1 General comments

This paper aims to develop an efficient model of wave breakup of sea ice floes including a random component of floe positioning that can be used to generate statistical descriptions of floe size (probability) distributions (FSD) that might emerge from wave breakup from sea ice and rapidly explore relevant parameter spaces within this setup (e.g. wave period, sea ice thickness). The study finds that the emergent FSD can be best characterised using a lognormal distribution and discusses implications of these results for finding the best fit to observations of floe size and for future parametrisations of floe breakup by waves in sea ice models. This work intersects two areas of research that have had significant focus in recent years: modelling the role of individual processes in determining the emergent FSD in sea ice models and modelling interactions between waves and sea ice and how sea ice can impact wave propagation. This study builds on earlier efforts to develop simple but accurate models of wave breakup of floes. The value of the model presented here is that it is efficient and can be used to rapidly explore relevant parameter spaces and include stochastic elements within the model to represent uncertainty / variability (in this case to capture variability in floe positioning without a full treatment of sea ice dynamics). I therefore believe this paper makes a useful contribution to both the sea ice and wave modelling communities, and also has potential value in understanding and characterising observations of floe size.

The scientific quality of the work presented is generally strong, with good associated analysis and discussion. The figures are of a very good quality and appropriate to the discussion. The structure of the paper seems fine and is easy to follow, though it would be good to see a more thorough overview of the paper structure at the end of the introduction. The paper reads well, is clear in its conclusions, and also has a representative abstract and title. I do have a couple of major concerns that would need to be addressed before I can recommend publishing. Firstly, I am not sure the methodology used has been sufficiently justified. Specifically, the choice to use monochromatic model runs and then taking the weighted average to determine the emergent FSD from a full wave spectrum is not properly justified / supported as a reasonable approximation. In addition, the study repeatedly refers to whether observations of the FSD should be fitted to a power law. Whilst this is an important discussion, I find the paper focuses too much on this point and insufficiently on other impacts / conclusions of the findings presented. Full details of these concerns are provided in the specific comments.

Overall, I believe that this paper is within the scope of The Cryosphere and, provided the above concerns can be adequately addressed, merits publishing.

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

2 Specific comments

2.1 General point

The study uses this result to backup conclusions from other studies such as Stern et al. 2018 that other possible fits should be tested against observations of floe size, not just a power law. These conclusions are justified on the basis of the evidence presented. However, throughout the manuscript the authors question the validity of power law fits to FSD data. Whilst this is a reasonable and justified question to ask and one several previous papers have discussed as noted in the manuscript, I find this point is too frequently made within the manuscript, at the expense of other important results that emerge from this study, given this study does not appear to present any new evidence to suggest that a power law does not produce a valid fit to observed FSDs (as opposed to new evidence to support the testing of alternative fits to observations, which the study does present, as noted above). Even in regions of high wave activity, observed FSDs are not necessarily solely a result of wave breakup. Even if they are, there are physical features that may determine the FSD not considered within the model used here (e.g. variable ice thickness, existing weaknesses in the sea ice, fractures that are not perpendicular to the direction of wave propagation). The emergence of a lognormal distribution from this model does not necessarily tell us anything about the validity of a power law fit to observations of floe size unless this model can be validated using observations of an FSD under wave control, which has not been presented in this study.

We agree that the FSD is impacted by more than just wave activity. We have shown in a separate paper (Montiel and Mokus 2022, manuscript under revision) that the lognormal model can be applied to observations previously analysed under the power law hypothesis. However, wave conditions at the time of these measurements were not available. Confronting our results to more controlled experiments would be the only way to validate them. We will make more obvious that our findings do not negate the collection of studies based on the FSD following a power law distribution.

We acknowledge that any parametric model would be a simplification, and that many non-controlled factors, such as pre-existing cracks, can locally impact the FSD. Additionally, limits on the floe sizes that can be remotely discriminated tend to truncate observations. We note that the tail of the lognormal distribution, excluding the region of values smaller than the mode, asymptotically identifies to a power law distribution.

2.2 P2 L28–29

'The individual description of these, floating pieces of sea ice is not possible.' What do you mean by this comment? Individual pieces of ice cannot presently be simulated in continuum models, but they can in discrete element models of sea ice.

