Author response to RC2

We thank reviewer #2 for the comments and detailed review of our manuscript, and we have tried our best to address and accommodate the suggestions. All in all, we hope this has improved the manuscript sufficiently to ensure that it can be published in The Cryosphere.

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

"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."

Thanks for this detailed review that gives a solid basis to revise and improve the manuscript.

Specific Comments

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.

We think it is important to introduce the full dynamical problem in order to demonstrate the excitation of the specific air-coupled flexural wave train. If one were to only consider the dispersion relation, the air-coupled flexural wave and ice flexural waves are indistinguishable, but in our experimental data the high amplitude non-dispersive air-coupled flexural wave train is quite distinctive. Nonetheless, the reviewer's point is a good one and we agree that clearly stating that the dynamical model (Eq. (1)) will be explicitly solved and compared to measured geophone data later will improve the manuscript. Upon reflection, we also see that moving Figures 10 & 11 (and related discussion) to the start of the results section could be advantageous and more consistent with the ordering in the Theory and Methodology sections.

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.

Yes, we agree and can make this change in the revised manuscript.

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.

Agreed, we can be clearer here. It's certainly not our intention to claim that the inclusion of a plate acceleration term is novel, we simply want to highlight that it is important in this case (which is not immediately obvious unless one goes through the calculations). As a response to the reviewer's comment, we suggest rephrasing from line 189:

"This solution of the dispersion relation is complete in the sense that it retains all physical mechanisms included in the dynamical model Eq. (1). However, at typical vehicle speeds that have been the main focus of moving load on floating plate studies, the flexural waves produced by the moving load have wavelengths much larger than the thickness of the floating plate. It has thus been common for these studies to neglect the effect of the plate acceleration (e.g. Schulkes and Sneyd, 1988; Wang et al., 2004). Under this assumption the dispersion relation, Eq. (11), can be approximated by"

To: *"This dispersion relation is complete in the sense that it retains all physical mechanisms included in the dynamical model Eq. (1), and it is well known from previous studies (e.g. Greenhill, 1886; Squire et al., 1996). At typical vehicle speeds and wavelengths that have been the main focus of moving load on floating plate studies, the plate acceleration may be safely neglected (e.g. Schulkes and Sneyd, 1988; Wang et al., 2004) giving the approximate dispersion relation…"*

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."

Yes, this is an excellent paper (much of the same material is expanded on in the Squire et al. 1996 book that we refer to). However, the conclusion of this paper

"we note that a single strain gauge frozen into the McMurdo Sound sea ice runway, or indeed any ice road or runway, could be used to monitor routinely the effective flexural rigidity D for the sea ice via a thin plate dispersion equation."

is notably similar to our manuscript and it would be advantageous to highlight this. Thanks for this suggestion, we will include this reference in the revised manuscript.

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.

Good point, we fully agree. We suggest the following passage to convey the key points of the transform:

"Since the arrival time of the air wave increases linearly with horizontal offset between the seismic source and receiver, we can estimate its arrival time and velocity using the linear Radon transform, also referred to as the slant-stack or $\tau - p$ transform (Yilmaz, 2001). When the independent variable is source to receiver offset, energy with a constant velocity follows a linear trajectory. Under the linear Radon transform, energy corresponding to a specific velocity and origin time is stacked along this trajectory to form a single point in the transform space. For a given explosive charge (see Figure 4a), we compute the linear Radon transform for offset sorted geophone signals and estimate the velocity (c_{air}) and intercept (t_{int}) that corresponds to the air wave by picking the local maximum of the transform

magnitude (see Figure 4b). The estimated arrival time of the air wave at a given geophone, t_{air}, is then given by, $t_{air} = \frac{d}{c_{air}} + t_{int}$, where d is the source to receiver offset."

