Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-392-AC2, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Uncertainties in Coastal Flood Risk Assessments in Small Island Developing States" *by* Matteo U. Parodi et al.

Matteo U. Parodi et al.

parodi.mat@gmail.com

Received and published: 1 April 2020

Dear Editor and Dear Reviewers,

We appreciate the reviewers' constructive comments. We addressed the comments and suggestions in this reply and have accordingly implemented changes in the manuscript, which have surely improved its quality. Please do not hesitate to contact me if you have further questions.

Kind Regards,

Matteo Parodi

Reviewer #2

Overall I am positive but I think some work is needed to make it publication-ready. The main issue is that the manuscript gives the impression to have been written in a hurry, as several aspects of the methodology and the ïňĄndings are not being properly explained. More speciïňĄcally: The methodology to estimate the uncertainty is not clearly described. Do the authors simulate all possible combinations?

Authors: We appreciate the reviewers' comment and apologize that the methodology and findings do not seem properly explained. To address this comment, we have tried to add more detailed explanation on the methodology and data used.

With regard to the specific comment we added on page 8, line 14 that: "We conducted a sensitivity analysis on the full parameter space of model inputs. This led to combinations of: (a) 3 Hs scenarios (b) 3 storm surge scenarios (c) 2 bathymetry scenarios (d) 6 DEM scenarios (e) 7 DDF scenarios (f) 3 SLR scenarios (g) 3 SSP scenarios over (h) 4 different time horizons (current , 2050, 2070 and 2100) ultimately leading to a total of 21,168 simulations for each community." The number of variations (scenarios) for each different variable has also been added to Table 1.

How do they discretize their parameter space? I assume that they keep all factors but the tested one to 'baseline' values and allow only the tested parameter to ïňĆuctuate. However, this is not clearly explained in the manuscript.

Authors: The parameter space has been discretized differently for each input. The different values used for each input are described in Table 1. Some inputs were represented by a probability distribution (e.g. storm surge and significant wave height). However, some other inputs (e.g. DEM and Bathymetry) were not represented by a probability distribution but we used the different datasets that were publicly available. In this case, for each input, the parameter space is composed by the selected publicly available datasets. The model simulations were run testing all parameter's combinations, as explained in the above comment, and not with a one at a time (OAT) method where all parameters but the tested one are kept at the baseline value.

Also in what way does the bathymetry and Hs result in damages?

Authors: Varying the bathymetry and Hs input values affects the computed flooding damages. Increasing Hs leads to an increase in wave-induced flooding and thus increased damages. Higher bathymetry values will reduce flooding and hence computed damages. The changes in the computed damages described in Section 4 and depicted in Figure 7 illustrate this situation.

Section 3.2.2: Considering only SLR is an acceptable assumption but the authors should at least mention other studies which discuss the other uncertainty sources (astronomical and meteorological tides). Note also that the text is almost the same as in the discussion (page 12, line 7).

Authors: We agree with the comment and included in the discussion studies that have considered the other uncertainty sources mentioned (Chowdhury et al., 2006; Karim and Nobuo, 2008), see page 7, line 15. The text in Section 5 expresses the same concept as in Section 3.2.2. In Section 3.2.2, the assumption is first introduced, while in Section 5 is expressed again in the discussion in order to further underline the importance of its implications of this assumption.

SSPs: there is literature on the compatibility of certain SSPs and RCPs and the authors should justify why they combine SSP2 with RCP8.5.

Authors: We acknowledge that different SSPs are compatible with different RCPs, according to literature. However, RCP are global scenarios and individual countries, especially smaller ones, could have a combination of SSP and RCP which would not be logical or possible at a local scale. We have clarified this in the manuscript: “Although some SSP scenarios are only compatible with certain RCP scenarios at the global or regional scale (van Vuuren and Carter, 2014), at the local scale of individual and small countries RCP and SSP may not be necessarily correlated, since RCP represent a global process while SSP reflect the socioeconomic development of the single country. ”, see page 8, line 10.

