The authors did a good job of addressing my concerns (Reviewer 2). I agree the novelty of this study stems from testing the sensitivity of the estimated hazard to the different data sources. I've listed a few minor comments below, in my view once these comments are addressed the manuscript will be acceptable for publication. Line numbers refer to the revised manuscript with track changes.

Thank you for your review and many helpful comments to improve our study. We hope our responses given below marked in red as well as our changes in the manuscript have helped to address these issues.

Comments

In the literature, "copula-based joint probability density function" are usually referred to as joint probability contours or isolines.

Thank you for your comment, we adjusted the manuscript accordingly.

Please clarify what the percentages stated in the abstract refer to.

Thank you for your comment, we adjusted the abstract in the manuscript accordingly (lines 20-21).

L128: Again, state the null hypothesis being tested as this will inform the reader of the correlation metric being used. The time series used to represent the river discharge also needs to be clarified.

Thank you for your comment, we adjusted the manuscript accordingly. As the correlation analysis has been explained in section 2.2 Bivariate analysis, we deleted this section to not confuse the reader.

L177: Perhaps the "model performs better .." is more appropriate than "performs best".

Thank you for your comment, we adjusted the manuscript accordingly.

L231: Thank you for adding more explanation on the terms "Weighted Average" and "Maximum Density" approach". It is however still unclear to me how the samples are generated for the two approaches. For the "Weighted Average" approach please expand "The weights are determined based on the critical joint RP." i.e., how are the weights calculated? Or is this sentence just stating how the joint probability contour is derived, if so, how do you obtain a sample from the contour. For the "most likely scenario", how is the density calculated? In other studies, such as Moftakhari et al. (2019) the probability density is estimated based on the observed sample. What do you mean by "adjusting these parameters" surely once the copula is fitted the parameters are fixed.

Thank you for your comment, we adjusted the manuscript accordingly, hopefully adding clarity to this section based on the method from Sadegh et al. (2018) in rewriting the text between lines 237 and 254 (track changes document).

L254: Add a comma after "only".

Thank you for your comment, we adjusted the manuscript accordingly.

L277: Could remove “the following”.

Thank you for your comment, we adjusted the manuscript accordingly.

L279: Consider amending “in the compound flood risk driven regardless of the situation (oceanographic or hydrological).” to “in the total flood risk driven regardless of the situation (oceanographic, hydrological or compound)/”

Thank you for your comment, we adjusted the manuscript accordingly.

L283: Please provide justification for using the rec Halmstad / E-Hype set. I assume from Table A2 it is due to the length of the two time series.

Thank you for your comment, part of this choice is already introduced and motivated in the manuscript (lines 167-171 and lines 200-204) but we adjusted the manuscript to remind the reader about this choice (lines 296-297 in the track changes document).

L286 and in Figure 5 caption: Change “both” to “the”.

Thank you for your comment, we adjusted the manuscript accordingly.

L381: “The differences between solid and dashed lines in Figure 7 are typically contained within about 20 cm sea level or 25 m3 s -1m3 /s river discharge, constituting about 10-15% of the extreme 5- and 30-year RLs for the site with a gap increasing with higher RPs.” This presumably varies greatly along the contour and is therefore subjective depending on where on the contour you choose to look. Defining the maximum distance between the copula and independence cases on rays coming from the origin is potentially a more robust approach.

Thank you for your comment, we adjusted the manuscript accordingly.

L481: “As discussed in section 2.3 and Serinaldi (2015) a careful interpretation comparing return levels from different methodologies is, however, always needed.” The term “methodology” is vague here. Serinaldi (2015) cautions against comparing results from different hazard scenarios rather than from different copulas.

Thank you for your comment, we agree with you and adjusted the manuscript accordingly replacing “methodologies” by “hazard scenarios”.

