Dear author,

after a second round of review, both reviewers concluded that further changes are necessary, but these were considered minor. In addition to the reviewers' comments, I would have liked to see more emphasis placed on the fact that the study is based on a very limited dataset. This is not yet reflected in the title, which seems a bit too general. In addition, I am not yet convinced by the assumptions regarding the similarity of the Sentinel-1 radar backscatter, given the potential ambiguities in several parameters, including roughness and not just snow depth, and the relatively large spread of backscatter coefficients. The discussion of the semi-variogram results is criticized by both referees and more clarification is necessary.

Best regards

Lars Kaleschke

Response to the Editor

The authors appreciate the editor's feedback on the paper and thank them for their detailed analysis of the paper. The authors agree that this study is based on a limited dataset needs to be stated explicitly and therefore the following lines in the abstract have been adjusted:

Line 21 now reads:

"In lieu of sea surface height estimates from leads, snow depths are retrieved using the absolute difference in surface heights (ellipsoidal heights) from ICESat-2 and Cryosat-2 after applying an ocean tide correction based on tidal gauges between satellite passes on 29th April 2022"

This ensures that it is clear to the reader that a single pair of passes were validated in the study.

An additional line has been included in the abstract which reads:

Line 30-31 now reads

'Moreover, the proposed methodology for getting snow depth over lead-less landfast sea ice needs to be validated using in-situ datasets in other landfast sea ice regions in the Arctic.'

This makes it clear that further studies are required to test the workflow proposed in this paper.

The authors would respectfully disagree with the Editor's suggestion on making modifications to the title. The suggested methodology is unique and has been tested using a representative dataset which included both satellite retrievals and in-situ field validation from lead-less, landfast ice in the Canadian Arctic Archipelago (CAA). The authors believe that the title is reflective of the new methodology proposed for obtaining snow depth over lead-less landfast ice using Cryo2ice. While the Canadian Arctic Archipelago (CAA) has been used as a study area, this methodology doesn't have any specific step that limits it to the CAA and therefore can be *implemented over lead-less landfast sea ice in other coastal regions where Cryo2Ice tracks coincidences and a tidal gauge station is present within a reasonable distance (~100 km).*

The authors agree that the assumption of Sentinel-1 backscatter to compare snow distribution along the tracks were not sufficiently clarified and therefore have been revised. The Sentinel-1 backscatter is primarily considered sensitive to the surface roughness features over sea ice (Cafarella et al., 2019). However, surface roughness features are correlated with snow depth as demonstrated from the in-situ observations in this study i.e. larger snow depths in rougher areas and vice versa(Figure 1). Therefore, the general assumption is that the Sentinel-1 backscatter being sensitive to roughness will be sensitive to hundred meter scale sea ice features and therefore the broader-scale snow features. This assumption was not clearly stated previously but has been included in the revised paper. Additionally, a new Section 2.3 has been added to explain the Sentinel-1 data and processing steps used to obtain the backscatter.

Please note that The Sentinel-1 backscatter values are not considered for the snow depth estimations from Cryo2Ice and therefore the ambiguities with other parameters in addition to snow depth doesn't propagate uncertainties to the final snow depth calculation from Cryo2Ice. Therefore, the ambiguities in the Sentinel-1 backscatter due to roughness should not change the general trend in the snow distribution along the IS2 and CS2 tracks. The coincident Sentinel-1 imagery is primarily used as a diagnostic tool in order to identify which IS2 tracks should be compared to the CS2 track.

Lines 243 to 248 now reads:

"One of the critical assumptions is that IS2 and CS2 tracks are roughly coincident i.e. both tracks are measuring roughly the same snow despite their reference ground tracks being ~1.5 km apart.To test this assumption, Sentinel-1 SAR VH backscatter was characterized across both the IS2 and CS2 reference ground tracks. The sentinel-1 backscatter is sensitive to surface roughness which roughly corresponds to the snow depths along-track (Cafarella et a;., 2019). Therefore, the Sentinel-1 backscatter which is used to compare the backscatter profiles along IS2 and CS2 tracks to determine if they are similar and therefore are seeing similar snow depth distributions."

