

## BRAINWAVES REPORT BW/027

### DATING MARK

A Response to Brandon Giella

*Chronology is a surly, churlish cur, and hath bit many a man's fingers. Blame me not, therefore, if willing to keep my own hands whole.*<sup>1</sup>

*[W]hat one looks for in vain in much recent scholarship is any serious wrestling with the external or internal evidence for the dating of individual books (such as marked the writings of men like Lightfoot and Harnack and Zahn), rather than an a priori pattern of theological development into which they are then made to fit.*<sup>2</sup>

*It has become a bad habit among New Testament scholars to repeat erroneous opinions unchecked so long that they become a general assumption.*<sup>3</sup>

In his paper on Dating Mark, Giella seeks to replace the majority view that Mark's Gospel was written between c.AD 65 and c.AD 75 with his own view which places Mark "just after Peter's death in AD 64 but sometime just before AD 70." (p.10) The writer bases his conclusions on both external (historical) and internal arguments.

As an ancient historian my main concern lies with his historical arguments, some of which I wish to address in this response.

The most obvious initial comment is that not one ancient writer places Mark in the range bracket Giella proposes; nor for that matter in that claimed for the 'majority view'. Such a charge might not worry us so much in the case of Matthew and John, of whose careers we know relatively little. But about Mark information is quite substantial, and indeed we have credible sources explicitly placing his Gospel within very recognisable bounds which are strongly consistent with our other ancient testimony, and we might reasonably expect that any attempt to date the work would take account of this. It is unfortunate that Giella, like so many modern writers before him, neglects to do so.

#### REFERENCES FROM EUSEBIUS

Giella very properly makes much use of Eusebius' *Ecclesiastical History*, which he very reasonably describes as "Eusebius' trustworthy witness" (p.10). The passages from this of which he makes use are:

2.15.1 – 2.16.1 (p.3: Clement and Papias on the writing of Mark.)

3.1.2 (p.5: Origen on Peter's crucifixion.)

3.39.15 (p.3: Papias on John the Elder on the writing of Mark.)

3.39.15 (p.3: Confirmation of this, which he assigns to Irenaeus, although Irenaeus is

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<sup>1</sup> Attributed to Thomas Fuller in Smith, *Life and Letters*, xi.

<sup>2</sup> Robinson, *Redating*, 8–9.

<sup>3</sup> Hengel, *Mark*, 28.

not actually mentioned in this section.)

6.14 .6 (p.5: Clement on Mark recording Peter's preaching in Rome.)

6.25.5 (p.5: Origen on Mark writing on Peter's instructions.)

#### WAS PETER STILL ALIVE WHEN MARK WROTE?

Let us consider first the question, did Mark write before or after Peter's death? Giella tells us quite rightly (p.5) that

“Ancient sources arguing for composition before Peter's death include Clement ..., Origen, ... and Jerome.”

#### Evidence of Clement (c.150 – c.215)

Clement of Alexandria's account of the writing of Mark's Gospel is given by Eusebius (*EH* 6.14.6-7), as:

“When Peter had publicly preached the word in Rome, and by the Spirit had proclaimed the Gospel, that those present, who were many, exhorted Mark, as one who had followed him for a long time and remembered what had been spoken, to make a record of what was said; and that he did this, and distributed the Gospel among those that asked him. And that when the matter came to Peter's knowledge, he neither strongly forbade it, nor urged it forward.” (tr.Lake)

Elsewhere, in his *Outlines*, commenting on 1 Peter 5:13, Clement repeats his account of Mark's origins in Rome, adding in the detail that Peter had been preaching in the presence of 'Caesar's knights' (*equites*, roughly speaking the middle-class property owners).

#### Evidence of Origen (c.184 – c.253)

According to Origen, as reported by Eusebius (*EH* 6.25.5) Mark wrote the second Gospel, “in accordance with Peter's instructions”.

#### Evidence of Jerome (c.347 – 420)

Giella gives two citations from Jerome supporting the belief that Mark wrote when Peter was alive, one from a letter to Hedibia (*Epistola ad Hedibia*, 11), and the other in his *De viris illustribus*, 8.

This is a battery of eminent scholars of very considerable force. Against them Giella cites Papias and Irenaeus. I shall argue that both of these *support* Clement, Origen and Jerome, rather than conflict with them.

#### Evidence of Papias (c.60 – c.130)

Eusebius reports in two passages the records of Papias, bishop of Hierapolis, about the writing of Mark's Gospel, in his fivefold history *Interpretations of the Oracles of the Lord*. In the first to be

referenced by Giella (pp.2-3), Eusebius reports Papias' memories of teaching he had received from John 'the Elder' about Mark the evangelist, who had been Peter's follower and interpreter. Papias defends Mark against charges of inaccuracy (*EH* 3.39.15).

The other passage, whose *reference* is correctly given by Giella as Eusebius *EH* 2.15.1 – 2.16.1, supplies Papias' and Clement's account of how Mark was persuaded by his Roman audience to make a written record of Peter's teaching. This is the earliest traditional account of the writing of Mark's Gospel, which continues to see off its challengers:

“[T]he hearers of Peter ... besought Mark, whose Gospel is extant, seeing that he was Peter's follower, to leave them a written statement of the teaching given them verbally, nor did they cease until they persuaded him, and so became the cause of the Scripture called the Gospel according to Mark. And they say that the Apostle, knowing by the revelation of the spirit to him what had been done, was pleased at their zeal, and ratified the scripture for study in the churches. Clement quotes the story in the book of the *Hypotyposes*, and the bishop of Hierapolis, named Papias, confirms him.” (*EH* 2.15.1-2, tr. Lake)

Unfortunately, Giella supplies the wrong text for this reference; indeed, like numerous other scholars, he seems somewhat unfamiliar with both it and its context. This is regrettable, because it is from here that Papias and Clement supply the key information from which Mark is to be dated. (We have already seen Clement's account in Eusebius, *EH* 6.14.6-7, quoted above.) What Giella gives us instead is

“After their departure [ἔξοδος] Mark, the disciple and interpreter [ἑρμηνεύτης] of Peter, also transmitted to us in writing those things which Peter had preached,”

which actually comes from Irenaeus, *Against Heresies*, 3.1.1, quoted from Eusebius *EH* 5.8.3. We are not told by Eusebius that these are Papias' words, although indeed Irenaeus did use him as a source.<sup>4</sup> So when he then goes straight on to quote

“After their [Peter and Paul] departure, Mark, the disciple and interpreter of Peter, did also hand down to us in writing what had been preached by Peter,”

he rightly attributes this to Irenaeus, but fails to recognise that it is in fact just a different translation of the same sentence in *EH* 5.8.3.

Eusebius *EH* 2.15.1 – 2.16.1, mentioned above, is notable not only for its *content* (how Mark's Gospel came to be written), but at least as much for its *context* (when and where it was written).

*EH* 2.15.1 is immediately preceded by *EH* 2.14.6:

“Close after [Simon Magus] *in the same reign of Claudius* the Providence of the universe ... guided to Rome ... the great and mighty Peter,” (tr. Lake)

*EH* 2.16.1 is succeeded after one sentence by *EH* 2.17.1:

“Tradition says that [Philo] came to Rome *in the time of Claudius* to speak to Peter, who was at that time preaching to those there.” (tr. Lake)

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<sup>4</sup> Eusebius, *EH* 3.39.13.

