

## ***Interactive comment on “An examination of land use impacts of sea level rise induced flooding” by Jie Song et al.***

**Jie Song et al.**

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Dear Reviewer,

We are very grateful for your constructive comments that greatly benefit the improvements of our paper. We have paid full attention to all comments and meticulously addressed them. Our responses can be found in the following sections. Please be advised that all line and page pointers (e.g., line 1, page 2, etc.) refer to the revised manuscript that was attached at the end of this document. We have also proofread the manuscript carefully before this submission, which can be shown by another authors' comment posted in the section of interactive discussions. Again, we highly appreciate your time and efforts on reviewing our paper.

1. General comments

1) Despite the article focuses on a specific geographic region that means with specific associated risks, lacking in generality, it is overall well-written, well-structured and findings are generally supported by the analysis carried-out.

Response: we would like to thank the referee for his/her compliments of our paper as well as the concern of the generality of this study. This research selected Bay County in the state of Florida as the study area; however, it does not necessarily indicate that this county of research interests is specifically chosen because of its uniqueness in hazard risks. In other words, it does not imply a lack of generality. We chose Bay County over other coastal areas majorly because it is highly susceptible to coastal flooding and storm surges, and it will be particularly true given future sea level rise. In fact, flooding and storm surges are widespread coastal hazards around the world, and sea level rise has also been observed globally by tide gauges. Many coastal communities in the US and around the world share similar or even higher exposure to such risks (i.e., coastal megacities such as New York and Miami). Thus, we would state that Bay County is somehow representable because it is facing increasing challenges by combined impacts of coastal hydrological hazards and sea level rise that are experienced in many other coastal communities globally. Another principal reason for choosing Bay County is the availability of data needed for modelling.

However, we definitely agree with the referee that we could explain more about the reason why we chose this specific region and how this study could be generalised to other coastal areas. As a result, we have now expanded the discussion of our study area to include why this area was chosen (line 16, page 3) and also provide information on why other coastal communities should be concerned (lines 2 to 5, page 4).

2) Overall, the article is scientifically sound although I have a number of comments and requests of clarifications as outlined below that in my view need to be addressed by authors to improve the clarity and presentation of some specific aspects.

Response: thank you very much for the referee's positive comments about our work.

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We have addressed each comment meticulously and illuminated the requests in the following responses and the text as much as possible.

3) Overall, the article would benefit if a more profound/critical description of choices made for the several steps leading to model outputs were made.

Response: thank you for the advice. We made the following overall modifications to justify our choices regarding model inputs and outputs. Specifically, we discussed the rationale of why we chose Bay County as a study area (the reply to specific comment #2). We demonstrated the selection of data inputs, as shown in the response to specific comment #3. We added references and rigorous explanations to support the technical details of calibration, such as the definitions of urban-growth parameters and annual growth rates. These are stated in details in the responses to specific comments # 4, 5, 6, and 7, respectively.

## 2. Specific comments

1) The Abstract is somewhat too qualitative. I suggest to strengthen it to give more emphasis on the methodology used. The SLEUTH model is mentioned without a reference (how this has to be done I guess depends on the specific Editorial formatting procedure).

Response: we appreciate the referee's suggestions regarding how to ameliorate the Abstract and a comment about the lack of citations. We have substantially improved the Abstract to focus more on the methodological parts: model calibration, prediction, and significant results. We added a reference when the SLEUTH was first introduced (line 10, page 1), and we will work with the Editor to deal with this issue if different procedures should be followed. Specifically, following is a new Abstract (lines 8 to 22, page 1).

“Coastal regions become unprecedentedly vulnerable to coastal hazards that are associated with sea level rise. The purpose of this paper is therefore to simulate prospective

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urban exposure to changing sea levels. This article first applied the cellular automaton-based SLEUTH model (Project Gigalopolis, 2016) to calibrate historical urban dynamics in Bay County, Florida (US)—a region that is greatly threatened by rising sea levels. This paper estimated five urban-growth parameters by multiple-calibration procedures that used different Monte Carlo iterations to account for modelling uncertainties. It then employed the calibrated model to predict three scenarios of urban growth up to 2080—historical trend, urban sprawl, and compact development. We also assessed land-use impacts of four policies: no regulations; flood mitigation plans based on the whole study region and on those areas that are prone to experience growth; and the protection of conservational lands. This study lastly overlaid projected urban areas in 2030 and 2080 with 500-year flooding maps that were developed under zero, 0.2-m, and 0.9-m sea level rise. The calibration results that a substantial amount of built-up regions extend from established coastal settlements. The predictions suggest that total flooded area of new urbanised regions in 2080 would be more than 25 times that under the flood mitigation policy, if the urbanisation progresses with few policy interventions. The joint model generates new knowledge in the domain between land use modelling and sea level rise. It contributes to coastal spatial planning by helping develop hazard mitigation schemes and can be employed in other international communities that face combined pressure of urban growth and climate change.”