Across the variogram lags up to the length scale of the analysed orbit, the variance of the snow depths are pretty consistent and this is likely due to range noise in the 20 Hz CS2 observations. The methodology has therefore been modified to exclude the autocorrelation analysis. The authors do agree with the reviewers that the 1-km smoothing does provide valuable insigts. The authors have decided to include the 1-km smoothing based on the fact that the impact of CS2 noise may be reduced in addition to the native 300-m snow depth estimates from Cryo2Ice.

Reviewer 1

Dear authors,

I'd like to thank you for the extensive and well-addressed responses to the reviewers. It's a treat to read well-thought-out comments and I'm happy to report that all my comments have been addressed – either completely or to some degree. I'm therefore happy to say that I support the publication of this manuscript after a few minor revisions.

In my previous review, my main issues revolved around the treatment of spatial scaling between ICESat-2 and CryoSat-2, and the alignment of the ellipsoidal heights of the identified CRYO2ICE observations correcting only for tidal adjustments. While the authors have done a great job investigating the impact and differences here, I'm not entirely convinced by some of the statements regarding the applicability of this treatment of spatial scaling. Hence, I'd like to ask the authors to consider the way the work is presented and open this up for discussion. I do believe that the methodology presented here is sound, but I worry that some of the statements are too rigorous and might be used "wrongly" in future studies when referenced. Thus, I urge the authors to consider, within the frame of these concerns, some of their statements and their choices for methodology highlighted below from the revised version of the manuscript or from the reviewer responses. Here, I believe discussing these points in the new section "4.3 Adjusting for the Difference in CS2 and IS2 Footprint" and in section "4.6 Surface Roughness and Cryo2Ice retrievals" would be enough.

The authors would like to thank the Review for their in-depth analysis of the paper. The authors agree with most of the suggestions made by the reviewers and provide point by point responses to the raised points below.

Major comments

Backscatter distributions

Line 230-232. I appreciate the inclusion of backscatter across all strong beams and the point-to-point comparison that you included in Appendix (Figure G1) – I actually think this is of more value to include in the manuscript as a main figure, than only Figure 4 alone – perhaps you could include both.

The authors have adjusted Figure 4 to include both the backscatter plot as well as the point-to-point comparison.

However, I also want to point out that Site 4 shows large variations in retrieved backscatter (more than -5 dB and 3 dB) suggesting some real differences here, which supports your conclusion that a lot of stuff has happened here (e.g., ridging, surface features not well captured in the coarser satellite sampling) – perhaps include this aspect in your roughness analysis. I would even consider making a backscatter histogram of the dB differences at each site (and include this along with Figure G1)…

The authors are grateful to the reviewer for pointing out the large variation in backscatter in Site 4, this point has now been included in line 475-477 in the revised paper.
Lines 475-477 now reads:

‘Significant variation in surface type in Site 4 is also evident from the large variation retrieved backscatter from Sentinel-1 (-5dB to 3dB)(Figure 4(b)) which was not very well represented from the snow depth estimations from Cryo2Ice.’

The backscatter histogram was not included for all the individual sites since the readers can pickup the spatial variation from the map in Figure 4(b) while the general backscatter trend along-track has already been provided in Figure 4(a)

Spatial scales

I appreciate the authors work on evaluating the spatial autocorrelation with semi-variograms and the inclusion of an averaged product to 1 km. However, what I really want to open a discussion on is:

I. Do we believe that values of CryoSat-2 are “good” enough – in their own right – to track the surface so that one can derive meaningful statistics from observations in this native resolution, or to which extent (spatially) do we believe that the noise impacts?

II. Whether the semi-variogram you show truly presents an inflection point/convergence level at 1 km and some convergence after 300 m, when in fact you do not know what has occurred at shorter scales than 300 m due to the sampling of CryoSat-2? That is to say, you cannot really state that 300 m is where it convergences, when it is simply the first distance lag that you are presented with. Especially considering the large variability in your semi-variogram, does it truly reach an inflection point?

