

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/nhess-2021-410-RC1>, 2022
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Comment on nhess-2021-410

Anonymous Referee #1

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.,
<https://doi.org/10.5194/nhess-2021-410-RC1>, 2022

This paper presents a spatial Bayesian hierarchical model of sea-level extremes and uses it to analyse tide gauge observations along the Finnish coastline. Estimates of extreme sea-level event probabilities, which typically are expressed in terms of return levels, are crucial to flood risk quantification. However, such estimates are often subject to large uncertainty owing to issues related to the small sample sizes and large data dispersion typical of tide gauge observations. Furthermore, when using traditional single-site approaches, estimates of event probabilities are only possible at gauged locations. These issues can be partly overcome by exploiting spatial dependencies in extreme sea levels, or simply by pooling information across data sites, which leads to estimates of return levels with reduced uncertainty and allows for estimation at unobserved locations. Despite the advantages of spatial modelling, most studies of sea-level extremes to date have analysed extremes on a site-by-site basis. In this regard, this paper represents a valuable contribution to the literature on sea-level extremes. The paper shows that pooling information across space leads to more robust estimates of event probabilities, though in this study all tide gauge records are relatively long and as a result the single-site model ('Separate') is still able to estimate the GEV parameters with high confidence. The benefits of spatial modelling are much larger in regions with short tide gauge records, and this should be more strongly emphasized in the paper. The paper is well written, the methods are valid, and overall the results are interesting. I do not have any major objections to the paper, but I do have some comments and suggestions, as outlined below, that would like to see addressed before the paper is published in NHESS.

General comments:

- One of the motivations for using spatial modelling is the ability to make estimates at ungauged locations. However, other than in Fig. 2, the paper focuses on estimates at gauged sites and does not sufficiently assess the skill of the Bayesian models at

ungauged sites. I would suggest the authors perform an experiment in which they leave one tide gauge out at a time, estimate the GEV parameters at the omitted site, and then compare the result with estimates based on all the data. I would also suggest the authors include a map of gridded estimates of 50-year return levels along the Finish coastline.

- Another motivation for using a spatial model is the reduction in estimation uncertainty. I would suggest the authors quantify and discuss this reduction in more detail. By which factor is the uncertainty reduced? Figures 6 and 7 already provide a visual indication, but I think the discussion should be more quantitative. Perhaps a figure or a table showing posterior standard deviations for the 50-year return levels is all that is needed.
- I think that authors should perform an analysis of sensitivity to prior choices, especially for the parameters defining the spline and GP models. It is well known that the GP parameters (standard deviation and length scale) are challenging to estimate. Also, please explain how and why these priors were chosen.
- please show the posterior estimates (with uncertainty estimates) for all the scalar parameters (and hyperparameters) of the model, either as a plot or a table.

Specific comments:

Extraction of annual maxima. Was the tidal component removed prior to extracting the annual maxima from the tide gauge records?

Equation 7. The Greek letters used to denote the GP standard deviation and length scale are different between the article and the Supplementary Information.

It is unclear to me what the authors mean by 'empirical estimates'. The estimates from the Bayesian hierarchical models are conditional on the observations, so they are 'empirical' too, aren't they?

Please add either posterior SDs or credible intervals to Table 2.

Discussion: Line ~335. While I agree that it should be emphasized that to quantify flood risk one should include mean sea level changes, I do not think that excluding mean sea level influences is a limitation of your study, rather it is a choice to focus on the storm surge component of sea level. The actual limitation is to assume stationarity, but this is discussed in the next paragraph.

Discussion. Another limitation that is not mentioned is that the Bayesian hierarchical models used in this study assume conditional independence in the likelihood. In other words, they assume that, after accounting for dependence in the marginal GEV parameter, the annual maxima are independent across stations. However, this assumption is unlikely

to hold because the stations are geographically close and thus they are going to be affected by the same extreme events, which means that the time series of annual maxima are going to be correlated between stations (what is called 'residual dependence'). Ignoring residual dependence means that your uncertainty estimates are narrower than they should be (probably only slightly), but other than that it should not significantly affect the estimates presented in the paper. This limitation should be discussed. Calafat and Marcos (2020) provide a way for addressing residual dependence, but I recognize that this is beyond the scope of this paper.