We warmly thank the reviewer for such an in-depth review that is a significant contribution to improving the manuscript. We have answered all points in details. Please see below.

General Comments

This is a nice manuscript that I enjoyed reading. The large catalogue of icequake waveforms that the authors have analyzed makes a new and important contribution to the field. The authors also demonstrate a methodology to efficiently isolate these icequake signals in long-term ambient recordings that appears to work well. The authors also make a nice attempt to calibrate their measurements so that the magnitudes and rupture lengths associated with the recorded icequakes can be quantified (roughly). I think there are several aspects that can be significantly strengthened in the manuscript, mostly relating to how the results of the study are presented and interpreted. I have outlined these aspects in detail in the following sections and expect that it should be quite possible for the authors to address these with relatively minor modifications to the manuscript.

Apparent variation in ice thickness

It is notable that the standard deviation corresponding to thickness estimates from individual estimates is quite small, 2 cm, but the range of thicknesses estimated from multiple events during any given period is much larger at around 20 cm (shown in Figure 6b). The authors only comment on the long-term increasing trend as reflecting ice growth over the month-long experiment but do not give much attention to the spread in estimates. Do the authors think that this spread reflects actual spatial variation in ice thickness, and can this be confirmed by the ice drilling? If not, could there be some other effect that explains why the thickness estimates vary so much?

The standard deviation of each thickness estimate is related to how well the model fits the data in the MCMC inversion, hence it should, in theory, not be related to the range of values found when one looks at all positions. This should be mitigated by the fact that the inferred values integrate 5 different propagation paths, and thus to a small extent they are sensitive to spatial variations. But not to the point that it translates in the Markov Chain. To make this point more convincing, we are adding the following figure to the manuscript.

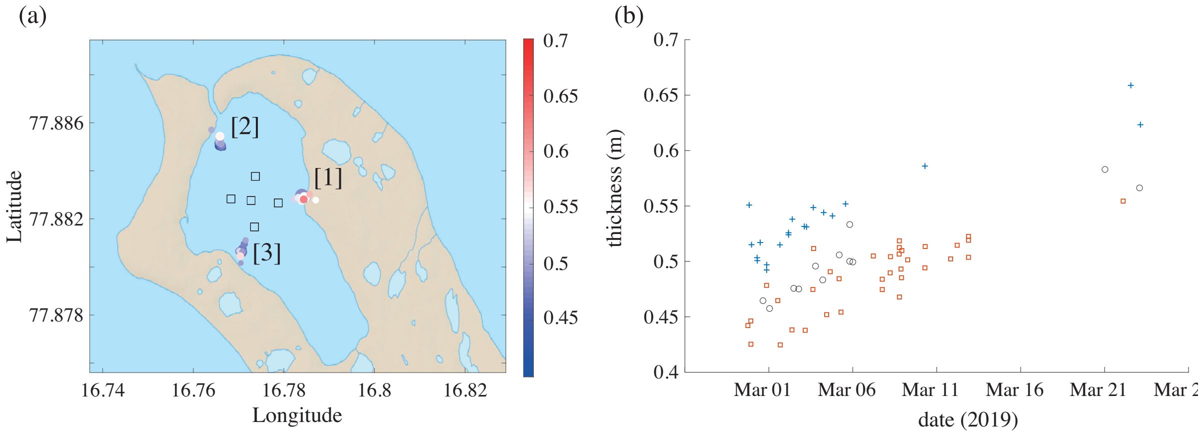


Figure 8 – a) Same as figure 7a, restricted to icequakes originating only from directions marked as [1], [2] and [3]. b) same as figure 7b, for the icequakes in the three groups shown in figure 8a: + are for group [1], o are for group [2] and □ are for group [3]. Inversions originating from a same region and at a similar time have a range of thicknesses that remain within the standard deviation. When comparing all directions, however, the range of thicknesses is of the order of 20 cm.

It looks like there is a trend that the ice close to the shoreline was thicker than the ice away from the shoreline (e.g. Figure 6a). Can this be confirmed as real spatial thickness variation by drilling? The apparent increase of flexural wave estimated ice thickness close to the shore is also consistent with observations of Romeyn et al. (2021). Could this be explained in terms of a finite-plate boundary condition effect as hypothesised in Appendix 1 of Romeyn et al. (2021)? According to that hypothesis, with a Poisson's ratio of 0.28, a correction factor of 0.62 should be applied to equate the thickness of a clamped plate (representing ice near the shoreline) with a simply supported plate (representing ice farther from shore) giving equal maximum tangential stresses. The thickness estimates in Figure 6 are about 0.7 m near the shoreline and $0.7 \times 0.62 = 0.43\ m$ which is strikingly consistent with the thickness estimates located further away from the shoreline. Could this be an explanation for the large spread in estimated thickness (~20cm) that is observed at a given time, as shown in Figure 6b?

To reiterate, there are several mentions of drilled thicknesses but the actual results i.e., thicknesses, and locations of these measurements are not given. These should certainly be added given the usefulness of physical thickness measurements for validating, calibrating and understanding the flexural wave thickness estimates.

This is an interesting hypothesis. We agree that the figure may leave this impression. However, this is an artefact that comes from the fact that sources positions were plotted with respect to the date of icequakes occurrence. Since ice thickness increased with time during March 2019, sources associated with large thicknesses appear on top of hundreds of other sources associated with smaller thicknesses. To the north of the stations, for example, many sources with small thickness appear to be closer to the shore than sources with larger thicknesses. Unfortunately, we do not have drillings to verify that.

We believe that the correction factor does not apply here, since this would lead to thicknesses that are not consistent with the few drillings that we made in the field near station S1. These were reported in Moreau et al. (2020a), with exact locations, but we have indicated them in figure 1b of this manuscript and reported the corresponding thicknesses in figure 7b. Three drillings were made within a radius of 30 m around station S1. One was performed on March 1, giving a thickness of 62 cm, and two were performed on March 26, giving thicknesses of 70 and 73 cm. It is worth mentioning that Marchenko et al. (2021), also reported an ice thickness of about 80 cm on March 11 near the area marked with a black square in figure 6a.

