Reviewer 2

This study presents a first estimation of snow depth from near-coincident spaceborne dual-frequency (laser and radar) altimetry over lead-less landfast ice in the Canadian Arctic Archipelago. The authors utilize one CRYO2ICE track (separated by 77 minutes and approximately 1.5 km), and estimates snow depth from ellipsoidal elevations after applying a tidal correction to the elevations. This tidal correction allows for derivation of snow depth from the difference in elevations assuming laser (ICESat-2/IS2) is reflected at the top of the snowpack/air-snow interface, and radar (CryoSat-2/CS2) is reflected at the snow-ice interface over lead-less sea ice. However due to the non-presence of leads, the impact of tides is accounted for through comparing the changes to water level caused by tides using the models from the satellite data with tide gauge estimates. The derived CRYO2ICE snow depth estimates (with negative snow depths removed) are compared with a dedicated ground-based campaign along the orbit.

An interesting paper that provides some interesting results to the dual-freq. snow depth approach – and, with the limited reference data available to compare with, especially along CRYO2ICE orbits, this paper can provide some interesting insights along this one orbit. The inclusion of much needed in situ reference data along such an orbit feed into some good discussion topics and the paper warrants publication. The paper read easily, although I have a few comments regarding the figures and tables, which I hope the authors will consider. Furthermore, I still have a few major points to be addressed before publication, as I believe there are more work required to ensure that CS2 and IS2 are comparable at the resolution used in the study, that the tide correction is applicable beyond just this example (and if not, that this is further discussed), and whether some of the assumptions of this study holds up.

Major comments

Tide correction and discussion relating this to different ice regimes/areas

I've yet to encounter this methodology before, but I am intrigued by it, and wonder how this may be applicable on a larger scale. However, I am not fully convinced by the correction applied as of now, was somewhat confused by it, and hope the authors could provide some more information regarding the ocean tides used in the study. You state that there is an average difference along track of 7.9 cm (**on the co-registered points or? – and if so, how are the IS2 ocean tide of co-registered points even computed?)** but the difference in water level from CHS was 6 cm. This is only compared to one point (tide gauge) vs. full along-track data. Could you provide a figure (maybe just in response to reviews, but potentially also in the manuscript) of the difference in ocean tide models along-track (maybe not using the average value, but maybe maximum and minimum too/or show distributions). This would also feed into your statement about the 1.9 cm correction (when compared with CHS water level) representing the systematic bias – this seems somewhat low, when we see that the ocean models may differ by +/- 12 cm (ranges +/- 50 cm vs +/- 62 cm) along the track. I think the study would greatly benefit from some more results and discussions on this.

The authors thank the reviewer for finding the methodology interesting and agree that some further details and analysis need to be presented in order to justify the methodology for accounting for the difference in ocean tides between the IS2 and CS2 passes. The basic assumption of this methodology is that the difference in the average ocean tide correction applied in IS2 and CS2 respectively account for the difference in water level recording from the tidal gauge dataset in the same period.

We plan to replace Figure C1 with the following figure comparing the along-track IS2 and Cs2 ocean tide:

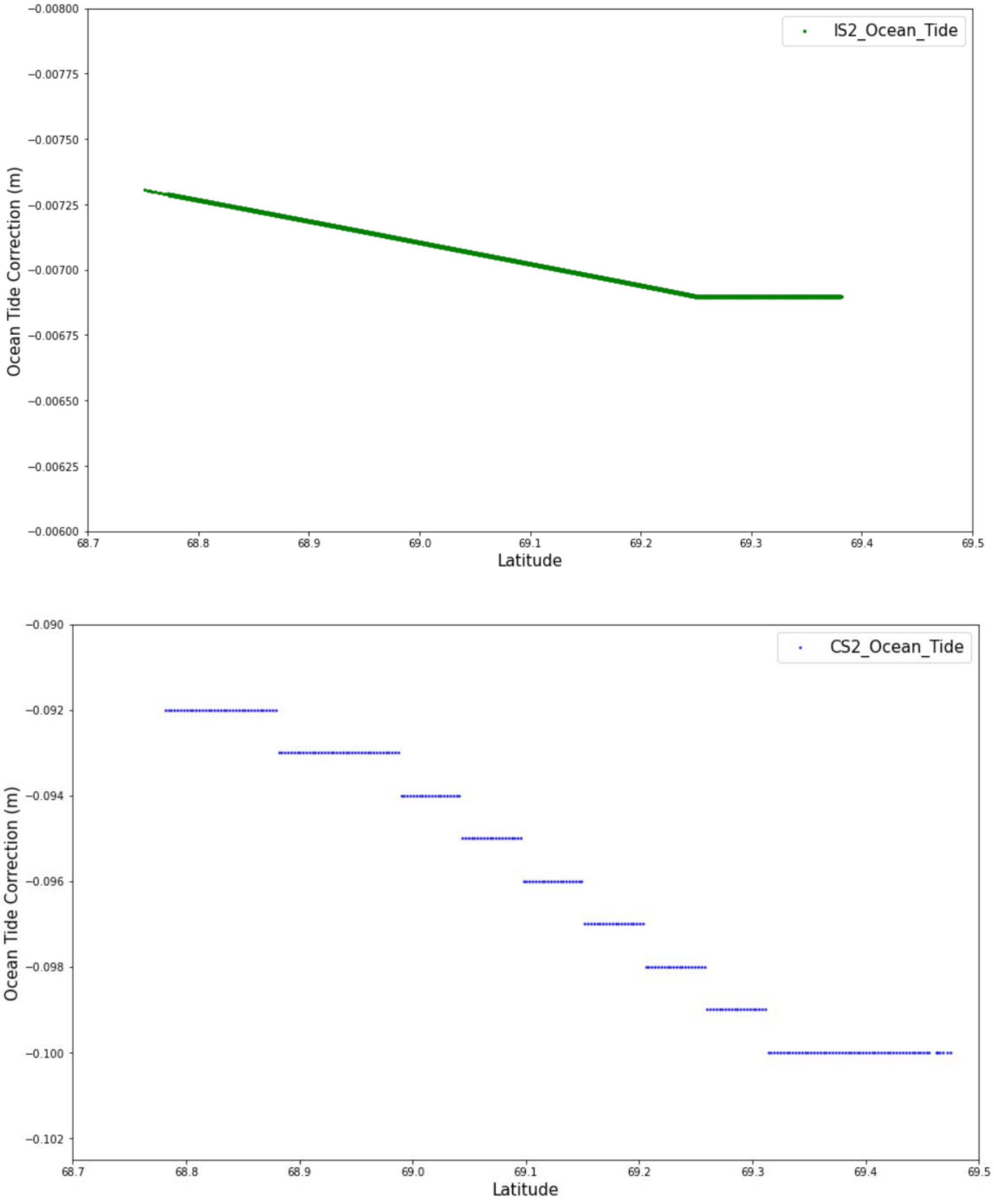


Figure R1 Along-track Ocean Tide Correction applied in the(a) IS2 and (b) CS2 products

The tidal corrections for collocated IS2 and CS2 are obtained from the tidal corrections provided for each product. For ATL07, a tidal correction value is provided for each segment which is then averaged over 300 meter. Similarly, the ocean tide correction applied in the CS2 Level-2 product is also provided in the 1-Hz Cryosat-2 Level 2 product which we converted to the 20 Hz level in order make this comparable to the 20Hz height product. Therefore, after collocating the two tracks based on latitude, we compare the 300 meter averaged tide correction IS2 with the collocated CS2 ocean tide corrections.