## Summary

*Eusebius has immediately sandwiched the writing of Mark's Gospel between two unambiguous references to the reign of Claudius (41-54). And this, he tells us, is the record of both Clement and Papias. In the absence of any rival account in the Fathers, or support for any other date, this has to be taken as decisive.*

## Evidence of Irenaeus (c.130 – c.202); Chapman Thereon

Irenaeus' ἔξοδος passage, discussed above, forms a major crux of our problem. Found as noted above in Eusebius *EH* 5.8.3, it has often been misread, so as to form the basis of the case for late datings, being mistaken as evidence that Mark wrote after Peter's death. Giella seems a bit ambivalent about this (p.4), but since he concludes (p.10) that Mark wrote after Peter's death, it appears that that is how he reads Irenaeus.

The definitive exegesis is that of Chapman,<sup>5</sup> written in 1905, but often overlooked today. He translates Eusebius' Greek, adding his own comments, as follows:

Matthew among the Hebrews in their own language published a writing also of the Gospel [*besides preaching it*],

Peter and Paul preaching the Gospel [*not to Jews but*] at Rome [*without writing it down*], and founding the Church there [*whose testimony I shall give presently, viz. Against Hereses 3.3*].

But [*although they died without having written a Gospel*] after their death (ἔξοδος) [*their preaching has not been lost to us, for*] Mark, the disciple and interpreter of Peter, has handed down to us, he also in writing [*like Matthew,*] the things which were preached by Peter,

And Luke besides, the companion of [*the other,*] Paul, set down in a book the Gospel preached by that apostle.

Finally, John, the disciple of the Lord, also published a Gospel, while he was living in Ephesus of Asia<sup>6</sup>.

By taking into account the Latin version of this text, which supplies the context that Eusebius has not quoted, Chapman demonstrates that so far from attempting to date Matthew, Mark and Luke, Irenaeus is explaining how the preaching of the four apostles Matthew, Peter, Paul and John was *transmitted* (παράδεδωκεν, not ἔγραψεν) to later generations in writing. Matthew and John wrote for themselves. But

"the preaching of Peter has been preserved to us *after* his death by being written down [by Mark] *before* his death."<sup>7</sup>

This is made evident by the continuous *present* participle κηρυσσόμενα in *EH* 5.8.3, 'still being preached'. And similarly with Paul and Luke, with Irenaeus' use of the participle κηρυσσόμενον. Mark and Luke were writing while Peter and Paul were *still preaching*. Chapman comments:

" there is no attempt to give any dates at all. The utmost we can gather is that all three Synoptists were thought by Irenaeus to have written before the death of St Peter and St Paul,"<sup>8</sup>

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<sup>5</sup> Chapman, 'St Irenaeus'.

<sup>6</sup> Chapman, *op. cit.*, 565-56.

<sup>7</sup> Chapman, *op. cit.*, 567.

<sup>8</sup> *Ibid.*

This makes perfect sense if ἔξοδος is rendered *death* as rightly does Giella. As Chapman makes plain, the suggestion that this might mean departure (from Rome) is a red herring.

I know of no scholar who has read Chapman and not been persuaded by him. Most significantly, he persuaded no less an authority than Harnack, who wrote, 'To Chapman...belongs the credit of having first correctly interpreted this passage'.<sup>9</sup> To my mind Chapman's classic article constitutes one of the foundations of the discipline of New Testament history. It is to be lamented that both Chapman and Harnack are not more widely read today.

One who has read both is E. Earle Ellis, who, after a meticulous examination of Irenaeus' evidence, concludes,

"[A]s Chapman and Harnack have shown, Irenaeus gives us no information in his statement on the date of the *writing* of the Gospel of Mark, nor, I would add, of its provenance.... [H]e either reflects or affirms the view that Mark transmitted his gospel while Peter was still active and that Luke wrote his while Paul was preaching, i.e. before AD 68."<sup>10</sup>

Irenaeus offers no more support than Papias for the notion that Mark wrote after Peter died. Hence both ancient supports for that view claimed by Giella and many others fail on examination.

This conclusion is important because later in his argument Giella is going to claim (p.5) that

"Papias' testimony—if one interprets ἔξοδος as Peter's death—is rather strong, and thus argues for Markan authorship after the martyrdom of Peter."

We may well concede that ἔξοδος does indeed refer to Peter's death. But in our text, Eusebius assigns this passage to Irenaeus, not Papias. So even though Irenaeus may have been following Papias, as Chapman believed, it cannot be claimed that from here that Papias' testimony is indeed 'rather strong' in favour of Mark having written *after* Peter's death. There is nothing here to reverse Papias' manifest opinion given above that Peter was alive when Mark wrote. Giella has mistaken Papias and Irenaeus for each other and reached a conclusion taught by neither.

#### The Old Latin (Anti-Marcionite) Prologue to Mark

The same ambiguity between Peter's death and his departure from Rome besets us when we come to examine the statement of the Old Latin, also known as the 'anti-Marcionite', Prologue to Mark, which was probably written in Rome in the second half of the first century.

"...Mark declared, who is called "stump-fingered" (*colobodactylus*) because he had short fingers in comparison with the size of the rest of his body. He was Peter's interpreter. After the death (*excessionem*) of Peter himself he wrote down (*descripsit*) this same gospel in the regions of Italy (*in partibus Italiae*)."

This appears to share a tradition with Irenaeus, with *excessionem* paralleling the disputed ἔξοδος, death. But *colobodactylus* suggests another, independent, source. Crucial is the meaning of *descripsit*. *Writing* a gospel "in the regions of Italy" reads strangely, where "passed on" or "transmitted" would

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<sup>9</sup> Harnack, *Date of the Acts and of the Synoptic Gospels*, 130.

<sup>10</sup> Ellis, *Documents*, 363.

seem more natural. We recall Clement's report that after writing his Gospel in Rome, Mark "distributed the Gospel among those that asked him"<sup>11</sup> This is transmission in action.

But scholars today continue to dispute the value of the Prologue's account, in terms of its age and relation to Irenaeus. On one theory it is later than Irenaeus, but its writer has misunderstood him.

Further, a variant edition of the anti-Marcionite Prologue replaces *excessionem*, translated "death" above, by *discessum*, "departure" (from Rome), as though some intelligent scribe has spotted the error in the original anti-Marcionite text before him, stating as it does that Mark wrote after Peter's death – and corrected the error by consulting the earlier and more trustworthy text of Irenaeus, which he correctly understood.

Earle Ellis concludes,

"If *descripsit* means to 'write', as it does in other contexts, the phrase, *in partibus Italiae*, makes little sense .... In conclusion, the Prologue to Mark is of questionable value in determining the Gospel's date or provenance."<sup>12</sup>

Peter's reaction to Mark's Gospel

The variant stories of Peter's reaction to the news of Mark's *oeuvre* suggest that he was still alive at the time; otherwise they could not have arisen. The question, What did Peter make of Mark's work? – very reasonable, given his known involvement in the Roman mission – would simply have been met with, "Nothing at all – he was already dead". The very fact that answers have survived – however disparate or far-fetched – demonstrates that *Peter was known to be still alive*.

