The Talpiyot (Jerusalem) Tombs: Some Sober Methodological Reflections on the Epigraphic Materials

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INTRODUCTION TO THE TALPIYOT TOMBS

Stunning claims have been made regarding some Late Second Temple burial remains which were found in two tombs in the Talpiyot neighborhood of Jerusalem. The first of these tombs was discovered in 1980 and so can reasonably be referred to as Talpiyot 1980. The second of these tombs was discovered in 1981 and so can reasonably be referred to as Talpiyot 1981. At the time of discovery, neither of these tombs attracted substantial interest within the scholarly community (much of this was due to the haste with which these tombs were closed after discovery). During recent years, however, these tombs have received significant attention, at least in certain sectors. Here is the reason: it has been posited by a few scholars that Talpiyot Tomb 1980 is probably that of Jesus of Nazareth and various members of his family and that Talpiyot Tomb 1981 is probably that of the figure known in the gospels as Joseph of Aramathea and some members of his family. That is, some people have posited that both of these tombs are "Christian" tombs and belong to prominent figures attested in the Greek New Testament.

Historians, epigraphers, and archaeologists have responded rather negatively to these proposals because of the dearth of supporting data and the general perception of

tendentiousness in the argumentation. Thus, (1) An entire issue of the scholarly journal *Near Eastern Archaeology* was devoted to Talpiyot Tomb 1980 and the dominant consensus of the contributors was that the ancient evidence does not support the claim that this tomb was that of Jesus of Nazareth and his family (Meyers 2006; Scham 2006; Gibson 2006; Pfann 2006; Rollston 2006; Rollston 2012a; but cf. Tabor 2006; Feuerverger 2007; Jacobovici and Pellegrino 2007). (2) Moreover, during 2012, James Tabor and Simcha Jacobovici published a volume which reasserted that Talpiyot Tomb 1980 is indeed the tomb of Jesus of Nazareth and his followers and within this same volume they contended that Talpiyot Tomb 1981 was probably that of Joseph of Arimathea (Tabor and Jacobovici 2012; see also Tabor 2012), but the response of most epigraphers, archaeologists, and historians has again been negative, with the scholarly community basically reasserting that the cumulative evidence of these Second Temple tombs cannot carry that kind of freight (for summary and bibliography, see Rollston 2012b; Rollston 2012c). In this connection, it is rather important to remember this standard dictum: dramatic claims require dramatic evidence. And I suppose that it is fair to say that most epigraphers, archaeologists and historians would suggest that we simply do not have cogent evidence for the dramatic proposals which Tabor and Jacobovici have made.

At this juncture, I shall briefly summarize the contours of the evidence from both tombs and then focus on standard epigraphic methodologies *vis à vis* the inscriptional remains from these tombs. It is not my intent here to rehearse the entire discussion, but rather to hone in on the sorts of data that would be necessary for the claims of Tabor and Jacobovici to be considered convincing. In short, I am desiring to use the data from Talpiyot 1980 and Talpiyot

1981 as a laboratory for conveying some of the standard principles of epigraphic and historical methodology.

TALPIYOT TOMB 1980 (= TALPIYOT TOMB A)

