I thank the reviewers for the extra material and for their responses to my comments. I have not been able to find the updated manuscript, but I suppose that it is getting close to a publishable version. In particular, authors have convinced me that it is suitable to keep the term air-coupled flexural wave, which was my main concern. However, the answers by the authors have raised some new comments that I think should be addressed before publication in The Cryosphere.

Title of the manuscript

"Elastic properties of floating sea ice from air-coupled flexural waves" means that the elastic properties of sea ice are expected as an outcome from this paper. However, the authors do not estimate them, only the ice thickness is inferred. Elastic properties cannot be constrained only by the (air-coupled) flexural wave: the longitudinal and shear-horizontal modes are required. Also, the term "floating" is unnecessary, given that it is implicit when one deals with sea ice. Wouldn't it be more relevant that the title was changed along the lines of "Sea ice thickness from air-coupled flexural waves"?

Resolving local thickness variations

"The frequency-wavenumber approach would undoubtedly decrease the variability of the thickness estimates, but an element of this decreased variability comes from spatially averaging real thickness variation over the aperture of the array. We interpret that the variability observed in figure 4d in the manuscript primarily reflects spatial variability in ice thickness"

The authors state that their method is sensitive to local thickness variations. Surely the method in this manuscript is a nice, original and complementary methodology to already existing methods for estimating sea ice thickness. While I understand the enthusiastic tone (this is a good paper indeed!), I think some conclusions should be tempered. I am not convinced that this method can resolve local thickness variations to a point where it becomes significant compared to uncertainties. For example:

- The range of spatial thickness variations remains within the range of variations in a 24-hours time window, which is up to 25 cm in 2013 (figure 6), 30 cm in 2016 (figure 7) and 30 cm in 2017 (figure 8). Obviously, the ice has not grown by such an amount in such little time. This means that the uncertainties of the estimates are larger than the inferred local variations. This can also be seen in the comparison with borehole measurements, which indicates that 68% of the estimates (15 out of 22) are out of the 25-75th percentile range.
- To explain the outliers in figure 8, where ice thickness is overestimated by 150-175%, the authors assume that this may come from an increase of Young's modulus (and thus an increase of the ice-bending rigidity), due to the additional support near the shore, where the ice is landfast. It is not at all obvious that the additional support near the shore could be such that it would explain such thickness overestimation. Please elaborate on this.

"We consider the methodology we propose is complementary to the Moreau et al. (2020a/b) methodology, which is an elegantly refined and modernized implementation of the frequency-wavenumber methods familiar from earlier studies (e.g. Yang & Yates, 1995)"

It is true that the frequency-wavenumber approach in Moreau et al. (2020a) cannot resolve local thickness variations. However, it is very important to note that the approach in Moreau et al. (2020b) is not at all based on a frequency-wavenumber analysis. Rather, it is based on the noise correlation function to infer the elastic properties of the ice, combined with a time-frequency analysis of icequakes waveforms to infer the thickness. This methodology is completely new, and its advantages are twofold: i) it tackles the fact that the frequency-wavenumber analysis averages thickness variations along the array aperture, and ii) only 3 stations are required, not an entire array.

l. 346 of the manuscript. "This is one of the main points of evidence that the frequency of air-coupled flexural waves is controlled primarily by ice thickness in the vicinity of the receiver."

Given the high uncertainty of the estimations, it is not clear to me that this is really the case. I also cannot see the physical reason for this. Some numerical investigations are needed to be more convincing.

Frequency-wavenumber analysis

From figure x (shown in the response by the reviewers), it appears that despite aliasing, the ice flexural wave can still be extracted and used for an inversion of the ice thickness. I am curious to see how different the thickness estimate from the FK analysis is from the estimation with the air-coupled flexural wave.