Summary

That leaves us with the strong evidence of Clement, Origen, Jerome, Irenaeus and Papias as witnesses for Mark writing his Gospel while Peter was still alive, without serious opposition. The unanimous testimony of such a galaxy of scholars is not lightly to be discarded. On such a strength of evidence, almost any jury would convict. *Peter was alive when Mark wrote his Gospel*. This lemma we may regard as established, and it forms an important part of our case for a Claudian Mark. Now we need to attach a date.

#### THE RELIABILITY OF EUSEBIUS' TESTIMONY

Eusebius' Book II sections 8 to 19 form a continuous and broadly chronological narrative about the life of the Church *in the reign of Claudius*, within which the writing of Mark's Gospel in Rome at the request of the Roman Church, to which Peter had been preaching, is seamlessly embedded in a narrative in which Simon Magus and Philo, whom Peter is said to meet in Rome, + are both known to history from elsewhere. Eusebius goes out of his way to emphasise that this all happened in Claudius' reign, naming that Emperor in sections 8.1, 8:2, 9.1, 10.9, 11.3, 13.3, 14.6, 17.1, 18:9, 19.1 and 19.2.

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<sup>11</sup> Eusebius *EH* 6.14.6, tr Lake.

<sup>12</sup> Earle Ellis, *op. cit.*, 360.

Eusebius identifies as his written sources for these sections in order as Clement of Alexandria, Josephus, Justin Martyr, Irenaeus, Papias and Philo of Alexandria (also, outside this block, Origen and Tertullian). That is a battery of scholars not lightly to be dismissed. When he cites any one of these to make a point, we must bear in mind that he was well acquainted with all the others, so that we may reasonably expect consistency. For instance we should be instinctively wary of comments like Giella's:

“Though Papias and Clement may be at odds regarding composition before or after Peter's death” (p.9),

when Eusebius himself indicates no such conflict but, as already seen (p.3), indicates that the two are in agreement. Given the range of sources he consulted, Eusebius' testimony on this point is unimpeachable.

#### Peter's Meetings in Rome

Eusebius reports two meetings of Peter while in Rome with historical figures known from elsewhere: Simon Magus, whom he has encountered already in Samaria (Acts 8:9-25; c.34-6), and the Jewish philosopher Philo of Alexandria. Eusebius supplies us with a detailed account of Simon Magus' activities in Claudian Rome (*EH* 2.13 – 2.15.1). Magus's presence in Rome under Claudius is affirmed by Justin Martyr (AD 100 – 165; *EH* 2.13.1-5)) and is well supported by Irenaeus. His meeting with Peter in Rome is attested by Hippolytus<sup>13</sup>

But Eusebius' comments on Philo are of particular interest:

“Tradition says that [Philo] came to Rome in the time of Claudius to speak to Peter, who was at that time preaching to those there. This would, indeed, be not improbable since the treatise to which we refer, composed by him many years later, obviously contains the rules of the Church which are still observed in our own time.” (*EH* 2.17.1, tr.Lake)

From there onwards to *EH* 2.18.8 Eusebius continues his account of Philo, about whom he is clearly well informed, showing a detailed acquaintance with his writing and stressing his accurate knowledge of Church practice. Philo's writings were popular in the Church. According to Josephus,<sup>14</sup> he led a party of Jewish delegates from Alexandria to the Emperor Gaius (Caligula) to seek arbitration in a dispute with Alexandrian Greeks. David Runia tells us:

“Vivid descriptions of this event and its background are given in his treatises *In Flaccum* and *Legatio ad Gaium*.”<sup>15</sup>

This embassy is datable to the year 40, which makes wholly plausible Eusebius' report that

“He came to Rome in the time of Gaius, and in the reign of Claudius is said to have read before the whole Senate of the Romans his description of the impiety of Gaius.”<sup>16</sup>

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<sup>13</sup> Hippolytus, *Refutatio Omnium Haeresium* 6.15. Robinson, *Redating*, 111-114, contains a helpful discussion of this thread of argument.

<sup>14</sup> Josephus, *Antiquities* 18.8.1.

<sup>15</sup> David T. Runia, in Metzger and Coogan, ed., *Oxford Companion*, 592.

<sup>16</sup> Eusebius, *EH* 2.18.8.

The *Legatio ad Gaium* still exists. So we have good independent testimony confirming Philo's presence in Rome at the start of Claudius' reign, making wholly plausible a meeting with Peter at that time.

Moreover, Philo is generally believed to have died c. AD 50,<sup>17</sup> which tells further against any view of Mark's Gospel as having been written after 65.

Even if his motive in visiting Rome was not to see Peter as Eusebius suggests (*EH* 2.17.1), it is difficult to see how such an interaction with Peter might have been made up, say, twenty years later when Philo's death would have been common knowledge. Otherwise, are we to suppose that our authors and transmitting scribes have contrived together to make it look as if Mark was written much later than was actually the case? Too much history would have to be rewritten. The arbitrary and usually uncommented rejection of so many details does the moderns no credit.

And as we shall see, the Claudian date is independently supported by historical evidence from the careers of Peter and Mark.

### Oral Transmission

We should remember that oral transmission within the early Church played an important part in the preservation of the Jesus tradition. Eusebius had access to oral traditions about the origin of the gospels – a shared understanding within the Church – as well as literary records like those of Papias, Clement and Irenaeus, and will have been alert to any conflicts between them, just as he was aware when his sources confirm each other (eg *EH* 2.13.5, 2.15.2). We today may have to rely on the single written threads of Papias and others that Eusebius has passed down to us. But oral tradition within the Church will have given Eusebius access to traditions way beyond those written records.

Papias himself made use of 'unwritten tradition', and made much of his researches by questioning the 'presbyters' (followers of the apostles) and those who had followed them (*EH* 3.39.2), commenting,

"For I did not suppose that information from books would help me so much as the word of a living and surviving voice." (*EH* 3.39.4, tr. Lake)

For his part, with so many other sources available to him, Eusebius was well able to make critical assessments of the value of Papias' writings. So at *EH* 3.39.11-13 he rejects Papias' notion, by then widespread, of a millennium, accusing him of a lack of intelligence (which nevertheless did not stop him from relying on Papias when it suited him).

All this gives us good grounds for broadly accepting at face value Eusebius' record of Papias' account of the writing of Mark's Gospel, in spite of the modern fashion within source critical circles for summarily dismissing Papias.<sup>18</sup>

### Summary

In any other topic of ancient history, and in the absence of any credible conflicting evidence, testimony of this strength would be taken as conclusive. *Mark wrote in the reign of Claudius*. We today, some

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<sup>17</sup> David T. Runia, *op. cit.*, 592.

<sup>18</sup> Phrases like 'Papias is sand', 'valueless', 'treat with a pinch of salt' are not infrequent among source critics who see his evidence as running contrary to their own preferred solution to the Synoptic Problem, especially as regards Matthew. For references see Mosse, *Three Gospels*, 108, and in Wiebe, *Christian Origins*, 5.



nineteen or twenty centuries later, may have to unravel such things as though through a glass darkly. Eusebius, with all the information before him, was merely expressing what was already common knowledge within the Church. I know of no good evidence that the early Fathers were in any doubt about the authorship and provenance of Mark's Gospel. The confusion is one of our own perception, in this case the result of misreading Irenaeus and ignoring the clear dating supplied by Eusebius – who was, after all, extremely well read and better placed than we are by several centuries to know what was going on.

