Reply to Referee Comment #2 (RC2)

The authors would like to thank the referee for the invaluable comments and suggestions. The following are the replies for each point of the comment, together with specific revisions that are made. The original comments are in *green italic* font and listed in paragraphs, with our reply following each paragraph separately. The revisions are also highlighted in the revised manuscript in green and marked by ***RC2***.

Dear TC editor and authors of the manuscript egusphere-2025-1069,

The manuscript topic is interesting for sea ice monitoring applications.
In its current form I can not recommend publication of this manuscript.
There exist significant deficiencies in the manuscript.
Therefore, I recommend a major revision before considering publication.

The major deficiencies that need to be addressed are:

A small amount of data: only 11 SAR images and these data along the measurement lines have been used in the study. It would be good to have more data included to be able to provide more general results. In its current for the study is both locally and temporally very restricted and remains only a case study.

If it is not possible to involve more data, it must be emphasized that this is a case study, also in the title, instead of "study" use "case study".

Reply: As suggested, we have revised the tite as a "case study" of the relationship between sea ice topography and microwave backscatter. The revised manuscript has the title: "*On the Statistical Relationship between Sea Ice Freeboard and C-Band Microwave Backscatter - A Study with Sentinel-1 and Operation IceBridge*"

Can any general conclusions be made based on this analysis? Future work should concentrate on analyzing larger data sets with different weather and ice conditions.

Reply: We thank the reviewer for the comment, and we believe that the analysis with OIB data and SAR images does indicate that there exist statistical relations between ice topography and C-band microwave backscatter. We totally agree that analysis with larger datasets is necessary and it is planned for future work.

Besides, we also want to emphasize the major points of our study:

1. Our analysis covers a range of ice regimes in the Arctic, including thick and thin multi-year as well as thin first-year ice. For all these types, there exists a statistically significant relationship between the backscatter and freeboard. We believe this is a new and important advance for our field.
2. The reprocessing of OIB ATM data allows us to examine this relationship at various spatial scales, from meter-scale freeboard distribution, to typical SAR pixel sizes, and

reaching scatterometer-relevant, kilometer-scales. This serves as the basis for working with different altimeter and SAR payload combinations.
3. We examine the locality of the statistical relationship, which is shown to be affected by sea ice type mixture spatially, as well as sea ice deformations due to large temporal separation. This information is key for large-scale synergy between altimeter and SAR observations.
4. One major focus for the freeboard prediction is on the freeboard distribution of the altimeter's native resolution. For OIB-ATM, the prediction is on the meter-scale, and for the prototype study with IS2, the prediction is on the scale of beam segments (~30m).

The need for future work to extend the study and directions of the future work taking thes aspects into account should be clearly mentioned in the concluding section (summary and outlook).

Reply: Based on the referee's suggestion, in the revised manuscript in the conclusion section, we will highlight the following key directions for future research:

1. Future work will incorporate a larger dataset to investigate the freeboard-backscatter relationship for regions with more first-year ice across seasons/stage of development (FYI). In this study, we have explored this relationship for MYI-dominated regions. So we plan to extend the study with the historical OIB campaigns with Sentinel-1 collocation, which covered various sea ice types and different areas of the Arctic basin.

2. We will also examine how the statistical relationship between freeboard and backscatter varies under different weather conditions. Factors such as melt conditions and heavy snowfall could potentially alter both the backscatter and the overall snow budgets. We will further study the changes and potential degradation of the freeboard-backscatter relationship.

Weather condition data, except for the wind data has not been included. SAR backscatter is significantly dependent on the ice or snow surface temperature and they are naturally dependent on the air temperature history before the data acquisition. Therefore, I propose to include this information and emphasize on what kind of ice and snow (on ice) conditions the proposed results are useful.

Reply: we have examined the weather conditions around April 8th and 12th when OIB campaigns were carried out (from April 1st to 16th). These weather conditions are based on the atmospheric reanalysis of ERA5. Hereby we summarize the weather conditions as follows:

