1 General comments

The manuscript „Wave-triggered breakup in the marginal ice zone…" by Nicolas Mokus and Fabien Montiel describes a numerical study of wave propagation in sea ice and wave-induced sea ice breaking. The main focus of the paper are the properties of floe size distributions (FSDs) resulting from breaking of ice with different properties (strength, thickness) by waves of different periods and amplitudes.

Undoubtedly, the problems discussed in the study are important for the current research on sea ice–wave interactions. Our better understanding of the physical mechanisms underlying wave-induced sea ice breaking is crucial for developing better parameterizations of those processes for large-scale sea ice and climate models. Although I find the manuscript and the results interesting and valuable, and the model developed by the Authors well presented, I have some doubts, described below, regarding some parts of the analysis. I recommend the manuscript for publication in The Cryosphere after a major revision.

We thank the reviewer for their positive comments and suggestions, which are addressed below.

2 Major comments

2.1

The main point in my critics is related to the procedure described in Section 5.1: the whole algorithm is based on an assumption that the FSD resulting from sea ice breaking on irregular waves is the 'weighted average of distributions resulting from monochromatic model runs'. Why?

I really can't see the reason why it should be so simple.

Let's consider a very simple example of a wave field composed of two monochromatic waves with very different wavelengths, and let's assume that wave #1 does break the ice and produces very small floes, and wave #2 is very long and doesn't break the ice at all (or produces very large floes). The ice sheet in that case would break into small floes, corresponding to that resulting from wave #1 anyway, so computing FSD from a weighted average would produce truly weird results!

It's the part of the spectrum that leads to breakup that's important, not the whole spectrum!

As the Authors rightfully demonstrate in their manuscript for monochromatic waves, the relationships between floe size, ice properties, and wave length are quite complex and nonlinear, so there is no reason why the FSD resulting from a wave energy spectrum should behave as the Authors assume.

This is a valid concern. Obviously, this is a model, hence a simplification of reality. We will make clearer in our manuscript that our approach in Sect. 5 is indeed heavily assumption-dependent. The underlying assumption is that waves, carrying the whole energy of the spectrum at discrete time periods, would act independently to break the ice. The resulting distribution would be the average of the distributions generated by these independent periods, with weights related to the spectrum, in coherence with linear theory. In reality, waves of different periods would interact with one another, but our model cannot represent these interactions.

I have the impression that the shapes of FSDs in Fig. 6 to a large degree simply reflect the shape of the wave frequency spectrum, and that this is an artefact of the algorithm (or, more precisely, its part related to the computation of weighted averages).

In my opinion, it is a very weak part of the analysis, but the Authors don't even discuss those weaknesses.

Of course, as I have serious objections regarding the above-mentioned assumption, I have also doubts regarding the results presented in sections 5.2–5.4 of the manuscript.

We ran comparisons using alternative weighting functions, represented on Figure 1. Descriptions of these functions can be found in Table 1.

The lognormal density function has, qualitatively, a shape similar to the Pierson-Moskowitz spectrum when displayed as as a function of frequency. However, we obtain similar, skewed unimodal densities with a range of symmetrical weighting functions, as displayed on Figure 2.

The one case that stands out, with several secondary peaks, is n_md. It corresponds to a function giving more weights to high frequency waves (5 to 15 m), which is unrealistic in the context of our model.

Why can't the model be forced by a superposition of monochromatic waves? The scattering model is linear, isn't it, so it shouldn't be difficult. All one needs to do is to add up the wave solutions for individual spectral components (assuming random phases) and use those to compute strain (as, e.g., in section 6 of Kohout and Meylan (2008)).

