Response to RC1

1 Opening statement

SWIIFT v0.10 presents an algorithm to predict wave-induced breakup of sea ice floes. Contrary to the most widespread assumption of a critical strain for breakup, the authors implement a different physical mechanism based on energy. I do feel that the model is appropriately described and assumptions are physically justified making it very valuable contribution to the active field of research on waves and sea ice, however some statements need to be recalibrated in view of recent literature, which in parts is omitted, and to avoid overstating the value of the present contribution.

We thank you for your thorough review of our manuscript and this positive foreword. The purpose of our paper is to present an alternative modelling approach to that currently existing and applied in floe-resolving models, as well as to perform a first validation of this approach based on a comparison to experimental data. In particular, our goal is not to invalidate existing frameworks. Additionally, our focus is on a single process: the mechanical fracture of sea ice in a brittle, cohesive solid state under wave action. We purposely exclude thermodynamics and failure of ice under other states and processes from our study. Thanks to your useful review, we have become aware that this scope and more precise objectives were not emphasised enough in our introduction and we have now stated it more clearly. We provide detailed responses to your individual comments hereafter.

2 Major comments

2.1

The authors focus on wave induced breakup. This is one of the possible mechanisms leading to the formation of the MIZ but not the only one, and this should be made clearer in the abstract and

introduction. For example, internal stresses can be induced by wind and current forcing, and the weakening of the ice cover that promotes breakup to thermodynamic effects (e.g. melting). Moreover, to my understanding, the paper focuses on the condition in which the floes are comparable to the wavelength. While I appreciate that in this condition waves 'build' the MIZ via breakup, this is only true in particular seasons and locations. The authors overlook the formation of the MIZ via for example the pancake ice cycle (in which floes much smaller than the wavelength) and is linked both to the agitation induced by the waves (mechanical process) and thermodynamic freezing.

In the abstract (first sentence), we state that 'The wave-induced breakup of sea ice *contributes* to the formation of the marginal ice zone in the polar oceans' (emphasis added), suggesting we do not assert wave-induced breakup is the only contributor to the MIZ. Early in our introduction, we list wind and ocean currents as mechanisms of fragmentation.

It is true that the focus of our introduction then shifts to wave-related matters, as the purpose of our paper is to present a method for parametrising the flexural failure of cohesive, solid sea ice under wave action. A mechanism, as you say, that contributes to building the MIZ, and which produces ice floes of sizes comparable to (but largely smaller than) the wavelength, starting from larger, unbroken floes. We now emphasise this point in the revised version of our introduction. We also explicitly state that our model is purely mechanical and purposely does not incorporate any thermodynamics—the omission of that clarification was an oversight on our part—and that, as a consequence, we do not represent any kind of ice formation, melt, or disintegration.

We also now make an explicit distinction in the introduction between the Marginal Ice Zone (MIZ), qualitatively defined as the area influenced by wave action, and the Seasonal Ice Zone (SIZ, see for example Roach et al. (2025)), qualitatively defined by opposition to perennial ice. The floe size distribution (FSD) can also be studied at the scale of the SIZ, and we focus here solely on one process: wave-induced breakup.

2.2

One of the claims, as highlighted in the abstract, is that maximum strain might not be the dominant mechanism. While the energetic criterion proposed might be physically sound, a more throughout comparison with different breaking modes as discussed in a re-

cent paper by Saddier et al (`https://journals.aps.org/prfluids/abstract/10.1103/PhysRevFluids.9.094302?ft=1`) should have been considered. Moreover, the calling in the question the maximum strain criterion is not completely novel. For example, in Passerotti et al, that the authors discuss, it was already shown that existing criteria do not match experimental observations.

We thank you for your comment but respectfully disagree on two aspects. First, we did not claim in our paper that maximum strain is not the dominant mechanism for flexural wave breaking. Rather, we presented a common paradigm (breaking parametrised with a strain threshold criterion) to introduce an alternative paradigm (breaking parametrised with an energy criterion). We take note to emphasise in the introduction that maximum strain is not a mechanism, but a criterion that can be used to parametrise a mechanism: wave-induced fracture. We actually gave examples of the strain parametrisations agreeing with observations and being used in models of various scales (line 45 of our original manuscript). In any case, both the fracture criteria we discuss are built around bending strain: the strain threshold method is a local comparison, and the energy method integrates its variation along the plate.

