**Response to Referees on egusphere-2023-1556**

We appreciate the reviews and comments from both Referees. Please find the response to Referee 1 on pages 1-15, and the response to Referee 2 on pages 16-23.

**Response to Referee 1 on egusphere-2023-1556**

First, we would like to thank the Referee for reviewing and commenting on the manuscript, which will improve the quality of the manuscript. Please find the item-by-item reply below, with the original comments in *italics* and the responses in blue. All the suggested changes are implemented in the revised manuscript.

*This paper details a study using machine learning (ML) to examine Antarctic firn density. The paper is interesting and needs some further revisions before it is suitable for publication. I have put some suggestions and questions below.*

*Major comments:*

*Introduction, I suggest you start bigger, why does Antarctica ice sheets matter to the globe? Also, I think you need to define firn for folks who are not clear on what it is.*

We appreciate the suggestion. This has been implemented in the revised manuscript Lines 21—26.

*On line 142, you say that the firn model has a resolution of 27 km – is that sufficient to capture the firn variations? This is quite coarse, in my opinion. Is this 27 km by 27 km grid cells? I think this needs to be stated more clearly.*

The 27km model resolution is indeed coarse as it corresponds to the resolution of Antarctic wide state-of-the-art climate models that typically drive firn models. This coarse resolution is therefore not expected to capture the fine scale variations on the steep slopes of the Antarctic Peninsula or along grounding lines as the 27x27 km horizontal resolution is too coarse to resolve atmospheric variables. However, this study focuses on dry pixels, which are mainly located in regions of the AIS where climatic gradients, and thus firn property gradients, are not that large.

Moreover, we want to stress that our study is also based on or limited by the coarse resolution of the satellite radiometer (25 km). According to Picard et al. (2014), who compared the metre-scale ground-based brightness temperature measurements to the coarse-resolution satellite brightness temperature measurements around Dome C in Antarctica, there is indeed metre-scale density variation, but "the study also shows that, for the hectometre to kilometre scales, the variations are much smaller. The average of the ground-based brightness temperature is close to the SSM/I and WindSat satellite observations meaning that the investigated area was representative of the pixel of the satellites including Dome C. An important consequence is that spaceborne passive microwave sensors cannot spatially resolve these wind-formed features, but they are very sensitive to the areal proportion of these features." Given the gentle slopes in the interior of Antarctica, we expect this representativeness also to apply to the dry region pixels we studied.

Nevertheless, based on the previous arguments for the representativeness of coarse resolution for both models and satellite observations, we do agree that the coarse resolution may raise questions. To address these, we added the impact of topography on the coarse resolution satellite data on Lines 391—394 and Lines 464—468.

*I think you need at least one study site figure that has all of the locations you refer to in the paper on one introductory map. See my comment from Line 152, for example.*
We improved the indication of locations in the revised manuscript. Following both reviewers' suggestions, we added an example figure in the revised manuscript (Fig. 2) and added labels to indicate specific locations we mentioned in the manuscript.

*Overall, the study design seems confusing. You take the time to cluster the data, but then you do not use it for the analysis, really. Why would you not use that to identify the dry-snow zones, and then perhaps build multiple RF models to see what zone could be best captured? This seems like an interesting approach to take but was not used. I think that this would also eliminate the need to only model the non-wet areas if you simply remove the regions that do poorly in satellite observations.*
We admit that the description of the study design could be better elucidated. To simply answer the reviewer's question, the purpose of clustering was indeed to identify the dry-snow zones. Then, the clusters are used to ensure that different regions are represented sufficiently.

Overall, we hope the following flowchart (Fig. 1) is helpful in resolving the confusion, which we also noticed in the other comments. This flowchart has also been added in the revised manuscript (Fig. 1). In this flowchart, the rectangles represent original parameters consisting of: (i) satellite parameters (TB and sigma0), (ii) IMAU-FDM densities, (iii) external datasets used for result analysis, and (iv) a set of hyperparameters to define the RF regressor. The ovals represent derived parameters. The rounded rectangles represent steps of our study. To be specific, the time series anomalies from TB and sigma0 are clustered to identify dry snow zones. Four distinct dry snow zones have been identified, but we have to admit that we could not relate the separation of dry snow zones to actual physical phenomena. Then, for the dry snow zones, estimation of firn densities using RF regressor is performed.

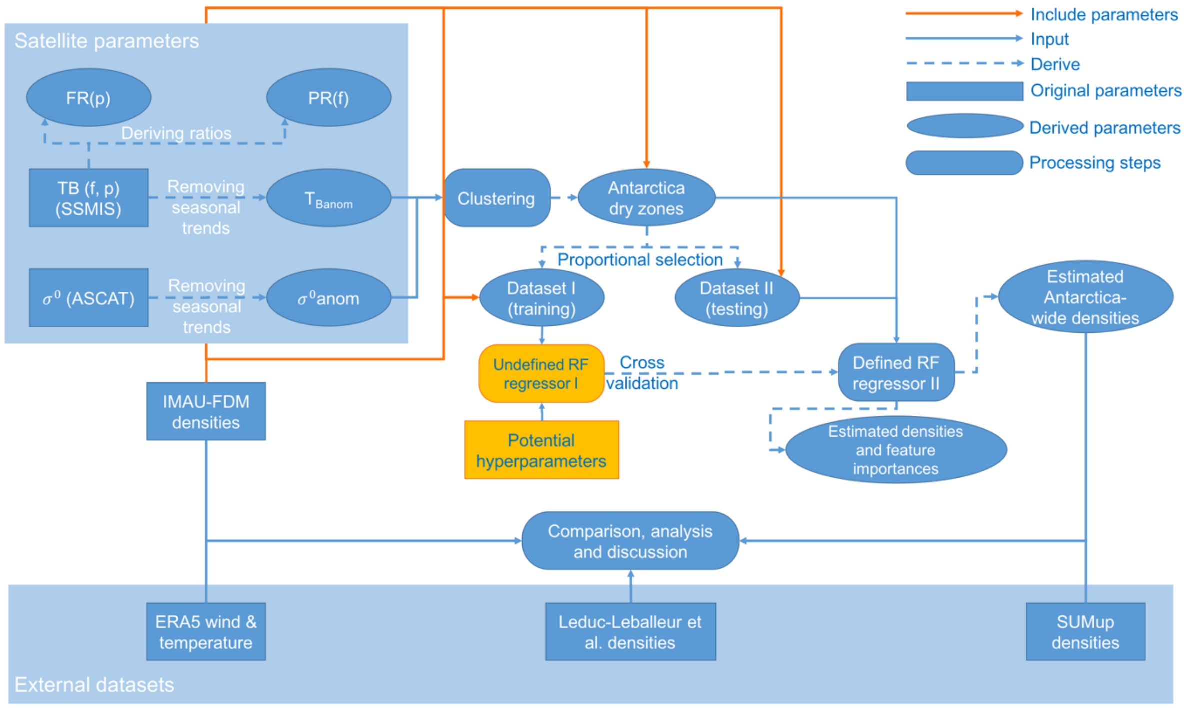


Figure 1. Flowchart of the study design.

The application of the RF regressor consists of three steps (Lines 253—263 of the manuscript). To reduce overfitting, the first step is to use a training dataset (Dataset I in Fig. 1) to perform a hyperparameter tuning through a 5-fold cross validation process (orange rounded rectangle in Fig. 1). The amount of pixels for Dataset I is 10% of the total pixels in each of the clustered dry snow zones, and the actual numbers of pixels are shown in Table 1 below. Please note that "Total" indicates the numbers of pixels, but the features include 10 years of satellite parameters with a temporal resolution of 10 days, therefore the training dataset consists of 1764pixels*366time_steps = 645,624 samples. RF is trained with the IMAU-FDM densities.