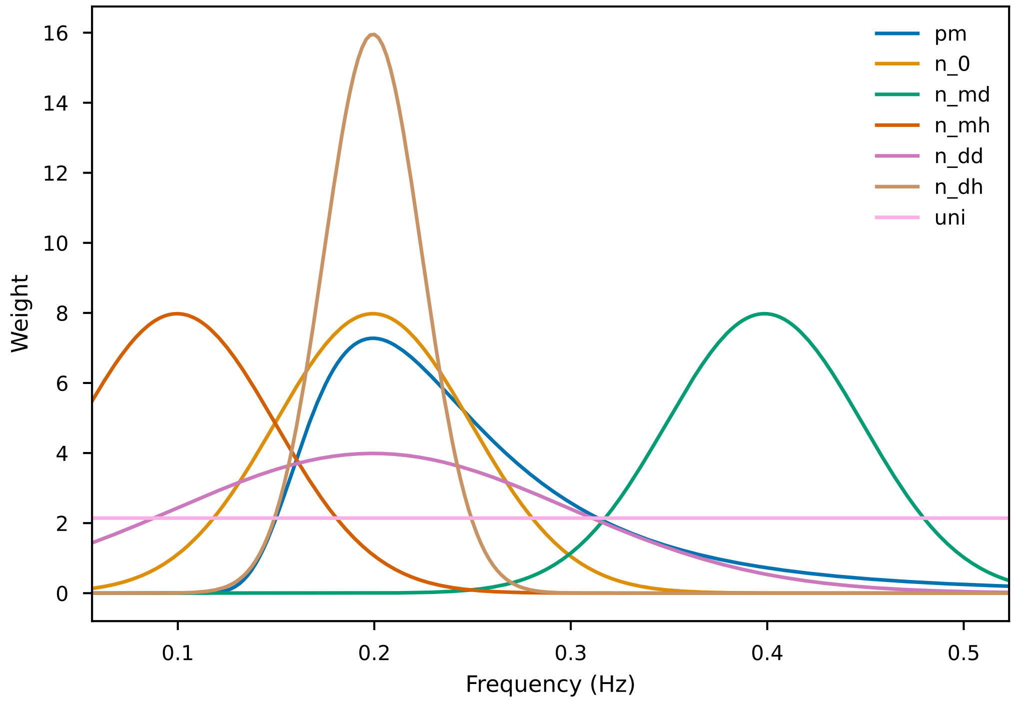

Figure 1: Representation of functions used as alternative weights. They are all normalised on the positive real half-line; because of the finite range of frequencies supported by our model, truncations imply some of them integrate to less than unity. Description of the legend entries can be found in Table 1.

We implemented the approach suggested by the referee (Mokus and Montiel 2022, conference proceedings). The results are different (Figure 3), which is not an argument in favour of either of the methods. The later approach has limitations has well, as it assumes steady state to be reached by waves of all periods at the same time, even though longer waves propagate faster. These models would need experimental validation for additional support.

We will insist on the assumptions and underlying limitations in the revised manuscript, and mention the alternative parametrisation (strain superposition) in the discussion.

2.2

Are the FSDs obtained for monochromatic waves lognormal as well?

Why is that pdf introduced first in Section 5.2 and not earlier? That would allow comparisons between FSDs obtained for regular and irregular