Second, we did not claim that calling the maximum strain criterion into question was novel: the results we present in this study draw heavily from the work of Auvity et al., under review for PRL, which concluded that this criterion was inconsistent with their experimental results. This is clearly stated from line 49 of our manuscript. Passerotti et al. (2022) compared their experimental results to the so-called universal criterion proposed by Voermans et al. (2020), and found the match not to be perfect, with some fractures observed below the threshold and some absence of fracture beyond the threshold. They were careful to frame this finding in the appropriate context of their physical setup, but did not discuss the possible effect of material fatigue: they use a single ice sheet across their experiments. The criterion of Voermans et al. (2020), however, already is a blend of wave properties and ice properties, and not just the value of a strain threshold, and they do not discuss whether considering a strain threshold in isolation is appropriate or not. However, we did look at the literature again, and could not find work clearly calling into question the maximum strain criterion framework.

Saddier et al. (2024) suggested the fracture they observed came from viscous stress and acknowledged this is not the mechanism that leads to the fracture of ice floe by waves. Their material is held together by capillarity, and is much thinner than the viscous boundary layer associated with their flow. Auvity et al. (2025) identified the failure of their material to be caused by wave-induced bending, as it is the case for ice floes. Therefore, we focus exclusively on mode I for the simple reason that our geometry is one-dimensional: it does not allow for representing anything else.

2.3

The authors make a thorough comparison to the experiments of Auvity, a preprint. The experiments are done for a standing wave, which is an unlikely condition to be observed in the ocean where waves are likely to propagate from the open ocean towards the sea ice. I wonder why a greater effort has not been made to make a comparison to laboratory experiments of Passerotti that the author mentions (noting that these encompass a more complex random sea state). Moreover, striking is the absence in their work of mention to the work of Saddier et al that, in my view, closely resembles the one of Auvity, albeit with few notable differences (e.g. propagating waves vs standing waves, and also random waves). In addition, I feel that the authors oversell the model agreement with the experiments (Fig 8).

In this line of thoughts, simplicity here is sought. We referenced the experiment of Passerotti et al. (2022) (line 520 in the original manuscript) and explained why it was not considered for comparison. We aimed to look at the smallest interesting problem involving fracturing, since we seek to validate the alternative energetic approach to fracturing implemented in our model; hence monochromatic forcing, no attenuation, and standing waves. We now make this point clearer in the introduction. As described in the paper, the model, in its present state, can do more complex things. As you pointed it out in your next comment, it would be relevant to demonstrate this capacity: we therefore now include a propagative example in an additional section.

However, the point of this paper is, first, to introduce the software and the bases it rests upon and, second, to make sure that it is relevant and its results sensible in a simple case. One of our conclusion, as stated in the paper, is that even in this simplest case, the results are not straightforward to interpret.

Studying standing waves makes it possible to calculate a threshold amplitude, a physical quantity measured by experimenters, allowing for direct comparison. We acknowledge that the threshold amplitude of a transient forcing could be different, as reported by Saddier et al. (2024). The presented setup allows for deriving results confirming that our breakup criterion, if not perfect, is not completely off either: and we think the comparison to Auvity et al. (2025) is the most relevant for doing so. We do not think we oversold the agreement: for example, text line 411–414, line 482–486, we insisted on having an agreement in terms of order of magnitude. We also do not tune the model parameters to obtain this agreement, an information that, as you point out in your later comment, was not made clear enough in the original version of our manuscript.

Even though similar in principle, the main difference between the works of Saddier et al. (2024) and Auvity et al. (2025) is that in the former case, the material floats because of capillarity and breaks because of viscous stress; in the latter cases, it floats because of buoyancy, and breaks under bending stress, a situation much more similar to what happens to ice floes, and that our model tries to emulate. Not mentioning this work was, however, clearly an oversight on our part, and we amended the introduction of our Section 3 to fix it.