Spatial interpretation of ice thickness estimates

The authors state on Line 246: "Our estimations of ice thickness represent an apparent value that is averaged between the icequakes source and the 5 geophones". Is this property known or is it an assumption (that the thickness estimate represents the average ice thickness between source and receiver)? To test this one would need to do a reciprocity test, i.e., does switching the source and geophone position give an identical signal over an area where the ice thickness is varying? How can we discard the possibility that the recorded signal is dominated by the ice thickness in the vicinity of the recording geophone, for example? Indeed, this would be consistent with the adiabatic wave concept whereby the phase velocity of guided waves varies smoothly according to the local thickness as they propagate through a waveguide with a gradually varying thickness. Here are a few references that give some background on this topic:

Ech-Cherif El-Kettani, Mounsif & Luppé, Francine & Guillet, A. (2004). Guided waves in a plate with linearly varying thickness: Experimental and numerical results. Ultrasonics. 42. 807-12. 10.1016/j.ultras.2004.01.071.

El Kettani, M. C., & Hamitouche, Z. (2009). Inverse problem for the geometry profile determination of waveguides with varying section using adiabatic behavior of guided waves. IEEE transactions on ultrasonics, ferroelectrics, and frequency control, 56(9), 2023-2026.

Hu, Z., An, Z., Kong, Y., Lian, G., & Wang, X. (2019). The nonlinear S0 Lamb mode in a plate with a linearly-varying thickness. Ultrasonics, 94, 102-108.

The jumps the authors studied near stations S3 and S5 could be used to test source-receiver reciprocity, although the result will still be ambiguous if the ice thickness is constant between stations S3 and S5. The tomographic inversion technique proposed by the authors for a future study might also help to resolve this issue, but I would be careful about assuming that a simple path average is the solution based on the data that has been presented to date. Please consider this point carefully and at least re-phrase along the lines of "we assume that the estimations of ice thickness represent an apparent value that is averaged between the icequakes source and the 5 geophones".

As stated by the reviewer, adiabatic mode propagation relates to phase velocity. Using this concept to estimate the ice thickness is therefore adapted for example in the case of the air-coupled flexural wave (Romeyn et al., 2021), or for guided waves in a free plate where local wavenumbers (and thus phase velocities) can be extracted such as in El Kettani, M. C., & Hamitouche (2009).

In the approach of the manuscript, however, we make use of the group velocity. The inversion is based on the arriving time of all frequencies, not just one. Hence it is not trivial to answer the reviewer's question. In Moreau et al. (2014), (now a new reference in the manuscript) it was shown that the dispersion curves of the modes propagating in a plate with a linear thickness variation can be fitted with those obtained using a forward model that accounts for the linear variations of the thickness. The fit is obtained when the thickness in the model corresponds to that directly at the center of the array of receivers.

In the present case, a model of constant thickness is used, so a definite answer to this question cannot be given without a dedicated and thorough study, which falls out of the scope of this paper, especially considering that we have started moving towards a full numerical model in order to not be limited by the low-frequency approximation or by mechanical and topological variations of the ice. We can, however, give a partial answer to the reviewer by including the results of a few inversions made using synthetic signals obtained in floating ice with a linear thickness variation, with a spectral element-based model.

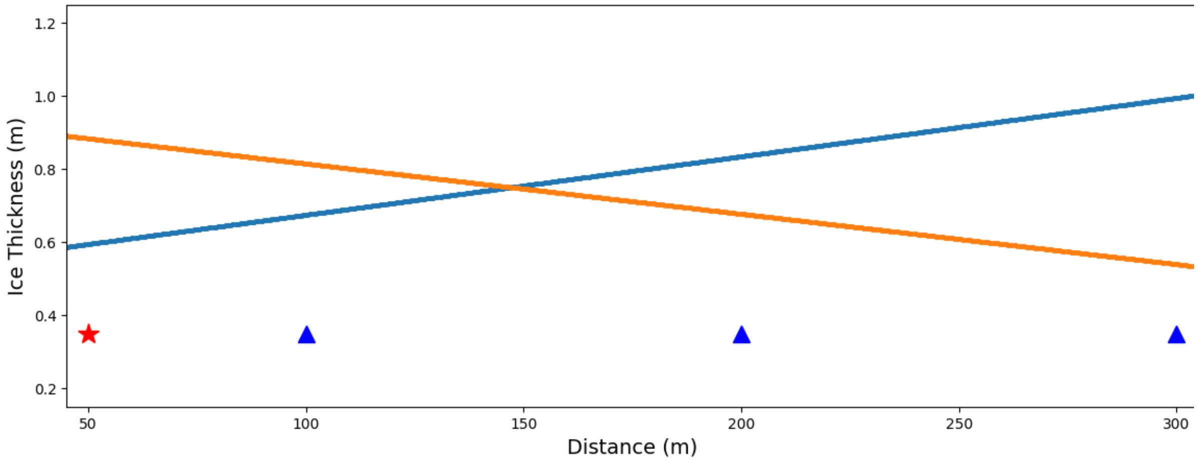


Figure 9 – Two profiles of ice thickness variations. Source position (vertical load) is shown as a star, and three receivers are shown as triangles.

We have considered two profiles of ice thickness. One where the thickness is decreasing, and one where the thickness is increasing, as shown in figure 9. The waveforms obtained from these models at the receivers' positions are shown in figure 10 for the decreasing ice thickness case and in figure 11 for the increasing ice thickness case. These waveforms are compared with waveforms obtained with a floating ice sheet of constant thickness equal i) to the average thickness between the source and the receiver on the one hand, and ii) to the thickness value that generates the best fit with the waveforms on the other hand.

