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 Referee, we are very thankful to you for your comments and the time you spent reading the manuscript raising your concerns. We have noticed that you think that there is a fundamental error in formulating this research, but we believe there is a possible misunderstanding leading to such a conclusion. We have viewed your comment as an opportunity. This, in essence, helped us enhance the quality of the manuscript as we can avoid any misunderstanding when we clarify the general idea behind the model. Our responses to your comments (marked with blue colour) are listed below. Also, following your comments, we have made some changes to a new version of the manuscript, which will be visible when a new version of the manuscript would be asked to be uploaded.

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 Referee for this comment and raising this concern. The models used for wave-ice interaction can be either related to water wave interaction with finite length body (or bodies) and water interaction with infinite or semi-infinite length ice cover (please see the introduction of Xu and Guyenne 2022, which introduces these two approaches as discrete-floe models and continuum models). The former approach may be used in the calculation of the wave-induced motions of any flexible body (*e.g.* ships, barges, energy converter devices, *etc*), wave scattering, wave transmission and may have applications in many different fields, ranging from oceanography to hydrodynamic of ships. The latter may be used for calculation of the ice-induced energy decay. If the ice cover is assumed to be semi-infinite, the ice edge dynamic and wave reflection can also be addressed. As known, continuum models can be used to formulate dispersion relationships (please see the introduction of Xu and Guyenne 2022), which can be utilized to calculate the wavelength (or wavenumber) and energy decay rate of waves travelling in covered water (please see Chapter 3 of Dean and Drlymple (1984)).

In the present study, as is explained in the Abstract, Introduction and Section 2, we are studying an integrated ice cover that is expanded over a long way, which means this research focuses on continuum models, not the finite length ones. Accordingly, previous studies introduced in the literature of the submitted manuscript highlight the wave interaction with infinite length or semi-infinite length covers (continuum models). The respected Referee is referred to different parts of the manuscript where the problem is introduced. We have listed some of them below:

Line 1: "*Theoretical models for the prediction of decay rate and dispersion process of gravity waves traveling into an* ***integrated*** *ice cover are introduced*"

Line 27: "*Mathematical modeling of the wave-ice interaction has first received the attentions of researchers in the 19th century. The first model was developed by Greenhill (1886), who formulated harmonic motions in a fluid domain covered with an elastic beam. To build the model, he assumed that the ice extent was spanned over* ***an infinite way*** *and the solid body had relatively small motions*."

Line 74: "*Consider a two-dimensional fluid domain containing water.* ***The domain extend is stretched over an infinite length***."

To avoid any misunderstanding, we have added the following to a new version of the manuscript, which is not uploaded yet.

"*The models assuming that ice length is spanned over an infinite length are known as continuum ones. Using different assumptions made for the fluid and solid behaviour, their dispersion relationships are developed. The real and imaginary part of a dispersion relationship inform us about the wavenumber and wave decay rates. The wet beam models developed in the present research are formulated using such hypothesis (i.e. an infinite length body covers the whole fluid domain).*"

The *missing references* are related to **finite-length** problems in which the radiation problem is considered. The reason that these studies were not mentioned in the previous version of the manuscript was that they are basically built to analyze a different physical problem (i.e., a finite length one). But it is worth noting that the present research is inspired by ship hydrodynamics (finite-length problems) where the radiation force was and still is considered. We believe that we should avoid any misunderstanding. We are thankful to the respected Reviewer for mentioning this point. So, we will add a brief explanation to introduce the finite-length problems, explaining that they are mostly developed for calculation of wave transmission and body responses to water waves. To address this comment, we will briefly explain the difference between those problems and the continuum ones in the new version of the manuscript. However, we will then explain that the radiation force incorporated in these models inspired us to develop the present continuum model. This may help the readers to understand how we have formulated the new model. The following will be added to a new version of the manuscript.

