

Reply to Response by Scott Stripling and Peter van der Veen
March 2, 2024
(Comments by Mark Haughwout are amended to the response below)

Response to Mark Haughwout's *Heritage Science* Article
Scott Stripling and Peter van der Veen
March 1, 2024

MH – Unfortunately, because the material was from a dump pile, it is impossible to date the lead tablet based upon archaeological evidence such as pottery or other datable finds. It is true that Adam Zertal's team was digging in layers that he dated to the Late Bronze and Early Iron Age. But we cannot be sure that all of the material in the dump pile was from a layer dating to those time periods. It is entirely possible that the lead object under discussion was laying [sic] on or near the top of the soil that was initially removed by Zertal's team.

SS – If Zertal only excavated material from the LB and IA, it is logical to assume that his dump contains material from one of those periods. Contamination is possible but unlikely.

MH – why is contamination unlikely? Methods? Was a piece of machinery used to relocate the dirt or did you carefully hand load the dirt until reaching undisturbed soil?

MH – The situation therefore is analogous to that of the Temple Mount Sifting Project, in that any finds must be dated from evidence drawn from the object itself.

SS – The TMSP is not processing dump piles from narrow time periods. They have an accumulation of jumbled debris from many time periods.

MH – I am interested in a description by SS of the method used to relocate the Mt. Ebal dump piles before wet sifting, including efforts to not disturb the soil under the dump piles during relocation to the wet-sifting site. (Wet sifting was not performed on location). This may help SS's argument. For the sake of fair debate, I accept that Zertal excavated LB and IA, but I also know enough about dump piles and archaeology to know that a dump pile can easily be contaminated and that there is often an initial layer of dirt to dig through before arriving at the LB and IA layers. For example, Tel Hazor was covered in several feet of dirt over the centuries – enough to cover the top of the remains of the lower city. I did not dig with Zertal (obviously), and I have not reviewed his incomplete excavation report, so I do not know how much soil he removed and dumped before arriving at IA.

MH – Stripling et al. date this lead object to the thirteenth century BC based on the stratigraphy of Zertal's excavation.

SS – We dated the tablet to the LB II period, which spans 200 years (1400–1200 BC).

PV – This is sound archaeological methodology.

MH – You are correct: to quote your article: "In Galil's opinion the inscription dates to the end of the thirteenth century BCE—close to the date of the Merneptah Stele (ca. 1208 BCE), but the other authors believe it could be older."

Earlier you also state: "...likely dating to the Late Bronze Age II (ca. 1400–1200 BCE), but no later than ca. 1250 BCE."

MH – Notice that the person being cursed is specifically named, which is expected in a defixio.
SS/PV – *MH needs a citation to validate this statement. Many defixiones do not have the name of the person being cursed.*

MH – I stated further down in the article: "...curse inscriptions name the person or the type of person being cursed. Likewise, the curses in the Bible have an object of the cursing (such as the disobedient Israelites). Here there is zero indication of who is being cursed or the type of person being cursed or even a type of action being cursed."

I think SS/PV will be hard pressed to find a curse that does not name one of the following:

- 1. A specific person or thing.*
- 2. A type of person or thing.*
- 3. A type of action.*

To be clear: Something/someone or some action has to serve as the object of the curse. A curse cannot lack an object.

However, if, as I pointed out in my article, the lead object is at all related to Numbers 5:21ff, it maybe that the 2nd person pronoun refers to a person standing in front of the priest who was writing out the curse. In such a case, this is not actually a defixio in the normal sense of the word, but a different type of curse tablet. But consider that in Numbers 5:17ff, the priest was to take dirt from the tabernacle floor as part of the ceremony, and the tabernacle is believed to have been in Shiloh at the time and not on the top of Mt. Ebal far to the north.

MH – The curse was typically written on lead and then folded, often multiple times and then pierced with a nail. ...though it is on the small end of the spectrum for a defixio and seems to lack the piercing by a nail.

SS – *MH needs a citation to validate this statement. A nail through a defixio is rare, not typical.*

PV – *Having handled several defixiones that are not pierced (spanning the period from the Greek Archaic down to the Byzantine period), I can confirm that this statement is simply inaccurate. During the Roman period some defixiones were not even folded and their inscriptions could be easily read.*

MH – I agree that not all are pierced – I was providing a general description of "typical" defixiones. Note that many view the word "defixio" to indicate "piercing", but others merely as "binding".

*This book which may add a lot of clarity to the general topic of defixiones:
<https://www.barpublishing.com/sylloge-of-defixiones-from-the-roman-west-volume-i-and-ii.html>*

The following articles also point to the use (magical use) of piercing the lead curse tablets with nails:

A Survey of Greek Defixiones Not Included in the Special Corpora - D. R. Jordan
(This article is a survey of hundreds of defixiones)

Mystifying Roman Nails: Clavus Annalis, Defixiones and Minkisi -David Dungworth
Pages: 148–159

Historic ‘Magic’ Nails: Their typologies and their ritual uses - Chris How

Clandestine Curses: Hidden Dangers to Charioteers Roman Spectacles and Entertainment - Anya Eber

But to be clear, I am not claiming that because the Mt. Ebal lead object lacks the piercing that it is disqualified as a defixio, rather that it merely lacks this one aspect. If it was pierced with a nail, that would add strong evidence that it was a defixio. Unfortunately for the authors, it does not have the nail piercing. Or perhaps that is fortunate, for the piercing by a nail seems to be a latter magical incantation and would not fit well in the LB or IA.

MH – The Grecian culture seems to be the origin of defixiones. Curse tablets begin to appear in other languages in the fourth century BC though not in Hebrew.

SS – Egyptian execration texts date as early as the Middle Bronze Age.

PV – This is simply inaccurate. Although we are unfamiliar with curse tablets made of lead that are earlier than the Iron Age II (we possess an Iron Age curse tablet from Moesia in our ABA collections, whose script and letter forms match Iron Age Phoenician), it is premature to claim that these inscriptions originated in Graecan culture. SS is right that objects with curses (execration texts) were commonplace in much earlier periods (including Middle Kingdom, New Kingdom, and Third Intermediate Period Egypt, while an inscription identified as a curse was also found at LBA Beth Shean).

