

Response to RC3

1 Opening statement

In this manuscript, the authors discuss a novel wave-induced sea ice criterion/fracture model. I think this is a generally interesting and well written study, and I am, in general, supportive of publication. I have a few comments that I would like the authors to consider and I think that the manuscript should be published once these are addressed.

We thank you for taking the time to review our manuscript and for your valuable comments, which we answer point-by-point below.

2 General comments

2.1

Regarding section 2.1: while this is definitely interesting, I wonder how big the effect of solving the ice motion, not just assuming that the ice follows the waves (which I agree is strictly speaking incorrect), is. If I understand correctly, the results from 2.1 are used in all the following? I would be curious to see (either as additional lines in some plots, or as an appendix), a quick analysis/comparison of how much difference there is between the results using the ‘floes following the water’ vs. the ‘floes moving following a balance between buoyancy and flexure’ approximations. Is this a large meaningful difference in ‘standard’ waves in ice swell conditions, or just a minor ‘distraction’?

The formalism we expose in Sect. 2.1 is, indeed, used throughout. However, it should be noted that Sect. 2.2 is largely independent from it. The focus of the current paper is a fracturing process and associated criterion. To inform it, we need the potential energy of the floe, derived from its curvature. In Sect. 2.2,

we merely offer a way to access this curvature through a linear, elastic plate model, in a way that can be time-stepped (a very important feature, as we intent to study with our model time-dependent fracture-related processes such as the propagation of fracture fronts) and is computationally efficient; but any other sensible way to input a curvature in our fracturing criteria could be substituted, after a dedicated implementation. In particular, measured curvatures could be used. We agree that this was not stated clearly enough in our original submission, and we have altered the manuscript to convey our meaning better by expanding Sect. 2.4.2. Additionally, we added an appendix comparing the results (in terms of curvature and elastic energy) of using the formulation presented here (ice not conforming to the sea surface), to that of Mokus and Montiel (2022) (ice conforming to the sea surface, for convenience, as it was developed by the first author and aimed at similar goals). Our results show 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.

2.2

I dont have any major concerns about the results presented from a ‘mathematical’ point of view. However, this field of study has had (in my opinion, but this may be controversial) a history of offering ‘mathematically rigorous’ explanations and models that may have actually turned out to be ‘physically wrong’ because in the real world, a different physical mechanism dominates. Since we are in a branch of applied physics, the ground truth we should compare to is field data, and a model per se, independently of its elegance and mathematical correctness, has no real value unless it explains the real world data better than similar or higher complexity competing models. I understand that the authors compare their results to idealized experiments, but there are so many issues with scaling, ice conditions and formation and structure, etc, that in my experience experiments often have a limited power of proof in this field— for example, the relative scaling between mechanisms and the dominating physics may be different between the field and the laboratory. In this regard, I would like to see more discussion about the following:

2.2.1

Though I understand this may be discussed in the reference provided, I believe that an in-depth discussion of the experimental conditions, ice conditions, etc, from Auvity et al, would be useful, being ‘self critical/self skeptical’ and making it clear what the possible limitations are, would be useful.

We think there is some confusion about the ice conditions in the experiment of Auvity et al. As explained in the beginning of Sect. 3 (Numerical experiment), which outlines the physical experimental setup, and mentioned in the Introduction, Results, and Conclusion sections, the material considered is not ice, but a mechanical analogue of ice (a varnish layer). Again, the goal of the comparison conducted here between the model and experiment is to place ourselves in conditions in which we can focus on and understand one physical mechanism, the fracture of a brittle solid by waves, and evaluate a fracturing criterion. Therefore, even though this material is not ice, the point is that it is a brittle continuous solid, with mechanical parameters that are comparable to ice. It presents the advantage to be solid at room temperature and easier to control experimentally in terms of thickness and homogeneity. Our Table 1 sums up these conditions by providing all necessary numerical values. Comparisons with similar experiments performed on ice are underway.

In terms of the possible limitations of our comparison, we had dedicated Sect. 3.2 to discussing perhaps the biggest one (the linearity of the waves) and discussed a second (layering of the varnish) in the Discussion section. We kindly ask you to refer to Auvity et al. (2025) for details not present in our study, as our goal was to present the necessary information while keep the length of our paper reasonable.

Again, we specifically chose this experiment for a basis of comparison because it is simple (it does not entail all of the variations in sea ice formation, conditions, etc. that an in situ experiment could), and therefore it allows us to isolate one physical (not mathematical) effect, that of brittle fracture. We hope our revised manuscript communicates these better.

2.2.2

Can you present simple scaling analysis between your experimental data and typical field conditions, focusing on non dimensional groups that are relevant/appear in your model? Do this seem to scale the same (in which case, one can reasonably hope that the present model may be transferable to real world field data if there

are no surprises (other physics) happening), or do these have large mismatches (in which case, there would still be a significant burden of proof)? Compiling all of this in a discussion and dedicated table would be useful.

