1 General comments

The authors use a 2D wave–ice model involving wave scattering, viscoelastic dissipation and a strain breaking threshold to conduct a detailed statistical analysis of the steady-state FSD produced by wave forcing. Strong evidence is given that the model predicts lognormal FSDs. The study is communicated clearly and the key outcome is potentially a valuable contribution towards modelling the marginal ice zone.

I recommend revisions before publication.

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

2 Specific comments

2.1 Introduction

The Introduction is missing an overview of the considerable literature on modelling wave propagation in the MIZ. At present, readers could be led into thinking that the model used is accepted by the community, when, as the authors surely know, debate and open questions remain. There are, for example, different methods for modelling wave scattering and many different models of viscous damping. Certain models have been validated using experimental data. A similar comment applies to models of ice breakup caused by waves. It should be clear at the end of the overview why the particular wave propagation and ice breakup models have been chosen for the present investigation.

We acknowledge that our literature review was biased towards models similar to that used in our study. We will add references and a discussion regarding other methods, such as continuous viscous layer and 3D wave scattering models.

2.2 Page 2

The two paragraphs starting from the bottom of page 2 are not particularly relevant for the study presented (e.g. the ideas are not picked up again later) and would be better in Sect 6, leading into a discussion on how the proposed model and findings could be implemented in CICE, etc. Sect 6 would also be strengthened by comments on possible implications of the reduced dimension of the model (e.g., in comparison to the 3D model of Montiel and Squire 2017) and whether the predicted FSD properties are consistent with the ideas used by Dumont, Williams and co to parameterize power-law FSDs (such as the maximum floe size being half a wavelength).

We thank the reviewer for this suggestion which would strengthen the discussion section. We will incorporate it in the revised manuscript.

2.3 Sect 5.1

At the beginning of Sect 5.1, the move from monochromatic to polychromatic forcing requires more explanation and justification. Presumably the definition of the FSD for polychromatic forcing in equation (23) is computationally efficient, but is it representative of the ensemble average FSD created by (random) irregular wave forcing that obeys the prescribed spectrum? Can examples be given to demonstrate this? Better understanding of this aspect of the model will improve interpretation of the results. Incidentally, I was unable to find f_L and $\tilde{f}_L$ when scanning back through the paper at this point. Perhaps the latter could be introduced in Sect 2.

We considered an alternative way to introduce the polychromatic forcing, with results shown in the conference proceeding paper Mokus and Montiel (2022). Instead of considering the weighted average of FSDs from monochromatic forcings, we considered the FSD resulting from the repeated breakup by an irregular-wave-induced strain field simulated from a discretised spectrum with random phases. The results are quantitatively different, but the distribution shape remains similar (unimodal, mode clearly distinct from the smallest observation), with the lognormal model significantly stronger than the power law model. An illustration of the difference can be seen on Figure 1. However, we do not think one of these two parametrisations of a polychromatic forcing is obviously better. Both rely on different sets of assumptions. In particular, the irregular wave-forcing simulation assumes steady state to be reached by waves of all periods at the same time, even though longer waves propagate faster. Our initial approach assumes different periods act independently to break the ice, and that their effects can be averaged over. Which one is physically the most sensible is unclear and will need experimental confrontation.

