Response to the respected Referee 1:

Thank you for the opportunity to give a peer review of this interesting article, "A Collection of Wet Beam Models for Wave-Ice Interaction".

Summary:

The article contributes to the wave-ice interaction, especially modeling the wave decay and dispersion when surface water waves propagate through an ice cover. The authors assumed the sources of wave energy dissipation from two mechanisms: one is water wave forces, and the other is the mechanical behavior of the ice layer, denoted as the fluid-based and solid-based energy damping mechanisms, respectively. They present "wet-beam" models that introduce the wave radiation term (heave direction only) in the Euler-Bernoulli beam theory and different rheologies for ice. The considered rheologies contain Kelvin Vogit (KV) model and Maxwell model and use pure elastic material as reference. Relevant dispersion relations are deduced.

The decay rates and wavenumbers are calculated using the dispersion relations with tuned rheological parameters to fit measurements from fields and lab flumes. The measurements cover landfast ice, broken ice from fields, and two lab flumes experiment with viscoelastic material and freshwater ice. The wet beam models using viscoelastic materials can agree with the measured wave decay rates in the landfast ice and broken ice fields. However, for freshwater ice, the models cannot give a well fit for decay rate and dispersion at the same time. The discrepancy is solved by introducing three-parameter viscoelastic rheologies into their dispersion relations.

The study found that the fluid-based energy damping mechanism is dominant for long waves, and the solid-based mechanism is important for short waves. The damping term in the wave radiation plays a more important role in decay rate than the added mass term. The heave added mass term can affect the wavenumber. It is also interesting to find that the equivalent Young Modulus of an SLS-type material using Maxwell approach is close to what is measured in dry tests.

The proposed idea of considering wave radiation in modeling waves propagating through ice cover will be of interest to the readership of the journal. Please see my reports below:

Dear respected Referee, we are very thankful to you for reviewing our paper and providing constructive comments to improve the manuscript. Your general comment on our paper really motivated us to further work on the manuscript and increase its quality. You will find our replies to your comments in this letter. Also, following your comments, suggestions, and queries, we have revised some parts of the manuscript, which will be visible if we are asked to upload a new version of the manuscript, though these changes are clarified in the present letter.

General Comments:

1. A few typos need to be corrected, which are listed in the specific comments.

All these typos will be corrected in a new version of the paper, which is not uploaded yet.

2. Do the dispersion relations Eqs. (13-15) have multiple roots features like the models mentioned in Mosig(2015)? For example, Figure 2 of Mosig (2015) shows a root distribution in the wavenumber and attenuation domain. In other words, are there multiple roots solved from Eqs. (13-15) satisfying ki>0 in this work? If so, what are the criteria for choosing the dominant root?

We are very thankful to the respected Referee for this question. That is an interesting question, and it could be much better to address it in the previous version of the manuscript. Any of presented dispersion relationships can have multiple roots as observed and discussed in Mosig (2015) and Fox and Squire (1990). The roots of dispersion relationships can be found using numerical methods, an example is presented in Section 3 of Das (2022). In the present manuscript, we have found the dominant root by using an initial guess, which was set to be equal or greater to open-water wavenumber. Following a numerical approach, the dominant root is found. In a new version of the manuscript, it will be clarified.

3. What is the reason for using different dimensionless viscosities for KV model and Maxwell model in the last row of figure 2?

Thanks for this comment and noticing this point. It would be much better to run both models with similar dynamic viscosities. Following the comment of the Respected Referee, we have corrected this problem and changed the inputs of the last row of Figure. Dimensionless viscosity of both models is now similar. Further, the respected Referee has suggested to remove the third column in one his specific comments. This has also been done by the authors. The new version of this Figure is shown below (Figure 1 of this letter). Also, the discussion (such as citation of each panel) is revised in the new version of manuscript. In the last row, two different curves for PE material are needed to be plotted as two different Elasticity numbers are considered. It can be seen that the decrease in Elasticity number may lead to increase of decay rate over the short-wave range. It is line with the results presented in Figure 9 of Mosig et al. (2015). Note that, in Figure 9 of Mosig et al (2015), the PR model (which is identical to PE material with added mass of nil), a non-realistic Shear modulus ($\approx 3.2 \times 10^{7}\ Pa$) is seen to give the best fitting for decay rate over short-wave range, though a realistic Shear modulus ($\approx 9.2 \times 10^{2}\ Pa$) is observed to under-predict the decay rate over short-wave period. The related discussion is added to a new version of manuscript, which is not uploaded yet.

