Responses to Reviewer 1's comments (*in blue*):

This paper presents the first Antarctic-wide combination of ICESat-2 and CryoSat-2 data over sea ice to provide snow depth, freeboard, thickness and volume. Honestly, I was concerned that the first paper of this type to cover Antarctica would feel rushed and leave me with a lot of unanswered questions. But this manuscript was clearly structured and very thorough. I appreciate that the authors were transparent about the limitations of the method, but have still published what is an interesting, unique study. I'm happy to say that I learned a lot. I commend the authors' efforts and strongly recommend this paper for publication. However, I do have some comments that should be addressed first. The number of comments is due to the length of the paper, not a reflection on the quality.

We thank the reviewer for her time in reviewing the manuscript and providing helpful feedback. The suggestions have helped improve the revised manuscript.

——————————- *General comments* ——————————

The authors need to be very clear and consistent with which "freeboard" they are referring to, i.e. radar(CS2)/lidar(IS2)/ice/snow. "Total" freeboard should really be "IS2 freeboard" for consistency with the fact that they are using "CS2 freeboard" as separate to "ice freeboard". We're still not fully aware of the uncertainties associated with IS2 penetration and retrievals, so to frame it as undisputed total freeboard is misleading.

Agreed. All occurrences of 'total freeboard' in the text have been replaced by 'IS2 freeboard'.

I'd like to know why they did not use a whole year of data.

The period of study covers April to November of 2019. The summer and transition months were not included because of the known impact of warmer temperatures on snow wetness and thus radar (CS2) penetration into the snow layer.

The long sentences are confusing at times (e.g. P1L12-15). I appreciate this is a style preference but it was an issue for me. I suggest the authors re-read the paper and check for clarity throughout.

The sentences from P1L12-15 were rephrased to read: "The remarkable mechanical convergence in coastal Amundsen Sea, associated with onshore winds, was captured by ICESat-2 and CryoSat-2. We observe a corresponding correlated increase in both freeboards, snow depth and ice thickness. While the spatial patterns in the freeboard, snow depth, and thickness composites are as expected, the observed seasonality in these variables is rather weak. This most likely results from competing processes (snowfall, snow redistribution, snow-ice formation, ice deformation, basal growth/melt) that contribute to uncorrelated changes in the total and radar freeboards."

- Specific comments -

*P1P7: "...freeboards, snow depth, **ice thickness** and ice volume.*
Corrected.

*P1P7: "...April **1st**..."*
Corrected.

P1L8: The phrase "stands out" isn't very explanatory, How about "is the thickest" or similar
Revised to read: "The multiyear ice observed in the West Weddell sector is the thickest with a mean sector thickness > 2 m…"

P1L15: Don't need the word "broadly" (or "surprisingly" above). These types of phrases distract from the narrative.
We deleted the word "Broadly".

P1L15: This relates to my general comment above, about clarity regarding real and observed freeboard. The authors mention "biases in CryoSat-2 freeboards" but a more accurate statement would be ""biases in CryoSat-2 measurements of the ice freeboard".
We replaced "biases in CryoSat-2 freeboards" by "biases in CryoSat-2 estimates of ice freeboard".

P2L2: "several decades" -> "four decades"
Done.

P2L21: The statement that Kurtz and Markus (2012) "assumed that the snow depth is equal to the ice freeboard" is misleading. The Kurtz and Markus paper assumed that snow depth is equal to snow/lidar/total freeboard ("ice freeboard" should unambiguously be used to refer to the snow-ice interface). Better phrasing might be "assumed that the ice freeboard is zero, and so snow depth is equal to the total freeboard". They should really spell it out here because it's a key concept of the manuscript.
We replaced "assumed that the snow depth is equal to the ice freeboard" by "assumed that the snow depth is equal to the total freeboard "

*P2L23: "ice **and snow** cover"*
Corrected.

P2L23-25: How are the author's familiar with the pros and cons of each method – have they been validated? If so, please provide references.
The approaches and shortcomings are discussed in the cited references - P2L20-22.

P22L26-27: Does "these approaches" refer to all approaches, or just empirical?
By "these approaches" we are referring to all of the approaches. To improve clarity, we changed to: "all these approaches".

P3L11-12: Why do the authors use the 10 km product and not the 150-photon aggregate product? It would be useful here to explain that higher spatial resolution IS2 products are available, and why they chose this one.
The freeboards that we use (from ATL10) have identical resolutions as those of the derived heights. Individual freeboards (i.e., 150-photon aggregate freeboards) are derived only if there was a local sea surface reference available within a 10-km along-track segment. The 10-km does not refer to the freeboard resolution, only the constraint placed on proximity to the sea surface reference.

*P3L23: "…freeboard estimates **in the Arctic**."*
We added "in the Arctic" at the end of the sentence P3L23.

Section 2.2: Are they using individual waveform thicknesses, and how is the concentration weighting done?
The weighting was done by multiplying the gridded CS2-freeboard (25-km averages) by the corresponding ice concentration at a given grid cell.

*P3L26: "…thickness measurements **in the Arctic**…"*
Added.

