Author response to reviewer comments

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We thank Ilker Fer and both referees, Tarmo Soomere and one anonymous reviewer, again for their comments and time. Our responses are given below.

1 Response to Ilker Fer

- Both reviewers are satisfied with your revision in the previous round and recommend minor clarifications. In addition to addressing their comments, please also include an estimate of the accuracy/uncertainty of the observational data in the relevant section.

Reponse We have added following sentences to Section: "2.2 Observational Gauge Data": "From both sources we have obtained quality controlled sea level data of hourly frequency. The sea level data is accurate to within one centimetre."

2 Responses to Tarmo Soomere

- The authors have taken into account almost all my suggestions, except for adjusting or more exactly specifying what is meant under "default". Thus, I am basically happy with the result and suggest the manuscript for publication, with a few very minor technical adjustments.

- Lines 52-54: it is of course true that "In general, sea-level rise shifts the ESLs (relative to present-day mean sea level) to higher base levels, and thus high ESLs become more frequent (Wahl et al., 2017)." However, it might be mentioned (for less informed readers) that projections of ESL events based, e.g., on generalized extreme value distributions contain the mean sea level as a linear term (e.g., Coles et al., 2001) and thus the frequency of ESL with respect to the new mean will not change. This remark gives inter alia an additional justification of the assumption on line 112: "Furthermore, the mean sea level is kept constant in order to study only the atmospheric-induced extreme sea levels themselves" and also for the procedure of de-meaning on lines 121-122.

Response You are right. We have slightly changed the sentence in the introduction to "In general, sea-level rise shifts the ESLs (relative to present-day mean sea level) to higher base levels, and thus high ESLs become more frequent, although surge events themselves may not have changed their frequencies (Wahl et al., 2017)."

For the sentence in the Methods section we have changed it to: "Furthermore, the mean sea level is kept constant in order to study only the atmospheric-induced extreme sea levels themselves (ESL distributions treat the mean sea level as a linear term Coles et al. (2001))."

- It seems important to me adjust one detail: It is strongly recommended to define around Line 120 (that says "default wind speeds") that default wind speed is the one that is taken directly from the atmospheric models without any adjustment; just to avoid misinterpretation.

Response This is a very good point to avoid confusion. We changed the sentence to: "For each atmospheric forcing, we ran one simulation with default wind speeds (directly taking the values provided by the atmospheric dataset) and one with increased wind speeds, see Tab. 1, for a total of 13 ensemble members."

- Line 170: should be Fig. 3.

Response Fixed.

- Lines 283-284: "These two (and more) compounding mechanisms follow different statistical distributions (e.g. Suursaar and Soor, 2007)." It is indeed true that Suursaar and Soor mentioned that the set of sea level values at Prnu "cannot be fitted by any single reasonable theoretical distribution" and that "the distribution /—/ probably consists of several distributions due to mixed populations." These distributions were extracted for the eastern Baltic proper and Gulf of Finland in [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, 2332, doi: 10.1016/j.csr.2015.04.018].

Response We have added to the sentence: "These two (and more) compounding mechanisms follow different statistical distributions (e.g. Suursaar and Sooäär, 2007) which can be extracted by separating the time series into different temporal components (Soomere et al., 2015)."

- Line 408 Nature communications should be capitalized

- Line 421: I guess it should be 208 pages

- Line 445: Grinsted, A.: is part of BACC II.

Response The bibtex entry includes BACC II as the book title, but the reference style does not show it in the pdf. I think the type setter will fix this.

- Line 475: this is from Special Issue No 85

- Line 478-479: Kärnët al. is incomplete

Response These are all the information that can be obtained from arXiv

- Line 480: Climate dynamics should be capitalized

- Line 501: Climate research should be capitalized

- Line 508: Peltier, 2004: journal title etc. is missing

- Line 531: Boundary-layer meteorology should be capitalized

- Line 539: Scientific reports should be capitalized

- Line 545: Nature communications should be capitalized

- Line 554: volume and page numbers are missing

Response Fixed the references

3   Reponses to reviewer 2

The authors have addressed my comments in the revised manuscript. I only have a few minor remarks before the paper can be recommended for publication.

- Line 173: For the ESL comparison, ... I would prefer to have the ESL definition already in the methods section 2 (or at least before presenting the results in Fig. 3).

Response We have moved it to Section "2.2 Observational Gauge Data" and have slightly rephrased the sentence: "For the direct comparison of ESL events with observations (Section 3.1), we use the German Federal Maritime and Hydrographic Agency's definition of a storm surge in the Baltic Sea. It defines a storm surge as a sea level of more than 1 metre above mean sea level."

- Line 362: [...] Therefore, this inverse method cannot be expected to provide a one-size-fits-all solution. Arguably the existing bulk wind stress formulae are indeed a generalisable "one-size-fits-all" solution. They have been calibrated using a realistic ocean simulation and a sufficiently large wind forcing data set. This is, in fact, a form of inverse modeling; The only difference is that the tuning is (presumably) done manually. Therefore, there is no fundamental reason why the adjoint method could not be used in the same fashion to provide generalisable wind stress formulae. It all depends on how the inverse model is configured, the extend and duration of the simulation, cost function etc. The above statement, therefore, is not quite valid and I would remove it.

Response You are right. We have deleted the sentence.

References

Coles, S., Bawa, J., Trenner, L., Dorazio, P., 2001. An introduction to statistical modeling of extreme values. Springer.

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. URL `https://www.sciencedirect.com/science/article/pii/S0278434315001077`

Suursaar, Ü., Sooäär, J., 2007. Decadal variations in mean and extreme sea level values along the Estonian coast of the Baltic Sea. Tellus A: Dynamic Meteorology and Oceanography 59 (2), 249–260.

Wahl, T., Haigh, I. D., Nicholls, R. J., Arns, A., Dangendorf, S., Hinkel, J., Slangen, A. B., 2017. Understanding extreme sea levels for broad-scale coastal impact and adaptation analysis. Nature Communications 8 (1), 1–12.