

Interactive comment on “Probabilistic characterisation of coastal storm-induced risks using Bayesian Networks” by Marc Sanuy and Jose A. Jiménez

Anonymous Referee #1

Received and published: 28 August 2020

I have reviewed the manuscript entitled ‘Probabilistic characterisation of coastal storm-induced risks using Bayesian Networks’ by Sunay and Jimenez.

Overall the article is of high quality and provides an alternative method for using BN in risk assessment that although it is based on the source-pathway-receptor-consequences concept it has some novel methods related with the storm selection. I believe that the article is of high interest for the journal and well within the journals scope. However, I believe that in order for the manuscript to be accepted some changes need to be addressed more for clarifying some aspects of the work and for providing further information and limitations of the method.

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General comments:

The Abstract of the article although correct is rather general and it is not highlighting the results and the novelty of the work. I believe some addition of more specific results that are present in the discussion will benefit the current version of the abstract. Most of my comments are concentrated in the methodology sections this is partly because the method is rather complex and the proposed novelty although important it is not obvious from the beginning. The results and discussion sections are very well written and explained with high quality figures that although sometime complex they concentrate a large amount of information.

I have made specific comments in the text where I have questions or doubts my main concerns at the moment is that novelty of the method is not properly described in risk terms. I believe that the BN approach proposed is valid for characterizing the risk for the entire storm climate and not for specific storms as proposed by previous works. However if this is true it needs to be highlighted by the authors in the abstract and in the title is necessary. My secondly concern is related with the scenarios proposed. Some more explanation is needed on why the shoreline retreat is extended to the entire shoreface.

Specific comments:

LINE 33: source terms are both the storms and the storm induced hazards.

LINES 53-58: Plomaritis et al 2018 select the events using the same methods as Poehekke et al., 2016. The method is based on a series of colula applications using Hs as a main parameters. I don't think that this method can be consider non-probabilistic but indeed the method can differ. Please explain with more detail the differences in the storm selection. Poehekke et al., 2016 also follows the ideas of response approach with the use of copulas but with triangular storms. I believe that the discussion over the different approaches that the authors provide is very interesting and I would suggest extending it or order for the reader to be better informed on the sometime small but

important details. The reference Duo et al., needs updating.

Study area: Provide the names of the areas in Figure 1 not only the code. Now they is given in Discussion but the codes are used before. I think some information of the areas and the logic behind the separation could be interesting.

LINE 95: I think the paper Sanuy et al. (2018) is not in the reference list.

LINE 143: Provide the number or persentnage of empty groups
LINES 174-175: How many storms per bin you have in the subset group and which are the output paramters you test? My understanding so far is that you have one storm per group in the subset so, I am not sure how you calculate the variance per bin. Are you evaluate the BN output or input with the equations 1 and 2 or the entire BN?

Hazard Assessment: Which are the indicator (model output parameter) you use for each hazard.

LINES 194-198: The area characteristics can be put in the study area. See my previous comment.

LINES 246-248: Given the steep slopes of the study area I understand the extrapolation of the shoreline retreat values to the upper beach (-2 to -4 m) but continues retreat up to -8 suggest a huge amount of sediment loss and that all sediment from the upper beach is removed by longshore drift. I am not an expert on Catalan coast but some additional justification for the selected scenarios must be provided.

LINE 272: Why the storm parameters are linked in Figure 6? How is te term of previous energy is incorporated in the BN?

LINES 274-277: The central variables i and ii are not shown in Figure 6. Please provide more details. Explane where the estimation of the total number of receptors is done, in the BN or before?

LINES 420-421: What are the advantages of this fully probabilistic BN? I suppose that

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the previous papers were focused on the individual storm assessment while here is attempted an integrated assessment of the storm conditions. If this is correct it has to be stated and event introduced in the abstract.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-99>, 2020.