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.
   Good point, no need to include unnecessary jargon. It will be advantageous to avoid introducing the terminology "trace" at all. We can simply replace "trace" with "geophone" in the Figure 4 caption and axes labels. In section 4.2 we can replace "trace" with "timeseries".
c. How many geophones are there? This would help clarify 2b as it would allow Fig. 4 to be associated with that number.
   Different numbers of geophones are used for different field seasons. Changing the axes labels to "geophone number" should clarify Fig 4.
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.

We chose to use Thomson's multitaper power spectral estimator because it is a state-of-the-art nonparametric estimator for conventional power spectral densities. Thomson's estimator utilizes a set of orthonormal data tapers called discrete prolate spheroidal sequences. The beauty of the estimator is that through the tuning of a single frequency-bandwidth parameter, one attains control over spectral leakage, and one stabilizes the estimate through an inherent variance reduction offered by the estimator. The superiority of Thomson's multitaper technique has been demonstrated in numerous publications, and it has become a widespread estimator among practitioners with demanding data.

We suggest to add a couple of additional sentences in the revised manuscript to give grounds for our preferred choice of spectral estimator, and we may add a couple of additional references in addition to Thomson (1982), e.g., these:

Hanssen, A. (1997). Multidimensional multitaper spectral estimation. *Signal Processing*, Vol. 58, pp. 327 – 332.

Hanssen, A. (2010). Wavefields, Ch. 1 in *Handbook on Array Processing and Sensor Networks* (eds. S. Haykin and K.J.R. Liu), Wiley-IEEE Press, ISBN: 978-0-470-37176-3

7. The value of the cubic equation (14) is that both the frequency ωf and the wavenumber kf 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.

Thanks for the catch, V_{air} in Fig 4b should indeed be C_{air} for consistency.

The Wikipedia page highlights that the cubic has a long history and there are a number of different solution methods. The highly cited Nickalls (1993) paper is pedagogically very clearly presented and accessible for non-mathematicians and was directly referred to in our derivation so it would be academically dishonest not to cite it. Perhaps a good solution would be to change:

"It is possible to derive a compact closed-form analytical solution for the cubic polynomial, following the approach of Nickalls (1993), leading to the following estimator"

To: *"Since Eq. (14) has a standard cubic form, a closed-form analytical solution can be derived. We followed the modified Cardan's solution method outlined by Nickalls (1993) to derive the following estimator"*

Testing that different solution methods give consistent results is of course excessive and unnecessary in an operational sense. We simply want to highlight to the reader that we have been careful enough to check the accuracy of the derived analytical solution. The sentence beginning on line 258 can be abbreviated to:

"We have confirmed that nonlinear numerical optimisation of Eq. (13), polynomial root finding of Eq. (14) and the closed form Eq. (15) all give identical results."

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 ωf, what is the associated variation in kf 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.

As highlighted by the other reviewer, for a given set of system parameters (including elastic parameters, air wave velocity and water >2m depth), the air-coupled flexural wave occurs at a constant frequency-thickness product. Halving the thickness will double the flexural frequency. By contrast, halving the Young's modulus will only increase the flexural frequency by a factor of ~1.3. Thickness variation is therefore the dominant control on the flexural frequency compared to the Young's modulus, which stems from the fact that flexural stiffness depends on thickness raised to the third power and Young's modulus raised to the first power. In addition, ice thickness can vary over a much larger range, from a few cm to many tens or hundreds of cm, compared with the Young's modulus of sea ice.

We will address the comment on the apparently constant nature of the Young's modulus which is also in line with the other reviewers' comments. Our point was that using a single set of constant elastic properties we estimate thicknesses that are consistent with borehole measured thicknesses for all four field seasons. 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. It was a surprising result that a single value of Young's modulus should fit as well as it did for four different field seasons of first-year sea ice with significantly varying thicknesses reflecting four quite different freezing seasons. We can make this clearer in the revised manuscript by highlighting that; if the Young's modulus for the 2013 field season was assumed to lie outside the range of 2.3-2.7 GPa, the median thickness estimate would fall outside the range of borehole measured thicknesses. While we lack independent measurement of Young's modulus, the independent thickness measurements from boreholes justify the comment on the Young's modulus, since thickness and elastic properties both contribute to flexural rigidity.

