EGUSPHERE-2022-842

Comments to RC2

Our replies are in blue

The manuscript presents a method to estimate the directional wave spectrum from satellite measurements taken in ice. As I understood, the method has two parts. First a GFT is used to calculate the slope spectrum, which will be a function of the along track wavenumber. Second, the coherence between different tracks are used to estimate the incident angle of the waves in relation to the track. This angle gives directional information, but since the relative angle is also used to transform along track wavenumbers to actual physical wavenumbers, even an omnidirectional spectral estimate is dependent on both parts of the algorithm. Finally, the authors remove the wave dependent elevations from the signal, thus getting an estimate of the ice properties only.

I find the topic of the manuscript to be novel and interesting. The authors have generally made a good effort in presenting the method and the choices they made. I still found some parts hard to follow, especially since all of the notations seemed to be not defined. While the method seems reasonable, it is very hard to actually judge the accuracy and limitations of the method and the following results without any other data to compare it with. I still think this methods paper is worth publication, but I do feel the authors should make it clear that the method is still not properly validated.

Thank you for these comments. As noted in our response to reviewer 1, we have endeavored to re-frame the paper as being methods-focused. We now additionally add text in the abstract and conclusion about the need for validation, in L507f:

> *"Even though this method outlines a better, more transparent wave-field inversion than a DFT, it remains to be seen how the interaction of those limitations can be used to provide a highly-resolved global wave-in-ice product. Comparisons with other data sources, either from in-situ or remote sensing observations, are needed to understand these limitations better and validate this method."*

and L513ff:

> *"However, even on these scales (80 to 350 meters), a separation between wave and sea-ice signal may only be possible when the sea ice variance is weak on those scales and the data is not too gappy, as in the chosen example tracks (Fig.~\ref{fig:GFT_result}, Fig.~\ref{fig:decomp}). High levels of sea ice variance or frequent data gaps will lead to systematic biases and aliasing effects in the wave spectral estimates."*

I can recommend that the manuscript is accepted for publication after major revisions. Please find my comments below:

#1 Not all of the notation is defined. Eg. on page 7 hat-b on line 146 is not defined, and "tr" is not defined on line 156.

Thanks for pointing this out. We went over all notations and made sure variables and operators were defined (section 2.3)

#2 The shortest wave being resolved is about 60 m, and this should be well resolved with a 10 m resolution. However, if we have missing data the actual resolution could be lower, and this would induce aliasing. Based on the figures presented the gaps seems to be few and long instead of numerous and short, so this is probably not an issue though.

Yes, that can be a problem, but as the reviewer mentioned, for “small” gaps, that should be okay. Note that fewer data points will generally lead to larger error estimates. We assume that the wave field is smooth in space and time, which implies that it may vary over larger scales than a 25 km segment and that the energy in one way number should be similar to its neighbors. That means that the estimated spectra should be similar in neighboring beams and neighboring 25km segments(Fig. 5). If they are not, that is a sign that the data structure is too *complex (too gappy) for this method. See the added sentence in L 223f:*

> *“Instances with a low photon density and more frequent data gaps may fail to invert for the wave signal, resulting in a spectrogram that may not follow expected spectral shapes. These can be identified through their substantially larger error (suppl. Fig. S4 g to i).”,*

and L450ff:

> *“A major advantage of the GFT is that it can be extended to inversions of the wave field for each IS2 track by coupling neighboring or overlapping segments, similar to Kalman inversion methods. We illustrate this by simply iterative updating the data segments and models priors (sec.~\ref{sec:suggsesive}, Fig.~\ref{fig:GFT_alter}). …”*

#3 Page 8 lines 181-182 mention that the Pierson-Moskowitz spectrum is taken as an initial guess at the outer edge. While this is reasonable, I worry how much the initial guess of a single peaked spectrum will affect the way the method resolves a double peaked spectrum?

