

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

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Dear editors and authors, I have received a comment to a published article at Solid Earth (SE) entitled “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)” and submitted by Michael Ezersky, Anatoly Legchenko, Lev Eppelbaum³, Abdallah Al-Zoubi, Abdelrahman Abueladas⁴Discussed paper published at 2018 at the same journal was entitled “Shear wave reflection seismic yields subsurface dissolution and subrosion patterns: application to the Ghor Al-

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Haditha sinkhole site, Dead Sea, Jordan", authored by Ulrich Polom, Hussam Alrshdan, Djamil Al-Halbouni, Eoghan P. Holohan, Torsten Dahm, Ali Sawarieh and Mohamad Y. Atallah, and Charlotte M. Krawczyk published at Solid Earth, 9, 1079-1098. I believe that a discussion on a previous article requires evaluating both the original published article but also the submitted discussion, and during the analysis period two interactive comments have been published by the different authors. The submission of a reply article is not as usual as it is the publication of a standard article and I am less used to review this kind of articles. I believe that a reply should be done when an article represents mistakes that are known by other researchers that invalid the conclusions from the carried out analysis in a manuscript, or when it presents an incorrect or partial data evaluation to get different conclusions than such are known from an area. In some cases when a controversy in the presented data can lead to incorrect knowledge, a reply or discussion can be also of interest. Looking at the web, possibly I have just done a very short review about the rules to perform a reply paper, I have found a manuscript found at PLoS Comput. Biol entitled "Ten simple rules for writing a reply paper" (PLoS Comput. Biol 2015, 11(19):e1004536. That I have followed in order to evaluate the submitted reply. Editor and authors can evaluate other rules of publishing but, at least in my opinion, it has helped in my evaluation of the submitted reply. About the original published article. Original article represents an interesting article, with detailed descriptions of the geological context, indicating that there are some "inconsistencies" from the carried out bibliographical analysis about the distribution of the salt layer in the underground. That can be an interpretation from the carried out analysis by those authors. These authors have considered the available information for their manuscript context and they have evaluated such data and have given their interpretation about the weight that these articles represent in the geological context where they study. Moreover authors from the preliminary manuscript also evaluate previous data, in some cases unpublished, that defines the complexity of evaluation of different geophysical data in such context. From this evaluation authors consider the interest of using another approach (technique or data processing) in order to evaluate

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the potential karst problems from the area and to obtain indirect information from the underground. Data description is detailed and processing explained, and problems related to the obtained results are described due to the potential characteristics of the area but also from the potential artifacts that processing can produce. This complexity was described in the introduction at the article, making reference to the potential heterogeneous series that can be present in the geological context (unconsolidated sediments) and also by the potential changes related to the karstic processes and faults in the area. Any geophysical technique has its resolution, potentiality and the possibility to obtain univocal interpretations or not depending of the available information and the consideration of the state of the art of the technique. Moreover the potential interpretation depends also from the geological context where the research has been carried out. It is obvious that indirect characterizations are not enough to obtain interpretations about properties that are different from the analyzed. In this sense, the interpretation of the geophysical records are always intuitive, open to re-evaluation depending the available information and the contour conditions defined for the interpretation and the conceptual model inferred from the expected geology from the area. Authors describe the results, and discuss data regarding different previous interpretations in the area, and potential differences to previous published articles. This seems for me a correct article that was accepted for publication, and in such case, if I have had to review it, I should suggest its publication. About the submitted reply. Ezersky et al., submit a comment to previous article highlighting some subjects about the incorrect carried out work from Polom et al., making reference to the “geological context”, “data acquisition”, “data processing” and about the discussion chapter from the article (thickness of salt layer, on salt layer degradation, application of seismic reflection to map unconsolidated sediments). Ezersky et al., point out a problem in the geological context evaluation from Polom et al., about the location of a salt layer in the underground that is evaluated in terms of correlation to the origin of some of the karst problems in the area. Ezersky et al., review literature from the area (both published and unpublished data) to arrive to the conclusion that part of the carried out geophysical survey in the area