MH- Be sure to take my words in context. I fully agree that curses exist from times before the fourth century. I also agreed in my article that writing on lead exists earlier. My argument is that lead defixiones are not known before the fifth or perhaps sixth century.

MH –Thus, the earliest known curse tablet is some seven centuries younger than the thirteenth century BC date proposed by Stripling et al. for this object, and particularly by Gershon Galil who was one of the authors.

SS – The Mt. Ebal tablet is indeed centuries older than when we previously thought this type of defixio appeared. Paradigms require constant adjustment. Prior to 2024, the overwhelming majority of archaeologists working in Israel/Palestine firmly believed there were no ceramic roof tiles in the region until the second century AD. Now, we know from the Givati Parking Lot excavation that they existed in the second century B.C. This is three centuries earlier than previously thought possible. The same is apparently true with the Mt. Ebal defixio.

PV – In archaeology one should never base such strong claims on the present status quo. The fact that earlier curse inscriptions exist (albeit made of pottery), makes it likely that one day other earlier lead inscriptions could be found. Lead inscriptions are altogether rare during earlier periods in the entire Mediterranean world.

MH - Yes, agreed that more discoveries will be made in the future and will shift paradigms. However, over 1600 defixiones have been discovered. None have been found dating even remotely close to the one claimed to have been found on Mt. Ebal. Of course, future evidence could completely change our current understanding. However, the current evidence raises a major red flag concerning your lead object.

MH – To be clear, this object found by Stripling is dated 700 years too early and is also assigned to a culture to which it does not belong. The writing and burying of curse tablets belong to European pagan culture, not to Israelite culture or to Yahweh worship.

SS – MH needs a citation to validate this statement. Many defixiones derive from non-pagan monotheistic cultures. Barbara Burrell excavated dozens of these at Caesarea Maritima. By the way, none of them were pierced by nails.

PV – Too many claims here that cannot be properly substantiated. Why too old? Curse inscriptions did exist well before the Late Bronze Age in the Ancient Near East and in Egypt. Hence, both the culture and the writing of such curses is to be expected in Canaan. As stated above (and cited in our HS article), a curse inscription was found in Beth Shean Stratum VII of the 13th century BC. See: S. J. Wimmer 'Ein Ächtungstext aus Israel/Palästina', in: Sesto Congresso Internazionale di Egittologia, Atti II. Turins: Società Italiana per il Gas p.A.; 1993. p. 571–8.

MH - See above – the existence of ancient curses is a matter of fact. I am not debating that, rather I am debating the use of lead to record curses and the specific practices related to lead defixiones. Perhaps someone did write a curse on lead at some point in the LB or Iron Age, but that lacks evidence. Additionally, it is another step to go from writing a curse on lead to actually producing a defixio. In other words, perhaps someday a piece of lead will be found with a curse on it, but was it used the same as a defixio, or simply as a warning, such as curses over tombs. Thus, you are two steps and not one from proving your point.

MH – It must also be noted that at this period the Hittite Empire was using hieroglyphic writing rather than the alphabetic writing claimed by Stripling et al. for the “Mt. Ebal Curse Tablet”.

SS/PV – We never claimed anything of the kind. We first dealt with Neo-Hittite/Luwian lead inscriptions and then we referred to the lead inscription from Late Bronze Age II Büyükkale (the acropolis of the capital of Hattusas). We never claimed that the Hittites used an alphabetic script. We simply stated that it is the oldest known lead inscription, whose signs (yes very likely hieroglyphic) are so poorly preserved, that to date the inscription has not been properly deciphered.

MH - You are still a few steps from your needed evidence – 1. A curse on lead dating to the LB. 2. A curse that is a defixio. 3. Writing on lead in early alphabetic (protoHebrew/Canaanite)

MH – Stripling et al. state that “Metallurgical analysis of the tablet’s lead by Professor Naama Yahalom-Mack at Hebrew University revealed that it derived from a mine in the Aegean (Lavriion, Greece), which was known to be in use in the Late Bronze Age.” However, the article by Stripling et al. fails to mention how long the mine was in operation, nor which particular mine in Lavriion was the origin of the lead. The reality is that the mines at Lavriion Greece were in operation intermittently from the 3rd millennium BC until the sixth century AD and again from AD 1859–1982.

SS – We in no way implied that the Lavriion mines were only in use in the Late Bronze Age. We merely established that they were in use at the time that the Mt. Ebal altars were constructed. MH misses the important point that exports from the Aegean ceased or drastically reduced ca. 1200 B.C. Since LB II and Iron IA are the occupational periods at Mt. Ebal (per Adam Zertal, the excavator) and exports from the Aegean to Canaan stopped or drastically reduced ca. 1200 B.C. and only resumed in the Iron Age II or Persian period, the Mt. Ebal defixio can plausibly be dated to the LB II period. We never stated or implied that the lead source proved the date.

PV – I fully agree with SS’s statement. At the time of publication Yahalom-Mack had not yet finished her article (see our article, fn 2), while Mycenaean expert Professor Jorrit Kelder (a well-known expert in his field) confirmed to me the LB use of the Lavriion mines. That was enough for us to accept that the source was in use at the time.

MH - I agree with her findings. My point is that import fluctuations of lead do not necessarily preclude a later date. Anyone could have carried this object in their pocket – a tourist, a soldier, a fisherman. Thus it could have easily been carried from Europe to Israel and subsequently lost just like Roman coins that were carried from abroad and subsequently lost in Israel.

MH – Additionally, if the lead originated in Greece, as claimed, it would be just as reasonable to assume that whoever placed it (or lost it) on Mt. Ebal was from Europe and not from the Levant.

SS – “As claimed”? This is not a claim – it is a scientific fact that the lead derived from the Lavriion mines. There are lead objects at many LB/IA sites in the Highlands. Everyone agrees that it was imported. European tourists were not just wandering around the Manasseh Hill Country.