That is a great question, and we were concerned about this as well. We used a single peak prior because we focus on the inversion of the dominant (i.e., most energetic) wave system, as those may have the most impact on the sea ice. To avoid overfitting to the PM, we perform two inversions on segments with no data available at previous segments (see Figure 4). As we now clarified in the text (L 205f) and Appendix A2, for initial segments, the 1st inversion is based on PM, and the 2nd inversion is based on the smooth data of the 1st inversion. Tests show that the initial PM-prior has only weak to no influences on the solution of the initial or following segments for cases with no single peak spectrum (updated figure A1) but helps to retrieve a single speak spectrum for cases with gappy data (Suppl. Figure S12). We show here the new Figure A1 for convenience:

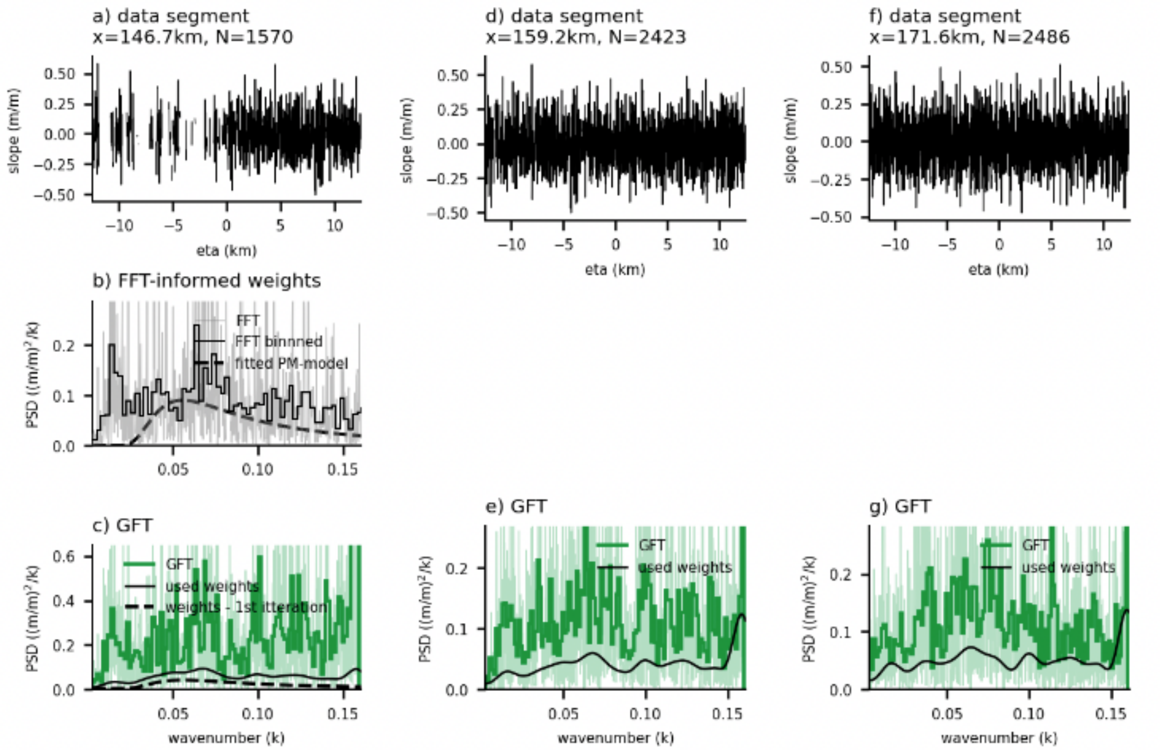
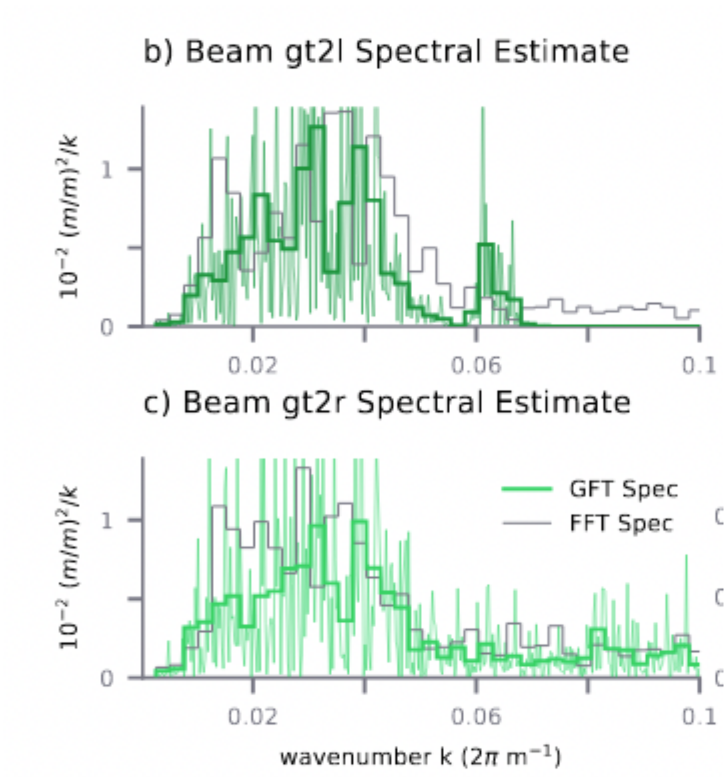


