Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-132-RC3, 2019

Solid Earth
Discussions

Interactive comment on "Comment on "Shear wave reflection seismic yields subsurface dissolution and subrosion patterns: application to the Ghor Al-Haditha sinkhole site, Dead Sea, Jordan" by Polom et al. (2018)" *by* Michael Ezersky et al.

Anonymous Referee #2

Received and published: 27 November 2019

Dear Authors, After reading all the material included in the discussion as well as Polom et al. (2018), Ezersky et al. (2013) and Al-Halbouni et al. (2017), I think your Comment Paper has some weaknesses that prevent its publication as it is. These are points that can be addressed in your future publication and that I am sure it will make a valuable contribution to all studies related to sinkholes and subsidence in the area as your work has already shown. I think a comment paper on geophysics must show arguments about why the original paper has failures in acquisition, processing or interpretation.

Polom et al. (2018) summarizes the two main interpretations about the origin of the sinkholes and subsidence in the Ghor Al-Haditha region (Jordan): a salt layer below alluvial deposits (hypothesis that is supported by your group) and erosion of weak material (evaporites included within fluvial sediments) combining chemical and mechanical erosion which is used for interpretation in Polom et al. (2018). In my point of view, these are two interpretations that are not necessarily incompatible. Your comment on Polom et al. 2018 relies on 1. the assumption that the salt layer is there and that part of the profiles do not cover the area delineated as "salt layer" 2. Problems during seismic reflection processing. 3. Previous geophysical surveys. Regarding the main points introduced in your comment. a) Introduction. You contend that geological content in Polom et al. 2018 is not "quite correctly displayed". I think that they present a complete review of the geological knowledge of the area up to now. b) Introduction. You say that new essential arguments are formulated. In my opinion, I think your disagreement with the geological interpretation of Polom et al. 2018 is based on preconceived notions not in new essential arguments that I am sure that you will include in your new work. Unfortunately, I cannot see them in this comment. c) Geological context. I find this section a little bit vague. Surely, ground-truthing is a requirement for this area in order to constrain the geological interpretations. Of course, this is not always possible. d) Data acquisition. Scattering observed in seismic data is interpreted as related to the salt area. This could be more related to near-surface heterogeneities. e) Data processing. One of your hypothesis is that over- filtering has removed an expected high amplitude character of the reflection originated at the salt layer. I think that that would have affected all the reflections. I presume that a priori strong reflection from the salt should keep higher amplitude after filtering than the reflections coming from seismic contrasts within the sediments. f) Discussion. Point 5.1. This is not a new essential argument but explain results from previous work. g) Discussion. Point 5.2. I think resolution of the seismic sections shown in Polom et al. (2018) is well explained. h) Discussion. Point 5.3. You add here an interesting point that highlights monitoring as a requirement to increase knowledge of the sinkhole processes. This also has been highlighted in Polom et al. conclusions. i) Discussion. Point 5.4. You are right that without boreholes any interpretation can be speculative but this works for seismic reflection and for any geophysical method. Anyhow, the new model of sinkhole formation is not only based on seismic but also in Al-Halbouni et al. (2017) interpretation. I do not think that this means that the salt layer model is wrong. I think that the Ghor al-Haditha zone is very peculiar since there is not any borehole information that confirms the salt layer presence. As far as I know, that layer was detected by drilling in the Israeli side. That makes interpretation of sinkhole phenomena on the Jordan side completely open. j) Discussion. Section 5.5. You are right that maximum investigation depth for surface wave methods is quite controversial. But I do not understand your point about the extension of 4.5 Hz natural frequency to a lower end (which of course is true, you have lower amplitude but you still can detect energy at lower frequencies). However, I do not think the problem for active seismic is a matter of the receiver frequency. It is more related to the source characteristics. A hammer can have problems to generate enough energy at the lower frequency. In addition, Ezersky et al. (2013) explained how maximum investigation depth was increased introducing higher modes in the inversion process. Hence, I do not think maximum depth is related to frequency anyway. I guess the modelling of fundamental and higher modes can be the best argument to support the detection of high velocity at depth. k) Last sentence of the conclusion. I think that the points introduced in the comment paper are not enough to arrive at that conclusion. In summary, Polom et al. discussion is well established and defended by data quality, processing, and uncertainties assessment. I do not see the point of publishing a comment on that paper without more datasets or ground-truthing only relying on another interpretation that of course can also be valid. I think interpretation of Polom arrives as far as possible always supported by their geophysical results. I am sure that your work will do the same with new datasets and this will be fruitful for scientific discussion since different points of view are one of the foundations for knowledge increasing.

References: Al-Halbouni, D., Holohan, E. P., Saberi, L., Alrshdan, H., Sawarieh, A., Closson, D., ... & Dahm, T. (2017). Sinkholes, subsidence and subrosion on the

eastern shore of the Dead Sea as revealed by a close-range photogrammetric survey. Geomorphology, 285, 305-324. Ezersky, M. G., Bodet, L., Akawwi, E., Al-Zoubi, A. S., Camerlynck, C., Dhemaied, A., & Galibert, P. Y. (2013). Seismic surface-wave prospecting methods for sinkhole hazard assessment along the Dead Sea shoreline. Journal of Environmental and Engineering Geophysics, 18(4), 233-253. Polom, U., Alrshdan, H., Al-Halbouni, D., Holohan, E. P., Dahm, T., Sawarieh, A., ... & Krawczyk, C. M. (2018). Shear wave reflection seismic yields subsurface dissolution and subrosion patterns: application to the Ghor Al-Haditha sinkhole site, Dead Sea, Jordan. Solid Earth, 9(5), 1079-1098.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-132, 2019.