Thank you for this interesting comment. We indeed spent quite a lot of time reflecting on non-dimensional quantities. The one quantity we have identified, is the dimensionless wavenumber kL_D . We use it as an explanatory (that is, independent) variable; it does not, in itself, control breakup but emerges naturally from the bending ODE. It is noteworthy that this quantity seems to exist on a very narrow range: Auvity et al. (2025) obtained fracture for $0.05 < kL_D < 0.9$ for their material.

We mention in our discussion that for experience on fresh water ice, Dolatshah et al. (2018) obtained fracture for $kL_D \approx 1$. We cast doubt on measurements of Young’s modulus for other experiments made in the laboratory, on saline (Herman 2018) or model (Passerotti et al. 2022) ice. Nevertheless, in the former case, they obtain fracture in the range $[0.29, 0.54]$; in the latter case, for $kL_D = 0.29$. Voermans et al. (2020) compiled a list of studies of wave-induced breakup observations. Mechanical parameters were, for the most part, not measured, but they suggest estimations based on known empirical relations. Following their methods, we can generate ensembles of dimensionless wavenumbers. We show these in Fig. 1. For a variety of ice and wave conditions, it appears that fracture happens for $0.017 < kL_D < 3.1$.

Unfortunately, the other dimensionless quantities we do compare to kL_D in our paper (Fig. 9 of the original manuscript) are based on a quantity (energy relaxation length) we do not have field measurements for. The dimensionless threshold proposed by Voermans et al. is based on the existence of a critical strain, for which we have no evidence for when it comes to the experimental results of Auvity et al. (2025), our or own numerical results.

2.2.3

As discussed above, at the end of the day, field observations and field data are the ‘ultimate arbiter’ of what is correct or not/happening in real life or not/a good model or not (the classical ‘all models are wrong, but some are useful’). I think this is maybe the biggest ‘criticism’ I have at present—if I understand correctly, this work is based on a mathematical model (and while I agree that the mathematics seem correct, I think it is fair as discussed above to ask if this is really what happens in real life) and a single, very ‘special case’/‘idealized’, experiment. I believe that to really be convinced of the applicability of the results of this paper, beyond ‘just’ being

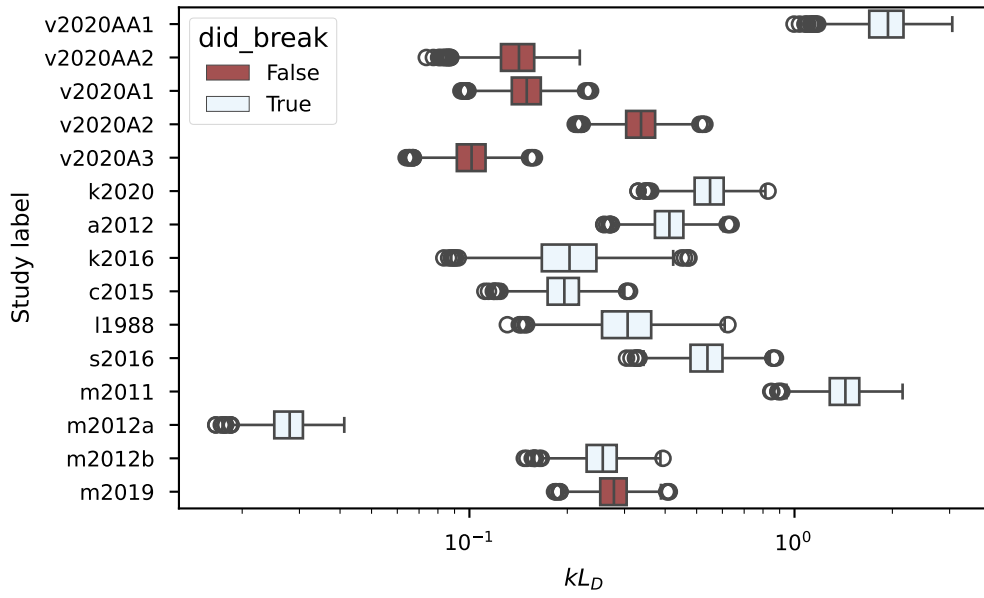


Figure 1: Distribution of dimensionless wavenumbers, determined from estimating wave and ice properties following Voermans et al. (2020), and sampling from the triangular distributions they suggest. The boxplots are colour-coded based on whether the study observed breakup or not for these properties. The whiskers are defined as 1.5 times the interquartile range in log units.

a neat mathematical exercise, I would need to see a comparison to field data. I see two possibilities here: either 1 use existing already processed field data, directly from some of the references provided (for example Voermans et al that is referred there and may have been relying on open data/provide enough data to ensure reproducibility of the results, or some of the other references), or 2 perform your own such comparison with your own methodology from scratch based on data you have gathered, or that are publicly available. However, I understand that this is possibly a significant amount of work (maybe not for 1 if there is a smart way to reuse the data analysis previously performed, but definitely with 2), so I don't really feel that I can 'require' the authors to do so in this paper. Still, I think that the authors should either go through the (possibly significant amount of work) task of doing such a comparison, or if not, at least have a very clear discussion about the fact that there is still a significant 'burden of proof' on the present method to demonstrate its applicability to real world data, putting more weight on the possible limitations of the present model, and suggesting how to test this model.