2.4

As a further suggestion, I believe that a working example with propagating ocean waves and a random sea state could be added to the manuscript and it would strengthen the paper.

We thank you for the suggestion. We added Section 2.4.5 to present such an example, illustrated in Fig. 1.

3 Additional comments

Additional detailed comments are listed below.

3.1

In their modelling paradigm, the energy release rate G is introduced. Can the authors please explain and or suggest how its value can be evaluated in the field and lab experiments. Otherwise, this remains as a fitting parameter.

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

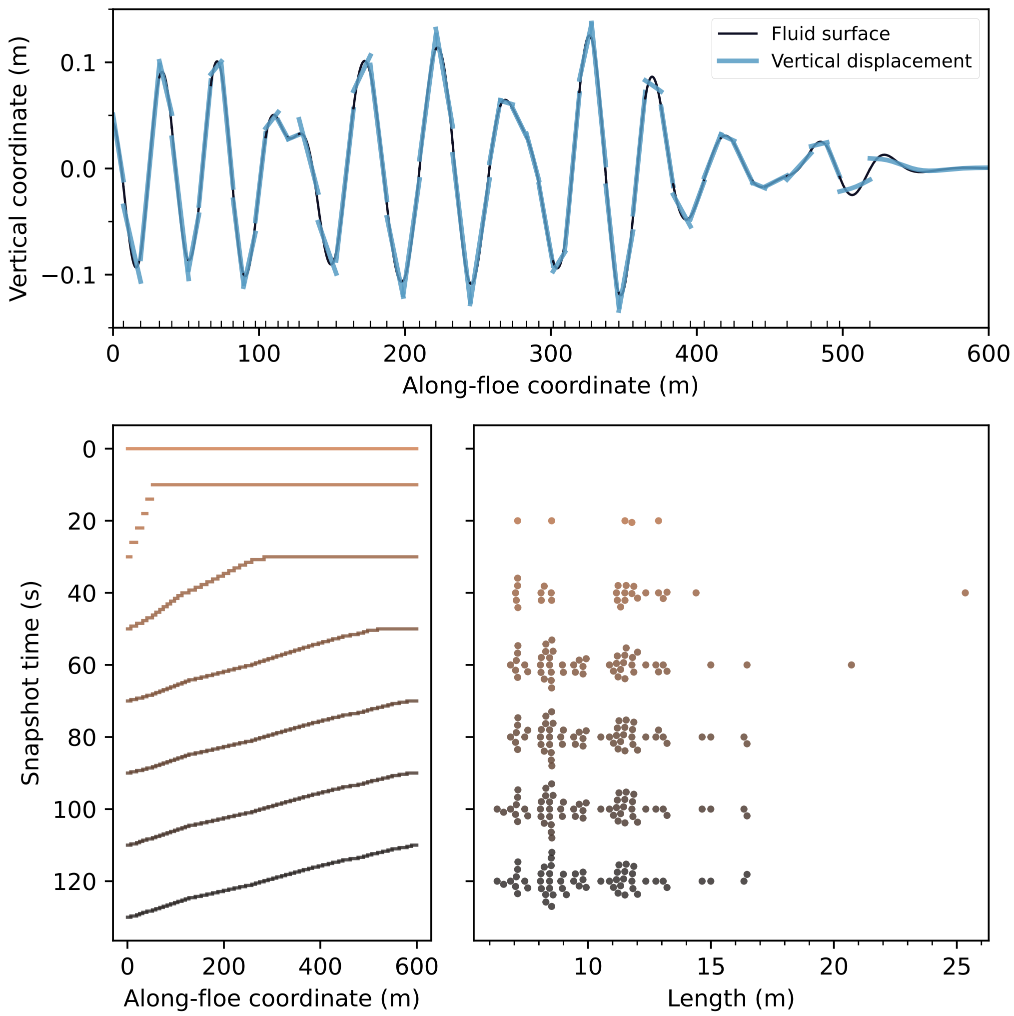

Figure 1: Snapshots of a fracture experiment. Top panel, view of the domain at $t = 60\,\mathrm{s}$. The continuous, dark line represents the fluid surface ($\eta(x)$), and the discontinuous, lighter lines the vertical displacements ($w(x)$) of individual floes. The marks along the bottom spine indicate the boundaries between fragments; about 80 m at the right of the domain have not yet been affected by the waves. Note that the vertical scale is greatly exaggerated: the aspect ratio of the graph, in physical units, is 5×10^{-4}. Because of the thickness of the lines, some floes appear to overlap, they actually do not. Bottom left panel, horizontal bars show the extent of individual floes. The height of the bars indicates the order of the floe in the array, and each group of bars, or "stair", corresponds to a snapshot. The time of the snapshots are indicated on the y-axis, and darker colours correspond to later times. Bottom right panel, we show size distributions as swarmplots, omitting the rightmost fragment. Each dot corresponds to a length as indicated by the x-axis, and within a group, the y-axis only serves to separate dots. Vertical clusters thus indicate a concentration of observations around the corresponding length. From $t = 0\,\mathrm{s}$ to $120\,\mathrm{s}$, there are respectively 1, 6, 27, 52, 58, 59 and 60 fragments.

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.

When it comes to the results we present in Section 4, ice is not the material under consideration, and estimation of the energy release rate was done by Auvity et al. (2025). We use this estimate to parametrise all our presented results; that is, we do not tune it to adjust our results.

3.2

The numerical experiments are done with a brittle layer of varnish (L268), I wonder if the hypothesis of elastic plate applies to a material that the authors define brittle.

In their manuscript, Auvity et al. (2025) do establish the material behaves as a solid. We oppose brittle to ductile, not to elastic; that is, the material can deform, but will fracture before exhibiting significant plastic (irreversible) deformation.

3.3

