The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-44-RC2, 2019

The Cryosphere
Discussions
Open Access

Interactive comment on "Impact of floe size distribution on seasonal fragmentation and melt of Arctic sea ice" *by* Adam W. Bateson et al.

Anonymous Referee #2

Received and published: 18 April 2019

This paper details a new parameterization for the sea ice floe size effects, which assumes a power law floe size distribution over a given size range of variable exponent and endpoints. The benefit of this approach is it may simulate the effective properties of the sea ice FSD without having to parameterize the underlying physics. This work outlines the WIPoFSD model and analyzes the variability of Arctic sea ice over the period 2007-2016 between standard mixed-layer-ocean-CICE simulations and those using the new parameterization, with a number of parameter perturbation experiments used to assess overall Arctic sensitivity to floe size parameters. They find a lateral-melt feedback on September sea ice extent and volume, where the redistribution of heat from basal to lateral melt leads to future reductions in sea ice volume, and find effective floe size is a good predictor of September sea ice volume/extent. This research

is clearly relevant to ongoing operational and predictive modeling efforts, with the potential to be a useful benchmark for understanding the sensitivity of Arctic sea ice to floe size variability and with the potential to be a useful model for simulation purposes.

The manuscript: (A) outlines a new computationally inexpensive model designed to capture aspects of FSD behavior, and (B) demonstrates a floe-size-melt-feedback at a scale only discussed by Asplin (2012, JGR), Kohout (2014, Nature), and others. I find that (B) is generally well-presented and would be an interesting addition to the scientific literature. Yet aspect (A), while again, a promising idea, lacks key details and, as presented, contains inconsistencies that may preclude publication or adoption of this technique.

I recommend the authors carefully re-examine the presentation and formulation of section 2 before reconsidering their results. I have outlined specific comments that relate to their new additions in Section 2.3, along with more general and copy-editing comments subsequently.

Section 2.3: This section must be carefully re-analyzed. When doing so, please be cognizant of variable definitions and units. For example in eq. 14, alpha $>$ 0, but in Sec.4 (alpha $<$ 0), and alpha is also the shape parameter. Is A a concentration (as it must be in eq (10)) or an area? What are the units of N? What is d_{frag}? What is d_{max}? Their definitions in Table 1 are not sufficient to follow through the text.

- What is the definition of the FSD, N? By eq.(9) you have adopted an number-weighted definition. Rothrock and Thorndike (1984) give a good way of presenting the FSD. Most modeling approaches have used area-weighted distributions for consistency with the ITD (Zhang et al, 2016) but there has been no consistency there, so defining it clearly is necessary.
- (and P15L29, e.g.,) The «truncated»FSD defined here is not the scale-truncated power law used in the literature, defined in Burroughs and Tebbens (2001), and

investigated in Herman (2011) and Stern et al (2018). It is also not zero at l_{max}. What about floes larger and smaller than the size ranges here? How much of the full range of area is being cut out? How low does l_{max} get?

- Eq (9) - Quantities like «max floe size»are not area tracers (you can see they are non-linearly related to the area in Eq (9)), thus the CICE scheme is not appropriate here. These variables must be advected differently (see Horvat and Tziperman (2017)).
- Eq (10) is not consistent: the square root requires that A be unitless. Then the units on the LHS are m, and the units on the RHS are m/s. But if A is unitless, it is a number less than 1, and the inconsistently defined l_{max} is imaginary.
- More importantly, Eq (10) is a main new contribution of this model but it is dictated, not derived. The impact of Eq (10) is easily explored, however, by plugging it into Eq (11) via Eq (18), at which point you can solve for A! Once corrected, is this consistent?
- Eq (13) needs to be re-examined as it is inconsistent with Eq (9). Since the growth rate of l_{max} is not derived based on the change in sea ice area, the constraint (9) can only be accomplished by changing the coefficient C introduced in (8). Essentially, this means reducing the area occupied by floes at all scales (except for the very largest), which is not what is happening! Restoring is ok, but T_{rel} should relate to dA/dt. This would eliminate the need for the sentences at L15 justifying this approach. If the authors wish to use an ad-hoc parameterization, then why invoke the FSD and its distributional features at all? Instead, just write a heuristic equation for l_{eff}.
- P7L6 - what does this mean? Is lambda the peak wavelength? The spectrum as outlined previously is a frequency spectrum.

Comments:

- P2L19 - As you mention, many models (LIM, e.g.) do not have any floe size, so «simplifies model code»isn't quite right - there were no FSD schemes when these models were written!
- P2L24 - I think you should be careful in this section - given the small FSD literature, what «dictates the best approach»may not be known yet.
- P2L35 (and where discussed later, e.g. P8L32) - The cited paper does not given evidence for a unique power law across that range of scales. In the abstract: «We found that the FSDs from the high-resolution images follow power laws over floe sizes from 10 m to 3 km.»Though similar-sounding, this is different to «we found all FSDs follow a unique power law . . .»over that range, and there is a good reason! Picking a fixed exponent to run the model is a great simplifying idea, but should not overemphasize the applicability of a single power law.
- P3L34 - I would re-write this passage to avoid making qualitative judgements about other sea ice and climate modelers. Model developments all come at a cost and it is not within the scope of this study to diagnose and prescribe best practices, particularly when trying to justify one's own parameterization.
- **Model description:** In general, there is too much extraneous information given in the model description. Is it important to discuss standard model physics of the CICE model? Or what NEMO is, especially since NEMO is not used in this study? There is much more detail given to previous work than the new work, I recommend cutting this substantially, and focusing on what directly affects the FSD model here. This could go in the Supporting Information if it is necessary.
- P4L14 - I'm not sure why NEMO is included here or the references to the SWARP project. You later use a mixed layer model to produce all results!

