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The Cryosphere Discussions

Interactive comment on "Wave–sea-ice interactions in a brittle rheological framework" *by* Guillaume Boutin et al.

Anonymous Referee #1

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This is a review of Boutin et al (2020) - who explore the interaction of wave fracture and improved rheological modeling in the neXtSIM model.

The paper is interesting and a piece of model development that ought to be done and published. My major comments are on their 2-FSD parameterization.

Main comment: a few questions about your model.

In your 2-FSD implementation, I would like some more clarity on the meaning of the "mechanical" FSD - this is an interesting idea. My read is the point is to provide memory of past deformation - but how is this separate from the role of damage in neXtSIM/MEB?

If one wanted to compare your output to observations, how would you do that?

Why should we expect your mechanical FSD to look like the thermodynamic one, i.e. obey the same evolution equations? Why should the mechanical healing term look like the thermodynamic one, couldn't it evolve independently? I think a figure to add would be plotting the mean floe size for both FSDs in time for the period documented in Fig 13, even for just a single grid cell.

The impact is clearly seen in Fig 4. Were I designing a separate depiction of sea ice fracturing, I'd expect it to be most relevant in the interior pack - this is where FSD models don't get crack features right yet. This mechanical FSD implementation seems to pinch in near the margins, not in dynamically active but waveless regions, but the neXtSIM model does get damage in the interior, doesn't it?

This leads me to believe that there is a difference between the description of where the mechanical FSD would be relevant (Sec 2.2, i.e. interior regions with leads) and where your model makes it relevant (exterior regions with waves). I think this approach is potentially fruitful for fixing the problem of bad pack ice fracturing, but you may be approaching it from the wrong place!

Minor Comments:

Please remove the mention of eddies from your abstract - the role of the ocean is not explored here except to cite a couple papers in the conclusions.

P2 L4 - I think you forget to explicitly mention the second main process?

Pg 2 - Using the power law FSD, especially in early days, is fine, but just note that meta-analyses (Herman 2011, Stern 2018) and new datasets (Horvat et al 2019) indicate an absence of power-law tails. Still... it is tough to justify (or putting the cart before the horse) designing a model that gets an answer, and then forcing its conservation via parameter choices. This is a particular problem because it is majority opinion that the "cutoff" power laws observed by Toyota and others are an artifact of the use of CDFs and finite measurement windows (Burroughs and Tebbens 2001, Stern 2018)

not physics. Now a model has been designed (more than one) that produce these distributions. But you have no windowing issues (so no expectation of a truncated power law distribution) or sampling issues (so no need to produce a CDF). I'd advise plotting the FSDs proper alone (as you do in Fig 5), living with the results. At these early days, you'll be forgiven for having weird distributions, and for making changes to your models, too.

Pg 3 L 5 and otherwise - (ITFSD –> FSTD).

Pg 5 L 30 it has been pointed out by Stern (and a wide literature from applied math, see Virkar and Clauset 2014) that fixed-width bins will bias your ability to represent or examine scale-invariant behavior.

P 6 - I think the most updated Roach model was published in 2019 and included coupled wave-ice physics. Might provide better sourcing for the comparison here.

P 6 L29 - do you mean that once the concentration is high, all the ice is in the highest size category? Is this also true for the mechanical FSD, or do you still require the relaxation?

P 7 L 25 "a quick and violent process" is a wonderful phrase albeit not exactly accurate. I know I should object scientifically but I really like it.

P9 L 25 - See earlier comment. At the very least, please explain these parameter choices naturally through your model design not as a post-facto requirement.

P13 L 5 "it also includes storms" - could you be more clear about what you mean here?

P13 L21 - What does it mean "very satisfactory results"? What is the metric?

P13 L27 - "Perfectly acceptable given the uncertainties" - I'm not sure what you mean - which is perfectly acceptable, and why does this relate to wave attenuation uncertainties?

P15 L2 - why not show this contour?

P16 L1 - "It is particularly true..." - rewrite?

P16 L17 - A bit confused here, "regenerate unbroken ice" isn't really the process - healing between floe joints is how you describe it.

Fig 3 - Again I'd advise not using the CDF here, preferring the FSD because as pointed out by Stern (2018) the CDF gives a false impression of scale-invariance, and

P19 - I would prefer a clearer description of this process. In effect, you are saying that the influence of fragmentation (at least in your model) is not because of wave events, but after them when the sea state relaxes?

P20 L 35 - "it depends on two factors" but then you mention it doesn't depend on reducing t_heal to 15 days. Also, this isn't a sensitivity experiment as you haven't also increased the healing time. You also don't really address the sensitivity to attenuation just mention it is uncertain.

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