Author response to RC1

We thank reviewer #1 for his comments and detailed review of our manuscript, and we have tried our best to address and accommodate his suggestions. All in all, we hope this has improved the manuscript sufficiently to ensure that it can be published in The Cryosphere.

In the following we give a point by point response, highlighting the reviewer's comments in blue and giving our responses in black.

"The paper introduces a method to infer sea ice thickness from the measurement of the acoustic wave that results from energy leakage at the ice-air interface when a flexural wave is propagating."

There are two sides to the same coin here, but we would emphasize that we specifically model the dynamic surface displacement of an ice sheet excited by a pressure pulse travelling across its surface at the speed of sound in air. We then give particular focus to the flexural wave-train of constant frequency that precedes the arrival of the pressure pulse. Since we refer to geophone experimental data that record ice surface velocity, it is relevant to focus on this aspect of what is, in reality, a bi-directional coupling between ice and air.

"**General comments**

The method is elegant. In particular, I like that the authors have come up with a closed-form solution for the ice thickness from the velocity of the flexural wave. The theoretical part is well-written, and the examples (both experimental and numerical) are convincing. Overall, I think this is a good paper about an interesting topic that fits well in The Cryosphere. I have, however, some important issues that I would like the authors to consider before the paper can be published. In particular, I think the concept of "air-coupled flexural wave" should be reviewed.

It was some 70 years ago when Press et al. introduced this terminology, and although it is intuitive for large audiences, it is also outdated and misleading, because "air-coupled flexural wave" is not a type of wave on its own, despite the fact that one may hear the sound of the flexural wave leaking in the air. From a theoretical point of view, it is inaccurate. In my opinion, the terminology needs to be reviewed to be made more rigorous and consistent with an up-to-date bibliography."

We thank the reviewer for the positive remarks to our submitted manuscript. We will present some additional arguments that the "air-coupled flexural wave" terminology is useful in response to the specific comments of the reviewer and in line with the requested complete review of wave terminology.

"The so-called "air-coupled flexural wave" is the manifestation of a flexural wave propagating in the ice and leaking energy in the air, and vice-versa. How much energy leaks from the ice to the air and from the air to the ice is a function of the product between the wavefield frequency and the thickness of the ice sheet (given some elastic/acoustic properties). For two sets of frequencies [f1,f2] and thicknesses [h1,h2], if f1h1 = f2h2, then energy leakage is the same and is proportional to the normal displacement produced by the wave at the ice-air interface. The same occurs in water. But energy transfer at the ice-air interface (as well as that at the ice-water interface), occurs at all frequencies, only the amount of energy changes."

It would be more accurate to say that the efficiency of coupling between the air and ice is regulated by the phase matching condition,

$$\cos\theta = c_{air}/c_{ice}, \qquad \text{(I)}$$

where c_{air} is the phase velocity of inhomogeneous bulk waves radiated into the air, c_{ice} is the phase velocity of flexural waves in the ice and θ is the angle between the horizontally propagating flexural waves and the bulk waves radiated into the liquid (Mozhaev & Weihnacht, 2002). That energy leaks at all frequencies seems inconsistent with guided wave theory. A first order approximation is that leaky waves exist only in the supersonic (with respect to the bulk fluid velocity) range of phase velocities (Brower et al. 1979). Subsonic flexural waves are fully trapped in the waveguide and can only excite evanescent waves in the fluid. Later studies have shown that leakage at phase velocities as low as ~98.5% of the bulk fluid velocity is possible due to the presence of a non-zero imaginary part of the wavenumber of leaky waves, alternatively explained as inhomogeneous waves in the fluid that are slower than homogeneous waves (Mozhaev & Weihnacht, 2002, Kiefer et al. 2019). These studies and the wider literature point to anomalously high energy leakage around the coincidence frequency, despite the fact that energy radiating at oblique angles leads to leakage also above the coincidence frequency (e.g. Kiefer et. al. 2019).

Brower, N. G., Himberger, D. E., & Mayer, W. G. (1979). Restrictions on the existence of leaky Rayleigh waves. IEEE transactions on sonics and ultrasonics, 26(4), 306-307.

Mozhaev, V. G., & Weihnacht, M. (2002). Subsonic leaky Rayleigh waves at liquid–solid interfaces. Ultrasonics, 40(1-8), 927-933.

Kiefer, D. A., Ponschab, M., Rupitsch, S. J., & Mayle, M. (2019). Calculating the full leaky Lamb wave spectrum with exact fluid interaction. The Journal of the Acoustical Society of America, 145(6), 3341-3350.

"Moreover, the analytical developments in sections 3.1 and 4.3 do not account for the coupling with the air. The thickness estimator Eq. (15) accounts only for the coupling between the water and the ice sheet, hence indicating that only the flexural wave is used here, not an "air-coupled flexural wave". Therefore, I think a more consistent terminology would consist in simply replacing "air-coupled flexural wave" with flexural wave. This is even more relevant because the flexural wave is measured with geophones that are directly in contact with the ice, and not with a microphone."

The compressional air-wave is considered as an external force pushing down on the ice surface, expanding radially from the origin at the speed of sound in air. The waves excited in the ice are therefore coupled to the air-wave. Since the air would not significantly alter the dispersion relation, it is omitted for the sake of simplicity from Eq. (1). We consider our model the simplest way to represent the ice response to a propagating air wave. This model is also directly relevant for comparison with our geophone experimental data that reflect ice-surface velocity. Given that we specifically model and extract from experimental data a specific subset of flexural waves, which are uncharacteristically non-dispersive and are excited by the pressure pulse from a propagating air wave, we find it necessary to apply the categorization "air-coupled flexural waves" in order to distinguish them from the dispersive ice flexural waves.