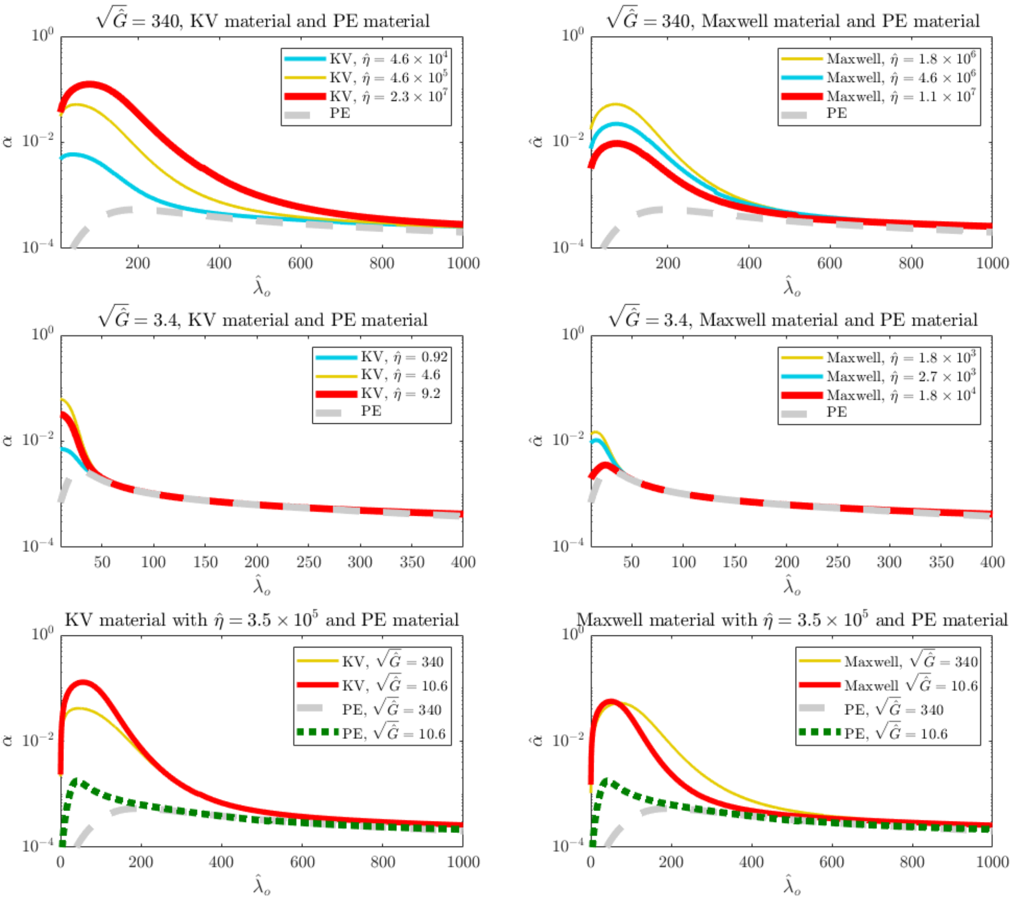

Figure 1: A new version of Figure 2 of the manuscript.

3. It is unclear what value of the added mass coefficient A is used except in Figure 4 of this manuscript.

Thanks for the comment. In all the cases, $A/\rho_w h^2$ is set to be 1. It will be clarified in the new version of manuscript.

4. Is there a comparison of wavenumber corresponding to the wave decay rate comparison with Wadhams et al. (1988) and Meylan et al. (2014) in figure 6? It would be comprehensible to have such a comparison.

The authors were keen to compare the dispersion plots against any of listed experiments. But the dispersion plots (or data) of those studies are not presented/available. As such, we were not able to compare the results of present model against those of Wadhams et al. (1988) and Meylan et al. (2014). It is clarified in a new version of manuscript which will be uploaded if we are asked to do so.

5. Do you consider the wave excitation force to be another necessary potential source? Because the excitation forces, radiation forces, and static forces are the common forces that need to be considered in hydrodynamics. It could occur in low ice concentration fields of ice floes.