2) The rationale for the choice of the Bay County has not been addressed. In connection to it the article should give evidence of a larger breath that is how the analysis carried out here could be done in other areas in the world? Despite the research questions clearly states “How would different urban growth patterns increase regional vulnerability to sea level rise induced flooding?” , not enough attention has been paid to why the specific area chosen should be of general interest. The limitations of this study should be clearly stated.

Response: we are grateful for these comments. We made substantial revisions to the section of the study area and clearly explained why Bay Country was chosen, as

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summarised below.

a) Page 3, Lines 16 through Page 4, Line 5: we clarified why Bay County as a particular area can be of general interests and why the results can be generalised to many other coastal regions around the world.

b) Page 4, Lines 6 through 14: we clearly pointed out the region's exposure to sea level rise – one basic rationale for case study selection.

c) Page 4, Lines 15 through 19: we illustrated the data availability issue as another reason for selecting Bay County.

d) Page 23, Lines 8 through 10: we specifically discussed the limitation of this study by adding to this section the following statements. “First, Bay County is a typical land-sea interface confronted with heightened pressure from SLR, and the results are analogous to those in other similar coastal zones. However, we inadequately evaluate the effect of elevation on urban exposure to flooding. Thus, our findings may have limited comparability with hilly areas.”

3) The description of the data set (section 2) is rather uncritical. Why these data have been chosen? Are all available data? Would this analysis possible without all these data?

Response: thank you for the comments. We have moved the descriptions of data sets to the sections 3.3 and 3.5, respectively. In the new manuscript, section 3.2 particularly discusses the rationale of data selection for the SLEUTH Urban Growth Model (lines 8 through 11, page 7). Following this is the new section 3.3 that introduce the sources and availability of necessary model inputs. The improved section 3.5 will include the mechanism of the flooding model as well as data requirements and sources (lines 16 through 26, page 15). In this way, the new paper will a better logic flow by integrating the model configurations and rationale and availability of data sets. In response to the last question, the study is fundamentally based on all these data. Specifically, we

addressed the comments in the following aspects.

a) Page 5, Lines 11 through 12: urban, transportation, slope, hillshade, and exclusion are five necessary inputs for the SLEUTH Urban Growth Model.

b) Page 7, Line 9 through 11: we explained why a certain number of maps from different dates are needed for the SLEUTH applications.

c) Page 15, Lines 16 through 26: we illustrated how flooding was influenced by sea level rise in a hurricane model developed in a similar study, what necessary data for modelling are, and where to collect these data.

4) Section 3 – I would consider to entitle this section “methodological approach” rather than “method”. Please note a typo. An Introduction to the SLEUTH model not “An Induction”. The section requires some adjustment. First: please add some references for “dispersion, breed, spread, road gravity, and slope” given that specific definitions of those variables/parameters are application.

Response: thank you for pointing out the typo and offering suggestions. We have changed the title of section 3 to “methodological approach” (line 2, page 5) and corrected the typo (line 2, page 6). We have also enhanced the logic flow of section 3 by first introducing the overall research framework (line 3, page 5), instead of the background of SLEUTH. We added several references for these five parameters and ensured that we had justifications when using these terms in section 3. Specifically, a reference was added when the parameters were first introduced (line 12, page 6). We also added couples of references in the section 3.2.2 “SLEUTH workflow” to make sure each definition is supported by a reliable source (lines 1, 2, 4, and 4, page 7). Table 1 (line 6, page 7) further gives the relationships between these parameters and four steps for a growth cycle. We made these interpretations based on Clark et al. (1997) who developed the SLEUTH model.

5) Overall section 3 is uncritical. The authors report on the method used to set-up the

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model but fail in explicitly comment on why? For example a function for the annual rate of urban growth (Eq. 1) has been taken that is reasonable but there is no comment on why this should be taken as a general rule or is just a common practice. If so what are the uncertainty associated to given choices?

Response: we appreciate the referee for raising concerns about the criticalness of section 3. Although we repeatedly stated the advantages and applicability of the SLEUTH model in section 1 (lines 16-19, page 2), the section 3, and conclusions (lines 10-16, page 17), we agree with the referee that in the methodological part the rationale for the model selection should be first stressed and made very clear. Thus, we made the following improvements and clarifications. First, we enhanced the logic flow of section 3 by first introducing the overall research framework (line 3, page 5), instead of the background of SLEUTH. Following this, we highlighted why the SLEUTH was selected and why it was applicable to our study region (lines 8-15, page 5). Third, the purpose of the annual rate of urban growth is to increase the credibility of weights that correspond to different levels of urbanisation probabilities. This methodology was justified lately (Onsted et al., 2014) and has a great potential to become a general rule in future SLEUTH applications. As suggested by the referee, in the revised manuscript we first explained why we selected this method (lines 1-3, page 10) and then stated its potential values and limitations (lines 4-6, page 10).

6) The authors acknowledge the problem of estimating model calibration to reach a good match with data based on metrics. Nevertheless after mentioning the problem they adopt OSM. It would be good to have some comments of the properties/efficacy of such selection.