I have also noticed shortcomings in the bibliography. Regarding the monitoring of sea ice thickness (and/or elastic properties) with the flexural wave, there have been more recent studies than those from Yang (1995) and earlier in the 1950s. Such references (see below) should be cited for an up-to-date bibliography.

We thank the reviewer for these suggestions, the Moreau 2020a/2020b references in particular are very interesting and highly relevant. We were surprised to find that we hadn't caught these already and they should certainly be referred to in the revised manuscript.

"Detailed comments

Line 19 (Abstract). Authors indicate that "the air-coupled flexural frequencies for sea-ice in this thickness range are ~60-240 Hz". I think this statement is misleading because it seems to indicate that the flexural wave leaks energy in the air only at some frequencies."

Thanks for this remark, it is a good point that this comment can easily be misconstrued. What we meant was the 60 Hz frequency corresponds to 80 cm thick ice, while the 240 Hz frequency corresponds to 20 cm thick ice. As noted earlier by the reviewer, it is highly relevant that, for a given set of system parameters (including elastic parameters, air wave velocity and water >2 m depth), the air-coupled flexural wave occurs at a constant frequency-thickness product. We therefore suggest re-phrasing to:

"The air-coupled flexural frequency for the sea-ice system considered in this study occurs at a constant frequency thickness product of ~48 Hz.m. Our field data includes ice ranging from ~20-80 cm thickness with corresponding air-coupled flexural frequencies from 240 Hz for the thinnest ice to 60 Hz for the thickest ice."

It is however our intention to indicate that the air-coupled flexural wave is monochromatic, because this is one of its key signal features.

“Maybe the authors want to say that their frequencies of interest remain in this range. However, it is wrong to say that air-coupling occurs only in a given frequency range, because the flexural wave propagates at all frequencies. What changes with frequency and thickness is how much energy leaks between ice and air, which influences signal-to-noise in the recordings. Maybe this sentence could be replaced by something along the lines of: Part of the energy of the flexural wave is converted into an acoustic wave transmitted in air and water. How much energy leaks in air and water at the ice-fluid interfaces depends on the product between the frequency and the thickness of the ice sheet. For our thicknesses, energy leaking in the air is strongest in the [60-240] Hz frequency band.”

Our dynamical model shows that one can start with a pressure wave in the air that exerts an external force on the ice and excites flexural waves in the ice of quite particular frequencies (in excellent agreement with experimental data). In the final section of this author comment, we also show that an equivalent description can be given in terms of the alternative theoretical interpretation of the problem from the perspective of leaky Lamb waves (which is noted but not elaborated on in the manuscript, line 414).

“Line 30. "The coupling in the case of “air-coupled flexural waves” is set up between pressure waves in air and flexural waves in the solid that have phase velocity equal to the speed of sound in air."

This contradicts what is said in the abstract and elsewhere in the manuscript when authors mention that the "air-coupled flexural wave" occurs at frequencies in the range of ~60-240 Hz. In this frequency range, the flexural wave velocity varies from 330 m/s to about 500 m/s, while the speed of sound in the air is about 320 m/s. Actually, I think it would be useful to add a figure that shows the theoretical phase and group velocities of the flexural wave between 0 and 240 Hz. Authors could use representative values for the properties of the ice and water, and set the x axis to be the product between the frequency and the thickness of the ice, since the dispersion curves are invariant for same frequency-thickness values.”

We observe that the air-coupled flexural wave has a constant frequency for a given set of elastic and system parameters. As mentioned previously, we will revise the wording to avoid giving a range of frequencies that can easily be misinterpreted. The phase and group velocities corresponding to our thin plate approximation of the system are shown in Figure 3 in the manuscript. Setting the x-axis of Figure 3 to frequency*thickness is of course an option. However, while the flexural wave branches are invariant for the same frequency*thickness product, the gravity wave branches are not because the acceleration due to gravity is constant. The finite water depth also has an effect that may be relevant depending on the frequency range and water depths under investigation. We would therefore prefer not to use frequency*thickness as the x-axis of Figure 3 as this could lead to a false impression that the system behavior as a whole is invariant. It was also interesting that the suggested additional reference Sutherland & Rabault (2016) highlights that gravity wave behavior can be relevant at low frequencies.

Line 52 and in the rest of the manuscript

"Surface waves" do not seem like the most relevant comparison, because Rayleigh waves are not dispersive in a homogeneous medium. The dispersion of surface waves observed in seismology is only due to a gradient of velocity with depth. A more relevant comparison is that of Lamb waves, which are naturally dispersive even in a homogeneous material.

Thanks, this is a good point and we see that it is very relevant to line 32 of the manuscript and suggest changing:

“Flexural waves propagating in a floating ice sheet are a class of surface waves analogous to Rayleigh waves on the surface of elastic solids, or bending waves in rods and beams.”

To: *“Flexural waves propagating in a floating ice sheet are a class of guided elastic waves analogous to bending waves in rods or beams and the Lamb waves of a free plate.”*

It remains important to highlight that the coupling between air and surface/guided waves is a general phenomenon, as we hoped to express on line 52. We suggest the following change for clarity:

“Coupling between air and surface or guided waves is not limited to the case of a floating ice sheet and can be anticipated in all cases where the phase velocity of the medium below the air can attain the speed of sound in air”.

