Reviewer 2 Response

Thank you for the comments and suggestions. We will follow up on the feedback and improve/clarify the manuscript accordingly.

1. *Using CDFs to link radar backscatter (especially HV polarization) to the distribution of sea ice freeboard has a certain degree of validity. However, questions remain about the method's underlying assumptions. CDF matching aligns distributions based solely on the observed data, without necessarily establishing a direct physical link between HV backscatter and freeboard. Consequently, when applying the extrapolation, high HV backscatter is mapped to high freeboard, while low HV backscatter is mapped to low freeboard. Given the complexity of sea ice conditions, such a straightforward relationship may be insufficient. Radar backscatter can be influenced by multiple factors, including salinity, surface roughness, and snow depth. Sea ice with identical freeboard heights might exhibit different backscatter signals, potentially leading to inaccuracies in freeboard estimation when relying on such a simplified mapping strategy.*

   We agree that the direct linkage of HV backscatter can only have a limited, and entirely statistical, relationship with freeboard and we have tried to be clear about the limitations in the manuscript - pointing this out multiple times. We believe that despite the limitations of this CDF mapping approach it is still an important baseline method for extrapolation. The advantage of this approach lies its simplicity, making it easy to understand the results. As there is further research currently being conducted into these topics, it is especially important to have a simple approach to compare future methods with, as they should be as least as accurate as the simple CDF-based extrapolation scheme presented here.
   We will add some of these thoughts and usefulness of the model for comparison with more advanced future methods into the discussion.

2. *The manuscript mentions 59 datasets. It appears that a separate CDF mapping is created for each dataset, rather than one universal mapping for all 59 datasets. If this is correct, it implies that the method cannot be directly applied to new Sentinel- 1 data for which no coincident ICESat-2 data exist. This limitation would significantly reduce the broader applicability of the technique. Clarifying whether a single CDF mapping was generated or multiple mappings were used—and if the latter, how the authors envision applying the method to future acquisitions—would be helpful.*

   You are correct that a mapping is constructed for each scene. However, it is not constructed from the coincident ICESat-2 data. Instead it is constructed from ICESat-2 from data within 24 hours of the SAR acquisition. As we are observing close to the poles, sufficient ATL-10 data to construct the map is usually available for scenes in the observed season (freeze-up) and this approach can be applied to most Sentinel-1 scenes.
   We will add some additional clarification on the paper to shore up this point.

3. *The study uses ICESat-2 data acquired within 24 hours of the corresponding Sentinel-1 data. However, some of the validation data were obtained under near-coincident conditions (e.g., time differences of less than 10 minutes). Given the rapid drift of sea ice, non-coincident ICESat-2 observations may not perfectly align with the*

Sentinel-1 pixels, thus introducing potential errors in correlations. It would be beneficial to explain why strictly near-coincident data (e.g., with a time difference of less than 10 minutes) were not used to establish the CDF mappings directly.

This goes along well with the comment/answer above. You are correct, that the validation data is all from near-coincident conditions. The non-coincidence of extrapolated data certainly introduces an error, that arises from the non-matching. However, accepting this error allows this method to be broadly applicable. Otherwise its use would be constrained to very rare near-coincident passes of both satellites. We will make sure to emphasise this in the manuscript in the next iteration.

4. *I guess that using both freeboard height from strong beams and weak beams is not a better way to calculate the mean freeboard height in a pixel. Generally, weak beam segments are typically about four times longer than strong beam segments, the resulting freeboard estimates from weak beams may be smoother. It would be more informative to analyze and present the correlations separately for strong and weak beams, thereby highlighting potential differences or biases in the derived freeboard estimates.*

   We actually tried using strong beams only, but saw (slight) improvement from including weak beam data as well. We currently expect there to be little difference between using weak and strong beams, as the uncertainties are probably smaller than those arising from the limited physical connection of freeboard and backscatter.
   We will investigate again with only weak and only strong beams and report the results in the next version of the manuscript.

Thank you also for the specific comments, we will implement these suggested changes.