

Dear Dr. Hutchings,

Thanks very much for your efforts here - we respond to your comments below and look forward to working to improve and understand this method in the future.

Line 30 page 4: "where shear stress determines sea ice shape". What shape do you refer to? floe shape? the shape of the ice between leads? Perhaps clarify.

We now clarify:

...in regions where shear stress determines fracture patterns and floe shapes ...

Line 23 page 7: "aggrement" Spelling.

Thanks - fixed!

Rather than ask the reader to find and interpret the code in the supplemental section, can you provide a brief description of the operations you performed in the paper, referencing the supplemental section.

A good point - we make this more clear:

In the Supporting Information (Text S1 and File S1), we compare these two estimates when they are evaluated against synthetic datasets drawn from a true power-law distribution. The two agree even when the size of the data is relatively small ($N < 25$).

End of page 9: "We interpret this seasonal cycle in size over time as due to the formation of large first-year ice pans in October which are later fractured into smaller floes throughout the winter months" - Personally I don't like speculation like this. In Autumn storm activity is high, pans of ice can get broken up. I get what you are implying here, that once sheet of ice form and the ocean is calm large pans with grow. Intuitively we would expect leads to crack the entire ice pack once it becomes consolidated to the coast and able to propagate stresses long distances, this is in December/January according to stress measurements taken by Jackie Richter-Menge in the Beaufort in 2001/2002 (Richter-Menge et al. 2003). So there may be some truth in what you say, but it surprises me. It would be best to clarify that further observations are required to understand your results.

We agree speculation of this type isn't great - we added such a comment only at the suggestion of Reviewer 2. To strike a healthier balance we now write:

A possible interpretation of this seasonal cycle is that large first-year ice pans form in October and are later fractured into smaller floes throughout the winter months. This concept is supported by observations that large-scale fracturing of sea ice in the Beaufort Sea is dominated by coastal processes and therefore only can occur once sea ice freezes

to the coast in mid-winter (Richter-Menge, 2002), although such an interpretation is speculative and must be evaluated further as this method is refined.

line 9 page 14: "With colored cells those where" may read easier as "colored cells are those where"

Thanks - fixed.

line 20, page 14. Consider splitting into two sentences after "disagree". The hyphen confused me on the first read.

Done!

Section 5: Do you consider the larger cut off flow size of 900m that you implimented earlier? This is getting to a scale with relatively few floes in the MIZ though.

We computed means using all FCD measurements to make the comparison as broad as possible - we now explain,

We include all FCD measurements here (chord lengths above 300 m) to make the broadest comparison, but note that the potential underrepresentation of floes with diameters near the sampling resolution may lead to inaccurate values of \bar{r} in regions mainly consisting of such floes.

I agree with reviewer 2 that this section seems a little out of place in the paper. However you make a reasonable point that the method was developed so models and observations could be compared. Perhaps you can clarify this at the top of the section. It is fair to say that floe size distributions in models is a newly developing field and your method might facilitate testing models following this example. With the caviate of the minimum detectable floe sizing being rather high and further work required to understand seasonality and regional variability.

I think this is important to add as well, we now say:

A principal aim of this work is to allow model-data comparisons and facilitate testing rapidly-developing FSD/FSTD models. Here we demonstrate how such a comparison can be made and provide useful information to modelers, even in the presence of the high uncertainties in this nascent FSD reconstruction technique.

page 18 around line 5: Can you comment on whether your choice of geographic regions is appropriate to isolate ice regimes that may have power law scaling for floe size? You do show that there is no global power law scaling for the satellite product, and I feel you should be careful to not draw conclusions about local dynamics from this. This does not negate your point that models accounting for floe-scale processes are needed.

Good point - we now mention that there may be issues when zooming in to these small areas -

While we found little evidence for power-law scaling throughout most areas of the Arctic, this may be sensitive to the geographic (here the CICE model grid of approximately 25 km x 25 km) and temporal (here all measurements between 2010-2018) windows we use to collect and evaluate chord length measurements for a power law.

page 18 line 24: "This implies that the principles of isotropy, homogeneity and stationarity of the FCD, required to produce such a distribution, are invoked on the length scale of the CICE model grid and time scale of a month." - define this length scale, because it is not set by CICE itself but by the modeller. Also, the model referenced does not need to be CICE, it can be any model that has a continuum or homogenous element assumption.

We now define the length scale and make clear it is a choice we have made:

This implies that we invoke the principles of isotropy, homogeneity and stationarity of the FCD, required to produce such a distribution, on the length scale of the CICE model grid (O(25 km)) and time scale of a month.

Please make sure you are consistent in your symbols throughout the manuscript, including appendices. For example, did you change beta consistently throughout?

We checked for this consistency as suggested.

page 22, line 4: Check the brackets.

Equation C10 needs a comma behind it rather than a period.

The line numbers are potentially different on our version vs. yours. We fixed a strange bracket in the bibliography around the word "Arctic", and fixed a period added to Eq. C4.

Reviewer 2 has valid points and I agree further work is needed. I also see that this work will not happen if the method is not published. I encourage you to be explicit in the limitations of the method as you present in this paper, and am looking forward to seeing the work evolve in the future.

We agree entirely, hope we have been sufficiently careful in describing the methodology, and will be working on this a great deal in the months and years to come.