#### THE MOVEMENTS OF PETER AND MARK

Having now established that Mark wrote in the reign of Claudius while Peter was still alive, can we be any more specific? Yes, we can. Later in the same book Eusebius reports Claudius' banishment of "the Jews" from Rome (*EH* 2.18.9), identifying this with the episode in Acts 18:2 in which Priscilla and Aquila are stated to be among those so exiled. Orosius *Hist. Adv. Pag.* 7.6.15 gives this as the ninth year of Claudius (49), which is not seriously disputed, although some scholars prefer the year 50. This places Mark between 41 and 49. Peter and Mark would never have been allowed to minister in Rome after that. What of the *terminus post quem*?

This is ascertained by considering the historical careers of Peter and Mark. I deplore the fashion among modern writers who see no need to write of our evangelists as real people who lived real lives which can be the subject of historical investigation.<sup>19</sup> This abandonment of history does nothing but confuse. All too often, 'Matthew', 'Mark', 'Luke' and 'John' are treated as a matter of principle as if they were no more than labels for four documents, with no concern for their identities and careers, *whether or not titular authorship is maintained*. Giella shows no interest in the movements of Peter and Mark<sup>20</sup> as shedding any possible light on our problem. The following passage is quoted from my essay 'Doing New Testament History', chapter 1 in Ben Wiebe's recent book *Jesus and Christian Origins*:<sup>21</sup>

"The jigsaw was first assembled by Edmundson<sup>22</sup> in 1913, but not widely known until he was rediscovered by Robinson<sup>23</sup> in 1976. Briefly, in 42 Peter fled persecution in Jerusalem to "another place" (Acts 12:17). This may have been Caesarea, where he had friends in the household of Cornelius. Soon after, we find him in Antioch as related in Jerome's version of Eusebius' *Chronicle* for that year, which reads,

'Peter the Apostle ... when he had first founded the Antiochean Church, is sent to Rome, where as bishop (*episcopus*) of the same city he continues preaching the gospel for twenty-five years'.<sup>24</sup>

"Jerome himself adds elsewhere the detail that Peter arrived in Rome in the second year of Claudius (42), that is, twenty-five years before Peter's death in the "last year" of Nero (67).<sup>25</sup>

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<sup>19</sup> So Richard Bauckham's reintroduction of real people and the parts they played in generating the New Testament in *Jesus and the Eyewitnesses* represents a most welcome change of direction.

<sup>20</sup> I have attempted to reconstruct these in *Three Gospels*, 323-34.

<sup>21</sup> Wiebe, *op. cit.*, 16-17.

<sup>22</sup> Edmundson, *Church in Rome*, followed by Robinson, *Redating*, and others including Wenham, *Redating*. I would describe Edmundson and Robinson as essential reading for anyone embarking on a study of New Testament history. Edmundson was an historian and a mathematician, but not a theologian. I have reproduced his argument in greater detail in *Three Gospels*, 262-65. Facsimiles of *The Church in Rome* are readily available on the internet.

<sup>23</sup> J.A.T. Robinson, *op. cit.*, 112-14.

<sup>24</sup> Edmundson, *op. cit.*, 70, prefers "overseer" to "bishop." This need not have required his continuous presence (see Acts 15:36, ἐπισκεψόμεθα, which implies a travelling oversight).

<sup>25</sup> Jerome, *de Viris Illustribus*, 1

“By now we have reduced our bracket for the writing of Mark to 42–49. With the death of Herod Agrippa I in 44, it is now safe for Peter to return to Jerusalem, where we find Mark in 46 (Acts 12:25).<sup>26</sup> If Mark remained in Rome to write his Gospel (apparently in the absence of Peter), Edmundson’s estimate of 45 for that event looks very reasonable.<sup>27</sup> This has been confirmed independently by Crossley’s detailed analysis of the attitudes to the Torah (Jewish Law) within Mark which suggests a date *between the mid to late 30s and the mid-40s*.”<sup>28</sup>

I find Giella a little confusing at this point (p.3):

“If this is the case, [that Mark was Peter’s ἐρμηνεύτης], we know Mark’s gospel could not have been written before AD 62, since “he is not mentioned in the Prison Epistles—Ephesians, Philippians, Colossians, and Philemon”<sup>29</sup> — which probably were written from Rome between AD 60 and 62; nor is he mentioned in Paul’s letter to Rome about AD 56–57.”

The argument from silence here defeats me. The term ‘Prison Epistles’ normally includes 2 Timothy, in which Mark is mentioned at 4:11. Further, as Giella himself notes in his concluding sentence on p.10, Mark does in fact feature in Colossians (4:10) and Philemon (24). And there is an excellent case that Ephesians, Colossians and Philemon were written by Paul from Caesarea during his two year detention there between 57 and 59.<sup>30</sup> Philippians was probably Paul’s last extant epistle written during Paul’s second Roman detention shortly before his death (c.66).<sup>31</sup>

#### Summary

Giella offers no further evidence against the view that Mark wrote while Peter was alive. There is absolutely no reason to reject the unambiguous testimony of Papias and Clement that Mark’s Gospel was written in the reign of Claudius. This was clearly the view of the Fathers, and is confirmed externally by the historical evidence from the careers of Peter and Mark. Hence our best estimate for its date remains that of Edmundson: *Mark’s Gospel was written c.AD 45*.

#### RESPONSES TO EUSEBIUS’ DATING

Giella is not alone. What is remarkable is the number of modern scholars who similarly ignore Eusebius’ testimony as to the date and origin of Mark. One could at least understand a case for impugning his reliability or the quality of his sources, or for rejecting it on other historical grounds; but for completely ignoring it and failing to inform one’s readers that such an account as Eusebius has given us in his second book was ever held or believed by the Church, in the absence of any ancient alternative – this I find very hard to comprehend. In an attempt to do so we shall examine three different writers.

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<sup>26</sup> At the end of Saul’s and Barnabas’ ‘famine visit’ to Jerusalem in 46 (Acts 11:30).

<sup>27</sup> Edmundson, op. cit., 68.

<sup>28</sup> Crossley, *Date of Mark’s Gospel*, 208.

<sup>29</sup> Citing Brooks, ‘Mark’, 29-30.

<sup>30</sup> Acts 24:27. See Reicke, ‘Captivity Epistles’, 279-82.

<sup>31</sup> Mosse, *Three Gospels*, 225.

W. G. Kümmel

Kümmel, for instance, writing his influential *Introduction to the New Testament* in 1975, tells us in his section on The Place and Time of Writing of Mark's Gospel,

“That Papias asserts that Mk was written in Rome can be deduced from Eusebius only with great uncertainty (*EH* 2.15.2), but Clement of Alexandria (*Eus.*, *EH* 6.14.6) unambiguously asserts this tradition, which is followed today by the majority of scholars.”<sup>32</sup>

What is he talking about? Kümmel's assertion about Papias is patently untrue, as anyone checking *EH* 2.15.2 can verify. Great uncertainty? Immediately after telling how Mark wrote his Gospel *in Rome*, and Peter's subsequent reaction, Eusebius goes on to attribute the story to Clement, adding as quoted above,

“and the bishop of Hierapolis, named Papias, confirms him.”