The results of this numerical investigation are shown in tables 1 and 2. They indicate that waveforms from a floating ice sheet with linear thickness variation are most of the time slightly closer to waveforms obtained from a floating ice sheet with a constant thickness equal to that under the receiver than they are to waveforms obtained from floating ice sheet with a constant thickness equal the average thickness between the source and the receiver.

	Thickness under receiver	Average thickness between source and receiver	Constant thickness providing best fit
Receiver 1	86	89	85
Receiver 2	75	83	78
Receiver 3	64	78	70

Table 1 – parameters of simulations for a floating ice sheet with a decreasing thickness

	Thickness under receiver	Average thickness between source and receiver	Constant Thickness providing best fit
Receiver 1	62	59	65
Receiver 2	75	65	71
Receiver 3	88	72	78

Table 2 – parameters of simulations for a floating ice sheet with an increasing thickness

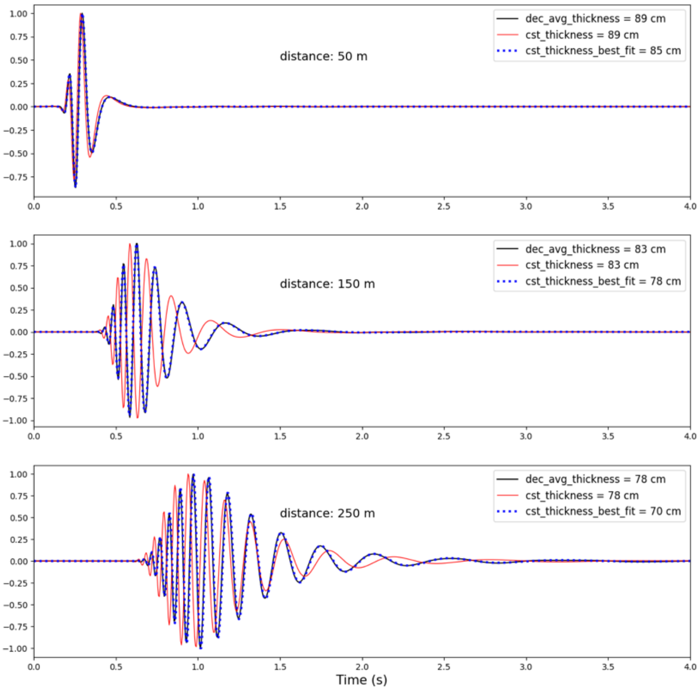


Figure 10 – Waveforms from a spectral element-based forward model of a floating ice layer on an infinite water column. Black solid lines: waveforms at the position of the three receivers located 50, 150 and 250 m away from the source in a floating ice layer with a decreasing thickness as shown in figure 9 (black solid line). Red solid lines: waveforms obtained in a plate which constant thickness is equal to the average thickness between the source and the receiver. Blue dotted lines: waveforms obtained in a plate which constant thickness returns the best fit with the waveforms from the decreasing ice thickness.

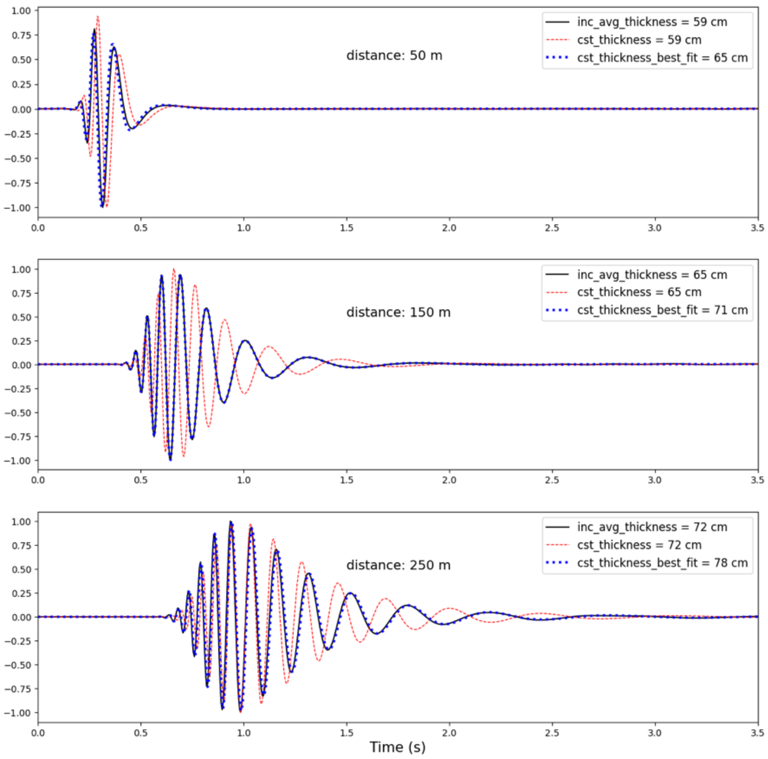
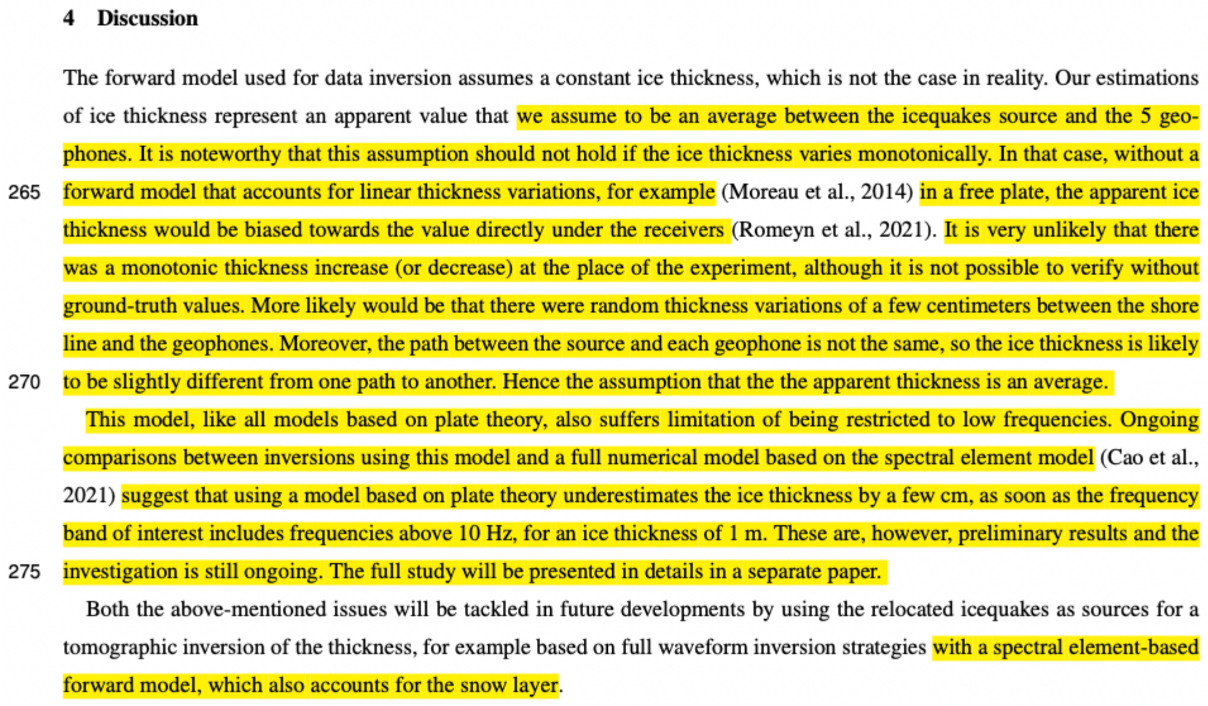