2.1 there are a couple of hypotheses in the modelling framework that, in my opinion, should be better highlighted. The plate is elastic (also the coefficients are those for a quasi-static model) and the ice does not drift.

We thank you for your comment. Indeed, we consider an elastic material: no time-dependent energy dissipation occurs, other than released by fracture. That is, the material is not viscous. Dissipation is parametrised in space.

We modified the manuscript to emphasise the elasticity hypothesis (second paragraph of Section 2.1) and the fact we do not consider drift (new paragraph at the end of Section 2.1).

3.4

2.3.2 the attenuation is parameterized as in Sutherland (eq 20). Can the author better justify this modelling choice and explain

why other approaches have not been considered. For example an emerging trend is the ones in DeSanti et al and Yu et al (`https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC013865; https://www.sciencedirect.com/science/article/pii/S0165232X2200101X`). Can the author please explain/comment on how different attenuation might affect their results.

This attenuation scheme was chosen simply because one of the authors was familiar with it. In fact, in its present state (v0.10), the model can accept any user-defined function as an attenuation parameterisation, as long as it relies on the quantity numerically represented (including, but not limited to, ice thickness or wavenumber). This capability was mentioned in the original manuscript, line 214. Moreover, the code is flexible enough that other parametrisations can be offered permanently. We modified our manuscript with the hope to make this clearer.

Yu et al. (2022) relates a nondimensionalised attenuation to a nondimensionalised angular frequency, so that

$$\alpha h = 0.108\left(\omega\sqrt{\frac{h}{g}}\right)^{4.46} \tag{1}$$

or equivalently,

$$\alpha = 0.108\omega^{4.46}h^{1.23}g^{-2.23}. \tag{2}$$

This is close to the parametrisation we suggested, which is approximately (when making a deep water, free surface substitution for the wavenumber) $\alpha = \frac{1}{4}h\omega^4 g^{-1}$, albeit with a smaller prefactor. We added the parametrisation from Yu et al. (2022) to SWIIFT v0.16.

The approach of De Santi et al. (2018) is focused on grease ice and pancake ice, while we consider discrete floes, long enough to be susceptible to fail from bending. Additionally, the models they consider depend at the very least on the viscosity of the ice, and eventually on the viscosity of the fluid and a pancake fraction parameter. These are in opposition with our current formulation, purely elastic ice and inviscid fluid. While the model would be amendable to accommodate these, we do not deem it to be a priority. It is, however, free and open source, and contributions are welcome.