We further suggest adding some additional references to the group of references on line 52, in addition to the group of references on line 407 to highlight that “air-coupled Rayleigh waves” is an active and directly analogous area of research. We propose to add the following references:

- *Novoselov, A., Fuchs, F., & Bokelmann, G. (2020). Acoustic-to-seismic ground coupling: coupling efficiency and inferring near-surface properties. Geophysical Journal International, 223(1), 144-160.*
- *Butler, R., & Lomnitz, C. (2002). Coupled seismoacoustic modes on the seafloor. Geophysical Research Letters, 29(10), 57-1.*

“The flexural wave in a free plate is the low-frequency asymptotic approximation of the fundamental antisymmetric Lamb mode A0. In a fluid-loaded plate, this asymptotic approximation is changed due to the presence of coupling with water. The coupling between the water and the solid produces an interface wave, similar to the Scholte wave at the interface between a semi-infinite solid domain and a semi-infinite fluid domain. However, because the ice sheet is a bounded domain, the Scholte wave becomes a quasi-Scholte wave. The flexural wave studied in this manuscript is the asymptotic behavior of this quasi-Scholte wave. See a detailed discussion on this matter in Moreau et al. (2020a)”

In this article we have chosen to focus on the thin plate approximate theory, where the flexural waves can be interpreted as true bending waves of the plate, resulting from the bending forces described in the differential equation Eq. (1). We fully agree that the exact physical reality of the air-ice-water system is more complicated than described by the approximate theory. We give a more detailed discussion including comparison with exact theory calculations, that is complementary to the discussion by Moreau et. al. (2020a), in the final section of this author's comment.

We recognize that we can be more rigorous by changing the statement:

"In this work we consider a thin elastic plate resting on an incompressible inviscid fluid of finite depth, that corresponds to the widely used "simplest acceptable" mathematical model advocated by Squire et al. (1996)."

to: *"In this work we approximate the air-ice-water system by a thin elastic plate resting on an incompressible inviscid fluid of finite depth, that corresponds to the widely used "simplest acceptable" mathematical model advocated by Squire et al. (1996)."*

Line 70: "…allowing improved testing efficiency compared to applications using sensors bonded to the surface (e.g. Zhu, 2008)."

In the field of nondestructive testing, ultrasonic waves couple with fluid as well (gaz or liquid), but they are not called air-coupled waves. They are just wave that transmit energy in air or liquid. The air-coupled transducers, however, do exist, but they do not improve testing efficiency as suggested in the manuscript. If anything, signals measured by such transducers are weaker than transducers directly in contact with the structure. The main advantage of air-coupled transducers is that they solve problems linked to imperfect contact or impedance mismatch between the transducer and the surface of the inspected medium. Also, air-coupled transducers operate over a very large spectrum, because of their high sensitivity to the normal displacement at the air/medium interface, and also because it is possible to adapt the focusing angle via Snell's law. For an example involving guided waves, see Castaings and Cawley (1995), The generation, propagation, and detection of Lamb waves in plates using air-coupled ultrasonic transducers, in the J. Acoust. Soc. Am.

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. 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). 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.

We suggest to add the following reference to the existing Zhu (2008) reference, since it nicely breaks down the elements of the fully non-contact air-coupled system including the receiver side which is of particular relevance to the present study:

> *Harb, M. S., & Yuan, F. G. (2018). Air-coupled nondestructive evaluation dissected. Journal of nondestructive evaluation, 37(3), 1-19.*

Line 122: "However, in this study we focus on the subset of experiments employing explosive sources and vertical- component gimballed geophones deployed on top of the ice "

I do not understand why authors have put such a focus on the so-called "air-coupled flexural wave", since they end up analyzing signals recorded from geophones, which are directly in contact with the ice. This is confusing and is not consistent with what precedes. Unless I am missing the point, would it not be more convenient and efficient to infer the ice thickness from the velocity of the flexural wave at a chosen frequency, instead of focusing on the part of the wave that leaks in the air? Eq. (15) is supposed to work in both cases, right?

Our theoretical framework describes a moving pressure pulse in the air that excites flexural waves in the ice, i.e., flexural waves that are excited by the propagating air-wave. Describing these waves in the most direct way then leads us to “air-coupled flexural waves”, which is an established term in the literature, so it would be unnatural to adopt a different terminology without good reason. These waves occur as a constant frequency wave-train with high amplitude in our data so it is quite natural to focus specifically on them. We are essentially doing as the reviewer suggests, but instead of choosing the frequency and measuring the velocity, we are choosing the velocity and measuring the frequency. It is correct that Eq. (15) will work in both cases, but there is a practical benefit to estimating the frequency instead of the velocity. For a monochromatic timeseries recorded by a single sensor it is straightforward to estimate the frequency and we know the phase velocity of the air-coupled flexural wave is equal to the speed of sound in air, our model treating the air-wave as an external moving load makes this point very clear. One could use other combinations of frequency and phase velocity, as has been done by several authors from Yang & Yates (1995) to Moreau (2020a/b), but resolving the wavenumber domain involves spatial sampling challenges that are neatly avoided by focusing on the air-coupled flexural wave. The air-coupled flexural wave gives the potential for ice flexural stiffness estimation using a single sensor.

“Line 137. I have not found the fluid depth, H, introduced in the manuscript. It should be introduced here”

It was introduced on line 142, but we agree it is more natural to include it earlier i.e, *“In this work we consider a thin elastic plate resting on an incompressible inviscid fluid of finite depth, H, that corresponds to…”*

“6) Figure 5

- In the caption, please indicate that the red dashed lines in 5a and 5d correspond to the time series in 5b and 5e.

