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

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Authors' response

RC1 Review by Anonymous Referee #1

We thank Anonymous Referee #1 for the helpful comments. The individual comments are addressed 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:
   'Extreme sea levels particularly arise when these components are in superimposition' Superposition.

   Response:
   s*uperimposition* has been changed to s*uperposition* (p.2, line 23)

2. Comment by Referee:
   'Yet, a comparison in terms of extreme value statistics is possible. considering storm flood statistics, we compare the simulated ESL with observations from..'
   I think the authors mean 'meaningful' rather than possible. A comparison is always possible, but it may be conceptually wrong.

   Response:
   The corresponding sentence has been clarified (p.7, line 26f.)

3. Comment by Referee:
   The return values at Cuxhaven derived from observations seem to be biased low. The authors write '...rather underrepresented, pointing to a bias towards too zonal (westerly) winds'.
   I am not sure whether this points to a bias. The estimations from the simulated 100-year segments cover the observations-derived value. As we only have one observations-derived value we cannot assert , I think, that the (theoretical) distribution of observational values is biased relative to the distribution of modelled values. I would rather write that the observational value is at the lower x-quantile of the model-derived distribution.

   Response:
   We do not claim that the return values at Cuxhaven are biased low, they indeed lie within the simulated spread. However, the sentence the referee is referring to concerns sites along the coastline of Lower Saxony (see Fig. 6 (former Fig. 5)), where the simulated return levels are – other than at Cuxhaven – *lower* than the observation-based return levels. We agree though that the word 'bias' is a bit far fetched as we indeed do not know the theoretical distribution of observational values based on one single observation-derived time series. Therefore, the statement about a possible bias has been rephrased (p. 9, line 5f.):
   'Yet, while the return values at Cuxhaven lie slightly higher than the observed ones, ESL along the coastline of Lower Saxony and the Netherlands are rather low compared to the observation-based estimates.'

4. <u>Comment by Referee:</u>
   Figure 4 includes a label 'observed'. However, these time series are not observed per se, they are derived from observations, and these derivations can be obtained with different estimation methods, e.g. POT or GEV. I would use 'observations-based' or 'derived from observations'.

   <u>Response:</u>
   The label has been changed accordingly. Note that due to a newly added Fig. 4, the mentioned Figure is now Fig. 5.

5. <u>Comment by Referee:</u>
   '*In agreement with observational studies (Gerber et al., 2016), simulated storm floods at Cuxhaven stem from predominantly west-north-westerly directions, while their associated daily pressure anomaly patterns are similar to observations of storm flood weather situations (Donat et al., 2010; Dangendorf et al., 2014c)'.*
   It would be helpful for the reader to include here either a reference to Figure 8 or, if the authors do not wish to disturb the ordering of in-text figure citations, to mention that this SLP pattern will be shown later. Otherwise, the reader may get stuck here wondering if the paper will show it or not.

   <u>Response:</u>
   Fig. 8 (now Fig. 9) does not show a daily pressure anomaly pattern, but rather a composite for periods of enhanced ESL based on the 3-year low-pass filtered ESL time-series (see p.13, line 30). A direct reference to the Figure would be misleading at this point.
   We decided against showing a corresponding figure about the *daily* anomaly pattern for length and readability reasons. However, an explicit statement of this has been added (p. 9, line 14).

6. <u>Comment by Referee:</u>
   '*The correlation between BSL and ESL is comparably low (r = 0.35) and highly variable over time (see black curve in Fig. 6 for a 100 year running correlation), while the different magnitudes of variances lead to a low explained variance.'*
   Here, it would be interesting to show the correlation between the indices describing the intensity of the SLP patterns that are linked to BSL and ESL (shown in Figure 8), and maybe also mention the correlation of both to the NAO index. Thus, it would become even more clear that, as I expect, the SLP pattern behind ESL is indeed different from the NAO, which in itself would be (the confirmation of) a quite important result.

   <u>Response:</u>
   A couple of sentences about the correlation between the SLP patterns behind ESL and BSL with the NAO have been added (p. 14, line 25f.): *"Compared to the pattern associated with high BSL (correlation with NAO of r = 0.9), this SLP pattern has a lower correlation with the NAO (r = 0.67). As a result, the ESL time series at Cuxhaven as well shows a weaker correlation with the NAO (r = 0.19) than with the newly defined SLP pattern (r = 0.31)."*