PV – The suggestion that it had been brought to Mt. Ebal by a European is quite hilarious. Lead and tin ingots were imported from Greece and even Sardinia (yes as far west as Britain) and have been found in sunken ship cargos off the northern coast of Israel. We did not cite it, but papers on this have been published by Yahalom-Mack and her colleagues. So, lead was imported from the Aegean by Cypriot or Canaanite ships and were subsequently worked by local workshops. Surely (and I am not saying that our object is a fishing weight) we are not going to argue that everything that was made of lead and was found in Cis- and Transjordan was dropped there by Europeans, are we?

MH - Would you claim that all Roman coins were minted in the land of Israel? Or that no coin minted outside of Israel was ever lost in the Manasseh Hill Country?

Also, I ask what is “hilarious” about a foreigner losing a foreign coin but not a losing similar sized foreign piece of lead?

MH – However in this case, initial analysis showed no clear lines of text, though a few letters were suspected initially in the upper right corner of “Inner B”.

SS – Epigrapher Professor Daniela Urbanova, while not an expert in the proto-alphabetic script, quickly observed man-made letters. Epigraphers Peter van der Veen and Gershon Galil later confirmed this.

PV – Indeed Professor Urbanova, who is an expert on classical scripts and who has extensively worked with tomographic scans made of later lead strips, identified the ‘mem’, the ‘he’ (of YHW) and the ‘aleph’ that MH queries.

MH - At the time of my research, I was only able to find two cases of tomographic scans of this type of lead object. Do you know of others?

I don’t doubt that she thought she saw what might be letters. Indeed, I noticed the stick figure man and the X shapes and perhaps other forms that might be letters. But I know from analyzing other surfaces, that scratches/dents can look exactly like letters. PV, you know this better than I. SS, I grant that GG has a lot of knowledge, but he has made multiple outlandish claims about other inscriptions. His Siloam tunnel “discoveries” are a good example.

MH – However the ALEPH that was initially seen is actually only a series of scratches. This is clearly shown by the fact that both “horns” of the proposed letter, which resembles an ox head, continue far past the “ceiling”. The “ceiling” is the equivalent of the line above a letter on modern notebook paper. Typically, ancient letters remained below this line.

SS – MH must be referring to Paleo-Hebrew, not Proto-Hebrew. The claim that the apparent aleph on the inside is merely cracks, is a tricky one. MH could be correct, but I doubt it. It all comes down to at which depth one looks at the letter shown in the different tomographic scans. On the right level (the actual surface on the inside) the horns are not cracking out. This occurs on scans that are nearer to the outside. The same situation occurs with he (in YHW) which is clear cut on the surface inside but cracks out the further one moves to the outside.

PV – If seen on the right level (depending on the depth indicated by the scans, i.e. those scans locating the surface of the inside inscription ‘Inner B’), ‘aleph’ can be clearly seen (as Professor Urbanova in fact did), and it is here that the apparent extensions (MH’s ‘scratches’ that continue ‘far past the ceiling’) are clearly seen as well. It can be detected, however, that the apparent ‘scratches’ extending upwards, are not part of the horns and are not found in a straight line with them. I show this in my PowerPoint lecture on the tablet, where I am not dependent on the Galil’s views. Once again, Urbanova is an experienced epigraphist.

MH – PV, I’m not going to argue against what you have seen, but only what your article with SS has presented. The scholarly world cannot assess information that is not presented to them. Perhaps a supplemental article on the interior with evidence you mention can be published by you and SS. For the time being, the two images in the

article show the horns extending. It's not that I don't believe you. I'm not analyzing what you claim in personal communication but rather what was published.

MH – “Analysis of the Proposed Text” Section.

SS – In this section, MH repeatedly applies Paleo-Hebrew grammar and syntax to a Proto-Alphabetic script. This would be like applying modern English grammar to an Old English poem like “Caedmon’s Hymn.”

PV – Indeed it is like comparing apples with pears.

MH - As I specifically said in my article, the corpus of early evidence does not support your claims, though the corpus is very sparse. I believe neither Paleo-Hebrew nor Proto-Alphabetic supports your claims. I may be wrong but show me the evidence to the contrary, I will gladly accept it. But the evidence must be within the alphabetic corpus of the Hebrew/Canaanite family of languages.

MH – Concerning the missing subject of the pronoun, we have a unique text here compared to known curse tablets and even to other curse inscriptions, such as those over tombs. In other words, curse inscriptions name the person or the type of person being cursed. Likewise, the curses in the Bible have an object of the cursing (such as the disobedient Israelites). Here there is zero indication of who is being cursed or the type of person being cursed or even a type of action being cursed.

SS – This is an overstatement. There are curses that do not name the person being cursed. It is our belief that this was likely a self-imprecatory curse.

PV – And even here we cannot be certain that no personal name is mentioned. The last word on the content has not been spoken. Even on the outside, some of the shallower letters may be part of a name or object, but the state of preservation is so poor, that we just cannot tell. But for the time being, yes, I would agree with SS that it may well be a self-imprecatory curse.

MH - “self-imprecatory curse” – Hmm, why would someone curse themselves? And does the grammar support this? I don’t think it wise to make this claim if you also claim the pronoun on the lead is in the second person.

MH – Thus, any spelling with a consonant acting as a vowel is incompatible with the date assigned by Stripling et al. to their proposed text inside this folded lead object.

SS – See Peter’s response.