"*Mathematical models for finite lengths problems have also been developed over time. They can be used for calculations of the wave-induced responses of the ice and wave transmission by the ice floes. In these models, the wave radiation forces are mostly considered, though they are not included in the infinite length problems. Many examples can be found in the literature* (e.g. Meylan and Squire (1994), Kohout et al. (2007), Meylan and Sturova (2009), Montiel et al. (2012)). *Note that these models can also have application in hydroelastic analysis of ships, very large floating structures, breakwaters, energy converters etc* (e.g. Michele et al. 2022)*. The radiation force considered in most of these studies motivated us to hypothesise that this force may also emerge during the interaction of an infinite length ice floe interacting with water waves.*"

The respected Referee has mentioned that we have directly implemented the heave damping term in the dispersion relationship. We are thankful to the respected Referee for opening this discussion. But such a thing has not be done. As it was explained in the manuscript, the governing equation on the elastic beam (let's call it the solid dynamic equation) is modified to a wet beam by considering the radiation force, which is related to a wet body, and finally, the dispersion relationship is built step-by-step in the way some other researchers have done to model infinite length problem. The respected Referee is referred to line 96 of manuscript, where the dry beam equation is introduced as

"*The above equation is formulated for a dry beam (see panel a of Figure 1), which vibrates under the force of water waves*"

In the next step, the respected Referee is referred to line 104, where the wet beam equation is described as:

"*Equation 6 is an extended version of the Euler-Bernoulli beam model which is adopted for a beam exposed to water radiation forces*".

The respected Referee has mentioned that the *heave damping is directly implemented into dispersion equation* (equations 12, 13 and 14). But, as discussed, the radiation force is incorporated in the solid dynamics equation (6) (not in the dispersion relationship), and then in the next steps the dispersion relationship is extracted by establishing the equation governing the free surface (Equation 7).

As we have mentioned earlier, we are inspired by ship hydrodynamics, and the way that motions equations of flexible floating bodies are formulated. *i.e*, Equation 6 comes from our understanding of ship hydroelasticity. We refer the respected Referee to one of the most famous references of ship hydroelasticity, Newman (1994). In this reference, solid dynamic equations related to elastic modes of a deformable body are formulated and radiation forces are also implemented (please see Equation 3.8).

The respected Referee has commented that "*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*."

We are thankful for this comment. But the radiation damping (called damping term by the respected Referee) can dampen the energy of the body, motions of which are coupled with those of fluid. To understand this fact, we refer this respected referee to the definition of radiation damping, introduced by McCormick (2010) (please see page 352 of McCormick (2010)):

"*As stated previously in the chapter, the body motions also result in a transfer energy to the sea, and the transferred energy flux is away from the body. This results in an energy loss to the body motion. The resulting damping effect on the body motion is called the radiation damping.*"

As clearly explained, radiation damping (caused by the motion of a solid body) results in the energy loss of the body motion. For our case, the solid body is the ice cover, the motions of which result in radiation, causing the energy loss of the ice motion (waves propagating in the ice cover), which is coupled with water motion. Simply stated, a proportion of the energy of flexural-gravity waves is turned into radiation energy. If the energy of the body (which is coupled with the gravity waves) is lost, the wave-body energy (the energy of original wave) is lost, and is turned into the radiation energy, which is different from the wave-body energy (the energy of original wave). Please note that, we aim to study the loss of the energy related to the wave-body system (the energy of original wave).

Here, we need to emphasise on the fact that the term *dissipation* is just a word here. It does not necessarily refer to the conversion of wave-energy into heat. For example, if the surface waves lose mechanical energy to the surface current in the upper ocean, or to generating the winds in the atmospheric boundary layer, their energy is lost and will never come back to them. Thus, from the point of view of modelling these surface waves, their energy is dissipated, regardless the fact that it did not turn into the heat, but rather into other form of mechanical energy (for water currents and for wind). In any model of these surface waves, this will result in imaginary part of their wavenumber. The same happens here. The original gravity wave which interacts with the ice cover, can be dissipated (imaginary wavenumber, i.e. decay of their mechanical energy), while the other mechanical energy (related to radiated waves) grows.

We believe to avoid such a misunderstanding, we can refer the readers to the textbook of *Ocean Engineering Mechanics with its Applications* (McCormick (2010)) and explain the above reasoning in the manuscript.