This tomb was discovered in 1980 by Yosef Gath during a salvage excavation at a site in the neighborhood of East Talpiot, Jerusalem. This tomb contained ten ossuaries, six of them inscribed. These were subsequently published in Rahmani's *A Catalogue of Jewish Inscriptions* (1994, nos 701-709). The personal names on the ossuaries of this tomb are as follows: (1) *Mariamē kai Mara* (Mariam and Mara).[1] (2) *Yhwdh br Yšw'* (Yehudah bar Yeshua'). (3) *Mtyh* (Mattiyah). (4) *Yšw' br Yhwsp* (Yeshua' bar Yehosep). (5) *Ywsh* (Yoseh). (6) *Mryh* (Maryah). The names Yehosep, Yoseh, Yeshua', Yehudah, Mattiyah, Maryah, Maryam, Mariamne, Mara and Martha (or the variants thereof) all have multiple attestations in the multilingual corpus of ossuaries and some are very common (Rahmani 1994, 292-297; Ilan 2002). In fact, even the name and patronymic "*Yeshua' bar Yehosep*" (i.e., "Jesus son of Joseph") is not unique in the epigraphic corpus. After all, some eighty years ago, Sukenik published an ossuary inscribed "Yeshua' son of Yehosep" ("Jesus son of Joseph") and the names Yeshua' and Yehosep ("Jesus" and "Joseph") are predominant in the family of Babatha's first husband. In fact, the father of Babatha's first husband was named Yeshua' and his father was named "Yehosep," so this is yet another "Yeshua' son of Yehosep" (i.e., "Jesus son of Joseph"; see Sukenik 1931; Lewis 1989, 35-40; cf. Yadin 1971, 233-234; Kraeling 1946, 18-19). Thus, even with the fairly small corpus of

[1] For these names, I am reading with Rahmani, but in place of his Mariamēnou Mara, I accept Pfann's reading (2006), namely, Mariamē kai Mara. Of substantial importance is the fact that in *CIIP* I, 477 it is noted that "Rahmani has accepted the correction to his reading in the *editio princeps*." That is, Rahmani now reads "Mariame kai Mara." I am grateful to Jonathan Price for bringing this to my attention. I should also note in this connection that the word *Mara* is most readily understood as masculine. On this, see below.

epigraphic attestations of personal names, the Talpiyot Tomb 1980 occurrence of "Yeshua' bar Yehosep" ("Jesus son of Joseph") is not unique.

TALPIYOT TOMB 1981 (= TALPIYOT TOMB B)

During course of construction work in Jerusalem during the spring of 1981, a tomb with nine *kokhim* ("burial shafts") was discovered. There were a total of eight ossuaries in this tomb (originally distributed in four of the *kokhim*, that is, "carved chambers"), one of which was removed in 1981 (one belonging to a small child or infant). It was noticed then (in 1981) that there were some Greek inscriptions on (at least) two of the ossuaries, but the tomb was not excavated and documented thoroughly because of various exigencies, including religious sensitivities. Ultimately, modern buildings were soon erected at this site. However, rather than destroying this tomb, the modern buildings were built above the tomb.

During the course of a few days in 2010, James Tabor, Rami Arav, and Simcha Jacobovici (now the primary researchers for this tomb) were able to send a robotic camera into this tomb (through the basement floor of the building which had been built on top of the tomb) and to photograph the tomb itself, the ossuaries in it, and some inscriptional remains. One of these inscriptions, consisting of four very brief lines, has garnered substantial attention (see Rollston 2012b; Rollston 2012c; Bauckham 2012a; Bauckham 2012b), as has some of the ornamentation (see Mark Goodacre's contribution to *Bible and Interpretation* for more discussion of this). Indeed, Tabor and Jacobovici have claimed that this four-line inscription on one ossuary, and the ornamentation on another, can be understood as referring to a belief in some sort of resurrection and that this inscription and its ornamentation are distinctively Christian. In

addition, they have also noted that another of the ossuaries in Talpiyot Tomb 1981 has the word “mara” on it. As a point of departure, I should note that I would find it very interesting if these were Jewish-Christian tombs, but I do not believe that the epigraphic evidence suggests this.

