Response to reviewer 3

Reviewer comment:

I find this manuscript to be well written and the methodology (linking SMRT with EFAST and an averaging kernel to quantify retrievability) to be well chosen for such study. The rigor of the analysis while working in a multi-dimensional parameter space is commendable. Overall, this manuscript delivers valuable insights into the complexity surrounding retrievals of snow depth on sea ice that is of great interest to the remote sensing community and has the potential to influence future snow depth algorithms. For this, I recommend this manuscript for publication in The Cryosphere following some revisions. In particular, I find that some specific details of the methodology require clarification and that there is little to no discussion placing the study into the context of existing retrieval algorithms or modelling studies, or discussion regarding the limitations of this study. Furthermore, I find the authors' discussion around "snow ice" in the Arctic to be questionable. Please see below my comments and suggestions. These are divided into overarching, general comments and line-specific comments.

Thank you for your recommendation, and we are pleased to hear it. We accept the reviewer's constructive comments. We will add additional experiments and add solid discussions in the revised manuscript. Please see our response (blue text) below

1: SMRT Configuration and Surface/Interface Roughness

The SMRT model is undoubtedly suitable for this study. However, by design, it is a highly configurable model. As such, user choices during model setup can influence the output. For example, the authors chose to represent the snowpack microstructure as sticky hard spheres (SHS). Several recent studies instead represent the snowpack using an exponential microstructure (e.g., Murfitt et al., 2024; Meloche, Sandells et al., 2024; Wivell et al., 2023; Meloche, Royer et al., 2024) as defined by a correlation length which can be derived using snow and ice densities and SSA. The cited literature on L95, Vargel et al. (2020), notably compares different SMRT snow microstructure configurations in reproducing observed brightness temperatures (Tbs). They found that a combination of the IBA model with snow layers defined by an exponential microstructure more accurately reproduced the Tbs than IBA with a SHS snowpack. Because this study focuses on relative Tb differences, the absolute observation accuracy may be secondary. However, if Tb errors scale non-linearly, then these results may be a reflection of this specific model configuration. To ensure robustness, I recommend running a subset of the experiment using an exponential snowpack microstructure and checking consistency. This could be added to the supplementary material and briefly referred to in the main text.

We thank the reviewer for this important comment. To evaluate whether our conclusions depend on the use of the sticky hard sphere (SHS) microstructure model, we repeated the sensitivity analysis using the exponential microstructure model while retaining the same SMRT configurations, parameter ranges, and sampling strategy. Because our objective was to isolate the influence of the snow microstructure model, snow depth, density, temperature, and effective grain radius were retained as the input parameters in the sensitivity analysis. For the exponential model, the correlation length (l_{ex}) was derived from the sampled radius (r) through the corresponding conversion based on snow density (ρ_{snow}):

$$SSA = \frac{3}{\rho_{ice} r}$$

$$l_{ex} = \frac{4(1 - \rho_{snow}/\rho_{ice})}{\rho_{ice} SSA}$$

where ρ_{ice} denotes the density of pure ice (910 kg m^{-3}), SSA denotes specific surface area. This conversion ensures consistency between the parameters prescribed in the two microstructure models. Because the exponential model does not use the stickiness parameter, stickiness was fixed at 0.2 in the SHS model and was not included as a variable in the sensitivity analysis.

The comparison, presented in the Supplementary Material (Also see Fig.R1), shows that the principal frequency-dependent sensitivity patterns are generally preserved between the two microstructure models. At 6 and 10 GHz, snow density remains the dominant parameter for both polarisations. At higher frequencies, particularly at 36 and 89 GHz, the snow grain radius remains dominant. Its total sensitivity indices are 0.66–0.71 at 36 GHz for both models and 0.60–0.67 and 0.53–0.59 at 89 GHz for the SHS and exponential model, respectively. Snow temperature has a consistently small influence in both models, whereas snow depth generally makes a secondary contribution. At 18 GHz V-polarisation and 23 GHz H-polarisation, the dominant parameter changes from density in the SHS model to the radius in the exponential model. At 18 GHz H-polarisation, density remains dominant, although its total sensitivity decreases from 0.92 to 0.76. At 23 GHz V-polarisation, the radius remains dominant, with total sensitivity of 0.59 and 0.66 for the SHS and exponential model, respectively.

