

THE OSSUARY AND INSCRIPTION ARE AUTHENTIC

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First, I should like to thank Drs Scott Carroll and Douglas C. Mohrmann, directors of the *Center for the Study of Antiquity*, for inviting me to present you the problem of authenticity and historical interpretation of the James ossuary. I published it 18 months ago in the *Biblical Archaeology Review*. During these 18 months, much has been told and written, sometimes by colleagues, more often by journalists, on the web and in the mass media, about the authenticity and meaning of this inscription, and it is time for me to check whether I have to change my mind.

Yet, before dealing with the problem of authenticity and historical interpretation of this inscription, I must underline the fact that the scientific debate has been often obscured by- and confused with- another debate, a political debate, about the problem of publishing materials discovered outside of professional excavations. I shall not deal here with this problem but will eventually answer later on, as an epigrapher, to the lecture given to you by Professor Eric Meyers. Actually, as an epigrapher, I am personally sticking to the usual practice of the great Northwest Semitic epigraphers of the XIXth and XXth century, Charles Clermont-Ganneau, Mark Lidzbarski, André Dupont-Sommer, my teacher, E.L. Sukenik, and Nahman Avigad, not to mention the still living Professors Joseph Naveh and Frank Moore Cross.

I - THE PROBLEM OF AUTHENTICITY

When you publish inscriptions seen in a private collection, the problem of authenticity is a classical problem. If you are an epigrapher, you have to deal with it, implicitly or explicitly. There are two main approaches to this problem:

- epigraphic aspect: you ask the questions: Are the shapes of the letters consistent? In the same script and from the same period? Is the inscription meaningful? Is this meaning consistent with the material support? Is it not a copy or imitation of another inscription?

- material aspect, with questions such as: Is the material support new? Is the inscription new? Are there traces of modern tools? For the study of the material aspect it may be useful to have the inscription examined by magnifying lenses (binocular) and in a laboratory using eventually a "Scanning Electron Microscope" (SEM) equipped with Electron Dispersive Spectrometer (EDS).

Actually, for the James ossuary, both aspects, epigraphic and material, were carefully studied before the publication as shown explicitly in *The Biblical Archaeology*

Review of November/December 2002. Furthermore the material exam done by Drs Amnon Rosenfeld and Shimon Ilani of The Geological Survey of Israel was confirmed, later on, by another exam done in the Royal Ontario Museum by Edward Keal whose conclusion was also published in *The Biblical Archaeology Review* (July /August 2003): like the GSI specialists, he noted an “over-cleaning of part of the inscription” but concluded (I quote): “it is clear that the inscription is not a modern forgery” (p. 70).

Now, as you probably all know, these conclusions have been challenged, first by several colleagues and, later on, in a press conference held by an Israel Antiquities Authority (IAA) Committee in June 2003. Let us try to appreciate these two challenges scientifically:

1 - Professors Robert Eisenman and Paul Flesher, Drs Rochelle Altman and Zdzislaw J. Kapera were among the first ones to challenge the genuineness of the inscription, more precisely of the second part of the inscription. Now: Dr. Kapera recognized himself that he is not competent in epigraphy and in Aramaic (“I am not well enough trained in Aramaic and epigraphy...”, *PJBR* 2/1, Dec. 2002, p. 35) while Pr. Eisenman, who is also not an epigrapher, explicitly said that he has nothing against the epigraphy or the patina of this inscription (*Folia Orientalia* 38, 2002, pp. 233-236). Pr. Flesher, who is also not an epigrapher, thinks that the second part is not genuine only because the spelling of ’HWY, without the letter “he”, is common in Galilean Aramaic of the IInd to VIth c. Yet, he recognizes that this spelling is already attested, at least once, before 70 CE, in Qumran Genesis Apocryphon. Furthermore he tries to put in doubt this same spelling read by Professor Joseph Naveh and Dr. Ada Yardeni in a separate ossuary inscription, Rahmani n° 570. Yet, in this difficult graffito, the reading ’HWY is confirmed by a new good picture.

