RC1: 'Comment on egusphere-2025-2657', John Yackel, 14 Jul 2025

Dear Authors and Handling Editor,

Iceberg influence on snow distribution and slush formation on Antarctic landfast sea ice from airborne multi-sensor observations by Steven Franke and co-authors presents a highly novel multi-sensor remote sensing assessment of of iceberg infiltrated snow-covered landfast seasonal sea ice in Atka Bay, Antarctica focused on early December 2022 during the ANTSI campaign. The datasets consist of quad-polarized, ultra-wideband microwave (UWBM) radar from CReSiS U Kansas, airborne laser scanner (ALS), the Modular Airborne Camera System (MACS), four Global Navigation Satellite System (GNSS) antennas, TanDEM-X band SAR imagery, along with coincident ground-based measurements including electromagnetic (EM) induction sounding (GEM2) and in-situ snow depth and sea ice drilling conducted reasonably close in time to the remote sensing data.

The research approach and its datasets are, in my opinion, highly novel and unique. The manuscript is extremely well written and organized and includes some of the most exquisitely constructed illustrations I have seen in a long time. Figure 3 is one such example. This manuscript makes a strong contribution towards improved understanding of snow processes on Antarctic sea ice including the interpretation and use of FMCW and X-band SAR and their polarization capability for snow depth estimation. I recommend publication subject to minor revisions and addressing my questions below.

> We gratefully thank John Yackel for the time and effort reviewing our manuscript as well as for the positive and helpful feedback and comments. We outline below how we intend to address the comments and how we will modify our manuscript accordingly.

General Comment:

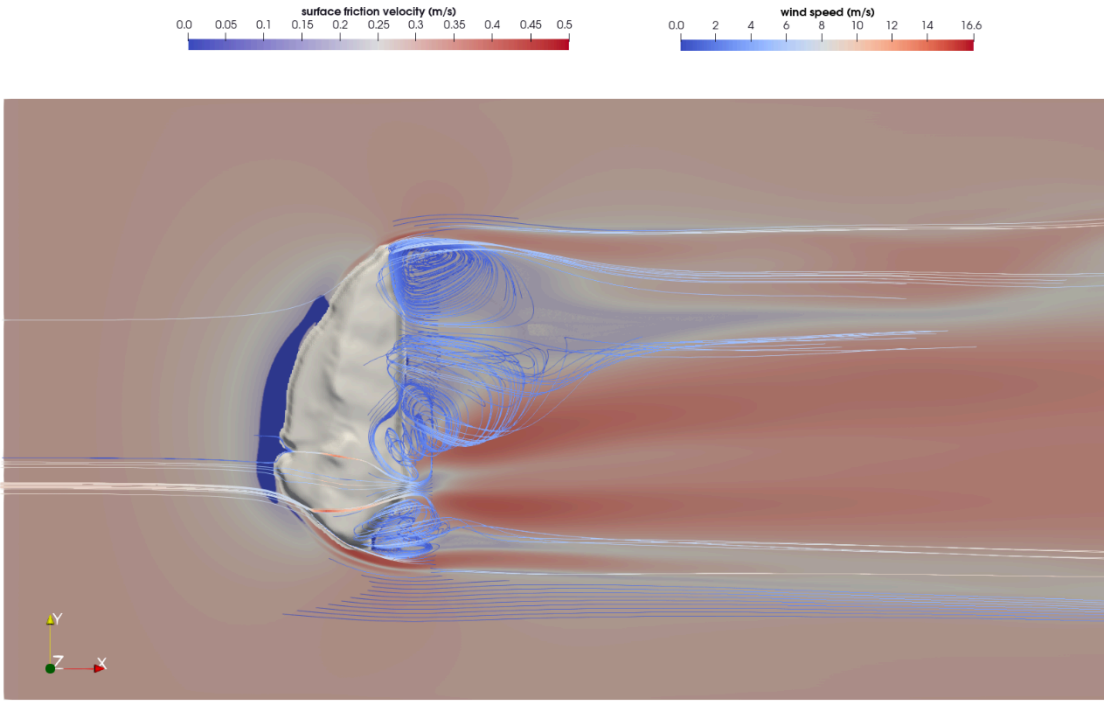
1) I found the Introduction written oddly in the sense that results/conclusions are alluded to on several occasions (L44-47; L67-72) without having read an objectives statement. I strongly recommend that the Introduction include explicitly written and tractable objectives statements and also remove the suggestive language as to what the results and conclusions of the analysis will show.

