The Cryosphere: Reply to Reviewer 1

“Simulating landfast sea ice breakage due to ocean eddies using a discrete element mode”

General comments

The authors present an analytical derivation of the bond forces experienced by a one-dimensional assembly of particles under a velocity pulse located in the middle of the assembly. They show that it is expected that such an assembly would break in 1–2 points based on the slope of the velocity pulse. Next, the authors forced a DEM initialized based on MODIS imagery with the upper velocity layer of a quasi-geostrophic (QG) model. They performed a sensitivity analysis on different parameters of the DEM, as well as different deformation radii (using the same EKE) for the QG model. They demonstrated that the ice pack area decreases in size as the deformation radius increases. They also showed that the number of floes strongly depends on the bond strength, but that this does not reflect in the FSD, as all simulated FSDs appear somewhat similar. They showed that the difference was primarily due to a difference in failure mode (more tension failure for large deformation radius). The research conducted is meaningful and will likely be appreciated by the community. This manuscript contains good ideas, and we would like the authors to have the opportunity to publish it. But, in this present condition, we cannot recommend this manuscript for publication. Instead, major revisions are recommended.

Thank you very much for your very detailed comments and feedback. In particular, for helping identify errors in derivations of equations and figures, that led to modifications that strengthened the paper. In the rest of this document, we address in blue all the revisions or changes requested. Afterwards, we will submit a new version of the manuscript with the revisions implemented. We have checked the material parameters and stability conditions, corrected any inconsistencies between plots and expressions, added the considerations provided (such as compressive failure) and implemented the clarifications requested.

One major drawback of the simulation tool used is the lack of compressive failure of sea ice. It is misleading to forget the various failure modes of sea ice and to refer only to crushing when justifying the exclusion of compressive failure. It is well established that pressure and shear ridges are prevalent wherever ice deformation occurs, indicating that sea ice indeed often fails under compressive loads. DEM tools capable of representing ridging have been used for more than two decades (see work by Hopkins); omitting compressive failure is not acceptable. Further concerns arise related to the parameterization of the simulations. The tensile strength of 15 MPa assigned to sea ice in the model is about 75 times higher than values typically measured in small-scale experiments, whereas large-scale values would be expected to be even lower. Similarly, the stiffness value of $30 \mathrm{Nm}^{-1}$is unrealistically high. The authors must reassess their assumptions, revise their parameterization, and likely rerun their simulations to ensure that the results are physically meaningful. For example, on lines 98–101, the authors disable the bending and compression bond-breaking schemes, arguing that these breaking modes are generally not reached.

We should point out that the bonds in our simulations may break under tension, shear, and compression. What we had previously reported as tensile failure actually included both tension and compression, but the number of bonds breaking due to compression was rather small compared to tension. We now make the distinction between these modes of failure more clearly in the updated manuscript. Note that we now also use different values for the bond strength; we get the following proportions for bond breakage: 97-99% in tension, <1% in compression, and <2% shear. The ratio between these compressive to tensile strengths is about 4-10, being in line with literature (West et al. 2022). We pick the smallest value of this range.

While our DEM particles can indeed sustain compressive stresses and their bonds can break in compression, they do not contain ridging representation. This is in part because the floes modelled in this work have a scale of ~1 km radius, while ridges have typical widths of several meters. Any ridge representation in our model would then have to be much smoother than real ridges, which would likely limit their relevance for the simulations considered. Moreover, our interest in this work is primarily in the formation of cracks and individual floes at the ocean boundary, rather than in the formation of ridges. We acknowledge ridges will produce heterogenous sea ice thickness, which may in turn impact stress distribution and crack formation. However, our study is idealized, and we do not consider thickness gradients that may arise from many different processes, including melt, refreezing, and ridge formation. These thickness gradients and impurities in sea ice are likely important for crack formation, but studying their impact is beyond the scope of this study. Here, we instead focus on the effect of a highly non-uniform forcing (ocean eddies) on a uniform sea ice material.

We have modified the parameter values for the DEM to be more consistent with past literature, namely a particle stiffness of 500-5000 MPa and a bond stiffness of 12.5-125 MN/m (Table 1 and Table 2). We also adapted the tensile strength to 10 kPa - 40 kPa, which is also within the range used in past studies. These values were selected to allow breakage within our simulation time of approximately 28 hours. Accordingly, simulations have been rerun, and results will be added to the final version of the paper.