Fig.R1 Comparison of EFAST sensitivity indices obtained using (a) the SHS and (b) the exponential microstructure models. The values above each channel indicate the largest total sensitivity index among the evaluated parameters.

Furthermore, the strong coupling effects associated with snow grain radius, snow depth, and snow density, which are a central focus of this study, are clearly evident under both microstructure models and exhibit highly consistent frequency- and polarisation-dependent patterns (Fig. R1). In both the SHS and exponential model, the coupling contributions of these parameters become particularly pronounced at the 18 – 89 GHz. This consistency further supports our conclusion that the TB response is controlled not only by the individual snow properties but also by their nonlinear interactions, and that this key finding is robust to the choice of snow microstructure model. We have therefore added this comparison to the Supplementary Material and briefly summarised the results in the new Discussion Section (Pages 27-28):

"***The principal conclusions of this study were obtained using the SHS*** *microstructure model. SMRT provides several alternative representations of snow microstructure, and the selected configuration may influence both the magnitude of the simulated TBs and their sensitivity to snow properties. In several recent studies, snow microstructure has instead been represented using the exponential model (Wivell et al. 2023; Meloche et al. 2024; Meloche et al. 2026; Murfitt et al. 2024). In contrast to the SHS model, which idealises the snow microstructure as spherical ice particles characterised by an effective radius and a stickiness parameter, the exponential model describes the spatial organisation of the ice and air phases using an exponentially decaying autocorrelation function characterised by a correlation length. It therefore does not explicitly assume spherical grains and does not include a parameter equivalent to SHS stickiness. Some studies indicate that the suitability of a microstructure model may depend on snow type, microwave frequency, and the electromagnetic theory employed. The SHS model can provide a physically meaningful description of snow as an assembly of clustered ice particles and has shown performance comparable to, or slightly better than, the exponential model for some weakly metamorphosed or rounded-grain snow (Löwe and Picard, 2015). However, in a direct evaluation using 119 Arctic and sub-Arctic snow pits, Vargel et al. (2020) found that the exponential model produced lower errors than the SHS model, reducing the RMSE by up to 24 % for Arctic snow.*

To assess the dependence of our results on this modelling choice, we repeated the sensitivity analysis using the exponential microstructure model for single layer dry snow. Snow depth, density, temperature, and grain radius were retained as the parameters, with the exponential correlation length derived from the sampled SHS radius through the conversion given by Debye et al. (1957) and Mätzler (2002). Stickiness was fixed at 0.2 in the SHS model because it has no equivalent in the exponential model. Details of the experimental design and complete results are provided in the Supplementary Material. The principal frequency-dependent sensitivity characteristics are generally preserved between the two configurations. Snow density dominates the TBs at 6 – 10 GHz, whereas the grain radius becomes increasingly important at the higher frequencies, particularly at 36 and 89 GHz. Differences occur mainly within the 18 – 23 GHz channels, where the dominant parameter and the magnitude of the sensitivity indices vary between the SHS and exponential model. Importantly, the strong coupling effects associated with snow

radius, depth, and density, which are a central focus of this study, are evident under both microstructure models and exhibit highly consistent frequency- and polarisation-dependent patterns."

Additionally, SMRT permits adding roughness to interfaces. Surface roughness is known to affect the Tb as observed by satellites (e.g., Stroeve et al., 2006; Lee et al., 2018); however, this is not mentioned in the manuscript. As there is no mention of this, I assume the study chose not to add any rough interfaces in the SMRT simulations, meaning the retrievability conclusions all assume ice and snow layers are flat. While I think the added complexity of accounting for surface roughness is likely outside of the scope of this paper, which focuses on snow microstructure, I have 2 recommendations based on this omission. 1) Discuss neglecting snow and ice roughness as a limitation of the method, and 2) Flatten the interfaces of the ice layers in Fig. 1 and Fig. 3., so that the reader is not misled into thinking rough interfaces were modelled (if indeed they were not).

Snow and ice roughness were not included in the SMRT simulations. We have therefore revised Figs.1 and 3 by replacing the wavy boundaries with flat interfaces.

