# Response to RC2

## 1 General comments

### 1.1

***This paper describes a method for determining ocean wave induced sea ice breakup patterns using the total bending energy, rather than a local maximum strain criterion. The authors simplify the model by assuming quasi-static bending and by only considering a pseudo one-way coupling from the fluid to the ice deformation. (I say pseudo one-way coupling because the authors still use a sea-ice specific dispersion relation and a model for attenuation by sea ice, although the fluid displacement appears in the equations as a forcing term rather than as an unknown.) Of course, simplifications such as these are necessary, but I would have liked to have seen a little more discussion around these modelling decisions in section 2.1.***

You are right that we chose to simplify the model as much as we could to focus on one process: the fracturing and associated temporal evolution of a fracture front. The original paper presented some of the reasoning behind our decisions in the introduction to Sect. 2. This introduction is now transformed into a subsection, slightly expanded, and the introduction section amended to justify our decisions earlier. In particular, a point we had not made clear enough, is that our model is meant to be iterated in time, while a model like that of Mokus and Montiel (2022) can only be iterated between successive steady state, an assumption we meant to relax as we are interested in the progression of a fracture front. Additionally, we added an appendix comparing the results (in terms of curvature and elastic energy) of using the formulation presented here, to that of Mokus and Montiel (2022) (for convenience, as it was developed by the first author and aimed at similar goals). Our results suggest that random fluctuations of the wave state have more influence than changes in ice thickness, significant wave height, or even floe length, when it comes to computing the elastic energy of a deformed floe.

## 1.2

***The results section is built around comparison with the experimental data of Auvity et al. (2025). I would have also liked to see a numerical comparison with the critical strain fracture criterion. For instance, by imposing an incident wave, the proposed energy method and the strain criterion method would lead to different breakup patterns, and I am left wondering what the qualitative differences between these might be. I don't think addressing this point is necessary for publication of this paper, but it would strengthen the current paper or be an interesting question to address in a follow up work.***

We thank you for this valuable comment, as it helped us clarify why we did not include a comparison of the two criteria in our paper. We indeed do not compare simulations with the critical strain fracture criterion here because in the experiments of Auvity et al. (2025), this critical strain does not seem to exist. Another way of saying this is: it does not come up as a material property, but depends on the wave forcing. We therefore could not prescribe such material property in our model for comparison of the two criteria. The absence of a constant critical strain is a salient point of the work of Auvity et al. (2025), and a motivation for our own work (which we alluded to in our introduction). We remind the readers of our choice of not performing a comparison between the two criteria in the current Sect. 3 (l 278) and Sect. 5 (l 478) of the manuscript.

However, you are right in that this comparison needs to be done. We have started work on establishing a mapping between the two criteria, both by making measurements in the lab and by using our numerical model. Ultimately, we want to establish whether choosing one of the two criteria will impact a modelled floe size distribution, which we mention in the closing sentence of this paper. It is still in early stages, and will hopefully be the subject of a later published study.

## 1.3

***With those issues pointed out, I must conclude by saying that the paper addresses an important point in the sea-ice breakup literature with a novel idea. It is very well written and well presented with excellent figures, and I recommend it for publication once the issues raised in this review have been addressed.***

We thank you for these kind words and the time you dedicated to reviewing our paper. Below we address your minor comments.

# 2 Specific comments

***Some more minor issues are listed below:***

## 2.1

***Line 41: be → been***

Corrected.

## 2.2

***It should be noted that equation (1) is Archimedes' principle.***

Thank you, this precision has been added.

## 2.3

***Is the energy release rate $G$ for ice floes/other materials known or easy to measure?***

This parameter can be measured in the laboratory or on the field, either directly, or from the fracture toughness to which it is related through the Young's modulus, for example by three-point bending tests. Our original manuscript alluded to it in the introduction (line 59), where we now make this point clearer. As it usually is the case, the mechanical properties of sea ice are less well constrained than that of other more standard material, or even fresh water ice, and can be expected to depend on temperature and brine volume fraction, and more generally on the history of the material. Timco and Weeks (2010) compiled previous studies of fracture toughness measurements. Wei and Dai (2021) conducted such measurements more recently, at the lab scale, and compared dry and wet samples. We added Section 2.4.4 to inform the reader on the values this parameter (as well as critical strain) can take.

