

Response to Reviewers

Manuscript: “Metoocean study for the planning of the FSRU terminal: a case study at the Estonian coast of the Gulf of Finland” (sp-2025-8)

Dear Editor and Reviewers,

We thank the reviewers for their careful reading of our manuscript and for the constructive suggestions. We have prepared a major revision plan that (i) clarifies the scientific contribution, (ii) strengthens the Baltic Sea-wide analysis, (iii) documents dataset selection and bias treatment, and (iv) improves transparency and reproducibility.

Below we provide point-by-point responses. Reviewer comments are reproduced in italics, followed by our responses and a list of changes that will be implemented in the revised manuscript.

We appreciate the Reviewer's assessment and the clear identification of the key issues. Below we respond to each numbered comment.

RC1: 'Comment on sp-2025-8', Anonymous Referee #1, 28 Nov 2025

Major comments:

The authors performed a metoocean analysis of extreme hydrodynamic and weather conditions for providing thresholds to define critical design parameters for offshore installations at the Gulf of Finland. The analysis was extended to the entire Baltic Sea in a second part of the study. Although I have no doubts about the importance of safeguarding offshore infrastructures through metoocean analysis, I don't see novel findings in terms of scientific knowledge. I also have some major concerns regarding the dataset and uncertainty in the analysis, which are listed below:

1. One of my main concern is about the scientific novelty in this study. The authors utilized existing datasets and performed extreme value analysis to derive the results. However, the manuscript reads like a technical report rather than a scientific paper due to lack of novelty either in the methods or findings. Please clarify this in your revision.

Response: We thank the reviewer for this important comment. We agree that the statistical techniques used in the study, including block maxima, peaks-over-threshold analysis, GEV, GPD, Gumbel, and exponential-tail fitting, are established methods, and that the manuscript should not imply methodological novelty in extreme value theory itself. We have revised the manuscript to clarify that the scientific contribution lies in the integrated application, cross-validation, and interpretation of multiple long-term metoocean datasets for a strategically important but previously under-characterized Baltic Sea infrastructure site, and in extending the same framework to a basin-wide assessment of metoocean extremes and their uncertainties.

Specifically, we have revised the Abstract, Introduction, Methods, Section 3.5, and Conclusions to make the contribution more explicit. The novelty is now framed as follows: first, the study provides a harmonized multi-parameter metocean extreme assessment for the planned FSRU terminal on the Estonian coast, covering wind, sea level maxima and minima, significant wave height, wave period, and surface currents. Second, it evaluates and cross-validates multiple independent data sources, including Copernicus Marine Service products, ERA5, satellite observations, tide-gauge data, buoy observations, and high-resolution local models, to determine which datasets are suitable for estimating extremes in a complex, fetch-limited, non-tidal basin. Third, the study extends the analysis from a local design-basis assessment to Baltic-wide maps of 50-year return levels and associated uncertainty ranges, thereby revealing spatial contrasts in metocean hazard exposure at major maritime infrastructure locations. Finally, the revised discussion now emphasizes the scientific implications of these results, including the strong spatial heterogeneity of Baltic Sea extremes, the role of data resolution in estimating sea-level minima, and the relevance of uncertainty estimates for regional risk comparison and infrastructure planning.

2. It seems that the wave modeling and hydrodynamic modeling is decoupled (lines 85-95), and provided by different data sources. I have a serious concern on this, since nearshore areas are strongly affected by interactions between waves and currents, and water levels. I strongly believe that a full coupling between waves and hydronamics should be considered for nearshore areas. Please provide an assessment on the uncertainty related to this and justify why a decoupling is sufficient.

Response: We thank the reviewer for raising this important point. We agree that in shallow nearshore environments, wave-current-water-level interactions can be significant and that fully coupled wave-hydrodynamic modelling is preferable for detailed design-storm simulations, harbour-scale impact studies, and final mooring-load calculations. We have revised the manuscript to clarify that the wave and hydrodynamic datasets used in this study are not fully two-way coupled, and we now explicitly discuss the related uncertainty. For that we have added subsection 3.6 Uncertainties and limitations to revised manuscript.

