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Comment on nhess-2021-410

Anonymous Referee #2

Referee comment on "Bayesian hierarchical modeling of sea level extremes in the Finnish coastal region" by Olle Rätty et al., Nat. Hazards Earth Syst. Sci. Discuss.,
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The future "climate" of water levels is one of the core problems for low-lying areas. The manuscript addresses this problem by means of advanced statistical modeling of parameters of extreme value distributions for future water levels and a sort of ensemble projection of extreme water levels and their return periods.

The analysis is theoretically sound, relies on high-quality data sets, has been performed professionally and leads to an interesting set of results. The presentation is clear and well structured, uses correct English and brings enough details for understanding the material.

General comments:

I am thus generally happy to recommend the manuscript for publication.

Before sending to print, however, I recommend to expand the presentation a little bit to cover some aspects that may mislead inexperienced readers and to make a few adjustments that would make the interpretation more exact and the message clearer. The recommended changes and additions only address single wording features and interpretation aspects (most of which are technically acceptable as presented in the manuscript) and do not involve any large changes to the presentation.

A potential trap for some readers may be the interpretation of the limited set of arguments of the Weibull distribution. Even though the authors mention on lines 307–308 that the shown values [of the upper threshold for the argument of the reverse 3D Weibull distribution] should not be interpreted as actual limits for the sea level, I would recommend commenting the related aspects in more detail to make the situation clear. There are two aspects worth of mentioning.

Firstly, the limited region of validity of the 3-parameter Weibull distribution could be interpreted differently. On the one hand, there is a temptation to think that this distribution provides the final truth about some properties of the described processes. On the other hand, the existence of this kind of threshold is not really physical and could be interpreted as showing that the entire GEV approach loses its validity near and behind this threshold.

Secondly, the set of block maxima may contain elements of different water level "populations" of the Baltic Sea. The reason is the well-known property of the Baltic Sea: its water volume may increase or decrease considerably for several weeks by water exchange through the Danish straits. The "population" of the background water level of the Baltic Sea roughly follows a Gaussian distribution whereas the local storm-driven surges roughly follow an exponential distribution [Soomere, T., Eelsalu, M., Kurkin, A., Rybin, A., 2015. Separation of the Baltic Sea water level into daily and multi-weekly components. *Continental Shelf Research*, 103, 23–32, doi: 10.1016/j.csr.2015.04.018]. It may thus easily happen that the block (annual) maxima do not necessarily come from the same distribution. In this case the GEV distribution is just a passable approximation of the distribution of the block maxima and nothing more. It may easily be that the large scatter of the threshold of question is a reflection of this feature.

In this sense it is better to remove the conjecture "From theoretical perspective, this suggests that there might be an upper limit that the sea level extremes can reach along the Finnish coast" on lines 369–370 from the manuscript and also to modify the sentence "This also suggests that the hierarchical models can be used to estimate theoretical upper limits of the extremes of short-term sea level variations along the Finnish coast" to make sure that the unexperienced readers are not misled.

Specific comments:

The Abstract seems too long, e.g., the sentence on lines 3–5 could be removed without any loss to the message and the material on lines 11–13 could be made more compact and smooth.

Line 21: probably "associated WITH" or similar.

Lines 23–24: even though the increase in the mean sea level has exceeded the global average during the past 50 years in the Baltic Sea in many locations, there are opposite examples, e.g., the sea level on the Latvian shores [Männikus, R., Soomere, T., Viška, M. 2020. Variations in the mean, seasonal and extreme water level on the Latvian coast, the eastern Baltic Sea, during 1961–2018. *Estuarine Coastal and Shelf Science*, 245, Art. No. 106827, <https://doi.org/10.1016/j.ecss.2020.106827>]. This feature very shortly reflected in (Weisse et al., 2021) and may easily be overlooked. Also, it seems to have local

character.

Line 33: it is recommended to insert a reference to the analysis of meteotsunamis in the study area even though such a reference appears later.

Lines 38–39: while piling up water in the ends of the Bay of Bothnia and Gulf of Finland for sure is one of the main reasons for very high water level in these locations, the role of piling and emptying the entire subbasin is probably minor there compared to harbor-type oscillations. See, for, example [Jonsson, B., Döös, K., Nycander, J., Lundberg, P. 2008. Standing waves in the Gulf of Finland and their relationship to the basin-wide Baltic seiches. *Journal of Geophysical Research-Oceans*, 113 (C3), C03004, doi: 10.1029/2006JC003862]. Still, this effect seems to be a decisive one in some other basins, such as the Gulf of Riga [Männikus, R., Soomere, T., Kudryavtseva, N. 2019. Identification of mechanisms that drive water level extremes from in situ measurements in the Gulf of Riga during 1961–2017. *Continental Shelf Research*, 182, 22–36, doi: 10.1016/j.csr.2019.05.014.].

Line 61: “However, they did not consider spatial dependencies explicitly in their analysis” is ambiguous and is better to be removed.

Line 80: consider replacing “extends” by “applies”.

Line 116: “which should reduce the correlation between the annual maximum values.” is of course correct but this operation most likely almost totally removes this correlation.

Line 139: What is the meaning of the plus sign at the end of square brackets?

Line 141: consider replacing “y has bounded upper tail” (that is mathematically nonsense for an argument) by perhaps a longer explanation that the GEV distribution function is only defined until a specific value of y which is often associated with the theoretical maximum or minimum value of the process under consideration.

Line 173 and in several locations below: the simple use of “Common” (or similar) makes reading fairly complicated. Consider using “The COMM version/approach/ model” etc., e.g., as on line 246.

Line 199: Do you have a specific reason for using norm when evaluating the expression in square brackets?

Line 234: consider treating "elpdLOO" as a variable, e.g., $\$elpd_{\{LOO\}}\$$ unless you have reasons for using the text mode. Anyway, unify the use of $\$P_{\{LOO\}}\$$ in Table 1 and as text on line 243.

Line 248: probably "location and scale PARAMETERS" are meant.

Line 253: it is not recommended to start the sentence from a symbol or expression.

Line 267: consider expanding the expression "Weibull-type distribution" towards explanation that the GEV approach uses so-called reversed 3-parameter Weibull distribution (and not, e.g., the 2-parameter Weibull distribution that is common in the description of wind speed, wave heights, etc.). Just to make clear the scene for the reader.

Line 269–270: "they used ocean model output instead of observations in their analysis" is only partially true. They also used measured data from five locations and noted strong variations in the shape parameter depending on both the particular location and the method for evaluation of the parameters of the GEV distribution.

Line 276: "separate fits": see comment to line 173.

Section 6 Conclusions: it is recommended to remove the short names of scenarios from the text in order to make the section readable on its own.

References:

Coles 2001/2004 is missing from the list