Table 1. Statistics of pixels per cluster and pixels used for further RF estimating.

| Cluster | Number of pixels | Number of Dataset I pixels | Number of Dataset II pixels |
|---|---|---|---|
| Firn 1 | 4540 | 454 | 26 |
| Firn 2 | 7360 | 736 | 42 |
| Firn 3 | 3465 | 346 | 20 |
| Firn 4 | 2284 | 228 | 12 |
| Firn 5 | 429 | 0 | 0 |
| Firn 6 | 325 | 0 | 0 |
| Firn 7 | 624 | 0 | 0 |
| Total | 19027 | 1764 | 100 |

The second step of the application of the RF regressor is to provide a simple visualisation of the performance of the tuned RF regressor, and the importance of each feature. In this step, another 100 pixels (Dataset II) are used. The selection of pixels for Dataset II is proportional to the total pixels in each cluster (Table 1). The target parameter is the densities of Dataset II, consisting of 100pixels*366time_steps = 36,600 samples.

The third step is using the tuned RF regressor to estimate the densities over the entire dry snow zones in Antarctica. Please note that after the hyperparameter tuning in the first step, we use the identical set of hyperparameters for the RF regressor in both the second and the third steps. The training dataset is also identical, which remains the samples from Dataset I. We would like to point out that the proportional selection of Dataset I is important, because we also tried using 100 random pixels not restricted by the clusters, and the result degraded in central Antarctica in terms of RMSE (see figure below).

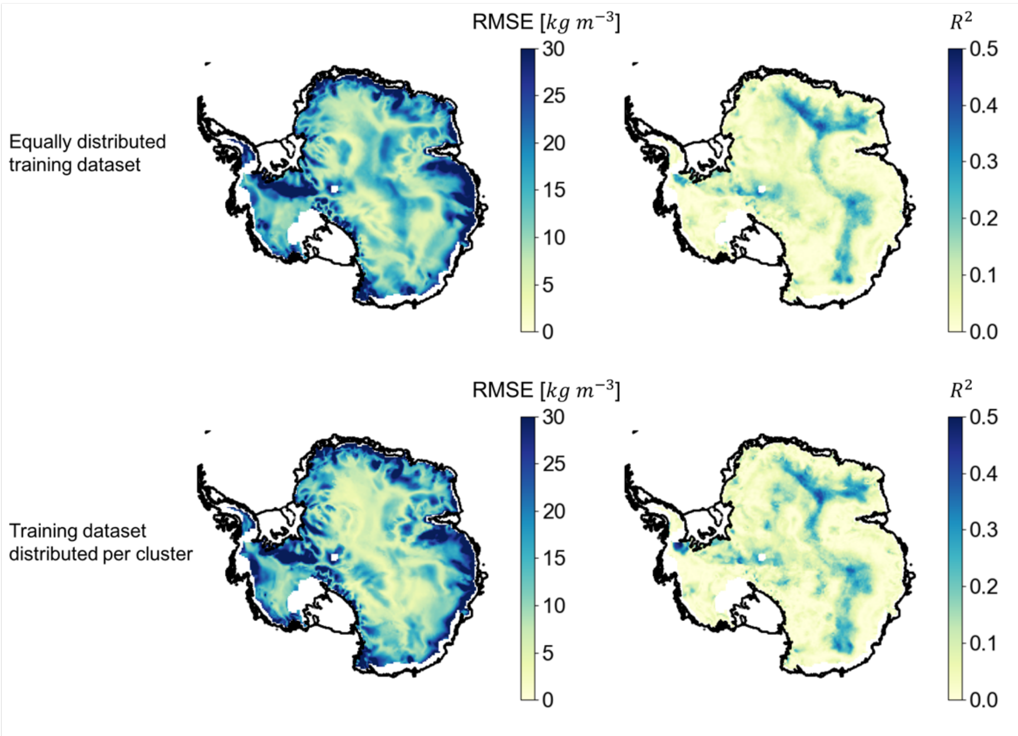


Figure 2. Comparison of performance between using randomly selected pixels (upper row), and proportionally selected pixels (lower row).

Therefore, the clusters are used to ensure that the training samples are selected in a way where different regions are sufficiently represented. We did not train different RF models for different clusters although this should be feasible and interesting, but is outside of the scope of the current paper.

*I do not understand why you didn't use the RF and importances to reduce your model variables. As you show in Figure 5, it looks like these anomalies are not adding much to the RF model. I think you might be able to remove them in the analysis.*

We appreciate the suggestion. However, the hyperparameters are already tuned based on the whole set of parameters. Changing the combination of parameters requires tuning another set of hyperparameters. Therefore, we added a sensitivity analysis to the manuscript regarding changing the combination of the parameters (Appendix B).

*Did you consider other types of ML models, or did you just decide to use RF approaches? Why not consider other approaches?*

We considered using support vector machines (SVMs), but as the previous major comment pointed out, we would like to take advantage of the importances from the RF regressor to understand which parameters are the most influential factors. Moreover, we would like to stress that the scope of this study is to assess the feasibility of "combining radiometer and scatterometer remote sensing data to assess Antarctica-wide dry firn density by using a state-of-the-art ML method" and not to compare different ML algorithms. Therefore, discussing the performances of different supervised ML algorithms is beyond the scope of our study. However, we appreciate the reviewer's suggestion, and agree that a comparison between different machine learning algorithms can be an interesting scope for future studies and we added it to the discussion (Lines 470—472).

*On lines 325, you say "that do not correspond to changes in densities in dry-firn regions?. This line has me wondering about the objective of your work. Are you interested in the firn estimation or are you interested in the change in firn over time? Is the RF model developed for this? Or, are the clusters? You say in the beginning of the paper (Line 71) that the objective of this paper is to "assess the feasibility of combining radiometer and scatterometer remote sensing data to assess Antarctica-wide dry firn density." But, you also say on Line 220 "As our goal is to relate the satellite time series to assess spatio-temporal variations in firn density, we adopt an alternative approach that uses the output of IMAU-FDM as training data instead of relying on in situ data.". What is the objective of this work? If it is average firn, then you can develop your model in one way, but if it is not, then you should develop it in another.*
The main objective is to propose and assess a methodology to derive firn density and its spatial and temporal variations over the Antarctic ice sheet based on on daily satellite observations (and not on changes in these observations). More specifically, assuming firn densities in several locations are known, our study tries to estimate firn densities of the unknown regions in space and time using a combination of satellite observations, namely brightness temperature (Tb) from SSMIS, and backscatter intensity (sigma0) from ASCAT. The motivation is that multiple drivers (e.g. wind velocity, firn temperature) of changes in satellite observation can also drive the changes in firn densities, but the mechanism has not been explicitly quantified or modelled. The "known densities" in our study, are assumed to be the modelled firn density from IMAU-FDM, which is a firn model. Therefore, this paper focuses on both the spatial estimation of firn density, which seems to work well, but also on the temporal variations, which performs less well. Since RF method is based on the daily observations, it does not directly account for changes (e.g. by including change parameters in the RF model), but it does so indirectly by assuming that the satellite data reflect these changes as well. We have rephrased the objectives in the revised manuscript (Lines 73—86).

*Minor Comments:*

*Line 17, short-term (or seasonal) variations, is it both or do you just mean seasonal?*
It should be both. The parentheses are removed in the revised manuscript.

*Line 30, This statement needs a reference.*
(Now Line 32) The sentences have been rephrased in the revised manuscript to include references.

*Line 63 However, the precise mechanisms underlying the interaction between firn densities and satellite observations cannot always be fully understood (Champollion et al., 2013; Fraser et al., 2016; Rizzoli et al., 2017). What do you mean by this? Interaction implies they are interacting, which they are not…*

(Now Line 67) We have rephrased it into "the impact of firn density on satellite observations".