Finally Dr. Rochelle Altman is now famous for having diffused on the web (*bibleinterp.com*) what she called herself “Official Report on the James Ossuary”. Among many strange affirmations such as “the size and shape of the James ossuary are non-standard” (p. 5) or: it is not clear “whether the inscription was incised or excised” (p. 8), she wrote that the inscription was made from two completely different parts, the first one, genuine, in a so-called “*inscriptional* cursive” and the second one, “a fraudulent later addition” in a so-called “*commercial* cursive” (p. 6). Now these strange appellations and affirmations have been repeated all over the world since Rochelle Altman declared herself: «I'm an expert on ancient scripts and I'm here to report that the “James ossuary” was genuine, but the second part of its inscription is a fraud » (see, for instance, *Jewsweek* April 3, 2003) but nobody asked who is Dr. Rochelle Altman? Although I know personally most of my colleagues working in Northwest Semitic epigraphy, I never read some publication by her in a scientific review and it took me two months to learn that she lives in the States: what she presents as her main publication is this so-called “Official Report” on the web, full of strange things, at least

from the point of view of a Northwest Semitic epigrapher. Actually her distinction between a “formal” first part and a second “cursive” part is not at all an objective description of the shape of the letters since, in both parts, there is a mixture of formal and cursive shapes, a well known phenomenon in the inscriptions on ossuary¹.

Let us have a look at each one of the 20 letters of this inscription:

- *yod* (n° 1, 8, 15, 17) is written as a simple short approximately vertical stroke without a small crook, hook or loop (serif) at the top: this shape is cursive.
- *‘ayin* (n° 2 and 20) can be considered as formal even if it appears sometimes in cursive script.
- *kuf* (n° 3) is formal.
- *waw* (n° 4, 9, 14, 19) is a simple vertical stroke, longer than the *yod*, but also without a small crook, hook or loop at the top: this shape is cursive.
- *bet* (n° 5,6) is formal.
- *resh* (n° 7) is formal.
- *samek* (n° 10) can be considered as formal even if it appears sometimes in cursive.
- final *pe* (n° 11) can be considered as formal even if it appears sometimes in cursive.
- *alef* (n° 12) is cursive.
- *het* (n° 13) is formal.
- *dalet* (n° 16) is cursive.
- *shin* (n° 18) is formal.

Thus, if we use the abbreviation *f* for formal letter and *c* for cursive one, we have the sequence:

effcfffccffcfccccfcf

Furthermore I cannot understand how the 'yod' of Ya‘akov (Jacob/James) and the 'yod' of Yoseph (Joseph) could be classified as ‘formal’ and the 'yod' of ’HWY (brother) with the same simple vertical shape could be classified as ‘cursive’. For me, this first challenge of the authenticity of the second part of the inscription is not serious!

- The second challenge by the IAA Committee looked a priori much more serious so I waited for the scientific publication justifying this IAA Committee negative conclusion.

¹ Cf. R. Hachlili, « The Goliath Family in Jericho. Funerary Incriptions from the First Century A.D. Jewish Monumental Tomb », *Bulletin of the American Schools of Oriental Research (BASOR)* 235, 1979, p. 31-66, spéc. 60 : « The ossuary script in the Jericho inscriptions combines cursive and formal elements, resulting in different forms of the same letters, appears together in a single inscription » (= in R. Hachlili - A.E. Killebrew, *Jericho. The Jewish Cemetery of the Second Temple Period*, IAA Reports 7, Jérusalem, 1999, p. 154); T. Ilan, « Ch. 3. The ossuary and sarcophagus inscriptions », in *The Akeldama Tombs. Three Burial Caves in the Kidron Valley, Jerusalem*, ed. G. Avni - Z. Greenhut, IAA Report n° 1, Jérusalem, 1996, p. 57-72, spéc. 57 : « An interesting paleographic feature occurs in this inscription: the use of cursive letters side by side with lapidary forms ». See also my detailed analysis in *BAR* 29/6, 2003, pp. 56-57, and in *The Polish Journal of Biblical Research* 2/2, 2003, pp. 40-41.

Actually there was and there is still no scientific publication by this Committee but only the diffusion on the web of the position of the various members². I read these preliminary reports very carefully and found them deeply flawed. I published my critical analysis in *BAR* and *PJBR*³ and, thus far, as far as I am aware, no member of the committee or somebody else wrote to me or published that I made mistakes in my critical remarks.

