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Radar Equivalent Snowpack: reducing the number of snow layers while retaining its microwave properties and bulk snow mass

Note to editor:

We thank all the reviewers for helpful, constructive and thorough comments that helped improve the manuscript. All comments were considered and are addressed in the document below.

Reviewer's comments (R1, …)

Answers to reviewer.

Modification to text. They are visible(yellow) in the track change version.

Reviewer: 1

Comments to the Author

1) Lines 35-38, end of paragraph "…, but can prove challenging for remote sensing applications." Could you elaborate on some of these challenges?

This sentence was removed from the paragraph.

2) Your proposed method is based on K-means clustering. Being a central part of the proposed method, could you add more detailed description of the method? (sections 2.4. and 3.1. of the manuscript).

We agreed that some details are missing on the K-means algorithm. This part was added at line 144.

K-means clustering (Ikotun et al., 2023) identifies groups within the parameter space by minimizing the variance within each group or cluster. First, it randomly initializes centroids for each group in the parameter space and then assigns each point to the initial groups based on the Euclidean distance to the nearest centroid. The centroids are updated to the mean position of all points within each group. The process is repeated iteratively until a convergence is reached (when the centroid positions no longer change significantly) or a fixed number of iterations is completed.

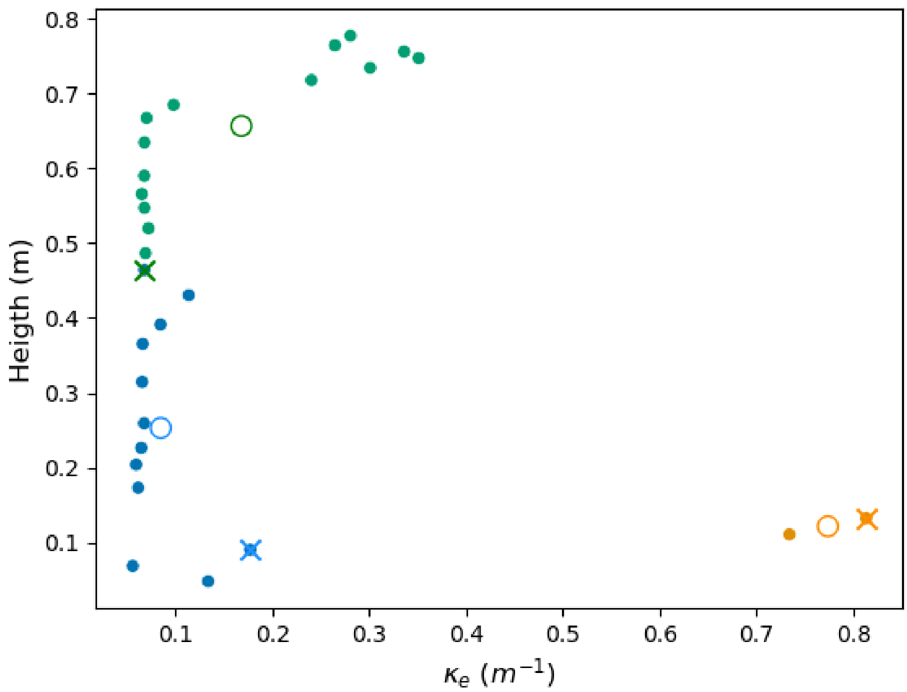
3) The K-means clustering algorithm is known to converge to a local minimum, which is not necessarily a global one. Therefore, the results can depend on the chosen initial points of the cluster means. The initial points should be mentioned in the manuscript.

We agree with your comment. We used the K-means++ initial method to choose our initial centroids and avoid this known problem of the K-means algorithm. This is the default option in the scikit learn package from scipy, which was used for this study. This part was added in section 2.4.

A known issue with K-means is that the random initialization of the centroids can lead to non-representative clusters due to a local minimum reached in the convergence. To avoid this issue, the K-means++ initialization (Arthur and Vassilvitskii, 2007) was used which ensures a smart initial choice of the centroids based on the empirical distance distribution of the points, essentially selecting centroids that are the furthest from each other. This speeds up the convergence and improves the quality of the clusters.