fig:GFT_priors
Figure A1. Examples of GFT inversion and their priors, for example, Track 1. (a, d, f) data used in the segment centered at $x = 146.7, 159.2$ and 171.6km. (b) DFT (gray), re-binned DFT (solid black) for data in (a), and PM-model fitted the DFT (dashed black line). (c, e, g) GFT (green line), re-binned GFT (thick green line), PM-prior for 1st inversion (dashed black line), and data-prior for second inversion (solid black line).

#4 Figure 3 mentions that the spectrum is smoothed by a rolling mean. **First, the rolling mean is perhaps not a very good filter and could possible introduce artefacts.** Second, noise in spectral estimates are usually accounted for by either splitting the time series into blocks (the Welch method) or averaging neighbouring wavenumber bins (theoretically these are equivalent). So I think the right thing to do here is to **average wavenumber bins.** This will reduce noise, but also the resolution. That way, however, the resolution will be an accurate account of the true resolution of the spectrum. Using a rolling mean means that the resolution appear higher, but the bind are no longer independent.

Thanks for the comment. We now present the estimated spectra (Figure 3b,c,b) and the weighted cross-beam mean spectra (Fig. 5h, Fig. 8a,b) as a binned average. It does not alter the overall result.

We here how the new Figure 3b,c as example



#5 page 10 line 212. I think cos-1 would usually mean the inverse function here? Maybe better to just use k'/cos to avoid all confusion.

Thanks for the suggestion. We changed the notation accordingly.

#6 The manuscript talks about using coherence to estimate the angle (e.g. page 12 line 220). I'm not sure how this is done using coherence. I would think that the phase lag between the signals would be used to determine the direction, and the coherence would only tell us if the phase lag measured from the corss-spectral estimate is "real" or not. This method reminds me a lot of the Wavelet Directional Method (see Donelan et al., 1996), although on a wildly different scale. This method has been used by the wave community, and I think it would be a good idea to point out the similarities and differences to this more established (although not extremely widely used) method.

Thanks for the suggestion. Indeed the use of the term spatial coherence is misleading in the context of spectral analysis. In section 3.1 and throughout the text, we now use the term '*phase lag'* when discussing the angle inversion method.

