We sincerely thank the reviewers for their insightful comments and constructive feedback. Their suggestions have significantly improved the quality of our manuscript. We appreciate their time and effort in reviewing our work. Below, all answers are written in cursive blue next to the original comments in black.

Answers: Reviewer 2

General Comments

In this paper, the authors develop 3D DEM simulations to model the formation of sea ice ridges. Although the methodologies are not novel, the application to the full 3D scenario is new and the results are potentially significant. The authors illustrate the necessity for all three dimensions and non-simultaneous failure in order to accurately capture the phenomenon, and is therefore, in my opinion, an advancement of the state of the art for simulating pressure ridges. The authors also argue that their simulations result in a linear relationship between ridging force and ice thickness (F h), which differs from previous relationships in the literature (F h3/2). However, this linear relationship appears analogous to recent publications of ice crushing against solid structures, as noted by the authors. Despite this, this result would be significant as the previous ridging relationships have been integrated into ESMs for decades. I have asked some questions below to clarify aspects of this linear fit. Overall, the document is well written, interesting, and presented in an accessible way.

Specific Comments

1. Lines 25-30 - It might be beneficial to provide some background information on the Hopkins model such as what particle geometry they used (polygons vs disks), how they handled contact mechanics, how they modeled inter-particle bonds (and their failure), etc. This would give the reader some context as to what the previous state of the art was versus your approach. *- We added information on the particle shapes into the mentioned paragraph. Additionally, we now included into Section 2.1 that the general features of our model are similar to those used in sea ice DEM since 90s.*

2. Lines 67-68 - I understand that the plastic portion of fn approximates local yielding and crushing. Do you have any comments on the relationship between this local deformation parameter and the large scale deformation ridging process? Did you do any analysis of how the magnitude of this plastic portion affects the ridging results? Or have any thoughts on how it may relate to the larger scale deformation? *- This is an interesting point with related changes now in Section 2.1 and at the end of the conclusions. We describe that ice thickness has been found to be a key parameter affecting ice loads. 2D simulations in Ranta (2019) suggest plastic limit explains scatter in the values of peak ice load on inclined structures, yet does not have an effect on load levels. We also now mention that a future detailed study on parameter effects would be beneficial.*

3. Lines 86-86 - Just confirming I understand this - is this saying that the ridging force was measured as the contact force on the rigid floe? *- Yes, we add 'sum of contact forces' to the*

sentence.

4. Line 88 - A 1 cm gap seems small for a domain spanning several meters. What kind of analysis/measurements were done to make sure there were zero frictional forces from the adjacent ice throughout the simulation? *- When developing the setup, we checked visually that the approaching ice was not touching the ice acting as side restrictions. As the direction of the ice velocity is parallel to the adjacent ice, contact did not occur. If contact occurs closer to the ridge, it would still not interfere with the recording of the ridging forces as the forces on the side restrictions are not included in the overall force recordings.*

5. Lines 92-94 - The discussion related to the particle aspect ratio tripped me up a couple times. I believe you are saying that a particle aspect ratio of 1.5 resulted in simulated ridges that matched the aspect ratio of ridges measured in Høyland, 2007. Is that a correct interpretation? Was the 1.5 value determined by iterating through different particle aspect ratios? Can you make any comments about the effect that particle aspect ratio has on the resultant ridge geometry? *- We reworded the sentence in question. We chose the aspect ratio so that it was possible for the ice to fail into blocks that could have the aspect ratios observed in the field. Discrete element size (defining the smallest fragment size) was, thus, chosen so that pieces could reach aspect ratios down to 1.5 observed in the field (Høyland, 2007).* 6. Lines 101-102 - Do you have any comments on how variable thickness may affect the simulated results, or how they might contribute to differences between the model and experiment? *- Hopkins et al. (1999) conducted two-dimensional simulations with ridges forming from variable ice thickness and state that the unevenness influences the ratio of ridging and rafting, but such study is out of the scope of the paper. We added this information to the manuscript as well.*

7. Equations 2 and 3 - Can you briefly explain why the scaling parameters have different exponents for the velocity term in Equation 2 and the force term in Equation 3? *- The scaling parameter adapts to the terms scaled. In Cauchy-Froude scaling all length scales (L) being scaled with λ, time (T) with $\lambda^{1/2}$ and mass (M) with λ^3. Thus, velocity results in $\mathrm{LT}^{-1} = \lambda^{1/2}$ and force in $\mathrm{MLT}^{-2} = \lambda^3$.*

8. Lines 138-143 - Both the simulated and experimental data seem like they are constantly increasing. Are there statistical tests that could show evidence of the two phases? Perhaps compute a moving window average and evaluate its slope in each phase? Or compute a regression line for each phase, and then compare the slopes of each? Along the same idea - you mention that the change from first to second phase is more pronounced in the simulations than in the experiments - do you have any comments or thoughts on why that is the case? *- We assume that the phases are less pronounced in the experimental data as not all the ice within the experiment always purely ridges, but rather rafts as well along the length of the ridge. Based on your suggestion, we looked into the distinguishing of the phases via applying running means to the data and then comparing the slopes of these running means. For the simulations, this analysis shows a clear change in slope and thus, we argue that the splitting in stages is justified. Our motivation is also partially included in the answer of point 9. For the experimental data, we decided to not split the data in stages and for the simulation data, we decided to include the analysis of the slope based on the running mean. Thus, Figure 3 changes in the manuscript and*

additional information for the identification of the phases is included.

