Reply to reviewer 2

Marvin Lorenz, Katri Viigand and Ulf Gräwe

January 6, 2025

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

> Overall, this paper leverages a validated model to analyse the components of extreme sea levels along the Baltic Sea, exploring their interactions and relative importance. The study introduces an innovative and engaging approach, and while the main conclusions are not entirely novel, they are well-structured, generalizable, and effectively capture the complexities of the Baltic Sea system. The paper is well-written and easy to follow, making it accessible to a broad audience. I recommend the paper for publication, subject to minor or moderate revisions. Below, I provide some specific comments to further enhance the quality of this already good work. The paper is well written, however, there are some typos here and there. Those that I noticed are mentioned, but I suggest the Authors re-read the document looking for minor typos.

Thank you for your positive comments.

> Line 35: The claim about second-order effects I believe should be corroborated a bit more?

Including non-linear interactions in the decomposition is complicated, yet can make a significant contribution to the peak sea levels, e.g. tide-surge interaction (Idier et al., 2019; Arns et al., 2020). Depending of the time series decomposition approach, the non-linear effects are actually automatically attributed to one of the linear contributions or to a residual term since tide gauge data of course include these effects. We have changed the wording to avoid "second order". We have rephrased the sentence to: "Interactions between the different contributors are often attributed to a residual term."

> Table 1: Check typo.

Fixed.

> Lines 104-105: Can you explain the reason of the 7% increased wind without only relying on the reference?

The calibration of the model showed that this increase was necessary to avoid a negative bias in the Western Baltic Sea which we already write in the text. That there is a bias in the first place in this region was partly attributed to the 'coarse' resolution of atmospheric models which do not resolve the orography of the Western Baltic Sea correctly since the resolution of approx. 10-20 km would cover the Western Baltic Sea with only a few grid cells (Lorenz and Gräwe, 2023). In our previous study (Lorenz and Gräwe, 2023) we found a negative bias in ESL heights for all of the simulations which have used six different atmospheric reanalyses as forcings. We believe that adding more information to this reasoning to the manuscript would be distracting and it would not the affect the results of relative contributions. Curious readers can always check the reference which is an open access publication in Ocean Science.

> Line 118: can you please specify the filter order? Any specific reason why you used Butterworth filter? Can you explain the physics behind 7 days?

The specific order of filtering is presented in section 2.4. To avoid repetition, we decided to avoid mentioning the filter order in this section. There is no specific reason to use a Butterworth filter. Any other filter would work as well. Regarding the physics, 7 days is the main time scale which includes both the mean filling state of the whole Baltic Sea, but also local filling aspects, e.g. due to prevailing winds. Furthermore, for comparability with previous studies (e.g. Soomere and Pindsoo, 2016; Pindsoo and Soomere,

2020) we have chosen a similar time scale. We added some information on the reasoning of 7 days: "The filling state η_{fill} is computed by applying a Butterworth low-pass filter with a cut-off frequency of 7 days, corresponding to the weekly timescale described by Soomere and Pindsoo (2016) and Pindsoo and Soomere (2020). This time scale includes both the average filling state of the whole Baltic Sea and local filling due to persistent winds such as storm systems."

> Line 120: "...a time window of +- 7 days" means a window of 14 days centred on the peak? Can you please clarify the overall approach that you used? What are the steps? Can you explain the link with the peak sea level?

Again, we refer here to section 2.4. which includes all the requested details.

> Eq 2: can you please show if the results of the fitting provide realistic amplitude and phases?

Fig. 2b of the manuscript shows an example of the fitting of the seiches. The amplitudes are certainly realistic. The phases are also reasonable. A better illustration that this approach works well, is shown in Fig. 1 below for the station Kemi in the Gulf of Bothnia in the north of the Baltic Sea. For the exemplary ESL event (panel b), the seiche signal is very large and the fitting approach is capturing the seiches really well, which gives us confidence that our approach is well suitable.

> Line 125-126: please clarify and justify your belief about the error. I do think that is negligible in your work, but it would be nice to have some solid ground to say so.

