Response to reviewer 1

The authors have made a commendable effort revising the submitted manuscript. There are a few remaining issues regarding failures in consistency that have been introduced due to changing interpretation in the revised manuscript and the interpretation of thickness estimates compared with drilled thicknesses that are now provided explicitly. These issues are detailed below:

We thank the reviewer for the second review, and we have made the requested changes to the manuscript. Please see below a detailed answer to each point. We hope that this revision will grant publication to our manuscript.

Line 144-146 "At the location of the deployment, semidiurnal tide reaches 10-20 cm, so it is likely that tides have less effect than changes in temperature between day and night, because the majority of icequakes occurs with a period of 24 hours (figure 6). This periodicity can also be seen in figure 3c, especially between March 1st and March 15th." This should be removed as it doesn't fit with the preceding interpretation. Maybe this was an oversight incorporating the revised interpretation into the original manuscript?

This part of the manuscript was meant to be removed in the revised version indeed. Thank you for pointing this out.

Line 204: "due to thermal expansion and the mechanical tension caused by tide." This also appears to be a sentence fragment from the original manuscript that can now be deleted.
Correct, we have removed this sentence

Line 282-286: Judging from Figure 7, it seems that the flexural wave thickness estimates in this study indeed systematically underestimate compared to drilled thickness by 5-10 cm. Perhaps this should be incorporated into the discussion here? The systematic underestimation of the flexural wave estimates is the dominant impression I am left with from Figure 7, and this seems like an essential point to discuss and try to understand. I agree that the relative increase in estimated thicknesses over the study period seems to agree well with the increase in drilled thickness. Another likely reason the drilled thicknesses are larger than the flexural wave estimates is heterogeneous ice, i.e., the drilled thickness likely includes a layer of superimposed snow-ice with much lower elastic modulus than the underlying columnar ice. In this sense, the flexural wave estimates could be considered effective thicknesses for the assumed elastic properties, though consideration of a sandwich plate with varying elastic properties may facilitate a more precise interpretation than the rather vague concept of "effective thickness". Perhaps the authors made some observations in the field that could shed further light on why the drilled thicknesses appear to be larger than the flexural wave estimates?

The heterogeneity of the ice through the thickness was indeed reported in Moreau et al. (2020a), with much porosity at the surface (compacted snow), and much denser ice at depth. We also discussed that this could affect our thickness and Young's modulus estimations, that would actually correspond to "effective" values.