I. TALPIYOT TOMB 1981 AND JOSEPH OF ARIMATHEA: METHODOLOGICAL EVALUATION

Tabor and Jacobovici have posited that Talpiyot Tomb 1981 is a tomb belonging to Joseph of Arimathea (i.e., the “Joseph of Arimathea” mentioned in the canonical gospels), and that this tomb also contains the actual ossuary of Joseph of Arimathea himself. Here are some citations of Tabor and Jacobovici’s views: Talpiyot Tombs 1980 and 1981 “are most likely located on the rural estate of Joseph of Arimathea, a wealthy member of the Sanhedrin who according to all four New Testament gospels took official charge of Jesus’ burial” (Tabor and Jacobovici 2012, 2). But he is framed as wealthy and so Tabor and Jacobovici believe they have to account for the “humble” (their word) nature of this ossuary, thus, they suggest that there may have been “something about his faith or piety as part of the Jesus movement” that led him to “prefer such a modest bone box” (Tabor and Jacobovici 2012, 89). Persisting with this point, they query a few lines later in their volume: “might Joseph of Arimathea have chosen a…modest ossuary for himself and his most immediate family—but one that boldly proclaimed their faith even in the midst of opposition and conflict?” (Tabor and Jacobovici 2012, 90). It should be noted that the reason they refer to this ossuary as “boldly proclaiming their faith” is because the ossuary they believe to be that of “Joseph of Arimathea” is the one with the

ornamentation they understand to be "Jonah and the Whale." Ultimately, they conclude that "it is not hard or even overly speculative for us to posit that the Talpiyot Tombs are a tiny but amazing glimpse into the life of Joseph of Arimathea" (Tabor and Jacobovici 2012, 128).

Methodologically, what is the sort of evidence that would be needed for Tabor and Jacobovici's proposal to be convincing? The requisite datum necessary to support this claim would be this: an inscription (with some nice ancient patina in it) which said "Joseph of Arimathea" and could be dated on the basis of the script to the first century CE. Extant Datum: There is no inscription which has been found in this tomb that refers to Joseph of Arimathea.

II. THE TERM MARA AND MARY MAGDALENE: METHODOLOGICAL EVALUATION

The Aramaic word *mara* (written in the Greek script in these Talpiyot tombs, rather than in the Aramaic script) occurs on an ossuary in Talpiyot Tomb 1980, namely, in the phrase *Mariamē kai Mara* (i.e., Mariamē and Mara; see Rahmani 1994, # 701). Tabor and Jacobovici assume that the inscription on this ossuary should be understood as referring to one person and so they render it "Mariam called Mara" (Tabor and Jacobovici 2012, 28). They cite Rahmani's reading in the *editio princeps* of this inscription, namely, *Mariamēnou Mara,* which Rahmani translated "Mariamene, who is (also called) Mara." Rahmani had originally assumed that this name was "in the genitive case" and was "a diminutive of Mariamēne" (Rahmani 1994, 222). Significantly, Stephen Pfann has published a most cogent correction of Rahmani's reading, noting that there are two words and a very clear *kai* between them (which Rahmani had unfortunately initially misread). Hence, Pfann renders this ossuary inscription as "Mariame

and Mara" (Pfann 2006). As I *mentioned above, Rahmani has now accepted the corrected reading, that is, Mariamē kai Mara*. Tabor and Jacobovici do not, however, accept the corrected reading. In terms of additional occurrences of *mara*, Tabor and Jacobovici note that *mara* also occurs on an ossuary in Talpiyot Tomb 1981 (Tabor and Jacobovici 2012, 67).

