

Tabor Notes on Kloner/Gibson Princeton Talpiot volume:

Here are a few random notes/observations re: your Talpiot tomb article. Use them as you see fit. The copy you gave me did not have page numbers so I have added those for easier reference.

My substantive observations are below. A couple of factual matters we have already discussed is whether you want to correct Kloner's references to his patio tomb investigation to 1980 (rather than his published 1981 that is incorrect) and update the number of ossuaries which we do now know—seven total, now in the tomb, with one at Bet Shemesh. You are welcome to cite “Recent camera probe investigations by James Tabor and Rami Arav have confirmed that there are presently seven ossuaries still in the tomb. Their positions relative to the Kloner drawing has also now been updated” (personal correspondence with Tabor).

p. 1. I cannot agree that the ossuaries would have merited no further scholarly study after 1996 were it not for the media attention that forced responses and attention from the scholarly community. After all, nothing was published at all on this tomb until 1996 when Kloner was more or less “forced” by the BBC media attention to publish his report in *Atiqot*. Why trash the media when it was that 1996 inquiry by Ray Bruce and others that prompted at least this publication?

It should also be noted that in Kloner's *Survey of Jerusalem: Southern Sector* the tomb is not even given a proper locus number and nothing at all is said about the six inscriptions. Instead it is described as “nearby” the “Patio tomb” and left at that (p. 84 English). This is surely inadequate since so many features of this tomb (Greek and Hebrew inscriptions, higher percentage of inscribed ossuaries than normal, not to mention the names themselves, etc.) are of interest.

In his subsequent *Necropolis* (with Zissu) Kloner does at least give the tomb a locus number (12-46) and offer a bit of a description with fig. 237 though the map co-ordinates are wrong. If I am not mistaken the Talpiot tomb façade actually appeared as the cover photo on the Hebrew edition?

All of the studies prompted by Jacobovici and Cameron were in my view commendable, even if one does not care for the film itself. Their work prompted DNA, epigraphy, prosopography, statistical analysis, and patina studies that hereto for had not been attempted in any way. Even the revised reading of Pfann and Price that you accept, incorrectly in my view, for *Mariamene e Mara* was essentially prompted by the “media” attention that your article distains.

I would argue it is far from the case that the “misinformation” about this tomb has been primarily because of the media attention. So much of what we now know has been a result of that very media scrutiny and contrary to your assertion the “scholarly treatments” (interviews, blogs, etc.) have been riddled with errors as I have documented in my blog. In fact, almost all of what we will never know about this tomb (location of ossuaries, disposition of the skeletal materials, etc.) was in fact the result of scholars not doing their job—not the media attention thereto.

p. 2, note 5. If you are going to cite Jodi Magness's piece on the SBL forum why not also include my response to her which Michael Stone and others have told me completely refutes her assertion about a "shaft tomb" burial for Jesus—a view you and Boaz also disagree with. See <http://sbl-site.org/Article.aspx?ArticleID=651>. It seems you only want to cite things that trash the "media" position even though these works you cite are largely those of evangelical and even fundamentalist scholars defending their "faith" positions at all costs.

p. 20. Your general assertion that the "names are common" and of no particular interest to scholars is simply false. First, the form of the name Yose is exceedingly rare, as I think you know, and only occurs three times in Tal Ilan—which covers both inscriptions and textual evidence. It is also the particular name used in Mark 6:3 for Jesus' brother. Whether we can identify this particular Yose as Jesus' brother is an open question, but to pretend it is not even worthy of consideration, and blame the "media attention" for asking the right questions about its frequency and usage, is hardly fair. Likewise Mariamene spelled with an "nu" is exceedingly rare. It is *only* found in two texts in antiquity (see TGL data base)—both referring to Mary Magdalene (Hippolytus and *Acts of Philip*). The point is not that these texts are so late that the "media" had to go to them to make a bogus point, but rather how is it that this form of the name, known only with reference to Mary Magdalene, appears on a 1st century Jerusalem ossuary? Further, the idea that two names are intended (Rather than Mariamene aka Mistress) is unlikely given the single hand with the sweeping flourish one finds in this elegant inscription. It is very unlikely that one individual died then the other, and a second name was then added. Also, have you thought that your take on Mariamene, disagreeing with Rahmani, assumes that he and Leah DiSegni made a *error* in reading that just happened to correspond to the only two references of that form of the name in Greek literature? What are the odds, since neither of them thinks the Mariamene ossuary has anything to do with Mary Magdalene. It is clear they were providing their readings based on what they saw—the *very clear cursive* nun—and that reading turns out to be a unique form of the name, used elsewhere only for Mary Magdalene.

As far as the names being common and the resulting statistical studies the references you cite in footnote 44 are very misleading. You have not kept up with the latest discussions, including those of Ingermanson. I would recommend you work backward from the very accurate recent analysis by Jerry Lutgen and update things, or at least offer a more balanced note with references, see: "The Talpiot Tomb: What Are the Odds?" conveniently available on the Web: <http://www.bibleinterp.com/articles/tomb357926.shtml>. What you say about Feuerverger and the flaws in his study are simply not the case unless you assume Yose and Mariamene are *not* appropriate names for Mary Magdalene and Jesus' brother Joseph—but I have just noted the evidence that these rare names are in fact more than appropriate. This does not *prove* the case, but it at least should be fairly discussed and not dismissed out of hand as you do in your article.

p. 21. Your final point about you and Kloner being convinced that the Church of the Holy Sepulchre was the "actual tomb of Jesus," rather than the Talpiot tomb missed the entire point of the Talpiot discussion. Even Kloner has published the view that Jesus was *moved* from that site to a permanent burial place (see Amos Kloner, "Did a Rolling Stone Close Jesus' Tomb?" *Biblical Archaeology Review* 22:5 (1999): 23-29, 26), making your point moot. Also, I have to assume, though you likely want to avoid "theological" discussions of resurrection, that since the

Church of the Holy Sepulchre tomb has been empty for nearly 2000 years, since the body/bones of Jesus were not taken to heaven, they would indeed have been given a proper burial.