Following the reviewer's suggestion, we added a discussion to fulfil the reviewer's recommendations in the new Discussion section of the revised manuscript (Pages 27-28):

"***An additional limitation of this study is that all snow and ice interfaces were treated as smooth boundaries. This simplification does not fully represent natural snow-covered sea ice, where roughness may occur between snow layers and at the snow-ice boundary. SMRT can explicitly represent rough interfaces, and this capability has been applied in several previous studies (Picard et al. 2018; Meloche et al. 2024; Meloche et al. 2026; Murfitt et al. 2024). These studies demonstrate that roughness can modify microwave reflection, transmission, and scattering, thereby affecting the magnitude and polarisation characteristics of the simulated microwave signal. Surface and interfacial roughness may consequently influence the retrieval of snow and sea ice properties. (Stroeve et al., 2006; Lee et al., 2018). However, explicitly accounting for roughness would require additional parameters and substantially expand the multi-dimensional parameter space, which is beyond the scope of the present study, which focuses primarily on the effects of snow microstructure. Future work should incorporate surface and interfacial roughness into the SMRT***

framework to assess whether the sensitivity rankings and snow depth retrievability identified here remain robust under more realistic interface conditions."

2: "Snow Ice" or simply Saline Snow?

The authors note relatively high salinity in the bottom ~10 cm of the snowpacks sampled in the MOSAiC dataset and deem this to be "snow ice" on the basis that salinity is assumed to be negligible for Arctic snow (L234). I disagree with this conclusion and the associated terminology. Firstly, I think it is generally accepted that snow at the base of the snowpack on FYI is often saline; one cause being the upward wicking of brine rejected to the ice surface during sea ice growth (e.g., Domine et al., 2004; Nandan et al., 2017; Mallett et al., 2024). Secondly, widespread snow ice formation due to flooding and subsequent refreezing of the snowpack is generally considered to be an Antarctic phenomenon due to the Antarctic's higher precipitation amount and higher fraction of seasonal ice. Although snow ice formation has been observed in the Arctic, I do not believe it is known to be widespread. The references provided on L259 (Merkouriadi et al., 2017; Merkouriadi et al., 2020) state that their results are confined to the Atlantic sector of the Arctic which is heavily influenced by storms.

Additionally, snow ice in this manuscript is treated as a snow layer, but in its conventional definition, it is a solid layer of ice that is often indistinguishable from sea ice without isotopic analysis (e.g., Jeffries et al., 1997; Granskog et al., 2017; Arndt et al., 2021). To avoid confusion with the conventional definition, I recommend renaming this layer to "Saline Snow," or similar, and removing the suggestion that the properties of the basal snow layers in these profiles were a product of flooding, unless there is clear evidence to support it.

We would like to clarify that the snow ice layer in our study is treated as ice rather than snow in the SMRT model. In the revised manuscript, we have replaced the term **"snow ice"** with **"saline interlayer"** throughout the manuscript, figures, tables, and Supplementary Material. We have revised the description in Sect. 3.3 to make this modelling choice explicit and to use more cautious wording regarding Arctic saline interlayer formation (in Section 3.3, Page 8).

"We include an idealised saline intermediate layer near the snow–ice interface. This layer represents a possible dense and saline transition between the snowpack and the underlying sea ice. For simplicity, we refer to this idealised intermediate layer as saline interlayer in the present study. The formation of saline interlayer may involve

several processes. In Arctic sea ice, enhanced basal snow salinity may also arise from upward brine migration from the ice surface into the basal snow (Domine et al. 2004; Nandan et al. 2017; Mallett et al. 2024). Therefore, we treat this layer as an idealised sensitivity scenario to examine the potential radiometric impact of a dense saline layer at the snow-ice interface. Given the limitations of the SHS model in simulating high-density snow (Picard et al. 2022), the saline interlayer is treated as a thin sea ice layer rather than a snow layer in the SMRT simulation. In this parameterisation, the saline interlayer is represented as an ice-rich medium with brine inclusions, rather than as a high-density snow layer composed of ice grains embedded in an air matrix."

In the SMRT simulations, the saline interlayer is treated as a thin ice layer rather than a snow layer. The difference lies in the physical parameterisation of the layer: when treated as sea ice, the dense saline layer is represented as an ice-rich medium with brine inclusions, rather than as a high-density snow layer composed of ice grains embedded in an air matrix. To describe this more clearly, we have added a model setup table to the Supplemental Material

3: Limitations

Although this study follows a thorough methodology, it is not without its limitations. However, the manuscript currently lacks a real discussion surrounding this. I strongly recommend adding a dedicated limitations paragraph or subsection.