The maximum along-track IS2 ocean correction applied was 0.7250 cm while the minimum 0.700 cm whereas the CS2 ocean correction applied varied between 9.20 and 10.00 cm. Therefore, since the relative difference between the ocean tidal corrections used in both IS2 and CS2 is not varying significantly (less than 1 cm) and we currently don't have tide data that may account for the less than 1 cm difference along-track, considering the average along-track relative tidal height difference between the 77 minutes of the passes seems reasonable in the context of landfast sea ice. Please note that the current technique relies on the assumption that landfast sea ice undergoes insignificant drifting and the difference in vertical height is accounted for in the relative difference between the tidal corrections in IS2 and CS2. Therefore, we assume that the relative change in water level will vary insignificantly along ~75 km track for the 77 minutes between the passes. This method may not be applicable where the along-track variation in tides is not significantly different. We consider the average difference in the difference in tidal correction (IS2-CS2) and compare it to the difference in water level from the tidal gauge station. Please note that we use compute the average along-track tidal height difference between the IS2 and CS2 passes. the relative along-track height difference between collocated IS2 and CS2 has an average value of 7.9 cm which is then compared to the 6.0 cm tidal gage difference. Please note that this method doesn't rely on the absolute tidal corrections applied on each sensor but rather on the relative difference between the applied corrections between IS2 and CS2 and whether that reflects the 'real' change in water level.

In addition, I appreciate that the study investigates only the snow cover on landfast ice along the one track where you acquired ground observations. However, this methodology of applying a tide-correction is interesting for coastal and landfast sea ice beyond just the Canadian Archipelago. The manuscript would benefit from providing a greater perspective on applying this on a larger scale (across the Arctic, discussing whether ellipsoidal height differencing is better/equal to freeboard differencing etc.). If possible, would be interesting to see if it was possible in e.g., the Laptev Sea or similar – and also, in areas with no tide-gauges to compare with. Or at least a discussion about this would be very interesting.

The authors appreciate that the reviewer's find this an interesting method to test in other geographical conditions with varying conditions. The authors agree that this method warrants further investigation perhaps in different parts of the Arctic such as the Laptav Sea. However, the authors think that a comprehensive validation campaign in such conditions similar to the one presented in the paper is needed for making concrete statements. Therefore, such campaigns may be proposed as part of future studies.

However, the authors agree that discussion points about the applicability of this tide correction to other regions needs to be included. The discussions now include the possibility of testing this method of using ellipsoidal height in near-shore sea ice environments.

Similarly, I believe that this work on landfast ice is important, but would have expected a discussion relating it to the bigger picture and sea ice in general (landfast and drifting) – **could you provide some insights/relate it to e.g., deriving this in drifting sea ice in the Arctic/Antarctic?** I acknowledge the very different sea ice/atmospheric patterns here, but I believe that this warrants some more discussion or at least, additional acknowledgements of the limitations/difficulties/differences when deriving this over landfast ice vs. drifting sea ice, beyond just the limitations regarding leads only.

The author’s appreciate the reviewer’s suggestion to include a discussion about the bigger picture i.e. landfast vs drifting sea ice in the Arctic/Antarctic. However, one of the critical assumptions made in this paper was that landfast ice in the narrow channels of the Canadian Arctic Archipelago during a significant portion of the winter months don’t have significant drifting and therefore the small temporal difference between the IS2 and CS2 may be assumed to be equivalent to the variation in ocean tide. However, in case of the drifting sea ice in the central Arctic, we believe various other factors including ocean currents and other dynamic sea ice processes will cause the sea surface height to change significantly even between the IS2 and CS2 passes. Therefore, we will need to have direct altimeter derived estimates of the sea surface height to be able to compute freeboards in case of the drifting sea ice in the Central Arctic.

Spatial scales, C2I snow depths and smoothing IS2 elevations

I believe there is more work to be done to ensure that differences in footprint and resolutions have been thoroughly considered. The fact that we are observing at much different scales (300 m x 1600 m vs. ATL07’s varying resolution), simply smoothing IS2 to 300 m does not seem sufficient. Also considering the average distance of IS2 and CS2 being 1.5 km, I believe there needs to be done more work on the IS2 data to ensure that they have processed to be “comparable”. **I suggest having a look at the data from the other strong beams, to investigate the variability along the IS2 tracks and from here, you may actually also see if they are seeing “similar surfaces/distributions” which you’ve otherwise support using the Sentinel-1 data. In addition, providing some statistics on segment lengths from IS2 would be great to fully understand the coverage of IS2 when smoothing and co-registering to CS2**.

The authors thank the reviewer for their valuable suggestions. We compared the IS2 height distributions which are presented in the Figure R2. It is noteworthy that IS2 2l strong bema’s height distribution is most similar to CS2 height distributions. This is because the distance of the strong beam is within ~1.5 kilometer of the CS2 Points of Closest Approach (POCA). Therefore, we believe using only the IS2 2l strong beam which is the closest to the POCA CS2 would make the best colocation of the IS2 and CS2 tracks. This is based on the assumption that the snow distribution corresponding to the CS2 POCA and the closest IS2 strong beam would be most similar. This assumption is further tested by comparing the Sentinel-1 Backscatter retrieved from all the IS2 strong beams and CS2 (Figure R3). We notice that while the mean ellipsoidal heights are similar, there is significant differences among the distributions of both IS2 1l and IS2 3l strong beams. Therefore, we don’t consider the IS2 1l and IS2 3l strong beams for the subsequent colocation and snow depth retrieval steps.

