Dear Reviewer 1,

Thank you for taking the time and effort to read our manuscript and provide feedback. We found your comments very helpful and believe it will improve our manuscript. We have presented our response to the comments below in **blue and bold**. As per TC guidelines, no revised manuscript is prepared yet, however, we have indicated the proposed changes to the manuscript in *italic*.

The fundamental difficulty in this from a theoretical point of view is that the sea state is random with a range of periods. It is therefore difficult to assign to any break-up event a single value for λ unless it is for a wave tank experiment. This point should be discussed.

> **We agree with the reviewer that it is difficult to assign a specific value of λ to the break-up events observed in this study and those reported in literature given that a wave field typically consists of a range of wave periods rather than a single period. Nevertheless, waves in sea ice are generally well-sorted due to the overwhelming dissipation of short waves a short way into the ice pack, such that the wave field is often narrow-banded and thus the peak wave length provides a reasonable choice to define the break-up. To attend the reader to this issue, we will extend the brief existing discussion (at line 324) on this topic with the following:**
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> *"This emphasizes the difficulty in assigning a single characteristic wave length to a break up event for a wave field that is inherently random and consists of a range of length scales. Nevertheless, as short waves dissipate rapidly near the ice edge, the spectrum is often narrow-banded and thus the peak period is likely to be the most representative scale to characterize a break-up event."*

The equations for Young's modulus etc. are essential and summarise literature which is not well known. Multiple authors, mostly those associate with Squire, have used 6GPa for the Youngs modulus, which is an overestimate. However, it should be explained clearly what the units in the formulae are, and the units should be made consistent is possible (e.g. the units of brine volume).

> **This is a very valid point. We will make the units of v_b and S_{ice} consistent throughout the manuscript (fraction instead of ppt).**

The breaking model, in fact, contains two contradictions/paradoxes. One is that in the limit of small thickness ice is unbreakable, and the other is that short-wavelength waves will break any ice. The second point seems to have been missed by the authors. However, the model assumes that the ice is moving compliantly with the sea surface and the wavelengths are so long that the sea ice can be modelled as a negligible surface. Some discussion of this point and the regime in which it is valid would be useful.

> **This is well noted by the reviewer. Indeed, our current definition of I_{br} suggests that capillary waves, for example, would be able to break meters thick sea ice which is, of course, physically near impossible (aside from the fact that short waves won't penetrate far into the ice cover as they fully dissipate/scatter near the ice edge).**
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> **We forgot to specify that Eq. 2 assumes that the ice sheet is thin compared to the wave length (i.e. $h/\lambda \ll 1$) and thus the break-up parameter I_{br} cannot be applied to relatively short waves (i.e. $h/\lambda \gg 1$) as the ice is simply too 'heavy' to be impacted by short waves (and thus the ice will not move compliantly with the ice).**

In what range of h/λ is the proposed threshold of I_{br} valid? We currently have insufficient data to determine this, but the data (see Figure 8) suggest that at least up to $h/\lambda \approx 0.02$ the assumption seems to be valid. More data at higher values of $h/\lambda \approx 0.02$ are required to confirm a more definite regime.

We will add the 'thin plate' assumption to the Introduction. Specifically, we will replace 'an elastic plate' (line 55) by '*a thin elastic plate*'.

We will add the following to the Discussion section:

While the current definition of I_{br} suggests that very short waves always break the ice, it is worth reiterating that the assumption underlying Eq. 2 is that the ice is considered to be thin with respect to the wave length (i.e., $h/\lambda \ll 1$) and elastic (i.e. Eq. 2), implying that the ice moves compliantly with the sea surface. Thus, the threshold of I_{br} defined in this study does not necessarily hold for short waves or, strictly speaking, for $h/\lambda \gg 1$. While the exact range of h/λ for which the observed threshold of I_{br} is valid is uncertain, based on the observations presented here (Figure 8), it seems that it upholds for $h/\lambda < 0.02$. More observations are required to clarify its validity for $h/\lambda = O(0.1-1)$. We note that this is not necessarily a limitation of the parameterization of I_{br} as short waves are, in general, attenuated rapidly when entering the ice cover due to wave energy dissipation and scattering.

The literature review is mostly complete. However, the first coupled attenuation and breaking model appeared in Kohout AL, Meylan MH. An elastic plate model for wave attenuation and ice floe breaking in the marginal ice zone. Journal of Geophysical Research: Oceans. 2008 Sep;113(C9). The authors concluded that their attenuation model was failing because of the overprediction of the break-up.

We thank the reviewer for this reference. We will integrate this reference in the revised manuscript.