

# ***Interactive comment on “Possibilistic response surfaces combining fuzzy targets and hydro-climatic uncertainty in flood vulnerability assessment” by Thibaut Lachaut and Amaury Tilmant***

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**We would like to thank the referee for his/her constructive comments.**

*“This paper develops new approaches for bottom-up decision making approaches considering joint uncertainties in the system response surface and the performance target. Three methods are proposed: a fuzzy logistic regression, an analytical approximation, and a convex hull method. A case study of flood risk in Canada is used to illustrate the*

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methods.

*The paper identifies important challenges in bottom-up methodology, and the proposed methods are new to the field while also drawing on historical developments in decision theory. However, the results do not clearly illustrate the benefits of the new approaches, and may introduce more complexity. I believe this can be resolved with substantial revisions, as the authors have done a nice job with the motivation and methods description.*

*1. My first concern is how the methods treat hydroclimatic uncertainty in the response surface. The paper notes that the variables sampled in the response surface only partially cover the space of possible uncertainties, which I agree with. However, I would not say that this can be captured by the uncertainty in the fit of the response surface using logistic regression. The uncertainties we are most concerned with are the hydroclimate timeseries and natural variability, which will not be captured using this approach.*

*It is not reasonable to expect the authors to find a way to quantify this uncertainty, which would be a different study altogether. But the claims about the types of uncertainties considered should be aligned with the experiment.”*

We agree with you that the hydroclimatic uncertainty cannot be captured by the uncertainty in the fit of the response surface. In the revised manuscript we will better explain that the main objective is to explore how we can integrate fuzzy thresholds in vulnerability assessment approaches, and how we can combine this ambiguity with the uncertainty inherent to a bivariate response. The first method indeed uses the logistic regression previously employed (Kim et al. 2019) as one of the ways to convey this uncertainty, proposing a division of the exposure space by probability of success. This probability of success at each coordinate aims at capturing part of the hydro-climatic uncertainty that the 2 variables of the exposure space do not capture. We do not here consider the additional uncertainty on the fit of the logistic regression itself, though we mention it could be incorporated.

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*"2. The results section is quite long, and does not clearly show the value of the new approaches within the decision-making context. The paper would be much stronger if the authors could resolve this. I would suggest refining and shortening the figure sequence to more clearly show the differences between the standard response surface and the new methods, especially if there is a way to highlight differences in the decisions that would result.*

*At present, the results seem to show that the new approaches yield only small differences from the standard stress-test, which may not be significant in the context of other uncertainties in hydroclimate as mentioned above."*

Thank you for your suggestions. Based on both referee comments we realize that the main objective of the paper, which is on the consideration of fuzzy thresholds in vulnerability assessment approaches, did not come across clearly. The confusion comes from the fact that this incorporation was analyzed for three alternative methods to generate regions of success and failure. In the revised version, we will shorten the material and method section by focusing on fuzzy thresholds combined with one generating method: the logistic regression. This choice is motivated by the fact that this approach has received a lot of attention recently in the literature (in addition to Kim et al., 2019; the paper will cite Quinn et al., 2018, Lamontagne et al., 2019, Hadjimichael et al, 2020; Marcos-Garcia et al., 2020).

We acknowledge the interest of discussing the effect on outcomes. However, when comparing the effects of using a crisp and fuzzy threshold, the crisp threshold is only counterfactual, not an alternative option to be compared to. It helps to visualize how fuzzy thresholds affect the division of the exposure space in regions, but we assume that the crisp threshold is not available in the first place. This should be further emphasized in the result section.

Cases where a fuzzy threshold can lead to a different decision – again, compared to a counterfactual crisp threshold – are also worth discussing. In the attached figure

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(to be added in section 2 of the revised manuscript), we see that it is theoretically the case if the response functions of two options have different slopes. This can also be illustrated with other case studies, e.g. Quinn et al. (2017) where an improvement in moderate flooding can make extreme floods worse, thus possibly changing the slope of performance as function of stressors. However, in the present paper, the difference in slopes between the alternative options is small and does not lead to a different decision in this case.