"*Note that the radiation damping can cause the transfer of energy to the water, resulting in energy loss of the ice motion. This consequences in the loss of the energy of the wave-ice system. Readers who are interested in the radiation damping are referred to Chapter 10 of McCormick (2010).*"

Finally, the general idea is based on the wave-induced motions, coming from textbook of *Marine Hydrodynamics.* We assume that the there is an exciting force caused by the progressive wave (please see equation 1 of the present letter), and a force related the radiation of waves (please see equation 2) due to oscillations of the body (which is the ice layer). The respected Referee is referred to Newman (1977)**,** Newman (1994)**,** Bishop and Price (1974, 1976)**.**

We assume that there is no rigid body motion since the ice cover is very long and waves are much shorter, compared to the ice length. Wave motion emerges in the body, since the problem is 2D and we have used the Euler-Bernoulli beam theory, no rotational motion emerges, we only consider modes related to vertical bending as the beam length is spanned over a long way. The beam has an infinite length, so the elastic mode(s) of vibration are what are found through the dispersion relationship. This is exactly like what has been done by other researchers studying semi-infinite or infinite length beams vibrating under any exciting force (see an example in Saito and Murkami (1969)).

We have two different types of force acting on the solid ice beam. These forces are independent, but their combination helps us formulate the dynamic motion of the upper layer and thus build the dispersion relationship. The first force is exciting one caused by the progressive (let's call it wave-induced force) waves is found through

$$f = -i\omega \int \phi \boldsymbol{h} dS \tag{1}$$

The respected reviewer is referred to Senjanovi et al. (2009) (also check Chapter 6 of Newman 1977).

The fluid force related to radiation force can be formulated as

$$f_R = -\omega^2 a - i\omega b \tag{3}$$

The above equation is well accepted by the ocean engineering community, studying radiation force (researchers and engineers aiming to study motions of ships and any floating bodies). The imaginary component of the radiation force is the damping term and has a theortical background (the respected Referee is referred to Newman (1977), Faltinsen (1993), Newman (1994), McCormick (2010), Senjanovi et al. (2009)). In all these references (which are either textbooks or journal papers), the damping force caused by the radiation is the imaginary component of the force. Note that damping force is in phase with the rate of displacement of the solid motion, and the heave damping is in phase with the vertical acceleration. The work done by the added mass force (over a cycle) is zero, though that of damping is non-zero, and is responsible for the loss of the energy (the respected Referee is referred to the Chapter 3 of Faltinsen (1993)). To avoid any misunderstanding, we have added the following to the manuscript:

"Note that to formulate the wet beam equation, two different forces are assumed to act on the solid body: radiation and the wave-induced force. The work done by the heave added mass is in the phase with acceleration, and the work done by heave damping is in phase with vertical velocity of the solid body, the work of which is responsible for the loss of the energy of wave propagating in the ice cover. Since the cover has assumed to have an infinite length, rigid modes are not involved, and the only elastic mode that involves is the one that is found through finding the root of the dispersion relationship. "

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 referee for this comment. Again, this helps us avoid any misunderstanding. The authors did not claim that the term representing heave radiation is mathematically different from what was introduced by Robinson–Palmer. What the authors have insisted on in the manuscript is that the radiation force, including *heave added mass* and *heave damping,* can be incorporated when the dispersion relationship of waves propagating into any body (either elastic or viscoelastic) is formulated. The fact is that, in the previous studies, the fluid-based damping (or let's call it the term $-iB\omega$) and the solid-based damping have not been considered simultaneously (the respected referee is referred to the sub-section 2.5 of Squire et al. 2021). In the present research, however, both two dissipative mechanisms, *heave damping* (fluid- based, $-iB\omega$) and the *viscoelastic effects* (solid-based) are assumed to emerge at the same time. Also, we would like to also explain that, in a more general view, the $iB\omega$ term can represent a combination of the viscous effects (let's call it friction) and heave radiation damping, and any other linear damping. As different linear damping terms may emerge at the same time (please see Section 10.1 of McCormick (2010).

The respected Referee has mentioned that the PR model has been used by many other researchers, but the fact is that the PR model is developed for an elastic plate (*i.e.* material is not assumed to be viscoelastic, the sub-section 2.5 of Squire et al. 2021 describes this) and they have never considered the *viscoelastic effects* when they use any $iB\omega$ term. Simply stated, other researchers have never considered *fluid-based* and *solid-based* damping at the same time, while we have tried to do so in the present research, and we believe this is the contribution of the present study. In the introduction section of the manuscript, we have clearly explained our hypothesis, which makes our research different from any previous (line 60 of manuscript).