We are now doubtful that the results presented in Section 5 are robust. There are very concerning questions about exactly how the model behaves that are not addressed in the text, notably, the fact that the bond stiffness is $K = 30 \times 10^9\,\mathrm{Nm}^{-1}$, which is a whole order of magnitude above the values typically used. In and of itself, this is not necessarily a problem, but considering the very high time step size (10^1s) when compared to usual time step sizes in other DEMs ($\Delta t \leq 10^{-3}$s), it raises the question whether the model is fully converged or not. Such a large combination of time step size and stiffness will inevitably lead to large overlaps, therefore, to large forces. We are surprised that the model does not explode at the edge. This means that the damping term must be very large, and it should be discussed in the text.

Thank you for pointing this out. It turns out that we had misreported the actual time step used in the previous version of the manuscript, which was 0.92 s rather than 10s. This problem occurred because the model is configured to operate in non-dimensional units, and we had made a mistake in the conversion to physical units. We also inspected snapshots of the simulations and did not find evidence of any instability.

We have now rerun the simulations with the updated parameters (E = 500-5000 MPa, k_bond = 12.5-125 MN/m, tensile strength = 10-40 kPa) and a time step dt = 0.0125-0.09 s. The timestep choice and scheme are within the stability limit according to the CFL condition for DEM (Damsgaard et al. 2018).

We also note that the stability of the simulations does not depend strongly on the DEM damping term (i.e. they are stable even without any damping). Particle damping is added to represent non-conservative forces such as inelastic deformations and inter-particle friction.

Furthermore, considering that the entire study focuses on the interactions between water and ice, it is surprising that the total drag forces were not included in the analysis. The authors specifically say on lines 96–97 that ”The model does not include form drag forces, as those are negligible for elements with a large length-to-height aspect ratio, as considered here.”, which may be an unjustified simplification. Body drag accounts for ~ 14% of the total water drag on disk-shaped particles of 1 m thickness with 500 m radius. The ratio of form to surface drag in that case is morally $F_d/F_s = 2hC_d/(rC_s)$, and C_s is around 2 orders of magnitude smaller than C_d, which brings this ratio to order 1 for particle radius of order 10^2. Therefore, the authors have reduced stresses that propagate in the interior of their assembly. Indeed, these forces would only apply at the boundaries, but the stress still propagates, and it is not negligible. We don’t see a valid reason as to why these forces would be overlooked. It is clearly not for computational efficiency, and including them would only strengthen the authors’ argument and streamline the simulations.

We have now added body drag forces to the model, as proportional to ice thickness, perimeter, and ocean velocity, assuming sea ice buoyancy to determine the submerged height. Body forces are in the order of 10-15% of total forces and have a smaller impact than skin drag on simulation results, but are now accounted for. The body forces tend to make breakage happen slightly earlier and over a larger area. Given their smaller role, body drag forces are not considered for the theory section 3, where only skin drag is included for simplicity.

Along the same lines, as one of the main results, the authors state that “for the same amount of eddy kinetic energy, ocean currents with a larger characteristic eddy size penetrate deeper into the pack and fracture more floes.” However, the model used by the authors does not allow for such a conclusion. The model lacks two-way coupling between the ice and the currents and includes an ad hoc damping model for the current, where the user defines the damping distance. Very little can be said about the physics or the effect of eddy attenuation under the ice cover based on the model used. We recommend moving this conclusion and the description of this numerical experiment from the paper.

We agree with the reviewer that we cannot make a conclusive statement about the penetration of eddies within the pack without a coupled model. Nevertheless, our results show that for two simulations with similar average eddy kinetic energy, the simulation with larger eddies fractures more floes and tends to generate FSDs with a higher power law exponent. We have therefore modified the sentence in the abstract to:

‘For the same level of eddy kinetic energy, ocean currents with a larger characteristic eddy size fracture more floes and tend to generate FSDs with a higher power law exponent as compared to eddies associated with smaller length scales.’

Note that we have also modified the damping scheme for ocean currents to take account of the local sea ice edge location rather than the mean sea edge. From here on, we will refer to ice-to-ocean damping as attenuation to avoid confusion. We believe this scheme is more realistic than what was used previously, though a fully coupled model would be needed to accurately capture the penetration of ocean energy under ice. We discuss this in Section 5.1 and mention the need for a fully coupled model to accurately capture the attenuation under ice in Section 6.

On lines 179–180, the authors are turning off global damping: ”For these idealized simulations, the damping term in LS-DEM is turned off, allowing a more direct comparison with the analytical solutions from Sec. 3.” What type of damping are the authors referring to here? Depending on its type, damping should not be turned off in DEMs, especially with such high stiffness, time step size, and for 11 hours. What integration scheme are the authors using that justifies turning the damping off? Is the contribution from the damping accounted for in determining the stress state of the bond to decide if the bond fails, and is this the reason it has to be set off? It would be helpful to see the bond stresses over time and ensure that the breaking is not simply due to spurious oscillations of the forces.