*Line 67, "to other areas or time periods therefore requires further assessment (Tran et al., 2008; Fraser et al., 2016; Nicolas et al., 2017; Rizzoli et al., 2017)". What did they find? Was it successful, i.e, did it work?*

Tran et al. (2008): this study classified snow facies over both Greenland and Antarctica in 2004 based on passive microwave data (brightness temperature) and altimeter data (backscatter intensity) using an unsupervised ML method. The study regarding Antarctica did not capture melt zones, but indicated "a strong topographic control on the class distribution". This is already different from our study, as we managed to detect melt zones in the more recent decade.

Fraser et al. (2016): this study discussed the scatterometer "backscatter response to surface forcing parameters (wind speed and persistence, precipitation, surface temperature, density and grain size)" by comparing the backscatter with modelled parameters between 2007 and 2012. The study shows that sigma0 is affected by surface temperature and wind speed, hence provides theoretical background for our study.

Nicolas et al. (2017): this study identified a melt region in West Antarctica, close to the Ross Ice Shelf, hence provides theoretical background for our study.

Rizzoli et al. (2017): this study characterised snow facies over Greenland using interferometric synthetic aperture radar (InSAR) acquisitions. The study identified melt zones using an unsupervised ML algorithm, hence provides theoretical background for our study.

We adapted Lines 42—64 to describe the previous works better and rephrased the sentence so it better indicates that these studies were already discussed.

*Generally, italicize In situ.*

We leave this decision of italicising to the copy-editor's decision but have seen other The Cryosphere publications (e.g. Orsolini et al. (2019)) where it was not done.

*On line 70, you talk about calibration. You did not mention calibration previously, and it is unclear what this is referring to. Models? The satellites? Fusion methods? I think this needs to be tied to modeling and why calibration is needed. Otherwise it seems to be coming in the text out of the blue.*

We agree and this statement has been removed.

*Line 72, you talk here at three experiments, did you compare /use the observations in situ ever? It seems like the SUMup is not used (or mentioned) in any one of the experiments. I think if you are going to mention SUMup, you need to say where it was applied in the experiments.*

SUMup is not used for setting up the experiments, but for the validation and analysis of where the potential errors come from. This has been rephrased in the revised manuscript (Line 85).

*Line 132, "outputs of the regional atmospheric climate model RACMO2.3p2" These scales seem really different… What resolution is the model run at?*
(Now Line 144) We have revised it as follows:

*"IMAU-FDM simulates the transient evolution of the Antarctic firn column, and is forced at the upper boundary by outputs of the Regional Atmospheric Climate Model (RACMO2.3p2) at a 27 km horizontal resolution (van Wessem et al., 2018)."*

*Line 140 – move these two sentences up to say this earlier (perhaps line 131), that will assist with my previous comment. The first sentence of this paragraph could be combined with the previous one.*
We have moved the description of the spatial and temporal resolution above to Line 145.

*Line 132, RACMO2.3p2 – define?*
We have given the definition with capitals (Line 145). Please see the comments above for the revised sentence as well.

*Generally, through the text, you refer to "the model output", or "models". As you have multiple models, I suggest calling the models by their names, or ensuring they are referenced clearly to distinguish the model.*
This has been referred to as IMAU-FDM in the revised manuscript.

*Line 135, we focus on the density of the… How many layers are there in this model in total?*
The model employs up to 300 layers in total of 3 to 15 cm thickness, which represent the firn properties in a Lagrangian way. The output is resampled to a regular grid with layers of 4 cm. This has been clarified in Lines 147.

*Line 142, the firn data are reprojected – this is modeled data, correct? I think you want to make sure to differentiate the model from the observations.*
(Now Line 157) Yes, this has been rephrased as "the firn density model data from IMAU-FDM".

*Line 138, "…have been acquired at approximately this depth…" Why? This seems kind of arbitrary. Also, 4 cm seems very shallow for firn. Is this because it is in Antarctica?*
In the revised manuscript we switched from firn depth of 4 cm to 12 cm (also in major comments of Referee 2) and we have clarified our selection for this depth on Lines 151—155:

"For the comparison with satellite observations, we focus on the density of the top 12 cm ($\rho$12cm) from the IMAU-FDM output. We also use $\rho$12cm for density estimation using the random forest (RF) regressor. The choice of the 12 cm depth is based on (i) the fact that many in situ measurements used for evaluating the density estimations have been acquired at depths that are several centimetres below the surface, e.g. Picard et al. (2012) and Leduc-Leballeur et al. (2017), and (ii) a compromise between the expected penetration depths at 19 GHz and 37 GHz (0.1–2 m; Surdyk, 2002)."

*Line 145, Surface Mass Balance and Snow on Sea Ice Working Group (SUMup) dataset. You have already used this acronym, define it earlier.*

This has been defined in the revised manuscript (Line 86).

*Line 146, "at the smallest mid-point depths" More clarity please, what is 'small' and what is the mid-point of?*

Mid-point refers to the mid-point of the ice sample. SUMup provides information on start-point, end-point and mid-point. We use the mid-point here to define the depth of the reference data. Since sometimes multiple samples are taken at each location, we use the shallowest depth of the mid-point at each location. This has been clarified in the revised version (Lines 161—165) by using the term "shallow" instead of "small".

*Line 151- For each date of measurement at each location, talk about the locations and dates first… What locations are these dates at?*

We are sorry but we did not really understand the nature of this comment. However, we have SUMup data at specific locations (shown in Fig. 7a and 7b of the revised manuscript) sampled at different moments in the period between 1984 and 2017 and a time series at Dome C (location shown in Fig. 2 of the revised manuscript). We have tried to clarify this in the manuscript (Lines 160—168).

*Line 152, Dome C, where is this? Map?*

This is indicated in Fig. 2a of the revised manuscript and has been added in the text (Line 170).

*Line 159, By incorporating this information… I don't understand how the ERA5 data was used and why it was used. This needs to be better explained.*

To assess the difference between the measured, modelled and estimated densities, it is important to understand the effects of climate conditions. Therefore, we use the climatic data as a comparison. This should also resolve the comment below. We have adapted the description in the revised manuscript (Lines 174—181).

*In Section 2.5, are you talking about comparing model and observations (at points?).. and the satellites? I think this needs to be thought through and justified in the text. Comparing satellite and model data with single point measurements is tricky. There are a lot of references out there about how to do this, particularly in the climate modeling realm. I suggest the authors read some of these papers and at least add a discussion in the text around this.*

The idea of this section is to point out that potential errors with IMAU-FDM are linked to certain climate conditions, which can be propagated through the training process to further bias the results. ERA5 serves to help understand in which conditions IMAU-FDM leads to more ideal results. This analysis is done Antarctica-wide, and has nothing to do with comparing model and observations at points. We have clarified this in the revised manuscript (Lines 174—181)

*Sometimes you say "firn data" and other times you talk dry firn. Should this be defined? Can you make sure you are being consistent through the text?*

The definition of firn has been added to the introduction (Line 25) and "firn data" has been removed in the revised manuscript.

*Line 168m dry-snow zones, what are these?*

Section 3 (until 3.1) is a high level description of the next section to provide an overview of the approach. The dry-snow zones are therefore explained in Section 3.2, as is also indicated between brackets. It is a preview that will be explained in Section 3.2.

*Line 184, model training procedure. Which model are you talking about?*
(Now Line 203) Random forest model. This has been changed in the revised manuscript.

*Line 190-195, this is not very clear. Can you rephrase?*
We have rephrased it (Lines 206—213) in the revised manuscript.

*Line 196, variations of other properties. What other properties?*
This statement intended to tell that by removing the surface temperatures, the non-annual variations such as melt—refreezing cycles, potential precipitations and density or snow grain size variations could be kept, which in turn helps us distinguish different snow regions especially distinguish melt from non-melt regions and facilitates the following steps (please refer to Fig. 1 of this document as well). This has been rephrased in the revised manuscript (Line 216).

