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Natural Hazards and Earth System Sciences
Discussions

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 Editor and Reviewers:

We would like to thank you for your constructive comments that help improve this manuscript. Below please find our detailed responses to the comments of two reviewers. For the revised manuscript, please refer to the supplement of this document, which can be accessed through the link at the end of this text.

Referee #1:

Comment 1: The authors have addressed an important issue in Land Change Science by addressing multiple issues relevant to the field simultaneously. First, they employed a CA model to understand historical parameters of land use change in the area of

interest. Second, they used this information to explore future scenarios of generalized land use policy cross-dimensionally with different levels of SLR. The issue of managing SLR adaptation in low-lying areas is absolutely critical and so this research is of great urgency.

Response: We totally agree with the reviewer. Hurricane Katrina, Hurricane Sandy, and other disastrous coastal hazards have led to enormous economic losses and serious life consequences. These lessons have urged coastal communities to take actions to protect their residents from coastal hazards. Thus, the research on SLR adaptation is of great importance. Additionally, it is crucial to identify potentially vulnerable areas to SLR, and therefore local governments can efficiently allocate recourses to optimize the deployment of SLR adaptation strategies. This study was thus initiated in order to develop an integrated framework that connects urban dynamics and SLR related hazards.

Comment 2: Having said that, I do have some comments on the methods used to reach the conclusions cited at the end of this paper. First, the authors borrow from the techniques developed by Onsted and Roy Chowdhury (2014) but they missed one of the most important points of that paper: measuring urban growth over an entire zone is not an accurate way of understanding differential impacts over a heterogeneous landscape. The results of their work show that utilizing such a technique results in worse goodness of fit metrics than treating the landscape as homogeneous. Instead, the authors recommend using the AMLEG technique which helps to address distance decay effects that dilute the efficacy of measuring all urban growth in a large zone. I strongly suggest the authors take a look at this technique (in Onsted and Roy Chowdhury (2014)) as its employment will increase the accuracy of the E values in the E-1 scenario.

Response: Thank you for the comments. We acknowledge that urban growth over a large study area is heterogeneous. Therefore, we developed a new E3 excluded layer that was based on the AMLEG approach. Significant improvements have been made in

terms of overall research framework, the methodology, results, and discussions. First, the new E3 excluded layer was incorporated into the new study framework (Figure 3 on Page 7). Second, a new section was added to the methodology part to illustrate the procedures by which the flooding-risk mitigation based the AMLEG was developed. In section 3.2.4, we will add "Most SLEUTH modelers apply the above-mentioned methods to develop excluded layers. However, such methods are deficient since they usually treat the whole study area homogeneously. Conversely, urban growth is more likely to involve heterogeneous changes across the study area. For instance, in coastal regions new residential developments largely extend from existing settlements that may only cover a small portion of the whole study region. Thus, Onsted and Chowdhury (2014) developed a procedure that corrects the growth rates in the AMLEG. The authors concluded that the AMLEG technique produced more accurate results than the other two methods: arbitrary guessing and the calculation based on the whole study area. Therefore, the AMLEG was applied based on the E1 scenario (flooding mitigation). The SLEUTH was run in the prediction mode for 100 Monte Carlo times for the period of 1995 to 2013. All five growth coefficients were set as 100, and the cells with an urbanization probability of 50% or more were considered in the AMELG, as shown in Figure 7" [Page 12, Line 1 through Page 13, Line 5].

In the section of results and discussions, we made the following improvements.

1) Page 18, Lines 3 through 10: The quantity of urban growth under the E3 excluded layer was discussed and compared with the other policy scenarios.

2) Page 18, Line 11 through Line 14: Urban growth up to 2080 under the E3 scenario was illustrated in Figure 10 and discussed in the text.

3) Page 18, Line 20 through Page 19, Line 2: The AMLEG effects on urban exposure to sea level rise induced flooding were shown in Tables 8-9 and Figure 12 and analyzed in the discussions.

Comment 3: Second, the authors made no attempt to scientifically derive the differ-

ences amongst the land use zoning categories in their construction of E2 as they did for E1. Instead, they just guessed because others have done so. However, other results from Onsted and Roy Chowdhury (2014) suggest that guessing results in poor accuracy as well, or at least worse than treating the entire area as homogeneous.

