

***25/6/1992***

***“Il Vangelo di Marco: sue  
origini e trasmissioni”***

***a cura di  
Carsten Peter Thiede***

THE ORIGIN AND TRADITION OF THE GOSPEL OF MARK  
IN THE LIGHT OF RECENT INVESTIGATIONS

There appears to be a consensus in recent research: The Gospel of Mark was written just prior to the destruction of Jerusalem and the Temple, that cataclysmic event which took place in AD 70 <sup>(1)</sup>. Mark's way of dealing with the prophecy of the destruction does not appear to presuppose the actual event. Even so, a period of some 40 years is at our disposal - for if we argue as literary historians, any time between the crucifixion and resurrection of Jesus and the eve of the destruction of Jerusalem would be possible, quite close or, indeed, still quite distant to the life of the historical Jesus <sup>(2)</sup>.

Of course, one may well ask: Does it matter? For those of us who find the arguing about dating or redating the New Testament rather boring, and for those who do not regard the gospels as primary sources of authentic, investigate information anyway, it is a subject of - at best - secondary importance. But as historians - and every New Testament critic should be a historian, too - we must try and get as close as possible to the primary evidence, to the eyewitness material, to reliable oral and literary tradition. Those 'great expectations' which we do not hesitate to cherish in dealing with 'secular' sources, we must apply them in New Testament matters as well. After all, it is one of the long-lived myths of New Testament criticism that it is the New Testament, i.e. the Gospels, which confound their theological message for historical story-telling. No-one who has ever read the prologues to Tacitus' "Agricola", to Livy's Roman history "Ab urbe condita", Thucydides about his approach to reporting speeches, or, closer still to the world of the New Testament, the liberal, cavalier attitude to research exemplified by Josephus Flavius, will seriously dare to accuse Mark, Luke, or any of the others of tendentious, unhistorical writing.

In such a context, the search for datable origins gains additional weight. And here it is precisely Mark's gospel which has profited by the results of recent research, covering a wide area from the Dead Sea Scrolls of Qumran to Irenaeus of Lyons.

## I

Ever since the Spanish papyrologist José O'Callaghan had suggested the identification of a tiny papyrus fragment from Qumran Cave 7 with two verses ~~of~~ Mark's gospel, the date of Mark and its context with Qumran have been on the agenda. Admittedly, a majority of critics approached the question with gritted teeth, and others tried to ignore it, but it just did not go away. At the Catholic University of Eichstätt in Germany, an international symposium was held last October and agreed that a surprising number of arguments can be mustered in favour of the identification. A leading papyrologist, Herbert Hunger of Vienna, analyzed the most controversial of the twenty visible and partly damaged letters by comparing them to contemporary Greek papyri, and he concluded that none of the solutions suggested by O'Callaghan, by himself and others contradicts the available evidence from first century papyri. <sup>(3)</sup> Even he, however, expressed the desire to see one particularly difficult letter clarified by further investigation:

In line 2 of the fragment, two clearly legible letters, a tau and an omega, are followed by lines and rests of lines which - according to José O'Callaghan - form a nu, but which, according to his critics, are a iota, in form of a iota adscriptum, linked with the preceding omega, and followed by traces which could be remnants of an alpha. Marie-Emile Boismard had suggested this combination of his letters in the first edition of the papyrus back in 1962 <sup>(4)</sup>. However, his suggestion remained a fruitless exercise, since he failed to suggest any text to which these and the other letter of the fragments would fit.

At this stage, we must remember a vital presupposition of papyrology: suggested identifications of individual letters must remain hypothetical as long as they cannot be connected with a readable text that makes sense. When O'Callaghan tackled the fragment and identified it with Mark 6: 52-53, he found that these remnants in line 2 must belong to a nu, for any other letter or combination of letters would immediately invalidate his identification. Thus, he suggested that nu and published it with a dot underneath, the obvious sign for



a damaged but reconstructed letter. (5) It is well known that O'Callaghan had arrived at his identification of the fragment by using undisputed elements of the papyrus: the letters nu, nu, eta, sigma in line 4, and the complete word kai after a gap, a spatium, in line 2. Only after these initial steps would one have to analyze every other of the 20 visible letters or remnants of letters to ascertain if they contradict or corroborate the identification.

Needless to say, there was and perhaps still is a lot to discuss in view of some 50% of the fragment consisting of partly or severely damaged letters. But a recent consensus, once again underlined at the Eichstätt symposium mentioned before, points in the following direction: All damaged letters could indeed fit O'Callaghans identification, with the one exception of those traces in line 2. Or, in the words of Gordon D. Fee:

"For me, this is the whole issue - the other items can all be spoken to, but this one is crucial." (6)

Can the identification of a fragment of the gospel of Mark really depend on one single letter? Could a whole edifice of consequences be made to rest on a possible nu? A fair question, certainly; and yet, the papyrologists must not be detracted by theological, exegetical or text-critical consequences of their work. O'Callaghan certainly never speculated about the effects a Gospel of Mark written prior to AD 68 and deposited at Qumran would have on New Testament scholarship.