Apologies for the confusion regarding the damping term. We will now refer to damping as the term included in the particle representation, representing non-conservative forces such as inelastic deformations and inter-particle friction. This is indeed the term we were referring to here. The term representing the effect of sea ice on reducing ocean current velocities will now be referred to as ‘current or ice-to-ocean attenuation’.

The only reason for excluding damping in the uniaxial tension experiments was to draw more quantitative comparisons with the analytical solution developed in Section 3, which does not consider these forces for simplicity. The damping term does indeed contribute to the total bond force, which is used to break bonds. Simulations to test the sensitivity of damping were done and revealed that DEM damping does not affect results as much as the oblique orientation of bonds. Animations of the breakage process are also provided in the video repository to track the development of stresses.

Section 3 on breakage length under uniaxial tension is well-constructed and could prove to be an effective method for testing bond schemes in discrete element models. On a casual note, it is also the reason why uncooked spaghetti breaks in more than 2 parts most of the time when you bend it (Feynman’s spaghetti problem). A similar analysis of the bending failure mode would reveal a similar behaviour.

Thank you for the positive comment and for the nice suggestion. We have briefly looked at Feynman’s spaghetti problem as in Zhang et al. 2022, and we believe it is indeed similar to our formulation. The failure of inter-floe bonds under spatially varying ocean forcing is analogous to the Feynman spaghetti problem, in which fracture is governed by gradients of elastic loading along an extended structure, rather than by absolute forcing. In our case, it is the sinusoidal idealization of the ocean velocity that causes these symmetric or periodic gradient regions. This is even clearer if we look at Figure 3.a. showing the derivative of the sinusoidal segment we are utilizing.

However, special attention should be paid to the coherence between the math, the graphs, and the text. The results should be adapted to reflect these changes. The method presented in section 3.2, while interesting, is plagued with issues in the coherence of the math, graph, and text, which lead us to doubt the subsequent results. Indeed, Eq. 13 does not lead to the graph presented in Fig. 3b (see specific comments below). The function should be zero when evaluated at λ_{cut} as described in the text (Eq. 14).

Apologies for the confusion. A plotting issue with Python related to an arcsin library was identified, and now Eq. 13 is plotted in its correct form. A new figure 3b. has been produced to address this issue. Equation 12 had an algebraic error when derived from Equation 11, and this has now been corrected to be consistent with Equation 13.

Maybe a better approach, as highlighted by the authors in section 4.2, is to simply use the maximum of the bond force as a reference point (at $\lambda/8$) to benchmark whether a model's bond schemes work properly in tension. Moreover, based on the authors' own math/figure, you can't use $\lambda > 40$ km for velocities $U_0 =$ 0.06 m/s, hence how is a $\lambda = 200$ km used for $U_0 = 0.03$ m/s? Shouldn't that be above the maximal allowed value as per equation 13? Section 4.2 will need to be rewritten to accommodate the changes required in Eq 13. Furthermore, if the authors were concerned about reproducing the exact theoretical calculation with the model, they should have performed their validation experiments in Section 4 using a simple 1D rod of identical particles. They point out this exact issue on lines 198–199.

Equation 13 has the right form as explained above, and we have modified its plot. Section 4.2 has been reviewed and for the values presented we have plotted results again. A 1D rod of particles could be simulated, but the richer behavior of a rectangular sheet provides a better insight on the limitation of this idealization. Even with multiple particles, simulation and analytical results are close enough.

In equation 15, y_{edge} is a free parameter, and the results (fig. 7) clearly are critically dependent on its value. The authors say, "The conclusions of our work are not too sensitive to this definition, since results without this kind of damping follow the same trends." Therefore, why include it at all?

We agree with the reviewer's comment. In the previous version of the manuscript, the attenuation scheme depended on the mean sea ice edge location. Since our ice edge is irregular, some parts of the pack did not experience much attenuation and could break easily with or without the attenuation, which likely reduced somewhat the sensitivity of the subsequent breakage behavior to that parameter. Upon reflection, we believe that it is likely more reasonable to base our attenuation scheme on the local rather than the mean sea ice edge. The new simulations include this new scheme. We have also conducted some experiments with different attenuation values and indeed find sensitivity to that parameter. This prescribed attenuation scheme is certainly an idealization, and a fully coupled model would be required to accurately account for it. We mention this in the discussion section.

