Author Response to:

Review for *The Cryosphere* of

Characterizing the Sea-Ice Floe Size Distribution in the Canada Basin from High-Resolution Optical Satellite Imagery

by Denton and Timmermans

Author Comments are in blue.

The authors analyzed 78 images from the Canada Basin of the Arctic Ocean. The images have 1-meter spatial resolution and span the period 1999-2014 during the months April to September. They identified individual ice floes in the images and analyzed the floe size distribution (FSD), where size is measured by floe area. They have four main results: (1) The FSDs follow a power-law distribution between areas 5×10^1 m^2 and 5×10^6 m^2 (50 m^2 to 5 km^2) with power-law exponents ranging from -1.65 to -2.03. (2) The FSDs are sensitive to the threshold used to separate ice from water in the images. Other studies may have erroneously found two power-law regimes by not setting the proper threshold between ice and water. (3) A linear relationship is found between power-law exponents and sea-ice concentration (SIC), with more negative exponents corresponding to lower SIC. (4) Locations that experience a seasonal cycle in SIC also have a seasonal variation in power-law exponent, but sites with high year-round SIC do not.

The analysis and conclusions of this paper are generally sound. I recommend publication after the authors consider the following comments and suggestions, which are given in page order.

Thank you for your thoughtful review with many valuable suggestions. We have taken into consideration each of your comments to improve the manuscript and our responses are inline below.

Comments and suggestions

Lines 9-11. "the structure of the FSD is found to be sensitive to a classification threshold value… and an objective approach to minimize this sensitivity is presented." I searched throughout the paper for the objective approach, but all I could find was this (on lines 163-164): "we iteratively increase the threshold above the minimum until the edges of small floes are appropriately delineated." I have no objection to this method, but I wouldn't call it an objective approach that minimizes anything. It sounds like "visual inspection" or "manual selection" to me. If that's what it is, please say so. If I'm missing the objective approach, please provide more detail.

Thank you. Our use of "objective" was referring to a choice that uses the histogram of pixel grayscale values, but we understand your point. We have altered the text on these lines to "an approach to account for this sensitivity is presented". We have also deleted the word "objective" in Sect. 2.2.

Lines 182-189. This describes the construction of the FSD by binning the data and fitting a line (in log-log space) to the binned values. That's fine, but it's a shame that the authors did not use Maximum Likelihood Estimation to find the best-fitting exponent, which does not require binning the data (and hence avoids the problem of having an adequate number of samples per bin), and which gives a more accurate estimate of the exponent than least-squares fitting of binned data. No changes necessary, I just wanted to bring up this point.

Thank you for your comment. We now also use the maximum likelihood estimator (MLE) to compute slopes; please see our response to Reviewer 2 with respect to this. We have found that the MLE returns slope values m_{MLE} that differ (on average) from our least-squares fit slope values m by about 3%. The mean m_{MLE} over our 78 images is -1.77 ± 0.11, while mean m is -1.79 ± 0.08 (where uncertainty bounds represent the standard deviation). We now include slopes m_{MLE} in Table A1 and have added the following sentences to the manuscript in Sect. 3.1: "We find no significant difference between slopes m and m_{MLE} (Table A1). The mean m_{MLE} over all images is -1.77 ± 0.11. Considering each image, m_{MLE} differs from m by about 3% on average".

Lines 199-203. The authors choose to use floe area as the measure of floe size, because once the pixels of a floe have been identified, it's a simple matter to count them and multiply by the area per pixel. However, it's also a simple matter to directly calculate the mean caliper diameter (MCD) from the coordinates of the floe pixels. I'm not talking about an approximation that relates the average MCD to the average area (as in Rothrock and Thorndike, 1984) – I'm saying that the MCD can be easily calculated exactly for every individual floe, as follows.

In a pixel-based coordinate system with (0,0) at the lower left corner of the image, let (x_k,y_k) be the pixel coordinates that comprise a floe, denoted by vectors x and y. Then:

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for angle = 0, 179 do begin          ; Loop over caliper angles from 0° to 179° in 1° increments
    c = cos(angle * pi / 180.)
    s = sin(angle * pi / 180.)
    r = c*x + s*y
    diameter(angle) = max(r) – min(r) ; This is the caliper diameter at the given angle
endfor
MCD = total(diameter)/180.           ; Average over all angles of the calipers to get MCD
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There's nothing wrong with using floe area as the measure of floe size, but in 18 previous studies of the FSD, 17 of them have used a diameter or dimensional length, and only one has used area. It would be easier to compare the results of this study with previous results if the authors had used MCD or another length scale. No changes necessary, I just wanted to bring up this point.

