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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 #1

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Title: Variability of distributions of wave set-up heights along a shoreline with complicated geometry
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Discussion paper

General comments

The study is aimed to investigate the alongshore variability of the empirical statistical distribution of maximum wave set-up occurrence in a morphologically complicated situation. The area in exam is embedded in the Gulf of Finland, in the eastern Baltic Sea. The selected shoreline has been divided in very small segments, each containing the coastal grid output of a triply nested climatological run of the WAve Model. The maximum set-up has been calculated algebraically from the properties of the wave field at the breaker line, the water depth and the orientation of the shoreline in each segment for each member of the climatology. At each segment, the frequency of occurrence is then plotted against the simulated maximum wave set-up and a quadratic-exponent (three parameter) law is fitted to the data. In 3/4 of the segments the higher-order coefficient is found equal to zero at the 95% confidence level. In all the other cases, the leading quadratic coefficient is not null at the 95% confidence level, so a Wald (invert Gaussian) distribution is assumed. The method used for the evaluation of the wave set-up is fairly standard and consistent, the statistical analysis is rather on the qualitative side, but the results show a sort of internal coherence. There are in my opinion several problems in the study that should be addressed in order to improve the quality of the work. Some of the problems, listed as points in the specific comments, would probably require some further analysis on the data, some work on the figures and a general review of the text. Many weaker points are listed in the technical corrections.

Specific comments

1. In my opinion the main problem is in the results: it was found that 3/4 of the coastal points have an exponential distribution, while all the others have a completely different distribution. But there is no clear indication in the study about the reason behind it. There are some educated hypothesis, but no direct link is provided which can relate the type of distribution with some physical quantity like the angle of approach to the coastline, the wave climate or the bathymetry. This means there is no way to generalise the results outside the area of interest. As a result the work may have a distinct 'geographical' interest but is affected by a lack of 'physical' significance. Further effort should be devoted to understand the reason.
2. The value of the leading coefficient in the quadratic expression in the exponent of the distribution is found remarkably close to zero in all cases, but it is not zero at 95% confidence level in a large fraction of cases. 95% is pretty high but it is not a matter of faith. My impression is that the results would change significantly if different levels were chosen, i.e. 98% or 90%. The dependence of the results (in terms of the number of cases having exponential or else distribution) on an arbitrary choice would show a weakness of the method, indicating a lack of robustness in the statistical analysis.
3. By the way, I would have expected as a first tentative analysis the standard extreme value approach, using a fitting of the empirical CDF by means of plotting position functions. It is less subjective than the method used in the present study and has the added value to introduce a return period, which would be welcome in this case. With 3 parameters at disposal and plotting over a log-log scale, it takes a lot to discard a Weibull distribution. If all cases could be described with a similar distribution then the observation at 1. would be irrelevant.
4. Some of the panels in figures 6,7 show that the higher values of the set-up have the same probability. This is very odd, and led me thinking if there might be some problem with the independence of the data. It looks like the entire block of data would belong to the same storm. In the description of the methodology it should be described in detail

how the problem of the serial correlation of the data has been taken care.
5. The wind data gaps are a big problem if they are systematic in the upper percentiles. It should be taken care of in some way, and discussed in the conclusion.
6. Some figures are very hard to read, in particular figure 4. What is the rationale about the choice of the cases illustrated in figure 6,7?
7. There are some technical points which should be better explained, in particular the presence of 'data gaps in the distribution (the lowest set-up height that did not occur in 1981-2016)' at page 11. Is the statistical analysis in the range [0.01-0.4] really necessary? How the angle of incidence with the normal to the coastline was evaluated? How the phase and group velocities were estimated?
8. The language should be improved.