Without entering here into all the details, I should like to stress four points:

1 - The “specific guidelines” given by the Israel Antiquities Authority⁴ to the members of the committee were generally good:

§1. “To arrive at the truth based on **pure research only - without taking into account any other related factors regarding the collector, current gossip, rumors or prejudices.**” (p. 2)

§2. “Each scholar would work **in his own discipline.**” (p. 2)

§3. “The exact location and size of the sample would be precisely documented.” (p. 2)

§4. “Each Committee member was given up to three months to submit a **final report** summarizing his/her opinion and **reasons** for their conclusions.” (p. 4)

I only note that, to convince their scientific colleagues, the members of this committee should have submitted and published a “detailed and well argued scientific report” and not merely a “summary”.

2 - Unfortunately, the content of the preliminary reports released on the web - and which everybody can read - reveal that these good IAA “specific guidelines” were not followed by several members of the Committee especially regarding the fact to work “in his own discipline” and not to take into account “factors regarding the collector, current gossip, rumors or prejudices”.

3 - Actually a detailed critical study of the Writing Committee reports reveals that the ossuary inscription does not present any paleographic, orthographic or linguistic problem, and that doubts were raised only because of the *historical identification* with James, brother of Jesus from Nazareth, I proposed.

4 - Some members of the material Committee have seen anomalies in the material composition of the surface in and around at least part of the inscription. Yet, their conclusions are ambiguous⁵: thus Professor Yuval Goren concludes (I quote): “the

² See, for instance, *bibleinterp.com*

³ « Ossuary Update: Israel Antiquities Authority's Report Deeply Flawed » *BAR* 29/6, 2003, pp. 50-59, 67, 70; « Critical Evaluation of the IAA Committee Reports Regarding the Ossuary Inscription », *The Polish Journal of Biblical Research* 2/2, 2003, pp. 29-60.

⁴ Cf., for instance, « Ossuary Update: The Storm over the Bone Box. Summary Report of the Examining Committees for the James Ossuary and Yehoash Inscription », *BAR* 29/5, 2003, pp. 27-31, esp. 28-29.

⁵ Yuval Goren and Avner Ayyalon recognize explicitly that both options (“The inscription is a modern forgery that was coated with faked patina; OR the inscription is

inscription was inscribed *OR cleaned* in a modern period” (final report, p. 6). The second working hypothesis, that of a cleaning, clearly concurs with the conclusion of the two previous laboratory exams and, without being an expert in the field of the material exam, I can tell that it is also one which concurs with the epigraphic analysis, so, the interpretation of a cleaning is **the only logical conclusion in the present state of the documentation**. Not only is the interpretation of the inscription as a forgery not proved but it is very improbable since it contradicts the epigraphic analysis and the conclusion of the previous laboratory exams.

Therefore, unless Dr Zahari brings forth new and convincing scientific arguments which would be appreciated critically with due care, this inscription must be considered as genuine and we may then consider the problem of the eventual identification of the person mentioned in the inscription.

II - THE PROBLEM OF IDENTIFICATION

The reading of this Aramaic inscription is clear and, thus far, no one has doubted the material reading:

Y‘QWB BR YWSP ’HWY DYShW‘,

“Ya‘akov/Jacob/James son of Yoseph/Joseph, brother of Yesu‘a/ Jesus”.

As is now well known, this appellation could fit a personage mentioned in the New Testament, in Flavius Josephus and in the first Christian literature. So we have to deal here with a problem of identification between someone known by an inscription and someone known by the literary tradition. This kind of problem is not new in epigraphy. For sake of comparison, it may be useful to mention here a few other ossuary inscriptions with a similar problem:

1 - In 1903, Charles Clermont-Ganneau⁶ published a bilingual ossuary inscription from a Jerusalem tomb:

In Greek:

Osta tôn tou Neika-

ancient but was cleaned in modern times with the coating produced either inadvertently as a result of cleaning or intentionally to disguise the cleaning”) agree with their conclusion in their common article "Addendum: Final blow or just a blow" at § 6, on the website: bibleinterp.com/articles/Goren_Jerusalem_Syndrome3.htm. Cf. also J.A. Harrell, “Final Blow to IAA Report: Flawed Geochemistry used to condemn James Inscription”, *BAR* 30/1, 2004, pp. 38-41