We have added more details and arguments to our justification why the consideration of more frequencies is unnecessary: "Note that even more frequencies could have been added, but the amplitudes of these frequencies are much smaller. In addition, since we are considering $\mathcal{O}(100)$ events per station, the uncertainty estimates based on these statistical samples will be larger than the missing frequency contributions. Therefore, the errors introduced by neglecting these additional frequencies are expected to be negligible for this study."

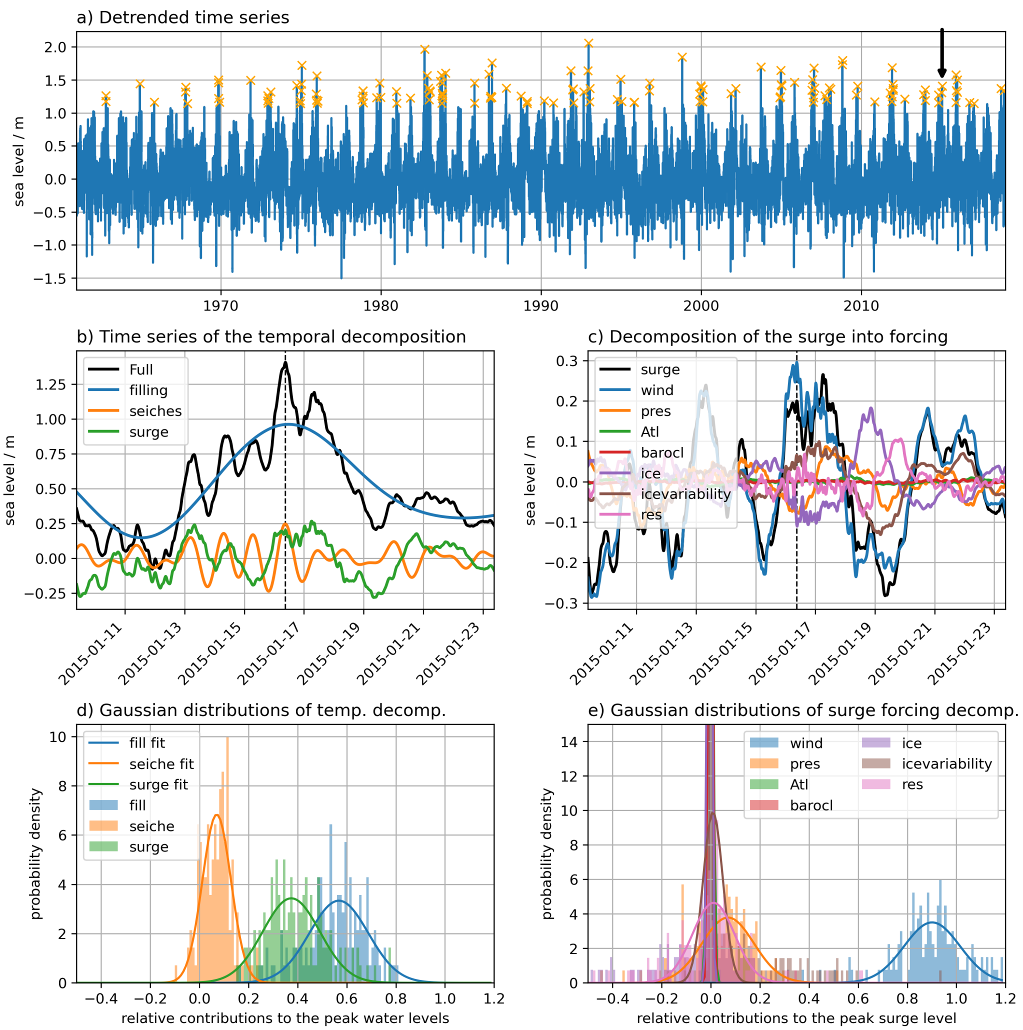

Figure 1: Exemplary results of the workflow for the decomposition of ESL events as in Fig. 2 of the manuscript, but for the station *Kemi (51)*.

Chapter 2.3.2: how do you justify your decompositions? I am referring in particular to the seiches component.

