The Cryosphere Discussions

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# Comment on tc-2022-130

Anonymous Referee #1

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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-RC1, 2022

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This paper compares the sea-ice "floe perimeter density," as calculated from three models, to satellite observations in the Chukchi Sea (CS) and Fram Strait (FS).

The length, or density (length per unit area), of floe perimeter is a factor in the lateral melting of ice floes in summer, and is therefore a potential diagnostic for models. For a given field of ice floes, the floe perimeter density is a scalar.

The sea-ice floe size distribution (FSD) is the number of floes as a function of floe size. The FSD may be normalized (e.g. by the total number of floes) or not.

The analysis in this paper is all about perimeter density, denoted P_i (P sub i) by the authors, and PD by this reviewer. However, the authors treat PD and FSD as if they are interchangeable or equivalent. They are not. Completely different FSDs can give rise to the same PD, and identical FSDs can give rise to different PDs. There is not a one-to-one correspondence between PD and FSD. The authors point out that a larger PD implies more smaller floes, and this is true, but the PD says nothing about the FSD. In light of this fundamental confusion between PD and FSD, I must recommend that this paper be rejected. Specific comments follow.

PD and FSD
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The title implies that the paper is about the FSD, but it is really about the PD.

In the Abstract, lines 11-16 are about the FSD, and lines 17-25 are about the PD, without making any connection between the two.

Suppose n(r) is the number of floes of size r. Consider the case of circular floes. The perimeter of each floe is 2*pi*r and the area is pi*r^2. Therefore the perimeter density is:

PD = INTEGRAL(2*pi*r * n(r)dr) / INTEGRAL(pi*r^2 * n(r)dr)

Now suppose the mean value of n(r) is MU, and the variance is SIGMA^2. Then the above equation yields:

PD = 2*MU / (SIGMA^2 + MU^2)

Now consider two cases:

(1) n(r) has a uniform distribution on [0,L], i.e. n(r) = 1/L.

Then MU = L/2 and SIGMA^2 = (L^2)/12, so PD = 3/L.

(2) n(r) has an exponential distribution with parameter LAMBDA,

i.e. n(r) = (1/LAMBDA) * exp(-r/LAMBDA).

Then MU = LAMBDA and SIGMA^2 = LAMBDA^2, so PD = 1/LAMBDA.

By choosing L = 3*LAMBDA, the uniform FSD has the same perimeter density as the exponential FSD. Same PD, different FSDs.

Now consider a set of circular floes with FSD n(r). Construct a set of elliptical floes with semi-major axis "a" and semi-minor axis "b" such that pi*a*b = pi*r^2. Each elliptical floe has the same area as its corresponding floe in the circular set. Therefore the FSD of the elliptical set is also n(r), by construction. But the perimeters of the elliptical floes are longer than the perimeters of the circular floes, so the PD for the elliptical set is larger than the PD for the circular set. Same FSD, different PDs.

Lines 359-361. "positive biases of P_i are closely linked to overactive wave fracture in the models. This suggests accurate parameterisation of wave-induced sea ice breakup is essential for simulating the summer FSD correctly."

The implication here (and throughout the paper) is that the PD tells us about the FSD. But a connection between PD and FSD has not been demonstrated, and the simple theoretical examples in the previous comment show that a connection need not exist.

Figure 6 caption. "(a) Change of FSD arising from lateral melt" and "(b) ... wave induced FSD change"

According to the scale bar in the figure, the panels show the change in perimeter density, not the change in FSD. But here (and throughout the paper) the authors seem to equate PD and FSD.

In summary, the authors have not said how the PD is related to the FSD, and therefore why it can be used to assess the FSD produced by the models. According to my calculations, the PD and FSD are not necessarily related, so any statements or conclusions derived from the analysis of the PD do not necessarily apply to the FSD. Since there is no easy way to rectify the confusion between PD and FSD in this paper, it should be rejected.

Comparison of Models and Observations

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The lack of agreement between the models and the observations, and between the models themselves, is truly remarkable. The histograms of PD are all completely different (Figure 2, panels (a) through (d)). The plots of PD vs. sea-ice concentration (SIC) (Figure 3) show that FSDv2-WAVE has values of PD that are an order of magniude larger than the observations, with vastly greater variability, and a slope (vs. SIC) with the wrong sign. CPOM-FSD is hardly any better. WIPoFSD has a slope similar to the observations but with PD values five times larger. The model/obs differences are noted by the authors at lines 196-206 and 219-231.

In Section 5, the causes of the differences between observations and models are attributed to three factors (lines 322-324). The first factor, image resolution, cannot explain such large differences (line 327). The second factor, underestimation of SIC in the models, "can partially explain" (line 338) such large differences. The third factor, overactive wave fragmentation in the models, was investigated by dividing each region (CS and FS) into north and south portions, and comparing observations vs. models in these sub-regions. In the CS region, all the observations are in the south sub-region. In the FS region, all the observations are in the north sub-region.