4) It would be interesting to see an example of the plane with the data points, initial mean points, and the converged clusters. If you don't consider this figure informative, it does not have to appear in the manuscript, but perhaps you could produce it in the comments?

Because we now specified the use of the K-means++, we don't see the benefit of adding this in the paper. However, below is a figure of an example of the initial cluster computed the K-means++ method and final cluster. The x markers represent the initial K-means++ centroids, and the circle represents the final centroids.



5) You are using a two-dimensional parameter space in the K-means clustering. Could you comment on the choice of the parameter space? Is there a reason to consider the extinction coefficient instead of both scattering and absorption coefficients in a three-dimensional parameter space?

That is a fair point. In previous work, we tried to only use the scattering coefficient but the best results came with the extinction coefficient. Future work will focus on augmenting the parameter space to improve this method if needed.

6) In section 2.4. of the manuscript, you describe the K-means clustering and the equal thickness grouping in rather equal terms, although the proposed method uses the K-means clustering. If you find it appropriate, please consider reorganising the section so that the K-means is described first as the primary method, and the equal thickness grouping is described then as a secondary method that is used for comparison. In my opinion, this would make the section easier to read.

Agreed, we made the modification in the revised manuscript, section 2.4. The K-means method is first introduced followed by the equal thickness grouping method.

7) In section 2.4. of the manuscript, you describe the weighting of the more complex snowpack into the simplified layers. You use the thickness based weighting (h-weighting) for the equal thickness grouping and optical thickness based weighting (τ-weighting) for the 3- and 2-means clusters. Does this not make the comparison between the two clustering methods unfair?

The optical thickness-based weighting was also used with the equal thickness grouping method as shown in Table 2. This was done to estimate the effect of both the clustering and the optical thickness average. We modified the text in section 3.2 (line 206):

The 2-cluster with τ-average and 3-equal with τ-average method produced the second-best overall RMSE of 0.7 dB and R^2 = 0.97. The 3-equal with τ-average method achieved similar performance to the 3-cluster with τ-average. This indicates that the K-means cluster is less important in terms of preserving the microwave behavior than the τ-average of the snow properties.

We also added a sentence in section 2.4 to clarify:
Both averaging methods were tested on the equal and cluster grouping to compare the performance of each method.

8) The last paragraph of section 2.4. (lines 162-170) describes the problem of removing interfaces when merging layers. This is a very important and an interesting point. However, I found the paragraph somewhat difficult to read. If you agree, please consider reorganising the paragraph so that the problem is introduced first, and then the solution.

This is a good point. The paragraph was reorganized, see revised manuscript.

It is known that the backscatter in snow comes from both volume scattering and reflection at these interfaces. although interface reflection is small with respect to volume (except at nadir). When reducing the number of layers from 50 to 2, 48 interfaces are removed from the simulations. Although interface reflection is small with respect to volume, this reduces the overall internal layer reflections of the signal in the snowpack because of the reflected signal at each interface. However, if the permittivity contrast between two layers is low, then the reflection will be negligible. To quantify the effect of the reflections at each snow layer interface, backscatter simulations using transparent internal layers were used to estimate the influence of all the interfaces in the multi-layered snowpack configurations. The experiment referred to as transparent was done by leaving the surface to the default SMRT interface (flat Fresnel) and changing all internal interfaces to transparent which yields no reflection and full transmission of the radar signal at each interface. This means setting transmission to 1 and reflection to 0. The snow-ground interface was not modified. The difference in backscatter was estimated between the reference simulation and the transparent simulation to estimate the contribution of the n-2 internal interfaces reflection that are removed when reducing the number of layers.

9) Paragraph on lines 221-226 discusses the case when K-means finds clusters of layers that are not connected. In the averaging of such layers, scattering and absorption properties of the layers are not the only things affecting, but also the (optical) depth of the layers in the snowpack. For example, if two layers with equal thickness and extinction coefficient are placed at the top and bottom of the snowpack respectively, their effect to the total backscatter is not the same. In such a case moving one of the layers either from top to bottom or from bottom to up might not make sense. Could you comment on this?

