Dear Editor and dear Reviewers,

We would like to take this opportunity to express our appreciation for your valuable and constructive comments. We have revised the manuscript following the comments and suggestions of respected Editor and Reviewers. All revised parts in the been highlighted with red color in the manuscript. Also, we have provided point-to-point responses to the comments of both Reviewers, following in blue colour.

Modifications based on the suggestions of the respected Editor

Dear respected Editor, we are very thankful to you for reviewing the manuscript, and providing us with your comments on Originality, Scientific Quality, Significance, and Presentation quality of the manuscript. Following your comments and suggestions on Originality and Significance of the manuscript, we have done two modifications, outlined below:

1 – We have added clear statements regarding what new approach and what new insight are presented in the paper (line 88 of the new version of manuscript).

2 – We have explained the possible application of the research in a more explicitly environmental context in Abstract (line 16 of the new version of manuscript), Introduction (lines 24 – 30 of the new version of manuscript) and Conclusion (line 560 of the new version of manuscript).

Point by point response to Reviewer 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. Please note that, after the interactive discussion we decided to remove the justification of the consideration of the forces through the radiation problem, as it can be physically dubious.

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 manuscript.

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. It is clarified in the new version of manuscript (line 180 of the new version of manuscript).

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 (please see the new version of Figure 2).

4. 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 is clarified in the new version of manuscript (line 318 of the new version of manuscript).

5. 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 the new version of manuscript (line 403 of the new version of manuscript).

6. 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. But, the wave force acting on the structure is the dynamic pressure, known as the Froude Kyrolve force. But in a low concentration field, the gap effects and the body-body interaction may happen. We have found it very interesting and have added the related explanations to the manuscript (line 128 of the new version of 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 and G is shear modulus (it is clarified in line 147). Dynamic shear modulus can include storage modulus (real component) and loss modulus (imaginary component). This point is clarified in the manuscript (Line 140 of new version of manuscript). 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 147 of the new version of manuscript).

Line 157, ko is not claimed.

Thank you for the comment. k_o is the open-water wavenumber and is introduced in the new version of manuscript (Line 198 of 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.

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 (Line 175). 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 (line 183 of the new version of manuscript).

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 (Line 306 of the new version of manuscript).

Line 276 typo, correct the word ‘viscoelastic’.

Thanks for the comment. In the new version of manuscript, the term “viscoleastic” is 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 is 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 is corrected in a new version of the manuscript (please see Figure 9 of the new version of manuscript).

Line 322, change “Left and right panels … Maxwell and KV materials.” to “Left and right panels … KV and Maxwell materials.”

It is corrected in the new version of manuscript.

Point by point response to Reviewer 2

My apologies to the authors for getting to this review later than I anticipated when I accepted the job. The delay is especially unfortunate as there seems to be a fundamental error in the theoretical framework of the study that means I cannot recommend revisions that give a pathway to publication.

Dear respected Reviewer, we are once again thankful to you for your general comment and time you spent reviewing the manuscript. Your comment and the interactive discussion benefited the manuscript. In relation to the theortical error, we believe that there is a misunderstanding.

The authors are proposing a model for wave propagation in ice covered water that includes wave radiation forces (added mass and heave damping), which they say are absent in most models. However, this is not correct as others (e.g. Squire, Meylan and co-workers) have developed many models that include radiation forces (none of which are referenced). Their models of elastic ice floes contain the rigid body modes of heave and pitch (in 2D) as well as elastic modes (see e.g. Meylan & Sturova, 2009, Journal of Fluids and Structures). Here, the authors have attempted to incorporate radiation forces directly into a dispersion relation for the floating ice but its implementation appears to be incorrect. Consider the damping term, which should express the transfer of energy from the body motion to radiating waves, so that no energy is lost from the wave–ice system. It should not, as it does here, induce an imaginary component of the wavenumber and hence wave energy dissipation.

We are thankful to the respected Reviewer for this comment. As was discussed during the interactive discussion, in this research we aim to develop a continuum model. We are trying to develop dispersion relationship under an integrated ice cover spanned over an infinite length. To avoid any misunderstanding, it is now directly mentioned in the new version of manuscript (line 2 of new version of manuscript) and the last paragraph of introduction (line 82 of new version of manuscript).

Following this, we have also modified the introduction section by introducing finite length models (lines 43-47 of new version of manuscript) and continuum models (lines 48-57 of new version of manuscript). This provides a better picture of the common approaches used to establish models. Following this, we have provided our understanding of the limitations of the continuum models and the opportunities (this opportunity is to combine fluid and solid forces to formulate the dispersion relationship) for their improvement in lines 60-83 of the new version of manuscript.