> We agree and restructured the introduction section focusing on removing suggestive statements in L44-47 regarding SAR backscatter and L67-72 regarding results about flooding. Moreover, we added our objectives at the beginning of the last paragraph of the introduction.

2) The Venturi effect or similar fluid dynamic principles appear to be operating here. I suggest the authors research and possibly mention this principal and relate the effect

to blowing snow around obstacles such as icebergs and discuss whether or not they expect the wind speed to increase leeward of the icebergs further enhancing wind scouring to keep the snow cover thin.

> The reviewer raises a valuable point. In our case, the Venturi effect is not exactly what we believe is acting here, because this refers to the speed-up and pressure drop that occurs through a narrow passage with solid boundaries. However, we agree with the reviewer that Bernoulli's principle, for which the Venturi effect is a specific manifestation, helps explain the flow deviations around the iceberg and the resulting local pressure drops. Other fluid-dynamic processes also play a role, governing the complex flow behavior around obstacles. Stagnation occurs at the windward side, while boundary-layer separation develops along the sides. In the lee, complex wake dynamics create a heterogeneous flow: turbulent eddies generate localized high friction velocities at the surface, contributing to snow erosion or preventing accumulation, while other regions experience flow deceleration. The figure below illustrates these phenomena, showing simulation results of surface friction velocity (i.e., shear stress) and flow streamlines around the iceberg. The streamline color represents wind speed, highlighting both flow deviations around the iceberg and velocity fluctuations in the wake. High-friction zones (red) coincide with areas of snow erosion.



> Furthermore, in a stably stratified Antarctic boundary layer, buoyancy suppresses vertical displacement, which is expected to divert the flow preferentially around the sides of icebergs, thereby limiting snow accumulation

in the immediate lee. At this point we'd like to point out that one of the authors of this manuscript worked on extensive simulations on snow transport and deposition in the iceberg-carrying AtkaBay setting. The results of this work are, however, content of a standalone manuscript, which is under review (Hames et al., 2025; in review at Journal of Glaciology). Therefore, we aim to address the reviewer's comment by adding a detailed suggestive statement that the thin snow cover in the iceberg lee results from two complementary effects: localized accelerations from turbulent structures that enhance snow erosion, and lateral deflection of the flow under stable conditions that reduces the supply of snow-laden air to the lee.

We cite the corresponding preprint of Hames et al. (2025) and added the following sentence in Section 5.3:

"It is expected that localized accelerations caused by turbulent structures in the lee of icebergs erode snow or prevent accumulation, while stable atmospheric boundary layers preferentially deflect the snow-laden flow around the iceberg sides, with both effects leading to a thin snow cover in the lee."

References:

Hames, O., Bouzdine, I., Helm, V., Haas, C., and Lehning, M. (2025): Snowdrift and Accumulation on Landfast Ice Around Antarctic Icebergs: Insights from Modeling and Observational Data, Journal of Glaciology [in review], Preprint available at: https://doi.org/10.31223/x5gq9d

3) The easterly wind direction is mentioned several times as the predominant wind direction. Can a wind rose be provided from the nearest weather station to support the Klöwer et al., 2013 study? Undoubtedly there are winds from other directions which can often produce secondary drifting patterns on the snowscape.

This is an excellent suggestion. We have incorporated a wind rose into Figures 2 and 4, illustrating wind speed and direction for the year 2022 as recorded at the weather station at Neumayer III. The observatory is located approximately 6.5 km inland from the western fast ice edge of Atka Bay. The prevailing wind direction is predominantly easterly, with the highest wind speeds observed from this direction. While winds occasionally originate from the south and southwest, their speeds are substantially lower.