In what sense is the confirmation by Papias “uncertain”? Whether or not Clement's story is true, what cannot be doubted is that according to Eusebius' text *Papias confirms it*. No deduction is necessary. We simply have to read the text in black and white on the page (not supplied by Kümmel, which makes it harder to follow his reasoning and decide on its correctness for ourselves). What is he playing at?

After some further discussion about Latinisms, Kümmel concludes (p.98),

“Otherwise nothing points to Rome, and a Gentile Christian community in the East is much more likely,”

offering no evidence in support of this, or any reason for rejecting Rome, or any explanation as to why “the majority of scholars” are wrong. “Nothing points to Rome”? What more does he need? The wool is being pulled over our eyes. The effect is to undermine our faith in Papias on the question of *location*. But the damage is done. Now he moves on to the bigger issue of *date*, writing,

“An early origin is improbable, because the development of the gospel tradition has already progressed far, and Mk 13 shows traces of the threatening nearness of the Jewish war. Most scholars assign a date in the years 64–70, since the destruction of Jerusalem in the year 70 is mentioned not unambiguously, but some scholars consider a date after 70 more likely for the composition .... Since no overwhelming argument for the years before or after 70 can be adduced, we must content ourselves with saying that Mk was written *ca.* 70.”<sup>33</sup>

“[B]ecause the development of the gospel tradition has already progressed far.” Does this mean anything? (How far? And how far would be enough to carry his point? How do we know? How do we judge?) Here if anywhere is the “great uncertainty”, where never was any before. We are being treated to a smokescreen.

Since again he makes no mention of *EH* 2.15, we his readers are not to realise that this passage is our principal source for the date of Mark's Gospel. Nor could we possibly have guessed that its context *EH* 2.14-16 places the episode fairly and squarely in the reign of Claudius, and in the city of Rome.

“Early origin improbable”? “No overwhelming argument”? This is flannel. Kümmel seems to be relying on the probability that few of his readers are going to obtain a copy of Eusebius, find the reference and check. After all, he has just shaken our faith in Papias on the question of location, has he not? Should we trust his judgement on date?

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<sup>32</sup> Kümmel, *Introduction*, 97.

<sup>33</sup> Kümmel, *op. cit.*, 98.

Why would any New Testament scholar do something like that? Well, at least it saves him from having to provide any solid justification for his preferred date near the destruction of Jerusalem in 70, in default of any historical evidence for it.

Do we have here the origin, or one of the origins, of the parade of moderns who for some reason or other prefer to ignore the Claudian date in our sources in favour of a later one associated with the events of the year 70<sup>34</sup> – in the absence of any historical evidence to support it? It appears that Giella was one of his victims. So, it seems, was Raymond E. Brown.

Raymond E. Brown

The highly respected scholar Raymond E. Brown, writing his *Introduction to the New Testament* in 1977 does scarcely any better. After setting a *terminus ad quem* of 75 from a circular argument about the dates of Mark and Luke/Acts, he continues,

“The other end of the spectrum is more problematic. For there is no way of knowing with certainty how early Mark was written.”<sup>35</sup>

“Certainty” is a philosophical red herring. a luxury frequently denied us in ancient chronology, used similarly above by Kümmel to put us off the scent. However, “high probability” will serve our purposes admirably. Are we being encouraged to ignore the mass of historical evidence actually available to us? Brown spares his readers any mention of Eusebius or Claudius at all. Whether he thought that we would find ancient evidence unduly troublesome, or whether he was genuinely ignorant of Eusebius’ *EH* Book II altogether, it might be prudent not to guess of one so eminent. But for whatever reason, it comes across as another smokescreen.

What would be the point of not telling readers that one’s preferred date for Mark close to 70 on so little evidence has a rival of some twenty-five years earlier made explicit by Eusebius, and so well supported by such a wide range of sources?

On dating the New Testament in general Brown offers a very revealing comment in a footnote:

“The failure of NT works to make specific and detailed mention of the destruction of Jerusalem and the Temple is very hard to explain. J.A.T. Robinson, *Redating the New Testament* (London: SCM, 1976), has used this factor entirely too simply to date most of the NT before 70; *but we should not pretend to have a satisfactory answer.*<sup>36</sup>

“[E]ntirely too simply”? Who is throwing stones? Brown repeats this disarmingly honest caveat when discussing the date of Acts later on:

Nevertheless, we admit that the absence of an indisputable, clear, specific Gospel (or, indeed, NT) reference to the destruction of the Temple as having taken place remains a problem, since it should have had an enormous impact on Christians.<sup>37</sup>

Yes, it should. Robinson’s challenge to conventional dating upheld by the majority of NT scholars remains unanswered even today. Brown happily offers conjectured dates for the NT in the knowledge that they were subject to an objection he was unable to counter. We may lament that he failed even to attempt to do so. So his NT Introduction comes to us with a kind of built-in obsolescence. He knows

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<sup>34</sup> See Giella’s n.3

<sup>35</sup> Brown, *Introduction*, 164.

<sup>36</sup> *Ibid.*, 163 n.93 (concluding emphasis added), discussing Robinson, *Redating*, title page.

<sup>37</sup> *Ibid.*, 273 n.102.

his chronology is wide open to significant challenge, but doesn't trouble to correct it. For whose benefit then is he writing? This does not engender confidence in his proposed date for Mark – or indeed for any other of the Gospels. As he opines,

“That Luke used Mark is most plausible from internal evidence; and if Mark is to be dated in the period 68-73, a date earlier than 80 for Luke/Acts is highly unlikely.”<sup>38</sup>

Martin Hengel

Very different, and very intriguing, is the approach of Hengel, no stranger at all to the business of ancient history. He is fully aware of Eusebius' account of the writing of Mark, and its implications for dating it, yet finally arrives at a date of 69. Part of his reasoning is his acceptance of the pre-Chapman interpretation of Irenaeus, that Mark wrote after Peter's death, which counters his full appreciation of Eusebius' evidence in favour of a Claudian date. It will be worthwhile examining his argument in detail. On the death of Peter he writes:

“The composition of the second Gospel even during the lifetime of the prince of the apostles would necessarily lead to an essentially earlier date. It would in fact make it about twenty-five years older,”<sup>39</sup>

while accepting that Eusebius does indeed presuppose such an early date. Given the lemma we established above, that Mark *did* write in the lifetime of Peter, it “necessarily” follows from this statement of Hengel that Mark was written under Claudius. Q.E.D. But as we shall soon see, this is not what Hengel wants to prove.

Meanwhile he continues Eusebius' narrative, translating *EH* 2.17.1,

“From Philo, however, we have word that [Philo] came to Rome under Claudius and was an associate of Peter, who preached at that time to the inhabitants there.”<sup>40</sup>

There follows,

“The *Chronicon Hieronymi* confirms this information, in that it makes Peter come to Rome after the founding of the community in Antioch in the second (according to some manuscripts even in the first) year of Claudius.... To some degree in agreement with this, later chronographers and a number of hypotheses about Mark claim that Mark composed his Gospels in Rome ‘ten years after the ascension.’”<sup>41</sup>

Indeed, in subsequent endnotes<sup>42</sup> Hengel brings together some very detailed references to the work of previous scholars on the later commentators such as Theophylact of Ohrid (late 11<sup>th</sup> century, but still well respected), who in his Prologue to Mark claims it was written in Rome ten years after the ascension. Putting the ascension at 33,<sup>43</sup> this gives us 43.

