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Interactive comment on "Variability of distributions of wave set-up heights along a shoreline with complicated geometry" *by* Tarmo Soomere and Katri Pindsoo

Anonymous Referee #3

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Review comments for the manuscript "Variability of distributions of wave set-up heights along the shoreline with complicated geometry" in Ocean Science Discussions (os-2019-25)

The authors present wave set-up calculations based on pre-computed wave model results for a shoreline with complex geometry located in the Gulf of Finland. The wave set-up distributions are based on long time series (1981-2016) and they are calculated for separate segments along the coast. The wave set-up is approximated by accounting also for varying incident angles, and the approximation is compared to a more simple approach. The main finding is that while the wave set-up distribution is well

represented by a standard exponential distribution in most segments, in about 25% the distribution does not follow any standard distribution (Gaussian, Weibull, Rayleigh). At these locations the distribution is fairly well approximated by an inverse Gaussian distribution, meaning that the probabilities of the highest wave set-ups are higher with respect to the mean value compared to e.g. a Rayleigh distribution.

The paper is mostly well written and within the scope of Ocean Science. The results are also interesting. I can recommend that this paper is published in Ocean Science after major revisions. I have three main concerns. The first is that this paper is apparently a resubmission, but the results and conclusions have changed since the first paper, and I don't know what to make of this. The second concern is regarding the pre-calculated wave data. The third is how the distributions are presented and, to a degree, interpreted.

Main comment #1: The manuscript seems to be a resubmission of this paper: https://www.earth-syst-dynam-discuss.net/esd-2016-76/. Still, the previous version of the paper presents different results, suggesting that the relevant distribution is inverse Gaussian at all locations instead of just 25%. The papers seem to be based on the same data, so I don't know what has changed. The first version of the paper claims:

"The distribution of wave set-up heights matches a Wald (inverse Gaussian) distribution along the entire study area. Even though different sections of the study area are open to different directions and host substantially different wave regimes, the leading term of the exponent in the associated inverse Gaussian distribution varies insignificantly along the study area and generally is close to -1."

The current version claims:

"Even though different segments of the study area host substantially different wave regimes, the leading term of this polynomial is usually small (between –0.005 and 0.005) and varies insignificantly along the study area. Consequently, the distribution of wave set-up heights substantially deviates from a Rayleigh or Weibull distribution (that

15 usually reflect the distribution of different wave heights). In about $\frac{3}{4}$ of occasions it is fairly well approximated by a standard exponential distribution. In about 25% of coastal segments it matches a Wald (inverse Gaussian) distribution. “

The authors should make it clear why their results have changed from the previous paper.

Main comment #2: It is not clear from the paper how the wave data has been obtained or how accurate it is. The main assumption is that the wave data in the nearshore areas reach a steady state quickly, which is why pre-computed maps can be used. I assume the maps are chosen based on the wind direction and the wind speed. Still, has the runs themselves been modelled using wind data from a numerical weather prediction model? If not, how have the Baltic Sea wide simulations been produced that provide the boundary data? Data from atmospheric models have been used by several studies in the Baltic Sea (including nearshore areas) and it is the industry standard. There is no reason to not produce the maps using proper wind data.

Are you using WAM 4.5.1 or a later version? Cycle 4.5.1 had a bug in the depth-induced wave breaking source term that lead to unphysical high values at certain points. It has been fixed in cycle 4.5.4. This must be ruled out as an explanation for your surprising results.

Using Kalbådagrund data to force nearshore areas will overestimate the wind speed and probably result in an inaccurate wind direction. The authors cite some of their previous work, but I think some more details are needed also in this paper. The validity of the modelling approach comes into question exactly because the authors present surprising results. Running a high-resolution model for 35 years is clearly a huge task. Creating the maps with a proper forcing is not. I think the Estonian Weather Service runs HIRLAM, which has been tried and tested in the GoF. A minimum requirement would be that the authors should identify the times that are responsible for creating the “surprising” high wave set-ups and validate the instances by running the “full model”.

This will satisfy that, to the degree we can trust wave models, the surprising results are real.

Main comment #3: Usually the distributions are viewed and fitted by looking at the cumulative function, not the probability density function. The cumulative function is more stable, and I don’t see any reason not to use it here. The comments regarding “gaps” in the distribution are also confusing. What is the physical meaning of these gaps? The “first gap” is controlled by the accuracy of the wave data (10 cm, 1 cm etc), so why is this a meaningful point? There is also no reason to expect that experimental/modelled data should be without these gaps, so the existence of one is not a defining point in the distribution.

I am also a bit worried about the highest points being correlated. Is this the case? Then one event that is overestimated might “destroy” the entire distribution. Are the highest values recorded during the time ice was present? This needs to be evaluated carefully. If you have found that the distribution of the wave set-up is really completely different for two neighrouring grid points about 500 m apart, then also give an example of the detailed shoreline structure that leads to such a dramatic variation so that the reader can be convinced what is going on and find some physical basis for this change in behaviour. Providing the structure of the shoreline that leads to this inverse-Gaussian behaviour would make this study highly useful for other coastal areas.

It is unclear how the second order polynomial fitting is done. It seems like you are fitting a polynomial to the data in the log-linear plot (is the variable p the probability density?). Please write the formula for the exponential distribution and how the polynomial fits into that, and for what fit this generalized exponential function becomes the “normal” exponential function. This will make it so much easier to follow the discussion and agree with the conclusions.

I will save minor comments for the revised manuscript.

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