Author response to RC3

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"?

We agree with the reviewer and we're happy to change the title to *"Sea ice thickness from air-coupled flexural waves"*.

Resolving local thickness variations

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.

We agree that we should revise the interpretation of results to be more conservative, particularly with regard to temporal change in ice thickness in 2013, 2016, and 2017, since we lack sufficient ground truth observations that could have shed more light on this. This was also highlighted by the other reviewer. This will also allow us to focus more directly on the 2018 example where an increase in ice thickness is clearly resolved and consistent with visual observations made in the field.

- 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.

This hypothesis is at least supported by the colloquial experience of ice skaters, from the Rankin (2018) article referred to in the manuscript:

> "Five centimeters [2 inches] is often the limit [to how thin it can be]. If you're close to shore, you can go thinner, up to about 3.5 centimeters, before it breaks."

This is, coincidentally, exactly in proportion with our observation that ice of 35 cm drilled thickness near the shore has a similar air-coupled flexural frequency to 50 cm thick ice further from shore (under the assumption that bearing capacity and flexural rigidity are related). A possible theoretical justification for these observations can be given by representing an ice sheet as a collection of arbitrarily small, finite, uniformly loaded, circular plates. The ice along the shoreline is represented by assuming a clamped boundary condition, while the ice far from shore is considered simply supported by the neighboring plate elements. We may then find the thicknesses of a simply supported and clamped plate that lead to equal maximum tangential stresses. From Hearn (1997), the maximum tangential stress, occurring at the center of the simply supported plate, σ_s, and the clamped plate, σ_c, is given by

$\sigma_s = \frac{3qR^2}{8{h_s}^2}(3+v)$ and $\sigma_c = \frac{3qR^2}{8{h_c}^2}(1+v)$.

Here, q is the uniformly distributed load, R is the plate radius, v is Poisson's ratio, h_s and h_c are the thicknesses of the simply supported and clamped plates, respectively. Equating the two expressions gives

$$\frac{{h_c}^2}{{h_s}^2} = \frac{(1+v)}{(3+v)}$$

so that $h_c \approx 0.63 h_s$ for $v = 0.33$. Therefore, it is anticipated that ice 50 cm thick far from shore will experience the same maximum tangential stress under load as ice that is $0.63 \times 50 = 32$ cm thick at the shoreline (consistent with the outliers discussed in the manuscript). We may similarly equate the maximum deflections of the plates, which also occur at the plate centers. Hearn (1997) gives the relevant expressions for the maximum deflection of the plates as

$w_s = \frac{3qR^4}{16E{h_s}^3}(5+v)(1-v)$ and $w_c = \frac{3qR^4}{16E{h_c}^3}(1-v^2)$,

where *E* is the Young's modulus. By equating the two deflections and simplifying we find
$\frac{{h_c}^3}{{h_s}^3} = \frac{(1-v^2)}{(5+v)(1-v)}$,

which again gives $h_c \approx 0.63 h_s$ for $v = 0.33$. Since both tangential stress and strain are equal for a clamped plate with 63% of the thickness of a corresponding simply supported plate, the boundary condition effect can also be understood as a change in effective elastic modulus. This supports the statement in the manuscript that the two outlier borehole thicknesses drilled nearest land could be modelled by spatially varying the effective elastic modulus in order to account for the increased support the ice receives from the land. We are aware that this theoretical description is rather simplistic since, e.g., the fluid-loading is ignored. It does, however, highlight that a simple change of boundary condition can be significant. We highlight that it would be beneficial to study this effect in more detail in future studies, particularly the length scales involved in the transition from clamped to simply supported behavior. Constraining this behavior would require the collection of more detailed field data in the zone near the shoreline as well as the development of a more complete model, such as a full finite element simulation. While this description is likely too lengthy to include in the main body of the revised manuscript, it could be included as an appendix if desired by the review team and the associate editor.

E.J. Hearn, Chapter 7 - Circular Plates and Diaphragms, Editor: E.J. Hearn, Mechanics of Materials 2 (Third Edition), Butterworth-Heinemann, 1997, Pages 193-219, ISBN 9780750632669, https://doi.org/10.1016/B978-075063266-9/50008-1.

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.

This is noted, we'll make sure to be careful to keep this distinction clear when the two references are introduced in the revised manuscript. It is indeed very exciting to see these innovations and alternative methodologies emerging in the field.

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.

