Review of
Elastic properties of floating sea ice from air-coupled flexural waves

This paper reports a series of experiments conducted on the shore fast sea ice of Van Mijenfjorden, West Spitzbergen, using explosive seismic sources to generate waves that travelled through the air and through the ice as a train of flexural oscillations. The work was a byproduct of a project to test different acquisition designs for reflection seismic surveying of sub-seabed sediments, i.e. the primary purpose of the field experiments was exploratory geophysics, but the authors properly recognized that the data sets obtained could also potentially be used to provide valuable information about the sea ice cover. Although this is approach is commended, it does cause the paper to have a particular weakness that will be documented below.

The paper focuses primarily on air-coupled flexural waves, a term coined by seismologists such as Ewing, Crary and Press who in the early-mid 20th century worked on the miscellanea of oscillations that can be created artificially or naturally in continuous sea ice plates. In the latter part of the paper the authors do actually solve the hydroelastic equation that characterises the fluid mechanics, namely a thin elastic sheet floating on water, but the bulk of the theoretical parts of the paper relates to the well-known hydroelastic dispersion relation alone.

In general, the paper is a solid piece of work that should be published subject to revision.

<u>Specific Comments</u>

1. The paper is rather long and reads as being somewhat unfocused. In the interest of increasing the relevance of the paper, the authors are giving readers too much peripheral information that detracts from its main theme. Moreover, as a result, the paper is at times unfortunately also platitudinous.
2. Whilst section 3.1 is fine, ‘in order to solve for the spatiotemporal deflection of the ice surface’ the authors use a Fourier transform in the manner of, e.g. Schulkes and Sneyd (1988), anticipating a particular load (Eq. 5), for example. Yet, because they are only interested in the dispersion relation *G*, this mathematical machinery seems overly complicated when a simple substitution of a single Fourier component $e^{i(kx-\omega t)}$ gives G directly. Perhaps the authors could signal at this point that they intend to solve Eq. (1) later, so that the Fourier transform discussion would seem more necessary at this juncture in the paper.
3. In Eq. (1), the non-cavitation condition is applied at $z = 0$ but the ice has a draught. Shouldn’t this condition apply at $z = d$, where d is the draught.
4. Although Schulkes and Sneyd (1988), and subsequent papers by Hosking and the same authors, neglect the effect of the plate acceleration, there is a large corpus of journal papers (including at least three separate review papers) relating to ocean waves travelling beneath sea ice sheets where the full dispersion relation is used. The authors don’t seem to be aware of this, but should mention it.

5. When comparing their study with moving load work, the authors may find the following useful: Squire, V., Robinson, W., Langhorne, P. et al. Vehicles and aircraft on floating ice. Nature 333, 159–161 (1988). https://doi.org/10.1038/333159a0.
6. As foreshadowed above, the paper is written assuming that the reader is familiar with seismological terminology, which is unlikely to be the case for the typical reader of *The Cryosphere.* This has caused the authors to miss out key information, on the assumption that it is "obvious". A specific example relates to section 4.1.
    a. Many readers of *The Cryosphere* will not know what a Radon Transform is, so this needs to be explained more clearly and thoroughly—especially what its purpose is and why it is being used.
    b. What exactly is a trace? It is presumed that each trace originates from a different geophone; is that correct? It is not obvious from the text that this is so and it should be.
    c. How many geophones are there? This would help clarify 2b as it would allow Fig. 4 to be associated with that number.
    d. Can the authors please add a sentence or two about why they are using the Thomson multitaper power spectral density method. I appreciate that a citation to Thomson (1982) is provided and Wikipedia has information, but readers may wish to know why this particular method is being used in this case and what its advantages are over other methods for computing PSDs.
7. The value of the cubic equation (14) is that both the frequency ω_f and the wavenumber k_f are known, the latter because c_{air} is known. (By the way, is V_{air} in Fig. 4b the same as c_{air}?) Regardless, Eq. (14) is a standard cubic, with a well-known solution (see Wikipedia, for example) so the reference to Nickalls (1993) and two other methods of solution for "quality control" seems excessive—rather likely checking that the quadratic formula works OK but slightly more messy. Parenthetically, *The Mathematical Gazette* publishes articles about the teaching and learning of mathematics, so citing Nickalls (1993) is unusual.
8. The statement "The range of Young's modulus [1.7–5.7 GPa] reported by Timco and Weeks (2010) would produce a variation of ~60% in air-coupled flexural frequency and is the most important parameter after ice thickness" would seem to be crucially important. With a 60% variation in ω_f, what is the associated variation in k_f and what is the consequent variation in the thickness h found by Eq. (15)? The effect of a different E on h would seem to be extremely influential, yet it is not obviously stated in the paper. Furthermore, no independent measurements were made of E so statements such as the following, are unjustified: "This indicates that the first-year sea ice which forms in Van Mijenfjorden has relatively constant macro-scale elastic properties for the range of observed thicknesses from 20-80 cm, despite significant differences in surface weather and ice conditions during the experiments." The influence of the uncertainty in Young's modulus would be a suitable topic for Section 6, Discussion.
9. What is the effect of water depth on the solution to Eq. (15)? I note from 'Skarðhamar, J., & Svendsen, H. (2010). Short-term hydrographic variability in a stratified Arctic fjord. Geological Society, London, Special Publications, 344(1), 51-60', that the fjord has major sea floor gradients. How does this variability in H affect the value of h?