PV – Step back for a moment and don’t rush to conclusions. First of all, the presumption that mater lectiones or plene spellings were not used before Iron Age IIB–C, is a conclusion based on the current status quo. And yes, we have many more inscriptions from those later periods. Egyptian hieroglyphs do use semi-vowels, however, to indicate pronunciation, and as Proto-Sinaitic letters were directly derived from hieroglyphs (by scribes who mastered the Middle Egyptian language), it would be reasonable to assume that they did use ‘reading aids’, i.e. how to pronounce the letters. Hence Brian Colless of Massey University, an expert on proto-alphabetic scripts for many years, suggests that proto-Sinaitic and Proto-alphabetic were syllabic scripts. Whether or not this is true, is a matter of further debate. Even our suggestion that ‘The use of mater lectionis in ancient

inscriptions was not uniform as evidenced by the following Ugaritic texts: KTU 4.171: 4: ṯmnym. šmn; KTU 1.6 VI: 10, 14: 'ahym . ytn . B'l; KTU 1.19: IV 24, 30: b . šmym; and more' must remain tentative, but as this conclusion was reached by Professor Daniel Sivan, an expert on Ugaritic, we respect what he says. Even so, I am not even certain that we have plene spellings on the Mt. Ebal tablet. This is widely based on Galil's maximalist readings of e.g. ar(u)r and ata(h), both which I am not certain of now. Arur may well be written without waw, i.e. 'rr, while 't may or may not be there.

MH - It is best just to distance yourselves further from Galil's claims here. It's not worth trying to defend this. The evolution of the implementation of Matres Lectionis seems pretty solid. PV, you especially know this.

MH – To be clear, the extant evidence of early alphabetic writing shows that it was purely consonantal, lacking any vowel letters.

SS – MH needs a citation to validate this statement.

PV – Much of an overstatement. MH is not an expert on these matters.

MH - PV needs to show the use of a single Mater from the Sinai Inscriptions and then I, and everyone else, will rethink the whole matter of mater. As always, I am willing to reconsider the position of scholarship on this, but there needs to be evidence.

MH – Douglas Petrovich pointed out during his presentation at the 2023 Near Eastern Archaeological Society meeting that texts never read from bottom to top.

SS – Petrovich also noted that there were exceptions.

PV – It is wrong that proto-Sinaitic inscriptions never read from bottom to top, as Professor Ludwig Morenz has clearly shown in his recent and detailed monograph (Sinai and Alphabetschrift 2019) on the Serabit el-Khadim inscriptions. Other specialists also working on proto-alphabetic inscriptions (including Brian Colless and Michael Bar-Ron) confirmed that our interpretation of most of the letter forms is sound.

MH – Petrovich noted cartouches as exceptions. PV, There is of course a lot of debate about how the Serabit el-Khadim inscriptions should be read and deciphered/interpreted... I have looked at them in publications. I cannot deny there is the possibility of writing from bottom to top vertically, but it would largely depend on the reading of the inscription. The lack of matres and also of likely shortened consonantal forms allow for widely variant readings.

MH – Attempts to prove the existence of an earlier shortened form by appeal to an Egyptian hieroglyphic inscription from the Soleb temple dating to the reign of Pharaoh Amenhotep III (early fourteenth century BC) is tentative at best. The inscription mentions the “Shasu of Yahwe”. This is likely a reference to Yahweh. However, it cannot be shown that the spelling and proposed pronunciation of the hieroglyphics is indicative of a shortened form in the Hebrew language.

SS – Although Titus Kennedy has recently claimed that the Soleb Inscription spells the divine name with four letters, most scholars believe it was written with three letters.

PV – Scott is correct. It must be added, that Soleb is not the only place where this name occurs. It is also found on the Temple of Amarah under Ramesses II. While Soleb reads Y-h-(w3), Amara-West renders Y-h-(w3. While the variant reading at Amara-West (N96) is defective, the consonantal sequence at Soleb appears to correspond to YHWH, but the final signs at Soleb are emended and it cannot be excluded that final 3 (if correctly read) served as a vowel marker for w, hence YHW. The reading by Titus Kennedy is therefore premature. That shorter versions of the divine could have been in use at a very early date (i.e. yh even without waw), has been suggested by Egyptologist Professor Thomas Schneider in his laudable monograph on Westsemitic names in Middle Kingdom and Second Intermediate Period Egypt (Ausländer in Ägypten, Part 2, 2003). At Serabit el-Khadim we find on a small obelisk from the time of Amenemhat III (c. 1800 BC) that one of the sons of Irsha, the chief of Retenu, is named yhn'm, which – though tentative also – may translate 'Yah has spoken' or 'utterance of Yah'.*

MH - As I pointed out, the Egyptian spelling of the name of YHWH is not a reliable indicator of either spelling or pronunciation of the name in Hebrew. I consider the only real value of the Egyptian inscriptions you mention to be that fact that the name YHWH was known in that time period and that it seems to consist of 4 letters – same as in latter times. As a side note, in theophoric names, YAH is generally used after the destruction of the first temple, and YAHU was generally used in the monarchal period. I will not deny the possibility that in LB you might have the stand-alone name YHW, but the evidence doesn't exist yet in Hebrew/Canaanite. If your lead tablet clearly showed this spelling in proto-alphabetic, then I, along with so many other critics would be really excited. Indeed your first press releases were exciting, but your published article squashed my excitement.

MH – Any appeal to the Khirbet Qeiyafa ostrakon (eleventh century BC?) for an example of multiple letter orientations should be ignored since that was most likely a child's practice writing tablet and is likely not a translatable text.

SS – This is a bold and sweeping statement. MH expresses here a minority viewpoint as if it were the majority viewpoint.

PV – MH can't be serious about this being a 'child's practice' exercise. This is sweeping at best. And truly it reveals that MH has no clue about early proto-alphabetic scripts in which the orientation of the letters was not yet fixed. They can point downwards, upwards, whatever. It is their form as derivatives of pictograms that mattered.

MH - The size of the letters, the lines under the letters and the lack of coherent sentences all indicate that this was a practice tablet and not the work of a professional scribe. The fact that it was written on the inside of a broken piece of pottery proves that this was not some type of important inscription (though it would not preclude it being a copy of an important inscription). This was very clearly a pupil's practice tablet – probably a child's, but perhaps an adults. Please also see the footnote in my paper. Don't let GG's outrageous claims about this one mis-lead you. Rather compare this to the Gezer calendar which is of similar time frame and seems to be by a more advanced student.