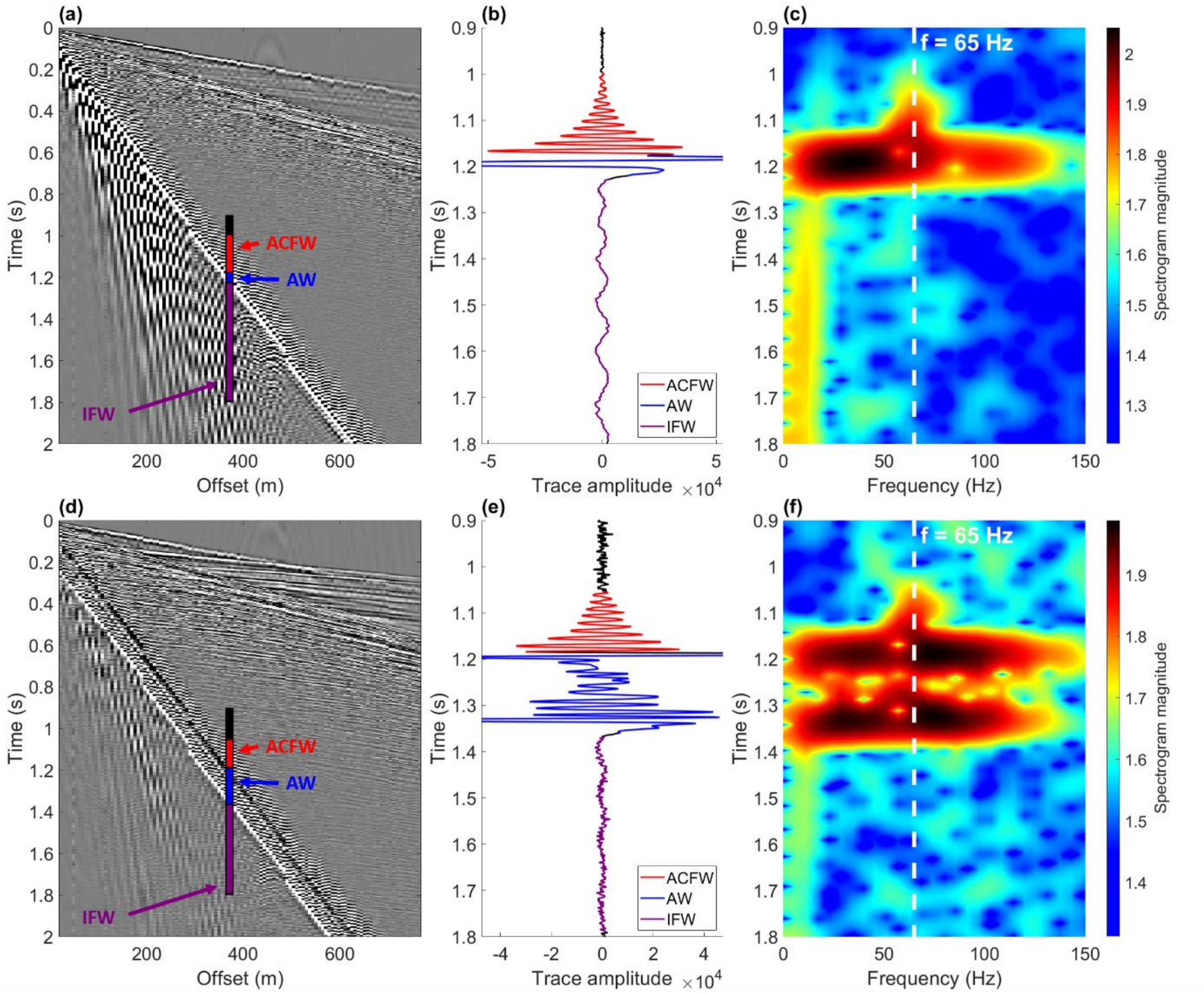
- Please highlight the compressional wave in the air in the moveouts.

- It appears very clear in figures 5a and 5d that the recording is dispersive, which does not match the definition of the "air-coupled flexural wave" (first line of the introduction), as "wave trains of constant frequency […] that arrive in advance of the pressure waves."

- This indicates that what is observed is just a flexural wave.

Thanks for these comments, we recognize the need to be much more specific in this Figure. We have prepared a new version that highlights (we hope) that the air-coupled flexural wave is quite distinct from the rest of the flexural wave-train for these data. It should now be clear exactly what part of the timeseries we interpret as the air-coupled flexural wave.

Our model does not produce broadband flexural wave excitation and agrees very well with the experimental data (ref. Figures 10 & 11 in the manuscript). Figure 4c in the manuscript further highlights that the part of the wavefield we isolate has a constant frequency at a given receiver, in line with the description of air-coupled flexural waves in the introduction. Though the frequency is constant at a given receiver, it varies between receivers (as shown in Figure 4d) due primarily to spatial variations in ice thickness.



Revised *Figure 5 – Example shot gathers for (a) a point charge and (d) a 50m long, detonating cord line source, (b) and (e) show illustrative time series enlarged around the air wave (AW) arrival, (c) and (f) show spectrograms for the time series (b) and (e). The air-coupled flexural wave (ACFW) arrives in advance of the broadband air wave with constant frequency of ~65 Hz. The characteristically dispersive ice flexural waves (IFW) are significantly attenuated due to destructive interference for the line source.*

"Section 4.2. The proposed method seems to limit data extraction capacity. Given the nice shot gathers shown in the figures, I wonder why the authors do not make use of a frequency-wavenumber analysis. It would allow the frequency-dependent velocity for the flexural to be extracted, even in the near-field where unwanted interferences occur because all waves are separated in the frequency-wavenumber space. Most likely, it would also be more accurate, given the variabilities observed in figure 4d for the frequency estimations."