This is very interesting discussion. We are thankful to the respected author for mentioning this point. If we have a look at the paper presented by Newman (1994), we can see that the term $\phi\omega^2$ can represent the wave excitation force (caused by the original wave propagating under the cover) acting on the lower surface of the solid body covering the water. It will be clarified in a new version of manuscript. However, the comment of the respected Referee inspired us to discuss another potential source. When the ice floe is not integrated (such as the ice fields of Wadhams et al. (1988) and Meylan et al. (2014)), ice-ice effects, which is also known as body-body interaction, may emerge. In such a condition, presence of a neighbor floating ice floe can lead to extra added mass forces and damping forces. Apart from that, in the gap in between any two ice floes, the water surface profile may lead to an extra damping, which is introduced as artificial damping (an example is presented in Lu et al. (2010) and (2011)). In this condition, the pressure caused by the wave is formulated as

$$p = \rho\phi\omega - i\mu\phi, \tag{1}$$

where μ is the artificial damping (Lu et al. 2010). We have not considered this artificial damping in the present research, but its consideration may affect the results presented in Figure 6. If such a term is applied, different values of the viscosity may work when any of models is used. The related discussion will be added to Section 3.5 of the manuscript.

Specific Comments:

Line 117, Eq. (9), shear stress modulus G_E is equal to shear modulus G. Do you mean G is the elastic modulus or Young's modulus?

Thank you for the comment. G_E is the dynamic shear modulus, which can include storage modulus (real component) and loss modulus (imaginary component). For a pure elastic material, the imaginary component is nil, and dynamic modulus equals shear modulus (G) of the material. It is now clarified in the new version manuscript.

Line 157, ko is not claimed.

Thank you for the comment. k_o is the open-water wavenumber and will be introduced in the new version of manuscript.

In the bottom row of Figure 2, the Elasticity number corresponding to the dashed gray curve is not specified. By the way, the right column could be removed since the data are already presented in the other columns.

Thanks for the comment. The curves presented in the right column are also presented in the two other columns. Thus, the last column is removed the new version of manuscript, which is not uploaded yet. Also, the Elasticity number of the PE model, presented in the last row is added to Figure 2 of the new version of manuscript. Please see Figure 1 of this letter.

In figure 3, the FS model corresponding to the blue curve is not defined in the left panel. in the right panel, what is the reason for the sudden drop of the blue curve near the nondimensional wavenumber = 580.

We are very thankful to the respected Referee for this comment. The relationship for FS model will be presented in the new version of paper. In relation to sudden drop, this was a subtle point which had not been noticed by the authors. It seems to be an error of the code used for calculation of dominant root of dispersion relationship, which may happen when the fluid damping is set to be zero. The error was due to the initial guess related to long wavelength. In the previous version of manuscript, the initial guess, related to this plot, was set to be much larger than that of open-water wavenumber, which resulted to a sudden jump at dimensionless open-water wavelength (≈ 580). We have found it very interesting, and also clarified it in a new version of manuscript which will be uploaded if we are asked to do so. The new version of this Figure is also shown below (Figure 2 of this letter).

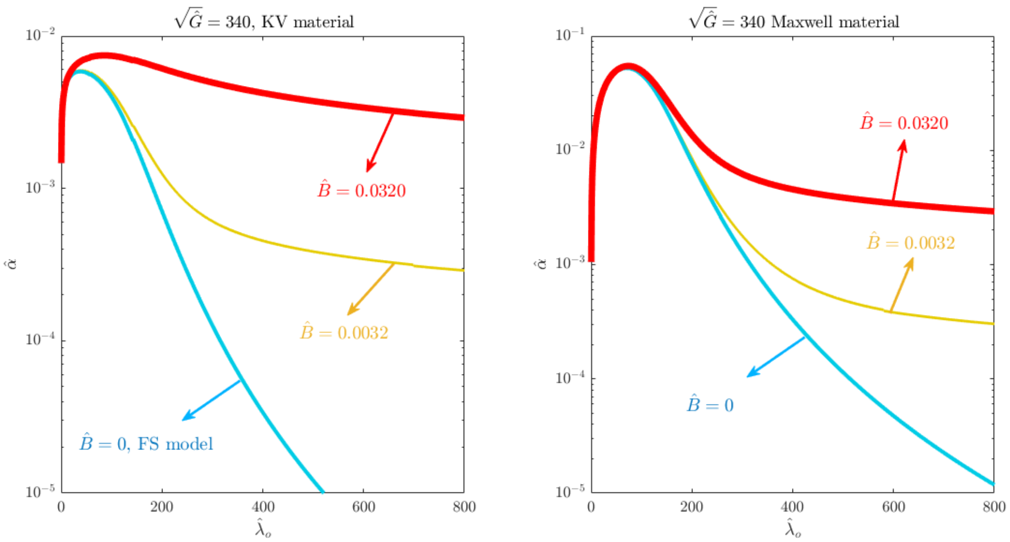

Figure 2: A new version of Figure 3 of the manuscript.