Thank you for providing the useful algorithm. We chose floe size to be represented by area as it is most directly relatable to floe models and the most computationally efficient in our set-up. Fortunately, the approximate relationship between mean caliper diameter (MCD) and floe area (Rothrock & Thorndike, 1984) works well. We have added the following phrase to a sentence in Sect 2.3: "In the present work, we use floe area because we obtain this directly in the segmentation (and it is directly relatable to floe models),..."

Lines 216-218. "the slope of the FSD is valid only for floe areas larger than 50 m^2 and we limit fitting in log-log plots (to estimate m) to floe areas between 50 m^2 and 5 km^2" How many floes fall outside these bounds, roughly? Is it 10% of all floes? 50%? Table A1 gives the "number of floes (whole) retrieved" (according to the caption on page 16). Does the number in the table include only the floes between the given bounds, or all floes? If it's all floes, then another column should be included in the table giving the number of floes with area between the bounds.

The number of floes in Table A1 includes all floes, and we now make this clear. Following your suggestion, we have added a column to Table A1 of the number of floes which are within the slope-fitting bounds (50 m^2 to 5 km^2). This value is between 10-55% of the total number of floes in the segmentation, and we have added the following sentence to Sect 2.3.1: "The number of floes which fall in this fitting range (see Table A1) is between 10 to 55 percent of the total number of whole floes segmented across the entire floe range between 5 m^2 and 100 km^2 (with the minimum floe count requirement of 2 per bin)".

Line 240 and Figure 3 caption. Some of the images are segmented (divided into ice and water) with high confidence and others with low confidence. Table A1 should indicate which images are segmented with high confidence and which with low confidence.

We now include a column in Table A1 indicating low and high confidence image segmentations.

Figure 3 panels (b) and (c) include "error bars representing one standard deviation" (lines 252 and 254). However, in some cases the number of samples is 1, so it makes no sense to calculate a standard deviation (which is 0) and call it an error bar. The number of samples for a given month and location never exceeds 7, and in most cases it's 1, 2, or 3. It's of dubious statistical rigor and utility to calculate standard deviations and call them error bars in these cases. Furthermore, the error bars make the plots more difficult to read. In my opinion, all the error bars should be removed from panels (b) and (c), and reference to them deleted from the caption.

Thank you for your comment. We have removed the monthly standard deviations as error bars and modified the caption accordingly; individual errors plotted on each slope value were too difficult to see on the plot, but these uncertainties are reported in Table A1.

Figure 3 panels (b) and (c) also include "mean monthly slopes" and "mean monthly SICs." In my comment below about Table A1, I note that not all the images provide independent estimates of sea-ice properties because some images were acquired on the same day of the same year at essentially the same location. For example, images #39, #40, and #41 are all from 20 May 2013 at the Northern Canada Basin site (see Table A1). Therefore, in calculating mean monthly slopes for May, the values from #39, #40, and #41 should be averaged first, and then their average should be averaged with the other May values from the same site (i.e., images #2, #13, #37, and #63). Perhaps the authors have actually calculated the means in this way. If not, I'm sure it wouldn't make much difference in the final results, but I just wanted to bring up the point about statistical non-independence of the images.

Thank you, this is a good point and we had not originally computed averages to account for this. We have amended our calculation of the monthly mean slopes and SICs at the three GFL sites

(Figure 3b and c), to be computed from all samples in a month after averaging any non-independent sample values acquired on the same day and site, and amended the panels to represent these newly computed means (there is negligible difference between these newly calculated means and our previous monthly means). In addition, we have added the following sentence to the end of Sect. 3.2: "Note that for each of the GFL sites, we compute mean monthly slopes after first taking the mean of any slopes from images acquired on the same day at a particular site, to account for the fact that these are not independent estimates (see Table 1). We do the same for mean monthly SIC at the GFL sites, discussed in the next section."