7. <u>Comment by Referee:</u>
   '....trends between ESL and BSL during the last century that have often been described (Kauker and Langenberg, 2000; Menéndez and Woodworth, 2010) might merely be an unusual state if compared to a longer time horizon as obtained from our long-term simulation.'
   This is perhaps my more substantial comment. The 20th century observations, according to this paragraph, show that there is a link between background sea-level and extreme sea-level in that century. It is not clear to me whether the model run also shows this link, and it

is also not clear to me which mechanisms may explain this link in reality. Is it that the two patterns in Figure 8 have tended to evolve coherently in the 20th century and not in previous centuries ? is it that the strong background sea-level rise in the 20th century has affected the probability of ESL, and that in previous centuries the variations in BSL were not strong enough to supersede the influence of internal atmospheric variability? In this regard, an important question is whether or not the simulation is able to replicate the observed sea-level rise both at global and at regional scales due to thermal expansion and maybe also to forced changes in the large-scale ocean circulation (AMOC), Perhaps I missed it in the manuscript, but I think this information is not present, and I think it is relevant, since it would support the simulation of past BSL variability. At least the thermal expansion component in the model and in the observations since 1950 should roughly agree at global scales. In Figure 3 (green line) it is difficult to eyeball. It seems that the regional not-land-ice related sea-level rise in the 20th century is not remarkably different from past variability, but accurate numbers would help the reader.

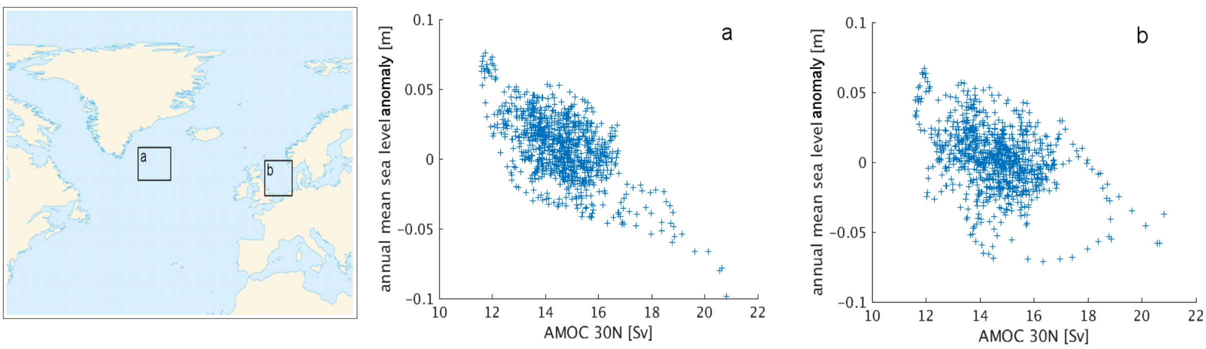
Response:

There is currently no overall consensus on the link between background sea level and extreme sea level, as for instance Mudersbach et al. (2013) found differences in linear trends in high sea level percentiles from those in mean sea level. Yet, most studies report similar trends.

According to the running correlation in Fig. 7 (former Fig. 6) and the wavelet coherency spectrum in Fig. A7 (former Fig. A8), the stronger coherence bewteen ESL and BSL at multidecadal timescales in the last century is also a feature in our simulation. However, the high internal variability of ESL, which is independent of external forcing or BSL variations if longer times are considered, masks this coherent behavior. It is only apparent in some centuries, like the 20th. The reason for this is not clear. It is indeed possible that this is related to the associated large-scale circulation regimes: ESL and BSL related SLP patterns evolve rather similarly (corr = 0.86, vs 0.78 over whole period) in the 20th century; it might also be by chance though. However, it does not stem from a gradual climate-change induced background sea level rise, as – other than in the observations – this transient thermosteric effect is not accounted for in the model simulation (see also Comment 10 by RV#2).

We added a statement about the coherency of the two SLP patterns (page 14, line 27f.). Furthermore, we specified the omission of the thermosteric effect in the ESL analysis in the method and discussion sections.

Concerning the question whether the simulation is able to replicate the observed sea level rise due to changes in the large-scale ocean circulation: The model is generally able to simulate realistic sea level changes due to changes of the AMOC strength. A stronger overturning circulation, for instance, leads to a stronger SPG and reduced sea level in the region (see scatter-plot below). However, this response is less pronounced on the North West European Shelf.



8. <u>Comment by Referee:</u>
   Fig 6 . Time series have been smoothed with a 11y moving window.
   It is not totally clear to me how this has been calculated. Were the 100-year gliding correlations later smoothed with a 11-year running mean, or where the initial series first smoothed and then the 100-year gliding correlations calculated. This is important for the set-up of the bootstrapping.

   <u>Response:</u>
   The initial series were first smoothed and then the 100-year gliding correlations calculated. A clarifying statement has been added (p.12, caption of Fig.7 (former Fig. 6))

9. <u>Comment by Referee:</u>
   … range of the Gumbel fit doubles though if the spread in RL100 is taken into account (grey bar). The non-parametrically obtained RL1000 lies with 3.7 within both distribution ranges, but closer to the median of the Gumbel distribution fits.
   3.7 meters, I guess

   <u>Response:</u>
   Yes. The unit has been added. (p. 18, line 22)

All technical corrections have been incorporated.