MH – The most glaring problem is the lack of a composite image. The few images of Inner B provided by them only show some partially focused areas with the rest blurred. This makes analysis of their claims more difficult.

SS – MH clearly has never worked with tomographic scans. This is what we must work with. It was incumbent upon us to demonstrate what we saw and to provide the readers with what we thought it was.

PV – So true. And then there is the troubling central fold, where I doubt the correctness of many of Galil's letters.

MH - With tomographic scans, it is possible to create a composite image. SS et. al. attempt to put together the video (linked in their article) is even more valuable than a composite image for analysis by other scholars. I would love to see a high quality version of that released to the public. I understand this was SS and PV first time working with tomographic scans of the interior of a folded lead tablet – since there are only two known examples of tomographic scans on lead tablets (unless I'm missing something) and as far as I know, they didn't work on the other one. I don't think SS or PV can claim great experience here. I salute the endeavor, but this is new science and tomographic scans of the interior of lead tablets are not ideal due to the nature of lead. Actually, when x-ray tomography is used on other objects, lead gets in the way. For example when x-ray tomography is used on layered paper such as scrolls, lead seals block the x-rays. As mentioned at the end of my article, new science is emerging that may help analysis of the interior of the lead object.

MH – They also failed to label their drawings and images of individual letters in their Table 1–10 in order to connect them to Fig. 7. This makes it difficult for the reader to check their claims.

SS – See Peter's comment.

PV – MH is right, this should have been done in an ideal world. But at the stage when fig. 7 was made, PV and Galil did no longer communicate (as Galil did not want PV to disagree with him). Working on the completion of the article was very difficult at this stage. Galil and PV/SS disagreed on the forms of the letters and today PV even believes that some of the letters in the tables should also have been placed in square brackets as I am not certain that they are there.

MH – However, the question begs to be asked: If better images exist, and those images would strengthen their case, then why were they not included in their peer reviewed publication? The existence of other images is not doubted but rather their usefulness in deciphering letters, for surely anyone who wanted to substantiate such great claims as those made by Stripling et al. would have included the very best and most convincing images in their publication.

SS – What I said was that we would make 3D files available of additional scans. An article is limited to static pics. MH incorrectly uses "the question begs to be asked."

PV – Indeed and the whole process is extremely complex. Often one needs images that are several hundreds of MB large (some TIFF files were more than 1 GB in size). These cannot be used in an article and hardly ever can one use one single scan to identify more than two or three letters. As the tablet is bent, and as presumably the inside is filled with

soil, the individual letters are distorted in most images and one would really need to create a composite image from multiple scans (all from the same depth) to make this happen. That is why I prefer to examine the outside now, to affirm what we have suggested.

MH – tiff files can be converted into Jpegs and large files can be reasonably reduced without losing sufficient image quality. Additionally, SS et. al. published on-line. There is no limit to article size on-line like there is in print articles. I see this argument on their part as invalid. Though I agree that work on the outside is the correct path. The lead is so thin any letters on the inside would very likely be detectable on the outside. For this reason, multiple images of the outside should be produced. I would be happy to produce the images myself. I have the equipment and have the skills. If I am given access, I will capture the images, and upload them to a website at no charge. Let the world see...

MH – They also provide a closeup in Table 11, image 1 of their article. The reader should note that ancient ALEPH resembles an upside down letter “A” and was drawn as an ox head with horns. These striations could be shown to be the letter ALEPH if there were other letters near the proposed ALEPH so as to make a word. The reality is that random scratches can look very similar to letters.

SS – MH’s denial of the extremely clear proto-alphabetic aleph on Outer A betrays his bias. The impression of this letter can also be seen on the scans of the interior. MH is not an epigrapher, nor is he an archaeologist. He is not qualified to interpret what is a scratch and what is an intentional man-made letter.

PV – Much agreed with SS. And as I told MH, there are further letters on the outside (recto) that can be clearly seen. There is certainly another wavy mem, a cross-shaped taw, a F-shaped he, etc. These letters already show that we have letters, and that the Ox-head should be read as an aleph. These ‘letters’ reveal incisions by a stylus and raised edges, as can also be seen on other incised lead strips. They are almost certainly man-made.

MH - I’ve addressed this so-called Aleph in my article. Its form is not ideal, it sits by itself, and it differs significantly in the only other published photo of it that is available. SS’s comments about MH not being an archaeologist are completely irrelevant to the discussion about Alephs or any other letter. Archaeologists, like SS, are not trained epigraphers, though some may study it. I will grant that PV knows a great deal more about epigraphy than either myself or SS, since that is PV’s profession. The statement concerning credentials by SS is simply a diversion from analysis of the image. I encourage the reader to look at other proto-alphabetic Alephs and to decide if this shape is ideal and to examine the image in the article by SS et. al. and try to discern anything that looks like a letter next to the proposed Aleph. I clearly stated in my article the correct view regarding individual proposed letters. I stand by what I stated. I will grant that PV’s discussion about evidence of incisions by a man-made object is worth discussion. Careful microscope analysis may be able to show deliberate incisions rather than accidental incisions. However, I remind the reader that scratches, are by nature, caused by an external object upon the surface. Thus, scratches by an external object do not automatically indicate use of a stylus by a scribe. The world must rely on what SS et.

al. have published, not what SS or PV have claimed in private. In order to substantiate their claims, PV and SS need to publish evidence.

MH – By observing Table 2 in their article, it is clearly visible that authors Stripling and van der Veen have drawn nine different proposed “Alephs” for Inner B and they all differ significantly in form from one to the other and also when compared to their tenth proposed ALEPH on Outer A.

SS – The scribe was writing on a very small surface, and the text was never intended to be read. Other proto-alphabetic inscriptions demonstrate variety of letter morphology.

PV – Indeed the miniscule size is responsible for these extreme variations. But even so, the letter types in Table 2 do not differ all that much once we agree that not the form but the pictogram as such is what matters, i.e. the ox-head. While I am no longer convinced that all the alephs in Table 2 are true alephs (1A-B, 4A-B [almost certainly not an aleph], 7A-B are tentative at best), their precise form is sometimes very difficult to detect due to the fuzziness of most of the scans. But also at Serabit el-Khadim letter forms do clearly deviate (we can have pictorial alephs next to cursive ones, even the more developed V-shaped one).