Moreover, we added a sentence in the "Survey Region" section describing the wind characteristics.

Minor Comments:

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L256-258; L383-384; L420-422. While I generally agree with the statement that high backscatter snow covered sea ice corresponds to larger topographic roughness, one has to be careful in entirely associating this high backscatter with surface roughness (even though your surface roughness metric from the ALS DEM data suggest as much). For example, in the attached supplement I have uploaded, there is a small iceberg (highlighted in red box) which has high backscatter in the lee of the iceberg but does not show the high roughness in the center region of the lee (other than the lateral side edges as described by the authors). So, it apparently does not occur in all cases. In my opinion, it is equally likely that this thin snow region can permit the warmer air temperatures to produce higher basal snow layer temperature and brine volume, altering dielectrics and increase volume scattering (as you allude to in L279; L341-350 and elsewhere). In other words, it could be MORE than just surface roughness, especially for your Type 2 reflections. This process is nicely described in https://ieeexplore.ieee.org/abstract/document/9000883

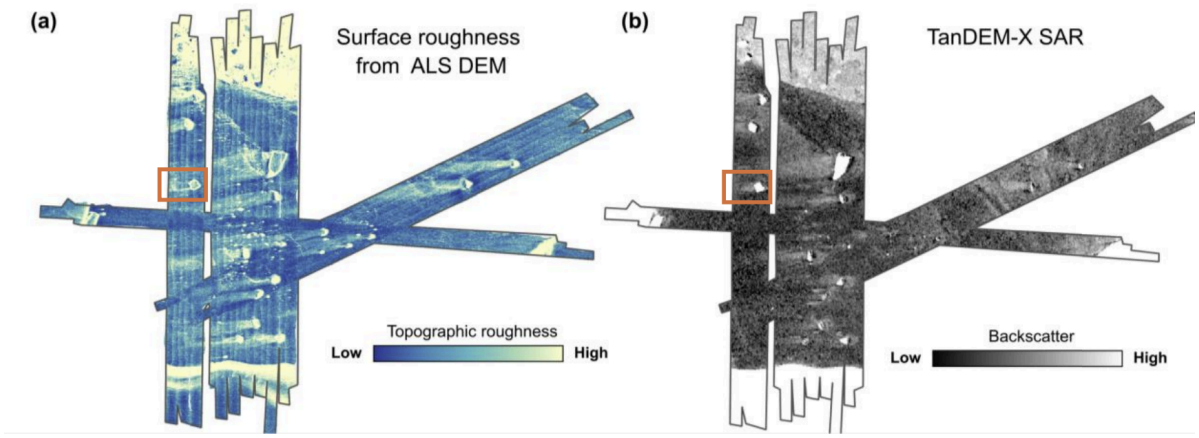

Figure from the reviewer's supplement highlighting high SAR backscatter on the lee of an iceberg, which is unrelated to topographic roughness of the surface (red rectangles).

We will gladly implement the reviewer's suggestion and agree that not in all cases (however in the vast majority of cases and we did not find another iceberg other than the one pointed out by the reviewer) the topographic roughness of the surface is most likely responsible for high backscatter in SAR images. We added the following sentence in the discussion:
"In the case of a single iceberg to the west of our DEM, the strong SAR backscatter does not appear to correlate clearly with increased roughness on the lee side of the iceberg."

Regarding the reviewer's statement *"While I generally agree with the statement that high backscatter snow covered sea ice corresponds to larger topographic roughness, one has to be careful in entirely associating this high backscatter with surface roughness"*, we would like to point out that we argue that the region in the lee side of the ice bergs is NOT snow covered. We justify this with our observations that (1) we lose a radar reflection interface that we would assign in

other regions to be the snow-ice interface, (2) we observe the lowest surface elevations in the laser scanner DEM, (3) we observe lower laser scanner reflectance (e.g., darker surface), and (4) we observe lower near-infrared radiation. All these observations point towards the fact that these regions are most likely not snow covered. Still, we cannot exclude that a minimal snow cover may exist and is potentially not composed of fresh snow but brine volume, which would additionally increase volume scattering on top of the measured topographic roughness. We added a corresponding sentence in the new version of the manuscript.