Interestingly enough, the development of the proposed methodology was motivated by the desire to maximize the data extraction capacity for the existing datasets that were available to us. Particularly motivated by the Yang & Yates (1995) study, frequency-wavenumber (FK) analysis was one of the first things we tried with this data. We include an example below, using the same shot gathers as Figure 5 in the manuscript. The main issue is that these data are undersampled in the wavenumber axis, leading to spatial aliasing in the frequency-wavenumber domain which complicates the analysis. Furthermore, instead of one thickness estimate per receiver, as in our method, we would only get one thickness estimate per shot with FK analysis, limiting our ability to resolve spatial variation in ice thickness. An alternative would be to apply FK analysis on sliding windows of traces, but then wavenumber resolution would suffer. Fundamentally, FK analysis requires multi-sensor data whereas our methodology can also be applied for single sensor data. This is the one of the attractive aspects of the air-coupled flexural wave, that it's constant frequency can be estimated from a single timeseries while the phase velocity is constrained by the speed of sound in air.

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 interfering components we seek to avoid typically have frequencies far outside this band. The existence of spatially variable ice-thickness is also discussed in Moreau et al. (2020a/b). 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):

- The air-coupled flexural wave method broadens the possibility to estimate spatial thickness variation and the use of single sensor records.

- The simultaneous resolution of multiple wave modes in the Moreau et al. (2020a/b) implementation/s allows the thickness and elastic parameters to be resolved simultaneously.

Given a suitable dataset it may well be advantageous to apply both methods in tandem.

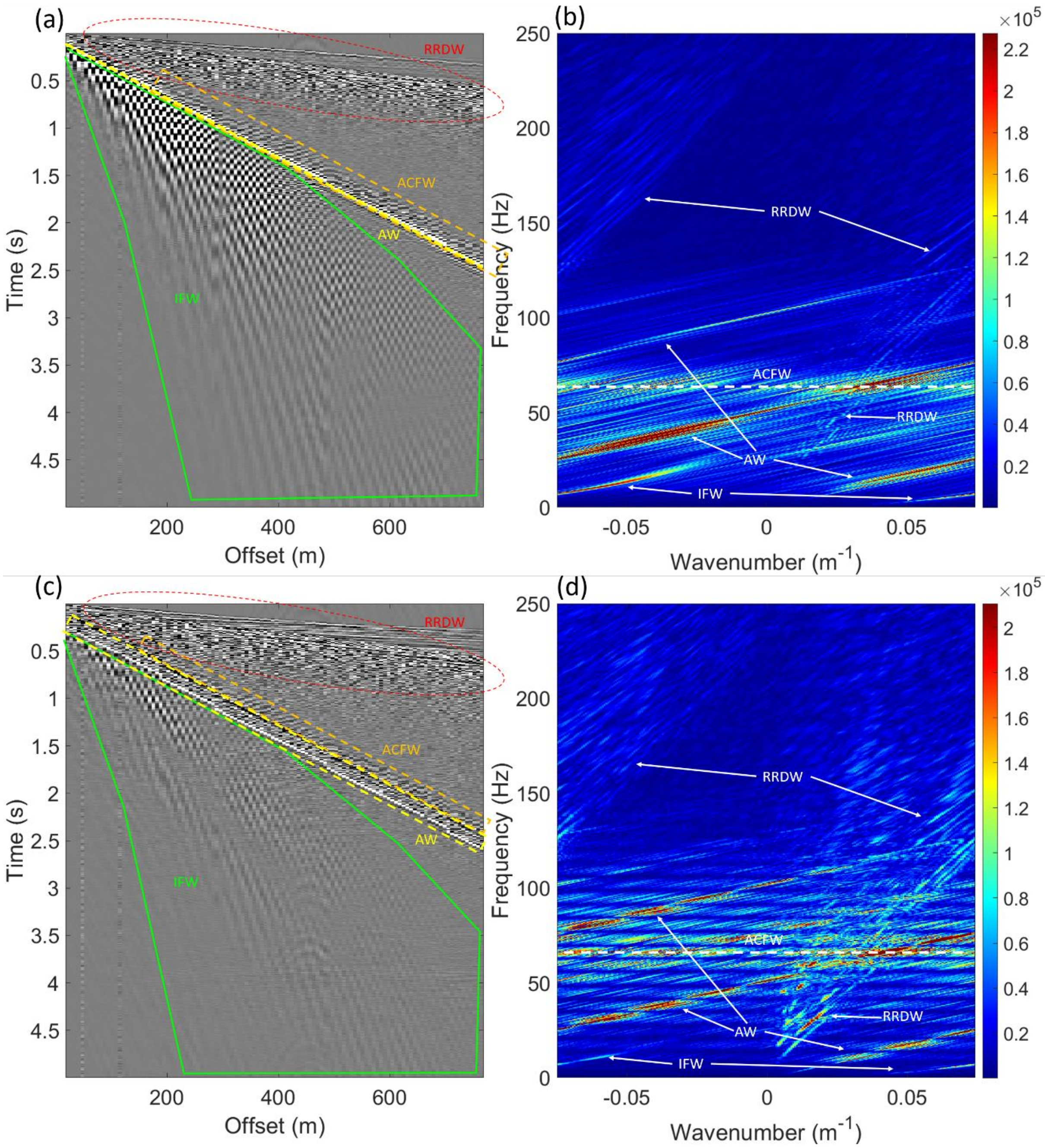


Figure x - Shot gathers with wave components labelled for (a) a point charge and (c) a 50m long, detonating cord line source and corresponding frequency-wavenumber spectra (b) and (d). RRW denotes reflected, refracted and direct waves, ACFW denotes the air-coupled flexural wave, AW denotes the compressional air-wave and IFW denotes the ice flexural waves.

"Line 280: I find the discussion regarding the choice of Young's modulus and Poisson's ratio unconvincing. First, authors report that the ice in "Van Mijenfjorden has relatively constant macro-scale elastic properties for the range of observed thicknesses from 20-80 cm."

