REVIEW OF 'DISCRETE-ELEMENT BONDED-PARTICLE SEA ICE MODEL'

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1. General comments

With some clarifications to the text I think the paper should be accepted. The wave results are much better presented — the 1D experiment was a good addition.

1.1. **Response to review.**

(1) *But the fact that a disk is exactly at a wave peak does not mean that it has to be in a horizontal position — it still can be tilted relative to the (in this case horizontal) sea surface. And if it is tilted, it experiences a torque trying to restore it to the 'untilted' position. The assumption that "exactly half of the disk experiences an excess of buoyancy, the other half an excess of gravity" is related to that situation, not to the net force acting on the disk.*

I believe I understand now the approach of the author relative to the one I was proposing. I think it is worth clarifying this in the text (§6.3.1, and possibly round P8, L692) and I will give my own interpretation here. The wave formulation is understandably designed to fit in with the existing model framework of drag forces etc.

What I was proposing was a more passive approach to the waves — prescribing the horizontal and vertical displacements and slopes everywhere and just using the stresses as a kind of diagnostic to determine if bond breaking occurs. That is, no momentum equations would be needed. There would also be no time delay varying with the drag coefficient, and no difference in slope or position from the slope or position due to the wave profile. This could make a difference to the bond stresses and maybe to the timing of any breaking, and it could be a good sensitivity study to do in the future if it was not too difficult to implement. However it could run into problems with regard to collisions for example and might be harder to combine with currents etc. However, both approaches would still have one-way coupling ie no feedback to the waves so both have errors.

A final comment on this comment is that the prospect of the model being coupled to a CFD model is very exciting.

1.2. **Manuscript.**

(1) quasi-threedimensional → quasi-three-dimensional
(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.
(3) P6 L483: Young modulus → Young's modulus
(4) P10, L 895: 1D equivalent to a certain 2D config — maybe not strictly true, as there is no Poisson's ratio effect?
(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)
(6) Fig 14: does the histogram follow a distribution like a power law or GLV as have been fitted to observed FSDs?