We mean they cannot be represented at the scale of an ocean-wide numerical simulation. We will clarify this point in the revised manuscript.

2.3 P3 L65–68

You should also describe/discuss the most recent study from Horvat and Roach 2022 that introduced a machine-learning-derived parameterization of wave breakup of floes that can be used within the prognostic model.

We were not aware of this study when we submitted ours. We will include it in our revised manuscript.

2.4 P2 L57–P3 L81

In this section you have described existing treatments of wave breakup of floes within sea ice models but there are other approaches that you have not described e.g. both Bateson et al. 2020; Boutin et al. 2021 include treatments of wave breakup of sea ice within FSD models. It would be helpful to either briefly discuss these treatments or at least highlight that your discussion is not exhaustive.

We will add these references.

2.5 P3 L80–81

'Nevertheless, the model sensitivity analysis conducted by (Zhang et al. 2016) revealed compelling improvement on ice extent simulation when considering their FSD formulation.' What were the improvements? This statement is vague and should be clarified.

Their implementation yielded improvements in terms of ice extent and location of the ice edge. Their simulations including the parametrisation were better able to replicate observations than those which did not. We will make this point clearer in the revised manuscript.

2.6 P4 L97–105

It would be helpful to describe the overall structure of the paper at the end of the introduction i.e. describe how the paper proceeds, section by section.

We will add this description to the introduction.

2.7 P8 L202–203

Why did you decide to use a fixed sea ice thickness in your simulations? Do you anticipate that a lognormal distribution would still emerge if the sea ice thickness was variable in a single evaluation of the model?

This stems from the experiment design. Our scattering formulation assumes individual floes are of constant thickness. As we populate the MIZ by breaking off floes off a single initial floe, it comes out that all resulting floes share this initial thickness. This thickness could be altered after breakup has happened, but we choose to keep it constant as breakup happens on time scales shorter than those of the processes that would alter the thickness. As stated in the text, the model is capable of handling floes with varying properties, including the thickness. However, such simulations are outside the scope of this paper, that focuses on FSDs resulting from repeated breakup events. Therefore, we cannot comment on the FSDs we would obtain in such a simulation.

2.8 P8 L205–206

'A sensitivity analysis (not shown here) proved $N_v = 2$ to be adequate in terms of convergence.' Please provide more details on this. How are you assessing adequate convergence here?

We studied the convergence in terms of energy conservation. We placed five floes of finite length and zero viscosity in the domain. Scattering does not dissipate energy, so energy carried by travelling modes is expected to be conserved for reflection/transmission by individual floes and overall, when considering the array of floes as a single scatterer. Note that under the integral method used to solve for the scattering (Williams and Porter 2009), the number of modes used to establish the scattering kernel ($N_k = 100$ in our case) is distinct from the number of modes used to expand the potential ($N_v = 2$), once determined. We found that beyond the first evanescent mode ($N_v = 1$), adding them slowly deteriorate the energy conservation (Figure 1). However, we decided to include the second mode as it is susceptible to have a marginal impact on the location of the strain extremum.

We have not formally assessed it, but we believe the behaviour would be similar for a larger number of floes, given the very small spread between the successive floes, presented in Figure 1. Additionally, we note that the domain-wide energy is very well conserved whatever the number of evanescent modes considered.

