Response to Reviewer 1

Spectral attenuation of gravity wave and model calibration in pack ice Cheng et al.
tc-2019-290

Authors' response:
We appreciate the reviewer's careful reading and the support of this study very much. A revised manuscript has not been prepared at the time due to the editor's request. We will revise the manuscript to respond to all of the issues raised by the reviewer. The review comments are listed below in black, and our responses are in red.

In their manuscript „Spectral attenuation of gravity wave and model calibration in pack ice", Sukun Cheng and colleagues present results of an analysis of wave energy attenuation based on data obtained from a set of SAR scenes from the Beaufort Sea. The analysis includes: (i) derivation of spectral wave characteristics in the area of interest, divided into two sub-regions with different ice types and morphology, (ii) computation of linear attenuation coefficients for a large number of pairs of points located in both sub-regions, and (iii) calibration of parameters of two selected models of wave attenuation, by Fox and Squire, and Wang and Shen, to the observed spectral attenuation. The manuscript also includes a discussion of possible sources of errors in the analysis, data deficiencies, as well as a more general discussion of problems with model calibration related to a large number of unknown coefficients and with the fact that a multitude of different physical mechanisms contribute to the net attenuation observed in the field. It is relatively easy to point out limitations of this type of analysis, but – as the Authors rightfully remark – our limited understanding of the processes involved, combined with limited availability of data for model calibration and validation, restrict our ability to develop complex, physics-based models and justify development of simplified, but practically applicable parameterizations (like those implemented in the WW3 wave model). Therefore, in my opinion, the work presented in the manuscript is very valuable and has several aspects practically relevant for spectral modeling of wave propagation and dissipation in sea ice. I think that the results are worth publishing in "The Cryosphere". My comments on the manuscript are listed below.

General comments:

1. The text of the manuscript contains a lot of (mostly small) grammar, punctuation and other language mistakes and should be carefully corrected before publication.
   We will clean up the language mistakes.

2. I'd suggest modifying the title of the paper. I understand the Authors wanted the title to be short, but in my opinion they over did it. "Model calibration in pack ice" – what kind of a model? It might mean anything. I'd also suggest changing "gravity wave" to "gravity waves".
   The title is revised as "Spectral attenuation of ocean waves in pack ice and its application in calibrating viscoelastic wave-in-ice models"

3. The location of FAL – and its very existence – is crucial to the analysis presented in this paper. The Authors first introduce this term on page 3 (lines 75-76), suggesting that it was used (or defined) by Stopa et al. (2018b). It should be Stopa et al. (2018a) – see also my technical comment no. 1 below. But, more importantly, even if that information is provided in the previous papers, I'd suggest adding it to the present manuscript as well: how was the position of FAL determined? How does the ice cover differ on both sides of the FAL-line? In the present form, the FAL seems rather "mysterious". For example, further on page 3 we read: "…the FAL (black dots) presumably marks the separation between discrete floes and a semi-continuous ice cover with dispersed leads". (A bit further, in line 120, again: "presumably a semi-continuous cover".) Presumably? Does it mean those features cannot be unambigously identified in the analyzed images? How then was the position of FAL determined? What was the criterion? What is the uncertainty

associated with the location of FAL? Very importantly: was the location of FAL determined independently of any information on wave characteristics? Could the authors add a figure showing fragments of the analyzed images on both sides of FAL (not necessarily in the main text, but in the supplement)?To make it clear: I'm not criticizing the analysis nor the way FAL was defined/identified, but the presentation in the manuscript.

We will include the explanation of the first appearance of leads (FAL) in Appendix A.

"Appendix A

The definition of the first appearance of leads was introduced in Stopa et al. (2018b). Here we provide more details of the methodology used. The SAR sea surface roughness imagery in Figure 1 of Stopa et al. (2018b) are divided into 5.1x7.2 km subimages with a 50% overlap of adjacent subimages in the range-azimuth domain. Each subimage contains 512×512 pixels. The FAL location for each range-position is defined as the minimum azimuth position where large-scale ice features were detected. A detection of large-scale ice features is applied on each SAR subimage as the following. We first compute a one-dimensional spectrum of the SAR subimage to produce an image modulation spectrum. The spectrum is then normalized by the maximum energy contained in wavelengths from 100 to 300 m (the wavelength range of the dominant sea state for this event). When the ratio of the average of the normalized image spectra with wavelengths in the range of 600-1000 m and the dominant ocean-wave wavelength range from 160-220 m exceeds 0.8, we deem that there is a "large-scale" feature such as lead within the image. Figure A1 shows two representative examples of detecting ice leads from SAR images captured before and after the FAL. From the criterion above, there is no leads in the top case, but leads are found in the bottom case. Also notice the change in the probability distribution of the roughness: the mean value changes (lower in the non-lead case compared to the lead case) and the standard deviation (lower in the non-lead case compared to the lead case).