"*The coexistence of solid-based and fluid-based dissipation mechanisms in formulations may help us reconstruct the decay rates a more realistic condition*"

The dispersion equation of an elastic beam we presented (Equation 12) is related to the case we only consider fluid-based energy damping, and no solid-based energy damping is considered. This is very similar to PR model. We have honestly and openly mentioned this fact (please see line 133 of manuscript):

"*Another version of this relationship has been previously documented by Mosig et al. (2015), which is originally formulated by Robinson and Palmer (1990) modeled interaction of an elastic body interacting with water waves. But the added mass term is included in the above equation, which makes it different from what was presented by Mosig et al. (2015)*."

We would like to ask the respected Referee to consider the two other dispersion relationships (Equations (13) and (14)), both of which are developed by considering fluid-based (heave damping) and solid-based (loss modulus related to viscoelastic martial) energy damping (they are built based on our hypothesis). We need to also recall that two SLS models are also introduced in the paper (Equations 26 and 27), which are completely different form PR model (as they use three-component solid models). These are not apparently considered by the respected Referee. We would like to ask the respected Referee to consider these dispersion relationships as well.

The respected Referee has commented that the key findings *"decay rates were observed to be poorly predicted if the fluid-based energy damping is not taken into account*" should be reinterpreted the context of the RP model. This quote from the Conclusion Section was mentioned without consideration of the whole paragraph. We would like to ask the respected Referee to consider the previous sentences as well. The complete paragraph is (please see line 465):

"*Predictions of models and field measurements were quantitatively compared against each other.* ***KV and Maxwell*** *models were seen to reconstruct the decay rates and dispersion process of landfast ice with a great level of accuracy. Decay rates were observed to be poorly predicted if the fluid-based energy damping is not taken into account, suggesting that this mechanism has a very important role in ice-induced energy decay over the range of long waves*"

It is clear we talk about **KV and Maxwell models**, not the elastic one (which is the only one that is similar to PR model). When we say, "*decay rates are poorly predicted when the fluid-based energy damping is not considered*", we are making a conclusion related to the behaviour of models built for KV and Maxwell materials, which also include *solid-based energy* damping. We need to recall that these two dispersion relationships are different from PR model as the solid-based energy damping is included. To make it clearer, a PR model uses a *one parameter* solid model, but KV and Maxwell models use *two parameter* ones. In general, ***the key finding of the research is that the solid-based and the fluid-based energy damping can be considered at the same time as there is no inconsistency between fluid damping and solid damping***. We again refer the respected Referee to the hypothesis of this research. What the respected Referee has mentioned is related to an elastic beam, not the two viscoelastic ones and the related findings are documented by Moisg et al. (2015) or previous researchers (found through using the PR model) are related to an elastic plate. We have mentioned this in the manuscript.

The respected Referee has pointed that many references are missing. When we wrote the early version of the manuscript, we only referred the readers to two main studies introducing the PR model are presented in the paper. The original paper of Palm and Robinson (1990) and the paper authored by Mosig et al. (2015). We agree with the respected Referee that it would be better to refer to some other studies which have used the PR model. But we need to emphasise on the fact that all these references present dispersion relationship of an elastic ice (with inclusion of the term $-iB\omega$) and no viscoelastic behaviour is considered as mentioned above. For instance, the respected Referee is referred to Appendix A of Williams et al. (2013a) (Here the appendix is called *Thin* ***elastic*** *plate model with the inclusion of damping*). But to avoid any misunderstanding, we will add these references

(Squire and Fox (1992), Williams et al. (2013a, b), Squire et al. (2009)) to Introduction section of the paper and clarify that dispersion relationship presented in these papers are different from what is presented in Equation (13), (14), (26) and (27), and only consider $-iB\omega$ term. Please again note that the hypothesis of the present research is that terms $-iB\omega$ and $-i\frac{G''\omega^2}{h^3}$ can be considered at the same time.