We have a recent example of problems caused by a single letter in a small papyrus: In 1989, Hannah M. Cotton and Joseph Geiger of the Hebrew University Jerusalem published a papyrus found on Masada, the Jewish fortress overlooking the Dead Sea.

It was a scrap of 16 x 8 cm, with one incomplete line in Latin on each side. (7) The text on the recto, fifteen visible letters, two of them severely damaged, could be identified as Virgil, Aeneid 4:9. The text on the verso, also consisting of fifteen letters, three of them damaged. One long and rare Latin word is clearly legible: "titubantia", which means wavering or stammering. And yet, the line could not be identified, not even with the help of the Ibykus computer programme of extant Latin literature. Things might be different if the beginning of the

incomplete line could be read otherwise. Cotton and Geiger had suggested a D followed by and E and a S. The D is only partly visible, the E and the S are unambiguous. How tempting it would be to suppose that the D is in fact an L and the E an I: With LIS instead of DES, the line could be identified as Calpurnius, Eclogae 5:4 -

TALIA VERBA REFERT TREMULIS TITUBANTIA LABRIS.

However, the D is likely and the E is evident; we have to admit that the verso of the Latin Masada fragment cannot be identified as part of extant Latin literature.

Even so, this is more than merely a negative result. For our present purposes, we can draw at least two helpful consequences from Masada Fragment 721:

1) An old myth, perpetrated, among others, by Kurt Aland, must finally be laid to rest. The myth can be summed up in Aland's own words, first uttered in 1974<sup>(8)</sup> and reprinted, curiously enough without any correction, in 1990<sup>(9)</sup>. Aland describes his method of identifying small papyrus fragments and concludes:

"Fragmente von Rollen kommen (...) für das hier beschriebene Verfahren nicht in Betracht, denn hier fehlt die Kontrollmöglichkeit durch den Text auf der Rückseite."

(Fragments from scrolls are out of the question as regards the procedure described above, for the possibility of controll by means of the reverse is missing here." )

The Masada fragment 721 warns us against relying on the verso of fragments for purposes of identification. Even though both lines were written by the same scribe and on the same height of the the papyrus, they obviously do not belong to the same text. An attempt to identy the recto with the help of the verso would have failed dismally. The text one the recto had to stand on its own feet, and it would have done so successfully even if not a single trace of ink had remained on the other side. This conclusion is all the more important as Aland linked his erroneous theory with an attempt to denigrate A'Callaghan's 7Q identifications. For Aland, those tiny scraps from cave 7 remained unidentifiable not least because of the missing controll text on the verso. He then qualifies his statement by adding:



"Bei ihnen (d.h. bei Rollenfragmenten) bedarf es für eine Identifizierung entweder der von vornherein feststehenden Zugehörigkeit zu anderen Fragmenten (...) oder eines ausführlichen Textbestandes, der ein sicheres Urteil erlaubt."

(In their case (i.e. with fragments from scrolls) one needs for an identification either their unity with other fragments, established beforehand (...), or an extensive amount of extant text which permits a safe judgment.) (10)

None of these criteria is met by the recto of the Masada fragment. Fifteen letters, two of them severely damaged, on one line: No one would want to call this "ausführlichen Textbestand"; and a "Zugehörigkeit zu anderen Fragmenten" is not given, either. What is more: no-one in his right mind would ever have thought of finding a fragment from Virgil on Masada: It was at least as unexpected as a New Testament fragment at Qumran. Thus, it did not even belong to an expected topographical context- no "Zugehörigkeit" here - unless, of course, one began to think about it, as one had to do in the context of Qumran, as well. In short: Cotton and Geiger reconstructed a Virgilian hexameter of thirty-nine letters from fifteen fragmentary ones. To put it differently, they had to work with a mere 38.5 % of the line which they then managed to identify. They did have neither a verso, nor a textual or socio-topographical context to help them. But they succeeded.

And this leads us to the second helpful information to be gained from Masada fragment 721:

2) We have to bid farewell to the myth of foreknowledge.

One just knows what can be found or what cannot be found. To quote Aland again: He knows that "es einen Papyrus mit dem Text des Markusevangeliums (vi. 52-53 z.B. gehört zur markinischen Redaktion, setzt also das fertige Evangelium, nicht eine von Markus benutzte Vorstufe voraus) aus der Zeit um 50 n.Chr. <sup>(in Qumran)</sup> nicht gegeben haben kann, es sei denn, man setzte die Niederschrift des Evangeliums etwa um 40 n.Chr. an. Denn gleich wo das Markusevangelium verfaßt wurde, in Palästina ist es mit Sicherheit nicht entstanden, es muß also ein gewisser Zeitraum - der mit 10 Jahren noch sehr niedrig angesetzt ist - für die Verbreitung vom Abfassungsort bis nach Palästina angenommen werden.