Seiches are inertial surface waves which are the "back-and-forth sloshing" of sea level perturbations. These perturbations can have multiple origins, but most prominent ones are from wind and air pressure. For example, a seiche can be the inertial response after a storm surge. Therefore, our decomposition into forcings should capture the origins of the perturbations. E.g. by switching off winds, the all seiches triggered by wind will not be present in the simulation, which should reduce the seiches' amplitudes. Of course we cannot exclude that the forced fit approach may fit higher amplitudes. But due to the large number of events, and the clear results of the seiche decomposition (Fig. 8), we have high confidence that our decomposition approach is justified.

Line 143: what did you do for detrending the time series? Can you please show both time series and what you removed to get the detrended one?

We have de-trended the time series by a linear regression over the whole time series which we mentioned in section 2.1. We added the information to the first sentence of section 2.4: "As a first step, the long-term changes in the mean sea level are removed from the time series by de-trending by subtracting the linear trend of the entire time series; see Fig. 2a." You can find the comparison of the original and de-trended time series below in Fig. 2. The de-trending not only subtracts the mean sea level rise over the time period, but also possible biases in the reference heights in the tide gauge observations. We decided to not explicitly show the de-trending in Fig. 2 of the manuscript because this approach of de-trending is straightforward.

Line 148: can you please rewrite the following part "....which are shown in Fig. 1, see Fig. 2a for the identified events." It is not clear.

Rephrased to: "For the modelled time series spanning 58 years from 1961 to 2018, approximately 80 to 250 events are identified for each station. The station locations are shown in Fig. 1."

Line 154: Please clarify the content of the bracket.

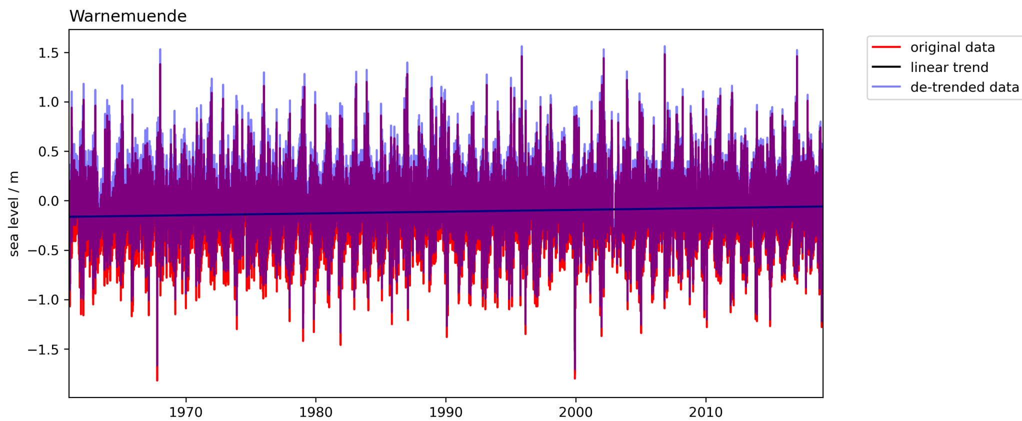

Figure 2: De-trending of the sea level data by linear fit over the time series, here the tide gauge data of Warnemuende as an example. For this example, the trend is 1.8 mm/yr, thus a mean sea level increase of ∼ 10.4cm from 1961 to 2018.

Now reads as "An exemplary temporal decomposition of the observed Warnemuende time series (Fig. 1, station 9) is shown in Fig. 2b."

> Line 156: it is not clear how you used the maximum values within +-24hours, can you clarify?

Rephrased to "In addition, the maximum filling and maximum seiche level values within a 48-hour window, i.e. ±24 h around the ESL peak, are stored to assess the potential ESL if all three components were to reach their peaks simultaneously."

> Line 158: Why do you need to normalise the components? It is not clear this step, please clarify what you did and provide the reasons to do so. Why did you not use the distributions from the original dataset?