Comparison with air-coupled transducers in NDT

"We suggest re-phrasing to *"allowing improved operational efficiency compared to applications using sensors bonded to the surface (e.g. Zhu, 2008)."* The point here is that fully non-contact air-coupled non-destructive evaluation systems have the potential to be more efficient from an operational perspective because they avoid having to bond sensors directly to the surface. This is relevant to the air-ice-water system where, as we hypothesize, sensors in air (microphones) along the shoreline may be used to estimate the thickness of the ice adjacent to the shoreline."

With much respect to the authors, I do not understand why the comparison with NDT is relevant here. It seems to me that the similarity is only in the name "air-coupled." On the one hand, the physics of the air-coupled flexural wave is quite specific, and on the other hand, air-coupled transducers is mainly about adjusting the transducer's orientation for maximizing the energy transmitted to the plate (what the authors refer to as coincidence frequency, l. 478 of the manuscript). But maybe I am missing the point… Currently, it is hardly less operationally efficient to put a sensor in contact with a structure than to use an air-coupled transducer, which require accurate positioning.

About the use of geophones in thin ice conditions and the potential of the method to be an alternative

"As we discuss, this could be of significant practical gain when the ice is too thin to safely traverse, which is otherwise a limitation for the geophone-based experiments presented in this and other studies, e.g. Moreau et al. (2020a/b)."

Actually, geophones can be installed on the ice using a drone and then transmit data continuously. Given that only 3 geophones are sufficient to monitor the ice (Moreau et al 2020b), their use should not be considered as a limiting factor, even for thin ice conditions. It is also possible to use fiber optics deployed by drone to apply the same methodology on distributed acoustic sensing:

Coutant et al. (2021), Measuring floating ice thickness with optical fibers and DAS, a test case study on a frozen moutain lake., EGU General Assembly 2021, online, 19–30 Apr 2021, EGU21-7404, https://doi.org/10.5194/egusphere-egu21-7404

The ice is too thin to safely traverse when its thickness is less than 10-15 cm. Given the large uncertainty of the estimations and also the error when comparing thickness estimations with borehole measurements (~ 5cm on average) discussed above, do the authors think that their method could be a viable approach?

Comparison with sounds from cracks generated by ice-skating on lakes

Abstract l. 20

Introduction l. 75

Section 1.2 l. 100

Section 6, l. 465

In the conclusion

"We would also argue that the microphone recording of ice-skating, illustrated in Figure 12 of the manuscript, demonstrates that this is a reasonable hypothesis as it has already been achieved for thin fresh-water ice."

Regarding this comparison with the sounds of cracks from ice-skating on frozen lakes, I respectfully disagree with the authors. The sounds heard in the audio tracks from reference Rankin (2018) are not monochromatic. It is quite clear, just by listening to them, that these sounds have a very dispersive nature, with the high frequencies arriving before the low frequencies. This is what produces the famous laser-like sounds. The perception of a monochromatic wave is quite different. This dispersive sound is the result of the flexural wave leaking in the air, and cannot be attributed to the air-coupled flexural wave.

I have checked this by extracting the audio file from the video in Rankin (2018), and then calculated the time-frequency spectrum of this signal. Figure 1 shows a zoom around 0mn8s. It is clear that these waveforms are very dispersive, and thus are not from an air-coupled flexural wave. They correspond to the flexural wave, like the ones from Moreau et al (202b), shown in figure 2. Therefore, I think all occurrences where this comparison with ice-skating sounds is made in the manuscript should be removed, including figure 12. I actually wonder how this spectrogram was calculated (which segment of the audio track?), and how trustworthy it is.