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

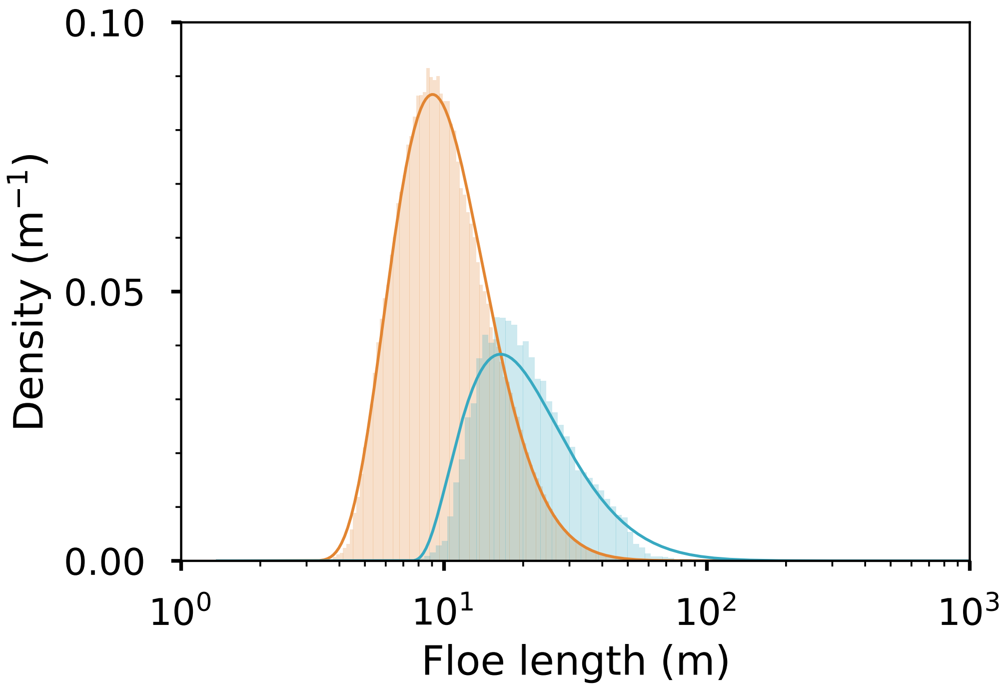

Figure 1: Comparison of results from two ways of considering the polychromatic forcing. Lognormal fits overlayed over histograms. The leftmost histogram (orange hue) corresponds to the method presented in Section 5 of the present paper. The rightmost histogram (blue hue) corresponds to the alternative 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.

2.4 Title

> A title that indicates the scope of the study would be better, e.g. Model predictions of lognormal floe size distributions in the marginal ice zone caused by wave forcing

> We will consider alternative titles.

3 Minor comments

3.1 25

With thinner and weaker first-year ice becoming dominant in the Arctic

'in the Arctic' added

3.2 28

Elaborate on the sentence starting The individual description.

We mean that the dynamic of every floe, at the basin scale, cannot be reasonably determined and kept track of. We will clarify this in the revised manuscript.

3.3 55

The sentence on short time scales for breakup appears to contradict the steady state model assumption.

Breakup happens on time scales shorter than thermodynamics processes, that our model does not resolve. Recent observations (Dumas-Lefebvre and Dumont 2021) showed the breakup front moves slower than the wave front within the ice cover, so we believe these assumptions hold. We will clarify the sentence in the revised manuscript.

3.4 Sec 3.1

Similar wave scattering models should be referenced at the beginning of the section Kohout and Meylan 2008; Montiel et al. 2012, and any notable differences identified.

Our model is indeed directly inspired by these. We will make the connection to other similar models more obvious in our description.

3.5 149

travelling and evanescent ...

We will add a reference to evanescent modes.

3.6 170

For completeness, say that the complex roots can become purely imaginary for high frequencies and/or thick ice.

We believe this point is slightly out of scope, as this is very unlikely to happen in any geophysically realistic setting (Bennetts 2007).

3.7 178

I think the phases are used to normalize rather than cancel out the exponential terms.

We meant cancel in the sense of making them neutral with respect to multiplication. We will make the phrasing clearer.

3.8 Eqn (13)

Replace the full stop with a comma.

Corrected.

3.9 248+250

for every floe and none of the floes break

Corrected.

3.10 253

Give the distribution used to randomly redistribute the floes after breakup.