I’m mostly worried about discussion point II, is it appears that you state over landfast ice that the semi-variance after 300 m begins to become constant (but this is again your first lag-distance…) to indicate the spatial autocorrelation becoming negligible (and would be after 1 km). **So, while I do not believe that you need to change your methodology, since the inclusion of the 1-km smoothing provides some insights into the impact of choosing CS2’s native resolution or not, I do believe you need to expand on this selection of footprints/smoothing scales and how applicable this is, in Section 4.3.**

i. *As noted in the Section 4.3, the native 300-meter product is helpful to estimate the snow depth closer to the small-scale roughness features such as ridges using Cryo2Ice. However, there is significant underestimation due to the introduction of range noise in CS2 20-Hz product which impacts the km scale snow depth retrievals. Therefore, the authors believe that the paper demonstrates the pros and cons of using the native and the smoothed resolutions and future studies can adopt either approach depending on the scale of the study. For example, for applications over few hundred meters, the native resolution may be used while smoothing needs to be applied for study areas over 1-km.*

Lines 527 to 529 now reads:
Therefore, future studies should consider analyzing both the 300 meter resolution product and the 1-km averaged product in order to get both the meter scale snow depth variations from the 300 meter snow depths as well as the more representative snow depth distribution from the 1-km averaged snow depths.

ii. *Across the variogram lags up to the length scale of the analysed orbit, the variance of the snow depths are pretty consistent and this is likely due to range noise in the 20 Hz CS2 observations. The methodology has therefore been modified to exclude the autocorrelation analysis. The authors do agree with the reviewer that the 1-km smoothing does provide valuable insights and further discussion on the rationale needs to be included. The authors have decided to include the 1-km smoothing as an assumption in order to reduce the impact of range noise in the 20 Hz CS2 observations.*

Minor comments

While the discussion and putting in perspective to results of previous studies provided in the conclusion is interesting (!), it should not be mentioned – as a first time – in the conclusion, but rather during your discussion. I encourage you to include it further up.

The discussion of the results from past studies have been shifted to Line 54 of the introduction instead of the conclusions.

You included a new citation of a recent study (Fredensborg Hansen et al. 2024) and I believe you are citing it several times throughout the paper, but in different, mis-spelled, ways it appears. In line 50, you've written Freedensborg Hansne et al. (2024); in line 152 you've written Fosberg et al. (2024); line 464 it says Freesborgen Hansen et al (2024), and in line 296 and 474-475 you've correctly (according to your list of references) written Fredensborg Hansen et al. (2024). Please check and correct accordingly.

The author apologizes for the sloppy error. The spellings have been corrected in the revised paper.

Data (section 2). You've not included any sub-section on the Sentinel-1 data that you have used here to the backscatter comparison and the background of several figures? Please include this along with any pre-processing applied, and please include information about which polarizations you use (also, check for consistency that the same is used, e.g. Figure G1 and Figure 8 seem to either use different polarisations or colour-scales? Also, on these figures, please include the colourbar to show the range of dBs), and describe what you would expect over the landfast ice in terms of backscatter changes here.

The authors thank the reviewer for their suggestion. The sentinel-1 backscatter information has been included in Section 2..3. The authors opted not to include the backscatter from the Sentinel-1 background imagery where the absolutely backscatter values are not relevant to the interpretation and therefore may potentially confuse the reader. The relevant backscatter analysis has been included in Figure 4.

Also, some of the figures are still not of great quality and the text at times is too small to read (e.g., lat/lon on maps). I urge you to have another look at that!

The authors appreciate the suggestion and the quality of the figures have been revised.

Line 14-17. Does this in fact mean, that it is not only the first assessment of snow depth from CRYO2ICE over lead-less ice, but a first assessment of any dual-frequency snow depth over lead-less ice?

The authors are not certain as previous airborne campaigns such as Operation Icebridge have only flown over landfast ice which focused on validating snow depth retrievals as well. Therefore, the authors would *limit the claim to Cryo2ice for this study as past dual-frequncy approaches may have been tested over landfast ice which the authors are not aware of.*

Line 20. “after applying an ocean tide correction (…)” - “after applying an ocean tide correction based on comparison with tide gauges (…)”

The line has been revised based on the reviewer’s suggestion.