We believe the above modifications will address the missing references related to finite length problems and will provide an idea of our motivation in this paper (the way we have viewed the limitations and opportunities).

In relation to the problem with the radiation problem, after the interactive discussion, we have decided to remove the justification as it may make understanding of the problem very hard and also may be physically dubious. We have tried to introduce the fluid forces just in the way other researchers do (line 124 of the new version of manuscript).

The term in the dispersion relation used to represent heave radiation is identical to that derived from the Robinson–Palmer model, which has been used by many previous authors and shown to be capable of giving reasonable predictions of wave attenuation (again, lots of references missing). Therefore, key findings, such as "decay rates were observed to be poorly predicted if the fluid-based energy damping is not taken into account", must be reinterpreted in the context of the RP model and lose their novelty.

We are thankful to the respected Reviewer for this comment. We believe this is a misunderstanding. The conclusion we made is related to viscoelastic models presented in this paper, not the elastic model. We believe we may avoid this misunderstanding by clearly mentioning what is new in this manuscript, which was also suggested by the respected Editor (Line 88 of manuscript). We have also mentioned that the pure elastic model is RP model with an additional term, and we are only attempting to see how it works in prediction of the decay rate, as compared to viscoelastic models (Lines 163-174 of the new version of manuscript).

Aside from the issues with the radiation force, the paper comes across as contributing yet more models of waves in ice covered waters with parameters tuned to particular datasets but without the general predictive capabilities needed for improved understanding of the wave–ice system. It is not surprising that adding more tuning parameters allows for better agreement with observations. Advances require connections between the parameter values and the ice properties associated to the different datasets.

We are thankful to the respected Reviewer for this comment. We have introduced new dispersion relationships in this manuscript, and we believe it is common practice to check the validity of the models with tuning parameters. Meanwhile, in sub-sections 3.4 and 3.5 we have tried to discuss why different inputs give the best agreement with experiments. Providing more discussion related to the mechanical behavior of the ice is out of the scope of the present research, and may need laboratory tests.

Point by point response to Reviewer 3

Theoretical model

The theoretical model is

- purely elastic ice, or damping in the ice from the imaginary part of the Young's modulus. The specific formulation for the damping comes either from the Kelvin-Voigt or the Maxwell rheology and gives different frequency dependance in the damping coefficient.

- damping in the fluid from B, the radiation damping coefficient. (This is the same as the Robinson-Palmer model.)

- extra inertia from A, the added mass coefficient

The main novelty to me are the different ice rheologies, but the fluid damping effectively has little novelty (with the exception of A) but only introduces a more complicated (and physically more dubious) justification for the Robinson-Palmer (RP) model. I would remove the physical justification completely as (a) unnecessary and (b) physically dubious. (Note I am not proposing to remove the RP model itself as applying an old model to new data can still be interesting.)

I say it is physically dubious as the added mass and damping are usually derived from solving the hydrodynamic equations (Laplace's equation + sea floor condition + boundary condition (7)) with A=B=0 when a body is forced to oscillate. So to put them into (7) seems a bit circular. (Incidentally, in equations 5 and 6, z^4 should be z_{xxxx}.)

In the authors' reply to Reviewer 2, they talk about continuum media (I guess effective media). Maybe they are trying to represent the attenuation due to scattering by a large number of scatterers. Phase-resolving scattering models do predict that wave energy does decay into ice, but they also conserve energy. While they would not be the first authors to represent the attenuation due to scattering with a dissipative model (eg Williams, Bouillon & Rampal, 2017, The Cryosphere)(for lack of a good alternative), they aim to represent it entirely with Robinson-Palmer dissipation instead of empirically, as most authors do.

It should also be noted that scattering models give quite different results to Robinson-Palmer especially at long periods, and since Robinson-Palmer (combined with the dissipation inside the ice itself) gives realistic results in these case it begs the question of why they are bringing in scattering at all.

The authors are very thankful to the respected Reviewer for his/her comment on the theortical models presented in the manuscript. This comment was very constructive and helped authors improve the quality of the presentation of the model and avoid any misunderstanding in the paper.

As the respected Reviewer has mentioned, it has been aimed to add the fluid damping term into dispersion relationship (combine the RP model with decay into the ice). We agree with the respected Reviewer that the Robinson and Palm (1990) has introduced fluid damping in their model (RP), which has been used by many scholars over years. In the present research, we tried to include the fluid damping into dispersion relationships of viscoelastic ice beam as we hypothesized that fluid damping term and a complex term in flexural rigidity can be considered at the same time. The first dispersion relationship (Equation 13) is therefore the RP model with an additional added mass term (which may weakly affect the wavelength), as the respected Reviewer has mentioned. We have clarified this point in the new version of manuscript that we do not view this dispersion relationship as a new one here, and the aim is to build relationships for viscoelastic models (Lines 164 to 171 of the new version of manuscript).

