after a secret dinner of Jesus and the disciples? How did he just happen to be alone with Jesus after "all" the disciples had fled? Blessed are the pure in heart, for they shall not see difficulties. The most recent study I have seen is that of M. Gourgues, "À propos du symbolisme... de Marc 16.5," *NTS* 27 (1981) 672-78. Gourgues cites all the articles referred to in this note, and also alludes in two footnotes to the new evidence, but says nothing of its importance for his subject — its indication that Jesus administered some sort of nocturnal ceremony to which recipients came in the costume customary for initiations.

himself brought it from Mar Saba to the Patriarchal Library. Fr. Kallistos, the librarian there, said that it had been received, but it had been taken out of the volume of Ignatius, was being studied, and was not available for inspection. Talley's article is to appear also in German in *Liturgisches Jahrbuch* and in English in *Scudia Liturgica*. In another paper, "Alexandria and the Origin of Lent," given at the International Patristic Congress in 1979, and to be published in the papers of that Congress, he argues that the initiatory text of Secret Mark was the source of a baptismal festival "of Lazarus" celebrated annually in the Alexandrian church prior to the time of Athanasius. A paper by Prof. Helmut Koester of Harvard, "History and Development of Mark's Gospel (from Mark to *Secret Mark* and 'Canonical' Mark)," will appear in *A Time for Reappraisal and Fresh Approaches: Colloquy on New Testament Studies*, ed. Bruce Corley (Macon, GA: Mercer University, 1982). In correspondence about the latter article, Prof. Koester made the following statement, which he has given me permission to quote: "If the letter is 'Pseudo-Clement,' — and I don't think it is — it must be ancient and the fragment from *Secret Mark* that it quotes (as well as the reference to the Carpocratians, etc.) must be genuine. The piece of *Secret Mark* fits the Markan trajectory so well that a forgery is inconceivable."

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- contains responses by a dozen scholars, of which some are mere notes, but the following deserve notice: H. D. Betz, 17-18; E. Hobbs, 19-25; S. Johnson, 26-28; H. Koester 29-32; A. Kolenkow, 33-34; C. Murgia, 35-40; D. Schmidt, 41-45; M. Shepherd, 46-52; M. Smith, 12-15.
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- See also the papers cited in n. 19 and the papers by Koester and Talley mentioned in the terminal note.