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Interactive Comment

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 referee #2:

The referee generally asked for more explanations and details in the technical parts. Accordingly, we suggest additional explanations at several points of the article. In particular, a table with an overview of all employed variables (as suggested by referee #1) should facilitate the readability of the revised manuscript. In the following, please find his specific comments in black and our responses in blue font.

Section 1

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Interactive Discussion
Discussion Paper

It seems to me that strength of this work is to provide approximate analytic forms to compute the expected damages and their standard deviations. Others may try finding similar quantities using a Monte-Carlo approach, for example by using the Poisson distribution to compute the number of storms and then comparing the water level given by the GPD with the critical threshold of coastal defenses. I think that the reader would be interested to read the author's opinion on the drawbacks and advantages of this approach.

The referee is right in saying that a Monte-Carlo simulation would be an alternative (less accuate) approach to estimate the annual damage and its uncertainty. However, the central finding of our work, namely the provided functional relations, could not have been obtained by this. As we do not see any advantages of following a Monte-Carlo approach we are not sure whether it was worthwhile to discuss this point in the manuscript.

Next, to integrate uncertainties on future sea-level rise (due to climate change) in the analysis, skewed distributions will be needed. Therefore, the expectation and standard deviation will not be enough to describe the uncertainties, and other moments of the distribution of the value of damages could be needed. Do the authors have ideas on how to address this issue? This could be a topic for the discussion.

The referee addresses the integration of sea-level scenarios which are highly uncertain and he assumes to be skew distributed. By considering the two SRES scenarios B1 and A1B, we have shown that the damage projection is highly dependent on the development of mean sea-levels (which we assume to be given in all our analyses). As mentioned in Sec. 3, the manuscript addresses only the aleatory uncertainty due to the stochastic occurrence of flood events. Although it was definitely worth studying, the integration of uncertain sea-level scenarios is clearly out of the scope of this paper. Regarding the general skewness of damage estimates, we agree with the referee that the standard deviation is not a perfect measure to describe the variability and will address this issue in a revised manuscript in Sec. 1. The standard deviation has been chosen as a measure since it is very comprehensible, interpretable, and furthermore easy to derive. Considering the scope of the article, we believe that the provided perspective is appropriate.

Section 2

General comment: this section is solid. Although the methods are known, I think that some more explanations would be helpful for the reader, especially because some key references are not easily accessible for anyone.

Page 6233 Formula (1) holds for $(1+\xi(x-u)/\sigma)^{1/\xi} > 0$ (?)

Eq. (1) is generally restricted to x-values above u, i.e. $x > u$. Furthermore, there is an additional condition if $\xi < 0$. Both conditions are mentioned right after the equation.

Page 6234 - The reference Embrechts et al. (1997) being not accessible (at least to me), I think a few explanations would be useful to help the reader understanding how formula (3) is obtained. I suggest to do this by showing how $P(X > u|X > \mu)$ relates to Λ.

We agree with the referee that the given reference might not be accessible for some readers. We therefore suggest to replace it by a more common reference (Coles, 2001). Sketching the proof of the formula would, from our point of view, only complicate this section without helping to understand the statement. In case a reader is interested, the given reference provides a comprehensive derivation.

- I suggest to discuss the possibility that tides are altered by sea-level rise, and, therefore, that the sea-level distribution is not just shifted upwards.

The referee addresses the assumption that rising mean sea-levels lead to simple shift of sea-level distributions. Although this assumption is justified by the given references, we agree with the referee that the possibility of additional effects should be discussed. We suggest to make the possibility of such effects more explicit in Sec. 2.3.1 and refer to Appendix A, where a changing scale parameter is discussed.

- Are there chances that because of sea-level rise, we are no longer in the asymptotic domain?

Increasing mean sea-levels move the damage behaviour further towards the asymptotic domain. We are confident that this is described sufficiently detailed at the beginning of Sec. 3 where we state that the damage behaviour converges to the provided relations for increasing values of μ.

