Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-59-AC1, 2019

Ocean Science
Discussions

Interactive comment on "The wave spectrum in archipelagos" *by* Jan-Victor Björkqvist et al.

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Received and published: 12 September 2019

We would like to thank the reviewers and the editorial staff for the swift and timely handling of our submission.

Anonymous Referee 1

R1: General Comments This is an excellent paper, adding new observational insights into wave fields in archipelagos, adding substantial new insight to an already large volume of wave observation literature. Well done!

Our response*: We are pleased that you liked our manuscript. Thank you for taking the time to review it.*

R1: Specific Comment / questions

R1: Page 5, section 2.3.1: In computing wave parameters from the spectrum, do the authors use a parametric tail, or is the integration stopped at the highest frequency of the observations. See also Page 3, line 16.

Our response*: We chose not to use a parametric tail in this study. The reason is that the material we present on the wave spectrum in the archipelago are new, and we want our results to reflect only the measurements, not any theoretical assumptions we have made about the tail of the spectrum. That said, an f^{-4} parametric tail would be very reasonable if such an approach was taken. We feel that it is better to leave the uncertainty that stems from the (at times) poorly measured tails in our results. Nonetheless, this limitation should be stated more clearly (please see our response to your next comment).*

We have edited the manuscript to clearly state that no parametric tail was used (Page 3, line 23-24 and page 7, line 6)

R1: Page 7, line 13: Very small wave heights are not used, but in Table 1 mean wave heights in the sheltered locations is still as small as 0.05m. Can such small mean wave heights in the sheltered areas be trusted?

Our response*: This is a fair point. We think that our results show that they cannot be completely trusted, which is seen in that the attempts to quantify the spectral narrowness κ^2 and the degrees of freedom in m_0 gives conflicting results with respect to the other locations (Table 2). We still wanted to include them for two reasons: i) we think they add value in the (partially qualitative) analysis in Figure 2, and ii) we think it is also important to present the limitations and challenges in measuring the wave field inside the archipelago using standard instrumentation.*

Referee 2 also commented on this issue. Please also see our response to that comment.

We have edited the manuscript to more clearly state the limitations of the measure-

ments at locations S1-S4 (page 12, line 13; page 13, lines 20-24 and page 27 lines 10-18).

R1: Page 10, lines 15-18: How did the authors concluded that peaks in the spectrum were due to refracted wave components?

***Our response**: A directional spectrum from Jätkäsaari (I3) has been presented in a conference paper that studied the reflection of waves from a steep shore (Björkqvist et al., 2017). There the directional separation of the two wave systems are clearly visible. The narrowness (in frequency) of the wave component is because wave slightly shorter (or longer) are refracted in a different manner and therefore propagate to a slightly different location.*

We have added a reference to the above-mentioned conference paper on page 11, lines 22-24 of our manuscript.

R1: Page 23, Section 5: I enjoyed the discussion of defining a representative frequency. It seems to me however, that the focus is mostly on getting a table parameter. Coming from an operational / application side, the usage of the parameter should be considered too (erosion, loads, roughness, etc.).

***Our response**: We regret that the fundamental nature of the study might leave the practical considerations slightly lacking. We tried to tie the properties to practical issues in Section 4, but the nature of the characteristic frequency is admittedly a bit light. While we think that the role of the characteristic frequency will be central, even if it in itself cannot function as the sole quantifier of an archipelago wave field.*

To illustrate our point we calculated the root-mean-square near bottom velocities (U_{rms}) from the spectra at Suomenlinna (T2), and these values were compared to a simplified monochromatic approach (see the Figure below). The comparison shows the comparison for both the peak frequency and the new characteristic frequency. While we could argue that the new frequency reduces the scatter somewhat, the truth is that

both approaches are fundamentally wrong in an archipelago setting, and goes against the core idea of this paper.

The main point is that the characteristic frequency defines where the energy is concentrated in some absolute sense (in Hz), while the spectral width defines how widely the energy is spread around this particular frequency. Using only one or the other can clearly not capture e.g. wave-bottom interactions. More elaborate parametrizations exist, but they can depend on some assumed spectral shape. The correct approach to build a parameterization for the archipelago would therefore require us to find an analytical functional form that uses the total energy (or H_s), the characteristic frequency, and some kind of spectral width. One reasonable parameterization would then be to approximate the spectrum with a box covering the dominant frequency interval of the spectrum, and deduce some expression for U_{rms} from that.