In Figure 7(a) for the CS region, the agreement between observations and FSDv2-WAVE(south) is still terrible, and the agreement between observations and CPOM(south) doesn't appear to be any better than in Figure 3(a). In Figure 7(b) for the FS region, the agreement between observations and FSDv2-WAVE(north) is still terrible, and the agreement between observations and CPOM(north) is not particularly good. In my opinion, the analysis by sub-region has not resolved or shed light on the large differences between observations and models.

Lines 352-353. To investigate "unrealistically high perimeter densities in our study regions" the authors "examined the P_i in the northern regions where wave-induced breakup is negligible. In these regions, most modelled P_i match our observations better." This seems to be saying that the authors have compared model results from the northern sub-regions with the observations. But for the CS region, all the observations are in the south sub-region, so it makes no sense to compare models in the north with observations in the south. For the FS region, Figure 7(b) does not show that "most modelled P_i match our observations better." I don't see any kind of match between models and observations, nor much improvement over Figure 3(b).

Lines 359-360. "positive biases of P_i are closely linked to overactive wave fracture in the models."
I don't believe that the authors have demonstrated this.

Looking at the big picture, I can only think of two possible explanations for the enormous differences between the models and the observations, and between the models themselves: either the models are complete junk, or the PD is meaningless as a diagnostic of model performance. Do the authors have any thoughts on this?

Other Comments
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About the MEDEA imagery used in this study (lines 59 and 88-90), please see Denton and Timmermans (2022), and say briefly how their data set and analysis relates to the present work.
Denton, A.A., and M.-L. Timmermans, 2022, Characterizing the sea-ice floe size distribution in the Canada Basin from high-resolution optical satellite imagery, The Cryosphere, 16, 1563–1578, https://doi.org/10.5194/tc-16-1563-2022

Lines 79-81. "the observations from the Chukchi Sea region captures a more dynamic and fragmented ice condition (e.g., Fig. 1b), while the observations from the Fram Strait capture a less dynamic environment (e.g., Fig. 1c)."
Looking at Figure 1, I don't see that the Chukchi Sea image indicates more dynamic and fragmented ice than the Fram Strait image. The two images look similar to me. How does a single image convey dynamics?

Section 3.1.2. What measure of floe size is used? All I can gather is that floe size is characterized by a radius. Is it half the mean caliper diameter? Is it the radius that a circular floe of the same area would have?
Also, at lines 103-105, "we first applied combined filters: median, bilateral and Gaussian filter" and "The smoothing term, KGC algorithm parameter, was set as 0.0001" -- either provide more detail so that the reader can understand what this means, or leave it out and just refer to Hwang et al (2017).

Section 3.1.3. What is the spatial resolution of the sea-ice data? Also, the analysis period is 2000-2014 but AMSR-E is only available 2002-2011 and AMSR-2 is only available since 2012. What sea-ice products (with what resolution) were used during what time periods?

Lines 121-123. Does 1-degree grid mean 1-degree in latitude and 1-degree in longitude? What does "gx1v6" mean (line 122)? Also, the models are run "for 37 years from 1 January 1980, followed by a 10-year period spin-up" so the spin-up period is 2017-2026 i.e. partially in the future. Is that correct?

Lines 155-157. "the model also simulates FSD evolution through the floe size parameter r_var ..."
Please define r_var. I see that it varies between r_min and r_max, and I see that it evolves according to four FSD processes, but no definition of r_var is given. What is it?

Section 3.3. This section (FSD definition, lines 158-171) is confusing and unnecessarily complicated.
-- "The FSD is usually defined as the floe areal FSD..."
It's confusing to use FSD in the definition of FSD!
-- "By integrating f(r) over floe radius between r and r+dr, f(r)dr (dimensionless) is obtained"

This makes no sense.
-- It's really not necessary to introduce the Heaviside function and equation (1) in order to define the FSD, especially since they're not used in the rest of the paper.
-- The cumulative floe number distribution, defined at lines 169-171, is also not used in the rest of the paper.

Line 207. "normalized" perimenter density is not defined in the text. The caption for Figure 2 says "The normalized perimeter density distributions were obtained by dividing the width of every floe size category into P_i at each region." The original P_i has units of 1/km and the "normalized P_i" has units of 1/km^2. In what way is this a normalized quantity? Usually I think of normalization as producing a dimensionless result such as a percentage. What is the point of "normalizing" by dividing by the width of the floe size category to produce another dimensional quantity?

Lines 270-271. "we constructed two data sets: monthly changes of P_i arising from lateral melt and FSD changes arising from wave breakup." How were these two data sets created?

Line 274. "CPOM-FSD produces negative changes in P_i from wave fracture (Figs. 6f and 6h)." But Fig. 6h shows changes arising from lateral melt, not wave fracture. Furthermore, Figs. 6e and 6f show that the change is positive, not negative. Finally, note that the panels in the bottom row of Fig. 6 are labelled (g) (e) (h) (f) from left to right. This might be the source of some confusion.