Still, we think that this case-study specific result does not diminish the validity of the underlying research question: how do we handle non-clearly defined (fuzzy) thresholds in bottom-up, vulnerability assessment studies? The paper needs to make clear that the method is not an alternative but an extension of bottom-up vulnerability assessment studies for particular situations, whereby crisp thresholds do not exist due to a variety of reasons including the lack of consensus amongst stakeholders, the ambiguous definition of the associated objective, etc.

*"3. The methods proposed by the authors provide a more formal way to incorporate uncertainties not usually considered in bottom-up modeling studies. However I am not sure of its practical value, because it replaces the subjective choice of a single threshold with the choice of a membership function, which is perhaps even more difficult to define. The authors recognize this challenge in the conclusion. This limitation would be somewhat resolved if the results clearly showed an advantage to the more complex uncertainty representation."*

This is an interesting point. Substituting a crisp with a fuzzy threshold indeed requires the definition of a membership function, which is not necessarily straightforward. But this issue is well known in fuzzy set theory and has been extensively investigated by various authors in several application fields, including multicriteria analyses (see Bouchon-Meunier et al., 1996; Haber et al., 2002; Garibaldi et al., 2003; Wu, 2012;

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Sadollah, 2018). Our position is that this difficulty should not preclude the development/refinement of bottom-up approaches so that they can handle non-crisp thresholds. Ultimately, it is up to the analyst and decision maker to decide whether the incorporation of a fuzzy threshold is worth the additional effort. In the revised version, we will discuss this issue in the concluding remarks and suggest further readings on how to interactively select membership functions.

*"Minor points - The introduction starts very broad, and could be edited for clarity - The bibliography contains references not cited in the paper, and vice versa"*

Thank you for highlighting this, the introduction will be revised with a more focused start. The errors in the bibliography are fixed.