Many practical applications of the characteristic frequency therefore depend on finding a good functional form for an archipelago type spectrum. This is to a high degree not trivial. We are also reluctant to present any results that we know to be, in a theoretical sense, flawed.

What we can do is to make it clear in the discussion (Section 5.2) that from a practical point of view the results about the characteristic frequency is still a stepping stone, and outline how the results in this paper provides some tools for further, both theoretical and practical, research. We have also edited the end of the abstract and the conclusions to this affect.

R1: Page 26, Section 6: The data selection results in wind sea presence in all cases. This focu on wind seas needs to be repeated in the Conclusions.

***Our response**: This has been added to the conclusions. (Page 28, lines 3-5)*

R1: Technical Correction

R1: Page 3, line 8: Add text in red “While observations at 12 out of 14 . . .”

***Our response**: Thank you for catching this. The sentence has been corrected in the manuscript.*

R1: Page 3, line 29: ". . .time series, used to compute a single wave spectrum."

***Our response**: This sentence has been corrected in the manuscript.*

R1: Page 7, line 16; please confirm that the 80th percentile is relative to the entire time series at the given location.

***Our response**: We have modified the manuscript to state that the 80th percentile is relative to all the available data for the archipelago measurements (page 9 lines 17-18). For the GoF the 80th percentile is calculated from the years 2016-2018 to be comparable to the Suomenlinna (T2) measurements.*

R1: Page 14, Table 4: Labeling of panels is not consistent. Why is (a) labeled d.o.f, whereas all other panels are d.o.f. too?

***Our response**: You are right. We have changed the label in panel (a) in Figure 4. It now reads "Wind direction U_d"*

Other changes in the manuscript:

1) *Page 7: We added a subscript "BFI" to the variables α and β in Eq. (13), since the variable β is used in Eq. (14) in a difference context. The variable α is used in another context in Eq. (19).*

2) *Page 8: We have changed the normalization of the spectra slightly by defining the coefficient β as a mean over $E(f)f^4$ instead of a mean over only $E(f)$. This has no real consequences for the results (which can be seen from the redrawn Figures 2 and 3), but this approach is slightly more flexible. From a theoretical point of view the averaging can now be done over different frequency intervals for each spectrum, if we know that a f^{-4} tail exists. While we use a fixed frequency in this paper, we wanted to introduce this slightly more general method since it might turn out to be useful in later studies,*

and our upcoming results will then be more consistent with the current ones.

3) *We added the information that two of the measurement sites were made for research purposes outside the commissioned work by the City of Helsinki. This has no consequences for the paper, but we wanted to represent the data more accurately (page 3 lines 12-14).*

4) *We corrected that the Harmaja wind station is 2 km from the Suomenlinna (T2) wave buoy (the Harmaja wave buoy was roughly 5 km from the T2 wave buoy, which is where the incorrect distance stemmed from). (page 5, line 3). This has no consequences for the results of the paper.*

5) *Some minor changes to the language and adding sentences explaining the reasoning. These are visible in the track changes version of the manuscript.*

References:

Björkqvist, J.-V., Vähäaho, I., and Kahma, K. K.: Spectral field measurements of wave reflection at a steep shore with wave damping chambers, in: WIT Transactions on the Built Environment, vol. 170, pp. 185–191, doi:10.2495/CC170181, 2017.

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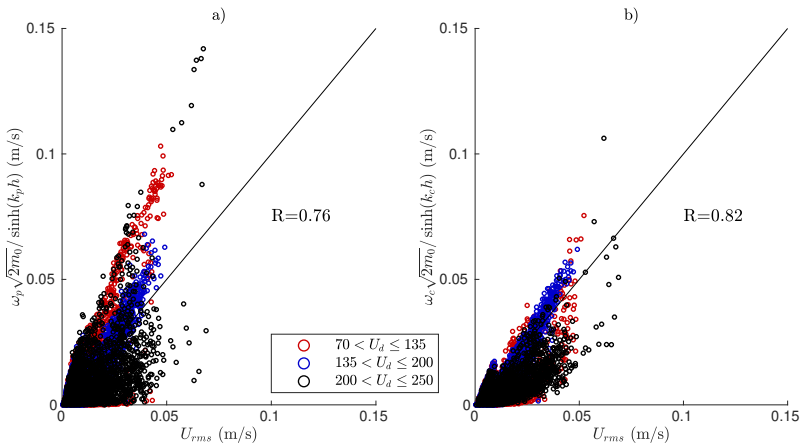

Fig. 1. Near-bottom velocities (Urms) calculated from spectrum compared to monochromatic approaches using fp (a) and fc (b).