We appreciate your suggestion of the WDM. There are indeed some similarities between both approaches. In fact, the approach the measure the phase lag per wavenumber is the same in both methods. Both operate with the same amount of information per observation, but the main difference is that WDM requires three observational time- or spatial series, while ours has to operate with only two. A 3rd laser beam close by would allow for applying the WDM. However, the ICESat-2 laser beams are too far apart (about 3 km) such that we have to introduce a prior for breaking the symmetry. We briefly discuss this in section 5.2 line 489ff:

> *"The proposed MCMC method shares aspects with the Wavelet Directional Method (WDM, Donelan et al. 1996, 2015), which decomposes the signals of at least three stationary wave observations into wavelets for each frequency. Similar to our method, WDM uses the phase lag of the wavelets between the three stations to identify a wave incident angle per frequency. WDM could be applied to transects of the wave surface as present in our analysis. However, ICESat-2 only provides two neighboring laser beams, and other beam pairs are too distant (about 3 km) for coherent phase analysis. In addition, the signal-to-noise may be substantially lower in the ICESat-2 observations, as wave crests are potentially distorted by sea ice structure. Therefore, we introduced a wave-angle prior (eq.\ref{eq:angle_cost}) to break the ambiguity in the observed phase lag (Fig.~\ref{fig:mcmc}b,d, shading)."*

#7 Connected to the above, I would think that several angles would result in the same phase lag (e.g. a small angle gives half a wavelingth lag and a large angle 1.5 wavelength lag). Is this what the authors refer to on page 14, line 293 with "multiple equally likely incident angles", or are they talking about some other source of uncertainty? I think it makes sense to use the longest waves to determine the angle, since they are least sensitive to this kind of "folding".

This method has to deal with multiple symmetries that we try to mitigate. In this case, we describe the problem that there can be a positive and negative leg, i.e., the waves can come from left or right. In addition, there is also the 2 pi ambiguity that leads to multiple minima in each quadrant. We clarify this in the updated manuscript L.317f:

> *"This joint distribution may have multiple equally likely maxima, i.e. multiple likely incident angles due to the periodicity of the wave ($2\,\pi$ ambiguity). As illustrated in figure \ref{fig:mcmc}d (shading) this can lead to a) maxima for positive and negative incident angles and b) multiple maxima on both sides."*

Longest wavelength would work better, but they are not necessarily the most energetic. That is why we speculate that this method may work best away from the ice edge when short waves have attenuated. We describe these challenges in L 466ff:

> *"The quality of MH inversion method depends on the wavelength, wave amplitude, and curvature of the wave spectrum. The longer the wave the better the phase lag can be observed, but those are not the most energetic. In turn, the most energetic waves have typically shorter wavelengths that are of 80-250 times the segment length (25km), which can lead to multiple minima in the optimization due to a 2π ambiguity. Finally, the curvature of the wave spectrum characterized the length of wave groups, which in 2D, erode the ability to observe the average phase lag between the two beams (2nd bullet point in sec.~\ref{sec:angle}, suppl. Fig.~S7)."*

#8 Page 16 line 319 mentions a “migration of the peak” from 275 to 300 m. However, from Fig. 8 this seems to be the observed (along track) peak, and not the actual peak wavelengt. In the corrected one there doesn’t seem to be much migration. But Fig. 9 shows spectra with peaks closer to the 0.02 rad/m, i.e. 300 m range. So am I reading this wrong somehow?

We understand the confusion. Figure 8 shows the corrected slope spectra, and Figure 9 shows the uncorrected height spectrum. Both figures have different purposes. Figure 8 illustrates the angle correction and is based on Figure 5, while Figure 9 shows how the frequency cut-off k’_c is defined and used to reproduce surface heights. We clarified this in the caption of figure 9 and changed k_c to k’_c throughout the text, and when introducing figure 9 (L362f):

> *“In Figure \ref{fig:cut_off} we show the identified low-pass filters and the displacement spectrum (m^2 k^{-1}) rather than the slope spectrum ((m/m)^2 k^{-1}), as in Fig.~\ref{fig:final_corrected}) to better separate the high-frequency noise from the lower-frequency waves.”*

We now also show the corrected wavelength in Figure 8 c) to d), and after reading the data, we weakened the statement of peak migration L. 348f:

> *“One could identify a migration of the peak wavelength from about 275 meters to about 300 meters within 12.5 km (Fig.8 d,e, similar to Alberello et al., 2022) we leave this analysis of the attenuation to future work.”*

