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O'Callaghan's Fragments: Our Earliest New Testament Texts?

by Paul Garnet

In recent issues we have made some editorial reference to the claim by Professor José O'Callaghan that he has identified portions of Mark and other New Testament books in the Greek fragments from Qumran Cave 7. Here an appraising look is taken at this claim by Dr. Paul Garnet, an evangelical scholar who once studied under the editor in the University of Sheffield and is now Assistant Professor in the Department of Theological Studies in Loyola College, Montréal.

1. O'Callaghan's "Discovery"

IN MARCH of this year our newspapers published the exciting report that papyrologist Fr. Jose O'Callaghan had claimed to have found a fragment of Mark's gospel, dating from about A.D.50, amongst the Dead Sea Scrolls.

1. Potential importance of the "find"

If this discovery proves to be genuine, the implications would be of tremendous importance. No longer would our first-written gospel be dated A.D. 65-70, after the death of Peter and Paul, following the almost unanimous consent of New Testament critical scholarship. Instead, we would have to say that Mark was composed within 10-15 years of the events it purports to describe, within the life-time of most of the original eye-witnesses. Theories of a radically developing tradition *before* the date of writing would be exploded, for there would be not time for such a tradition to grow.

Perhaps this is a good time to remind ourselves of the weaknesses in the case for insisting on a late date for Mark. This case stands on two bases:—

1. The miraculous elements in the book represent a somewhat late stage in the development of the tradition.

2. Irenaeus's statement about the date of Mark, the earliest in the writings of the Fathers, places the composition of this gospel *after* the death of Peter. Peter died in Nero's persecution, A.D. 64-66. Each of these bases proves to be quite shaky on examination. The first is an unscientific argument based on an *a priori* judgement against the main content of the book: its witness to the supernatural power of Jesus. As for the second, Papias wrote before Irenaeus. Eusebius (*H.E.* VI 14. 6) quotes a passage from the works of Clement of Alexandria, which states that Mark wrote during Peter's life-time. Eusebius adds "Papias confirms this." Even Irenaeus's statement

can be interpreted as compatible with this (e.g. A. Harnack and, more recently, F. F. Bruce).

2. What are O'Callaghan's claims?

Fr. O'Callaghan claims to have identified several fragments as deriving from the New Testament, four of which he ascribes to Mark's gospel. He did not discover these fragments himself. They have been available to the public since 1962, when they appeared in the primary edition of the smaller Qumran scrolls, *Discoveries in the Judaean Desert* III, Planche XXX. They were unearthed as long ago as 1955 by the team of archaeologists that explored Cave 7 at Qumran. Up to now only two of the Greek papyrus fragments from this cave have been identified: 7Q1 with the Septuagint of Ex. 28:4-7; 7Q2 with the apocryphal Epistle of Jeremy 43-44. What O'Callaghan has done is to offer identifications for most of the rest.

Interest centres round the fragment 7Q5, whose photograph appears on the front page of the June *Eternity* issue, well magnified and in colour. The type of script used in this manuscript is called *Zierstil* by palaeographers, a style which was in vogue from about 50 B.C. to A.D. 50. From a palaeographical point of view *alone*, we would have to say that the most likely date for 7Q5 was A.D. 1, but this would be impossibly early for a fragment of Mark, a book which relates events which took place about A.D. 30. If this fragment comes from Mark, it must be dated towards the end of the palaeographers' limits. Consequently it is unsound to argue that the date of A.D. 50 cannot be taken too seriously, since palaeographical dating is inexact. Palaeographers are aware of the inexactitude of their science and have already allowed for this when they say that A.D. 50 is the latest feasible date for this fragment. If 7Q5 is from Mark, Mark is early.

3. The initial impact

Soon after the announcement in the local press, national magazines both religious and secular, were featuring O'Callaghan's claims. Reports appeared in both *Time* (May 1st) and *Eternity* (June, 1972)! These articles highlighted the potential importance of the "finds". Biblical scholars wisely delayed judgement until they had seen O'Callaghan's article which appeared, in Spanish, in the scholarly review *Biblica*.¹ This article has now arrived on the journal racks of

¹ "¿Papiros neotestamentarios en la cueva 7 de Qumran?", *Biblica*, 53 (1972), pp. 91-100. This is followed by an article, in Italian, by Carlo M. Martini, discussing the date of the fragment and how it came to be deposited in a cave at Qumran ("Note sui papiri della grotta 7 di Qumran", pp. 101-104). These articles have since been translated into English by William L. Holladay (Suppl. to *J.B.L.*, 91 (1972), no. 2).

our libraries and it is possible to form some kind of judgement. What is likely to be its impact?