We agree with this comment since the attenuation is not the same for a buried layer than for surface layer. However, in our case, this extreme case is unlikely to happen since the layers would be pretty far from each other in the parameter space. Here, it is really when some layers are both close in terms of scattering but are not "direct neighbors" in the snowpack as illustrated in Figure 2. Restricting only neighboring layers in the K-means would restrict much of the clustering and was a methodological decision we made. In the end, we can see from the results that this effect is probably present but less than 1 dB for all sites. We added a sentence on this issue and modified the following sentence.

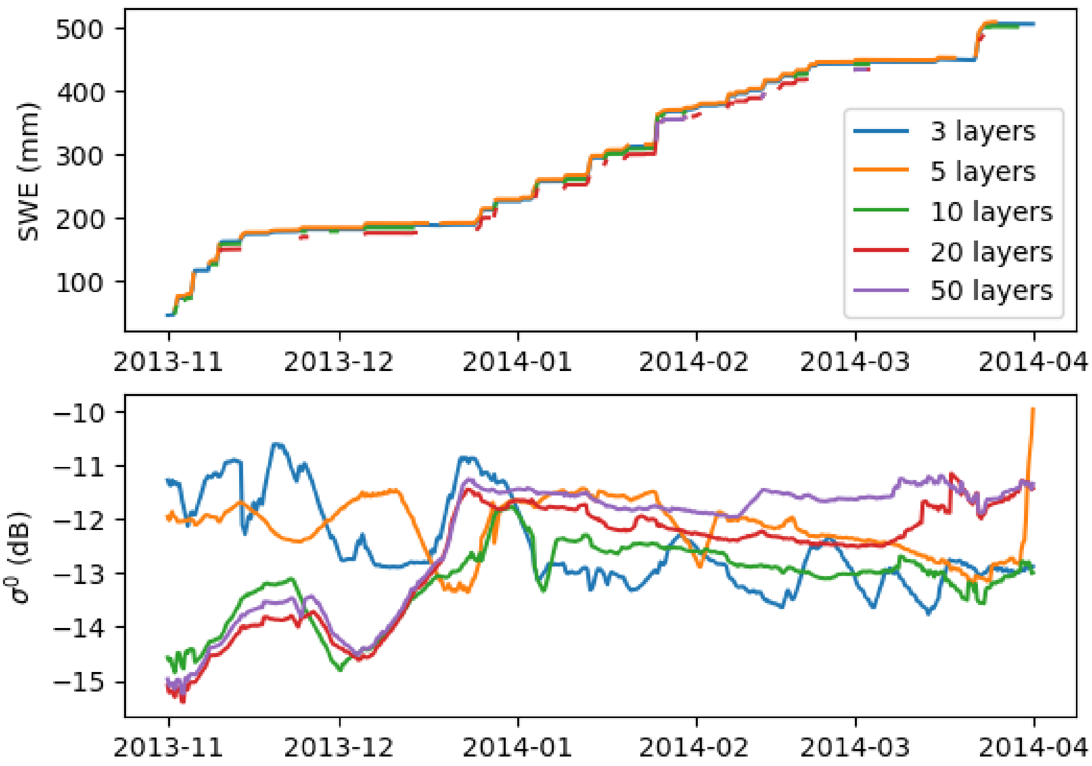
However, attenuation of the scattering from these layers can differ if the layers are moved upward or downward in the snowpack. The other effect was the vertical change in permittivity that was modified, impacting the reflection of the signal on the internal layers. However, because the reflection of the internal layers was minimal (≈ 0.3 dB) and change in backscatter was < 1 dB for all sites, it was concluded that the overall effect of this special grouping case was minimal.

10) The proposed method is based on an idea of running a snow process model with a high number of layers. However, this can also be computationally challenging. Can you comment on how computationally demanding it is to run the snow process model compared to the radiative transfer model? How does the computational cost increase with increasing number of layers (e.g. is running a 4-layer simulation much less expensive than running a 40-layer simulation?)
Running our snow process model at multiple layers is not much more expensive than at 3 layers. For example, running the model with 3 layers comes at an average of 3.0 s per season compared to an average of 3.7 s per season for a 50-layer simulation. This is substantially less than with radiative transfer.