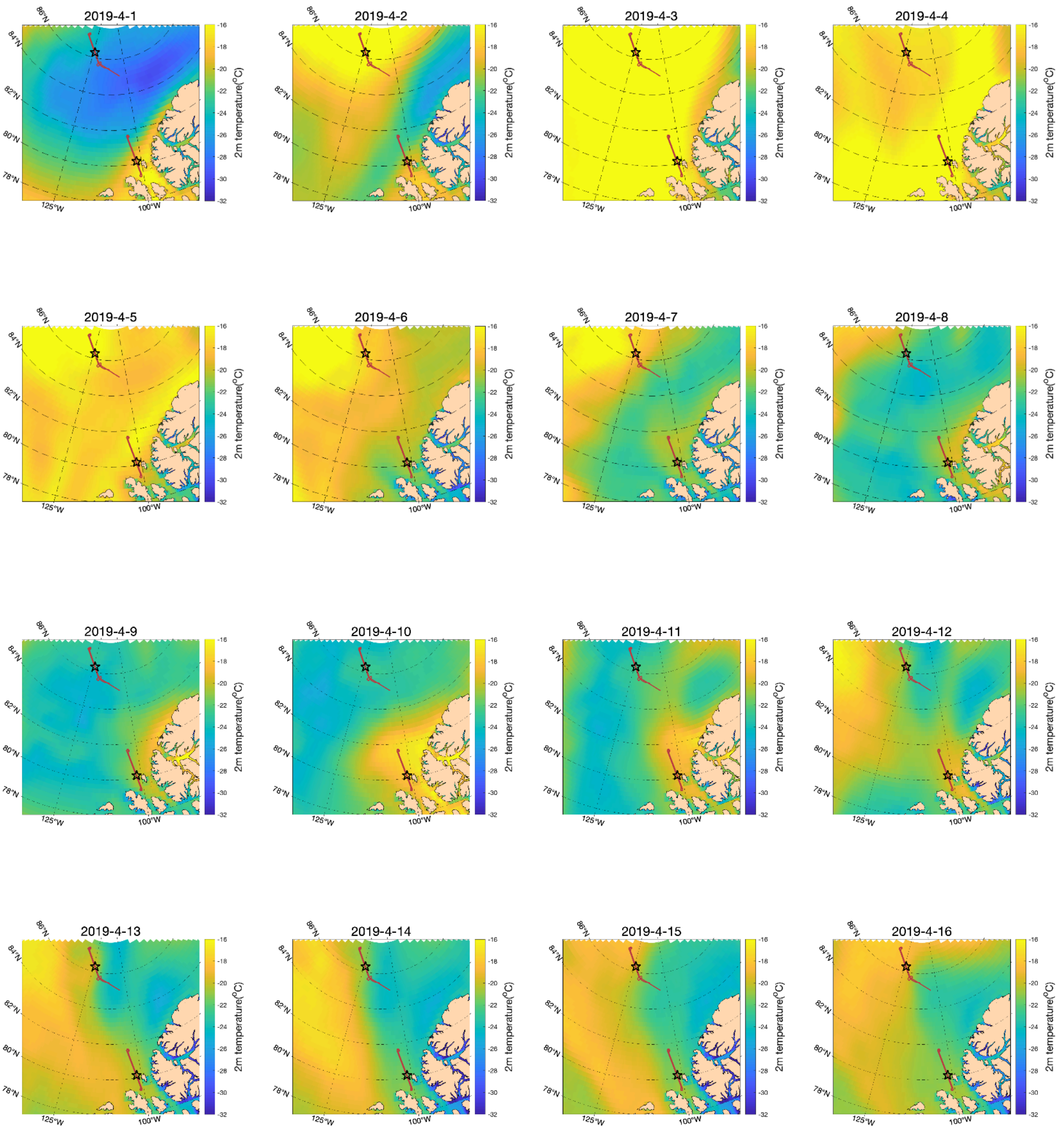
1. For both campaigns, the atmospheric conditions were cold (Tair < 16degC), typical of the condition in April in the surveyed regions.
2. The SLP fields around the region surveyed on 8th indicate very weak wind forcing on the sea ice, with limited sea ice drift. For the surveyed region on 12th, the (geostrophic) wind is weak during the days around 12th, with no evident sea ice drift.

3. The precipitation during the whole week in the OIB-surveyed regional before the campaign is minimal (SWE<3mm) for both campaigns.