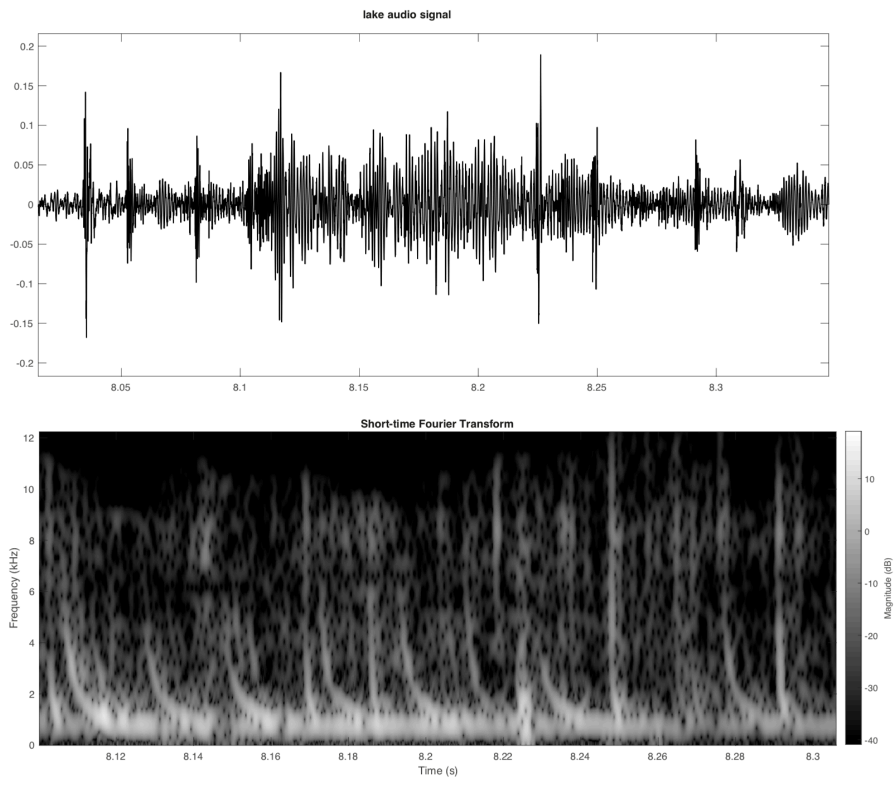

Figure 1 – Top: audio signal extracted from the video in Rankin (2018) round 0min8s. Bottom: short-time Fourier transform of the signal.

Applying the methodology to passive data

Air-coupled flexural waves appear to be absent from the icequake data recorded by Moreau et al (2020b) at the Van Mijen Fjord, and on drifting sea ice during the DAMOCLES experiment. See for example figure 2, where a few representative icequakes are presented, together with the associated time-frequency spectrum. Also, the frequency-thickness at which energy is exploitable is less than ~ 25 Hz.m, which is far below the minimum frequency-thickness required to record air-coupled flexural waves (48 Hz.m). Therefore, it seems quite unlikely that air-coupled waves can be measured without an active source. The conditions would most-likely be even worse when recording with a microphone and the presence of a snow cover.