Lines 290-299. This paragraph is about the lack of a relationship between surface air temperature (SAT) and power-law exponent m of the FSD. The lack of a relationship is not surprising. During the Arctic summer, the SAT over ice is pegged at the melting point of ice because the surface is an ice bath (ice and water). For example, see the plots of SAT in Rigor et al. (2000). From the time of melt onset to the time of freeze-up (roughly June through August in the Beaufort Sea) there is essentially no variability in the SAT over ice, so it can't possibly explain the variability in FSD. The solar energy that goes into the ocean melts ice, potentially changing the FSD, but it does not raise the SAT over ice. In my opinion, it would have made more sense to look for a connection between m (FSD) and some other thermodynamic variable that characterizes ocean heat content or energy balance.
Rigor, I.G., R.L. Colony, and S. Martin (2000). Variations in Surface Air Temperature Observations in the Arctic, 1979–97. J. Climate, pp. 896-914, https://doi.org/10.1175/1520-0442(2000)013<0896:VISATO>2.0.CO;2

We agree that the range in SATs (here we consider the 2-m value) in summer may not be the most appropriate indicator to characterize the state of the pack because the presence of sea ice (at least when the concentration is sufficiently large) ties the 2-m SAT to a fairly small range. Because SAT is the most accessible and reliable external parameter to characterize the system, we use it here more as an indicator of when we might expect sea-ice melt, both at the atmosphere-ice and ice-ocean boundaries (i.e., when SATs fall approximately above 0ºC). We agree that the relationship between FSD and surface-ocean heat content (or ocean-to-ice heat fluxes) would be more insightful, and we now refer explicitly to ocean-to-ice heat fluxes in the manuscript. Although the concurrent data in this regard are limited, a small subset of case studies where this relationship can be reliably examined would be a valuable follow-up study.

Lines 303-307. The authors state that a power-law probability density function (pdf) of the form $n(a) \sim a^m$ where a is floe area is equivalent to a power-law pdf of the form x^{2m+1} where x is floe diameter. This is correct, but the reason given is wrong. It has nothing to do with the binning of data or the normalization of bin counts. It follows from a straightforward application of basic probability theory – see the notes at the end of this review. Lines 303-307 should be re-written to simply state that a^m is equivalent to x^{2m+1} as a result of basic probability theory.

Thank you for your comment and notes. We now state in these lines that: "From an application of basic probability theory, slopes reported in studies that examine the non-cumulative FSD using normalized floe number densities constructed from x are equivalent to $2m + 1$ (where m refers to slopes found in this study)."

Line 315. "image not included in our analysis due to partial cloud-cover" Somewhere in the paper the authors should state how many images were rejected due to partial or total cloud cover, and how those decisions were made. Was it by visual inspection?

Generally, the cloud covering (partial or full) is unambiguous in the Medea images and it is visually straightforward to decide to reject an image from analysis. We do not note how many images were rejected due to cloud cover, as there were many on the GFL which clearly indicated cloud cover and were rejected outright and not included in data download. See for example the image below taken on 22 May 2012 at the Chukchi Sea GFL site.
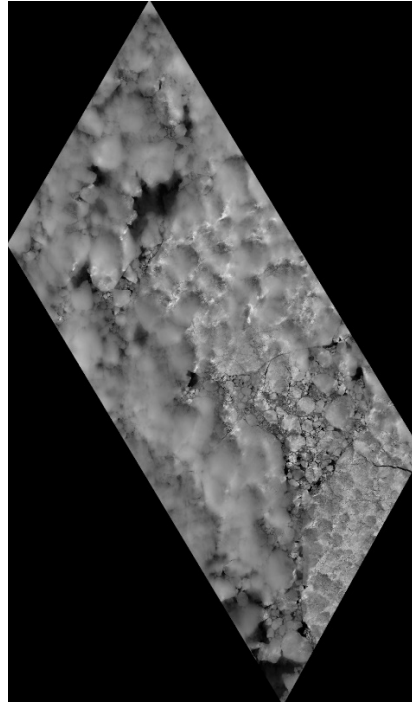
Cloud-covered Medea image acquired at the Chukchi Sea Global Fiducial Site on 22 May 2012. (Image from the United States Geological Survey Global Fiducials Library).

We have added the following sentence in Section 2.1, paragraph 1: "We note that partially or fully cloud-covered images on the GFL were generally unambiguous and rejected outright from our analysis. Cloudy pixels either fully obscure information about the ice cover below or interfere with the proper identification of floe outlines."