## 2.4

***Figure 2 is very helpful for understanding the fracture process. I would suggest adding a little further discussion about this figure at line 174. For instance: It would be helpful to demystify the algorithmic/procedural steps. E.g. if I understand correctly, for each fracture location, the bending must be computed from (6), before the energies can be calculated.***

We thank you for this suggestion. We added a more detailed description of how the fracture search is implemented, as well as a flowchart illustrating it, at the end of section 2.3.1.

## 2.5

***Is it correct to say (in a simplified sense) that the right fragment energy is generally decreasing in 2a because the right fragment is becoming shorter.***

Yes, it is correct. The situation illustrated in this figure corresponds to the "fully developed" case, which has the analytic solution presented in appendix. The expression of the elastic energy is the sum of fifteen terms (for monochromatic cases) and is not straightforwardly analysed. Intuitively though, it tends to increase with increasing floe length (while oscillating). If wave amplitude is attenuated ($\alpha \neq 0$), it eventually tapers off; otherwise, it keeps growing. For finding fractures, we compute this energy for two hypothetical floes that conserve the length of the original floe. The shorter they are, the smaller their potential energy. If the hypothetical fracture lies close to the left edge of the (original) floe, the left fragment will have a small energy; if it lies close the right edge of the floe, the right fragment will have a small energy. The abscissa of Fig. 2a (original manuscript) being the location of that hypothetical fracture, $x_1$, for increasing $x_1$ the length of the left fragment increases (so does its energy) and the length of the right fragment decreases (so does its energy).

## 2.6

***Line 222: Please define a semi-normal kernel***

The definition is given in Eq. (24) of the original manuscript. We consider a Gaussian function with centre $\mu$ and width $\sigma$; our kernel is defined piecewise, 1 left of $\mu$ and Gaussian right of $\mu$. We changed the formulation to 'semi-Gaussian' in the revised manuscript, as the Gaussian is not normalised in the way a normal density function would be (so that our kernel is continuous and equal to 1 on both sides of the transition at $\mu$).

## 2.7

***When discussing Auvity et al. (2025) in section 3.2, can the authors elaborate on what is meant by the requirement of fracture on***

***nonlinear waves? What kind of nonlinearities are they referring to?***

We mean that in the lab, for fracture to occur, the forcing amplitude has to be high relative to the wavelength. This is typically quantified with the wave slope $ak$, with linear water wave theory classically considered valid for $ak < 0.1$. The wave profiles observed by Auvity et al. (2025) thus depart from sine waves, and more closely resemble triangular waves close to the fracture onsets. Their measured wave slopes are about 0.14, a precision we added to our manuscript.

## References


Auvity, B., L. Duchemin, A. Eddi, and S. Perrard (2025). *Wave induced fracture of a sea ice analog.* DOI: 10.48550/ARXIV.2501.04824. arXiv: 2501.04824 [physics.flu-dyn].

Mokus, N. G. A. and F. Montiel (2022). "Wave-triggered breakup in the marginal ice zone generates lognormal floe size distributions: a simulation study". In: *The Cryosphere* 16.10, pp. 4447–4472. DOI: 10.5194/tc-16-4447-2022.

Timco, G. W. and W. F. Weeks (2010). "A review of the engineering properties of sea ice". In: *Cold Regions Science and Technology* 60.2, pp. 107–129. ISSN: 0165-232X. DOI: https://doi.org/10.1016/j.coldregions.2009.10.003.

Wei, M. and F. Dai (Aug. 2021). "Laboratory-scale mixed-mode I/II fracture tests on columnar saline ice". In: *Theoretical and Applied Fracture Mechanics* 114, p. 102982. ISSN: 0167-8442. DOI: 10.1016/j.tafmec.2021.102982.