Wenn 7Q7 mit Mark xii. 17 von O'Callaghan noch vor 50 n.Chr. angesetzt wird, rückt die zeitliche Ansetzung des Markus-evangeliums in eine noch frühere Zeit. Ein solches Datum will schlechterdings nicht zu den Resultaten der neutestamentlichen Einleitungswissenschaft passen (...)." (11)

(There cannot have been a papyrus with the text of Mark's gospel (vi. 52-53, for example, belongs to the Marcan redaction and therefore presupposes the complete gospel, not a preliminary stage used by Mark) from the period of about AD 50 at Qumran, unless one places the composition of the gospel at about AD 40. For, no matter where the Gospel of Mark was written, it did certainly not come into existence in Palestine, so that a certain period of time - for which ten years are a very moderate calculation - must be assumed for its spreading from the place of origin to Palestine. If 7Q7, with Mark xii. 17, is placed by O'Callaghan to before AD 50, the dating of Mark's gospel is moved to an even earlier period. Such a date just cannot be reconciled with the results of New Testament Introductions.)

It could perhaps be regarded as unfair to illuminate the naiveté of an otherwise highly respected New Testament scholar by quoting from an article first published 18 years ago; but Aland and his disciples seem to be convinced that this still is the correct approach to 7Q and New Testament criticism. They included this very article, with not the slightest alteration, in the "Supplementa" published in 1990. (12)

Let us look at the unscientific presuppositions heaped upon one another in these few lines:

- He knows that there cannot have been a papyrus of Mark from about AD 50 at Qumran (unless the gospel was written in the 40s);
- he knows, even with certainty, that Mark was not written in Palestine;
- he knows that therefore (!) there must have been at least ten years to make it possible for the gospel to reach Palestine from its place of origin;
- he insinuates that O'Callaghan regarded fragment 7Q7 as a certain fragment from chapter 12 of Mark's gospel - which O'Callaghan never did (13), and then proceeds to infer from his own impropriety that O'Callaghan and his followers would



- argue for an even earlier date of the gospel;
- and finally, he tops it all by knowing what fits and what does not fit the " Resultate der neutestamentlichen Einleitungswissenschaft".

None of these self-confident positions is fully accurate, and some even fly in the face of international research to such an extent that one wonders how this paper could be reprinted in 1990. The real problem, however, is the damage such presuppositions have done and still do to the progress of a cultivated debate on the textual tradition of literary documents from antiquity. In such a context, the Virgil fragment from Masada helps us to correct the myth perpetrated by Kurt Aland and to concentrate on the facts.

Before fragment 721 was found and identified as Virgil, Aeneid 4:9, it would have been a risible idea to suggest Virgil on Masada, the Jewish nationalistic stronghold, where Zealots and Essenes met in a final stand against the occupiers of their country. In fact, had Hannah Cotton and Joseph Geiger been of Kurt Aland's ilk, they would have ruled out the mere idea as preposterous even before it might have occurred to them ... However, they acted as scholars and papyrologists and let the facts speak for themselves. Once the tiny fragment was identified as Virgil, they then proceeded to analyze the follow-up question: How and when could it have got there ? In other words, they did not try to argue it away (nor, in fact, has any other scholar worldwide, as far as I know); they much rather tried to find convincing answers to given data. The answers they give, in the end, take care of the literary and socio-topographical world as far as it can be reconstructed. They established that the Latin fragments found at Masada -all of them in one place, the so-called locus 1039, had to come from the Roman camps around Masada, that the "collection" (with papyrus 721 as the only literary text) was created shortly after the fall of the fortress in Spring 73 or 74, and that they were written in situ, i.e. in the camps, just before that fall, and that this ~~is~~ corroborated by palaeographical comparison <sup>(14)</sup>. Furthermore, they ventured the suggestion that it cannot have been a writing exercise since ~~that~~ line from the Aeneid does not contain the characteristics usually chosen for such exercises, but that it probably was a line addressed to



someone called Anna - in which case "soror" would have erotic undertones not unknown in Latin literature; or that verse 4:9 reflects the writers feelings of horror at what he had witnessed at Masada. (15)

# ANNA SOROR QUAE ME SUSPENSAM INSOMNIA TERRENT -

Anna, my sister, what nightmares terrify me in my anxiety - the first line from Dido's first speech, found at Masada, in what now is the oldest extant Virgil papyrus, takes on a whole range of meanings which do justice to the context of the discovery, and, as Cotton and Geiger helpfully add, they also contribute to our understanding of the transmission of literature in the Roman army, perhaps even of the literacy and education of Roman soldiers.

