

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-71

Ludovic Moreau (Referee)

Referee comment on "Sea ice thickness from air-coupled flexural waves" by Rowan Romeyn et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-71-RC1>, 2021

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.

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 introduced this terminology, and although it is intuitive for large audiences, it is also outdated and misleading, because "air-coupled flexural wave" is not a type of wave on its own, despite the fact that one may hear the sound of the flexural wave leaking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.

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 $[f_1, f_2]$ and thicknesses $[h_1, h_2]$, if $f_1 h_1 = f_2 h_2$, 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. 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.

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.

Detailed comments

0) Line 19 (Abstract). Authors indicate that "the air-coupled flexural frequencies for sea-ice in this thickness range are $\sim 60\text{-}240$ 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. 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.

1) 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 $\sim 60\text{-}240$ 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.

2) 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.

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):

- Moreau, P. Boué, A. Serrapietri, 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)

3) 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 (gas 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.

4) Line 122: "However, in this study we focus on the subset of experiments employing explosive sources and vertical- component gimbaled 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?

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

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.

7) 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.

8) 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.

9) 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. Serrapietri, 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)

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

- Marsan *et al.* (2012), *Sea-ice thickness measurement based on the dispersion of ice swell*, J. Acoust. Soc. Am. 131(1).
- Sutherland, G. and J. Rabault (2016), *Observations of wave dispersion and attenuation in landfast ice*, J. Geophys. Res. Oceans 121
- Moreau, L., C. Lachaud, R. Théry, M. V. Predoi, D. Marsan, J. Weiss and M. Montagnat (2017), *Monitoring ice thickness and elastic properties from the measurement of leaky guided waves: A laboratory experiment*, Acoust. Soc. Am. 142(5)

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".

Sincerely,

Ludovic Moreau