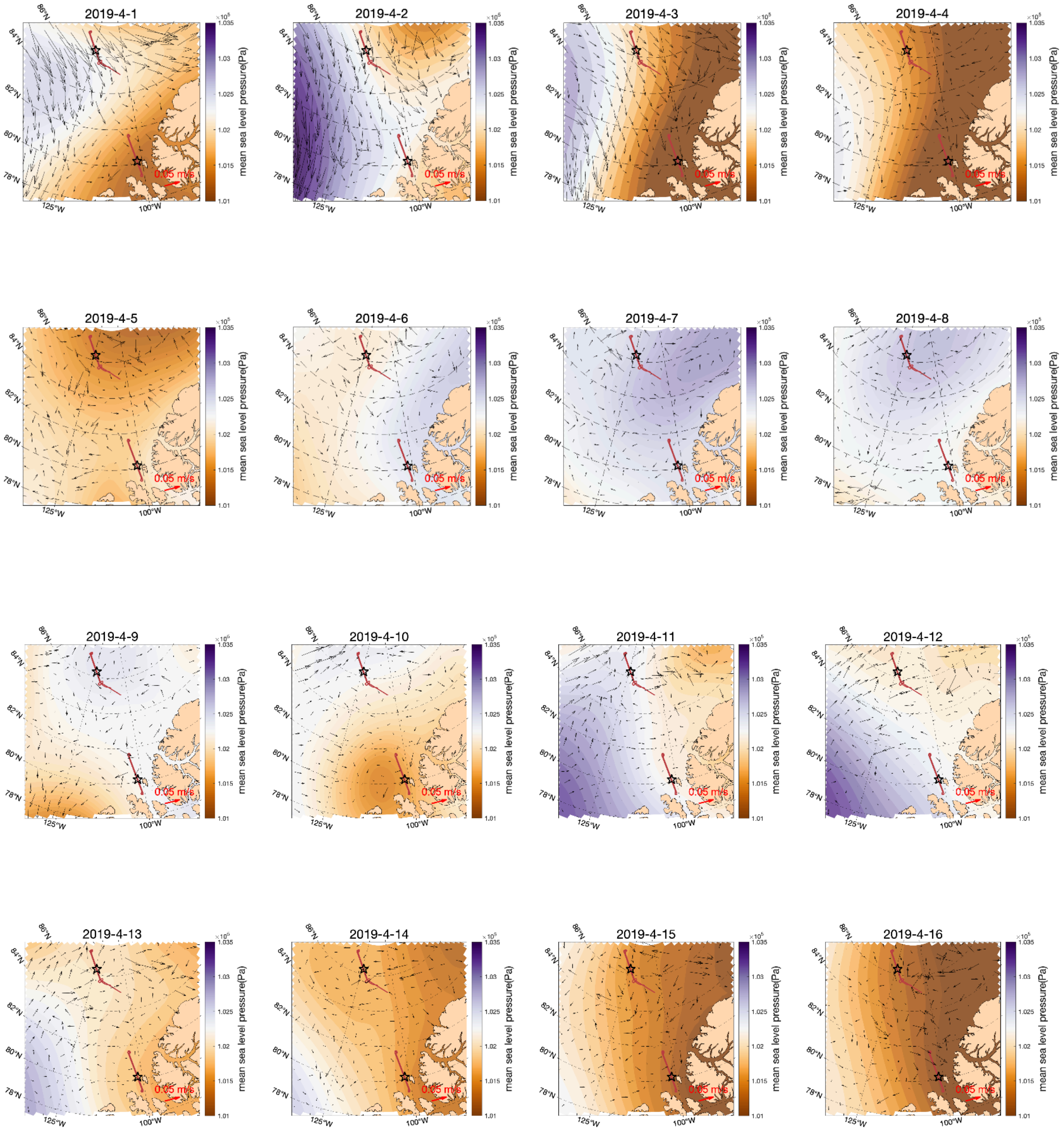
From these results, we draw the conclusion that the large-scale atmospheric conditions are typical of the later-winter condition in the respective regions. There were no sudden warming events and evident precipitation that potentially changes the SAR backscatter signature of the sea ice.

Moreover, at C-band, the SAR backscatter is dominated by the wavelength-scale roughness at the bottom of the snow cover. However, at X-band which the wavelength is much shorter, the backscatter is much more sensitive to snowfall and stratigraphy changes such as warming and refreezing.

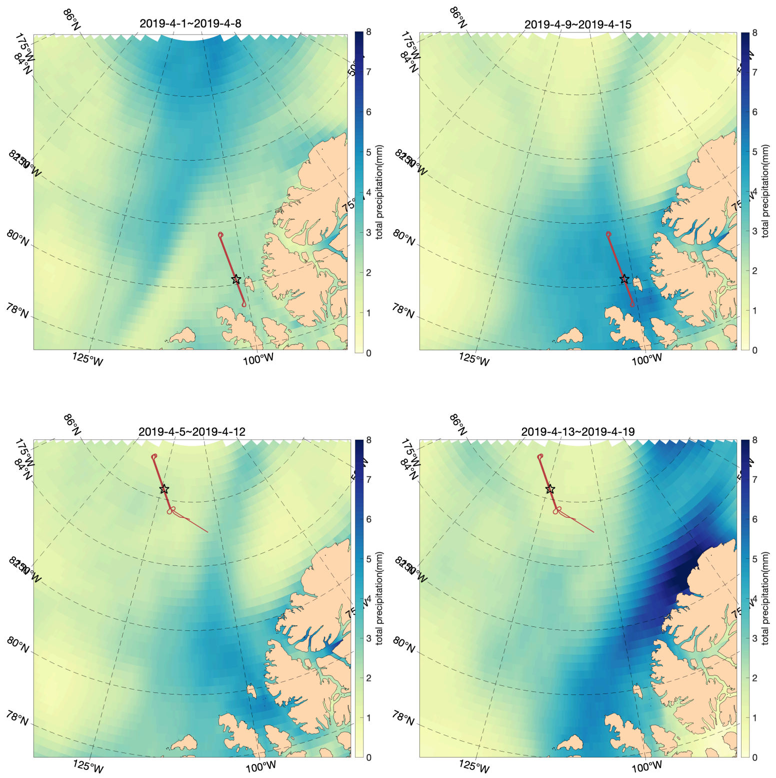
Air temperature (daily mean, degC):



Sea level pressure (daily mean, filled contour, in Pa) and large-scale sea ice drift (arrows, from OSI-SAF LR product, 48hr):



Total precipitation (weekly accumulated SWE, in mm)



It seems that the results for the HV channel are presented in the supplement. Why? The HV results should be part of the manuscript. Also, the information in appendix A and B could be included in the manuscript sections and leaving the appendices out.

Reply: We thank the referee for pointing out the importance of the analyses with the HV channel. The motivation of providing them in the supplementary instead of the main manuscript is its conciseness.

