Response to the comments of Reviewer #2

This paper deals with theoretical aspects relevant to the dissipation of waves traveling in sea ice. Particular focus is devoted to the role and parameterization of the ice-water drag as relevant dissipation mechanism. A discrete-element model (DEM) was employed in order to simulate the motion and collisions of the ice floes under the wave action, coupled to the wave energy transport with phase-averaged source terms. As the aim of the paper was to explain wave dispersion and attenuation observed in a wave channel, wave energy dissipation due to overwash was also considered for completeness. Indeed, laboratory wave data are the subject of a companion paper (Part B), for which I was also asked to review. Unlike the water-ice drag, a minor role was recognized to the overwash mechanism to account for wave energy dissipation. Wave energy attenuation was analytically analyzed in the case of compact, horizontally confined ice cover. Interestingly, the authors show that a non-exponential wave attenuation law with the distance has to be expected if a quadratic drag law at the ice-water interface is assumed. Current wave field observations do not allow to discriminate between the widely accepted/assumed exponential wave energy decline against other types of wave attenuation as a result of the large data scatter provided by in situ wave measurements. This means that new technologies should be envisaged to overcome this experimental limit. Authors also show that the attenuation rate is frequency-dependent and the dependence is related to the dispersion relation used. To this end, the authors assumed a wave dispersion relationship which blends shortening (mass loading) and lengthening (elasticity) of the open sea wavelength proportionately to the nature and rheology of sea ice. I support this paper. Some specific comments will be reported below, which I would like to read in the final version of the paper: 1) a discussion to explain the choice of the wave dispersion (eq. 6) could be added. The reason is the presence of the mass loading term. The weak point is that it could not adequately represent the ice floes assumed in the paper in terms of horizontal size/ wavelength ratio. In fact, the mass loading term is considered valid for really point-like ice floes (compared to the wavelength). 2) The relevance of papers like this is the possibility to extrapolate to the real world what learned for the in-door environment, also in simulation. So, to what extent do the authors think their model formulation can represent the complexity of our changing Arctic and Antarctic MIZ?

Thank you for the very positive reception of our paper and for all comments.

1) Speaking of “the mass loading term” is a bit misleading, as this term is present in Eq. (6) in its full form, in other words, both the elastic energy and the inertia effects are included in the thin-elastic plate theory.
   Answering the doubts related to the usage of the dispersion relation (6), we will repeat our arguments from the reply to the comments of Reviewer #1:
   “There were at least two reasons that we decided to use dispersion relation (6).
   The practical one is that all variables it contains (elastic modulus, ice density, etc.) were known from measurements. The usage of another dispersion relation, dependent on the viscous parameter of the ice, would introduce a new unknown to our analysis (direct measurement of the viscous parameter has been shown in the past several decades as a very challenging task; the authors have attempted such measurements in the lab but did not succeed).
   The second reason was consistency between the dispersion relation used and the assumptions underlying the DEM model (individual ice floes, elastic interactions between them).
   It is also worth pointing out that equation (6) was used in the previous analysis of the LS-WICE data by two of the present authors (Cheng et al., JGR, 2018). We do believe that equation (6) well represents LS-WICE observations (see Fig. 2 in part B), and that viscous damping in those experiments was not significant (notably, the ice floes floated in clear water, as opposed to many

observations of wave damping in the MIZ, where the presence of frazil/pancake mixture gives the surface ocean layer high effective viscosity).
In our opinion, an important conclusion from our study is that the wave attenuation *is very sensitive to wave group velocity, and thus to dispersion relation.* We demonstrate it on the example of equation (6) and its two limiting cases (EP and ML) – but the conclusion is more general (we comment on that further in response to comment #9).
We agree that we should add the above explanation to the revised manuscript."

2) Although our model and laboratory experiment are very simple (one-dimensional setup, monochromatic waves, etc.), we believe that several aspects of the results are practically relevant, e.g., the fact that collisions and overwash are likely to be relevant only in a narrow zone close to the ice edge, where they are responsible for very fast attenuation, and that turbulent dissipation due to ice-water drag is likely to dominate further downwave from the ice edge (as the most recent field observations from the Beaufort Sea seem to confirm; see Voermans et al. 2019).
Regarding part B of the study, the conclusion that several different mechanisms might produce similar attenuation rates, so that measuring wave attenuation alone is not sufficient to identify the underlying dissipative processes, is extremely important and practically relevant for both observations and modelling of wave energy attenuation in sea ice.