Here are the statements by Tabor and Jacobovici regarding the Aramaic word *mara*: "it is the feminine form of *Mar*, which in Aramac means 'Lord' or 'Master'" (Tabor and Jacobovici, 46). Or again, "Mara is the feminine form of Mar in Aramaic, which means 'Lord' or 'Master,' as explained in the previous chapter" (Tabor and Jacobovici, 67). They state that "we are convinced that *Mara* is an honorific title, not a proper name." They also state that "if you add the feminine ending to *Mar* you get *Mara*" (Tabor and Jacobovici, 115). The footnote accompanying that statement is: "The Aramaic name *Marta* (Martha) is derived from Mar/Mara. Some argued that *Mara* is just an alternative form of Martha but as we explain chapter 5, such is not the case" (Tabor and Jacobovici, 221). Again they state that "Mara, which comes from the Aramaic masculine Mar, is the absolute feminine, whereas *Martha* (Martha) is the emphatic feminine. They both come from the same masculine noun and mean the same thing, but Martha evolved into a name and is common" (Tabor and Jacobovici, 227; cf also Tabor 2012, 13-14). Of course, it should be mentioned and emphasized that they also contend that the ossuary with the words *Mariamē kai Mara* (which they believe should be read *Mariamēnou Mara*) should be understood as the ossuary of Mary Magdalene and that the title *Mara* is a title that "can potentially refer to her place of leadership and authority in the emerging Christian movement" (Tabor and Jacobovici, 131; cf. 96). In any case, as is apparent, these statements from Tabor and Jacobovici assume entirely that *mara* is demonstrably a

feminine form. Moreover, as noted, they claim that this ossuary inscription refers to Mary Magdalene.

Methodologically, what is the sort of evidence that would be needed for Tabor and Jacobovici's claims to be convincing? The requisite data necessary to support this claim would be that the Aramaic term *mara* would need always to be feminine *and* the geographicon "Magdala" would need to be present (as the personal names *Mariamē a*nd *Mariamēnou* are not names that are uniquely associated with Mary Magdalene).

However, in terms of extant data, the geographicon "Magdala" is not present and this constitutes a fundamental problem for Tabor and Jacobovici's claims. In fact, methodologically, it is a "deal breaker." That is, for methodological reasons, the identification simply cannot be sustained without the presence of the geographicon.

Furthermore, the Aramaic word *mara* is not always feminine in the epigraphic record. In fact, the word *mr'* (*Mara'*) is an Aramaic, masculine, singular noun meaning "sir," "master," "lord." It is well attested (as a masculine noun) in the Aramaic corpus of Northwest Semitic inscriptions, in both Old Aramaic and also in Imperial Aramaic (sometimes with the spelling *mry*). Note that in the case of the Old Aramaic occurrence in Tell Fakhariyeh (e.g., line six) the Akkadian text of this Akkadian-Aramaic bilingual uses (the Sumerian logogram to indicate that the Akkadian word should be understood as) *bēlum*, obviously a masculine form, not a feminine. This word even occurs in Nabataean and Palmyrene (which are later dialects of Aramaic), with the masculine form spelled *mr'*. The feminine singular is attested in Imperial Aramaic as *mr't*, and the feminine singular determined form occurs as *mr't'* (Hoftijzer and

Jongeling 1995, 682-689). The masculine form of this word also occurs in the Aramaic of the Hebrew Bible, with the spellings *mr'* and *mry* (see Dan 2:47; 4:16, 21; 5:23; Koehler and Baumgartner 2000, 1921-1922). Moreover, it also occurs in Jewish-Palestinian Aramaic with the spelling *mr* and *mr'*. The feminine form of this Aramaic word occurs in Jastrow as well and is *martha'* (see Jastrow 1950, 834-835, s.v., *Mar* IV). It is often stated that (for some of the Late Second Temple occurrences) the word *mara* can sometimes be a shortened version of the word *martha',* and thus can sometimes refer to a woman (either as a personal name, or as a title meaning 'lordess' or the like). Thus, Tal Ilan states about the name *mr'* (also spelled *mrh* during the Second Temple period) that "this is one of the rare cases of a name serving for both males and females" (Tal Ilan 2002, 392; cf. also 423-424). In sum, one can make a philological case for *mara* as a feminine but one can also make a philological case for this term as a masculine (cf. also Rahmani 1994, #561). In short, it is plausible to contend that in Talpiyot Tomb 1981, the word *mara* refers to a man, not a woman. Also, with regard to Talpiyot Tomb 1980, I would suggest that it is entirely plausible to suggest that this is the ossuary of a woman and a man, that is, a woman named *mariamē* and a man known as *mara*. Someone might suggest that the woman's name would not come first in this culture. However, I would note that order of death could reasonably account for the ordering of the names. Moreover, we do sometimes find a woman's name first in literary texts that refer to a woman and a man (e.g., Acts 18:18; 18:26; Romans 16:3; 2 Tim 4:19).

