The long-term variability of extreme sea levels in the German Bight

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Authors' response

RC2 Review by Anonymous Referee #2

We thank Anonymous Referee #2 for the helpful comments. Responses to the individual comments are listed below. Page and line numbers in the responses refer to the updated version of the manuscript; changes therein are marked in red.

1. Comment by Referee:
   My biggest objection to the manuscript is the structuring, where I think the supplementary material is much to extensive. Parts of it like the comparison to observations would be better put in the text, and other parts, I think, could be dispensed of. Specifically I think figs. A2,A3,A7 would be fit better as part of the article. Figs A1,A9 and A10 I believe did not add much and could likely be dispensed of. However, I think if would be nice to add quantile plots of model vs observations for different 100 year periods, too have a more direct comparison to observations

   Response:
   We agree with the referee that the supplementary material is very extensive. We therefore decided to follow the referee's suggestion of leaving out two of the specified figures (A1 & A9). However, we believe that the other figures give useful additional information to the manuscript and should therefore be kept in the supplementary material. To not disturb the reader's flow with additional, yet not vital material for the analysis, we prefer not to move any more figures into the main text, since the main focus of the manuscript is the understanding of long-term variability of ESL rather than the evaluation of model results against the observational record.
   Yet, we did add the suggested quantile plot of model vs observation for each century (Fig. 4, see comment 4).

2. Comment by Referee:
   The 16 m minimum depth. This is discussed a bit, but not in relation to the representation of bottom topography. Being myself unfamiliar with the German Bight, I don't know if representation of complex bottom topography is a problem in the area. However, I think that it should be stated in the article if this is a potential problem or if the bottom is essentially flat.

   Response:
   We apologize, the expression '16 m minimum water depth' is imprecise and probably misleading. It rather refers to the uppermost layer thickness (see page 5, line 11), the effective water depth is ultimately dependent on tide, surge etc. and can thus result in lower water depths in the German Bight. This layer thickness is needed in order to prevent grid-points to become 'dry', which is not permitted in a climate model.
   Statements on page 9, line 7 and on page 20, line 27 have been rephrased. Further, a clarifying statement has been included in the Methods (page 5 line 9f):

‘In order to prevent ocean grid-points to fall dry due to strong tidal sea level variations, as for example in the English channel, MPIOM's uppermost layer thickness is set to 16 meter.’
This means, however, that rather minimum than maximum sea levels are subject to this feature, as the tidal coastline changes in the Wadden Sea are not represented in the model. At the main study location Cuxhaven, the sea is deeper, which increases our confidence in the results at this location compared to other points along the Wadden Sea. Besides the shallowness of the shelf, the bottom topography in the German Bight is rather smooth (see also Fig. 1) though. During surge events, the sea level rises rather uniform in the region, and the influence of bottom topography plays a subordinate role compared to the rather complex horizontal coastline geometry. A sentence about this has been added in the discussion (page 20, line 29f):
‘At Cuxhaven, this effect [lower wind surges due to simplifications in the model bathymetry] should be smaller than at points along the flatter Wadden Sea where the tidal oscillations and the shallow waters lead to coastline changes which cannot be represented here. The influence of the bottom topography is expected to play a subordinate role compared to the rather complex horizontal coastline geometry.’

3. Comment by Referee:
Sect. 3 how mean high water is defined should be in the paper.

   Response:
   A definition has been included (time-mean over tidal maximum values; p. 8, line 6)

4. Comment by Referee:
Sect. 3.1 row 25 comparison to observations. It is stated that ESL relative to MHW compare well to observations, but to me it seems that ESL is likely somewhat overestimated. I think the suggested quantile plots would be helpful here.

   Response:
   The main point is that one realization (i.e. the observation-based record) lies within the range of simulated ESL, thus “compares well”. The added quantile-quantile plot (new Fig. 4, see Comment 1) shows that for most sea levels, observed and simulated ESL agree well, and that the bulk of the disagreement between both is related to the very upper end of the ESL distribution, which is subject to substantial variability.

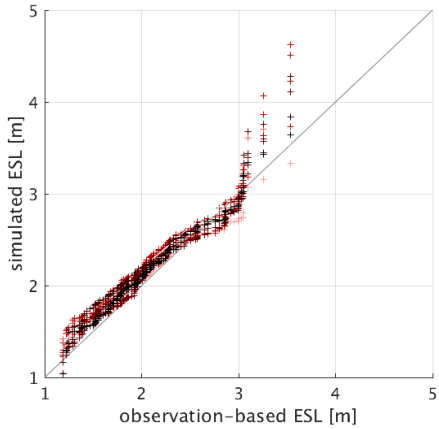


Figure 4: Quantile-quantile plot of 100-year segments of simulated ESL at Cuxhaven against the 100-year long ESL from the tide gauge record. Colors following the gradient from light red to black represent ascending 100-year segments from 1000-2000.