In total, we agree with the respected Reviewer that the references should be included in the manuscript as we can provide a better understating of what has done before. We have addressed the comment of the respected Referee in the new version of manuscript.

"Note that the previous version of the dispersion relationship of pure elastic material was presented in some other studies (e.g., Williams et al. (2013a, b), Squire et al. (2009)), and is mostly called the RP model. But note that this model has only considers one dissipative mechanism, though in this research, it is aimed to show that coexistence of two different dissipative mechanism (fluid-based and solid-based) help us predict decay rate and dispersion relationship."

Apart from that, other researchers, specifically, Mosig et al. (2015) have shown that the PR model may provide us with a perfect fitting with experimental data. But, in Mosig et al. (2015), elastic modulus of 3.2×10^7 Pa was observed to give the best fitting, though the shear modulus of ice is expected to be greater than 1 GPa. This should be explained through investigating the difference between nature of the viscoelastic models and the PE one, which gives different behaviour for α_i vs. ω. The log-log plots of decay rates as a function of frequency are presented. This again provide clear evidence of the novelty of the present research. This is a clear piece of evidence showing that the presented decay rates of dispersion relationships of viscoelastic materials (Equations (13) and (14)) show different behavior as compared against those of the PR model in large frequencies.

Back to the problem with non-realistic inputs for elastic modulus that may give perfect matching for decay rate when a pure elastic model is embarked. As seen in Figure 1 (of this letter), a PE model gives a dependency of ω^3 up to a specific limit, and then it gives a dependency of $\omega^{-0.35}$ at large frequencies (short wave periods). If the elastic modulus of the material is decreased, that specific limit is shifted towards larger frequencies (please see left column of Figure 2 of the paper). This is the main reason that in sub-section 7.2 of Mosig et al. (2015), the curve predicted by a realistic elastic modulus diverged from the field data at large frequencies (short periods), but the one predicted by a low elastic modulus followed the data. However, viscoelastic models do not show such behavior as $\alpha_i \propto \omega^{0.5}$ at large frequencies.

This is very interesting point. The authors believe they need to mention this point in the new version of manuscript (not uploaded yet) to avoid any misunderstanding and show the difference between the viscoelastic models and Equation (12) (which is also similar to PR model). The following explanting and Figure have been added to that eversion of manuscript.

"One of the main differences between the PE model and the viscoelastic ones is the dependency they give for decay rate as a function of wave frequency. The log-log plots of α_i vs. ω are plotted in Figure 4 to show these differences. The PE model gives a dependency of ω^3 up to a specific limit, and it gives a dependency of $\omega^{-0.35}$ at large frequencies. The two other models give the same dependency the PE model gives at short frequency. But at large frequencies, these models give a dependency of $\approx \omega^{0.5}$. This well shows that when viscoelastic models are used, $\alpha_i$as a function of ω shows a different behavior, especially at larger frequencies. With the decrease in elasticity modulus of the material, the range over which α_i follows ω^3 increases, which means that the viscoelastic effects become less dominant. This may be one of the main reasons that we may be able to fit the experimental or field data when the PE model is embarked if we remarkably decrease the elastic modulus of the material (please see sub-section 7.2 and Figure 9 of Mosig et al.). But when a viscoelastic model with fluid damping is used, a more realistic elastic modulus works."

