Dear Dr. Kleinherenbrink,

Thank you so much for your helpful and positive recommendations. We have followed all your recommendations, which has resulted in a much improved paper. Please see below for detailed response to your suggestions.

Best regards,
Dyre Dammann

Review of "Observing sea ice flexural-gravity waves with ground-based radar interferometry" by Dammann et al.

General comments
The article describes the first application of GPRI measurements to study infragravity waves in sea-ice covered regions. The study is original and the suggested approach to detect infragravity waves is novel. The GPRI data shows potential to study infragravity waves in detail. The manuscript is clearly written and is well structured. I recommend it to be published in the Cryosphere after some modifications.

I have two concerns that should be addressed:

1) The authors should clarify why we need GPRI observations of infragravity waves. In the introduction only one line ("From a suitable … indefinitely.") is dedicated to this. Do we want to study ice-wave interactions, or spatiotemporally varying wave dynamics? In the conclusions it is suggested that sea-ice properties can be derived, but it is not clear if it is very useful on the spatial scale we are considering.

A great point. We have now clarified why we think this is valuable as part of prior efforts to evaluate the GPRI for landfast ice monitoring:

In a recent study, Dammann et al. (2021a) used a Gamma Portable Radar Interferometer (GPRI) stationed on floating sea ice to observe microscale horizontal strain. This demonstrated the ability of the GPRI to quantify and separate transient processes from a large-scale strain field and dynamically discriminate between regions of different properties. Additional work has been done to observe landfast sea ice from shore using a GPRI to discriminate stabilized zones and monitor ice movement in response to wind and current conditions (Dammann et al., in review). A key motivation for such work has been to investigate the potential for the GPRI system for seasonal monitoring of landfast ice and evolving stability due to changing ice and environmental conditions. This could help determine the application of the GPRI to detect conditions or dynamics as precursors to ice failure and breakout events such as horizontal strain and tidal displacement (Dammann et al., in review). However, an open question has been whether the GPRI could characterize waves in sea ice which together with long-term strain monitoring could help characterize ice conditions and impacts of waves on ice stability.

2) From the presented material in the paper alone in the paper I did not get fully convinced that we can derive infragravity wave properties from the GPRI. Of 238 records only two records showed a clear long-wave signal that matches the frequency of infragravity waves. I think figures of the other acquisitions should also be provided in supplementary material to give the reader a sense of their content.

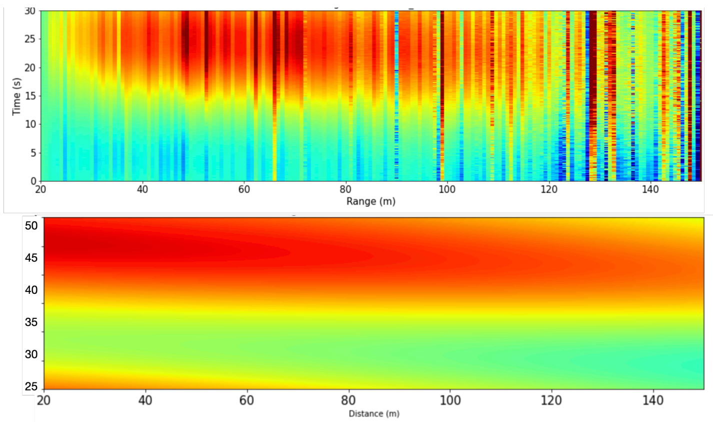
All plots have now been included in the supplementary material and a mentioning of this data included in Section 4.3: "In addition to E1 and E2, many other examples exhibit wave-like motion in the GPRI data (see supplementary material), but can be challenging to interpret, likely due in part to the presence wave fields with different sources and frequencies as well as horizontal motion."

The first acquisition (E1) appears to have a nearly monochromatic signal of 30 s, but the data does not match 'part of the second crest' in figure 5. However, there is no IWR data available for this record. The IWR data for the stretch of three hours, however, shows that monochromatic waves hardly appear and typically there is some smearing or there are multipeak signals. So even though it appears we look at a monochromatic wave in E1, interference due to smeared spectrum might prevent the 'second crest' to show. So, I wonder if the assumption of a monochromatic wave is valid.

We agree. The title of the section has been changed as it implied the observations of a monochromatic wave field. The section heading now reads: Comparison of observations with a modeled wave. We have also elaborated in the section: "It is worth noting that although the single observed wave observed in Figure 5a matches close with the modeled wave field, it lacks sign of the prior wave modeled as a second red area in the bottom of Figure 5b. This suggests that although we observe an onshore wave, it may not be a part of a strict monochromatic wave field."

For E2 there is IWR data available, but the phase velocity is different from expected. Several arguments are given for this discrepancy, but they appear to rely on rather strong assumptions (for example, reflected waves have amplitudes nearly equal to those of the inbound waves). Additionally, the amplitude differs an order of magnitude (in the IWR spectra of figure 7 a 10 mm vertical displacement is present, while the vertical displacement in figure 9 is only 1 mm). The IWR33/34 data at the time of E2 also show two/three peaks in the spectrum. It would be nice to see a plot like 5a for record E2, and if possible, a model realization of 5b for E2 using the spectral information from the IWR33/34 as input (maybe using only three frequencies).