In the New Testament sessions of the recent annual convention of the Canadian Society of Biblical Studies, held in Montreal, O'Callaghan's article caused not even a ripple on the surface in the formal discussions, though some colleagues ventured a few comments in private conversation. The general feeling seemed to be one of disappointment. There are so few letters on the fragment which have survived the ravages of time that it seems futile to pin the document down to a particular source.

We have already seen that if 7Q5 is from Mark, Mark is early and the consequences for biblical scholarship are tremendous. The question that is now likely to occupy New Testament research is, "How likely is it that 7Q5 is a fragment of Mark's gospel?" To this question we must now turn.

2. Reasons for Reservations

To follow this section of the article the reader will require a copy of the fragments from Cave 7 (e.g. *Discoveries*, Planche XXX, or p. 25 of *Eternity*).

1. The major fragment—the entire letters

7Q5 is identified by O'Callaghan with Mk. 6: 52-53. According to the table given in *Eternity* (p. 28) he considers this identification to be certain. In this fragment only nine letters have remained entire, in the following configuration.

T̄O
KAIT
NĒ
Ē

O'Callaghan maintains that this is a fragment of

- ¹ sunekan epi tois artois
- ² all' en auTON (h)e kardia peporo-
- ³ mene KAI Tiaperasantes
- ⁴ elthon eis genNESaret kai
- ⁵ prosormisthEsan . . .

This reconstruction involves a shorter text than usual of Mk. 6:52-53, also a rather bizarre spelling of *diaperasantes*. These two points will be examined later. Here I just wish to point out that Mk. 6:52-53 is not the only place where the above configuration can be found in the Greek Bible. The writer has found it also in the Septuagint of Ex. 36:10 (39:3).

phura kai sun TO kokkino to dia-
nesmeno KAI Te busso te
keklosmeNE ergon (h)uph-
anton epoiEsan auto . . .

This "find" is a result of searching through Ex.25-31, 36-40, likely chapters for this configuration, since KAI T ("and the . . .") is repeated in them so often. It is possible that the same configuration would turn up several times in the whole of the Greek Bible, but I have neither a computer, nor the patience to look. In any case, our fragment is highly unlikely to have come from LXX Ex. 36:10, for reasons which I shall shortly mention. Whether Mk. 6:52-53 is a more likely source for 7Q5 than LXX Ex. 36:10, I shall leave the reader to judge. At least Ex. 36:10 is from a passage very close to that of 7Q1, which has already been identified as deriving from LXX Ex. 28: 4-7. Now 7Q1 was written in the same script and found in the same cave as our fragment.

2. The major fragment—the partial letters

Although only nine entire letters have survived, the parts of letters remaining at the edge of the fragment give valuable clues. These clues refute the plausible identification just made with LXX Ex. 36:10, which was based on the entire letters alone.

Following the entire letters TO there is an upright letter, or part of a letter, followed by an undulating feature resembling the bottom of an omega. This combination is most naturally read as IO (cf. these letters in 7Q1).

Just before KAI T there is a space preceded by an upright line, curved forward towards its base. O'Callaghan interprets this space as the mark of the beginning of a paragraph at Mk. 6:52-53. It could be due, however, to erosion near the edge, which intrudes upon the line of writing at this point. Following KAI T we have an upright feature, slightly curved. It looks more like the beginning of an omega than anything else. If it is an iota, as O'Callaghan maintains, why is there no tick at the bottom towards the left, as in all the other iotas in 7Q1 and 7Q5?

Preceding NE in the next line we can clearly distinguish the second half of a nu. This identification is virtually certain and proved to be the clue which led O'Callaghan to identify the manuscript with Mk. 6:52-53. The combination NNĒ is much less frequent than NĒ. The most likely place for it to occur is in the genealogies of the Old Testament (*egennēse* = "begat"), but he found no passage there which would fit the configuration of letters in 7Q5. Then, by "a special illumination from heaven" (*Eternity*, p. 26), he decided to look up the New Testament occurrences of *Gennēsaret* and quickly lit upon Mk. 6: 52-53. Eventually he persuaded both himself and his colleagues at the Rome Pontifical Biblical Institute that this identification was correct. The combination NNĒ also occurs, however, wherever a word beginning in NE is preceded by a word ending in N, since these early manuscripts usually leave no spaces

between words. The present writer has checked all instances of this in the Septuagint, but found none which fitted our fragment.

In the last line, the letter following \bar{E} can be either theta, epsilon or sigma.

3. The major fragment—some special pleading?

O'Callaghan is chargeable on two counts with having produced *ad hoc* hypotheses to buttress up his theory.

(a) His supposed text omits the words *epi 'en gen* after *diaperasantes*. This omission has very little support from ancient textual witnesses, but it is just possible that it would commend itself, quite apart from any "evidence" from 7Q5, simply on its own merits.