For example, the MOSAiC dataset represents a spatially limited sample of Arctic floes due to the nature of the expedition being "locked-in" to the drifting ice. Additionally, the simulations are specific to observations at AMSR2 frequencies and 55-degree look angle. Furthermore, as mentioned in GC1, SMRT configuration choices can impact such simulations, and this study does not assess the quality of the absolute simulated Tb values. Perhaps another SMRT configuration (e.g., exponential snow microstructure) could be used to check for consistency, or the configuration could be evaluated by also simulating the Tbs observed by the ground-based radiometers on MOSAiC.

Finally, in the context of simulating AMSR2 data, it is important to note that observed Tbs do not come from a single, homogeneous snowpack. While the study investigates different ice types within a footprint, it does not investigate the potential spatial heterogeneity of the snowpack within a footprint. Because this is a fundamental study, it is acceptable to assume a homogeneous snowpack; however, it should perhaps be noted as a limitation when drawing conclusions for practical remote sensing applications

We have added a new Discussion section to state the limitations of our study. As described in our response to GC1, we have added a discussion of the influence of SMRT configuration choices on the Discussion section. We have further expanded the Discussion section to the restricted spatial representativeness of the MOSAiC dataset and the omission of sub-footprint spatial heterogeneity in the simulated snow cover (Pages 27-28):

"***Although this study combines SMRT with EFAST to conduct sensitivity and snow depth retrievability analyses, several limitations should be considered when interpreting the results. First, the MOSAiC measurements represent a spatially and temporally restricted sample of Arctic snow and sea ice conditions rather than the full variability of the Arctic Ocean. Consequently, our results may not capture the full range of conditions across the Arctic, particularly in marginal ice zones, where the snow-covered sea ice is often thinner, more fragmented, and more spatially heterogeneous. Second, the simulations were performed specifically at the AMSR2 frequencies and a fixed incidence angle of 55°, and the resulting sensitivity patterns may have to be adapted to other sensor configurations. Finally, the simulations assume horizontally homogeneous snow and ice conditions. Although variations among sea ice types within a satellite footprint are considered, sub-footprint heterogeneity in snow depth, density, temperature, and microstructure is not explicitly represented. Because the microwave response to these properties may be nonlinear, additional footprint-scale simulations and observational validation may be required before directly applying these results to operational retrievals."***

We have added an additional TB simulation experiment using MOSAiC measurements together with collocated AMSR2 TBs. In this additional analysis, we mainly used the single layer snow configuration corresponding to Sect. 4.1.1 of the sensitivity analysis. Because direct measurements of snow liquid water content were not available in the MOSAiC dataset used here, we restricted this comparison to the winter period, when dry snow conditions are expected to dominate. Values for snow depth, density, temperature and radius were taken from the MOSAiC measurements. For each snow observation site, profile measurements were averaged to obtain a representative value for the corresponding snow column.

To make the modelled TBs more comparable with AMSR2 observations, we also included sea ice and atmospheric datasets in the forward simulation (See Table R2). Specifically, the SMRT model was first used to simulate the microwave emission from the snow-covered sea ice surface. The simulated surface TBs were then propagated through the atmosphere using the Wentz atmospheric radiative transfer model (Wentz 2000).

Tab. R2 Snow, sea ice and atmosphere configurations used in the forward simulation

Layer	Parameter	Setting or Source
Snow	Depth	MOSAiC measurements
	Density	
	Temperature	
	Radius	
	Stickiness	0.15
Sea ice	Thickness	Same as that in the original Manuscript Tab. 4
	Density	
	Temperature	
	Radius	
	Brine volume fraction	
	Porosity	
	Multi-year ice concentration	Dataset from University of Bremen (Ye and Heygster, 2015)
Atmosphere	Total water vapour	ERA5 reanalysis (Hersbach et al, 2018)
	Total cloud liquid water	

Overall, this SMRT configuration can reproduce the general magnitude and temporal variability of the AMSR2 observed TBs, particularly at the intermediate-frequency channels from 18 to 36 GHz (See Fig. R2). 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 of these channels to sea ice background properties. The best agreement is found at 18, 23 and 36 GHz. At 89 GHz, both the observed and simulated TBs exhibit much stronger short-term variability. The model captures part of the observed fluctuation and the increase in late winter. Overall, the comparison supports the use of the adopted snow and sea ice configurations as a physically plausible basis for the sensitivity analysis. Following the reviewers, we have included this analysis in the Supplementary Material.