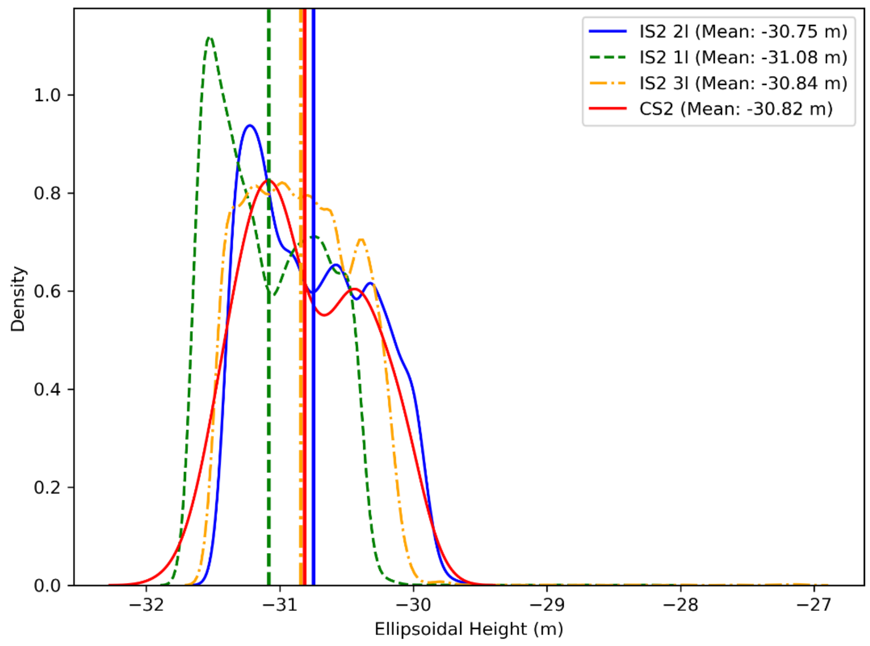


Figure R2: IS2 strong beam height distributions

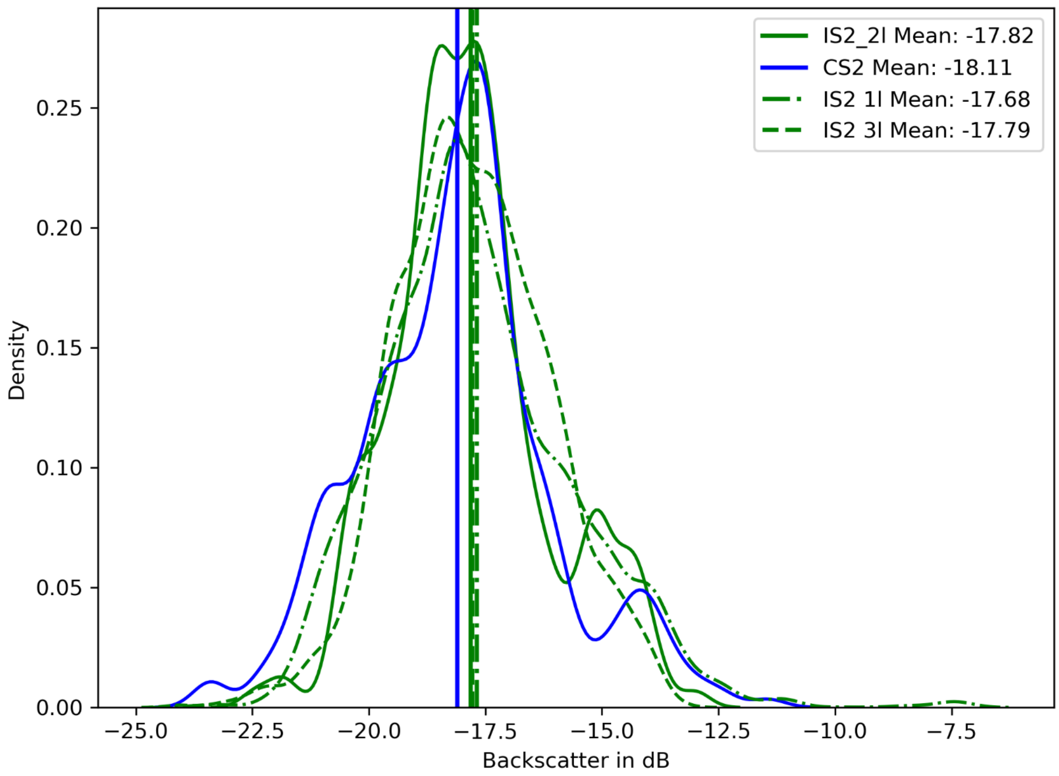
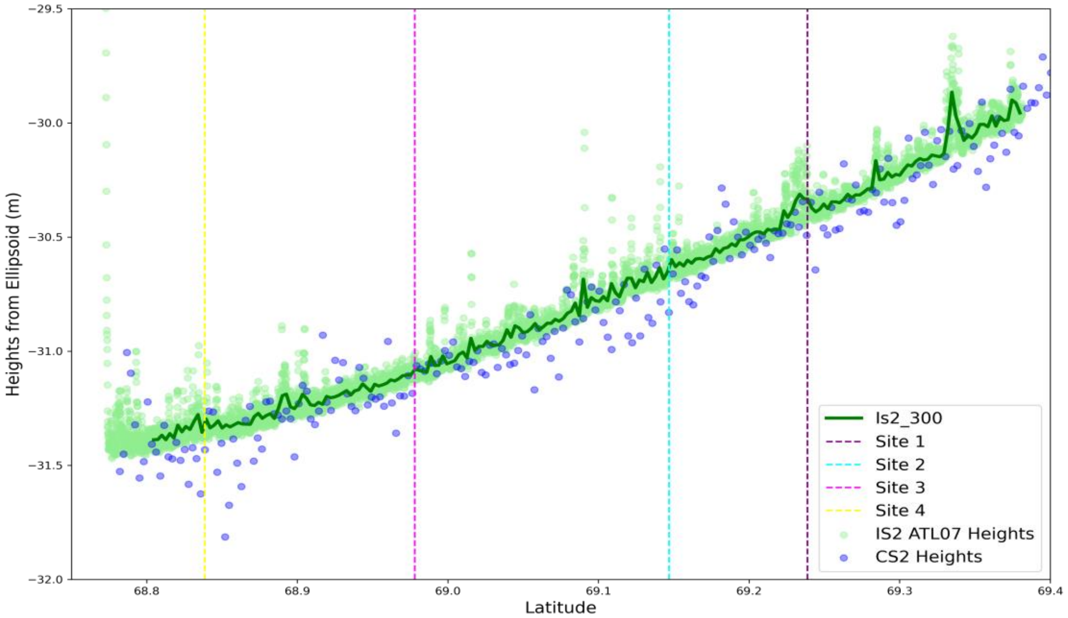


Figure R3 Comparing the backscatter retrieved from all IS2 strong beams with CS2

The IS2 300 meter length segments have been added to Figure 7:



In addition, **I would have liked to see the semi-variogram (or just variogram)** that you have created showing this spatial autocorrelation of 1 km of the in-situ snow depth distribution. That somewhat counters the assumption that IS2 and CS2 are seeing similar snow – or at least similar snow variability (as they are separated by 1.5 km). The assumption that smoothing IS2 to CS2's along-track resolution of 300 m is sufficient is not well supported, **as studies on along-track radar altimetry data usually apply some level of smoothing to the along-track radar data due to the noise (which is evident in your figure too).** The CS2 data is – to a large extent – impacted by noise, off-nadir reflections (of ridges, leads and other) etc., which you see in Figure 7. This might also explain your "lack" of correlation with IS2 roughness. Ideally, we would expect less difference in the CS2 ellipsoidal heights along the orbit than in the laser if there is full penetration to the snow-ice interface, but that is not the case. A value of R2=0.04 basically states that there is no correlation. However, I also do not think – based on the ellipsoidal heights already shown – that we would expect this due to the noisy behavior of CS2. Another comparison you could use to identify what is primarily contributing to the variability in your snow depth estimates, is **computing the correlation between IS2 ellipsoidal heights/C2I snow depth and CS2 ellipsoidal heights/C2I snow depth (or another measure of the CS2/IS2 elevations that are of smaller magnitudes than ellipsoidal heights along your track)**.

I do believe there is room for more speculation/discussion related to the C2I snow depths and how they compare with the in-situ data – just comparing mean/median seems … insufficient or at least, as if there might be more to discover, especially since the distributions are different even if the average values are similar. **But I'll leave that up to you to see if you think there is more to extract from these plots and/or analysis**.

