

Interactive comment on “Vulnerability of tourism development to salt karst hazards along the Jordanian Dead Sea shore” by Najib Abou Karaki et al.

Anonymous Referee #3

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The manuscript describes damaged infrastructures along the northeastern shores of the Dead Sea and presents results from three different methodologies that demonstrate the damage and are aimed to be used as inputs for a vulnerability map of the region. Comments: 1. The title of the paper and the expected product of this study is a vulnerability map (section 4.4). The definition of vulnerability is the degree of (potential) exposure to damage, and in this respect a vulnerability map should show levels of potential (future) damage, in areas that were damaged and in those that were not damaged yet. What is shown here are damage classification maps of specific areas that cannot serve as vulnerability maps for future planning in any nearby or other areas in NE Dead Sea. The workflow for preparation of such maps is described in page 8

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and Fig. 3 and includes InSAR, vegetation, wells data, salinity variations and more methods, but the maps shown in Fig. 10 were not prepared by any of these methods and a resulting vulnerability map is not shown in the paper. The proposed approach is said to have been proved several times before (page 8, lines 28-30) but no references are given for the reader to understand what has been actually done before. 2. The title and many places in the text use the term "salt karst". In many places along the Dead Sea (the entire west coast and possibly also the southern east coast), a layer of salt is dissolved and salt karst sensu-stricto develops. What is described here as the cause for landslides, subsidence and sinkholes is chemical and mechanical erosion of interstitial salt that remained between the grains when the DS level dropped and was washed by fresh groundwater. This is not salt karst. Furthermore, the proposed mechanism is speculative in its basis and has not been proved by any of the methodologies used here. What can be the size of a cavity that is formed from such salt remains? If fresh water dissolves that salt, it should show chemical evidence for dissolution in the springs, such as Na/Cl ratios, density, etc. Without such evidence the entire theory cannot hold. 3. On page 9 the authors write that the velocity map supports the hypothesis that the subsidence is the result of chemical erosion and that the landslides and sinkholes are consequences of mechanical erosion by underground water flows. No other mechanism (e.g., consolidation-driven subsidence; gravity-driven landslides) is even considered (or rejected) and no independent evidence is shown to prove this hypothesis (see also section 2 above). The fact that highest subsidence is found in the exposed muddy plains may support the consolidation mechanism. Furthermore, how do the authors prove the existence of mechanical and chemical erosion and how do they distinguish between them and relate each mechanism to a different phenomenon (landslide, sinkhole, subsidence). 4. The proposed mechanism for landslides is also by "increased lateral water injection into soft sediments on a slope balance profile created under the DS level. ...favored by a sudden drop of the DS level that usually occurs during the dry period" (page 12, lines 3-5). There are continuous monthly measurements of the DS level since 1976, and if the authors looked at these results they would have

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found that there is no sudden drop in the DS level in any of the 3 periods, and on the contrary, from February to May 2009, the level even rose by about 3 cm. This speculation adds to the previous ones and gives the impression that although the damage observations are clear, the mechanism is far from being explained. 5. Subsidence is also interpreted as a consequence of permeability increase due to fractures, but no evidence is given that certain areas are more fractured than others. 6. Geological setting: The reference provided for the Lisan Formation is Landmann et al., 2002. This formation has been defined much earlier (Begin et al., 1974 and 1980). In a similar manner the fact that the DST is an active structure has been shown long before Al-Awabde et al., 2016 (e.g. by Garfunkel, Freund, De Sitter, and many others in the second half of the 20th century). Citations should refer to the earliest or to the key papers that mention the feature. 7. InSAR: In page 3 the authors write that they analyse both D-InSAR and A-DInSAR. In page 5 the authors write that the derived products are intensity and coherence maps, interferograms, differential interferograms (what is the difference between the two?), velocity/displacement maps and ground displacement time series. In the paper we can see only one map of vertical velocity (Fig. 4.). My questions are: (a) What is A-DInSAR and where are all the other radar products mentioned in the paper (including time series that were mentioned again on page 8 line 1-2)? (b) InSAR measures satellite to ground line of sight (LOS) displacements, while Fig. 4 shows vertical velocities. How were the LOS measurements converted to vertical velocities, and how did the authors take into account possible horizontal movements (particularly important in cases of landslides) that could also be components of the measured displacements? (c) As InSAR is one of the major techniques, some elaboration should be added regarding to the noise level, the elevation model (DEM) used in the processing, incidence angles, etc. This is particularly important because velocities lower than 10 mm/year are also interpreted as real (Fig. 4 and page 11, lines 21-23). (d) West of the 2000 shoreline there is no SRTM DEM (page 7, line 17), so how was topography corrected for these important areas (where most of the subsidence occurs). The coastline of February 2000 should be shown on Fig. 4 so that the reader could get an

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impression of where topo corrections were made and where not (or made by another way). 8. Discussion: A discussion should deal with the results of the paper. The majority of the discussion (from line 9 in page 12 to the end of the discussion) is made of declarations about the importance of the area and of carrying out the research in this area, meetings, strategy, etc. This is not an appropriate discussion in a scientific paper. 9. Conclusions: The conclusions should also deal with the results of the paper and instead they mention (for the first time in the paper) a future EWS, and repeat some general declarations about the world changing and the need for expert overviews of the environmental situation. 10. To summarize this review, in this manuscript there are many examples of damage, in pictures and maps, but no demonstration of their use for vulnerability assessment and no proved mechanism. The paper is on a level of a technical report and not of a scientific paper in a high-impact journal. My comments above deal with every aspect of the paper and thus only complete rewriting may bring it to the required standard. Thus, I recommend rejection of the paper.

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