In Section 5.3 and Figure 7, we have concerns about the simulation results. We can see these long, thin rods of particles stacked on top of themselves. This seems to point toward the fact that their bonds can only break in tension and shear. The fact that the "center" of the broken parts is entirely broken should be addressed in the text. Does that happen all at once, or is it building up? When examining Fig. 8, we can observe these sharp steps in the different panels. The authors note that the FSDs are all somewhat similar, despite the number of floes varying significantly. This is not something we would have expected, given the characteristics of the vorticity field. We would have assumed fewer larger floes and more small floes for r^d_5 when compared to the simulations with r^d_{30}. How is the radius computed in the FSD? Because all the resulting FSDs appear the same, this suggests that the model may simply explode in bursts rather than fracture. The fact that the mode of failure differs (tension vs. shear) does not give us confidence that the model is not exploding; it just does it differently.

Regarding numerical instabilities, our code is designed to report occurrences of high overlap between particles, and this does not occur during our simulations. Moreover, we do not notice any particles being stacked on each other in snapshots of our latest simulations. We have chosen a time step based on the CFL condition for the material parameters, which ensures that numerical instabilities do not arise. This choice is now detailed in Section 2. As requested, we have provided animations for our simulations. Those do not show any evidence of spurious bursts in the fragmentation. The bonds may break in tension, compression or shear. For our updated choices of parameters, most bonds break in tension, but some also break in compression and shear.

Regarding the reported FSD slopes, the equivalent radius or diameter is computed by taking the total area of the bonded floe segments and finding the equivalent diameter resulting in this area. The breakage behavior under turbulent eddies in Section 5 is consistent with breakage under the pulse forcing discussed in Sections 3 and 4. In the rising portion of the analytical curves shown in Fig. 5b, a larger forcing length scale leads to a larger breakage area, and within that breakage area, we find that all bonds are broken (Fig. 4). This behavior leads to larger scale forcings producing a large number of small floes, and thus a slightly steeper FSD slope (Fig. 8d). In reality, fragmentation is not near-instantaneous, as assumed in our DEM, which could allow some stress redistribution within the susceptible breakage area, and prevent all the bonds within it to break all at once. This could affect the FSD behavior reported here, but it is not within the scope of this study to explore.

Broken patches and breakage at the center of the sea ice pack are dependent on the variability of the ocean current gradients and the ice-to-ocean attenuation model. If ocean currents are not attenuated, then transient areas of higher gradients may result in isolated and sporadic broken patches, This occurrence has now been explained in the discussion and results sections.

While we affirmed that FSDs look similar for the same bond strength, it is also evident that the slope is controlled by both strength and currents. Weaker ice will break into smaller fragments, generating a more uniform FSD with numerous small floes. Stronger ice will break into less small fragments, decreasing the FSD slope or power law exponent. We imply that rd_30 will generate more small floes than rd_5. Sections 3 and 4 showcase that longer or more diffuse eddies break more sea ice overall and will produce more smaller floes and can generate a larger number of big floes, as longer currents are more successful in breaking sea ice than concentrated filaments with a more limited range. These results are consistent with Figure 8c.

Specific comments

line 7: ". . . a slightly higher FSD slope *when* compared . . . " what FSD slope refers to isn't clear, please reformulate.

FSD slope is now referred to as the power law exponent in the abstract and the rest of the paper. We will add this clarification for Section 5, as well.

line 9: ". .. notably shallower FSD." what does shallower mean for a distribution?

This indicates a lower power law exponent; the text has been changed.

line 19: ". . . by approximately one half . . . " what are the units? Meters?

It is one half of its original value from 1980s estimates. The text has been modified to indicate this.

lines 19–21: This sentence does not flow very well, please reformulate.

Sentence rewritten and split into more than one sentence for better flow.

line 34: ". . . arrested . . . ", not sure arrested is the right choice of word.

The word has been changed to ‘dampened’.

lines 34: ". . . ice–ocean . . . " usage of an en dash in this case, please check throughout the manuscript.

All cases for this combination have been modified to en dash.

line 37: ". . . wave–ice . . . " en dash.

All cases for this combination have been modified to en dash.

lines 40–42: This sentence is hard to follow, please reformulate.

Thanks for the advice. The sentence has been reformulated.

line 85: "A given pair of elements is connected via one bond at most, for simplicity." Isn't that a given? Because $F_{ij} = -F_{ji}$.