The authors thank the reviewer for their valuable comments and suggestions. The authors realise that only the along-track variability in snow depth is being represented in the variogram given that the snow depths are colocated to the CS2 POCA points. However, the variogram gives us a sense of the spatial autocorrelation along-track which helps in understanding the overall variability. Therefore, we observe that the vairogram in Figure R4 starts to reach a inflection point and reaches a sill ~ 1km. We also notice that beyond the 300 meter lag distance, the semi-variance starts to become constant. Due to the high variability in the data, the variogram analysis doesn't provide a concrete lag distance but does give an indication that beyond the 1 km point, the spatial autocorrelation would be negligible.

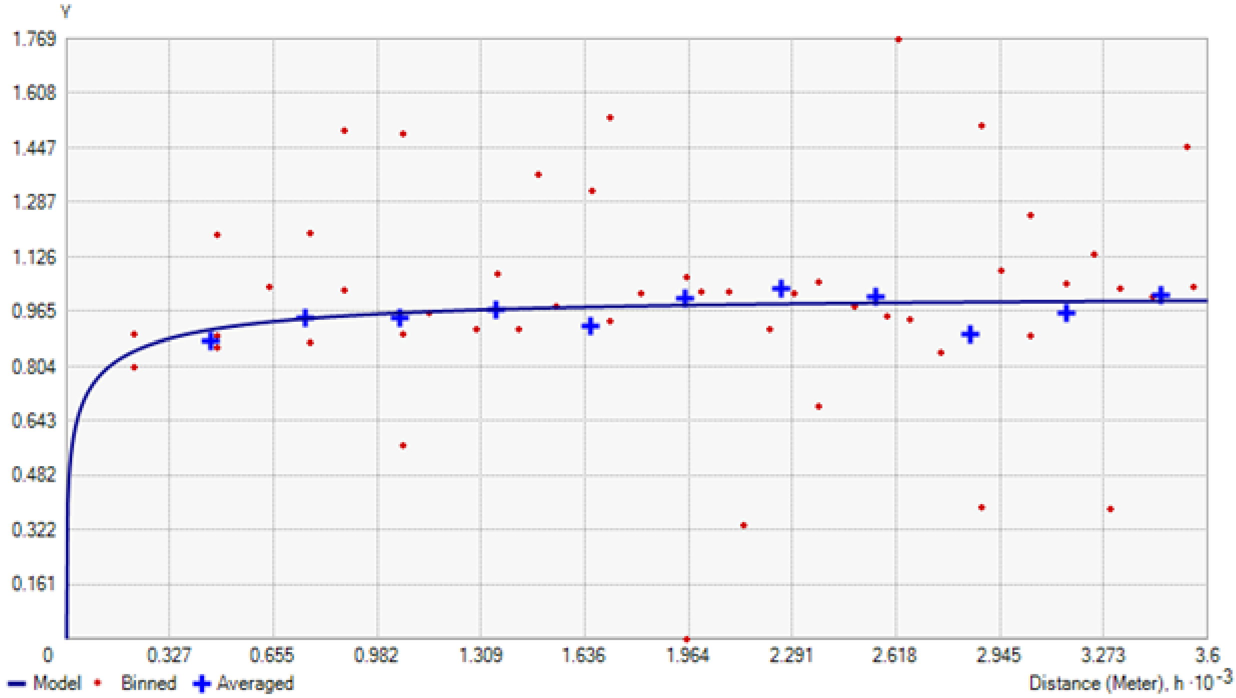


Figure R4 Semi-variogram analysis of the Cryo2Ice snow depths. Y axis represents the semi-variance while the x-axis shows the lag-distance.

In order to test the impact of smoothening the CS2 footprints to reduce noise, a 1-km filter was applied to both IS2 and Cs2 native heights (Figure R5). We notice that averaging over 1-km causes IS2 to be less sensitive to the roughness features which is very well represented in the native resolution. We also notice that some of the noise if still present after applying the smoothening. Therefore, the approach that we take is to consider the finest possible resolution i.e. 300 meter corresponding to the CS2 along-track footprint size and then to include the impact of noise which is reflected as an uncertainty in the revised analysis. This allows us to understand the impact of major differences in roughness which characterize each in-situ site surveyed. Therefore, adopting the finest possible resolution allows us to understand the impact of roughness features on the final snow depth outputs while also accounting for the uncertainty due to noise.

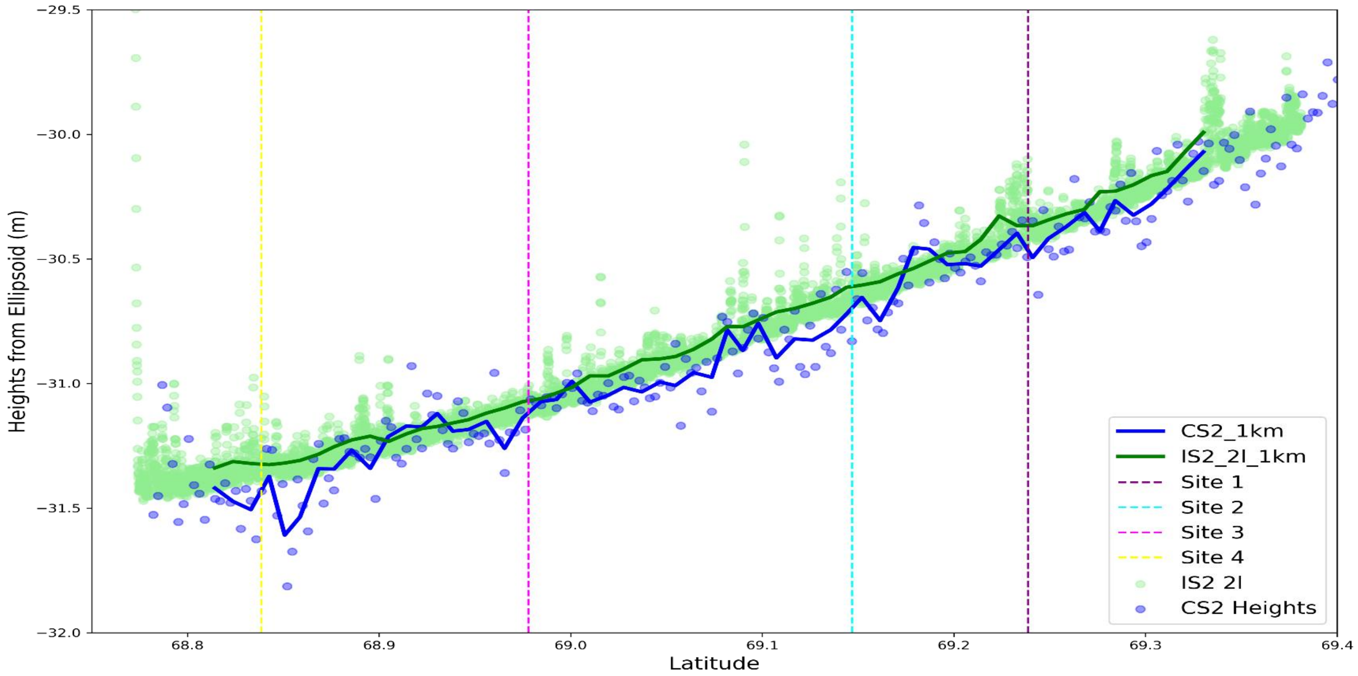


Figure R5 IS2 and CS2 Height distributions. IS2 2l height in native distribution is shown in green while CS2 height is shown in purple. The blue line shows the smoothed IS2 and CS2 lines over 1-km.

