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Response to reviewer 1

Reviewer comment:

I am pleased to see that the authors have made substantial improvements to the paper by thoroughly addressing several of the reviewer comments and provide detailed analysis in their supplementary material.

I don't see any major concern for publication in the Cryosphere.

That said I would maybe incorporate Supplementary Material sections 2 and 4 (including figures SFig1 and SFig3) in the manuscript. The comparison with observational AMSR2 TBs was asked by all reviewer so does the sensitivity to a different microstructure model. Given these results are in good agreements with what the authors state, I think this would be valuable to the manuscript and future readers.

Thank you for your positive and valuable comments. Based on your suggestion, we have added the key findings from these analyses and provided clear references to the corresponding supplementary figures in the manuscript. Considering the overall length of the manuscript, we have decided to retain the corresponding figures in the Supplementary Material. This allows readers to understand the implications of these results within the main text, while those interested in further examining the radiative transfer model simulations and their sensitivity to the choice of microstructure model can refer to the detailed results presented in the Supplementary Material.

Response to reviewer 2

Reviewer comment:

I find the manuscript titled Assessment of Snow Depth Retrieval from Passive Microwave Observations over Arctic Sea Ice: A Global Sensitivity Analysis “ by Yan et al. to have improved compared to the previous version, and I see my comments and questions of the first review round (as reviewer 2) addressed. I appreciate the extra effort the authors took to evaluate the sampling choice (uniform vs. Gaussian, available in the supplements), the clarification regarding snow ice and the simulation setup for section 4.2, and providing more information about the SMRT configuration (Tab.R3).

Regarding the new additions, I have two more concerning questions (and one comment):

1. Figure 2: As suggested, the figure now includes the number of measurements, which I find very useful. However, this makes me wonder what data source was actually used. The number of datapoints ($N > 1000$) and in fact also the dataset cited in the data availability sections indicates that snow density profiles were not or at least not only based on density cutter measurements as described in lines 204-206 (for those, less than 1000 measurements are available, see <https://doi.pangaea.de/10.1594/PANGAEA.940214>). For reproducibility I find it very important to give correct data descriptions and I encourage the authors to check the ones here. I suspect the density data used and shown in Figure 2 here is based on CT measurements or maybe on a combination of CT and density cutter measurements.

We thank the reviewer for carefully examining the data sources and for pointing out this inconsistency. We confirm that both the snow density and grain radius data used in this study were derived from micro-CT measurements, rather than density cutter measurements. We have now corrected the corresponding text to accurately reflect the data source.

2. I welcome the attempt of comparing AMSR2 TBs and the model output and I understand that it is a bit beyond the scope of this work, still the presented results in Fig. R2 did raise concerns: The simulations at 6 GHz are very far off and seem to vary way too much throughout the time series. I am not sure where these fluctuations stem from, could it be that the atmosphere is not taken into account correctly (as [0] also use

the same atmospheric model and much better agreement at this frequency is achieved, they do use a different radiative transfer model for the surface but I would not expect such a big difference between these models)?

While a common bias across frequencies would not concern me much, such a case where an agreement at, e.g., 18 GHz is achieved but not at 6 GHz gives gradient ratios (that many snow depth retrieval algorithms are based on) that were not at all encountered in reality (at least for the MOSAiC case, they are possibly unrealistic on a pan-Arctic scale as well). For example, in this case, the gradient ratio $GR(18/6)$ would be very positive. I wonder if something is off in these extra simulations at least at this frequency and I am not completely satisfied with the explanation given in the supplements, section 2 (“At the low-frequency channels (6 and 10 GHz), the simulated TBs are systematically lower than the AMSR2 observations. This discrepancy is likely related to the stronger sensitivity to sea ice background properties.”). I suppose, this refers to multiyear ice as background, since the authors also discuss positive GRs (on page 25) over multi-year ice, is that right?

But also here I find it puzzling - both from what has been described in literature and from my understanding of the physics, as I would have expected the GRs to be more negative over multiyear ice than over first-year ice. Seeing the difference between the 6 GHz simulations and AMSR2 TBs, I now worry: Can the authors provide references to literature or are there satellite observations that support their findings regarding positive GRs over multi-year ice? Or do the authors have an explanation regarding either the physics or possibly model issues at 6 GHz (or the other frequencies)?