⁶ « Archaeological and Epigraphic Notes on Palestine », *PEFQS* 35, 1903, pp. 125-131; idem, « La “porte de Nicanor” du Temple de Jérusalem », dans *Recueil d'archéologie orientale* V, Paris, 1903, pp. 334-340; J.-B. Frey, *Corpus Inscriptionum Judaicarum*, II, *Asie-Afrique*, Roma, 1952, pp. 261-262, n° 1256.

noros Alexandreôs

poièasantos tas thuras

"Bones of the (*sons/descendants*) of Nicanor the Alexandrian who made the doors"

And in Hebrew:

NQNR 'LKS'

"Nicanor the Alexandrian or Nicanor (and Alexas)".

He identified this Nicanor with Nicanor the Alexandrian who offered beautiful bronze doors for the Herod Temple in the famous Nicanor gate mentioned several times in the *Mishnah* (*Yoma* 3,10; *Sheqalim* 6,3; *Sotah* 1,5; *Middot* 2,3.6; *Negaim* 14,8; cf. also the *Tosephtah: Kippurim* 2,4, the *Palestinian Talmud: Yoma* III,41a and the *Babylonian Talmud: Yoma* 38a). This identification is generally accepted to-day⁷.

2 - Another ossuary, discovered by E.L. Sukenik in the Kidron Valley in 1941 and published by N. Avigad in 1962⁸, presents three inscriptions:

a - On one of the long sides, in Greek:

ALEXANDROS

SIMÔN

"Alexander (son) of Simon"

b - On the other long side, also in Greek:

SIMÔN ALE

ALEXANDROS

SIMÔNOS

"Simon (son of?) Ale [corrected to]

Alexander (son) of Simon"

c - On the lid, a bilingual inscription:

In Greek: ALEXANDROU, "To Alexander",

In Judeo-Aramaic: 'LKSNDRWS QRNYT/H, "Alexander the Cyrenian"⁹.

⁷ Cf. N. Avigad, « Jewish Rock-Cut Tombs in Jerusalem and in the Judaeen Hill-Country,» in *E.L. Sukenik Memorial Volume*, Eretz-Israel 8, 1967, pp. 119-125, esp. 124-125; J.P. Kane, « The Ossuary inscriptions of Jerusalem », *JSS* 23, 1971, pp. 268-282, esp. 279-282. The tentative interpretation of J. Schwartz, « Once More on the Nicanor Gate », *HUCA* 62, 1991, pp. 245-283) is not convincing.

⁸ « A Depository of Inscribed Ossuaries in the Kidron Valley », *IEJ* 12, 1962, pp. 1-12; T. Powers, « Treasures in the Storeroom. Family Tomb of Simon of Cyrene », *BAR* 29/4, 2003, pp. 46-51.

⁹ The attestation of this Cyrenian Jew buried in Jerusalem concords with the attestations of the Cyrenian towns Ptolemais (n° 99) and Berenike (n° 404) as well as of another "Cyrenian" (J.T. Milik in B. Bagatti - J.T. Milik, *Gli Scavi del 'Dominus Flevit'*, I, Jerusalem, 1958, p. 81, n° 9) on other ossuaries. These four ossuary inscriptions confirm the existence of a (probably Hellenist) Cyrenian community in Jerusalem as said in *Acts* 2,10 and 6,9.

This inscription was rightly compared to the Gospel of *Mark* 15,21:

«Then they took him (Jesus) out to crucify him. A man called Simon the Cyrenian, the father of Alexander and Rufus, was passing by on his way in from the country, and they pressed him into service to carry the cross...»

From the inscription, it is clear that the deceased was not « Simon the Cyrenian, the father of Alexander and Rufus », himself but he could well have been his son: "Alexander son of Simon," also called "the Cyrenian."

Although the probability of this identification is difficult to estimate, it looks like a serious one.