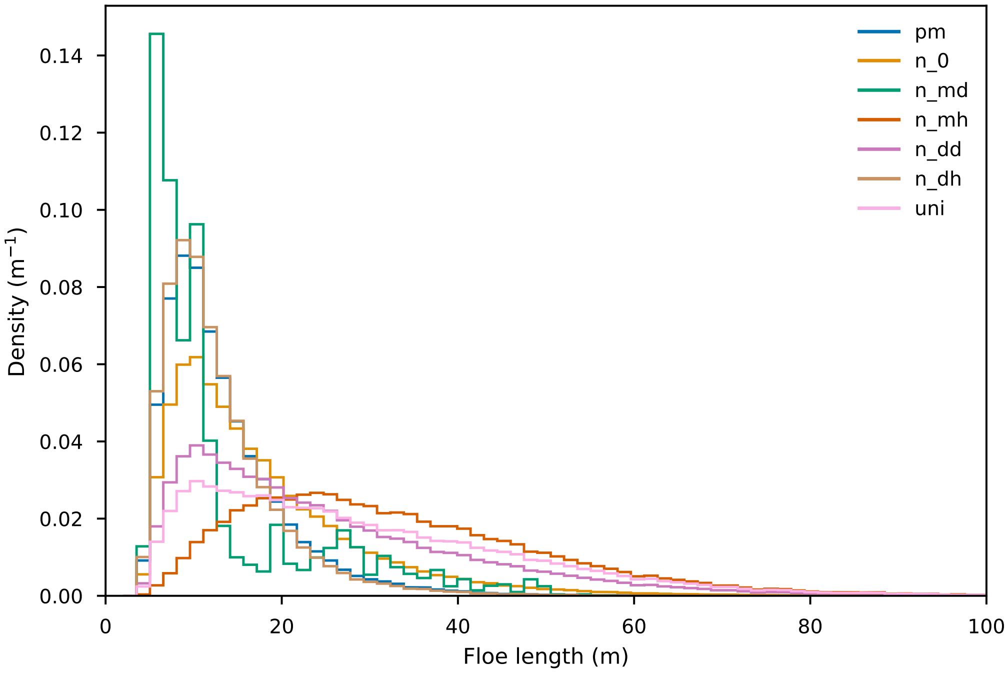

Figure 2: Densities obtained when combining monochromatic model runs with different weighting functions, as represented on Figure 1.

wave forcing.

The PDFs obtained for monochromatic simulations seem to be well fitted by lognormal distribution as well (see Figure 4). This will be discussed in the revised manuscript. We delayed the introduction of the lognormal PDF to Sect. 5 of the manuscript for two reasons. First, Sect. 4 of the manuscript was meant to be an introduction to its Sect. 5. Therefore, we did not want to deep dive into a parametric representation of these FSDs. Second, the linear combination of lognormal PDFs (as presented in Sect. 5) is not, in general, a lognormal PDF. The opposite can be true, as a lognormal PDF can arise from the sum of other functions.

2.3

The algorithm, as described in Section 3.3, does not take into account the time evolution of breakup – in the sense that the breaking events during one “sweep” are all taking place at the same time instance, and a breaking event at one location does not influence what is going on in an immediate vicinity of that location (sudden stress release etc.).

Table 1: Details of several weighting methods.

Name	Type	Effective sample size
pm	Pierson-Moskowitz, $T_p = 5\,\mathrm{s}$	23451
n_0	Gaussian, $\mu = 0.2\,\mathrm{Hz}$, $\sigma = 0.05\,\mathrm{Hz}$	42859
n_md	Gaussian, $\mu = 0.4\,\mathrm{Hz}$, $\sigma = 0.05\,\mathrm{Hz}$	550
n_mh	Gaussian, $\mu = 0.1\,\mathrm{Hz}$, $\sigma = 0.05\,\mathrm{Hz}$	65347
n_dd	Gaussian, $\mu = 0.2\,\mathrm{Hz}$, $\sigma = 0.1\,\mathrm{Hz}$	71280
n_dh	Gaussian, $\mu = 0.2\,\mathrm{Hz}$, $\sigma = 0.025\,\mathrm{Hz}$	19158
uni	uniform	77486