Moreover, the suggested alternative mechanism due to reduced insulation of thin snow layers (warmer air temperatures leading quicker to warmer snow, higher brine volume and therefore altering the dielectric properties - or by brine wicking) could in general apply to the other regions, which we defined as snow covered. Therefore, the suggestion of the reviewer is very valuable and in line with the main concern of the second reviewer (that a brine-wetted layer is not considered).

We acknowledge and implement the suggestion of the reviewer in the following way (and combined it with comments from the other reviewer):

1. We introduced a third component to our material system on landfast sea ice (fresh snow, brine-wetted snow, and slush) to account for the warmer air temperatures and the slush itself creating a brine layer (see updated Figure 3).
2. We acknowledge a potential layer of highly saline snow at the base of fresh snow of a few centimeter thickness, resulting from brine wicking (e.g., Nandan et al., 2017, 2019, Mallett et al., 2024), which would lead to increased volume scattering and, thus, stronger EM wave depolarization. However, we consider its contribution to depolarization to be significantly smaller than that observed in flooded regions, making it clearly distinguishable in the radar signal. If a snow layer in the lee exists it should be so thin that it would not permit a lot of brine wicking and also not lead to flooding. Moreover, a thin brine-wetted snow layer can potentially also exist at positive freeboard resulting from the initially highly saline surface of thin, new ice. However, we consider the scattering effect of this thin saline layer to be weak as the cross-polarized backscatter at our snow-ice interfaces is constantly considerably low. Moreover, Atka Bay's landfast sea ice is generally thicker compared to the Weddell Sea's sea ice (Arndt et al., 2022). In addition, the sub-ice platelet ice layer might buffer warm ocean water directly reaching the bottom of the sea ice (Arndt et al., 2020) and, thus, limit brine infiltration.

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References

Nandan, V., Geldsetzer, T., Yackel, J. J., Islam, T., Gill, J. P. S., & Mahmud, M. (2017). Multifrequency Microwave Backscatter From a Highly Saline Snow Cover on Smooth First-Year Sea Ice: First-Order Theoretical Modeling. *IEEE Transactions on Geoscience and Remote Sensing*, *55*(4), 2177–2190. https://doi.org/10.1109/tgrs.2016.2638323

Nandan, V., Scharien, R. K., Geldsetzer, T., Kwok, R., Yackel, J. J., Mahmud, M. S., Rösel, A., Tonboe, R., Granskog, M., Willatt, R., Stroeve, J., Nomura, D., and Frey, M.: Snow Property Controls on Modeled Ku-Band Altimeter Estimates of First-Year Sea Ice Thickness: Case Studies From the Canadian and Norwegian Arctic, IEEE J. Sel. Top. Appl. Earth Obs. Remote Sens., 13, 1082–1096, https://doi.org/10.1109/jstars.2020.2966432, 2019.

Mallett, R., Nandan, V., Stroeve, J., Willatt, R., Saha, M., Yackel, J., et al. (2024). Dye tracing of upward brine migration in snow. *Annals of Glaciology*, *65*, e26. https://doi.org/10.1017/aog.2024.27

Arndt, S., Hoppmann, M., Schmithüsen, H., Fraser, A. D., and Nicolaus, M.: Seasonal and interannual variability of landfast sea ice in Atka Bay, Weddell Sea, Antarctica, Cryosphere, 14, 2775–2793, https://doi.org/10.5194/tc-14-2775-2020, 2020.

Arndt, S.: Sensitivity of Sea Ice Growth to Snow Properties in Opposing Regions of the Weddell Sea in Late Summer, Geophys. Res. Lett., 49, https://doi.org/10.1029/2022gl099653, 2022.