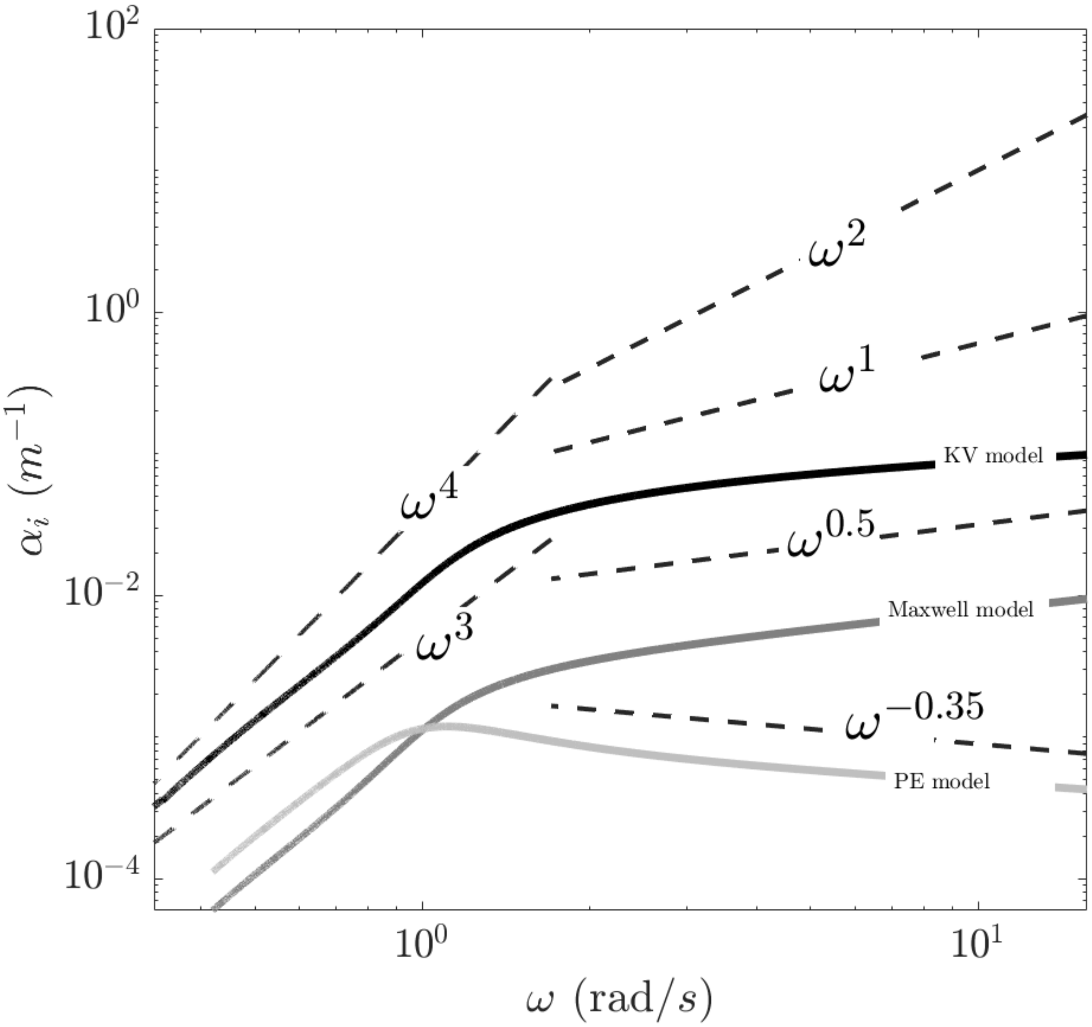

Figure 1. Log-log plots of decay rates as a function of wave frequency. (This is Figure 4 in the new version of the manuscript). The Young's Modulus is set to be 3 Gpa and ice thickness is set to be 35 CM.

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 for this comment. The aim of the present research as explained in the manuscript is to provide models that include both fluid-based and solid-based energy damping, which helps us predict wave decay rate and wave dispersion with less limitation. So, we need to test how we can fit models with the field or flume data. Note that, doing so can be viewed as a common task when a new model is introduced. We refer the respected Referee to two recent references (Xu and Guyenne 2022 and Southerland et al. (2019)). In these two studies, new models for prediction of the ice-induced attenuation are developed, and the accuracy level of models in the prediction of the decay rate is assessed by tuning up inputs. Similar approach is followed in the present research.

The respected Referee has commented "*Advances require connections between the parameter values and the ice properties associated to the different datasets*". In response to what the respected Referee has mentioned, we need to clarify that this manuscript is not limited to tuning up the parameters and developing model. In the manuscript, we have included a section in which we entirely discuss our understanding of the models and why different inputs give the best fitting. Following points were explained:

1- Both fluid-based and solid-based energy damping may emerge when water waves propagate into ice. The respected referee is referred to line (410):

"Energy dissipation is caused by solid-based energy damping and fluid-based energy damping. The former is dominant over high frequencies (corresponding to short waves in an open-water condition), and the latter is dominant over small frequencies (corresponding to long waves in an open-water condition)"

