

## ***Interactive comment on “Sinkholes, stream channels and base-level fall: a 50-year record of spatio-temporal development on the eastern shore of the Dead Sea” by Robert A. Watson et al.***

### **Anonymous Referee #1**

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General: The manuscript shows a detailed work on geomorphological processes related to the Dead Sea base-drop for the last 50 years as they express in alluvial incision and sinkholes formation at Ghor al-Haditha, Jordan. The methods include topographic analysis, orthophoto analysis, hydrological isotopes analysis and field observations. The research shows some findings of channel morphology as expected from the local slope, strata and inflow and sinkhole formation in accordance with previous findings along the western Dead Sea shores. A major missing component (as admitted by the authors) is an analysis of the hydrological boundary of the fresh-saline water in particular and the underground water levels and composition in general. In addition, the authors fail to properly contextualize the results with previous findings along the Dead

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Sea area. The manuscript and the readers will benefit from detailed comparison with similar results described in the papers already cited in the current manuscript. Furthermore, the isotopic analysis is incomplete, its results are faintly included in the discussion and cannot support any conclusion regarding salinity. Na/Cl content for example would provide more conclusive information regarding dissolution processes. Overall, I find this paper findings to be of much interest showing the Dead Sea base-drop effects are similar on similar environments on either side of the Dead Sea. However, in its current form, I find it is more of a summary of observations and has limited scientific value. I would suggest a major revision and addressing the points below:

specific comments: Line 31: The response of the surface and subsurface hydrological systems to the base-level drop have been presented previously by e.g. Arakin et al., 2000 env. geol.; Bowman et al., 2007, Geomorphology; Avni et al., 2015, JGR; Shviro et. al., 2017, Geomorphology; I would suggest avoiding using the term “first” here, or explain in detail this research novelty in this context. Line 142: Some error estimations should be provided for the co-registration as done for the DSMs. I’m concerned 9 GCPs are not enough for proper geocoding. Line 189: Please add a theoretical line, based on water level drop and slope. I suspect the non-linearity origin is from the non-linearity of the water-level drop rates. As it is described now, one might think it is an abnormal observation, while it might be an expected one. If it does not in agreement with the expected line, a more detail discussion should be added. Lines 291-299: I would suggest putting the sinkhole morphology in context with previous (similar) findings from the western Dead Sea shore. This will strengthen the globality of the findings and put them in proper context rather than highlighting a very local phenomena. Line 401: It is not clear why there should be higher evaporation in the salt-edge ponds with respect to mud-edge ponds? They are situated in very close proximity and same environmental conditions. Further water composition analysis would be useful for determine if water samples are of evaporative fractionation or mixing of different compounds. I suspect the difference between the two pond types is mainly due to salt dissolution. In addition, the isotopic result is not included in the

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discussion and have little to no support to the conclusions. I would suggest expanding the isotopic and hydrochemistry analysis and to include it in the interpretation. An example of such analysis could be found in e.g. Avni et al., 2016. Line 438: In line 426 it is stated the northern part has steeper bathymetry and here that they are similar. Line 440: Discharge rates are only quoted for the meandering channels and no information is provided for flash floods. I fail to understand how sediment load is related to the morphology. Here you refer the sediment deposits only to support the assumption of the discharge rates. I would suggest obtaining estimations of flash floods discharges to support this assumption. Could the coarser sediments might be originally forming the channel beds and not transported by flash-floods? Lines 512-513: These findings should also be discussed in context of Baer et al., 2018 (doi: 10.1002/2017JF004594) findings. Lines 532-536: The depth of the water table in the area and that of the Halit deposits (if present) are required to make this comparison between shallow limestone karst and the Dead Sea Uvalas. Without additional data, the depressions are "widening without deepening" where the base-level fall can be as easily explained by the fact that the karstic layer (Halit) is limited in its thickness as observed on the western side of the Dead Sea (e.g. Ychieli et al., 2016). Line 541: I fail to see the new insights here. The link between the Uvales formation and sinkhole process is documented in several previous papers cited in the manuscript. Line 556: The statement "Evidence . . . is weak" is simply wrong. See for example Avni et al., 2016, figure 6. The seaward shift with time is much more pronounced than in the current paper. Line 559: I cannot see why this is a stronger evidence than that of e.g. Abelson et al., 2017. Without any information on the fresh-saline interface, it cannot support this theory. Channeling may explain the observations much as well (Arakin et al., 2000) without any evidence of a salt layer and dissolution processes. Line 564: The findings of Polom et al., 2018 of missing salt layer in the fan area may indicate a local area of increased fresh water streaming and accelerated dissolution that removed the salt layer in that area by the time of survey. These results, should be considered with much care for inferring general process related conclusions. The conductivity and mineral contents of the water

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samples may indicate dissolution processes which is in contrast with Polom et al., findings. A more detailed hydrological analysis may better resolve this issue. The fact that with time, sinkhole distribution is along the whole area, (almost) without gaps, along a very distinct sub parallel line to the shore indicates the possible presence of an underlying salt layer undergoing dissolution processes. Line 583: technical corrections: Line 97: “there three” should be “there are three”. In general to all figures with topographic data: I would suggest overlaying the color coded elevation over a hillshaded elevation to better express fine details. Line 144: Please add a proper citation to the GDAL library (see: <https://github.com/OSGeo/gdal/blob/master/CITATION>). Line 484: “is agreement” should be changed to “is in agreement” Line 600 (fig 16): Please correct the green arrows color, they are nowhere to be found in the plot.

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-105>, 2018.

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