Table 1 - is it possible to provide AFIN drill site labels for Figure 1 circles?

> Done.

L335 ... typo 'single'

> Corrected.

L358 .. while snow-ice formation horizon is a likely candidate, a brine-wetted snow snow layer and its effect on dielectric properties and scattering, owing to the warm air and snow temperatures, is equally likely.

> We agree and added a brine-wetted snow layer to our concept throughout the manuscript.

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L369 .. typo ... space needed

Corrected.

RC2: 'Comment on egusphere-2025-2657', Anonymous Referee #2, 08 Sep 2025

The manuscript 'Iceberg influence on snow distribution and slush formation on Antarctic landfast sea ice from airborne multi-sensor observations' by Franke et al. illustrates how grounded ice bergs on landfast Antarctic sea ice affects snow redistribution on the windward side of the icebergs that induces thick snow drift accummulation and flooding. This is observed from a multi-platform and scale campaign on Atka Bay from December 2022 using UWBM Radar, ALS, NIR imagery, ground-based validation and imaging SAR. The paper is well-written, however, the theoretical 'treatment' and 'handling' of radar scattering mechanisms (mostly speculated and neglectance of double-bounce scattering), in combination with assumption of snow only as dry and flooded snow, makes the paper slightly speculative and needs considerable improvement. Therefore, I recommend major revisions for this round. My comments are general for this round and do not deal with specific line-by-line comments for now. The paper is structually well-written with outstanding depiction and quality of figures that may require some change once my feedback is incorporated or defended.

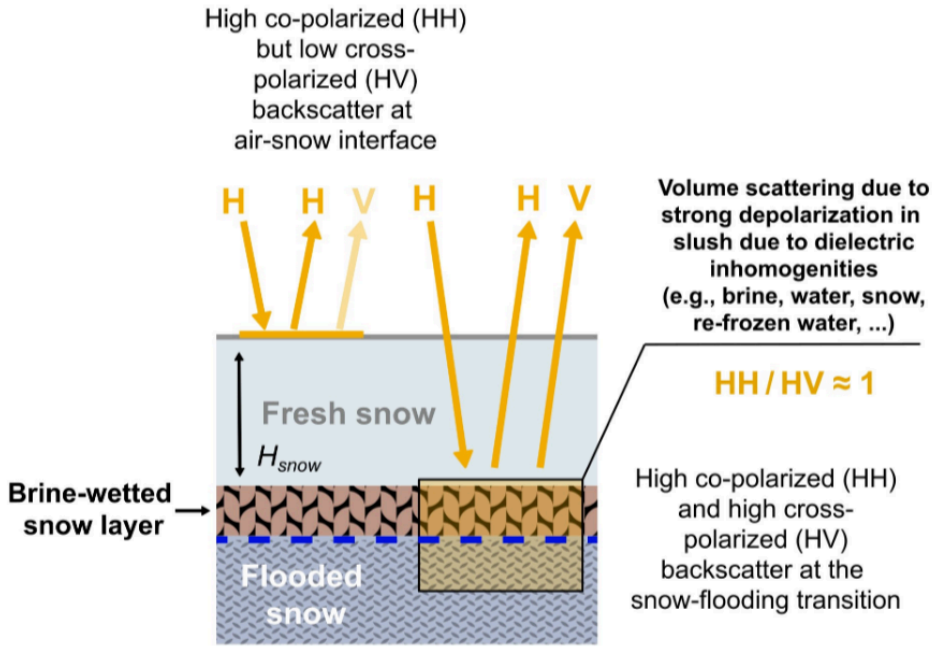
> We thank the reviewer for the time and effort reviewing our manuscript as well as for the helpful feedback and comments with respect to material properties and scattering interpretation. We outline below how we intend to address the comments and how we will modify our manuscript accordingly.