One must assume that the waveform of the air-coupled flexural wave measured at a certain position relates to the ice properties at some position or along some path. If one assumed the air-coupled flexural wave represented an average of the ice thickness along a straight line separating the source and the geophone, it could be plotted at the midpoint between the two. This is described in the manuscript, lines 347-353, but the figure is omitted for brevity. We include the figure below (Figure w). We see a clear trend that the real variation in ice thickness, as represented by borehole measurements, is better represented by assuming that the air-coupled flexural wave estimate represents the ice thickness at the geophone location, rather than the midpoint between source and receiver. A straightforward physical explanation of this is that the air-wave is travelling over the ice surface, exerting downwards pressure that causes it to flex. The wavelength of flexure depends primarily on the ice properties in the vicinity of the load, therefore the geophone recording of the air-coupled flexural wave reflects the ice properties in the vicinity of the geophone (spatially averaged over some footprint relating to the length scale of the flexure).

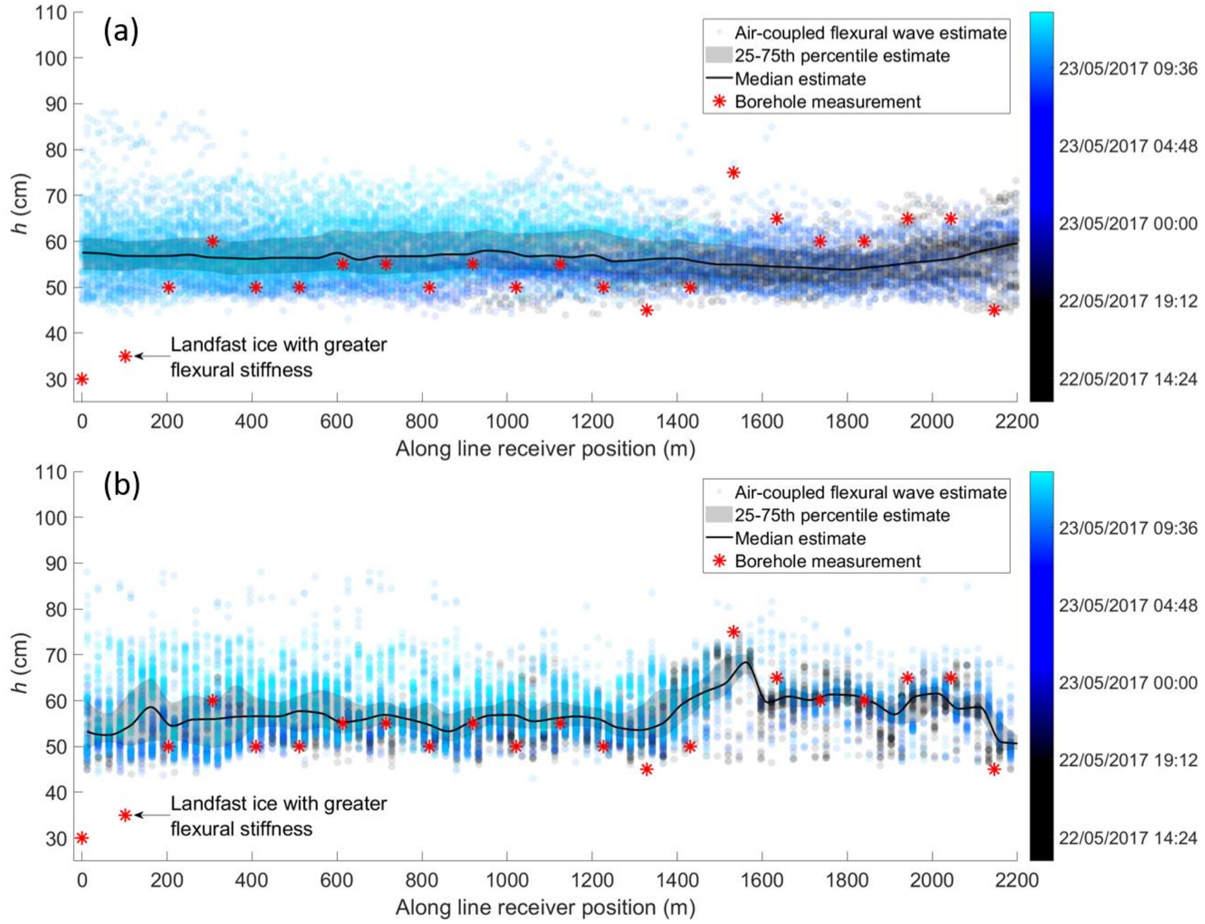

Figure w – 2017 field campaign air-coupled flexural wave estimates placed at (a) the midpoint between the source and the recording geophone and (b) at the position of the geophone.

