Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-345-RC1, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Linking source with consequences of coastal storm impacts for climate change and risk reduction scenarios for Mediterranean sandy beaches" *by* Marc Sanuy et al.

Anonymous Referee #1

Received and published: 7 November 2017

general comments

The authors present a risk assessment for coastal storm impacts to support decisions on disaster risk reductions. For that purpose a Bayesian network approach is used to link process-oriented models, that predict the hazards at the receptors, with vulnerability relations to obtain the final expected impact. In a case study two Mediterranean sandy coasts are considered.

The paper is well structured and provides a well-argued motivation and problem defini-

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tion. Further the study areas are described in detail and underline the relevance of the presented study. Despite a good structure in the Methodology section, some aspects of the method remain unclear to me, which might be due to the complexity of the model chain. This affects especially the Bayesian network (BNs) application. Eventhough I am familar with the constuction and application of BNs, I have problems to follow the construction (i.e. parameter setting) of the BNs and to understand the motivation for and advantages of using BNs in the presented context. The results section provides an extensive analysis of different climate change and adaptation scenarios for the considered Mediterranean coasts. Yet, I did not understand which storm intensities are considered here (this mights be due to a missing understanding of the methodology). The discussion names several aspects that pose challenges or are negleted in the presented model approach and might consequently be taggled in follow up studies. Yet, to my impression important critical points of the presented approach are missing, as will be specified in the specific comments.

specific comments

I found it quite difficult to keep track of all abbreviations used in the paper.

abstract:

line 15: "a large number of storm characteristics" What is a large number? To my understanding 3-4 storm characteristics were considered.

line 17: "The tool has been proven successful in reproducing current coastal responses at both sites". I could not find any model verification in the paper. Only a reference to a paper that verifies a part of the model.

Section 3.2:

page 7, line 7: The discretization of the variables is hardly motivated. What is the motivation for choosing 2 or 3 or n intervals for a certain variable? How are the interval boundaries selected (equidistant, equifrequent, entropy-based, ...)? How is the

probable range determined (only so far observed values)? Some information about the distribution of these variables might help to motivate the discretization.

What are the effects of discretizing?

In the discussion it is mentioned that accuracy comes at computational costs, but this information is quite sparse (no information about number of intervals scales with computational costs or what are the computational costs of the current network for parameter determination and for inference).

page 7, line 12: "time series" of what?

page 7, line 19-20: the "(24 simulated storms)" confused me? Why do you consider 24 simulated storms for 12 state combinations?

page 7, line 24: What are synthetic triangular events?

page 7, line 27-28: "water level and Hs are uncorrelated" <- a reference is needed?

page 8, line 1: How are the driving variables identified? Why are the remaining variables considered to have no effects?

How is the distribution of the storm defininig variables defined? To my current understanding an equal amount of storms for each state combination is considered, which infers a uniform distribution of the variables. Yet, I would expect that small Hs values or smaller durations are more likely than higher values? Is this accounted for?

Section 3.3:

Only one event (storm) is considered for each combination of states. Yet, similar events might result in different outcomes. Further, the applied model chain seems to provide deterministic results. Consequently no uncertainties are considered/captured in the model construction. Since BNs are explicitely designed to capture uncertainties, I wonder why this approach was chosen here.

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The distribution of hazard at the receptors results from the different location of the receptors, but does not reflect the uncertainty related to the inundation or erosion at a specific object. In a strict sence, I would not judge the resulting distribution to represent probabilities.

Section 3.4:

To model the consequences flood damage curves are applied. Those are generally related with huge uncertainties (a wide range of relative damage can be observed for equal water levels), which are again neglegted and not included in the BN. On top, a damage curve that was derived for river floodind is applied. Since the process of storm surges is very different from river flooding the applicability should be discussed.

In terms of risk levels the values selected for both study sites differ significantly. E.g. medium impact building damage ranges from 0.26 to 0.45 compared to 0.1 - 0.2. Why are these intervals chosen?

Section 3.6:

To my understanding the probability tables of the BN are constructed by simulating a strom scenario for each combination of states and running the deterministic model chain to receive a predicted hazard value for each receptor in the study area. Due to the deterministiv character of the model chain, the resulting distribution for the hazard variables does not represent probabilities, but the expected fraction of receptors with the single hazard levels or impact levels respectively.

Since no uncertainties are considered, I see no need to apply BNs in this context. The same calculations can be done by applying the model chain directly. A direct application of the model chain would also avoid the discretization of the variables and consequently achieve a higher accuracy.

In my point of view the revised paper should either do without the BN approach or account for the uncertainties related to the single model components.

Section 4:

I do not understand which storm intensity is considered here? Are the presented results the joint distribution for all possible combinations of storm characteristics? If so, what is the meaning? Is this a kind of average storm? <- I don't think so.

I would rather prefer to consider specific storm scenarios in combination with their return period. E.g. what are the effects of DRR measures for a once-in-a-year or once-in-10-years event or for an extreme event.

To judge the efficiency of DRR measures, it would also be interesting to get some information about the costs of their implementation and their probability of failure.

Section 5:

page 21, line 11: "a first test to check the method was presented" <- Where?

I could not find any validation of the presented model. There is only a reference to a (not published) paper to validate the hazard component of the model chain.

page 21, line 11-15: A more detailed justification for the choosen amount of intervals and the interval boundaries, would be nice. Additionally, some information about how the computational costs scale with the number of intervals could be provided.

Several uncertainties related to the study are not disscussed (see comments about section 3).

technical corrections

page 4, line 24: 2-3m?

page 5, line 16-17: Armaroli et al (2012) is cited double

page 11, line 19: check >0.05m or >0.5m

page 14, line 31: "it also provided ..." What is "it"?

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