The authors thank the reviewer for the suggestion for computing the correlation between the snow depths retrieved and individual ellipsoidal heights. We see that the Adjusted C2I snow depths only have weak correlation to IS2 and CS2 ellipsoidal heights. Therefore, the impact of CS2 noise doesn't necessarily cause a major change in the snow depth retrievals and may be attributed to other physical factors.

	Correlation Coefficient
IS2 Ellipsoidal Height vs Cryo2Ice Adjusted Snow Depths	*-0.1213*
CS2 Ellipsoidal Height vs Cryo2Ice Adjusted Snow Depths	*0.2509*

Based on the reviewer's feedback, the authors have decided to include the following statistical analysis to compare not only the mean and median values but also other parameters:

We include (a) comparison of the shapes of snow depth distributions including comparing the skeness and and kurtosis of the site wise in-situ vs Cryo2ice snow depth values and (b) comparison of the site-wise standard deviations of in-situ vs Cryo2Ice (c) correlating IS2 and CS2 ellipsoidal heights with the retrieved snow depths.

Minor comments

Figures:

- Tables in figures should be removed (following TC policy) – either make the tables into individual tables, or simply include the information in the legend/on the figure (since it is primarily average values and such).

  The authors thank the reviewer for pointing this out. The figures and tables will be adjusted to be compliant with TC guidelines.

- In addition, have a look at the resolution of the figures (e.g., Figure 11) and consider improving the DPI for better visualization.

  The resolution of the new figures will be adjusted.

- Consider using the same color for the line representing locations of different sites across figures – it will make comparison between figures (e.g., Figure 7, 8 and 11) easier. Also, double-check that your figures work for readers with color deficiencies.

  Noted. The color consistency will be considered while revising the figures.

Tables:

- In general, there are quite a few tables that provide limited insights which could just as easily have been mentioned in the figures and would likely provide some more connection having it next to e.g., the distribution figures. I encourage you to reconsider the number of tables you have, and whether the content of the tables could be put in the figures instead.

  The authors thank the reviewers for the suggestion to include some of the numbers along with the distribution graphs.

Data policy/statement:

“Available upon request” does not follow TC guidelines. Please provide a DOI for where to obtain the data – either as raw data, or as the processed data that you are presented in the manuscript. In addition, I am not able to open the link for the CryoSat-2 data – in addition, I have not encountered this website before (you did not use the science server or FTP site?). Please have a look at this link again to ensure that the link is active.

The authors thank the reviewer for pointing out the inconsistency with the TC data distribution policy. The raw data will be made available and shared in the revised manuscript. The link for the Cryosat-2 data seems to be active, however, the reviewer may try to access the data using the following link: https://eocat.esa.int/sec/#data-services-area. The EO-CAT server was used to download the Cryosat-2 data.

Specific comments

Line 71-73. Do you include low-confidence data (that is, ATL03 flag of low confidence or another ATL07 flag) across the entire track, or only close to the coast – or what is meant by low confidence? Could you provide a measure of the amount of low-confidence data or was it flagged somehow? And where this low confidence is along the track?

ATL07 corrected heights are not provided within 25 km distance from the coast primarily due to the low-confidence in the tidal models close to the coast. However, The ATL07 Version 6 product includes an uncorrected product that includes the low-confidence heights within the 25 km buffer from the coast as well. The portion of the track with low-confidence data is demonstrated in the Figure 1 below:

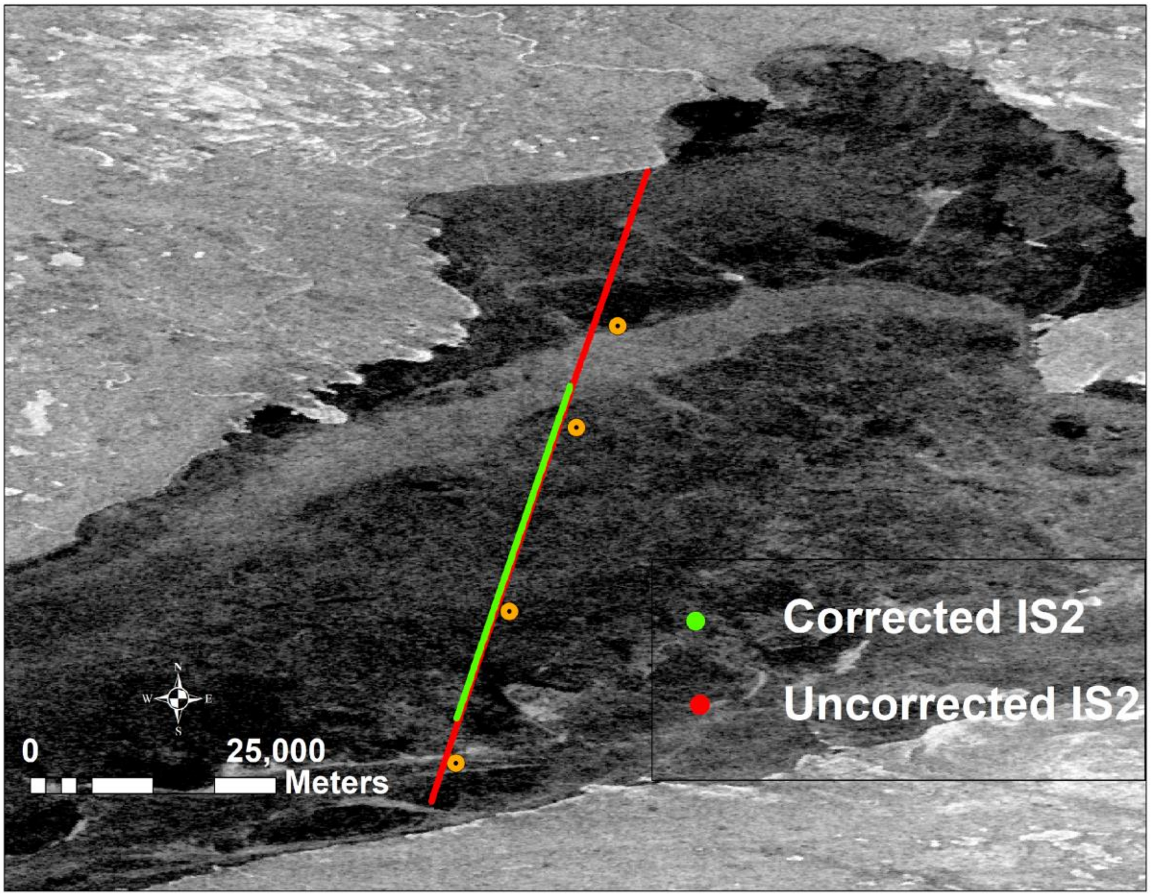


Figure R6 Spatial Distribution of the Corrected and Uncorrected IS2 data used.