*Line 196, In addition, although may not have such large dependence on firn temperature as TB, we use its time series anomalies to maintain consistency with TB. This is unclear, can you rephrase?*
There was a mistake. sigma0 is also affected by firn temperature. We have rewritten the concept in the revised manuscript (Line 217).

*Line 204, 'distance' between pixels. Make sure to clarify that this is not spatial distance.*
Should be distance between features of the pixels. We rewritten it in the revised manuscript (Line 227).

*Line 205, between the parameters of different pixels. What parameters?*
TB anomalies and sigma0 anomalies. We introduced Fig. 1 in the revised manuscript to clarify which parameters are used in each step.

*Line 210, different satellite parameters, together with the IMAU-FDM density for each cluster. I do not understand what this means. What parameters? I think you need to make a table of parameters.*
The satellite parameters include brightness temperatures and derived ratios, as well as scatterometer backscatter intensity. We introduced Fig. 1 in the revised manuscript to clarify which parameters are used in each step.

*Line 217, RF regressor. Add more references since this approach is now widely used in climate science, for snow distribution mapping and other work.*
We have added Vafakhah (2022) and Viallon-Galinier (2023) in Line 241.

*Line 225, for pattern recognition in noisy datasets. Add a reference here.*
We refer the advantages of RF to Hastie et al. (2008), which is added in Line 249.

*Line 226, reduce the variance of the model and prevent overfitting. Add a reference.*

Please see the comment above.

*Did you consider other ML approaches?*
Please refer to the major comments and Line 470 of the revised manuscript.

*Line 230-onward. Are you building a RF for each time step to estimate the timeseries at each grid cell? How many samples in total went into the model? Line 244 talked about pixels, and the resulting sample size. Is this the total number of samples? Do you think this is enough to train a RF, especially considering the results?*
No, we do not rebuild an RF for each time step. We build one RF model that can be used for multiple time steps. The RF is tuned for multiple pixels (please refer to Fig. 1 of this document) and multiple time steps (366 daily samples between January 1 2011 and December 31 2020) using the training dataset through a 5-fold cross validation process, and the tuned RF is then used throughout the other experiments. For the details please refer to the major comments.

However, we do appreciate the suggestion to increase the sample size, and also considered using 10% of all pixels instead of 100 pixels for training. Please note again that when we use 10% of the pixels, the total number of samples consists of 1739pixels*366time_steps. The performances (RMSE and R^2) are shown below. From this comparison, we see that increasing the training samples slightly improves the RMSE overall, and enhances R^2 in some regions. Therefore, we have switched using 10% of pixels as training dataset in the revised manuscript.

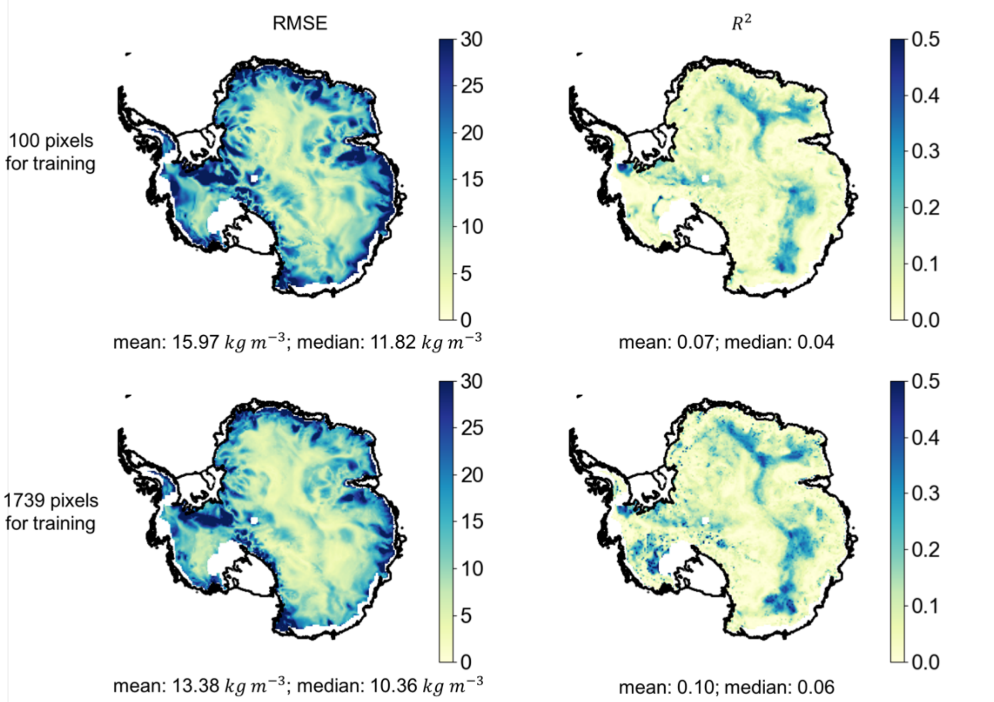


Figure 3. Comparison between using 100 pixels and 10% of the data for training.

*Table 1. These are all hyperparameters, do you call them parameters (which you use other times in the paper for the actual input parameters to the model, which in itself is confusing).*
They are not the same. Hyperparameters are hyperparameters used by the RF regressor, as stated in the caption of this table. It is also a typical element of supervised machine learning

(see Anilkumar et al. (2023)). Input parameters are referred to as parameters, including radiometer and scatterometer measurements and the derived products. Please refer to the major comments. However, we did make a mistake in writing the column name of this table, which has been corrected in the revised manuscript.

*Line 258. Why did you only use Gini importance vs other importance metrics? Did this choices affect any of your importance rankings?*
We considered the permutation importance, and the comparison has been shown in Fig 6 of the revised manuscript. The descriptions of the importance metrics have been added in Lines 284—293. Using the permutation importance indeed changed the rankings of the absolute values.

*Line 264, by means of the RMSE. Do you mean averages or do you mean via using RMSEs?*
Sorry for the confusion. We mean by using the RMSEs. This has been clarified in the revised manuscript Line 296.

*Line 269, this is the first time you refer to Figures… why do it here and not in the rest of the methods?*
This figure has been updated (see major comments) and specific locations have been shown in Fig. 2a of the revised manuscript.

*Line 277, 'cluster Firn 5', you have not introduced the cluster results yet so we do not know what these are.*
We have removed the reference to cluster 5 to avoid preliminary reference to the clusters.

*Figure 1, can you tell us which ones are from the satellites parameters and which ones are from the model? Again, a table might help with this.*
The datasets and the differences of data sources are: 1) the observable Tb from SSMIS satellite mission, 2) sigma0 from ASCAT satellite, and 3) densities from IMAU-FDM, which is a modelled parameter. We have also added a brief introduction in the revised Fig. A1.

*Line 283, especially at the location of Dome C, again, need to show on the maps or include a figure.*
We have added the locations in Fig. 2 of the revised manuscript (please also refer to the major comments).

*Line 287, There are a lot of other good reasons why the RF should be used, I do not think that this is the strongest one.*
We refer to Anilkumar et al. (2023) for the performance of the RF. We have added the advantages and references in the revised manuscript (Lines 243—247).

*Section 4.2 Do you think that you could produce different RF models for each cluster, perhaps? I think this would be very interesting to understand the difference between the performance of each of these models. For instance, if the dry firn can be modeled with lower RMSE /error than some of the other clusters. Honestly, I am still unclear if you did it this way or not.*
We are sorry that the approach was not 100% clear. The clusters are used to ensure that the training samples are selected in a way where different regions are sufficiently represented.

We did not train different RF models for different clusters although this should be feasible, but is outside of the scope of the current paper (Please also see the major comments). We have clarified this in the manuscript (Lines 206—213).