The authors have provided point-wise response to my comments. However, they have not addressed several of my comments, as pointed out earlier, and some of their responses are not satisfactory. As a result, the paper's quality remained nearly unchanged, with only minor changes to the written portion. Before accepting for final publication, the following points should be addressed and I have highlighted them again:

Thank you for your review and many helpful comments to improve our study. We hope our responses given below marked in red as well as our changes in the manuscript have helped to address these issues.

1) The paper is basically addressing compound hazards and not the 'risk' as risk is a function of hazard, exposure and vulnerability. In their analysis, the last two terms were not addressed. In Abstract and elsewhere, they should replace the word with 'hazards' and not the risk. At the same time, 'risk' here is not any indirect consequences as pointed in the Abstract.

On line 12: They may clearly state, "“The compound flood hazards from high sea levels and high river discharge are often estimated using copulas”.

Thank you for your comment, we agree with you and adjusted the manuscript accordingly.

2) Line 92: “Hybrid statistical-hydraulic/hydrodynamic modelling frameworks….

Ganguli et al. (2020), in a coupled statistical-hydrodynamic modelling framework showed projected changes in compound flood hazard is limited to 34% of the sites with a substantial role of SLR in modulating compound flood hazard.

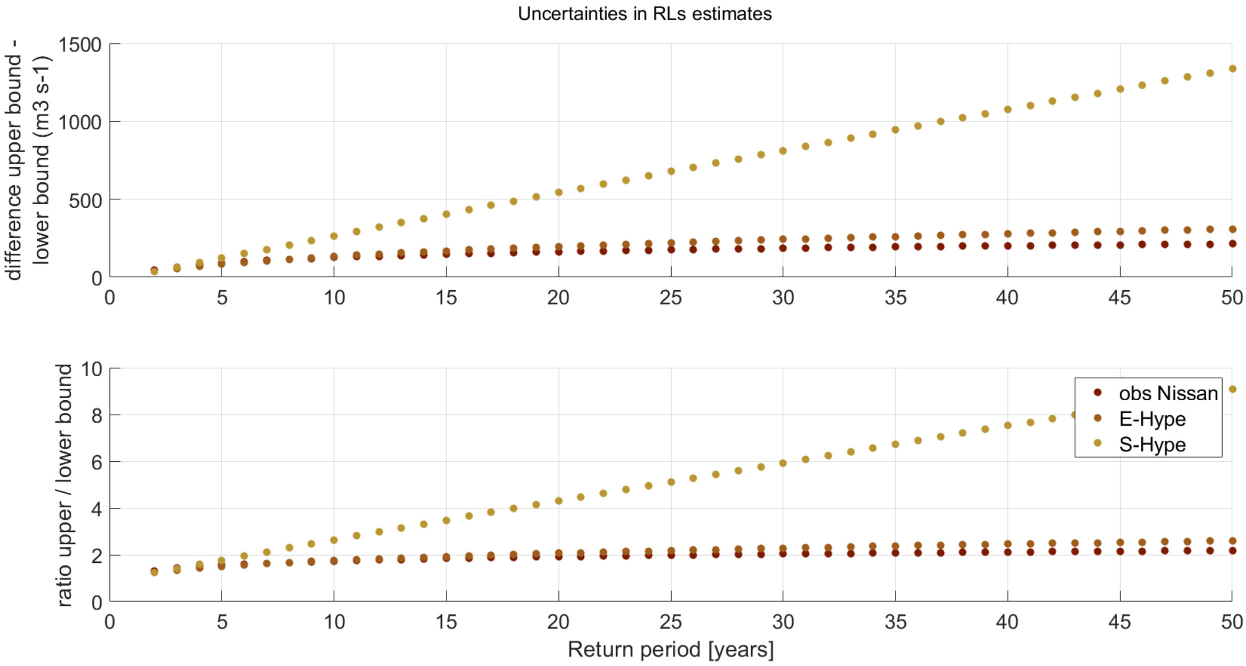
Thank you for your comment, we adjusted the manuscript accordingly and added information about this in the introduction where we think it is the most relevant (lines 66-69).

