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Interactive comment on "Estimating The Sea Ice Floe Size Distribution Using Satellite Altimetry: Theory, Climatology, and Model Comparison" *by* Christopher Horvat et al.

Anonymous Referee #2

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General comments: This paper aims at developing a new method to obtaining the floe size distribution in the Arctic Ocean using the chord length data derived from the CryoSat-2 altimeter data. The conversion of the chord length distribution to floe size distribution statistically seems to the highlight of this paper. The authors accomplished this with a strong mathematical background and an assumption of circular shapes of ice floes and homogeneous and isotropic distribution. With this method, they attempted to show the geographical, seasonal, and interannual properties of the floe size distribution in the Arctic Ocean for the first time. They also tested the validity of power law distribution which has been applied for the floe size distribution frequently. As a result, they concluded that although power law scaling cannot be ruled out, the statistical ba-

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sis is limited and especially the assumption of power law is weak in the Chukchi and Beaufort seas. They also emphasized the refinement of this method for the operational use.

Floe size distribution is one of the important parameters of sea ice and should be considered to understand the behavior of sea ice area. However, due to the limited observations, its statistical properties are not clarified yet. The idea of using the chord length is interesting in that it can cover wide areas and the truncation error caused by the traditional analysis of finite area seems to be reduced. Therefore, I agree this paper presented an interesting and useful method to analyze the floe size distribution. But to do so, I think the new method should be validated carefully with observational data and the consistency should be confirmed by comparing it with the traditional method. This paper focuses mainly on the availability of the new method with their strong mathematical background. While I feel this is an important step, the validation of this method seems relatively weak. In this case, I think the validation is especially important because their method is based on several assumptions such as circular shapes or homogeneous distribution. Thus, while I agree their method is interesting, my evaluation for this paper is somewhat reserved. In addition, the descriptions are not necessarily readable at places for me, and might be improved, I feel. The major points are as follows:

1) Assumptions of this method. The authors assume that the floe chord distribution data is homogeneous, isotropic, and stationary within the region, and time data is collected (P3L17-18). For simplicity in mathematical treatment, it might be allowed. But I think the validity of these assumptions should be examined somewhere in the paper. For example, fracture patterns caused by shear stress near the shore are far away from circular shapes (e.g., Schulson and Hibler, 1991 Journal of Glaciology). As a preliminary step, I encourage the authors to confirm this method is available even in such situation based on the real floe patterns. Besides, to make an assumption of stationary distribution, the time scale on which this assumption is valid should be discussed

because they discuss the seasonal and interannual variation of FSD later. If they examined that their method is applicable to obtain floe size distribution by comparing with the traditional analysis based on real satellite images, the value of this paper would have been enhanced significantly. Theoretical support with mathematics is important, but that is not enough, I think.

2) Technical matters * To represent floe size, they used a radius of the equivalent circle (P3L7). Is it common? To my knowledge, a diameter has usually been used in the past studies. I think a diameter fits the sense of floe size better, although this might be an essential matter.

* In the past studies, floe size distribution has been represented in two ways: cumulative number distribution and non-cumulative one. To avoid confusion, it might be better to declare which way this paper takes somewhere in section 2.

* In section 3, how they determined the individual chord lengths from the Cryosat-2 records is still unclear to me. When ice floes are contacting the neighboring floes, how did they determine the boundary? How much is the measurement accuracy of vertical distance? If it is about 10 cm, it would be quite difficult to identify the edge of the floe for thin ice especially. I think this is a critical matter.

* In section 4, they tested the validity of power-law distribution. It might be possible that the real FSD may have different regimes although the power-law is applicable for each regime. Figure 4 may suggest such possibility. In such a case, how do they judge the validity?

3) Interpretation of the results Overall, I feel the discussion of the results might be a bit weak compared with the preparatory statement about mathematical treatment. For example, they showed “During the months of October-December, the climatological representative radius is roughly 35% larger than February-April” (P8L32-34). I would like to know how they interpret this result because from intuition floe size tends to become larger at the later growth stage. I am wondering if the measurement accuracy

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might affect this result significantly.

4) The applications of the results They examined the validity of power-law distribution (section 4) and the application to numerical sea ice models (section 5). But since the validity of this method is not fully investigated from the observations, I am wondering if these applications might be really useful. Especially, it was difficult for me to understand the authors' intention about section 5. Personally, at this stage I prefer focusing on showing the validity of this new method to extending the results to models.

Specific comments: *(P1L2) "spare" should be replaced by "sparse".

*(P1L15) "covered in sea ice" might be "covered with sea ice".

*(P1L18, P2L12) "Rothrock and Thorndike, 1984b" I could not see the difference between 1984a and 1984b in the reference lists. If they are the same, please take "b".

*(P2L19-20) This is true. But to describe this, it would be needed to show that FCD is better than FSD in accuracy.

*(P3L15) "the fraction of floe chords" should be "the number fraction of floe chords"?

*(P3L3) In Eq.1, "F(r;D) S(D)" can be replaced by "F(r;D) S(D) dD dr" to show the number of floes which have radius between r and r+dr. The same applies for the righthand term.

*(P3L24) I wonder if "F(r;D) dr dD" should be "F(r;D) S(D) dr dD".

*(P3L27) Likewise, I wonder if "F∼(D;r) dr dD" should be "F∼(D;r) P (r) dr dD".

*(P4L1) D/r should be from 0 to 2 (not 1).

*(P5L1) I wonder that since they consider a circle rather than a semi-circle, theta should take between 0 and 2*pi (not pi).

*(P5L1) Please state the definition of the function T.

*(P6L18) Please insert "under the condition, alpha > n+1" after "explicitly".

*(P6L23) In Eq.13, I wonder if “e” (epsilon) should be “1-e”.

*(P7L5-12) Sorry, but I could not follow this paragraph. It would be helpful if you rewrite this paragraph with an emphasis of your intention.

*(P9L5-6) This sentence may contain grammatical error. Please rewrite it.

*(P9L9) “representative radius from fall and spring” might be “representative radius between fall and spring”.

*(P11L11-13) I could not understand this sentence.

*(P12L4-5) “Assuming. . . parameterizations.” is hard to follow.

*(P13L16) “The two. . . hypothesis.” I could not understand why.

*(P13L29) “below 300 m” should be “below 30 m”. Please check Steele’s paper. Accordingly, “necessitating a maximum floe size of 1 km” should be reconsidered.

*(P16L19-20) “Floe size modeling efforts have focused on the marginal ice zone” I think some citation is needed.

*(P16L30) I could not understand the meaning of “structural uncertainty”.

That is all. Faithfully yours.

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