We normalise the components to study the respective relative contribution of the different components to the ESL. Normalisation ensures that the different events are comparable to each other which allows us to compute a mean composition of ESLs. We added more information and justification to the approach: "To study the relative importance of each component, the components are normalised to the peak level for each event and stored in a histogram with a bin size of 0.01, ranging from -0.5 to 1.2. By the normalisation, we can use the all events to make general statement of the mean composition of ESLs."

> Line 173: if you use the countries to explain the figure 3.a, please add the borders in the figure.

We added the borders and country names in Figs. 3, 7, 8, and 9.

> Lines 187-188: due to the layout it is not clear the resolution you are referring.

We don't have influence on the layout of the text. For type-setting we will keep an eye open for formatting issues like the line break you mention. We now explicitly mention the model to make the sentence more clear : "Differences are found for gauges that are located within coastal lagoons or estuaries, such as Ueckermuende, Althagen, Barhoeft, Kappeln, Schleswig, and Gdansk, since the hydrodynamics are not resolved at the 1 N.M. resolution of the model, as discussed by Lorenz and Gräwe (2023)."

> Lines 191-195: explain more clearly the reasoning behind your claims. Moreover, please provide at least some names of the locations on the map too. Moreover, Cuxhaven is not in the Baltic sea, why do you present?

We included station numbers instead of names and elaborate more on the reasoning. The Cuxhaven station is included as a contrasting station where different dynamics should be present. We rephrased the paragraph to: "Although located outside the semi-enclosed Baltic Sea, stations in the Kattegat and Skagerrak (stations 20-28) and in the North Sea (station 73) still exhibit large filling contributions of 40-50%. This shows that, at least for ESL events, low-frequency waves contribute to the slow sea level variability. This makes sense as the mean filling of the Baltic Sea is controlled by the water exchange with the North Sea, which requires long periods of elevated sea level in front of the Danish Straits. Since tides are excluded in the simulations, the low-frequency variability cannot be a superposition effect such as the spring-neap cycle, further indicating that low-frequency waves also contribute significantly to ESLs in the more open eastern North Sea. As a contrast to the Baltic Sea stations, we included one station in the North Sea, Cuxhaven (72). The model deviates from the observations for this station in the southeastern North Sea because tides are not included in the model. Nevertheless, the filling contribution of about 25% to the ESLs is a noteworthy result, indicating that persistent westerlies can elevate the mean sea level for a period of at least one week and longer in this region."

> Chapter 3.1.1: As you mentioned, the selection method (i.e. POT) and the assumed linear summation together induce at least some of the mentioned negative correlation. Can you please explain why this is less important in the surge/seiches correlation and why you keep having a positive correlation? Your explanation of the positive correlation is ok, but why the induced negative part is here less important?

We now discuss the low, but significant, negative correlation between the seiche and the filling: "The negative correlation between filling and seiches shows smaller coefficients than between filling and surge components, indicating that seiches tend to be small when filling is high and vice versa. Since there is a positive correlation between surges and seiches, and a negative correlation between filling and surges in the same areas, it makes sense that seiches should generally be negatively correlated with filling as well."

> Can you please provide the details of the correlation analysis? Which correlation coefficient did you use? Can you please mask the map points having the p-values lower than a reasonable threshold? Without any diagnostic checks, the map can be misleading.

Thank you for the suggestion. We use the Pearsons's correlation coefficient which we now mention in the text. We now marked areas where the p-value is below 0.05, i.e. statistical significance by hatches, see Fig. 3 below for the new version of the plot. This is a very good addition to support our argumentation.

> Chapter 3.1.2: I am unsure whether it is needed for the paper. I do not require the chapter to be removed, I leave this to the Authors, but I believe it's not so interesting as the rest of the document.