Line 20 now reads:

In lieu of sea surface height estimates from leads, snow depths are retrieved using the absolute difference in surface heights (ellipsoidal heights) from ICESat-2 and Cryosat-2 after applying an ocean tide correction based on tidal gauges between satellite passes on 29th April 2022.

Line 23. “significant” to “significantly”

The line has been revised based on the reviewer’s suggestion.

Line 23 now reads:

All four in-situ sites had snow with saline basal layers and different levels of roughness/ridging which significantly impacts the accuracy of the Cryo2Ice snow depth retrievals.

Line 26. “attributing” to “attributes”

The authors believe that “attributing” is grammatically correct here.

Line 26-27. When you say surface roughness, do you mean snow surface roughness or ice surface roughness – or both?

Primarily snow surface since the estimates are based on the standard deviation of the IS2 values.

Line 37. “coincident” to “monthly composites of “: I’d be careful with the use of “coincident” here, as this is not really the case for the monthly estimates (there is some degree of spatial and temporal overlap, but in general they do not see the exact same ice).

The line has been revised based on the reviewer’ suggestion.

Line 49. “A few hundred kilometers”? Fredensborg Hansen et al. (2024) shows overlap of more than 500 km, up to almost 1000 km for some tracks when consistent ICESat-2 and CryoSat-2 data was present. Perhaps change to “hundreds of kilometers”. It sounds like there is little data available along transect, which is truly representative.

The line has been revised based on the reviewer’ suggestion.

Line 60 now reads:

This realignment means that once in every 19 CS2 (20 IS2) cycles, the two ground tracks nearly align for hundreds of kilometers over the Arctic providing new opportunities to improve and validate snow depths retrieved by combining laser and radar freeboards.

Line 80. Remove “~” before “150 photons”. I’m not aware of a change to the ATL07 methodology to not be exactly 150 photons, but maybe it has?

The line has been revised based on the reviewer' suggestion.

Line 82. "For this study, the ATL03"… I'm questioning this segment-length value based on a response to reviewer, where it was stated that you got 36 ATL07 segments within each 300-m segment, resulting in ~8.3 m segment-length. Is this the case for all your segments, or is this in fact an average of number of segments along your ~75-km track? That would suggest that you get exactly the same photons across the entire track, which seems unlikely. Could you clarify how this was computed?

This is not the case for all the length segments, this is in fact the average of the number of segments along the ~75 km track.

Line 103. "over sea ice in the SARIn mode" to "over sea ice floes in the SAR/SARIn modes". The re-tracking over leads is different! (although not relevant for your study, but important nonetheless).

The line has been revised based on the reviewer's suggestion.

Line 114 now reads:

This fixed threshold retracker is used in the CS2 Baseline E level product over sea ice floes in the SAR/SARIn mode.

Line 144. Could you include a reference to data/where you observed this showing the high pressure during this period?

Reference to ECCC data has been included.

Line 150. Sentence seems to stop abruptly. Remove "to" before references, perhaps?

The line has been revised based on the reviewer' suggestion.

Line 161. Re-check formula for refractive index! Should be ^1.5, right?

The line has been revised based on the reviewer' suggestion.

Line 179. How did you get this 6 cm difference? Tide gauge or from the tide models? It's not entirely clear from the updated text.

From the tide gauge data, the line has been revised.

Lines 205-207 now reads:

'The tides varied over a range of ~ 6.0 cm in Dease Strait in between the two passes based on the tide gauge data, so it was crucial to check if the tidal corrections contained within the products accurately accounted for tide differences in the ~77 minutes between passes.'

Line 186. You state here that it is a semi-variogram of the in-situ snow depths, but the semi-variogram that you have shown in response to reviewers was for the CRYO2ICE snow depths? If that is true, it is striking that they both reach an inflection point at ~1 km as you conclude, although I must say I don't fully believe the semi-variogram of CRYO2ICE at 1 km fully shows a deflection/convergence at 1 km due to the sampling of CryoSat-2 and the variability (as discussed in Major comments). If you have semi-variograms of both, I strongly encourage you to include it!