3. Since LNG infrastructures such as FSRU are built very nearshore, which is subject to changes in seabed morphology by erosion or deposition. This might not be a problem in the study area that is dominated by hard substrate (nevertheless, the authors should justify in the study why impact of sediment transport and morphological change are not considered in the study area). However, for sandy coasts such as the southern and southeastern Baltic, morphological change in nearshore may have significant impact on local metocean conditions. This would lead to large uncertainty in the results of the entire Baltic Sea for very nearshore areas, which should be clearly pointed out in the manuscript.

Response: We thank the reviewer for this important comment. We agree that sediment transport and morphological change can be important for very nearshore LNG/FSRU infrastructure, especially along sandy coasts where erosion, deposition, sandbar migration, dredging, and changes in local bathymetry

can modify wave transformation, currents, and water-level impacts. Sediment transport and morphological change were not explicitly modelled because the objective of the study was long-term metocean extreme-value estimation using fixed-bathymetry datasets. For the Pakrineeme berth, the resulting uncertainty is considered limited because the site is relatively deep and bedrock-controlled compared with sandy surf-zone environments. However, this limitation is important for very nearshore sandy coasts elsewhere in the Baltic Sea and is now explicitly stated in the manuscript. For discussion we have added subsection 3.6 Uncertainties and limitations to revised manuscript.

4. The authors applied some correction factors to align better model data and observation data in the local study area, e.g. for wind speeds, significant wave heights and wave periods. However, it is unclear whether the same correction factors should be applied for other parts of the Baltic Sea, and the potential errors caused by ignorance of such local corrections. The authors did not address this uncertainty in the second part of the study for the entire Baltic Sea

Response: We thank the reviewer for this important comment. We agree that the local correction applied for the Pakrineeme/Paldiski FSRU site cannot automatically be transferred to the entire Baltic Sea. The corrections for wind speed, significant wave height, and wave period were derived from comparisons with local observations near the study site, and they reflect local effects related to coastal sheltering, grid-cell representation, fetch, bathymetry, and the specific position of the terminal relative to the model grid. Applying the same factors uniformly over the Baltic Sea would therefore not be justified and could introduce artificial regional biases. For discussion we have added subsection 3.6 Uncertainties and limitations to revised manuscript.

RC2: 'Comment on sp-2025-8', Anonymous Referee #2, 22 Dec 2025

Major comments:

This is my first review of 'Metocean study for the planning of the FSRU terminal: a case study at the Estonian coast of the Gulf of Finland' by Raudsepp et al., in which the authors present extreme value statistics for metocean variables, based on model data and observations in the Baltic Sea. The study is motivated by the vulnerability of marine infrastructures – especially the Paldinski FSRU terminal. As the first reviewer pointed out, as it is now, the manuscript finds itself somewhere in between a report and a manuscript. The scope of the 'state of the planet' is such that a more report like submission may do, but below I make some suggestions for improvements – mainly focusing on reducing the uncertainties in the Baltic wide analysis (see comment 9) which I think should be the main focus of the manuscript (see comment 1). I consider this to be a major revision that requires some dedicated work, but at the same time I think the revisions can be quite straightforward to implement.

1) I would suggest flipping the manuscript upside down. I think the readers would find it much more interesting to learn about Baltic Sea wide extreme values, rather than this one very specific location off the coast of Estonia. I would therefore change the title to be something like 'Extreme winds, sea level, waves, and currents in the Baltic Sea' and start with section 3.5 and use the FSRU site as a validation case study → if possible, I would also consider adding more station observations (e.g. from the Copernicus in-situ TAC) for validation.

Response: We considered the reviewer's suggestion to reorganize the manuscript, but we have retained the present structure because the study is motivated by a specific FSRU infrastructure case. The Baltic-wide EVA is an extension that provides regional context for the local design-basis assessment, rather than the primary product of the paper. Reversing the structure would shift the focus from a site-specific metocean study to a general Baltic Sea extreme-value atlas, which is not the objective of the manuscript. In addition, the FSRU site cannot be treated simply as a validation point for the Baltic-wide maps, because the local analysis uses additional observations, local correction factors, high-resolution datasets, and different temporal resolutions for some variables. We have revised the manuscript to make this structure and distinction clearer.