*Figure 4 and Figure 5a*
*These figures are making me wonder what it is that you are trying to do. For instance, are you trying to estimate the time series of the seasonality and variability you see in Figure 4 with the RF? What is the RF estimating, exactly…? You say "firn densities based on satellite parameters" and you talk about a time series, but I am wondering how you are doing this. What is X in Equation 4, actually? I do not know if this is ever said.*
Figures 4 and 5 (Figs. 5 and 6 in the revised manuscript) are from separate experiments. First, we would like to refer to the answer to the major comments. So, Fig. 5 shows that after clustering, dry firn zones and firn zones that experience melt can be distinctively recognised. However, between different dry firn zones, we cannot intuitively relate the time series to actual physical firn properties (mainly due to lack of field measurements). Nevertheless, by proportionally choosing training points within each cluster, we do observe an optimal performance of the experiment (see Fig. 2 of this document). We attribute this to the reason that all types of regions are represented sufficiently in this way.

X refers to the set of features.

*Figure 5b, are all these parameters standardized? Is the importance based on the standardized inputs? I just wonder because the anomalies appear to be the least important, which makes me wonder if perhaps the other parameters are not. Again, if these are not contributing much to the model, did you play around with them being removed? Does the model improve with fewer parameters? Are there any strong correlations between these parameters at all? How are they related or not related to each other?*
(Figure 6 in the revised manuscript) The parameters are not standardised. We assumed that random forest does not require standardising, as the tree partitioning depends on the scales of the independent variables. Moreover, Fig. A1 of the revised manuscript shows that sigma0 varies between -25dB and 0dB, yet ranks as an important feature.

Typically, all TB values are highly correlated to each other, as they are mainly affected by firn temperature. We performed a sensitivity analysis in the revised manuscript, shown in Appendix B.

*Line 301, The differences between these clusters mainly arise from deviations in TBanom and, to a lesser extent, sigma0anom. What is different about them?*
We notice that for cluster 1, TBanom varies between -5K and 5K, for cluster 2 and 3, TBanom varies between -5K and 10K, and for cluster 4, TBanom varies between -10K and 10K. Moreover, compared to cluster 2, TBanom of cluster 3 experienced a decreasing trend over time. What we could assume is that cluster 1 consists of most interior regions, hence is overall most stable, whilst cluster 4 is located in West Antarctica, hence is least stable (with the largest variations). The separation between cluster 2 and cluster 3 resembles Fig. 4 in Stokes et al. (2022), in which cluster 2 tends to lose mass while cluster 3 tends to slightly gain mass. However, we can only infer that this result might indicate that cluster 2 has a less stable

condition than cluster 3, but the conclusion is not solid. We have clarified these differences in the manuscript as well (Lines 327—332 and Lines 422—427).

*Line 298, If 1-4 are the basically the same, why are they not being treated as a single cluster?*
We would not conclude that clusters 1—4 are “basically the same”. Rather, for instance, Firn 1 shows the smallest variations in TB and sigma0 anomalies compared to the other clusters, which corresponds to the most stable firn conditions (as it also locates in the interior of the ice sheet). Firn 2—4 also exhibit such difference accordingly. Please also refer to the previous comment.

*Line 303, Are the melt events shown /evidenced in time in the region? Can you talk about this a little bit? You refer to a paper, but don’t go into detail otherwise.*
We refer to de Roda Husman et al. (2022), where it shows that satellite-based melt events are commonly well recognised.

*Line 305, Can you describe why how density would change under these melt events, and why? You do not give much background on that.*
After melt events, the density increases by refreezing. This is a typical phenomenon, which is also documented in Fig. 4 of Nilsson et al. (2015) that showed the high-density melt layers during the famous melt over Greenland in 2012. We added a reference to clarify that in this context (Line 420).

*Line 306, Firn 5, where the melt event of 2016 shows a prolonged effect on the anom time series due to the formation of a sub-surface refrozen high-density layer in IMAU-FDM. Again, what I the implications for this, and what does it mean for firn?*
We refer to Nilsson et al. (2015), where it shows that a sub-surface refrozen layer drastically changes the volume scattering mechanism hence changes the backscattering signals. This process is also clearly described in a recent review on firn, which is now referenced in the revised manuscript to clarify that (Line 337).

*Figure 6. Again, this figure only shows results as temporal averages. How did the time series of the RF do?*
We cannot show all the time series as there are 17649 pixels all together. That is the reason why we took 9 sample pixels to visualise the time series in Fig. 9, and conclude that the precise temporal performance of RF is compromised. Additionally, we want to stress that Fig. 8a and b show the RMSE and the correlation coefficient, therefore reflecting the time series behaviour of the RF model and not only the temporal average.

*Line 383, It is important to note that the wet firn clusters are not used in the following RF steps due to the complex impact of the melt–refreeze cycle on satellite observations. Again, I am thinking that the RF and this cluster analysis is not related.*
Please also refer to the major comments and Fig. 2 of this document. To briefly address this question, the clustering separated the wet firn from the dry firn, so it helps with the following analysis. The selection of training data based on the clusters also facilitates the RF process.

*Line 317, Exhibiting a linear relationship between predictors and the predicted variable – predictand? Saying it this way is confusing.*

(Now Line 348) This has been changed into the IMAU-FDM and RF densities in the revised manuscript.

*Figure 5, add units.*
(Now Fig. 6) This has been corrected in the revised manuscript.

*Figure 5a, Why do no values exceed this amount? I wonder if perhaps your training data set somehow selects lower firn values… are you randomizing between your training and test sets?*
As y-axis (IMAU-FDM densities) of the original figure shows, we have selected density values up to 500kg/m^3. Please note that within 4cm depth of the snow in Antarctica, it is normal to have most of the density below 400kg/m^3, so it is possible that the values that exceed 400kg/m^3 are less represented in the RF training process, which could indeed indicate a sampling issue.

In the revised manuscript, we use the 12cm densities and 10% of the pixels as training samples. New results show that although we manage to select more training data with densities up to 450kg/m^3, RF still shows an underestimation (up to 425kg/m^3). RF also shows an overestimation when the IMAU-FDM densities are lower than 325kg/m^3. We attributed this phenomenon to the limitation of using satellite data to represent firn processes in coastal regions and in regions with more varying topography, and added the discussion in the revised manuscript (Lines 391—394, Lines 465—468, Lines 485—487).

*Figure 6 shows that the model is basically as good as the RF (if not better at anything other than the mean). So, why do you need an RF model in this case? How difficult is the model to set up and apply? Again, is there a good reason for the RF here if it doesn't perform that well, is not finer in scale, or it doesn't really do that well except on average?*
Please refer to the major comments. The objective of our study is to assess the ability of using a combination of ML algorithms and satellite parameters to estimate firn densities, not to reproduce the modelled density. To do this, we require sufficient training data. However, due to the limitation of the in situ measurements, we use IMAU-FDM as an assumption of "real densities". Actually, as Fig. 7 and Fig. 10 indicate, IMAU-FDM does not capture many variations in the in situ data, resulting in temporal gaps in the RF estimations. This has been pointed out and analysed in Lines 442 onwards in the revised manuscript.

*Figure 6d, can you show which is which? Use different symbols instead of colors? They are difficult to differentiate. This intercomparison with observations is likely very challenging to achieve (which I think is what you are attempting to do). I might suggest some sort of spatial upscaling for single point /insitu observations.*
This has been changed in the revised manuscript (Fig. 7d).

*Figure 8 illustrates how poor the RF is for a time series. But, I am unclear if you are doing this in the right way. Clarity of methods is required.*
Please refer to the previous comments. IMAU-FDM can also introduce biases, however, we only use IMAU-FDM as a "known" data to assess our method, but not as an absolute ground truth. The recommendation for further studies have been proposed in Lines 454 onwards.