We have incorporated the HV channel results into a newly added appendix, including Figure S3 and Figure S5 (which were in the supplementary file), along with the HV relationship corresponding to Fig. 6.

Regarding Appendix A, we understand the importance of clarity and conciseness in the manuscript. Appendix A focuses on the collocation between OIB passes, which is a critical part of our data preprocessing. While Figures A1 and A2 are large and primarily serve to support the correct collocation between OIB passes, their direct contribution to the main narrative of the manuscript is limited. Therefore, to prevent the manuscript from becoming lengthy, we have decided to retain Appendix A as appendix material.

The methods should be persented in detail in a specific section (named e.g. "Methods" or "Methodology"). This should be after Section 2, i.e. Section 3.

Reply: According to the suggestion, we add a new section titled "*Methodology*", which is Section 3 of the revised manuscript. This section provides a detailed description of the methods for the statistical analyses of the relationship between freeboard and backscatter, as well as the method for predicting freeboard distribution using σ0 maps.

It is not very clear how the SIT distribution prediction based on SAR sigma0 is exactly performed. Please, describe this essential phase in detail. All methods/algorithms should be described in detail in a specific methodology section.

Reply: The detailed description of the method for predicting freeboard distribution is included in the newly added "*Methodology*" section.

More detailed comments:

How the data is divided into independent training and test data sets for the regression.

This should be described in detail.

Reply: The OIB flights were organized into racetracks, with the outbound (north-bound) and the inbound (south-bound) paths covering adjacent, but different sea ice cover. We use the inbound flight data (OIB-ATM and collocated SAR) for the training, and the outbound flight data for testing the regression. Therefore, the training and the testing use independent data. This information is now described in detail in the "Methodology" section of the revised manuscript.

Incidence angle is mentioned and in the ice type classification it is taken into account.

However, it seems IA has not been taken into account in the sigma0 analysis. Would there be any effect if e.g. a simple linear incidence angle correction were applied (different slopes for sea ice C-band HH and HV sigma0 can be found in literature)?

Reply: We agree that with a simple incident angle correction, the regression relationship will quantitatively change. Given that the segment length of 9km is used for our analysis, the correction induced change in Sigma0 within the segment is considered small. Therefore, the correction will change the intercept of the regression model, but not its slope. However, if a more complex angle correction scheme (such as the sea ice type dependent correction) is applied, both the slope and the intercept of the regression model are susceptible to changes.

In this study we refrain from using an incident angle correction mainly for two reasons:

(1) The incidence angle dependence of Sigma0 is clearly type dependent, which is different between FYI and MYI, as well as between level ice and ridged ice. A simple correction is not sufficient to account for these factors (see also Lohse et al. (2020), Kwok et al. (1992), etc.).
(2) The *optimal* incidence angle for the correction is unknown for deriving the statistical relationship between Sigma0 and freeboard. Whether it should be the near range

(i.e., 20deg) or the far range (i.e., about 50deg) in the SAR image's swath (Sentinel-1 EW mode) should be studied further in future work.

We do notice that the incidence angle dependency is relatively weaker for HV backscatter (Lohse et al., 2020). As a result, the changes in the regression parameters (i.e., the slope and the intercept) after incidence angle correction at HV channel is expected to be smaller than those at HH channel.

Lohse J, Doulgeris AP, Dierking W. Mapping sea-ice types from Sentinel-1 considering the surface-type dependent effect of incidence angle. Annals of Glaciology. 2020;61(83):260-270. doi:10.1017/aog.2020.45

Ronald Kwok, Eric Rignot and Benjamin Holt. Identification of Sea Ice Types in Spaceborne Synthetic Aperture Radar Data, Journal of Geophysical Research, 97(C2), 2391-2402, 1992

At HV band it is well-known that S-1 has a significant noise pattern in range direction, especially near the boundaries of the subswaths. Also, some scalloping noise in the azimuth direction at HV may appear. Could the effect of these noise components be estimated or evaluated somehow? At least, this should be mentioned in the manuscript.