Moreau et al (2020a) (see above) used a passive seismic method in the Van Mijen Fjord, in 2019, to simultaneously infer ice thickness, Young's modulus and Poisson's ratio. They also observed that the ice was quite porous in the first 20 cm or so, and then becomes quite dense. This translates into strong gradients of Young's modulus and density through the thickness. They report values around 4 GPa for Young's modulus and 0.32 for Poisson's ratio, with an ice thickness of about 55 cm at the beginning of March 2019."

Our point was that the using a single set of constant elastic properties we estimate thicknesses that are consistent with borehole measured thicknesses for all four field seasons. The independent thickness measurements give some constraint on the elastic properties, since both contribute to the flexural rigidity that the flexural waves are sensitive to. We therefore infer that, at a broad scale, the bulk/effective elastic properties of first year sea ice in this area are relatively consistent from year to year.

We fully agree that for clarity we should use the terminology "bulk elastic properties" or "effective elastic properties" rather than "macro-scale elastic properties", which was a bit unclear. We simply mean the vertically averaged properties that control the effective elastic behavior of the ice sheet as a whole. We fully agree that it is important to keep in mind that in reality ice is more like a sandwich composite with strength that varies significantly across a depth profile through the ice. However, the elastic constants that describe its bulk/effective strength still have relevance to, e.g., the bearing capacity of the ice. The Moreau et al. (2020a) estimates are highly relevant and will be referred to in the revised manuscript. It is worth noting that the elastic properties inferred by Moreau et al. (2020a) are bulk/effective properties averaged over the vertical ice profile, though strong gradients in elastic properties through the vertical profile of the ice sheet were observed by direct observation.

We find the elastic properties inferred by Moreau et al. are in broad scale agreement with our estimates. It is in line with intuition that the Moreau et al. (2020a/b) estimates from Vallunden Lake in Van Mijenfjorden give slightly higher Young's modulus, ~4 GPa compared with ~2.5 GPa in the present study. The more protected setting and support from the surrounding shoreline likely increase the effective Young's modulus of the ice here, similar to the 2017 data we present (Figure 8 in the manuscript) where we discuss that the flexural strength of the ice is likely enhanced close to land in Sveabukta (Vallunden Lake is directly adjacent to this area as shown in Figure 1 in the manuscript).

"Section 6. Line 460. " 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". Regarding the possibility of studying icequakes for monitoring sea ice thickness and rigidity, the authors cite a reference from the field of nondestructive testing. It would be more relevant to use a reference where this has actually been done on sea ice. In Moreau et al. (2020a) and in Moreau et al. (2020b), the authors introduce methods for passive monitoring of sea ice thickness and elastic properties, using seismic noise interferometry and icequakes:

• Moreau, P. Boué, A. Serripierri, J. Weiss, D. Hollis, I. Pondaven, B. Vial, S. Garambois, E. Larose, A. Helmstetter, L. Stehly, G. Hillers and O. Gilbert (2020a), Sea ice thickness and elastic properties from the analysis of multimodal guided wave propagation measured with a passive seismic array, J. Geophys. Res. Oceans 125(4)

- Moreau, J. Weiss and D. Marsan (2020b), Accurate estimations of sea-ice thickness and elastic properties from seismic noise recorded with a minimal number of geophones: from thin landfast ice to thick pack ice.J. Geophys. Res. Oceans 125(11)"

We fully agree that the Moreau et al. (2020a/b) studies should be cited in this section. We should put forward that what we propose is an alternative and complementary means of passive monitoring to that implemented by Moreau e. al., that departs significantly in implementation and opens up the potential for the use of single sensors on ice or microphones in air. It is wonderful to see that icequakes are such a prominent feature in the Moreau et al. studies since the temporal occurrence of icequakes exciting flexural waves and leaking acoustic energy to the air is a potential limitation of the hypothesized non-contact measurement approach we describe in the manuscript. The non-destructive testing paper of Haider and Giurgiutiu (2018) remains highly relevant since it deals with the problem of solid-air coupling by modelling acoustic emission.

In addition to introducing the Moreau et al. studies we therefore need to re-phrase statements such as:

"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"

To: "*The excitation of air-coupled flexural waves by natural crack formation/propagation in floating ice sheets raises an alternative possibility for passive monitoring of ice thickness and rigidity*"

Moreover, it seems that authors are also missing other recent developments about passive seismic monitoring of sea ice.

We thank you for the suggested references, these can be woven into and used to strengthen the revised manuscript. Sutherland & Rabault (2016) is quite interesting from the perspective that they record the propagation of gravity waves in sea ice, which are also included in our model but not evident in our experimental data.

"In summary, besides the points listed above, I believe it is very important that the authors make an effort to completely review their manuscript for more rigorous terminology of the waves involved in their calculations, and to drop the inaccurate terminology of "air-coupled flexural wave"."

We have chosen to apply the thin plate/low frequency approximation of the air-ice-water system that is used extensively throughout the literature and the terminology flexural wave is therefore justifiably applied. However, we find it unsatisfactory to solely apply the term flexural wave because we want to refer to a very specific sub-category of flexural waves having phase velocity equal to the speed of sound in air. The term "air-coupled flexural wave" has already been coined for this very purpose, it fits and describes this subclass very well, so we find it difficult to see why it should be improper to apply it here as well. The constant frequency wave-train of the air-coupled flexural waves that we isolate in our experimental data and reproduce by dynamical modelling exist in stark contrast to the typically dispersive character of “standard” flexural waves. It would therefore be potentially misleading to simply refer to these waves as flexural waves. Furthermore, we note that the equivalent terminology “air-coupled Rayleigh wave” is in active use by, e.g., Novoselov et al. (2020), indicating that air-coupled wave is a useful concept even though they are, by nature, closely related to the surface/guided waves they are coupled to.