P4L10-13: This is a really nice summary!
Thank you.

P5L28-29: From this I understand that they're using the whole month for CS2 but only 2 weeks for IS2? This wasn't clear in the manuscript until now, or in the abstract. I'd suggest repeating the analysis for just 2 weeks of CS2 so it's a like-for-like comparison even though I know this isn't ideal for coverage. If they really feel strongly that they shouldn't, then the averaging windows and reasoning needs to be very clear in Section 2 and the abstract.
The November CS-2 composite and distribution (in Figures 3 and 4) have been updated using only two weeks of CS-2 data – to be consistent with the availability of IS2.

P5L33: Would benefit from a more up-to-date reference than Lange & Eicken, 1991
Added Vernet et al. (2019): Vernet, M., Geibert, W., Hoppema, M., Brown, P. J., Haas, C., Hellmer, H. H., . . . Verdy, A. (2019). The Weddell Gyre, Southern Ocean: Present Knowledge and Future Challenges. Reviews of Geophysics, 57(3), 623-708. doi:10.1029/2018rg000604

P7L15-19: Although it's inferred, perhaps really spell it out here that wave propagation is more of an issue in the Pacific and Indian Ocean Sectors because of the small spread in extent. I appreciate that the authors are consistently transparent about the complexities of the signal.
We agree and added the following text: 'This effect is predominant in the Pacific and Indian Ocean sectors because of smaller sea ice extent'.

Section 4.1: What's the reference for a bulk density of 0.32 g/cm3 and uncertainty of

_0.07 g/cm3 for Antarctic sea ice?

There is no generally accepted average value of the bulk density of snow over Antarctic sea ice. Massom et al. (2001) suggest 200-300 $kg.m^{-3}$ under cold/dry condition and higher density (350-500 $kg.m^{-3}$) for warm windy conditions – not unlike the Arctic. We elected to use the average winter bulk density of 320 $kg.m^{-3}$ (like that of the Arctic) but increased the spread to 70 $kg.m^{-3}$ to cover the range of average conditions. We have added this discussion to the text.

Section 4.1.2: I struggled with this description of the method. The way I understand it, they create daily snow depths only in grid cells where data are available. Therefore, the monthly composites should be weighted by the number of measurements in each grid cell. Please describe if/how this weighting was done. How do they account for anomalous cells, or cells that are only present for a few days and may bias averages (especially as they're allowing such large temporal separation)? This is a critical section, and the method should be made clearer.

For clarification purposes, we substituted the text P9L11-12 by: '…First, the daily along-track IS2 and CS2 freeboards are binned on a 25-km resolution grid. Gridded IS-2 freeboards are averages of the three strong IS-2 beams and thus provide a better sampling of the spatial mean (compared to CS-2 freeboards). In grid cells with available data, an average is computed while the other bins are assigned with a missing value. Freeboard differences are then computed at each valid IS2 grid cell using CS2 binned freeboards (weighted by ice concentration) with time separations |DT|<10 days and within a 75-km box. We find that this sampling strategy provides the best spatial coverage without sacrificing precision'.

P9L10-11: Are the IS2 thickness estimates also concentration weighted?

The IS2 estimates are not weighted by passive microwave ice concentration because the open water samples (i.e., H=0) are included in the population for computing thickness.

P9L32: "may be" or "is"? There's an important distinction!

We used "may be" because the results of the sensitivity analysis may not have sampled the range of conditions expected.

Section 4.3: I appreciate that they've included this section, but I think it's unnecessarily complicated. The same point could be made by just this final sentence on Page 10. That sentence does need rewording, for clarity, and I suggest something like "The negative intercepts observed in the scatterplots imply that h_f is an underestimate of true snow depth by +2.4 to +3.9 cm."

We think this section is useful, as it constitutes the basis of the discussion about the possible sources of biases in the snow depth estimates. The final sentence on page 10 has been rephrased as suggested.

P11L4: Reference for the -5C value? I'd really like to read this work.

Figure 5 of Winebrenner et al. (1994) - Instead of T=0C, the appearance of moisture in the snow layer from around -5 C would cause changes in bulk dielectric constant affecting penetration and backscatter at radar frequencies. This can be seen in the

fluctuations in radar backscatter even before the air temperature reaches the melting temperature.
Winebrenner, D. P., Nelson, E. D., Colony, R., & West, R. D. (1994). Observation of Melt Onset on Multiyear Arctic Sea-Ice Using the Ers-1 Synthetic-Aperture-Radar. *Journal of Geophysical Research-Oceans, 99*(C11), 22425-22441. doi:Doi 10.1029/94jc01268

P12L33: A more accurate statement would be that "the ASPeCt data are biased towards thin and level ice types"
Revised as suggested.

——————————————- *Technical comments* ——————————————

P4L7: "export" -> "exports"
P5L26" Remove "generally"
P5 L28: Delete "due"
P6L9: "on" -> "in"
*P6L13: "The tails **of** freeboard…"*
*P7L19: "…**but** some…"*
P11L31: Delete "viz"
P12L27: "data set" -> "data"
P13L37: "sector" -> "section"

The above have been corrected as suggested.