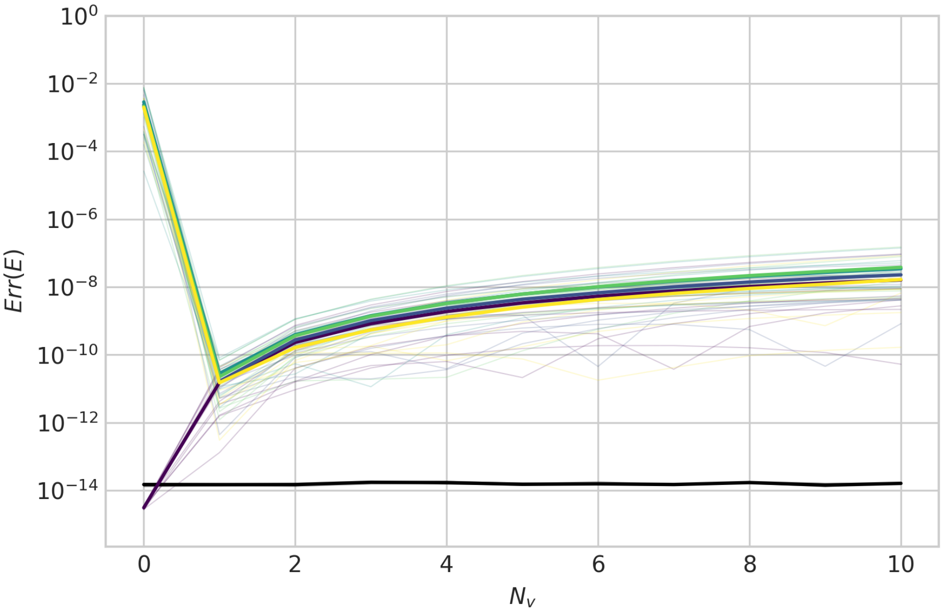

Figure 1: Energy conservation in a five-floe setting, with 100 kernel modes. The coloured lines, from darker to lighter hues, identify floes from left to right, where the wave forcing is incident from the left. The black line characterises energy conservation over the whole domain. Thinner lines indicate 10 individual runs, with randomly placed and spaced floes, and the thicker lines the average of these runs. With non-viscous floes, we expect the difference $1 - \left(|R|^2 + |T|^2\right)$ (vertical axis) to be 0, where R and T are the complex reflection and transmission coefficients associated with the propagating mode, respectively.

2.9 Section 4

In this section you provide a physical explanation/interpretation of the results presented in Fig. 4 but not Fig. 3. It would be good to see more discussion of the results in Fig. 3; in particular, can you explain the different trends in the variability/dispersion of floe size shown in panels (c) and (d) in Fig. 3?

For the experiments related to the strain threshold (panels (a) and (c)), we believe that the apparent translation of the histogram is due to the fact that higher strains are reached further from the floe edges (as we use free edge boundary conditions), leading to more frequent longer broken-off floes. We will investigate this behaviour further.

For the experiments related to the ice thickness (panels (b) and (d)), we expect non-linear patterns as the thickness impacts both the strain undergone by the floe and the wave transmission coefficient, which translates to the under-floe wave amplitude, hence the deflection undergone by the floe and ultimately, the strain. Short waves (4 s) are much more effectively reflected by thickening ice than longer waves (Figure 2), leading to very little breakup (apparent oscillatory behaviour between 1.5 m and 2 m for $T = 4\,\mathrm{s}$ on panel (d)), which can be seen on Figure 4 (b).

We will add a discussion on these trends in the revised manuscript.

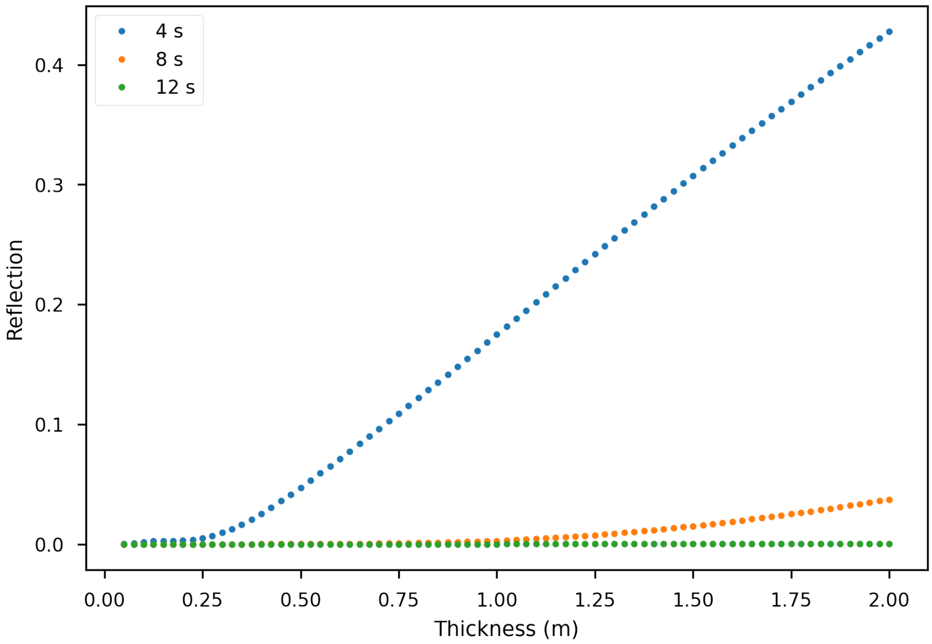

Figure 2: Reflection coefficient ($|R|^2$) for increasing ice thickness and various wave periods.