The main difference between our simulations and those of Rückert et al. (2023) lies in the representation of the underlying sea ice. In our study, we adopted a relatively simplified sea ice model, in which fixed background parameters, including ice thickness, density, temperature, and scattering properties, were prescribed separately for FYI and MYI. In contrast, Rückert et al. (2023) employed a more detailed sea ice representation and incorporated observations of the snow–ice interface temperature during the MOSAiC expedition. Their results demonstrated a strong relationship between the 6.9 GHz TBs and the snow–ice interface temperature, highlighting the importance of accurately representing sea ice thermal conditions for low-frequency microwave simulations.

We therefore consider the simplified representation of sea ice properties, particularly the use of fixed temperature profiles, to be a likely major contributor to the discrepancies observed at low frequencies in our simulations. Due to the different penetration depths and sensitivities of microwave channels to sea ice properties, uncertainties in the prescribed sea ice parameters can lead to frequency-dependent bias, with the largest discrepancies occurring at 6 GHz in our case. We expect that incorporating more realistic sea ice temperature profiles and other physical properties would improve the agreement with AMSR2 observations.

Considering that the primary objective of our study is to investigate the sensitivity of microwave TBs to snow properties. Prescribing fixed sea ice background parameters allows us to isolate the influence of snow properties and systematically evaluate their contributions to microwave emissions under consistent FYI and MYI conditions. Therefore, although the simplified sea ice representation introduces uncertainties into the absolute TB simulations, we do not expect the discrepancies identified in this forward simulation experiment to substantially alter the main qualitative conclusions of our global sensitivity analysis.

Regarding the positive GR(18/6) values over MYI, as shown in Figs. S7(a) and S7(b) of the Supplement Material, our analysis of AMSR2 observations over snow-covered MYI reveals a small fraction of positive GR(18/6) values, providing observational evidence that such conditions can occur. This implies that MYI can dominate the GR signal and outweigh the snow depth contribution. Such a regime is problematic for snow depth retrieval because GR is no longer primarily controlled by snow depth, when GR becomes strongly conditioned on ice type and other confounding factors.

Comment for completeness on Supplement 4, Sensitivity test using exponential microstructure model: Recent studies [1] use a Debye modification factor when calculating the exponential correlation length from SSA (see their equation 1). I do not expect for that to change the results qualitatively and therefore require no more action of the authors with regard to this comment.

We appreciate this valuable suggestion and will take this factor into account in our future research on optimizing snow and ice microstructure parameterizations.

Response to reviewer 3

Reviewer comment:

The authors have done an excellent job addressing my initial comments and present a robust and interesting revised study on the retrievability of snow depth from passive microwave observations. I appreciate the thoroughness taken by the authors in addressing my concerns regarding the SMRT configuration and the saline interlayer. I am also pleased to see the addition of more remote sensing context and a dedicated discussion section of limitations of the study.

Overall, I see this as an excellent study suitable for The Cryosphere and recommend its publication pending some minor technical revisions. I have one overarching comment on the structure: I think that the Results section includes much discussion beyond the results presented, and the Discussion section, while discussing the study limitations, also comments on a new experiment and results (the SMRT exponential snow microstructure investigation). Therefore, I believe having “Results and Discussion” as a single section and appropriately subsectioning the current Discussion section within this would be more structurally sound. Besides this, please find a list of my inline minor technical comments below:

We sincerely thank the reviewer for the positive assessment of our manuscript and the constructive suggestion regarding its structure. Following your suggestion, we have further revised the structure of the manuscript.

L94–95: I suggest adding a noun for the ice, such as ‘substrate’ (in keeping with the SMRT naming conventions) to improve the readability here. Additionally, there are inconsistencies in the hyphenations. For example, this could be revised to: “In this study, we considered both single-layer and multi-layer snow over a representative first-year ice substrate. In addition, a representative multi-year ice substrate ...”