11) Your proposed method merges the snow layers based on their microwave properties, whereas the snow process model merges them (when the maximum number of layers is reached) based on their physical properties. The latter preserves the physical properties of the snowpack, while in the former does not. In terms of the computational cost of the radiative transfer simulation and the complexity of the cost function, the same benefits are obtained by running a snow process model with the maximum number of layers set to three. How does the simulated backscatter from such a snow process model run compare with that simulated using your proposed method? Does your method have a clear advantage in terms of the backscatter when both options are compared with the 50-layer run?
Running the snow model with a few layers (e.g. 3 layers) does not preserve the physical properties compared to running with > 10 layers, it would preserve the mass. The simulation of layered snow microstructure depends in part on simulated temperature profiles, which are not well represented when using a few model layers. Therefore, running our snow model with fewer than 10 layers results in poorly represented vertical temperature profiles, hence, layer microstructure. The introduction was modified to clarify this:

Numerical snow models that use large numbers of layers can improve the representation of the dynamic physical processes within the snowpack, such as heat and mass fluxes, resulting in a better representation of the temperature profile. A better simulation of the vertical temperature profile within the snow improves the simulation of microstructure evolution and spring snowmelt initiation (Cristea et al., 2022) or the identification of weak layers in the context of avalanche hazard forecasting (Morin et al., 2020). Therefore, there is a benefit to adding layers in physical modeling and improve the full vertical profile of snow properties.

Below is an example of Crocus simulations over 1 season (2013-2014) at WFJ. We show the SWE for Crocus simulations with 3, 5, 10, 20, and 50 layers do not change but that the backscatters vary wildly between the simulations but become more similar to each other for simulations with > 10 layers



12) How could your proposed method be applied to different snow retrieval problems?

- Is it only suitable for retrieving SWE or can it also be used to retrieve other snow variables? When simplifying the snowpack, the averaging of the physical snow variables is done using extinction coefficient. Therefore, the effective values don't have the same physical meaning. Does this limit the potential application of the proposed method?

Yes, it is mostly suitable for volume scattering SWE based retrieval, but it can also be used in other microwave retrieval problems. The algorithm aims at focusing on the layers that are relevant based on a given frequency. The snow properties values have an electromagnetic physical meaning, so the potential application must be specific to this. This was added to the conclusion.

The algorithm averages snow layers to obtain effective layers with snow properties that have an electromagnetic equivalent, so the potential application must be specific to the chosen frequency. This method could be adapted to passive microwave remote sensing where the signal is also highly dependent on snow scattering, including ice sheet or sea ice remote sensing where the microwave signal could be simplified to focus on radiative relevant layers.

- In the Bayesian retrieval, the cost function is defined as the sum of the mismatch term (between the observed and the modelled microwave signature) and the prior term. Your proposed method affects both terms; the mismatch term through the forward model configuration (how many layers are assumed) and the prior term. Can you comment on the use of the proposed method in this case?

We added these two sentences in the conclusion.

In the TSMM Bayesian retrieval, the mismatch term is affected by running the radiative transfer solution and optimizing the posterior (parameters) to a reduced number of layers. The prior *distribution on snow properties from SVS2 are obtained with the algorithm again to a reduced number of layers.*

- On the other hand, in the data assimilation approach (e.g. particle filter), your method seems to have a more straightforward application to simplify the snowpack before the radiative transfer simulation to save computation time in the radiative transfer simulation. However, this approach would require an ensemble of snow process model runs, which can be computationally expensive. Can you comment on the use of the proposed method in this case?

Yes, the method would be used to reduce computation in a particle filter by simplifying the calculation of backscatter for all the ensemble snow members. The backscatter would be calculated on a reduced number of layers for each snow member. This was added in the conclusion:

It can also be used in assimilation schemes to reduce the computational time required to calculate a backscatter ensemble from a collected of snowpack members.

Reviewer 1 technical comments:

T1) Line 151: "We investigated three different ways of averageing: …". Only two are mentioned in the text.
Text was modified to "two different ways".