The authors have decided to exclude the semi-variogram analysis based on feedback from reviewers.

Line 231-232: I would probably change this sentence to reflect that in the majority of cases, the assumption is likely valid. But, in your point-to-point comparison you show some variability (which might be significant!).

The line has been revised based on reviewer's feedback.

Section 4.1 seems a bit short or insufficient, perhaps? Is there not more you can state when comparing with former studies? Perhaps on snow depth variability along the transect (e.g., your standard deviation compared with the ranges observed)? Expectations regarding the periods in question (beginning or end of late winter etc.)?

The authors agree that the comparison with the past studies sub-section did seem to be short and therefore have been merged with the following Section 4.2 which discusses the snow depths from Cryo2Ice in further details.

Figure 7. You did not update this to include the 1-km averages too? I strongly encourage you to do so.

Figure 7 was included in Section 3.3 which included results from the native 300 meter resolution of IS2 and CS2 heights and snow depths. The 1-km averaged product was included as a part of the discussion section 4.2. Since the 1-km averaged heights are discussed later in the Discussion, Figure 7 only includes the original IS2 and CS2 heights along with the IS2 averaged over 300 meters.

Line 312-315. You mention here the semi-variogram, but do not show it either as a main figure or in Appendix? I strongly recommend you do include it.

The semi-variogram analysis has been removed based on feedback from reviewers.

Line 374. What 100-km-averaged product?

The line has been revised based on reviewer's feedback.

Lines 376-387. Interesting discussion! It appears to me, that there is then some compensating biases between CryoSat-2 and ICESat-2 when smoothing to the 1 km length scales, since very smooth or very rough ice appears to have the largest differences, but transition zones (Site 1 and 2) match well. Interesting what could be driving this in the processing.

Figure 7 and 12. I do wonder whether instead of just a line, you could shade the "area" of equivalent coverage (the roughness segments/equivalent along-track coverage you show in the maps), to highlight the observations compared in the Site-specific plots.

The roughness zones are computed based on point-wise information from IS2 and not 'area' of backscatter and therefore visualizing this as an area could mislead the readers to think that the retrievals were done over a set area instead of point-wise comparisons. This would also potentially bring in ambiguities related to the across-track variation in roughness for the labelled 'area'. Therefore, the authors believe that a line to represent the portion of the track that was defined as having similar roughness along-track is a better representation.

Section 4.6. Since this is surface roughness from ICESat-2, it is essentially snow roughness – and here, it is based on the Gaussian Width of the photon distributions, if I am not mistaken. I think it would be worth including a short paragraph on how well you believe this roughness parameter actually represents the roughness of the surface that you've encountered.

The surface roughness in this study has been computed as the standard deviation ATL07 sea ice heights over 300 meter length segments.

Lien 478 to 482 now reads:

The surface roughness from IS2 computed and compared well to the roughness features picked up from the snow depth variations with higher roughness zones having higher snow depths from Cryo2Ice e.g. Site 4. However, the difference in spatial resolutions between IS2 and snow depths from Cryo2ice means that finer scale surface roughness features were missed by Cryo2Ice especially in the 1-km averaged snow depth product.

Line 474. Could it also be because the negative snow depths in Fredensborg Hansen et al. (2024) are not at individual CS2 footprints, but rather at the 7-km averaged windows that you mention?

The authors agree with the reviewers comment that the reason is definitely linked to the spatial scale considered.

Line 474 now reads:

We note that the number of negative freeboards (20%) is much larger than the 3% negative snow depths reported in Fredensborg Hansen et al., (2024) which we believe is mostly due to the fact that this study considers a single track averaging averaging over a 300 m and 1-km window compared to a 7-km window in the aforementioned study.

Line 484. "(…) overestimation" to "(…) overestimation within vicinity of significantly ridged ice", or something similar to highlight that this was observed where you had large heterogeneity in the ice surface.

The line has been revised based on reviewer's feedback.