General Comments:

1) My major concern is interrelated. a) The paper assumes radar reflections originating '**either the transition from snow to ice or the transition from dry snow to flooded snow, which could be wet snow / slush or refrozen, salty snow ice**.' Now our issue is this. When the thick snow on thinner ice induces negative freeboard, yes, slush and refrozen snow-ice forms. But, what the authors missed (surprising) is that slush formation during water percolation into the snow also causes snow layers above the slush to be highly saline and completely brine-wetted. This is not 'dry-snow' especially at air and snow temperatures between -4 and -1C. The authors in Line 220 mention '**we specifically define our snow column thickness as dry-snow thickness. The dry-snow depth represents either the interface between snow and ice or the interface between dry snow and flooded snow (i.e., slush).'** This treatment of 'snow column' neglects this intermediate but highly scattering layer of this brine-wetted snow volume, highly sensitive to radar waves, even before the scattering originates from the slush layers below. This needs to be accounted for and addressed.

We thank the reviewer for the feedback and agree with the reviewer's concerns regarding point 1a). We refined the original description of our materials (dry snow and slush) and now consider the effect of water percolation in snow, as well as its impact on the scattering properties in the radargrams. We re-named the upper snow

layer as “fresh snow”, which is non-saline and non brine-wetted. Correspondingly, we defined the thickness of this layer as fresh snow thickness. Then we added the brine wetted layer on top of the flooded snow / slush layer (see Figure below). We corrected every instance of this throughout the text as well as in conceptual Figure 3. We also expanded the discussion in that regard (see tracked-changes pdf).



b) My second concern is tied to my first concern above. The paper's treatment of scattering mechanisms depicted in Figure 3, sections 4.2 and 5.2, figure 5 is very speculative and vague. First of all, thick snow on Antarctic sea ice, exhibiting dirunal air and snow temperature changes during the spring season leads to constant microstructural variations vertically, with formation of melt-refreeze layers, snow grain metamorphism and most importantly (in this study context), changes in snow brine volume from both the brine-wetted layers (omitted in the study) and slush layers underneath. This significantly modifies the snow/slush dielectrics leading to ambigious penetration of radar waves. Next, the paper uses reflection and scattering side by side which needs to be corrected. Figure 5 for example uses snow reflection from surface, volume and interfaces, but they are scattering processes than reflection. Next, with an iceberg sitting grounded on sea ice, double bounce scattering is the major contributor from SAR imagery which also needs to be addressed. Next, the theoretical interpretation of scattering mechanisms needs some modeling evidence to be conclusive than speculative. But I also understand there are no in situ samples of snow and slush from the site (or do you?). I understand the logistic difficulties to collect samples from close to an iceberg due to risk of its tipping. But I feel, irrespective of that, there are slush studies from past that could be used to 'less speculatively' interpret the role of snow and slush on radar signatures. I also

strongly suggest to remove 'dry snow' from the paper as the temperatures are well above the dry snow thresholds.

We also thank the reviewer for this comment, which will definitely help improving the paper. Addressing point 1a, we revised the complexity of the snowpack (now: fresh snow, brine-wetted and slush). Regarding point 1b), we clarified use of the terms "scattering" and "reflection" throughout the text by replacing every instance with “scattering”, “backscattering” or other appropriate terms. Moreover, we replaced the term dry snow with “fresh snow” to avoid a simplification of the more complex snow structure.

While we acknowledge that diurnal air and snow temperature changes during the spring season lead to microstructural variations and snow metamorphism, we found no evidence for the formation of considerable layers of melt-freeze clusters. Note that our study took place during early spring when snow and ice were still only warming up. Particularly, we consider it as very unlikely that refrozen melt layers could represent what we interpret to be the snow-ice or snow-flooding transition as they would be higher up in the snow column. AFIN snow profiling did not report refrozen melt layers across Atka Bay.

In Figure 5 we have replaced “specular reflection” with “weak diffuse scattering” to avoid the term “reflection” here.