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.

Here we include the analysis as requested, though for clarity we suggest it is unnecessary to include in the revised manuscript (we could add it as an appendix if requested to do so by the review team and the associate editor). Figure z illustrates the FK analysis for an explosive, in-line point charge from the 2013 field campaign. It was necessary to unwrap the wavenumber axis in order to interpret FK pairs corresponding to the dispersive ice-flexural wave above a frequency of 7 Hz. At higher frequencies we also observe splitting in FK space, which is likely due to the spatial variation of ice thickness. Running the extracted FK pairs through Eq. (15) gives ice thicknesses in the range of 73-79 cm, using the same elastic parameters as given in the manuscript. This is consistent with the air-coupled flexural wave estimates that lie in the range 71-79 cm for the same geophone records and the 74 cm and 79 cm thicknesses measured in boreholes at each end of the geophone array. As already discussed, the spatial aliasing issues and smearing/splitting of the dispersive ice-flexural wave in FK space, which we attribute to spatial variations in ice thickness, lead us to prefer our proposed method of analysis of the air-coupled flexural wave.

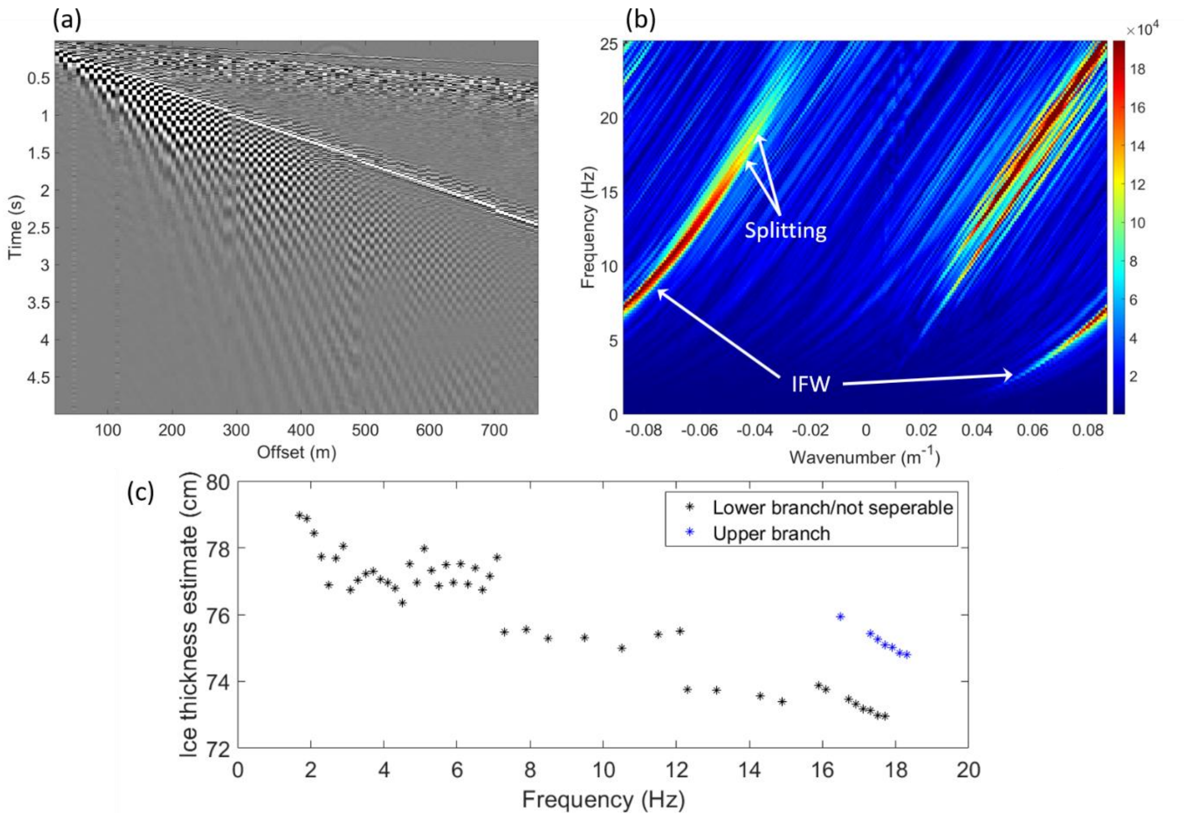

Figure z – (a) geophone records for an inline point charge during 2013 field campaign. (b) frequency-wavenumber spectrum highlighting the dispersive ice-flexural wave (IFW) which splits into two branches for the higher frequencies (c) ice thickness estimates calculated with Eq. (15) using frequency wavenumber pairs of the IFW extracted from (b).