Figure 5 is mostly added to show an initial observed interferogram and the full wave signal. This example makes for an interesting comparison as the wave speed is only 10 m s-1 so that the speed can be easily visualized and tracked. Example E2 look more horizontal due to the much higher speed and is more difficult to use to determine speed. Below is an example of a modeled wave with three frequencies 0.024, 0.017, and 0.03 Hz. A fair comparison is more challenging than in Figure 5 due to the unknown orientation and relative phase of the individual waves with each frequency.



I needed the Mahoney et al. (2016) article to convince me that we see infragravity waves. I suggest to clearly state in the introduction that cm/mm-level infragravity waves have been observed in the Arctic near the considered region (Mahoney et al., 2016). State why this site is selected to study infragravity waves. To convince the reader that the signals in the wave rider data are in fact infragravity waves, it should be supported with references to literature that show there are (regularly) ~0.02 Hz waves present in this area.

This has now been included in a new subsection 2.4

Technical corrections

Line 40: A reference to the review of Collard et al. (2022), "Wind-wave attenuation … " could be included.

Done

Section 2.1: I think it is good to remind the reader that the GPRI is very directional. The azimuth footprint is several meters.

This has been clarified: "This limits observations to a single line as the antenna generates a fixed fan beam spreading 0.4º in azimuth"

Line 78: The threshold for the coherence appears to be very strict. What is this threshold based upon?

This has now been clarified: "We then subset the 30 s displacement timeseries based on low variability (RMSE < 0.3-0.5 mm compared to a 1 s running mean) as well as coherence. The reduced sensitivity to vertical motion with range in combination with small ~ 1 mm observed waves we found it optimal to limit observations to areas with high coherence (>0.999) to ensure low noise in the observations."

Line 85: I assume that an acquisition is 30 seconds, like the evaluation length discussed in line 78.

Absolutely. This has now been stated.

Line 85: I wonder why the authors use the phrase 'every few minutes' and do not give a precise number. Is it operated manually?

Agree, this was not very precise. Now improved clarity by stating: "The radar alternated between staring in a direction across and along a ~200 m wide refrozen lead (cyan lines in Figure 1b) with a two minute lag repeated every ten minutes"

Line 86: I guess this sentence refers to one of the cyan lines in the figure 1. Maybe it is good to indicate this in the figure and refer to it. As it is, the sentence can be read as if waves are only visible if they travel in the stare direction.

This has now been done by using only a solid color for the across-lead direction and clarifying this: "Clear wave signals were only identified with the GPRI facing across the lead (solid cyan line) possibly due to the smooth, uniform ice conditions."

Line 125: I would rephrase this sentence. It practically always differs, so remove 'may differ … the wave, c, and,

Done

Line 127: 'between crest is greater than'

Done

Line 128: 'If the propagation'

Agree, that is better. Done

Line 135: unit missing for alpha.

Added

Line 125-135: I have the feeling a lot of words (and some repetition) are used to describe the geometric transformation with cos(alpha). This can be shortened.

This has now been significantly shortened

Section 2.3 and elsewhere: While swell system have typically a very narrow angular spreads, (bound) infragravity waves have much larger angular spreads (Reniers and Zijlema, 2021). I am not aware how (free) infragravity waves propagate and evolve under sea ice. The authors should argue why using a model with one or two monochromatic waves suffices.

We have now created an additional subsection, now Section 2.4 where we both justify the assumption of monochromatic nature of infragravity waves and also adjusted the model according to infragravity waves.

Section 2.3: Why do the authors give an example of waves within the swell regime, while the topic of the article is infragravity waves?

It was a bit easier to see, but we realize that may be confusing. This has now been changed in the plot.

Line 172: Remove 'can'

Done

Line 184-190: I feel figure 5 needs a more detailed description. I see several vertical stripes in figure 5a, which are not explained in the text. Secondly, a clear crest (peak) is visible along the line, but the data (figure 5a) doesn't show the emergence of a second crest, which is visible in the model (figure 5a).
This has now been clarified: "It is worth noting that although the single observed wave observed in Figure 5a matches close with the modeled wave field, it lacks sign of the prior wave modeled as a second red area in the bottom of Figure 5b. This suggests that although we observe an onshore wave, it may not be a part of a strict monochromatic wave field. Also, some vertical lines in Figure 5a differ significantly from surrounding lines and Figure 5b as they represent locations with low coherence."

Line 230: "This corresponds to"

Done

Line 140/235: The ratio of amplitudes between the reflected waves and the incoming waves are not considered. Is it valid to assume that they are equal? If not, the reflected wave might have quite different properties than estimated.

Wave reflection at an ideal wall conserves wave amplitude and we assume a reflected to incident amplitude ratio of 1 simply to illustrate our case. We have clarified this now by stating: "We are also modeling a standing wave as a result of a wave reflecting off a wall in the case the amplitude is conserved (Figure 3c). This is a potential scenario for waves interacting with the lead boundary/iceberg, but with uncertainties related to reflected amplitude and propagation angles."

Line 260: Is there any reason to suspect the ice is not in hydrostatic equilibrium?

Ice near and attached to grounded ridges can be held down for instance at high tide. This can sometimes be clearly observed when water comes up auger hole.

Line 263: This sentence is not completely clear.

This has now been clarified

Line 293: Something wrong with the sentence. I would also rephrase it, because it is suggestive. A 30 second integration time is too short to do a careful spectral analysis. For wave observations in the ocean integration over 10 minutes to 30 minutes is often used.

This refers to the IWR data, which considers a longer timespan.

Line 311: ~1 mm wave propagation -> waves with amplitudes of 1 mm

Changed