Response: The authors appreciate the comments. This is a limitation of our work. Although we tried to cite reliable sources to develop adequately scientific estimates for the excluded values of E2, we fail to point out potential bias due to the experience-based method in the original manuscript. The E2 layer should be recreated using historical data and more robust techniques such as the AMLEG, but we had difficulty collecting historical zoning maps corresponding to the past land cover maps. Therefore, we will highlight and discuss the methodological deficiencies by making the following improvement in the revised manuscript.

Page 23, Lines 19 through 21: we will add "Third, this paper bases on couples of assumptions to forecast urban landscape evolution. It determines the excluded values of the E2 scenario according to future land use plans and suggestions from other studies (e.g., Akin et al., 2014). Although the lack of zoning information forced us to make this assumption, the predictions under the E2 may become problematic."

Comment 4: Third, best practice usually discourages forecasting further into the future than you have calibrated in the past. The authors have data from 1974 to 2013, which is 39 years of data. But they use this to forecast 67 years into the future. Please see the figure from Goldstein et al. (2014). Goldstein, N.C., J.T. Candau, and K.C. Clarke. 2004. "Approaches to simulating the "March of Bricks And Mortar"". Computers, Environment and Urban Systems 28:125-147.

Response: Thank you for the insightful comments. The authors totally agree that urban growth predictions should not exceed the time range of calibration. In initial experiments, we considered a short time frame but found that sea level rise impact was extremely limited. Thus, the authors used a longer forecasting time range to

make significant the sea level rise consequences. In addition, some studies may suggest that scenario-based predictions may choose an aggressive extrapolation option to show any emerging patterns, while the interpretation of results should be very careful. This can be seen in a recent report that forecasts future urban developments in 2070 for the whole Florida (Mapping Florida's Future – Alternative Patterns of Development in 2070, retrieved through: http://1000friendsofflorida.org/florida2070/wp-content/uploads/2016/09/florida2070technicalreportfinal.pdf). However, the authors made the following discussions regarding the issues associated with extrapolation.

Page 23, Lines 21 through 27: we will add "Besides, the extrapolation of future urban growth beyond the calibration range can be questionable and generate uncertain results (Goldstein et al., 2004). Modellers ought to make a trade-off between land use predictions and the projections of climate change related hazards. Climate change is slow going, but urbanisation may be rapid in populated coastal regions. For instance, SLR may become significant only after an adequate period that probably exceeds that of historical urban data. Such coupled analyses should aim at identifying the general impacts of climate change on future urbanisation, rather than replicating the past urban patterns."

Comment 5: Fourth, the future scenario results are less emergent from interacting and hard to predict factors than they were engineered by the authors to fit a priori expectations. For example, a sprawl scenario was designed by tweaking the model's growth parameters until sprawl was achieved. However, it is important to reflect upon the utility of such scenarios for understanding the nature of urban growth in the area as well as how it can help us improve our modeling methods. For example, from a policy maker point of view, what suite of policies will lead to such a sprawl scenario? What suite of policies will lead to a compact growth scenario? The sprawl and compact growth scenarios are implicit in the sense that they suggest "if a series of circumstances happen that result in sprawl, this is what sprawl would look like". However, do we need a model to tell us what future sprawl will look like if we already decide what that future

is? The more the outcome is controlled the less predictive quality the model has.

Response: The authors are very grateful for the comments. Many historical urban growth cases have witnessed urban sprawl and compact development. These urban forms are two of the more representative urban growth patterns than others. So we choose them to examine the sea level rise impacts on possible urban change scenarios. In fact, scenario-based predictions were adopted by numerous similar studies (see, for example, Refiee et al. (2009), Zheng et al. (2015), and Sakieh et al. (2015)). We admit that more model controls mean less flexibility, so we used the historical growth pattern as a reference. Additionally, we highly agree with the referee that it is crucial to identify the policy implications of various growth patterns, which is absent in many simulation papers. Thus, we reorganized the section of policy implications by following a logically connected flow of the text. First, we discussed how urban forms were exposed to sea level rise and which urban form appeared less vulnerable to climate change related hazards. Following this, we expanded the discussions by talking about other dimensions of urban forms such as their influence on social-ecological welfare and different policies leading to different urban patterns. Lastly, we narrowed down our discussions by proposing general policy frameworks that promote both societal prosperity and hazard mitigation. In summary, in the new manuscript, we made the following changes.