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?

On line 211 we state "The air-coupled flexural frequency is insensitive to water depths >2 m" which is true for the elastic properties considered in this study. With the possible exception of the part of the 2017 profile closest land, the water depths in the surveyed area are much greater than 2 m and have no effect on the flexural frequency and determination of *h*. We can restate this in the section spanning line 276-295 to make this point clearer. Investigations of ice flexural waves with much lower frequencies than the air coupled flexural wave are likely influenced to some extent by the finite, variable water depth of the fjord.

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?

This is essentially what we have done, i.e., using the borehole measured thicknesses as a calibration reference to constrain the chosen value of Young's modulus. Interestingly we found that a single, constant set of elastic parameters gave thickness estimates that were consistent with borehole measurements for all four field seasons. We can make sure this is communicated by being more specific about the origin of the assumed values presented in Table 1 (which was also flagged as lacking by the reviewer). Thanks for bringing this up, we agree it is important to be clear on this subject.

It remains most natural to interpret local variations in flexural rigidity as local variations in ice thickness, since thickness mathematically dominates the flexural rigidity (see our response to point 8). Significant spatial thickness variability is also quite well known from borehole measurements like those in the present study, and from upward-looking multibeam sonar imaging studies like Wadhams et al. (2006) and Sandven et al. (2010), which could be added to our reference list. Notably, Sandven et al. (2010) include observations of ice in Van Mijenfjorden.

Wadhams, P., Wilkinson, J. P., & McPhail, S. D. (2006). A new view of the underside of Arctic sea ice. *Geophysical Research Letters*, *33*(4), L04501.

Sandven, S., Hansen, R. K., Eknes, E., Kvingedal, B., Bruserud, K., Nilsen, F., Wåhlin, J., Sagen, H., Kloster, K. (2010). ICESONAR Monitoring of sea ice thickness from a subsea 4D sonar, NERSC Technical Report no. 294, pp1-45.

More systematic integration of flexural wave methods with other methods like satellite observations, direct measurements and array based multimodal seismic methods like Moreau et. al. 2020a (highlighted by the other reviewer) would certainly be an interesting direction for future research. Parenthetically, we cannot claim that the use of air-coupled flexural waves to measure ice thickness is a particularly novel concept, since Press and co-authors already put forward this idea in the 1950's. We do, however, see that there appears to be hitherto underutilized potential to this effect.

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)

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.

We agree that the discussion section can be tightened up and condensed, we will address this in the revised manuscript. We maintain that despite the model simplicity and assumptions, it allows us to explain the key variability in the experimental data which spans multiple seasons and ice thicknesses. We would argue that there is utility in a simple dynamical model that is able to capture the essential dynamics of a complex system to the extent that the dominant features of a set of experimental data can be explained. The present study is, in fact, a demonstration of the utility of air-coupled flexural waves for ice thickness estimation/monitoring. That a simple model works well for this purpose stems from the fact that ice thickness is raised to the third power and dominates the mathematical expression for flexural stiffness. We think it is important to highlight in the discussion that there is clear scope to tailor the data acquisition approach further if one wishes primarily to monitor ice thickness with air-coupled flexural waves (rather than conduct a reflection seismic survey).

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.

Point taken; we can give the values rounded to the nearest cm. It should be noted that the MAD and drilled thickness ranges are high due to real variation in ice thickness spatially and temporally. Thickness variation of 1 mm corresponds to variation in the air-coupled flexural frequency on the order of 0.1 Hz that is in the realm of what can be resolved spectrally. We do agree that accurately resolving the mm scale is unlikely due to uncertainties in parametrization so we support quoting at the cm range in the table.