In short, it is philologically, contextually, and historically plausible to suggest (A) that the persons referred to as *mara* in these two Talpiyot tombs were men, not women; (B) and it is philologically and historically plausible to suggest that one was a man and one was a woman;

(C) and it is also philologically permissible to suggest that both were women. It is methodologically prudent, therefore, not to assume something about the gender in these two cases. In conclusion, therefore, it must be stated that (1) without the geographicon "Magdala," and (2) with the difficulty of determining with certainty the gender of the person referred to as *mara*, the assumption that Talpiyot Tomb 1980 contains a probable reference to Mary Magdalene would be most difficult to maintain.

III. DNA ANALYSES OF BONE FRAGMENTS: METHODOLOGICAL EVALUATION

Tabor and Jacobovici had DNA tests performed on the few bone fragments still present in the "*Yeshua'* Ossuary" and the "*Mariamē kai Mara* Ossuary" (i.e., from Talpiyot 1980). They have stated that it was not possible to recover Nuclear DNA but that it was possible to recover Mitochondrial DNA (Tabor and Jacobovici 2012, 196-202). Here are the precise statements Tabor and Jacobovici have made: In the "Mariamene" ossuary "we found only tiny bone chips." Or again, "the bone chips we found contained no marrow." And yet again, "There was no possibility of nuclear or gDNA with these samples due to their degradation." Or again, "It is unfortunate that we were not able to conduct full DNA tests on all of the bones found in all the ossuaries from the Jesus tomb. Ideally that would have allowed one to construct a kind of provisional 'family tree,' at least in terms of the familial genetic relationships between those individuals buried therein. Since the bones themselves were never examined scientifically, and no one is even sure what happened to them, that opportunity is forever lost" (Tabor and Jacobovici, 199-202).

Striking is the fact that because the mitochondrial DNA from these two ossuaries did not match (i.e., that is, they did not have the same mother, or were not those of a son and mother), Tabor and Jacobovici presumed that it was safe to conclude that the people whose bone fragments were removed from these two ossuaries might have been married. Tabor and Jacobovici also presumed that the bone fragments found in the *Mariamē kai Mara* Ossuary should be considered those of the person who was the mother of the person buried in the *Yehudah bar Yeshua'* Ossuary. That is, it is assumed that some very precise genetic relationships can be assumed or posited, based on the extant data (epigraphic and DNA).

For the claims of Tabor and Jacobovici to be convincing, certain data are required. Among the required data are the following: (1) Ossuaries in antiquity must have contained only the bones of one person; (2) The bones in an ossuary must be demonstrably those of the person named on the ossuary inscription; (3) Mitochondrial DNA must indicate gender; (4) The DNA evidence must demonstrate that the bones of the person buried in the *Yehudah bar Yeshua'* Ossuary are those of a the son of the person whose bone fragments were recovered in the *Mariamē kai Mara* Ossuary.

Here are the basic known data: (1) Ossuaries in antiquity often contained the bones of multiple people. (2) Even inscribed ossuaries do not always contain all the names of all of the people buried in them; therefore, it is often not possible, even under the best of circumstances (e.g., an undisturbed burial, with all of the bones recovered and sent off for DNA analysis, etc.) to correlate a particular set of bones with a particular inscribed personal name. (3) Gender cannot be determined on the basis of Mitochondrial DNA. (4) No bones or bone fragments

were recovered by Tabor and Jacobovici from the *Yehudah bar Yeshua'* Ossuary. In short, the requisite data are simply not there.

