Review comments to "Variability of distributions of wave set-up heights along a shoreline with complicated geometry" (os-2019-25) by Tarmo Soomere, Katri Pindsoo, Nadezhda Kudryavtseva, Maris Eelsalu

I have reviewed a, once again, revised manuscript that presents how wave set-up is distributed in the southern shore of the Gulf of Finland. I want to thank the authors for putting in the effort to making substantial improvements. Especially the statistical tests are welcome and add rigor to the work. I feel my main concerns have been answered, and I can therefore recommend that this work should be accepted after minor revisions.

There are still quite large uncertainties in how reliable some of the results are, but think that the improvements made by the authors have made them more transparent in that the reader now has a better possibility to put the results in context. I think that few definite conclusions can be done based on this study, but it documents interesting findings and ideas that can possibly be studied further in other areas that e.g. have more validation data available.

My comments:

Comment #1
P18, L11: "Selected segments"

How are these selected? Or are you simply trying to say "some segments"?

Comment #2
P18, L13-14: "it is likely that they mainly stretch the resulting distributions of set-up heights towards larger values but do not modify their basic shapes."

Do you have a citation for this, or is this just your speculation? If the latter, I think it should be conveyed more clearly to the reader.

Comment #3
P6, L26 We employ time series of wave properties (significant wave height, wave period and propagation direction)

Perhaps write that it's peak period and mean direction (I know you mention that later)

Comment #4
page 7 line 18 "The nearshore of the study area is divided into 174 coastal segments"

Do you mean "The shoreline of the study area"?

Comment #5
P7, L19-21 "Ignoring the presence of sea ice may lead to a certain overestimation of the overall wave energy in the region but apparently does not significantly distort the shape of the probability distribution of different wave heights (Fig. 3)"

There are so many factors that impact the differences here, that drawing conclusions about what the role of ice is from this is a bit questionable. Perhaps just state "*Ignoring the presence of sea ice may*

lead to a certain overestimation of the overall wave energy in the region.", as one of the uncertainties that exist?

Comment #6
P7,L32-P8,L9

Here you explain why you didn't use numerical atmospheric models. I find this "model bashing" a bit uncalled for and you don't actually need it. The model of Keevallik and Soomer (2010) used was a 11 km HIRLAM run for 2007-2008. Today the current Harmonie-model has a resolution of 2.5 km (I think). Some studies using atmospheric models as forcing has also showed an accurate wave direction entering the coast (in the GoF, at least Björkqvist et al. 2017, Fig 2). Can't you just mention that Kalbådagrund provides an accurate direction, which is something that you need for this study? (PS. Didn't have access to Nikolkina et al. 2014)

Björkqvist et al. (2017): Improved estimates of nearshore wave conditions in the Gulf of Finland. https://doi.org/10.1016/j.jmarsys.2016.07.005

Comment #7
P9, L30 and P10, L1

Why do you introduce additional symbols for the significant wave height?

Comment #8
P12, L1-3 "The processes that are not resolved by phase-averaged wave models such as reflection and diffraction may add even more wave energy to seemingly sheltered coastal segments."

This is not entirely true. From the SWAN manual (bolded by me):

The following wave propagationprocesses are represented in SWAN:
•propagation through geographic space,
•refraction due to spatial variations in bottom and current,
•**diffraction,**
•shoaling due to spatial variations in bottom and current,
•blocking and reflections by opposing currents and
•transmission through, blockage by or **reflection against obstacles.**

(SWAN Scientific and Technical documentation, http://swanmodel.sourceforge.net/download/zip/swantech.pdf)

Perhaps just mention that they are not typically simulated by phase-averages models? You are, however, correct that diffraction is not "resolved" but parameterized in spectral models, but so is almost everything else. Reflection, however is probably resolved (haven't checked to code).

Comment #9
P12, L20 "These aspects will be addressed in more detail elsewhere."

Where? Also the entire section (lines 10-20) seems more like it belongs to the discussion.

Comment #10
P12, L22-25

I think the point was that the bins might be larger, not smaller, than 1 cm. Also, doesn't this belong to the part starting on page 14 line 7?

Comment #11
P13, L9 "(grid point 106, Fig. 6d)"
P13, L15 "(e.g., grid point 129, Fig. 6e)"

Should be 7d and 7e?

Comment #12
P13, L17 "(e.g., grid point 1, Fig. 7)"

Should this point to another figure?

Comment #13
Fig 8: Suggestion, put the value for the parameter "a" here to guide the reader to which way positive and negative values of a makes it bend?

Comment #14
Several references have incorrect formation in the doi: eg. Folkjs & Chhikara "https://doi.org./ 10.2307/2984691". Please go over the details of the references once more.

Comment #15
Make Figure 11 a four panel figure with one panel for each distribution? The panel with five distribution on top of each other is hard to read.

Comment #16
The title *"3.3 Fitting the empirical distribution with theoretical distributions"* doesn't really work, because you have been fitting distributions already in section 3.2. Perhaps one section with just the title "Testing goodness-of-fit", and the try to organize the other stuff logically before that?

Comment #17
I think this paper needs a separate section for discussion and conclusions. There is so much stuff and so many uncertainties, that you need to end with a section just for conclusions without muddling it up with discussion to answer "what can we actually take home from this study?"

I also have some general remarks that are more suggestions in that it is ultimately up to the authors to decide when they are satisfied with the structure of the manuscript. I do, however, feel that it would strongly be in the authors best interest to give it one more critical thought. The writing is sometimes quite verbose, and adding discussion in the results, along with some technical calculations, makes the reader lose the bearing easily. This isn't helped by the quite long, and sometimes weirdly names, sections.

Suggestion #1

Get rid of the general results section 3, and split that up into a couple of sections with clear titles.

Suggestion #2
P14, starting from line 7. This is mostly a sensitivity test. Would it be possible to put this into an separate appendix?

Suggestion #3
Subsection *2.2 "Wave time series in the nearshore of the study area"* is long, contains different subjects, and doesn't have a descriptive title. Perhaps split it up to one section for the "Study area", one for "Model implementation", one for "Measurements", and one for "Model validation" (I don't know if this would work exactly, but you get the point).

Suggestion #4
I think a lot of the text and stuff from the results section actually would belong to the discussion. Streamlining results, concentrating discussion to its own section, and ending by a well formulated conclusion section would make this paper a lot more approachable.

Suggestion #5
Have you though about having two subsections in the discussion? One discussion the stuff regarding the calculation of the set-up and the other about the distributions. Again, this would add a ton of structure to the latter part of the manuscript.