Comparison with air-coupled transducers in NDT

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.

This is the statement in question (line 69-70 in manuscript): *"Air coupled waves are also utilised in non-contact applications of non-destructive testing of engineered structures, like concrete slabs, allowing improved testing efficiency compared to applications using sensors bonded to the surface (e.g. Zhu, 2008)."*

The relevance of the comparison is the concept of non-contact, non-destructive measurement, based on acoustic excitation/emission of elastic waves in a plate. The use and optimization of air-coupled transducers in NDT is a detail of an implementation of a non-contact acquisition scheme that is of secondary importance to the analogy. The relevance is that the concept of non-contact, non-destructive guided wave measurement of plate properties is directly analogous to the possible recording of air-coupled flexural waves with microphones in air for the air-ice-water system, or the excitation of flexural waves in a floating ice sheet by a propagating pressure wave in air. We have paraphrased "*allowing improved testing efficiency compared to applications using sensors bonded to the surface*" from statements in the cited Zhu (2008) article like:

> *"Most NDT techniques require good contact between the sensor and tested concrete surface to obtain reliable data. But the surface preparation is often very time- and labor consuming due to the rough surface or limited access of concrete structures. One approach to speed up the data collection process is to eliminate the need for physical contact between the sensor and tested structure.... Therefore, it is necessary to search for advanced NDT techniques that will provide efficient, low-cost and reliable condition assessment to the existing concrete infrastructure. Air-coupled sensing has undergone rapid development in recent decades, especially in guided wave detection for layered structures [71]. With the advantages of being noncontact and having elastic wave based characteristics, air-coupled sensing has the potential to be an excellent candidate for NDT application in concrete structures."*
>
> Zhu (2008)

and *"Air-coupled sensors will greatly improve testing efficiency by eliminating the need of surface coupling."* Zhu (2008).

But perhaps to be on the safe side we could simply state: *"Coupling between air and guided waves in engineered structures, like concrete slabs, is also relevant to non-contact applications of non-destructive testing (e.g. Zhu, 2008, Harb & Yuan, 2018)."* While it is common to highlight analogies between related fields in the introduction to an article, this is not essential to convey the fundamental points of the manuscript, so the reference to non-contact NDT can be removed entirely if desired by the review team and the associate editor.

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

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 use of drones and DAS are exciting alternatives, indeed. It is a great thing that new technology continues to open up new possibilities. We agree that these technologies have great potential, but they do not invalidate other alternatives. The concept of non-contact flexural wave acquisition, i.e. microphone recording of air-coupled flexural waves remains attractive since it involves very simple, inexpensive equipment that can be mounted on shore where there is no risk of loss to e.g. open cracks that may form, ice detaching and floating away from land or becoming inaccessible due to open cracks parallel to the shoreline.

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

Since the air-coupled flexural wave occurs at a constant frequency-thickness product, one may expect the percentage error to remain constant but the range of to be proportional to the thickness. Sea ice with thickness of 10 cm would have an air-coupled flexural frequency ~480 Hz, which is near the Nyquist limit of the geophone data presented in the manuscript (500 Hz), but well within the sensitivity range of geophones/microphones. Sea ice with thickness of 5 cm or 15 cm would have air-coupled flexural frequencies around 960 Hz and 320 Hz, respectively, which we would expect is easily separable from a frequency of ~480 Hz. The lake ice in the ice-skating example is ~4.5 cm thick with air-coupled flexural frequency of ~724 Hz, which is very straightforward to separate from frequencies of 330 Hz (corresponding to 9.5 cm thick ice) or 6200 Hz (corresponding to 0.5 cm thick ice).