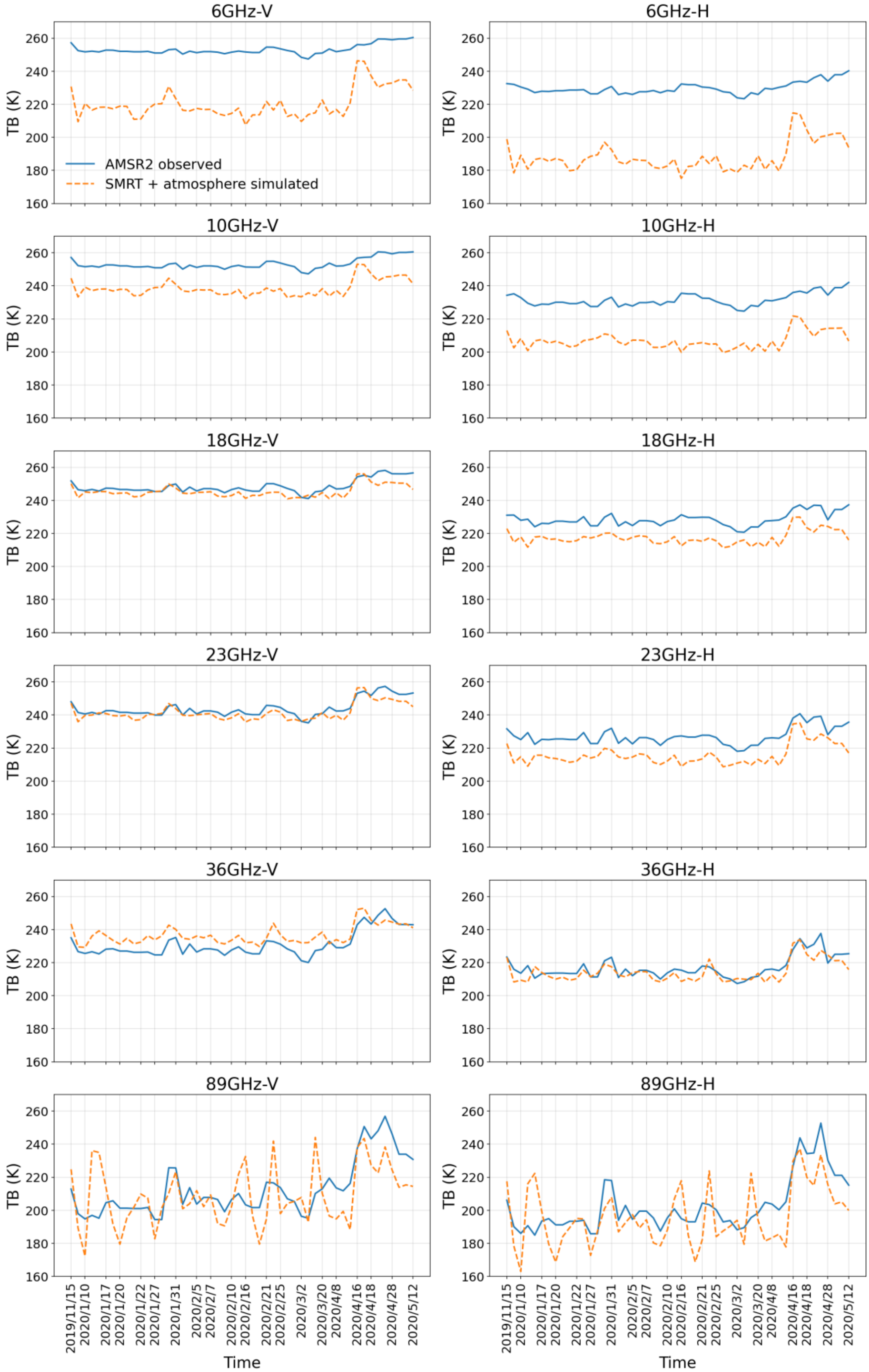

Fig. R2 Time-series comparison of SMRT-simulated TBs and AMSR2-observed TBs during the MOSAiC winter period.