We added Nandan et al. (2017) as modeling evidence for the scattering behavior for brine-wetted snow. Moreover, we added the reference Geldsetzer et al. (2009) with respect to the strong dielectric contrast between snow and brine-wetted snow. However, we consider robust modeling the snow response to radar waves beyond the scope of our study but agree that such modeling represents promising future directions.

To account for the effect of double bouncing, we added the following sentences to the discussion: *“While double bounce scattering would be a prominent contributor for ice bergs on grounded sea ice, we consider it as unlikely that it plays a dominant role on the signal from the lee of ice bergs. The effect would also only be truly expected in the immediate vicinity of the iceberg and only in the satellite's line of sight.”*

Indeed, there are no in situ samples from the regions where we detected slush, and the reviewer is correct in assuming that these could not be collected due to the proximity to icebergs. We could only obtain a few samples from the level snow away from icebergs. Based on the results of this study, however, we believe that in situ samples from these regions would be highly valuable and should be included in future research.

References

Nandan, V., Geldsetzer, T., Yackel, J. J., Islam, T., Gill, J. P. S., and Mahmud, M.: Multifrequency Microwave Backscatter From a Highly Saline Snow Cover on Smooth First-Year Sea Ice: First-Order Theoretical Modeling, IEEE Trans. Geosci. Remote Sens., 55, 2177–2190, https://doi.org/10.1109/tgrs.2016.2638323, 2017.

Geldsetzer, T., Langlois, A., and Yackel, J.: Dielectric properties of brine-wetted snow on first-year sea ice, Cold Reg. Sci. Technol., 58, 47–56, https://doi.org/10.1016/j.coldregions.2009.03.009, 2009.

c) Assumption of snow densities: The paper assumes an average snow density of 0.38 ± 0.06gcm-3. Well, with such a flooded snow volume, the assumption of 0.38 is highly underestimated. Is it possible to conduct a quick sensitivity analysis to use both dry snow density and slush density from literature to average them and redo the snow thickness calculation (provided you consider slush also as a part of the snow volume)? Also, Figure 4, the classified imagery is also not interpretive unless value ranges are shown. For example, SAR backscatter showing 'low' and 'high' values can be of any range correct? unless we show it. Also, instead of classifying scattering mechanisms can Types 1 to 3, I suggest to use scattering from geophysical interfaces/volume (e.g. air/snow, within-snow, slush etc etc), as using Types makes it difficult for a future author to use them as a proper terminology. Makes sense?

We agree with the reviewer that the snow density for snowpacks containing slush is underestimated. However, we would like to point out that the effect of the higher density of slush is minor, as the top surface of the slush and the brine-wetted layer already represents the fresh-snow-base interface, and we expect little backscatter deep within the slush due to strong attenuation. We would therefore agree with the reviewer in the sense that—as the reviewer points out—we do not consider slush and brine as part of the volume through which the radar waves we measure propagate to derive snow thickness, except possibly for a few centimeters, i.e., the volume where depolarization occurs. Instead, we would argue that the regions where we detect slush also exhibit greater snow thickness. The reference measurements in the literature were conducted for snow thicknesses of less than one meter, whereas the regions where we detect slush have snow thicknesses of several meters. This results in greater compaction and, consequently, increased density. As a result, snow thickness is overestimated in these areas—a point we already address in line 291 of the initial draft, which also explains the apparent down-bending of the reflection in Figure 5c.

If we were to use the average density of dry snow and slush for the entire study area, as suggested by the reviewer, we would underestimate snow thickness in

areas without slush, which constitute the majority of the study area. In the revised version of our article, however, we acknowledged the issue of higher density in regions with slush in the method section of snow thickness determination.

For Figure 4, have displayed the full value ranges of the colormaps.

We replaced Types 1-3 throughout the text and in all figures with the following terms:

- Air-ice interface
- Snow-ice interface
- Snow-flooding transition

These are my major comments for now. Like I said, the paper is novel but needs considerable changes with respect to my concerns. I am sure the manuscript can be considerably improved if you think about these changes (or defend).