T2) Line 26-27: "More typically, observations are unavailable, so snowpack information must come from …". "must" is perhaps too strong word?
Text was changed to "can come from"

T3) Line 104: "The choice of the electromagnetic model does not influence the final result of this method, …". This could be wrongly understood to mean that the used electromagnetic model does affect the simulated backscatter (the result) and through that, the conclusions of the manuscript. Could you use another expression?
This sentence was removed to avoid any confusion.

T4) Line 119: "where p_11= … and p_22 = … are defined from the phase function." repeats line 116.
The first definition (line 116) was removed.

T5) Line 121: Reference should be (Sihvola,1999)?
The reference was corrected in the text.

T6) Lines 145,149,150,etc.: I don't think symbols after text require parenthesis. E.g. "extinction coefficient (κ_e)" --> "extinction coefficient κ_e" and "layer thickness (h_i)" --> "layer thickness h_i".
The parentheses were removed

T7) Should section 3 of the manuscript be "Results and Discussions"?
Yes, the section was modified

T8) Lines 176,177: The word "scattering" is used but is supposed to be "extinction"?
Yes, the word was changed to "extinction".

T9) Suggestion: Consider using "h-weighting" and "τ-weighting" (instead of keD) for the two layer-averaging methods.

Thanks, we modified as you suggested.

T10) Line 190-191: “3-Kmeans-hi” can be slightly confusing. “K” stands for the number of clusters; would it make more sense to write “3-means”? Also, in my opinion separating the clustering and averaging methods could make the expression clearer. Suggestion: “3-means clustering with h-weighting.” Apply as you see fit.
Thanks, we modified as you suggested.

T11) Similarly, “3-equal-h_i” could be “3-equal-thickness” or “3-equal-h” omitting the subindex i as it is used to refer to the 50-layer snowpack. Using “3-equal-thickness” for clustering and “h-weighting” for the weighting could help to avoid confusion between the two. Apply as you see fit.
Thanks, we modified as you suggested.

T12) Lines 202,205: Same suggestion; consider changing “3-Kmeans-keD” to something along the lines “3-means clustering with τ-weighting” or “3-means clustering with τ-averaging”. Apply as you see fit.
Thanks, we modified as you suggested.

T13) Figure 3: Y label of the bottom figures should be “difference” as “bias” represents systematic or average difference. Same in figure caption.
Bias was changed to difference

T14) Line 230: “Frequency analysis” has other meanings. Consider different expression.
It was changed to “An investigation on the frequency dependence”

Reviewer: 2

1) One question for this topic is whether the layer simplification was utilized directly in the snow process simulation (e.g., using Crocus) or it was solely utilized in the microwave simulation. A clarification may be needed in the abs.
The simplification was only done on the snow properties prior to the microwave simulation and after the Crocus simulation. We modified the abstract by adding the following sentence:

The layer simplification is done as an intermediate step between the physical modeling (SVS2-Crocus) and the microwave radiative transfer (SMRT)

2) Another point is that the authors did experiments for 11 sites globally, whereas only results from three sites were explicitly presented in Fig. 3-5. However, I may be more interested in knowing how snow differs in different sites and what is the key characteristic (wind slab, melting?) that results in the requirement for 2-3 snow layers.

The three sites were selected because they represent most snowpack conditions (Arctic, dry and wet alpine). However, some details are missing as you mention. We added this part to section 2.1 .

From the 11 sites, 3 were selected to more easily illustrate the methodology. These sites (TVC, WFJ and SAP) have distinct snowpack characteristics. The TVC arctic snowpack is characterized by a layer of highly scattering depth hoar with a dense wind slab on top. The WFJ alpine snowpack is characterized by a deep snowpack with progressive density increase from top to bottom with some melt-freeze crusts due to warming events throughout the season. The SAP snowpack is characterized by a similar alpine snowpack but more impacted by wet precipitation.

3) The authors didn't clearly state whether they focus mainly on dry snow.

We only focused on dry snow in the context of SWE retrievals for a volume scattering method in mind. We added this in the introduction.

This study focuses only on evaluating our snowpack reduction method on dry snow in the context of SWE retrievals based on volume scattering. However, this method could potentially be used for wet snow since the extinction coefficient would be sensitive to liquid water via the absorption coefficient if the melt is correctly estimated by the physical model.