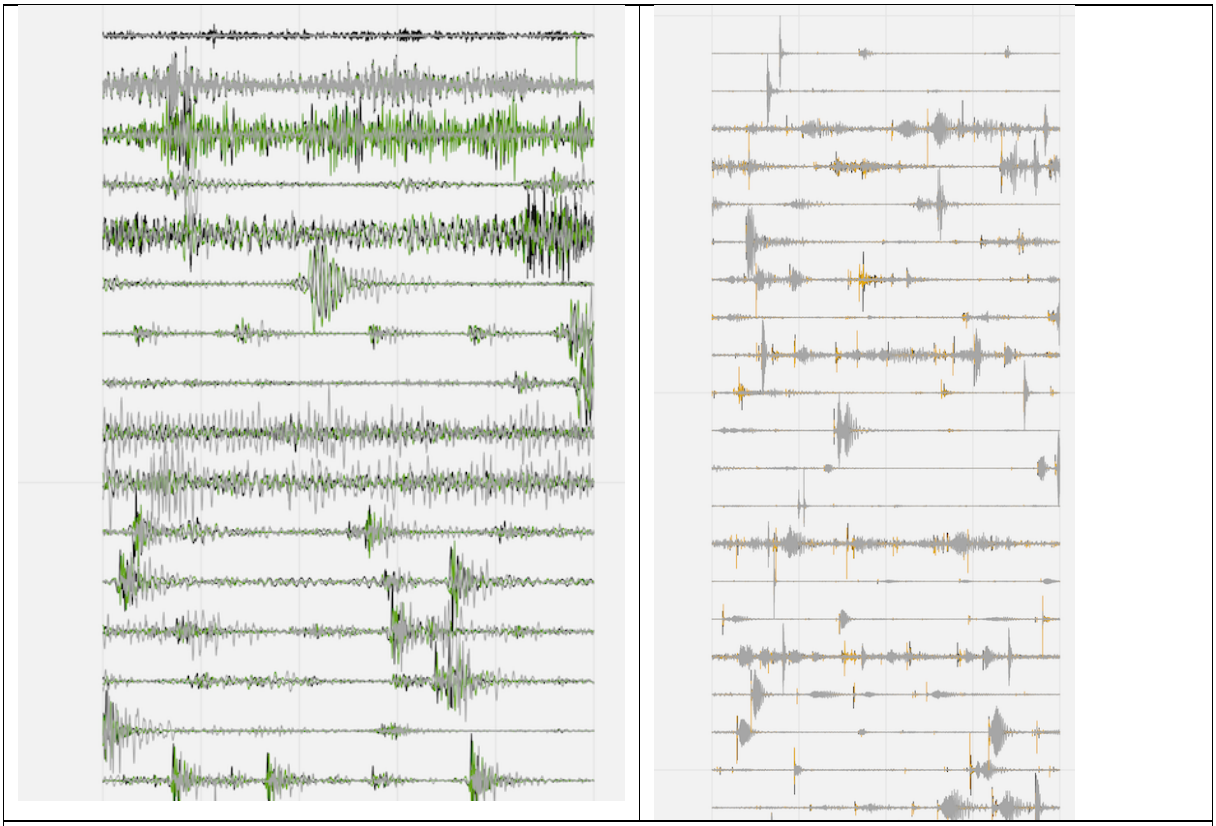
We have, however, explained in the discussion part that using plate theory-based models systematically lead to underestimation of the ice thickness as soon as frequency content above 10 Hz is used in our inversions. The numerical model allows a snow layer to be added on top of the ice. Snow makes the speed of the flexural wave lower and results in underestimation of the ice thickness by a few cm as well. The combined effects of snow and plate theory limitations explain the discrepancies between ice drillings and estimations, which can also be a consequences of spatial thickness vairations by the way.

This is now discussed further in the revised manuscript.

This model, like all models based on plate theory, also suffers limitation of being restricted to low frequencies. Ongoing comparisons between inversions using this model and a full numerical model based on the spectral element method (Cao et al., 2021) suggest that using a model based on plate theory underestimates the ice thickness by a few cm, as soon as the frequency band of interest includes frequencies above 10 Hz, for an ice thickness of 1 m. Moreover, these numerical investigations also reveal that the snow layer, if not accounted for in the model, lead to some estimations that reflect "apparent values" for the ice thickness and mechanical properties (Moreau et al., 2020a). In particular, the snow layer introduces a gradient of porosity through the thickness which makes the flexural wave velocity lower, resulting in potential underestimation of the ice thickness. These are, however, preliminary results and the investigation is still ongoing. The full study will be presented in details in a separate paper. These limitations to the model (plate theory and not accounting for snow), together with the spatial heterogeneity of the ice thickness in the field, explain why the thickness estimations appear to be slightly less that those measured by drilling the ice (figure 7b).

Line 302: Can we expect that the Scatseisnet clustering will give the same arrangement of clusters/families for icequakes and noise sources for polar pack ice in the open Arctic ocean? Given that automation is a key goal of the study, it might be worth adding that manual intervention may still be required during an initial calibration phase at a given field site, in order to interpret the type of events that the automatically extracted clusters correspond to?

Yes, this is expected. Scatseisnet was actually also applied to data recorded during the Damocles expedition on drifting pack ice (2007), and to data recorded on a lake in Finland near Lammi (Lake Pääjärvi 2021 and 2022). Similar clusters were obtained using identical parameters for each dataset. See the following figure for a screenshot of icequakes waveforms in the clusters obtained from these datasets.