Please note that the uncorrected IS2 data shown in Figure refers to the raw ATL07 uncorrected data which is available in ATL07 Version 6. Therefore, approximately 37 Kilometers of the track close to the land fall under the low-confidence of tidal model zones close to the coast. Therefore, due to this uncertainty, it is important to correct the tidal corrections and ensure that the relative difference between the tidal corrections applied in IS2 and CS2 is similar to the change in water level from tidal gauge stations.

Line 74. Could you provide the distance to the other strong beams too?

The distance between the CS2 Point of Closest Approach (POCA) and the 1l strong beam is ~2200 metre, distance to the 3l strong beam is ~ 4500 metre whereas the distance to the 2l strong beam is ~1500 metre.

The following line has been added after Line 74:

"The strong beam 2l was ~1500 metre from the CS2 point of closest approach whereas the beam 1l and 3l were ~2200 metre and ~4500 metre away."

Line 88. Provide which threshold is used by the re-tracker. Also, I thought it was known as the "CPOM" re-tracker? Maybe I'm wrong.

The Cryosat-2 Baseline E Product Book mentions UCL sea ice retracker but in other studies such as Nab et al., (2023) the CPOM retracker is used. We assume both has been used interchangeably. The UCL sea ice/CPOM retracker determines the retracking point by applying a fixed percentrage threshold of 70% to the waveform's first maximum power return. The authors have included the threshold in Line 88.

Line 105-110. Include abbreviations (Site 1 – S1; Site 1 Ridged; S1R etc.), as they are used later on in figures and text, but not typed out in the text.

Noted and will be fixed in the revised manuscript.

Line 135. Is Kwok et al. (2020) deriving it from total freeboard and sea ice freeboard? I believe it is the radar freeboard of CryoSat-2.

Kwok et al., (2020) computed the ice freeboard from Cryosat-2 radar freeboard by accounting for the change in Ku-band velocity through the snow on sea ice using.

The line 135 has been revised and now reads: 'Kwok et al (2020) calculates snow depth (SD) as the difference between IS2-derived total freeboard (snow + ice) and CS2-derived radar freeboard (CS2), using the difference between the surface height and the instantaneous sea surface height interpolated from sea surface measurements from along-track leads to obtain the freeboards (Kwok et al., 2020; Ricker et al., 2014). The CS2 radar freeboard is also adjusted for reduced Ku-band speed to derive an accurate estimate of the sea ice freeboard.'

Line 140. Ensure consistency with naming of h(IS2)/hIS2 and h(CS2)/hCS2 throughout text.

Noted and will be fixed in the revised manuscript.

Line 157. MSS is not mentioned in section 2.6. Please do.

The line now reads: "However, the mean sea surface (MSS) ensuring that the IS2 ATL07 heights are referenced to the WGS84 ellipsoid. The MSS is calculated based on decadal averages and therefore are not representative of the variation of sea surface heights within the 77 minutes' interval between the IS2 and CS2 passes".

Line 167. Do you have a reference for the Moran's I test?

The following reference has been added to Line 167: Moran, P.A.P.: The interpretation of statistical maps, Journal of the Royal Statistical Society, 10, 243-251,https://www-jstor-org.uml.idm.oclc.org/stable/2983777.

Figure 3. You mention co-registration of heights. What is meant here – how are you specifically co-registering the observations? Also, ensure consistency in the naming of h(CS2)/hi(CS2).

The authors thank the author for pointing out that the details about the co-registration had not been mentioned in the text.

Line 171 now reads: 'After the spatial averaging of the ATL07 heights over 300 metre, the co-registration is conducted based on the distance to the closest CS2 Point of Closest approach. Therefore, each CS2 point is co-registered to the closest 300 metre ATL07 height segment.'

Figure 4. I think that it is an interesting discussion and I like the idea of identifying comparable surfaces using Sentinel-1 backscatter. But I don't think the full picture of what is shown in the figure is being discussed in the text. You state that mean values are similar, therefore the assumption is that the same surfaces are observed – even when the distributions differ with a bi-modal distribution of IS2 and higher amount of high backscatter (between -16 and -14dB) observed than for CS2. Instead of doing a distribution-to-distribution comparison, could you instead do it **per smoothed, co-registered point and look at residuals of backscatter between the points**? To see how much they vary and at which locations they do differ. In addition, the standard deviation lines do not make sense to me – why are they not separated by the same distance around the mean values?

The authors thank the reviewer for their valuable input and suggestion. As suggested, we computed the co-registered point wise difference between the retrieved IS2 and CS2 backscatter and tried to map the difference in backscatter (Figure R7). We see that the average difference in basckscatter between the colocated points are within -+1 dB. The average difference in backscatter is 0.9 dB. Therefore, based on this analysis, we can assume that IS2 and CS2 may be seeing snow based on the Sentinel-1 backscatter and therefore may be colocated. The authors will include the findings of this analysis in the paper and also include the Figure R7 in the appendix.

The standard division of both the IS2 and CS2 varies by the same value about the mean backscatter. However, the visualization of the figure might have made this a bit harder to interpret. We will revise the figure to make the lines clearer.