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has been performed in a sector where regional geology should indicate the absence of such continuous level in the underground. About data processing Ezersky et al., points out about the potential incorrect carried out data processing. While it is true that the processing must try to improve data quality to favor its interpretation, and this requires improving the visibility from shallow and deep data, in some cases this is an equilibrium that can produce that signal improvement requires a loose of resolution from the shallow data to look for changes below. In this sense, the criticism by Ezersky et al., can be correct but not means that authors have done this intentionally and moreover they have made reference to such problem in the original article. The criticism from Ezersky et al., is related with an independent potential interpretation to explain the presence of a layer of salt that can be in the area where the signal/noise ratio has decreased. However, as previously stated, the interpretation requires robust geological and direct data, and the carried out interpretation is just an independent interpretation that arise from the inverse method in geophysics. This interpretation is done without real data to be compared or to false the carried out interpretation in the preliminary article. This alternative interpretation is an interesting hypothesis, that can be correct but without data to permit to identify a mistake from the original manuscript (e.g. an independent interpretation from the same geophysical data). The rest of comments included in the manuscript make reference to what seems a trajectory of different interpretations from the same data from different researcher groups, as in some cases authors make reference to previous articles that are also discussed in the actual reply of Ezersky et al. Summary. The carried out reply points out to a mistake about the presence of a salt layer in the underground. In some cases, the state of such layer can be affected by solution decreasing the potentiality to be identified from the geophysical data. Preliminary authors pointed out to some controversy from the previous bibliographical data and evaluated the variability that can arise and the presence or absence in the underground in a sector where no direct data exists. Moreover, I am not expert in geology from the study area, but I believe that the contact of a unit, mainly related to evaporites, do not require a lonely straight contact as included at the figure 2 from the reply. The

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levels can change laterally and the evaluation is being carried out from indirect data and regional correlation from something that do not outcrop. I believe that the interpretation from Ezersky et al. can be correct, but it does not decrease the interest of the original manuscript, as they pointed out the eventual controversy about such data, and the article just contextualize the geophysical data and try to interpret such data at the evaluated context. Authors' reply do not give new data or independent data that justify their interpretation, that can be correct, but they are evaluating not evident direct indicators to false what is said at the original article. They just evaluate bibliographical data that is also open to controversy as there are some different interpretations from the same matter in previous literature. Some considerations about the geophysical data and data processing are also carried out. Original authors evaluated and described them in their article. In this sense, it can be difficult to get conclusions from data that can be complex to be processed and evaluated, but preliminary authors described the identified problems producing that their interpretation are given with the required caution. All of this data produces that, in my opinion, reply article does not give any new data, it just represents that the same data can be interpreted in a different manner and it does not decrease the interest or application from the preliminary article and their robustness of their interpretations. With these data, I return to the referred article about the rules to produce a reply article that I have referred previously. I believe that there are some subjects that require a deeper analysis in order to produce a reply respect what was submitted. In this moment, is just an evaluation of other potential interpretations that goes farther than the lonely discussion of the presented data in the original article. In the reply is said that authors are preparing an article about the geophysical data from the area, that is referred in the reply as Ezersky et al., at the line 94-95 "(this paper of Ezersky et al. has been submitted for publication" but is not included at the reference chapter of the manuscript and it can be the place to develop a detailed analysis with robust data and where authors can discuss their interpretations against previous one. In my opinion, the submitted reply defines a different interpretation from authors supported with previous bibliographical information that were described by Pollom et al.,

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that can be interpreted as non univocal. The reply article just evaluates what someone interpreted previously that is within the resolution of the carried out approaches before in the area. The criticism about the processing and interpretation of the geophysical data is within the expected for an indirect approach, and preliminary authors describe the identified problems in the manuscript, they do not hide the identified problems, while they evaluate data with their knowledge. All of these subjects are inherent to the usual “inverse problem” in geophysics. In this sense, I believe that the submitted reply/discussion does not represent a problem with the preliminary publication, and the different interpretation that can arise from the same data can be objective of a research article where new data can be presented, where discussion can be performed and where Ezersky et al., can present their interpretation with the required supported data. In my opinion, this reply do not fit with the expected for a reply article, and in this sense, I believe that it should not be published.

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

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