Done

L126: The phrasing here reads awkwardly. I recommend rewording, for example, to “Secondly, we input the samples into the SMRT framework and expand the resulting outputs using the Fourier series:”

Done

L219-220: I recommend dropping “The” from “The wind slab” as wind slab is being discussed in its general sense. I also think “wind driven” should be hyphenated as “wind-driven” here.

Done

L237: The grain radius here is referred to as the “equivalent” grain radius; however, Fig. 2 calls this the “effective” grain radius, and “effective” is further used throughout the manuscript. Consider changing “equivalent” to “effective” here for consistency.

Done

L248, L249: Fig. 2 references need updating to reflect the new figure. Fig. 2d referenced in the context of salinity, but Fig. 2d now shows snow effective radius.

Done

L258: Add space between “Figs.” and “2b”.

Done

L266: “Fig. 2g” reference needs updating to Fig. 2i for summer snow temperature.

Done

L298: Consider splitting into two paragraphs starting from “The porous, air-rich upper layer of MYI ...” to improve the readability of this section.

Done

Fig. 2 caption: I recommend amending “ (a) locations of snowpit” to “ (a) snowpit locations”.

Done

L339: Amend “Under dry snow condition” to “Under dry snow conditions” for grammatical correctness.

Done

L532: As above, “Under wet snow condition” should read “Under wet snow conditions.”

Done

L380: The article “a” should be added to “... the presence of saline interlayer” (i.e., “the presence of a saline interlayer”).

Done

L383, 386, 388, etc.: I realise that “saline interlayer” is treated as an uncountable noun – like depth hoar or wind slab. However, given it is a term coined for this study and given its formation from an adjective (“saline”) and countable noun (“layer”), I find that omitting an article before “saline interlayer” impedes the reading flow. I recommend adding the appropriate article (a/the) where possible to improve readability.

Thank you for your advice, we accept it.

Fig. 4 and Fig. 5 caption: The updated figures do not include the underlining of TSI. “The upper underlined number denotes the total sensitivity index, ...” requires amending accordingly.

Done

L447: Consider adding “Here,” to clearly distinguish the parameter setup for this section from the previous one, i.e., “Here, snow temperature and stickiness are fixed at 260 K and 0.1, respectively, and only snow depth, density, and grain radius are varied.”

Done

L476: It would be nice to add a sea-ice-specific reference on the measured density of fresh snow under wind packing.

Done

L485–486: The 23 GHz is certainly affected by the atmosphere also. It sits near a water vapor absorption line at 22.235 GHz, making it even more sensitive to atmospheric water vapor than the 36 GHz channel. I recommend revising this sentence to include a reference to the 23 GHz channel.

Done

L659: There is an erroneous space in “snow- ice” to remove.

Done

L675: I recommend changing “For dry snow condition” to “Under dry snow conditions”.

Done

L688: I recommend changing “For the wet snow with basal superimposed ice” to “For the wet snow with basal superimposed ice configuration” or, dropping the article to give “For wet snow with basal superimposed ice”.

Done

L694: A space is required between “Figs.” and “7” (i.e., “Figs. 7 and 8”).

Done

L710–711: Regarding the conclusion that “GR algorithms utilising 18 GHz or 23 GHz TBs are highly vulnerable over MYI surfaces, providing little to no effective snow depth retrievability for snow with grain radius below 0.5 mm,” I recommend rewording this slightly to specify that using these frequencies as the upper anchor in the GR algorithm limits retrievability. When used as the lower anchor (e.g., GR(36/18)), there is still some retrievability. Furthermore, GR(23/6) appears to show a decent retrievability range at 0.40 mm grain radius – significantly more than GR(18/6), and is perhaps comparable to GR(36/18) and GR(36/6) at their maximums (based on integrating under the curves in Fig. 9). Therefore, I think that the broad conclusion that algorithms utilising 23 GHz provide little to no retrievability under 0.50 mm is not fully supported. Consider drawing separate conclusions for 18 GHz and 23 GHz here.

We thank the reviewer for this insightful comment. Following the reviewer's suggestion, we have revised the corresponding conclusion.