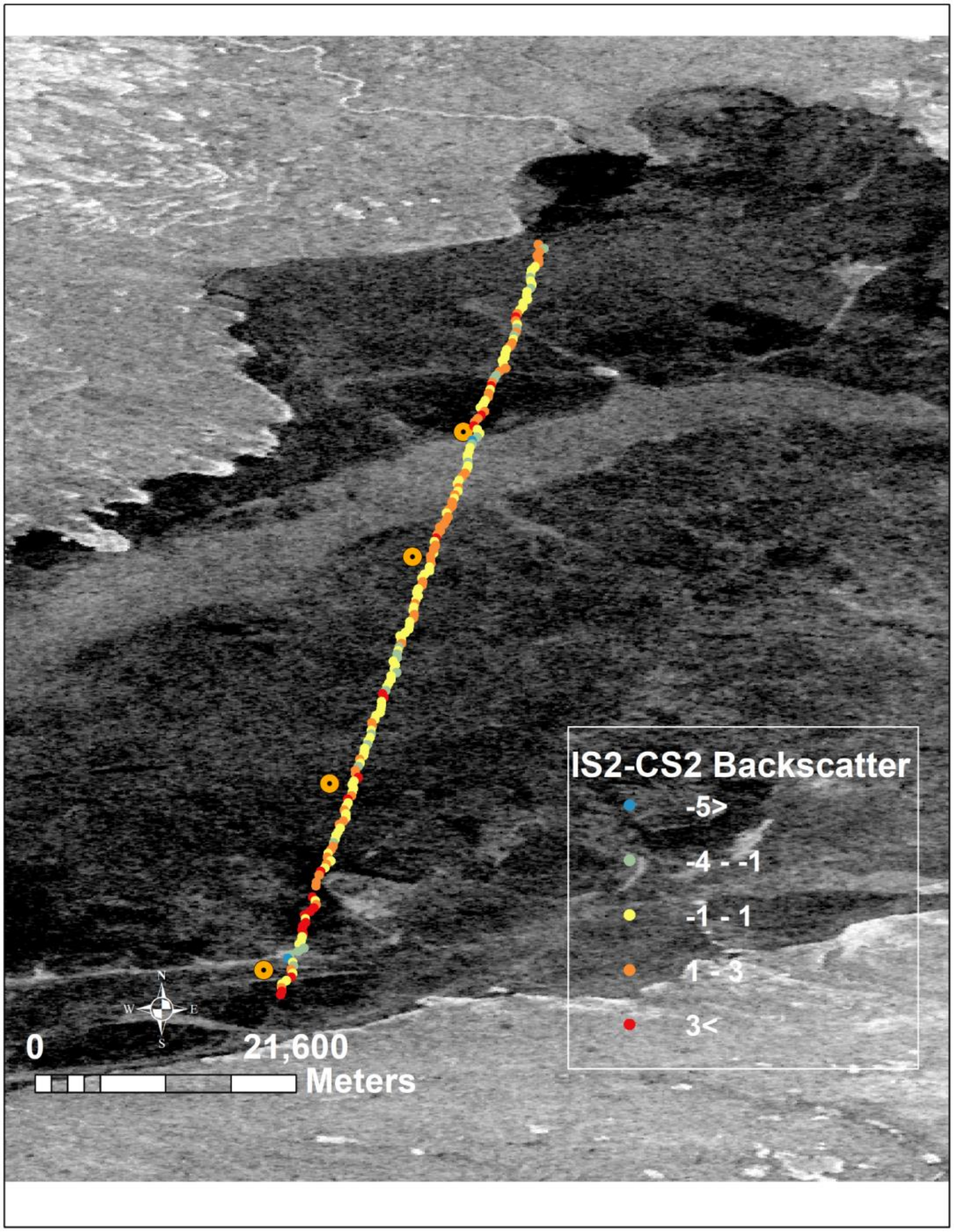


Figure R7 Distribution of the difference in IS2 and CS2 Backscatter retrieved from IS2

Line 207. Consider re-phrasing this, as it reads as if Farrell et al. (2020) computed surface roughness from standard deviation of 300-m segments, where it in fact was 25-km segments.

The line now reads 'Surface roughness was calculated as the standard deviation of ATL07 sea ice height product following Farrell et al., (2020), however, instead calculating the roughness over 25 km, the regional differences in surface roughness were calculated over 300-meter length segments to maintain consistency with the spatially averaged ATL07 heights'.

Line 213-214. I don't believe that Mallet et al. (2020) demonstrated that the use of fixed snow densities introduced significant biases in the snow depth retrieval, but rather significant biases in the sea ice thickness estimates. You are stating yourself, that the difference in snow density (to compute the refractive index) didn't make much of an impact on the snow depth (Line 219-220).

The authors agree with the reviewer that the impact of snow densities on the retrieved snow depth retrievals have been demonstrated to be negligible. And yes, there was an error in interpreting Mallett et al., (2020) and therefore the line has been omitted from the revised manuscript.

Figure 5. The text is quite small on the figure. I do wonder about distance sampled vs. number of samples at Site 2 (and for the others as well), since it was stated that you sampled every 5 m? Why is there such a difference in sample distance? Also, remove the table from figure and either incorporate into figure or include as separate table. Also, provide some comments/thoughts about the fact that you don't have an equal number of samples for each location.

The authors thank the reviewer for identifying the difference in number of samples and distance sampled. The distance sampled was calculated based on the actual distance that was covered during the survey for each site. Although the sampling interval was intended to be 5 metre, the sampling interval varied between 2 to 3.8 metre due to variability in sampling by the magnaprobe user. We will include this limitation in execution of magnaprobe sampling in Line 124 which now reads 'The sampling interval was intended to be 5 m intervals to ensure spatial heterogeneity and avoid spatial autocorrelation of the sampled snow depth values following (Iacozza and Barber, 1999). However, the sampling interval was higher (2 to 3.8 metre) during the field sampling for all sites.'

The authors believe that the variable number of samples would not impact the comparison with the satellite derived snow depth distributions since the spatial scales are significantly different.

Figure 6. Consider changing the set-up of the subplots, so it is 1 row and 2 columns, as it will allow you to compare the salinity and density as a function on depth more easily. Also, remove table (incorporate in to figure perhaps?).

Noted and will be incorporated in the revised manuscript.

Line 250. **I do not believe negative snow depths should be removed** – albeit not physically possible, they show the variation between IS2 and CS2 and provide insights in the differences between IS2 and CS2 too. In addition, it biases your statistics higher (which is already somewhat of an issue, since most of your C2I on average are smaller than the in-situ depths), so you should actually **be observing an even bigger difference**. Interesting that you do not see thicker snow than ~50 cm... I think you need a **figure of the actual co-registered ATL07 smoothed vs CS2 along-track data**, to truly see what is going on (as Figure 7 seems to show ATL07 in native resolution).

The authors agree with the reviewer that it is important to include the negative snow depth values as part of the final computation of the snow depths. However, the impact of large snow depths caused largely due to the noise of CS2 needs to be accounted for in the final snow depth estimation. As shown in Figure R8, there is a significant portion of negative snow depths lower than -10 cm which also corresponds with noise portions of the CS2 ellipsoid height which ultimately biases low the mean snow depth retrievals. These snow depths area attributed to the noise in CS2 data and would negatively bias the mean snow depths retrieved from Cryo2Ice across the track. Therefore, we *disregard negative snow depths that are lower than 2 standard deviations from the mean and attribute this to the uncertainty due to the CS2 noise. Therefore, after adjusting for the outliers in the negative snow depths, we get a mean snow depth distribution of 7.52 cm. The new distribution of the Cryo2Ice snow depths are presented in Figure R9.*