3) In their response letter, they indeed acknowledge that uncertainties are linked to available/length of datasets; however, it should be highlighted in the manuscript. Further, different sources of uncertainty, for example, re-analysis driven storm surge as well as different modelling approaches, such as S-hype and E-hype as implemented here should be clearly explained in the Discussion section.

Thank you for your comment, we added clarity (lines 479-483 and 522-526) emphasizing that uncertainties are partly linked to the available/length of datasets in addition with already included sentences on this topic (lines 120-121 / 166-169 / 462-465).

4) Although it was suggested how to estimate the uncertainty of the E-hype and S-hype models using a simple ratio approach, this part was not addressed. This is indeed not an outside scope of the research when one is estimating the probabilistic hazard of compound floods using a multivariate copula approach. This is because the uncertainty from the individual modelling components, including the univariate flood hazard estimates, are also propagated in multivariate risk estimation.

Thank you for your comment and we agree, the uncertainty from the individual modelling components are are also propagated in multivariate risk estimation. This study, actually aims to quantify those uncertainties in investigating the sensitivity of the choice of data sources within the bivariate analysis. As we can notice in the figure below looking at the uncertainties within the univariate study (the upper panel shows the difference between the 95th percentile and the 5th percentile of the confidence interval from the univariate GEV fit and the lower panel displays the ratio between both), uncertainties from the GEV analysis are larger for the S-Hype model than for the E-Hype model and, as expected are larger for higher RPs as we can also expect from looking at Figure 4b.



5) The definition of 'AND' and 'OR' return period estimates are still not revised properly: 'AND' case corresponds to when both high sea levels and river discharge exceed the respective random variables concurrently, whereas the 'OR' case indicates when either of the extremes exceeds the respective random variable with a time offset within a limited time interval. Please refer to the Requena et al. (2013).

Thank you for your comment, we adjusted the manuscript lines (258-261).

6) The organization of this paper should be revised again: I re-emphasize that the copula sensitivity, i.e., selection of copulas on joint hazard estimation can't be the crucial point and novelty of this paper. Such studies were already been shown in the literature. Further, there is already a large body of literature that focuses on the selection of the best copula models as well as the goodness-of-fit of the copulas. The focus of this study should be insight-driven and to decipher whether it is the high sea level or the high river discharge that contributes to coastal compound floods and if it is the latter, what are the uncertainty sources that potentially mediate the river flood hazards across the Nordic countries, which eventually have the potential to shape the coastal compound flood hazard.

Thank you for your comment. We agree the selection of copulas can‘t be the crucial point and novelty of this paper. However, we think, as mentioned by reviewer 2, that “the novelty of this study stems from testing the sensitivity of the estimated hazard to the different data sources.“ This comment follows our argumentation given in the last review process to reviewer 2 and, we think, has been addressed fairly clearly within this study but has been re-emphasized in the paper (lines 86-87). The correlation analysis indeed shows that high river discharge contributes to compound coastal floods over high sea level and has been shown in the manuscript. We also think that a broader spatial study over Scandinavia would be needed to address which driver between high river discharge or high sea level contributes mainly to coastal compound floods across the Nordic countries.

7) One of the main findings of this study is that river discharge dominates over coastal surge in shaping coastal compound floods, which is interesting and is true for gauges > 60°N latitude (Ganguli and Merz, 2019), where rare occurrences of compound floods is reported in the literature due to decrease in relative sea level rise across Nordic countries due to vertical crustal movement (Weisse et al., 2021).

Thank you for your comment, we agree that this is interesting and added this to the introduction where we think it fits best (lines 53-57). However, we do not think this is the core of the paper (see previous answer to comment 6). The specific site of Halmstad is located at around 56.6°N latitude where the mean sea level is expected to rise slightly even under RCP2.6 at Ringhals and Viken stations (located North and South of Halmstad respectively (Hieronymus and Kalén, 2020).

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