- *Novoselov, A., Fuchs, F., & Bokelmann, G. (2020). Acoustic-to-seismic ground coupling: coupling efficiency and inferring near-surface properties. Geophysical Journal International, 223(1), 144-160.*

We appreciate that the air-ice-water system is complex and the term “flexural wave” is perhaps not completely rigorous, although it is widely used in practice. In an effort to completely review the wave behavior of the system, according to the suggestions of the reviewer, we have calculated the dispersion curves of the air-ice and water-ice system according to the exact theory of Kiefer et al. (2019). We include this additional discussion here, but suggest it not be included in the revised manuscript in the interest of brevity and because it is not central to the arguments developed in the manuscript.

Kiefer, D. A., Ponschab, M., Rupitsch, S. J., & Mayle, M. (2019). Calculating the full leaky Lamb wave spectrum with exact fluid interaction. The Journal of the Acoustical Society of America, 145(6), 3341-3350.

Lowe, M. J. (1995). Matrix techniques for modeling ultrasonic waves in multilayered media. IEEE transactions on Ultrasonics, Ferroelectrics, and Frequency Control, 42(4), 525-542.

The Kiefer et al. (2019) methodology is attractive because one can be confident that all possible wavemodes have been found at a given frequency. We will discuss the behavior of three important configurations; sea ice/air, sea ice/water and lake ice/water, using physical properties as defined in Table 1.

Table 1 – Physical properties used to calculate dispersion curves shown in Figures y and z. Note that the fluid depth only affects the thin plate model calculations, the fluid in the Kiefer et. al. (2019) model is infinite.

	Sea ice/Air system	Sea ice/water system	Lake ice/water system
Young’s Modulus (GPa)	2.5	2.5	8.5
Poisson’s ratio	0.33	0.33	0.33
Ice density ($kg.m^{-3}$)	931	931	917
Fluid density ($kg.m^{-3}$)	1.3	1027	1000

Fluid bulk velocity (m/s)	320	1500	1500
Fluid depth (m)	20	20	20

If we first consider the air-ice system as illustrated in Figure y, the dispersion curves predicted according to Kiefer et al. (2019) are essentially those of the free-plate, with the addition of an extra branch corresponding to the compressional bulk velocity in air. Where the A0 mode of the free-plate crosses the speed of sound in air, the dispersion curves split into two modes with the quasi-Scholte branch remaining trapped with a phase velocity less than the speed of sound in air and a leaky mode branch that radiates energy into the air proportional to the imaginary part of the wavenumber (Figure y(b)). We observe that energy radiation into the air is concentrated over a narrow band of frequency-thickness and the spread that is observed is due to radiation at different angles satisfying the phase matching condition (see Eq. (I)). Radiation at the grazing angle occurs where the leaky mode branch crosses the bulk compressional velocity in air.

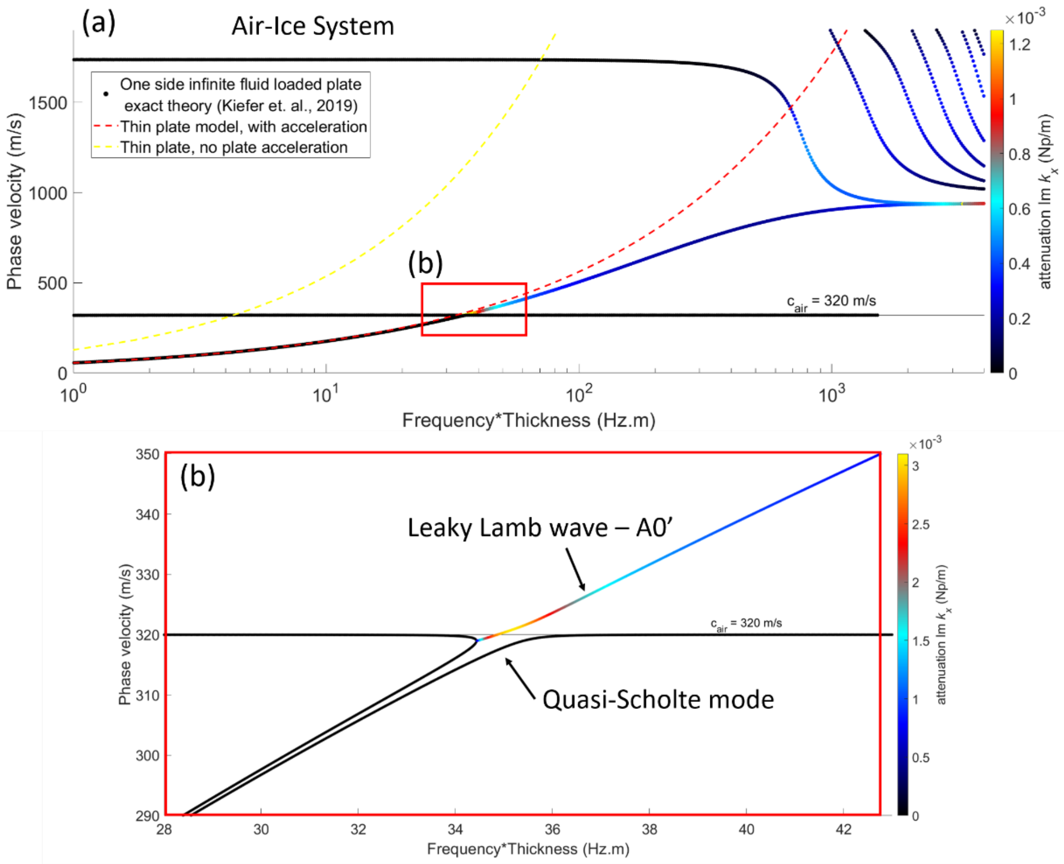


Figure y – (a) Dispersion curves for the air/sea ice system calculated by the exact spectral collocation method of Kiefer et al. (2019) compared to thin plate approximate model used in the present study with (dashed red line) and without (dashed yellow line) the inclusion of plate acceleration. (b) enlargement highlighting the dispersion curve splitting and transition to leaky behavior at the phase velocity corresponding to the bulk compressional wave velocity in air.