Lines 346-350. "seasonal variation in m is more directly related to changes in SIC" (than SAT). Yes, but as noted above, there's no reason to expect a connection between m and SAT. "Future studies are needed to investigate the relevant dynamics … and thermodynamics…" Yes, undoubtedly the same forces that drive changes in SIC also drive changes in FSD. This raises the question: why try to relate SIC and FSD in the first place? Neither one drives the other, they're both the result of underlying dynamic and thermodynamic forcing. It seems like SIC could never be more than an imperfect reflection of FSD. Imagine an image with a power-law FSD and a certain value of SIC. Now double the size of the image by adding only ocean pixels. The FSD remains exactly the same, but the SIC is cut in half. Conversely, it's easy to imagine a scenario

in which the power-law exponent of the FSD changes but the SIC does not. What is the motivation for relating FSD to SIC? Wouldn't it make more sense to look for connections between FSD and, say, wind stress?

Thank you for the comment. We were motivated to explore the FSD in context with SIC because a relationship between the two leads to follow-up scientific questions along the lines of what common dynamic/thermodynamic forcing might be responsible, and whether this relationship differs across seasons or regions. We do not see evidence for an independence like in the example case you give (SIC cut in half while FSD stays the same), although we could not rule that out as being a factor. The other case you note (varying FSD in the absence of varying SIC) would be a useful result. There are cases, for example, where the FSD slope could steepen (fewer large floes, more smaller floes) while the SIC remains the same, and this might indicate a fracturing. Conversely, a shoaling of the FSD slope associated with loss of small floes (e.g., via relatively rapid lateral melt of smaller floes compared to larger floes) may be associated with a different SIC-FSD relationship than some change to the floe field as a result of a wind event, for example. Our follow-up work is examining these effects (and as you mention, the relationship between FSD and wind stress).

We have added the following sentences to Sect. 4: "Future studies are needed to investigate the relevant dynamics (i.e., wind-forced sea-ice deformation and breakup) and thermodynamics (e.g., ocean-to-ice heat fluxes) of the sea-ice pack to explore the precise mechanisms by which the sea-ice concentration relates to the structure of the FSD, and how this relationship might differ in different settings. For example, a scenario might be envisioned where the FSD slope could steepen (e.g., as a result of fewer large floes, and more smaller floes) while the SIC remains the same, and this might indicate a fracturing. Conversely, a shoaling of the FSD slope associated with the loss of small floes (e.g., via relatively rapid lateral melt of smaller floes compared to larger floes) may be associated with a different SIC-FSD relationship."

Table A1. This is a good and useful table, but it is incomplete. As noted above, it should include the number of floes with area between 50 m^2 and 5 km^2 (i.e., the bounds used in determining the power-law exponent m) as well as the total number of floes (which is given already, I believe).

We have included a column in Table A1 which provides the number of floes with area between 50 m^2 and 5 km^2.

Also, the table should indicate which images were segmented with high confidence and which with low confidence.

We have included a column in Table A1 which indicates high and low confidence segmentations.

Also, there should be a column to indicate whether an image is from the Beaufort fiducial site, the Chukchi fiducial site, the Northern Canada Basin fiducial site, or another site. I realize that this can be inferred from the latitude and longitude, but it is extremely tedious to go through the table and extract that information.

Thank you for pointing this out. We have included this information in Table A1.

Another point that should be noted somewhere in the paper is that some of the images were acquired on the same day of the same year at essentially the same location, and therefore do not provide independent estimates of sea-ice properties (FSD and SIC). In particular:
#3 and #4 are both 27 July 2000 at the N. Basin site
#5 and #6 are both 15 Aug 2000 at the N. Basin site
#14 and #15 are both 23 May 2002 at the Beaufort site
#39 and #40 and #41 are all 20 May 2013 at the N. Basin site
#43 and #44 are both 10 June 2013 at the Chukchi site
#45 and #46 are both 12 June 2013 at the Beaufort site
#57 and #58 are both 28 April 2014 at the Beaufort site

We have recomputed the means and noted this in the text (see our earlier response).