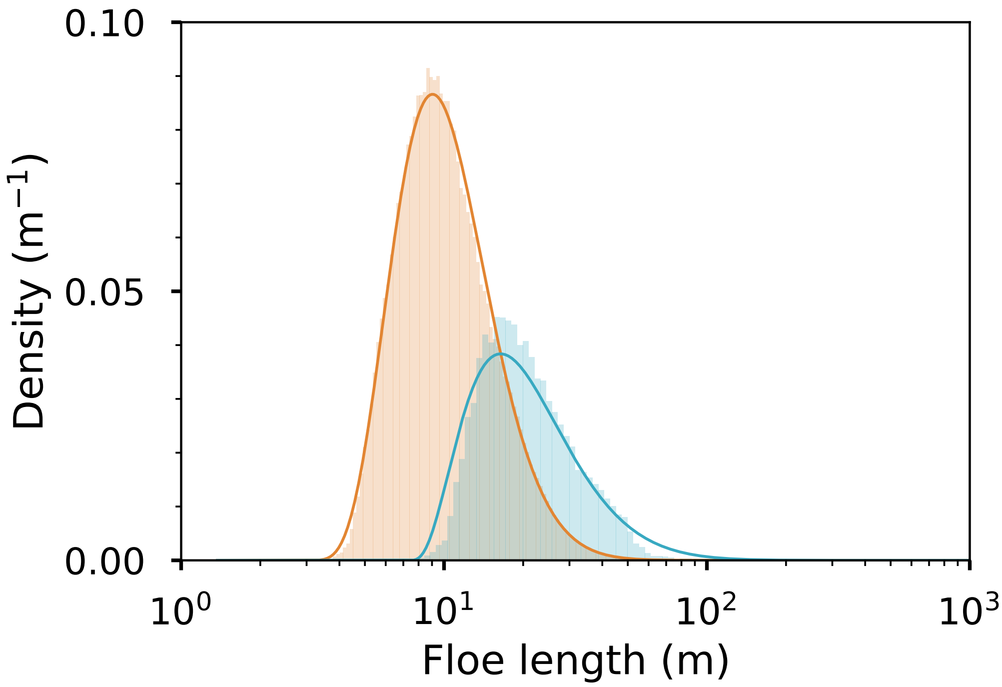

Figure 3: Comparison of results from two ways of considering the polychromatic forcing. Lognormal fits overlayed over histograms. The leftmost histogram (orange hue) corresponds to the method presented in Section 5 of the present paper. The rightmost histogram (blue hue) corresponds to the alternative method presented by Mokus and Montiel (2022), where we use strain superposition to determine the fracture points. Both histogram areas are normalised, the log x-axis skewing this perception.

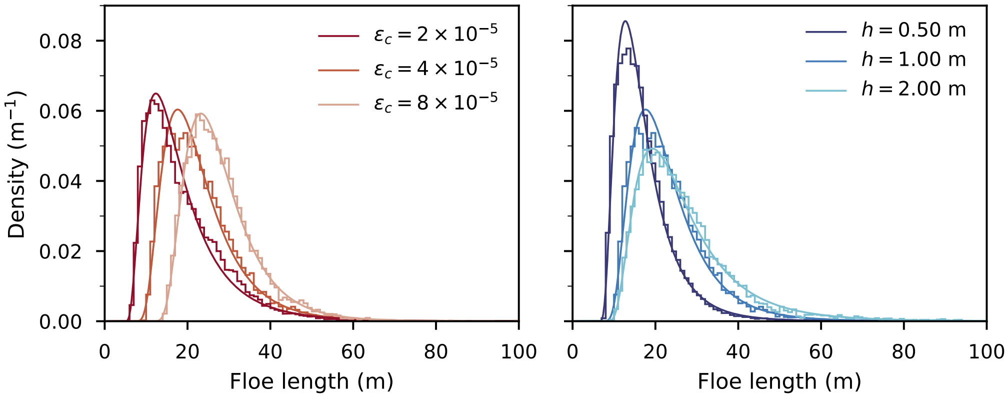

Figure 4: Histograms from Fig. 3 of the manuscript, and associated lognormal fits.

I'm not criticizing it, I just wonder whether/how this limitation can influence the resulting FSDs. What is the Authors' opinion about that?

This is hard to answer. Wave scattering being solved in the frequency domain is an obvious limitation of our model, on which we will insist more in the revised manuscript. A truly transient model would be necessary to evaluate the influence of this simplification, and that is out of the scope of the present manuscript. In a sense, our iterative approach, allowing for each floe to break several time, is a (restricted) substitute for true time evolution.

2.4

Figure 4a,b shows the total number of floes for various combinations of the model forcing. How does the width of the MIZ (i.e., the total length of the broken ice) change? It is an important parameter for several reasons, so it would be interesting to see plots analogous to those in Fig.4ab, but showing the MIZ width. Or at least some comments on that in the text.