Floes are positioned from left to right and localised by their left edge. The leftmost floe is placed at a random location, drawn from a uniform distribution, around its location at the previous iteration. For subsequent floes, the left bound corresponds to the right edge of the last positioned floe (on their left). The right bound corresponds to the previous right bound, augmented by the length of the last positioned floe. A location is drawn from a uniform distribution between these two bounds; an illustration is given in Figure 2. As the width of that interval quickly tends to 0, we enforce a minimal length for our random draw. Floes can still get arbitrarily close to one another, as long as they do not overlap. The room allocated

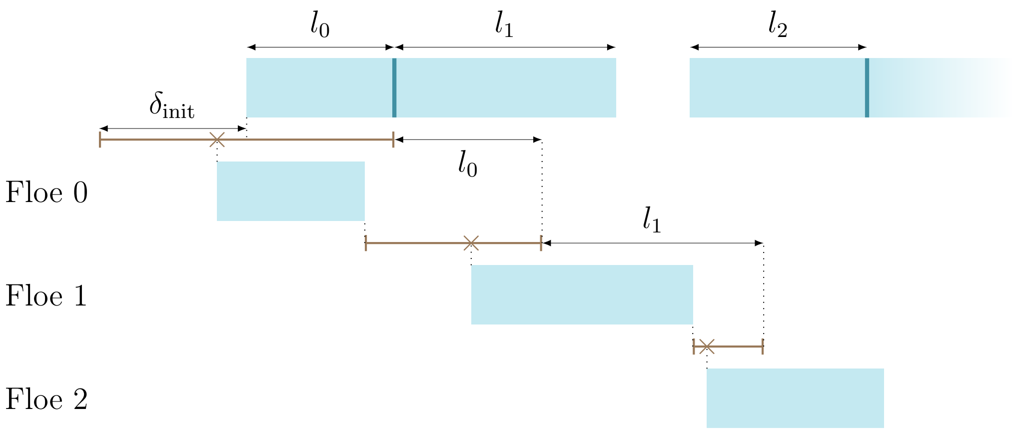

Figure 2: Illustration of the floe repositioning method. The top row shows current floes with identified breakup location marked by vertical bars and the resulting lengths. Successive rows show the iterative positioning as described in the text. Below each row, a segment shows the interval from which a location will be randomly drawn; the cross marks that location.

to the first floe (labelled δ_{init} on the schematic in Figure 2) as well as that minimum width are set to 100 m and 1 cm, respectively. We ran simulation with alternative values to ensure these values do not have any impact on our results.

We will include these details as an appendix to the revised manuscript.

3.11 258

Give details on the local resonances plus references.

For any single realisation of the array, local resonances can take place due to additive interference between scattered waves. These can be filtered out through ensemble averaging. This behaviour, and the solution, are described by Kohout and Meylan (2008). The reference will be added in the revised manuscript.

3.12 Figure 3d

The levelling off/decrease of the median floe size with increasing ice thickness for T=8s is interesting and worth discussing in the text.

We will discuss this feature in the revised manuscript.

3.13 Figure 4 caption

Figure 4 caption: State the amplitude(s) used.

It is the same as in the previous figure, 50 cm. We will correct the omission.

3.14 348

348: Space needed after the full stop.

Corrected.

3.15 428

Note that the value $\gamma = 13.5\,\mathrm{Pa\,s\,m^{-1}}$ was derived from measurements in the Antarctic MIZ (Massom et al. 2018).

We thank the reviewer for this reference, that we did not know about. A smaller value ($6.9\,\mathrm{Pa\,s\,m^{-1}}$) is derived in Mosig et al. (2015). The value $13\,\mathrm{Pa\,s\,m^{-1}}$ is used in Williams et al. (2013) and subsequent studies; however, it is unclear how this parameter may depend on, e.g., the ice thickness or rigidity, so we settled on a slightly more conservative estimate. Even though not presented here, we conducted experiments with a range of viscosities. We will develop this point in the discussion, and add the suggested reference to our review.

3.16

Mathematics needs a capital M in the institution name.

Corrected.

References

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