

Reply (No 4) to Reviewer #2

Dear Reviewer #2.

A comment paper is not a peer review of a published paper, but it expresses additional points on the subjects discussed in the principal paper. A comment paper may contain errors or to be unacceptable for the technical reasons. It is why a comment paper is open for discussion before publishing. As far as we understand, it is not this case. In your "manuscript evaluation" message, you do not show that our comment paper is incorrect but recommend blocking it by some incomprehensible reasons. We think your position is not correct, and we do not understand why you are so against our comments. These comments do not attack the principal paper but just extend the subject discussed in.

However, we will respond your main objections.

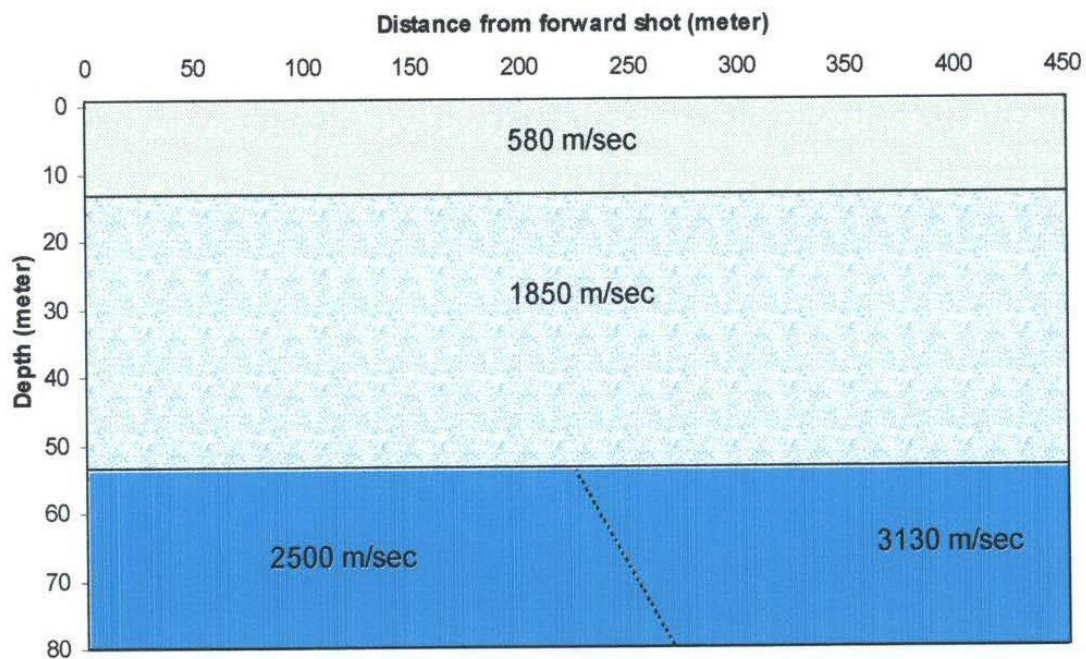
Our comment put attention on main conclusions of the Polom's et al. (2018) paper. In the abstract Polom et al. (2018) state: "...this study aimed to clarify the subsurface characteristics responsible for sinkhole development... The most surprising result of the survey is the absence of evidence of a thick (> 2–10 m) compacted salt layer formerly suggested to lie at ca. 35–40m depth. Instead, seismic reflection amplitudes and velocities image with good continuity a complex interlocking of alluvial fan deposits and lacustrine sediments of the Dead Sea between 0 and 200m depth".

The questions arise: 1. Is it geophysical paper or geological, where absence of salt is goal of this paper to insert new (really, very old) mechanism? It is exactly the same as abstract of Sc. Krawczyk (2015). 2. Is the tracing of alluvial fan deposits goal and possibility of seismic reflection method?