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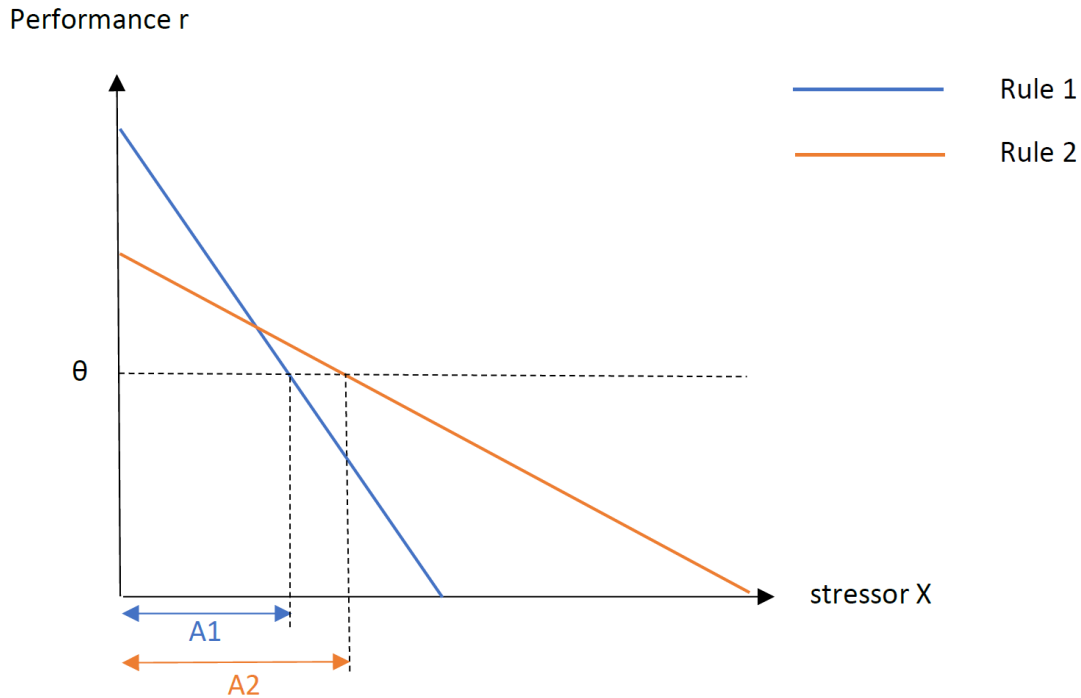
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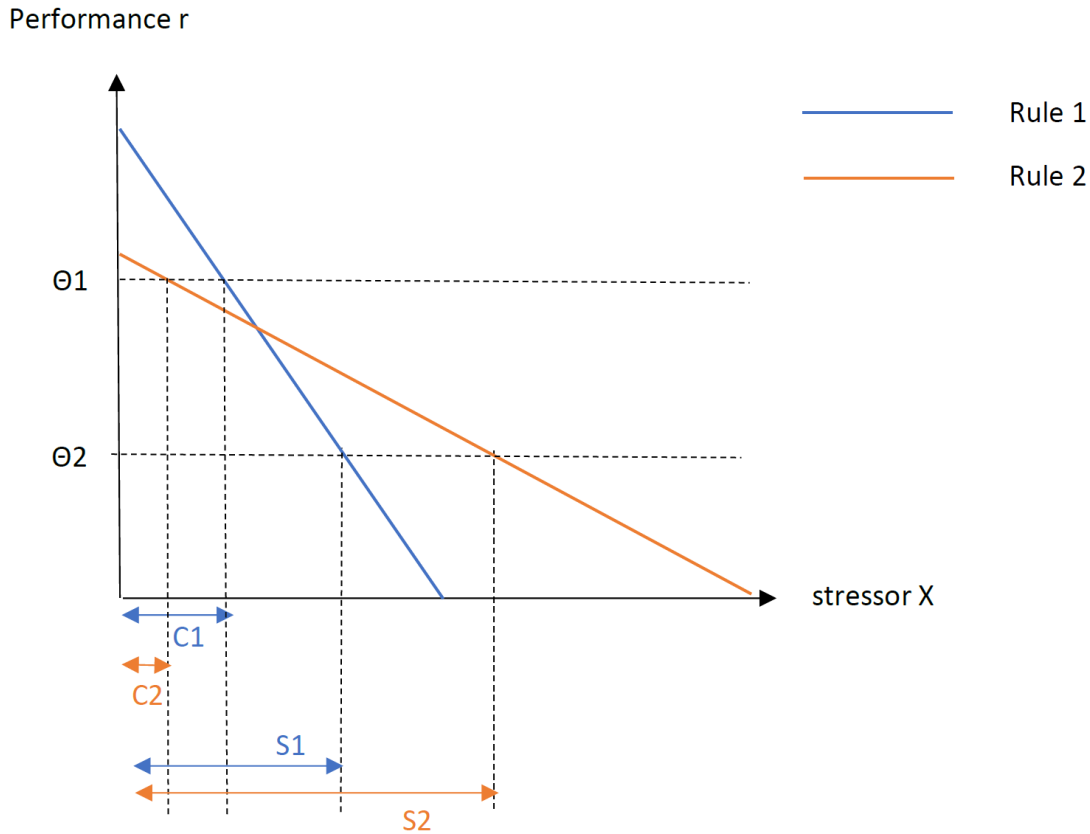


**Fig. 1.** With a crisp threshold  $\theta$ , rule 2 has a larger success region  $A2$ .

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**Fig. 2.** With a fuzzy threshold ( $\theta_1, \theta_2$ ), Rule 2 has a larger “at least partial” success domain  $S_2$ , but a smaller “full” success domain  $C_2$ , than Rule 1.

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