Variability of the estimations in the manuscript includes true spatial and temporal thickness variation that cannot be adequately "ground truthed". In addition, our field experiments consist of different source types (line and point charges of different sizes), orientations (in-line and oblique to the linear receiver array) and different source distances (from 25-1500 m from array center for the 2013 survey). This is a key reason that while we show all results, we place most weight on the median estimates that compare very well with borehole constrained thicknesses for all four field seasons.

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

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.

The audio track records a complex sound field containing multiple signal components, but our signal analysis clearly shows the presence of a strong monochromatic component that corresponds to the air-coupled flexural wave. The concept of a pure tone being associated with a certain ice thickness is well known within the ice-skating community (where unfortunately a lot of knowledge exists in a colloquial form). This is a key reason we think it is useful to draw the comparison we do, since this manuscript highlights the physical basis of these tones. We can add an additional reference to Lundmark (2001) to further reinforce the point that a specific tone (the monochromatic air-coupled flexural wave component) is associated with a specific ice thickness. This point is central to the arguments presented throughout the manuscript and is important to retain.

> Lundmark, G., 2001. Skating on thin ice-And the acoustics of infinite plates. *In INTER-NOISE and NOISE-CON Congress and Conference Proceedings* (Vol. 2001, No. 7, pp. 410-413). Institute of Noise Control Engineering, The Hague, Netherlands.

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 12 in the manuscript shows the segment 0:16 to 0:33 seconds as indicated by the x-axis labels, though perhaps including the leading zero would make this clearer. The spectrogram employs 90%

overlapping Kaiser windows with a length of 2^{12} samples (0.09 sec) and a shape factor β=10. The zero padded Fourier transform length is 4 times the window size. We can include these specifications in the revised manuscript. Time-frequency analyses involve an inherent trade-off between temporal and spectral resolution. We have prioritized spectral resolution to accurately localize the frequency of the air-coupled flexural wave. The spectrogram shown by the reviewer highlights temporal structure at the expense of spectral resolution, appearing to employ a window length of ~2^7 samples (0.003 sec). The time-frequency structures highlighted by the reviewer are interesting. It is possible they might represent flexural wave leakage into the air at different radiation angles as we discussed in the previous author comment (with reference to Kiefer et al., 2019). This interpretation is consistent with the spectral magnitudes in the reviewer's analysis and the fact that they asymptotically approach the air-coupled flexural frequency. One must also remember that the ice skates striking the ice and the cracking of the ice produce broadband impulses that appear as vertical lines in the spectrogram. It remains clear, also in this spectrogram, that there is a strong component with a constant frequency somewhere below 2 kHz, but its frequency is difficult to assess from the figure due to the short window length.

We did not anticipate that interpretation of this strong monochromatic component would be controversial, but we recognize there is a need to improve Figure 12 so that it is clearer to the reader. The inclusion of a panel showing a zoomed in portion of the waveform in the revised figure, below, highlights that the dominant signal component has a constant frequency.