4: Contextualisation of Study

I think the reader would appreciate some more context surrounding existing snow depth retrieval algorithms in the introduction and why they are limited, i.e., that they are typically parametric equations or neural networks trained on airborne or ground-based springtime data (often biased to the Western Arctic, or even Antarctica), therefore representing a limited snapshot of the metamorphic state of the snowpack and making their year-round applicability uncertain. Furthermore, there should be some introduction and discussion regarding prior modelling works that have sought to provide some understanding of the influence of snow properties on the retrievability of snow depth. For example, Markus et al. (2006), Powell et al. (2006), and Rostosky et al. (2020).

We have revised the introduction to describe existing snow depth retrieval algorithms and their main limitations (in Section 1, Page 2):

"Existing passive microwave snow depth retrieval algorithms rely mainly on empirical regressions or neural networks trained using airborne or ground-based observation collected primarily in spring, with many datasets concentrated in the western Arctic or derived from Antarctica (Markus and Cavalieri, 1998; Braakmann-Folgmann and Donlon 2019; Kilic et al., 2019; Rostosky et al. 2020; Rösel et al. 2021; He et al., 2024). The training datasets capture a limited range of regional metamorphic snow conditions, despite substantial seasonal and spatial variations in snow density and grain size. Consequently, snow depth retrievals may exhibit seasonally and regionally varying biases when the actual snow microstructure differs from that represented in the training data or assumed by the retrieval algorithm (Yang et al., 2021; Gao et al., 2023; He et al., 2024)."

Regarding the prior modelling works, we have added the following sentences to the Introduction (in Section 1, Pages 2-3):

"Previous studies have combined radiative transfer models with local sensitivity or Monte Carlo uncertainty analyses to quantify how uncertainties in the snow and ice properties propagate into snow depth retrieval (Markus et al. 2006; Powell et al. 2006; Rostosky et al. 2020)."

5: Quoting Results

I find the results section to be well done and interesting. However, I do suggest integrating more specific quantitative values (e.g., of TSI and SSI values) into the text to support your points and save the reader from needing to continuously scroll back to the figures.

We have added specific TSI and SSI values to the relevant paragraphs where they directly support the main findings.

Line-specific Comments:

L30-31: The snow not only restricts oceanic heat transfer through the ice but also insulates the ice and ocean from atmospheric heat fluxes, both of which influence the sea ice mass balance as per the following sentence.

Agreed and added.

L36: I think “all-sky” here refers to day and night / regardless of the solar zenith angle. In which case, I do not think it should be hyphenated.

Agreed and removed.

L38-39: Penetration depth is itself a function of frequency-dependent absorption and scattering within the system and perhaps listing it individually is redundant.

The new text reads: “***most notably the penetration, absorption, and scattering of radiation within the snowpack and the underlying sea ice***”

L43-45: Such variability does not inherently show that the algorithms are problematic, as snow depth is expected to vary in space and time. Consider rephrasing this sentence to highlight that biases caused by unaccounted for snow microstructural properties can lead to unrealistic variability, using appropriate references.

Agreed. The new text read:

“As a result, unaccounted snow microstructural properties within snow retrieval algorithms can lead to large biases or unrealistic variability of snow depth (Yang et al., 2021; Gao et al., 2023; He et al., 2024).”

L45: I think further context on the “limited application ranges of existing snow depth algorithms” would be appreciated here (in line with GC4); explaining why current algorithms are limited, e.g., perhaps due to their simplistic parametric form that was derived using airborne campaign data that is spatially and temporally limited, reflecting a metamorphic state of the snowpack on Arctic sea ice that is unrepresentative of the entire winter. Overall, there is little explanation of how current algorithms "work" or were derived in this paragraph.

We have revised this sentence in Introduction (Page 2):

"Existing passive microwave snow depth retrieval algorithms rely mainly on empirical regressions or neural networks trained using airborne or ground-based observation collected primarily in spring, with many datasets concentrated in the western Arctic or derived from Antarctica (Markus and Cavalieri, 1998; Braakmann-Folgmann and Donlon 2019; Kilic et al., 2019; Rostosky et al. 2020; Rösel et al. 2021; He et al., 2024). The training datasets capture a limited range of regional metamorphic snow conditions, despite substantial seasonal and spatial variations in snow density and grain size. As a result, unaccounted snow microstructural properties within snow retrieval algorithms can lead to large biases or unrealistic variability of snow depth (Yang et al., 2021; Gao et al., 2023; He et al., 2024)."