Figure 11 – Same as figure 10 with an increasing ice thickness.

This is, however, specific to a case where ice thickness varies monotonically. In the fjord, however, it is more likely that thickness variations are both increasing and decreasing along the wave propagation paths. In that case, the effects of increasing and decreasing thickness cancel each other. In the revised manuscript, this is now discussed, without going as deep as the analysis shown here, in order not to blur the main message of the paper. Here is what we have modified in the Discussion section.



4 Discussion

The forward model used for data inversion assumes a constant ice thickness, which is not the case in reality. Our estimations of ice thickness represent an apparent value that we assume to be an average between the icequakes source and the 5 geophones. It is noteworthy that this assumption should not hold if the ice thickness varies monotonically. In that case, without a forward model that accounts for linear thickness variations, for example (Moreau et al., 2014) in a free plate, the apparent ice thickness would be biased towards the value directly under the receivers (Romeyn et al., 2021). It is very unlikely that there was a monotonic thickness increase (or decrease) at the place of the experiment, although it is not possible to verify without ground-truth values. More likely would be that there were random thickness variations of a few centimeters between the shore line and the geophones. Moreover, the path between the source and each geophone is not the same, so the ice thickness is likely to be slightly different from one path to another. Hence the assumption that the the apparent thickness is an average.

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 model (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. These are, however, preliminary results and the investigation is still ongoing. The full study will be presented in details in a separate paper.

Both the above-mentioned issues will be tackled in future developments by using the relocated icequakes as sources for a tomographic inversion of the thickness, for example based on full waveform inversion strategies with a spectral element-based forward model, which also accounts for the snow layer.

Interpretation of icequakes as dominantly thermally driven due to 24-hour periodicity

I tend to disagree with the authors interpretation that the 24-hour periodicity of the recorded icequake seismicity counts against tidal stress and in favour of thermal stress as the dominant icequake source mechanism. I have given more details in the specific comment on Line 123-125, but in general the tidal forcing does have a 12/24 hr periodicity and the fact that the tidal magnitude is on the order of tens of centimetres does not necessarily mean the stresses will be insufficient to initiate cracking and produce icequakes. On the other hand, it seems straightforward to demonstrate that the air temperature during the study period does not have a 24-hour periodicity (see specific comment on Line 123-125), due to the low height of the sun and dominance of synoptic weather patterns in driving temperature variation in this region (e.g. Bednorz, 2011). It would certainly make sense to include an illustration of the air temperature timeseries in the manuscript, given that it is also mentioned in several other places relating to the timing of ice growth.

Bednorz, E. (2011). Occurrence of winter air temperature extremes in Central Spitsbergen. Theoretical and Applied Climatology, 106(3), 547-556.

The clustering of icequake seismicity around the perimeter of Vallunden Lake near the shoreline is also consistent with movement on tidal cracks, which are a typical feature of fast ice, driven by the tidal cycle. The authors may find the following reference useful, which gives further detail on stress cycling in the vicinity of tidal cracks based on measurements from Van Mijenfjorden (within a couple of kilometres from Vallunden Lake, the study area of this manuscript):

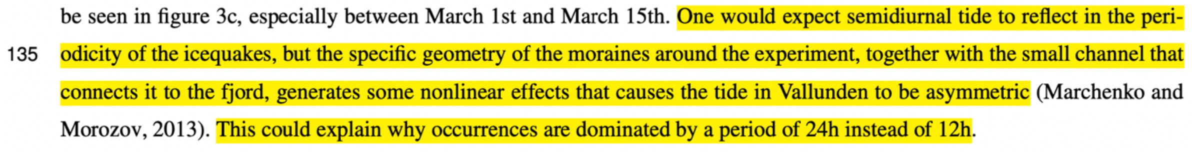
Caline, F. & Barrault, S. (2008) *Measurements of stresses in the coastal ice on both sides of a tidal crack*. In 19th IAHR International Symposium on Ice, Vancouver, British Columbia, Canada, 2008. URL: http://malemuk.com/olofee/wp-content/uploads/2015/12/Paper-070- Caline-and-Barrault.pdf

See also the definition of “tide crack” given, for example, in the McGraw-Hill Dictionary of Scientific & Technical Terms:

“A crack in sea ice, parallel to the shore, caused by the vertical movement of the water due to tides; several such cracks often appear as a family.”

tide crack. (n.d.) McGraw-Hill Dictionary of Scientific & Technical Terms, 6E. (2003). Retrieved December 1 2022 from https://encyclopedia2.thefreedictionary.com/tide+crack

We agree with the reviewer, but we could not find evidence that a 24h period could be associated with tidal cracks, since we were expecting a semidiurnal periodicity. The manuscript is now modified and we have added the reference to Caline and Barrault (2008), as well as that to Marchenko et al (2013) that could explain the specific 24h period at Vallunden.



be seen in figure 3c, especially between March 1st and March 15th. One would expect semidiurnal tide to reflect in the periodicity of the icequakes, but the specific geometry of the moraines around the experiment, together with the small channel that connects it to the fjord, generates some nonlinear effects that causes the tide in Vallunden to be asymmetric (Marchenko and Morozov, 2013). This could explain why occurrences are dominated by a period of 24h instead of 12h.

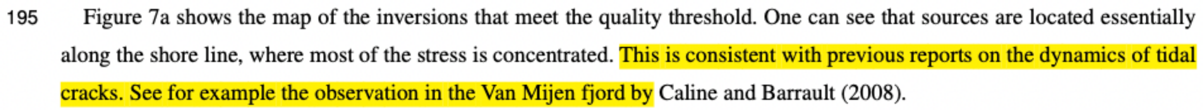


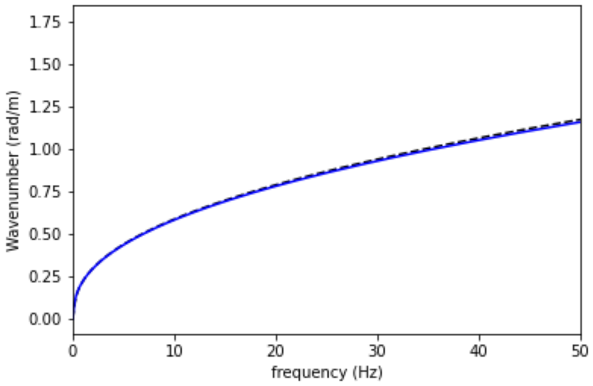
Figure 7a shows the map of the inversions that meet the quality threshold. One can see that sources are located essentially along the shore line, where most of the stress is concentrated. This is consistent with previous reports on the dynamics of tidal cracks. See for example the observation in the Van Mijen fjord by Caline and Barrault (2008).

Water depth effect on QS mode dispersion?

It is worth noting that the authors assume a model of dispersion for the QS mode that does not account for the effect of finite water depth. The maximum water depth in Vallunden Lake appears to be ~10 m based on Marchenko et al. (2021). Using the dispersion relation of Romeyn et al. (2021) that does include the finite water depth, one can estimate that ignoring the water depth (assuming it is infinite) leads to an overestimation of phase velocity by 226% at 1 Hz, 23 % at 4 Hz and 4 % at 8 Hz. The impact on the results of this study may have been assumed to be minor since the dominant frequency of the QS energy is around 8 Hz? However, since the authors state the icequakes are associated with signals spanning the frequency range 1-50 Hz it would be reasonable to give a justification for ignoring the finite water depth in the manuscript.

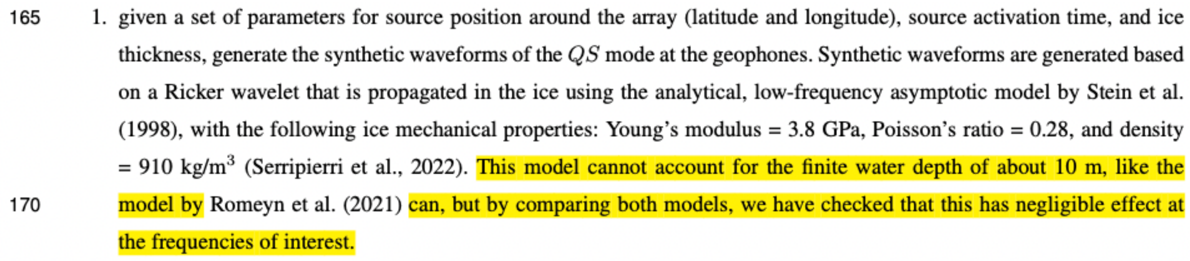
Marchenko, A.V., Morozov, E.G., Ivanov, A.V., Elizarova, T.G, Frey, D.I., (2021) *Freezing of Tidal Flow in Lake Vallunden (Spitsbergen),* Port and Ocean Engineering Under Arctic Conditions, Proceedings 2021 URL: https://www.poac.com/Proceedings/2021/POAC21- 054.pdf

On the vertical channel, dominated by the QS mode, the amplitude of the spectrum of icequake waveforms remains (on average) over -30 dB between 1 and 35 Hz, with a peak value around 8 Hz. On the horizontal channels, where the QS0 and SH0 modes are dominant, the spectrum remains over -30 dB up to 50 Hz (this is now indicated in the manuscript). We assume that ignoring the finite water depth of 10 m has a negligible effect on the inverted thickness, based on a comparison between the model used in the manuscript by Stein et al. (1998) and the model by Romeyn et al. (2021). See for example the following figure. Wavenumbers are almost identical in the frequency range of interest.



Wavenumber vs frequency for the QS mode, calculated in a 60 cm-thick ice sheet floating on water. Blue solid line: model by Stein et al. (1998) based on an infinite water depth. Black dashed line: model by Romeyn et al. (2021), based on water with a 10 m depth.

We have added the following sentence, at the beginning of section 3.1 to explain this.