Technical corrections

There are many slightly inaccurate statements in the text which should be adjusted:
1 p.1 line 29: actual tides are not perfectly regular in many coastal areas (astronomical tides are).
1 p.2 line 16: 'neither completely independent nor completely dependent' does not give a lot of information.
1 p.2 line 19: significant wave heights. And it must be defined somewhere, because there is no definition in the manuscript. Add a symbol like H_{m0} if spectral. What is H_0? Use it in all the manuscript consistently.
1 p.3 line 16: Normally instruments and model refers to statistical properties of wave fields: significant wave height, peak period, mean period and mean direction.
2.1 p.4 line 25: reference not found.
2.1 p.5 eqn (2): Here averaged eta is a function, it is customary to indicate the arguments in parentheses.

2.1 p.5 lines 5-6: the meaning of ‘formal’ is unclear, the choice of ‘formal’ and ‘actual’ is not particularly fortunate.
in Figure 1: introduce the axis - it is not obvious the sign of h and eta, introduce d and d*.
2.2 p.6 line 8: Wave directions? wind?
2.2 p.6 line 9-10: Suggestion:it is possible to analyse the set-up for different values of forcing and wave propagation geometry.
2.2 p.6 line 13: Highest significant wave height is sufficient.
2.2 p.6 line 14-16: In meteorology it is customarily is to indicate wind coming from west as westerly, wind going toward west as westward. Eastern storm is unclear.
2.2 p.6 line 28: It is not actually the model implementation and it does not increase the efficiency of the model. It is a simplified method of reproducing the wave climate avoiding to processing all the time series.
2.2 p.7 line 6: That is an understatement. The wave simulation depends on wind, if the wind is not adequate the simulation is just noise.
2.2 p.7 line 6: ‘In particular..’ actually that is a completely different matter.
2.2 p.7 line 7: Someone might argue that wave directions and propagations in shallow waters and complex morphology depend more on bathymetry than on wind direction.
2.2 p.7 line 19: This is a huge problem for the statistical analysis. In my opinion every other choice (interpolation, replacement with model data, looking for other sources of data) would be better than simply not considering the data corresponding to gaps. see point 5.
2.3 p.7 line 21: Suggestion: in water depth $>$4 m
2.3 p.7 line 25: See note 2.2 p.6 line 14-16
2.3 p.7 line 28: Suggestion: oversimplified
2.3 p.7 line 31: How could significant wave height be monochromatic? ‘As usual’ is not enough to justify the assumption.
2.3 p.8 line 1: The mean wave direction provided by the model is not referred to the normal to the shoreline. This derivation must have been a successive operation which

should be described appropriately.
2.3 p.8 line 7: If H_b is a water level and H_0 is a significant wave height they are different quantities having the dimension of length.
2.3 p.8 line 7: Missing reference
2.3 p.8 line 8: It should be explained how the phase speed and the group velocity were evaluated.
2.3 p.8 line 14-15: How the assumptions might affect the results? to be discussed in the conclusion.
2.3 p.8 line 14-15: But it is used in the successive section, isn't it?
3.1 p.9 line 5-15: See previous observation. The 'simpler method' is used throughout the manuscript: it should really be described better. Fig.4 is very hard to read but it gives the impression that the results are very different. The text is rather confusing and seemingly incoherent. One is tempted to understand that the set-up is greater for greater angles of incidence than for normal waves. It may be worth to observe that numerical statistical models like WAM are not able to deal with diffraction and reflection of waves.
3.2 p.10 line 7-10: It is not clear how to verify the statement, the analysis is rather qualitative and the figures describe only a very small part of the set of 174 segments. Suggestion: replication $\rightarrow$ simulation
3.2 p.10 line 18: The discussion seems to exclude the possibility that somewhere in the whole region considered there could be a poisson process, which is contradicted by the results in p. 11 line 10-15.
3.2 p.10 line 24: Suggestion: approximation $\rightarrow$ fitting procedure
3.2 p.10 line 28: 'Unexpected' does not explain the reason of the high values. On what basis the high values are assumed outliers? Looking at fig. 7, if the range of the setup is considered only in the range [0.01,0.4] maybe the distribution could have been 'forced' to be exponential. This part of the text is not sufficiently clear.