**Reference**

Anilkumar, R., Bharti, R., Chutia, D., and Aggarwal, S. P.: Modelling point mass balance for the glaciers of the Central European Alps using machine learning techniques, The Cryosphere, 17, 2811–2828, https://doi.org/10.5194/tc-17-2811-2023, 2023.

de Roda Husman, S., Hu, Z., Wouters, B., Munneke, P. K., Veldhuijsen, S., and Lhermitte, S.: Remote Sensing of Surface Melt on Antarctica: Opportunities and Challenges, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, pp. 1–20, https://doi.org/10.1109/jstars.2022.3216953, 2022.

Hastie, T., Tibshirani, R., and Friedman, J.: Random Forests, pp. 587–604, Springer New York, https://doi.org/10.1007/978-0-387-84858-7_15, 2008.

Nilsson, J., Vallelonga, P., Simonsen, S. B., Sørensen, L. S., Forsberg, R., Dahl-Jensen, D., Hirabayashi, M., Goto-Azuma, K., Hvidberg, C. S., Kjaer, H. A., and Satow, K.: Greenland 2012 melt event effects on CryoSat-2 radar altimetry, Geophysical Research Letters, 42, 3919–3926, https://doi.org/10.1002/2015gl063296, 2015.

Orsolini, Y., Wegmann, M., Dutra, E., Liu, B., Balsamo, G., Yang, K., de Rosnay, P., Zhu, C., Wang, W., Senan, R., and Arduini, G.: Evaluation of snow depth and snow cover over the Tibetan Plateau in global reanalyses using in situ and satellite remote sensing observations, The Cryosphere, 13, 2221–2239, https://doi.org/10.5194/tc-13-2221-2019, 2019.

Picard, G., Royer, A., Arnaud, L., and Fily, M.: Influence of meter-scale wind-formed features on the variability of the microwave brightness temperature around Dome C in Antarctica, The Cryosphere, 8, 1105–1119, https://doi.org/10.5194/tc-8-1105-2014, 2014.

Stokes, C.R., Abram, N.J., Bentley, M.J., Edwards, T.L., England, M.H., Foppert, A., Jamieson, S.S.R., Jones, R.S., King, M.A., Lenaerts, J.T.M., Medley, B., Miles, B.W.J., Paxman, G.J.G., Ritz, C., van de Flierdt, T., Whitehouse, P.L. Response of the East Antarctic Ice Sheet to past and future climate change, Nature, 608, 275–286. https://doi.org/10.1038/s41586-022-04946-0, 2022.

Vafakhah, M., Nasiri Khiavi, A., Janizadeh, S., Ganjkhanlo, H. Evaluating different machine learning algorithms for snow water equivalent prediction, Earth Sci Inform 15, 2431–2445. https://doi.org/10.1007/s12145-022-00846-z, 2022.

Viallon-Galinier, L., Hagenmuller, P., and Eckert, N.: Combining modelled snowpack stability with machine learning to predict avalanche activity, The Cryosphere, 17, 2245–2260, https://doi.org/10.5194/tc-17-2245-2023, 2023.

**Response to Emanuele Santi (Referee 2) on egusphere-2023-1556**

First, we would like to thank the Referee for reviewing and commenting on the manuscript, which will improve the quality of the manuscript. Please find the item-by-item reply below, with the original comments in *italics* and the responses in blue. All the suggested changes are implemented in the revised manuscript.

*The subject of this manuscript is of definite interest for the scientific community. Introduction correctly frames this study in the existing literature, language is clear, and thread deploys smoothly. Innovation with respect to other studies should be however better pointed out and description should be improved in some respects, as well as the presentation of the results. Beside this, the paper suffers from some lacks in the microwave background and I'm suspecting two conceptual issues: the first deals with the attempt to retrieve the density for the 4 cm top layer, which should be quite transparent at the considered MW frequencies in dry conditions. The second concern is about merging direct satellite measurements and derived indices in the RF inputs: based on the information theory, the indices should not bring any additional information independent of the Tb from which they have been computed, so, also based on my experience, these indices should negligibly affect the results.*

We thank the referee for the constructive review and suggestions. We have changed the introduction and the presentation of the results in the revised manuscript. Regarding the penetration of the MW frequencies, while we agree that theoretically a depth of 0.1—2 m should be a more reasonable choice for both 19 GHz and 37 GHz, as we cited in Line 99 of the revised manuscript (Surdyk, 2002; Brucker et al., 2010), our study is also based on the assumption that the frequency ratios should reflect near-surface (0—2 cm) density, as in Champollion et al. (2013) and Leduc-Leballeur et al. (2017). Therefore, as a compromise between the theoretical penetration depth and the aforementioned applications, we switched our FDM data to a depth of 0.12 m (instead of 0.04 m) to perform the experiments in the revised manuscript. But certainly, as the Referee also pointed out in the detailed comments, the relatively reasonable results can be obtained based on indirect correlation of the top layer density with deeper layers which indeed influence the adopted frequency more. This has also been included in our discussion in Line 461 of the revised manuscript.

Regarding the validity of using the derived indices, our study was motivated by Tran et al. (2007) and Champollion et al. (2013). Tran et al. (2007) combined a derived Tb ratio with Tb values to cluster snow facies in both Greenland and Antarctica, and Champollion et al. (2013) could associate frequency ratios to near-surface grain size and density at Dome C, Antarctica to a certain extent. In both studies, the validity of using such ratios exists to an extent that should be interesting to discuss, hence we included them.

Moreover, since we use Random Forest regression, we do not agree that the indices cannot bring additional information or performance. While the principle of information theory indeed suggests that indices derived from the original should not introduce additional information, it's important to consider the context in which certain techniques, such as random forest regression, operate. Random forest regression is a powerful ensemble learning method that harnesses the collective strength of multiple decision trees. In the case of random forest regression, the combination of diverse decision trees allows for the detection and extraction of intricate patterns and relationships within the data that may not be readily apparent in the original dataset. Each tree contributes its unique perspective, and the ensemble's output is often more robust and accurate than that of an individual tree. Therefore, although the indices derived from the original data may seem, from an information theory standpoint, to contain similar information, the strength of random forest lies in its ability to uncover latent, complex patterns that might not be explicitly present in the raw data. This enables the model to provide more nuanced and accurate predictions, surpassing the limitations of a single decision tree.

However, since our study aims to assess a method and discuss the validity of the parameters, we assume that a sensitivity analysis of using different combinations of parameters could be added, where we use as input:

- All parameters as what we are using now
- Only absolute Tb and sigma0
- Only absolute Tb and sigma0, and derived ratios (as also pointed out by Referee 1)

The comparison has been shown in Appendix B of the revised manuscript. The comparison shows that using all parameters still slightly outperforms the other combinations, so we still adopted its results in Figs. 6—9 in the revised manuscript.

*Detailed comments:*

*Introduction.*

- *The introduction contains a review of the state of the art more than enough to frame this paper. I would only suggest clarifying the aspects related to different spatial resolution, coverage and revisiting when mentioning active and passive MW.*
  We appreciate the recommendation and tried to add the information. It has been added in Lines 46 and 56 in the revised manuscript.

*Section 2*

*Section 2.1.*

- *Equation 1 and 2 are properly referred to the original publications, however a short sentence about the physical principles behind would be useful for the reader.*
  This has been added in the revised manuscript (Lines 114—117).
- *Line 94-95. The dramatic change in emission mechanism due to the presence of liquid water within the ice sheet might be commented, although this point is mentioned later in section 3.2. Same applies to the scattering in section 2.2.*
  This has been added in the revised manuscript (Line 101 and Line 137).