4) Lines 35-38: Actually, multiple layers provide a continuous, high-resolution temperature profile inside the snowpack, which benefits most to the accuracy of snow microstructure simulation. To retain snow mass, people don't need a fine-resolution snow process model. In addition, this kind of model has great potential to accurately simulate the snowmelt process, because the fine-resolution snow layers can have different snow temperatures, for the model to evaluate whether the melting point has met.
Thanks, we added your point about the microstructure and snowmelt.

Numerical snow models that use large numbers of layers can improve the representation of the dynamic physical processes within the snowpack, such as heat and mass fluxes, resulting in a better representation of the temperature profile. A better simulation of the vertical temperature profile within the snow improves the simulation of microstructure evolution and spring snowmelt initiation (Cristea et al., 2022) or the identification of weak layers in the context of avalanche hazard forecasting (Morin et al., 2020).

5) Line 40: In Pan et al. (2017), only two snow layers were retrieved. Therefore, it does not have the "large numbers of layers" problem mentioned in the lines 42–43 followed. This part needs a revision.
We agree, this is not what we meant. We revised the whole paragraph, see the revised manuscript.

Some algorithms couple a physical snow model and a snow RTM to retrieve SWE using microwave remote sensing data (Langlois et al., 2012; Larue et al., 2018; Singh et al., 2024). Snow RTMs can model the radar backscatter using snow parameters from complex layered snowpacks. In a SWE retrieval like Pan et al. (2017), the SWE (depth and density) of the different layers is estimated by minimizing the difference between the modeled and measured backscatter. To simulate the backscatter, most RTMs solve the radiative transfer equation based on the discrete ordinate and eigenvalue method (Picard et al., 2004), which discretizes the radiative transfer equation and solves a nonhomogenous system of linear equations based on the number of layers. Increasing the number of snow layers thus increases the computational cost at many levels within the retrieval algorithm. Also, a larger numbers of layers increase the complexity of the retrieval by increasing the number of variables in the cost function. This is why current retrievals typically use a two-layer model (Saberi et al., 2021; Pan et al., 2017). Completely neglecting stratigraphy by using a one layer model can affect the performance of the retrieval (Durand et al., 2011) because layering strongly influences the backscattering properties of snow (Rutter et al., 2016). A one-layer model oversimplifies the scattering behavior of the snowpack and so is not adequate in most cases (Rutter et al., 2019; Meloche et al., 2024; Montpetit et al., 2024). For this reason, a two or three-layer model provides notably better SWE retrievals by accounting for stratigraphy in a certain way (Pan et al., 2017; Saberi et al., 2021). In the end, there is a disconnect between needing several layers in physical model to simulate a realistic microstructure profile but needing only 2 or 3 layers in SWE retrievals for computation simplicity.

6) Lines 44-49: It is suggested to separate the retrieval studies that considered only one layer and

two layers. The use of two layers had been a tradeoff between simulation accuracy and retrieval feasibility. It is ok that the authors think they should at least consider one more layer, i.e., to consider 3 layers in total. However, the following sentence sounds really strange in the current manuscript:

"This is why current retrievals typically employ a one or two-layer model (Saberi et al., 2021; Durand et al., 2024; Pan et al., 2017). However, neglecting stratigraphy by using a small number of layers model reduces the performance of the retrieval (Durand et al., 2011) because layering strongly influences the backscattering properties of snow (Rutter et al., 2016)."

Actually, the studies in Saberi et al., 2021 and Pan et al., 2017 have considered the snow stratigraphy in some sense.
We agree, this is not what we meant. The message is that we need to consider at least 2 layers for SWE retrievals and there is a disconnect with Crocus since it needs a lot of layers to properly simulate a realistic microstructure profile. We revised the whole paragraph, see the revised manuscript and previous comment.

7) It is suggested to consider this reference:

Yu, Y., Pan, J., Shi, J., 2021. Evaluation of the effective microstructure parameter of the microwave emission model of layered snowpack for multiple-layer snow. Remote Sens. 13. https://doi.org/10.3390/rs13102012.