Thank you for the suggestion. While this chapter may not be as interesting as the other chapters, we believe that the quantification of potential maximum

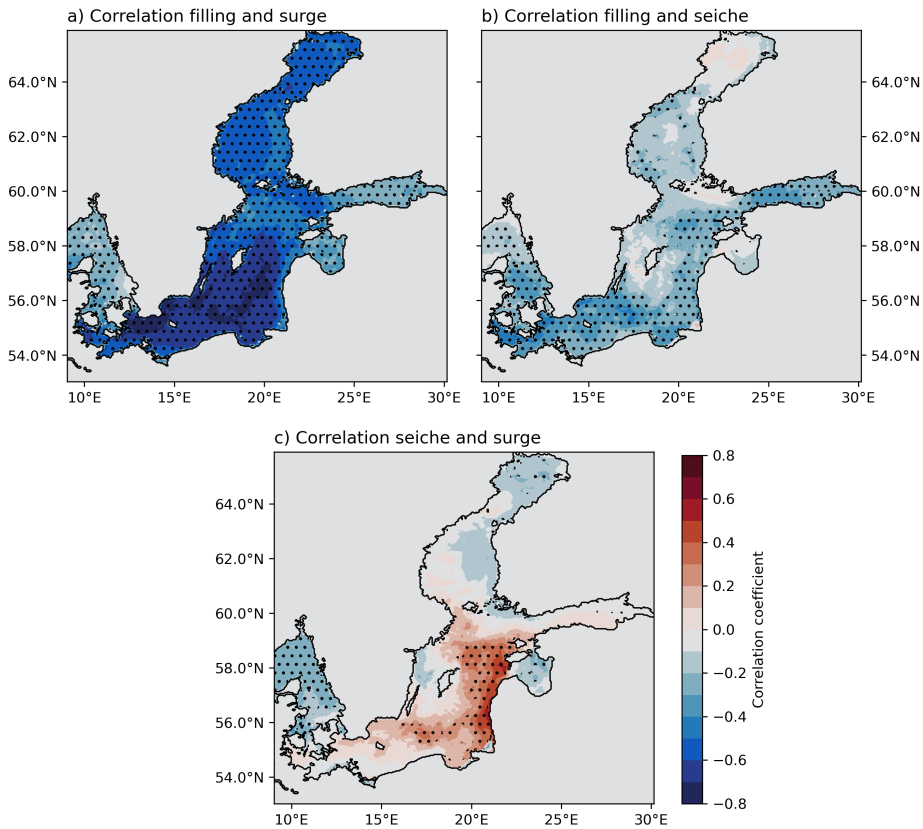

Figure 3: Correlation maps for the three temporal components: a) Correlation coefficient for *surge* and *filling* components. b) Correlation coefficient for *filling* and *seiche* components. c) Correlation coefficient for *seiche* and *surge* components. The hashed areas indicate where the p-value is below 0.05.

sea level increases to be useful information since it illustrates the partial randomness of the phases of the three contribution to each other. Furthermore, it provides an indication that it does not necessarily require climate change to significantly increase ESLs which experts of course know, but non-experts may overlook.

> Figures 3 and 7: an option to have an idea of the location would be to add to the name of the locations on the x axis a number and reproduce these numbers in the map. In any case, something to help the localisation of the different locations on the maps should be done. I leave it to the author's preferences how. Please increase the y axis of the bar plot series.

We added the numbers of the stations to the x-labels. However, we do not add the numbers to the pie charts to avoid making the plots too messy. Fig. 1 shows the locations of the gauges by the numbers which we believe is enough. Already in Fig. 1 our feeling is that the numbers make the map a little messy which would be worse when the pie charts are present in the same plot. We further added country borders and names to each map. We have also stretched the y-axes in panel b) of Figs. 7-9 which enhanced the clarity of the plots.

> Lines 255 and everywhere within the document specify the meaning of low-frequency waves? What are you referring to?

With low-frequency waves, we mean the filling component, which is a surface wave with a long period, thus it has a low-frequency. Since we already always mention "filling" right after the term "low-frequency wave" we believe that we cannot be more precise.

> Line 274: check the typo, "is" is missing in the sentence.

Fixed.

> Lines 284-285: is this temporal shift something realistic?

The delay between the filling and surge should be random due to the different time scales of these components. For the seiches, this is different. Only where the correlation between the surge and seiche are close to zero, there is a potential of increased ESLs, e.g. everywhere, but in the central Baltic Sea. Therefore, we believe that it is indeed realistic.

> Lines 288-290: the sentence seems not finished. If these values do not represent that, then they represent what. . . ?