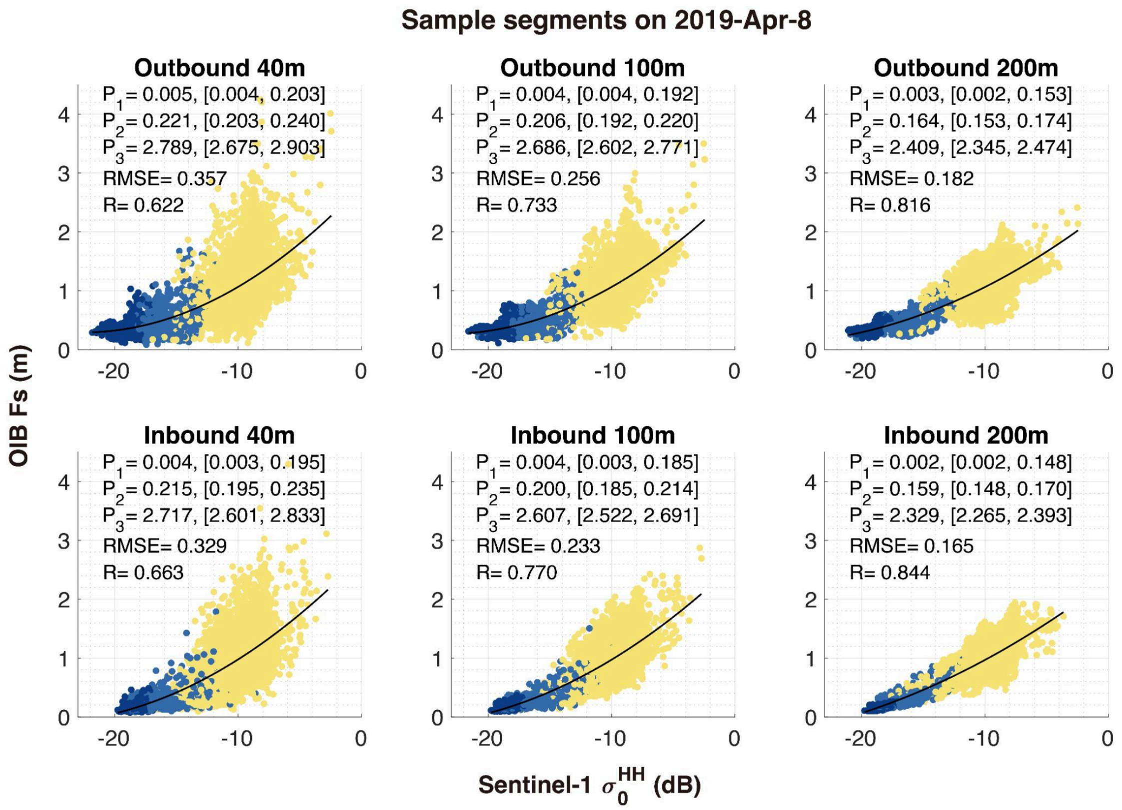
Reply: We do notice evident noise patterns related to subswaths in both HH and HV channel, with the HV channel showing higher noise, which is pointed out by the referee. In this manuscript, we do not correct for the incidence angle, nor the subswath-related noise patterns (see our reply to the previous comment). Their effect on the statistical relationship is not fully studied, which is planned for future work.

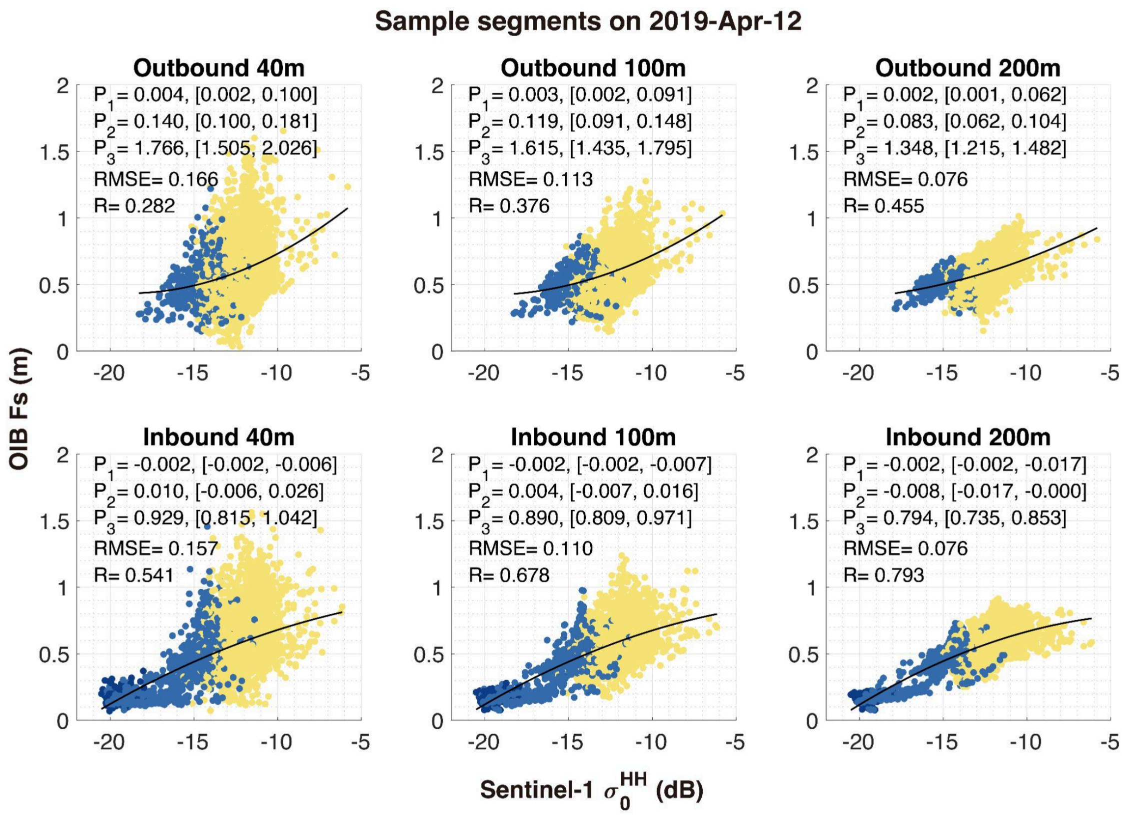
For revision, we add the proper statements for Sentinel-1 EW mode image noises to the data description part in Section 2 of the revised manuscript (Sun et al., 2021).