L64-65: Although this work may provide the most comprehensive study into the impact of microstructural properties on the retrievability of snow depth on Arctic sea ice and may be the first to consider coupling effects into the analysis, I do not agree there has not been "similar work done before." Others have also modelled Tb as a function of snow and ice microstructure to assess uncertainty in snow depth algorithms, and I think these should be noted in this manuscript. For example, Markus et al. (2006), Powell et al.,(2006), and Rostosky et al. (2020), per GC4.

Agreed. We modified our description accordingly (in Section 1, Page 2).

"Previous studies have combined radiative transfer models with local sensitivity or Monte Carlo uncertainty analyses to quantify how uncertainties in snow and ice properties propagate into snow depth retrieval (Markus et al. 2006; Powell et al. 2006; Rostosky et al. 2020)."

L90-91: I am a bit unsure of the phrasing here regarding "coupled snow and sea ice stratification" configuration. Presumably this is referring to the experiment in which the fraction of each ice type is varied? However, it makes it sound like the single- and multi-layer snow configurations are not coupled to a sea ice substrate. Consider rephrasing.

Done, the new text reads (in Section 2.1):

"In this study, we considered both single layer and multi-layer snow over a

representative first year ice. In addition, a representative multi-year ice was included to assess the influence of sea ice type on snow depth retrieval."

L93-95: Please see my GC1 regarding the SMRT setup and possible extension to ensure robustness using another model representation of snow microstructure.

We have added the corresponding experiment and a new Discussion section.

L193: There is a minor inconsistency in this list. The instrumentation used is listed for all parameters but salinity, in which the location the sample was measured (the lab) is listed. Listing the lab is redundant and it would be better to list the instrument (presumably salinometer?) to match the rest of the list.

We modified the sentence accordingly. The new text reads: "***Snow depth was measured by scale, temperature profiles were measured using a waterproof needle probe thermometer, snow density profiles were measured by density cutter, specific surface area (SSA) was derived from micro-CT analyses, and snow salinity profiles from snow pit samples measured by salinometer."***

L202/Section 3.1 and 3.2: Consider swapping the order of these two sections. The MOSAiC observation section flowing into Section 3.3, how it was used to create the layered snow scenarios, seems more natural.

Agreed and done.

L210: Perhaps reference Table 1 in Fuhrhop et al. (1998) directly to help guide the reader.

Done.

L216-217: While the description of snow ice formation is true, in accordance with GC2, it may not be relevant here.

See our reply to GC2.

L219: I recommend changing "freezes in the snow-ice surface" to "freezes at the snow-ice interface."

Done.

Figure 2: Check punctuation in the panel descriptions (inconsistencies with colons and semicolons). Also, consider changing "the horizontal bars are the standard deviations" to "the horizontal bars represent the standard deviation".

Done.

L221/Fig. 2 caption/306/419. British English "colour" is used, however the rest of the manuscript is consistent with American English (e.g., "utilizing", "summarised", "parameterized"). I recommend changing to the American English "color".

Thank you, we have applied British English throughout the entire manuscript.

L233-239: Please refer to GC2.

Please see reply to GC2.

L244: The choice of parameter space is key to this study. However, it is not clear exactly how the "physically plausible ranges" were derived. Was there a quantitative selection process, or were they selected based on the authors' judgement? Furthermore, how were the MOSAiC measurements combined with the snow properties listed in Fuhrhop et al.(1998) to define the study's ranges? Please add a sentence or two detailing the range selection process more clearly.