10. A case is being made that the air-coupled flexural waves can provide a novel effective way of measuring ice thickness. However, depending on how *h* changes with *E*, i.e. in the unlikely event that a change of *E* has a negligible effect on thickness, isn't it rather easier to measure the ice thickness by drilling through the sea ice and using a thickness tape, or even using CryoSat-2 or (better) ICESat-2 satellite data to get thickness and then use *h* to find the unknown elastic modulus *E* or the flexural rigidity itself?
11. It is suggested that section 6 is too long and parts of it are unnecessary, as it is really a kind of extra quasi-conclusions section. The authors are again attempting to keep the attention of the reader by placing their work in context but are unwittingly embellishing it beyond its significance and practicability. This is because the sea ice is assumed to be a homogeneous and isotropic, Kirchhoff-Love plate of *known* elastic modulus, Poisson's ratio and density floating with zero submergence on inviscid water of constant depth. Whilst the dispersion relation is well known, dating back to Greenhill's original work more than a century ago, and the full dynamic model shows a reasonable correspondence with data, it is a stretch for the authors to use the simplest of models and their data sets as a possible methodology for thickness monitoring. The data are robust enough to stand alone without such ornamentation.
12. With serious consideration given to the above-mentioned comments and recommendations, the paper will be a worthy addition to the sea ice literature.

Detailed comments on Figures and Tables

Table 1: It seems odd to quote values of the flexural wave "estimates" of sea ice thickness to a millimetre, especially when MAD is centimetres and drilled values had ranges of tens of cm. There are no references for the chosen values of Young's modulus, Poisson's ratio, densities, etc. used.

Fig 1: What are the colours? Are they bathymetry? Are the red lines arrays of geophones? Please state this. What are the black X and Y and L1- L3? Why do the land contours matter in this study?

Fig 2: Line 111-112 states that this figure indicates the radial propagation of a strong air-wave over the ice surface. How does this figure do that?

Fig 3: Again there are no references for *E*, *s*, ρ_w, ρ_i, etc. The black f_{flexural} font is smaller than the blue and is almost illegible.

Fig 4a: Is the air-wave arrival in red?

Fig 4b: It is difficult to read red on black. What is the grey scale that is shown? What does "amp" mean?

Fig 5: What is a "shot gather"? What does "offset (m)" mean? I assume this is the same offset as mentioned on line 223? Is it "*d*"? Are panels (b) and (e) at the times of the dotted red line?

Fig 6 – 9: It is not clear why the air temperature after the field event is relevant. Please explain.

Fig 7: The trends in ice thickness are overemphasised, especially where measurements are only over one day.

Fig 8: How is the shoreward portion of the line (discussed on Line 338) identified? The statement that "The fact that the borehole thickness measurements, that were made while deploying the equipment prior to data recording, mostly track the lower range of ice thickness estimates gives an indication that some ice growth may have occurred during the field campaign." seems unjustified, given the accuracy of the data. Furthermore, the arguments (lines 341-344) regarding the flexural thickness of the landfast sea ice are not justified. As stated, there could be a change in E or other parameter. Or do the authors believe that the landfast ice was sufficiently restrained by the coast that it was no longer freely floating?

Typographic comments

Line 187 Eq (11): I assume the numerator is $(Dk^5/\rho_w) + gk$ and NOT $Dk^5/(\rho_w + gk)$. Whilst it is correct as it stands, it would be clearer to put the bracket in to make this obvious.