RESPONSE TO REVIEWS

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"Discrete-Element bonded-particle Sea Ice model DESIgn, version 1.3. Model description and implementation"

I would like to thank the Reviewers for their valuable and insightful comments and questions.

Before I proceed to answering them, I'd like to clarify one thing, regarding the wave-related part of the model, as this is the part that received most of the (well-grounded) critics:
I am fully aware of the incomplete character of that part of the model and, as the Section 6.3.1 says, it should be treated as experimental and preliminary. It is a starting point for a more advanced treatment of wave–ice interactions. Some additional elements are being tested at the moment, other are planned for the future.
The purpose of the results presented in Section 8.2 is to verify the functioning of the present version of the "wave algorithm". Because it takes into account only some isolated aspects of wave–ice interactions, it illustrates just a part of the whole picture. But, in my opinion, studying selected aspects of complex processes can be instructive. Moreover, the manuscript has been submitted as a "Model Description Paper", and therefore I decided to include in it the description of all model features available in the present version. Some of them are well tested (e.g. those analyzed in my previous papers, which are only briefly mentioned in the present manuscript; or the functioning of bonds in purely 2D model configurations, without waves), whereas other are in the preliminary, development phase. The main goal was to make the model available to everyone, with a full description of its components and a technical documentation.

In the attached corrected manuscript, all modified or new fragments are marked in blue.

RESPONSE TO THE COMMENTS OF REVIEWER #1

`1.1. Bonded elements response to tension, compression and shearing.`
`(1) The results look very nice – my main question is about the reproducibility of results with smaller grain sizes: eg how much do macroscopic quantities like the area of unbonded grains or integrated stresses change? As the author points out in the results section, under compression the unbonded grains transmit a lot of the stress.`

The size of the grains does have influence on the macroscopic behavior and properties of the modeled material. This is the case in discrete-element models, much in the same way as in continuum models. For example, my experience with continuum sea ice modeling in the Baltic Sea shows that changing the model resolution requires recalibrating the model coefficients, including those related to the sea ice rheology. In the case of DEM, the size of discrete elements determines the size of the features – like fracture zones etc. – that can be resolved. Larger grains typically lead to wider deformation zones.
Usually, after the mean size and the size distribution of grains for any particular model application is determined, a series of simple simulations similar to those presented in Section 8.1 is performed in order to calibrate the model coefficients (i.e., material properties of grains and bonds) until desired macroscopic properties of the material are obtained.
Also, obviously, the size of grains should be adjusted to the characteristic spatial scale of the forcing, for example the wavelength in the case of ocean surface waves (a topic you raise in your further comments).

`(2) Perhaps randomisation/variation of properties instead of having constant properties might make a difference? (I will return to this point when I discuss the waves part.)`

The randomization of certain properties of grains and/or bonds certainly influences some aspects of the model behavior, whereas it has little influence on others. I discuss some examples of these effects in Section 8.1, where the influence of the width of the bond-thickness distribution δ_h on the damage of the sample is analyzed. For example, simulations show that the slope of the stress curves in Figs.6a,b and 7a,b is not sensitive to this parameter, but the spatial pattern of breakage is different for identical and variable bond thicknesses – if many weak bonds are present in the sample, they provide many potential spots where breaking can be initialized.

1.2. Waves causing ice breakage.

(1) Around eq 31, p5500: There are some big assumptions here — mostly relating to assuming that the wave is unaffected by the ice. Local effects at the edges/corners of each floe are neglected, but when the floe is small enough compared to the wave it might be reasonable to make this assumption. (This should be mentioned at least in the paper.) Alternatively if the floe is very large compared to the wave, it might be reasonable to modify the dispersion relation (DR: relates ! and k) — eg use the thin elastic plate DR.

Yes, I fully agree.
The waves in the present version of the model are given and remain unaffected by the ice, which is a very serious drawback.
Some of the effects that you mention, e.g., floe-edge effects, are probably not very relevant in the simulations presented in the manuscript, especially in the initial phases of the simulations when there is a single, huge ice floe covering the whole model domain. However, these effects may be of course extremely important in other situations. Properly taking them into account would require coupling the model with a wave model.
As for modifying the dispersion relation, it can be done quite easily, as the wave parameters (wavenumber, frequency etc.) may be both space and time dependent. Also, note that the calculation of the torques acting on grains (Section 6.3.3) is based on the local slope of the sea surface, so that it can be done for any given $\xi(\mathbf{x},t)$.

