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.
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. 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.
   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).
   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.

Technical corrections

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

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

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

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

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

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.

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

Line 127: ‘between crest is greater than’

Line 128: ‘If the propagation’

Line 135: unit missing for alpha.

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.

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.

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?

Line 172: Remove ‘can’

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).

Line 230: "This corresponds to"

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.

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

Line 263: This sentence is not completely clear.

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.

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