

Review on the manuscript “Seasonal evolution of the sea ice floe size distribution from two decades of MODIS data” by Buckley et al.

Thank the authors for detailed responses to my previous review and they made a major revision to this manuscript. Most of the raised questions and comments were addressed. There are still some issues need to be further clarified, mainly concerning the floe segmentation method and accuracy validation.

A major comment:

The improvement of this floe segmentation method compared to the previous methods is minimal. These fundamental algorithms were proposed based on a relative small amount of data or small coverages. Therefore, the present comparison and validation are still in doubt. Why the improved algorithm can be applied to a large amount of dataset and large spatial coverage are not very convinced. In their responses, it is mentioned that "Also each Sentinel-2 image is 110 km x 110 km, so the analysis of three Sentinel-2 images and coincident MODIS imagery allows for comparison of 36,300 km², a sufficient area for validation.", and "We need to keep in mind that the availability of validation imagery is very limited." However, these 36,300 km² is like “a drop in the ocean” compared with the three-years data in the vast Beaufort sea. I still suggest to add some quantitative methods to measure the effectiveness of the proposed segmentation method, rather than relying only on comparisons with the limited Sentinel-2 data.

Specific comments:

1. P3 Fig.1 (c): Rather than showing the annual average ice floe numbers here, it might be more suitable to show the data amount of processed MODIS in this figure. In particular, long-term statistics need to know the amount of cloud-contaminated data for each year.
2. P6 L118: The authors mentioned in the revised article, "The morphological erosion operation is applied to the binary image, removing pixels on the object boundaries with a diamond-shaped structuring element with a radius of 1." Why is this

improvement more suitable for the task compared to previous methods? Why is the radius set to 1?

3. P7 section 3.6, the authors mentioned: "The same algorithm was applied to both datasets, and there is a step to remove low-intensity objects." For floes in the S2 data, is it reasonable to remove floes with an intensity lower than 150, just like in the MODIS data?
4. Despite the comparison with S2, as shown in Figure 3, the method has produced results with a significant amount of undetected floes. Consequently, conclusions drawn from the analysis based on these segmentation results would be greatly affected by the algorithm's performance. For the large amount of MODIS data that cannot be compared with S2 data, it is impossible to ensure the comprehensiveness of the detected floes. Therefore, the conclusions may be unreliable.
5. I am particularly interested in how floes are defined in areas with high sea ice concentration, as shown in Figures 3(g) and (h). How can we determine whether a floe ice is an independent entity rather than having re-grown together with other floes?
6. In the authors' response to the specific comments on L149-151 in the first-round review, they stated, "There is another question of the omission of smaller floes that I believe the reviewer may be referring to. We are carrying out a further error analysis to understand these limitations, but this is beyond the scope of this paper." In addition to the smaller floes, some large floes were also omitted.