It seems we might have different points of view on what is mathematical and physical. We consider that our model is 'mathematical' in the sense that it is a closed set of equations programmed into a computer. But we try to approach a physical problem, with a 'physical' parametrisation of the breakup process based on Griffith's fracture theory and a 'physical' constitutive relation. Yes, at the end of the day, the model is 'wrong' but still might be useful. For us, the most logical thing to do to 'try' it was to compare it to a laboratory experiment also designed to study in isolation a single process, that of brittle fracture of an elastic solid by waves. The laboratory setting has the advantage of being a much more controlled environment; the observation of wave-induced ice breakup being serendipitous in nature, and uncertainty on field measurements being what they are.

We would like to point out Voermans et al. did not provide processed data, but provided their buoys data. Most of the data, in particular pertaining to the ice properties, necessary to compute their fracture threshold was estimated from empirical relations. Additionally, their breakup criterion can be rewritten as the scaled ratio of the maximum curvature undergone by the ice to a critical curvature; a quantity that, again, we do not have access to in the case of the analogue material, for we believe it does not 'exist', that is, it is a function of the wave forcing.

Finally, we are in the process of evaluating our model on data we partici-

pated in acquiring (Kuchly et al. 2025). We have amended our discussion to highlight the present study is (beyond the presentation of the model itself) a first step towards validating our formalism, and what our intentions are for the future.

2.2.4

The authors discuss quite a bit previous, existing parameterization methods in the introduction. I would like to see this thread picked up more in the results and discussion, and ideally a comparison of both the mathematical behavior of these pre existing parameterizations vs. your present model (typical scaling—is it the same? different? in scaling itself, or prefactors?), and possibly theory prediction power comparisons. Are you predictions significantly different from previous parameterizations? If not, what is the added value of your model? If yes, given that in particular the ‘empirical criterion based’ methods seem to do a reasonable work at fitting observations with ad hoc tuning, do you trust that your model is right and previous parameterizations fitted on field data are wrong / how do you do better with your present model than the previous fitted parameterizations?

One limitation of the previous method we discuss in our paper, the maximum strain criterion, is that it requires an extra parametrisation. We mention this point in our introduction. Our approach does not require this extra parametrisation, which we consider a plus, and is grounded in fracture mechanics. We believe however that the ‘good’ approach would be to identify what quantities are actual material properties (intensive physical properties, which do not depend on the geometry or the size of a floe), with eventual dependencies to environmental factors such as temperature or brine fraction, but not to the kind of forcing (typically, in a linear regime and ignoring fatigue, we expect no dependency to the wavenumber). The Young’s modulus, the energy release rate should be among these. The critical strain does not seem to be one, at least when studying a physical analogue, which was chosen because it also behaves as a brittle elastic solid. As mentioned in our response to your previous comment, experiments on ice are ongoing and will be used to determine whether the absence of a constant critical strain can be assumed for ice, as well. We explicitly added this mention to our discussion section.

In a sense, we can consider that our integrated, energy fracture criterion is more general than a parametrisation based on maximum strain. It may be however that the two are equivalent in some conditions and with respect

to some metrics such as the size distribution of fragments, or the speed of the fracture front; evaluating this would require some sort of mapping between fracture energy and critical strain (the values parametrising either formulation). It is likely this mapping will also involve the forcing wave, in some fashion. We are in the process of deriving such a mapping (which we alluded to in the closing paragraph of our manuscript) and conducting these experiments, the results of which will hopefully be part of a future publication. If we establish the parametrisations are equivalent, of course, it will be sensible to stick to the simplest, or ‘numerically cheapest’, one. Our postulate is that for now, we do not know, but we note that a constant strain threshold independent of wave conditions does not explain existing experimental results or our own results.

Finally, the other advantage of our model, that we had not made explicitly clear in our original submission, is that it can be stepped in time in a rational way, which allows for not only looking at the final state of a breakup event (for example, Mokus and Montiel 2022), but at the transient evolution of a breakup front: its speed, sizes of initial fragments, secondary fracture, etc. We hope we have made this point clearer in our revised manuscript.

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