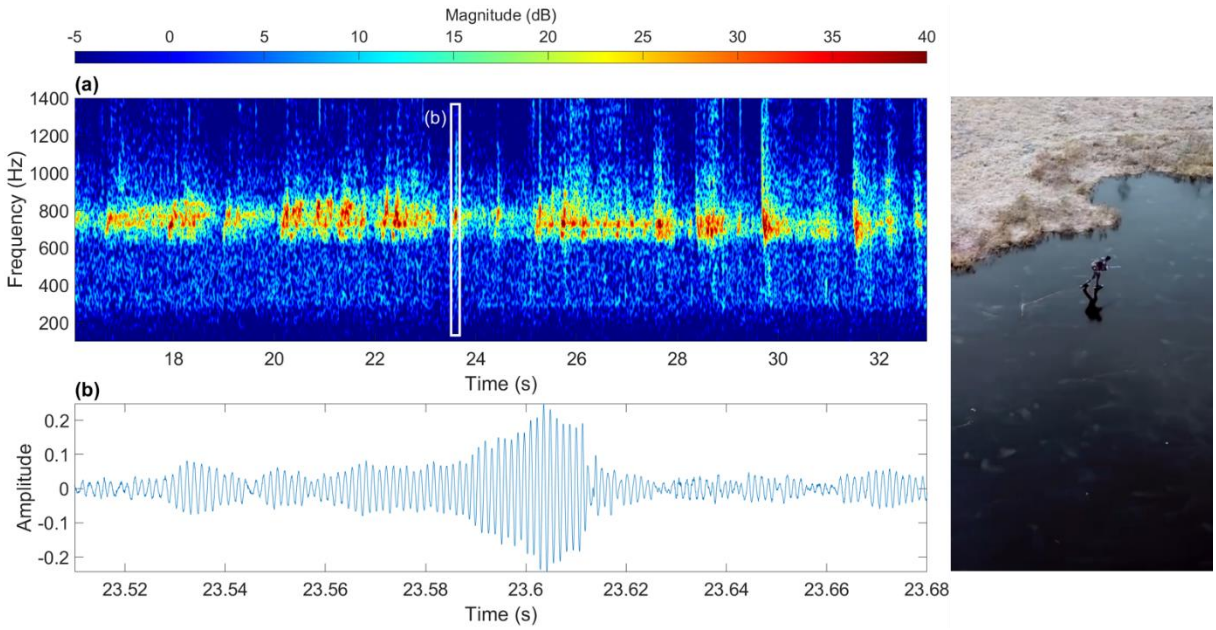

Figure 12 (revised) – (a) Spectrogram of audio track from 0:16 to 0:33 of the National Geographic short film "How Skating on Thin Ice Creates Laser-Like Sounds" (Rankin, 2018) (b) example of waveform dominated by the monochromatic air-coupled flexural wave and broadband impulse from skate blade cracking ice at 23.61 seconds. Spectrogram is composed of 2^{12} sample (~0.09 s), 90% overlapping Kaiser windows with shape parameter β=10.

Further examples of air-coupled flexural waves excited by ice skates can easily be provided, though it may be superfluous to introduce such examples in the revised manuscript (they could be provided in an appendix if desired by the review team and the associate editor). Below is an example recorded 9-Jan-2021 on Glimvannet, Ringvassøya, Norway, with an off-the-shelf Olympus LS-P4 handheld sound recorder and ice that was consistently measured as ~20 cm thick in boreholes drilled by a group of ice-fishermen. The air-coupled flexural frequency was 175 Hz to 169 Hz when the ice was tapped along a circle of ~25 m radius around the sound recorder (see Figure xx). These frequencies give thickness estimates of 19 cm to 20 cm respectively (assuming the same physical properties for lake ice as in the manuscript), in excellent agreement with the drilled thickness. There was a ~2 cm layer of snow on top of consistent black ice with re-frozen cracks at an average spacing of ~100-200 m across the lake.