We have clarified this issue in the revised manuscript. We have added this explanation to the revised manuscript (in Section 3.3, Pages 9-10):

"***The parameter ranges were primarily constrained by the MOSAiC observations, while also accounting for the applicable range of the SMRT configuration and the ranges adopted in Fuhrhop et al. (1998). For example, the snow density upper limit was set to 500 kg m^{-3}, rather than including MOSAiC measurements reaching 700-900 kg m^{-3}(Figs.2b and 2c), because the performance of the IBA+SHS model deteriorates at high snow densities (Picard et al. 2022). This is also broadly consistent with the snow density upper range of 546 kg m^{-3} used by Fuhrhop et al. (1998).*** "

L283-286: The MYIF study is a great addition due to the known challenge of PM snow depth retrievals over MYI but the unknown nature of how this may couple with snow microstructure properties and the fraction of ice types within a footprint. However, please consider adding a brief 1-2 sentences on why the MYI bias is perceived to exist (i.e., scattering within the porous upper ice layer that mimics the scattering of a snowpack). Additionally, it should be stated what snowpack is assumed for the MYI here – I assume it is the same as the FYI dry snow single layer?

We analysed the influence of the fraction of MYI on snow depth retrieval in Section 4.2.3 using the same single-layer dry snow configurations as that used for FYI. We have added an explanation of why the MYI bias is thought to occur and clarified the snowpack configuration in the revised manuscript (in Section 3.4, Page 11):

"***The porous, air-rich upper layer of MYI can produce volume scattering similar to that of snow, thereby introducing a bias in snow-depth retrievals based on GR algorithms (Markus and Cavalieri, 1998; Rostosky et al., 2018).***"

In Section 4.2.3 (Page 24), we also added:

"In this analysis, the same single-layer dry snow configuration was used over FYI and MYI, while the fraction of MYI was varied."

L302: I suggest changing the section header to "Results and Discussion." Discussion points are interwoven into this section and there is no stand-alone discussion section.

We added a new "Discussion section" in the revised manuscript.

L359-361: See GC2 regarding Arctic snow ice and its occurrence.

See response to GC2.

Figure 4: What determines when TSI and SSI values are overlaid on the figure? There are clearly valid instances for a given channel, layer, and physical parameter that do not have a corresponding number set overlaid. Are TSI and SSI values over a certain threshold deemed to be of interest?

The TSI and SSI values were overlaid only for parameters with TSI values greater than 0.1. We have clarified this criterion in the revised Figs 4 and 5 captions.

L413: Why "based on V-pol TBs"? I assume this is because V-pol is the typical choice for existing snow depth algorithms, however the reader may not know this without context (see GC4).

The reviewer is correct, and we have clarified this in the revised manuscript.

L495: Consider adding an "e.g.," before Markus and Cavalieri, 1998 as it is not the only study demonstrating this.

Done.

L496: Perhaps an opportunity to relate this result (GR(18/6) extending the snow depth retrievable range under large grain snow conditions) to the studies of Powell et al. (2006) and Rostosky et al., (2018).

Agreed.

L522-525: It is my understanding that the lower frequency is typically considered to be the 'anchor' for GR-based snow depth retrievals because it is conventionally regarded to be less sensitive to volume scattering within the snowpack. Check terminology.

We have revised the sentence and checked the terminology throughout the manuscript.

L525-528 and L566-568: The discussion surrounding replacing the 36 GHz (L525-529) and 18 GHz (L566-568) channel with 23 GHz for these applications is interesting. But it may be worth noting the ~23 GHz channel is seldom used for sea ice applications, I believe due to its strong sensitivity to atmospheric water vapour. If this channel were to be used operationally, it would likely require an atmospheric correction using a radiative transfer model. On this topic, I appreciate the supplementary material demonstrating that the higher frequency channel observations from AMSR2 can correlate better with in situ data following an atmospheric correction (depending on the atmospheric state) – perhaps this operational requirement could be reiterated in your conclusions?.

We pointed out this aspect in the conclusion accordingly.

L543: Reiterating that Sect. 3.4. lacks clear information of the snowpack set up for this ice type experiment. Were both types considered to have the same snowpack here?

Yes, we used the same snowpack.

L544-545: It may be worth noting that the spatial response of the AMSR2 antenna is not uniform. Because the antenna gain function is approximately Gaussian, a floe's actual contribution to the measured Tb depends on its relative position in the footprint. Therefore, a simple linear weighting is a simplification.

We have clarified this assumption in the revised manuscript and noted that a more realistic application would require convolution of the spatially distributed surface TBs with the AMSR2 antenna gain pattern in Section 4.2.3, Page 25:

"***This represents a first-order approximation because it assumes a uniform spatial response within the footprint. In practice, the AMSR2 antenna gain is approximately Gaussian, and the contribution of each ice type therefore also depends on its position within the footprint.***"

References

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