Floe size distribution (FSD) becomes a very important parameter in nowadays sea ice modelling; however, high-resolution imagery seems to be the only source to obtain such kind of information. Thus, an automatic image-processing method is also important in this field. This study provided a deep learning-based segmentation method to process airborne and optical satellite images, and obtained good results of FSD. It seems that a completely automatic method to get FSD becomes possible.

Actually, it is not the first time for me to review this manuscript. I understand the solid revision that the authors have conducted to improve the paper. I still encourage the authors to address the remaining issues, and make the manuscript smoother to follow. Such an interesting topic merits publications and will be valuable for more accurate access on FSD.

We thank for reviewer for careful review and helpful feedback on our manuscript. Please find our responses to your comments below.

The abstract talks more about the background, instead of the solid achievements in the present study. I suggest a shorter background, and more results of the present study should be presented.

Thanks for the suggestion! We will revise the abstract.

1. Is there any relationship between the airborne data in 2.1 and the satellite data in 2.2? Or both of them are employed here just to test the effect of the new method on different kind of imagery.

There is no relationship between the airborne data and satellite data.

The local-scale airborne MIZ data were used to train and test DL models. The satellite data were additional data only used to test the generalization ability of the DL models from local to global scale, and they were never encountered by the DL models during the training.

2. Lines 210-215. “U-Net++ with the depth of 5 achieved the best floe instance segmentation”, is this a result of “experiments to compare the performance of U-Net++ with other SoA semantic segmentation architectures”? I mean if you have known U-Net++ is the best among all, why do you compare them again? And for the other methods such as ResUNet, ResUNet++, additional explanations should be added here to tell the difference between them.

Yes, it is the result of the comparison among different DL models.

It is common to show performance comparisons between different DL models if using DL method. We will move this model comparison subsection to the appendix and add more descriptions and diagrams about models.

3. There are two fig10e. And for figures 9-10, the difference between these results are very difficult to distinguish if no additional notations such as in fig11e are presented.

Thanks for the suggestion! We will add notations to these figures and correct the wrong figure numbering.

4. It is a little difficult for me to follow the contents in sections 4 and 5. A possible reason is that so many names of processing methods are presented here, and also two kinds imagery are included as examples to show the effect of these methods. I was not told why airborne data were employed here but satellite imagery were employed there. Thus the main improvement of the present study are submerged by these information.

We apologize for the confusion. We will adjust the structure of the manuscript and revise sections 4 and 5.

5. There are some very interesting results in section 6, for the variations in the power-law exponent, can you give some more explanations on them? Otherwise, it is not necessary to present so many pictures as example without any discission.

It is common to use some typical images to show the effect of the method. Therefore, we chose these floe images with low to high ice concentrations to present segmentation results made by the DL-based methods.

The determination of the ice floe size distribution is a further application after the ice floe segmentation, and it is also part of our ongoing project. We thanks for the suggestion, and we will give a brief explanation on it in the revised manuscript.

6. There is a so quick stop in the conclusion section, can you give some evaluations on the limitations of the present study?

Apologies for the short conclusion and thanks for the valuable suggestion! We will add some descriptions of the limitations of this study in the revised manuscript, e.g., floes with many melt ponds and cloud masking.