The other dispersion relationships, however, incorporate different rheological behavior, as the respected Reviewer has mentioned. We need to recall that our main aim in this paper was to formulate these relationships, not the pure elastic model. We believe we needed to make it clearer in the manuscript. The respected Editor had suggested to add statements in the new version of manuscript to make what approach is new in the manuscript. We believe that can also be helpful (Line 88 of the new version of manuscript).

As the respected Reviewer has mentioned, it is interesting to compare the results of recent tests against those of an old dispersion relationship (the RP model). This was a part of the reason that we compared the results of all experimental tests against those of pure elastic model (RP model with an additional added mass term). The other part of the reason was that we found it interesting to discuss the fluid damping coefficients giving the best fitting when different dispersion relationships (pure elastic versus viscolestic) are used. We have clarified it in the new version of manuscript (Lines 164 to 171 of the new version of manuscript).

In relation to justification, we completely agree with the respected Reviewer. Introducing the added mass and fluid damping through radiation problem may make the understanding of these two forces very complicated and dubious, and we may end up in a loop to justify their presence. To avoid this, we have removed the justification and tried to introduce the damping term in the way other scholars did, i.e., we only introduce it as a damping force coefficient (please see line 124 of new version of manuscript). The phrase heave damping is changed to fluid damping in the whole manuscript and the term radiation problem is removed from the whole sections except introduction where we mention finite length problem.

We are also very thankful to the respected Reviewer for noticing the editing error in the solid beam equations (the z*4 term). It is now corrected. We have also replaced z with ξ as it would be easier to follow equations in this case.

In relation to consideration of a continuum model, we agree with the respected Reviewer. If we want to introduce the problem through considering the added mass and damping of radiation problem, then we may unintentionally deliver a message that different scatters have been used to treat the problem (similar to Williams et al. 2017), though we have not used such an approach It is clarified in the manuscript (Line 106 of the new version of manuscript). A fluid damping (Robinson-Palmer combined) with the dissipation inside the ice itself are the only mechanisms used to calculate the decay rate and no scatterer is assumed in the present research. We have found the research done by Williams et al. (2017) very interesting and have introduced it in the Section 1.

Results

- right hand columns of fig 2 not needed

We agree with the respected Reviewer. The right-hand column is not needed and is removed (please see the new version of Figure 2).

- why not just have k_i instead of α since the attenuation is only coming from the dispersion relation?

The reason is that authors are also introducing wavenumber in the ice covered sea and use the k_i to represent it. Note that, wavenumber in open-water is used for normalizing decay rate and thus we have used the subscripts o and i to denote the wavenumber in open and ice covered regions, respectively.

- ice rheologies give different attenuation behaviours (peaks in attenuation) at high frequencies. This is interesting that peaks can be produced with different rheologies.

Thanks for this comment and noticing this point. The peak predicted in the decay rate curves is because the decay rate is normalized using open-water wavenumber. The dimensional decay rate only peaks when the RP model is used. Two dimensional figures are presented, and related discussions are added:

Figure 4 and the discussion presented in Lines 372 to 286.

Figure A1 and the discussion presented in Appendix A.

However, once you start to introduce more complexity (I am thinking especially of the SLS models) there are more parameters to be tuned and there is a danger of overfitting.

We agree with the respected Reviewer. Adding more parameters, while interesting, may lead to overfitting of data. Understanding of the mechanical behavior of the ice becomes very hard under such an assumption. Perhaps, more studies need to be carried out in the future to investigate whether a three-parameter model (or even models with greater number of elements) can be used or not. It is explained in the manuscript. We have made it clear in two different parts of the paper (Lines 450 to 453 and Line 524 to 527 of the new version of manuscript).

Moreover, the peak in attenuation may not be real as instrument noise and local non-linear wave generation of high-frequency waves can give the appearance that high frequencies are being attenuated more than they are (Thompson et al, 2021, J. Geophys. Res.), so trying to fit them too accurately may not be wise.

Thanks for this very constructive comment. Authors believe this point should be explained in the manuscript. As we explained in response to the other comment (the third comment of the respected Reviewer on the Results), only the RP model gives a peak in decay rate plots, though the viscoelastic ones do not. To make it clearer, we have added an Appendix A in which we have presented an example of the dimensional data. It can be seen that the curves constructed using the Maxwell and KV models do not peak in the short-wave regime, though the field data and RP models reach a peak in this zone. Generally, KV and Maxwell models can never predict peak a peak in decay rate plots over the short-wave range.