2- The effect of solid-based (the effect of viscoelasticity of the material) damping may be dominated over a wider range of frequency when the elastic modulus of a material is increased. This had been shown in Figure 2 (a new version of this Figure is provided). The respected referee is referred to line (414):

"With an increase in Elasticity number, the wavelength range over which solid-based energy damping is dominant becomes wider"

3- It is discussed that KV and Maxwell models can predict the decay rates of the landfast ice if the fluid-based energy damping is activated. We need to emphasize that it is different from the PR model as this model does not consider any viscoelasticity. It is explained that the viscosities that gives best fitting are different, but we believe the KV material is a better representative as a landfast ice as it is likely to be more similar to a solid viscoelastic material (i.e. the ice is a KV material). The respected referee is referred to line 423:

"Both viscoelastic models were seen to construct the decay curves, fairly follow field measurements. But the dynamic viscosity values were seen to be much different. A KV material may be a more realistic indicator of ice behavior as the lanfast ice is expected to be solid."

4- We then explain that, for the broken ice field (Figure 6), viscoelastic-based models give fitting with different viscosities as compared against viscosities by applying which the same model gave the best fitting for lanfast ice. We have presented a discussion on this difference. We have explained that a volume fraction method may help us understand this. If we assume the ice layer is a mixture of water and ice, then the viscosity of the layer can be represented by a volume fraction method. It is explained that for a KV material, the dynamic viscosity is lower as compared against that of landfast ice, and for the Maxwell material, the dynamic viscosity is lower as compared against landfast ice. It means that if water entrapped in between broken ice floes increases the viscosity of the whole layer, the Maxwell material can be describing the ice behavior, otherwise KV material is a better representative of the ice behavior. This is also seems to be a potential discussion on "*connections between the parameter values and the ice properties*". The respected referee is referred to line 433:

   "The KV model gives the best fitting of decay rate when the dynamic viscosity is reduced, compared to the landfast ice. In contrast, the Maxwell model gives the best fitting with a larger dynamic viscosity. Thus, what can be concluded is that if water reduces the dynamic viscosity of the upper layer, the KV model will be more likely to be a better indicator of the ice behavior. Otherwise, the Maxwell model prescribes the mechanical behaviour of the material. "

5- We have shown that for the freshwater ice formed in the flume viscoelastic models cannot construct relative wavenumber versus open-water wavelength curve and decay rates versus open-water wavelength with the same inputs. This motivated us to build dispersion relationships by assuming SLS models and using three-parameters viscoelastic models. When we found the parameters giving the best fit for both relative wavenumbers and decay rates, we observed that the SLS Maxwell model works with a realistic equivalent elastic modulus (3Gpa), though the SLS KV model works with a non-realistic value (2.5 10^4 Pa). This again has provided us with understandings of "*connections between the parameter values and the ice properties* ". We concluded that the freshwater ice, which was also very young (around 8 hours old), may not be properly modelled by two-parameter viscoelastic models (Equations (13) and (14)), though the SLS Maxwell model provide us with accurate outputs with reasonable inputs. Please see Line 440 of the manuscript.

   "Models, however, were not able to capture the dispersion process under freshwater ice covers with the same input observed to give the best decay rates. Effective values were seen to construct dispersion plots with an acceptable level of accuracy. This has been observed and reported by other scholars over the last two decades, who measured the wavelength and phase speed of disintegrated elastic/viscoelastic covers. The interesting point is that, when a Maxwell model is used, the dynamic viscosity can affect the dispersion process. This motivates us to build other models which formulate the storage modulus by applying the dynamic viscosity. Two available linear models were introduced. One is SLS KV and the other is SLS Maxwell. Both models include two springs and one dashpot. The equivalent Young Modulus giving the best fitting when SLS KV is used is 2.5× 10^4 *Pa, which is much smaller compared to*

that of real freshwater ice. But, for SLS Maxwell, the equivalent Young Modulus was seen to be 3.3× 10^9 Pa which is close to that of freshwater ice."

In the end, we are again very thankful to the respected Referee for her(his) comments and discussions. As his(her) concern helped us provide more details about the model and avoid any misunderstanding.

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