The date given to the Masada Virgil, 73/74, is a mere five to six years later than the terminus ante quem for any text found at Qumran (over against later texts found in the Wadi Murabba'at, the Nahal Hever, and so forth). And Masada is within easy walking distance of Qumran. The same world, it seems, and yet, the classical philologists dealing with the Latin and Greek finds at Masada appear to be capable of a balanced, fact-orientated approach to their material that has not yet permeated a surprising number of New Testament critics. On sober reflection, the identification of a fragment from the gospel of Mark at Qumran could be interpreted along the same lines which helped us to understand the Masada Virgil:

- Texts could reach destinations across the Mediterranean within weeks. There was of course the imperial post which carried mail for civil servants and the military. And there were tabellarii, letter-carrying messengers, (16) as well as individuals who acted as voluntary couriers (17). They could reach the Italian harbour of Puteoli from Corinth in five days, or, like Cato once did, Africa from Rome in under three days. (18) A place like Masada was not cut off from the outside world, and the writer of fragment 721 could have sent those lines to his "Anna", had he intended to <sup>do</sup> so, as much as the officer Iulius Lupus could receive a letter sent there and documented in Masada papyrus No. 724.

One wonders why and how that old legend is still being kept alive by Aland and others, that one has to reckon with at least ten years for a gospel to be spread from its place<sup>of</sup> origin to a place in Palestine. Let us assume for a moment that Mark's gospel was written in Rome - I think, with the majority of scholars, that it was indeed written in Rome, although the American scholar E. Earle Ellis has just resuscitated and corroborated arguments in favour of an origin in Caesarea Maritima, thereby demonstrating, at least, how shaky Aland's certainty about ruling out Palestine really is<sup>(19)</sup> let us, assume, for our present purposes, that it was written in Rome: and we may rest assured that it could have reached a Palestinian harbour within a fortnight. If it was written in Rome, the Roman church would of course have done everything in its power to see it distributed to the communities in the Holy Land as soon as possible. It was, after all, no "secret gospel" . And, needless to say, it would have taken care to send more than one scroll - after all, there was more than one community to supply. Aland states elsewhere in his paper:<sup>(20)</sup> The possibility of four different fragments from Mark, by four different scribes and therefore from four different scrolls - a theory, incidentally, that is not shared by any other papyrologist who has analyzed those fragments - such a possibility "would go beyond the scope of phantasy and turns O'Callaghan's hypothesis into a chimera". Does it really ? Should the "chimera", the illusion, not much rather be seen in the unreal idea that a church interested in the distribution of its documents sends only one copy to one of its central communication places, and that it takes ten years to do so ? The historical facts tell a different story. If the Christians in Rome (or Alexandria, or wherever) had intended to spread the news, the gospel, to their home communities, the whole process of getting it there and of passing it on to, say, a target group like the inhabitants of Qumran, was indeed a question of a few weeks. That concept of at least ten years is so preposterous that it should be relegated to the scrap heap of critical legends as soon as possible.

And if we continue with the obvious next question, how and why Qumran, of all places, we encounter answers that are at least as plausible as those offered by Cotton and Geiger for



the existence of a Latin literary text on Masada.

There is one characteristic of the early Christian communities which has never been doubted by historians: It is their immediate concern to talk to others about Jesus as the suffering and risen Messiah. But not every day was Pentecost, when thousands of pilgrims came to Jerusalem and could be used as easy target groups. One had to think strategically, to begin where success was at least conceivable. In other words, a group like the Sadducees would have been ruled out, initially at least: The Sadducees did not even expect a Messiah - any Messiah, that is, regardless of who the claimant may be; and they refused the mere possibility of a bodily resurrection. (21)

On the other hand, there was one fellow Jewish community which could be approached on these counts - the Essenes (22).

Our knowledge of their thinking and their theology is increasing rapidly, thanks to the general availability of all Qumran texts. The recent dispute about the tiny fragment BM5, which, according to Robert Eisenman, tells the story of a suffering Messiah (23), is a case in point. However, we still do not know what the Essenes called themselves. The name we and contemporary authors like Pliny the Elder and Josephus Flavius give them, is not documented in their own writings. Thus, we should not be surprised that they are not mentioned by name in the New Testament. But there can hardly be any doubt, by now, that they are referred to more than once. Most importantly, they appear as converts to Christianity:

"So the word of God spread," it says in Acts 6:7, "The number of disciples in Jerusalem increased rapidly, and a large number of priests became obedient to the faith."