1. given a set of parameters for source position around the array (latitude and longitude), source activation time, and ice thickness, generate the synthetic waveforms of the QS mode at the geophones. Synthetic waveforms are generated based on a Ricker wavelet that is propagated in the ice using the analytical, low-frequency asymptotic model by Stein et al. (1998), with the following ice mechanical properties: Young's modulus = 3.8 GPa, Poisson's ratio = 0.28, and density = 910 kg/m^3 (Serripierri et al., 2022). This model cannot account for the finite water depth of about 10 m, like the model by Romeyn et al. (2021) can, but by comparing both models, we have checked that this has negligible effect at the frequencies of interest.

Discussion

In general, I think the discussion focusses a bit much on future research prospects without fully discussing the results of the present study and their implications. There are some interesting results presented in this study from a large catalogue of icequakes and I think that these should be focused on a bit more since this, to me, is the most novel aspect of this study.

We have modified the discussion, which now focuses more on the limitations of our approach and on the average thickness hypothesis.

Specific Comments

Line 17-18: “...sea ice is an essential element of polar regions because of the role it plays in phytoplankton production, and in several atmosphere-ice-oceans interactions”. Please add one or more references to support, particularly since the interactions are not explained here.

done

Line 19: “...important negative trend of about 12.6% per decade, according to the National Snow and Ice Data Center”. A specific reference should be included to support this result.

Unfortunately, we could not find another reference to this statement. But surely the National Snow and Ice Data Center is to be trusted.

Line 27-28: “...thick ice filters light more than thin ice, hence thickness influences phytoplankton production”. This should be supported with a reference.

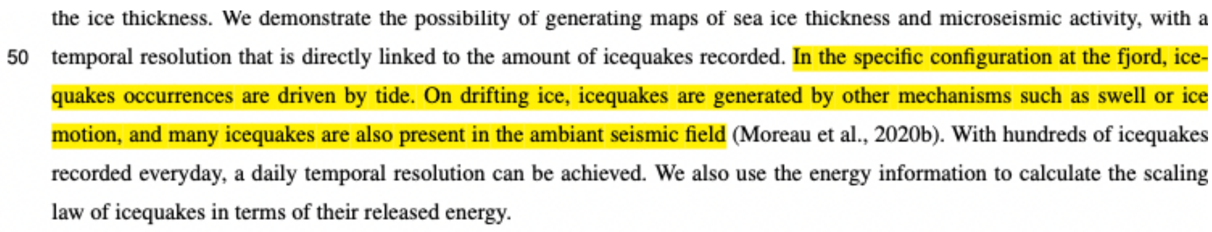
done

Line 28-29: “...Thicker ice is also more resilient to external forcing such as swell or wind forcing.”. This should be supported with a reference.

done

Line 50-51: “With hundreds of icequakes recorded everyday, a daily temporal resolution can be achieved.” It could be worth adding that deployment location is an important consideration for this type of monitoring. In this case the periodic tidal forcing is an important driver of icequake seismicity, but different mechanisms may operate at other locations so that understanding the local icequake seismicity will be important for others aiming to implement this methodology.

We have added the following sentence.

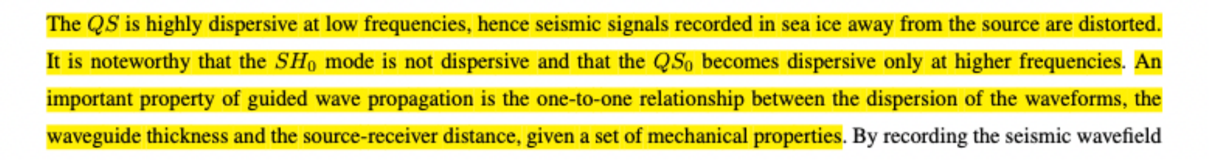


the ice thickness. We demonstrate the possibility of generating maps of sea ice thickness and microseismic activity, with a temporal resolution that is directly linked to the amount of icequakes recorded. In the specific configuration at the fjord, icequakes occurrences are driven by tide. On drifting ice, icequakes are generated by other mechanisms such as swell or ice motion, and many icequakes are also present in the ambiant seismic field (Moreau et al., 2020b). With hundreds of icequakes recorded everyday, a daily temporal resolution can be achieved. We also use the energy information to calculate the scaling law of icequakes in terms of their released energy.

Line 64: “Guided modes are dispersive, hence seismic signals recorded in sea ice away from the source are distorted.” Only some guided wave modes are dispersive, so this sentence should be revised. The following is quoted from Moreau et al. (2020a) which this manuscript follows: *"The SH0 mode is not dispersive, and the QS0 mode becomes dispersive only at much higher frequency- thickness values (above 1000 Hz·m)."*

Line 64-66: "An important property of guided wave propagation is the one-to-one relationship between the dispersion of the waveforms, the mechanical properties of the ice, its thickness and the source-receiver distance". I suggest removing "one-to-one" from this sentence, since these properties are not necessarily independent of one another as a one-to-one relationship would imply. Alternatively, the authors should demonstrate that these properties are independent so that each possible combination gives a unique waveform. Alternatively remove "mechanical properties" since these are assumed to be constant and it is probably justifiable that there is a one-to-one relationship between dispersion of the waveforms, the ice thickness and source-receiver distance. Yet another alternative would be to mention the utility of multimodal for constraining all of these properties (as mentioned several other places in the manuscript).