That is, (1) because multiple people were often buried in the same ossuary, (2) and because the names of all those buried in an ossuary are not always inscribed on an ossuary, (3) and because it is not possible to determine gender based on mitochondrial DNA, (4) and because no bones or bone fragments from the *Yehudah bar Yeshua'* Ossuary were available for DNA analysis, it follows that the bone fragments from the "*Yeshua'* Ossuary" could readily be those of a woman and those of the "*Mariamē kai Mara* Ossuary" could readily be those of a man; moreover, these bones may not be those of the person whose name is inscribed on the ossuary; furthermore, there is certainly no way to determine the relationship between the person named *Yehudah bar Yeshua'* and *Mariamē* of the *Mariamē kai Mara* Ossuary as no bones were recovered from the *Yehudah* Ossuary. In this connection (based on the things mentioned above), it is striking that Tabor and Jacobovici simply assume that the bone fragments from the "*Yeshua'* Ossuary" are definitely those of a man and that they are those of *Yeshua' bar Yehosep*, and they assume that those from the "*Mariame kai Mara* Ossuary" are definitively those of a woman. After all, multiple people were sometimes buried in ossuaries and not everyone's name was written on and ossuary in such cases. And, again, on the basis of Mitochondrial DNA, gender cannot be determined.

Further eroding the entire thesis of Tabor and Jacobovici is the fact that the bones from the ossuaries of Talpiyot 1980 were not even "available to Amos Kloner [in 1996] for study since they had been transferred to the religious authorities for reburial, in accordance with an

agreement that was made between the Israeli government and the religious authorities who objected to the storage of human bones within the Antiquities Authority's storerooms" (Gibson 2006, 120). Thus, the full collection of bones from Talpiyot 1980 were certainly not available to Tabor and Jacobovici (and they concede this point, as noted above). What does all of this mean? It means that there are so many lacunae in the DNA data-set that any and all attempts to weave together the epigraphic and the DNA data for the purpose of making prosopographic determinations (i.e., who is related to whom and how?) will always be speculation.

IV. THE FOUR LINE GREEK INSCRIPTION FROM TALPIYOT 1981: TABOR'S READINGS AND CLAIMS

Tabor and Jacobovici read the four-line Greek inscription on the ossuary from Talpiyot 1981 as follows: "DIOS IAIO UPSŌ AGB." They translated their readings as "Divine Jehovah, Lift up! Lift up!" They suggest that this four-line inscription is to be understood as reflective of an early Jewish-Christian confession of a belief in the resurrection, with Yahweh as the person doing the resurrecting. Along those lines, it should be noted that Tabor and Jacobovici have argued that the graphemes *AGB* (line 4) should be understood as the Greek transliteration of an H-stem verbal root *gbh*, although they also mention (and dismiss) Richard Bauckham's (2012a) proposal that these three letters be considered a Semitic personal name transliterated into Greek graphemes, namely, "Agabus" (Tabor and Jacobovici 2012, 90-94; Tabor 2012).

During 2011, I served as an epigraphic consultant for National Geographic on the Greek inscriptions of Talpiyot 1981 and so had access to images of the inscriptions for several months