Given the fact that the Pharisees had no priests, and that the Sadducees, with their priests, theologically refused all basic presuppositions of a messianic message, only one group remains: the Essenes. Luke's statement in Acts is becoming increasingly trustworthy thanks to the latest analyses of Qumran texts, and of archaeological investigations into the living quarters of the Essenes in Jerusalem. (23)

In brief, it cannot only be shown that the first Christians and the Jerusalem Essenes were next-door neighbours on the south-western hill of Jerusalem, today's Mount Zion.

It can also be shown that they had a common gate on that hill, the so-called "Gate of the Essenes" mentioned by Josephus <sup>(24)</sup> and recently rediscovered <sup>(25)</sup>, the very gate through which Jesus in all probability entered Jerusalem on his way from Bethlehem to the Last Supper <sup>(26)</sup> - a Last Supper, incidentally, which may well have taken place in an Essenian guest-house <sup>(27)</sup>.

If we consider these two elements - the evidence for the conversion of a number a leading Essenes, and the archaeological evidence for <sup>their</sup> vicinity in Jerusalem, with a gate that connected Jerusalem with Bethany, but also with Bethlehem and, of course, with Qumran itself, we arrive at a scenario which makes the existence of New Testament documents among the Dead Sea Scrolls from Qumran not only likely: it makes it almost mandatory.

The first Christians would have resumed the contacts they had with the Essenes during the lifetime of Jesus - contacts as normal and unsuspecting as anything <sup>(28)</sup> - immediately after Pentecost, and they would have used their first chance to hand them the first documents. If it had not happened before, they could have done so in AD 66 at the latest - in that year, the Jerusalem Christians fled the city, during the initial stages of the uprising against the Romans, and went to Pella in Transjordan <sup>(29)</sup>.

Since Cave 7 is obviously a collection or library in its own right - with exclusively Greek texts exclusively on papyrus - , it is not unlikely that the fragments found in that cave represent the remnants of a Christian collection handed over to the Essenes for perusal and safekeeping. In fact, we may assume that the carriers of that collection were to be found among those priests who had converted to the Christian faith according to Acts 6:7 .

We have drawn a wide circle, with Masada fragment 721 as <sup>our</sup> starting point, to arrive back at the actual cave and the actual papyri. We have seen to what extent the conclusions to be drawn from the Masada Virgil can be applied to the Qumran Mark. Having established, to put it bluntly, that a copy (or several copies) of Mark, and possibly <sup>of</sup> other New Testament writings at Qumran is not only to be expected, but highly probable in the first place, we now have to tackle



the question with which I started: Is there a copy of Mark at Qumran, in form of papyrus fragment 7Q5, and can the problem be solved by analyzing that one decisive letter in line 2 ?

Since 1955, a handful of scholars have had access to the original papyrus, among them José O'Callaghan, , his severest opponent Pierre Benoit, and myself. Others have worked on the basis of photographs. It has to be admitted that none of these methods is absolutely fool-proof. Least reliable, of course, is the analysis of photographs. A German scholar, Hans-Udo Rosenbaum, managed to distort the factual evidence of the papyrus by relying on an insufficient photo of 7Q5<sup>(30)</sup>. A computer reconstruction of 7Q5 by the Australian papyrologist Stuart Pickering arrived at impossible results, because it relied - admittedly - on photographs. In fact, even one of the greatest papyrologists, Sir Frederick Kenyon, committed a similar mistake in his edition of the Chester Beatty codex p<sup>46</sup>: Editing the codex on the basis of photographs, he read a "X" on plate 74 v, where there clearly is none on the original papyrus folio<sup>(31)</sup>. I would myself have fallen into that trap if I had relied on photographs alone when I edited the first edition of the New Testament papyrus p<sup>73</sup>, the Bodmer L. On the photograph with which I had been supplied by the Bibliotheca Bodmeriana, there is <sup>a</sup> clearly visible horizontal line underneath line 2 of the recto. What a wealth of speculations could have been based on that line ! However, when I went to Cologny-Geneva to work with the original papyrus, I quickly established that there is no such line on the recto (nor on the verso, for that matter): It just was a technical error on the photographic plate - not the first one, and probably not the last one, either, which will be encountered by papyrologists. (32)

What about the reliability of <sup>to work</sup> working with the original papyrus, then ? I have been able <sup>to work</sup> with the original 7Q fragments on four separate occasions, , and the results of these visits to the John-Rockefeller-Museum in Jerusalem have helped me to clarify and improve my publications (33). But perhaps even here, truth - like beauty - lies in the eye of the beholder.

Benoit, who saw the original, insisted on his "alpha", and Baillet, who also claimed to have seen the original, called the possibility of a nu "absurd" and "impossible" <sup>(34)</sup>. They continued to insist on a iota as iota adscriptum, followed by what they thought could be an alpha.

