

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC3  
<https://doi.org/10.5194/nhess-2021-406-RC3>, 2022  
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## **Comment on nhess-2021-406**

Anonymous Referee #3

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Referee comment on "Incorporating historical information to improve extreme sea level estimates" by Leigh R. MacPherson et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-406-RC3>, 2022

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The manuscript by MacPherson and co-authors addresses the problem of extreme sea level assessment and its improvement using historical information. They propose a new procedure that relies on the stochastic simulations of artificial extremes to model the historical events. They explore the added value of their approach using Travemünde, Germany as a test case and show that the estimation of 200-year return water level (denoted HW200) is larger than the current official value that is determined using systematic data. This suggests a possible underestimation of the current design water level for the flood defense.

### **Main comment**

The manuscript is well organized and the presentation of the methods and results are very clear. The conclusions are sound. I very much appreciate the efforts made by the authors to discuss their results with respect to existing studies (Sect. 3) and to the implications of their work (Sect. 4). However a few aspects should be clarified and further elaborated before publication. Therefore, I recommend additional corrections by incorporating, if possible, the following recommendations.

#### **1. Comparison between the proposed approach and competitors**

1.1. The authors very well describe in Sect. 2.2 the three major methods available to incorporate historical data. What confuses me is that in the extensive comparison exercise in Sect. 3.3 only MLA is used for comparison. To reach sound conclusions, the other two approaches should be included as well. Besides, the reference DWA (2012) is in German; is there any document in English to read more about these methods?

1.2. The goodness of fit of the proposed approach is assessed via the Bayesian Information Criterion. Though I believe this is an appropriate approach, other complementary criteria could have been envisaged. For instance the residual quantile and probability plot on Gumbel scale would be informative as well; see e.g. Méndez et al. (2007) for an example of application at San Francisco. By doing so, the following limitation (1.3) could be overcome.

1.3. It is true that lower BIC between the proposed approach and MLA indicates a better fit of the proposed approach. However, this is significant provided the difference remains sufficiently large. For instance, Raftery (1995) provide the following guidelines: a difference greater than 10 indicates very strong evidence; 6–10 indicates strong evidence; 2–6 indicates positive evidence; and 0–2 indicates weak evidence for the more complex model. See also Burnham and Anderson (2004) for a discussion regarding AIC difference. The validity of the results in Fig. 6 should be re-assessed on this basis.

## **2) Uncertainty estimate**

2.1. The authors mention the use of two measurements from nearby Lübeck (line 198, Sect. 3.1). How far are they located from the site of interest? Is there any difference in sea level from both sites, and if so how to account for this uncertainty? More broadly, recent studies (Calafat and Marcos, 2020; Frau et al., 2018) have proposed flexible framework to incorporate the spatial dimension as well. Could the authors consider such studies to enhance the outlook section?

2.2 In Sect. 3.1 (page 11), the authors propose a bootstrap-based approach to account for the uncertainties in the systematic data. As far as I understand, the authors use the bootstrap-based quantile estimates of the GPd to derive the confidence intervals depicted in Fig. 5. Do the authors consider the dependence between the scale and shape error for this calculation? If not, the confidence interval may here be biased.

## **3) Non stationary**

3.1. One major assumption of the proposed method is the stationary of the data. What confuses me is that on the one hand, the authors clearly underline the difficulty of confirmation this assumption (line 450), but on the other hand, section 4.3 seems to indicate that there is not real barrier for this implementation. Could the authors add a comment on that?

3.2. In line 90, the authors indicate that they use a detrending approach. Does it mean that the authors use a linear model to remove the inter-annual signal? Could the authors comment on the use of more advanced methods based on the removal of MSL signal calculated from tide gauges, for instance using data from PSML (<https://www.psml.org/>)

and some statistical techniques like Kalman filter (e.g. Visser et al., 2015)?

3.3. As far as I understand all sea levels are provided with respect to MSL. To ease the comparison throughout the paper, could the authors also provide the 'official' value HW200 with respect MSL (in particular to compare with Table 1)?

### **Minor comments**

- Equations (4) and (5): the terms in the last product seems not to be on the same line than the remaining of the terms.
- Figure 4's caption: 'GPD' should be 'GPd'

### **References:**

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