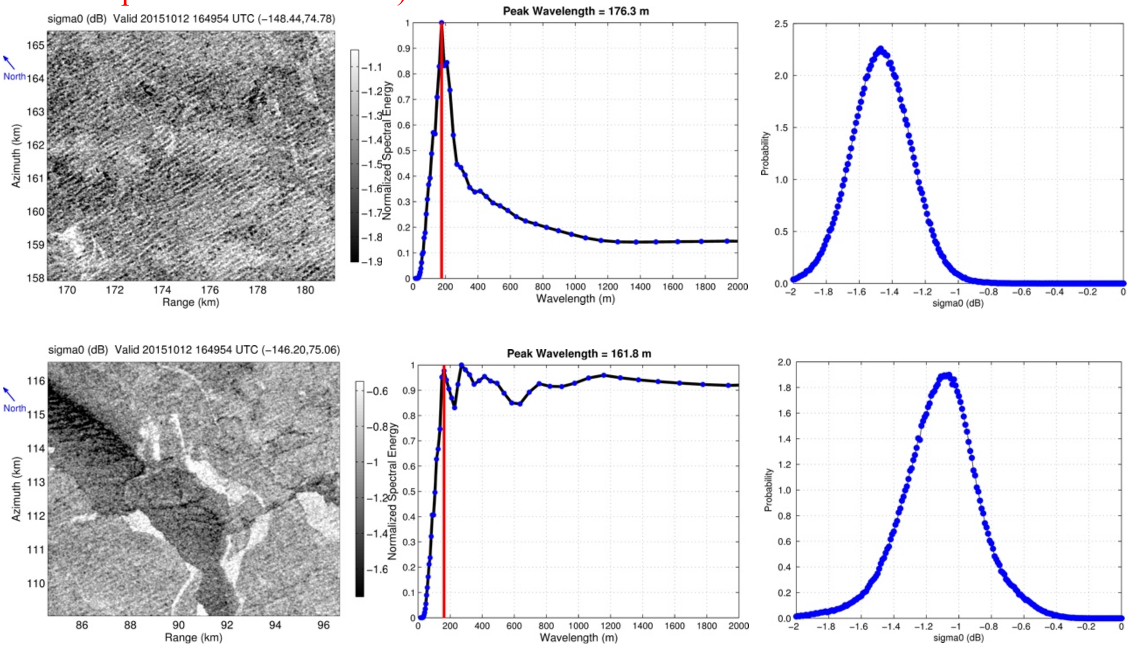

Figure A1. Illustration of the process to determine the FAL using two representative SAR subimages. (left) Surface SAR subimage roughness for a case located before the FAL (top) and a case located after the FAL. (middle) Normalized spectral energy (normalized by the maximum energy within the 100-300 m wavelengths) of the SAR subimages where the red line indicates the dominate wavelength. (right) The probability density function of the SAR roughness (backscatter or sigma0 of thermal noise in the SAR imagery) for the two cases."

As far as I know from other studies (I'm not an expert in satellite data analysis), the satellite algorithms used to determine ice concentration and thickness perform relatively poor in thin, "new" ice types (frazil, grease, pancake ice).

Could the Authors comment on the reliability of the concentration and thickness maps (Fig. 1b,c) in the region south of FAL, where the thickness is 10 cm or less? Is the apparent west-east gradient of ice concentration and thickness in that region really present or is it possible that in fact it is a change of ice type? Those questions are important for some aspects of the analysis, for example, in line 118, where the Authors say that the wavenumber "varies with ice concentration but is insensitive to ice thickness variation…".

To explain the use of AMSR2 and SMOS, we will add the following comments

"As shown in Cheng et al. (2017) (Supporting information Figures S6 and S7), these two ice products compared the best with in-situ observations in the MIZ. Their accuracies in the pack ice zone are uncertain."