After Herbert Hunger's comparative analysis of letters in 7Q5 and other fragments from Cave 7 and further contemporary texts, demonstrated at the Eichstätt symposium last October, I saw only one immediate avenue that could lead to additional clarification: an investigation with the tools of forensic science. I applied to the Director of the Division of Identification and Forensic Science, at the Investigations Department of the Israel National Police, Brigadier-General Dr. Joseph Almog. He immediately offered his services, and when the Board of Directors at the Israel Antiquities Authority, in charge of the scroll fragments kept at the John-Rockefeller-Museum, gave their permission to have the plate with the 7Q fragments taken out of the museum and to the police laboratory in another quarter of Jerusalem, the official preparations were made. One of the the Curators of the John-Rockefeller-Museum, Joseph Zias, accompanied me with the plate to the laboratory at Sheikh Jarakh. It was Sunday, the 12th April - a working day in Israel, of course.

The first step of our procedure was a simple routine measure: an infrared scan of the papyrus to look for later alterations. The result was clear: all ink left on the fragment is original. On the other hand, the papyrus is torn on the right <sup>hand</sup> side and turned upwards to the right. Since <sup>this</sup> is an unnatural occurrence, not a consequence of natural decay, a theory may be advanced which can also be applied to a similar phenomenon in Cave 4: There, too, jars were broken and scrolls torn - an intrusion which was not due to the shepherds who found the jars, but, as archaeologists can substantiate, to the Roman Tenth Legion "Fretensis" which overran Qumran in AD 68 and apparently found and opened some of the caves.

(That shepherd boy, incidentally, who is credited with the discovery of the first cave in 1947, is yet another long-lived myth: A few months ago, he celebrated his 75th birthday <sup>(35)</sup>).



As a quick calculation reveals; he thus was thirty years old when he found Cave 1. Quite some age for a boy !)

The second step of our forensic analysis was the scan under a Sony stereo microscope which enabled the police officers at Sheikh Jarakh to illuminate every angle under any magnification desired. Since time was scarce on this occasion, we concentrated on that single decisive letter in line 2. Chief Inspector Sharon Landau, who operated the microscope, soon found the key to the riddle:

A diagonal line became visible which descended from the top of the left vertical stroke (that stroke, remember, which Benoit, Baillet and others think is a iota) and continued towards the right-hand bottom. In other words: The typical diagonal stroke of a nu in Greek, or N in Latin writing, to this very day. The first third of that stroke could be made visible. Enough to establish its straightness and to rule out any alternatives, such as the rho which Stuart Pickering had suggested instead of the alpha <sup>(36)</sup>.

If therefore that letter "is the whole issue", in the words of Gordon D. Fee, then, of course, the issue is now settled: Since the letter must be a nu, O'Callaghan's original identification can be regarded as safely substantiated. Qumran fragment 7Q5 is Mark 6: 52-53.

Needless to say, the success of this initial analysis has encouraged people at the John-Rockefeller-Museum, at the forensic laboratory of the Israel National Police, and myself, to continue along those lines. First of all, a computer print out, in colour, of the magnified detail will be published in a couple of month's time as an appendix to the documentation of the Eichstätt symposium <sup>(37)</sup>. We shall then continue to scrutinize the whole fragment, carrying on where we left it in April: there, the last comparison we just managed to conclude was that between the different etas in 7Q5 - a comparison which led to the conclusion that the remnants of ink partly destroyed by the gap in line 2, on the right hand side, must belong to an eta: yet another corroboration of the Markan identification.

Even so, one has to realize that lines of ink which once

must have been there cannot yet be made visible again - at least not with existing technical equipment. For example, the left vertical stroke of the kappa in line 3 is no longer complete, but it once must have been, and the same is true of the link between alpha and iota in the kai of line 3, or of the right horizontal stroke of the nu in line 2.

It may well be that the renewed visibility of the diagonal stroke of our decisive nu in line 2 is due to the scribe almost "incising" the papyrus with his nib in the process of beginning that stroke on the top left. In fact, his action may even have caused what looks like a "warping" of the papyrus material just at the very top of the stroke.

A third follow-up step will then be the analysis of the other 17 papyrus fragments from cave 7, and of the one reversed, "mirror-image" imprint on clay. Fragment 7Q4 is continuing to grow in acceptance as 1 Timothy 3:16-4:3 (38). But there are others, smaller and very small ones, and in their case, a close examination under laboratory conditions may well yield further letters or clarify doubts about damaged ones.