If it is accepted, it would mean that Alexander, the son of Simon the Cyrenian, died before 70. Since the way he is mentioned in the Gospel of *Mark* supposes that he is still living, this would confirm that the main redaction of the Gospel of *Mark* was anterior to 70, which is a known exegetical opinion.

3 - In 1984, the Israel Department of Antiquities and Museums acquired an inscribed ossuary: Rahmani 871. The Aramaic/Hebrew inscription reads as follows:

1 YHWHNH

2. YHWHNH BRT YHWHNN

3. BR TPLWS HKHN HGDL

1. "Yehohanah

2. Yehohanah daughter of Yehohanan

3. son of Theophilus the high priest.

As shown by D. Barag and D. Flusser¹⁰, this Yehohanah was apparently the granddaughter of the high priest Theophilus, high priest about 37-41 CE according to Flavius Josephus (*Jewish Antiquities* XVIII, 123-124; XIX, 297).

3 - In 1989, a salvage excavation in Hakeldama¹¹, south of Jerusalem, at the confluence of the Kidron and Hinnom valleys discovered several inscribed ossuaries. One of them, ossuary 31, belonged to « Ariston of Apamea ('RSTWN 'PMY) » possibly or probably¹²

¹⁰ « The Ossuary of Yehohanah Granddaughter of the High Priest Theophilus », *IEJ* 36, 1986, pp. 39-44;

¹¹ Cf. G. Avni, Z. Greenhut and T. Ilan, « Three New Burial Caves of the Second Temple Period in Aceldama (Kidron Valley) », in H. Geva ed., *Ancient Jerusalem Revealed*, Jerusalem, 1994, pp. 206-218, esp. 215; G. Avni - Z. Greenhut, « Akeldama, Resting Place of the Rich and Famous », *BAR* 20/6, 1994, pp. 36-46, esp. 43-45; *idem*, *The Akeldama Tombs*, IAA Reports n° 1, Jerusalem, 1996, esp. p. 35 (Avni and Greenhut) and p. 66 (T. Ilan).

¹² Cf. T. Ilan, « New Ossuary Inscriptions from Jerusalem », *Scripta Classica Israelica* 11, 1991-1992, pp. 149-159.

to be identified with an individual mentioned in the *Mishnah* (*Hallah* 4,11), bearing gifts to the Jerusalem Temple from abroad.

4 - In November 1990, bulldozing to make a park in North Talpiot, south of Jerusalem, brought to light a second Temple tomb still containing 12 ossuaries. Two of them looked especially interesting:

- n° 5 was inscribed with the Judeo-Aramaic inscription: QP', "Qafa",
- n° 6 was inscribed with the Judeo-Aramaic inscription: YHWSP BR QP', "Joseph son of Qafa", on the smaller side and YHWSP BR QY/WP', "Joseph son of Qai/ofa", on the long back side.

Professor Ronny Reich¹³ proposed to identify the Aramaic name "Qa(i)fa" with the Greek name *Kaiaphas*/Caiaphas, a family of high priests contemporaneous with Jesus, well attested in the New Testament (Mt 26,3.57; Luke 3,2; John 11,49; 18,13-14.24.28; Acts 4,6) and Flavius Josephus (*Antiquities* XVIII,35.95), as well as in the rabbinic literature (*Mishnah*, *Para* 3,5; *Tosephtah*, *Yebamot* 1,10; *TJ Yebamot* 1,6.3; *TJ Ma'asrot* 52a).

Yet this identification has been discussed¹⁴ and is somehow problematic since, twice, the spelling does not fit exactly and the title "high priest" is not written. Although the identification cannot be excluded, especially since these names, "Qafa, Qaifa, or Qofa" and Caiaphas are very rare, one may hesitate and it is difficult to consider it as certain in the present state of the documentation.