We also agree that additional in situ observations, including Copernicus In Situ TAC data, would be valuable for a dedicated Baltic-wide validation study. However, such an analysis would substantially expand the scope of the manuscript and would require careful station-by-station evaluation of record length, quality flags, temporal resolution, vertical datum consistency, ice-season gaps, and model-grid representativeness. We have therefore not added a new basin-wide station-validation section, but we now explicitly state that the Baltic-wide maps are regional screening products and that site-specific applications elsewhere in the Baltic Sea require local validation and bias assessment.

2) In relation to the previous comment, framing the study around offshore infrastructure makes sense, but falls a bit short if is not taken to the engineering level (computing actual expected loads to structures, which would allow structural engineers to directly use these results). I think such work is out of scope for

the present study, which is why I suggest recasting the study along the lines of comment 1 – also in the abstract/introduction.

Response: We thank the reviewer for this helpful suggestion. We agree that the manuscript should be framed more clearly as a metocean extreme-value assessment rather than as a full engineering-level infrastructure study. We have revised Introduction and Conclusions accordingly. The revised manuscript now states explicitly that the reported return levels describe marginal metocean extremes of wind speed, sea level, waves, and currents, but do not provide vessel motions, mooring loads, fender loads, structural loads, under-keel-clearance exceedance probabilities, or operational downtime estimates. These require additional engineering information and coupled load-response modelling, which are outside the scope of this study. The manuscript has therefore been recast as providing metocean design-basis parameters and regional hazard context for subsequent engineering assessment, rather than as computing the engineering loads themselves.

3) The authors do not provide much quantitative details on the basis by which the higher resolution regional wind products were dropped out (apart from the general statements about the biased extreme values) – was this decision based on the Paldiski site alone? Although it is a bit of work, I would ask the authors to use CERRA data in addition to the ERA5. CERRA5 has in many occasions been shown to outperform ERA5 and I believe it should allow reducing the uncertainties in the Baltic wide analysis.

Response: We agree that the current manuscript does not provide sufficient quantitative justification for excluding COSMO-REA6, BaltAn65+, and NORA3. We have summarized the deficiencies of the datasets in Data and methods section *“COSMO-REA6 and BaltAn65+ consistently overestimated wind speeds during high wind events, with COSMO-REA6 also exhibiting unrealistic wind spikes. NORA3, despite its higher resolution, substantially underestimated high wind speeds.”*

CERRA is valuable but adding it would not simply “reduce uncertainty” unless the whole Baltic-wide marine hindcast chain were re-run and revalidated with CERRA forcing. For the present manuscript, ERA5 is retained because it is consistent with the CMEMS Baltic wave and physics products used in the study.

We have added discussion about using CERRA or the other regional products in the subsection 3.6 Uncertainties and limitations in the revised manuscript.

“ERA5 was retained as the primary wind dataset for two different reasons in the local and regional parts of the study. For the Pakrineeme/Paldiski FSRU site, ERA5 was selected after comparison with the available local wind observations and satellite wind products. The comparison focused on the representation of high-wind events, seasonal behaviour, consistency with observed maxima, and the absence of unrealistic spikes. Some higher-resolution regional products showed larger deviations from the local observations during high-wind conditions, whereas ERA5, after a modest local correction, provided the most stable basis for the site-specific extreme-value analysis. This evaluation is site-specific and should not be interpreted as proof that ERA5 is the optimal wind product at every location in the Baltic Sea.

For the Baltic-wide analysis, ERA5 was retained for consistency with the CMEMS Baltic marine products used in this study. The Baltic wave hindcast and physical reanalysis are both forced by ERA5 atmospheric fields. Using CERRA or another regional atmospheric product only for the wind-speed EVA would make the wind extremes inconsistent with the wave, sea-level, and current extremes derived from ERA5-forced marine products. A physically consistent CERRA-based analysis would require CERRA-forced wave and hydrodynamic hindcasts and their validation against Baltic Sea observations, which is beyond the scope of the present work.”

4) Overall I think it is a bit misleading to list the different datasets in table 1 and then not use them. However, linked to the comment 9, I would encourage the authors to reconsider using those datasets (and CERRA) in order to narrow down the uncertainties in the extreme values.

Response: We agree that it is misleading to list datasets that we did not use in the analysis. In the revised manuscript we have deleted these datasets from Table 1. We have summarized the deficiencies of the datasets in Data and methods section “COSMO-REA6 and BaltAn65+ consistently overestimated wind speeds during high wind events, with COSMO-REA6 also exhibiting unrealistic wind spikes. NORA3, despite its higher resolution, substantially underestimated high wind speeds.” Our argument was that compared to ERA5, we increase uncertainties instead of decreasing.