We thank the reviewer for this suggestion. We will modify Fig. 4 in the manuscript to show the MIZ width, as shown in Figure 5. The peak of MIZ width as a function of wavelength is reached at a wavelength slightly larger than that causing the largest number of broken floes. If weakening the ice increases the MIZ width, making it thicker, counterintuitively, does so as well.

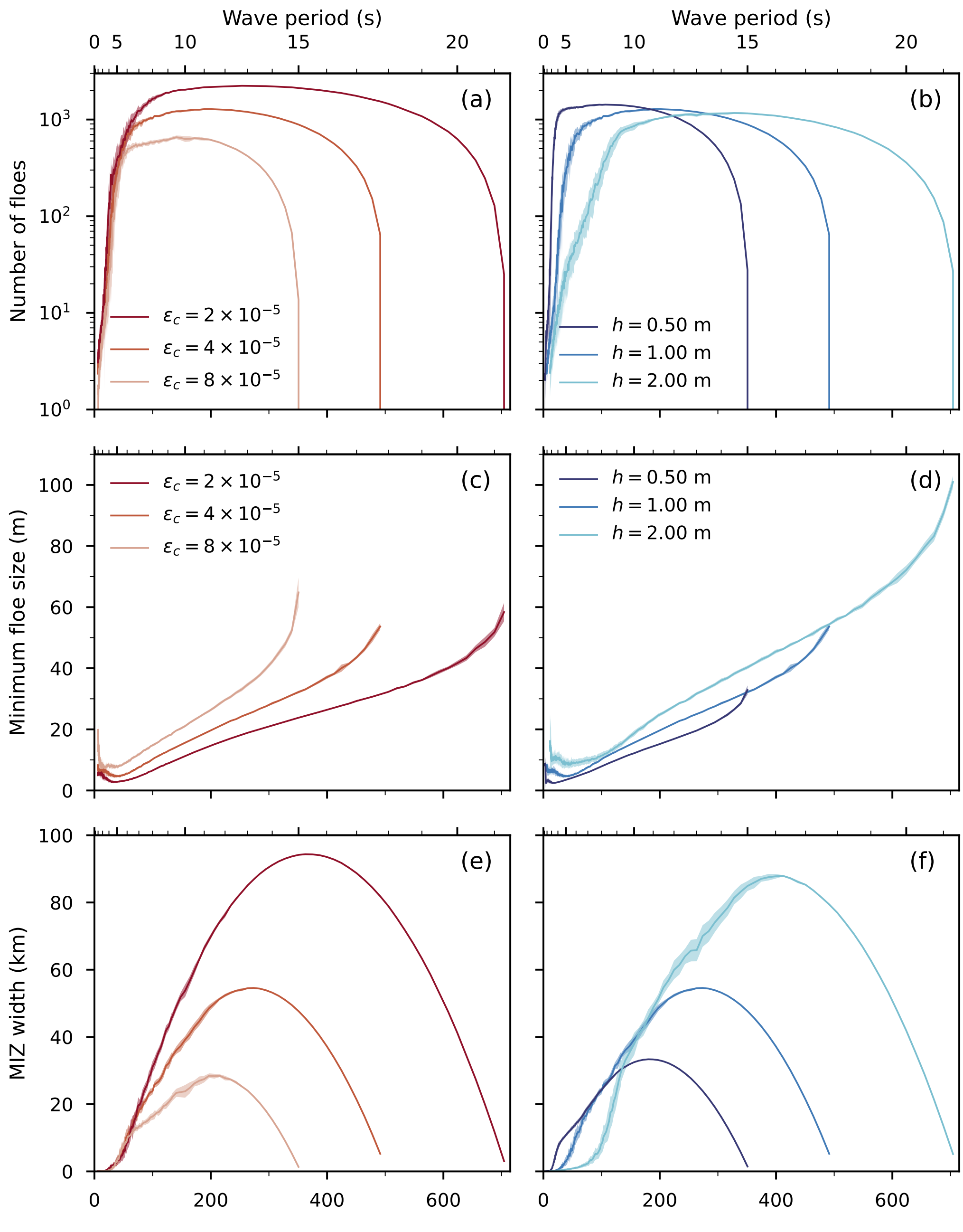

Figure 5: Figure 4 from the manuscript with additional panels showing the width of the MIZ.

