The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
https://doi.org/10.5194/tc-2022-130-RC2, 2022

Comment on tc-2022-130

Fabien Montiel (Referee)

Referee comment on "Summer sea ice floe perimeter density in the Arctic: high-resolution optical satellite imagery and model evaluation" by Yanan Wang et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-130-RC2, 2022

First, I would like to apologise to the authors and editor for the delay in submitting my review. The manuscript attempts to compare floe perimeter density data obtained via satellite imagery at 2 locations in the Arctic Ocean against the predicted density of 3 floe size distribution (FSD) models, which all include a parametrisation of wave-induced fracture. The goal is to evaluate the performance of the models. Results show that the discrepancy is quite significant in a number of ways. In particular, the models generally predict much larger perimeter densities than the observations, meaning an overestimation of small floes. The authors then attempt to explain the discrepancy by discussing potential issues with specific parameterised components of the models considered.

The manuscript presents original work, is overall well written and the topic is highly relevant to improving the modelling capabilities of FSD resolving sea ice models. That said I have a number of important issues with the way the study has been designed, which may explain the poor agreement between models and data. These are detailed below as well as other comments. I therefore recommend the manuscript undergoes major revisions before it can be further considered for publication.

Main comments

- My main concern relates to the way the study regions for both models and observations were selected in relation to one another. If I understand correctly, the satellite images were chosen at 2 specific location, one in the Chukchi Sea and one in the Fram Strait. In contrast, the regions selected for analysing model outputs are much larger. They do include the specific observational locations but extend over much wider regions, selected to include the ice edge. My issue is that the variability in FSD in these regions is likely to be much larger than that at the locations of the data. If the data is collected far from the ice edge (which could have been estimated), floe sizes are likely much larger than closer to ice edge. Therefore, I am not sure the comparison between model

outputs and data is fair with respect to the models. In fact, when the comparison is refined to a subdivision of the initial study regions, the agreement is improved. I am not an expert in analysing satellite imagery, so my following suggestion could be naïve and/or uninformed, but this is what I would have done: (i) select all the imagery available in the model study regions, not just those at the specific locations selected, OR, if this is too much data to analyse, (ii) take a random sample of images spanning the study regions. Either way, the variability in the data would be a lot more representative of that predicted by the models. I am not suggesting that the authors redo the entire analysis for this paper, but I feel like this limitations needs to be given a lot more emphasis in the manuscript. At the very least, bring this up in the Discussion section, but what would be even better is to add a sub-section looking at the comparison models/data when the model outputs are only selected in a smaller region around the data locations, even more localised than the subdivision shown in figure 6.

- The title of the manuscript is misleading and should be changed, as the authors do not analyse the FSD but the floe perimeter density, which is different. The abstract also needs some work as it starts with statements on the FSD and then switches to perimeter density without making a link between them. I am not saying the perimeter density is a bad metric, but it is not the one advertised! It took some time to fully appreciate the meaning of Pi (again not an expert!). It would have helped me to show the Pi results with units km per km^2. I would have liked the relationship between these 2 quantities discussed in more details in relation to the results. For instance, do we expect FSDs to have similar qualitative and quantitative properties as the perimeter density distributions shown in figs 2e-k?

Other comments

- L28-30: This statement is confusing, especially for the uninformed reader. Quantifying the MIZ is still an active an area of research. Mentioning the 2 definitions (i.e. wave-based and SIC-based) in this way makes it seem that they are equivalent, but that's not true (see Brouwer et al paper). I suggest you pick one definition or expand the discussion.
- L43: "viscous dissipation" is not the process governing attenuation, it refers effective/homogenised dissipative rheology of continuum viscous layer models often used to approximate attenuation caused by a non-homogeneous ice cover. In any cases, "dissipative processes" would be a better choice of wording here.
- L53-55: The limitations of the power-law should be discussed. See Montiel & Mokus (2022, Philosophical Transactions A) for an overview.
- L77: I'm not sure I follow the argument that a low-resolution model output justifies the choice of a large model study area.
- L90: What's the area of the uncropped images? 250 km^2? It would be interesting to know.
- L91-93: A reference for the WV images and more details about the sensor should be given.

- L98-102: how is “floe size” defined? There are multiple definitions out there.
- Section 3.2 is confusing. The intro paragraph discusses differences between the different models, before describing the models themselves in subsequent sub sub-sections. It would make more sense to describe the models first and then discuss their differences/similarities.
- Section 3.2.1: More details about the WW3 configuration are needed. Is it a global or regional run? What ice attenuation parametrisation did you choose? Also, what do you mean “attenuation in the open ocean”?
- L149: “process” is the wrong word. Maybe “theory” or “model”?
- L153-154: I feel more details are needed about how the fixed power was determined. Was it the same data as those used later for comparisons with model outputs. Were all the floe size data from all the images collated into a single dataset and then a power law fit was performed or were power law fit done for each image and then averaged? What was the range of floe sizes considered for the fit(s)? Also is 3 significant figures appropriate? What’s the uncertainty on alpha? Was a goodness-of-fit test evaluation conducted?
- Eq (2): I don’t n(r) has been defined.
- L175-177: Rephrase these 2 sentences as they appear to contradict each other.
- Eq (4): gamma needs to be defined.
- L197: How do you estimate central tendency and spread in the figures provided for Pi?
- Fig 2e-k: there appears to be a plateauing in the observations for small floes. It could be due to a resolution issue as discussed in section 4.2, but it could also be a signature of the limitation of the power law. I know you are not trying to fit a power law here, but I believe this is an important point to make, especially when comparing to
- L212: use big O notation for order of magnitude.
- L215-216: I believe an explanation was provided for the presence of the “uptick”. It would be good to mention it here.
- L254-255: That statement seems to be quite a stretch at this stage and sort of comes out of nowhere. Given the seasons considered, I would expect lateral melting to be a more important contributor to underestimated SIC.
- L270: “floes welding are negligible during this season” seems to contradict the statement in the previous subsection (see comment 21).
- L334-339: again, I don’t find the argument about underpredicting welding very convincing. What about potential overestimation in the data? See Roach et al (2018, The Cryosphere)
- L243: Again, how was the fit performed? Across all floe sizes or a limited range? What about goodness-of-fit?
- L245-247: What about the possibility that the power law is not appropriate? I don’t see why a power fitted through historical data should predict a power law in the current dataset, even at the same location.

Typographical errors

- L58: delete “the”.
- “welding” is misspelled a couple of times.