You write that Polom et al. (2018) present a complete review of the geological knowledge of the area up to now. You mean report published by El-Isa et al. (1995). But they do not discuss results published by Taqieddin et al. (2000) published in the respective International Journal. They mention only that Taqieddin et al. (2000) presumed a massive salt layer. Would you please explain why S. Taqieddin and M. Abdallah, participated in the El-Isa et al. (1995) survey, constructed geological model of the massive salt layer that is not corresponds to the drilled boreholes. This disagreement in interpretation can be explained only by absence of the data from the bottom of the borehole BH 2. It is what we try to explain in our Comment in Fig. 1. (By the way, only Taqieddin et al. (2000) mentioned "massive" salt layer. They do not define what means here the term "massive").

With respect to your comment (e). We present the paragraph of the Polom et al. (2018) (see line 65-70 of our initial Comments) where authors explain the result, that "Because of these general improvements, some structures in the near surface down to 50 m became weaker than in the first iteration". If Polom et al. (2018) write that his strong filtering has softened reflections within 50 m depth interval, how he can see salt layer at 35-40m? According to our knowledge, the data processing in seismic reflection allows to define the depth interval where filtering can be applied without influence to more deep structures.

We do not understand your sentence on Sawarieh et al. (2000). Polom et al. (2018) use results of Sawarieh as support of the salt absence concept. Any geological (geophysical) model should explain available data. If you suggest that the salt border occurs along sinkhole line, all results of the previous researchers become clearer. If we place lines of Sawarieh et al. (2000) on a map with salt border, (Fig. 2a of our initial Comments) one can see that  $V_p$  from the both sites are essentially different (Fig.5-18 below) from Sawarieh et al. 2000. Left part of the section (eastern) is characterized as low velocity ( $V_p < 3000$  m/s) whereas right part (western) is characterized as high velocity section ( $V_p > 3000$  m/s). Border between these sections divides salt and no salt areas. The same idea is supported by Abueladas and Al-Zoubi (2004).



**FIG.(5-18) Depth section along Profile 11+Profile 12.**

And it relates directly to the salt layer concept. East to this border numerous sinkholes are formed. Similarly, line 4 and 5 are mentioned in Sawarieh et al. (2000). Most of the lines are located out of the salt area and characterized by  $V_p = 2200-2500$  m/s.

In subsection 5.4. we are discussing on applicability of the seismic reflection method to mapping of the unconsolidated sediments, but not to the salt layer presence. You constantly try to lead us from the main problem under study to other side. However, Al-Halbouni et al. (2017) results do not give any support to reflection seismic ones. These results consider surface data that do not relates to underground structure of the subsurface. Vice versa, Al-Halbouni et al. (2017) refer to results of Polom et al. (2018) based his model on salt layer absence (subsection 5.4 of Al-Halbouni et al. (2017) (lines 1050-1070, before subsection 5.5)

You are right in paragraph J (subsection 5.4). Generally speaking, the maximum depth is related to properties of medium, minimum frequency of records, energy of the hammer, and to presence of higher modes. However, we comment conclusions of Polom et al. (2018) related to frequency of geophones and line length (p. 94, left column

(lines 29 and below)). It should be added that Bodet et al. (in Ezersky et al., 2013) used the fundamental, first and second modes and visible frequencies were lower than 4.5 Hz. So, above authors evaluated penetration depth as 60 m taking into account that in the Ghor Al-Haditha area profiling with 120m line length, 4.5 Hz geophones and 3 modes were carried out. It enabled to reach 60m deep. It is our remark.

The "Comments" is not article, but only comments to published article. Its publishing have to allow geophysical-geological and other readers be judge and do their own conclusions.

We must note that Reviewer did not disprove our arguments, and sometimes simply rejects our conclusions. The article of Polom et al. (2018) was published and we have no objection to this in any way. Authors and Reviewers of the aforementioned paper carried out a large work and our goal is not downplaying significance of this work. We want to express doubts to arguments of the authors aimed to disregard the salt layer presence in the Ghor Al-Haditha area.

In addition we want to declare our opinion that disputants should participate with their original names to exclude the possible conflicts of interests. Disputants are not reviewers of our publication. Otherwise there are interested do not allow by anywise to publish our criticism.

## References

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