It is in this context that we must evaluate the problem of the identification of the deceased of the James ossuary with "James son of Joseph brother of Jesus", mentioned in the gospels (Mt 13,55-56; cf. 12,46; Mark 6,3), Acts, St Paul's letters and Josephus who tells us how he has been stoned to death at the high priest Ananus' instigation in 62 CE. James appears always as the first of Jesus' brothers; he is called « James the Lord's brother » in Galatians (1,19-20) and was the leader of the Judeo-Christian community in Jerusalem (Acts 15,13-21; 21,18). His designation as "Jesus' brother" became **a kind of nickname** to identify him without ambiguity as shown by Josephus, *Antiquities* XX,

¹³ R. Reich, « Caiaphas Name Inscribed on Bone Boxes, » *BAR* 18/5, 1992, pp. 38-44, 76; *idem*, « Ossuary Inscriptions from the 'Caiaphas' Tomb, » *Atiqot* XXI, 1992, pp. 72-77; *idem*, « Ossuary Inscriptions of the Caiaphas Family from Jerusalem », in H. Geva ed., *Ancient Jerusalem revealed*, Jerusalem, 1994, pp. 222-225. Cf. also Z. Greenhut, « The Caiaphas Tomb in North Talpiyot, Jerusalem », *ibidem*, pp. 219-221.

¹⁴ Cf. for instance, E. Puech, « A-t-on découvert le tombeau du grand-prêtre Caïphe ? », *Le Monde de la Bible* 80, 1993, pp. 42-47; *idem*, *La croyance des Esséniens en la vie future: immortalité, résurrection, vie éternelle ? Études bibliques*, Paris, 1993, pp. 193-195; W. Horbury, « The 'Caiaphas' Ossuaries and Joseph Caiaphas », *PEQ* 126, 1994, pp. 32-48.

200, who presents him as (I quote) « the brother of Jesus who was called the Christ, a man named James ».

Professor Fuchs, professor of statistics at Tel Aviv University, did a statistical onomastical study of the number of people probably called « James son of Joseph » and had a brother called « Jesus ». He estimated the probable number at 1.71 in Jerusalem before 70. Thus there is already a strong possibility of identification.

However, to estimate more precisely the probability of the identification, one must also take into account the fact that while the mention of the father is common enough on ossuaries, the mention of the brother in such a way, i.e. as kind of nickname of the deceased, is very rare. Actually it appears only once in all the other published inscriptions: in Rahmani 570. Such a designation is also very rare in the literary tradition so that it is really very improbable that among the very few contemporaneous Jerusalem people named « James son of Joseph » having a brother called « Jesus », two of them would have been designated in the same way, i.e. the appellation « brother of Jesus » being a kind of nickname.

So, besides the identity of the name, of the patronym and of the name of the brother, it is the unusualness of the designation of somebody as the brother of somebody else which renders the identification of the two homonyms very probable.

So, now, if this identification is very probable,

1. We *very probably* have here the first epigraphic attestation of a disciple of Jesus, actually of the first leader of the Jerusalem community, one of the "pillars" of the early Church (Gal 2,9).
2. We *very probably* have also here the first clear attestation of the Jewish funerary rite of the *ossilegium* for a disciple of Jesus.
3. The language of this inscription *very probably* confirms that the language of Jesus' family was Aramaic.
4. Finally we have *very probably* here the earliest epigraphic attestation of Jesus of Nazareth.

In conclusion, looking back at the last 18 months and at the so-called debate about the authenticity and historical interpretation of the James ossuary,

- I see much confusion between a *scientific* - I mean an epigraphic and historical - problem, and a *political* problem;
- Against the Israel Antiquities Authority « specific guidelines », I see much discussion about **the collector**;
- Again, against the IAA guidelines, I see many, many, many **gossips, rumors or prejudices**;

- Again, against the IAA guidelines, I see also many scholars taking position outside of their **own discipline**;
- *There is something which I do not see: I do not see any paper against the authenticity of the inscription published by a Northwest Semitic epigrapher;*
- *Finally I do not see any scientific reason to change my mind.*

[Although I have already clearly published my arguments about the ossuary inscription and about the IAA Committee reports, I accepted to speak the first one in this series of 4 lectures. I am aware that it will be very easy for Professor Meyers and Dr. Dahari to criticize my position and my arguments and, speaking the last ones, to seem to give the last word, while, on my side, I cannot criticize their position and their arguments since they are not published. So, for a fair debate, I insist that I should have the opportunity to answer, at least on the web, to the negative position presented in the two last lectures. Otherwise, there is clearly again a danger of manipulation of the information.]