2.10 Figure 3

Why did you decide to use the median floe size to characterise the average floe size (rather than, for example, a linear-weighted mean)? An explanation in the text somewhere would be useful.

Given the skewness of the distribution of floe sizes (top panel), we decided to use the combination median–interquartile range as measures of central tendency and dispersion, rather than the combination mean–standard deviation, the data being clearly non-normal. The results presented on Figure 3 are average over realisations of a single, monochromatic case: no weighting is used. This choice is clarified in the text.

2.11 Figure 3

Do you have any explanation for the oscillatory behaviour in panel (d) for the two shorter wave periods when the ice thickness exceeds 1.5 m?

We do note this apparent oscillatory behaviour. For the shortest period, we believe it to be no more than a spurious effect due to the fairly low amount of breakup, enhancing the apparent variability. We will add a comment to the revised manuscript.

2.12 P13 L295–297

'To estimate the effect of a developed sea on the FSD f_L, we take the weighted average of distributions resulting from monochromatic model runs,'. This appears to be a significant model assumption to only consider single amplitude-frequency pairs at once rather than the full wave spectrum since it ignores possible interactions between the different pairs in fracturing the sea ice. What is the justification for this model approach? There needs to be some evidence presented (e.g. test cases evaluating the model using full polychromatic forcing) to show that the error resulting from this approximation is not large enough to impact the conclusions.

The assumption behind our approach is that wave periods act independently, which is in line with our linear theory. We did consider 'full polychromatic forcing' in the sense of evaluating the strain induced by each period separately, taking the weighted average, and using it to determine whether floes should break (Mokus and Montiel 2022, conference proceedings). The two approaches yield different results. However, it is not clear which one is physically more justifiable, as the strain-averaged approach 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.

2.13 P13 L300

Can you comment on the sensitivity of your results to the choice of spectrum?

We ran comparisons using alternative weighting functions, represented on Figure 3. Descriptions of these functions can be found in Table 1.

The lognormal density function has, qualitatively, a shape similar to the Pierson-Moskowitz spectrum 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 4.

The one case that stands out, with a lot of 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.

We will comment on this sensitivity in the revised manuscript.

