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Interactive comment on "Uncertainty quantification of flood mitigation predictions and implications for decision making" *by* Koen D. Berends et al.

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Overall the paper is well structured, well written and quite clear. However, I have a number of reservations regarding the manuscript. Several of these overlap with the points raised by Joseph in his comments, so I won't discuss those here in any further detail.

My main reservation is regarding the contribution that the paper is trying to make. In the abstract the authors in the second sentence mention the role of model-based analyses in supporting decision making, while ending the abstract with claims about model-based decision making. In the middle of the abstract, the authors mention a new metric ‘relative uncertainty’. Next, they argue that using this new metric they can provide insight into the uncertainty about the effects of a variety of flood risk reduction intervention.

I have two reservations here. First, regarding the claims related to decision making. In my view, these claims are not well developed in the paper. Outside of the introduction and conclusion, the term itself only appears once. Moreover, if I look at the presented results and their visualization, I doubt these would be used by a decision maker or even someone directly advising a decision maker. Rather, the presented analyses are useful for people working on the design of flood risks reduction strategies, while some of the results might indirectly be used in decision making. As such, I would suggest removing the term from the title as well as tone done any other claims made in the abstract and introduction. Alternatively, the authors would need to expand their discussion in the main text on how providing insights into the range of uncertainty about expected effects of measures can assist decision making on flood risk. This, however, requires discussing notions such as robustness (see e.g., McPhail et al., 2018) and flexibility, as well as discussions on well characterized uncertainty (i.e., you have a meaningful pdf) and Knightian or deep uncertainty (i.e., for whatever reason you don’t have a meaningful and uncontested pdf).

My second reservation is with the new metric itself. As also indicated by Joseph, this metric is closely related to the coefficient of variation. Moreover, the more theoretical discussion of this new metric is confined to one short paragraph around equation 5. Too play devils advocate: what is the merit of publishing a paper whose only contribution is a single equation closely related too an already established metric? The case study mainly serves to establish the value of this metric, while the models are taken from earlier work. If you insist on having the metric as a key contribution, than a comparative perspective would be more appropriate. So, what other metrics already exist that could serve a similar function? Classic robustness metrics as well as the coefficient of variation would be logical candidates. How is this metric different from these, and what are the relative merits of the new metric relative to the others?

Basically, both reservations have to do with how the authors currently position their work. I am having profound reservations regarding the first two paragraphs on page 3. First, the authors cite Pinter on fuzzy math, suggesting that not providing explicit uncertainty quantification leaves decision makers free to interpret the uncertainty in any way they like. My argument would be that the converse also often happens. As for example elaborated nicely in Pilkey and Pilkey-Jarvis (2007), a lot of quantitative analysis for supporting decision making becomes useless arithmetic that is used strategically. In a different line of literature, researchers working on post normal science often claim that many models have not even one significant digit. That is, models give only a false sense of precisions. A third line of research, as exemplified by Sarewitz (2004) shows how more research and increasing efforts to quantify uncertainty can often make environmental controversies worse. In short, the relationship between models, model results, uncertainty about models results and decision making processes is quite a bit more complicated than the authors seem to think. It would benefit the manuscript it the claimed contribution is better positioned relative also to these strands of literature.

Minor remarks

Might it not be more convenient to show the interventions (2.2.1-2.2.6) in a table?

What is the runtime of the detailed model for a single run?

Page 18, line 17, “for new ones greatly the unexplained” I guess some words are missing here

Page 17, line 4, partly overlapping with Joseph’s comment, but what justifies the linear interpolation?

McPhail, C., Maier, H. R., Kwakkel, J. H., Giuliani, E., Castelletti, A., & Westra, S. (2018). Robustness metrics: How are they calculated, when should they be used and why do they give different results? Earth’s Future. doi:10.1002/2017EF000649 Pilkey,

O. H., & Pilkey-Jarvis, L. (2007). Useless Arithmetic: Why Environmental Scientists Can't Predict the Future. New York, USA: Columbia University Press. Sarewitz, D. (2004). How science makes environmental controvesies worse. Environmental Science & Policy, 7, 385-403. doi:10.1016/j.envsci.2004.06.001

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Discussion paper