2.5

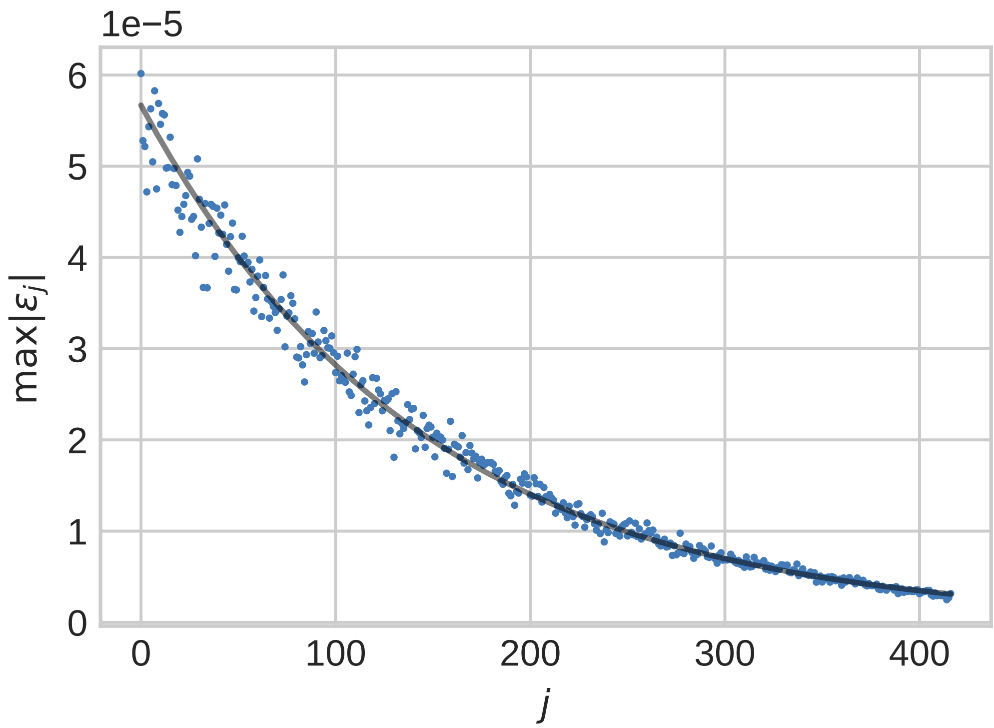

Figure 6: Attenuation of the extremum strain along a model transect, and associated exponential fit. The xaxis is the ordinal floe number in the propagation direction.

I know it's beyond the scope of this paper, but I'm just curious: Have the Authors analyzed the shape of the attenuation curves produced by their model? Are they approximately exponential, or are there deviations from the exponential curve (as in eq. 2.1 of Squire, Phil Trans A, 2018), especially close to the ice edge?

We have not done so consistently for all our tests. We did, at a preliminary stage, study the shape of the strain attenuation. These were indeed exponential (Figure 6). We may add a short discussion of wave attenuation in Sect 4.

3 Minor, technical and other comments

3.1 Line 38

‘Hence…’ suggests this sentence follows from the previous one, but I don’t really see the connection. I think I know what is meant here, but I’d suggest formulating it more clearly.

We mean that the role waves played in the emergence of an observed FSD is not well established for many of these studies. We will alter the phrasing to make it clearer.

3.2 Lines 41–43

I’d suggest to add here that this technique not only leads to erroneous values of the power law exponents, but, in the first place, suggests the existence of power law tails even when there aren’t any and when the pdfs aren’t heavy-tailed at all.

We will make that addition.