The use of 5th and 95th percentile values is an assumption which may be inevitable but has limitations and should be discussed. It is not said that the 95th Hs will result in the 95th damage and to assess this properly a Monte Carlo framework is needed considering the whole PDF and more cases. I expect that the computational effort is prohibitive but this should be at least discussed.

Authors: The issues have been mentioned and added “To further improve the presented methodology, a Monte Carlo analysis that considers a pdf for each uncertain input to estimate the pdf of the expected damages could be performed, although the computational effort is prohibitive”, in Section 5, page 12, line 2.

Also as the methodology is not sufïňĄciently explained it is not easy to follow what simulations have been really done.

Authors: This comment is similar to Reviewer 2’s first one, and we have addressed it there and on page 8, line 24.

The Hs uncertainty sources are not fully covered. Given that values come from a reanalysis one should also include model error and EVA uncertainties beyond the one related to ïňĄtting (e.g. other pdfs). These aspects should be discussed.

Authors: We have included EVA uncertainties, related to estimating values for a return period longer than the length of the available data. “Commonly, extreme hydrodynamic boundary conditions are represented with probability distributions. However, these distributions are fit to measured data and attempt to estimate values for return periods longer than the length of the available data, thus already introducing uncertainty in the model.”, see page 5, line 13.

Our analysis focused on uncertainty related to input values and model uncertainty and error were not taken into account. However, we have mentioned the Hs uncertainty brought by model error in the analysis. “Furthermore, the nearshore wave conditions were estimated from transformation matrices in the DELFT3D-WAVE (SWAN) model,

which increases the uncertainty of Hs by introducing model errors." See page 5 line 15.

All bathymetry related uncertainty is also not addressed: e.g. effect on wave/storm surge simulations. Again discuss

Authors: The discussion on bathymetry related uncertainties has been improved. "Therefore, uncertainty and errors in bathymetric datasets could lead to an increased uncertainty in wave and storm surge simulations, increasing the potential for modeling error or biases.", see page 5, line 28.

Other minor comments:

I would recommend expressing EAD in USD since EUR is not relevant to people living outside of the EU and in this case so the study areas are also out of EU territory.

Authors: The objective of the manuscript is to show the relative importance of the considered input uncertainties on the estimated flood damages and the relative changes in damages, so we have decided not to change the currency used.

Page 1, Line 24: Rephrase 'which challenges the safety and sustainability of their society and the growth of their economies'

Authors: This has been rephrased on page 1 line 27.

page 2, Line 18: 2017) and CoastalDEM

Authors: This has been corrected on page 2, line 25.

page 4, Line 20: I am a bit skeptical about whether direct damages are dominant. I would suggest removing this statement unless the authors can support it with references/data

Authors: The statement has been removed.

Page 6, Line 1: The work of Bove et al (https://doi.org/10.1016/j.scitotenv.2019.136162)

is also relevant to the present study and should be discussed.

Authors: Thank you, we now cite this paper on page 6, line 13.

Page 6, Line 26: correct 'being most representatives'

Authors: The sentence has been corrected on page 7, line 7.

Table 1: SLR projections columns 3 and 4, is this correct? Also it is not clear where exactly SLR comes from. Vousdoukas et al. 2016 is cited but not with sufiňĄcient details. Maybe the data come from the 2018 Nature Communications paper?

Authors: The Table columns have been corrected. Also, the citation of the data has been updated to provide enough details to access the sea level rise predictions data on page 7, line 21.

Figure 7 would be easier to follow with x labels explaining each bar

Authors: The Figure has been updated accordingly, including x labels.

Explain the vertical datum used for the UAV DEM

Authors: The UAV DEM was referenced to the WGS84 datum, and we have updated the paper including this information on page 6, line 18.

Figure 10 compares the damages driven by socioeconomic vs climate change so I would suggest expressing that way

Authors: The Figure caption has been updated accordingly.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-392/nhess-2019-392-AC2-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-392, 2019.