Response: we thank the referee for this comment. We justified our selection by adding "The authors evaluated different combinations of the thirteen metrics and found that OSM contributes to more accurate and superior predictions than single-metric approaches. Recent studies have furthermore suggested OSM's robustness (Jantz et al., 2010; Sakieh et al., 2015). Hence, it was applied in this work to narrow parameter

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ranges after each stage.” (lines 17-20, page 14)

7) The statistics is used somehow without properly justifying the choices. We read (par. 15, page 9) “Seven Monte Carlo iterations with narrower parameter ranges were employed in the fine stage.” Why 7 and not 8, 9, 10...what is the impact of this choice? We read “Therefore, a derive calibration with the candidate set were performed with 100 Monte Carlo iterations” Why 100? Can the authors justify and provide more insight on the choice made?

Response: thank you for these comments. The selection of different numbers determines the level of model fit and analytical times. Since SLEUTH applied a "brute force" algorithm, a marginal increase in accuracy is at the expense of exponentially rise in computational time. However, we totally agree with the referee that we should prove our selections appropriately and explicitly in the text. Thus, we added some references to our choices and the following statement.

“While increasing the number of MC iterations can slightly enhance accuracy, the rise in calculation time is extremely pronounced. To balance model fit and efficiency, SLEUTH developers and users experimented in different study areas and developed experiential numbers of MC runs during different steps: 4-5 (coarse); 7-8 (fine); 8-10 (final); and 100 or greater (derive) (Project Gigalopolis, 2016). Hence, this work utilised 4, 7, 9, and 100 MC iterations for each of the four steps respectively. This set is consistent with Sekovski et al. (2015) who examined coastal vulnerability to flooding at a similar geographical scale.” (lines 5-10, page 14)

8) Paragraph 4.1 is interesting but needed to be expanded.

Response: we appreciate this comment. We have enriched this subsection by tying the coefficients of calibrated parameters with historical land-use changes. Specifically, we added the following discussions.

“As indicated in Figure 2, the previous urbanisation primarily occurred in the vacant

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areas immediate to central Panama City and southwest shorelines. Such an outward expansion of cities is demonstrated by the breed parameter—the most influential factor affecting urban growth. Additionally, two newly urbanised clusters in the north have appeared and been expanding since 1995 (Figure 2). Such a spatial structure is largely captured by the dispersion and breed factors: their values are the second (71) and third (70) highest respectively. By contrast, the low value of the slope parameter is understandable since Bay Country has few mountainous areas, and therefore elevation is not a limiting factor. This finding suggests that the weight of elevation can be further reduced in plain regions, pointing out a direction for customising the data structure of SLEUTH. The road gravity's coefficient is much lower than those of the dispersion, breed, and spread parameters, indicating a limited impact of road systems upon land use allocation. This effect is intuitively reasonable in that transportation networks in the study area have remained stable since the 1980s.” (lines 5-15, page 16)

9) As a general remark I suggest wherever possible to point-out that this work is a methodology type of work. Also, the level of approximations, uncertainties associated to each step of the analysis performed are so many that it should be clarified as much as possible that conclusions have to be put in context and somehow used as a general indication of possible risks.

Response: we are grateful for this comment. We have created a new section in the discussion part to talk about modelling and uncertainty issues (line 2, page 23). We also redeveloped and polished the discussion section to stress what the limitations of this work are and what readers should be aware of when employing or interpreting the results. Specifically, we have revised the paper in the following aspects.

a) Page 23, Lines 3 through 7: we stated that this article is majorly a methodological work and that we would talk about the study's limitations related to assumptions and uncertainties.

b) Page 23, Lines 8 through 18: we addressed the referee's concerns regarding the

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levels of approximations and the generality of our conclusions.

c) Page 23, Lines 19 through 30: we addressed the referee's comments about the assumptions and their potential risks.

d) Page 23, Line 31 through Page 24, Line 4: in respond to the reviewer's comments, we discussed two aspects of the uncertainties – parameter estimation and the generation of SLR-induced flooding maps.

To address a similar comment from the first referee, we expanded the section 4.4.3 entitled "Policy implications" (line 14, page 24) to deal with the issues of three urban growth scenarios. Since this paper applied three urban forms to represent future land-use dynamics, we made it very straightforward the potential problems of associating urban shapes and their exposure to flooding risks (line 20, page 24). We particularly discussed the relationships between proposed urban forms and policies to improve the practical contributions of this paper (page 24, line 23 through page 25, line 15).

10) Figures overall are of a poor quality. They would also benefit from more substantial captions - at present it is difficult to understand much without a careful reading of the text.

Response: thank you very much for pointing out picture quality issues. We have made significant efforts to enhance the delivery of visual presentations in our manuscript. First, we redesigned almost all figures to increase their readability (please refer to the attached high-resolution figures). Specifically, we increased font size, added important information that was neglected in the original paper, and incorporated subheadings in the figures with subparts. Here, we also would like to thank the first referee for offering perceptive comments about figure problems. Second, we reprocessed all images and optimised visual quality while controlling overall file size. For more information on enhanced figures, please refer to the supplemental materials.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-157/nhess-2016-157-AC2-supplement.pdf>

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