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

Anonymous Referee #1
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I have reviewed the manuscript entitles 'Probabilistic characterisation of coastal storm-induced risks using Bayesian Networks' by Sanuy 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.

We thank the reviewer for constructive comments. We have performed a thorough revision to address all the comments and incorporated all the suggestions in the manuscript, as detailed below.

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.

[R1] The Abstract will be modified to incorporate reviewer's suggestions.

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 begining. 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.

[R2] We agree with the reviewer and, in fact, this is one of the main novelties of the work. Following reviewer's suggestion, this will be highlighted in different parts of the text (abstract, introduction, and discussion sections).
We will also propose a modified title: *Characterizing coastal erosion and inundation risks for the entire storm climate using a Bayesian Network*

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.

[R3] Future (morphological) scenarios have been defined to consider the background evolution of the area. This is important when assessing risks in dynamic areas because if not, the assessment will strictly be valid just for current conditions (small time scale, few years) and, in consequence, of limited validity for coastal (risk) management. This is the key message, the need of updating beach coastal morphology for an effective risk assessment. <u>We will reinforce this message in the text</u>. With respect to how to do it, it will depend on the specific conditions of the area and on the used tool to mimic/simulate such evolution. Whereas there are many different options, we have chosen a simple one by extending shoreline rates of change to reproduce nearshore bathymetric changes, although as mentioned in the work, it can be substituted by a different choice (e.g. by using a morphodynamic model valid at the appropriate time-scale, e.g. Hanson et al. 2003).

In the study area, observed shoreline retreat is the result of the deltaic front reshaping due to a decrease in river sediment supply whereas the wave-induced littoral dynamics maintained its intensity. Transferring this shoreline retreat to the entire active shoreface implies to apply a hypothesis about the shape of long-term (decadal) profile changes. Thus, the most widely used hypothesis used to convert longshore transport – induced shoreline changes to sediment volume is the one applied in one-line models, where a horizontal displacement of the profile from the emerged beach to the closure depth is assumed (e.g. Hanson, 1989). On the contrary, other works on deltaic reduction processes assume that whereas the shoreline is rapidly eroded, the submerged front retreats at a slower rate (e.g. Refaat and Tsuchiya, 1991). This pattern would be consistent with a wedged-shaped change over the closure depth (instead of a parallel one as before). Other type of approach is the one adopted by Stive and de Vriend (1995) when modelling the long-term shoreface evolution. They proposed a varying type of change through the shoreface, from an upper part experiencing a parallel displacement, to a declining/inclining lower shoreface down to the inner shelf limit. As it can be seen, there are different options to reconstruct beach profiles from a modelled/forecasted shoreline, from which we selected one of the most used (albeit not necessarily the best one).

Regardless of the method used, the most important message is that it is necessary to anticipate future coastal morphology in order to make a reliable risk assessment valid not only for current but also for future conditions. <u>We will highlight this in the discussion section and will also introduce a text discussing how the scenarios were constructed</u> (similar to the previous one, but shorter).

References:
Hanson, H.: GENESIS: a generalized shoreline change numerical model, J. Coast. Res., 1-27, 1989.
Hanson, H., Aarninkhof, S., Capobianco, M., Jiménez, J.A., Larson, M., Nicholls, R.J., Plant, N.G., Southgate, H.N., Steetzel, H.J., Stive, M.J.F, and de Vriend, H.J.: Modelling of coastal evolution on yearly to decadal time scales, J. Coast. Res., 19, 4, 790-811, 2003.
Refaat, H., and Tsuchiya, Y.: Formation and reduction processes of river deltas; theory and experiments, Bull. Disaster Prevention Res. Inst. Kyoto Univ., 41, 177-224, 1991.
Stive, M.J.F., and De Vriend, H. J.: Modelling shoreface profile evolution, Mar. Geol., 126(1-4), 235-248, 1995.

Specific comments:

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

[R4] Adopting the S-P-R-C framework to analyse the risk induced by erosion/inundation (storm-induced hazards), the source (S) term is just defined by the storms. The pathways (P) of flooding/erosion are composed by the beach, defences and even, in some cases, the coastal floodplain. In fact, pathway and receptor (R) can be considered as relative definitions since they may simultaneously function as pathways to "landward" receptors and as receptors in their own right (e.g. Narayan et al. 2012). We shall slightly rephrase this paragraph in the text for clarification.

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 copula applications using Hs as a main parameter. I donot 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.