I found it useful to summarize the number of images by location and month in a table. Consider including something like this in the paper:

	Beaufort	Chukchi	N. Basin	Other	Total
April	5 (*)	2	1	17	25
May	7 (*)	2	7 (*)	4	20
June	2 (*)	5 (*)	1	7	15
July	1	1	3 (*)	3	8
Aug.	1	0	3 (*)	2	6
Sept.	1	0	1	2	4
Total	17	10	16	35	78

The (*) symbol indicates that some of the images are from the same day of the same year at essentially the same location, i.e., not independent samples.
Beaufort site is shown in Figure 1(a) at the letter e.
Chukchi site is shown in Figure 1(a) at the letter c.
N. Basin site is shown in Figure 1(a) at the letter d.
Other sites are shown in Figure 1(a) at white dots.

Thank you for your suggestion. We have included a Table 1 in Sect 2.1 with the number of images (per month and total) at each GFL site.

Minor typographical notes

Line 6. Change "atmosphere and ocean" to "atmospheric and oceanic" as on line 22.

We have amended the text accordingly.

Line 64. "Aeronautical" should be "Aeronautics"

Thank you. We have altered the text accordingly.

Line 66. "The images AT FIDUCIAL SITES are panchromatic…"

All images used in our study are panchromatic, not just images at fiducial sites.

Last sentence on page 3. Please say that the surface air temperature (SAT) is at the 2-meter level.

We have amended the text accordingly.

Figure 1 legend at upper right. The blue circle is labeled "August 1999" but it should be August 2014 as in the figure caption (line 90).

Thank you for catching this typo; we have corrected it. We have also corrected the location of e on the map and added asterisks to more clearly delineate GFL fixed sites. We have altered a sentence in the figure caption: "USGS fiducial sites (black asterisks), for which there are images from multiple years, are noted to the southeast of e (Beaufort Sea), at c (Chukchi Sea), and at d (northern Canada Basin)".

Lines 101-107 and 125-142. This is a description of the floe-identification algorithm. If it's the same algorithm as in Stern et al (2018b) then that should be explicitly stated; if not, no changes necessary.

Thank you for your comment. The algorithm is influenced by multiple studies' floe segmentation approaches which are referenced in the text. While inspired by and similar to Stern et al. (2018b), and specifically including Stern et al. (2018b)'s hierarchical segmentation scheme into our segmentation algorithm (as is referenced), it is not clear based on the description by Stern et al. (2018b) whether the algorithm is exactly the same. For instance, we do not use the morphological dilation operation from Serra (1982) to expand floes to their original size, as referenced by Stern et al. (2018b), but rather a rule-based expansion using the numerical mode of the floe label neighborhood as presented by Paget et al. (2001). Stern et al. (2018b) indicate that their algorithm is "similar to that of Paget et al. (2001)", but not that it is identical. We therefore have not made any changes to our description except for the addition of a reference to Stern in Sect. 2.2 under "Erosion and Expansion".

Line 199. "The floe size may be taken to be any scalar representative of the floe size" – consider re-writing this.

We have amended the text to read "The floe size may be taken to be floe area a, perimeter, or a diameter proxy such as the mean caliper diameter (MCD), …".

Line 206. Delete "e.g." and change "black dotted and dashed lines" to "black, red, and blue dashed lines"

We have amended the text accordingly.

Line 210. "Fig. 2c through e" – should this be Fig. 2b–d?

Thank you. We have amended the text accordingly.

Figure 3 caption
(i) Both "grey" (line 246) and "gray" (line 250) are used. Pick one spelling.

We have altered the text to use the American English spelling of the color, "gray".

(ii) Line 248 refers to the "black dotted line" of 19 June 2014 in panel (a). To me it looks like a solid black line with black circular symbols. See also the legend in the lower left corner of (a).

We have amended the caption text accordingly.

(iii) Line 255, change sites' to site's

Thank you for noting this; we have amended the text accordingly.

Line 286. "m appears to generally shoal with distance to the ice edge" – does this mean with INCREASING distance to the ice edge or with DECREASING distance to the ice edge?

We have altered the text to read "m appears to generally shoal with increasing distance from the ice edge".

Line 319. "conclude" should be "concluded"

We have altered the text accordingly.

Lines 347-348. This sentence repeats what was just said two sentences earlier at lines 342-343. It is redundant.

We have deleted the corresponding sentence on lines 347-348.

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