EGUsphere, referee comment RC1
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Comment on egusphere-2022-842

Anonymous Referee #1

Referee comment on "A method for constructing directional surface wave spectra from ICESat-2 altimetry" by Momme C. Hell and Christopher Horvat, EGUsphere, https://doi.org/10.5194/egusphere-2022-842-RC1, 2022

My apologies for the long delay in returning this review. I understand that it is very unfair to wait for so long, but a few issues concurred to this delay, including the complexity of the manuscript itself. I would like to state from the beginning that I am not an expert in the specific techniques used by the authors, and that I have sought assistance with a colleague applied mathematician. I am however well informed on wave-in-ice measurements and modelling, and I feel this manuscript represents a major advance in the field. It presents a two-part algorithm. The first is a linear harmonic fitting procedure applied to single beams. This part of the algorithm is almost fully explained by the authors and in the referenced literature. The main novelty here respect to literature is the use of GFT-derived surface slope spectrum. The second part of the algorithm is a non-linear inversion method to estimate the most-likely wave incident angles. It is the most articulated part of the paper, and not always well-structured. The approach is based on the cross-beam coherence, and uses marginal distributions derived from independent MCMC samples of monochromatic plane waves. The samples are limited to the most energetic waves. As commonly occurs for 2D spectra retrieval from satellites, the algorithm returns multiple potential incidence angles and therefore need of a good prior guess for directional spectra is needed. These combined procedures provide two-dimensional wave spectra derived from along-track data. The authors also discuss how the so-obtained two-dimensional wave spectra can be used to decompose wave and ice surface variability in Section 4, which becomes one of the main discussed components in the last section.

Despite the novelty, I found the manuscript difficult to read, and especially to fully grasp the implications of this method. I would thus recommend publication of the manuscript, pending some major revisions that would make it more accessible to a wider audience, or at least would clarify that it is directed at the technical audience that is interested in the retrieval of waves in ice from space. The introduction is excellent, but unfortunately not entirely connected to the discussion through the results. I list below a few major points that the authors may consider in their revision:

- My main criticism is that this is a methodological paper, which is however presented as a result paper. The results are functional to the methodology. From the title, I expected to be exposed to a reconstruction and interpretation of directional surface wave spectra from a variety of regions, while the manuscript is focused on 4 selected tracks from the Southern Hemisphere, each pair coming from two consecutive tracks in February and May. By the way, the reader would find out that the focus is on Antarctic sea-ice waves only from the supplementary table or at L85, where Antarctic is mentioned for the first time. This is not at all to diminish the value of the manuscript, because a methodology must be demonstrated by example, and I regard the work done by the authors of very high value. I would suggest that the title reflect that the reader will find a description and application of the methodology, rather than expecting a thorough analysis of 2D spectra. I would also recommend to add a description/discussion of the results presented, which is currently left to the readers. For instance, Fig. 5 and Fig. 8 show the power spectra from one example track from May 2019. This is the main result of this paper, but it is offered with very little interpretation or discussion in Sec. 4. Only a brief comment about the attenuating swell signal and a second energy maximum at shorter wavelength (L200-201), and similarly, a migration of the peak due to the attenuation of shorter waves at L316-320. The findings seem to me to be coherent with the local observations by Alberello et al. (2022), who differentiated between wind sea and swell. Almost no discussion is offered on these phenomena, while the matter of the discussion is mostly about the methodology and its limits (again, I find it very well done and interesting and not at all a fault if properly framed).
- Alberello et al. (2002) found that the wave field along a transect in the Antarctic MIZ was composed of wind and swell from different directions. With this procedure, a unique most likely incident angle is estimated. This implies that both swell and wind waves propagate in the same direction, which may not be true. The authors should consider this in their discussion.
- As of the ice structure mentioned in the title, this is mostly a potential application that is not exploited in the present manuscript. Such a component would require a more thorough validation that has not been included. It is not clear if the considerations on the structure of sea ice should come from Sec. 4. This leads to the interesting discussion in Fig. 10 and 11, but I do not see how this would inform about the ice type.
- Also related to Sec. 4, in Figures 9 and 10, k_c ranges from 0.05 to 0.1. This is the range of wind waves. Therefore, the residual z_{free} (defined after Eq 12) should estimate both the sea ice freeboard height and the surface wind waves. It seems that the low pass filter can only remove the contribution due to swells. The authors should elaborate on this in their applications of the method.
- I like Figure 1, which is well done and illustrative. However, and partly related to the first point, it would be useful to have an example of a track location, together with the configuration of sea ice (for instance sea-ice concentration). This could be done for either the February or May cases in Supplementary Table 1. The May tracks are from the same day, so it may be easier to show them on the same map with the daily SIC. It is also the track used to show the spectra in Fig 6 and 8.
- This method will require an independent set of assessments before being considered viable. There are many subjective (though reasonable) choices that the authors have made both during the pre-processing phase and in the method implementation. This may have implications that are hard to assess without a benchmark. I understand that these datasets of combined wave measurements in sea ice and in the adjacent open ocean do not exist, and the few that are available are prior to ICEsat2, or are not done in the vicinity of available tracks. However, the method is being sold as fully functional, which I agree from a technical viewpoint, although it is still doubtful whether it is reliable. This should be made clearer in the discussion.
- This manuscript contains quite a few English inconsistencies, as though it has been submitted without thorough proofreading – possibly from someone else besides the authors. It is clear that the authors have a good mastering of their methods, but this