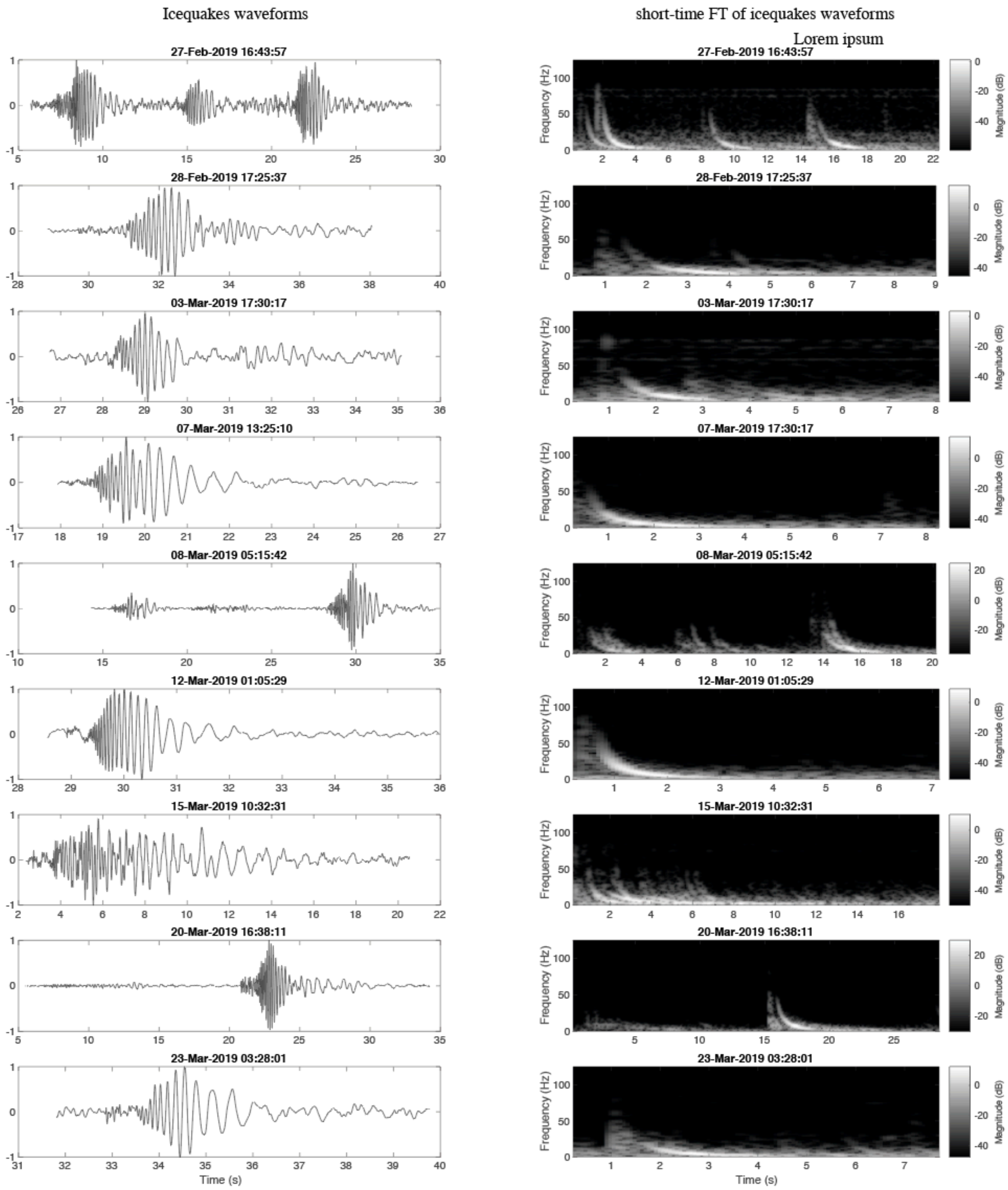

Figure 2 – left: waveforms of the icequakes recorded at the Van Mijen fjord in 2019 by Moreau et al (2020a/b). Right: associated time-frequency spectrum. The ice thickness varied between ~50 to ~70 cm.

"The excitation of air-coupled flexural waves by natural crack formation/propagation in floating ice sheets raises the possibility of passive monitoring of ice thickness and rigidity. "

From what precedes, it appears that the method in the manuscript is extremely unlikely to be suitable for application to passive monitoring using natural cracking of the ice.

Conclusion

"a key benefit of air-coupled flexural waves is that they should also be recordable with very simple equipment, such as a microphone located in the vicinity of where a thickness measurement is desired, either above the ice-sheet or along the shoreline. Cracks in the ice, either produced artificially by, e.g., ice skates on thin ice, or naturally occurring, represent possible alternative impulsive sources capable of exciting air-coupled flexural waves."

Given that what is heard from ice-skating cracks is not an air-coupled flexural wave, but simply a flexural wave, and also given that natural micro-seismicity is not producing an air-coupled flexural wave (as shown in figures 1 and 2), this statement remains wrong until the authors can prove that a naturally-generated air-coupled flexural wave can be measured, especially in sea ice. Therefore, this should also be removed from the conclusion

Sincerely,

Ludovic Moreau