Sun Y, Li X M. Denoising Sentinel-1 Extra-Wide Mode Cross-Polarization Images Over Sea Ice[J]. IEEE Trans. Geosci. Remote. Sens., 2021, 59(3): 2116-2131.

In the figures, e.g. Fig. 2 and 3, the plots do not look line there were a linear dependency. It would be good to test regression with second order terms also and see whether they provide better results. In a linear case the higher order regression coefficients would be close to zero anyway.

Reply: We totally agree that incorporating a nonlinear (i.e., second-order) term in the regression model could potentially improve the fitting results. Below, we show the 2nd-order polynomial regression for two sample segments on April 8th and April 12th (Fs = a*Sigma0^2+b*Sigma0+c):





For both segments, the results show slightly better fitting (i.e., higher R2) than that in the manuscript. Based on the referee's suggestion, we have included the results with nonlinear fitting to the supplementary, along with necessary revisions in the main manuscript.

However, we show the results with the simple linear regression model for the following reasons:

1. We do not fully understand the relationship between the backscatter and the freeboard, which is dominated by the different scattering mechanisms of various sea ice and its snow cover's conditions. Remarkably in the figure above, the relationship in the FYI is already nonlinear for the survey on 12th. Since these underlying mechanisms are not fully understood, we consider exploiting complex fitting models unnecessary at this point.
2. The linear regression model, although simple, captures the statistically significant relationship between the two. Especially, after binning to Sigma0 (Fig. 4 and 5 of the manuscript), the relationship is highly linear at large spatial scales (i.e., 100m and 200m).
3. Last but not the least, we want to reemphasize that our focus for the freeboard upscaling is the prediction of freeboard distribution, not the point-to-point prediction of the freeboard map. The prediction of freeboard distribution does not rely on the specific form of regression from Sigma0 to freeboard.

Pay attention to the subfigure labels. The labels are now within the subfigures and in some cases in a colored area making them difficult to see, at least in printed versions. The labels (a, b, ...) should be in a fixed position and preferably on white background.

Reply: These figures have been revised in the manuscript.

In Figs. 2 and 3 also describe the subfigures b and c in the image captions.

Reply: The descriptions have been added in the revised manuscript. (Fig. 2 and 3)

On page 5 "Single Product Speckle Filter" is mentioned. It is a part of the SW package but still it there should be a reference to a publication where the filter is described or a description of the filter.

Reply: We have added the detail of the treatment and the necessary citation to the reference. The added sentence is: "*To reduce speckle noise in the SAR images, we applied the Lee-Sigma filter using a sliding window of size 7×7*".

The reference is listed below:

Mansourpour M, Rajabi M A, Blais J A R. Effects and performance of speckle noise reduction filters on active radar and SAR images, Proc. Isprs. 2006, 36(1): W41.

Do not refer to supplement figures in the manuscript. Include the figures referred in the manuscript.

Reply: It has been revised in the manuscript.

The "statistical fitting" in Section 3.3.1 is not well described. Please, include a detailed description of the method in the manuscript. Have e.g. EM algorithm been used in the fitting?

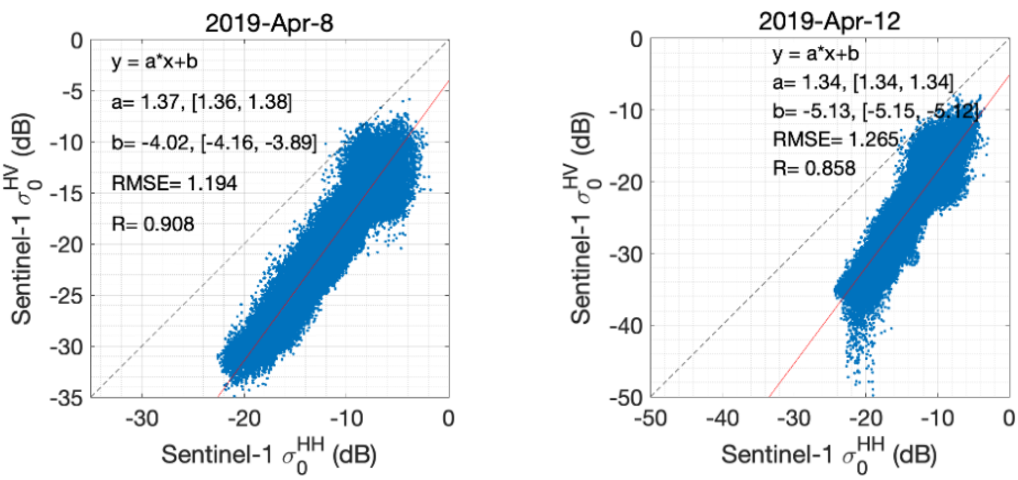
Reply: We have further described these details in the added "*Methodology*" section.