MH – Variations in letter morphology are a reality, however, everyone who writes has a handwriting style. Wild variation within one scribe’s work causes a red flag to rise. PV’s statements regarding his doubts about the Alephs are worthy of public knowledge.

MH – None of the images or drawings are convincing.

SS – Again, MH states fact as opinion.

PV – As MH is not an expert on proto-alphabetic scripts, this statement should be ignored.

MH - If it was only me saying this, then yes, worthy to be considered with suspicion. However, I’m in line with the vast majority of the scholars, including professional epigraphers. I’m not saying SS/PV are wrong, but rather that their evidence is not convincing. I believe C. Rollston made the same point in his article (December 2023 IEJ). Surely SS/PV know by now that their lack of convincing evidence is the reason their conclusions have not been accepted. The scholarly world can only rely on what SS et.al. have presented, not on what they say they saw. We cannot rely on what SS/PV think they saw if they don’t present solid evidence. If the images were convincing, the scholarly world would not be giving SS et. al. such a hard time about their claims.

MH – The reader must remember that what confirms markings as letters and not mere striations in a surface is the proximity to other clear letters. A lone proposed ALEPH, whether real or imagined, is of no value by itself and ought to be ignored as coincidental scratching. Such scratching or denting can even occur during the excavation or later in the dry-sifting or in the wet-sifting process, both of which are performed on mesh trays.

SS – MH’s point is unclear. If he is implying that clear letters on the defixio are not near other clear letters, he is incorrect.

PV – Again the tiny size of the letters, let alone the incision of such letters into lead/metal with a metal or wooden tool, is very difficult to do. The recourse to ‘striations’ is an easy way to avoid the inexplicable, but it is not necessarily the right direction. Some incisions

on the inside are clearly incised (and are therefore not striations) and so are the letters referred to above on the outside. These are not coincidental but apparently man-made, with toolmarks and raised edges.

MH – SS, needs to produce evidence. SS – please take the image provide in the article and draw the other letters, or circle them. I'll accept the Aleph on the outside if SS can show it is part of a word, with another letter(s) on at least one side, whether left, or right or above or below or even diagonally, even sideways or upside down.

I have found lone "Alephs" on my mother's old dining room table – and they look as good or better than this one. Scratches happen – and the very nature of a scratch or dent is that it was caused by a foreign object. I will grant that a stylus should leave behind evidence that maybe will distinguish it from scratches caused by other external objects. But proving a lone letter is a difficult thing. Even in the Serabit al-Khadim inscriptions, scholars would be suspect if only one letter was claimed to be seen. But when that one letter appears next to another letter, or even partial letter, then the first letter is validated. Carved letters are different than painted or inked letters, and must be shown not to be naturally occurring in the surface or caused by accidental external forces. By way of analogy, Gershon Galil has "deciphered" whole lines of text in the Siloam Tunnel – which are likely his wild imagination and are merely natural shapes in the stone (the reader is advised to search that topic on the internet). A lone suspected carved letter must have clear evidence of not being accidental or even naturally occurring shapes in the surface. I must also point out that the nature of lead is softer than stone and therefore even more prone to scratches, dents, bends, striations of all types. Especially after being jumbled in the original excavation, and the dry-sifting on site by Zertal, the later relocation by SS and the eventual wet-sifting on mesh screens. I'd be happy to hear evidence otherwise.

MH – Both Stripling and van der Veen have mentioned in personal communication, the future publication of their proposed decipherment of letters on the outside of the tablet. However, any such future publication will likely be met with strong criticism by the scholarly community, for the outside does not rely on tomographic scans but can already be analyzed visually.

SS – Again, MH's point is unclear. Almost all inscriptions, or potential inscriptions, are criticized by the scholarly community. The tablet's outside clearly shows man-made markings, which are reinforced by the corresponding interior bulges. Many of the interior letters also bulge or protrude to the outside. A clear example is the aleph in the upper right-hand corner of Outer A.

PV – Indeed tomographic scans were made as the tablet could not be unfolded. The study of the outside can be done through direct examination of the lead's surface. It is here that manmade tooling can be observed. We plan to ask a material scientist to test our observations.

MH – My point is that I know of no scholar who has come forward to accept SS et. al. claims regarding the outside. Both scholar and layperson can view the image of the outside provided in the article by SS et. al. I look forward to reading SS/PV's future article, but as of yet, the image provided does not help their case.

MH – It is also important to note that there is another higher quality image of Outer A available

dating to 2022, in which the proposed Aleph is much less visible and indeed the entire left side of the Aleph seems to be missing. A single image can be misleading due to shadows and contrast levels. For this reason, Stripling et al. should have provided multiple high-quality images in their article, both to reinforce their claims and to allow other scholars to make their own assessments more accurately.

SS – Articles have space limits. Obviously, we presented the image of Outer A that most clearly showed what we wanted the readers to see. Our next article will be on the Outer surface and will have multiple pics.

PV – This is how images work. They depend on the right lighting. These images were not made by us, but by the Czech team, especially by the technical members who are not epigraphists. That is why one needs to use the correct images where the lighting comes from the right angle. As for the outside, the identifications are not based on tentative pictures only, but can be made in the light of actual toolmarks, etc. But that is part of the second article on Outer A. Once we can prove that the toolmarks are there, colleagues may wish to say whatever they like to say.

MH – The claim by SS that “Articles have limited space” is invalid. The article by SS et. al. was published in an on-line journal. Space was not a constraint. I published in the same journal and also read other articles in the same journal (Heritage Science) and also in the other journals that also fall under the Springer Open family of journals. I can confidently state that “space limits” are not an issue. Their article could have been several times larger. I was given no space constraints and actually encouraged to provide the highest quality images.