Page 6234:

- Line 20: equation: indicate that n is the year? In addition, another "n" is used for the computational approach for estimating the integrals in equations 8 and 9 – I suggest to identify them differently.

We thank the referee for raising this point and we agree that the variable n has been used already in another context. Accordingly, we will choose a different notation.

Section 3: Sea level rise impacts

should it be "coastal flooding impacts" ?

We would argue that the section title is well chosen since it investigates the impacts from sea-level rise on coastal flood damage.

Page 6237, line 21: this sentence is confusing (which mean sea level and which μ is this?). In this section and in other parts of the manuscript, I suggest to distinguish water levels reached during a storm and the long term sea-level rise trend

Throughout the entire document we use μ to denote the 1-year event at a given location which needs to be distinguished from the mean sea level. Apparently, this has not become clear in the mentioned sentence. We are confident that this confusion can be resolved by adding a reference to Sec. 2.3.1 where the terminology and the notation is explained.

Page 6239 line 5: 5/7: I would be interested to read from which previous hypothesis this results comes: in practice, two storms with same water level return periods but different wind/waves directions patterns can affect different sectors in the city and cause different damages depending on the topography (I don't think that the world "orography" is appropriate here).

Our statement is based on the fact that the exponent γ plays only a minor role in the presented results. The orography as well as the distribution of assets are important factors for the damage function of a city and hence for the exponent γ. This is the argumentation of our statement. We agree with the referee that this requires additional explanation in a revised version of the manuscript. In general, the referee is right in saying that the flood damage depends not only on the flood level. As introduced in Sec. 2.4 our analyses are based on stage-damage functions which consider the flood level as the only determining factor and the resulting uncertainty is addressed on p. 6239, l. 19.

Page 6239 line 16: this is completely right. At one point, it could be useful for the reader to mention all sources of uncertainties of interest in this type of approach, including, for example, uncertainties on the parameters of the GPD functions that apply to each specific site, or uncertainties on future sea-level rise.

As any kinds of epistemic uncertainties are not in the focus of this article, we are not sure whether it makes sense to discuss them in detail. Nevertheless, we suggest to extend the list of disregarded sources of uncertainties and to provide an additional reference.

Section 4

- The sea-level scenarios are mean or median projections, but real projections also include uncertainties (see my second comment in the introduction). I was wondering if the asymptotic behavior still holds by 2050 for the A1B scenario, and/or, if the authors could advice by which value of sea-level rise one should avoid using a GDP-Poisson approach.

The question whether the asymptotic behaviour still holds by 2050 in the A1B scenario can be answered by Fig. 4(a) where the damage as well as the asymptotic approximation is illustrated for varying mean sea levels. In general, the provided asymptotic results become more precise the closer the considered parameters approach the asymptotic range (see Sec. 3). Our damage projections for rising mean sea levels are based on a "no adaptation" scenario. In practice, however, actions would be expected to be taken before very large values of sea level rise are reached. Whether this is already the case for the considered A1B scenario with 26cm of sea level rise is not clear and will probably depend on how severe the city will be struck by floods in the next decades. We appreciate the referee's comment as it points to a practically relevant issue. We propose to address this aspect at the beginning of Sec. 3 as well as in Sec. 4.

- Considering Fig. 3, it is not clear to me why the threshold 100cm was chosen in Copenhagen (?)

Thank you for making this point. As in our response to comment 6 from referee #1, a more thorough explanation for the threshold choice will be added in the revised manuscript.

- The values (in Euros or DKK) provided in figures 4 and 6 seem high to me (I am not familiar with the specific site). While it is presented in previous work, could remind how they get these figures from the damage function and discuss them?

We agree with the referee that the illustrated values appear very high. However, their magnitude is dominated by the damage functions which have been published previously. As the employed damage functions are well discussed in the given references we do not see a need to describe them here. Regarding the estimated damage magnitude, it should also be considered that the illustrated values in Fig. 4 comprise damage estimates for very extreme mean sea-levels which are clearly not realistic in the near future. Nevertheless, we think it is important to show them in order to support our results.