Page 24, Line 16 through Page 25, Line 5: we reviewed policies leading to urban sprawl. "Population and economic growth largely drive urban expansion. Nevertheless, policies behind such growth should also be investigated since these policies (e.g., land use plans and economic strategies) represent developmental blueprints. Thus, it is beneficial to reflect upon how policies contribute to distinct urban growth patterns: urban sprawl and compact growth. Urban sprawl is characterised by unplanned and scattered developments in suburban areas. Uncoordinated growth in the city edge has been suggested to relate to multidimensional factors regarding economic incentives, housing development plans, and transportation policies (De Vos and Witlox, 2013;

Lopez and Hynes, 2003; Yue et al., 2016). Economic incentive packages launched by the central government have contributed to urban sprawl in the developing world. For instance, China took an economic reform in the 1970s by opening up land markets and commercialising housing units. This economic stimulus gave rise to many sprawling mega-cities such as Beijing, Shanghai, and Chengdu. In Europe and North America, though, microeconomic theories may majorly explain urban sprawl. For example, households begin to relocate to the suburbs when the land prices in city centres become prohibitively high. Their relocation decisions are further strengthened by housing and land development policies. Developers promote low-density communities in the city periphery. Local governments help build large retail centres to accommodate the increased demands. Motorization policies and low fuel costs result in automobileoriented cities. Even public transit policies aggravate outward city growth by charging long-distance commuters less than the riders for short distances (De Vos and Witlox, 2013)."

Page 25, Lines 6 through 15: The policies promoting compact development were reviewed. "As urban sprawl increasingly threatens public health, social equity, and the built environments, people start to develop different urban containment policies. There are primarily two forms of containment policies that were adopted in the US. The State law in Oregon and Washington requires that local land use plans should clearly define an urban growth boundary. In other states such as Florida and Maryland, governments develop urban service limits, public facilities ordinances, and other policies to promote compact urban forms (Aytur et al., 2008). However, the effects of urban containment policies have been hotly debated. For example, not all urban growth boundaries significantly affect housing markets and the rates of urbanization (Dempsey and Plantinga, 2013). Thus, De Vos and Witlox (2013) suggest the integration of spatial planning policies, mobility policies, and road pricing. Spatial planning can strictly limit new developments outside urban areas. Transit-oriented development benefits nurturing high-density and mixed land-use neighbourhoods. Lastly, road pricing increases long-distance travel costs, thereby curtailing urban sprawl."

Comment 6: I suggest that authors, at most, redo the aspects of the methods described above. However, at the least, the issues I mention above should be discussed in the article as possible deficiencies in the current methodology.

Response: Thank you for the suggestions. We have spent great efforts on redoing the crucial aspects regarding the current methodology. As stated in the response to the second comment, a new E3 excluded layer was created based on the AMLEG technique, and some parts of the simulation were run again to incorporate this change. Accordingly, substantial changes were made in terms of the methodology, the visualization of results, and discussions. However, we fail to redo a few methodological aspects raised by the referee due to data limitations. We did highlight and discuss these method limitations in the discussion section [see Page 23, Line 1 through Page 25, Line 27].

Comment 7: The strongest aspect of this work is the integration of SLR scenarios vis-à-vis future urban growth scenarios. This line of inquiry is absolutely critical for coastal resilience and thus the authors should be applauded for the great importance this kind of research has on the sustainability of our worldwide coastal civilizations.

Response: We are grateful for the referee's applause. We hope that the research could be a small yet important effort in the coastal resilience science. Informative decision support tools can greatly benefit coastal communities and mitigate the economic and life consequences of coastal hazards.

Comment 8: (Page 4, Lines 25 thru 30) The impervious surface percentages for the various zoning categories mentioned are incorrect (I checked). The authors need to revise this accordingly.

Response: The authors apologize for the confusion. We carefully read the source document again and rephrased the explanations to make clear the impervious surface percentages [see Page 7, Line 23 through Page 8, Line 3].

Comment 9: (Page 6, Line 12) This should read “percentage slope” instead of percentage rise since percent slope is rise over run.

Response: Thank you for pointing out the accurate terminology. The text was then corrected according to this comment [Page 7, Line 10].

Comment 10: (Page 10, Line 14) Increased SST does not cause higher pressure over the ocean, but lower pressure. The magnitude of a hurricane is often directly related to how LOW the pressure in the eye is, thus Lower is stronger. The authors seem to have this reversed.

Response: Thank you for this comment. The authors carefully went through the original citation and found that we had some misunderstanding on this phenomenon. Thus, the text was revised based on this comment [Page 15, Line 18].