I added a comment on that at the end of Section 6.3.1. Possibilities of coupling the DEM sea ice model with CFD and PFEM models, and thus of fully resolving the wave–ice coupling, are also discussed in the (slightly extended) discussion in Section 9.

(2) Assuming one wavelength is long enough to fit enough discs (perhaps about 10?), the discs could be placed along the profile of the wave and could follow the 3d orbital motion of the wave. Eqn (31) would give the horizontal motions (uwv,i) and vertical motions _i of the discs. Also the tilts of the discs could be calculated from the gradient of the wave ∇_ = ak cos('). The relative motions of the discs should surely be enough to compute the forces due to the connecting springs. This requires high spatial resolution but I think this is needed to model the break-up of individual floes due to waves as the author proposes.

But this is exactly how this is done!
For example, in the reference model run (Table 2), the diameter of the largest grain in the sample was less than 8% of the wavelength. On average, there were 25 grains per wavelength. The individual grains experience torques resulting from the sloping sea surface (equation 34) and – if they are bonded to their neighbors – moments transmitted by the bonds.

(3) Following on from the previous point, I don’t think the model shown in Fig 4 would be able to resolve a wave.

See above. Figure 4 shows a single grain, which is small compared to the wavelength. It may be bonded to its neighbors, but this is not shown in the figure.

(4) It was very hard to judge the results shown. Perhaps these “interesting results” presented (range of floe sizes) correspond to cases where the waves are not resolved, and the boring ones are when they are? Maybe a simpler case could be presented eg a plane wave coming in with breaking being expected in parallel lines every half wavelength or so. It would be very interesting to then see if other forces could break the floes in the other direction — ie long strips seem quite rare in the field, and one question is how do they end up as rectangles or other shapes? One explanation could be pre-existing weaknesses, but another could be that forces like drag combined with some heterogeneity in drag coefficient or

bond strength make these long strips very prone to further break-up. In other words, it could be very interesting to investigate the interaction between waves and other forces to see if the "sharp" FSD produced by swell waves could be turned into something resembling a power-law like FSD. Perhaps check the video of Dany Dumont (https://vimeo.com/106835989) for a potential simulation (swell waves plus a shearing current).

Thank you for the link to the video!
As for your questions:

1. The waves are resolved in all simulations, as I already wrote earlier. What changes is their length, and thus steepness, and thus potential to break the ice.
   I'm afraid that the fact that I used the term "interesting" to describe the relatively wide FSD obtained in between the "no breaking" and "strong breaking" cases could be a bit misleading. I find the results interesting from the point of view of understanding the model behavior, but I don't think they are very relevant for real-world situations. To obtain this non-trivial behavior, fine tuning of the model parameters is necessary. In other words, the properties of the ice – its strength, thickness etc. – have to be adjusted to the properties of the waves, which is unlikely in real-world conditions. I don't think that this mechanism is a likely candidate for explaining the observed wide FSDs in the MIZ.
2. The results presented in the manuscript have been obtained with simple plane-wave forcing that you propose. However, obtaining parallel stripes as a result of breaking would be possible only with identical grains arranged on a regular matrix, with bonds aligned either parallel or perpendicular to the wave direction. In the configuration with randomly distributed grains with variable diameters, the geometry of the system prohibits formation of very long, perfectly parallel stripes.
   The problem is analogous to that analyzed e.g. by Hopkins et al ("Formation of an aggregate scale in Arctic sea ice"), where the method of discretization of the modeled ice cover (in their case: Voronoi tessellation versus Delaunay triangulation) influenced the resulting pattern of ice floes.
   If one looks carefully at Fig. 13, elongated floes composed of linearly arranged grains can be seen.
3. I strongly agree with your suggestion that not waves themselves, but another process is responsible for transforming the initially narrow FSD in ice broken by waves into very wide FSD often observed in the MIZ. I think shearing combined with compression is a very likely candidate, i.e., a process analogous to that known, e.g., from fracturing of rocks and glacier ice, when grinding of initially almost monodisperse fragments leads to power-law fragment-size distributions.
   I'm planning to perform some simulations related to these processes in the near future – but only after improving the wave-related part of the model.

I modified/extended the discussion in Section 8.2 with some of the above –discussed issues.