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Line 230, it seems to be a typo, change the word 'travailing' to 'traveling'

Thanks to the respected Referee. This error is corrected in the new version of manuscript.

Line 243, I feel the paragraph is confusing, except "The heave added mass coefficient is seen to affect the dispersion process of waves propagating into the cover with lower Rigidity", which can be read from Figure 2(right). It is acceptable to continue with " the heave added mass coefficient can …". But I don't see why it 'matches with' large rigidity.

We agree with the respected Referee. This paragraph is re-written.

"The heave added mass can affect the dispersion process of waves propagating into cover with lower Elasticity number by increasing the wavenumber, through its influences on the cover with lower Elasticity number are more noticeable compared to cover with larger Elasticty Number".

Line 276 typo, correct the word 'viscoelastic'.

Thanks for the comment. In the new version of manuscript, the term "viscoleastic" will be changed into 'viscoelastic' in the new version of manuscript.

Figure 6's caption, a typo, move a 'by' from '... data measured by by Wadhams et al. (1988), upper row, and Meylan et al. (2014) ...'.

Thanks to the respected Referee. This will be corrected in a new version of manuscript.

The fluid damping coefficient B of red solid curves in the legends in the top row of Figure 8 is partially missed.

We are thankful to respected Referee for pointing this out. B is 100 Pa / s and this problem will be corrected in a new version of the manuscript. The new version of this Figure is shown below (Figure 3 of this letter).

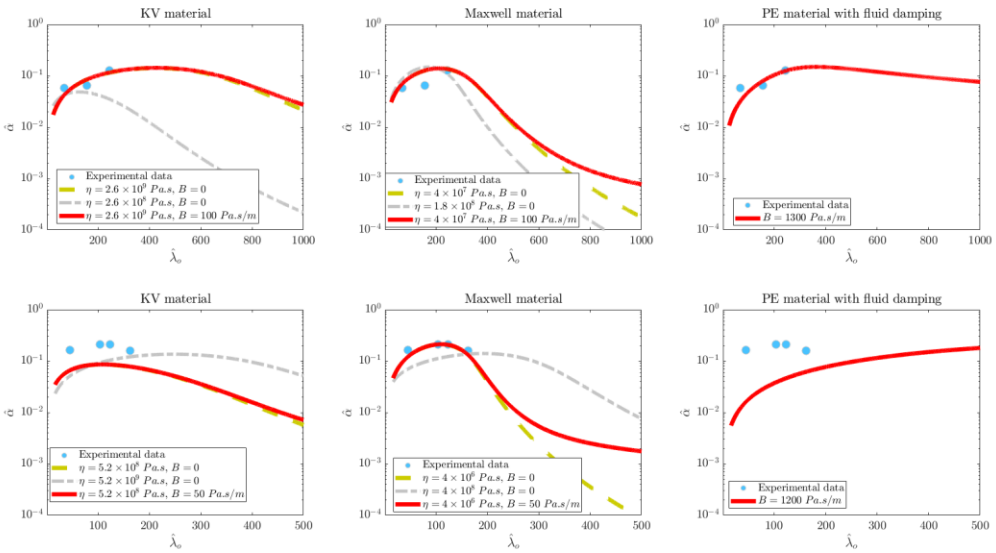

Figure 3: A new version of Figure 8 of the manuscript.

Line 322, change "Left and right panels ... Maxwell and KV materials." to "Left and right panels ... KV and Maxwell materials."

It will be corrected in a new version of manuscript.

Line 455, a grammar error in “dispersion curves Maxwell model give is sensitive to dynamic viscosity”

We are thankful to the respected Referee. “give” will be changed to “gives” in a new version of manuscript.

References:

Das S, 2022, Flexural-gravity wave dissipation under strong compression and ocean current near blocking point, Wavesin Random and Complex Media, DOI: 10.1080/17455030.2022.2035847

Lu L, Teng B, Sun L, Chen B, 2011, Modelling of multi-bodies in close proximity under water waves—Fluid forces on floating bodies, Ocean Engineering, 1403-1416.

Lu L, Cheng L, Teng B, Sun L., 2010. Numerical simulation and comparison of potential flow and viscous fluid models in near trapping of narrow gaps. J. Hydrodyn. 22 (5s1), 120–125.