

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

Anonymous Referee #1

Received and published: 15 July 2020

Possibilistic response surfaces combining fuzzy targets and hydro-climatic uncertainty in flood vulnerability assessment

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 methods.

C1

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.

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.

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 thresh-

C2

old 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.

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-214>, 2020.