2. Specific comments
(1) p5491, L4: "new bonds may be created" — this is quite interesting. Perhaps there should be a minimum time that 2 floes should be next to each other — maybe enough frazil ice or something like that that could act as a glue between floes. It would be interesting if the "herding" of small floes into larger ones could be reproduced somehow.

The herding, or cluster formation, of ice floes can be reproduced by the model – it was a subject of my earlier work on DEM modeling of sea ice. Clustering under the influence of wind or, in the simplest case, due to inelastic collisions between the ice floes, can be reproduced even in very simple model configurations.
The subsequent freezing of neighboring floes is another matter and, exactly as you point out, it requires some rules for "producing" new bonds. The rule you propose makes it necessary to keep track of the "history" of the system (i.e., how long neighboring grains remain close to each other). This possibility is not available in the present model version (unless the grains are in contact), but of course it can be implemented in the future. Presumably, it would make the model quite expensive computationally.
In any case, in the present version of the model, new bonds can be created based on simple criteria, i.e., instantaneous distances between grains. I think even this simple method could work quite well provided that the new bonds could have very low initial thickness and strength that would then increase in time. But I haven't tested this option yet.

`(2) Perhaps it would be easier to understand the differential forms of (14-17)?`

These equations are written in this way everywhere in the literature on bonded-element models – presumably because it is a specific formulation of the Hooke's law, typically written as $F = kX$. I added a short comment on that before equations 14,15.

`(3) p5508 L1-5: I would expect the opposite: large floes to flex with the wave, while smaller ones should behave as rigid bodies - moving with it without bending very much. Note that the "stress breaking criteria" of [1] was removed by [2] for the reason that it became easier for longer waves to cause breaking than short ones (when in fact the slope becomes smaller as they get longer).`

See the modified text in Section 8.2.

RESPONSE TO THE COMMENTS OF REVIEWER #2

`1) section 4 should describe in more detail the origin of the grain-grain contact forces. They may be described in lots of detail in one of the other papers, but a brief description of the physical origin of the forces should be given. For instance no mention is given of pressure ridge formation during convergence - is this process the origin of one of the contact forces? How is ridging handled?`

But the contact forces are described in detail not only in "one of the other papers", but first of all in the Supplementary Note S1! It contains all relevant definitions and equations for both spherical grains (typically used in DEM models) and disk-shaped grains used in DESIgn. Additionally, there are also figures illustrating the variability of the contact forces with changing disk thickness etc.
As for ridging, it is not taken into account in the present version of the model – which is a serious limitation in some applications. I'm planning to include a crude ridging parameterization in one of the next model versions, in which the material of a bond broken in compression will be used to increase the thickness of the two grains "belonging to" this bond.
In any case, in my opinion ridging can never be an "origin of one of the contact forces". It is a mechanism of dissipation, not of force generation. There are many different contact models, but in general, the two most important forces are the repulsive force (normal force experienced when two objects are pressed together) and the friction force (tangential force resisting the sliding motion).
In the discussion in Section 9, I added some comments regarding the lack of ridging parameterization as one of the limitations of the present model version.

`2) A more detailed description of the numerics should be given. Are the equations of motion solved implicitly or explicitly? With a velocity Verlet solver? What are the timestep constraints? How do they scale with floe size? How computationally intensive is the model?`

I wrote this part of the manuscript in a very compact way – partly because my contribution to the numerical part of the model is very minor, and the sea ice toolbox fully uses solvers and tools provided by the LAMMPS/LIGGGHTS libraries; and partly because the technical documentation of both DESIgn and LIGGGHTS provides all necessary information concerning numerical aspects of the model.
I know that the supplementary material accompanying the manuscript is not subject to review, but in this case it is an important part of the whole paper (I mean both the Supplementary Material, and the User's Guide available in the `doc' folder that is part of the model code in the attached zip file).
Nevertheless, following your suggestions, in the revised manuscript I extended Section 7 with some additional information – although it still remains rather short.

`3) The floe size distribution seems non-uniform from the diagrams but I didn't see a description of how it is initialized?`

The grain-size distributions used in the simulations, together with other model parameters, are given in Tables 1 and 2. In all examples presented in the manuscript, a uniform grain-size distribution was used (see also the next comment below).