*Section 2.2.*

- *the linear correction for local incidence (LIA) sounds me a bit odd. LIA should be already accounted for when computing NRCS to extract the backscattering (σ°). In any case the backscattering dependence on LIA is not linear at all. Finally, as far as I understand* *from pag. 5 line 125, at the end you did not use data corrected with eq. 3. Could you further clarify?*

We apologize for not specifying the parameters properly. Equation 3 does not describe how we processed the data, but which kind of dataset we used. The same equation can be found in Lindsley and Long (2010), Eq. 3 on page 3. What we are using is the σ° normalized to the reference angle (40°), referred to as $A$ in this equation. The $A$ products we are using are already available via Brigham Young University (BYU) Microwave Earth Remote Sensing (MERS) laboratory platform and are directly used in our study. However, since $A$ as a single letter could be misleading, we called it σ° again in the following texts of the manuscript, which is more familiar to the common knowledge. We have changed that in the revised manuscript (Lines 131—136).

- *The spatial and temporal co-registration between ASCAT and SSMIS should be better described, this could lead to error and artifacts depending on the processing you applied. At the end, how many co-located Tb and σ° you obtained? It is an important information for better understanding the RF implementation, although something is addressed later.*

  We have tried to clarify the spatial and temporal co-registration between ASCAT and SSMIS better by adding some information on the interpolation methods we used (Lines 139, 157 and 182).

  Eventually, we obtained 19,027 valid pixels within the Antarctic ice sheet range (Table 1 of this document). We admit that with a linear interpolation, artifacts occur at the edge of the images. However, we filtered them out using the coastline from Depoorter et al. (2013). What falls within the range of the Antarctic ice sheet should be reliable.

*Section 2.3*

- *line 135 – 138. As stated in the general comments, the attempt to retrieve density at 4 cm raises a conceptual issue. The top 4 cm layer should be almost transparent not only at C-band but also at Ka band in case of dry firn. I'm wondering if you are obtaining results based on indirect correlation of the top layer density with deeper layers to which microwaves are instead sensitive. No wonders if RF achieves successful retrievals: machine learning can exploit almost any kind of input/output relation, but the risk of finding out something based on apparent relationships is always around the corner. If used as "black boxes", ML could potentially relate newborns in China and weather in USA, but which is the utility? I believe a robust physical justification is needed.*

  We agree that it is likely that we have obtained results based on indirect correlation of the top layer density with deeper layers. We have corrected this throughout the manuscript by using FDM data from 12 cm depth (Please refer to the major comments).
- *Line 138 – 140. The sentence is unclear to me, could you rephrase please. Where was density at 1m depth used later?*

  It was shown in Fig. 4 of the original manuscript to prove that melt events have a prolonged impact on deeper snow densities, hence our clustering step to separate dry and melted pixels was quite reliable. However, we agree that overall it does not have added values to the following analyses, hence removed all comments about the 1 m density in the revised manuscript.

*Section 3*

*Section 3.2.*

- *Is Tb Ratio the same of eq. 1? If so, no need to introduce it again with reference.*
  The Tb ratio from Tran et al. (2008) is not the same as our Eq. 1. However, we agree that this reference is repetitive to the introduction, hence removed it in the revised manuscript.
- *In my understanding, volume decorrelation was not introduced before. The cited work by Rizzoli is using X band SAR, it is not clear if this finding is also valid for radiometric measurements (scattering and emission are complimentary each other)*
  This application was introduced in Line 61 as previous studies as a motivation for applying the unsupervised classification method. We also removed it in Sect. 3.2 of the revised manuscript.
- *Line 195 – 199. The normalization by firn temperature is embedded in both parameters you defined in eq. 1 and 2. Which is therefore the reason for removing the average seasonal Tb signal? And which the one for doing the same with backscattering that is almost insensitive to temperature? Moreover, machine learning techniques as RF can cope with redundant, noisy, and biased data, so dealing with timeseries of measurements or their anomalies should not change much the results. Finally, there is also a concern in merging Tb with their ratios that is commented below.*
  This step (everything in Sect. 3.2) serves to separate dry pixels from pixels that suffered from melt, therefore Tb ratios and RF are not used here. While backscattering is almost insensitive to temperature (although Fraser et al. (2016) showed some correlations), it is very sensitive to melt events and subsequent melt layers, hence the anomalies should be a good indicator of melt pixels. We agree that this motivation has not been clarified in the manuscript, hence we rephrased it as Lines 201—213 of the revised manuscript.
- *Lines 201 – 211. The clustering algorithm should be better explained maybe with a supporting figure/diagram. I don't believe a reader unfamiliar with Ward algorithm can understand this section.*
  This has been implemented in the revised manuscript (Fig. 2 and Lines 222—231).

*Section 3.3*

- *Lines 231 – 246. With "sample" do you refer to the set of temporally and spatially coregistered SSMIS and ASCAT measurements for the given pixel? In my understanding, for both subsets I and II you selected randomly 100 pixels from the 7 clusters over Antarctica described in section 3.2 (that is spatial, 25 km resolution each pixel) and you considered the timeseries of satellite measurements (that is temporal, approx. 1 set of SSMIS + ASCAT measurements per pixel day per 10 years). At the end you should have used 365300 sets for training and the same data amount for testing. In other words, you considered about 125000 Km2 for training and testing and applied the trained RF on the remaining ≃14000000 Km2 of Antarctic surface, which is notable. Maybe some more information could be provided…*

We indeed refer to “the set of temporally and spatially coregistered SSMIS and ASCAT measurements for the given pixel”. This has been changed in the revised manuscript (Line 257). But for subsets I and II we selected random pixels from the 4 dry clusters instead of the 7 clusters (Table 1 below). We should also clarify a mistake in the original manuscript that since we use the 10-day resolution IMAU-FDM, the total time slots should be 366 instead of 3653. Therefore, we used approximately 0.6% of the total data for training. We now use 10% of the total data for training (as Dataset I) in the revised manuscript, as is slightly improves the result (Fig. 1 of this document) and is theoretically more reasonable than using 100 pixels (~0.6% of the data).

Table 1. Statistics of pixels per cluster and pixels used for RF estimating in the original manuscript.

| Cluster | Number of pixels | Number of Dataset I pixels | Number of Dataset II pixels |
|---|---|---|---|
| Firn 1 | 4540 | 454 | 26 |
| Firn 2 | 7360 | 736 | 42 |
| Firn 3 | 3465 | 346 | 20 |
| Firn 4 | 2284 | 228 | 12 |
| Firn 5 | 429 | 0 | 0 |
| Firn 6 | 325 | 0 | 0 |
| Firn 7 | 624 | 0 | 0 |
| Total | 19027 | 1764 | 100 |

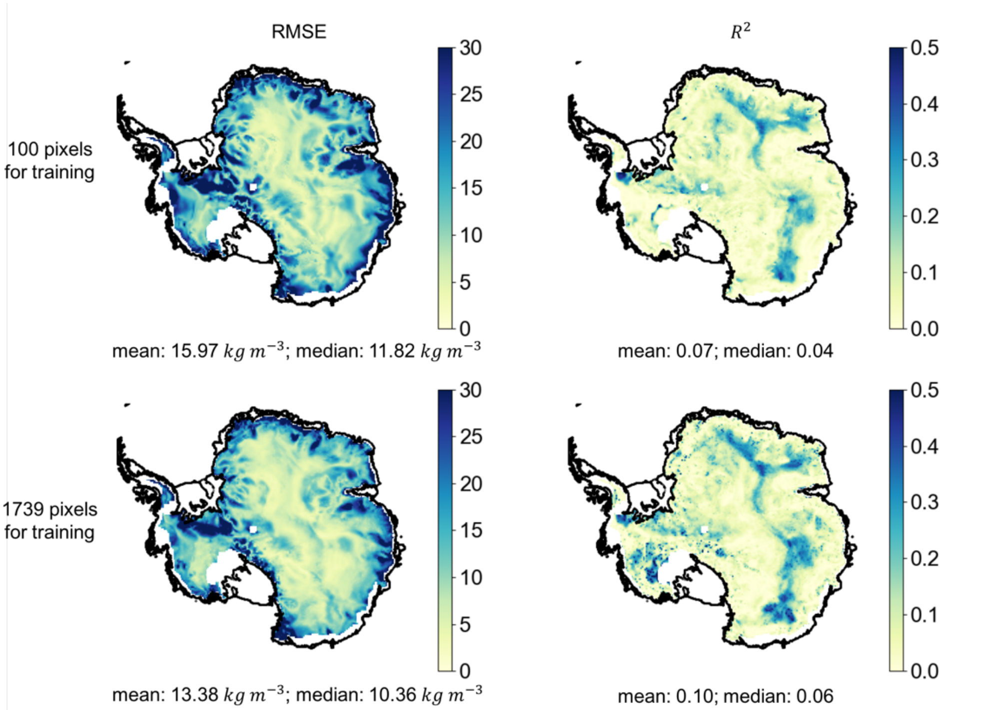


Figure 1. Comparison between using 100 pixels and 10% of the data for training.