[R5] Following the reviewer's suggestions, we describe/analyse further the differences between approaches. The reviewer is right in stating that the use of the term "non-probabilistic" to classify the method followed by Poehekke et al'16 and Plomaritis et al'18 is not entirely correct and confusing. We have modified the text to avoid such confusion.

The above methods use copulas to statistically represent storms, which are the events (drivers) that induce the analysed hazards. Adopting a strict response-approach involves calculating the induced hazards for the entire storm climate and performing the statistical analysis on the results obtained in terms of hazards/impacts. This difference is especially relevant when analysed hazards depend on multiple storm variables which are not necessarily correlated and not included in their definition through copulas. Moreover, the mentioned works use a selected group of events, instead of a set representing the storm climate.

The reference Duo et al., needs updating.

[R6] Duo, E., Sanuy, M., Jiménez, JA, Ciavola, P. 2020. How Good Are Symmetric Triangular Synthetic Storms to Represent Real Events for Coastal Hazard Modelling. *Coastal Engineering*, 159, 103728.

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.

[R7] We prefer to do not include names in Figure 1 so as not to "overload" it. However, we have included a text in the study area section in which we give the full name of each sector and give the reasons for their selection (this text was included in section 3.4 in the original version of the manuscript).

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

[R8] Sanuy, M., Duo, E., Wiebke, Jäger, W, Ciavola, P., Jiménez, JA. (2018) Linking source with consequences of coastal storm impacts for climate change and risk reduction scenarios for Mediterranean sandy beaches. *NHESS*, 18, 1825-1847.

LINE 143: Provide the number or persetnage of empty groups

[R9] This will be provided, see also **[R10]**

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?

[R10] This question is related with the previous comment. The subset method fills with one storm all combinations showed in Table 1 that have at least one historical event. Some combinations remain empty and this will now be introduced following [R9]. Then, the subset is used to fill the BN, which, as shown in Figure 6, has a different number of bins per variable than classes depicted in Table 1, leading to more than one event in many variable combinations.

The variance per bin is calculated following Bityukov et al., 2013, where the observed standard deviation per bin is estimated with the observed value per bin (i.e., $n_{ik} = \sigma_{ik}$ in eq. 1).

We evaluate both BN input and output variables with equations 1 and 2 (now they can be interpreted from Table 5 and Results Figures). We perform the evaluation on (i) unconstrained output, (ii) output constrained to given input combinations and (iii) input constrained to a given output. In the modified version of the manuscript, the evaluated variables will be detailed, and Table 5 will be adapted to help the correct interpretation of the method.

Hazard Assessment: Which are the indicator (model output parameter) you use for each hazard
[R11] The XBeach model outputs used are *maxzs* for water depth (inundation hazard) and *sedero* for erosion. They will be mentioned in the revised version of the manuscript.

LINES 194-198: The area characteristics can be put in the study area. See my previous comment.
[R12] Done. See also [R7].

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.

[R13] When building the morphological scenarios, we are using recorded decadal-scale shoreline rates of displacement, that for the study area are mostly controlled by longshore sediment transport (e.g. Jiménez et al. 2018). The objective of the extrapolation was to build "possible coastal morphologies" to illustrate future changes in coastal risk associated with morphodynamic changes. We adopted this simple approach in absence of a robust criteria to select a different one. This point has been extensively covered above in [R3] and, as mentioned there, we will include this point in the discussion section to let readers to make their own choice when applying the method to a given case.

LINE 272: Why the storm parameters are linked in Figure 6? How is te term of previous energy is incorporated in the BN?
[R14] The storm parameters are linked so that empty combinations of source characteristics do not propagate noise into the outputs. The term "previous energy" will be removed from the BN (figure and description) as it is not used in the present study.

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?

[R15] In the revised version, Figure 6 will be adapted to show the two variables (and also to remove "previous energy" as mentioned above). The estimation is done before, crossing XBeach output with receptor polygon data, and introduced as an additional variable, at each receptor, that captures the overall number of affected receptors per storm peak. It allows for the assessment, in the same network, of the relation between source characteristics and extension of the impacts, although the presented results put the focus on other variable dependencies found more relevant. A phrase will be introduced in this part to clarify.

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

[R16] This has been raised by the reviewer in previous comments. We have introduced some changes in the text (abstract, introduction, discussion) to explicitly mention that the representation of the entire wave climate, to obtain integrated or conditioned risk-oriented results, is the advantage of the presented BN.