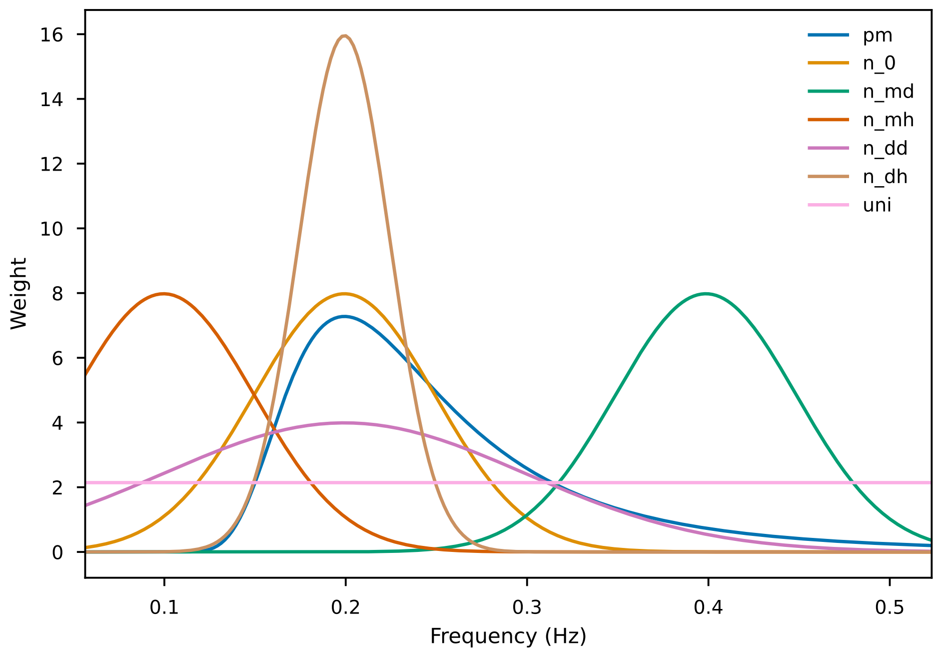

Figure 3: Representation of functions used as alternative weights. They are all normalised on the positive real half-line; because of the finite range of frequencies supported by our model, truncations imply some of them integrate to less than unity. Description of the legend entries can be found in Table 1.

2.14 P14 L310

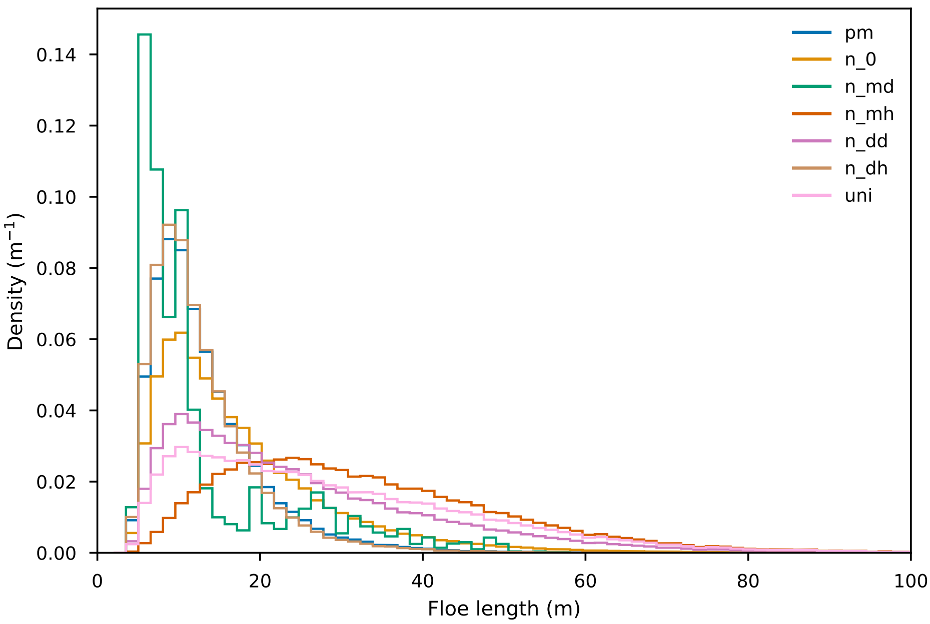

Figure 4: Densities obtained when combining monochromatic model runs with different weighting functions, as represented on Figure 3.

What is the reason for drawing a single FSD f_l at random rather than including all 50 realisations?

For each wave period, we obtain 50 distinct FSDs $\tilde{f}_L$. We select at random one of each before combining them, according to Eq. (23), into a FSD f_L. Proceeding like so allows us to observe variations between different f_L, as represented on Fig. 5 of the manuscript, and virtually gives us infinitely many ways to build f_L ($50^{200} \approx 6 \times 10^{339}$, as we have 50 realisations of 200 periods).

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

2.15 Section 5.3

As it currently exists, I am not sure this section is adding much insight to the manuscript and it could be removed without detracting from the paper. All this section demonstrates is that the average floe size increases moving away from the ice edge, a behaviour several previous observational and modelling studies have identified. What might make this section more insightful would be if the results could be used to generate a mathematical description of how the emergent FSD changes with distance from the ice edge or plots to show how the parameters of the lognormal fit change with distance from the ice edge.

We added this subsection as we found the displayed features interesting. We agree it can be further extended. Most importantly from our perspective is that the behaviour stays lognormal. In the revised manuscript, we will change our non parametric density estimates to lognormal fit and add the evolution of the parameters with respect to distance from the edge.