Both the above remarks have been accounted for by changing the text as follows



The QS is highly dispersive at low frequencies, hence seismic signals recorded in sea ice away from the source are distorted. It is noteworthy that the SH_0 mode is not dispersive and that the QS_0 becomes dispersive only at higher frequencies. An important property of guided wave propagation is the one-to-one relationship between the dispersion of the waveforms, the waveguide thickness and the source-receiver distance, given a set of mechanical properties. By recording the seismic wavefield

Line 74: "...with cracks located for the most part along the shoreline". Don't these cracks fit quite well with the definition of tide cracks? This is given, for example, by the McGraw-Hill Dictionary of Scientific & Technical Terms:

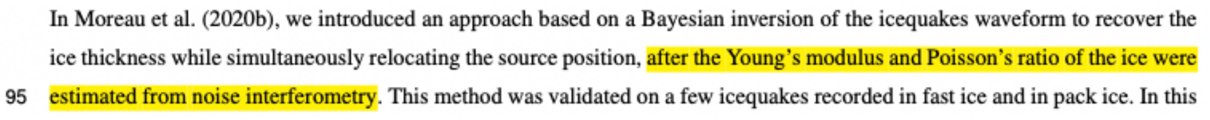
"A crack in sea ice, parallel to the shore, caused by the vertical movement of the water due to tides; several such cracks often appear as a family."

tide crack. (n.d.) McGraw-Hill Dictionary of Scientific & Technical Terms, 6E. (2003). Retrieved December 1 2022 from https://encyclopedia2.thefreedictionary.com/tide+crack

We agree with the reviewer and the manuscript now mentions tide cracks, although the above reference was not included, since it is not a scientific paper.

Line 89-90: "we introduced an approach based on a Bayesian inversion of the icequakes waveform to recover the ice thickness while simultaneously relocating the source position". Consider adding something like "for elastic parameters E and ν assumed *a priori*". It would be useful for the reader to keep track of which parameters are inverted for and which are assumed or held constant.

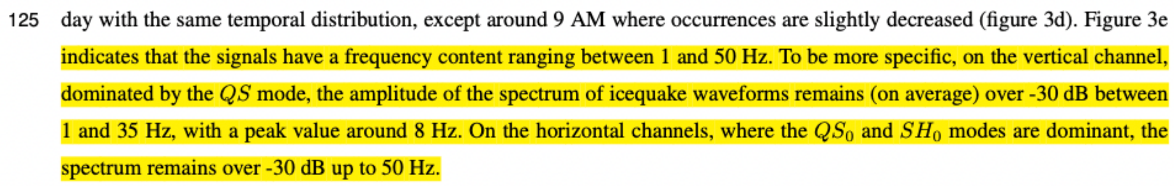
We have modified the sentence:



In Moreau et al. (2020b), we introduced an approach based on a Bayesian inversion of the icequakes waveform to recover the ice thickness while simultaneously relocating the source position, after the Young's modulus and Poisson's ratio of the ice were estimated from noise interferometry. This method was validated on a few icequakes recorded in fast ice and in pack ice. In this

Line 121: "the associated signals have an average frequency content between 1 and 50 Hz" Average frequency content is a bit ambiguous in this context since it can be confused with the second part of the sentence dealing with the dominant or central frequency. What about "the associated signals are composed of frequencies spanning from 1 to 50 Hz"?

We have modified this part as follows



day with the same temporal distribution, except around 9 AM where occurrences are slightly decreased (figure 3d). Figure 3e indicates that the signals have a frequency content ranging between 1 and 50 Hz. To be more specific, on the vertical channel, dominated by the QS mode, the amplitude of the spectrum of icequake waveforms remains (on average) over -30 dB between 1 and 35 Hz, with a peak value around 8 Hz. On the horizontal channels, where the QS_0 and SH_0 modes are dominant, the spectrum remains over -30 dB up to 50 Hz.

Line 122: “Icequakes are likely produced by thermomechanical forcing.” Please add a reference here. I would suggest the following as highly relevant: Olinger, S., Lipovsky, B., Wiens, D., Aster, R., Bromirski, P., Chen, Z., Gerstoft, P., Nyblade, A. A., and Stephen, R.: Tidal and thermal stresses drive seismicity along a major Ross Ice Shelf rift, Geophys. Res. Lett., 46, 6644–6652, 2019.

This reference was added.

Line 123-125: “the majority of icequakes occurs with a period of 24 hours (figure 5). This periodicity can also be seen in figure 3b, especially between March 1st and March 15th.” It must be figure 3c that is referred to here? Moreover, the authors comment on the magnitude of the semidiurnal tide being 10-20 cm, but not on the magnitude of diurnal temperature variations, which should be simple enough to include and could be a very useful addition to the discussion of the 24-hour periodicity of icequakes and its thermomechanical interpretation. At this high northern latitude, the sun only reaches a maximum of ~10 degrees above the horizon in mid-March and it is not a given that diurnal insolation patterns will be the main driver of temperature variations compared to the passage of synoptic weather systems.

Yes this is figure 3c, thank you for pointing this out. We have now changed our interpretation of the original of icequakes to meet the reviewer’s remark (please see next answer).

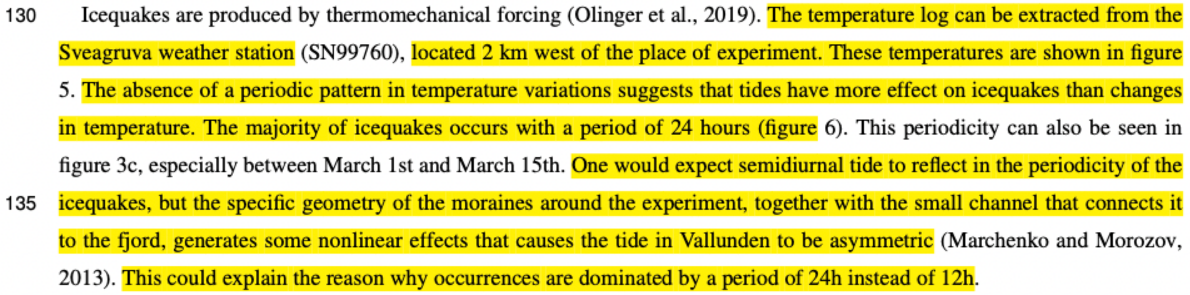
As an example, below is the air temperature record for the nearby Sveagruva weather station (SN99760), obtained from the public database https://seklima.met.no/observations/ for March 2019. The lack of an apparent 24-hour periodicity in the temperature record indicates that, contrary to the interpretation of the authors, the tidal forcing is a more probable driver of icequake seismicity than temperature.

Thank you for pointing to these temperatures data! We have added this figure to the manuscript.