4) The model is described as able to achieve compact ice cover. How is this possible with circular floes that with hexagonal close packing you would expect a maximum ice coverage? What is the maximum coverage achievable with circular floes?

This depends on the size distribution of the circles. With power-law distribution, a 100% coverage is possible (see, e.g., a very nice paper on “Space-filling bearings” by Herrmann et al, PRL 1990). Of course, this is not possible in a discrete-element model, where a prescribed lower limit on the possible grain sizes reduces the maximum attainable packing to <100%. For example, in my previous works (Herman 2013a,b,c), in which the power-law grain size distributions were used, concentrations within 90-95% were achieved without any problems.

In any case, identical grain sizes are almost never used in DEM models in order to avoid formation of regular, hexagonal packings that you mention. As mentioned in the previous comment, in the case of the simulation presented in this paper a uniform grain size-distribution was used.

Also, do not forget the bonds between the grains: in the context of the present model, the term “compact ice cover” is used to describe ice composed of grains that are fully bonded with their neighbors – as stated, for example, in the first sentence in Section 8.1.

Specific comments: ———————

p5483: l5-7: Implies observations have no use in the study of sea ice dynamics.

I don’t agree. This statement is clearly related to modeling, to the difference between models resolving physical processes of interest and those parameterizing them.

p5483: l23: I’m a bit confused my the name of model. I think its DESIgn, but the way the capital letters are written implies is DESI-DESIgn. I think it would be clearer to say Discrete-Element, bonded particle Sea-Ice model design (DESIgn).

I’m afraid I don’t understand. What the word “design” in your version is for? It’s not part of the name.
The name could be DESI, but DESIgn simply sounds better.

p5484: l3: "as described in last section" -> "as described in the last section"

Corrected.

p5484: l10: "but it enables to take into": Not correct English

What is wrong about that? My English is definitely far from perfect, but in this case everything seems correct to me (even after I’ve consulted the Webster dictionary…)

p5486: l9: "dilatation" - explain what this is

I slightly modified this sentence. Now it says: “…a parametrization of dilatation effects, i.e., flow-induced expansion of granular materials accompanying their slow shear deformation and related to the directional distribution of contacts between neighboring grains.”

p5486: l14: "allows to obtain": Not correct English

Changed to “…permits ice concentrations …”.

p5487: l4: "more suitable for MIZ" -> "more suitable for the MIZ"

Corrected.

p5488: l29: "ice cover Herman (2013)." -> "ice cover (Herman 2013)"

Corrected.

p5489: l1: "polydispersity": define

The new formulation is: “polydispersity (i.e., heterogeneity of sizes) of sea ice floes and the role that it plays in sea ice dynamics”

p5491: l8: "ice grains with a constant density nrho" maybe change to "ice grains each with a constant ice density nrho". On first read I thought density referred to the number density of grains rather than a material density of the individual grains.

Changed to: “…grains, each with a constant mass density ρ”

p5491: l18: "are still equal zero" -> "are still equal to zero"

Corrected.

p5492: l21: "net momentum" surely this should be torque or moment rather than momentum.

Yes. Corrected.

p5495: l16: "not included in calculation" -> "not included in the calculation"

Corrected.

p5496: l9: "damping coefficient" Explain what this is for.

A short comment on the damping coefficient has been added.

p5497: l17: "enables to specify": Not correct English

As previously, I think it is correct.

p5499: l5-10: How are the C constants determined?

As in every sea ice model (or ocean circulation model, for that matter), the drag coefficients are adjustable model parameters. The formulae that are used for the atmospheric and oceanic drag in Eqs. (24)-(27) are standard in sea ice modeling.

p5499: l9: "The torque of Fai equal zero." This is an assumption. Clarify.

This is *not* an assumption. But it is a direct consequence of an assumption made earlier, namely that \tau_a does not depend on the floe’s velocity. When we calculate \tau_a based on the wind velocity alone, the natural consequence is that the net torque due to wind vanishes.
A comment on that is added in the modified manuscript.

p5499: l15: "In MIZ" -> "In the MIZ"

Corrected.

p5505: l26: "amounts to a sudden hit into the modeled sample" rephrase

Rephrased to “…amounts to suddenly hitting the modeled sample”

p5505: l27: "with wide": with wide what?

“with wide damage zones”

p5505: l27: Figure 10d doesn’t seem to exist.

Corrected to Fig. 5d.