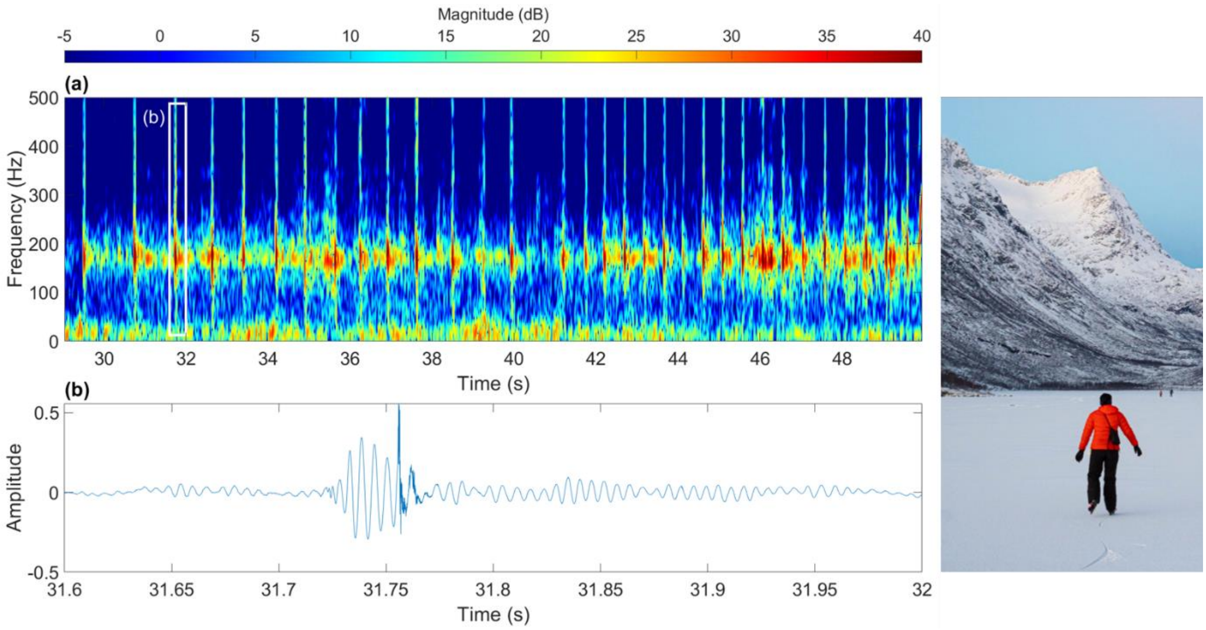

Figure xx - (a) Spectrogram of repeatedly tapping (vertical lines in spectrogram) an ice skate on ~20 cm thick lake ice at Glimvannet, Ringvassøya (northern Norway) on 9/01/2021, recorded by Olympus LS-P4 handheld sound recorder at ~1.2 m height above the ice. Spectrogram is composed of 2^{12} sample (~0.09 s), 90% overlapping Kaiser windows with shape parameter β=10. (b) The constant frequency component arriving in advance of the broadband impulse arrival at 31.76 s is the air-coupled flexural wave.

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.

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.

Thanks for taking the time to make this investigation on your data, it is very relevant and valuable experience. However, the fact that air coupled flexural waves are not observed may simply be because the geophones were located in holes drilled in the ice and covered over with snow. From Moreau et al. (2020a):

> *"To maximize the coupling, a milling tool was specifically designed to drill the ice at the diameter of the nodes. They were installed in the holes at about half their height and covered back with snow to insulate them for preserving their battery life."*

It is, respectfully, not possible to conclude that air-coupled waves could not have been recorded by an acquisition system more tailored to this purpose, e.g., microphone in air. Thick snow cover could be an issue, but thin snow cover does not prevent the recording of air-coupled flexural waves by gimballed z-component geophones resting directly on the snow/ice surface in our active source field data spanning four different seasons of first year sea ice. The influence of snow covers on flexural waves in ice is, in general, an underexplored topic with scope for considerable improvement and deserving of attention in future research. For example, a relevant point is that snow insulates the ice thermally, decreasing the chance of ice-quakes due to thermal expansion and contraction and this could limit the realistic acquisition season for passively excited air-coupled flexural waves in some settings (notably, tidally forced cracks are likely unaffected by thermal insulation of a snow layer).

Given that what is heard from ice-skating cracks is not an air-coupled flexural wave, but simply a flexural 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.

> Statement from manuscript: "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."

The first point is not correct, the association between a specific monochromatic tonal frequency and a specific ice thickness is well established in this manuscript and within the ice-skating community, that is why we draw this parallel. We have given further literature and data examples to illustrate this fact. The revised version of Figure 12 may also make clearer that our interpretation of a monochromatic air-coupled flexural wave is valid. The absence of air-coupled flexural waves in the Moreau et al. (2020a) dataset is a useful observation and we appreciate the time taken by the reviewer to make this investigation. However, it in no way invalidates the statement above. The observation rather supports the conclusion that buried geophones are ill-suited to passively recording air-coupled flexural waves, which is useful to keep in mind but hardly an invalidation of the concept in general.

If the review team and the associate editor deems it necessary, an example of air-coupled flexural waves generated by natural ice quakes could be added as an appendix to the manuscript. Figure yy shows a series of icequakes recorded around midday, Jan-03 2021 at Storvatnet, Kvaløya (Norway) using the built-in mic of a Sony a6500 digital camera. This is a relatively low-quality microphone with no shielding from wind noise, but the monochromatic air-coupled flexural waves can still be clearly discerned (in addition to the broadband impulses of the ice crack ruptures). The ice was not drilled here, but its thickness was estimated to be 10-20 cm by visual assessment of vertical cracks running through the clear, black ice. The air-coupled flexural frequency of ~195 Hz indicates the thickness was around 16 cm, using the same physical properties for lake ice as used in the manuscript. The occurrence of these ice-quakes during the middle of the day, when the air temperature was relatively high, indicates they were probably caused by thermal expansion stresses (e.g. Ruzhich et. al., 2009). The statement that crack formation in ice is a possible alternative source capable of exciting air-coupled flexural waves therefore seems well justified.

Ruzhich, V. V., Psakhie, S. G., Chernykh, E. N., Bornyakov, S. A., & Granin, N. G. (2009). Deformation and seismic effects in the ice cover of Lake Baikal. *Russian Geology and Geophysics,* 50(3), 214-221.