We clarified the text to: "These values represent the theoretical maxima of ESLs in the respective regions for the specific events."

> Lines 292-293: The sentence "This result is expected since most ocean surface waves are forced by momentum transfer from the atmosphere to the ocean by winds or by atmospheric pressure via the inverse barometric effect." might also be removed.

Removed

> Lines 299-302: I do not see the link between the following sentences and the paper. I suggest to remove because not relevant for this paper, or otherwise justify the reason to be mentioned. "However, with decreasing levels of sea ice due to climate change (e.g. Meier et al., 2022a, b, and references therein), the contribution of storm surge to ESLs is likely to increase in the future in regions that are currently covered by annual sea ice. In addition, with decreasing ice cover, the average wave loads and annual wave energy flux are expected to increase by about 5% and up to 82% respectively (Najafzadeh et al., 2022)."

We decided to keep the sentence regarding the future decline of sea ice which is likely increasing the relative importance of storm surges to ESLs. However, we removed the sentence regarding the wave loads, since wind waves do not fit to this section.

> Chapter 4.1.2: can be removed and for each excluded components specify what is the expected effect(s) on the main outcomes. It is already partially done, but I believe is interesting can be detailed a bit more addressing the effects rather than the reason why each component was not considered.

We do not understand what the "removed part" of the comment refers to. Nevertheless, we agree that we should add more details on the consequences for our results, especially for meteotsunamis and river discharge. The last two paragraphs now read as: "Similar to wave setup, we have excluded meteotsunamis (Monserrat et al., 2006; Pattiaratchi and Wijeratne, 2015b). These are tsunami-like surface waves generated by the matching of the propagation speeds of a small atmospheric pressure jump and the induced surface wave, e.g. a Proudman resonance (Proudman, 1929). The reasons for neglecting meteotsunamis are simple: First, the temporal and spatial resolution of the meteorological forcing is too coarse to resolve the propagating pressure system accurately enough to generate meteotsunamis in the numerical model. Second, the hourly resolution of the observational data used is also too coarse to resolve meteotsunamis that occur on faster time scales (minutes). Nevertheless, meteotsunamis could occur during an ESL event (Pattiaratchi and Wijeratne, 2015a) and are a common phenomenon in the Baltic Sea (Pellikka et al., 2020, 2022) with high sea level contributions in the order of decimetres. Meteotsunamis could easily be included in the temporal decomposition using a high-pass filter. We have also ignored the influence of river discharge since the coarse resolution of our model does not sufficiently resolve the estuaries and constrictions where river discharge increases sea level. However, compounding ESLs with very high river discharge can elevate the peak sea level (Talke et al., 2021) and have been observed in the southwestern Baltic Sea (Heinrich et al., 2023). We do not expect major changes in our results, as these effects are restricted to estuaries and we have studied ESLs at the open coast. Nevertheless, we acknowledge the importance of river discharge in estimating coastal flooding."

> Lines 319-320: "However, the potential contribution of wave setup can be substantial in specific locations." can you be more specific?

We added specific examples: "However, the potential contribution of wave setup can be substantial in specific locations, e.g. for exposed coasts of islands (Su et al., 2024) or coastal bays (Soomere et al., 2013)."

> Lines 375-378: if you consider the two statistics completely independent, the final event resulting from the sum of the two components having the same probability of exceedance is larger than accounting for the correlation between the components. Can you please revised the text?

Rephrased the text to: "If the statistics of two (or more) components were considered independently, the peak sea level resulting from the summation of the sea levels of the components with the same return period (the same probability) would be overestimated, because correlations between the components are neglected."

> Line 389: "…which could serve as a peak-over-threshold for GPD statistics,…" I think is redundant and slightly misleading.

Removed.

References

Arns, A., Wahl, T., Wolff, C., Vafeidis, A. T., Haigh, I. D., Woodworth, P., Niehüser, S., and Jensen, J.: Non-linear interaction modulates global extreme sea levels, coastal flood exposure, and impacts, Nature Communications, 11, 1–9, https://doi.org/10.1038/s41467-020-15752-5, 2020.

