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Discussions

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 #2

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General comments:

The study of Abou Karaki et al. deals with the sinkhole/landslide hazard at the north-eastern shoreline of the Dead Sea. Specifically, the authors use a multi-methodical (times series of InSAR, analysis of optical satellite data, in-situ observations, public science) approach to derive the vulnerability of 5 hotel areas, which, in the past, have been subject to severe infrastructural damage. Looking at the InSAR time series alone lets the gentle reader directly draw the connection between subsidence and its consequences as several pictures and documented damages depict. The authors do not fail to emphasize that despite the existence of possible and available methods, hotel

construction and development plans for the very same area are pursued as originally intended, ignoring the fact of the natural hazard or being unable to cope with it. Especially these sections, which deal with the consequences and the neglect of decision makers, are well written and underline the authors' passion concerning the subject, which they investigate since decades always offering help and seeking for solutions.

Specific comments:

However, the study lacks one important facet: it is completely non-transparent concerning the derivation of the vulnerability, which is the central core of the manuscript. The authors state to derive the vulnerability map and to understand the dynamics of the geological hazards in the Dead Sea using a "combination of inputs coming from three independent data collection approaches". These approaches comprise i) InSAR, ii) optical data, and iii) field surveys and ancillary data. While for InSAR the authors refer to an earlier publication, optical data is totally confusing. Several data sources are mentioned (Landsat, Sentinel, WorldView2, Corona) but seemingly, only WorldView and Corona data have been used for the study. The same is true for the derived products. NDVI and NDWI are mentioned to detect vegetation cover and soil moisture, but only NDVI seems to be included, at least this is what can be assumed from included figures. Moreover, if both indices are used it is important to state the procedure how it was calculated (e.g. NDWI can be calculated using the approach by McFeeters or Gao that may lead to different results concerning soil moisture) and further processed (threshold procedure), but the reader is left in the dark throughout the entire method section and beyond.

The various times I wrote "assumed"/"seemed" within the last eight lines indicate what I meant with non-transparent. Neither is clear which of the data or the derived products was really used for the vulnerability map, nor do the authors fully present the data preprocessing, nor do the authors describe the way how they calculate the vulnerability map, which, by the way, is never shown.

Given the fact that vulnerability is indeed the core aspect of the authors' manuscript as the title suggests, I would expect a clear definition of how they define vulnerability, to which concept their definition belongs, what method they used to infer vulnerable areas, to present and apply unequivocal calculations/derivations and possibly the derivation of a vulnerability curve to be used for further analysis or the early warning system, which is mentioned several times but not part of the manuscript. To conclude, at this stage the method is only descriptive, not reproducible and thus not assessable.

But even beyond the non-transparency of the approach itself, the manuscript in large parts of the discussion and conclusion sections does not discuss the results aside from the hypothesis that landslides appear to occur during the summer and the already observed and published fact that sinkholes are formed along faults. Instead it is a plea for integrating any sort of sophisticated geomorphological in-depth analysis already in the planning phases of touristic structures. I totally agree with the statements given by the authors and I cannot understand the short-sighted planning and construction activities that seem to neglect obvious natural dynamics and will deliberately accept any possible loss of lives that may occur in the near future. Yet, here we deal with a scientific publication that, objectively, ought to present/discuss results and address the bigger picture in which the results fit. In the present case, this would be the vulnerability map as the title of the manuscript most prominently suggests. However, neither the discussion nor the conclusion contains any word on vulnerability (except for one instance on P12L32) raising either the question of the suitable manuscript title or the proper content.

Apart from the vulnerability, the study is a bit vague in its terms. When referring to landslides/sinkholes/subsidence, the authors throughout the entire manuscript mention various terms: salt karst hazard, hydrogeological hazard, human-induced geological hazards, geo-hazards, geological hazard, karst geo-hazards. Although in its core all terms resemble each other it remains vague. Is it a human induced hazard? Is it only a geological hazard or does water has serious role in this play to justify the hydrogeological hazard vs the geological hazard? Is geo-hazard something like an umbrella

term? Those are the questions that may arise for the reader unfamiliar with the subject or the Dead Sea. Of course, the authors, whose work I value tremendously, know the answer to all of the partially provocative questions since they have a profound knowledge of the system, the mechanics behind, and of course, the causes. All I wanted to point is that it is of utter importance to be concise and consistent to transport the knowledge to the reader. It may be worthwhile to define the hazard once with a single term and provide sufficient facts supporting the definition and keep the hazard term throughout the entire manuscript.

Speaking of valuing the work of the authors who investigate the subject since the years, the authors have shown their profound knowledge in numerous publications during the last years. However, from my point of view it is imperative to reduce the number of self-citations. From 57 references 27 (47%) are first-author publications of one of the present authors. I do not arrogate a right to myself to judge which of the references could be excluded but the number should significantly be reduced.

In conclusion, considering the number and the weight of the abovementioned aspects, I have to reject the manuscript.

I provided numerous comments in the pdf itself regarding further specific but also technical comments that may help to improve the manuscript. I also added the questions from HESS that reviewers are asked to answer.

Does the paper address relevant scientific questions within the scope of HESS? Yes

Does the paper present novel concepts, ideas, tools, or data? Yes for data if I think of the public science data seemingly included in the approach which comprises 25.000 photographs from social science platforms.

Are substantial conclusions reached? Yes and No. Yes for the region itself and it should be directed to decision makers to include the knowledge and tools the authors seemingly have to prevent any more loss of lives. No for the scientific publication as from a scientific point of view the conclusion do not reflect the approach but are a plea to include the any sort of geomorphological analysis in the planning process.

Are the scientific methods and assumptions valid and clearly outlined? Absolutely not, and I refer to the lines of the previous pages and the comments in the pdf to underline the rigorous statement of mine.

Are the results sufficient to support the interpretations and conclusions? Does not apply, as the results are not properly interpreted in the discussion or in the conclusion sections.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Absolutely not, and I refer to the lines of the previous pages and the comments in the pdf to underline the rigorous statement of mine.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Mostly yes, in certain parts of the manuscripts I indicated where further credit could be included.

Does the title clearly reflect the contents of the paper? No, and I broached this issue several time in the lines above.

Does the abstract provide a concise and complete summary? Well, as the entire manuscript, the abstract is very descriptive, leaving out factual aspects of e.g. applied methods, final results etc.

Is the overall presentation well structured and clear? Yes.

Is the language fluent and precise? Yes.

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Does not apply.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced,

combined, or eliminated? Yes, some figures can be combined to reduce the total number. Clarification is need for some maps. All have been commented in the pdf.

Are the number and quality of references appropriate? No, the self-citation number is quite high and should be decreased.

Is the amount and quality of supplementary material appropriate? Does not apply.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-479/hess-2018-479-RC2-supplement.pdf

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