## II

In spite of some recent doubts or claims to the contrary (39), there is sufficiently solid evidence for the archaeological date commonly given to the closure of the Qumran caves, AD 68. This is the latest possible date for the deposit- needless to say, the date of copying and the date of composition must each be earlier still. Since palaeographical comparison has shown that fragment 7Q5 was written in about AD 50 (40) - at one stage, C.H. Roberts had even stated that AD 50 was the latest possible date (41) - a surprisingly early date for the origin of the complete gospel of Mark was brought into focus from a different perspective. From a different perspective: for there had always been sober New Testament scholars and historians who had dated Mark to the 40s of the first century - for different reasons, but with solid arguments (41). The most influential of these scholars may have been John A.T. Robinson, with his "Redating the New Testament", first published in 1976. John Wenham, in his "Redating the Synoptic Gospels", continued this line of thought in 1991 (42), and <sup>(c. 1987)</sup> a leading proponent of Matthean priority, Bernard Orchard, also dated Mark to the mid-forties (43).



Most of the reasoning for such a date can be and was <sup>(based)</sup> on a re-evaluation of evidence from church history. An important ingredient is to be seen in the reliability of the tradition of a first visit of St. Peter's to Rome in AD 44, i.e. in the second year of the reign of Claudius <sup>(44)</sup>, a visit which lasted for only about two years, until the apostle was free to return to Jerusalem after the death of Herod Agrippa in AD 44 <sup>(45)</sup>. After several stopovers, he was certainly back in Jerusalem in time for the so-called Apostolic Council in AD 48.

It had always been clear to me that another ingredient in this line of reasoning is the famous statement by Irenaeus, in his "Adversus Haereses" 3,1,1, where he says that Mark's gospel was written (and passed on) meta de tēn toutōn éxodon, i.e. after the exodos, of the apostles Peter and Paul. For philological reasons alone, one had to doubt that this statement was meant to imply a composition after the deaths of Peter and Paul.

In Greek literature, exodos usually means "departure", "going away". A very obvious example of this usage is known to every reader of the Bible: The Greek title of the second book of the Torah, of the Pentateuch, is Exodos- and no-one would ever have theorized that it is about the death of the people of Israel rather than about their going away, their departure from Egypt.

In the New Testament, exodos occurs three times (Luke 9:31, 2 Peter 2:15, Hebrews 11:22). In Hebrews, it alludes to the Israelites' flight from Egypt; in the other two passages, it alludes to the deaths of Jesus and Peter respectively. But here, a vital condition is met: The context establishes the meaning. No such certainty is provided by the passage in Irenaeus. "Death" or "departure" is precisely the question. One could have argued, as I did elsewhere <sup>(46)</sup>, that a lack of context substantiating the rarer meaning of "death" should be used as an argument in favour of "departure". One could also have argued, as John Chapman did back in 1905 <sup>(47)</sup>, that the grammatical tense employed by Irenaeus for the verb paradidomi, i.e. paradedoken, obviously implies that the message of Peter was passed on after his death by having been written down before his death - otherwise Irenaeus would have used the aorist instead of the perfect tense, or he would have said something else altogether.

Furthermore, Irenaeus uses the present participle "kērysōmenos" to tell us that Luke <sup>wrote</sup> while Paul was still preaching. And if Paul was still active while Luke wrote, this implies, at least according to a philologically correct understanding of Irenaeus, that Peter was also still alive while Mark's gospel was being composed. And this leads us to a date of AD 67 at the very latest <sup>(48)</sup>. So far, one could have gone and did go on philological grounds, by a mere, strict analysis of the passage concerned.

And yet, since J.D. Michaelis' "Introduction to the New Testament" of 1793, commentators have insisted on the death of Peter as the prerequisite for the composition of Mark's gospel. <sup>(49)</sup> Exodos means "death" - this equation has remained stronger than all other arguments.

The breakthrough in this matter came last autumn, when the American scholar E. Earle Ellis delivered a paper at the Qumran symposium held at the University of Eichstätt. <sup>(50)</sup> Again, it was a matter of taking a detail seriously, of asking a sensible questions which should have and could have been asked long ago. Ellis perused the works of Irenaeus to find out if and when this author unmistakably uses exodos to signify "death". The result was as clear-cut as it is surprising to many: Irenaeus never uses exodos when he wants to say "death" <sup>(51)</sup>. { uses For "death", he always | "thanatos". In the same Book III of "Adversus Haereses", he does so no less than 38 times.

Thus, Irenaeus can no longer be quoted, against the apparant convictions of Papias, and Clement of Alexandria, as a witness for a late date of Mark's gospel. Quite on the contrary, he now joins them and even strengthens them by stating quite clearly that this gospel was composed during Peter's lifetime.

As long as it must remain an open question if the so-called "Anti-Marcionite" prologue was written just before or just after "Adversus Haereses", we cannot use the anonymus Latin author to verify or to falsify the results of this analysis. However, since the prologue's expression, "post excessionem". is at least as ambiguous as "exodos", without a proper context, it must be left out of the debate. It will be much more interesting to try and determine what Irenaeus (and possibly the prologue) mean by "departure". Whose, and when, after all ?