As to the eventual impact of the selection of an attenuation scheme on the results, we can for now simply say that higher attenuation would lead to a smaller extent of fracture. Additionally, we would like to again attract the attention of the readers on the fact that the results presented in Section 4 are issued of simulations where attenuation was turned off.

3.5

2.3.3 I do not understand the opening statement. This is reinforced by the choice of the authors of choosing a wave expressed as a variable of the x, whereas in ocean wave applications the more common approach is to provide a time series at the edge of the domain and let it evolve along the x coordinate.

We do not fully understand this comment, or what is the difference you mean to convey between ‘a wave expressed as a variable of the x’ and ‘a time series at the edge of the domain [...] evolve along the x coordinate’. Dropping attenuation and superposition in the interest of simplicity, we do express waves as $\eta(x,t) = a\sin(kx - \omega t + \phi_0)$, so that the surface of the fluid at $x = 0, t = 0$ sits at $a\sin(\phi_0)$. As we consider a succession of quasi-static states, we eliminate the explicit time dependency by aggregating it into the phase, so that $\eta_n(x) := \eta(x, t = t_n) = a\sin(kx + \phi_n)$, with $\phi_n = \phi_0 - \omega t_n$. In the original manuscript, this is detailed from Section 2.3.2 to Section 2.4.1, inclusive. Additionally, to ease further computations, $x = 0$ can be chosen relatively to any floe of the domain. Thus, what we do is, in a way, precisely providing a time series at the edge of the domain (in a matter of fact, the edge of any floe) and letting it evolve along the x coordinate (along the considered floe). The time information is simply encapsulated into the phase of the complex amplitude.

In Section 2.3.3, we explicit how we modify η to allow the fluid surface to transition from a rest state (in the vicinity of the ice cover) to a ‘wavy’ state, in order to be able to simulate the progression of a fracture front. Equations (23) and (24) of the original manuscript are the translation of this opening sentence in mathematical terms. What we do here is simply providing a Gaussian envelope to our plane wave to locally reduce its amplitude. However, we are not interested in the progression of a single wave packet, so we only impose this envelope in a half-plane, allowing for the transition from rest to ‘fully developed’ sea in a continuous and regular manner.

3.6

3.2 The authors make the assumption of linearity. There is no discussion on the possible effect of capillarity. In the wave regime explored in the paper (small wavelength) capillarity effect might affect the wave dispersion relation.

Auvity et al. (2025) establish in their manuscript that in the case of their material, elastic effects completely dominate. They do so experimentally, and it can be understood from the flexural length to capillary length ratio (about three), and their respective powers in the dispersion relation. This applies for all wavenumbers, and can be easily verified analytically. On the contrary, in the experimental conditions of Saddier et al. (2024), the flexural length to capillary length ratio is about 0.013.

As explained in Section 2.3.1, we use the gravity–mass-loading–elastic dispersion relation for our ice-covered regions. It can be extended with a compression term (Liu and Mollo-Christensen 1988), a term analogous to surface tension for a fluid–fluid interface. This term is, however, poorly constrained (Collins et al. 2017) and can be minor (Sutherland and Dumont 2018). In the case of sea ice, it is likely that the 'appropriate' dispersion relation depends on the type of ice considered, and its spatial scale, or the spatial scale of the floes.

We added a paragraph to Section 3.1, to make clear why we do not consider capillarity.

3.7

Fig 4 the kL axis only spans one order of magnitude and I wonder if the log scale is really needed. Moreover, in the discussion the authors state that they only look at the plate between 0:L/2 because of symmetries. When a breakup occurs how do the authors make sure that this is in the first half of the plate and not in the second half? Is there a reason to believe that the floe breaks synchronously at two points (one in 0:L/2 and one in L/2:L) therefore forming 3 smaller floes.

Even though kL only spans one order of magnitude, we want to expose a power relationship in panel (b), which is more easily done by using a log–log scale.

