

Summary

The authors describe a new sea ice Discrete Element Model based on polygons. The model is unique because it uses quaternions to solve the equations of motion on a sphere and conserves energy. The paper is split into two parts. Part one describes the model, while part two mostly describes its validation. It is unclear to me why the paper is split into two parts, and I strongly recommend that the authors consider merging them, as it would streamline the text, reduce the length, and make it easier to read. Part one does a relatively good job of describing the model; however, it consistently overlooks explaining the reasons behind certain modeling choices. Also, the order of some sections is confusing. I recommend moving parts of Section 3 into Section 2, which would reduce the time spent scrolling between pages to find definitions on the same topic. Major revisions are recommended.

General comments

The authors should discuss the model's regime of applicability in much more detail, and much earlier in the text. I was confused about the regime to which the model applies, both in space and in time.

line 262: In the collision model, it is said that the force "must" be on the intersection centerline. I am not convinced that this is true. Wouldn't one expect the force to be weighted by the amount of deformation along that line, and therefore be closer to the peak on the right in Fig. 3? Another point about the contact model is that it does not seem to include any tangential forces or moments due to the collisions. The model should either include these or state clearly that it omits them.

Regarding the scaling factor γ , I find that the argument for using polygons over disks is moot given its use. For a given polygon area, $\bar{s}$ scales with the inverse of the number of sides for regular polygons (this also raises the question as to what happens with extremely thin polygons, because in that case, the perimeter can be made to be arbitrarily large). For example, a regular triangle, when compared to an octagon, has $\bar{s}_3 \sim 3\bar{s}_8$. How does one choose the number of sides of the polygons? If one has too many, calculation time grows; if one has too few, the forces become smaller, and the pack becomes less responsive. I find that to be an advantage of using disks.

Regarding the joint failure. Is there a reason as to why the authors chose a rectangle as the bounding region? This is an important modeling choice, and I see no justification for it. I am also confused about durability. In Section 2, it is implied that failure occurs when a critical stress is reached, but Section 3 presents a different view. I recommend moving durability to Section 2 and removing Section 3.2 altogether. The same applies to Sections 3.1 and 3.3; I would move them to Section 2.4, and present the full elastic force right away.

Section 3.4: I don't understand why contacts include durability. What happens when collision durability reaches 0? To me, this seems like a problem that needs to be addressed because the collision springs are linear. Shouldn't extreme overlap simply be considered as ridging instead?

Section 4.1.1: I think this section on quaternions should be expanded, as this is the model's novel part, and it is insufficient, as is, for comprehension. Maybe as an appendix?

Specific comments

line 13: ...mind, like others in its class, this model ...

line 47: I disagree that polygons are fundamentally better than disk formulations. The 100% coverage really only applies at the start of a simulation. In Hopkins' case, the model's floes were reinitialized once in a while to reset the coverage. This is because angular gaps between elements can occur when there is overlap.

line 65: Use `\citep` instead of `\citet`

line 66: ...at different scales ...and use `\citep[following][{}]`

line 71: The contact law should always be a function of the contact area, not overlap.

line 93: I see no reason why this paper should have two parts. It makes reading more tedious.

line 105: That was not clear to me before. Therefore, I was confused by the word "planetary" earlier on line 15. Can this be mentioned earlier?

line 138: The authors might want to consider doing a velocity-Verlet integration. It's much more stable, second-order, requires only one force calculation, and is trivial to implement.

line 180: Shouldn't there still be moments even when there are no bonds due to the geometry of the contact plane? Or does that refer only to moments due to the bonds?

line 206: It is unclear to me how the authors define w_b . Is it changing over time? Taken from the previous timestep? Defined at the start of the simulation because that is the only moment when the two sides are the same length?

eqn 13: The authors should d the integration d and not the w to be consistent with the line above. Overall, check symbols throughout the manuscripts.

lines 209–213: Very interesting. I don't have an opinion on which view is better.

lines 222–223: I don't understand this sentence. Please reformulate.

eqn 14: What are the units of η ? The overall units are not Newtons if we use the standard units for viscosity, $[\eta] = \text{Pa} \cdot \text{s}$.

line 261: Do not start a sentence with a symbol.

line 263: Missing space "...force,the...".

line 308: Do not start a sentence with a symbol.

eqn 27: The absolute values around σ_s are missing the left part.

line 357: "...is presented *in* an..."

line 426–427: This sentence has a syntax problem; please reformulate.

line 477: This equation comes without any explanation. Either explain it or remove it and the paragraph before.

line 483: For the reasons given above, I do not think that polygons are inherently better than disks, given the model presented here. It is simply a different modeling choice.

line 514: You get the same problem as in a uniformly distributed disk-based model.

line 544: I point out that initial conditions are complicated for all DEMs, as the authors show in Section 5. I don't think that disk- or polygon-based models have an inherent advantage over the other in that regard.