The Anti-Marcionite Prologue, interestingly enough, mentions only Peter - no trace here of Paul. In fact, none of the other fathers links Peter with Paul in the context of gospel origins. Could it be that Irenaeus just carried both names over from his general statement that Peter and Paul both founded the Roman church? I think so. <sup>(52)</sup> The construction of these two sentences, following immediately on each other, strongly suggests that "meta de ten touton exodon" is linked with the statement about the establishment of the church in Rome - and, as Irenaeus himself must have known, they did not found that church simultaneously and together, but strengthened and structured it in consecutive visits. Even if one does not accept the likelihood of a first visit of Peter's to Rome in AD 42, there can be no doubt about a chronological precedence of Peter's arrival in the city to that of Paul's <sup>(53)</sup>. Thus, Irenaeus condenses his information, in a conscious effort to highlight the equality of rank and importance of both apostles.

The Anti-Marcionite Prologue, as well as Clement and Papias, who have no such strategical aims, concentrate on Peter and Mark alone. The prologue's sentence has it in sober terms: "Post excessionem ipsius Petri descripsit idem (Markus) hoc in partibus Italiae evangelium."

On the basis of how we have to understand both Irenaeus and the Prologue with their terminology, this narrows down the date of the gospel to the period between AD 44 - the earliest possible moment for a departure of Peter's from Rome, and his return to the city in c. AD 59, after the despatch of Paul's letter to the Romans, where he is not mentioned in the list of greetings. Since there is no trace in early literature of any departures of Peter's from Rome after his arrival in c. AD 59, we therefore can also take both Irenaeus and the Prologue to imply that Peter did indeed spend some time in Rome on a first visit.

It might be possible to narrow down the date of Mark even further. For example, one could argue that Luke betrays knowledge of Mark's authorship of a book about Jesus as early as Acts 13:5, where he calls him "tén hyperétén". It is certainly noteworthy here that he does not call him the

"hyperetes" of Barnabas and Paul, but uses the word like an attribute: Mark, the "hyperetes". And this may of course be an allusion to Luke's usage of the term in his prologue to the gospel, where the "hyperetai" are described as "hyperetai tou logou" and are defined as those who composed Jesus tradition in writing (Luke 1:2). <sup>(54)</sup> Soon after this description in Acts 13:5, Mark suddenly leaves his two companions in Perge and returns to Jerusalem. Why ? No reason is given (Acts 13:13). There could be a rather compelling one, however: Mark could have heard of Peter's return to Jerusalem. For him then, to meet the very source of his gospel and to show him the result of his work, would have been considerably more important than playing third fiddle to Barnabas and Paul. Peter must have been back in Jerusalem by AD 48, for the Apostolic Council; Perge can be dated to ca. AD 46. In this scenario, Mark's gospel was indeed written in the comparably short span of time between AD 44 and AD 46. And perhaps two statements by Clement of Alexandria elucidate the matter further: First, apparently, Peter reacted in a neutral, non-committal way (in Eusebius, HE 6,14:7), but then, on a separate occasion, he endorsed and ratified it for study in the churches (in Eusebius, HE 2,15:2). Could it be that this faithfully reflects the process after Peter's departure from Rome ? Mark and the apostle met in Jerusalem, in c. AD 44. Peter was not quite satisfied with the text he saw, and Mark composed his own "redaction", the second version, which was finally approved by Peter.

None of the known facts would contradict such a reconstruction, but it must of course remain hypothetical. What concerns us in our present context, is something else - it is the conclusion that the re-evaluation of Irenaeus and its consequences, however far one would want to take them, fit the evidence from Qumran Cave 7. In fact, the earlier the gospel must be dated, the easier it becomes to understand two textual peculiarities of fragment 7Q5: The omission of "epi ten gen" makes sense prior to AD 70, when the "ge" was there, for everyone to see, and no confusion was possible with the settlement which the Romans destroyed in AD 70 - before that date, "epi ten gen" would have been sheer pleonasm <sup>(55)</sup>; and the spelling variation of tau for delta at the beginning of "tiaperasantes", not unknown in other contemporary texts, of course <sup>(56)</sup>, makes even more sense before



the Temple was destroyed in AD 70 - for until then, everybody could see the barrier stone prohibiting non-Jews from entering the holy precinct: a stone on which the very word for barrier, dryphakton, was spelled with tau instead of delta .

We are working at a mosaic, with stones still missing, but with a recognizable picture appearing before our eyes. This process should encourage scholars to try and contribute to an even better understanding of Mark's gospel, and, by implication, of the literary tradition of the New Testament.

\*\*\*\*\*