(b) The bizarre spelling *tiaperasantes* is defended by O'Callaghan by reference to the substitution of tau for delta in many Greek papyri from Egypt. This represented a popular pronunciation there. It is clear, however, that not every delta was changed into a tau, as can be seen even from the examples O'Callaghan has given in *Biblica*. What proportion of taus in Greek papyri represent an original delta? If the answer to this question is a very low figure, the intrinsic probability of this tau representing a delta must also be low and this argument will look like special pleading. KAI T occurs extremely frequently in Greek and no special argument is required to explain its presence in a papyrus fragment.

We conclude that the identification of 7Q5 with Mk. 6: 52:53 is by no means "certain". At best it is "feasible", at worst "impossible", depending on one's weighing of the evidence of the partial letters.

4. The smaller fragments—identifications claimed to be "certain"

7Q4.2 is identified with I Tim. 3:16-4:1. In this fragment we can clearly read "... ON ... AI ... PNEU ... MO" as the final letters of each four lines. O'Callaghan has not yet published his stichometry for these lines. Here is my own attempt.

... apostesontAI
tines tes pisteos prosechontes PNEU
masin planois kai didaskaliais daiMO
nion ...

The only trouble is that, in the place where $\bar{O}N$ would have to fit, we have only *rhēiōs legei hoti en husterois*. Unless O'Callaghan can explain this satisfactorily, his identification is not even plausible.

7Q6, identified with Mk. 4:28, has $\bar{L}\bar{E}$ clearly on the second line, and what appears to be EIT on the first. This configuration does indeed occur in Mk. 4:28, but it also occurs in LXX Isa. 40: 1-2. Why should we prefer the former identification to the latter?

7Q8, which O'Callaghan identifies with Jas. 1:23-24, contains only two undamaged letters, ES, in the second line, but probably the

whole fragment should read, "S . . . ESO . . . LE . . ." O'Callaghan can only fit this into Jas. 1:23-24, however, by omitting the words *gar heauton*, an omission which he admits is nowhere attested in ancient authorities.

The most that can be said for these "certain" identifications is that one or two of them are somewhat plausible.

5. The smaller fragments—identifications claimed to be "probable"

O'Callaghan identifies 7Q6.1 with Ac. 27:38, *ekOUPHizon*. This fragment was found stuck to 7Q6, discussed above. It is interesting to note that the combination OUPH also occurs in Mk. 1:2-3 *sOU, PHonē*. Could these be two parts of the same scroll of Mark, one being the next turn of the scroll to the other? Hardly, for too many columns of papyrus would separate Mk. 1:2-3 from Mk. 4:28, giving a scroll with too big a diameter.

He identifies 7Q7 with Mk. 12:17, but all that can be made out on this fragment is the configuration NA . . . NA. Since the combination NA is extremely frequent in Greek, this configuration is likely to occur quite often, and a positive identification is impossible.

7Q9, where it is extremely difficult to make out anything at all, is identified with Rom. 5:11, 12.

Judging by the published photographs, we must say that none of these "probable" identifications is any more than "possible".

6. The smaller fragments—identifications claimed to be "possible"

O'Callaghan identifies 7Q10 with II Pet. 1:15 and 7Q15 with Mk. 6:48. In these fragments very few letters are clear, so that many identifications would be possible in principle.

It should be emphasised that "possible", "feasible" or "plausible" identifications are of no help to the historical researcher. They have to be at least "probable", viz. more likely than not.

7. A ninefold cord?

"A threefold cord is not quickly broken." O'Callaghan claims to have identified nine New Testament fragments from Cave 7, not just one. Does each identification fortify the rest, by vindicating the researcher's openness to a New Testament source? If any two of the identifications had a probability of over 50%, we might have a case for saying that each identification reinforced the other. As it is, we have only possibilities and feasibilities. No amount of these can produce an increase in probability for the total picture.

By the use of a computer it should be possible to pick out all occurrences of any given configuration of letters in extant Greek

literature. If one occurrence only emerges, there is a considerable degree of probability that we are dealing with a copy of the passage thus located. If none emerges, however, we are still not justified in identifying the fragment with the passage having the nearest approximation, because the possibility always exists that we are dealing with a fragment of a work which is no longer extant.

8. Evangelicals beware!

It is possible to write an article on this subject, presenting at great length and with considerable emphasis the potential importance of the "discoveries" and the support they would bring to conservative scholarship, whilst relegating to a few subordinate clauses any doubts about the identifications. This would not serve the cause of Christ. It would lead the unsuspecting reader to place too much reliance on these fragments in his apologetic.

The writer has chosen to share his doubts with the reader. As a Christian and as a researcher he would have welcomed O'Callaghan's identifications if they had proved firm. Unfortunately it is not yet possible to rely on them. Too many questions and objections are still unanswered. They are like Saul's armour, "I cannot go with these; I have not proved them" (I Sam. 17:39).

Loyola College, Montreal.