Line 484 now reads:

We note that while Cryo2Ice generally underestimates snow depths by 2 to 4 cm compared to in-situ, the 1-km averaged snow depths also show the possibility of overestimation over significantly rough ice.

Line 490. Remove "centimeter level" or "few centimeters".

The line has been revised based on reviewer's feedback.

Line 495. Remove "finer" after IS2. Not sure what is meant here. Perhaps "high-resolution" instead or something similarly, if that is was is hinted at?

Additionally, there are uncertainties such as the use of a fixed threshold retracker in CS2 which is not tuned for the landfast sea ice and uncertainties associated with the IS2 fine- tracker that may also contribute significantly to the snow depth retrievals.

Reviewer 2

2nd Review of “Snow Depth Estimation on Lead-less Landfast ice using Cryo2Ice satellite observations” by Saha et al.

All of my comments have been answered and I think the paper has been improved. But there are a few concerns left. Please find some additional comments below.

The authors thank the reviewer for their responses and appreciation of the improvements made during the revision. The additional valid comments or concerns have been addressed below.

Abstract, L25-29: I suggest to explicitly mention potential biases due to the CS2 main scattering horizon not being at the snow-ice interface (due to salinity, snow properties, etc.). Moreover, the choice of the retracker also affects the results as we do not know if it optimized for the given conditions.

The authors agree that the point about the position of the CS2 main scattering horizon needs to be explicitly mentioned and this has been added in Line 26-27 of the abstract. This point has also been been introduced in Line 41-42 and finally discussed in Section 4.4. The authors do agree that the bias due to the choice of retracker has not been mentioned in the abstract and has now been added in Line 27.

Line 26 to 30 now reads:

The results suggest that it might be possible to estimate snow depth over landfast sea ice without leads. However, the observed biases of 2-4 cm likely stem from several factors: (1) discrepancies in sampling resolution between ICESat-2 and CryoSat-2, (2) the CryoSat-2 scattering horizon not aligning with the snow-ice interface due to snow salinity, density, and surface roughness, (3) the choice of retracker, and (4) potential errors in the altimeter’s tidal corrections. Further investigation is needed to address these issues.

Conclusions: See above, I think this should be also mentioned in the conclusions.

The authors believe that the impact of snow geophysical properties on the position of the CS2 not being at the snow-ice interface has now been explicitly mentioned in Lines 531-532. Additionally, the impact of the choice of retrackers have also been mentioned in Line 498.

Line 531 now reads:

Snow geophysical properties, especially snow salinity in the deepest few centimeters of the snowpack, may impact the dominant scattering surface of the CS2 radar, resulting in the scattering surface shifted upwards into the snowpack, leading being further above the snow-ice interface which return and can leads to underestimation of the snow depths.

Line 541-543 now reads:

Additionally, there are uncertainties such as the use of a fixed threshold retracker in CS2 which is not tuned for the landfast sea ice and uncertainties associated with the IS2 fine- tracker that may also contribute significantly to the snow depth retrievals.

Regarding the semi variogram analysis: I am still not convinced here. I think the noise, mainly in the CS-2 data is too high to derive values about the autocorrelation. Looking at Figure R5, there is no binned point indicating a drop for small distances, which is also because of the limited along-track resolution.

My interpretation of the Figure is that the 1 km distance that is identified in the paper, is mostly a result of the model being forced to meet the (0,0) point. Is this autocorrelation analysis really needed? I would rather state that something is based on a simple assumption rather than a method with potentially misleading results.

The authors agree with the reviewer's suggestion that the autocorrelation analysis is indeed creating confusion and therefore can be excluded.

Regarding negative snow depth: I agree that removing negative snow depths below the 2x standard deviations is a reasonable approach.

Figure 5: I suggest that you also show the negative snow depths in the histograms here, so it is consistent with the other figures.

There were no negative snow depths measured from the in-situ snow depth measurements collected using a magnaprobe which was accurately calibrated. Therefore, negative snow depths don't make physical sense to be included as part of the in-situ snow depths shown on the histograms in Figure 5.