3.3 Line 93

The recent paper by Dumas-Lefebvre and Dumont (currently under discussion in TCD: https://tc.copernicus.org/preprints/tc-2021-328/) is worth citing here, as it describes a wonderful observational dataset of sea ice breaking by waves. (It’s not self-advertisement, I’m not an author of that paper.)

We agree. The paper is a more detailed version of Dumas-Lefebvre and Dumont (2020), that we cite. We will make the substitution.

3.4 Lines 256–258

I understand that those tests suggest that the details of how the floes are placed after breaking are not important.

Maybe it’s a naïve question, but are those empty spaces between floes necessary? Does the algorithm work for densely packed ice field, with zero spaces between floes?

We consider the fluid to be inviscid, so the distance travelled by the waves between floes does cause attenuation. The numerical model would need a bit of tweaking for zero spacing to work, but currently spacing could be as small as machine precision allows (double precision floating numbers). We could have settled on assigning a random phase to the forcing wave, at each algorithm iteration, instead of randomly positioning the floes, to achieve the same result.

3.5 Lines 265–266

'FSD dispersion'. Dispersion? As the term 'dispersion' has a clearly defined meaning in the context of waves, I'd suggest replacing it here with 'median floe size'.

'Dispersion' also has a clearly defined meaning in the context of statistics. Variance and interquartile range are typical measures of dispersion. It is indeed confusing and so we will replace the word by e.g. 'spread'.

3.6 Lines 268–269

'a positive relationship between the ice mechanical resistance [...] and the presence of larger floes'. But the skewness is larger for smaller strength and thinner ice, isn't it? The presence of larger floes itself can result from a simple shift of the distribution to the right and is not directly related to the skewness, so this sentence is a bit misleading.

The distributions being skewed is a general, qualitative comment and we do not quantify skewness in the paper. The presence of larger floes can indeed result from a shift of the distribution to the right; we do not mean to link it to the skewness in any way. We will alter the phrasing to make it clearer.

We did compute skewness here for the sake of the discussion (Figure 7). We do not observe any trends for stronger ice at higher time periods (from $T = 8\,\mathrm{s}$); however, skewness does increase for weaker ice at shorter time periods ($T = 4\,\mathrm{s}$). Interestingly, at a given ε_c, it does not evolve monotonically with T. The evolution of skewness as a function of ice thickness is not monotonic at any period, and we do not attempt to analyse it further.

3.7 Lines 268–269

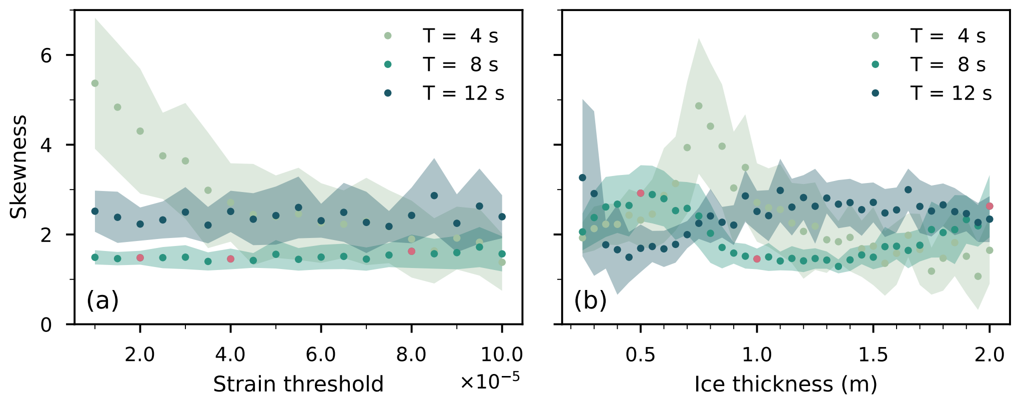

Figure 7: (a) Sample skewness for different forcing periods and evolving strain threshold. The dots correspond to mean values, and the shaded area to one standard deviation around it, over 100 realisations. (b) Same as (a) for evolving ice thickness. In (a), $h = 1$; in (b), $\varepsilon_c = 4 \times 10^{-5}$. In both, $a = 50\,\mathrm{cm}$.