Hengel goes on to cite the *Chronicles* of Cassiodorus, “which puts the death of Jesus in 29 and Peter's journey to Rome twelve years later, in 41.” And while precision is not to be expected, it is significant that such tentative dates cluster around the very beginning of Claudius' reign, c.42. Not one of them

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<sup>38</sup> Brown, *op. cit.*, 273 (emphasis original).

<sup>39</sup> Hengel, *Mark*, 5.

<sup>40</sup> Hengel, *op. cit.*, 5. I prefer Lake's translation in the Loeb edition, “Tradition says that he came to Rome in the time of Claudius to speak to Peter, who was at that time preaching to those there.”

<sup>41</sup> Hengel, *op. cit.*, 5, citing n.35 on p. 121.

<sup>42</sup> Hengel, *op. cit.*, 121.

<sup>43</sup> For the date 3 April 33 of the crucifixion see Humphreys and Waddington, ‘The Date of the Crucifixion’. I know of no refutation of this.

targets a date in the reign of Nero or later. What a lot of converging evidence we have to reject if we choose a later, post-Claudian date, as Hengel then does.

For having constructed such a convincing case for an early Claudian date, he then abandons it in favour of a series of circumstantial parallels between Mark's text (mostly chapter 13) and political events leading up to just before the destruction of the Temple in 70.

"Despite what has been said so far, nevertheless a whole series of specific reasons tell against too generously early a dating, say, along the lines of Eusebius, and for a later dating, of the kind that I already hinted at, in connection with Papias and which we find advocated in Irenaeus."<sup>44</sup>

"Despite what has been said so far". Note how easily he dispenses with the case that had seemed so compelling a few pages before, *while leaving its arguments unanswered*.

Writing of Eusebius' quotation from John 'the Elder' about Papias' account of Mark, *EH* 3.14-15, Hengel says:

"Even if, as I believe, the 'Elder' in the Papias quotation gives us an indication which historically is to be taken seriously, it does not give us a clear date. In other words, the time of the composition of the second Gospel must be inferred from the indications given by the second evangelist in his work."<sup>45</sup>

The *non sequitur* here leaves us gasping. "Does not give us a clear date"? "Must be inferred"? We already know Papias' belief about the date of Mark, from the account in *EH* 2.15, firmly located by Eusebius in the reign of Claudius from his source, Papias' *Interpretation of the Oracles of the Lord*. (*EH* 3.39.1, 3.39.14). We don't need Papias to tell us again. There is no sign he has changed his mind. So what are we to understand by Hengel's "which historically is to be taken seriously" followed immediately by, "it does not give us a clear date"? What he means is, he is about to abandon altogether Papias and the Eusebian case for a Claudian Mark. Hengel is riding two horses at once and is about to leave one for the other.

He is worried. Unaware of Chapman, like so many others, and consequently misinterpreting Irenaeus' ἔξοδοϛ passage, he is committed to the view that

"according to the earliest tradition of the church we can arrive at, the origin of the Gospel of Mark is to be put at the earliest in the period after the death of Peter in the Neronian persecution."<sup>46</sup>

He has then reached an impasse. Most of his sources lead unambiguously to a date some twenty-five years earlier. Irenaeus, as he reads him, leads to another, after Peter's death. These two positions are irreconcilable, and he knows it. Rather than attempting to square the circle, he abandons on the flimsiest pretext the external evidence altogether and seeks a solution involving the internal evidence (Mark 13) on its own.

He has already prepared the ground well in advance after a detailed discussion of the variant traditions about Mark's writing, concluding,

"Thus there may have been a number of variants in the legend of the origin of the second Gospel which developed and were harmonized."<sup>47</sup>

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<sup>44</sup> Hengel, *op. cit.*, 12.

<sup>45</sup> Hengel, *op. cit.*, 7.

<sup>46</sup> Hengel, *op. cit.*, 3. Note that if this is correct, not one of the martyrs under the Neronian persecution had ever seen a single page of any of our canonical Gospels.

<sup>47</sup> Hengel, *op. cit.*, 6.

The sands continue to shift, as though the existence of ‘harmonized variants’ entitles us to reject the ‘legend’ in its entirety. However, these ‘variants’ – such as the different accounts of Peter’s reaction to Mark’s document<sup>48</sup> - share one important characteristic: *none of them suggests any time frame other than the early years of Claudius*. So they in no way support the jump to the seventh decade that he now makes. What a lot of evidence he has to reject in order to make this jump.

Hengel’s thinking here exemplifies the approach typical of the form criticism of recent times, according to which passages of Jesus’ teaching in the Gospels are not as presented but instead reflect the local political or religious situation at the time and place when they were penned.<sup>49</sup> So for instance he writes

“[W]e must consider the context, Mark 13:6-13 (and 21-23), in which Mark describes the actual situation of his community (or the church of his time as a whole).”<sup>50</sup>

Note that the possibility that Jesus may actually have spoken the words attributed to him is given no consideration. After several pages of rather convoluted narrative, Hengel finally opts for 69,<sup>51</sup> the year of the four Emperors (pp.22, 30) – ‘year of the revolution’ as he calls it – thereby rejecting outright his previous strongly argued case for composition under Claudius. It is a date supported by not one ancient authority. We shall return to Mark 13 below.

How are the mighty fallen! What a very curious *volte-face* from so eminent a scholar. And all, it seems, because he misreads Irenaeus.

#### THE SYNOPTIC PROBLEM

Giella does well to see the relevance of the wider Synoptic Problem, even though this takes him outside his chosen scope, so that he can offer us little more than “A brief aside”, which amounts to a historical survey of the Problem from its earliest beginnings (pp.5-7). He cannot be faulted therefore for tentatively adopting the widely accepted ‘majority view’, the ‘two-source’ hypothesis according to which Mark wrote first, and Matthew and Luke made use of his Gospel, and another, hypothetical, document commonly designated ‘Q’.

He is equally correct in realising the importance of any proposed solution to the Synoptic Problem to his own topic, the date of Mark. The two matters are inextricably combined. As he writes (p.6),

“If the two-source hypothesis is correct, then the circulation of Mark must have had enough time to become well known in order for Matthew and Luke to compose their gospels.”

In fact there are good reasons to call the two-source hypothesis into question. The Q hypothesis, which Giella to his credit recognizes as being only a *hypothesis*, has in fact not a shred of historical evidence to support it. This has nevertheless not prevented ambitious source critics from building upon it a

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<sup>48</sup> We can safely discount as an unreliable witness the apocryphal Acts of Peter which Hengel cites on his p.6, dating it to 180.

<sup>49</sup> See Wiebe, *Christian Origins*, 42-50 for a critique of form criticism and why it has of late fallen out of favour. As he says, “Form-criticism, focused on the text of the Gospels and operating with the attendant assumptions, discounted much of the internal and most of the external evidence for the origin of the Gospels.” (44)

<sup>50</sup> Hengel, *op. cit.*, 21.