5) L148 – which model output was used to fill the tide gauge data-gaps?

Response: In the revision we will specify explicitly that the gaps in the Paldiski North Port tide gauge record (2010–2016) were filled using the CMEMS Baltic Sea Physics Reanalysis sea surface height time series at the nearest grid cell, after aligning the model series to the gauge datum and mean level over the overlapping period.

6) L164-166. The authors mention the rather large (15% to SWH and 5% to periods!) corrections to wave properties, but there is no figure or reference to back this up. These corrections are quite large so perhaps the authors could add a supplementary figure with a bit more careful description on how these numbers were decided. These percentages seem to be quite round so the reader is left wondering if they are really coming out of a fit, or rather based on expert judgment?

Response: We have provided Supplementary material to the revised manuscript. It includes explanations and figures how the correction for wind speed and wave variables have been implemented.

7) L176 – to my understanding the Peaks Over Threshold method requires some way of ensuring that the same event is not considered multiple times. How did the authors do this?

Response:For the POT data a censoring time of 24 hours was used to guarantee independence of the events. We have clarified this in the Methods section.

8) section 2.6 – did the authors apply the site specific corrections for winds and waves to the full regional dataset? I am assuming not, but it would be good to explicitly state that.

Response: In the revised manuscript we will clarify that site-specific correction factors derived for the Paldiski case study were not applied uniformly across the Baltic Sea-wide fields.

9) The wind speed return values seem low and the spread is very large – almost to the point that it becomes useless. The error has also a very peculiar round maximums, possible around points where ERA5 has assimilated (or not?) some actual observations. I think the authors should make their utmost to reduce that uncertainty range and provide explanation for the peculiar shapes. For example, it seems very unlikely to me that moving a few kilometers east to west in the region north of Gotland island, one would have close to the same return period (22 m/s, which, btw seems very low) but a difference in the confidence interval that ranges from 1 m/s to well over 10 m/s (colormap is saturated).

I had a look at some other publications, for example this one

<https://www.sciencedirect.com/science/article/pii/S2212094721000530> who show similar maps of return values. Their colormap is not the easiest, but it looks to me like many models produce much higher values over the Baltic for 30-year return values than what the authors show for 50-year return values. Especially in coastal areas, I wonder how much of this is due to the ERA5 resolution.

I would ask the authors to check if the other datasets produce similar uncertainty range to the return period values and/or if the peculiar shapes could be attributed to the assimilation methods used in ERA5 (would be an important result I think). Use of CERRA dataset might help with robustness. I also encourage the authors to explore other statistical methods to narrow down the confidence interval, although I don't have direct suggestions here.

Some of the other variables show similarly variable uncertainty fields, some of which might be understandable and linked to strong gradients, but for example the one for maximum SLA is rather difficult to grasp. Why is the relatively low return period in the central Baltic so uncertain (by well over a meter)? Again, I think it would be important that the authors review their statistical methods and aim to narrow the uncertainty range and/or explain where the uncertainty comes from.

Response We thank the reviewer for this important comment. We agree that the original manuscript did not explain the large and spatially irregular uncertainty fields sufficiently, especially for the Baltic-wide wind-speed return levels and maximum sea-level anomaly. We have revised the manuscript to make clear what the uncertainty maps represent, why some regions have large uncertainty, and how the statistical treatment was checked.

The wind-speed return levels shown in the Baltic-wide maps are based on 10 m ERA5 wind speed at hourly resolution and should be interpreted as regional-scale return levels of gridded hourly wind speed, not as gusts, 10-minute station maxima, or engineering wind loads at other elevations. ERA5 provides hourly atmospheric fields on a regridded 0.25° grid, and its spatial resolution smooths small-scale coastal and convective wind maxima. We have therefore revised the text to avoid direct comparison between the mapped values and observed gusts or short-duration wind maxima.

We also now explicitly acknowledge that ERA5 may underestimate the strongest wind events in the Baltic Sea. Also assessment of ERA5 wind with open-sea wind fields from ASCAT showed that correction of +2.5 m/s is needed to bring the wind speed extremes to the observed level (Sup1). This means that the ERA5-based wind return levels should be interpreted as internally consistent regional screening estimates rather than definitive local design wind values.