Scatsesinet waveforms for icequakes clusters obtained on drifting pack ice during the DAMOCLES expedition in 2007 (left), and on a lake near Lammi in Finland in 2021 (right).

Obviously, the icequakes cluster from data in 2007 have lower SNR, but it was still possible to obtain thickness estimates (with an average of about 2 m) that were consistent with drillings (see Moreau et al, 2020b).

Different parameters settings would be necessary only when the duration of the events to cluster are of different orders of magnitude (less than a sec, a few seconds, or long-lasting events), which is not the case for icequakes, that all last between 1s and a few seconds.

Line 323-324: "occurs with a recurrence of about 24 hours. This indicates that cracking is likely associated with thermomechanical forcing resulting mainly from both temperatures changes between day and night" Is this another fragment of the original manuscript that was missed upon revision of the interpretation to tidally forced cracking? As written, it doesn't fit with the rest of the manuscript.

This was overlooked after the modifications made to the original manuscript, and is now changed to be consistent with tidal icequakes.

Figure 7: Drilled thicknesses are distinctly along the upper range of the flexural wave estimates. Can this be due to an azimuth effect as Figure 8 indicates can lead to systematic differences in thickness estimates? Or does this relate to the low-frequency effect discussed from line 282-286? It would be useful to annotate the positions of the drill holes on Fig. 7a. See also comment on Line 282-286.

Ice drilling positions are shown in figure 1b of the revised manuscript, and are restricted to the area inside the geophone's locations. It is possible that ice thickness was slightly larger in this area.
We believe that the small discrepancies between the estimations and drillings are mainly associated with modelling limitations. Azimuthal differences in Figure 8 only reflect the natural spatial variations of ice thickness that were also observed in the field.

Response to reviewer 2

We have made all the requested corrections, and we thank the reviewer for pointing these out in the second review.

Line 64 : Suggest the more commonly used “flexural gravity wave” which 1) emphasizes the importance of buoyancy, 2) differentiates it from the (strictly elastic) flexural plate wave approximation commonly used in mechanical engineering & ultrasonics, and 3) will improve search results for this paper.

We have made this change.

In general, I would argue against using the ‘quasi’ terminology for a seismology paper since those terms are mostly restricted to ultrasonic NDT references. In this case, the authors do make a valid justification based on consistency with prior works by the same author (which *were* in the ultrasonics regime).

Yes, this terminology is to remain consistent with our previous papers.

Line 69 : “one-to-one” implies a linear (m=1) relationship. Suggest simply “direct”. Line 121–122 : “another stations”. Should be “other stations” or “another station”.

Lines 144–146 : Incongruous statement from previous version suggesting that the icequakes are temperature- driven.

Line 155–156 : Please specify UTC vs Local. UTC is mentioned in Line 154, but the use of “noon” on Line 156 makes this ambiguous since noon is generally only meaningful for local time.

Line 169 : Suggest “...we briefly recall the inversion method...” Line 204 : Orphaned sentence fragment.

Line 210 : “which mass was 39 tons”. Suggest “with a mass of 39 tons” or “for which the mass was 39 tons” or “whose mass was 39 tons”.

Line 240 : “isolate inversions for which source position...”

Line 243 : “The amplitude of a guided wave...”

Line 269 : “Gute**n**berg-Richter”

Lines 273 : “...between the icequakes source and the 5 geophones...”

Line 307 : Suggest “...that can telemeter the continuous recordings via satellite...”

Line 317 : “The analysis consists of a two-steps...” (‘step’ should be singular, at least in American English.)

Line 321 : “...the average ice thickness between the source**s** and the geophones.” Should be plural for consistency of “seismic sources” in first half of sentence.

All done

Line 323–324 : Incongruous statement from previous version suggesting that the icequakes are temperature- driven.

Line 334 : “...waveform inversions strategies...” Inversion should be singular.