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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Quantifying the effect of sea level rise and flood defence – a point process perspective on coastal flood damage" *by* M. Boettle et al.

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Response to anonymous referee #1:

We are thankful for the detailed remarks from referee #1 who has, judging from his comments, read our manuscript very carefully. In general we are willing to implement all her/his suggested modifications. In the following, please find her/his specific comments in black and our responses in blue font.

1. Abstract: the statement "a doubling of losses can be expected from a mean sea level increase of only 11 cm" derives from a modelling study described only briefly and not

from the analytical approach taken in most of the paper – this is not clear from reading the abstract. On the other hand, as noted below, it would be good to see the modelling linked more strongly to the rest of the work.

We suggest to emphasise the link between numerical and analytical calculations by additional clarifications in Secs. 4 and 5. We are confident that such modifications could also resolve the referee's concern with the abstract. The abstract on it's own should be ok, since the mean sea level increase of 11 cm is independent of where it's taken from, i.e. any other sea level projection could be combined with the analytical derivation. Please see also our response to comment 10.

2. Abstract: I would avoid the use of the word "error" then describing stochastic uncertainty. Perhaps the last sentence can be expressed as something like "While the absolute value of the uncertainty in the flood damage increases with rising sea-levels, we find that the uncertainty decreases as a fraction of the expected damage."

We agree with the referee that the word "error" is somewhat inappropriate and we appreciate his suggested phrasing.

3. It would help the reader if a table of the main parameter symbols and meanings were included: $\mu, \xi, \sigma, \mu, \Lambda, \gamma$.

We thank the referee for this suggestion as it will facilitate the readability of the manuscript (as also suggested by referee #2). A corresponding table will be inserted in Sec. 2 in a revised manuscript.

4. The paper uses a mixture of currencies, EUR and DKK. The results would be easier to interpret if one currency was used consistently.

The reason for using different currencies is the availability of the corresponding damage functions. From our point of view, a conversion of the one or the other would only be necessary if the results of the two case studies would be compared – which is not the focus of our manuscript. Moreover, the conversion would require additional

explanations which might distract the readers attention from the essence of the paper.

5. p.6238, l.2: "divided by the provided expression". For clarity, I suggest including a reference to the equations, e.g. "E_D divided by the expressions given in equations 11-13 converges...".

We appreciate the referee's suggestions and will consider it in a revision.

6. p.6240, l.17 "the mean excesses show a linear behaviour". I don't find the choice of threshold obvious from the plots in Figure 3, especially for Copenhagen. Please include a little more discussion.

We agree with the referee that the choice of the threshold has not been explained well. In a revised manuscript we envisage a better introduction to the method of *mean-excess-plots* and thus a more comprehensible justification for our choice.

7. p.6240, l.24 The threshold for Kalundborg has been moved a long way, almost to the highest levels observed. Can we be confident that the GPD estimated using the 80 cm threshold will still apply, even with the adjustment to σ?

The adjusted threshold does not affect the stochastic properties of flood events above 135cm and is therefore not problematic.

8. p.6240, l.22-27: I get $\sigma = 18.05$ and $x_{\mathsf{max}} = 212.79$, but this may just be due to rounding errors.

As speculated by the referee, his results deviate from our results due to rounding errors. Since they are more precise, we therefore plead to keep the provided values.

9. p.6241, final paragraph: I agree that the asymptotic behaviours provide good estimates for Kalundborg, but this needs more discussion for Copenhagen, where the values of E_D and STD_D at the current 1-year event are considerably higher than the asymptotic values and the slopes are noticeably lower.

We agree with the referee and propose to add a more detailed discussion.

10. p.6242 first paragraph: This introduction of results from a widely used model is welcome, but it would be helpful if it was tied more closely to Fig. 4 and to the previous paragraph.

We are not sure whether this comment is due to a misunderstanding of the paragraph. The DIVA tool has been only used to extract sea-level projections. Based on these, the damage estimates are calculated as described in Sec. 2.6. We believe that this part is clearly tied to the previous paragraph as it just provides a different parameterisation of the damage, namely by the time (Fig. 5) instead of the mean sea-level (Fig. 4). However, the comment emphasises the need for a more precise explanation.

11. p. 6244 final paragraph: As in my previous comment, I think the claim that the analytical relations approximate the numerical results "very well" needs more discussion.

As for comment 9, we suggest to discuss this more extensive in a revised version of the manuscript.

12. p.6245 Final paragraph: Storm surges can last 2-3 days, so would it be appropriate to consider events repeating in that time as just one event?

The referee is right in saying that storm surge events typically last for more than one day. As the daily sea levels are assumed to be independent, this is not modelled in our approach and the issue of how to deal with several consecutive flood days does not arise. Nevertheless, we appreciate this comment and will address the aspect in the revised manuscript.

13. Appendix B2: there is slight inconsistency in the use of the terms m_k (p.6250 l.10), m_n (p.6251 l.13) and m_k (p.6254 l.1). One expression would seem to be enough for all three cases.

The notation will be adjusted as suggested.

14. p.6255, l.15: The equation is missing the term $2\gamma^2\omega^{2\gamma-2}\xi^2\Delta\omega^2 m_2$; this has a knock-on effect to the next equation (but does not affect the conclusion).

The referee is right and the equation will be corrected.

15. Figure 1: This is a very nice representation of the methods described in the paper

Thank you.

All technical corrections 1 through 11 will be implemented as suggested in a revised manuscript.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6229, 2015.