The Cryosphere Discussions

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Comment on tc-2021-391

Anonymous Referee #3

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-RC3, 2022

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 parameterisations 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.

Specific Comments

- General point: A key conclusion from this study is that the FSD that emerges from this model of wave breakup of sea ice is a lognormal distribution. 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.
- 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.
- 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.
- 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) and 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.
- 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.
- 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.
- 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?

- P8 L205-206: ‘A sensitivity analysis (not shown here) proved Nv = 2 to be adequate in terms of convergence.’ Please provide more details on this. How are you assessing adequate convergence here?
- 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?
- 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.
- 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?
- P13 L295-297: ‘To estimate the effect of a developed sea on the FSD fL, 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.
- P13 L300: Can you comment on the sensitivity of your results to the choice of spectrum?
- P14 L310: What is the reason for drawing a single FSD f_l at random rather than including all 50 realisations?
- 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.
- 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.
- 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.
- 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).
- 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)?

Technical Corrections

- 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.
- 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).
- 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?
- 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.
- P3 L92: ‘ensuing’. Should this be ensuring?
- P4 L96: Reference is incorrect. Boutin et al. (2020b) should be Boutin et al. (2021).
- 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.
- P18 L394-395: ‘The prevalence of smaller floes, however, tends to build up slightly.’ Phrasing here is awkward.
- P19 L399: ‘shows’ / ‘points out’ rather than ‘point out’.

References

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