If we now consider the water-ice system as illustrated in Figure z, the exact dispersion curves calculated according to Kiefer et al. (2019) differ substantially from the free plate, due to the heavy fluid loading. Interestingly, for the sea ice/water system the transversal wave velocity in ice drops below the longitudinal wave velocity in water, leading to a multiplicity of trapped modes at high frequencies (Figure z(a)) compared to the lake ice/water system (Figure z(b)). This behavior is far above the frequency range of the present study but could become relevant for ultrasonic studies. As discussed by Moreau et al. (2020a), the thin plate approximation follows the quasi-Scholte mode branch of the ice-water system.

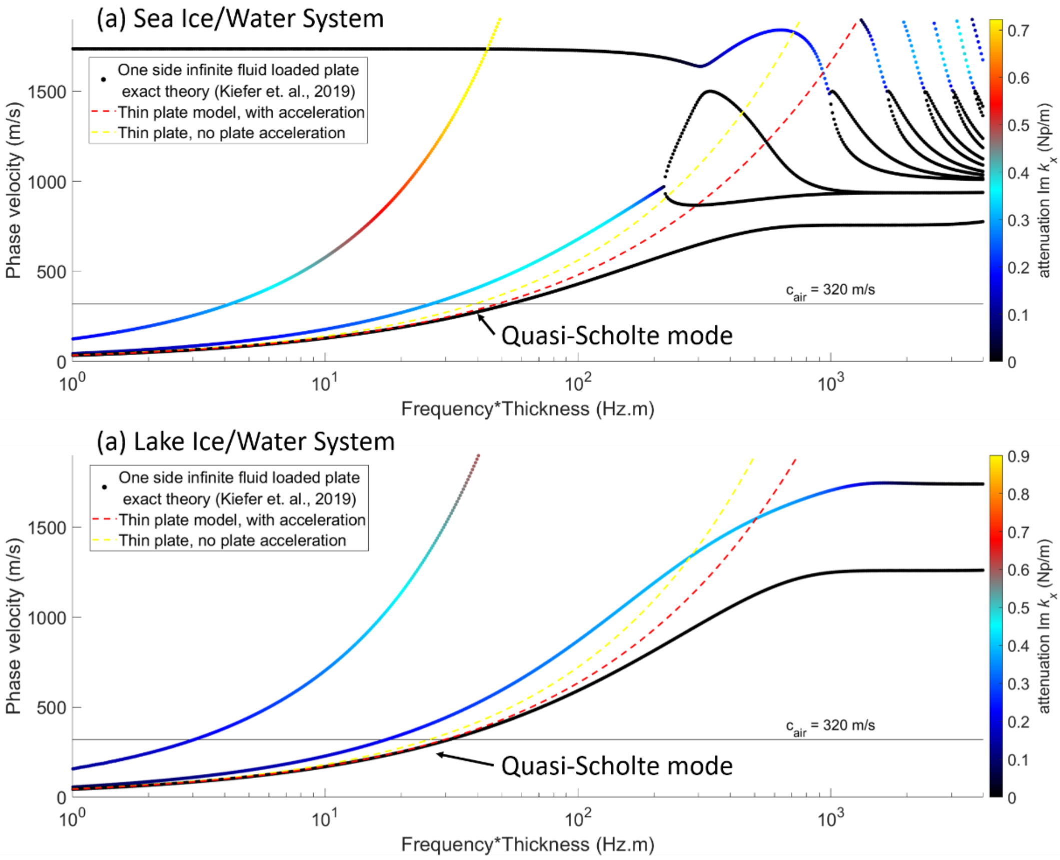


Figure z - (a) Dispersion curves for the sea ice/water system calculated by the exact spectral collocation method of Kiefer et al. (2019) compared to thin plate approximate model used in the present study with (dashed red line) and without (dashed yellow line) the inclusion of plate acceleration. (b) as in (a) but using elastic parameters corresponding to fresh water lake ice.

We expect the dispersion curves for the complete air-ice-water system to be virtually identical to the ice-water system with the addition of the mode splitting around the bulk compressional velocity in air as illustrated in Figure y(b). At this point however, it becomes difficult to apply the A0 and quasi-Scholte wave terminology because we, in fact, have two quasi-Scholte modes and two sets of leaky modes corresponding to the ice-air and ice-water interfaces. The air-coupled flexural waves we discuss in the present study may then be considered the leaky ice-air Lamb branch of the ice-water quasi-Scholte mode. We find this terminology impractical and this stems from the fact that the air-ice-water system is a rather special example of a plate that is heavily fluid loaded on one side and lightly fluid loaded on the other side. This deviates significantly from the free plate for which the true Lamb waves are defined, and the one side slightly fluid loaded plate for which the quasi-Scholte mode is best defined. As used extensively in the floating ice literature, the term flexural wave seems most appropriate for the low frequency thin-plate regime that is described in the present study (where the thin plate theory describes, physically, a pure bending mode of plate motion). The already established terminology “air-coupled flexural wave” then also seems appropriate to describe the flexural waves excited in the solid by a travelling pressure wave in air, as well as the energy radiating at the grazing angle from the plate to the air.