Heinrich, P., Hagemann, S., Weisse, R., Schrum, C., Daewel, U., and Gaslikova, L.: Compound flood events: analysing the joint occurrence of extreme river discharge events and storm surges in northern and central Europe, Natural Hazards and Earth System Sciences, 23, 1967–1985, https://doi.org/10.5194/nhess-23-1967-2023, 2023.

Idier, D., Bertin, X., Thompson, P., and Pickering, M. D.: Interactions between mean sea level, tide, surge, waves and flooding: mechanisms and contributions to sea level variations at the coast, Surveys in Geophysics, 40, 1603–1630, https://doi.org/10.1007/s10712-019-09549-5, 2019.

Lorenz, M. and Gräwe, U.: Uncertainties and discrepancies in the representation of recent storm surges in a non-tidal semi-enclosed basin: a hindcast ensemble for the Baltic Sea, Ocean Science, 19, 1753–1771, https://doi.org/10.5194/os-19-1753-2023, 2023.

Monserrat, S., Vilibić, I., and Rabinovich, A. B.: Meteotsunamis: atmospherically induced destructive ocean waves in the tsunami frequency band, Natural Hazards and Earth System Sciences, 6, 1035–1051, https://doi.org/10.5194/nhess-6-1035-2006, 2006.

Pattiaratchi, C. and Wijeratne, E. M. S.: Observations of meteorological tsunamis along the south-west Australian coast, pp. 281–303, Springer International Publishing, Cham, https://doi.org/10.1007/978-3-319-12712-5_16, 2015a.

Pattiaratchi, C. B. and Wijeratne, E.: Are meteotsunamis an underrated hazard?, Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 373, 20140 377, https://doi.org/10.1098/rsta.2014.0377, 2015b.

Pellikka, H., Laurila, T. K., Boman, H., Karjalainen, A., Björkqvist, J.-V., and Kahma, K. K.: Meteotsunami occurrence in the Gulf of Finland over the past century, Natural Hazards and Earth System Sciences, 20, 2535–2546, https://doi.org/10.5194/nhess-20-2535-2020, 2020.

Pellikka, H., Šepić, J., Lehtonen, I., and Vilibić, I.: Meteotsunamis in the northern Baltic Sea and their relation to synoptic patterns, Weather and Climate Extremes, 38, 100 527, https://doi.org/10.1016/j.wace.2022.100527, 2022.

Pindsoo, K. and Soomere, T.: Basin-wide variations in trends in water level maxima in the Baltic Sea, Continental Shelf Research, 193, 104 029, https://doi.org/10.1016/j.csr.2019.104029, 2020.

Proudman, J.: The Effects on the Sea of Changes in Atmospheric Pressure, Geophysical Supplements to the Monthly Notices of the Royal Astronomical Society, 2, 197–209, https://doi.org/10.1111/j.1365-246X.1929.tb05408.x, 1929.

Soomere, T. and Pindsoo, K.: Spatial variability in the trends in extreme storm surges and weekly-scale high water levels in the eastern Baltic Sea, Continental Shelf Research, 115, 53–64, https://doi.org/10.1016/j.csr.2015.12.016, 2016.

Soomere, T., Pindsoo, K., Bishop, S. R., Käärd, A., and Valdmann, A.: Mapping wave set-up near a complex geometric urban coastline, Natural Hazards and Earth System Sciences, 13, 3049–3061, https://doi.org/10.5194/nhess-13-3049-2013, 2013.

Su, J., Murawski, J., Nielsen, J. W., and Madsen, K. S.: Coinciding storm surge and wave setup: A regional assessment of sea level rise impact, Ocean Engineering, 305, 117 885, https://doi.org/10.1016/j.oceaneng.2024.117885, 2024.

Talke, S. A., Familkhalili, R., and Jay, D. A.: The Influence of Channel Deepening on Tides, River Discharge Effects, and Storm Surge, Journal of Geophysical Research: Oceans, 126, e2020JC016 328, https://doi.org/10.1029/2020JC016328, e2020JC016328 2020JC016328, 2021.