The following paper by Marchenko & Morozov (2013) would also be highly relevant to cite, since it deals exactly with the tidal cycle at the study location presented in this manuscript.

Marchenko, A. V. and Morozov, E. G.: Asymmetric tide in Lake Vallunden (Spitsbergen), Nonlin. Processes Geophys., 20, 935–944, https://doi.org/10.5194/npg-20-935-2013, 2013.

We thank the reviewer for this remark, which we fully agree with. We have revised our interpretation of the 24h periodicity of the icequakes, as follows:



Icequakes are produced by thermomechanical forcing (Olinger et al., 2019). The temperature log can be extracted from the Sveagruva weather station (SN99760), located 2 km west of the place of experiment. These temperatures are shown in figure 5. The absence of a periodic pattern in temperature variations suggests that tides have more effect on icequakes than changes in temperature. 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. One would expect semidiurnal tide to reflect in the periodicity of the icequakes, but the specific geometry of the moraines around the experiment, together with the small channel that connects it to the fjord, generates some nonlinear effects that causes the tide in Vallunden to be asymmetric (Marchenko and Morozov, 2013). This could explain the reason why occurrences are dominated by a period of 24h instead of 12h.

Line 178-180: “sources are located essentially along the shore line, where most of the stress is concentrated due to thermal expansion and the mechanical tension caused by tide.” I think this point could be made more rigorously. The observation seems to be in very good agreement with the dynamics of tidal cracks, a commonly observed feature associated with fast ice. The authors may like to investigate other references in addition, but Caline and Barrault (2008) appears highly relevant and is also based on observations from Van Mijenfjorden.

Caline, F. & Barrault, S. (2008) *Measurements of stresses in the coastal ice on both sides of a tidal crack*. In 19th IAHR International Symposium on Ice, Vancouver, British Columbia, Canada, 2008. URL: http://malemuk.com/olofee/wp-content/uploads/2015/12/Paper-070- Caline-and-Barrault.pdf

The reviewer is right and we have added this reference.

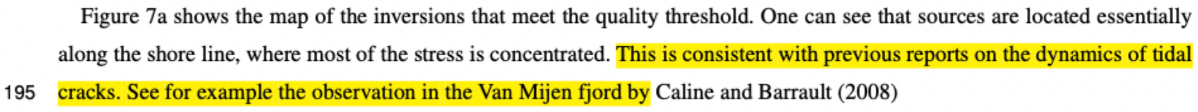


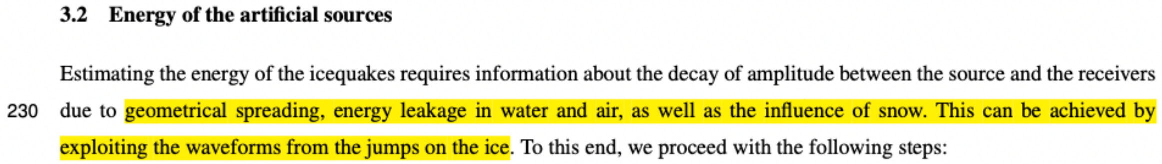
Figure 7a shows the map of the inversions that meet the quality threshold. One can see that sources are located essentially along the shore line, where most of the stress is concentrated. This is consistent with previous reports on the dynamics of tidal cracks. See for example the observation in the Van Mijen fjord by Caline and Barrault (2008)

Line 202-203: “The ice thickness increase was also confirmed by ice drillings on March 1 and March 25.” What were the drilled thicknesses and where were they measured? Please add the drilled thicknesses to Figure 6, it would be quite instructive to see how they fit with the range of estimates. Given the authors also state that the ice thickness is not constant in reality (Line 249), it is important to back this up with measurements supporting this.

Drillings positions and corresponding thickness values now appear in figure 1b. They values now also appear in figure 7b (previously figure 6b).

Line 212: “geometrical spreading, and energy leakage in water” should be changed to geometrical spreading, and energy leakage in water and air. Probably there is also loss/dissipation of energy into the snow pack resting on the ice?

Done



3.2 Energy of the artificial sources

Estimating the energy of the icequakes requires information about the decay of amplitude between the source and the receivers due to geometrical spreading, energy leakage in water and air, as well as the influence of snow. This can be achieved by exploiting the waveforms from the jumps on the ice. To this end, we proceed with the following steps:

Line 273: “The 24-hours periodicity of icequakes, as shown in figures 3 and 5, suggests that the former effect is dominant compared to the latter.” Similar to the earlier comment on Line 123-125 the lack of 24-hour periodicity in air temperature, and the fact that the tidal forcing does have a 12/24hr periodicity (Marchenko & Morozov, 2013) rather suggests the opposite, i.e., that tidal forcing is the dominant driver of icequake seismicity recorded in this dataset.

Correct. This has been modified in the manuscript.

Specific comments on figures

Figure 2: Since the raw data was converted to displacements and the instrument response has been deconvolved, the units of displacement should be included in the figure.

After double-checking this, it appears that there was a mistake in our response to the editor who asked a similar question about instrument deconvolution and data conversion. We apologize for this confusion.

We converted the raw data into miniseed format using the Fairfield software, but without Instrument response deconvolution, since it is not necessary for our methodology. Also, the data are expressed in mV, but could be converted to a velocity by dividing the waveforms data by the proportionality factor 89 V/m/s, and further converted to displacement by integration with respect to time. However, this is not necessary either for our methodology. This is now explained in the manuscript.

Figure 3: Black vertical line indicating threshold distance should be annotated in the figure caption.

done

Figure 6: Since Figure 6a includes both spatial and temporal thickness variation it is hard to interpret. Consider adding an additional panel showing the results from one day of recording (discussed as a possibility from Line 207-209)?

We have added a new figure (now figure 8) which is introduced in the response to the question about the standard deviation of the thickness estimations.

Technical Corrections

Line 146: Acronym MCMC should be stated in full as Markov Chain Monte Carlo on first use.

Done