<sup>51</sup> Hengel, *op. cit.*, 30, 22.

massive construction, even recreating its very text,<sup>52</sup> and the entirely imaginary society in which Q was supposed to have circulated,<sup>53</sup> all devoid of historical support.

Newcomers to the Synoptic Problem could be forgiven for not realizing that this ambitious Q hypothesis is supported by not a word in the Fathers and not a scrap of papyrus from any Middle Eastern rubbish dump or anywhere else, out of the hundreds or thousands now in our possession. As to this sacred text, from which two of our canonical scriptures are supposed to have been derived, not only are the Fathers completely silent about its catastrophic loss; they give no indication that they had ever possessed it in the first place.<sup>54</sup>

We may fairly contrast the Gospel of Thomas, unearthed on a deposit at Nag-Hammadi in Egypt in 1945, but of whose existence scholars had long been aware from references and quotations in the Fathers.

The Farrer hypothesis,<sup>55</sup> the most credible and most durable alternative to the two-source hypothesis, manages to explain the same phenomena of similarities between the first three gospels by the simple suggestion that Luke was working from a memorized copy of Matthew, and simply borrowed passages he happened to like; while both Matthew and Luke made use of Mark. If so, then the need to postulate an extra source vanishes, and Farrer wins by application of Occam's Razor.<sup>56</sup>

I share with Giella the majority view that "the dates of Matthew and Luke are a bit later than Mark." (p.5) By general agreement also, Acts postdates Luke. Robert B. Moberly's meticulous paper of 1993 on the date of Acts, still to my knowledge unanswered, is a game changer: Acts can now be securely dated to the reign of Nero, and indeed within that, to 62-63<sup>57</sup>. Given its consistently favourable depiction of the Roman state and its officials, it can scarcely be placed *after* the persecutions which were Nero's response to the Great Fire of Rome in 64. But if Acts, whose narrative stops at 62, predated the Fire, while Luke predates Acts and postdates Mark, we have a *terminus ad quem* for Mark as 62 or 63. This raises serious questions about Giella's *terminus a quo* of 64, but accords well with our previous conclusion that Mark's Gospel was written under Claudius.

## MARK 13

Again, Giella writes:

"Thus, Mark 13 and Jesus' reference to these events indicate considerable foreign rule and persecution in the region of Judea in the latter half of the first century." (Giella, p.9)

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<sup>52</sup> As Catchpole, *Quest for Q*, Such enterprises are self-defeating when they assume that the ancestry of our present Gospels was entirely *documentary*, that is, totally uninfluenced by oral tradition. This strikes me as improbable.

<sup>53</sup> Tuckett, *Q*.

<sup>54</sup> Those being tempted into a dalliance with the Q hypothesis may perhaps find illuminating Eta Linnemann's detailed analysis 'The Lost Gospel of Q – Fact or Fantasy?'.  
<sup>55</sup> See Farrer, 'On Dispensing with Q', which is still standing after six decades of onslaught from the proponents of the 'majority view'. For a modern critique of Q see my pp.98-101 in Wiebe, *Christian Origins*. One is tempted by the hypothesis that Q did indeed exist, and that both Matthew and Luke would have been delighted to consult it, but that it perished in the Great Fire of Rome before either of those two evangelists managed to set eyes on it.

<sup>56</sup> Occam's Razor: *Entia non sunt multiplicanda praeter necessitatem* - 'Entities are not to be multiplied beyond what is necessary'.  
<sup>57</sup> See Moberly, 'When was Acts Planned?' I have summarised and slightly extended Moberly's case in *The Three Gospels*, 242-24 and more briefly in Wiebe, *Christian Origins*, 20. Many of Moberly's original arguments from the language of Acts, e.g. the way names are employed, are very similar to the kind employed by Giella to demonstrate 'a relatively early date for the Gospel' (pp.7-8). See also the very solid discussion of the date of Acts in Hemer, *Book of Acts*, 403, who opts for AD 62-63.



That Jesus may have referred to these violent events may well indicate “considerable foreign rule” in the late first century, as Giella suggests. But they tell us nothing at all about the date of Mark, unless we take as our premise (as many do, for reasons usually unstated) the dogma that Jesus was incapable of accurately predicting the future. This in turn would appear to rest on the ground that prophecy is on principle inadmissible. We shall discuss this view, and the logical problem it poses, below.

For instance, Hengel dates Mark to 69 in the light of perceived parallels between prophetic passages attributed to Jesus in Mark 13 and the political situation during the late 60s. He gives no consideration to the possibility that the attribution may actually be correct. There seems to be an assumption (unargued, unstated, unchallenged and possibly unconscious) that Jesus could not have foretold the future. So certain is he of this that he allows it to overturn the case for a Claudian date which he had expounded so convincingly beforehand.<sup>58</sup>

Hengel’s premise is inimical to the best supported date for the writing of Mark’s Gospel that we have found above, leading us to a total impasse. But 69 is a fragile date: As Hengel himself was aware, any evidence that Peter was still alive when Mark wrote would refute to it. We cannot have both. We have to choose.

We can follow the converging historical case for a Claudian date and reject the premise that Jesus could not predict the future.

Or we can accept the premise, reject the historical evidence for a Claudian Mark, and opt like Hengel and many others for a date close to the fall of Jerusalem.

How do we decide?

Let us consult the wisdom of C. S. Lewis:

“Thirdly, I find in these theologians a constant use of the principle that the miraculous does not occur. Thus any statement put into our Lord’s mouth by the old texts which, if he had really made it, would constitute a prediction of the future, is taken to have been put in after the occurrence which it seemed to predict. This is very sensible if we start by knowing that inspired prediction cannot occur. Now I do not here want to discuss whether the miraculous is possible. I only want to point out that this is a purely philosophical question. Scholars, as scholars, speak on it with no more authority than anyone else. The canon ‘if miraculous, unhistorical’ is one they bring to their texts, not one they have learned from it. If one is speaking of authority, the united authority of all the Biblical critics in the world counts here for nothing. On this they speak simply as men; men obviously influenced by, and perhaps insufficiently critical of, the spirit of the age they grew up in.”<sup>59</sup>

Austin Farrer had already pointed out in 1951 the logical fallacy involved:

“The maxim about true prophecies being all afterthoughts is sufficiently refuted by stating its contrary, that false prophecies always precede the events they misdescribe. If we take the two maxims side by side, we get the surprising joint conclusion (a) that people sometimes prophesy the future event; (b) that they are never right.”<sup>60</sup>

There seem to be logical problems which undermine the premise. They lead us into a contradiction, or what the logicians call *reductio ad absurdum*. So, like any mathematician, we must reject it. So Jesus

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<sup>58</sup> Hengel, *op. cit.*, 13-4.

<sup>59</sup> C. S. Lewis, ‘Fern-Seed and Elephants’, in *Fern-Seed and Elephants*, 113.

<sup>60</sup> Farrer, *St Mark*, 358-9. See my *Three Gospels*, 183-96.

must be allowed to predict the future. Then all shall be well, and we lose our impasse. Eusebius can be allowed his Claudian date after all.

But in fact we can go further than that. We can actually square the circle and offer an olive branch to those who in spite of Farrer find themselves constitutionally unable to accept the phenomenon of prophecy.