Again, I look forward to reading the proposed future article by SS/PV. I believe it will be better than the first by avoiding GG’s involvement. I highly recommend that PV and SS consider collaborating with a scholar or two that would bring balance to the team (a skeptic).

MH – As mentioned above, they also make the spectacular claim that “YHW” appears also on Outer A. Such a claim lacks evidence in the clear image of Outer A (see above).

SS – Apparently MH has decided we are wrong about the tablet’s exterior without waiting for our forthcoming article.

PV – The fact that MH does not see what seems so obvious, shows how little experience he has with reading proto-alphabetic scripts. It did not take the experts (Brian Colless and Michael Bar-Ron) long to see them and the divine name.

MH – all I’m saying is the scant evidence of the one image SS et. al. provided does not support their claims. I again remind both SS and PV that only the evidence they have provided can be the basis of making their claims. If they have more evidence, they should provide it. I would love for this piece of lead to have early Hebrew writing on it. Maybe it does, but they have not provided proof.

I also ask PV, Is there an article by these two men you mentioned? Please provide a citation.

I did find this article by Michael Bar-Ron – (who is/was PV’s student):

https://www.academia.edu/102614827/An_Initial_Summary_of_My_Interpretation_of_The_Ebal_Curse_Tablet

In the article he makes some interesting connections between the Bible and this piece of lead (namely that the woman being tested for adultery was caused to drink lead – which could cause serious issues for her pregnancy). Maybe he is right – definitely worth a discussion.

Bar-Ron also mentions private access he was granted to the images. Fine. But that doesn't have any bearing here. ONLY published images can be discussed.

I have not found anything written by Brian Colless on the subject.

MH – As was mentioned previously, Stripling et al. took the very confusing approach of drawing diagrams of Inner B in mirror image of the actual scans. This unusual approach only added confusion to their article.

SS – Again, MH states opinion as fact.

PV – Agreed, but only Galil's are shown in mirror image, as he wished to write them as if one were able to unfold the tablet. I disagreed and therefore the letters in the tables by PV/SS are in the right direction, as seen on the scans. Galil and PV disagreed so strongly on many points, that at this stage no cooperation was possible.

MH - Well, on this I don't know what to say. If SS doesn't think drawing things backwards is confusing...!

The drawings by GG were not only backwards, but are very, very sloppy.

This is not worth defending by SS. SS, states "he wished to write them as if one were able to unfold the tablet" but that doesn't make sense. Inner B is on the right side. Even if the tablet were unfolded, Inner B would still be on the right side. Inner A is the side that would be flipped open and would make sense to draw in mirror image. At least that is the way it was presented it in SS et.al. article.

MH – Another claim they make is that bulges on Outer B confirm the letters on Inner B. However, they have failed to show that these bulges add clarity to deciphering any of the proposed letters.

SS – We can do a better job of supporting our claim. See the comments above on bulges.

PV – Again, I explain this a lot better in my PowerPoint presentation and this material will go into a future article.

MH - I'd be happy to look at PV's powerpoint if PV will make it available to the world. Or better yet, make a video and present it online.

MH – The only thing this [bulges] would prove is that the scratches on the inside are indeed there and are not x-ray anomalies. The scratches or bulges do not prove letters.

SS – The marks were identified as letters by three trained and experienced paleographers. MH has no proof that minor scratches would appear on the opposite side of a lead writing surface.

PV – Striations may be visible here and there, but these bulges are so pronounced that they contain the same form in the same place as on the inside. They are so pronounced that they must have been made by a stylus, which literally incised the lead to a depth so

that they became visible on the outside. These incisions are also seen on the shallower scans made of the 'layers' (slices) in the lead nearer the outside's surface. MH hasn't done his homework properly and therefore his article is premature at best.

MH – The lead, according to SS et. al. is 0.3 mm thick. It would be exceptionally difficult to write on a piece of lead that thin, with a stylus, without causing the writing to not appear on the outside. If you don't believe me, simply read PV's response directly above. SS, regarding "three trained paleographers" identifying the letters, some clarity is necessary. The first epigrapher (epigraphy is a profession that overlaps with paleography, but is different) saw what she suspected might be letters but her specialty was not in Semitic. The epigrapher Gershon Galil is unreliable at this point in his career – which is unfortunate, for he is very knowledgeable. I respect PV's knowledge of epigraphy. PV may see clear letters, but again, I stress that SS and PV have to bring the evidence forward. I stated clearly in my article that I saw what might be letters, but there is simply not enough evidence available to be convincing. I stand by what I have said. There may be writing, but the four shapes that most resemble letters could just be striations. It would be fantastic if they are letters, but we need solid evidence to review, not just opinions from SS et. al.

Regarding PV's comment that "MH hasn't done his homework properly" is simply wrong – I have reviewed what has been made available. The fact that SS et. al. have withheld the rest of the images is not my fault. I, along with the rest of the scholarly world, can only review what is made available.

MH – An examination of the close-up images of the bulges in Table 10, is not at all convincing regarding letters on the inside.

SS – Again, MH states opinion as fact.

PV – Based in miscomprehension of what is seen on the inside and what is there on the outside.

MH – Yes, after I reviewed the limited evidence provided, that is my opinion. My opinion could change with better evidence.

MH – It may have been a clasp around a piece of string, as publicly suggested by Dr. Robert Cargill and also Dr. Gad Barnea.

SS – Citations are needed.

PV – It may have been anything, but please first prove your case. Dr. Michael Eisenberg, excavator of Hippos, who has literally excavated many dozens of fishing weights and clasps, told me that the Mt. Ebal lead strip is nothing like it. Barnea and Cargill have not examined the original object, and their opinions are directed by extreme bias, as I personally know from direct communications with them.

MH – Both Cargill and Barnea refuse, to date, to acknowledge your claims in a scholarly article, but their comments are easily found on-line. The editor of Heritage Science specifically asked me to try to focus on scholarly articles. However, I needed to give

these men credit for the idea that the lead is a clasp, since it originated with them and not with me. Robert Cargill's Youtube channel has several videos addressing the Mt. Ebal lead object. I certainly do not agree with all his claims, but the clasp idea is one possibility. Additionally, as I pointed out in my article, if the lead was clasped around a string, it might be expected to have writing inside. I was actually helping SS et. al. case here.