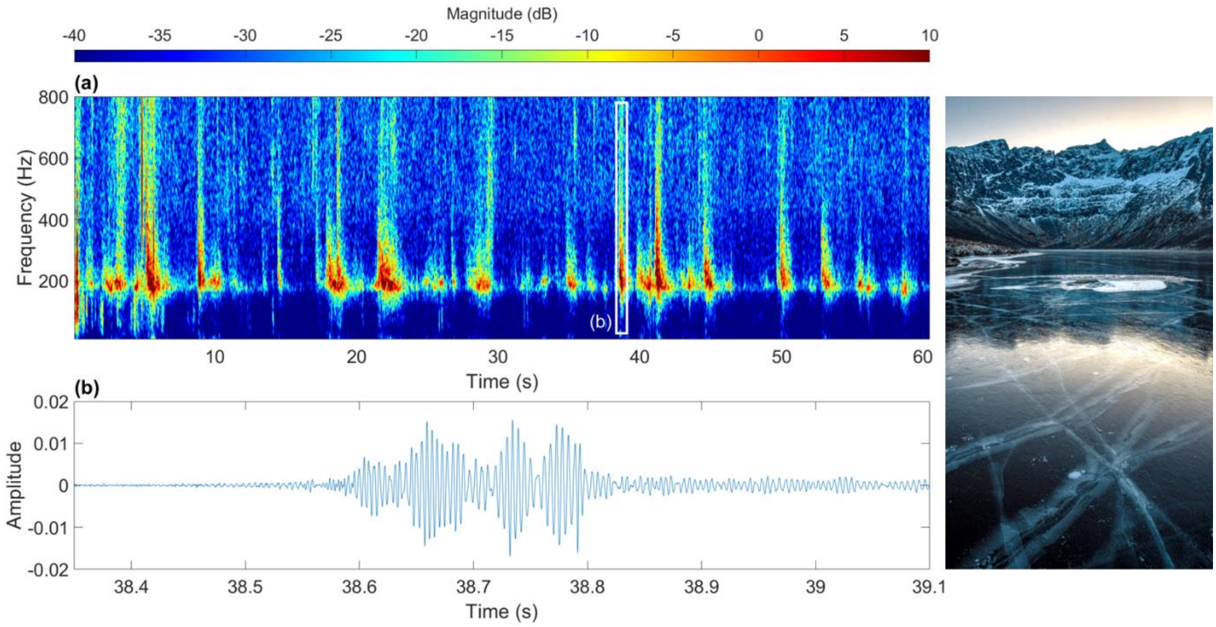

Figure yy - (a) Spectrogram of natural ice quakes at Storvatnet, Kvaløya (northern Norway) recorded at midday 3/01/2021 with the built-in microphone of a Sony α6500 camera at a height of ~1.2 m above the ice. Spectrogram is composed of 2^{13} sample (~0.17 s), 90% overlapping Kaiser windows with shape parameter β=10. The waveform of the strong ~190 Hz monochromatic component corresponding to the air-coupled flexural wave is shown in detail in (b).

We see that an important contribution of this manuscript is to highlight the physical equivalence of observations made on sea ice, to a phenomenon that is colloquially well-known from human experience on and around relatively accessible lake ice. We present the simple physical dynamics that govern air-coupled flexural waves to highlight that they are indeed real and measurable waveforms being an inherent part of the total wavefield, as predicted by Press et al. in the 1950's. While the active source experiments we present are clearly valuable, we see that there is a bright future for passive methods across the whole breadth of seismology, as motivated by the emerging field of environmental seismology (e.g., Larose, et al., 2015). We hope that highlighting that it may be possible to record air-coupled flexural waves passively, also for sea ice, might motivate other researchers to consider collecting some simple microphone recordings during future field campaigns. Looking towards the future, we would advocate that it may be more constructive to carry microphones and patience into the field, rather than detonators and explosives.

Larose, E., Carrière, S., Voisin, C., Bottelin, P., Baillet, L., Guéguen, P., Walter, F., Jongmans, D., Guillier, B., Garambois, S., Gimbert, F. & Massey, C. (2015). Environmental seismology: What can we learn on earth surface processes with ambient noise?. *Journal of Applied Geophysics*, 116, 62-74. http://dx.doi.org/10.1016/j.jappgeo.2015.02.001