Some bonded discrete element methods, particularly for very irregular grains (including some iterations of LS-DEM-BPM), can use multiple surface bonds instead of just one bond between two grains. That is the reason this additional explanation is provided, to avoid ambivalence for other potential configurations.

Eq. 13: This is a major comment. See above. We think we understand the authors' definition of the breakage length. Our understanding is that it is the length over which the bonds linking the particles in the row will be broken, effectively breaking the row into two or three parts. The length of the broken sections between the parts is given by that equation. Please clarify the text. Moreover, there is a factor of 4 missing in front of the arcsin, based on Eq. 12.

Typo in Equation 12 has been corrected. We have clarified the definition of breakage length as the total of separate lengths across the ice pack.

fig. 3b: This is a major comment. See above. The graph does not make sense with Eq. 13. Indeed, at λ_{cut}, the breakage length should be zero: when putting Eq. 14 in Eq. 13, one gets $l = \lambda(1/2-\pi^{-1} \arcsin(1)) = 0$. Even considering the missing factor of 4, one still obtains negative values. Whatever is plotted here is not Eq. 13, as presented. We suspect the following: the factor of 1/2 in Eq. 13 became 3/4. What was indicative of that is the fact that $\lambda_{cut} \approx \lambda/4$ in the figure, and this is the only way to achieve this. The fact that the equation is zero on the right side of the domain should be addressed in the text.

Plot of Equation 13 has been updated to remove numerical plotting error from arcsin function.

line 163: "l^{br} simply scales linearly with λ, as long as the forcing magnitude is strong enough". This statement is true irrespective of the forcing applied (as long as you respect Eq. 12). If $t \to \infty$, then $l^{br} \to \lambda/2$. The $\gg$ sign inside the parentheses is confusing, because it seems to imply the following: when $rK\gamma t^2U^2 \gg F^c\lambda/2$ $\Longrightarrow$ $\arcsin(x) = x + O(x^3)$ around $x = 0$. And $l^{br} = \lambda/2 - A\lambda^2/\pi + O(\lambda^4)$, where $A = F^c/(2rK\gamma t^2U^2)$, which is not what the authors are saying in the text.

The fragment has been rewritten to avoid any confusion.

lines 184-185: We have a hard time understanding how you estimate breakage length.

Breakage length estimation for the simulation is now more explicitly explained.

line 191: "λ = 100 km" but in fig. 4b it says 200 km. Which is it?

Text corrected to lambda = 200 km, which is the right value.

lines 193–194: You would expect that to be the case, no? Because F_{bond} is also a function of time, since it is based on the elongation, and the elongation needs a few time steps to build up. Once the stress is relieved, you would not expect new breakage lines to form. See above.

Yes, breakage length over lambda/2 is expected to be the case. Once stress has developed in the breakage length, there will be no new lines, unless the sea ice pack advects significantly.

lines 196–198: "We find that l^{br}_{sim} is consistently lower than $l^{br \cdot}$

That should come as no surprise, considering that Eq. 13 is not what they used.

Passage modified to fit the correct plot of Eq. 13.

lines 201–202: "Boundary effects at the edge of the pack may also play an important role." We are not sure that that's the case. We believe that most of the discrepancy arises from the misuse of Eq. 13 and the shear forces. For example, in Fig. 4a, it appears that the pack started separating at the top and fractured downward, which is why there is an X breaking pattern at the bottom.

Passage modified to fit the correct plot of Eq. 13 and also use of new material properties.

lines 205–206: "On the other hand, when the forcing is more diffuse (large wavelength), the breakage is spread over a larger area, but the fracture lines are less well distinguishable." That is not what we see from Fig. 4. We observe clear fracture lines in both cases, with residual stresses in the bonds resulting from the weak velocity gradient. What we are seeing is different magnitudes of local divergence, but the fracture lines are both well-defined.

Sentence has been modified to better explain the difference between longer and shorter wavelength breakage while also acknowledging they both have well-defined fracture lines.

line 208: "ocean–ice" en dash.

Change done.

fig. 5: Needs to be redone in light of Eq. 13.

Figure 5 has been updated to correctly plot Equation 13.

table 2: It is concerning to me that the bond stiffness is that high. See above. Its units are in a different format than those in Table 1.

Format has been made uniform for stiffness units. A lower stiffness is now being used.

fig. 7: We don’t think the floe color scheme is optimal. Can you change it?

The color scheme has now been changed to a more structured gradient that facilitates visualization rather than simple random color assignment. The main sea ice pack sheet is always colored with the same dark gray.

Sincerely,
Antoine Savard and Arttu Polojarvi