9. It's not exactly clear to me why splitting the data into two phases is significant. Is the main idea that if the ridging force is more or less constant in the second phase, and that you can then use that to formulate some F h law? Assuming you do not need to split into two phases, could you use the maximum/peak ridging force instead of the mean force in the data fit? *- In essence, we repeated the analysis by Hopkins (1998): We split the force records the data into two periods similarly to Hopkins (1998) (and a conceptual model of ridging in Tuhkuri and Lensu (2002)). It is correct that then idea is that the second phase has quasi-constant force and the relationship between F and h is investigated based on this force. Similarly, Hopkins (1998) used the mean force for the fit. Following the suggestion, we also investigated the fit for five highest peak forces per simulation during* $\delta = 40 \ldots 60$ *m. The results did not change; linear fit describes the data. Information on this is added to the manuscript.*

10. Line 145 - How were the simulated ridge profiles computed? Referencing Figure 2b, the bottom surfaces look more "bumpy" than the profiles in Figure 4. Were the bottom-most particle positions sampled at some sort of regular interval along the width? *- Figure 4 shows mean ridge profiles, which is why they look less "bumpy". For the experiments, the dashed lines represent the individual profiles, while the solid line is the mean of the individual profiles. For the simulations, the solid line again is the mean of individual profiles, while the shading is the standard deviation. To compare the ridges with each other, all data was centered around the deepest point of the keel. Additionally, both datasets where "regridded" in regards to the width (x-axis). That means, that depth of the ridge is displayed along a width with a regular interval of 0.2m between each step. We treated both datasets the same for consistency. We highlighted the word mean more in regards to the ridge profiles from the simulations and added some explanation in the figure caption.*

11. Line 156 - W appears to continuously increase, and does not "plateau." *- We agree that it is not a constant plateau, but rather periods where ridges do not grow in width. We adapted the text accordingly.*

12. Line 196 - What is the "higher" in comparison to? 90% higher std dev than what simulation case? *- The comparision is between the simulations with* $L = 10\,m$ *to simulations with* $L = 60\,m$*. We adapted the sentence to enhance clarity.*

13. Line 203 - Can you explain a little more what is meant by 'the setup as described above"? Do you mean your general simulation geometry? Or the general 3D DEM approach? Something else? *- ´the setup described above´ refers to the setup used for this study, which is described in section 2.2.1. We exchanged the word 'above' with a reference to the section.*

14. Lines 214-215 - Did you try to fit a FII h 3/2 type formula to this data? I would be curious to know what the Pearson coefficient is for that kind of fit. If you are arguing that a linear fit is more appropriate, then it makes sense to show the correlation coefficient of that fit, too, for comparison. The tail end of the data in Figure 7 appear to trend above the linear fit - did you run any simulations with thicker ice? It may be interesting to see if the F h relationship holds as h increases. *- We tested a fit with* $h^{3/2}$ *resulting in a Pearson coefficient of 0.99. We added this information to the manuscript as well as well as our justification to argue for the*

linear fit, based on the lower polynomial order. We did not run any simulations with thicker ice. We already extended the thickness compared to the initial validation and think that with these thicknesses we cover the majority of typical level ice thicknesses occurring.

15. Lines 218-219 - You reference your 2022 study that showed a linear relationship between ice load and thickness for simulations of ice against a rigid cone structure. Can you comment on the novelty of finding a similar relationship in this current manuscript? The larger floe in this paper was also modeled as a rigid structure, so the simulation setup seems fairly similar to the 2022 paper. *- The upwards-bending cone in the 2022 study had a diameter of 11 m. This is a narrow structure, which we included now into the text as well, and the ice failure process is different from here: In the case of a cone the process is inherently three-dimensional, whereas ridging has traditionally been considered a two-dimensional problem. Also in ridging, clearing of ice rubble does not occur.*

Grammar/Spelling Corrections

Suggested corrections are indicated in **bold**. 1. Line 1 - "...discrete element method simulations **of** pressure ridge formation." *- Changed.*

2. Line 21 - "**The** first theoretical models for..." *- Changed.*

3. Lines 76-77 - "We validated our simulations by comparing **the modeled** ridging force **magnitudes** and ridge profiles to those **measured in** the laboratory-scale experiments by Tuhkuri and Lensu (2002)." *- Changed.*

ADDITIONAL REFERENCES

Ranta, J. and Polojärvi, A. (2019). Limit mechanisms for ice loads on inclined structures: Local crushing. Marine Structures, 67, 102633.