Review: Three-dimensional discrete element simulations on pressure ridge formation

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1 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 ($F \propto h$), which differs from previous relationships in the literature ($F \propto h^{3/2}$). 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.

2 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.

2. Lines 67-68 - I understand that the plastic portion of f_n 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?

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?

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?

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?

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?

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?

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?

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 $F \propto h$ 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?

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?

11. Line 156 - W^* appears to continuously increase, and does not "plateau."

12. Line 196 - What is the "higher" in comparison to? 90% higher std dev than what simulation case?

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?

14. Lines 214-215 - Did you try to fit a $F_{II} \propto h^{3/2}$ 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 $F \propto h$ relationship holds as h increases.

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.

3 Grammar/Spelling Corrections

Suggested corrections are indicated in **bold**.

1. Line 1 - “...discrete element method simulations **of** pressure ridge formation.”

2. Line 21 - “**The** first theoretical models for...”

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).”