prior to the release of Tabor and Jacobovici's 2012 volume. Within a couple weeks of the release of their volume, I provided all of my readings (Rollston 2012b, replete with epigraphically significant images). I noted then that Tabor and Jacobovi's reading of an *iota* at the beginning of line two was not palaeographically tenable and I posited that the simplest and most convincing reading is that of a *tau* (see the images and discussion posted on the web site of the American Schools of Oriental Research, namely, Rollston 2012b). Of course, without an *iota* at the beginning of line two, reading the tetragrammaton was no longer possible (NB: I had mentioned this to Tabor and Jacobovici and James Charlesworth during the summer of 2011). In terms of the rest of the readings, I posited then the following: DE OSTAE OU PSŌ AGB. Understanding the verbal to be *psaō*, I have suggested that it is reasonable to render this inscription: "Here are bones. I touch (them) not. Agabus. " As such, "Agabus" could be the name of the deceased, and thus this could be translated "Here are bones. I touch them not, O Agabus" (it should be remembered that the deceased would sometimes be addressed directly in mortuary inscriptions). Conversely, it could also be that the first person singular is used here of the man who asserts that he does not touch bones. Thus, this could then be translated quite nicely as "Here are bones: I, Agabus, touch (them) not." I also suggested at that time that the intransitive meaning is also viable. Thus, something such as "Here are (my) bones. I, Agabus, crumble not away." At that time, and now still, I also consider it possible to read the verb *upsoō* here. In this case, it would read something along these lines: "Here are the bones. I lift not (the bones/ossuary), O Agabus," or "Here are the bones. I, Agabus, lift (the bones/ossuary) not. In this connection, I should like to mention that I consider Tabor and Jacobovici's desire to read the verb as an imperative to be difficult, as it is most naturally a first person common

singular ending. I should also note in this connection that I accept as satisfying the proposal of Bauckham regarding line four, namely, that it contains the personal name "Agabus." Finally, I should mention again (as also in Rollston 2012c) that although I prefer *epsilon* as the reading of the second letter of line one (based on the existing photographs), I also consider reading an *iota* as the second letter of line one to be viable (that is, "because of…."). In any case, the point is that the content of this inscription falls within the traditional sorts of statements that occur in Late Second Temple and Early Post-Biblical tomb contexts: it is about bones. I should also add (as in Rollston 2012b; and in my discussions with Tabor and Jacobovici and Charlesworth during 2011) that even if one were to argue that this inscription referred in some fashion to a resurrection, this certainly does not make it a Christian inscription, as resurrection is a concept well attested in late Second Temple Judaism (e.g., from Daniel 12:2 to 2 Maccabees and Wisdom of Solomon and beyond). Tabor and Jacobovici and Charlesworth would concur that resurrection is not a concept confined to Early Christianity. In short, the contents of this inscription are interesting, but not dramatic. After all, the tetragrammaton is not there and even if one wishes to suggest that this inscription refers to a resurrection, the concept of a resurrection is not confined to Early Christianity.

V. READING "JONAH" ON THE AMPHORA OSSUARY

More recently, James Charlesworth (2012) has stated that he believes the name "Jonah" is incised in the Aramaic script on the "Amphora Ossuary" (i.e., the ossuary Tabor and Jacobovici have dubbed the "Jonah" Ossuary"). I have looked closely at the photographs of

this ossuary, especially the place where Charlesworth believes he sees letters encrypted. I stated at the time of Charlesworth's announcing his reading that these are not letters but rather just standard decorative incising, which in this case are part of the incised "drawing" of the amphora. That is, this ossuary has a great deal of incising-work on it and the incisings which Charlesworth considers to be letters are just horizontal and vertical incising-marks which were part of the production-design of the drawing of the amphora on this ossuary. The name "Jonah" is simply not there. Particularly useful in this regard are the drawings and discussion of Robert Cargill (2012), nicely demonstrating that there are no letters there.

CONCLUSIONS

Much more could be said about these tombs, and it probably will be. I would like to affirm strongly that I consider the technology Tabor and Jacobovici used in the exploration of Talpiyot 1981 to be stunning and auspicious. We owe them a debt of gratitude and I believe that these sorts of technologies can and should be used now and in the future when necessity demands it. Of course, full scale excavations are always superior, but sometimes the exigencies of a situation do not allow full scale excavations. Therefore, I welcome the impressive technologies Tabor and Jacobovici have brought to the fore. At the same time, I must also conclude that I do not believe the epigraphic and laboratory evidence supports the contention that the Talpiyot Tombs are early Christian tombs and are to be associated with Jesus of Nazareth or Joseph of Arimathea. The evidence seems to me to be much too thin for the acceptance of this sort of proposal.

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