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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

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We very much appreciate the comments of the Referee and are grateful for the overall positive attitude to the manuscript.

Main comment #1: The manuscript is a substantially revised version of our paper titled "Inverse Gaussian distribution of wave set-up heights along a shoreline with complicated geometry", originally submitted to Earth Systems Dynamics and available at https://www.earth-syst-dynam-discuss.net/esd-2016-76/.

In our cover letter we informed the editor of Ocean Science about the history of the

manuscript.

The Editor of Earth System Dynamics, Dr Anna Rutgersson mentioned in her final comment that "the presentation of the methods and results was not very clear" but encouraged some rewriting and a resubmission (which we understood as a hint that the paper could be better placed elsewhere).

The rejection from Earth Systems Dynamics was, in some ways, fortuitous. Namely, during the rewriting and additional check of the results we discovered a bug in the script for the calculation of set-up heights and for the subsequent evaluation of the parameters of their probability density function.

Removing this bug lead to the conclusion that typically (in about 75% of coastal segments of the study area) set-up heights follow a classic exponential distribution but still in about 1/4 of the cases they follow a Wald distribution. In this sense the conclusions have changed significantly and it might be sensible to retract the early version from the website of Earth Systems Dynamics. We are willing to do so if applicable and acceptable according to the relevant policy of Earth Systems Dynamics. Alternatively, we can discuss this issue in the current paper, with our apologies to the readers of the discussion version in Earth Systems Dynamics.

We also note that the manuscript under consideration has been submitted to Coastal Engineering but was rapidly returned because the editors felt that the results did not have clear direct applications for engineering.

Main comment #2: We admit that a weak point of this manuscript is the way we produce the wave data. A detailed simulation of the Baltic Sea wave time series over several decades with a resolution suitable for the study area (grid size less than about 500 m) is an enormous task. It is technically and computationally possible but, to our understanding, not strictly necessary in the context of our manuscript. The reason is that the presented results only depend on statistical properties of wave fields. The joint probability of occurrence of seas with different wave height, period, and direction in the nearshore is already sufficient for estimates of probability density functions of wave set-up (provided the change in wave properties owing to refraction and shoaling is adequately represented). We understand that our approach does not guarantee that these properties of the wave climate are exactly represented; however, a reconstruction of time series of wave properties is not necessary to support the results presented in our manuscript. Even though our approach for wave modelling seems oversimplified, we still think that it makes it possible to reveal, perhaps in a relatively contrast manner compared to the real situation (because sea ice is ignored), the basic properties of the "climate" of wave set-up.

The representativeness of modelled wind data is still problematic in areas such as the Gulf of Finland. As it is an elongated water body, replication of wind directions is crucial. Research in the 2000s has indicated that atmospheric models nicely reproduced wind speed but the wind directions for a certain range of directions had systematic deviations from the measured directions (Ansper and Fortelius 2003). It is of course a question of judgment which data better represent reality.

Another, more serious problem is that coastal measurement stations on the southern coast of the Gulf of Finland (that is, in the study area), on many occasions, completely failed to represent offshore wind properties (Keevallik, 2003; Soomere and Keevallik, 2003). Even though the atmospheric models have become much better over the last 15 years and new physics, data sources and assimilation procedures have been added, reconstructions of older wind fields still rely on the same ground truth. It is therefore our view that the use of even high-quality modelled wind data for the research in this manuscript would basically replace one source of uncertainty with another.

The wave patterns have been calculated using several realisations of early versions of cycle 4 of the WAM model. They may have older versions of wave physics (in particular, they tended to overestimate wave heights in shallow areas) but, to our knowledge, they did not contain any major bugs. All nearshore grid cells are located at depths >4 m and most of them even in deeper than 8 m sea. Given the short wavelengths of severe

seas in the study area (usually <6 s), major problems with incorrect representation of wave properties in shallow areas are unlikely (albeit not totally excluded). To be on safe side in this respect, we will indicate the model water depth along with the presence of a Wald distribution.

As mentioned above, we fully agree that the use of alternative wind data would considerably strengthen the message of the manuscript and that the identification of the origin of outliers of set-up heights is necessary. However, we would like to stress that the shape of the probability density function is evaluated using relatively frequently occurring set-up heights. Moreover, our analysis explicitly discards all very large examples of this height. We only stress in the text that if the set-up heights really follow a Wald distribution, unexpectedly large values of set-up would be more probable than in the case of an exponential or Gaussian distribution.

Main comment #3: It was our intention to understand which type of theoretical distribution should be used for the description of set-up heights. For this reason we specifically analysed the shape of the probability density function as a clue to the answer. The problem is wider. Namely, other drivers of high local water levels and wave attack have very different distributions. Water volume of the entire Baltic Sea follows a Gaussian distribution, storm surges are Poisson-like processes reflected by an exponential distribution, different cases of significant wave height are likely to be Weibull (or Tayfun) distributed, and a Weibull or Rayleigh distribution usually works for wave run-up.

In this context it would be nice to demonstrate that set-up heights follow one of these. This is, however, not always the case. To our understanding, the most convincing way of proving that is to directly estimate the shape of the governing part of the relevant distribution. As this point was also raised by other Referees, we will include some information about the behaviour of the cumulative distribution functions.

The gaps in the calculated empirical distributions certainly depend on how the "classes" of the set-up height are defined. In a physical interpretation, the presence of such a gap shows that the number of occasions for the relevant set-up height (and for the even higher values of set-up) is too small for the use of this value in the estimates of the shape of the entire distribution. In other words, we think that the distribution we are looking at has large uncertainty for set-up values that are higher than the first gap. Moreover, very large set-up heights may be parts of a population that follows some extreme value distribution. Visually, these very large values would all support the match with a Wald distribution. To avoid "false positive" detections of this distribution it is better to look at the distribution of relatively frequently occurring set-up heights. This is especially important in the log-linear representation where data points based on a few outliers may easily override the behaviour of data points that reflect thousands of cases.

The issue of potential serial correlation has also been touched on by Referee #2. Serial correlation is not a problem in our study. In particular, the use of 3-hourly wind data and the assumption that the memory of wave fields is short already removes most of the correlations between the highest set-up events. The typical duration of severe wave conditions in the study area is just a few hours. To maximally remove the impact of serial correlations, we limit the consideration of the shape of the probability density function to the relatively frequently occurring set-up heights.

We are thankful for the suggestion to describe in more detail the link between the geometry of the particular coastal segment with the presence of a Wald distribution. We shall definitely do so. We also apologize for the quite compressed manner of presentation of the background mathematics and the details of fitting, and will describe the procedure in more detail.

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