

**RESPONSE TO THE REVIEW**  
**of the manuscript No. gmd-2015-112**  
**“Discrete-Element bonded-particle Sea Ice model DESIgn, version 1.3.**  
**Model description and implementation”**

**RESPONSE TO THE GENERAL COMMENTS**

The general comments of the reviewer were – as previously – related to the sea ice–wave simulations. Already in his/her previous review, the reviewer had suggested to perform simulations in which the positions and tilts of the grains would be prescribed, so that the stresses acting on bonds would be the only prognostic variables. However, if I understand the suggestion correctly, there is nothing really interesting to calculate in this case. If the shape of the ice floe, i.e., the positions and tilts of all grains are prescribed, then the stress acting on bonds (which depends only on the relative motion of neighboring grains) can be directly calculated from the wave profile and reaches maximum values in locations with the largest curvature of the floe. As the wave propagates across the floe, these locations of maximum stress travel with it, so that every bond experiences it twice in every wave period. If all bonds have the same strength, either all of them or none at all will be broken. There’s no interesting behavior, no dependence of the floe motion on its size etc., because breaking doesn’t influence the motion of the grains, which remains prescribed before and after breaking.

Of course, I fully agree with the reviewer that the approach used in the paper is wrong in the sense that it does not take into account the influence of the ice on waves. I am planning to do more extensive simulations in the nearest future, in which I will consider different model configurations – from the simplest one used in this paper, up to coupled simulations in which the wave parameters will be modified by sea ice – in order to analyze how these changes in complexity influence the pattern of sea ice breaking.

**RESPONSE TO THE PARTICULAR COMMENTS**

(All most recent changes to the manuscript are marked in blue in the text.)

(1) quasi-threedimensional → quasi-three-dimensional

Corrected.

(2) P3, round L195: Horvat & Tziperman (2015: A prognostic model of the sea ice floe size and thickness distribution. *The Cryosphere*, 9:2119–2134) is worth referencing here too.

The reference to Horvat & Tziperman is in the discussion paper (the first version, published on the internet) and it got lost somehow later... Thank you for pointing this out!

(3) P6 L483: Young modulus → Young’s modulus

Corrected.

(4) P10, L 895: 1D equivalent to a certain 2D config — maybe not strictly true, as there is no Poisson’s ratio effect?

This is true – although the Poisson effect didn't have any noticeable influence on the results (and it definitely had no influence on the breaking patterns). I modified this fragment accordingly.

(5) Fig 12: why are both red lines zero? L should be defined in the caption also (in all other figures too). Where do the rigid and flexural parts come in? (caption says dashed and solid are max/min, colors are different lengths of floe, but this seems to disagree with the text at start of p11)

They are not zero, but the amplitude of the motion in the “red” cases is much smaller than in the remaining ones.

As for the meaning of the lines in the left panels in Figs. 12 and 13: continuous lines denote the flexural motion and dashed lines denote the rigid motion. The caption in Fig. 12 has been modified to make it more clear.

(6) Fig 14: does the histogram follow a distribution like a power law or GLV as have been fitted to observed FSDs?

No. The tail of the distribution is exponential, and the range of values is much too narrow to speak about a power law or any other heavy-tailed distribution.

I doubt if any of the published analyses of the observed heavy-tailed FSD refers to a situation of sea ice freshly broken by swell with one dominating wavelength.  
I added a comment on that to the revised manuscript.

#### **OTHER (MINOR) CHANGES TO THE MANUSCRIPT**

1. I added a reference to Yulmetov et al. (2016) to section 2.3, in order to keep the review of DEM applications to sea ice modeling complete and up-to-date.
2. During the past few months, I found and corrected a few small bugs in the code of the model. The corrected version, numbered 1.3a, is available on the model homepage and attached to this manuscript. The User's Guide has been updated accordingly.  
I changed the version number in the title of the manuscript (1.3 → 1.3a), but if the title of a discussion paper and that of the final paper should be the same, we can keep the old number in the title, as 1.3a is simply a bug-fixed version of 1.3, without any new features.