General Comments

This is a nice manuscript that I enjoyed reading. The large catalogue of icequake waveforms that the authors have analysed 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?

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.

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".

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

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

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 focussed on a bit more since this, to me, is the most novel aspect of this study.

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.

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.

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

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.

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.

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).

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*

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.

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"?

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.

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.

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.

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.

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

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.

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?

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.

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.

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

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)?

Technical Corrections

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