Line 280. "CPOM-FSD shows a stronger reduction in P_i arising from lateral melt (Figs. 6e and 6g)." But Fig. 6e shows changes arising from wave fracture, not lateral melt. See also the previous comment.

Lines 302-303. "The close match between CPOM-FSD and the observations for the northern Fram Strait region..."
I'm looking at Figure 7(b) for the Fram Strait region. The observations (black circles) were acquired in the northern part of the region. The CPOM(north) results are indicated by open yellow circles. I don't see a close match between the black circles and the open yellow circles.

Lines 314-316. "The observation results show clear regional differences between the two study regions, i.e., much larger perimeter density P_i (smaller floes) in the Chukchi Sea region than in the Fram Strait region."
I'm looking at Figs. 3(a) (Chukchi Sea) and 3(b) (Fram Strait). The observations are indicated by black circles. When I look back and forth at the black circles in (a) and (b), I just don't see the "clear" regional differences.

Lines 320-322. "The observations and WIPoFSD model both show a positive correlation between SIC and P_i ... while the two prognostic models show the opposite (negative) correction."
(Note that the word "correction" should be "correlation").
In Figure 3, I see the opposite of what's stated here: observations and WIPoFSD both show NEGATIVE correlations between SIC and P_i; FSDv2-WAVE and CPOM-FSD both show POSITIVE correlations between SIC and P_i.

Supplementary Materials
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Equation (1) and following.
-- It's bad notation to use "i" as a subscript on the left-hand side and "i" as an index of summation on the right-hand side. See also my comments below about equation (3) of the main text.
-- The parameter GAMMA is not defined. "Here GAMMA is a floe shape parameter, for example..." (line 22) -- this is not a definition. I gather from equation (1) that the floe perimeter is 2*GAMMA*r, which perhaps defines GAMMA (if so, that should be stated explicitly). In that case, for circular floes, GAMMA = pi, as noted on line 22, but for square floes, GAMMA is not equal to 1, as erroneously stated on line 22. Consider a square floe of side s, perimeter 4*s, area s^2. If r is the radius of a circular floe of the same area, then s^2 = pi*r^2. The perimeter of the square floe is 4*s = 4*sqrt(pi)*r. If this is equal to 2*GAMMA*r then GAMMA = 2*sqrt(pi) = 3.54.
-- Rothrock and Thorndike (1984, hereafter RT84) calculated a "shape parameter" similar to GAMMA, finding that AREA / MCD^2 = 0.66 +/- 0.05, where AREA is the area of a floe and MCD is its mean caliper diameter. In the present context, if AREA = GAMMA*(r^2) and MCD = 2*r then the RT84 shape parameter is GAMMA/4 which implies GAMMA = 2.64 +/- 0.20, which is not too different from the values given on lines 23 and 24.
-- The meaning of the terms in equation (1) should be explained more clearly. For example: (r_i_max - r_i_min) is the bin width of the i-th bin; n_i is the number of floes in bin i per unit bin width per unit area; therefore their product is the number of floes in bin i per unit area. Therefore the quantity inside the summation is the perimeter of the floes in bin i, per unit area, and the numerator is the total floe perimeter per unit area. After dividing by the sea-ice concentration c_ice, one obtains the total floe perimeter per unit area of sea-ice -- the floe PD.
-- Lines 23-24, "From the analysis of MEDEA-derived FSD results..." This sentence belongs in the section on "Calculation of P_i from the observations" at line 47, not in the section on the calculation of P_i from models.

Equation (4) (line 33) is the same as equation (3) of the main text (line 181). Also, lines 42-46, including equation (8), are an exact repeat of lines 184-188 of the main text, including equation (4) there. Why repeat the same material in the Supplement?

Minor Comments
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Line 173. The units of perimeter density are given as 1/meter but in much of the rest of the paper the units are 1/kilometer. Figures 3 and 7 use 1/km but Figure S1 uses 1/m.

Equation (3) -- notation.
-- On the right-hand side, the index "i" is used in the summation, and on the left-hand side, the index "i" is used as a subscript on P. This is bad notation.
-- Same comment for equation (5) and most of the equations in the Supplementary Materials.
-- The bad notation is easily fixed by dropping the subscript "i" on the perimeter density -- just use "P" (line 173 and following). There's no reason for a subscript.
-- Also, doubly-subscripted variables r_i_max and r_i_min are confusing and unnecessary. The quantity (r_i_max - r_i_min) is just the bin width of the i-th bin or category. It could be denoted w_i or something else with a single subscript i.

Equation (4). GAMMA is not defined. Also, I believe ALPHA = 2.56 in this case, which is perhaps worth repeating here.

Figure 1 caption. The date for panel (b) should probably be 12 June, not 6 June.

Figure 2. In panels (e) through (k), what is the meaning of "Effective" floe radius?

Table 2. The "a" and "b" superscripts are missing from the table.

Table 3. The caption says "FSDv2-WAVE and WIPoFSD" but the table itself lists FSDv2-WAVE and CPOM-FSD.