As far as the fishing weight idea, I have reviewed the evidence briefly and this doesn't seem like a fishing weight, but I can't rule that out completely. It could have been anything... that is my point. And even as a fishing weight, it might have had writing on it. Thus again, it may actually help their case.

MH – If there are a few early alphabetic letters inside this folded lead object, they may contain a completely different message from what Stripling et al. have claimed.

SS/PV – I agree. We have never insisted that we know we have the reading correct. We merely presented to the best of our ability, what we thought was the most likely reading.

MH - If the four letters in the upper right corner of Inner B do exist and are not merely striations in the lead, then we have either the word “Depths” or possibly the name “Tiamat”. The word “depths” may derive from the name of the Mesopotamian goddess of the sea, Tiamat, and might be spelled the same in early alphabetic script. If the final TAV is not there, then we are left with the word “depth” in the singular. This reading appropriately lacks internal or external vowel letters, but is from left to right (dextrograde) instead of the later Hebrew direction of writing during the monarchical period which is always right to left (sinistrograde).

SS/PV – We agree that this is a possibility.

MH - Prof. Amihai Mazar has suggested that this folded lead object resembles a fishing weight. If this is correct and if the fishing weight contains letters, the word “depth(s)” would be consistent with the intended use of a fishing weight.

SS – The lead tablet is certainly not a fishing weight as I have demonstrated in my response to Mazar. Here MH accepts the possibility that there could be proto-alphabetic writing on the tablet, but he argued against the possibility earlier in the article.

PV – See my comment above.

MH - Probably not a fishing weight, that is Mazar's idea, not mine. However, in the New Testament we have the fishermen, Peter, Andrew, James and John all traveling with Jesus through the Manasseh hill country (Samaria). Anyone of these men, as an example, could have dropped one of their fishing weights there, or even placed it there on purpose. Perhaps they tossed it there thinking they would never go back to fishing. Obviously, I don't hold this hypothesis, but this example shows that fisherman traveled through the area. There are numerous possible explanations for what this lead object is and numerous possibilities regarding letters on the inside. I think if SS finds more lead objects in future sifting at Mt. Ebal, this current object will be more clearly understood. For this reason, I sincerely hope the sifting project can be restarted soon.

MH – The markings they claim to be a WAW (VAV) as part of the shortened name YHW in the upper right register (laying [sic.] on its side on top of the proposed MEM (Table 4, Scan 1A), actually resembles the Chi-Rho of Emperor Constantine’s time (fourth century). But such a proposal is also based on imagination rather than clear evidence.

SS – The point of this sentence remains unclear.

PV – No, what can be seen is nothing like Chi-Rho.

MH - My point is simply that imagination can play a large part in perceived decipherment. GG clearly has an active imagination as shown by his other recent “decipherments” including another curse inscription. Is it possible GG influenced the rest of the team to see letters where none existed? Christopher Rollston (world-renowned epigrapher) has pointed out GGs very similar curse “decipherments”:
<https://zwingliusredivivus.wordpress.com/2022/07/08/chris-rollston-responds-to-gershon-galil-a-guest-post/>

MH – It also possible that this lead object is a tesserae plumbeae (tablet of lead) used by the Romans for labeling. The size is consistent with this possibility as well as the fact that they were inscribed on both sides. These labels would contain artistic markings and/or inscriptions. They would even be used as admission tickets to the theater or circus. In other words, this could have been a theater ticket, containing artistic markings, that a Roman soldier folded in half and lost on Mt. Ebal.

SS/PV – If we are incorrect about the presence of proto-alphabetic letters on the interior and exterior of the tablet, then this might be a plausible identification.

MH – Agreed. If you can prove the early-alphabetic letters exist, then dating is narrowed significantly.

MH – The only substantiated claim that Stripling et al. can make at this time is that they have found a very old, small piece of folded lead on Mt. Ebal using wet-sifting. While such a find highlights the usefulness of wet-sifting, there is currently insufficient epigraphic evidence to conclude that writing exists on the interior or exterior of this folded piece of lead.

SS/PV – Again, MH states opinions as facts.

MH – Better evidence in the SS et. al. article would dissolve my opinions quickly. The article simply does not present enough evidence to establish facts in this case. My opinions are based on the evidence before me. Provide more and better evidence and my opinions will adapt accordingly. Substantiate the case sufficiently and not only my opinion, but the opinions of so many others will change. I’m not bound to any particular viewpoint, but I can only view what is presented.

MH – As of July 2023, Stripling and Associates for Biblical Research continue to promote this folded lead object as a curse tablet containing ancient Hebrew writing, including the name of the God of Israel.

SS – Many scholars and organizations (like ABR) have evaluated the evidence and concluded that there is likely proto-alphabetic writing on the tablet and that it contains the word curse and the divine name of Israel’s God.

PV – Also other very learned epigraphists agree that letters are there. But unfortunately, they do not yet dare to stand up for themselves.

MH –because the evidence is not sufficient and because of GG’s blatantly wrong claims here and regarding other epigraphic and imagined epigraphic finds, most scholars won’t touch this. I wrote my article as a response to SS et.al. article because I believed (and also the editor stated) that a response in the same journal was appropriate. My purpose is to bring balance to what was claimed and to not leave the claims unchecked before the public. I hope there is proto-alphabetic writing on the inside and even on the outside. I do not hold the bias that certain other scholars hold. I hope for more early alphabetic discoveries.

MH – A recent scientific development is a technique called “neutron tomography”, which has been implemented successfully to read the inside of the lead Bispegata amulet and perhaps could be implemented to shed more light into the folded piece of lead from Mt. Ebal.

SS/PV – I agree that this new technology could be helpful.

MH – Agreed all around – apply the neutron tomography to this piece of lead and let’s get to the bottom of this debate.