I only really have two comments, but they are potentially pretty major ones:

Section 2.1.1 – I'm not convinced by the accuracy of the sea level data set, since the observed and reanalysis extremes do not correspond well with each other and then the reconstructed data is trained on these contrasting datasets. There are many sites globally where a longer accurate observational sea level record exists, so why not chose a different site?

The idea proposed here is to get as local as possible and extract data from a longer available time series from another station would lead us to not so local sea level conditions. That is why we carried out a reconstructed time series. The reconstructed data is only based on observations, the reanalysis data has known issues that makes it difficult to work with. However, when it comes to carrying this sensitivity analysis, we consider it is not a major issue as the sea level data does not seem to have a strong impact on the copula results and even more strengthens our conclusion that, for this case, input hydrological data influences the results the most. Here, Halmstad has been chosen as the site is potentially prone to compound events on the Swedish coast where the highest sea level has been recorded so far (lines. 65 to 68). In addition, the Swedish west coast has in previous work been found to be an important area to study (Andersson, 2021; Hieronymus and Kalen, 2020) due to its multiple aspects causing risk for flooding. To help guiding and communicating with the local municipalities about their continued work to protect coastal areas from flooding we considered it useful to pick one site in this area as an example to showcase the applied methods and their results. Halmstad was then further decided upon because it has the highest observed sea level of all the Swedish measurement stations. We do not expect significantly different results from other sites in this area. We understand the reviewer's comment that other sites globally on completely different geographic areas could also be studied, but this would not necessarily be informative for the local focus area and we will clarify this aspect and motivate our choice of study area more clearly in the revised manuscript (lines 62 to 73).

In the end, you produce a 44 year record of sea level variability. Despite this being a long data set, you only select the annual extremes for the analysis - why do this when in effect this reduces this large data set down to only 44 (suspect) data points. Since the purpose is to assess joint probabilities, this could be done on a larger subset of extremes, e.g. >99th percentile peaks.

We decided to only use the annual extremes for the analysis which is a common approach in the literature. However, we also carried out a really brief analysis on defining extreme events as sea level above the 95th percentile value and another test in using the threshold value of the 99th percentile. This brief analysis did not seem to make any difference in our conclusions as also found in Ward et al., 2018; but a more extended sensitivity analysis could be, we think, highly relevant. However, we believe this is outside the scope of this study. We also refer to this point in the section 4. Limitations.

Ward, P. J., Couasnon, A., Eilander, D., Haigh, I. D., Hendry, A., Muis, S., Veldkamp, T. I. E., Winsemius, H. C., and Wahl, T. (2018) Dependence between high sea-level and high river discharge increases flood hazard in global deltas and estuaries, Environmental Research Letters, 13(8), 084012. 10.1088/1748-

9326/aad400.Andersson, M.: Climate Adaptation by Managed Realignment. Future mean and extreme sea levels, SMHI, Report number: 2021/912/9.5, 16–17, 2021.

Hieronymus, M. and Kalén, O.: Sea-level rise projections for Sweden based on the new IPCC special report: The ocean and cryosphere in a changing climate, Ambio, 49, 1587–1600, https://doi.org/10.1007/s13280-019-01313-8, 2020.