We have clarified that the wide confidence intervals arise mainly from three factors: the relatively short record length for 50-year return-level estimation, the use of a three-parameter GEV model at individual grid cells, and the influence of one or a few major storm years on the annual-maxima series. A 50-year return level estimated from approximately 30 annual maxima is necessarily extrapolative. In some grid cells, especially near complex coastlines, in transition zones between land and sea, and in semi-enclosed sub-basins, the fitted GEV shape parameter is weakly constrained. This can produce broad confidence intervals and spatially noisy uncertainty patterns. We now explain this explicitly and treat these areas as regions of high statistical and structural uncertainty rather than as precise local estimates.

We have also corrected the interpretation of the confidence-interval maps. In the original wording, some values were written in the form " 28 ± 10 m/s", although the figure showed the full width of the 95% confidence interval, not necessarily a symmetric $\pm$ half-width. This was potentially misleading. In the revised manuscript we report return levels and uncertainty as "95% confidence-interval width", not as a symmetric $\pm$ value unless the half-width is explicitly used.

Regarding CERRA, we agree that it is a valuable high-resolution atmospheric reanalysis and may be useful in future Baltic Sea wind-extreme studies. CERRA is produced at 5.5 km horizontal resolution and was designed to add value relative to ERA5, while being driven by ERA5 at the lateral boundaries. However, we have not added CERRA to the present manuscript because the Baltic-wide metocean analysis is based on an internally consistent set of ERA5-forced marine products. The CMEMS Baltic wave hindcast is based on WAM and ERA5 atmospheric forcing, and the CMEMS Baltic physical reanalysis is a NEMO-based product.

Adding CERRA only for the wind-speed map would therefore not reduce the uncertainty of the coupled metocean interpretation. It would instead create an inconsistent framework in which wind extremes were derived from CERRA, while wave, sea-level, and current extremes remained derived from ERA5-forced marine products. A physically consistent CERRA-based assessment would require CERRA-forced wave and hydrodynamic hindcasts, followed by validation against Baltic Sea wind, wave, sea-level, and current observations. That exercise is outside the scope of the present site-focused metocean study.

For maximum sea-level anomaly, we agree that some uncertainty fields were difficult to interpret. We have revised the text to clarify that the Baltic-wide sea-level analysis is based on daily CMEMS physical-reanalysis output, whereas the local FSRU site analysis uses higher-temporal-resolution local sea-level data. Daily fields can smooth short-lived storm-surge peaks and may therefore underestimate local high-water extremes compared with hourly tide-gauge records. The uncertainty in the maximum

sea-level anomaly maps is also strongly affected by the dominance of individual major storm-surge years in narrow sub-basins such as the Gulf of Finland and the Gulf of Riga.

10) Data Availability statement. I would ask the authors to share the resulting return periods (i.e. data that can be used to reproduce figures 2-3 and especially 4), their uncertainties, along with codes to reproduce them from original data. This could be done via Zenodo repository. Sharing the data and the codes is a standard today, and allows the interested readers to understand what has been done exactly (especially since the manuscript has no equations), and since the resulting data can be very useful, and is not very large, I see no reason why the authors should not share it.

Response: We agree. In the revision we will substantially improve transparency and reproducibility by providing (i) the derived return level fields and uncertainty bounds used in the figures and (ii) the analysis scripts required to reproduce the processing and EVA from the original Copernicus and ERA5 products, subject to the licensing terms of the input datasets. We have uploaded the derived data products and scripts in a public repository (Zenodo) including a short README describing the workflow, dependencies, and steps to reproduce the key figures.

11 Aug 2025

Uploaded files validated by Polina Shvedko

Notification to the authors:

1. A "Short summary" system section contains scientific abbreviations. Please be aware that if you included scientific abbreviations (excluding chemical elements) without providing their full written explanations, you must write out them in full in your next file upload request. However, do not forget that there is a limit to characters (not words!) for "Short summary": it must be < 500 characters. 2. The section "Author contributions" with the authors' initials must be included in *.pdf manuscript with the next revision.

Response: We have edited the short summary to fit within the 500-character limit. We have also added an **Author Contributions** section at the end of the manuscript.