In the text (line 341 of the original manuscript), we explain that the free energy profile is symmetric. Therefore, if we consider binary fracture, there exists two identical free energy minima (or a single one in the exact middle of the plate). Either one can be chosen as the 'true' fracture location, as what we quantify here is the amplitude of fracture onset. They cannot be distinguished and the only reason our minimisation step would consider one over the other, is numerical fluctuations on the order of floating point precision. We simply restrict our analysis by showing, in Figure 4a (original manuscript), fracture locations constrained to $[0, \frac{L}{2}]$. For completeness, we could have added points symmetrical to those represented with respect to $y = \frac{1}{2}$, but it would have made reading the graph harder without adding any information.

In their experiment, Auvity et al. (2025) observed fracture on wave crests one at a time. As their goal is to identify the threshold amplitude leading to fracture, the experiments were stopped as soon as a first fracture had appeared. In the time interval necessary for stopping the experiment, secondary fractures usually appeared, not unlike what Saddier et al. (2024) observed.

3.8

L21 I feel that in addition to the reference to Auclair there is observational evidence showing that the marginal ice zone affected by waves is close to free drift regime and therefore substantially different from the interior. Addition of appropriate references would strengthen the statement. Moreover, in addition to reference to Thomson, I suggest adding the recent work by Toyota et al (https://www.sciencedirect.com/science/article/pii/S1873965225000520).

We thank you for this suggestion. We expanded this paragraph of our introduction with references to recent observations of ice motion, and the suggested reference to the study of Toyota et al. (2025), which was not published when we submitted our manuscript.

3.9

L35 I find this sentence unclear.

Our response here refers to Figure 2 of the original manuscript. The shaded areas represent intervals, over the floe lengths, where the critical strain is exceeded, for the typically considered $\varepsilon_{\mathrm{cr}} = 3 \times 10^{-5}$. These cover almost the entire span of the floe, except for some small regions around edges and curvature nodes. Therefore, simply looking for where the critical strain is reached is not sufficient, one also has to devise a way to select where to break the floe within these intervals. The two more obvious options are to fracture at the first point where the threshold is reached, or to choose the global or a local extremum. We choose the latter, which is explicitly stated in Section 2.2.2. Other methods could be suggested. Horvat and Tziperman (2015), for example, chose not to compute the contiguous strain along a floe, but a local approximation, considering only successive extrema of the sea surface realisation (their analogue to our vertical displacement; see their supplementary material). They did so to address that very same limitation of strain-derived fracture parameterisation. To quote these authors: ‘If the strain is calculated locally from $\eta(x)$, the critical strain is reached almost everywhere for a realistically generated wave field (see the Supplement, Fig. S10)'.

3.10

L137 for the readership benefit, can the author state what it means unstretchable.

Thank you for your comment. We mean that the plate does not undergo any in-plane deformation. We added a clarification to the manuscript.

3.11

L255 can the value of Y and nu be explicitly specified?

The values reported in the cited study are $Y = 3.8\,\mathrm{GPa}$ and $\nu = 0.33$. However, we used $Y = 6\,\mathrm{GPa}$ and $\nu = 0.3$ to compute the values presented in our manuscript.

We added a clarification on the values used, and updated the results of our calculation to use these. We insist on the fact that this calibration is illustrative, and will need to be adjusted depending on the configuration of a model run.

3.12

L264 the relationship for polychromatic cases should be explicitly stated for clarity.

We made this addition to the manuscript.

3.13

L420 can the author better clarify why the definition of the relaxation length differs from Auvity. Can the two be reconciled?

The clarification is given in the following paragraph. The definition of Auvity et al. is based on the full width at half maximum. It presupposes the location of fracture is were the curvature maximum was before fracture happened, which is expected for bending failure. However, it would not be compatible with the behaviour we observe in region 4, were fracture happens away from deflection and curvature crests.

3.14

L515 the example does not refer to "typical field conditions" as this is a transient ship wake and not a MIZ formed by open ocean waves.

We replaced 'typical field conditions' by 'field scale', which is what we meant, by opposition to lab scale.

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