The elastic properties are those that fit the data and their choice is discussed from line 281-289. The choice of densities is admittedly not discussed in detail since we don't expect that they play a large role. To be more specific we could add a reference to Timco and Frederking, (1996) - A review of sea ice density, in Cold Regions Science and Technology, vol. 24, pp. 1-6. For the relevant temperature/salinity range the ice density is likely to vary from 920-930 kg.m-3. The water density may range from 1020-1026 kg.m-3 according to the Skarðhamar, J., & Svendsen, H. (2010) article highlighted by the reviewer. The combination of density variations in ice and water could correspond to an apparent variation in ice thickness on the order of 3 mm, which is rather small but could be added to the discussion of chosen parameters.

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?

Good point, a label "bathymetry" can be added to the colour legend to avoid any possibility for misinterpretation. The phrase in the caption "indicating the seismic profiles" can be changed to "representing the geophone arrays (red lines)". The X, Y and L1-L3 are name designators for the profiles that are referred to in Figures 6 & 8. Land contours give context to the place names which are mostly glaciers and mountain tops in this region and these place names are included to give geographic context.

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?

Good point, the photo can stand on its own and we can change:

"As indicated by the photo, these seismic sources produce a strong air-wave that propagates radially over the ice surface…"

To:

"These explosive charges produce a strong air-wave that propagates radially over the ice surface…"

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.

We can refer to Table 1 for the elastic properties and can increase the font size to improve legibility.

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

Yes, this will be stated in the figure caption in the revised manuscript.

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

We can improve the legibility and include colour bars for figures (a) and (b), which can be rather compressed since really only the relative values are important. V_{air} will be changed to C_{air} according to an earlier comment, “int” can be changed to “t_{int}” to be more consistent with the text, “amp” is the local maximum Radon transform amplitude and can be omitted once a colour bar is added.

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?

Agree it will be better to eliminate jargon here so “shot gather” can be changed to “geophone records”, “offset” can be written explicitly as “source to receiver offset”. It will be stated explicitly in the figure caption that (b) and (e) are the timeseries corresponding to the dashed red lines in (a) and (d).

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

It simply gives some context to the temperature variation that occurred during the field campaigns. While the temperature after the field campaign does not affect the measurements directly, it gives an indication of whether or not the field campaign coincided with an unusual weather pattern. We can certainly remove the period after the field campaigns, but it seemed natural visually to provide some context.

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

This may be true, there is not a clear trend here and we have given a tentative comment, but it may just as well be omitted in the revised manuscript. The most obvious case of temporal change in ice thickness occurred in 2018 due to a rainstorm and freezing of surface water.

Fig 8: How is the shoreward portion of the line (discussed on Line 338) identified?

The shoreward portion of the line is identified by referring to Figure 1, but we recognize the need to include a reference to Figure 1 in the text/figure caption and add North and South labels to the profile in Figure 8 so that the spatial orientation is clear.

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.

We can remove the statement referred to, as we agree it may be too speculative given the lack of detailed field observations covering the whole time period of the field campaign.

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?

We simply want to highlight that we have chosen a single set of constant elastic parameters that fit the observed data for four field seasons well with the particular exception of the very end of this profile which happens to be very close to land. We would suggest that the main effect is that we (as is common) have assumed an infinite plate with a free lateral boundary. Close to land it would likely be necessary to model a finite or semi-infinite plate with a fixed/clamped boundary condition. Since it was such a small part of a much larger set of data, we chose not to pursue this further in this study. An increase of Young's modulus from 2.5 GPa to 6.2 GPa could explain why the measured thickness decreases from 50 cm to 35 cm, while a value of 9.5 GPa would be required to explain the 30 cm borehole measurement. However, these values are well outside the range expected for first year sea ice (ref. Timco & Weeks, 2010) and would therefore have to be interpreted as apparent/effective values that incorporate the pinning effect of the land.

Sensors positioned close to, or on land are attractive from the perspective of field safety so this could be an interesting area for further study with scope for significant model improvement.

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.

Ok, no problem.