And further: "Qualitatively, increasing ε_c has only a moderate effect on the FSD and seems to be only affecting its mode, shifting it towards larger floes, while its shape remains the same." Is it really so? Are the shape parameters of the pdfs in Fig.3a really so similar? My impression from the figure is quite different. It might be wrong, of course, but please back up this statement by some numbers, e.g., skewness values (maybe you could add them to the panels in Fig.3a,b for those three cases presented?).

As far as the mode is concerned, in Fig. 3a it changes by 100% between case 1 and 3, so I'd say it is a quite substantial change.

The skewness can be seen as a measure of shape. As shown in Figure 7 (a), the skewness is indeed quite stable, for $T = 8\,\mathrm{s}$ and increasing ε_c. For reference, the skewness values corresponding to the histogram presented in Fig. 3 (a, b) of the manuscript are given in Table 2.

The modes in Fig. 3 (a) of the manuscript do increase with ε_c, and we point it out. It can result from a horizontal translation of the distribution. It is a noticeable quantitative change, but not in contradiction with the shape evolving little. We will clarify that statement in the revised manuscript.

3.8 Line 274

Table 2: Skewness values for the different model configurations presented in Fig. 3 (a, b) of the manuscript. The given values are the means over 100 realisations and the associated standard deviations.

ε_c	h	skewness
2×10^{-5}	1 m	1.48 ± 0.15
4×10^{-5}	0.5 m	2.92 ± 0.62
4×10^{-5}	1 m	1.46 ± 0.23
4×10^{-5}	2 m	2.63 ± 0.69
8×10^{-5}	1 m	1.63 ± 0.37

Line 274: 'the dispersion in floe sizes': again, it's not clear what exactly is meant here. The range of floe sizes? (i.e. pdf width?)

We mean the spread of the distribution, as represented in Figure 3 (c,d) by the interquartile ranges. We will change it in relation with comment 3.5.

3.9 Line 277

Line 277: crisp -> sharp? rapid?

Changed to rapid.

3.10 Line 349

Line 349: 'the definition of the ice edge is not clear, as it is period-dependent'. I don't understand this statement, please clarify. And further: 'the total length of ice in each period category'. Period category? Overall, I'd recommend rephrasing this whole paragraph, as it contains a lot of statements that are hard to follow (although the overall meaning is clear, of course).

By period category, we meant the period bins used to discretise the spectrum. The ice edge location, or the MIZ width, will depend on the wave period used to force the system in a particular monochromatic run. We agree on this paragraph needing some rephrasing.

3.11 Lines 426–427

Lines 426-427: ‘scattering alone is not effective enough at dissipating wave energy’!!! Scattering does not dissipate energy at all! Moreover, in a 1D setting, scattering alone does not lead to wave energy attenuation within sea ice: even for an extremely long ice cover, the wave energy at its downwave end must be equal to the energy of the incomming wave minus the energy reflected from the from the upwave edge. In other words, if any attenuation is observed in the scattering-only model runs, it only results from numerical inaccuracies.

‘Attenuating’ is indeed better suited here than ‘dissipating’. We believe it is clear from the context this comment is about multiple scattering. Successive reflections do lower the downwave amplitude, as expected by comment 2.5.

References

Dumas-Lefebvre, E. and D. Dumont (Feb. 2020). “Aerial observations of sea ice breakup by ship-induced waves”. en. In: *ArcticNet Annual Scientific Meeting*. DOI: 10.13140/RG.2.2.23493.40164.

Kohout, A. L. and M. H. Meylan (Sept. 2008). “An elastic plate model for wave attenuation and ice floe breaking in the marginal ice zone”. In: *Journal of Geophysical Research* 113.C9. DOI: 10.1029/2007jc004434.

Mokus, N. and F. Montiel (Apr. 2022). “Floe size distributions in irregular sea states”. In: *Proceedings of the 37th International Workshop on Water Waves and Floating Bodies*.