Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-132-RC2, 2019

Solid Earth
Discussions
Open Access
EGU

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 #1

Received and published: 9 November 2019

Dear editor and authors, Thank you very much for your detailed answer and tone from the carried out answer to my previous comments to the submitted reply to Polom et al., article. As authors indicate, there are some subjects that should require a long complete manuscript to be detailed evaluated. Authors indicate that "to comprehensive reply to it we need to write a new article". This is something that I share, as the background problem can affect to security and risk from the region. In this sense, while the considered data can be used to infer a later hazard evaluation, manuscript from

Printer-friendly version

Discussion paper

Polom et al., does not evaluate this subject, or it does not evaluate that the sedimentological and paleogeographical evaluation should be change at the light of the obtained data. Polom et al., evaluate some of the available local geological data that are considered as different regarding some of the previous publications at the area (as they pointed out that they found some “inconsistencies” in the previous literature). Polom et al found that their results permitted to obtain, within the resolution of the used technique and processing methodology, an interpretation about the potential vertical series inferred from indirect data and from regional geology. These authors do not produce a conceptual hazard mapping or considerations about the security from the area, as these authors make their survey along an area where surficial karst evidences exists. This geophysical data interpretation can be evaluated later from an author or group of authors in order to evaluate other geological subjects or to perform a karst hazard analysis. However such model will be required to be congruent with both the available data and the resolution and meaning from the indirect data from the area. However, some of the considerations from the reply article comprise more about “what potentially can be done from the data” than “what the article from Polom et al., describe and analyze”. I agreeing with authors in the submitted answer letter that a hazard evaluation done from these data could orient incorrectly about the real problematic in such area if this is carried out incorrectly. However this is not the objective of the manuscript and any analysis with this objective will require evaluating data in terms of resolution and potential meaning from the geophysical data. All in all, I consider that the background from the discussion is not about the geophysical model and the obtained results from the carried out processing of such data from Polom et al., and it is related with the eventual potential data that can be obtained or inferred from such data in future articles or works in the area. I believe, that in such cases, the discussion about the geophysical data can be of interest when their meaning is going to be used for other analysis. As pointed out in the letter, but also in the answer from author′s reply, the background from the analysis or the discussed subjects overcame Pollom et al., article, and considering the required detailed analysis that can be needed in order to constrain hypothesis from

Printer-friendly version

Discussion paper

the area, data from the geological underground and the meaning of regional and local geological data will be needed to be discussed and evaluated. About the discussion of the propagation velocity of vp waves, they can change due to the materials and their state, considering the potential solution of part of such materials, velocity can change. At Ezersky et al., 2013, a reference is given to the expected velocity for the evaporitic unit with 2900-4080 m/s (as also included at table 2 from 2013 article). However these data, as happens with Polom et al., are obtained from the indirect evaluation of geophysical data, and they are compatible with the evaluated model for the area. However if a general evaluation is carried out for the velocity at such materials, considering that they can partially be affected by solution, they can present lower velocities than the raw salt levels. I must consider that the evaluated propagation velocity for Polom et al., it is unusually low for a salt layer, but they can present inter-beds of different materials or they can be partially karstified. In this sense, again, the same results can be interpreted in different ways. With independence of which group of authors can obtain a better approach defining the geological data from the area from their research, the arguments proposed in this moment are not balanced. There is an analysis of data from Polom et al., against some considerations pointed out by Ezersky et al. that do not false the considerations from Pollom et al. Polom et al., presented falsifiable arguments about the obtained data and, in this case, the carried out analysis at the reply′s article is not refutable with the presented and given data. I invite authors to prepare a complete manuscript about this and another subjects, presenting data from the same area or a similar context in order to show their model, in a similar approach than Polom et al., carried out. This work should give an interesting article and discussion for the geological community about this interesting subject and their meaning in terms of the geological karst hazards from the study area. However, the comparison in this moment between Pollom et al., article and Ezersky et al., are not in the same scale in scientific terms. This does not mean that Ezersky et al., cannot demonstrate their arguments, it is just that they cannot be carried out in a reply as presented, and requires a complete article that is what I suggested to authors and it seems that authors are

Printer-friendly version

Discussion paper

preparing in this moment. Such article can be defined in terms of a review article in the area, where authors can present the different potential interpretations, discuss the state of the art from the used techniques and data, and then produce a discussion of all the previous available data. My evaluation is just a consideration from my personal perspective about what I consider should be a reply; however editor will evaluate the different opinions and arguments in order to get a decision. Considering that my letter was considered by the authors, and they used part of my text in order to answer, I should suggest that it is better don′t to change what is said, I didn′t say that the reply "must not be published", I suggested that in my opinion "it should not be published".

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

Printer-friendly version

Discussion paper