What do the Fs estimation would look for the HV data?

Would the estimates be better if HH and HV channels were combined in the Fs distribution estimation.

Reply: In our response to the next comment, we show the freeboard map predictions of the entire OIB track with either HH or the HV channel.

Below we show the linear regression between HH backscatter and HV backscatter. The results indicate the RMSE of approximately 1.2 dB and the correlation (Pearson's r) of around 0.9 (see figure below). This suggests that while there is a strong correlation between HH and HV backscatter, HH-channel backscatter alone cannot fully account for the variability observed in HV channel. This result also implies that using the HV channel may offer additional information that could improve the freeboard prediction. We have not fully explored the potential of using both channels for the prediction. This topic serves as a potential direction for future research.

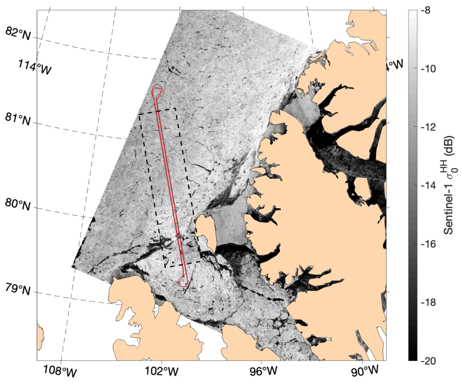


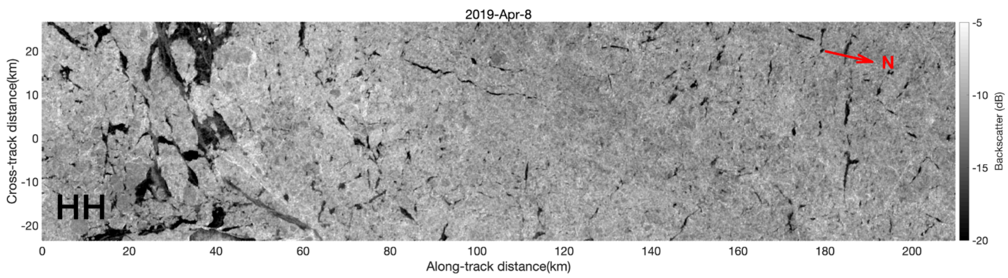
Would it also be possible to provide some examples of average ice thickness estimated for whole S-1 SAR scenes, e.g. in the current Section 4 and possibley provide a visual comparison to a available coarser scale operational Fb estimates, (e.g. based on

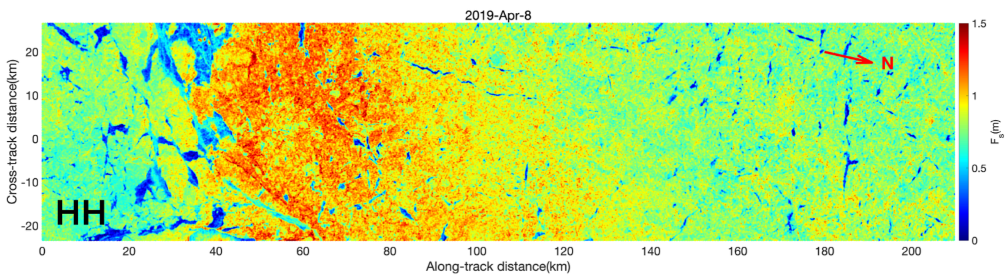
CryoSat-2, ICESat-2, SMOS)? The SAR image(s) could be acquired outside of the time window of the study.

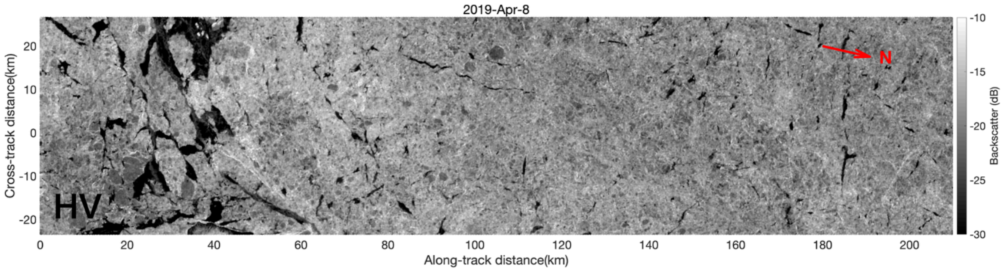
Reply: As suggested by the referee, we have applied the regression model between fs and backscatter to predict the freeboard around the whole OIB track. Specifically, the regression model of 27km-long segments at 200m-scale is adopted, with 9km overlapping by adjacent segments. The figures below show the region of freeboard map prediction (dashed black box), the Sigma0 map in detail, and the corresponding predicted freeboard map. We refrain from providing sea ice thickness maps, since we have limited knowledge of the snow depth over the sea ice cover. These freeboard maps are also added to the supplementary of the revised manuscript.