It is evident from the Gospels that Jesus was exceedingly well versed in the Hebrew Scriptures. As such, he would have been fully aware of the conditions upon which his people were granted by God the use of the (first) Temple, as specified to Solomon in 1 Kings 9:1-9: the Israelites were required to exhibit integrity and obedience to God's decrees and laws, and to remain true to their ancestral religion. A devout Jew of Jesus' day might well have seen in the destruction of the Temple by the Babylonians in 586 BC a fulfilment of God's warning to Solomon on those terms (Jeremiah 37:1-2; 52). That—as he might have concluded from the history of his people—is the way the world is, and Jesus was astute enough to know it. In Luke 13:34 he famously wept over Jerusalem, in effect, for the apostasy which he saw there. In his perception, the Israel of his day had similarly gone too far, in which case destruction of the Temple was as inevitable as it had been in 586 BC. This was a logical deduction from the spiritual state of his contemporaries, as seen by the eye of faith.

So for those who have a problem with prophecy, we can offer an alternative: the application of simple logic. In predicting the destruction of the Temple, Jesus was just reporting what to him was patently obvious. This presents us with what may nowadays be described as a 'win-win' solution, disposing as it does of what appears to be the principal motive for dating Mark in relation to the Jewish War and the consequent destruction of the Temple. So all that ink that has been spilled in favour of late dates for Mark has been spent in vain. We could have trusted the historical evidence after all.

## CONCLUSIONS

### DATE OF MARK'S GOSPEL

Having now a perfectly satisfactory alternative explanation for what had appeared to be prophecy, we can revert to the Claudian date after all. No longer need we rely on sleight of hand like Kümmel, diverting our attention from the key passage which refutes him. We don't have to suppress our own realisation of a major flaw in our chronology and hide it in a footnote like Brown. We can avoid the embarrassment of a *reductio ad absurdum* that follows a general rejection of prophecy in general. We can escape confrontation with the cohort of Early Fathers such as those listed by Giella (p.5) and others, who are united in their witness that Mark wrote when Peter was still alive. We can spare ourselves from Hengel's dilemma, riding two horses at once, as if we have to choose between two radically different dates, forcing us to reject the one that in terms of *historical evidence* has the more persuasive case in favour of the other that has none. Instead, we are free to accept Eusebius' powerful evidence for a Claudian date for Mark with a clear conscience and no loss at all to our academic integrity. Wherever we come from, we win. Is that really so bad?

We have seen our estimate of the date of Mark steadily refined from having been written *before* Peter's death in Rome (probably 67), and the well-supported date of Acts (62-63) which Mark's Gospel preceded, to within a range bracket supplied by Eusebius' account of the reign of Claudius (41-54); *after* the probable arrival date of Peter and Mark in Rome on their first visit (42, 25 years *before* Peter's martyrdom), which very plausibly overlapped with an independently attested visit to Rome by Philo of Alexandria at the start of Claudius' reign, where a *cluster of details* converge, as it were, like iron filings to a magnet. Peter will have met Philo *before* the latter's death (c.50). Again, Mark will have written *before* that Emperor's expulsion from Rome of "the Jews" (49 or 50); and then again *before* Mark is found back in Jerusalem c.46, shortly *before* leaving with Paul and Barnabas on Paul's First Missionary Journey in 47.

This accords with the overwhelming record of the Fathers, including such as Theophylact who effectively gives us a date of 43. Conversely, later (Neronian or Flavian) dates which have underscored the 'majority view' find no support in the ancient sources. They also imply later dates for Matthew, Luke and Acts (after the fall of Jerusalem in 70) which are also unsupported in the Fathers and today look increasingly unlikely. Instead, we can boldly look Robinson in the eye and acknowledge the wisdom of his perception: none of our proposed Gospel dates forces us to look *backwards* upon the fall of Jerusalem and destruction of the Temple as past events. This would solve Brown's problem.

*Our best estimate then for the date of Mark's Gospel is Edmundson's of AD 45.*

Advocates of a later date for Mark, like Hengel, have tended to set much store by a passage in Irenaeus which appears to indicate that Mark wrote after Peter's death. This turns out to have been a misreading; since Chapman in 1905, it has been evident that Irenaeus in fact indicates the reverse, as does the variety of stories purporting to give Peter's reaction on seeing Mark's *opus* later on.

Objections to our date based on prophecies attributed to Jesus in Mark 13 tend to rest on the personal philosophies of their proponents rather than historical evidence and fail under logical analysis. But if Jesus' words were founded upon deductions from the Hebrew Scriptures which had already been fulfilled once, they ought to trouble no one. The quotation from Hengel about bad habits which heads this essay applies. We need to move on.

Should it turn out that Mark's Gospel was not written then after all, we are left with a conundrum: *how was it that so many diverse sources should have combined so convincingly to indicate that it was?*

#### A PLEA FOR HISTORY

The early date for Mark that we have resurrected in this paper, if accepted, is likely to have knock-on effects on our recalculated dates for the other Gospels, and indeed the rest of the New Testament. So I suggest that a reissue of Chapman's historic paper of 1905 – and for that matter a completely revised edition of Edmundson – is long overdue. We can then rework our New Testament chronology, finally addressing Robinson's bold challenge, of which Raymond Brown reminded us, to take into account the significance of the year 70.<sup>61</sup> R. B. Moberly's paper on the date of Acts also deserves a higher profile than it has so far been accorded.

In the course of our explorations, two issues of historical practice have surfaced. The first is the reluctance of modern scholars generally to engage with the historic evidence upon which our understanding of New Testament history rests. This is the point raised by Robinson at the head of this essay. It comes up principally in two contexts.

We have seen the failure of an assortment of scholars to take any account of the one writer who supplies the original dating bracket within which our solution lies: Eusebius, whose account of the reign of Claudius in *Ecclesiastical Histories* Book II is frequently passed over without comment. It would be difficult to find a charitable explanation for this that was not ungracious. Accusations of ignorance, incompetence or *suppressio veri* are unprofessional and do not read well. But it is still true that, as noted above, Papias, sometimes alongside the Fathers generally, habitually gets an undeservedly bad press these days among the source critics and other scholars; to the detriment in my opinion of their own solutions to the Synoptic Problem. It would be good to reinstate him.

Conversely, in the case of Q, a similar class of source critics have built an immense edifice which rests upon no historical foundations whatever; and, worse, they seem totally unconcerned by this.

It does seem that certain schools of New Testament scholarship exhibit a prevailing floccinaucinihilipilification in respect of historical evidence. But as a means of understanding history, rejecting a high proportion of our ancient sources does not have a lot to commend it. It is this lack of regard for ancient evidence that I suggest underlies Robinson's lament at the head of this essay about the alarming change in attitudes to chronology which had already taken place in his day.

Second, the dogmatic suggestion that the report of any correct prophecy must always indicate a *vaticinium ex eventu* needs to be challenged. So does the equally dogmatic rejection of miracles. We have left the twentieth century.

Let us hope for better things.

Martin Mosse,  
July 2020.

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<sup>61</sup> I have made some suggestions on how to go about this in the section 'How to do New Testament History' of B. Wiebe, *Christian Origins*, 32-33. Robinson, *Redating*, Wenham, *Redating*, and Earle Ellis, *Documents*, will all pay dividends as starting points.

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