Comment 11: (Page 11, Lines 12 thru 18) The methods the authors list (differences of 25, etc.) does not match what they actually have listed in Table 5.

Response: Thank you for the correction. We found the typo in Table 6 and revised it accordingly [Page 17].

Comment 12: (Page 14, Line 14) The authors seem to suggest that sprawl leads to less vulnerability in all coastal areas and thus policy makers must choose as a tradeoff between sprawl with all of its negative environmental consequences or flooding. However, the most important factor is not necessarily proximity to the coast but, rather, simple elevation. Thus Panama City is not necessarily representative of the topographical constraints and opportunities in all coastal areas.

Response: We appreciate the comment. The previous statement suffered from the inappropriate generalization of the exposure of different urban forms to sea level rise. Thus, we rephrased the statement and the conclusion about urban vulnerability. We will add: “However, this might not be true in flat coastal areas from the perspective of hazard mitigation [Page 24, Line 16] ... However, such conclusions are made only

based on our case study area where slopes change insignificantly [Page 24, Line 20]”.

Comment 13: (Page 14, Lines 28 – 29) As Florida sits on porous limestone a seawall will not keep out SLR since the ocean will just come up underneath on the other side.

Response: Thank you for the comments. The following text was added to include this information suggested by the referee. “Furthermore, in the long term, policy makers should formulate adaptation plans that address other SLR aspects such as groundwater pollution and saltwater intrusion beneath protective structures [Page 25, Line 26]”.

Comment 14: (Figure 1) There are three maps at three different scales but only one scale bar is used. Each frame should have its own scale bar.

Response: Thank you for the comments. The figure was improved with each map having its own scale bar. The enhanced figure could be found on page 3.

Comment 15: (Figure 6) Though technically an Excluded Layer can be portrayed however one wants in the actual publication it is confusing to see the actual Excluded Layer in grayscale but portraying the opposite grayscale values of their E scores. Thus the ocean should be 100 or over, but instead is represented as 0, etc. It could be helpful to those in the SLEUTH community if the authors showed the grayscale Gif Excluded layers exactly as they are.

Response: We appreciate the comments. The authors apologize for such confusions. Thus, in our new figures, the water bodies were not assigned any excluded values. We hope this improvement helps to clearly depict the excluded layers [see Figure 4 on Page 9, Figure 5 and 6 on Page 11, and Figure 8 on Page 13].

Comment 16: (Figure 9A, 10A, and 11 A) The bounding boxes should be removed as they serve no purpose. They should instead be used in Figure 12 as it appears that is where they correspond.

Response: We removed the bounding boxes according to the referee’s suggestion. In addition, these three figures were combined to reduce the total number of figures

[Figure 10 on Page 19].

Comment 17: (Figure 13) The dark blue color appearing in the maps of this Figure does not appear in the legend. I am assuming it is urbanized land that is also flooded. However, guesswork should not be required by the reader.

Response: Thank you for the comment. A complete legend was added in Figure 12. In addition, the authors greatly enhanced the readability and appearance of figures by increasing figure resolution, enlarging legends and texts, and redesigning the layout [see Figure 2, Figure 4, Figure 7, Figure 9, Figure 10, and Figure 12].

Comment 18: (Technical) There are numerous spelling issues, missing articles, etc. throughout the manuscript. For example, “Talbe 4 About here”. Or poses “unnecessarily” risk instead of poses “unnecessary” risk in the abstract. Another pass of proofreading is recommended.

Response: The authors highly apologize for grammatical mistakes. We have carefully proofread the whole manuscript to exclude language issues as much as possible. Special attention has been given to misspelled words and article issues. Furthermore, we inserted figures and tables into the main text and rearrange the overall structure in the new manuscript. Readers can then relate the information with illustrations and numbers more easily.

Referee #2

Comment 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 haz-

ard 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).

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

Comment 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 the comment

#5). We demonstrated the selection of data inputs, as shown in the response to the comment #6. 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 the comments #7, 8, 9, and 10, respectively.

Comment 4: 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 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."

Comment 5: 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 summarised below.

a) Page 3, Line 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.”

Comment 6: 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.

Comment 7: 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.

Comment 8: Overall section 3 is uncritical. The authors report on the method used to set-up the 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).

Comment 9: 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 ranges after each stage." (lines 17-20, page 14)

Comment 10: 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)

Comment 11: 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 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)

Comment 12: 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 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).

Comment 13: 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-AC4-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-157, 2016.