- P5L31 - because of the many types of wave spectra can become confusing, it may be helpful to write this as $S(\omega)d\omega$ to make comparison easier with other studies using the spectra. See Michel (1968 and 1999).
- P6L23 - it appears as if this is the place that waves reduce the FSD maximum size. But how is not explained, and should be included in the text.
- Sec. 2.4 - I think the use of perimeter per square meter is fantastic - the right approach.
- P7L36 - Equation 16 is a ratio of ice perimeter to ice area, i.e. the 1st moment of N divided by the 2nd moment of N (if N is an area). The shape parameter (which is not introduced as part of N prior to this comment) does not cancel from this expression.
- P10 - How does this parameter space exploration differ from what was performed by Roach et al (2017), who reduced the CICE floe size parameter in much the same way as did Steele (1992)?
- P13L10 (and discussion on P14) - I don't believe these sensitivity experiments are sufficient to test variability in ocean response - as mentioned the minimum mixed layer depth is a numerical crutch because Kraus-Turner models become singular - is this parameter really what controls mixed layer feedbacks? There is such a wide range of Arctic mixed layers (see Peralta-Ferriz and Woodgate (2015)) that you may be exiting the range of interest here.
- **Discussion:** The sensitivity experiments are referenced to a time-varying baseline (2007-2016). What is the rationale for choosing this period? Why should deviations from such a strongly forced system be used as a sensitivity? I do not think concepts like the multi-year memory of Arctic sea ice can be understood based on two averages across 10-year periods. In general here I would support simply explaining how certain parameter changes affect results, but there is

not enough information presented to support qualitative claims about the Arctic system response.

- We do not know the relative importance of the three factors that affect the FSD: melting, waves, and freezing. A needed figure is one that breaks down the seasonal variation in l_{max} (better, l_{eff}) as a function of each forcing components. Furthermore we don't see how N evolves in time, neither l_{max}, but these are key variables of the WIPoFSD model!
- **Figures:** This may be a question of style you can ignore, but the figure captions contain descriptions or context that aren't directly describing the figure (e.g., the final 3 sentences in Fig 4). These may be better off in the text. Same for using parentheses instead of division «/»(or a comma, in Fig 1) for units on axis labels. Feel free to ignore this comment.

Copy-editing comments

- P1L1 - Authors' choice, but «sea ice floe size distribution»is probably good in the title, even if obvious.
- P1L15 - «climate sea ice models»(pick one! or sea ice models in climate models)
- P1L21 - «this feature is important in correcting existing biases»- true (see Roach et al 2017), but not supported within the text or referenced again.
- P1L36 and elsewhere - be sure to say «sea ice»instead of «ice»throughout.
- P2L2 «described as marginal»- seems mean, perhaps say «the region with X characteristic, referred to as the MIZ, etc». See again the use of marginal as an adjective on L16.
- P2L4 - no need to capitalize Marginal Ice Zone.

- P2L7 - The S+J citation here is about past changes to Arctic cyclones, not about future changes to sea state/storminess.
- P2L18 - «calliper»can be removed here.
- P3L3 - Are these autonomous techniques recovering the FSD?
- P3L9 - Consider looking at Perovich and Jones (2014) here.
- P4L2 - P4L6 «allow time», probably best to say «permit more sensitivity studies». You may also remove the statements about «lends itself to sensitivity. . .»because this is true of any new parameterization with new parameters to tweak.
- P5L4 - Please re-consider using alpha here or later.
- P6L1 - this aside about interpolation needs to be copy-edited as it introduces undefined terminology (e.g., what is a smooth variation?)
- P6L6 - why are waves advected in 5 directions? And is there any scattering or reflection?
- P6L29 - please mention the values of d_{min} and α here.
- P8L16 - Replace this acronym with a description of the reanalysis product.
- P8L36 - this is the first time «cumulative distribution»is introduced - please outline what the distribution is prior to here.
- P9L6 - «peak melting season»- what time period you are referring to?
- P9L21 - index these quantities to their definitions in the text.
- P9L25 - do you mean «between Stan-ref and ref?»

- P9L34 - «fractional ice area»→ «ice concentration»(throughout)
- P9L36 - The strong restoring and imposed external forcing fields complicate statements like this, I'd consider removing this statement unless you plan to examine the ice flow field.
- P10L7 - explain in words what changing the distribution means - how much does the effective floe size drop? Same at L26.
- P1039 - PL-FSD model?
- P11L26 - Why is this parameterization chosen? Perovich and Jones (2014) provide a useful relationship between power law exponent and area, as does Birnbaum and Lupkes (2001).
- P11L30 - Which effective floe size? Averaged over the Arctic? How is this done? What does «predictor»mean here?
- P11L41 «earlier observations»→ «the previous result»?
- P12L21 - In general, I do not see how changing the restoring is reflective of memory in the system. You have not presented any evidence as to the magnitude of the restoring response. You could look at seasonal tendencies in l_{eff} which would be far more helpful when analyzing these sensitivity experiments.
- P12L29 - «are uncorrelated parameters»→ «may be uncorrelated parameters».
- P12L30 - As before, consider re-defining this term, alpha is already in use.
- P14L1 - «melt potential»is not previously defined.
- P14L1 - which simulation?

- P15L32 - «The WIPoFSD»→ In the «WIPoFSD model, the FSD»- I know, this is ugly, but you have to!
- Figure 1 - please include some schematic depiction of the axis and its scale (logarithmic or linear).
- Table 3 - Runs are in lowercase, but uppercase in the text.
- Table 3 - why not put in parentheses the percentage change instead of absolute change?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-44, 2019.