makes the explanation often convoluted and difficult to follow. I have included a few specific recommendations below when I felt I understood the intent, but I would advise the authors to take a slightly more distant approach and focus on the information transfer rather than on the flow of concepts.

Specific comments

- Sec. 2. Figure S1 is not mentioned in this section but later in the manuscript.

- L 82-83 Some more clarity on the pre-processing method would help: is there a rationale for the choice of 0.02 photons per 100 km? Is this done prior to stencils determination explained at L94-95

- L96 The photon height in each 20 m stencil is a series of heights. Maybe the authors refer to the photon height mean

- L99 This must be S3. And what are the data shown on the x-axis in that figure? Is \sigma_x the standard deviation?

- Figure 2. There is a typo in ATL03 and ATL07. What is the offset of the grey line?

- L149 misleading

- In Eq 8 and Eq 9 data are assumed Gaussian. Nevertheless, also the authors recognize that data are not Gaussian in Sec 4.

- In subsection 2.3.1 model's degrees of freedom are treated. How many times the problem remain overdetermined, i.e what is the mean value for N_i?

- Figure 3 is not discussed in the paper, specifically the peak observed in panels b and d for k>0.06. Are these the same beams shown in Figure 5? Also, the spectral energy for each wavenumber is positive in Fig. 3 but it is negative here.

- L200 is X the distance from the ice edge or from the first point on the track? It is

ambiguous because most of the axes labels indicate “distance from the edge”. And if yes, how is the edge estimated?

- L201 should be a typo: wavelength is indicated as k'.

- line 213 "k = k'cos^{−1}(\theta)", and hereafter: cos^{-1} is also used to indicate the arccosine. It is better to use a different notation. Moreover, the equation needs a reference (eg Yu et al., 2021)

- at the beginning of page 12 "The coherence between beam pairs can only usefully be calculated for not too oblique angles (about ±XX°, Suppl. Fig. S3, and Yu et al., 2021) and high enough photon densities in both beams." The figure should be S4 which is also not so readable. It is also unclear what \pm XX° means, please add some specific examples.

- Eq. 11 should specify the cost function even without the prior (I guess set P=0)

- L268. How different is this track example from the other cases? Since there is no discussion on the results, it is difficult to imagine how different are the results in February and May.

- L281 and following. The authors speak about statistical robustness. The meaning should be explained

L295-296 This is related to may main point above. The authors state that the effect of the prior is visible in the figure. But there is little comment and no discussion.

- line 306 "This method is limited to angles of about ±85. Oblique incidence angle cannot be captured by this method. In addition, the model has a 180 ambiguity such that samples in the +95 to −95 arc, that are waves coming from higher latitudes rather than from the equator, would be equally possible", please clarify.

- Line 318: "In the case of Track 3 (granule 05160312) for example, we see a migration of the peak wavelength from about 275 meters to about 300 meters within 12.5 km (Fig. 8 d,e) as shorter waves tend to attenuate faster". Colorbar in Figure 8 is missing and the sentence is not clear enough to estimate wave attenuation.

- L373 It is not clear what the authors mean with improved understanding of photon variance. Also, the authors could comment on why the variance is so large in the interior of the MIZ, especially in track 2 and 4, but not in Track 3.

- points at L395. This is a methodological paper, and I find quite ambitious that all of this can be achieved by a non-validated methodology

- In a few figures and captions the authors often refer to something undeclared, or declared only in supplementary materials (eg: FFT, granule, gt1l,...)

References

Alberello, A., Bennetts, L.G., Onorato, M., Vichi, M., MacHutchon, K., Eayrs, C., Ntamba, B.N., Benetazzo, A., Bergamasco, F., Nelli, F., Pattani, R., Clarke, H., Tersigni, I., Toffoli, A., 2022. Three-dimensional imaging of waves and floes in the marginal ice zone during a cyclone. Nat Commun 13, 1–11. https://doi.org/10.1038/s41467-022-32036-2