Reply to Mika Rantanen

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We are grateful for your constructive and in-depth review of our manuscript which helped us to clarify the presentation and interpretation of our results. Our responses to your comments are in blue text below.

1 Responses

General comments

This manuscript provides quantitative decomposition of extreme sea level (ESL) events in the Baltic Sea into storm surges, filling and seiches. These components are further decomposed into components from various forcings such as wind and atmospheric pressure. The authors use sea level observations from the entire Baltic Sea coastline and simulations with a numerical model. One of the key results is that storm surges dominate ESL events in the western Baltic Sea, while the filling contribution is more important in the central and northern sea regions.

I found the topic of this manuscript highly relevant and valuable. Here in Finland, it is often stated that surges, filling (preconditioning), and seiches together cause the highest sea levels in the Baltic Sea. However, concrete evidence quantifying their relative contributions has been lacking. The same applies to the specific roles of wind and atmospheric pressure in driving high sea levels. In my opinion, this study fills an important gap by providing detailed quantification of these processes.

I think the overall presentation of the manuscript and its language was very good. The structure was logical, and there was a "red line" in the story which made the reading enjoyable. The background was nicely covered with relevant references, making the impression that the authors do know the topic very well. The results were discussed in a detailed way from various perspectives at the end of the paper. While I am not a marine scientist, I still understood most of the text.

Despite the positive feeling I got from the reading, I still found a few aspects which in my opinion require clarification: these are related to 1) methods and 2) the negative correlation between storm surges and filling. These are explained below. In any case, I can recommend publication after these (minor) comments have been addressed.

Thank you very much for the positive feedback.

Minor comments

In section 2, you present the observations (2.1), the model simulations (2.2) and the diagnostic decomposition method (2.3). There were some parts which I think were missing:

In 2.3.1 (L115) you do not explain whether the decomposition is done for observations or model simulations or both. When I first did read these sections I assumed that the decomposition was only done for simulated sea level heights, so it was surprising to see in Fig. 3 that the method is applied to both. There is a brief mention in L92 that the decomposition is also done for observations, but this point should be emphasised later when presenting the decomposition method.

We now explicitly mention at the end of Section 2.3.1 that the decomposition of the observed ESLs is performed, and this is further explained in the first sentence of Section 3.1 to clarify that the temporal decomposition is first done for the observations. As a second step, we compare the model's decomposition and show that the model can replicate the statistics. Section 2.3.1: "This temporal decomposition is done for both the observed and modelled ESLs." and section 3.1.: "The temporal decomposition of the observed ESLs [...]"

At Section 2.3.1, you could explain how you derive η_{ESL}. Is that extracted from modelled data or directly from observations?

Added: "ESL events η_{ESL} are identified by applying a peak-over-threshold method. The threshold is determined by the 99.7th percentile of the time series, and we only consider events that are separated by morethan 48 hours (Arns et al., 2013)."

> At L95, it would be clearer to write that the observational time series for different tide gauges are of different lengths. Or are they? And I assume that the detrending is based on the linear trend of the whole time series and not on a fixed period.

Yes, the time series are of different lengths. However, we only consider the time span which is also covered by the model run, so 1961 to 2018 in our case, where many gauges are of shorter lengths. We added these information to the section: "Before any analysis, we selected the years 1961 to 2018 (the same time period covered by the model run, see next section) and subtracted the long-term linear trend of the mean sea level for each tide gauge. De-trending removes mean sea level rise and glacial isostatic adjustments (GIA Peltier, 2004) from the time series. Note that some gauges have records shorter than the considered time period and many contain temporal gaps (Lorenz and Gräwe, 2023)."

> In Section 2.2, it would make the choice of the model more robust if you could briefly mention whether the model has been used successfully in some previous studies.

The model has been used for over 2 decades and has been applied in several published hydrodynamic studies including extreme sea levels in the past. To not inflate the references, we kept the literature to previously cited studies on the Baltic Sea, although GETM has also been applied to several estuaries and marginal seas around the globe. "GETM has been used to model the hydrodynamics of several marginal seas and estuaries around the world. It has successfully demonstrated its ability to capture the complex hydrodynamics of the Baltic Sea (Gräwe et al., 2015), the mean sea level dynamics of the Baltic Sea (Gräwe et al., 2019), and its extreme sea levels (Lorenz and Gräwe, 2023; Kiesel et al., 2023)."

> In Section 2.2. It was not clear for me what was the temporal resolution (hourly?) of the simulations, and how long were the simulations? And did you simulate the whole year, including the summer season? Overall the time period which was studied should be written more clearly (I found it from L147 but it could come earlier).