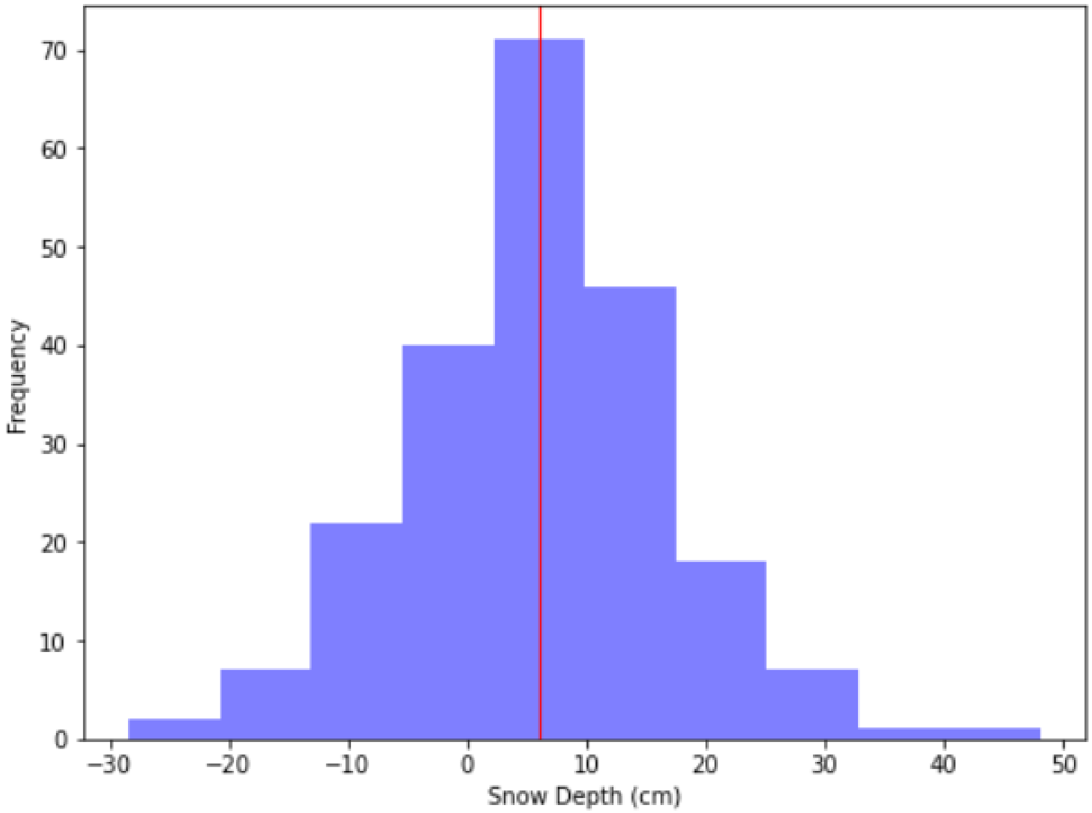


Figure R8: Distribution of raw Cryo2Ice snow depth values including negative snow depth values

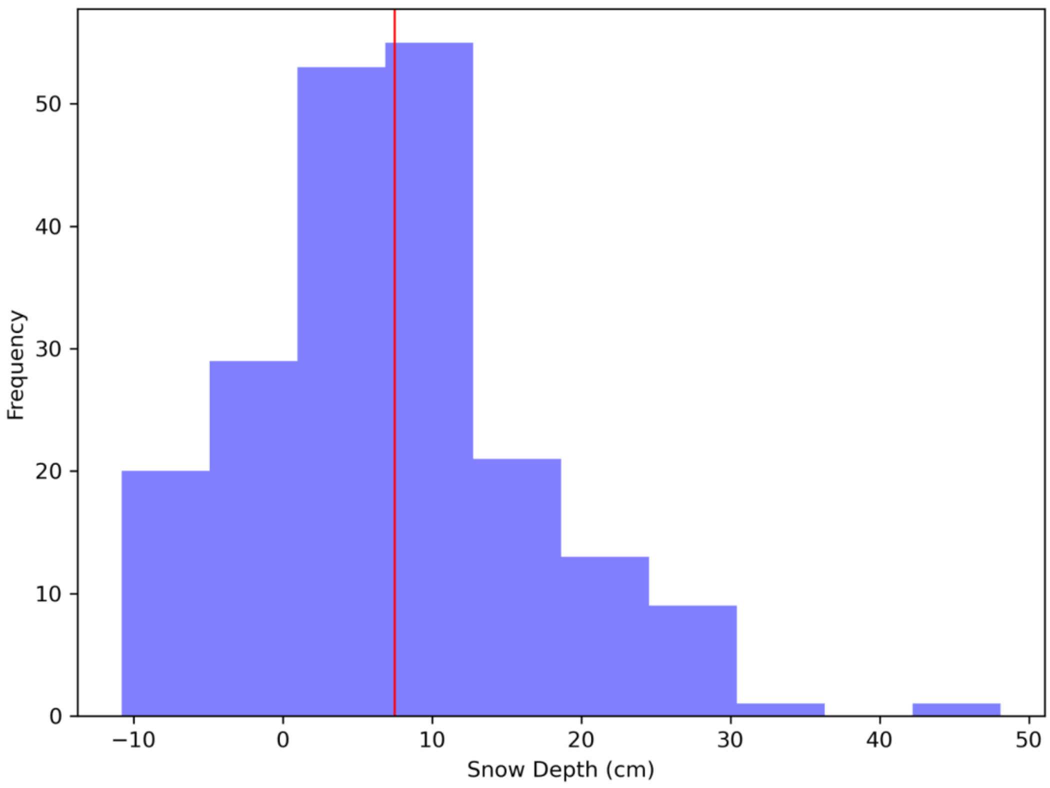


*Figure R9: Cryo2Ice Snow Depths adjusted for negative snow depth values greater than 2 standard deviation from the mean snow dept*h

Figure 7. It is unclear in the legend, whether you're looking at ATL07 heights (in their native resolution) or your 300-m smoothed ATL07 data. Also, consider making the colors comparable across figures (sites have different colors to the vertical lines across figures). In addition, why are we seeing 5-km averages – they are not used nor mentioned in the text? **However, here you also see the variability of CS2 being more evident due to the majority of thin ice observations in IS2 data when smoothing at 5-km and CS2's noise**.

Line 264. What is meant by delineated with roughness? Please provide some more insights into this statement and reason for it.

As mentioned in Section 2.5, Line 179, 'Roughness zones corresponding to each Site are defined as a portion of the CS2/IS2 track which had IS2 surface roughness (Section 2.6) within one standard deviation of the IS2 derived surface roughness directly adjacent to the in-situ sampling site (Figure 1)'. Therefore, in order to define representative portions of the CS2/IS2 tracks corresponding with to the in-situ sites, the IS2 roughness was compared. The portion of the tracks which had roughness values within 1 standard deviation of the IS2 roughness closest to the Site was defined as a similar roughness site.

Figure 8. Consider putting the latitude/snow depth plot on the y-axis of the image, so you can more easily compare it… Also, remember to include subplot numbers. In addition, perhaps I missed it, but how many C2I observations are used in this figure (and overall)? Somehow, these two plots make it look as if there are different number of observations shown.

The authors agree that the plot with the snow depth would fit better in the y-axis. The figure will be revised accordingly. The total number of C2I observations is 214 and the number of observations is consistent with the map and graph.

Figure 10. Consider providing the specific information (bulk snow density) used to derive the snow depth of each site on the plot, for the reader to be reminded that for the site-specific calculations, different densities were used. Consider also providing information about the spread or similar in figure (or in other words, include Table 2 in Figure 10 as text).

Noted. The following information will be added to the revised figure.

Section 4.3. I really appreciate this section and discussion, very nice!

The authors thank the reviewer for their appreciation of the section.

Line 330. "Radar heights can potentially be impacted by snow properties" – I would say that they certainly are!

The line now reads: *'While the IS2 green laser is mostly impacted by the air-snow interface conditions, CS2 radar waveforms interact with different layers of the snowpack and the dominant scattering horizon and subsequently radar heights are impacted by the snow properties'*

Table 1. Why are there two values at time in the mean snow depth column? Not fully clear.

The two values are the range of means between different sites observed in these studies. The caption has been edited to make this clearer.

Line 356. "Found bias of 2-5 cm, we can expect 15-40% systematic biases"… Maybe I missed it, but this seemed to be skipped relatively quickly. Could you provide more insights here? Also, what about the contribution of random uncertainties?

The authors agree that this point needs further clarification. The systematic bias is ascertained based on the range of bias in snow depth compared with the mean snow depth retrieved. The random biases have been quantified individually (surface roughness, tidal variation etc) but it is difficult to ascertain the portion of bias caused by each uncertainty and therefore we are not mentioning a single range.