- The number of significant digits should be reconsidered (e.g., “xmax = 215.28 cm” should be rounded)

We agree with the referee that the chosen precision is not appropriate in all cases. This will be adjusted in the revised manuscript.

- Page 6241, line 14-15: I understand that the expected and std damages are calculated by a monte carlo approach and compared to the asymptotic equations (14 and 15). Is that the case ? Note also that the text includes Fig. and Figure – should be made consistent

The “numerically calculated” damage is not obtained by a Monte-Carlo approach but by the methodology described in Sec. 2.6. This is explicitly mentioned at the beginning of Sec. 4 but possibly requires to be made more clear at other places and will be considered in a revised version. Regarding the Fig./Figure comment, we read through the entire document but could not find any inconsistencies. As common, the abbreviation Fig. has been used whenever it is not at the beginning of a sentence.

Section 5: the effect of protection measures; Section 6: Comparison with GEV; and section 7 discussions, are interesting.

Page 6245, line 10: actually, the method enables to identify situations where the water level is higher than the coastal protections. However, often, coastal flooding occurs just because of waves overtopping. The related damages would come in addition and can be considered by none of these methods.

The referee is right in saying that wave overtopping can lead to considerable damage also for sea-levels below the protection standard. However, our framework does not cover this case. This is mentioned on page 6243, where we state that protection failures are disregarded.

Typo: page 6244 line 27: “i.e.” (little i) (same issue page 6245)

The typo error will be corrected.

Appendix B

Several points are not clear to me:

- in many places, I am not sure that the hypothesis of large ω/σ or μ would be valid under actual values for flooding protection in many places. Figure 7 seems to confirm this. For those interested in using the formulas of this article, it would be useful to add more details regarding the domain of applicability of the formula (in numerical terms).

The referee points at a crucial point of our analyses, namely the parameter range where our asymptotic results provide a reasonable approximation. In fact, this cannot be said in general terms and depends on the given case study. Consequently, the quality of convergence in the two considered regions differs significantly. In order to make this more clear, we suggest to extend the discussion about the goodness of fit of the asymptotic relations (Sec. 4). However, since the required precision of an approximation depends on personal preferences and also the purpose, it will not be possible to provide a numerical range of applicability.

- The authors chose not to mention integrals boundaries, in order to simplify the notations. However, it could facilitate the understanding of the demonstrations (especially theorem 2), since there are changes of variables and swaps of limits / integrals signs involved. I suggest also to write the exact version of the uniform convergence theorem that is invoked at least once, since it is key in the demonstrations.

During the preparation of the manuscript we had the same considerations about the integral limits as the referee. After trying both versions, we decided to skip the limits since they are not the same for different signs of ξ. That is, a differentiation between several cases and additional explanation after each equation would be required. We found that this hampers the readability significantly and are therefore convinced that the appendix has been improved by skipping them. Also, by saying that "all integrals are integrated over the whole support of the corresponding density function", all integral limits should be unequivocally be defined.

- Annex B2 explicitly presents the Taylor expansions that are used to obtain the approximate formula. Similarly, it seems to me that in annex B1, equation (B2) could incorporate the Taylor expansion of equation (3) for large sigma. In addition, adding details on how equation B3 is obtained would help the reader (not clear to me).

For the derivation of our results in Appendix B1, we do not need any Taylor expansions. We will add an additional explanation on how Eq. (B3) has been obtained.

As a general note, the Annex B is not easy to follow for readers, who, like me, have not been doing this type of maths since 10 years. I suspect that many readers of NHESS interested in this article could face similar problems in this section, so I strongly encourage the authors to make this annex as clear as possible.

We are certainly aware of the fact that the appendix is hard to read for most NHESS readers and we are of course aiming at making it understandable. However, as the appendix is not essential for understanding the main text, we plead for keeping it short and concise. We therefore suggest to work through the entire appendix and to make modifications wherever they appear appropriate.

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