We simulated the whole period from 1961 to 2018. We added the requested information plus additional information on the resolution of the atmospheric forcing: "The spatial resolution of the UERRA forcing is 11 km and the temporal resolution is hourly." and "The simulation period is 1961 to 2018. We save sea level data in a time step of 20 minutes."

In Section 2.4 (L158-163), the method of calculating the relative contributions of the forcings for sea level remained a little unclear to me. Could it be demonstrated using a single station example? Like writing down the magnitudes of the relative contributions from a station in Fig. 2d. This came back to me when I tried to interpret the sea ice contribution from Fig 7. You say (probably correctly) that its contribution is negative, but in Fig 7 they all look positive because they are presented as pie charts. Is there a contradiction, or have I misunderstood?

We now include a list of relative contributions for the example in Fig. 2d in the text: "For this station, the mean temporal contributions are: 60.5% surge, 26.5% filling, and 12.9% seiche (sum: 99.9%)." and "For the station Warnemuende, the mean forcing contributions are: 84.5% wind, 8.0% air pressure gradients, 2.9% Atlantic sea level, 0.2% baroclinicity of seawater, -0.5% sea ice, 0.2% sea ice variability, and 1.8% residual (sum 97.1%). Due to the uncertainty in the fits, we cannot expect the sum of all forcings to add to 100.0%." For the pie charts, only the absolute values are considered since negative pie pieces are graphically not representable. The contribution of sea ice is indeed negative on average. Sorry for the confusion. We are mentioning this now in the captions of Fig. 7-9. "Note that the pie chart depiction only considers the absolute values and not the sign."

At Section 3.1.1 (L201-213), I didn't really understand the reason why filling and surges are negatively correlated, especially because in Fig. 2b they seem to be positively correlated (both are positive at the time of maximum). I read several times the sentence "Since the peak sea level of each event is fixed, a particularly high surge would naturally coincide with a lower filling state relative to the mean of the Gaussian distribution.", but I still didn't get the idea.

If one would consider the whole time series of the filling and the surge components, then the correlation could indeed be positive as Fig. 2b suggests because high filling (compared to mean filling sea level of zero) usually occurs during ESLs. Figure 5 indeed shows that all filling levels are well above zero for all considered events. However, we correlated the statistical samples that make up the Gaussian distributions in Fig. 2d. The Pearson's correlation coefficient compares the samples to their respective means. In our case, the "means" are the mean values from the respective Gaussian distributions. Since the surge and filling are making up most of the relative peak sea level ($\sim 60\%$ surge and $\sim 25\%$ filling, Fig. 2d), both components must be negatively correlated, as otherwise the average relative peak level would be above 100% which would indicate an error in the decomposition. We added more information to the paragraph which hopefully makes our argumentation more clear: "The negative correlation between surge and filling is surprising at first sight, as it is known that most ESLs in the Baltic Sea are a coincidence of both contributions. At second glance, it does not contradict the fact that most ESLs are a combination of high filling states and storm surges which would intuitively indicate a positive correlation. The negative correlation between the relative filling and surge components is partly an artefact of the decomposition method since we do not correlate the whole time series, but only the extracted statistical samples which we used for the Gaussian distribution fits. Since the peak sea level of each event is fixed, a particularly high surge (right side of the surge's Gaussian distribution) must naturally coincide with a lower filling state relative to the mean of the Gaussian distribution (left side of the filling's Gaussian distribution), which emphasizes a negative correlation. Otherwise, the average relative ESL would be higher than 100% and thus the fitted Gaussian distributions would be wrong. The same applies to a low surge and a high filling state. Therefore, these two components must be negatively correlated in this decomposition approach of the mean ESL."

From a meteorological perspective, strong cyclones are typically associated with (long-lasting) westerly winds, which would intuitively lead to a positive correlation between storm surges and filling. Given that this result appears to be one of the key findings of the study, and also being in an apparent contradiction with other studies, I suggest clarifying the mechanism in greater detail. Providing additional explanation would help resolve this apparent contradiction and strengthen the manuscript's conclusions.

This point is based on the misunderstanding of the sample that went into the correlation analysis from the previous comment. Our results are no contradiction to the mentioned mechanism and the co-occurrence of high filling and high surges which intuitively indicate a positive correlation, but only if the means would be zero which is not the case for our samples. Since this is a methodological artefact, we cannot provide details to a mechanism.

Other, specific comments

L12: This phenomenon: does this refer to the rising mean sea level or ESL events? Isn't the ESL events the main cause of flooding, with a smaller contribution from rising sea level?

