This paper documents the process of observing ocean surface gravity waves in sea ice and the estimation of MIZ width based upon the wave height measurements. The initial observations used are surface elevation measurements using the laser instrument on ICESat2. Individual elevation measurements within sea ice are combined through several spectral analysis techniques in order calculate the wave heights within sea ice. A clear difference in elevation spectrum is presented between the MIZ and thicker ice further from the sea ice edge. There is a clear region of decreasing significant wave height within the sea ice pack for the case studies presented. The attenuation rate of the significant wave height is successfully modelled an estimate of MIZ width is presented. The satellite based observations are compared to in situ buoy measurements of wave height and the implications of wave based measurements of are discussed.

I find that the paper is well written and the results are carefully discussed and contrasted to existing research. I enjoyed reading the discussion and conclusions sections. These new results showing observed wave attenuation rates within sea ice are of high importance to current research and I recommend the paper for publication after the authors deal with some editing concerns.

First I found that my reading of the paper was greatly hampered by the volume of data presented in the paper. In particular I recommend that the authors remove many of the figures to supplemental material. Each case study needs only 2 figures within the paper body. One for the initial data, and another for the retrieved parameters, for example. Currently each study has 7 figures. The inclusion of 7 different analysis techniques and 3+ plots for each technique for both case studies, and then a further figure inter-comparing these techniques these techniques (figure 17) when they appear to produce very similar results, makes the the reading of the paper a chore and lessens the impact of what is a high quality paper. Further in the paper a single FIRF wave length analysis was presented and the best to use for MIZ width estimates. Please consider presenting the case studies for this method only, with a table or figure summarising the other methods and the extended figures in supplemental material.

My second concern is with the method of MIZ width estimation. Currently the authors use the point at which the modelled wave height drops below the model error. However, there is no data that is apparent at this point, as the authors point out that other effects then dominate the signal, such as ice dynamics. As the authors state that the extent of the MIZ is the region where wave energy is of greater importance than ice dynamics, then surely the ‘first local minima’ in figure 5, or the ’break point’ in figure 6, are also valid points at estimating the MIZ width. The measurements for the case studies show will also give a MIZ the exceeds that from Ice concentration. Including them as other estimates of MIZ width in the rest of the study (say in figure 18 and 19) will improve the strength of the results.

Specific points:

L 5 Are these new techniques presented within this paper?

L31 are you saying that long-period wave impact the size distribution far into the sea ice pack? This sentence is long and hard to follow.
L33 SO int he southern ocean wave do not penetrate? These tow sentences seem to contradict each other
L80 New paragraph here will enable readers to find the purpose description more easily.

L 95, an extended description of the paper contents here will be useful This is a long technical paper and a description of every section and key subsections will ease the navigation of it.

L129 what is a ‘segment height’?

Figure 4 Caption ‘distance to the continent’ is ‘distance from the ice edge’ surely?

Figure 2. What benefit is there in having all three FIRF wavelengths presented here and in further figures? Is it possible to pick a best use case, and allow further description in supplemental material?

Figure 2. Please put the acquisition time and date in every caption where there is a figure representing data from a single track. This makes it much easier to cross reference to other studies and to make sure the reader is comparing like for like about the paper.

L275, I'm struggling to see how the peak shift towards longer wavelengths further from the ice edge. Figure 4, top pane, has a peak for all colours at around wavenumber = 0.001. Is this what you mean? I can see no significant movement in this peak.

L277, there is no clear peak the second pane in Figure 4,

L 281. I am struggling to see how figures 5-7 describe the model. There are three figures here with lots of detail that is not referred to in the paper. For example the letters a-g for figure 5 are not mentioned. Is it possible that these three figures can be abbreviated into one for the paper with the full figures moved to supplemental material?

Figures 6-7 I cannot find a description for how the red line is generated. Is it the point of intercept for the line of best fit in Hs? Please refer to this in the description in section 4.1.

Figure 6, you do not refer directly to what exact value is shown by the grey scatter points. Are they identical to the grey points in figure 5?

Figure 8. This figure without the previous figures 5-7 may be all that is needed for the main paper.

Line 305. I don't not find it helpful for 19 lines of text to be supported by 7 figures. It makes the paper very cumbersome to read, and lessens the impact of the results. Please re edit the figures to have two figure per track. Perhaps illustrating a single processing option, Hann Hm0, or a single FIRF, with the rest in supplemental material.

L 320, is it possible to give the distance from each buoy to the ice edge?

L 322, you present 7 techniques for obtaining Hs from IS2, which was used for this comparison?

L 328, for the satellite to buoy distance measurement, are you able to comment on whether the satellite measurements are closer to the ice edge than the buoy, or vice versa? Knowledge of this is helpful for estimating whether the satellite is underestimating wave height, or whether the buoy is in a location of higher waves. The distances referred to in this section are larger than the MIZ extents presented elsewhere in the paper. Is it possible to use ice concentration data to get a distance from the buoy to 15% concentration? Also it would be nice to see the time series of buoy records against the coincident satellite retrievals, is this possible to display?

L328, why does the regression slop indicate that there is an underestimation? Please expand. My interpretation of this is that there is less of a correlation in measurement as separation increases, which is difficult to interpret if the distance from the buoy to ice edge is not considered.

L336 if a single technique is chosen, then please present only that one in the main paper. The rest is only helpful in supplemental material as there are far too many repeated plots.

L340 This suggest that only one SDF technique is required. Please reduce the main paper to include only that one.

Figure 11, again, does the caption mean to say 'distance from the ice edge' instead?

L 344, I'm not surprised that all these techniques agree, as it is very hard to distinguish between all the previously displayed data.

Figure 13, the MIZ width estimates for linear model appear to be the linear intercept, what is the definition for the log scale model? I am struggling to find the definitions in the paper body.

Figure 19. This caption needs expanding. What are the black dots? Why are they not included in the February IS2 cases? What statistical values are used to create the boxes?

L 380, In all the examples you show, your “wave affected” region exceeds the SIC based MIZ in all cases, what are the reasons, physical and technical, for the Horvat et al. region having a far smaller spatial extent?

L 386 Ok this is your definition of MIZ width. This definition was not clearly defined early in the paper and it is frustrating to find it so late in the paper. What is the ‘estimated error’? Is this calculated per track, or a constant parameter? The error displayed in the plots is highly variable, particularly in the log models.