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Discussions

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

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This article assesses the impact of 12 interventions on flood water levels in the Waal River, including estimation of uncertainty. It goes further than most other studies in 1) estimating uncertainty in uncertainty attributable to the methods used, 2) discussing the potential to approximate the uncertainty, in ways that make the results of the analysis more accessible to a broader audience. The article is well written and clear. The issues I would like to highlight are relatively minor - clarifying some claims rather than fundamentally questioning them. I do suggest some small extensions to the analyses to help in clarifying these claims.

Specific comments ——————

I would rather that the abstract avoid emphasising that "relative uncertainty" is a "newly introduced" metric. As the authors do acknowledge in the main text, it is closely related to the coefficient of variation. If this metric is kept as a contribution, it would require greater discussion of why is preferred over the CV. The greater contribution here, in any case, is that relative uncertainty is identified as a key parameter to obtain approximations of uncertainty in this type of problem.

p9 L25 "We would need to obtain both the model output probability distributions, as well as the covariances between the two distributions" This is not entirely true - most of the parameters would be identical in both states of the river, so the difference can be calculated directly as a single distribution - covariances probably do not need to be calculated. The authors do end up doing this with the "union stochastic parameter space" (p11), so it seems worthwhile to clarify that you do not explicitly estimate covariances. It is still correct that this is probably not tractable for 12 interventions.

p15 To support the claim that values are relatively constant, it would be useful to show the range of E(Ur90) over the length of the river, either in Figure 7 or Table 2 E(range(Ur90)) would also be useful, given it is available, i.e. the range of Ur90 over the length of the river, averaged over all realisations There is enough variation there assuming relative uncertainty is constant seems like it would still be a substantial simplification.

p16 Figure 8 "FLPSMOOTH is significantly more uncertain" The use of the word "significantly" suggests this is a statistical statement. It might be worth putting a p-value on this, given you have uncertainty on the uncertainty. Visually, it seems high intensity FLPSMOOTH could be significantly more uncertain ($p=0.05$), but that low intensity is comparable to groyneflow or minemblow, also confirmed by Figure 7. L6-14 could also comment on statistical significance of differences in uncertainty.

p17 Figure 9 This figure is illustrative of the interpolation method, but currently only hints at how good the interpolation method would be. You have two sources of information on this: 1) the uncertainty in MCI90 and E(dH) 2) the variation in these values along the river The error bars for (1) in the figure do suggest that there is quite a bit of uncertainty involved in the interpolation. I would suggest showing a ribbon around the line encompassing all potential lines that could be interpolated using 1 and 2. This doesn't detract from the utility of using linear interpolation, but would provide a better sense of the confidence one should have in using this simplification. If the uncertainty due to using CORAL is too large, perhaps more runs might also help make your point more effectively.

p17- exceedance probabilities If I understand correctly, this now linearly interpolates exceedance probabilities rather than relative uncertainty. It's not clear why this is a permissible inference. Are there characteristics of the distributions that allow this jump? What makes (all!) exceedance probabilities linear if relative uncertainty is constant with regard to expected effect? I assume at least some kind of symmetry comes into play? Even if the assumption of linearity doesn't end up holding completely, I would be in support of keeping this analysis in the paper, with some representation of the uncertainty in uncertainty involved (as for Figure 9). I think it's very important to be able to say something about the extremes of the distribution, not just about the size of the uncertainty.

p19 Discussion It would be useful to mention that the study only considered individual interventions, whereas it would be possible to combine measures in practice. I assume the combined effect is unlikely to be a linear combination of individual effects, so I don't think this study can be used to support any claims regarding such combinations. It would, however, be useful to highlight it as future work and speculate about possible issues that might crop up.

Technical comments ——————-

Table 1 The link with subsections in 2.3 could perhaps be emphasised, e.g. by using

consistent ordering and grouping terms

p7 L14 "general extreme values distribution" Should be "Generalized Extreme Value distribution"?

p7 L20 "a 95% confidence limits at 0.31 m and 1.0 m." drop the "a"

p14 Table 2 Should "coefficient of variation" be average relative uncertainty E(Ur90)? Otherwise, this raises the question of whether relative uncertainty was indeed needed?

Figure 7 y axis label should read Ur90? (9 is missing)

Figure 9 also seems to have some text rendering issues

p18 L17 "greatly the unexplained variance" Word missing (increased?)

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