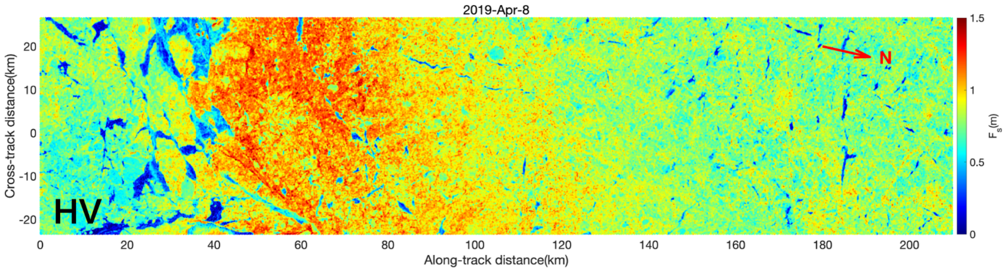
For OIB segment on April 8th:



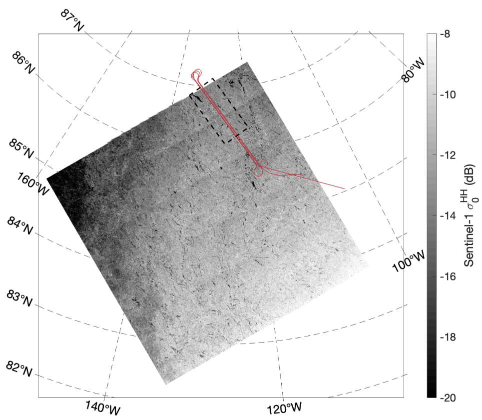


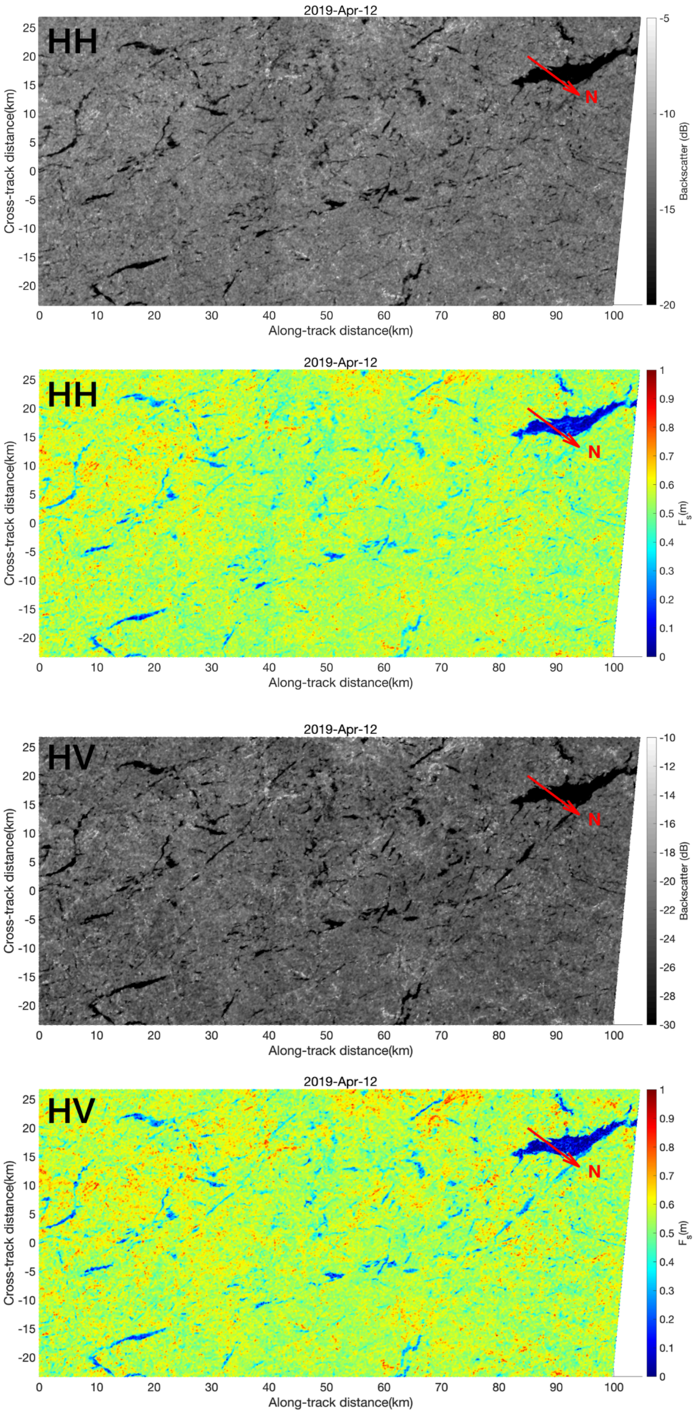






For OIB segment on April 12th:





It would be preferable to have the information in appendix A and B in the manuscript, e.g. the table B1 should be in Section 2.2. In the table the SAR image names could be replaced by the acquisition times to make the table narrower.

Reply: According to the suggestion (also mentioned previously), we have incorporated the content of Appendix B into the revised manuscript. Additionally, we have retained Appendix A as supplementary material. We have also further refined the format of Table B1 to enhance clarity and readability.