By retaining air-snow and snow-soil boundary reflectivities using the topmost and bottom-most snow density instead of the profile-average density, the idea of Yu et al. (2021) becomes an indirect supporter of your 3-layer configuration suggestion.
Thank you for suggesting this paper we didn't know about. We added this in the introduction.

Yu et al. (2021) proposed an interesting method to estimate an effective 1-layer snowpack for passive microwave applications that calculates a SWE weighted average for the microstructure parameter and preserves the reflectivity of the air/snow and snow/ground interface from the multilayer snowpack. With this approach, the scattering properties are better preserved. To our knowledge, a robust method still does not exist to effectively reduce the number of layers of a given snowpack while minimizing changes in scattering properties.

8) Line 56: The snow type in Pan et al. (2017) was taiga snow. It does not have a wind-slab layer.
True, the reference was removed.

9) Line 5: According to line 65, it is better to say, "2-3 layers" directly in the abs.
We modified the abstract to 2-3 layers

10) Line 88: Could you add more details for the wind-slab and basal vegetation consideration and state why they are important in your study?
We added this sentence.

This allows for a better "arctic" density profile by increasing the wind slab density and lowering the depth hoar density.

11) Line 127: The use of "presented" is confusing here. Better say, could be different for different snow grain types, according to Picard et al., 2022.
We removed the last part of the sentence to avoid confusion.

The polydispersity was assumed to be 1 for all grain types in this experiment but future work could *include a polydispersity for different Crocus-simulated grain types.*

12) Line 130: However, the key point and importance for evaluating using different frequencies is that one can use a single profile to simulate multiple frequencies correctly, despite different penetration depths.
This is a fair point. We would need to investigate this in another study but since TSMM frequencies are fairly close to each other, this not something we anticipate being a problem.

13) Section 2.4: A K-means classification will not work well directly, because you usually will not be allowed to combine layers that are not adjacent. More details are needed here.
We added more details on the K-means classification, see comment 2 from reviewer 1.

14) Line 153: To preserve mass and snow depth, one can also actually only tune the snow microstructure parameter, because any change you make will not go into the subsequent CROCUS simulation. The snow layer simplification for RT calculation was operated stand-alone.
We believe that also including the temperature is important because it impacts the permittivity and the absorption coefficient. It also makes the methodology compatible to passive microwave remote sensing because the physical temperature is important in the brightness temperature calculation.

15) Figure 2 shows two examples where the existence of surface wind slabs and intermediate melt-freeze crusts inside the snowpacks can be two reasons to suggest using 3 layers, respectively. Are the first and second rows for the same site?
Yes they are from the same site but different time in the season.

16) Line 189: Which does the "keD method" refer to in the context of Section 2.4?
Following the comment from reviewer 1, this notation was removed.

17) From Figure 2, did you compare the differences in efficiency between applying K-means grouping on extinction coefficients and on original snow physical parameters (SSA, density, etc.)?

Yes, we tried this, and separating the scattering and absorption coefficient and the extinction coefficient was always better.

18) Lines 195-196: The error increases with increasing snow depth and increasing SSA, too, as the snowpack evolves with time. From my experience, your results shown in Fig. 3 have not included the snowmelt period yet. Therefore, it should not be caused by warmer snow temperatures, at least solely. By the way, it could be better to add subplots showing snow depth and air temperature time series together in Fig. 3.
We agree with this comment and we removed that sentence. This is probably due to the grain size error and not caused by warmer temperature.

19) Lines 214: What does "transparent layer simulation" mean again? Did you mean mandatorily setting the interface transmissivities to 1 and interface reflectivities to 0, although there are 3 layers with different refraction indexes?

Yes, the transparent layer simulation does mean setting the interface transmission to 1 and reflectivity to 0 but the number of layers did not change. This is to estimate the backscatter contribution of the n – 2 interfaces in the simulation. We added this in the methodology:

This means setting transmission to 1 and reflection to 0. The snow-ground interface was not modified. The difference in backscatter was estimated between the reference simulation and the transparent simulation to estimate the contribution of the n-2 internal interface reflections that are removed *when reducing the number of layers.*