

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-391

Anonymous Referee #2

Referee comment on "Wave-triggered breakup in the marginal ice zone generates lognormal floe size distributions: a simulation study" by Nicolas Guillaume Alexandre Mokus and Fabien Montiel, The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-391-RC2>, 2022

First of all, I'd like to apologize to the Authors and the Editors for a very long delay in submitting this review. Unfortunately, I was not able to finish it at an earlier date. I am very sorry for that.

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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.

Major comments:

- 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.

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.

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 & Meylan 2008).

- 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 wave forcing.

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

- 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.

- 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?

Minor, technical and other comments:

- 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.
- 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.
- 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.)
- 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?
- 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".
- 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.
- 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 $\sim 100\%$ between case 1 and 3, so I'd say it is a quite substantial change.
- 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?)
- Line 277: crisp -> sharp? rapid?
- 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).
- 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 incoming wave minus the energy reflected from the upwave edge. In other words, if any attenuation is observed in the scattering-only model runs, it only results

from numerical inaccuracies.