- *Equation 4. The proposed input combination raises another concern: from the information theory, the Tb ratios do not bring to the RF additional information independent of the Tb from which they have been computed, therefore (this is also my* *personal experience) the results should not be affected by these inputs (or conversely by Tb if you use the ratios). Clarification is needed.*

Please refer to the major comments, where we argue why derived indices can effectively add information when used in Random Forest regression as these are based on decision trees and derived indices can play an important role there (as they might be important in different phases of the decision tree)

- *Line 258. Gini importance should be better referred and briefly commented. Which is the difference with e.g., predictor importance proposed by Breiman?*

  This is improved in the revised manuscript by including also predictor importance. Regarding the difference between the Gini importance and the permutation (Breiman) importance (Fig. 6 of the revised manuscript), we notice that using the permutation importance, the ranking of the original horizontal channels goes down. Therefore, we have used both in the revised manuscript.

*Section 4*

- *Section 4.1. Following the comment above, this is the core of my concerns: the scarce correlation with density at 4 cm could be depending on the microwaves' scarce sensitivity to such shallow depth. Also, the reverse correlation along the coasts should be depending on melting not entirely removed that occurs more frequently than in the central part of Antarctica. Again, the physics behind should be analysed.*

  We agree. A deeper snow density (12 cm) has been assessed, and the potential melting has been added in the discussion (Lines 417—422).
- *Figure 1. Although referred to in section 4.1, I find this figure poorly informative. My suggestion is to remove or replace with something more meaningful.*

  We would like to keep it to give the reader an overview of the parameters we used, including their spatial patterns. However, we moved it to Appendix A instead of using it as in the original manuscript.
- *Figure 2. Did you evaluate the correlation with density at 1 m? At the end which was the role of this parameter in your study?*

  We did not evaluate the correlation with density at 1 m. Originally we intended to show that our analysis should serve for multiple depths, however this was not well addressed in the original manuscript. Please note that in the revised manuscript, we opt for assessing the densities at 12 cm depth instead of 4 cm, therefore the descriptions have been revised accordingly.
- *Figure 4. The plots in the figure are quite small and difficult to read. I would suggest revising.*

  This has now changed into Fig. 5 in the revised manuscript.
- *Figure 5 left: the scatterplot should refer to the test results (i.e. those obtained on subset II), not to the training results (Subset I). Usually, retrieval scatterplots show the estimated vs. target, not vice-versa. The plot or caption should also cite the statistics and total data amount. Finally, the R value seems even worse than the one of direct correlation with Tb Ku and Ka in figure 2 for most of the pixels. Isn't it? Which is the explanation?*

  It was a mistake in the captions. We indeed used Dataset II for this analysis. This figure is changed in the revised manuscript where we also swapped the axes.

However, here the R^2 value (ranging between 0 and 1) refers to the linarity, i.e. if we fit a line to the estimated vs. target scatter plot, how the goodness of fit is. This is not the same indicator as the correlation coefficient (ranging between -1 and 1) in Fig. 3 of the revised manuscript. However, using a linear regression, R^2 is also equivalent to the correlation coefficient squared. For consistency, we switched to the correlation coefficient as the indicator of temporal performance in the revised manuscript. It can be observed that the correlation between IMAU-FDM and RF densities most resembles that between Tb(37V) and IMAU-FDM densities, which matches the importance shown in Fig. 6.

- *Figure 6 with doubled colorbar is difficult to interpret (especially figure 6d). I would suggest revising.*
  This has been changed into different markers (Fig. 7d of the revised manuscript), as also pointed out by Referee 1.
- *Figure 7 why do not also add the Correlation/Determination coefficient maps as those in figure 2? In my view this is more informative than e.g. the 10- years averaged maps of figure 6.*
  We have switched to the correlation coefficient in Fig. 8 of the revised manuscript (please also see comment above). An averaged map in our opinion shows that our method works reasonably well spatially (in contrast to the performance temporally, as shown in the sections afterwards).

**Reference**

Brucker, L., Picard, G., and Fily, M.: Snow grain-size profiles deduced from microwave snow emissivities in Antarctica, Journal of Glaciol ogy, 56, 514–526, https://doi.org/10.3189/002214310792447806, 2010.

Champollion, N., Picard, G., Arnaud, L., Lefebvre, E., and Fily, M.: Hoar crystal development and disappearance at Dome C, Antarctica: observation by near-infrared photography and passive microwave satellite, The Cryosphere, 7, 1247–1262, https://doi.org/10.5194/tc-7-1247-2013, 2013.

Depoorter, M. A., Bamber, J. L., Griggs, J., Lenaerts, J. T. M., Ligtenberg, S. R. M., van den Broeke, M. R., and Moholdt, G.: Synthesized grounding line and ice shelf mask for Antarctica, https://doi.org/10.1594/PANGAEA.819151, supplement to: Depoorter, MA et al. (2013): Calving fluxes and basal melt rates of Antarctic ice shelves. Nature, 502, 89-92, https://doi.org/10.1038/nature12567, 2013.

Fraser, A. D., Nigro, M. A., Ligtenberg, S. R. M., Legrésy, B., Inoue, M., Cassano, J. J., Kuipers Munneke, P., Lenaerts, J. T. M., Young, N. W., Treverrow, A., van den Broeke, M., and Enomoto, H.: Drivers of ASCAT C band backscatter variability in the dry snow zone of Antarctica, Journal of Glaciology, 62, 170–184, https://doi.org/10.1017/jog.2016.29, 2016.

Leduc-Leballeur, M., Picard, G., Macelloni, G., Arnaud, L., Brogioni, M., Mialon, A., and Kerr, Y.: Influence of snow surface properties on L-band brightness temperature at Dome C, Antarctica, Remote Sensing of Environment, 199, 427–436, https://doi.org/https://doi.org/10.1016/j.rse.2017.07.035, 2017.

Lindsley, R. D. and Long, D. G.: Standard BYU ASCAT Land/Ice Image Products, Tech. rep., Brigham Young University Microwave Earth Remote Sensing (MERS) Laboratory, https://www.scp.byu.edu/docs/pdf/MERS1002.pdf, [Access date: Oct. 12, 2023], 2010.

Surdyk, S.: Using microwave brightness temperature to detect short-term surface air temperature changes in Antarctica: An analytical approach, Remote Sensing of Environment, 80, 256–271, https://doi.org/10.1016/s0034-4257(01)00308-x, 2002.

Tran, N., Remy, F., Feng, H., and Femenias, P.: Snow Facies Over Ice Sheets Derived From Envisat Active and Passive Observations, IEEE Transactions on Geoscience and Remote Sensing, 46, 3694–3708, https://doi.org/10.1109/tgrs.2008.2000818, 2008.