Phenomenon refers here to the ESLs. Due to the rising mean sea level, critical sea levels are nowadays already reached with smaller surges than before.

L31. By input data you mean weather prediction models or reanalysis? Can you mention them explicitly as I was wondering what input data is specifically meant here.

Here we refer to long-term reanalyses data. Rephrased to: "However, long-term, high-quality, high-resolution reanalysis data that meet these requirements are often not available."

L62 These three temporal. Would it be better to put the three components together in brackets, for example, so that the reader does not have to go back to the previous page to see what the three were?

Added the components in brackets: "Together with *storm surges*, these three temporal components (filling, seiches and storm surges) are the main contributors to ESL events."

Table 1. TSClim: temperature or salinity?

The 'of' is an error and should be an 'and'. We rephrased to: "Simulation where the inter-annual variability of temperature and salinity are excluded by using a climatology of temperature and salinity fields."

Table 1. IceClim: inter-annual is written twice. And what does it mean by neglecting the inter-annual variability? Do you run the model with climatological sea ice cover?

Fixed the word doubling. Yes, this simulation is using an ice climatology, i.e. for each simulation year, the same sea ice is present. Similarly, for the TSClim simulation, we use a climatology of T and S, see previous comment for the rephrased description. We rephrased the description of the IceClim simulation: "Simulation where the inter-annual variability of sea ice cover is excluded by using a climatology of sea ice cover."

L109. Does this mean you performed seven 58-year simulations?

Yes. We added the information: "We exclude different forcing components in distinguished model simulations to study the effects of different forcings on the sea level, see Tab. 1, i.e. seven simulations of the period 1961 to 2018."

L237. .. up to 30 %. This sentence remains a bit incomplete. Where does it contribute and what? Can you rephrase it?

Rephrased the sentence to: "However, the Atlantic sea level component can contribute up to 30% of the surge levels for this region."

L252 and L254 I think you write two times the residual term contribution? Is the 2nd (40%) for Danish Straits?

You are right. We meant the Skagerrak and Kattegat and not the Baltic Sea. Rephrased to: "The wind and Atlantic sea level forcings explain most of the filling for the Skagerrak and Kattegat. However, the residual term shows values up to 40% across these regions."

L258. As a meteorologist, I thought first that baroclinicity means atmospheric baroclinicity. Could it be rephrased to add seawater here?

changed to: "[...] baroclinicity of seawater [...]"

Figure 7-9. Related to minor comment 1f. I don't understand how the negative contributions from e.g. sea ice forcing is presented in these pie charts. For me it looks like all the forcings are contributing positively.

Yes you are right. For the pie charts only the absolute values are shown since negative values are not representable in such depiction. We added this information to the captions, see also a previous comment above.

L271 wind systems. Maybe wind climatology is a better term here?

Changed to "wind climatology".

L279 its mean → the mean contribution of filling

Changed.

L281 on this time scale. Which time scale?

We mean that the water exchange with with the North Sea occurs on the same time scale as the time scale of the filling, i.e. ∼ 7 days. Rephrased to: "This is partly because water can flow into the Kattegat, Skagerrak and the North Sea on the time scale of the filling, which is about a week."

L288 Do you speak about the potential increase due to seiches here? It could be added to the sentence.

We speak about the increase by both filling and seiches, although seiches being the main contribution. Rephrased to: "Nevertheless, the potential average increase due to temporal shifts in the seiche and filling is in the order of 10-20 cm."

L296: 10% on average. Was this result shown in some figure? If not, better to add "not shown".

Yes, Fig. 7b shows that sea ice can reduce the surge component for the northernmost stations by almost 10%.

L303 ... currently very small. Maybe add reference to Figure?

Added reference to Figs. 7-9: "The same arguments can be made for the inter-annual variability of sea ice. Our results show that this contribution to the ESLs is currently very small (Figs. 7-9)"

L339. Aren't meteotsunamis more of a summer phenomenon, so that they generally don't occur at the same time as wind-driven extreme sea level events, which tend to occur in the winter season? If this is the case, it could be mentioned here.

In principle, the occurrence of meteotsunamis should not be limited to summer. Pellikka et al. (2022) differentiate between summer and winter type events of meteotsunamis, at least for the Northern Baltic Sea. As there is evidence that meteotsunamis in principle can occur at the same time as a surge, e.g. Pattiaratchi and Wijeratne (2015), we rephrased to: "Nevertheless, meteotsunamis could occur during an ESL event (Pattiaratchi and Wijeratne, 2015) and are a common phenomenon in the Baltic Sea (Pellikka et al., 2020, 2022) with high sea level contributions in the order of decimetres."

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