Dear Dr. Marchenko

We greatly appreciate that you have taken the time to thoroughly read our manuscript and provide insightful and constructive suggestions. We have followed each of your recommendations which has helped improve and clarify the results and discussion around them.

Best regards,
Dyre Dammann

General comments to the paper are as follows.

The paper consists of theoretical part focused on the description of the methods of the data processing and experimental part describing organizing of the field works and obtained results. Performed measurements are new and interesting, but the theory doesn't explain observed features of wave motion. Discussion in the end of the paper is not specific enough. I recommend improving this gap.

We have now incorporated more discussion around superposition of waves which now provides a more clear link between the modeled examples and previously incorporated results as well as newly included observations.

Authors discuss that superposition of waves including waves reflected from the iceberg and waves reflected from the lead may influence the GPRI records. In this case wave motion in the region is not reduced to one harmonic wave. Therefore, methods described in Section 2.3 should be adjusted to the situation of wave superposition. Some simple demonstrations of possible effects would be useful to understand if wave superposition may explain the observations.

This is a good suggestion and something that we now have included by incorporating superposition in the methods section in terms of an equation (Eq 7) and modeling examples (Figure 3), which do support the observations later on. We have provided a better explanation for the reflected edge wave as now discussed as a case of superposition supported by a simple model result. We have also incorporated a possible example of a standing wave to demonstrate another case of wave superposition (Figure 10) also modeled in Figure 3.

Incoming long waves may influence iceberg tilts and tilts of the GPRI. It leads to changes of the angle theta in equation (1.2). Analysis of this effect could be useful for the interpretation of the GPRI records.

We have now included a statement in Section 4.2 discussing uncertainties related to the angle theta. The largest uncertainties will be related to the antenna elevation and we think the simulations carried out in the Appendix are helpful in demonstrating potential effects in the case the iceberg were to move. We thus now write: "The derived amplitude is dependent upon the incidence angle, θ, which is subject to uncertainties, predominately driven by inexact estimation of the antenna height atop of the iceberg. Uncertainties from sensor tilt due to iceberg motion are not considered as significant changes in θ could be identified in the antenna leveler and interferometric phase as illustrated in the Appendix."

Most strange result is that in E2 the GPRI records didn't show the wave propagating onshore which was registered with the IWRs. It is possible that incident onshore wave excites natural oscillations of ice in the lead propagating along the lead. But the incident wave should have an input in ice motions in the lead. Theoretical explanation of this result would benefit the paper.

This is a great point. We naturally have to concider the incoming wave field as well. In response to your previous comment, we included a description of superposition and now aregue that E2 represents such a case of superposition of edge waves with the incoming wave field. Overall, this does not change the results significantly, but leads to a more reasonable explanation and also a predicted edge wave orientation directly parallel to the lead direction, which gives reason for strengthened confidence in this overall explanation.

Detailed comments.

Line 170. Reference on Table 2 is given in the text, but Table 2 is not presented in the text.

Good catch. The reference has been taken out

Line 170. C_0=-10 m/s. If it is speed, then it should be positive. The onshore direction is mentioned in the text.

Changed.

IWR35 is listed in Table 1, but the information about records of IWR35 is not given in the text.

Now included: "IWR#35 is situated behind grounded ice, in shallower water with thicker ice and does not exhibit the same signal as the other IWRs."

Line 195. C_0=-27. m/s ?. What means sign - ? The speed is positive/

Good point. Sign has been removed

The paper fits the journal profile and worth the publication after the revision. According to the comments above I recommend major revision.

Dear Dr. Thomson,

Thank you so much for your time reviewing this paper and for your constructive comments and recommendations. We have made changes according to all your suggestions. Including measurements of expected noise in the IWR data was great hand has led to a stronger paper.

Best regards,
Dyre Dammann

This is a well-organized manuscript presenting exciting new observations of wave propagation in sea ice using Gamma Portable Radar Interferometer (GPRI). The results are mainly limited to two idealized examples from the larger GPRI dataset; these examples are both novel and convincing. Additional measurements from Ice Wave Recorders (IWRs) provide wave direction estimates that are essential to GPRI data interpretation, as well as ground-truth measurements for the waves inferred from the GPRI data.

The IWR measurements could use a slightly more careful treatment prior to final publication. Infragravity waves are notoriously difficult to measure with buoys (seafloor pressure gages are preferred). Certainly, the contrast between the broad-band signals of Fig 3 and the narrow-band (low frequency) signals of Figure 6 is striking... but what are the confidence levels for such small amplitude signals? I think it would be helpful to show an ensemble spectrum of the IWR observations from 24 Apr 2021 with confidence intervals based on the classic Chi^2 distribution for a spectrum of that many degrees of freedom. Something like Figure 7 from the recent Squire et al, Wave Motion, 2021. Does the infragravity peak at 43 sec stand-out above this confidence level? My guess is that it does, but it is not resolved particularly well.

Another way to improve the IWR measurements would be to present more details about the noise floor of the instrument. What does an ensemble spectrum from this instrument look like when place on land, away from industrial vibrations and other signals? Based on this empirical noise estimate, or on the sensor specifications, is it reasonable to expect to observe mm scale motions? These questions do not intend to undermine the results presented, but rather to provide better uncertainties estimates in comparing the GPRI and IWR results.

This is a great suggestion. We have now incorporated the noise floor both from the sensor manufacturer as well as estimated from when the IWRs are at rest. We have now included the following sentence: “Although these peaks are small with an amplitude of ~1 cm, they are significantly above the derived noise floor as indicated in Figure 7.”

Some minor additional comments, by line:

line 21: This opening paragraph needs a sentence specific to infragravity waves in sea ice, which have many unique aspects (e.g., bound versus leaky modes, edge wave propagation) compared to sea and swell waves. Two recent papers by Kovalev (ECCS 2020 and CRST 2020) are worth citing.

Great suggestion. Included

line 28: a better reference for stereo measurements of waves in ice would be Smith and Thomson, Anal. of Glac. 2019.

Perfect, this is now changed

line 197: identical is a big word.   Suggest changing to "the same, within measurement uncertainty"

Agree. this has been changed