2.16 P16 L364–365

'For simplicity, even though we did conduct multivariate simulations, we focus here'. Why mention this if you are not going to discuss the results? It would be beneficial to discuss some of these results—since in the results you present much of the parameter space is unexplored leaving open the potential for different behaviour elsewhere in the parameter space.

We acknowledge that some new behaviour may emerge from multivariate simulations. We will alter the manuscript to state that such simulations are outside the scope of the study.

2.17 P17 L382–384

'As the peak propagating wavelength is proportional to the significant wave height, this non-monotonic evolution does not support wave properties alone govern the dominant floe size,'. Can you provide a more precise explanation of why this happens? Given the simplified model treatment used, it should be possible to explain how this behaviour emerges.

We believe this is due to the period-averaging. For small waves, wave periods leading to the most breakup do not match the wave periods dominating the spectrum. We will investigate this behaviour further.

2.18 Section 6

It would be good to see more focus in the discussion/conclusions on what needs to be done to validate this model using observations of floe size i.e. what are the key emergent features of the FSD produced by this model that could potentially be identified in observations (not just the general lognormal shape, but how the distribution evolves with changes to key parameters such as the distance from the ice edge).

We are currently engaged in discussions with experiment-focused teams regarding that matter. We will expand this Section accordingly. We would also like to highlight the lognormal model proved to be successful at describing observation data (Montiel and Mokus 2022), even though these could not be linked to wave activity.

2.19 P21 L446–447

'These results aim at being a step towards the parametrisation of wave action in FSD-evolving models.' Working towards this parametrisation seems to be a key result of this study and merits more than a single line in the discussion/conclusion section. What more needs to be done to develop this parameterisation? How will this parameterisation compare to the alternative scheme developed by (Horvat and Roach 2022)?

Results can be used to, e.g., parametrise the distribution of floe sizes used in the wave fracture parametrisation implemented in CICE by (Roach et al. 2018) and labelled SP-WIFF in the aforementioned reference. The current scheme relies on an histogram ($A(r)$ in Horvat and Roach 2022) derived from a strain-based breakup scheme applied on an ice transect, i.e. a configuration with geometry assumptions similar to the model we present here. Binned floe sizes are computed off-line for a number of ice and wave conditions. A parametric distribution, whose parameters may depend on these ice and wave conditions, could be used as an alternative to this ad-hoc breakup parametrisation.
We will expand on this in the text.

3 Technical Corrections

3.1 P2 L30–31

'In particular, fragmentation caused by ocean waves makes the floes more sensitive to melt'. Maybe change 'In particular' to 'Of particular interest here' or something similar, since there exists other mechanisms of ice fragmentation that can drive the same feedback.

‘Of particular interest here’ conveys the intended meaning and we made the change.

3.2 P2 L36–37

Most studies listed fit the observed FSD to a simple power law (or combination of the two). I am not sure it is correct to describe these as Pareto distributions (see e.g. Herman, 2010).

In Herman (2010), the author called densities proportional to $x^{-1-\alpha}\exp\left(\frac{1-\alpha}{x}\right)$ truncated Pareto distributions, or GLV distributions. Without any constraint on the normalisation term, it is actually an inverse gamma distribution, whose density is $\frac{1}{\Gamma(\alpha)}x^{-(1+\alpha)}\exp\left(-\frac{1}{x}\right)$ when setting the scale to 1. The Pareto distribution probability density function is $\alpha x^{-(\alpha+1)}$, when setting the scale to 1 ($\alpha > 0$), and often abusively called power-law distribution.

3.3 P2 L37–38

‘However, a variety of processes such as failure from wind or internal stress, lateral melting or growth, ridging, rafting or welding, are susceptible to alter the FSD.’ Can you provide references for these processes having been observed to influence the FSD?

These processes are listed by Rothrock and Thorndike (1984) in their seminal paper. Many studies treating the floe size distribution reference them without further justification by observation. Some modelling studies give them a mathematical treatment as well (Horvat and Tziperman 2015). For more accuracy, we will change our statement to highlight its conjectural nature.

3.4 P2 L48–49

‘evaluate the impact of its introduction on other quantities such as ice thickness or concentration (Roach et al., 2018)’. There are other studies you should consider referencing here e.g. Bateson et al., 2020; Boutin et al. 2021.