5. Comment by Referee:
Fig 4. I was first confused over how the return levels were estimated here. I think it should be stated in the caption that they are non-parametric, and I also suggest that you add a second panel showing there parametric counterparts for the same time period. Also a non-parametric return level curve for the whole data set would be interesting.

   Response:
   The specification 'non-parametric' has been added in the Figure caption (now Figure 5). Additionally, a short description about how they were inferred has been added in the method section (p. 7, lines 9f.). However, we decided not to show the full parametric counterparts due to length reasons; the corresponding parametric 1000-year return level estimates are already shown in the bars on the right. The non-parametric 1000-year return level is also indicated in the figure, a curve for the whole data set though is also left out for length reasons.

6. Comment by Referee:
Page 10 just before Sect. 3.2, it is stated that other grid points are similar to Cuxhaven. I don't dispute this, but the figure shows a mean of the other gridpoints, and says noting about the spread, so I would rephrase.

   Response:
   We modified the figure which now includes the range of other gridpoints along the German Bight, and thus shows the spread rather than the mean only.

7. Comment by Referee:
What about the correlation of the running mean series from Fig 4. It looks like it might be higher?

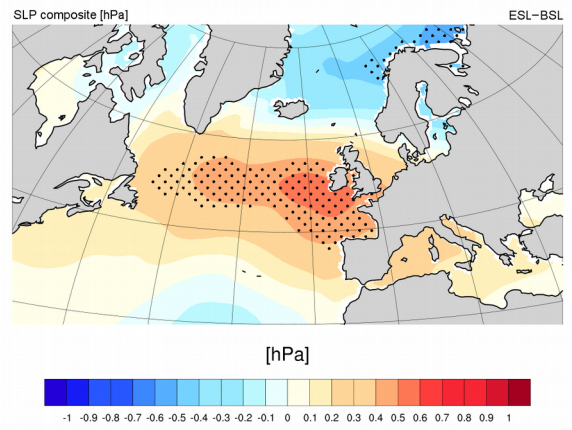
   Response:
   This only appears to be higher. We double-checked and got the same value.

8. Comment by Referee:
Fig. 9 and all wavelet figures. Please state which series leads which for a given arrow direction, not just the first series.

   Response:
   The respective captions have been adjusted.

9. Comment by Referee:
Sect. 3.4. I think you could develop the last thoughts in this paragraph more, and also show the SLP pattern when BSL has been removed as an additional panel in Fig. 8

   Response:
   We added a couple of lines to this paragraph (p. 17, lines 13f.). For length and readability reasons, however, we prefer not to explicitly show the Figure in the mauscript. Due to the high fluctuations in ESL, the corresponding SLP pattern also shows quite a large variance, which manifests in changes in spatial extent and strength of individual years of the composite. Due to this large variability, the SLP pattern where BSL has been removed is not identical to the one associated with high ESL. However, the spatial character with a tendency for a rather clockwise spinned dipole remain similar (see Fig. below).



SLP composite over times of enhanced ESL after the reduction of BSL

Lines added (p. 17, lines 13f.):
"[...] the large-scale circulation pattern associated with high ESL qualitatively persists if annual median is subtracted, although weaker. The comparably large spatial variance in the ESL pattern, however, leads to slight shifts in the location of the centers of action of the corresponding SLP dipole; yet, both ESL and ESL-BSL share a tendency towards a less meridional character of the associated SLP patterns whose centers turn clockwise compared to the BSL-related SLP anomaly. This similarity emphasizes that the SLP pattern associated with high ESL is linked to the surge residual variations. BSL variations are of much smaller amplitude than ESL variations and thus become marginal amid the strong variability of the latter."

10. Comment by Referee:
    Page 19 around line 30. You mention the lack of melting ice-sheets, but not the thermosteric effect. I assume your model is Boussinesq so you don't have this either?

    Response:
    Yes, it is Boussinesq. Even though the thermosteric effect is calculated by the model prognostics and could in theory be added linearly to the sea level time series, this is problematic as the global model exhibits considerable drift; with the effect from the deep ocean, the 100-year spin-up used in our setup is too short to account for this. We therefore rather focus on changes in dynamics.
    A clarifying sentence has been added in the methods (p. 6, line 1-2), and we now included the omission of the thermosteric effect also in the discussion (p. 21, line 5).

All technical corrections have been incorporated.