Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2020-646-RC1, 2021

Comment on hess-2020-646

Anonymous Referee #1

Referee comment on "Possibilistic response surfaces: incorporating fuzzy thresholds into bottom-up flood vulnerability analysis" by Thibaut Lachaut and Amaury Tilmant, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-646-RC1, 2021

General comments

This study was to use the fuzzy theory to refine the "bottom-up" vulnerability analysis. The authors suggest that the decision threshold itself could be ambiguous for policymakers to use the system response surface, because it would be strongly affected by stakeholders' subjective opinions. Thus, the authors proposed to incorporate fuzzy thresholds into the probabilistic response surface, and provided a case study assessing flood risks under climate change. The topic is interesting and meaningful for impact assessment research communities. To my knowledge, prior vulnerability-based assessments have been focused mostly on how to address system responses to climatic or hydrologic stressors rather than on how to set the threshold that directly determines the status of system failure. In practice, as argued by the authors, the threshold is unlikely crisp, and hence its ambiguity needs to be condensed in the response surface. I think the authors' quantitative approach is novel and reasonable, and recommend minor revisions for final publication in Hydrology and Earth System Sciences. Please find my specific comments below.

- I recommend to distinguish ambiguity of the threshold from modeling uncertainty through the manuscript. While ambiguity and uncertainty are similar and thus some authors often lump the two concepts=, sources of the two seem different in the bottom-up assessment. In a climate change impact assessment, typical uncertainty sources are uncertain future emissions, uncertain general circulation models, uncertain system and hydrologic models, i.e., mostly from things outside of humans' psychological behaviors. On the other hand, a major source of the ambiguity would be stakeholders' subjective opinions about the decision threshold, which are likely affected by their sub-conscious behaviors. If uncertainty is treated as a modeling problem and separated from the fuzziness of the threshold in the manuscript, readers could understand the authors' intention more clearly. I do not mean that uncertainty cannot be lumped with ambiguity, but separating the two would better guide potential readers. The manuscript is starting with "Uncertainty is a defining feature of water management." Uncertainty seems to include ambiguity in the manuscript from the beginning.

- Please shorten the introduction. To me, it was too long. For example, the sentences about top-down assessments is not the core of this work. Just introducing some shortcomings and leaving some relevant references would be better to lead readers directly to the main objectives of this work.
- A remaining task in the case study might be to validate the probability estimates from the logistic regression. Kim et al. (2019) handled this problem by additional simulations with random climatic stresses independent of the logistic regression. Please consider any method that could show validity of the probability estimates. Obviously, it will improve reliability of this work.
- I think the alpha-cut approach has an issue of how to set the alpha value appropriately, as though the traditional logistic regression has an issue of how to appropriately set the pi threshold. The authors need to leave some discussion on this issue.

Following is line by line comments on technical errors and some issues on the authors' discussion.

L5: please remove the duplicated "the"

L45-46: Please explain what the climate impact response function first. Then, use the acronym. Perhaps, a relationship between such stressors and the performance of the system in L45 is the CIRF. The reverse CIRF might be used to find the range of climate stressors within which system performance is acceptable. I feel that this part needs clearer explanation.

L53-56: This sentence is too long. Please consider rewriting.

L79-96: Some of this part could be moved to the section 2, because it includes how fuzzy theory is applied in this work.

L128: With two variables

L130: just underestimate (no hyphen)

L147: Zadeh (1965)

L163: Please check if the inequality symbol is correct in Eq.(3)

L173: … threshold, showing …

L185: if you chose just two explanatory variables (x1, x2), then the eq. 5 should have x1 and x2. Please remove "+…"

L240: Please be consistent with the citation policy of the journal. Nazemi et al. (2013), Borgomeo et al. (2015), …

L242: Here too. Vormoor et al. (2017)

L248-256: Could you add any statistics resulted from the bias correction methods? It could inform reproducibility of CMIP5 GCMs. I guess runoff projections were likely used for those bias-corrected projections. Then, the scale and boundary mismatch between the GCMs and the watersheds are still a problem. If I am correct, please leave discussion on this issue in the manuscript. If not, please more clearly describe the inflow projections were made.

L315: 5-year period

L336: please remove the unnecessary a.

L340: The pseudo R2 is about 75%. Is it acceptable performance? And, what are potential sources of the remaining 25%? Please add the authors' opinion on this result.

L420-428: Maybe, this part is related to how to set the alpha threshold. Am I right?

L429-436: The introduction of the loss aversion function and the membership function is somehow abrupt. Please explain those concepts first.

L473: The proposed method is probably one of various ways to consider fuzziness of the decision threshold in the bottom-up impact assessment framework. We don't know yet whether or not it works for other problems with very high complexity. Please tone down at

least somehow.

Reference

https://hess.copernicus.org/articles/23/1145/2019/