We will add these references.

3.5 P3 L92

‘ensuing’. Should this be ensuring?

We will change it to 'leading to' which might better convey the intended meaning.

3.6 P4 L96

Reference is incorrect. Boutin et al. (2020b) should be Boutin et al. (2021).

Indeed, we referenced the Discussion paper, this will be corrected.

3.7 P9 L243–244

'Hence, the number of floes at most doubles, if all the floes break in a single simulation.' If my understanding is correct, single iteration would be a clearer choice here rather than single simulation.

In this subsection, we try to present the breakup scheme in a general manner. Using 'iteration' conveys the idea of repeating breakup event. It is, indeed, what we present in the following subsection; but it has not been introduced so far. This is why we settled on using 'simulation', as in 'breakup simulation'.

3.8 P18 L394–395

'The prevalence of smaller floes, however, tends to build up slightly.' Phrasing here is awkward.

We can replace it with 'The fraction of floes in the smallest size categories tends to increase'.

3.9 P19 L399

'shows'/'points out' rather than 'point out'.

Corrected.

References

Bateson, A. W., D. L. Feltham, D. Schröder, L. Hosekova, J. K. Ridley, and Y. Aksenov (Feb. 2020). "Impact of sea ice floe size distribution on seasonal fragmentation and melt of Arctic sea ice". In: *The Cryosphere* 14.2, pp. 403–428. DOI: 10.5194/tc-14-403-2020.

Boutin, G., T. Williams, P. Rampal, E. Olason, and C. Lique (Jan. 2021). "Wave–sea-ice interactions in a brittle rheological framework". In: *The Cryosphere* 15.1, pp. 431–457. DOI: 10.5194/tc-15-431-2021.

Herman, A. (June 2010). “Sea-ice floe-size distribution in the context of spontaneous scaling emergence in stochastic systems”. In: *Physical Review E* 81.6. DOI: 10.1103/physreve.81.066123.
Horvat, C. and E. Tziperman (Nov. 2015). “A prognostic model of the sea-ice floe size and thickness distribution”. In: *The Cryosphere* 9.6, pp. 2119–2134. DOI: 10.5194/tc-9-2119-2015.
Horvat, C. and L. A. Roach (Jan. 2022). “WIFF1.0: a hybrid machine-learning-based parameterization of wave-induced sea ice floe fracture”. In: *Geoscientific Model Development* 15.2, pp. 803–814. DOI: 10.5194/gmd-15-803-2022.
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*.
Montiel, F. and N. Mokus (2022). “Theoretical framework for the emergent floe size distribution: the case for log-normality”. In: DOI: 10.48550/ARXIV.2205.06014.
Roach, L. A., C. Horvat, S. M. Dean, and C. M. Bitz (June 2018). “An Emergent Sea Ice Floe Size Distribution in a Global Coupled Ocean-Sea Ice Model”. In: *Journal of Geophysical Research: Oceans* 123.6, pp. 4322–4337. DOI: 10.1029/2017jc013692.
Rothrock, D. A. and A. S. Thorndike (1984). “Measuring the sea ice floe size distribution”. In: *Journal of Geophysical Research* 89.C4, p. 6477. DOI: 10.1029/jc089ic04p06477.
Stern, H. L., A. J. Schweiger, J. Zhang, and M. Steele (Jan. 2018). “On reconciling disparate studies of the sea-ice floe size distribution”. In: *Elementa: Science of the Anthropocene* 6. Ed. by J. W. Deming and T. Maksym. DOI: 10.1525/elementa.304.
Williams, T. and R. Porter (July 2009). “The effect of submergence on the scattering by the interface between two semi-infinite sheets”. In: *Journal of Fluids and Structures* 25.5, pp. 777–793. DOI: 10.1016/j.jfluidstructs.2009.02.001.
Zhang, J., H. Stern, B. Hwang, A. Schweiger, M. Steele, M. Stark, and H. C. Graber (Jan. 2016). “Modeling the seasonal evolution of the Arctic sea ice floe size distribution”. In: *Elementa: Science of the Anthropocene* 4. Ed. by J. W. Deming and T. Maksym. DOI: 10.12952/journal.elementa.000126.