#9 **I am slightly skeptical to the last part of the method that removes the wave signal and estimates the ice surface roughness.** I might be missing something**, but isn’t the scales of the surface roughness that can be resolved determined by kc?** I other words, when removing the wave signal for k<kc, we are also removing any possible NON-wave related signal in that wavenumber range. The final roughness (and the scales that will be represented) will therefore depend on which wavenumbers happen to have wave energy. As a corollary, the **wave spectrum also includes the variations of the changes in ice roughness over the same scales, right? Won’t that taint the spectral estimate?**

Yes, these are limitations of the method presented. The use of a simple cutoff frequency assumes a separation of scales between longer wave scales and shorter “roughness” scales. There can clearly be cases where a scale separation may not be correct. This and the need for in situ validation are stated in the conclusion section, for example in L507ff:

> *“…, it remains to be seen how the interaction of those limitations can be used to provide a highly-resolved global wave-in-ice product. Comparisons with other data sources, either from in-situ or remote sensing observations, are needed to understand these limitations better and validate this method.*
>
> *Waves and sea ice have scales ranging multiple orders of magnitude such that it is challenging to separate both in the IS2 observations. The choice of the parameters in this analysis (10-meter bins, 25km segment length, and the slope-based cut-off frequency k'_c) focus on identifying swell wave events routinely created by synoptic*

> *storms \citep{Hell2021Swell}. However, even on these scales (80 to 350 meters), a separation between wave and sea-ice signal may only be possible when the sea ice variance is weak on those scales and the data is not too gappy, as in the chosen example tracks (Fig.3, Fig.10).“*

In future work, one could think of including a model of sea ice roughness or simply height in the inversion method to resolve the sea ice structure explicitly.

#10 The manuscript has a discussion, but no conclusions. This leaves the reader hanging a little bit. I would strongly urge the authors to **conclude their findings**, and maybe also make it clear that this is still an **non-validated method**. Even if the theoretical approach seems sound to be, and a lot of the choices made by the authors seems reasonable, **it is very hard to say how much e.g. the values of the first prior spectrum or the value of the beta parameter actually affect the results.** To be clear, I'm not saying that the authors need to get data and validate, since such data is probably not readily available. I'm saying it should be made very clear to the reader that this has not been done.

We added a conclusion section that emphasizes the need for validation and the choice of parameters to focus on swell waves (L507ff, same citation as above).

We share the reviewer's concern about the impact of the impact of priors and beta parameters. Those are always subjective to some extent, but we tried to make plausible choices. As discussed in the reply to #3, the GFT priors are chosen rather conservatively (see Appendix A2, suppl. Figure 11). Similarly, the beta-parameter of the angle inversion is chosen such that it just breaks the symmetry of the otherwise symmetric likelihood function, as shown in figure 6 b, d shading. The actual value of the prior angle does not matter much, it just gives a preference for one side, and the actual angle value is determined by the gradients in the data, not in the regularization. We clarify this in the main text L328ff:

> *Since validation of the WW3 prior is limited, we set $\beta_\theta=2$. Its effect on the objective function can be seen by comparing the shading in figure 6b and d. The choice of $\beta_\theta=2$ leads to the desired result in breaking the directional ambiguity while not fully determining the incident angle distribution (Fig.7a). We tested other values of β_θ but found empirically that higher values tend to overfit to the prior, and lower values do not break the ambiguity well.*

and L486f:

> *“The lack of certainty in WW3's peak direction and frequency is expressed in the value of the hyperparameter β_θ (eq.~\ref{eq:angle_cost}). A value of $\beta_\theta=2$ leads to the desired behavior of breaking the symmetry (compare shading in figure 6 b and d) but not imposing the optimization result through the prior (Fig. 7d blue and orange lines).”*

References: Donelan et al. (1996). “Nonstationary Analysis of the Directional Properties of Propagating Waves“, JPO,
https://doi.org/10.1175/1520-0485(1996)026<1901:NAOTDP>2.0.CO;2